Essays on Individual Decision making

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Abstract

This thesis consists of three chapters exploring how individuals make decisions (mostly in relation to time), how decisions are influenced by subtle behavioral interventions called *nudges*, and under which circumstances the effectiveness of the nudges can change. The first chapter shows, in several online experiments and one field experiment in the context of a real market, that the endowment effect (or difference between buying and selling prices) systematically increases as transactions are delayed into the future. In the second chapter, present bias is studied in the gain and the loss domains in a two-stage incentivized experiment, which reveals that both domains show the bias but it is stronger in the loss domain. The third chapter studies how emotions affect the effectiveness of nudges in four experiments. However, emotions consistently failed to have an influence on the effectiveness of nudges, and the expected effects of the nudges themselves failed to replicate previous findings. These results raise doubts about the general effectiveness of some of the most prominent nudging tools.

Resumen

Esta tesis consta de tres capítulos que exploran cómo las personas toman decisiones (principalmente en relación con el tiempo), cómo las decisiones se ven influenciadas por sutiles intervenciones de comportamiento llamadas "*nudges*", y en qué circunstancias pueden cambiar la efectividad de las nudges. El primer capítulo muestra, en varios experimentos online y un experimento de campo en el contexto de un mercado real, que el efecto dotación (o diferencia entre los precios de compra y de venta) aumenta sistemáticamente a medida que las transacciones se llevan hacia el futuro. En el segundo capítulo, el sesgo hacia el presente ("present bias") se estudia en los dominios de las ganancias y de las pérdidas en un experimento incentivado de dos etapas, que revela que ambos dominios muestran el sesgo pero es más fuerte en el dominio de las pérdidas. El tercer capítulo estudia cómo las emociones afectan la efectividad de las nudges en cuatro experimentos. Sin embargo, consistentemente, las emociones no tuvieron influencia sobre la efectividad de las nudges, y los efectos de las nudges en sí no lograron replicar investigaciones previas. Estos resultados generan dudas sobre la efectividad general de algunas de las herramientas de "nudging" más prominentes.

Preface

How do people make decisions and how can researchers effectively help them improve their decisions? It is known that individual behaviors often deviate from economically rational behaviors assumed by conventional economic theory. In many cases, these behaviors can be systematically predicted. For example, many people overvalue present outcomes and show time-inconsistent behavior. These people tend to save less money, drink more alcohol, go to the gym less often and finish their academic papers later than they planned. These behaviors sometimes result in bad consequences such as obesity, saving deficiency and alcoholism.

Because countless empirical studies have shown these economically irrational individual behaviors, behavioral scientists have started to use a new type of intervention called *nudge* to change individual behaviors, instead of solely relying on more conventional interventions such as influencing people through financial incentives. Nudges that subtly change environments in which people make decisions can greatly help people make better choices, without interfering with freedom of choice. These kinds of nudges have had much impact on both the private sector and in economic and social policy. Several countries have created government bodies (at the national and at the local level), known as Behavioral Insights Teams, dedicated to the design of economic and social policies based on nudging to guide the behavior of consumers and citizens.

Individual decision making and nudges have been actively investigated, but many open questions are still unexplored. This thesis tackles some of the important open questions through experimentation, including online, lab and field experiments. Chapter 1 investigates how the difference between selling and buying prices changes when the timing of transactions is delayed; Chapter 2 examines the difference between time preferences in the gain and loss domains with an incentivized experiment; and Chapter 3 studies the effect of emotions on the effectiveness of nudges.

Chapter 1, "The Endowment Effect in the Future: How Time Shapes Buying and Selling Prices", co-authored with Daniel Navarro-Martinez, examines how the endowment effect (or gap between buying and selling prices) changes when transaction timing is delayed into the future. The endowment effect is one of the most prominent phenomena in behavioral economics, with important implications for a variety of situations related to buying, selling and evaluating resources (for reviews, see Horowitz & McConnell, 2002; Kahneman et al., 1991; Tunçel & Hammitt, 2014). A leading explanation of the endowment effect is loss aversion. In other words, sellers are reluctant to give up items they are endowed with because they are averse to losing it (Kahneman & Tversky, 1979; Tversky & Kahneman, 1991). However, virtually all research on the endowment effect has investigated transactions that take place in the present (i.e., buying or selling items that will be exchanged here and now). This is a very significant limitation, given that many real-world transactions have a temporal dimension. In many circumstances, people agree on a purchase or a sale but the transaction does not materialize until a later time in the future, for example in almost all forms of online buying and selling.

We present five experiments to investigate how the endowment effect (in terms of buying versus selling prices) is affected by delaying transactions into the future. We demonstrate that the endowment effect is systematically amplified as transactions are moved into the future. Buying prices consistently decrease as transactions are delayed, while selling prices remain roughly constant, resulting in an increasing gap between them. This pattern is not a result of discounting the money involved in the transaction and is largely a feature of moving the exchange of the target item in time. The same pattern holds across different types of items, and it is also obtained in the field, in a real market and with real transactions. In addition, we provide evidence that the phenomenon cannot be explained by sellers anticipating becoming increasingly attached to the items over time.

Our experiments provide converging evidence that endowment effects significantly increase as transactions are delayed, as we see in many real-world settings, such as online markets. This suggests that existing experimental research on the endowment effect may have actually underestimated its magnitude in some more realistic environments and has important implications for the design of market institutions. Exchanging goods as soon as possible might be important to reach agreements between buyers and sellers.

Chapter 2, "Time Preferences in the Gain and Loss Domains: An Incentivized Experiment", co-authored with Shotaro Shiba and Nobuyuki Hanaki, investigates time preferences in the gain and loss domains with an incentivized experiment.

Many of our important decisions involve a time component, and these decisions often involve gains and losses. Individual time preferences, in particular present-biased preferences, have been investigated for decades (Abdellaoui et al., 2013; Andersen et al., 2008; Andreoni & Sprenger, 2012; Benhabib et al., 2010; Frederick et al., 2002; Laibson, 1997; Loewenstein & Thaler, 1989; Thaler, 1981). A person with present-biased preferences overvalues present outcomes or undervalues future outcomes. Such preferences could explain many serious problems in our life such as insufficient savings (Laibson, 1997), credit card debt (Meier & Sprenger, 2010), excessive body mass index (BMI) (Courtemanche et al., 2015) and smoking addiction (Ida, 2014).

Time preferences are different in decisions involving gains and in decisions involving losses, in terms of not only simple individual discount rates but also present bias (Abdellaoui et al., 2013; Shiba & Shimizu, 2019; Thaler, 1981). However, no study has investigated present bias in the loss domain with an incentivized experiment.

Our two-stage experiment allows us to achieve an unbiased incentivization of time preference elicitation in the loss domain. In the first stage, the participants took a part of a non-verbal IQ test (the advanced version of Raven's Progressive Matrices Test, Raven, 2003) and earned an amount of money which was enough to cover the maximum possible loss in the second stage. Two weeks after the first stage, time preferences in the gain and loss domains were elicited. Using Raven's test in the first stage also allows us to analyze the relationship between cognitive skills and present bias, as well as impatience (IDRs), in both domains.

The results from our incentivized experiment consistently show that time preferences in the gain and loss domains are different. A descriptive analysis shows that immediate future losses are more heavily discounted than immediate future gain, while both are only mildly discounted in the further future. It appears that, therefore, the present bias is more severe in the loss domain. Further investigation through regression analyses reveals that there is a significant level of present bias in both domains and that the present bias is indeed more severe in the loss domain. These results are in line with Abdellaoui et al. (2013).

Chapter 3, "Nudges and Emotional States", co-authored with Daniel Navarro-Martinez, Jordi Quoidbach and Satoshi Akutsu studies the effect of emotions on the effectiveness of nudges.

Despite the widespread (and increasing) application of nudging, it is still unclear which are the determinants of the effectiveness of particular interventions. When, where and in which circumstances are default options, social norms, reminders, etc. most effective? In this paper, we focus on one particularly relevant aspect: people's emotional state when they are being nudged.

Currently, there is a large volume of evidence showing that emotional states substantially affect decision making (see Lerner et al., 2015, for a review). There are also several influential theoretical frameworks that focus on this idea. Research in this area shows that emotions play an important role in decision making, which leads to our hypothesis that emotional states will substantially affect the effectiveness of nudging, that is, how and how much nudging affects decisions.

Given the lack of previous research on this particular question, we can only conjecture about the exact effects we will obtain. In this sense, our research is somewhat exploratory in nature. What seems clear is that we should expect distinct emotional states to affect different nudging interventions differently. We focus on two different types of nudges to answer these questions: default options and social norms. These are two of the most widely used nudging tools and two of the ones with the strongest effects. Moreover, they rely on different psychological processes and we expect them to be affected differently by emotional states.

Experiment 1 used an experience-sampling approach to test if emotional states affect the effectiveness of default and social nudges designed to push people into doing effort tasks. In this experiment, we measured emotional states instead of inducing them, to be able to cover a wider range of emotions and to investigate emotions that people naturally experience (as opposed to induced emotions). Experiments 2 and 3 used a simpler online design to examine the effect of specific emotion inductions on the effectiveness of a default nudge. Experiment 4 studied the effect of the same emotion inductions on the effectiveness of a social nudge.

Our experiments consistently found no impact of emotional states on the effectiveness of nudges. Furthermore, our results are complicated by the fact that, in three of our four experiments, we did not replicate the expected effects of the nudges, which raises doubts about the general effectiveness of some of the most prominent nudging tools.

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Chapter 1

THE ENDOWMENT EFFECT IN THE FUTURE: HOW TIME SHAPES BUYING AND SELLING PRICES

Abstract

Previous research has focused on studying the endowment effect for transactions that take place in the present. Many real-world transactions, however, are delayed into the future (i.e., people agree to buy or sell, but the actual transaction does not materialize until a later time). Here we investigate how transaction timing affects the endowment effect. In 5 studies, we show that the endowment effect systematically increases as transactions are delayed into the future. Specifically, buying prices significantly decrease as the transaction is delayed, while selling prices remain constant, resulting in an amplified endowment effect (Experiment 1). This pattern is not produced by a discounting of the money involved in the transaction (Experiment 2), and it holds across different types of items (Experiment 3). We also show that the phenomenon cannot be explained by sellers anticipating becoming increasingly attached to the items over time (Experiment 4). Finally, we demonstrate that this increased endowment effect in the future holds in the field, in the context of a real market and with real transactions (Experiment 5).

1.1 Introduction

It has been widely documented that, when people are endowed with an item, they ask for a greater compensation to give it up than they would be willing to pay to acquire it. This pattern has been called the *endowment effect* (Thaler, 1980) and it is one of the most prominent phenomena in behavioral economics, with important implications for a variety of situations related to buying, selling and evaluating resources (for reviews, see Horowitz & McConnell, 2002; Kahneman et al., 1991; Tunçel & Hammitt, 2014). However, virtually all research on the endowment effect has investigated transactions that take place in the present (i.e., buying or selling items that will be exchanged here and now). This is a very significant limitation, given that many real-world transactions have a temporal dimension. In many circumstances, people agree on a purchase or a sale but the transaction does not materialize until a later time in the future, for example in almost all forms of online buying and selling. In this paper, we investigate how delaying transactions into the future affects the endowment effect.

The most typical account of the endowment effect is in terms of loss aversion (Kahneman & Tversky, 1979; Tversky & Kahneman, 1991). According to this explanation, buyers see an item they may acquire as a potential gain, whereas sellers view the same item they may give up as a potential loss. Because losses have been shown to loom larger than gains, this creates the asymmetry between the two parties known as the endowment effect. Many other explanations and moderating factors have been suggested (see, e.g., Burson et al., 2013; Georgantzís & Navarro-Martinez, 2010; Johnson et al., 2007; Morewedge et al., 2009; Morewedge & Giblin, 2015; Plott & Zeiler, 2007; Walasek et al., 2014), but loss aversion remains the prototypical account.

The most established way to measure the endowment effect (and the way we elicit it in this paper) is in terms of willingness to accept (WTA) and willingness to pay (WTP). In a typical experiment, participants are randomly assigned to one of two conditions: one in which they are endowed with a target item and are asked for their WTA to sell it, and one in which they are not endowed with the item and are asked for their WTP to acquire it. WTA is normally higher than WTP, which constitutes the endowment effect (also called WTA-WTP disparity in this framework).

We find it surprising that few papers have investigated how the endowment effect, WTA and WTP, or loss aversion relate to time, given that transactions with a temporal component or delay are very common in daily life. One of the clearest examples of this is arguably online markets such as Craigslist, eBay or Facebook Marketplace, where people buy and sell items, typically by agreeing on an exchange sometime in the future (in one day, one week, one month, etc.). These markets are growing and are home to billions of transactions of very diverse goods every year. For instance, Mark Zuckerberg stated in May 2018 at Facebook's F8 developer conference that Facebook Marketplace was used by 800 million people per month. On all these platforms, the endowment effect, with its associated reluctance to trade, is likely to make agreements and exchanges between buyers and sellers more difficult (see Bar-Hillel & Neter, 1996; Kahneman et al., 1990; Knetsch, 1989). If the endowment effect is mitigated when transactions are moved into the future, then delaying transactions may be a way to alleviate these frictions. If, on the contrary, delayed transactions amplify the endowment effect, then sooner exchanges will maximize the chances of getting to an agreement. Apart from these markets, online shopping more generally usually involves time delays, for example from Amazon or AliExpress, travel agencies, supermarkets, etc. Also outside the Internet, delayed transactions are widespread. A typical example would be buying or selling a car. The parties typically agree on the sale, but then there are several steps before the actual exchange happens (paperwork, often ordering the car, etc.). The same holds for countless other items of different types.

There is a small literature that has related loss aversion and the endowment effect to time in different ways, although none (to the best of our knowledge) in terms how delayed transactions affect the endowment effect. Several papers have documented the so-called *sign effect*, in which gains of money are shown to be discounted in time more than losses (Frederick et al., 2002; Thaler, 1981). Hardisty & Weber (2009) investigated this pattern in three different domains (money, the environment and health), showing that the sign effect holds in all three but is stronger in the health domain. Molouki et al. (2019) then showed that the effect is linked to the emotional reactions experienced when contemplating the delayed outcomes in the process of waiting for them. Bilgin & Leboeuf (2010) suggested that it may also be partially explained by the fact that time intervals that finish with a loss are perceived as shorter than intervals that finish with a gain. The sign effect, however, has not been studied in the context of the endowment effect or of the valuation of goods more generally. Loewenstein (1988) showed that WTP for a cassette recorder decreased as obtaining the recorder was delayed for one year, but he did not elicit WTA. If the sign effect holds in the context of the valuation of goods, we should expect an increasing endowment effect as transactions are delayed, because WTP would decrease more than WTA.

But goods are different from money because they generate attachment, and this could interact with time delays in different ways. On the one hand, there is evidence that people adapt to owning things and get increasingly attached to their possessions over time (Strahilevitz & Loewenstein, 1998), at least under some circumstances. If people anticipate this adaptation, this could magnify the sign effect in the context of goods, potentially even leading to an increasing WTA as transactions are delayed. This effect, however, is unlikely to be substantial, given that people have been shown not to significantly anticipate attachment in endowment effect situations (Loewenstein & Adler, 1995; Van Boven et al., 2000, 2003).

On the other hand, there is evidence that the endowment effect is linked to some extent to affective reactions (Peters et al., 2003; Reb & Connolly, 2007; S. B. Shu & Peck, 2011; Y. Zhang & Fishbach, 2005), and we know that affective reactions are much more prevalent in relation to the present than to the future (Loewenstein, 1996, 2000). This could potentially undermine the endowment effect when transactions are delayed, by decreasing WTA. In other words, giving up something one owns might feel less dramatic if one only has to part from it in the future.

Overall, there is not a clear-cut prediction coming from previous literature and our research is, in that sense, exploratory. We present five experiments to investigate how the endowment effect (in terms of WTA versus WTP) is affected by delaying transactions into the future. In Experiment 1, we demonstrate that the endowment effect is systematically amplified as transactions are moved into the future. Buying prices consistently decrease as transactions are delayed, while selling prices remain roughly constant, resulting in an increasing WTA-WTP gap. Experiment 2 shows that this pattern is not a result of discounting the money involved in the transaction and is largely a feature of moving the exchange of the item in time. In Experiment 3, we replicate the same effect across different types of items. Experiment 4 provides evidence that the phenomenon cannot be explained by sellers anticipating becoming increasingly attached to the items over time. In Experiment 5, we show that the same pattern of an increased endowment effect in the future is obtained in the field, in a real market and with real transactions.

Our experiments provide converging evidence that endowment effects significantly increase as transactions are delayed, as we see in many real-world settings, such as online markets. This suggests that existing experimental research on the endowment effect may have actually underestimated its magnitude in some more realistic environments and has important implications for the design of market institutions. Exchanging goods as soon as possible might be important to reach agreements between buyers and sellers.

1.2 Experiment 1: The Endowment Effect Moves to the Future

Our first study was designed to test how the endowment effect, in terms of the WTA-WTP gap, changes as transactions are progressively moved into the future, as it is typically seen in online markets.

1.2.1 Method

Participants. We recruited 300 participants for our experiment via Amazon Mechanical Turk (50% female, M_{age} = 38 years, age range: 19-76 years). The study took an average of 5 minutes and 47 seconds to complete and subjects received a fixed fee of $0.5 for their participation. We excluded from our sample one subject who did not enter the code participants needed to provide to receive payment.

Design and procedure. Following standard practice in endowment effect experiments, participants were randomized into a buyer or a seller condition. In the seller condition, people were asked to imagine that they had received an item as a gift, so that they now owned the item. In the buyer condition, they were asked to imagine that they had the opportunity to buy that same item, without being endowed with it. The item used in this experiment was a framed Game of Thrones poster with a retail price of €18.92.

Participants were then asked to evaluate either selling or buying the poster (depending on the condition) and making the transaction in the present and in different future moments. Specifically, the sellers were asked "what is the minimum amount of money ($X) that you would require to sell the item and do the exchange (of money and item) [at time t]?"; the buyers were asked "what is the maximum amount of money ($X) that you would be willing to pay to buy the item and do the exchange (of money and item) [at time t]?" The transaction timing [at time t] was either today, tomorrow, in 1 month, or in 1 year. These four different time scenarios were randomized within subjects. We also used a graphical display to clarify the transaction timings (see Figure 1.A1 in the Appendix).

Before responding to each of the four time scenarios, all participants had to correctly answer a qualification question to verify they had understood the task. If participants chose an incorrect response, this information was recorded and a pop-up window appeared and warned them that the answer was wrong. Participants could not proceed until they answered correctly. After the main scenarios, participants were asked how much they liked the item (on a scale with seven stars) and, in the seller condition, also how strongly they felt ownership of the item (on a 7-point scale from 0 = *not at all* to 6 = *very strongly*). Finally, they were asked to complete a brief demographic survey, asking about their gender, age, English level, field of professional specialization, level of education, native language, and also how clear the instructions were.

1.2.2 Results and discussion

Table 1.1 reports summary statistics for Experiment 1; Figure 1.1 presents a box plot showing the main patterns obtained in WTA and WTP across the different time scenarios.[1]

Table 1.1: Descriptive Statistics (Experiment 1)

	Time	Median	Mean	SD	Total N	Wrong&outliers
WTA	Today	20.2	36.7	41.2	150	21
	Tomorrow	20.0	32.8	39.4	150	34
	1 month	20.1	35.5	39.5	150	34
	1 year	25.0	50.5	59.7	150	29
WTP	Today	10.0	14.3	14.3	149	15
	Tomorrow	10.0	12.4	13.3	149	28
	1 month	6.2	11.8	13.1	149	22
	1 year	5.1	10.8	12.7	149	26

When discussing our results, we will focus mostly on the medians (rather than the means), which are more robust to extreme values. All the main patterns, however, hold in terms of means as well (see Table 1.1). Consistent with previous findings on the endowment effect, the median WTA today ($20.2) was substantially higher than the median WTP today ($10.0), and this difference was statistically significant (Mann-Whitney test: $z = 5.51$, $p < .01$). As Figure 1.1 shows, WTA was roughly constant over time, with even a small increase in the 1 year scenario, while WTP consistently decreased as the transaction was delayed in time, resulting in an increasing endowment effect (i.e., WTA-WTP disparity) across time scenarios.

[1] WTA and WTP included a few disproportionately high values that suggested either mistakes or a lack of understanding, with a maximum as high as $1,000 for WTA and $600 for WTP. Therefore, the descriptive statistics and graphs reported here exclude observations with values more than one standard deviation above the mean. On average, 11.8% of the participants gave wrong answers to the qualification question presented before each scenario. Our descriptive statistics also exclude these observations. All the fundamental patterns obtained are the same without these exclusions (the analyses are available on request). Our regression analyses, however, include all observations, using quantile regression methods to minimize the impact of outliers (Hao & Naiman, 2007) and controlling for the wrong answers with an additional variable.

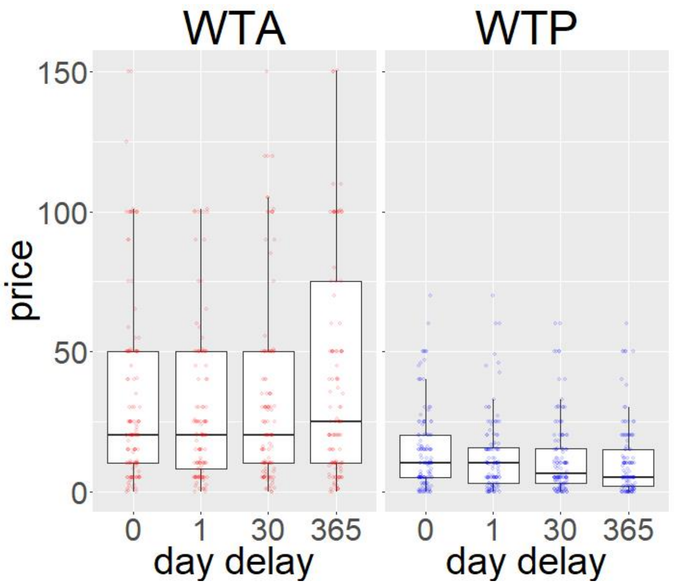

Figure 1.1: Selling (WTA) and Buying (WTP) Prices across Time Scenarios (Experiment 1). Each dot represents one observation. The horizontal line inside each box is the median; the bottom and top of the box are the first and third quartile, respectively.

To further analyze these patterns, we conducted an analysis based on quantile regressions using both conventional and clustered standard errors (at the level of the individual) (Table 1.2). We separately regressed WTA and WTP on two variables called Delay and Wrong. The Delay variable captures the different time scenarios measured in days of delay, so that it takes the value 0 if the scenario is today, 1 if it is tomorrow, 30 if it is in 1 month, and 365 if it is in 1 year. The variable Wrong is a dummy variable, taking the value 1 if the answer to the qualification question was incorrect. The regression results confirm that WTA did not significantly change across time scenarios, showing even a significant increase in Regression 2 (with clustered standard errors). On the contrary, WTP significantly decreased as the transaction was delayed. Specifically, median WTP decreased by around 1.3 cents per day of delay on average.

Overall, Experiment 1 clearly shows that the endowment effect is amplified as transactions are moved into the future, in the form of a flat (or even somewhat increasing) WTA across time and a consistently decreasing WTP.

1.3 Experiment 2: Separating the Discounting of Item and Money

In Experiment 1, both the transaction of the item and of the money happened at the same time in the future. This resembles many real-world settings, such as online markets, in which buyers and sellers agree on a future moment to exchange money and item. However, this makes it difficult to know how the temporal discounting of these two elements (item and money) contributed to the pattern we observe. While it has been argued that buyers do not evaluate the money paid to acquire items as a loss (Novemsky & Kahneman, 2005), it could still be that to some extent sellers discount the future money they will receive (which is a gain for them) more than buyers discount the money they will pay (which is a loss for them). This could contribute to the increasing

WTA-WTP disparity we obtained in Experiment 1. The main goal of Experiment 2 was to investigate the endowment effect in the future, controlling for this aspect. To achieve this, we fixed all the money transactions to take place in the present. This also corresponds to some real-world settings, such as buying and selling with upfront payments.

Table 1.2: Quantile Regression Analysis (Experiment 1)

	(1)	(2)	(3)	(4)
	WTA	WTA	WTP	WTP
Delay	0.015	0.015^{**}	-0.013^{***}	-0.013^{***}
	(0.012)	(0.007)	(0.005)	(0.003)
Wrong	5.000	5.000	0.390	0.390
	(5.602)	(5.178)	(2.535)	(2.086)
Constant	24.552^{***}	24.552^{***}	10.013^{***}	10.013^{***}
	(2.360)	(3.099)	(0.954)	(1.502)
Clustered SE	No	Yes	No	Yes
N	600	600	596	596

Notes: Standard errors in parentheses; *, ** and *** stand for statistical significance at the 10%, 5% and 1% level respectively.

1.3.1 Method

Participants. We recruited 200 participants (50% female, M_{age} = 36 years, age range: 19-86 years), who had not participated in Experiment 1, via Amazon Mechanical Turk. The study took an average of 5 minutes and 16 seconds to complete and participants received a fixed fee of $0.5 for their participation.

Design and procedure. The design and procedure used in this experiment were the same as in Experiment 1, except that all monetary transactions were fixed to take place in the present.

In this case, the sellers were asked "what is the minimum amount of money ($X) that you would require receiving today to sell the item and give it up [at time t]?"; the buyers were asked "what is the maximum amount of money ($X) that you would be willing to pay today to receive the item [at time t]? " As in Experiment 1, the transaction timing of the item [at time t] was today, tomorrow, in 1 month, or in 1 year, with the different time scenarios randomized within subjects. We also used the same type of graphical display to clarify transaction timings.

1.3.2 Results and discussion

Table 1.3 reports summary statistics for Experiment 2; Figure 1.2 shows the patterns obtained in WTA and WTP across the different time scenarios.[2]

Table 1.3: Descriptive Statistics (Experiment 2)

	Time	Median	Mean	SD	Total N	Wrong&outliers
WTA	Today	20.0	36.1	41.1	93	10
	Tomorrow	20.0	31.0	36.5	93	27
	1 month	20.0	30.4	31.1	93	20
	1 year	20.2	32.4	33.4	93	26
WTP	Today	10.0	13.3	14.8	107	15
	Tomorrow	10.0	12.7	13.5	107	21
	1 month	5.1	9.9	10.4	107	16
	1 year	3.5	6.5	9.0	107	21

Again, the results were broadly in line with previous findings on the endowment effect, namely, median WTA today ($20.0) was higher than median WTP today ($10.0), and this difference was statistically significant (Mann-Whitney test: z = 4.50, p < .01). As Figure 1.2 shows, WTA was again roughly constant over time (in this case without the slight increase in the 1 year scenario obtained in Experiment 1), while WTP again progressively decreased as the transaction of the item was delayed, which resulted in an increasing endowment effect across time scenarios. We also conducted the same quantile regression analysis as in Experiment 1 (Table 1.4). The first two columns of Table 1.4 confirm that WTA did not change across time scenarios. The last two columns of the table show that WTP significantly decreased over time, by an average of 1.4 cents per day of delay (slightly more than in Experiment 1).

As in this experiment only the item was moved in time, we can also cleanly estimate discount factors for it based on the WTA and WTP valuations, which we have done using the classic exponential discount function used in economics (Samuelson, 1937), $D(t) = \delta^t$, where t is the time delay to receive the relevant outcome and δ is the discount factor. $\delta = 1$ implies no discounting of outcomes as they are delayed; values of δ closer to zero imply greater temporal discounting. Including all observations, the yearly

[2] As in Experiment 1, the descriptive statistics and graphs reported exclude WTA and WTP values more than one standard deviation above the mean and observations in which participants failed to correctly answer our qualification question (which happened on average in 15.1% of the responses). Our regression analyses, as in Experiment 1, include all observations and use quantile regression methods, with an additional variable for the wrong answers.

discount factor in the seller condition was δ_{WTA}^{365} = 1.01; in the buyer condition, it was δ_{WTP}^{365} = 0.59. This shows that in the seller condition the value of the item was not discounted, while in the buyer condition the item lost on average 41% of its value in one year. Table 1.A1 in the Appendix contains the details of these discount factor estimations.

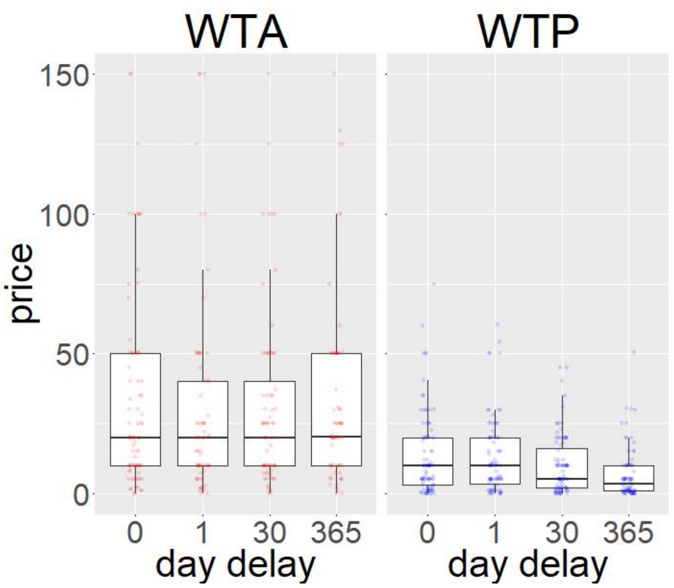

Figure 1.2: Selling (WTA) and Buying (WTP) Prices across Time Scenarios (Experiment 2). Each dot represents one observation. The horizontal line inside each box is the median; the bottom and top of the box are the first and third quartile, respectively.

The results of Experiment 2 show again that the endowment effect was consistently amplified as the transaction of the item was moved into the future, this time controlling for the discounting of the money involved in the transactions by fixing all monetary exchanges to take place in the present. More specifically, WTA remained constant as the item was delayed, but WTP progressively decreased, resulting in an increased WTA-WTP disparity. The patterns obtained in Experiment 2 are very similar to the ones in Experiment 1, which means that if differences between sellers and buyers in the discounting of the money involved in the transactions play a role, it is a very minor one. The patterns obtained seem to come primarily from the discounting of the item.

1.4 Experiment 3: Robustness across Items

In Experiments 1 and 2 we used the same item: a framed Game of Thrones poster. This raises questions about the generalizability of the patterns obtained and the extent to which they might depend on particular characteristics of the item used. To test the generalizability of our findings across items, in Experiment 3 we elicited WTA and WTP valuations in different time scenarios for three different items, the Game of Thrones poster (to be able to compare patterns directly) and two additional items with markedly different characteristics.

Table 1.4: Quantile Regression Analysis (Experiment 2)

	(1)	(2)	(3)	(4)
	WTA	WTA	WTP	WTP
Delay	0.000	0.000	-0.014***	-0.014***
	(0.014)	(0.007)	(0.004)	(0.004)
Wrong	-0.500	-0.500	0.014	0.014
	(5.720)	(4.313)	(1.590)	(2.766)
Constant	20.500***	20.500***	10.000***	10.000***
	(2.667)	(4.188)	(0.675)	(1.969)
Clustered SE	No	Yes	No	Yes
N	372	372	428	428

Notes: Standard errors in parentheses; *, ** and *** stand for statistical significance at the 10%, 5% and 1% level respectively.

1.4.1 Method

Participants. We recruited 299 participants (56% female, M_{age} = 37 years, age range: 20-72 years) who had not participated in Experiments 1 and 2 via Amazon Mechanical Turk. The study took an average of 9 minutes and 52 seconds to complete and participants received a fixed fee of $0.5 for their participation.

Design and procedure. The design and procedure of Experiment 3 were the same as in Experiment 2 (which was cleaner than Experiment 1 in terms of controlling for the discounting of the money), except that the participants evaluated three different items instead of one. In addition to the Game of Thrones poster, they were presented with an ordinary IKEA mug with a retail price of € 3.99, and with a hypothetical CD autographed by their favorite music artist or band. Participants were first asked to indicate their favorite artist or band, and then they were told to imagine that there was a CD autographed by them. The order of the three items was randomized within subjects. These three items were chosen because they have very different characteristics in aspects such as link to the self, emotionality, practical value and depreciation.

In this experiment, we eliminated the tomorrow scenario to keep the number of evaluations more manageable for participants, so the transaction timings were today, in 1 month and in 1 year.

1.4.2 Results and discussion

First of all, our results show that the three items used in the experiment were indeed different in terms of liking, emotional attachment and monetary valuation. The CD was liked the most, followed by the poster and the mug (mean values: CD = 5.89, poster = 3.75, mug = 3.03; Friedman test: *Fr.* = 298.84, p < .01). In terms of emotional attachment, the CD was also rated higher, followed by poster and mug (mean values: CD = 4.30, poster = 2.08, mug = 1.30; Friedman test: *Fr.* = 312.64, p < .01). Taking WTP in the today scenario as a benchmark, people were also willing to pay more for the CD, followed again by poster and mug (mean values: CD = $41.11, poster = $14.72, mug = $4.88; Friedman test: *Fr.* = 181.40, p < .01).

Table 1.5 reports summary statistics and Figure 1.3 shows boxplots like the ones used in the previous experiments.[3] The results clearly replicated the patterns obtained in Experiment 2 across all three items. In all cases, median WTA today was substantially higher than median WTP today, in line with the endowment effect literature. More importantly, WTA was always essentially flat across time scenarios, while WTP consistently decreased as the transaction of the item was delayed, resulting in an increasing endowment effect.

Our quantile regression analysis, summarized in Table 1.6 for WTA and in Table 1.7 for WTP, confirms that WTA did not significantly change as the transaction time was delayed for any of the items, while WTP significantly decreased for all of them (by 0.6 cents per day of delay in the case of the poster, 3 cents in the case of the CD and 0.4 cents in the case of the mug).

Like in Experiment 2, we can estimate discount factors based on the WTA and WTP valuations, which we have done using the classic exponential discount function. Including all observations, the estimated yearly discount factors are $\delta^{365}_{WTAposter}$ = 1.02, δ^{365}_{WTAcd} = 0.88 and δ^{365}_{WTAmug} = 0.96 in the seller condition, and $\delta^{365}_{WTPposter}$ = 0.56, δ^{365}_{WTPcd} = 0.54 and δ^{365}_{WTPmug} = 0.60 in the buyer condition. This shows that discount factors are always substantially lower (implying more discounting) in the buyer condition. In the seller condition, the discount factors for poster and mug imply virtually no discounting, and the factor for the CD shows a mild degree of discounting. In the buyer condition, all discount factors are fairly similar and they entail substantial degrees of discounting (at least 40% of lost value with one year of delay). The details of these discount factor estimations are in Tables 1.A2 and 1.A3 in the Appendix.

Overall, Experiment 3 clearly showed that the patterns obtained in Experiments 1 and 2 hold across different types of items. As transactions are delayed into the future, WTA remains largely constant, while WTP substantially decreases, resulting in an increasing endowment effect.

[3] Like in Experiments 1 and 2, the descriptive measures and graphs reported exclude WTA and WTP responses over one standard deviation higher than the mean and observations with wrong answers in the qualification questions (10.4% on average in this case). Our quantile regression analysis includes all observations.

Table 1.5: Descriptive Statistics (Experiment 3)

Item	Condition	Time	Median	Mean	SD	Total N	Wrong&outliers
Poster	WTA	Today	20.0	30.5	29.7	148	22
		1 month	20.0	29.2	27.5	148	34
		1 year	20.0	30.2	30.1	148	37
	WTP	Today	6.5	9.6	8.4	151	23
		1 month	5.0	7.5	8.0	151	22
		1 year	3.0	4.3	4.7	151	22
CD	WTA	Today	75.1	131.8	138.0	148	33
		1 month	75.0	123.8	141.5	148	41
		1 year	75.0	126.1	144.5	148	43
	WTP	Today	20.3	27.9	22.6	151	15
		1 month	20.0	22.2	17.6	151	23
		1 year	10.0	15.3	14.1	151	27
Mug	WTA	Today	5.0	5.7	4.9	148	16
		1 month	5.0	5.8	4.8	148	22
		1 year	5.0	5.8	5.1	148	32
	WTP	Today	3.0	3.4	2.7	151	18
		1 month	1.7	2.2	2.0	151	21
		1 year	1.0	1.5	1.8	151	22

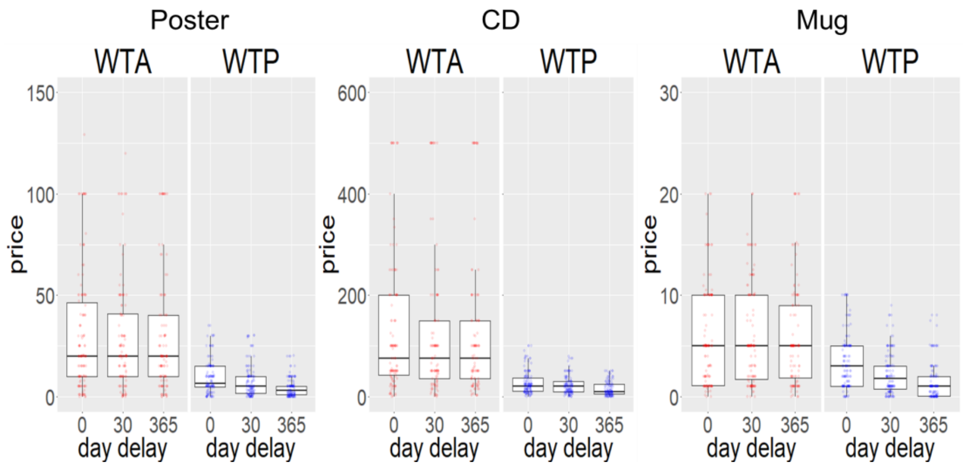

Figure 1.3: Selling (WTA) and Buying (WTP) Prices of the Three Items across Time Scenarios (Experiment 3). Each dot represents one observation. The horizontal line inside each box is the median; the bottom and top of the box are the first and third quartile, respectively.

Table 1.6: Quantile Regression Analysis of WTA (Experiment 3)

	(1)	(2)	(3)	(4)	(5)	(6)
	Poster	Poster	CD	CD	Mug	Mug
Delay	-0.001	-0.001	0.000	0.000	0.000	0.000
	(0.010)	(0.005)	(0.053)	(0.027)	(0.001)	(0.001)
Wrong	-3.700	-3.700	-49.010*	-49.010**	0.000	0.000
	(4.719)	(4.144)	(26.023)	(21.416)	(0.686)	(1.347)
Constant	20.200***	20.200***	100.000***	100.000***	5.000***	5.000***
	(2.079)	(2.848)	(11.357)	(17.947)	(0.280)	(0.627)
Clustered SE	No	Yes	No	Yes	No	Yes
N	444	444	444	444	444	444

Notes: Standard errors in parentheses; *, ** and *** stand for statistical significance at the 10%, 5% and 1% level respectively.

1.5 Experiment 4: Do People Anticipate the Effects of Extended Endowment?

Experiments 1 to 3 provide converging evidence that the endowment effect is amplified as transactions are delayed into the future, in the form of a virtually constant WTA across transaction timings and a consistently decreasing WTP. This suggests that people discount the value of acquiring an item as the acquisition is delayed, which seems logical, but they do not discount the (negative) value of giving up an item they own, or at least not to a substantial extent.

Table 1.7: Quantile Regression Analysis of WTP (Experiment 3)

	(1)	(2)	(3)	(4)	(5)	(6)
	Poster	Poster	CD	CD	Mug	Mug
Delay	-0.006**	-0.006***	-0.030***	-0.030***	-0.004***	-0.004***
	(0.003)	(0.001)	(0.006)	(0.003)	(0.001)	(0.001)
Wrong	-0.889	-0.889	3.100	3.100	0.500	0.500
	(1.637)	(1.509)	(3.259)	(5.451)	(0.628)	(0.648)
Constant	6.189***	6.189***	20.896***	20.896***	2.500***	2.500***
	(0.581)	(0.758)	(1.264)	(1.496)	(0.211)	(0.320)
Clustered SE	No	Yes	No	Yes	No	Yes
N	453	453	453	453	453	453

Notes: Standard errors in parentheses; *, ** and *** stand for statistical significance at the 10%, 5% and 1% level respectively.

There is, however, another possibility that can be derived from the small literature on endowment and time. Strahilevitz & Loewenstein (1998) showed that people's valuation of an item they are endowed with increases with the duration of ownership. Potentially, if people anticipate this increase in how much they will value the item, this could push WTA valuations up as the moment to give up the item is delayed. So, it could be that people are actually discounting the value of giving up the item, but this is compensated by their anticipated increase in how valuable the item will be to them. This would not undermine the findings of Experiments 1 to 3 in any way, but it would imply a different interpretation. As indicated in the introduction, this possibility seems unlikely, given that a few papers have shown that people do not anticipate becoming attached to items in endowment effect situations (Loewenstein & Adler, 1995; Van Boven et al., 2000, 2003). However, in our setting, people are already (hypothetically) endowed with the item and they only need to anticipate this endowment to have a stronger effect on them as time passes, so this possibility merits investigation.

The goal of Experiment 4 was to test if, in our set-up, people anticipate becoming increasingly attached to the items and valuing them more as time passes.

1.5.1 Method

Participants. We recruited 200 participants (50% female, M_{age} = 39 years, age range: 19-77 years) who had not participated in Experiments 1 to 3 via Amazon Mechanical Turk. The study took an average of 9 minutes and 12 seconds to complete and participants received a fixed fee of $0.5 for their participation.

Design and procedure. In this experiment, all participants faced the same scenarios and responded to the same questions (i.e., there was only one condition). Like in the seller conditions of the previous experiments, participants were asked to imagine that they had received the target item as a gift, so that they now owned it. Then they were asked “how valuable do you think the item would be to you [after owning it for t]?” And [after owning it for t] was either "today", "after owning it for 1 month" or "after owning it for 1 year", which are the same time delays used in Experiment 3. These questions were answered on an 11-point scale (from 0 = *not valuable at all* to 10 = *very valuable*). Participants responded to these scenarios for the three items used in Experiment 3 (poster, autographed CD and mug). To deal with potential cross-contamination issues among the different items, participants always evaluated the poster first, because we considered it the most relevant item in terms of relating it to the results of all the previous experiments. The order of CD and mug was randomized. Within each item, the different time scenarios were also randomized.

Like in the previous experiments, participants had to answer a qualification question before responding to each scenario. After the main questions described above, people were also asked how much they liked the item, how strongly they felt ownership of the item, and to complete our demographic survey, as described in Experiment 1.

1.5.2 Results and discussion

Figure 1.4 presents box plots showing the valuations of the different items across time scenarios.[4]

There were clear differences between the items in terms of how valuable they were considered. The CD was perceived as more valuable than the poster (Wilcoxon signed-rank test: z = 32.34, p < .01), which was in turn more valuable than the mug (z = 12.18, p < .01). This shows that participants were using the scale in a meaningful way. More importantly, valuations did not change across the different owning periods. As the plots show, for all the items, the medians were the same in the different owning periods. People did not seem to anticipate any changes in how valuable the items would be to them as they owned them for longer.

To further investigate this pattern, we conducted a quantile regressions using both conventional and clustered (at the individual level) standard errors, as we did in the previous experiments (Table 1.8). In this case, our dependent variable was people's valuations of the items, and we changed the name or our daily Delay variable used before to Period, to reflect the fact that we are now looking at ownership periods (in terms of days) rather than time delays. The regression results confirm that people's valuations did not significantly change across ownership periods for any of the items.

[4] As in the previous experiments, our descriptive statistics exclude observations with mistakes in the qualification questions (5.2% on average). Our quantile regression analysis includes all observations.

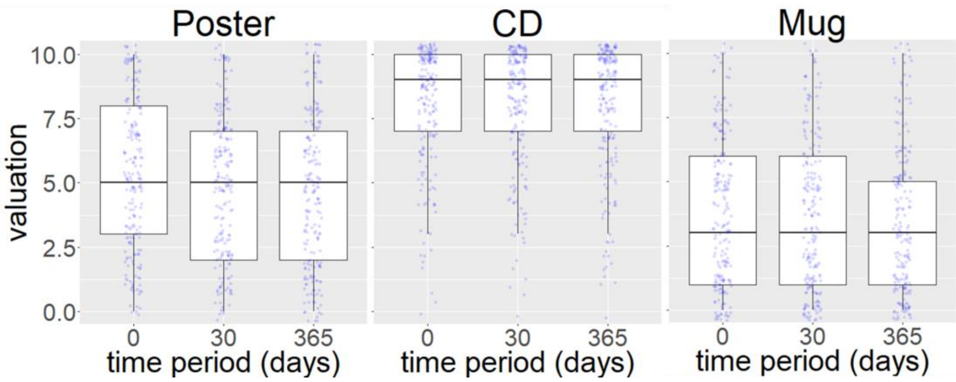

Figure 1.4: Valuations of Items across Owning Periods (Experiment 4). Each dot represents one observation. The horizontal line inside each box is the median; the bottom and top of the box are the first and third quartile, respectively.

Table 1.8: Quantile Regression Analysis of Valuations (Experiment 4)

	(1)	(2)	(3)	(4)	(5)	(6)
	Poster	Poster	CD	CD	Mug	Mug
Period	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Wrong	0.00	0.00	-3.00***	-3.00*	2.00*	2.00**
	(0.75)	(0.56)	(0.50)	(1.71)	(1.19)	(0.99)
Constant	5.00***	5.00***	9.00***	9.00***	3.00***	3.00***
	(0.23)	(0.26)	(0.12)	(0.22)	(0.30)	(0.29)
Clustered SE	No	Yes	No	Yes	No	Yes
N	594	594	594	594	594	594

Notes: Standard errors in parentheses; *, ** and *** stand for statistical significance at the 10%, 5% and 1% level respectively.

Overall, these results show that participants did not anticipate that the items would be more valuable to them if they owned them for a longer period of time, which suggests that people simply discount the value of acquiring an item as it is delayed in time but do not discount the (negative) value of giving it up.

1.6 Experiment 5: Transactions in a Real Online Market

Experiments 1 to 4 provide clear and converging evidence of endowment effects being amplified as transactions are delayed into the future. Two limitations of the previous experiments, however, are that the decisions are hypothetical and they are not linked to a real-world context. In Experiment 5, we tested the robustness of our findings in the context of a real online market called Wallapop and with incentivized decisions. Wallapop is the largest online flea market service in Spain, currently with more than 40 million users who have uploaded over 100 million products (according to the Wallapop website). It is essentially a Spanish version of the American Craigslist, where people buy and sell second-hand items and agree on a price and a time to exchange them. This provides a perfect platform for our study.

1.6.1 Method

Participants. We used a web service to recruit participants who were active Wallapop users, defined as people who had (right before being contacted for the experiment) a Wallapop account with at least one item on sale. They also had to live in the city in which the authors were based and be willing to provide the URLs of the web pages where their items were posted on Wallapop. These URLs allowed us to check the details and history of the items on Wallapop. Following these criteria, we recruited a sample of 130 valid participants (48% female, M_{age} = 34 years, age range: 18-72 years), who were paid a fixed fee for their participation (managed by the recruiting company) and also had some probability of conducting one of the transactions they were asked about for real. The study took an average of 19 minutes to complete.

Design and procedure. In this experiment, we manipulated two factors within subjects: role (seller and buyer conditions) and time scenario (transaction tomorrow and in 1 month). The order of the role and of the time scenario was randomized within participants. The transaction timings were reduced to two in this case to make the whole experiment simpler for the participants.

In the seller condition, people were told to provide the URL of the last active (i.e., still on sale) item they had posted on Wallapop. Then they were asked about their WTA to sell this item for the two different time scenarios (exchanging money and good tomorrow and in 1 month). The set-up here is analogous to that in Experiment 1, where money and item were also exchanged at the same time, so the specific questions used were the same as in Experiment 1. This also mimics the typical situation found in Wallapop, in which sellers and buyers need to agree on a future time to exchange items and money.

In the buyer condition, participants were asked to pick the item they liked the most out of a selection of five different items that were on sale on Wallapop: a smartwatch, a wireless speaker, a backpack, an electric toothbrush, and a ukulele. These items were selected based on a pre-test of various Wallapop items to make sure that they were on average well-valued by people. The items were presented to the subjects in the standard Wallapop format. Then the participants were asked about their WTP for the item they had picked in the two time scenarios (exchanging money and good tomorrow and in 1 month).

It is important to note that in this experiment WTA and WTP valuations were elicited for different items, so they are not directly comparable. We can, however, analyze the pattern of valuations across transaction timings within WTA and within WTP, which is the key aspect of our findings.

Like in the previous experiments, all scenarios were preceded by qualification questions to make sure that people had understood the instructions, and they included graphical displays to clarify transaction timings (see Experiment 1). Participants completed also a final survey, asking when they had bought the item, the purchasing price, how many buyers had contacted them about the item, the condition of the item, if they had reposted the item on Wallapop, the reason for selling the item, how many times they had bought items on Wallapop, and if they would be in the city in 1 month from the day of the experiment.

Incentive system. In this experiment, we also incentivized people's valuations with the widely used Becker-DeGroot-Marschak (BDM) method (Becker et al., 1964). Several randomly selected participants had the chance to implement for real one of the transactions they had been asked about. Specifically, we randomly selected three people to implement one of their WTA valuations (also randomly selected) and two people to implement one of their WTP valuations (also randomly selected). Participants knew from the beginning that they could be picked to carry out one of the transactions, so that any one of their valuations could have real consequences.

In the case of the selected WTA valuations, the computer then generated a random number (from a pre-specified range). If the valuation was smaller than or equal to this number, people were asked to sell the item to us for the generated amount; if the valuation was higher than the number, the item was not sold. If the item was sold, we then agreed with the selected participant on a suitable location to exchange money and item at the corresponding transaction time (or as close to it as possible), as is usually done on Wallapop. These participants were also asked to immediately change the status of their item on Wallapop to "sale already agreed".

For the selected WTP valuations, the computer also generated a random number. If the valuation was higher than or equal to the number, people were entitled to receive the item; if the valuation was lower than the number, people were entitled to receive an amount of money equal to the generated number. This set-up is often used when applying the BDM method to elicit WTP to avoid making people pay money out of their own pockets. We then agreed with the selected participants on a suitable location to give them their outcome at the corresponding time (or as close as possible to it).

1.6.2 Results and discussion

Table 1.9 reports the summary statistics for Experiment 5; the box plot in Figure 1.5 shows the main patterns observed in the different scenarios.[5] The results obtained are broadly similar to Experiment 1. Focusing on the medians, WTA slightly increased as the transaction was delayed (i.e., in the 1 month scenario compared to the tomorrow scenario), while WTP considerably decreased.

[5] Like in the previous experiments, both WTA and WTP included some disproportionately high values. So, the descriptive measures and graphs reported exclude again WTA and WTP responses over one standard deviation above the mean, and also observations with wrong answers in the qualification questions (23.1% on average in this case). Our quantile regressions include all observations.

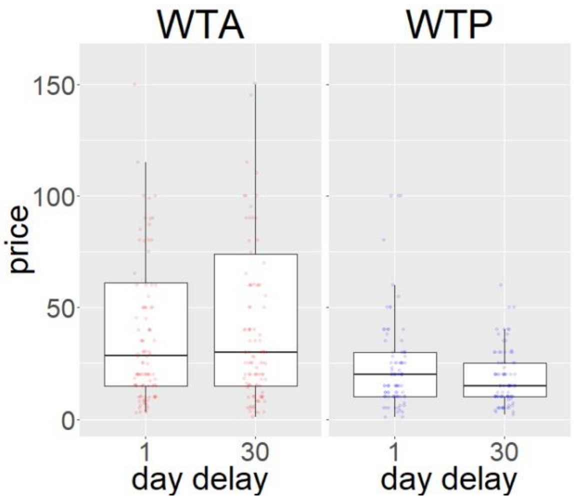

Figure 1.5: Selling (WTA) and Buying (WTP) Prices across Time Scenarios (Experiment 5). Each dot represents one observation. The horizontal line inside each box is the median; the bottom and top of the box are the first and third quartile, respectively.

Table 1.9: Descriptive Statistics (Experiment 5)

	Time	Median	Mean	SD	Total N	Wrong&outliers
WTA	Tomorrow	28.5	62.8	98.3	130	22
	1 month	30.0	70.3	113.2	130	20
WTP	Tomorrow	20.0	31.3	65.8	130	38
	1 month	15.0	20.0	22.8	130	41

Like in the previous experiments, we further analyzed the results using quantile regressions, with both conventional and clustered standard errors (at the level of the individual) (Table 1.10). Given that in this case we only had two transaction timings, we substituted the Delay variable used in Experiments 1 to 3 with a dummy variable called 1_month, which takes the value 1 if the transaction was in 1 month and 0 if it was tomorrow. The regression results show that WTA was not significantly different between the time scenarios, but WTP significantly decreased as the transaction timing was delayed.[6]

The results of Experiment 5 show that the same pattern we consistently observed in the previous experiments is also obtained in the context of a real market, in which sellers evaluated items they already owned and were already planning to sell, and with incentivized valuations. Again, WTA was roughly flat across transaction timings and

[6] We also analyzed if more experienced Wallapop users showed different patterns and if the time that the items had been posted on Wallapop prior to the experiment affected the results, but both of these effects were non-significant. More details on these analyses are available from the authors on request.

WTP consistently decreased, which would result in an amplified endowment effect in the future.

Table 1.10: Quantile Regression Analysis (Experiment 5)

	(1)	(2)	(3)	(4)
	WTA	WTA	WTP	WTP
1_month	1.00	1.00	-5.00**	-5.00***
	(6.11)	(1.73)	(2.26)	(1.34)
Wrong	0.00	0.00	0.00	0.00
	(8.30)	(7.23)	(2.47)	(2.66)
Constant	26.00	26.00***	25.00***	25.00***
	(21.68)	(7.20)	(3.64)	(3.18)
Clustered SE	No	Yes	No	Yes
N	260	260	260	260

Notes: Standard errors in parentheses; *, ** and *** stand for statistical significance at the 10%, 5% and 1% level respectively.

1.7 General Discussion and Conclusions

Our five experiments provide clear evidence that endowment effects are amplified as transactions are delayed into the future. Across experiments, WTA remained roughly constant and WTP consistently decreased as the transactions were delayed. Experiment 2 showed that this pattern is not produced by the discounting of the money involved in the transactions, but comes largely from moving the transaction of the item in time; Experiment 3 proved that the pattern holds across diverse items; Experiment 4 ruled out people's anticipation of changes in value related to owning the item as an explanation for the non-decreasing WTA; and Experiment 5 showed that the same WTA and WTP patterns hold in the context of a real market, with goods that were meant to be sold, and with incentivized decisions.

Our findings in the context of goods are partially in line with the sign effect typically observed in the context of money (Frederick et al., 2002; Thaler, 1981). In the sign effect, both gains and losses of money are discounted, but gains are discounted more than losses. This pattern has also been obtained in the context of health (where it is actually stronger) and of decisions that relate to the environment (Hardisty & Weber, 2009). In our experiments, the value of acquiring the items is also discounted more than the (negative) value of giving them up, but in our case giving up the items does not seem to be discounted at all. This could be seen as a more extreme form of sign effect in the context of goods.

The rather small literature on the sign effect has so far not explored much the specific psychological mechanisms behind the effect, so that its psychological underpinnings are still a bit unclear. The prevailing explanation is in terms of loss aversion (Kahneman & Tversky, 1979), and it simply posits that losses are more impactful than gains of the same magnitude and are therefore discounted less, which is in a way just a description of the pattern obtained. Reasoning along these lines, it seems natural that this effect is stronger in the context of the valuation of goods, because goods tend to create psychological attachment and this could make the loss even more impactful. This is consistent with our findings, in which losing the item is virtually not discounted.

In a recent paper, Molouki et al. (2019) proposed a "contemplation-emotion" account of the sign effect, according to which it is the more impactful emotional experience of waiting for the outcome in the case of losses that produces the sign effect. In the context of this explanation, our findings also seem quite natural. The psychological attachment component of goods is likely to make waiting for their loss more impactful than waiting for the loss of less emotional outcomes such as money, which would result in less discounting of the loss and a more pronounced sign effect. There is still room to further explore the psychological mechanisms behind different types of sign effects for money, goods and other outcomes, but this is beyond the scope of this paper.

Finally, our findings are of clear practical relevance. We live in a world in which delayed transactions are more and more prevalent. In virtually all forms of online buying and selling transactions are subject to some form of delay. The rise of online platforms such as Amazon, AliExpress, Craigslist, Facebook Marketplace, etc. has made delayed transactions one of the most standard practices. On the one hand, this implies that existing studies of the endowment effect (based on transactions in the present) are likely to have underestimated the strength of the effect, at least in relation to some real-world settings. On the other hand, our results provide relevant guidelines on how to design market institutions. Providing tools for buyers and sellers to exchange goods as soon as possible, or even nudging them into doing so, might be important to maximize agreements and minimize the market frictions associated with endowment effects.

Appendix

Table 1.A1: Estimation of Yearly Discount Factors (Experiment 2)

	(1) WTA	(2) WTA	(3) WTP	(4) WTP
δ^{365}	1.01	0.96	0.59***	0.57***
	(0.06)	(0.06)	(0.03)	(0.04)
Constant	23.87***	19.74***	8.83***	7.59***
	(0.39)	(0.32)	(0.13)	(0.13)
All obs.	Yes	No	Yes	No
N	372	289	428	355

Notes: Clustered standard errors in parentheses; *, ** and *** stand for statistically different from 1 (i.e., from no discounting) at the 10%, 5% and 1% level respectively. These models assume an exponential discount function (Samuelson, 1937) with a daily discount factor δ. The variable δ^{365} reported is the yearly discount factor. Columns 2 and 4 show results excluding observations one standard deviation above the mean and with wrong answers in the qualification questions. All regressions include individual fixed effects.

Table 1.A2: Estimation of Yearly Discount Factors for WTA (Experiment 3)

	(1)	(2)	(3)	(4)	(5)	(6)
	Poster	Poster	CD	CD	Mug	Mug
δ^{365}	1.022	0.962	0.884***	0.859***	0.957	0.942
	(0.057)	(0.039)	(0.043)	(0.055)	(0.047)	(0.041)
Constant	22.714***	18.682***	102.028***	73.782***	5.914***	5.267***
	(0.455)	(0.257)	(1.788)	(1.630)	(0.105)	(0.078)
All_obs	Yes	No	Yes	No	Yes	No
N	444	351	444	327	444	374

Notes: Clustered standard errors in parentheses; *, ** and *** stand for statistically different from 1 (i.e., from no discounting) at the 10%, 5% and 1% level respectively. These models assume an exponential discount function with a daily discount factor δ. The variable δ^{365} reported is the yearly discount factor. Columns 2, 4 and 6 show results excluding observations one standard deviation above the mean and with wrong answers in the qualification questions. All regressions include individual fixed effects.

Table 1.A3: Estimation of Yearly Discount Factors for WTP (Experiment 3)

	(1)	(2)	(3)	(4)	(5)	(6)
	Poster	Poster	CD	CD	Mug	Mug
δ^{365}	0.558***	0.536***	0.542***	0.508***	0.603***	0.591***
	(0.025)	(0.028)	(0.033)	(0.035)	(0.022)	(0.024)
Constant	7.863***	6.612***	21.499***	19.600***	3.754***	3.279***
	(0.129)	(0.126)	(0.477)	(0.467)	(0.050)	(0.047)
All_obs	Yes	No	Yes	No	Yes	No
N	453	386	453	388	453	392

Notes: Clustered standard errors in parentheses; *, ** and *** stand for statistically different from 1 (i.e., from no discounting) at the 10%, 5% and 1% level respectively. These models assume an exponential discount function with a daily discount factor δ. The variable δ^{365} reported is the yearly discount factor. Columns 2, 4 and 6 show results excluding observations one standard deviation above the mean and with wrong answers in the qualification questions. All regressions include individual fixed effects.

Figure 1.A1: Graphical Display used to Clarify Transaction Timing (Buyer Condition, 1 Year Scenario).

Chapter 2

TIME PREFERENCES IN GAIN AND LOSS DOMAINS: AN INCENTIVIZED EXPERIMENT

Abstract

Present-biased preferences in intertemporal decisions have been actively investigated. While these preferences have been elicited through incentivized experiments in the gain domain to avoid potential hypothetical bias, they have been elicited only through hypothetical experiments in the loss domain. We conducted a two-stage experiment that enabled us to elicit these preferences in the gain and loss domains in an incentive-compatible way. We found that present bias, which is exhibited in both domains, is more severe in the loss domain.

2.1 Introduction

Many of our important decisions involve a time component, and these decisions often involve gains and losses. Individual time preferences, in particular present-biased preferences, have been investigated for decades (Abdellaoui et al., 2013; Andersen et al., 2008; Andreoni & Sprenger, 2012; Benhabib et al., 2010; Frederick et al., 2002; Laibson, 1997; Loewenstein & Thaler, 1989; Thaler, 1981).

A person with present-biased preferences overvalues present outcomes or undervalues future outcomes. Such a preference leads to time-inconsistent behaviors. For example, a person with present-biased preference planning to save €200 in 30 days would end up saving less than €200 when the future comes because shopping or other options at the time become more attractive than the person expected. This planning failure would cause insufficient savings (Laibson, 1997). Using the same reasoning, present bias could explain many serious problems in our life such as credit card debt (Meier & Sprenger, 2010), excessive body mass index (BMI) (Courtemanche et al., 2015) and smoking addiction (Ida, 2014).

Are people's time preferences the same in decisions involving gains and in decisions involving losses? Some papers, such as Benzion et al. (1989), MacKeigan et al. (1993) and Thaler (1981), have argued they are not. These papers compared the estimates of individual discount rates (IDRs) in the gain and loss domains.[1] Some previous experiments reported the *sign effect*, in which IDRs are higher in the gain domain than the loss domain (Thaler, 1981; see Frederick et al., 2002, for a review). Shelley (1993), however, did not find the sign effect, and the opposite of the sign effect has been found by merely changing the frames of the intertemporal decisions, for example, an acceleration frame in which the default is to realize the outcomes in the future and a delayed frame in which the default is to realize the outcomes today (Appelt et al., 2011; Benzion et al., 1989; Shelley, 1993, 1994).

Disagreements about the differences between the gain and loss domain also exist in the literature on the magnitude of the present bias. Thaler (1981) found that discount rates dropped sharply as the timing of the future outcomes was delayed in the gain domain, which is consistent with present-biased preferences, but this pattern was not found in the loss domain. On the other hand, Abdellaoui et al. (2013) and Shiba and Shimizu (2019) showed that the majority of participants in their experiments were present-biased in the gain and loss domains. While Abdellaoui et al. found that the level of present bias in the loss domain was higher than in the gain domain, Shiba and Shimizu could not find a difference between the two domains in most cases. An important characteristic of all these previous studies investigating the present bias in the loss domain is that they used hypothetical decisions. Thus, they potentially suffer from hypothetical bias (see Frederick et al., 2002; Johnson & Bickel, 2002).

There have been debates about whether using hypothetical scenarios instead of conducting an incentive-compatible experiment affects time preferences in the gain domain. Frederick et al. (2002) argued that it is uncertain whether with hypothetical rewards people are motivated to, or capable of, accurately predicting what they would do if outcomes were real. Another paper found that IDRs were lower for hypothetical rewards (Kirby & Maraković, 1995) although other studies observed no difference in

[1] The IDRs are commonly used to measure impatience and describe how heavily an individual discounts future outcomes regarding money, goods, health, the environment, etc. For example, if the individual is indifferent between receiving $105 in a year and receiving $100 today, their yearly IDR is 5%.

discount rate with real and hypothetical rewards (M. W. Johnson & Bickel, 2002; Madden et al., 2003). There have been similar extensive debates in risk preferences in both domains. Camerer and Hogarth (1999) reviewed 13 papers on risk preferences and report, when incentivized, individuals became more risk averse in 8 papers, became more risk seeking in 2 papers and did not change their risk preferences in 3 papers. Other studies found that offering incentives increased risk aversion in lottery choice questions (Holt & Laury, 2002, 2005). Furthermore, Weber et al. (2004) showed that individuals became more risk averse for gains and less risk seeking for losses in incentive-compatible conditions. Importantly, the effects were stronger in the loss domain. This implies that the extent of the hypothetical bias is different in the gain and loss domains. *Construal-level theory* (Trope & Liberman, 2010) claims that both time (delay) and probability (risk) perceived as introducing psychological distance. Thus, it is reasonable to assume that experiments on time preferences may also suffer from the hypothetical bias and that the extent of the bias is different in the gain and loss domains as well. In this paper, we elicit time preferences both in the gain and loss domains in an incentive-compatible manner to fill this gap in the literature.

Incentivizing choices that involve losses in experiments is challenging because, generally, it is hard for experimenters to take money away from the participants (Frederick et al., 2002). Thus, in experiments involving losses, participants are typically given an initial endowment that is enough to cover the maximum possible loss (Tom et al., 2007). This method has been also used to measure IDRs (Xu et al., 2009; Y.-Y. Zhang et al., 2016) and loss aversion (Kirchler et al., 2018).[2] However, providing an endowment to participants could also introduce an unwanted bias in the experiment known as the *house-money effect* (Thaler & Johnson, 1990). That is, individual behaviors may be affected (e.g., display more risk-loving behaviors) when prior windfall gains are given. The house-money effect can be avoided by making participants earn money through unrelated tasks before the main part of an experiment (Bosch-Domènech & Silvestre, 2010). The present study applies this two-stage method.

Our two-stage experiment enables an unbiased incentivization of time preference elicitation in the loss domain. In the first stage, the participants took a part of a non-verbal IQ test (the advanced version of Raven's Progressive Matrices Test, Raven, 2003) and earned an amount of money which was enough to cover the maximum possible loss in the second stage. Two weeks after the first stage, time preferences in the gain and loss domains were elicited. Using Raven's test in the first stage also allows us to analyze the relationship between cognitive skills and present bias, as well as impatience (IDRs), in both domains.[3]

The results from our incentivized experiment consistently show that time preferences in the gain and loss domains are different. A descriptive analysis shows that immediate future losses are more heavily discounted than immediate future gain, while both are only mildly discounted in the further future. It appears that, therefore, the present bias is more severe in the loss domain. Further investigation through regression analyses reveals that there is a significant level of present bias in both domains and that the

[2] Xu et al. (2009) and Zhang et al. (2016) found the sign effect but they did not measure the present bias.

[3] Some previous papers found that cognitive skills are positively correlated to patience (i.e., low IDRs) (Benjamin et al., 2013; Burks et al., 2009; Dohmen et al., 2010; Oechssler et al., 2009; see Shamosh & Gray, 2008 for earlier studies). Among them, Benjamin et al. (2013) and Burks et al. (2009) investigated the relationship between cognitive skills and present bias in the gain domain and showed that higher cognitive skills were associated with a lower level of present bias. However, to the best of our knowledge, the relationship between cognitive skills and present bias in the loss domain has not yet been explored.

present bias is indeed more severe in the loss domain. These results are in line with Abdellaoui et al. (2013).

Our results imply that one should be careful about using the degree of present bias estimated in the gain domain to design a policy to intervene in time-inconsistent behaviors involving losses. Furthermore, we found that the participants with higher cognitive skills tend to be more patient and less present-biased in both the gain and loss domains.

2.2 Method

Participants. We recruited 80 students at ESC Dijon Bourgogne who agreed to participate in the entire experiment, including the first stage conducted on February 19th and 20th and the second stage conducted on March 2nd and 3rd on campus in 2015. There were three sessions in the experiment. In total, 68 participants (73.5% female, M_{age} = 22.3 years, age range: 20-29 years) completed both stages of the experiment. On average, the experiment took approximately 65 minutes in total (20 minutes in the first stage and 45 minutes in the second stage), and participants received €19.23 in total (€17.79 in the first stage and €1.44 in the second stage). The total payments were positive for all participants.

Design and procedure. The entire experiment was computerized and implemented by z-tree (Fischbacher, 2007). The subjects were told that there were two stages in the experiment and that they might lose money in the second stage. They were also told that the possible maximum loss was the same as the minimum gain in the first stage, so they would not lose money in total, and there was a high possibility that they could earn a substantial amount of money.

The first stage was the earning stage. The participants took the non-verbal IQ test and earned either €20 or €15 depending on whether their scores were above the average score of the participants in the same session or not (38 participants received €20 and 30 participants received €15). The same participants came back to the lab after two weeks and brought at least €15 with them. In the second stage, they made two sets (gains and losses) of intertemporal decisions followed by demographic questions.[4] Half of the participants made decisions in the gain domain first while the other half started with the loss domain.

Our intertemporal decision tasks were based on Tanaka et al. (2010). Tanaka et al. used the multiple price list (MPL) method which provides a list of choices between two options: a sooner but smaller outcome and a later but larger outcome. The drawback of the MPL method is that only ranges of IDRs can be estimated. To estimate the exact values of IDRs (Benhabib et al., 2010; Manzini & Mariotti, 2014), we employed matching tasks incentivized by the Becker-DeGroot-Marschak (1964, BDM) mechanism (to be described in detail in the incentive section below). With this method, the IDRs can be estimated by asking today’s equivalent value of a certain outcome in the future. Specifically, in the gain domain, the participants were asked to declare the *minimum* amount of money that they preferred to receive today instead of receiving Y

[4] The demographics include age, gender, height, father’s highest degree, mother’s highest degree, the city where they are from, the number of siblings, the number of packs of cigarettes they smoke per week, the amount of alcohol they take per week, the amount of money they spend per month, the amount of money for leisure they spend per month and their mother tongue.

euros in t days. Similarly, in the loss domain, the participants were asked to declare the *maximum* amount of money that they preferred to lose today instead of losing Y euros in t days. Four values of Y (3, 6, 10, 15) and five values of t (3, 7, 14, 21, 28) were used, so there were 20 intertemporal decisions to make for each domain. Figure 2.1 shows a screen-shot of the decision screen used in the experiment.

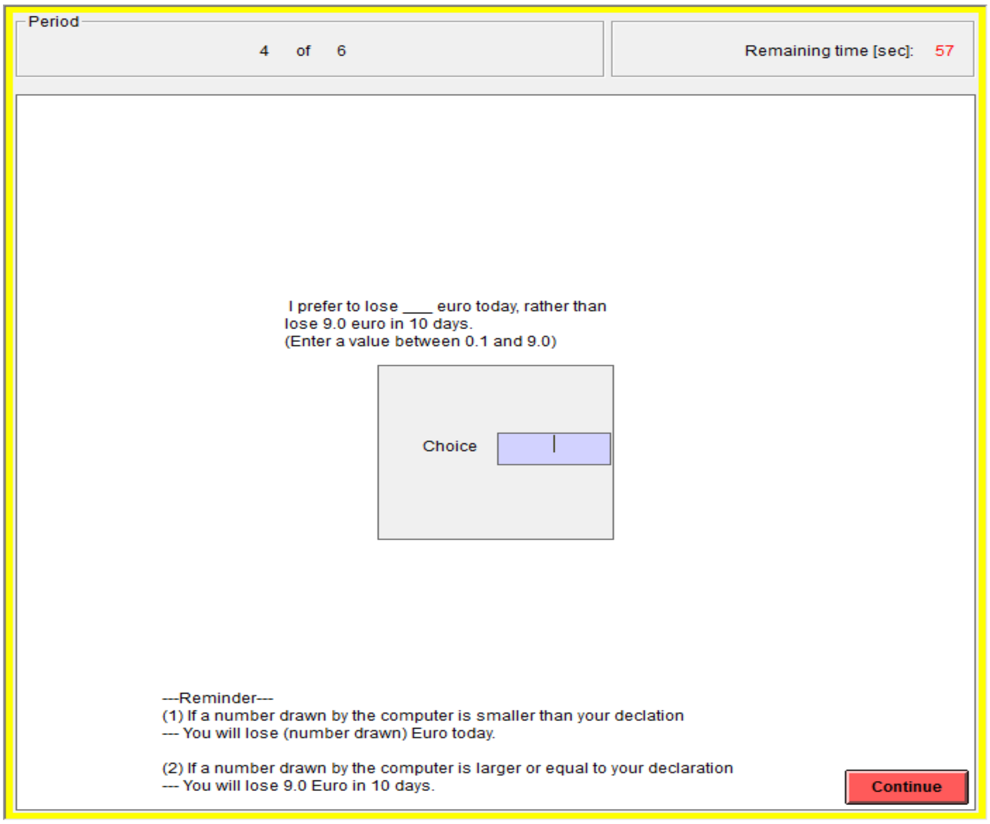

Figure 2.1: Screenshot of one of the Questions in the Experiment.

Incentive system. At the end of the experiment, one question was randomly chosen from each domain, and then a computer generated a random number for each of the chosen questions to determine the outcomes. In the gain domain, if a participant's declared value was *smaller or equal* to the random number, the participant received "today" an amount of money equal to the number drawn. Otherwise, the participant received Y euros in t days, as specified in the chosen question. Similarly, in the loss domain, if the declared value was *larger or equal* to the random number, the participant lost today an amount of money equal to the number drawn. Otherwise, the participant lost Y euros in t days, as specified in the chosen question. In both domains, the dominant strategy for the participant is to declare the amount that makes the participant indifferent between gaining or losing such an amount today and the Y euros with the delay of t days. The participants were told so in the instructions for each set.

In order to facilitate the understanding of the BDM mechanism by participants, we supplemented our instructions with a figure (Appendix 2B). The instructions were computerized and read aloud by a computer-generated voice. To check participants were paying attention to the pre-recorded instruction audio, in the middle of the instructions, one number (not shown in the text on the display) was announced, and the participants were asked to enter it in order to proceed to the next page. All the participants entered the correct numbers.

At the end of the experiment, the participants received a document stating the amount of money they would receive and pay on specific dates. They had to go to the school's administration office to collect their payments, thus the transaction costs (which were anyway very small for them because most of them came to the campus every weekday) were the same for both domains. Only 4 participants, whose gain and loss were the same amount to be realized on the same day, did not show up for the payments.

Models. For our parametric analysis to estimate time preferences, we employ the quasi-hyperbolic discount function βe^{-rt} (when $t > 0$ and 1 otherwise) (Laibson, 1997). A number of experimental studies (e.g., Loewenstein & Thaler, 1989; Tanaka et al., 2010) have estimated the level of present bias with this function. Under this formulation, between any two future periods, later values are discounted by a constant rate r. r describes the level of long term impatience, and we call it the conventional discount rate. Between *today* and any future point, however, in addition to discounting by the conventional discount rate r, future values are discounted even further as represented by the multiplicative term β (that represents the degree of present-bias). This function captures typical behaviors of disproportionately discounting immediate future outcomes (i.e., overvaluing today's outcomes) (Laibson, 1997). The quasi-hyperbolic discount function implies that people become more impatient only for immediate outcomes, otherwise their level of patience is the same across time.

2.3 Results

Figure 2.2 shows a ratio of the participants' declared values X to the future values given in intertemporal questions Y across time t. This represents how heavily the participants discount delayed outcomes at time t. For example, the median values of the figure show that the future gains in 21 days are approximately 70% of their values today whereas the future losses in 21 days become approximately 50% of their values today. For each delay t, the median values of the ratio in the loss domain are lower than those in the gain domain, that is, the future losses are discounted more than the future gains. Furthermore, in both domains, while the shortest delayed outcomes (t = 3) are drastically discounted, further delays are discounted at an almost constant rate. These patterns are consistent with the quasi-hyperbolic discounting function. In fact, the IDRs from $t = 3$ further toward the future do not deviate from a constant (repeated measures ANOVA: $F(3, 67) = 0.237$, $p > 0.1$).

Parameter estimates. To further analyze the time preferences in the gain and loss domains, we conduct a set of regression analyses based on the quasi-hyperbolic discounting model.[5] Assuming the linear utility function ($U(X) = X$), the present value

[5] Our analysis is based on the quasi-hyperbolic discount function, but we also considered two other common discount functions to estimate participants' time preferences: the exponential discount function (Samuelson, 1937) and the general hyperbolic discount function (Loewenstein & Prelec, 1992). We compared the goodness of fit of the three models. Nevertheless, the quasi-hyperbolic discount function appears to be the best of all. According to a non-linear regression analysis, the quasi-hyperbolic discount function gives the highest adjusted R^2 in both domains. More details on these analyses are available from the authors on request.

(X) of a certain future value (Y) with delay t declared by the participants can be expressed as $X = Y\beta e^{-rt}$ (results relaxing the linear utility assumption as well as allowing for a degree of loss aversion are shown in Appendix). This equation can be easily linearized to estimate the time preference parameters:

$$ln\left(\frac{X}{Y}\right) = ln(\beta) - t \cdot r \qquad (1)$$

We estimate the present bias β and the conventional discount rate r using OLS with clustered standard errors (at the individual level). Columns (1) and (2) of Table 2.1 show that β is significantly different from 1 in both gain (Column (1)) and loss (Column (2)) domains, suggesting that participants' preferences are present-biased on average. The present bias in the gain domain β^{+} is less severe than in the loss domain β^{-}, and the conventional discount rates in the gain domain r^{+} are higher than the discount rates in the loss domain r^{-}.

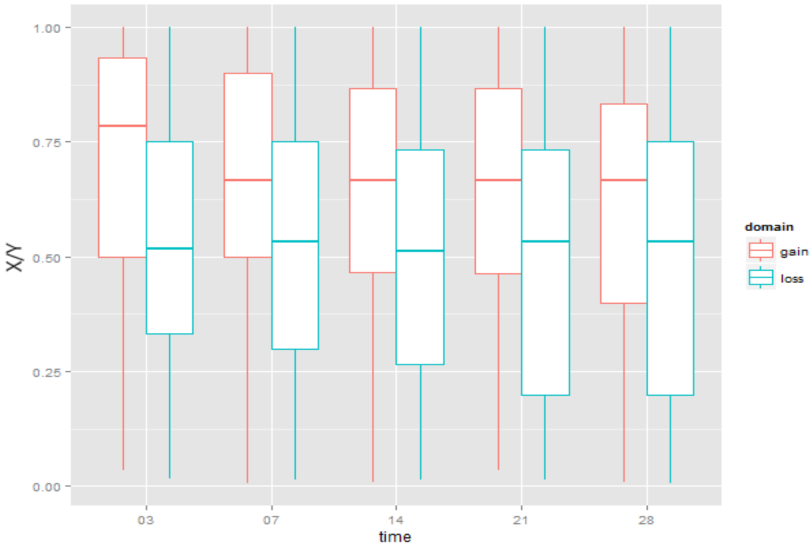

Figure 2.2: Ratio of Declared Present Values X to Future Outcome Y across Delay Scenarios. The horizontal line inside each box is the median; the bottom and top of the box are the first and third quartile, respectively.

To test whether these estimated values of parameters are statistically different between the two domains, we add a dummy variable called Loss (which takes the value one if questions are from the loss domain, and zero otherwise) to the equation (1) above to obtain:

$$ln\left(\frac{X}{Y}\right) = ln(\beta) + \theta_1 \cdot Loss - t(r + \theta_2 \cdot Loss) \qquad (2)$$

Column (3) of Table 2.1 reports the result of this regression. It shows that the estimated coefficient of Loss (θ_1) is significantly different from 0, indicating that the degree of present bias is indeed different in the gain and loss domains, while the estimate of θ_2 is not significantly different from 0, suggesting that the conventional discount rates are not different in the two domains. This pattern of higher values of β and r in the gain domain than in the loss domain is in line with Abdellaoui et al. (2013), although the absolute values of the estimated parameters are different (Table 2.2).

Table 2.1: Regression Analysis with Quasi-hyperbolic Discounting Model

	(1) Gain	(2) Loss	(3) All
β [= $exp(constant)$]	0.634***	0.432***	
	(0.0348)	(0.0343)	
r [= $time$ * (-1)]	0.00950***	0.00715**	
	(0.00315)	(0.00334)	
Constant	-0.455***	-0.839***	-0.455***
	(0.0548)	(0.0794)	(0.0548)
t	-0.00950***	-0.00715**	-0.00950***
	(0.00315)	(0.00334)	(0.00315)
Loss			-0.384***
			(0.0869)
t * Loss			0.00235
			(0.00399)
N	1360	1360	2720
adj. R^2	0.014	0.004	0.049

Notes: Clustered standard errors in parentheses; *, ** and *** stand for statistical significance at the 10%, 5% and 1% level respectively.

Individual level analysis. We found, on average, the present bias in the loss domain is more severe than in the gain domain. In this section, we conduct analyses for each individual. This is meaningful given the large degree of heterogeneity suggested by the wide inter-quantile ranges of the boxplots shown in Figure 2.2. First, we estimate each participant's conventional discount rate and present bias based on the quasi-hyperbolic discounting model. Figure 2.3 shows the histograms of the estimated values of participants' present bias $\hat{\beta}$ and conventional discount rate $\hat{r}$ in both domains. $\hat{\beta}^{+}$ is less than 1 for 92.6% of participants and $\hat{\beta}^{-}$ is less than 1 for 98.5% of participants. Both $\hat{\beta}^{+}$ and $\hat{\beta}^{-}$ are significantly different from 1 (Sign test: $p < .01$; Sign test: $p < .01$), and

$\hat{\beta}^-$ is significantly lower than $\hat{\beta}^+$ (Sign test: p < .01). As for the conventional discount rates, $\hat{r}^+$ and $\hat{r}^-$are significantly different from 0 (Sign test: p < .01; Sign test: p < .01), but they are not different from each other (Sign test: p > .1). As Figure 2.4 shows, we found a weak positive correlation between $\hat{\beta}^+$and $\hat{\beta}^-$ $(r(66) = .25)$ and a weak positive correlation between $\hat{r}^+$ and $\hat{r}^-$ ($r(66) = .25$). This degree of correlation of the present bias between the two domains is weaker than what Shiba and Shimizu (2019) reported.

Table 2.2: Comparison with Previous Papers Using Quasi-hyperbolic Discounting Function

	Tanaka et al. (2010)	Abdellaoui et al. (2013)	Benhabib (2010)*1	This study
N	178	65	27	68
Elicitation	MPL	MPL	BDM	BDM
Highest outcome	300,000 dong	€500	$100	€15
Longest delay	3 months	3 years	6 months	1 month
Incentivized	Yes	No*2	Yes	Yes
$\hat{\beta}^+$	0.644	0.94	0.979	0.634
$\hat{r}^+$	0.008	0.000301	-	0.0095
$\hat{\beta}^-$	-	0.91	-	0.432
$\hat{r}^-$	-	0.000137	-	0.00715

*1The estimated parameters are calculated from the average of individual parameters. *2Incentivized experiment was conducted only in the gain domain.

We also examine if cognitive skills are correlated with the time preference variables (IDRs, present bias and conventional discount rates). First, we regress IDRs in both domains on the score of Raven's test (IQ score). To control for the income effect, we add the dummy variable Earnings, which takes the value 1 and 0 if a participant received €20 and €15 in the 1st stage, respectively. Table 2.3 shows that the IQ score has a significant negative effect on IDRs in both domains (i.e., cognitive skills are positively correlated with patience). This finding is consistent with previous studies (Dohmen et al., 2010; Frederick, 2005). Table 2.4 also shows that the IQ score has a significant negative effect on $\hat{\beta}^-$ and a marginally significant negative effect on $\hat{\beta}^+$ but not on $\hat{r}$, indicating that the IQ score is correlated only with the present bias but not with conventional discount rates, especially in the loss domain.[6] Finally, we examine the correlation of the time preferences variables with self-reported behavioral variables

[6] In the regressions, we use the dependent variables estimated from the quasi-hyperbolic discount function, so the results might suffer from heteroscedasticity. Thus, we repeated the regression analysis with the feasible generalized least square (FGLS). Nonetheless, the results are fairly close between the two models. More details on these analyses are available from the authors on request.

which potentially relate to impatience, namely the amount of tobacco and alcohol consumption and the amount of money spent on leisure. However, none of them displays even mild correlations ($\rho < 0.2$).

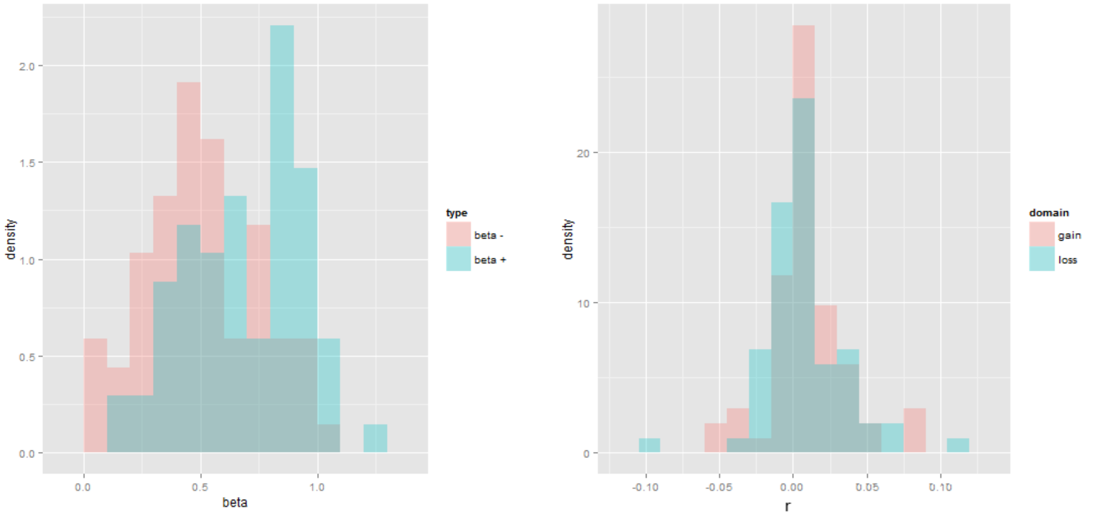

Figure 2.3: Distribution of Estimated Present Bias (on the Left) and Conventional Discount Rates (on the Right) in the Gain and Loss Domains.

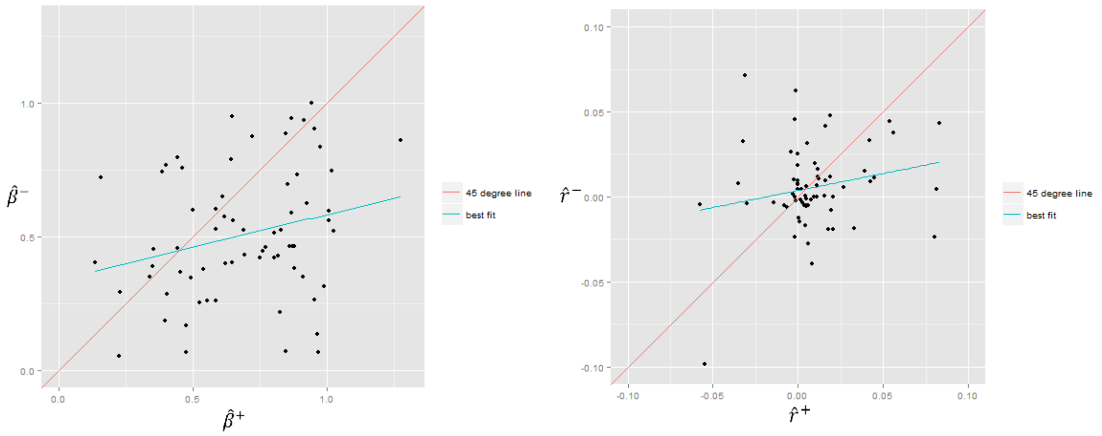

Figure 2.4: Scatterplot of Estimated Present Bias (on the Left) and Conventional Discount Rates (on the Right) in the Gain and Loss Domains.

2.4 General Discussion and Conclusions

Our two-stage design enabled us to conduct an incentivized experiment on time preferences in the loss domain. With multifaceted approaches, we found significant differences in time preferences in the gain and loss domains. First, in both domains, our boxplot shows substantial discounting of the outcomes with a short delay, and then discounting at approximately constant rates for future outcomes with further delays. The shortest delayed outcomes were discounted more in the loss domain. These patterns suggest that a more severe present bias is exhibited in this domain. Second, we performed a regression analysis with the quasi-hyperbolic discounting model. This model was chosen because statistical analysis showed that the IDRs did not deviate from a constant after the shortest delay. It showed that the present bias existed in both domains, and it was more severe in the loss domain than in the gain domain on average. This pattern is consistent with Abdellaoui et al. (2013). The individual-level analysis also indicated that participants exhibited a more severe present bias in the loss domain than in the gain domain. Furthermore, the IQ test score was negatively correlated with IDRs and the level of present bias in both domains. In other words, the participants with higher cognitive skills were more patient and demonstrate the lesser degree of present bias on average.

Table 2.3: Regression Analysis of Daily Individual Discount Rates

	(1) Gain	(2) Gain	(3) Loss	(4) Loss
IQ_score	-0.0140***	-0.0121**	-0.0208**	-0.0224**
	(0.00526)	(0.00535)	(0.00927)	(0.00956)
Earnings	0.00931*	0.00888	0.0158	0.0167*
	(0.00545)	(0.00544)	(0.00959)	(0.00973)
Age		-0.000575		-0.00399
		(0.00452)		(0.00808)
Female		0.0318*		-0.0265
		(0.0186)		(0.0333)
Constant	0.0564	0.0334	0.0719	0.181
	(0.0620)	(0.111)	(0.109)	(0.197)
N	68	68	68	68
adj. R^2	0.078	0.091	0.044	0.029

Notes: The dependent variable is the daily individual discount rates. Standard errors in parentheses; *, ** and *** stand for statistical significance at the 10%, 5% and 1% level respectively.

Our study employed a matching-task elicitation method rather than the choice-task elicitation via multiple price lists (MPL) to estimate IDRs. Both choice tasks and matching tasks were frequently used in previous experiments on time preferences. We chose the matching tasks because the exact values of IDRs can be estimated with them whereas only the range of IDRs can be estimated with choice tasks. Another reason is that the matching tasks allow us to avoid *the ordering of choice set in the MPL* to influence the stated preferences (the *anchoring effect*, Frederick et al., 2002). That is, when people make decisions between immediate and delayed rewards, the first choice they face often influences subsequent choices in choice tasks while the matching-task elicitation does not have this problem.[7] However, some studies on risk preferences argue that, due to the differences in the complexity of the task, matching-task elicitation can bias the estimations more than choice tasks (e.g., Bostic et al., 1990). Furthermore, a preference reversal occurs when the ranking of two (or more) items depends on the method used to elicit it (Lichtenstein & Slovic, 1971; Lindman, 1971). To see if our estimations are affected by the specific elicitation method, we compared our results with Tanaka et al. (2010) which used the same methods as ours except that they employed MPL for their elicitation. The parameters estimated by Tanaka et al. were fairly close to ours, suggesting that our data are reliable (Table 2.2).

The IDRs are larger in the loss domain in our data, in other words, the future losses are discounted more than the future gains. These results are the opposite of Thaler (1981), but in line with Appelt et al. (2011), Benzion et al. (1989) and Shelley (1993). This may have been caused by the *direction effect* (Appelt et al., 2011; Read, 2004), which claims that IDRs differ depending on how intertemporal decisions are framed. Specifically, intertemporal decisions can be presented using an acceleration frame (when the default is to realize the outcomes in the future) or using a delayed frame (when the default is to realize the outcomes today). Our intertemporal decisions use an acceleration frame because we asked participants to specify the present values of certain future outcomes. While Appelt et al. (2011), Benzion et al. (1989) and Shelley (1993) employed as acceleration frame as we did, Thaler (1981) used a delayed frame.

Our findings have potentially important policy implications. Many interventions for present-biased and time-inconsistent people have been discussed to help them improve their intertemporal decisions (Bryan et al., 2010; Hershfield et al., 2011; Milkman et al., 2013; Thaler & Benartzi, 2004). Our findings suggest that an optimal degree of intervention may differ between the gain and loss domain. For example, Gruber and Kőszegi (2004, 2008) estimated an optimal tobacco tax taking into account present-biased smokers overvaluing pleasure from smoking at the present moment. They estimated that the optimal tax is higher than the conventional one if average smokers have more severe present bias. The optimal tax was computed based on the degree of present bias estimated by previous experimental studies in the gain domain ($\beta \in [0.6, 0.9]$). However, smoking decisions can be considered as intertemporal decisions in the loss domain, because smokers have a tradeoff between painful smoking cessation and future health damage. Since our estimate of present bias in the loss domain (0.43) is much lower than the range of β they used, the optimal tobacco tax may be even higher than the one estimated by Gruber and Kőszegi (2004, 2008). As this example shows, our results point to the need for stronger interventions for intertemporal decisions in the loss domain than in the gain domain.

In addition, it is widely known that even when providing logically equivalent information to individuals, merely changing the framing of questions affects preferences and behaviors (Kühberger, 1998; Levin et al., 1998; Tversky & Kahneman, 1981). This framing effect is also a possible effective nudge to help improve individual behaviors

[7] For example, people would be more likely to choose \$60 in 1 month over \$50 today if they first chose between \$50 today and \$51 in 1 month than if they first chose between \$50 today and \$70 in 1 month.

(Thaler & Sunstein, 2008). Less severe present bias in the gain domain than in the loss domain implies that presenting choices as a gain frame instead of as a loss frame can potentially mitigate time-inconsistent behaviors in important intertemporal decisions. For example, for obese people to reduce their food intake, a policymaker may want to present a recommended meal plan in a gain frame (e.g., you can have 2,500 calories today) instead of presenting the meal plan in a loss frame (e.g., you need to reduce 500 calories today).

Table 2.4: Linear Regression Analysis of the Estimated Beta $\hat{\beta}$ and Conventional Discount Rate $\hat{r}$

	(1)	(2)	(3)	(4)
	$\hat{\beta}^{+}$	$\hat{\beta}^{-}$	$\hat{r}^{+}$	$\hat{r}^{-}$
IQ_score	0.0376*	0.0525***	-0.00268	-0.00146
	(0.0196)	(0.0191)	(0.00210)	(0.00233)
Earnings	-0.0168	-0.0441**	0.00478**	0.000871
	(0.0200)	(0.0195)	(0.00214)	(0.00237)
Age	0.00528	0.00657	0.000913	-0.00120
	(0.0166)	(0.0162)	(0.00177)	(0.00197)
Female	-0.0939	-0.0609	0.00651	-0.00166
	(0.0684)	(0.0666)	(0.00730)	(0.00811)
Constant	0.549	0.643	-0.0728*	0.0349
	(0.406)	(0.395)	(0.0434)	(0.0481)
N	68	68	68	68
adj. R^2	0.077	0.082	0.045	-0.049

Notes: Standard errors in parentheses; *, ** and *** stand for statistical significance at the 10%, 5% and 1% level respectively.

Appendix

2.A Estimation with Constant Relative Risk Aversion Utility Function

Many experimental studies (not only with regards to time preferences) assume a linear utility function when small payments are involved, as we did in our analyses. We nevertheless should consider non-linear utility functions, since this could affect our estimations of the present bias and conventional discount rates. We therefore repeat our regression analysis to estimate the parameters allowing for Constant Relative Risk Aversion preferences here.

The sign-dependent utility function is defined in the gain domain by $U(x) = x^{\zeta^+}$ and the loss domain by $U(x) = -\lambda(-x)^{\zeta^-}$ (c.f., Abdellaoui et al., 2007). The parameter λ indicates the level of loss aversion. For gains (losses), the power function is concave (convex) if the value of ζ is less than 1, linear if the value is equal to 1 and convex (concave) if ζ is more than 1. With this utility function, the model to be estimated becomes:

$$ln\left(\frac{X}{Y}\right) = \frac{1}{\zeta}(ln(\beta) - t \cdot r) \qquad \text{(A1)}$$

Our approach is to estimate β and r assuming various values of ζ because all the three parameters cannot be simultaneously estimated in Equation (A1).[8] Figure 2A1 summarizes the results, and it shows that our findings are mostly robust to different values of ζ. For example, Table 2A1 shows the results when we use the estimated values of the parameters from Abdellaoui et al. (2007) ($\zeta^+ = 0.576$ and $\zeta^- = 0.567$).[9] The present bias is significantly different from 1 although it is closer to 1 compared to the estimates under the linear utility assumption. The difference of β in the two domains is still highly significant, and the value of *r* is not statistically different.

As one can see in equation (A1), the loss aversion parameter λ is canceled out in the intertemporal decisions in the loss domain, thus our results are independent of the degree of loss aversion. This specification of loss aversion has been commonly used in the previous literature (Bleichrodt et al., 2001; Booij & van de Kuilen, 2009; Fishburn & Kochenberger, 1979; Tversky & Kahneman, 1992). However, this independence from loss aversion is no longer true in other specifications. For example, if loss aversion is time-dependent (i.e., the level of loss aversion changes as the timing of outcomes is delayed). This is beyond the scope of the paper, but future research could investigate whether loss aversion is time-dependent or not.

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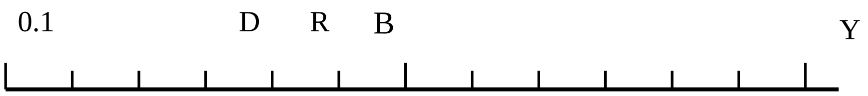
[8] The number of unknown parameters (β, r, ζ) is larger than the number of known variables from our experiment (*X/Y*, *t*) in Equation 3. Therefore, the equation has infinite combinations of the three parameters that minimize its econometric model's error term.

[9] We believe these values are much lower than what would be applicable to our experiment because they used very large payments in their experiment (more than 40,000 French francs: approximately €5200).

2.B Instructions of the Incentive Mechanism in the Experiment

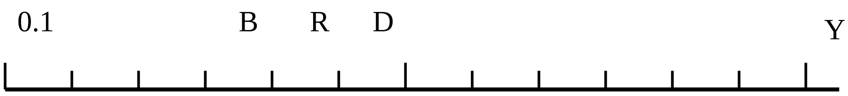
How much to declare?
If you think about it, you will see that **the best option (B) for you is to declare the amount that makes you indifferent between receiving such an amount today or the whole Y euro with delay (D=B).** We show you why with the following simple examples.

①



Let's suppose that your declared value D is smaller than your best option B (so you prefer receiving Y euro with delay to receiving less than or equal to B euro today) as in graph 1 above. If random number R selected by the computer is between D and B, you end up receiving R euro today instead of Y euro with delay. However, you would receive Y euro with delay which is better than R euro today for you if you declare a value equal to the best option (D=B) since R euro is less than B euro.

②



Let's suppose that your declared value D is larger than your best option B (so you prefer receiving more than B euro today to receiving Y euro with delay) as in graph 2 above. If random number R selected by the computer is between D and B, you end up receiving Y euro with delay instead of R euro today. However, you would receive R euro today which is better than Y euro with delay for you if you declare a value equal to the best option (D=B) since R euro is more than B euro.

Table 2.A1: Regression Analysis with Quasi-hyperbolic Discounting Model Assuming CRRA Utility Function

	(1) Gain	(2) Loss	(3) All
β [= $exp(constant)$]	0.769***	0.621***	
	(0.0243)	(0.0280)	
r [= $time$ * (-1)]	0.00547***	0.00405**	
	(0.00182)	(0.00189)	
Constant	-0.262***	-0.476***	-0.262***
	(0.0316)	(0.0450)	(0.0316)
t	-0.00547***	-0.00405**	-0.00547***
	(0.00182)	(0.00189)	(0.00182)
Loss			-0.214***
			(0.0495)
t * Loss			0.00142
			(0.00228)
N	1360	1360	2720
adj. R^2	0.014	0.004	0.049

Notes: Clustered standard errors in parentheses; *, ** and *** stand for statistical significance at the 10%, 5% and 1% level respectively.

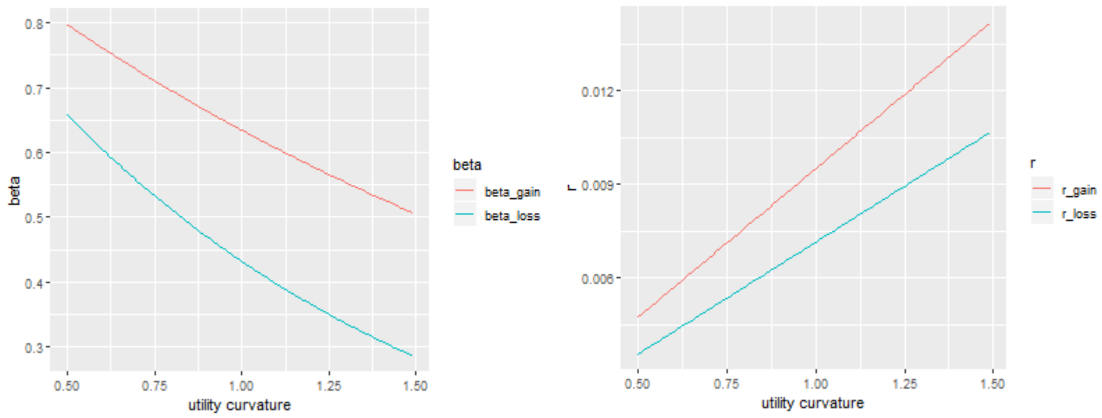


Figure 2.A1: The Values of β and r in the Gain and Loss Domains with Different Utility Curvature ζ.

Chapter 3

NUDGES AND EMOTIONAL STATES

Abstract

In this paper, we explore how emotions affect the effectiveness of nudges. We conducted four experiments. One experiment asked the participants to report their emotions and three experiments induced particular emotions in them. After this part, they were nudged by default and social nudges in all the experiments. We could not find any effects of emotions on the effectiveness of nudges. Our results are complicated by the fact that, in three of our four experiments, we did not replicate the expected effects of the nudges, which raises doubts about the general effectiveness of some of the most prominent nudging tools.

3.1 Introduction

Choice architecture or "nudging" has become in the last years one of the main frames of reference in the application of behavioral economics. This terminology was introduced by Thaler and Sunstein in their highly influential book Nudge (Thaler & Sunstein, 2008), building also on previous work by the same authors (see Sunstein & Thaler, 2003). The main idea is that small changes guided by behavioral science in environments in which people make decisions can have big influences on behavior. In other words, if we understand (through social and behavioral science) the principles that guide people's decisions, we can use them to introduce small systematic changes in decision environments and push people to behave in certain ways, without limiting their freedom of choice.

The nudging framework has mostly been developed with application and intervention in mind and it has had great impact in both the private sector and in economic and social policy. Several countries have created government bodies (at the national and at the local level), known as Behavioral Insights Teams or Nudge Units, dedicated to the design of economic and social policies based on nudging to influence the behavior of consumers and citizens.

In the framework of nudging, many tools have been developed that have been applied to a wide variety of environments and behaviors. Some prominent examples include: the use of default options to increase organ donations or to make people save more for retirement (Johnson & Goldstein, 2003; Madrian & Shea, 2001); the use of social norms to increase tax payments (Sunstein, 2014); the introduction of apparently irrelevant or extreme options to push people to pick particular products (Ariely, 2008); the use of pre-commitment to increase saving rates (Thaler & Benartzi, 2004); signing at the beginning instead of the end of forms to increase honesty (Shu et al., 2012); the use of reminders to increase saving (Karlan et al., 2016); or the projection of a future self to improve investment decisions (Hershfield et al., 2011); among many others.

Despite the widespread (and increasing) application of nudging, it is still unclear which are the determinants of the effectiveness of particular interventions. When, where and in which circumstances are default options, social norms, reminders, etc. most effective? In this paper, we focus on one particularly relevant aspect: people's emotional state when they are being nudged.

Currently, there is a large volume of evidence showing that emotional states substantially affect decision making (see Lerner et al., 2015, for a review). There are also several influential theoretical frameworks that focus on this idea, such as the Somatic Marker Hypothesis (Damasio, 1994), the Affect Heuristic (Finucane et al., 2000), the Risk as Feelings framework (Loewenstein et al., 2001), and the Appraisal-Tendency Framework (Lerner & Keltner, 2000, 2001).

Research on all these frameworks shows that emotions play an important role in decision making. For instance, fear and anxiety lead to less risky decisions and anger to more risky ones (Lerner & Keltner, 2001); sadness produces a stronger drive for immediate gratification (Lerner et al., 2013), while happiness generates more patience (Ifcher & Zarghamee, 2011; Pyone & Isen, 2011); and sadness also leads to more reflective and less impulsive behavior (Lerner & Keltner, 2011). From all this research, we derive our hypothesis that emotional states will substantially affect the effectiveness of nudging, that is, how and how much nudging affects decisions.

Given the lack of previous research on this particular question, we can only conjecture about the exact effects we will obtain. In this sense, our research will be somewhat exploratory in nature. What seems clear is that we should expect distinct emotional states to affect different nudging interventions differently. For example, an emotional state of sadness is likely to increase the effectiveness of a nudge that aims to increase spending in the present moment. On the contrary, a state of happiness could increase the effectiveness of nudges that seek behavior that takes future consequences more into account. It is also likely that nudging interventions that push people into taking less risk work better in a state of fear or anxiety and nudges that look for more risky behavior have a stronger effect in a state of anger, and so on.

We focus on two different types of nudging to answer these questions: default options and social norms. These are two of the most widely used nudging tools and two of the ones with the strongest effects. Moreover, they rely on different psychological processes and we expect them to be affected differently by emotional states. The first one consists in establishing one of the choice alternatives as the default option, which generates a strong tendency to pick that option (Johnson & Goldstein, 2003; Madrian & Shea, 2001). This effect has been explained using principles such as psychological inertia and loss aversion. The second tool (social norms), consists in suggesting a social norm established in a particular context, for example by giving information on how others behave in that context, which produces a tendency to decide in line with that norm (Goldstein et al., 2008; Sunstein, 2014). This pattern has been explained using principles like the need to belong to a social group and the informational value of social norms.

Given that these two nudging tools relate to distinct psychological principles, we expect their effectiveness to be differently affected by emotional states. Due to the lack of previous research on the topic, we can only hypothesize about the exact effects. In any case, any systematic relationship found between emotional states and the effectiveness of nudging will constitute a substantial contribution. Our prediction is that emotions associated to a feeling of certainty and control, such as happiness, anger or pride (Lerner & Keltner, 2000) will lead to greater effectiveness of the default nudge, given that these emotions produce less reflective behavior and an increased psychological inertia (Tiedens & Linton, 2001). Previous literature has found that individuals who are in a happy mood are more likely to adopt a heuristic processing strategy (Schwarz, 2000; Schwarz & Clore, 2007). On the contrary, sadness may lead to lower effectiveness of default nudges. One of the reasons why default options are thought to work is that people evaluate other options in reference to the pre-selected option with which they are already endowed (Dinner et al., 2011). And there is evidence showing that sadness lowers the valuation of items that people are endowed with (Lerner et al., 2004).

In the case of social norms, we predict that these patterns could reverse, given that a stronger feeling of certainty and control should lead to a lower incidence of social norms or, in other words, to a reduced influence of the behavior of others. These differing characteristics of default options and social norms allow us to study the effects of emotional states on different types of nudging tools.

To implement these two types of nudging in our experiments, we chose scenarios for each nudge based on previous literature. We used three different decision scenarios in the case of the default nudge: decisions about accepting tasks, charitable giving and volunteering; and two different scenarios in the case of the social nudge: decisions about accepting tasks and product choices. These scenarios reflect important areas of application of nudging that have been studied before.

To examine the effect of emotions on the effectiveness of nudges, Experiment 1 used an experience-sampling approach to test if emotional states affect the effectiveness of default and social nudges designed to push people into doing effort tasks. In this experiment, we measured emotional states instead of inducing them, to be able to cover a wider range of emotions and to investigate emotions that people naturally experience (as opposed to induced emotions). Experiments 2 and 3 used a simpler online design to examine the effect of specific emotion inductions on the effectiveness of a default nudge. Experiment 4 studied the effect of the same emotion inductions on the effectiveness of a social nudge.

Our experiments consistently found no impact of emotional states on the effectiveness of nudges. Furthermore, our results are complicated by the fact that, in three of our four experiments, we did not replicate the expected effects of the nudges, which raises doubts about the general effectiveness of some of the most prominent nudging tools.

3.2 Experiment 1: Nudging Willingness to Work

This study investigates if emotional states affect the effectiveness of nudges designed to push people into doing real effort tasks. To do this, we employed an experience-sampling method to send people notifications on their smartphones, in which we asked them to report their emotions and we invited them to do the effort tasks. Thus, this method enables us to track their emotions when performing day to day activities. In those notifications, we also included social and default nudges. The notifications were sent randomly throughout the day to obtain rich daily data for each participant. As we discussed in the introduction, our prediction was that happiness would lead to greater effectiveness of the default nudge, while sadness would do the opposite. On the contrary, we predicted that these patterns could reverse in the case of the social nudge.

3.2.1 Method

We recruited 27 participants (56% female, mean age 20, minimum age 18, and maximum age 30) who agreed to participate in the whole study. The experiment lasted eight days, which allowed us to obtain rich data for each participant. They came to our lab only on the first day of the experiment and participated in the rest of the experiment (from the second to the eighth day) online through their smartphones.

This study consisted of three parts which we refer to as: initial survey, online surveys, and real effort tasks using sliders (Gill & Prowse, 2011). In the effort tasks participants had to move the position of 30 sliders to a specific position. We set the number of the sliders to 30, based on a pilot study, because for this number the participants sometimes accepted to do the task and sometimes they rejected it.

The initial survey was conducted in the lab. First of all, the participants were asked to download the smartphone app *RealLife exp* (https://www.lifedatacorp.com/mobile-app/) on their own smartphones. They then used this app throughout the experiment. The initial survey asked them to do go through an example of the effort task so that they understood what it was about. At the end of the initial survey, they completed demographic questions.[1] All participants had to correctly answer a qualification question to verify that they understood the whole structure of the study. If an incorrect answer was given, a warning message popped up and this information was recorded on the system. They could not proceed until they answered it correctly. After the initial survey, the participants left the laboratory.

From the second to the eighth day, each participant received notifications through the smartphone app eight times per day (Figure 3.1). The notifications were sent at random times throughout the day. There were four types of notifications: no-task condition, control condition, default nudge condition and social nudge condition. One of these types was randomly selected to be sent each time. The timing of the notifications in the experiment was based on Taquet et al. (2016). The minimum time between two notifications was set to 2 hours to avoid large autocorrelations between answers to the same question in consecutive surveys. An Internet connection was not required to receive the notifications. They could respond to the notifications within 30 minutes after receiving them, and then they expired.

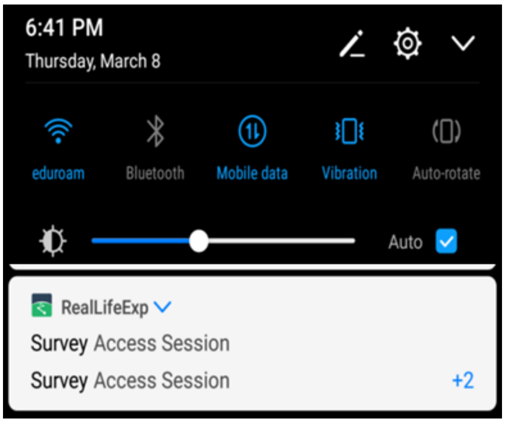


Figure 3.1: Screenshot of a Notification on a Smartphone

After responding to the notifications, they always connected to the Internet and answered an online survey asking for happiness and energy levels with affective sliders (Betella & Verschure, 2016), including also other related questions.[2] By moving the handle of the affective sliders, the participants expressed their current levels of happiness and energy. We converted the position of the handle to a number between 0 (very negative) and 100 (very positive). After this part, they saw a different invitation message depending on the condition: in the no-task condition participants saw no invitation message and finished; in the control condition they had the opportunity to do the slider task by checking the option “Yes, I would like to do the slider task”; in the default nudge condition they saw the same option as in the control condition except that the option was pre-selected by default and they had to uncheck the option if they did not want to do the task; in the social nudge condition they also had the opportunity to do the

[1] The questions included gender, age, English level, native language, happiness level in general, how clear the instructions were, and how often they wanted to complete the effort tasks when they had opportunities to do so.

[2] The other questions include their current location, type of activity, person they were interacting with, and their other emotional states of anger, fear, sadness, embarrassment, serenity, pride, joy, and love.

task (without a default), and they were shown information of previous participants' completion rates, which varied randomly between 85-95%.[3]

The participants received a payment depending on the completion rates of the online surveys and the number of the completed slider tasks. Partial completion of a particular online survey or slider task was considered as not doing it. As for the online surveys, they received €20 if they completed more than 50% of all the possible surveys in which they could participate; or €5 if they did not get to 50%. As for the effort tasks, they received an additional €0.5 for each effort task that they completed. They received their payment when they completed the eight days of the experiment via bank transfer.

3.2.2 Results and discussion

All participants did more than 50% of the online surveys, so all of them received the corresponding €20 for this. The frequency of recorded results and the acceptance rate of the tasks for each day of the week are reported in Figure 3.2. We do not find systematic patterns in these variables depending on the day. On average, the participants accepted 80.3% of all the tasks that they could possibly do. 10 participants always accepted the invitation to do the tasks.

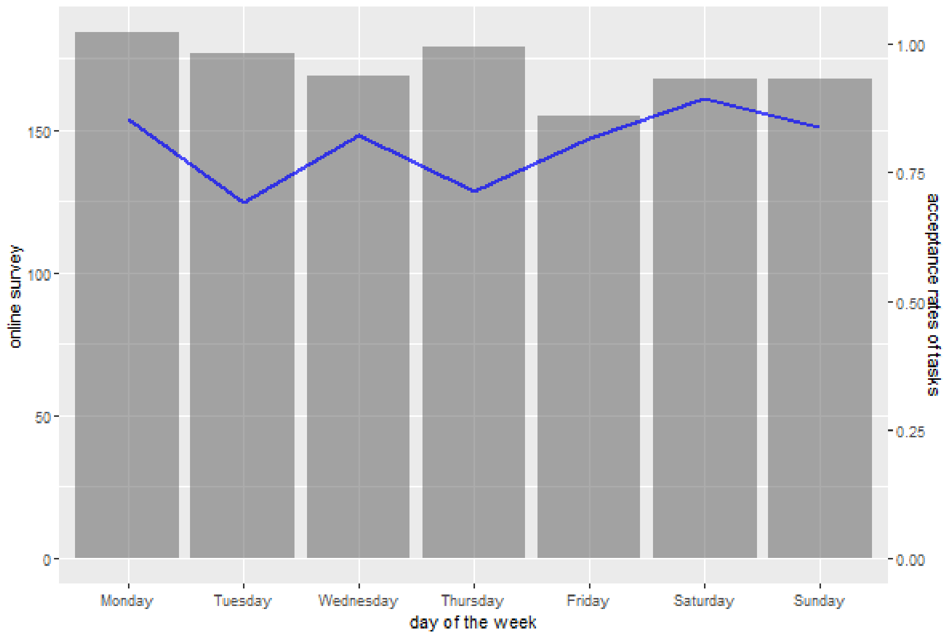


Figure 3.2: Acceptance Rates of Tasks (blue line) and the Number of Online Surveys Taken (bars) across Day of the Week

Figure 3.3 presents the acceptance rates of the effort tasks in the three different conditions (control, default nudge and social nudge), depending on the reported level of happiness. The variable Happier is a dummy variable taking the value of 1 if the

[3] The percentage of the pilot study was more than 95%. We varied the number each time for the participants to feel natural.

average level of happiness reported by the participant is higher than the median across all observations, and 0 otherwise. From the graph, the acceptance rates seem slightly higher in the two nudge conditions, and the effect of both nudges seems slightly stronger for less happy people.

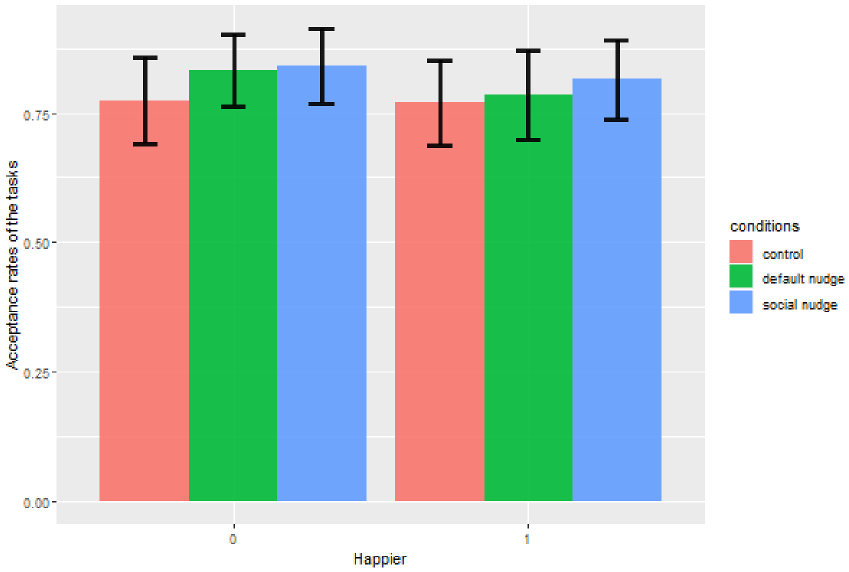


Figure 3.3: Acceptance Rates of Tasks in Happier or Less Happy Moods across Control (colored in pink), Default Nudge (colored in green) and Social Nudge (colored in blue) Conditions (Experiment 1). The error bars indicate 95% confidence intervals.

Table 3.1 shows the results of an individual-level fixed-effects linear regression analysis of the acceptance rates on emotions and the nudges. The variable Happy takes the converted values from the affective slider. The dummy variable Social takes the value of 1 if the condition is the social nudge condition and 0 otherwise. Similarly, the dummy variable Default takes the value of 1 if the condition is the default nudge condition and 0 otherwise. The variable Happy is not significant (column 1), showing that happiness did not have an effect on acceptance rates. The default nudge had a positive impact on the acceptance rates, while the social nudge did not (column 2). So, we replicated the effect of one of the nudges, but not the other. Finally, the interaction terms of the two nudges and the happy variable are not significant (column 3). This shows that happiness levels did not moderate the effectiveness of the nudges.

We replicated the effect of the default nudge on acceptance rates of tasks. However, we did not find a main effect of the social nudge (or of happiness). Our main hypothesis that emotional states have an impact on the effectiveness of the nudges was not confirmed in the experiment.

One potential concern here is that, despite our large number of observations, the number of participants was not too high. In particular, we did find that acceptance rates in the social nudge condition were higher than in the control condition, but the difference did not reach statistical significance. This insignificant result might be due to the specific people we had in our sample. In our results, 10 participants always accepted the tasks, regardless of the condition, and on average the participants accepted 80.3% of all the tasks that they could possibly do. These results were higher than in our pilot study. Thus, it is possible that some of the effects that are insignificant here would reach significance with a larger sample. The effects, however, do not seem to be large in terms of size. Future research could also increase the cost of the effort tasks (e.g., increasing the number of sliders from 30 to 60), so that acceptance rates are not so extreme and there is more room for influence.

Table 3.1: Regression Analysis of Task Acceptance Rates (Experiment 1)

	(1)	(2)	(3)
	Task	Task	Task
Happy	-0.000319		0.000107
	(0.000420)		(0.000964)
Social		0.0499	0.0728
		(0.0327)	(0.109)
Default		0.0702**	0.107
		(0.0329)	(0.0815)
Social#Happy			-0.000339
			(0.00140)
Default#Happy			-0.000576
			(0.00107)
N	595	595	595
R^2	0.000	0.009	0.009

Notes: All regressions include individual level fixed effects. The dependent variable is the task acceptance rates. Clustered standard errors in parentheses. *, ** and *** stand for statistical significance at the 10%, 5% and 1% level respectively.

3.3 Experiment 2: A Default Nudge on Donation Behavior

In Experiment 1, we used an experience-sampling method to try to link emotional states to the effectiveness of nudging interventions. This approach clearly has some desirable aspects, such as eliciting naturally-occurring emotional states that people experience in their daily lives. However, Experiment 1 is quite different from the typical studies found in the literature on emotions and decision making and on nudging, and this may partly explain the non-significant results. For instance, the same people were repeatedly nudged at random times to do the same task using different nudges, and the emotional states were not cleanly induced before the decisions were made.

In terms of the decision domain, here we focused on charitable giving. This is a domain of decision making that has attracted great interest, and also one in which default nudges have been studied before. For example, donation decisions have been shown to be affected by default nudges (Altmann et al., 2018; Goswami & Urminsky, 2016) and by suggested donation amounts (Edwards & List, 2014).

Furthermore, there is already some evidence indicating that induced emotions are likely to have an influence on the effectiveness of defaults (Scheibehenne et al., 2014) and on donation decisions (see Fiala & Noussair, 2017). Our specific hypothesis is again that the effect of defaults on donation decisions will be mitigated by inducing sadness and amplified when happiness is induced.

3.3.1 Method

Participants. We recruited 600 participants from English-speaking countries for our experiment via Prolific Academic (60.8% female, M_{age} = 37.6 years, age range: 18-78 years).[4] These people had not participated in Experiment 1. The study took an average of 7 minutes and 29 seconds to complete and subjects received a fixed fee of £1 for their participation.

Design and procedure. This study employed a 2 (emotional induction: happiness, sadness) X 2 (active choice, default nudge) between-subject design. The participants were randomly allocated into one of the four conditions. After signing the consent form of the experiment, the participants were asked to complete a written task to induce the target emotional states and then they were asked to answer demographic questions. Finally, they had to make a decision about donating £0.1 of their payment to a charitable organization (under either the active choice or the default condition).

Emotion induction. The emotion induction procedure employed was based on a procedure developed by Strack et al. (1985) to manipulate emotional states and validated in other papers (see Lerner & Keltner, 2001; Milkman, 2012; Tiedens & Linton, 2001). The induction asked participants to first "briefly describe 3 things that you feel very [happy/sad] about." The following question asked participants to "describe in some detail the one situation you can remember that has made you feel the [happiest/saddest] you have been in your life, and describe it such that a person reading the description would feel [happy/sad] just from hearing about the situation." The last questions asked them to "describe how experiencing [happiness/sadness] generally makes you feel" and "write down, as specifically as you can, what you think physically happens to you when you feel [happy/sad]."

Then, after our demographic questions,[5] we included a manipulation check that consisted in asking for happiness and energy levels using affective sliders (Betella & Verschure, 2016), like in Experiment 1. We converted the position of the handle to numbers between 0 (very negative) and 100 (very positive).

Donation. After the manipulation check, we asked the participants to write any comments they may have on the experiment. Then they had to click on a button called "FINISH" to continue, which we expected to give the impressions that this was the end

[4] We restricted the sample because the participants must write a fair amount of texts in the experiment. Specifically, their nationality is either the United Kingdom, the United States, Ireland, Australia, Canada or New Zealand.

[5] We asked about age, gender and native languages.

of the experiment. When they clicked the button, they were directed to a donation page, where they had to decide whether they wanted to donate £0.1 of their payment (£1) to a charity or not. In the active choice group, they had to click on one of the two options (Figure 3.4). They donated £0.1 by clicking on the option “Yes, I would like to donate”, and they kept their whole payment by clicking on “No, I prefer not to donate”. In the default nudge group, only one option saying “Yes, I would like to donate” was presented and this option was pre-selected (Figure 3.4). Participants could keep the full payment of £1 by unselecting the option.

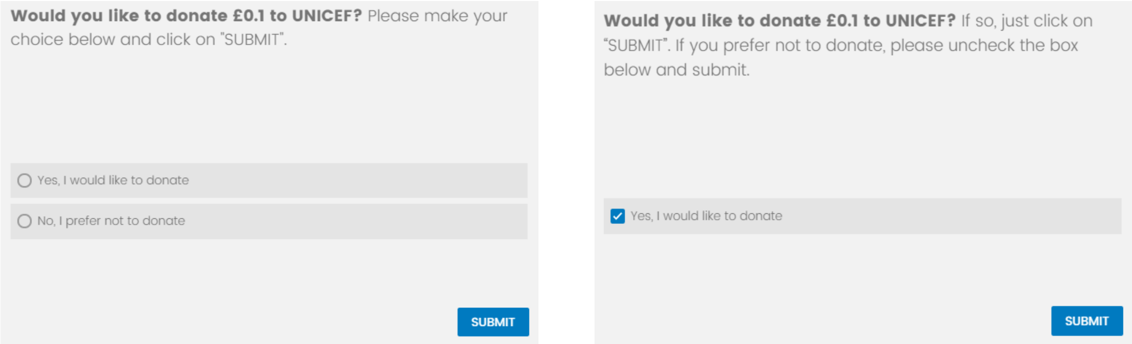


Figure 3.4: Screenshots of Donation Decisions. The active choice condition on the left and the pre-selected choice condition on the right.

3.3.2 Results and discussion

A Mann-Whitney test confirmed that our manipulation worked as intended. The participants in the happiness condition felt happier (z = 8.143, p < .01), based on the scores on the affective slider for happiness. Figure 3.5 shows the effects of the default and the emotion inductions on the donation decisions. Surprisingly, neither the default option nor the emotion inductions had an effect on donations (Mann-Whitney tests: z = 0.029, p > .1; z = 0.029, p > .1). For our regression analysis, we use the dummy variable Happy which takes the value of 1 if the condition is the happiness condition and 0 otherwise. Similarly, the dummy variable Nudge takes the value of 1 if the condition is the default nudge condition and 0 otherwise. A logistic regression analysis further confirmed that the default and the emotion inductions had no effect (Table 3.2, columns 1 and 2). The interaction of the default and affect variables is also insignificant, which shows that the induced emotions did not moderate the effect of the default nudge (Table 3.2, column 3). Therefore, our main hypothesis in Experiment 2 is not confirmed.

It is puzzling that we did not find an effect of the emotion induction on donations, even though we successfully induced happiness and sadness. This finding is inconsistent with some results reported in the previous papers, such as Fiala & Noussair (2017).

Furthermore, we did not even find an effect of the default nudge. There are, however, other cases in which choice proportions have been found not to be different between active choice and pre-selected choice. For instance, Johnson et al. (2002) asked web users participating in an online health survey whether they wanted to be contacted with further surveys using two different defaults (a default to accept and another one to reject) and an active choice. They found a sizable difference between the effect of the default to accept and the default not to accept the offer. However, there was little difference between active choice and the default to accept the offer. Similarly, Pichert & Katsikopoulos (2008) presented their subjects with a choice between two suppliers. When an eco-friendly supplier was the default, 68% of participants stuck with it, but when the default was another cheaper supplier, only 41% of people chose the eco-friendly supplier. On the other hand, about the same percentage of participants (67%) chose the eco-friendly supplier in active choice as when it was the default. Based on these results, we conjecture that if we had added another nudge condition where the option “No, I prefer not to donate” was pre-selected, we might have found an effect of the nudge on donation decisions. This possibility is investigated in Experiment 3.

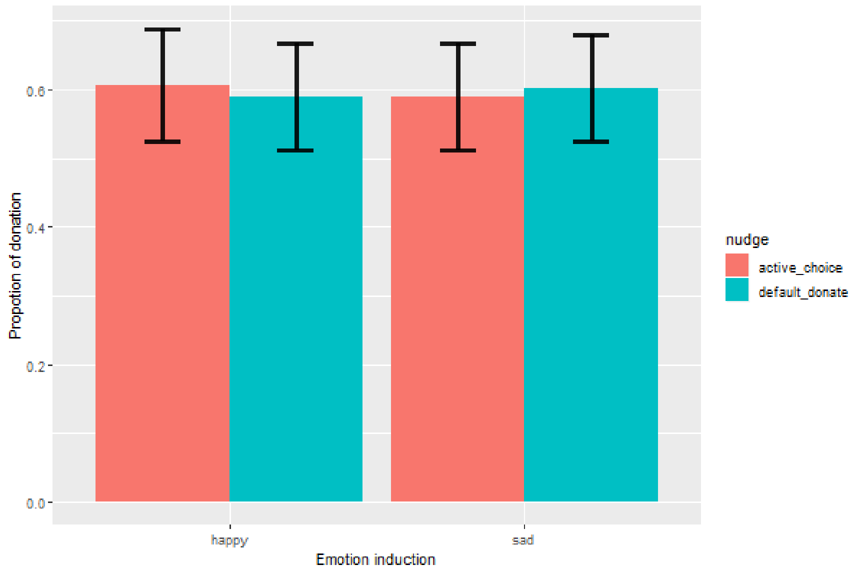


Figure 3.5: Proportion of Donation after Happiness and Sadness Emotion Inductions across Active Choice (colored in pink) and Pre-selected Choice (colored in blue) Conditions (Experiment 2). The error bars indicate 95% confidence intervals.

3.4 Experiment 3: A Default Nudge on Volunteering Behavior

Our hypothesis on the effects of happiness and sadness on the effectiveness of a default nudge to donate was not confirmed in Experiment 2. Moreover, we did not even find a main effect of the default nudge on donations, which was inconsistent with some previous studies investigating the effect defaults on charitable giving (see Altmann et al., 2018; Goswami & Urminsky, 2016).

Therefore, the main purpose of Experiment 3 was to replicate the effect of the default nudge with an improved design and to test the same hypothesis as in Experiment 2: that the effectiveness of the default nudge is affected by emotions. To do so, in Experiment 3 we used different default nudge conditions, namely a default to accept and a default to reject (instead of the active choice used in Experiment 2). In line with previous evidence (see Johnson et al., 2002; Pichert & Katsikopoulos, 2008), we expected this set-up to produce stronger default effects and to provide a better basis to test our main hypothesis.

Table 3.2: Logistic Regression Analysis of Donation Decisions (Experiment 2)

	(1) Donated	(2) Donated	(3) Donated
Nudge	0.995		1.055
	(0.166)		(0.246)
Happy		1.005	1.069
		(0.167)	(0.255)
Happy#Nudge			0.886
			(0.296)
N	600	600	600
pseudo R^2	0.000	0.000	0.000

Notes: Odds ratios of coefficients from Logit regressions are reported. The dependent variable is a binary variable that takes the value 1 if the participants donated. Standard errors in parentheses. *, ** and *** stand for statistical significance at the 10%, 5% and 1% level respectively.

3.4.1 Method

Participants. We recruited 600 English-speaking participants for our experiment via Prolific Academic (55% female, M_{age} = 35.8 years, age range: 18-73 years). They also had not participated in Experiments 1 and 2. The study took an average of 9 minutes and 25 seconds to complete and subjects received a fixed fee of £1 for their participation.

Design and procedure. The design of the emotion induction part of this experiment was the same as in Experiment 2. We used a slightly modified version of the setting used in Johnson et al. (2002), where the authors found a sizable effect of their default nudge on accepting an offer to be conducted by an experimenter, notifying an additional survey. This experiment used a similar scenario in which the participants decide to accept an offer by doing an additional survey or rejecting it. This study had a 2 (emotion induction: happiness, sadness) X 2 (pre-selected choice to accept to do an additional survey, pre-selected choice to reject to do an additional survey) between-subject design. The participants were randomly allocated into one of the four conditions. After completing the emotion induction part, the manipulation check and the demographic questions as in Experiment 2, subjects made a decision to accept or decline the offer. The additional survey asked mostly about their opinions on Prolific Academic. This survey took around two minutes to complete.

3.4.2 Results and discussion

As for our manipulation check, a Mann-Whitney test on the happiness affective slider revealed that the participants induced to feel happiness were indeed happier ($z = 6.893$, $p < .01$). Figure 3.6 shows the effects of the defaults and the emotion inductions on the solicitation acceptance decisions. This time defaults had a significant impact on the volunteering (Mann-Whitney test: $z = 3.829$, $p < .01$), but again, emotions had no main effect (Mann-Whitney test: $z = 0.335$, $p > .1$). We used the same dummy variables Nudge and Happy as in Experiment 2 for our regression analysis. A logistic regression analysis confirms that the defaults had a significant impact (Table 3.3, column 1), but the emotions did not (Table 3.3, columns 1 and 2). The interaction of defaults and emotions had again no impact on solicitation acceptance rates (Table 3.3, column 3).

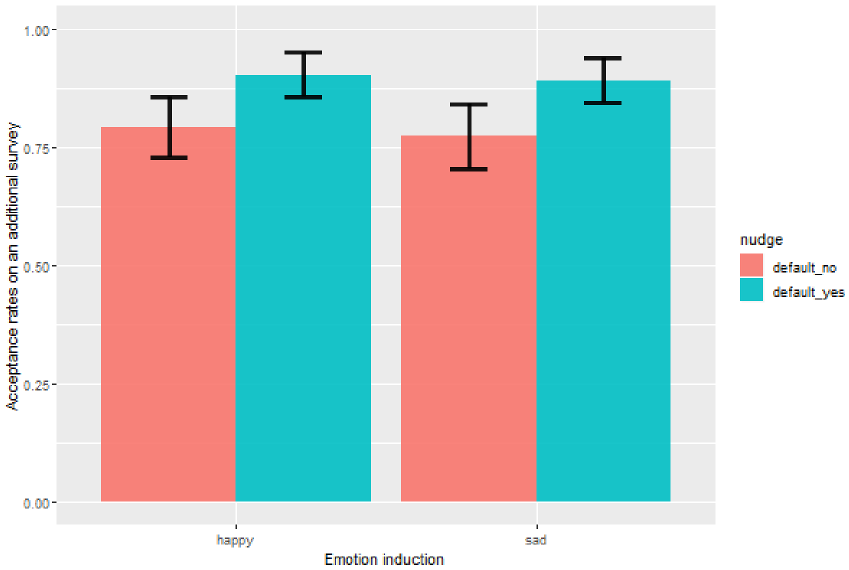


Figure 3.6: Acceptance Rates on an Additional Survey after Happiness and Sadness Emotion Inductions across Default to Reject (colored in pink) and Default to Accept (colored in blue) Conditions (Experiment 3). The error bars indicate 95% confidence intervals.

We replicated the results of Johnson et al. (2002) and found the effect of the default nudge. However, as in Experiments 1 and 2, there was no significant main effect of emotions, and also no significant interaction of emotions and nudges. Again, this fails to support our main hypothesis on the moderating role of emotions on the effectiveness of default nudges.

3.5 Experiment 4: A Social Nudge on a Product Choice

Across the three previous experiments, we failed to find moderating effects of emotions on the effectiveness of default nudges. In order to check the generalizability of these results, in Experiment 4 we tested the same hypothesis and applied the same type of methodology as in the previous two experiments to a social nudge. Social nudges are another one of the most widely used nudging tools, and as explained in the introduction, they rely on quite different mechanisms compared to default nudges.

Table 3.3: Logistic Regression Analysis of Acceptance of Questionnaire (Experiment 3)

	(1)	(2)	(3)
	Accepted	Accepted	Accepted
Nudge	2.429***		2.413***
	(0.574)		(0.787)
Happy		0.928	0.880
		(0.208)	(0.335)
Happy#Nudge			1.025
			(0.486)
N	600	600	600
pseudo R^2	0.028	0.000	0.029

Notes: Odds ratios of coefficients from Logit regressions are reported. The dependent variable is a binary variable that takes the value 1 if the participants accepted to do the additional survey. Standard errors in parentheses. *, ** and *** stand for statistical significance at the 10%, 5% and 1% level respectively.

To maximize our chances of replicating the social nudge part of the experiment and provide a good basis to test our main hypothesis, we used a modified version of Huang and Chen (2006) for our main decision scenario. These authors found a large effect of providing information about relative sale volumes on their participants' choices of products in an online survey.

3.5.1 Method

Participants. We recruited 530 English-speaking participants for our experiment (50 for a first batch and 480 for a second batch) via Prolific Academic (65% female, M_{age} = 33.2 years, age range: 18-73 years). These subjects had not participated in Experiments 1, 2 and 3. The study took an average of 9 minutes and 16 seconds to complete and subjects received a fixed fee of £1 for their participation. They also had the chance to receive an item they had selected, as explained below. We excluded from our sample one participant who did not enter the code participants needed to provide to receive payment.

Design and procedure. The design of the emotion induction part of this experiment was the same as in Experiments 2 and 3. After completing the induction part, the demographic questions, and the manipulation check, as in Experiments 2 and 3, they had to pick the ballpoint pen they preferred out of a selection of two different pens from different brands: Cross and Pierre Cardin. This decision making scenario was based on Huang and Chen (2006).

This experiment was conducted in two batches. We first ran a *no-nudge batch* and then a *nudge batch*. The participants in the no-nudge batch made a choice without any peer information. The purpose of the no-nudge batch was twofold. The first purpose was to obtain results without nudges that could be used as a control condition to compare with the results of our social nudge. The second purpose was to gather enough product choices from the participants to be able to construct social nudges for the second batch, based on what these first participants had done.

The nudge batch was the same as the no-nudge batch, except that people were provided with social information based on the choices of the participants in the no-nudge batch. In particular, we created two different social nudge conditions. In both of them, we showed people 10 selected pen choices from previous participants (taken from the no-nudge batch). In one of the conditions, which we named *Popular Cross*, 9 out of the 10 previous choices shown to the participants favored the Cross pen; in the other condition, 9 out of 10 favored the Pierre Cardin pen. We also provided participants a table to help visualize this information (Figure 3.7).

At the end of the experiment, some participants were selected to receive their chosen pen for real and were informed of their selection. All participants were informed of this possibility before they made their pen choices. As for the selected participants, their chosen pen was then dispatched from Amazon to their desired address.

3.5.2 Results and discussion

To begin with, a Mann-Whitney test on the happiness affective slider showed that the participants induced happiness felt happier as intended ($z = -5.257$, $p < .01$). Figure 3.8 depicts the effects of the social nudge and the emotion inductions on the pen's choices. In clear contrast to the typical social nudge effect (that we expected to obtain), participants chose the Cross pen more in the Popular Pierre condition than in the Popular Cross condition and than in the control group. They also chose the Cross pen slightly less in the Popular Cross condition than in the control group. The difference between the conditions is statistically significant (Kruskal-Wallis test: $\chi^2 = 9.46$, $p < .01$). As in our previous experiments, the emotion induction had no main effect on the choices (Mann-Whitney test: $z = 0.038$, $p > .1$). According to Figure 3.8, the nudge seemed to have a somewhat stronger impact in the happy condition. We checked the statistical significance of this pattern in the following analysis.

We conducted a logistic regression analysis to further analyze the effect of nudges and emotions. The variable pop_pierre is a dummy variable, taking the value of 1 if the condition was Popular Pierre. Similarly, the variable no_nudge is a dummy variable, taking the value of 1 if the observations were from the no-nudge condition. As Table 3.4 shows, people chose the Cross pen significantly more in the Popular Pierre condition, while the difference between the Popular Cross condition and the no-nudge group is not significant (Table 3.4, column 1). The emotion inductions had no significant effect on the product choice (Table 3.4, column 2). The interaction of social nudges and emotions is also non-significant (Table 3.4, column 3), which again fails to support our main hypothesis about emotions moderating the effectiveness of nudges.[6]

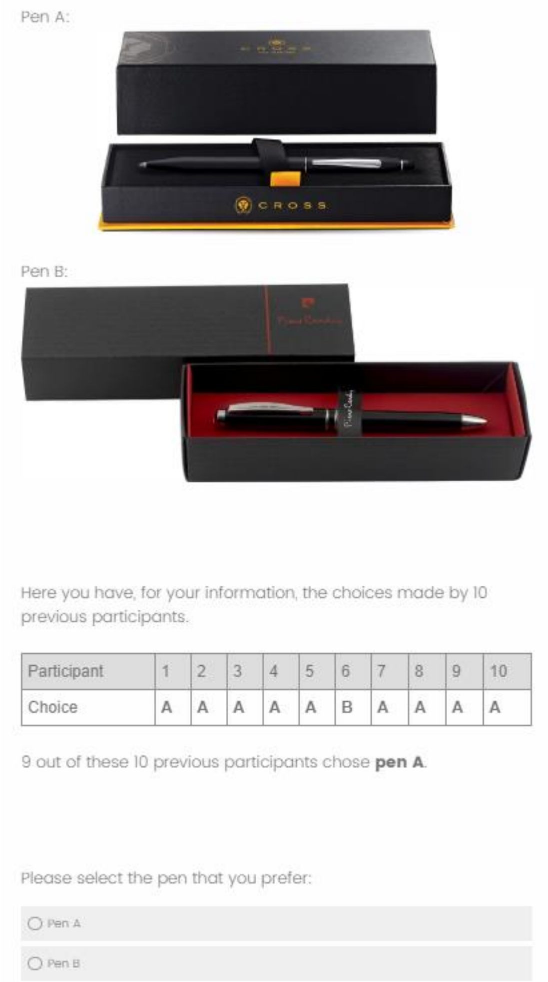


Figure 3.7: Screenshot of Product Choice in Popular Cross Condition (Experiment 4)

The results of Experiment 4 are in line with the patterns we obtained in the previous experiments. The effects of emotions on decisions and on the effectiveness of the nudges were not significant. Interestingly, we found an adverse effect of the social nudge on the product choice. This is the opposite of what we expected and inconsistent with similar previous experiments (see Huang & Chen, 2006; Huh et al., 2014; Sunstein, 2014).

However, this experiment is not the first one to find that social nudges backfire. Bicchieri and Dimant (2019) claimed that peer information may not be effective on people's behaviors if: (1) the information is not from their reference networks, such as their family or close friends; (2) the messenger is not trusted; and (3) the information is inconsistent with participants' beliefs. So, these might all be reasons why our social nudges did not work. In addition, some participants might have wanted to separate

[6] We repeated this regression analysis excluding the control condition, but the main results did not change.

themselves from the norm, or might have reacted negatively to what they perceived to be an attempt to influence them, as psychological reactance theory implies (Brehm, 1966).

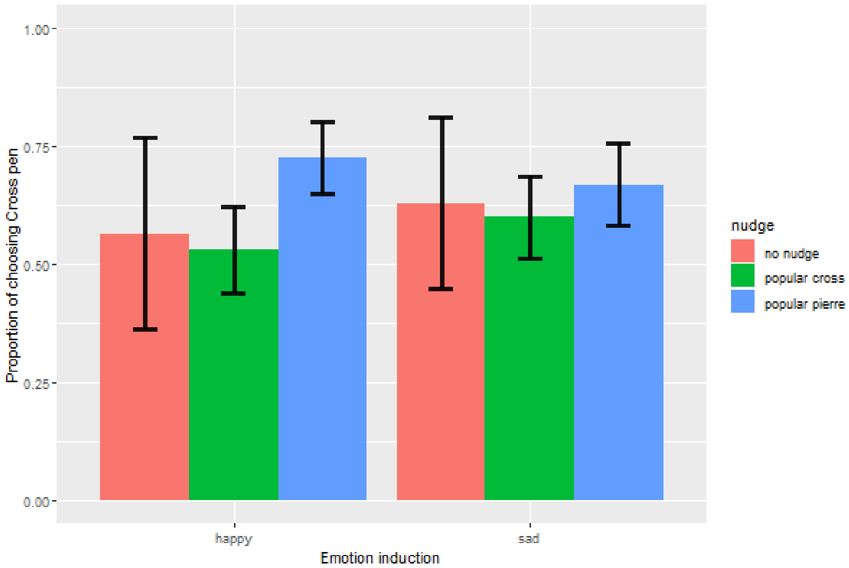


Figure 3.8: Proportion of Choosing Cross Pen after Happiness and Sadness Emotion Inductions across Control (colored in pink), Popular Cross (colored in green) and Popular Pierre (colored in blue) Conditions (Experiment 4). The error bars indicate 95% confidence intervals.

3.6 Conclusion

We conducted four different experiments in terms of decision making scenarios, experimental methods, and emotion elicitations. However, our hypothesis that emotions have an impact on the effectiveness of nudges was not supported in any of the experiments. In fact, we consistently found no impact of emotional states on decision making either, and we did not even replicate the expected effects of the nudges in some of the experiments, which raises doubts about the general effectiveness of some of the most prominent nudging tools.

While all the four experiments consistently showed that emotions did not have an impact on decision making and on the effectiveness of nudges, the main effect of nudges varied across our experiments. Experiment 1 employed an experience-sampling method in which we sent invitation messages to the participants to do tasks using social and default nudges. The default nudge worked as intended while the social nudge did not. Experiments 2, 3 and 4 used a simpler design and were more similar to previous experiments, so that we expected to replicate at least the effects of the nudges. However, Experiment 2 showed no influence of the default nudge on charitable giving, compared to the active choice condition in which the participants were forced to choose an option. In Experiment 3, we found the expected effect of the default nudge on a volunteering decision to participate in an additional questionnaire, using the default to accept and the default to reject the offer. Experiment 4 showed that product choice was affected by peer information but the effect was in the opposite direction of what we expected.

It is surprising that the types of nudges we used did not work as intended although these nudges are known to be among the strongest ones. This suggests that the effects of nudges are far from universal and may depend a lot on the environment. It also suggests that more, and more systematic, research should be conducted to determine the circumstances under which different nudging tools are expected to work. Also it should be noted, practitioners should cautiously apply nudge theory into practice because nudges can backfire as we showed in Experiment 4.

Table 3.4: Logistic Regression Analysis of Pen Choices (Experiment 4)

	(1)	(2)	(3)
	Cross pen	Cross pen	Cross pen
pop_pierre	1.791***		1.352
	(0.344)		(0.370)
no_nudge	1.144		1.133
	(0.363)		(0.497)
Happy		0.993	0.755
		(0.179)	(0.198)
Happy#pop_pierre			1.742
			(0.672)
Happy#no_nudge			1.013
			(0.645)
N	529	529	529
pseudo R^2	0.014	0.000	0.017

Notes: Odds ratios of coefficients from Logit regressions are reported. The dependent variable is a binary variable that takes the value 1 if the Cross pen was chosen. Standard errors in parentheses. *, ** and *** stand for statistical significance at the 10%, 5% and 1% level respectively.

We failed to obtain any significant effects of emotions on the effectiveness of nudges across all the experiments. More evidence is needed to confirm that emotions have no impact, but it seems to suggest that if these effects exist, they are likely to be small. These results are intriguing given that many papers have shown that emotional states significantly affect decisions and play an important role in decision making (see Lerner et al., 2015, for a review).

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