Two Different Experimental Approches For Testing Temptation And A Test Of Stability Of Individual Risk Preferences

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Contents

Abstract

English version

This thesis presents three experiments conducted online between 2018 and 2021. The first two experiments aim to test the temptation model proposed by Gul and Pesendorfer [2001] (hereafter G-P). The first experiment studies the impact of hard and soft commitment on risk preferences. It confirms previous results in the literature whereby about 30% of the subjects display a demand for a commitment and allows us to sehd light on the way subjects use their self-control when the limit they requested is not applied. This allows us to show that asking for a constraint has the effect of reducing the level of risk taken by the subjects and that this reduction of risk is larger when the commitment is hard instead of soft. The second experiment tests the descriptive performance of the G-P model and compares it to the expected utility model and to two statistical models. This experiment shows that the G-P model is able to describe subjects' choices for lottery menus more accurately than the expected utility model in terms of precision, tendency and preferences. However, we show that neither model is significantly more accurate than an individual constant response model that does not take into account the composition of the menus. The third experiment tests the stability of individuals' risk preferences. It shows that subjects' preferences are extremely variable both between and within subjects. This experiment shows that the choices of the subjects are not only not normally distributed but that they are not even distributed in such a way that their mean is normally distributed. This experiment also shows that the distribution of individual choices has the consequence of increase the type-I risks for statistical tests on both witin- and between-subjects experimental designs. It also results in poor predictive and descriptive performance of the expected utility model on individual choices. The predictions made by a model based on a CRRA-type utility function are less efficient than those made by a dummy model that simply predicts the average value over the possible interval. Although we do not propose an explanation for these results, we show that simple linear regression models that take into account a subject's previous choices perform much better than models based on a risk aversion parameter. This thesis also shows that it can be relevant to compare economic models with statistical reference models

in order to test their descriptive or predictive capacity. This approach allows to apply the methods of comparison of models developed in machine learning during the last years and will allow to compare the contributions of economic models and machine learning models.

Version Française

Cette thèse présente trois expériences menées en ligne entre 2018 et 2021. Les deux premières expériences visent à tester le modèle de tentation proposé par Gul and Pesendorfer [2001] (ci-après G-P). La première expérience étudie l'impact d'un engagement contraignant ou non sur les préférences en matière de risque. Elle confirme les résultats précédents de la littérature selon lesquels selon lesquels environ 30% des sujets manifestent une demande pour un engagement contraignant et nous permet d'éclairer la façon dont les sujets utilisent leur self-control lorsque la limite qu'ils ont demandée n'est pas appliquée. Ceci nous permet de montrer que l'engagement réduit le niveau de risque pris par les sujets qu'il soit contraignant ou non. De plus cette réduction du risque est plus importante lorsque l'engagement est contraignant. La deuxième expérience teste la performance descriptive du modèle G-P et le compare au modèle de l'utilité espérée et à deux modèles statistiques. Cette expérience montre que le modèle G-P décrit les choix des sujets pour des menus de loterie plus précisément que le modèle d'utilité espérée en termes de précision, de tendance et de préférences. Cependant, nous montrons qu'aucun des deux modèles n'est significativement plus précis qu'un modèle de réponse constante individuelle qui ne prend pas en compte la composition des menus. La troisième expérience teste la stabilité des préférences individuelle en matière de risque. Elle montre que les préférences des sujets sont extrêmement variables à la fois entre les sujets et pour un même sujet. Cette expérience montre que les choix des sujets ne sont pas normalement distribués et que leur moyenne ne l'est pas non plus. Cette expérience montre également que la distribution des choix individuels a pour conséquence d'augmenter les risques de type I pour les tests statistiques quand le protocole et en within ou en between. Il en résulte également une faible performance prédictive et descriptive du modèle d'utilité espérée sur les choix individuels. Les prédictions faites par un modèle basé sur une fonction d'utilité de type CRRA sont moins efficaces que celles faites par un modèle simple qui prédit la valeur moyenne de l'intervalle possible. Bien que nous ne proposions pas d'explication à ces résultats, nous montrons qu'un modèles de régression linéaire qui intégre les choix précédents d'un sujet sont plus performants que les modèles basés sur un paramètre d'aversion au risque. Cette thèse montre aussi qu'il peut être pertinent de comparer des modèles économiques a des modèles statistiques de référence afin de tester leur capacité descriptive ou prédictive. Cette approche permet aussi d'appliquer les méthodes de comparaison de modèles développées en apprentissage automatique au cours des dernières années et permet de comparer les avantages des modèles économiques et d'apprentissage automatique.

Introduction

In this thesis, we try to provide elements to test temptation models in the laboratory. By temptation model we mean here the model proposed in Gul and Pesendorfer [2001] and the models inspired by it, like Noor and Takeoka [2010] or Noor and Takeoka [2015].

Specifically, we are interested in models with the following two characteristics: They model preferences for menus, that is, preferences for sets of choices in which only one alternative can be chosen.[1] This approach was initiated by Kreps [1979]. And this models must allow preference for the restrictions, that is to say that an individual can prefer a menu to a menu which is his super set (i.e to prefer a menu containing less elements).

We are interested in this model because it allows us to rationalize behavior that cannot be rationalized within the expected utility theory. For example, the demand for a costly commitment [Bryan et al., 2010, Ashraf et al., 2006], the exercise of self-control [DellaVigna and Malmendier, 2004] or the choice of subscriptions inconsistent with the use of a service [DellaVigna and Malmendier, 2006]. We chose to focus on the temptation model from an experimental point of view because the theoretical literature already proposes a large number of models (the reader who wants to be convinced can consult the literature review on the subject Lipman et al. [2013]), but the experimental literature on the subject does not propose a design allowing to compare these models; Both in terms of their descriptive and predictive performance and in terms of their accuracy in describing behavioral frequencies such as the exercise of self-control or the willingness to pay for a commitment. The experimental literature on temptation is focused on the demonstration of existence of behavior predicted by theoretical models of temptation and in contradiction with the expected utility model. To mention only a few of these works, the exercise of self-control and the costs it induces first by Mischel et al. [1989] and then in Kuhn et al. [2014], the preference for a restricted set of choices in Toussaert [2018] and the demand for an expensive commitment in Houser et al. [2018]. This leaves room for an

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[1] More technicaly menus are subset of the set Δ endow with the weak topology. Where Δ is the set of all measures on the Borel σ-algebra of Z a compact metric space (Z, d) of all prizes.

experimental design that would allow us to measure the effects of temptation on preferences and thus differentiate the different temptation models.

This thesis is composed of three chapters, each describing an online experiment. The first two experiments aim at testing temptation models. The first experiment focuses on a behavioral aspect through the effect of commitment on risk taking. The second one tests the descriptive capacity of temptation models by eliciting preferences for menus. The last one does not test temptation but the stability of preferences for money. The latter highlights that the instability of individual preferences has important consequences on the tools used in experimental economics to test a theory and on the reliability of the preferences elicited.

The original title of this thesis was "Temptation and strategic interaction: theory and experiment". The objective was to propose an extension to the existing temptation models, notably those of Gul and Pesendorfer [2001] and Fudenberg and Levine [2006], to take into account the combined effects of temptation and strategic interactions. The original PhD thesis project was based on work carried out for my master thesis. My master thesis consisted in a model that extended the Fudenberg and Levine [2006] individual-choice model to two individuals, each composed of two selves and having to decide on the distribution of a budget when their choices are strategic substitutes. The model developed for the master thesis highlighted that when actions are strategic substitutes, the individual with the lowest self-control costs must compensate for the lack of self-control of the other individual.

My first original work within the scope of this original plan was to propose an experiment to test the behavioral predictions of the model developed in my master's thesis. For that I realized a small experiment (40 subjects) built on the experimental design used by Houser et al. [2018]. In this first experiment we tested the self-control capacity of the subjects by asking them to perform a boring, paid task (looking at a screen displaying the current time) and giving them the possibility to switch to a more interesting, but unpaid task (surfing the internet). The subjects were also given the opportunity to costly commit: they could give up a small part of their earnings to continue the boring task without being offered the choice of switching task. My experimental design consisted of two parts. The first part was a replication of the Houser et al. [2018] experiment and aimed at estimating the individual self-control capacity of the subjects. The second part aimed at testing their self-control capacity in a context with strategic interactions. For this purpose, the remuneration of the daunting task was modified to place pairs of subjects in a situation where their choices are strategic substitute. The – ex-ante unexpected – results of this experiment are that all of our subjects chose to keep performing the boring task for the whole 2 hours that experiment lasted. This did not replicate the results of Houser et al. [2018] who found that only 28.7% of the subjects keep performing the tasks without paying to avoid temptations and, did not allow us to test the influence of strategic interactions. The differences in results with

Houser et al. [2018] can be explained by the differences in dates between the 2 experiments, the original experiment having taken place in the early 2010s and mine in late 2018. The relationship to the internet has strongly evolved between these two dates (think of the actual use of smartphones for example) and makes an activity like surfing the internet in a lab much less attractive. This first experiment taught me two lessons that were then applied in the remainder of the thesis. First, the choice of the tempting alternative is difficult because its tempting nature can vary between individuals but also over time. making direct replications of past successful design far from trivial. Second, to test the impact of strategic interactions it would be easier to have an experimental protocol that does not rely on binary choices but allows for greater variability in individual behavior.

I therefore started to work on an experimental design, which would give rise to chapter 2,

that would allow the measure the effect of temptation at the individual level and which does not depend on the a priori level of temptation generated by an alternative but which allows us to identify which are the alternatives that the subject considers tempting. In order to do this, I chose to construct the experimental design in such a way as to be as close as possible to the theoretical framework proposed by Gul and Pesendorfer [2001]. This allowed me to directly elicit individual preferences for menus, and to build the menus in such a way as that they induce different level of the temptation according to different theories.

In parallel to the preparation of this protocol, Rustam Romaniuc (Montpellier Business School) and Dimitri Dubois (INRAE Montpellier) offered me through Paolo Crosetto the opportunity to propose a treatment concerning temptation in their experimental project on risk. Their project planned to measure risk preferences repeatedly before and after an intervention. I proposed to test the impact of a freely chosen limit on the maximum level of risk a player could take. The intervention consisted in proposing to the subjects to limit the level of risk they could take in the second half of the experiment and to let them choose this level. But with the particularity that this limit would only be applied to a quarter of the people who would ask for it. This allows us to observe the limit that subjects desire as well as their behavior when this limit is not applied. This allows us to relate the level of the requested limit to the capacity for self-limitation, from our point of view the link between demand for commitment and capacity for self-control. This treatment also allows us to compare the impact of the limit when it is binding or not.

While analyzing the data from this experiment I noticed an unexpected difficulty. I had five observations of risk preferences for each of my subjects before and five after the treatment, but these five observations seemed to be extremely variable. This variability between the individual observations was also found in the observations of the subjects of our control group, who was not exposed to any treatment. However, if there are many experiments which inform us about the average level of preference for the subject (for example the Lejuez et al. [2002]

or Crosetto and Filippin [2013] to quote only the ones which inspired chapter 3), there is to our knowledge no article concerning the whitin-subject individual variability of the measurement of preferences for risk. There exists works, like Ert and Haruvy [2017], that take multiple measures of risk but don't compute variability; and works like Wilcox [2007] that compute estimation error across repetitions; but no one, to the best of my knowledge, has directly computed individual variability. Individual variability is necessary to judge the quality of the estimates, as well as to use simulations to calibrate the experiments and to judge the reliability of the experimental results. I have therefore realized an experiment to test the variability of risk preferences which is described in the chapter 3 of this thesis. This experiment shows that the variability of individual preferences is large and must be taken into account in the analysis of experimental results in the domain of risk attitudes, on the one hand by having as often as possible a difference-in-difference analysis as other analyses cause an underestimation of the Type-I risk , and on the other hand by recurring as little as possible to individual level estimations. In order to take into account the results of chapter 3 I opted for a difference-in-difference analysis for chapter 1 using a Bayesian rather than a frequentist approach for the estimates to account for individual variability. For chapter 2 I chose to modify the analysis to test the validity of the temptation models against statistical models in order to have a reference point.

As during these three years of thesis my work deviated from the initial subject, I chose to rename my thesis *Two Different Experimental Approches For Testing Temptation And A Test Of Stability Of Individual Risk Preferences*. Indeed, the central point of this thesis is to explore experimental methods to test temptation. The last two chapters are attempts to answer technical problems on the subject. Therefore the approach chosen throughout the thesis is somewhat different from the usual approach in experimental economics. I have chosen to use methods inspired by statistics and machine learning; notably the use as much as possible of training and test samples to control for overlearning of the models, the use of bootstrap sampling to compare results over a large number of samples and smooth the results, and comparisons of the results of our models with reference models in terms of metrics such as Mean Square Error (MSE). The use of these methods aimed at focusing our analysis on errors due to our observation methods, as well as those of the subjects and model errors that are rarely taken into account in economics as highlighted in Taleb [2005]. This error-centered approach has two objectives, on the one hand to test the reliability of the models we are studying and the contribution of the experimental protocols proposed through this thesis. On the other hand, it allows us to approach the models we study with a refutation objective inspired by Popper [2005] and Popper [2014]. We compare here the models with dummy models and we show that these dummy models are better descriptors of individual behaviors. Of course this method is far from allowing a formal refutation of the models, but it is the closest method to refutation that I could propose. The disadvantage of this approach is that it implies focusing on models chosen specifically in advance

and constructing the experiment in such a way as to have a training set as well as a test set for the model. The approach chosen as well as the initial objective of answering a technical problem such as measuring temptation places me in a situation where the links with the existing literature are limited. This is particularly true with regard to chapter three, which deals with a question that to best of my knowledge has not been directly studied.

Even if this thesis raises more problems than it answers, I think it brings the following elements:

1. The first chapter provides new information on the differences between hard and soft commitment. It allows to put forward a form of complementarity between these two forms of commitment. This chapter also highlights the way in which subjects choose to use their self-control by allowing themselves to exceed their limit at times but by reducing their risk-taking below this limit at other times.
2. The second chapter shows that taking into account the effect of temptation marginally improves the standard model but that these models are not better descriptors of subjects' choices on menus than a constant choice model.
3. The third chapter highlights that individual choices in the risk domain are very variable to such an extent that it does not seem to be pssible to correctly describe them using a distribution with a central moment. This puts into question both the expected utility model and most random models such as Gul and Pesendorfer [2006], Ratcliff and McKoon [2008] or Cerreia-Vioglio et al. [2019]. This chapter also highlights a ghost treatment effect, finding significant differences between similar groups.

Finally, while this thesis does not propose an experimental protocol for measuring temptation, it does highlight problems in temptation models as well as in the expected utility model that invite us to further explore the variability of individual preferences.

Chapter 1

Intra-personal conflict and self-commitment

1.1 Introduction

Self control is an important non-cognitive skill that is associated with favorable economic and social outcomes [Laibson et al., 1998, Heckman et al., 2006, Alan and Ertac, 2015]. However, abundant evidence suggests that people have a hard time controlling their instantaneous passions and often succumb to temptation [Milkman et al., 2021]. Consider the two-pack a day cigarette smoker who went through many attempts to quit but was never successful in kicking the habit. In New Year's resolutions, one intends to eat more healthy foods in the future, exercise more regularly, and watch television less often, but many of these intentions fail because of self-control problems. One popular solution to self-control problems is to use a commitment device. For example, in "How to get ready for retirement: Save, save, save", Rankin [1993] suggests to "Use whatever means possible to remove a set amount of money from your bank account each month before you have a chance to spend it."[1]

There is ample empirical evidence showing that hard commitments work well to reduce one's tendency to err – as judged by the person's own standards – in the direction of instantaneous gratification. For instance, it has been shown that some people use specific ordering strategies that enforce watching "high brow" movies [Read et al., 1999] or that some individuals accept to put their money in temporarily locked savings accounts in order to keep it away from themselves (for more examples, see Milkman et al. [2021]). Hard commitments allow individuals to "bind" themselves as Ulysses did before setting out to the Syrens [Elster, 2000]. However, people often avoid using hard commitments

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[1]See Deborah M. Rankin, "How to get ready for retirement: Save, save, save", New York Times, March 13, 1993, p. 33.

because of their lack of flexibility. This is why in the Ashraf et al. [2006] study that offered customers to commit to restrict access to their savings, only 28% accepted the offer and opened a locked bank account.

An alternative to hard commitments that can work to avoid succumbing to temptation while providing more flexibility are soft commitment devices, i.e., commitments that can be easily broken (See Bryan et al. [2010], for an extensive review of the literature on commitment devices). Deviating from soft commitments involves psychological costs such as shame (if the commitment was made public) or guilt (if it was made privately) or some degree of both shame and guilt. Examples of soft commitments backed by non-pecuniary costs include taking a fixed amount of money when going out with friends (one can always borrow on the spot, so the commitment is soft), brushing one's teeth earlier in the evening to avoid late night snacking (the cost of redoing it is low), renting a place in an open space to avoid taking a nap when working from home (couches may also be available in open spaces). Given the ubiquity of soft commitment devices, it is important to understand to what extent making a commitment soft as compared to hard/binding changes people's behavior. However, to our knowledge, there is no empirical evidence on the effects of soft commitments relative to a condition where the commitment is hard as well as compared to an environment without any form of commitment device. Our aim in this paper is to fill this gap.

To study the comparative effects of soft relative to hard and no commitments, we designed a controlled experiment that we implemented online. Our sample of 1527 participants who, in the last 12 months prior to their participation in our experiment, engaged in some sort of legal gambling with *la Française des Jeux*, the operator of France's national lottery games, with whom we partnered for this study. Specifically, our sample is representative of *la Française des Jeux*'s gamblers. In that sense, this is the first study on self-control that uses such a large sample of participants and that are representative of the population of gamblers of a major national operator of lottery games.[2]

The experiment consists of two conditions, a *Baseline* that implements a modified balloon analogue risk task (BART, Lejuez et al. [2002]) and a *Commitment* condition. The BART is a risk-elicitation game in which subjects pump air in a fictional balloon, and collect money proportional to the number of pumps, unless the balloon bursts, in which case they get no reward. In the *Commitment* condition, subjects were given the opportunity to select an upper limit on the number of pumps for future rounds of the BART. Furthermore, in the *Commitment* condition, subjects are informed that the limit would be binding with a 25% chance. After subjects made their choice regarding the self-imposed limit, they are informed whether the limit is binding or not. The fact that the limit is

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[2]While there are field studies on self control that employ a non-student population [Ashraf et al., 2006, Milkman et al., 2014] , we are not aware of any online study on the topic of temptation and self-control using a sample of participants with similar characteristics to our participants.

binding with a 25% chance allows us to capture the demand for a commitment device as well as to compare decisions under two different environments: (1) when commitment is hard given that the limit is binding and (2) when the commitment is soft given that the subject is free to choose any number of pumps but knowing that she had committed to limit her behavior to a certain extent. Given that the limit has a positive probability of being applied, revealing one's true preference is a dominant strategy.

We found that 35% of our subjects ask for a limit to the maximum risk they can take when offered one. Asking for this limit has the effect of decreasing the level of risk taken by a subject, even if this limit is not applied. The decrease in risk-taking is greater for subjects for whom the limit is applied, but by studying saturation of the limit as well as the impact of ex-post application of the limit we show that the effect of accepting the limit on risk-taking is complementary to the mechanical effect of the limit and not a substitute. We can thus see the effects of a hard or soft commitment on risk-taking behavior.

Our paper relates to the literature that sought to test whether and how commitment devices can make people succumb less to temptation. The literature on hard commitment devices has hitherto been considered quite independently from research that investigates the effects of soft commitments. For instance, Trope and Fishbach [2000], Ariely and Wertenbroch [2002], and Houser et al. [2018] compare hard commitments to a control without any commitment. Despite the importance of such comparisons, a better understanding of hard commitments requires a comparison between hard and soft devices. Indeed, hard commitments impose both a pecuniary and a non-pecuniary cost in case of breach of commitment. The pecuniary cost can go to infinity if the individual decides to remove altogether the tempting option from the choice set. At the same time, hard commitments come also with non-pecuniary costs in case of a breach such as shame or guilt [Kast and Pomeranz, 2014]. The decision to deviate from one's commitment may also signal to the individual a lack of willpower, which may represent another source of psychological discomfort [Bénabou and Tirole, 2004]. On the other hand, soft commitments are backed solely by non-pecuniary costs. When present, pecuniary costs are mostly symbolic. Our experimental study allows us to compare the impact of hard commitments relative to soft and no commitments, thus disentangling the effect of pecuniary from non-pecuniary costs.

Our work is also related to the theoretical investigation of commitment by Gul and Pesendorfer [2001]. In their model, there is a cost of avoiding the most tempting item in a choice set. Individuals, therefore, benefit from removing these items. Our experiment allows subjects to eliminate tempting options from their choice set by choosing an upper limit on the number of pumps that they will be able to select. However, whereas Gul and Pesendorfer [2001] are interested in modelling the demand for such commitments, we empirically analyze their behavioral effects after some participants to our study decide to eliminate tempting options from their choice set.

1.2 Experimental Design and Procedure

1.2.1 Experimental conditions

Our experiment consists of two experimental conditions: a *Baseline* that implements a modified balloon analogue risk task (BART, Lejuez et al. [2002]) and a *Commitment* condition where subjects were given the opportunity to select an upper limit on the number of pumps.

In each condition, subjects played 10 rounds of a modified version of the BART. The screen showed a small simulated balloon. Each subject had to choose a number of pumps between 1 and 64, knowing that each pump would inflate the balloon and would yield a gain of €0.15. However, each pump could result in the explosion of the balloon. The probability that a balloon would explode was arranged by constructing an array of numbers containing the integers 1–64, as in Lejuez et al. [2002]. The number 1 was designated as indicating a balloon explosion. On each pump of the balloon, a number was selected without replacement from the array. The balloon exploded if the number 1 was selected. Thus, the probability that the balloon would explode if the subject chose one pump out of the 64 possible was 1/64. If the subject chose 20 pumps out of the 64 possible, then the probability that the balloon would explode was 20/64. As in Lejuez et al. [2002], choosing a higher number of pumps (i) increased the amount to be lost because of an explosion and (ii) decreased the relative gain of any additional pump. In our experiment, the average break point was 32 pumps. That is, a risk-neutral subject would maximize her gains by choosing 32 pumps.

Note that in the original study by Lejuez et al. [2002] subjects had to click on a pump button to inflate the balloon and they had to click on it as many times as they wanted knowing that the balloon could explode at any moment. We modified the original BART study along two dimensions. First, because we are interested in behaviors under risk rather than ambiguity, we decided to inform subjects about the range of outcomes and that a priori each pump is equally likely to result in an explosion. Second, we asked subjects to choose the desired number of pumps before the balloon started to inflate. That is, a subject had to indicate a specific number of pumps and only then the balloon started to inflate until it reached the chosen number of pumps or exploded – whichever happened first. This way, we did not constrain the number of chosen pumps on balloons that exploded, which allowed us to capture the subjects' risk preferences in an unrestricted manner and avoid the truncation of the data that is usual for BART studies.

In all conditions, at the end of each round, subjects were informed about the outcome of the balloon task (whether it exploded or not before it reached the indicated number of pumps) and about their earnings in that particular round. At the end of the experiment, one round out of the ten was randomly chosen for payment and this was common knowledge from the outset of the experiment.

The "pay one" approach can help to avoid wealth effects and hedging [Charness et al., 2016].

In the *Baseline* condition, subjects played 5 rounds of the BART followed by a 10 seconds pause where they saw a message informing them that the game would resume after a few seconds. After the pause, they had to play for 5 more rounds that were identical to the first 5 rounds. Subjects were informed at the beginning of the experiment that there was a total of 10 rounds. The 10 seconds pause was implemented to mimic the break that we implemented in the *Commitment* treatment, with the exception of the commitment mechanism introduced in the latter but absent in the *Baseline*.

In the *Commitment* condition, the first 5 rounds were identical to the *Baseline*. However, at the end of round 5, instead of the pause, subjects were offered the possibility to select an upper limit on the number of pumps that they could choose in all of the rounds that would follow – i.e., from round 6 to round 10. All subjects were informed that the limit would be binding with a 25% chance. For those who opted for no limit, they went on to round 6 of the BART as in the *Baseline*. For those who opted for a limit, after subjects made their choice, they were informed whether the chosen limit was binding or not. Then, subjects proceeded to round 6 of the BART. In case the limit was binding, subjects could choose a number of pumps between 1 and the chosen limit. If the limit was not binding, subjects could choose any number of pumps between 1 and 64, as in the *Baseline* condition.

Two design choices are worth discussing. First, the choice of having the first 5 rounds identical across the two conditions. This sequence was implemented for two reasons: (i) to allow subjects to get accustomed with the game and (ii) to capture subjects' "natural" risk preferences in the absence of any commitment device. This way, we can ensure that our subjects have overall similar risk profiles across the two conditions by looking at behaviors in rounds 1-5. Additionally, we make sure that those who are offered the possibility to choose a limit after round 5 have been exposed to the game and that they had gotten a feeling of their temptation level.

The second design choice that requires a detailed discussion concerns the stochastic aspect of the limit. The fact that the limit was binding with a 25% chance allows us to capture the demand for a commitment device as well as subjects' self-control when the limit is effectively implemented (thus, making it a hard commitment) compared to when it is non binding (making it a soft commitment). Since the limit has a positive probability of being applied, revealing one's true preference is a dominant strategy. Since it is applied to a minority of subjects only, we can study self-control under a soft and a hard commitment device.

1.2.2   Participants

The recruitment process started in October and ended in December 2019. 803 subjects participated in the *Baseline* and 724 in the *Temptation* condition. Subjects were recruited by a private company, named *Bilendi*, within the framework of a partnership that some of the authors of this study concluded with *la Française des Jeux* (FDJ), which is the operator of France's national lottery games.[3]

Bilendi recruited subjects for this study from a pool of more than 1 million individuals who had a personal account with FDJ. The study includes individuals who had declared that they played at least once one of FDJ's games during the 12 months prior to the study (in one of FDJ's physical sale points or online). Therefore, all our subjects have some appetite for gambling, which makes this study original compared to using a population that may be less prone to temptation than the general population.[4]

The other novelty of our study is that our subjects are more representative of the general population of the country where the study was conducted than the standard subjects included in many experiments that have dealt so far with the topic of temptation and commitment (for example, Ariely and Wertenbroch [2002], Casari [2009], and Houser et al. [2018], rely on a population of students).

The sample in our experiment was 40.52% female and 59.48% male, and relatively more evenly distributed than traditional student samples: 7.50% of participants were between 18 and 24 years old, 20.67% between 25 and 34 years old, 37.43% between 35 and 49 years old, 24.89% between 50 and 64 years old and 9.51% were 65 years old or older. Considering the socio-professional categories, the sample was composed of 46.25% of CSP- (employee and worker), 26.78% of CSP+ (farmer, craftsman, merchant, company manager, liberal professions and intermediate professions), 14.05% of retired or pre-retired, 9.45% of inactive (not working or looking for a job) and 3.47% of students.

1.2.3   Control questions

All subjects were recruited by the data collection company *Bilendi*. Before reading the instructions of the BART, each subject was asked to answer a series of questions about their gambling habits during the last twelve months. The questionnaire was used to collect information for a different project than the present one.

Additionally, subjects had to answer a series of questions meant to collect socio-demographic information and to check whether they had played one of FDJ's

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[3]For more information about the two companies, see their respective websites: FDJ and Bilendi

[4]For example, Milkman et al. (2013) study temptation and commitment in a sample of gym users. It is quite possible that people who have a gym subscription differ in terms of willpower from people who have no gym subscription.

games in the last 12 months (lottery, online poker, and sport betting). Specifically, subjects had to report their gender, age, professional activity, and whether they had bought a lotto ticket, played poker online or made any sport bets within the last twelve months.

At the very end of the BART, subjects were asked to answer 10 questions that correspond to the self-efficacy questionnaire. This information was collected for a different project than the present one. The questionnaire was implemented at the end of the experiment and therefore did not influence subjects' risk-taking and commitment decisions.

1.2.4 Procedure

The online application was developed using the oTree platform Chen et al. [2016]. We created a virtual "Room" to generate the URLs and to ensure that each subject would participate only once. More precisely, the Laboratory for Experimental Economics of Montpellier (LEEM), created a dedicated "Room" on its oTree server with identifiers composed of letters and numbers, ranging from A1A1 to Z9Z9. Consequently, the individual URLs generated by the platform were of the form https://expe.leem.umontpellier/room/leem/?participant_label=A1A1. Bilendi, which was in charge of sending the invitations by email, assigned one identifier to each subject, and therefore sent the corresponding URL in the invitation email. On the LEEM server we only had the identifiers and decisions, while Bilendi had the identifiers and identifying information of the subjects, but did not have access to the decisions of the subjects in the experiment. This procedure made it possible to guarantee the anonymity of the data collected. At the end of the experiment, the LEEM sent a file with the identifiers and associated payments to Bilendi, which then paid the subjects via the PayPal platform according to their earnings in the experiment.

The experiment lasted on average 20 minutes and was divided into two parts : first, the modified BART, with 10 rounds, and then a self-efficacy questionnaire. Subjects were not informed on the paid round of the BART game until they completed the self-efficacy questionnaire. The average payoff was €1.60 (std 1.84). In addition to their earnings related to their decisions, each subject was paid a €5 participation fee.

1.3 Results

We proceed to the analysis using 2 sets of data. The first one corresponds to the baseline treatment and contains the observations for the 863 subjects who were not subjected to any treatment. As these subjects simply repeated the elicitation task 10 times, this data serves as a counterfactual. The second data set concerns the 724 subjects who were subjected to the temptation treatment. It is on this data set that our analysis focuses.

For these two data sets we have 10 observations per subject. But we have chosen

Table 1.1: mean numbers of pumps

	before treatment	after treatment	total
baseline low type	21.00	20.65	20.81
baseline middle type	21.51	21.06	21.26
baseline hight type	21.62	22.90	22.33
temptation low type	21.21	21.08	21.14
temptation middle type	18.78	19.99	19.46
temptation hight type	23.90	21.28	22.44

not to consider the observations for the first round. These observations are different from those of the other rounds both in terms of distribution (evaluated with a Kolmogorov-Smirnov test at a 95% alpha threshold) and mean (test of equality of means at a 95% alpha threshold) for both the baseline and temptation treatment data. It is assumed that this is different data because it captures a learning effect. We therefore apply our analysis to rounds 2 to 10.

For both datasets, we calculated the average before and after treatment for each type of player. The results are reported in the table 1.1.

We can make the following observations:

1. The subjects are globally risk averse with an average number of pumps of 20.93.
2. The high type players are more risk prone than the others but the middle and low type players do not differ.
3. There is no global difference between the before and after treatment. The p-value of the t-test is 0.94 between the pre- and post-treatment choices and the p-value for the post-treatment choices between the baseline group and the temptation group is 0.4.

Since we cannot reject the hypothesis that the temptation treatment as an effect on the amount of risk chosen by subjects, we will now look at the choice of constraint for subjects in the temptation treatment.

To begin with, it is important to remember that the constraint that is proposed to the subjects in this treatment cannot theoretically serve them to improve their situation. Indeed, if the constraint is applied, it only makes the choice of a higher risk amount impossible. But even without the constraint, the subjects have no incentive to choose a higher amount of risk than they want. Theoretically, we expect subjects not to ask for a constraint.

But we observe that 35% of the subjects have asked for a limit. This quantity is consistent with that observed by Houser et al. [2018] (28.6% when the limit has no cost) and Toussaert [2018] (35.8%). This subject confirms that there is a demand for a constraint in our subjects. Moreover the limit chosen by the subjects is binding for at least a part of them. In the table 1.2 we report the

Table 1.2: share of subject who ask for a limit

	limit not apply	limit apply
limit exceed before treatment	0.41	0.42
limit reach before treatment	0.25	0.25
limit exceed after treatment	0.30	0.00
limit reach after treatment	0.31	0.42

number of subjects who requested a limit and exceeded or reached it before and after it was proposed to them.

We see that for 41% of the subjects the limit that is requested is restrictive because it is lower than the maximum that was chosen in the first period. The share of the subjects who reached and exceeded this limit is the same whether the limit is applied or not before treatment. On the other hand, the number of subjects who reached their limit without exceeding it increased after treatment even when the limit was not applied. This seems to indicate an ability of the subjects to self-constrain. However, it is interesting to note that the number of subjects who reached or exceeded their limit remained constant for subjects for whom the limit was not applied but that this number decreased for subjects for whom it was applied. This indicates that the application of the limit does not only have the effect of preventing subjects from taking more than a certain amount of risk.

But our data allow us to go further in the study of the behavior in front of a constraint. We can observe not only if the subjects have reached or exceeded the limit but also to what extent they have moved away from it. To do this we use a saturation indicator:

$$r_{saturation} = \frac{pumps}{limit}$$

And we refer to this inverse as the degree of constraint of the limit. We will also refer to the truncated saturation ratio which corresponds to the same ratio but for which the values for which the number of pumps is higher than the limit are reduced to 1. This saturated version has the advantage of not being influenced by the application of the limit and will thus allow us to compare the behaviors of the subjects for whom the limit has not been applied with those to whom it has been applied. The table 1.3 shows the mean value of the saturation ratio and in parenthesis the truncated ration value before and after treatment for the subjects who requested a limit.

The following can be observed:

1. The subjects do not saturate their constraint on average. Indeed the saturation ratio is less than 1.
2. The subjects exceed their constraint when they can, the saturation ratio is higher than this truncated value. The difference indicates that in terms

Table 1.3: mean saturation ratio (truncated saturation ratio)

	before treatment	after treatment	difference
limit not apply	0.89 (0.66)	0.8 (0.66)	0.09 (0)
limit apply	0.83 (0.68)	0.66 (0.66)	0.17 (0.02)

  of the chosen limit, the overshoot is important.
3. We find the fact that when the limit is applied, the saturation reduction is more important. But that a reduction is also visible in the subjects for whom the constraint was not applied.
4. On the other hand, the truncated saturation ratio does not seem to evolve before and after treatment whether the limit is applied or not.

We can therefore conclude that choosing a constraint encourages subjects to self-constrain by reducing their overshoot of the constraint. But this does not allow us to know if this self-constraint only reduces the overshoot of the constraint or if it also has an impact for the choices below the value chosen as limit.

To complete our analysis we studied the impact of proposing a limit, whether it is accepted and applied, on the level of risk taken by the subjects. To do this we used the Markov-chain Monte Carlo implementation implemented in the PyMC package (Salvatier et al. [2016]). This method allows us to obtain a distribution for the mean rather than a point value. And thus to compare the parameters on their distribution rather than on a hypothetical normal distribution which turns out to be less precise than the estimates that we obtained thanks to PyMC. As MCMC is a Bayesian algorithm, we had to make assumptions on the distribution of the data and on the associated parameters. We chose as a prior a Poisson distribution of the data whose parameter is distributed according to an exponential law with a mean equal to the observed mean of the data. The Poisson distribution is justified in that the observed data are discrete, and for the values of the studied parameters the number of theoretical observations outside the sample is very low. Moreover this hypothesis offers better results than the alternative of using a binomial distribution.

The differences in mean between the before and after treatment periods are presented in the figure 1.1. The differences are compared for 5 different situations. First in the baseline situation where no differences should be observed between the first 4 periods and the next 5. Then in the temptation treatment according to the choice of the subjects when they were proposed a limit and its consequences. Finally, we studied what would have happened if the limit they had requested had been applied to the subject who had requested it but was refused.

We have for the 5 different situations the following estimates in terms of average difference between the first 4 rounds and the next 5:

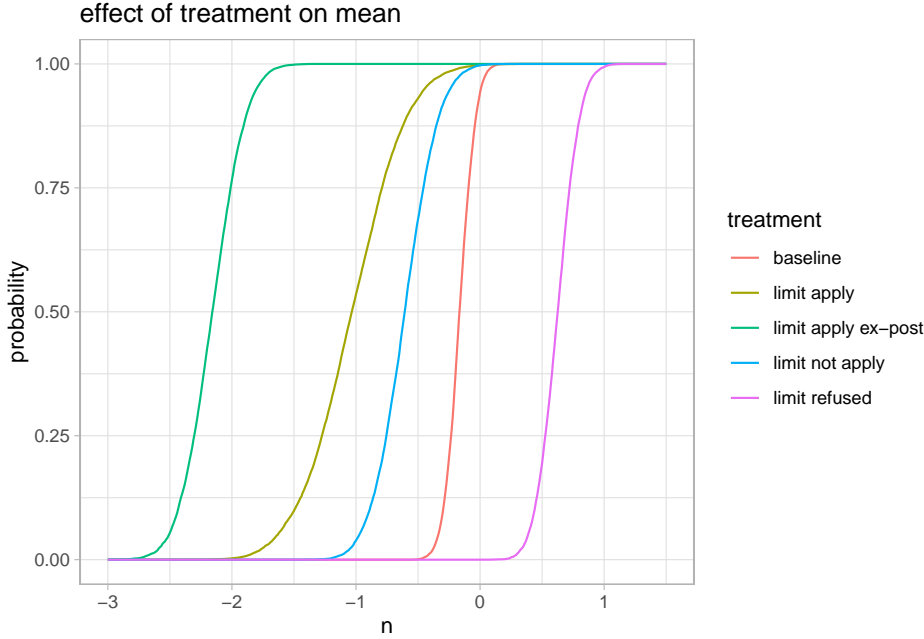1. *limit refused*: mean of 0.63 with credible interval at 95% [0.34, 0.91]. We

effect of treatment on mean

Figure 1.1: effect on mean for the different groups

can see that refusing a limit tend to increase the risk taken by subjects.

2. *baseline*: mean of -0.17 with credible interval at 95% [-0.37, 0.04]. So for baseline their is no significant difference between the four first rounds and the subsequent rounds.

3. *limit not apply*: mean of -0.61 with credible interval at 95% [-1.04, -0.17]. The fact that subjects ask for a limit even if it is not enforced, decreases the risk taken by the subjects. Although this reduction is small, it is significantly different from what is observed for the baseline.

4. *limit apply*: mean of -1.03 with credible interval at 95% [-1.74, -0.32]. When the limit is applied to the subject who requested it, the average risk reduction for subjects is greater than for those to whom the limit was not applied. However, it is estimated that the probability that the application of the limit will result in a greater reduction in the limit is only 84.54%.

5. *limit apply ex-post*: mean of -2.16 with credible interval at 95% [-2.58, -1.73]). The most significant reduction in risk-taking is observed when the limit is applied ex-post to subjects who asked for the limit without receiving it.

This analysis of the effect of proposing a limit allows us to complete the elements mentioned above. First of all, if the temptation treatment does not create a difference in terms of risk behavior compared to the baseline, this is due to the

fact that even if the subjects who asked for a limit reduced their risk taking, the subjects who refused it increased their risk taking. Second, we show that asking for a limit, even if it is not enforced, leads subjects to reduce their risk-taking. This shows that subjects are able to self-constrain. But this self-constraint does not seem to be as effective as a binding limit because the reduction in risk-taking is greater when the limit requested by the subjects is applied. Finally, we see that the reduction in risk-taking is even greater when the limit is applied ex-post. This confirms what we have seen with the saturation of the constraint, that is to say that the effort of self-constraint to reduce the level of risk taken is applied by reducing the risk taken below the limit requested but that the subjects do not seem to succeed in not exceeding their limit by themselves. This observation is consistent with the idea of convex self-control costs but this hypothesis will have to be tested by future work.

1.4 Conclusion

The extant literature has investigated the effects of hard commitment devices, that impose financial and non-financial costs in case of breach of commitment (e.g., Ashraf et al. [2006]) separately from soft mechanisms, such as signing a pledge, that are backed solely by guilt and discomfort (e.g., Bhanot [2017]). As not everyone is comfortable with the idea of a commitment device that imposes significant penalties or restricts future freedoms, those who cannot stomach the thought of hard commitments may do better with a different flavor of commitment device. It is, therefore, important to understand to what extent soft commitments are a good substitute for hard ones. Our experimental study is the first, to the best of our knowledge, that compared the impact of a hard commitment device relative to soft and no commitment, thus disentangling the effect of pecuniary and non-pecuniary costs on one's capacity to not succumb to temptation.

To study the effects of hard versus soft commitments, including a condition with no commitment device, in a comparable and controlled environment, we implemented an online experiment with 1527 participants. Compared to more standard experiments that examined temptation in the lab, our subject pool is more diverse in terms of age and occupation (only 7.5% were between 18 and 24 years old). Furthermore, compared to some field experiments (e.g., Ashraf et al. [2006]), our study retains the advantages of laboratory studies in terms of control over the subject's decision environment as we implemented an online balloon analogue risk task. Finally, in terms of our subject pool, contrary to some field studies (e.g., Milkman et al. [2014]), our participants are particularly tempted by the activity that they can choose to limit as we recruited participants a pool of individuals who have engaged in sort of gambling activity over the last 12 months prior to the study. Thus, our experiment can be viewed as an artefactual field experiment, to use Harrison and List [2004] taxonomy because we use a standard task with an abstract framing, but with a nonstandard pool

of participants.

Our results demonstrate that there is a demand for constraint in about one third of the subjects. The subjects who request this constraint decrease their risk-taking even when this constraint is not applied. We see that the decrease in risk-taking is more important in subjects for whom this constraint is hard. We have shown that the decrease in risk-taking in subjects for whom the constraint is soft is different and complementary to the decrease in risk-taking in subjects for whom the constraint is hard. This complementarity of behaviors between hard and soft constraints invites us to explore theoretical models that postulate that individuals are both requesting constraints and capable of self-constraint. Among these models, temptation models such as those proposed on the Gul and Pesendorfer [2001] model allow to rationalize the behaviors highlighted in this article. More specifically the model of Noor and Takeoka [2015] allows to rationalize the effect of the application of the ex-post limit as well as the non-saturation of the constraint by subject.

Chapter 2

Descriptive Power of Tempting Model

2.1 Introduction

In this chapter we present an experiment which aims to test the descriptive capacity of the model proposed by Gul and Pesendorfer [2001] (hereafter G-P). The predictions of this model are compared with those of different economic models and with simple statistical models. To evaluate the descriptive capacity of the models, our experiment places subjects in a situation as close as possible to the theoretical framework of G-P. Subjects are asked to choose by means of an incentivized elicitation mechanism among menus composed by lotteries. A menu here is a set of 1 to 6 lotteries, from which the subject knows that she will have to choose only one lottery in the end. Our approach differs from that of the previous chapter and from experiments such as those of Houser et al. [2018] or Toussaert [2018] on the subject. Our experiment does not aim at showing the existence of a particular behavior predicted by the theory such as the demand for commitment or the capacity of the subjects to exercise their self-control. It aims instead to evaluate the ability of G-P to describe the actual behavior of the subjects, and to compare its performance to various alternatives, ranging from economic to statistical models.

This experiment does not question the existence of behavior at odds with expected utility theory, such as the demand for constraints (Chow and Acland [2011], Giné et al. [2010] and Uhl et al. [2011]),
self-control effects (Burger et al. [2011], Mischel et al. [1989] and Kuhn et al. [2014]) or more surprising attitude like in DellaVigna and Malmendier [2006]. These behaviors are well established in the experimental literature. Our experiment questions the menu preference approach as a way to rationalize these behaviors. This approach, initiated by Kreps [1979], aims at rationalizing the

preference that individuals may have for larger sets of choices. The idea being that an individual who is uncertain about his future preferences would prefer to have a larger number of options to choose from when making his decision in order to maximize his utility. Later G-P proposed a model in which individuals may be averse to the
presence of certain options and therefore prefer smaller choice sets. G-P's model rationalizes the demand for constraints by subjects while allowing for the possibility of costly self-constraint. The G-P model was later extended to rationalize more behaviors. For example Gul and Pesendorfer [2004] for repeated choices , Noor and Takeoka [2010] for anticipated temptation cost or Noor and Takeoka [2015] for more complex interactions among elements of a menu – for a review of application and extensions of the original G-P model see the review by Lipman et al. [2013].

Our experience shows that in terms of descriptive power the G-P model is a slight improvement over EUT. But the G-P model does not do better than a model where subjects all evaluate their menus in the same way and independently of their composition. Moreover, a simple regression model outperforms the G-P model.

The goal of our design is to be as close as possible to the theoretical framework formulated by G-P. We have therefore focused on the ellicitation of the menu value. Therefore we do not have a mechanism that would allow us to identify the different states of the world that are at the basis of the temptation models proposed by Dekel et al. [2001], Dekel et al. [2007] or Dekel et al. [2009]. Nor can we identify the choices of different selves as in the multiple-self models proposed by Fudenberg and Levine [2006]. Therefore, we will not propose any interpretation of our data in the framework of these models.

Our approach will allow us to propose a method allowing us to elicit the two functions used in the G-P model for each of our subjects. G-P shows that an individual whose preferences are complete, transitive, continuous, independent and satisfying the axiom of *Set Betweenness*:

$$A \succsim B \text{ implies } A \succsim A \cup B \succsim B$$

has a utility function for menus of the form:

$$U(A) = max_{x \in A}(u(x) + v(x)) - max_{y \in A}(v(y))$$

With A and B was menus and $u(.)$ and $v(.)$ was Von-Neumann Morgenstren utility functions. What we will do in our analysis is to estimate for each menu the value associated for the function $u(.)$ and for the function $v(.)$ and to compare the theoretical value of the resulting menu to the one indicated by the subject. We do not test the validity of the axioms but that of their theoretical consequence.

2.2 materials and methods

2.2.1 Experimental design

Our experience took place from 12/06/2020 to 19/08/2020. It took place online and the recruitment of the subjects was done via the Amazon Mechanical Turk (AMT) platform. On the AMT platform a job offer was published indicating the approximate duration and the fixed payment as well as an average bonus higher than 5$. From this offer, subjects could accept to participate in our experiment by accepting the task on AMT. No selection criteria are applied a priori to the subjects. But only the subjects having filled in a valid end-of-experience code were included in the analysis. Conditions of minimum duration of working time and number of trials are also applied but do not exclude any subject [1].

The software itself is developed using the R language and the Shiny framework. Once the experiment is completed, the experimental software displays a summary of the subjects' bonus and a unique code to be filled in the corresponding field on the AMT platform. This code allows us to uniquely identify the subjects' responses between our software and the AMT platform. The target number of subjects was 300. 304 subjects filled in a response code on the AMT page but this code was only valid for 297 of them. The 7 subjects whose code was not valid were excluded from the dataset and were not paid.

The experience proceeds as follows:

1. The subject faces a screen with instructions. This screen describes the next steps in the experiment and details how the subject will earn his payoff. The subject is given a description of the menu and how the lottery works. Particular emphasis is put on the impact on the bonus of the mechanism of choice of a menu. Instructions are provided in Appendix B.
2. The subject has to answer a short series of multiple-choice control questions. To continue, the subject must answer all questions correctly. To do so, he has as many attempts as he wants and a help is displayed for the questions to which the answer is wrong at the first attempt.
3. Start of incentivized learning phase:
4. The subject has to bid on 10 pairs of menus by indicating with a slider the maximum amount he is willing to pay between -1$ and 1$ (the bidding mechanism is described in detail in the following section). The menus on which the subject bids are built in the same way as those of the rest of the experiment but the values used for the lotteries that compose them are different.
5. One of the bids that has just been made is drawn and the subject chooses one of the lotteries that it contains.

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[1]Only subjects with a valid code are kept because this code allows us to pay the subjects and the author refuses to include subjects who were not paid in the study. The exhaustive inclusion criteria are listed in the pre-registration of the study available with asPredicted code qj8mr

6. A screen indicates to the subject what amount he has won with the steps 4 and 5, either the amount resulting from the selected auction and the amount corresponding to the resolution of the chosen lottery.
7. The subject is asked to complete a second quiz similar to the one in step 2 but with different questions.
8. Main taks: the subject bids on 35 comparisons between 2 menus.
9. 5 of these comparisons are drawn at random and the subject can choose 1 lottery from each of the 5 menus assigned to him according to her bid and the elicitation mechanism.
10. The lotteries are solved and the subject's bonus is calculated as the total won on the 5 bids on the selected comparisons and the results of the 5 chosen lotteries plus what was already won in the learning phase.
11. On the final page, a table summarizes the subject's winnings by selected comparisons indicating the amount won via the auction and the lottery results. The total of the 6 auctions and lotteries is also displayed along with a reminder that the subject must fill in the code displayed on this page on AMT.

Our experiment is composed of only one treatment. The earnings of the subjects are paid via the AMT platform. They are composed of a fixed part of 1\$ paid to all the subjects who filled in a valid code in the AMT form, and a variable part calculated according to the bids on the 6 selected comparisons and the resolution of the 6 lotteries they chose (1 in the learning and 5 in the main phase).

Our subjects were thus paid an average of 6.97\$ for an average time of 41 seconds spent on our experiment. So an average wage per hour of 10.3\$

The objective of our experimental protocol is to elicit the value that subjects place on menus. A menu is a set of items from which the subject must choose 1 and only 1 item. To be as close as possible to the theoretical model of G-P we chose to build our menus with lotteries. In order to keep our experiment as simple as possible we chose to use binary lotteries similar to the bets that can be made in roulette games in casinos. That is to say lotteries allowing to win n\$ with a probability of $\frac{1}{n}$ and 0\$ otherwise. The menus can be composed of 1 to 6 lotteries with the possible values of $n \in [2, 4, 6, 8, 16, 32]$. All lotteries have the same expected value of 1, and differ in variance only, that is increasing in n. This keeps the individual elements of the menus as simple and intuitive as possible.

To elicit the value of the menus we have chosen to use a variant of the BDM auction method proposed by Becker et al. [1964]. This auction method consists in asking the subjects the maximum amount they would be willing to pay to acquire a good, then to randomly choose a number; if the number drawn is lower than the value announced by the subject then the subject must buy the good for the amount drawn. This method encourages the subject to reveal the true maximum amount he is willing to pay for a good. Indeed, a subject who does not indicate his true value runs the risk of paying more for a good than he is

willing to pay or risks not acquiring the good for an amount lower than what he would have been willing to pay. The method we use differs from the one presented in Becker et al. [1964] because we want to compare menus between them. So we ask subject for the maximum amount he is willing to pay to *exchange* one menu for another (we will call this amount willingness-to-change, WTC). So to elicit the WTC between 2 menus we present to subjects the one menu on the left of the screen and the other on the right, the subject has to indicate an amount between -\$1 and \$1 corresponding to the maximum amount he is ready to pay to exchange the menu on the left against the one on the right. If the randomly chosen number is less than the value indicated by the subject, the subject receives the right menu and must pay an amount equal to the number drawn (paying a negative amount means receiving money). If the number drawn at random is strictly higher than the amount indicated by the subject, the subject receives the left menu.

In our experiment, subjects indicate the maximum amount they are willing to pay using a slider located between the 2 menus. In order not to induce an anchoring effect, the initial position of the slider is determined randomly for each comparison. This allows us to see that in 85.39% of the cases the subjects have effectively indicated their preference by moving the slider from the place where it was initially placed.

As with 6 different items it is possible to build 63 different menus and therefore 1953 different 2-by-2 comparisons, we restricted the comparisons made by the subjects to the one that seemed the most interesting for us[2]. The 35 comparisons that we have retained for each subject are the following:

These comparisons were chosen to allow reliable estimates of preferences with many observations of comparison of size 1 menus with each other and with size 2 menus; but also to cover a wide range of items and menu sizes.

1. 10 comparisons between menus of size 1. The menus of size 1 are chosen randomly but in such a way that each menu appears at least once and that it is possible by 2-by-2 comparisons to reconstruct 1 chain of all menus of size 1. This comparisons is used to estimate singletons preferences.
2. 12 comparisons between menus of size 1 and size 2. The menus to be compared are drawn at random but we make sure that the menus of size 2 containing the elements 2 and 32 (the extreme lotteries) are compared at least once with the menus of {2} and {32} in order to facilitate the imputation by a utility function of the elements which were not directly compared. This comparisons is used to estimate temptation preferences
3. 3 menus of size 2 between them drawn at random.
4. 3 menus of size 3 with menus of size 2 drawn at random.
5. 1 menu of size 5 drawn at random with 1 menu of size 4 and 1 menu of size 2.

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[2]In retrospect, knowing the results of the experiment and given the problems found on value imputation with a utility function it would have been better to restrict the number of items to 4 and thus be able to explore more thoroughly the space of 2-by-2 comparisons.

6. menu of sizes 6 with 1 random menu of each other size.

Finally, as the subjects have chosen menus and once all the comparisons are done, 5 comparisons among the 35 are be drawn at random and a menu for each of the chosen comparisons is selected using the described procedure. The subject then have to choose in each of these menus 1 and only 1 lottery.

2.2.2   Estimations and Models

In order to implement the economic models on our data, it is necessary to estimate the preferences of the subjects. In the following sections we present the methods we have chosen. In order to estimate individual preferences on singletons, we use the following methodology:

1. We consider a menu comparison set of size 1. For each comparison we construct 2 equations. For example, if we consider the comparison between a menu A and a menu B we construct the 2 equations:
   - $A = B + x$
   - $B = A - x$
2. We assign a value of 0 to one of the menus.
3. We solve each equation for which the right-hand side can be calculated.
4. Each singleton is assigned the average value of the equations for which it is the left-hand member that could be calculated.
5. Repeat step 3 until the desired number of iterations is reached.
6. Each singleton is assigned the average value of the last n iterations (or n an arbitrary number).
7. We normalize the values by subtracting the value of one of the singletons.

We tested this methodology on our data and the procedure seem to converges quickly after about 30 iterations the variation in the estimated value is negligible. And the result is independent of the singleton chosen as starting point.

This methodology allows us to estimate the preferences for the singletons while taking into account the variability in the subjects' responses. However, this method is based on 2 assumptions. The first one is that there is indeed a preference value for the singletons to be estimated and the second one is that the comparison of elements is symmetrical. We do not test either of these two hypotheses in our experiment but consider them as valid or at least as reasonable approximations.

To estimate the effect of temptation, we use a procedure similar to the one used to estimate preferences for singletons. The only difference is the way we build the equations from a comparison between 2 menus. As an example, we focus here on comparisons between menus of size 2 and size 1. We use the estimates made on the singletons to identify the preferred item in each menu of size 2, once this item is identified we compute a theoretical difference with the menu of size 1 by subtracting the estimated value of the preferred item of the menu of size 2 from the one of size 1. We calculate the corrected comparison of

preferences that we use to build our 2 equations. For example let's consider the comparison between the menus $A = \{a_1, a_2\}$ and $B = \{b\}$, and suppose that the estimates of the preferences for the singletons yield $hatu(\{a_1\}) > \hat{u}(\{a_2\})$. We note $wtc_t = \hat{u}(\{a_1\}) - \hat{u}(\{b\})$. We construct the 2 equations:

- $A = B + x - wtc_t$
- $B = A - x + wtc_t$

With these equations we use the same procedure as for the estimation of singletons. The preferences estimated in this way allow us to calculate the value predicted by the expected utility model for each of our 35 menus.

In our analysis we will compare the results of 5 different models. These models are grouped in two categories, the statistical models which are the application of statistical methods to our data and the economic models which are implementations of economic theories adapted to our data. The two statistical models are the following:

1. Constant response model. This model consists in calculating for each individual the aggregate average WTC he is willing to pay, over all comparisons between menus of size one and between menus of sizes two and one. This model predicts that the WTC of an individual for any pair of menu items is his average aggregate WTC. This model is dummy but it is useful as a lower-bar reference point. It seems reasonable to assume that a relevant model is more efficient than this constant response model. Using this kind of dummy model as a comparison point is a common practice in machine learning.

2. Linear regression. This model consists in estimate a linear regression model for each individual. The explained variable of this regression is the WTC between 2 menus and the explanatory variables are variables indicating the presence of the elements in the compared menus. This type of model is the usual starting point in machine learning, it is relatively simple, not expensive in terms of calculation and can easily predict new values if we have the corresponding explanatory variables, which is our case.

The economic models that we have chosen are the following:

1. The expected utility model (EUT). This is the model that corresponds to the *standard theory* in economics where the value of a menu depends only on the value of the item inside it that will be chosen, therefore on the preferred item that it contains. We have chosen to call it the expected utility model because our menus contain lotteries. The WTC between two menus is calculated as the difference between the preferred items that each menu contains. We have to estimate for each individual the relative value of each item contained in the menus.

2. The Gul and Pesendorfer model (G-P). In this model, we estimate the

value of a menu with the following formula:

$$U(A) = max_{x \in A}(u(x) + v(x)) - max_{y \in A}(v(y))$$

We have to estimate for each individual two preferences for each item present in the menus. and we estimate the WTC as the difference between the estimated values of the two menus.

3. The cumulative temptation model.

We estimate the value with the following formula:

$$U(A) = max_{x \in A}(u(x) + v(x)) - \sum_{y \in A}(v(y))$$

This model uses the same estimation as the previous one but this time the temptation effect of all the items of a menu is taken into account not only the one of a particular item.

2.3   Results

Our analysis is based on the assumption (that we don't test) of symmetry of the WTC. Indeed, we consider that if an individual is willing to pay x\$ to switch from menu A to menu B, this individual will be willing to pay -x\$ to switch from menu B to menu A. As we consider this hypothesis to be true, we applied it as a pre-processing on our data so that the left menu is always the larger of the two menus to compare by modifying the WTC multiplying it by -1 when necessary. This modification is intended to make the analysis easier to understand and does not change the results.

The starting point for estimating the different economic models presented is the estimation of individual preferences. We will then estimate the preferences of each individual by following the method explained. To estimate the preferences on the singletons we use the 10 comparisons of menus of size 1 made for each individual. We iterate on the resulting equation system 500 times and we keep as value for each item the average of the last 50 iterations. Finally as we compute relative preferences, we normalize each value by subtracting the value of the singleton {2}. This method gives us estimates for the 6 possible singletons for 98.32% of our subjects. An error in the programming of the experimental software has wrongly validated strings of singletons for 1.68% of the subjects making our estimation procedure invalid for them, so we have removed these subjects from our study.

Since singletons can be seen as lotteries, we can test if subjects have consistent preferences in terms of risks. We consider preferences as consistent in terms of risk if the singleton {N} is the singleton whose estimated utility is maximum then the estimated utility of the singleton {P} is higher than that of the singleton {Q} if $N > P > Q$ or if $N < P < Q$ i.e. single-peaked preference.

Among our subjects we find that 10.27% have consistent, single-peaked prefer-
ences. This may seem low and could call into question our method of estimating
preferences. However, our estimates are strongly correlated with the subjects'
responses as presented in the figure 2.1. The small number of subjects with
single-peak preferences combined with the elements presented in the next chap-
ter concerning the difficulties of estimating individual utility functions leads us
not to try to smooth values in our estimates. An erroneous smoothing could be
harmful to our analysis. If this does not pose a major problem in the case of the
estimation of singletons, we will see that it is on the other hand problematic for
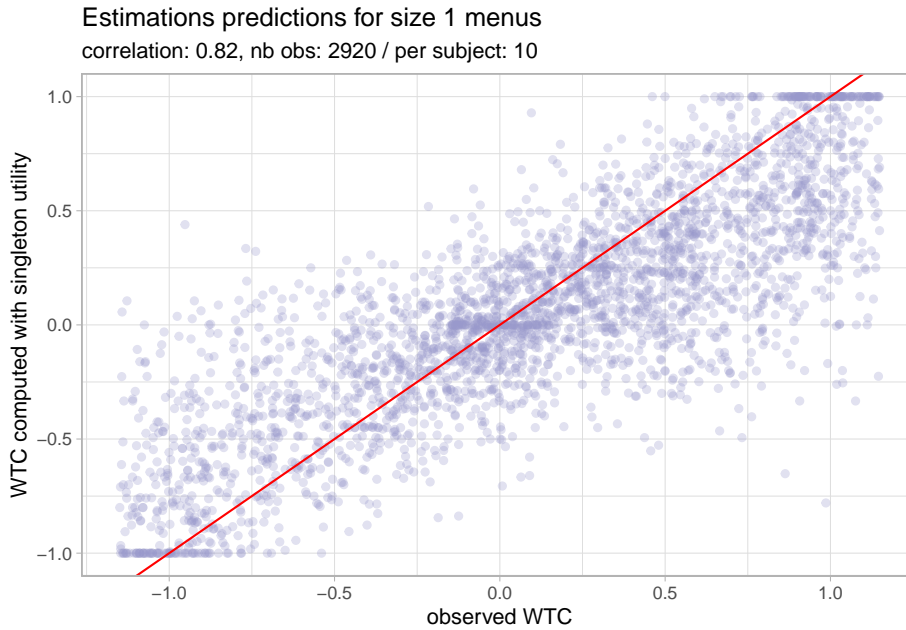the estimation of the temptation effect.

Figure 2.1: High correlation between observation and estimation WTC for size
1 menus

Using the estimated utilities for the singletons and the 12 comparisons between
size 2 and size 1 menus we can estimate the temptation utility of each item for
each subject. We iterate again 500 times on the equation system provided by
the 12 comparisons and we retain for the estimation of the relative utility of
each item the average of the last 50 iterations. To normalize we substract to all
value the estimate for the temptation element 2. We obtain estimates for each
subject, but as foreseen by our design, we do not have the estimates for each
item. The table 2.1 summarizes the percentage of subjects for whom we have
no estimate by item.

With 37.67% of the subjects for whom 1 item has no estimate and 13.36% for

Table 2.1: Share of subjects without estimation by items

2	4	6	8	16	32
0	15.75	19.18	15.75	13.7	0

whom 2 items have no estimate. As these missing estimates are not located on the extreme values, theoretically it would have been easy to impute them by smoothing the estimated values with a utility function. But we have seen with the estimates of the singletons that such a smoothing is not reasonable in practice, as the level of consistency is too low to make this a meaningful exercise. We will therefore keep this value as missing in the rest of the analysis. But missing values do not seem to have an important impact on the predictions of the G-P model. Indeed, by comparing the predictions made on all the comparisons between menus the correlation between prediction and observation is 0.45 on all the available value and 0.47 when we restrict on observation for which we have all item estimated. Since restriction to full estimate item menus don't seem to have an important impact in the analysis, in the following we willuse all available data. The figure 2.2 shows the distribution of the estimates for each of the items.
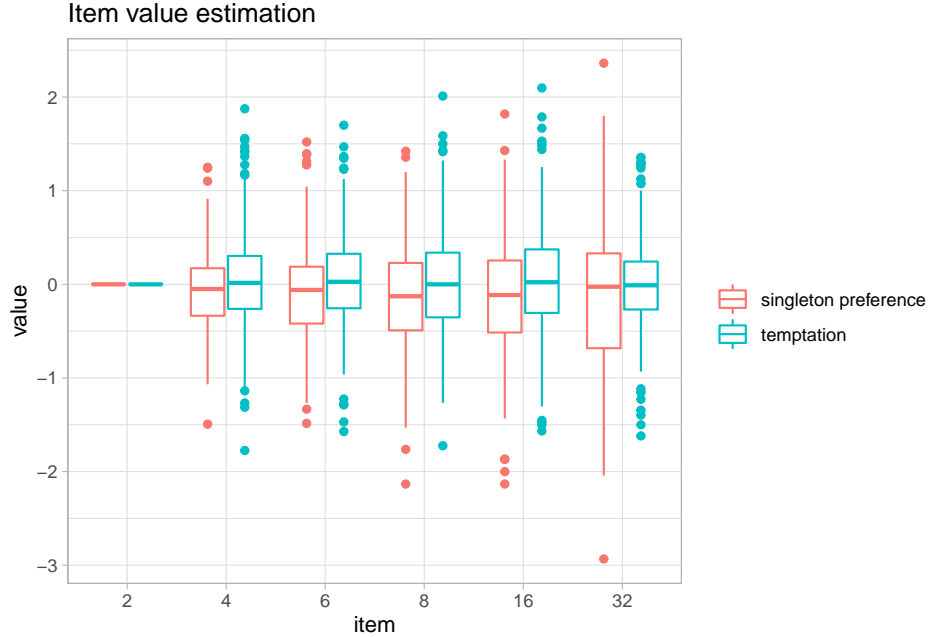
Item value estimation

Figure 2.2: Distribution of the estimate value of item for all subjects

We can see that the estimates are of the same order of magnitude across items

and across preferences for singletons and for the temptation effect. The estimates for singletons seem slightly lower than for temptation but this does not seem to be a significant difference. There are no elements that appear to be outliers in these estimates. Using these estimates we can calculate for each of our economic models the WTC of each comparison made by our subjects.

As a comparison tool to judge the quality of the predictions of our models we estimate two statistical models, a constant response model and a regression model. To estimate the constant response model, we simply compute the average of the subjects' responses in terms of WTC for the size 1 menu comparison and for the comparison between size 2 and size 1 menus. The average of these 22 comparisons for each subject is the prediction for any comparison made by this model.

The regression model is estimated as follows. For each subject, a linear least square regression model is estimated with the data concerning the size 1 menu comparisons and the size 2 menu comparisons with size 1 menu. The model has the following form:

$$WTC = \beta_0 + \beta_1 L_2 + \beta_2 L_4 + \beta_3 L_6 + \beta_4 L_8 + \beta_5 L_{16} + \beta_6 L_{32} + \beta_7 S_2 + \beta_8 S_4 + \beta_9 S_6 + \beta_{10} S_8 + \beta_{11} S_{16}$$

Where L_i is an indicator variable for the presence of item i in the largest menu and S_i the same for the presence of i in the smallest menu. Note that our model does not contain the indicator variable S_{32} because it is a linear combination of the other variables given the menu size constraint. The estimates of each of the model parameters for each subject are shown in the figure 2.3.

We can see on the graph that the effect in the different variables of the model are of the same order of magnitude and close to 0. The estimated values are close to the one estimated for the economic models especially by comparing the indicator variables for the largest menu with the preferences for singletons and the one for the smallest menu with the estimates of the preferences for the temptation. However, it would be premature to interpret the coefficients of the regression as individual preferences for the different items. Indeed our model does not take into account the interaction between the different variables which could be captured in the estimates of the different coefficients by bias the the interpretation. We can however eliminate the hypothesis that subjects evaluate a menu according to the sum of the items that compose it. Indeed in this situation there would be no interaction between the different elements of a menu and we should observe that all the coefficients for the largest menu are higher or equal to 0 and all those for the smallest menu are lower or equal to 0, This is not the case here.

As our objective is to use the regression model as a tool for comparison with other models, we will not try to interpret its coefficients at the individual level. We will simply compare the predictions generated by this model with the predictions of other models and the responses of the subjects.
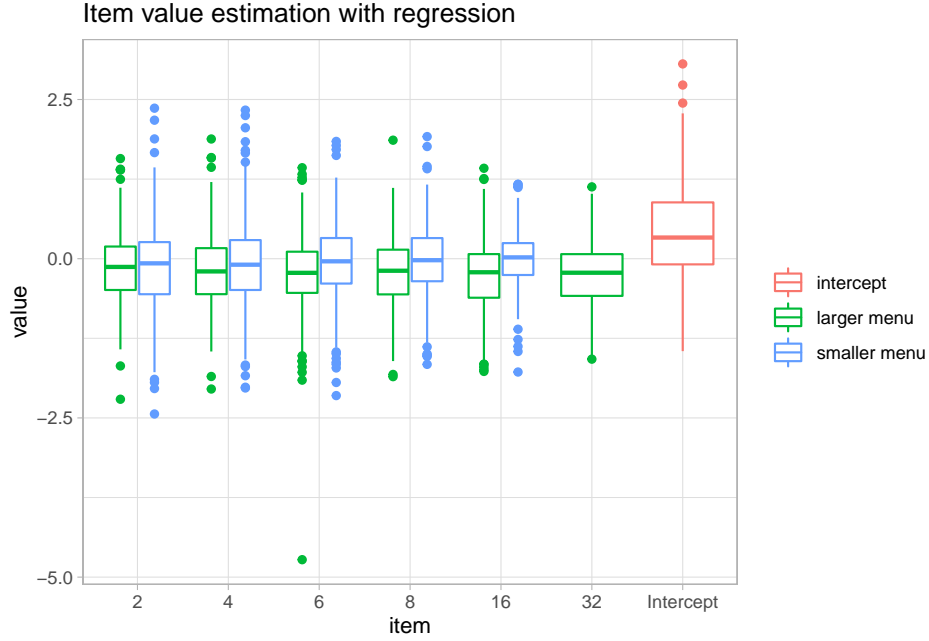
Figure 2.3: Distribution of the coefficent by items for all subjects

Now that we have estimated the different parameters for our 5 models, we can compare their performances using 3 different metrics:

1. Mean Square Error (MSE), which is the mean square difference between the predictions of a model and the observed values. This metric is the default metric in many machine learning applications because of its mathematical form and because it penalizes models with errors that are very far from the observations.
2. Pearson correlation coefficient, which is an indicator of linear co-tendency between two sets of values. As our economic models are estimated from relative utility estimates, we use this indicator to test that a model is at least consistent with the data in terms of trend even if its predictions are biased.
3. Percentage of correct sign estimate: our study concerns preferences between menus, so we expect a model to be able to correctly predict whether an individual will prefer one menu to another or whether he will be indifferent between two menus. For each model we compute the number of times it predicts a WTC of the same sign as the one observed[3].

In the table 2.2, we present the results of the five models according to the three

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[3]Note that the possible observations are +,- or = so it is not an exercise of binary prediction or we can improve the predictions of a model by taking the opposite of these predictions.

Table 2.2: Performance of the models, all comparisons

model	MSE	correlation	correct sign
Linear model	0.31	0.59	0.73
G-P temptation	0.32	0.45	0.54
Constant response	0.33	0.38	0.59
Expected utility	0.36	0.37	0.47
Cumulative temptation	0.46	0.33	0.56

metrics, calculated on all the comparisons.

We see that the model that performs best by all metrics is the linear regression model. The constant response model is second in terms of percentage of correct sign and MSE, which tells us that the economic models are globally poor descriptors of the subjects' behavior. Among the economic models, we see that the best performing model is the G-P model. It is only beaten by the cumulative temptation model for the percentage of correct sign predicted. However, the latter model performs very poorly in terms of MSE and correlation. Finally, if we compare the G-P model with the expected utility model, we see that taking into account the temptation effect represents a marginal improvement in descriptive power. But insofar as these two models perform less well than the constant response model, it seems unwise to use them.

The table 2.3 shows the performance of each model according to the type of comparison. Before detailing the results, it should be remembered that the different models were trained using the size 1 menu comparison and the size 2 menu comparison with the size 1 menus. These two categories represents 62.86% of the observations.

The first noticeable element in this table is that the different economic models produce the same results for comparisons between menus of size 1. The results for this type of comparison are quite good, they are far superior to those of the constant response model and only slightly inferior to the regression model. On the other hand, if we look at the comparisons of size 2 menus against size 1 menus, the performance of the economic models falls below that of the constant response model. And this remains true for the other types of comparisons. The linear regression model is the best model on the training data but it too performs worse than the constant response model on the new comparison types, it remains superior to the economic model on these data. This may indicate an overlearning problem on the training data.

Now if we look at the performance of the economic models we see that the G-P model performs better than the expected utility model. It seems that integrating temptation in the evaluation of the comparison between size 2 and size 1 menus allows to improve the predictions according to our 3 metrics. But this effect does not seem to have any impact for the other types of comparisons. This may

Table 2.3: Performance of models by comparaison type

model	1 vs 1	2 vs 1	other
correlation			
Expected utility	0.82	0.10	0.18
G-P temptation	0.82	0.36	0.18
Cumulative temptation	0.83	0.30	0.14
Linear model	0.86	0.85	0.31
Constant response	0.27	0.48	0.39
MSE			
Expected utility	0.12	0.49	0.43
G-P temptation	0.12	0.37	0.43
Cumulative temptation	0.12	0.51	0.63
Linear model	0.09	0.11	0.68
Constant response	0.35	0.30	0.34
correct sign			
Expected utility	0.78	0.36	0.33
G-P temptation	0.78	0.51	0.37
Cumulative temptation	0.78	0.52	0.47
Linear model	0.81	0.83	0.57
Constant response	0.54	0.63	0.60

indicate that the improvements for the comparison between size 2 and size 1 menus is due to an overlearning phenomenon. It may also be a sign that the G-P model does not use the right functional form. Indeed, alternatives to the G-P model like Noor and Takeoka [2015] postulate that the effect of temptation is not linear. This could be a way to improve the performance of the model for other types of comparisons but such a model would also be less efficient than a constant response model for comparisons between menus of sizes 2 and 1, which therefore does not seem to be a promising direction for improvement. Finally, the cumulative temptation model is less efficient than the G-P model except in predicting the sign of the WTCs, but it is still inferior to the constant response model on this metric too.

We have just seen that economic models are poor descriptors of subjects' behavior when dealing with menus of more than one option. In the figure 2.4 we show that these models have in common to predict that subjects are indifferent between two menus much more frequently than what we observe. And that this is also their main difference with the linear regression model which has better performances.

If the economic models often predict the value 0 – i.e., indifference between the two compared menus – this is because they evaluate the menus according to the preferred item they contain. In the case of the expected utility model this implies that all menus that share the same preferred item will have the same
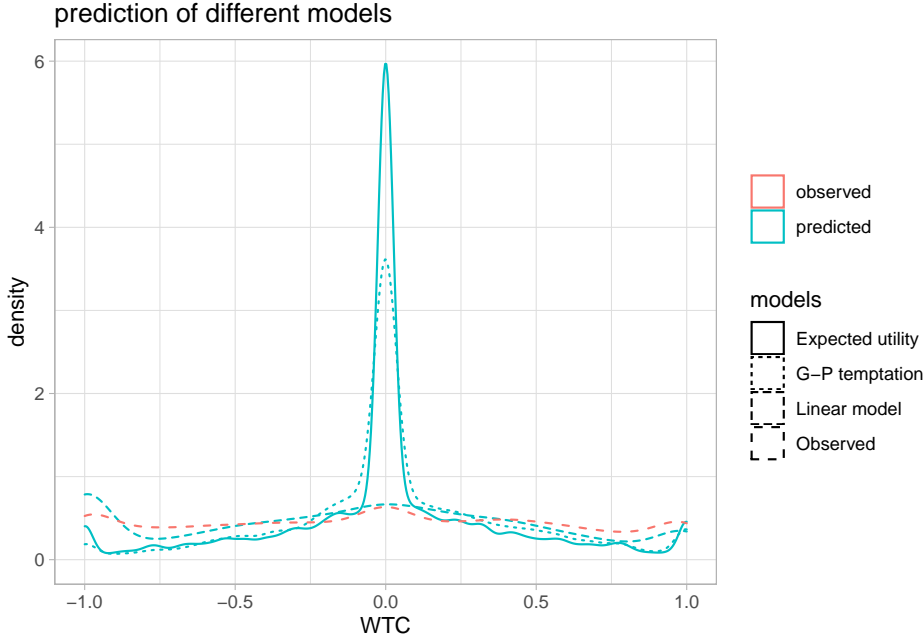
Figure 2.4: Distribution of the WTC value predict by models

estimated value. In the case of the G-P model this phenomenon is attenuated by the effect of temptation of another menu item but remains important and the menus that share their preferred item will have close values. However, we observe on the comparison of the menus that subjects are rarely indifferent between two menus. This seems to contradict the fact that subjects evaluate menus based on a particular item.

2.4 Discussion

In this chapter we have proposed an experiment to test the temptation model proposed in Gul and Pesendorfer [2001]. In this chapter we have proposed an experiment that compares the descriptive quality of the model in Gul and Pesendorfer [2001] with other economic and statistical models. Our test does not aim at testing the existence of a behavior predicted by the theory as the rest of the experimental literature on the subject but at testing the theoretical framework proposed to rationalize this behavior. Our experiment thus allows us to highlight that the theoretical approach proposed by G-P does not adequately account for the observed behavior of the subjects. We show in effect that a dummy constant response model produces predictions as close to the observations as G-P's model in terms of precision, tendency and preference between

menus.

Our experiment globally questions the menu choice approach proposed by Kreps [1979] and followed by G-P and the temptation models presented in the review of Lipman et al. [2013]. These models, although an improvement over the expected utility model, are poor descriptors of our observations. Their specific item comparison-based approaches underestimate the difference between two menus reported by our subjects, notably by predicting that subjects will be indifferent between options over which subjects dare not indifferent.

In this respect, machine learning methods such as simple linear regressions – that take into account all elements of a menu and not just some preferred ones – are at least better descriptors on our data than economic models. We show in effect that a simple model trained at the individual level offers better predictions in terms of trend predictions and preferences between menus than the economic models and the constant response model.

Nevertheless, it should be noted that our analysis, like the menu choice framework, is based on expected utility theory. And although we used the most robust estimation methods possible, it is possible that our results are biased by a discrepancy between the observed behavior of the subjects and that predicted by the expected utility theory. Part of this issue will be studied in the next chapter.

We believe that in order to propose models with better descriptive power it would be useful to have more information on the structure of individual errors. This would allow us to improve the descriptive performance of risk preference models as well as models derived from them such as the Gul-Pesendorfer model.

Chapter 3

Repeated Choice

3.1 Introduction

In this article we are interested in the following question: When an individual is confronted several times with the same situation, does he always make the same choice? This question has few concrete implications. It is indeed unlikely that an individual is to make two identical decisions in the same context. But this question seems important from a theoretical point of view. First, for modeling, the answer to this question is intimately linked to the choice of static versus dynamic, deterministic versus stochastic models. Secondly, for the analysis of results, especially in experimental economics. Insofar as we consider that individuals always act in the same way in the same situation, it is only necessary to have one observation per situation. But if the behavior varies in the same situation it may be necessary to have multiple observations.

In order to answer this question, we conducted an experiment. We limited ourselves to a situation where the subjects had to choose between different lotteries. We chose the risk preference framework for multiple reasons. First of all, to our knowledge, the question of the stability of preferences has not been treated in this framework, and this is a central domain in economics. Moreover, in this field, we have a theory that has already been widely tested on other themes and whose extension has difficulty in responding to the contradictions highlighted by, among others, Friedman et al. [2014]. These models are for the most part developments of the von-Neumann and Morgenstern model of expected utility which is a static and deterministic model. Even the stochastic models, of which the best known is that of Luce [2012], are built around the existence of a central value for the risk preference parameters. It seems important to us to question the static nature of the models used. Finally, from a practical point of view, as the question of risk preferences has already been treated in experimental economics, we have tools to measure the parameters of risk behavior. In our

experiment, we will use a variant of the method used in Lejuez et al. [2002] and Crosetto and Filippin [2013] to elicit the risk preferences of our subjects. We also have results on similar questions in the context of risk preferences. For example, Hey and Orme [1994] conducted an experiment to test different developments of the expected utility model. Even if the situations in the experiment were not the same, this experiment shows the ability of different models to explain individual decisions. Following the same idea, Wilcox [2007] uses simulations and Hey and Orme [1994] data to estimate the predictive power of the models on a new set of situations. Finally, the Ert and Haruvy [2017][1] experiment, which is interested in the learning of subjects, provides them with feedback and thus modifies the situation between the different elicitation of preferences, but also studies the variation of preferences for risk between close situations.

Our experiment was conducted online at the end of May 2021. The recruitment was done via Amazon Mechanical Turk and we collected data on 300 subjects as planned in our pre-registration[2]. The experiment itself was done on a dedicated application developed with the Shiny framework of the R language. It is built on an extremely simple experimental design. After making sure that our subjects understood how the risk elicitation task works, we measured their risk preferences 100 times. The measurements were taken one after the other without any feedback, the payoffs and resolution of uncertainty being calculated only at the end of the experiment.

As the measurements are made in similar situations to each other, we expect that the 100 measurements will be identical for each subject. However, we observe that this is only the case for 6.67% . We show that only for 47.67% the answers are normally distributed and that for at least 20.67% , the data does not seem to admit a unique central value. Moreover, we show that measures of risk preferences can lead to the wrong conclusion that a treatment has an effect. And that models built on a risk aversion parameter are of poor quality both in describing the data and in predicting the future behavior of subjects even in the simplest situation. But it seems possible to significantly improve the quality of predictions by using models that take into account the past behavior of the subjects.

Our experiment shows that it should not be taken for granted that when confronted with the same situation several times an individual will always respond in the same way. Responses can vary significantly not only between but also within subjects. Within-subjects variations can be so extreme that the behavior of a subject cannot be satisfactorily summarized by a central value. This implies that we must pay particular attention to the conclusion that we draw from a difference between two measures, but also to the behavioral conclusion that we can deduce from a risk aversion parameter. Finally, these results must be put into perspective by the fact that our experiment has few equivalents and that,

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[1]We would like to thank Ernan Haruvy for sharing data with us. This allowed us to test our analysis on existing (albeit different) data prior to conducting our experiment.

[2]Pre-registered hypothesis are available with asPredicted code zx667

consequently, the results we have obtained are uncertain and must be supported and corrected by other studies.

3.2 Experimental design

Our experimental design allows us to measure 100 times the risk preferences of each subject. Each of these measurements is performed under conditions as close to each other as possible. Each of the measures is performed using an elicitation mechanism regularly used for similar purposes in economics and psychology. Each of the elicitations is incentivized by an amount that even if relatively small in absolute value is much higher than the average remuneration used in experimental economics on the target population. Moreover once the amount is put in relation to the effort and duration of the task, the corresponding remuneration is similar or even higher than the remuneration of subjects in laboratories. Our protocol also includes a learning period with feedback in addition to the traditional control questions in order to ensure that the subjects have fully understood how the elicitation procedure works and its impact on their compensation.

We believe that our experimental design allows us to satisfactorily study how individuals behave in the face of risk on a repeated basis. We have a large number of observations per subject which allows us to perform robust statistical analyses at the individual level. And the observations are comparable at the individual level because we made sure that the conditions are as close as possible in terms of incentive, information, form and time.

Our experiment took place from May 25th to May 27th, 2021. It involved 300 subjects[3]. The recruitment was done using Amazon Mechanical Turk and no constraint of competence or localization was applied. The subjects were paid a fixed amount of 0.5\$ plus a variable amount depending on the lotteries they chose. In order to ensure the quality of the answers provided and rule out automatic replies by bots, we controlled for a number of parameters on the answers provided by our subjects:

1. A response time for the experiment of less than 10 minutes.
2. A number of trials higher than 3 for the control questions.
3. A number of changes in the answer before validation less than 112 (the minimum possible is 110) or more than 200.
4. A small variation in response time to the lottery (a standard deviation of response time of less than 1 second).
5. A small variation in response time between responses (a standard deviation of time less than 2 seconds).

None of these criteria is in itself a sign of poor response quality, but the combination of several of them could be problematic. Fortunately, only 23 subjects

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[3]317 subjects finished the experiment but only those having entered a valid payment and control code at end of experiment were taken into account in the analysis.

accumulated 2 and only 2 accumulated 3 (and no subject was flagged for 4 or more criteria). This leads us to believe that the answers provided by the subjects can be considered of good quality and not polluted by automatic answer programs.

The course of the experiment for the subjects was as follows:

1. The subjects select the task on Amazon Mechanical Turk, they are informed of the approximate duration as well as the amount of the fixed payment and the nature of the task. They are presented with a link to the experiment.

2. When they arrive at the experiment website, they are presented with a login screen that asks them to choose a username and password (or to provide their own if they have experienced a logout problem). If the identifiers are valid they are secretly and randomly assigned to one of the 9 treatments.

3. The instructions of the experiment are presented to them. The instructions explain the process of the task as well as how they will be rewarded according to the lotteries they have chosen. Instructions are provided in appendix Appendix C.

4. The subjects are then asked to answer 4 simple control questions. They have as many tries as they want to find the right answers, but they have to succeed in order to continue.

5. The subjects of the treatments concerned must then answer a questionnaire on risk behavior.

6. Subjects are then asked to select 10 lotteries and are shown the bonus they would have obtained based on their choices, but they are warned that these lotteries will not affect their bonuses. This step serves as a learning phase so that the subjects can take the mechanism in hand by themselves.

7. Subjects select the 100 lotteries that will be used to calculate their bonus. This phase represents the main body of the experiment.

8. The subjects of the concerned treatments have to answer a short questionnaire about their preference for the risks similar to the one at the beginning of the experiment.

9. The lotteries that will be taken into account for the subjects' bonus are drawn and uncertainty resolved in order to calculate the bonus of each subject. The results are presented in a table with a summary of the gains as well as the code to be filled in on Amazon Mechanical Turk so that the subject can receive the bonus. Only the subjects correctly filling the code to AMT are paid and kept for the analysis.

Our experiment includes 9 treatments, over two independent dimensions, each with 3 modalities. The first dimension concerns the risk behavior questionnaire, that can be run at the beginning, at the end of the experiment or both. The second dimension concerns the lotteries proposed to the subjects. The 3 modalities are the following:

1. Each lottery is presented on a separate screen and each of the 100 lotteries is payoff-relevant. Each experimental currency unit is worth 0.005$.
2. The lotteries are presented 10 per screen and one lottery per screen is randomly drawn to be payoff-relevant. Each experimental currency unit is worth $0.05.
3. The 100 lotteries are presented all on one screen and a single lottery is randomly drawn to be payoff-relevant. Each experimental currency unit is worth $0.5.

In each of the treatments the maximum expected payoff is $8. The different treatments should not impact the behavior of the subjects, the situations being theoretically similar. Having different payment modality is a robust test against the bipolar behaviorist bias pointed out by Harrison and Swarthout [2014]. However, we cannot use any of our processing to perform robustness tests. Our analysis of the data revealed a ghost treatment effect (detailed in the following analysis) which may make treatments appear statistically different without reason. We therefore chose to conduct the analysis by pooling the treatments.

Incentives may seem too small, but this is not the case. The maximum average gain for our experiment is $8.5 which makes it an extremely well-paid task given that the average time to complete it is 42 minutes. This amount is also extremely dependent on the subject's choices. It can indeed vary from 0.5$ to 8.5$ depending on the choice, and even in treatments where the value of a unit is 0.005$ the amount expected from a lottery can vary from 0$ to 0.08$ which may seem low but should be put in comparison with the time needed to make this decision (in the order of a second) and the amount usually paid for tasks on Amazon Mechanical Turk (e.g. Sjåstad and Ekström [2021] pays subjects 0.01$ to correctly count the number of colored cells in a 150 cell matrix in 50 seconds). This leads us to believe that the collected responses reflect, a decision similar to the one usually observed in experimental economics when eliciting responses from subjects in a brick-and-mortar laboratory.

The objective of our experimental design is to have subjects make the same choice multiple times (or at least choices that are as close as possible) and to incentivize them, within the framework of utility theory, to make the same decision each time. We have therefore chosen to repeat the same risk elicitation task 100 times in a row. The repetitions are done one after the other without any feedback until the end of the experiment. The different elicitations are performed in a short time (less than 2 hours for the slowest). The different choices do not or can not differ between them in terms of information, time or gain already acquired. The main difference is that as the experiment progresses, the number of decisions made by a subject increases, but this factor does not influence decision making in the expected utility model (and in many other models in decision theory).

To ensure that subjects are encouraged to make the same decision in each of their choices, we chose to use an elicitation method inspired by the Balloon

Analaog Risk Task (BART, Lejuez et al. [2002]) and the Bomb Risk Elicitation Task (BRET Crosetto and Filippin [2013]).

Our task is the same as BART but without the ball. Removing the balloon is a way to avoid the subject being distracted by the balloon animations or being biased by his experience with real ball as highlighted in Steiner and Frey [2021] and De Groot [2020]. In our task subjects have to choose a value n between 0 and 64. This value corresponds to a lottery which allows them to win n units (whose value depends on the treatment as explained later) with a probability of $\frac{64-n}{64}$ and 0 otherwise. By considering subjects whose utility function is of the CRRA type and using the form used by Wakker [2008]:

$$u(x) = \begin{cases} x^r & \text{if } r > 0 \\ -x^r & \text{if } r < 0 \\ ln(x) & \text{if } r = 0 \end{cases}$$

We can show that this task allows to elicit the preferences of individuals with a risk aversion parameter between 0.016 and 64. And that for a value r between these 2 values :

$$n^* = \frac{64r}{1+r}$$

With n^* the value that maximizes the utility function $u(\cdot)$. We can also associate to each possible value of n the parameter r for which the utility function would be maximized (except for the values 0 and 64 which are choices strictly dominated by all others):

$$r = \frac{n}{64-n}$$

For each iteration of the task performed by a subject, we can therefore easily associate a risk aversion parameter.

3.3 Results

Our analysis will be composed of 3 parts. In the first part we will describe the individual choices in terms of variance and associated distributions. In doing so we will show that individual behavior is highly variable and that they are not normally distributed as is often assumed in economic data analysis. In the second part we will show the consequences of these specificities and their impact on the statistical methods used to test hypotheses and the reliability of the results. Finally, we will briefly look at a potential way to improve the methods usually used to estimate risk aversion.

Using the elicited values we will estimate different models. We will then compare these models according to their Mean Square Error (MSE) on subsets of our data of different sizes. In all situations we adopt the standard practice in machine learning of training the models on sets distinct from the test set on which we measure the MSE. The models we use are the following:

1. **dummy**: This model simply predicts for each subject and each period 32. This value is the central value among those available. This model does not learn anything about the behavior of the subjects and is only used as a reference to judge the performance of the other models. The models performing less well than this model are probably not relevant.
2. **mean, median, mode**: These models predict for each subject the simple mean/median/mode of the values observed during the training periods.
3. **r_irr**: This metric is proposed to have a metric that internalizes individual errors. We therefore use this index *r of irrationality* which for a set of choices of a subject indicates the value r which minimizes the irrationality in terms of certain equivalent. This model predicts the value corresponding to the specific risk aversion parameter *r* of a CRRA utility function that minimizes for each subject its irrationality index over the learning period. We define irattionality index as the sum of the difference between the certainty equivalent chosen by the subject and the certainty equivalent of the optimal choice for a given utility function.

$$d_{irr} = \sum_{i=1}^{i=N} c(n^*, u) - c(n_i, u)$$

   with $c(n, u)$ the certainty equivalent of the choose n for an individual with an utility function u, and n^* the choice that maximize the utility function u. From this metric we estimate utility function for subject by minimising d_{irr}. This model has the advantage of being able to be calculated for different sets of choices and to take into account the strictly dominated choices made by a subject.
4. **r_local**: This model predicts for each subject the value corresponding to the average of the r-values calculated for each training period. This model has the advantage that it can be computed for different sets of choices like the previous one.

The first notable feature of our results is the significant heterogeneity in individual behavior. Indeed, the subjects have in the experiment extremely different attitudes both in terms of average values chosen and variance around this average. But even for the individuals closest to each other in terms of mean and variance there can be significant differences in behavior both in the frequency of variations and in their amplitudes. Figure 3.1 gives a visual glimpse of the behavioral heterogeneity. Each line corresponds to the choices of an individual and the individuals are divided by increasing average choice over the rows and by increasing variation over the columns. Each cell of the graph contains 12 individuals.

This large variation in individual behavior is less evident in the statistics. In the table 3.1 we have reported the quantiles of the mean and standard deviation per subject. In this table we have also indicated the quantiles for the 95% confidence interval for the parameter *r* per subject under the assumption of normal data, as well as the part of the observations that should be outside the observed values

Figure 3.1: All choice. Each line show the 100 choices of a subject

Table 3.1: Statistique of individual choice

	quant 5%	quant 25%	median	quant 75%	quant 95%
mean	7.56	19.06	27.64	34.47	45.06
sd	0.00	4.35	11.43	16.50	19.10
mean r	0.19	0.61	1.66	3.97	7.47
sd r	0.00	0.16	2.94	9.71	14.71
width of r C.I.	0.00	0.55	3.92	63.76	64.00
% estim not in [0,64]	0.00	0.04	4.19	7.73	16.54

(inferior to 0 or superior to 64) under the assumption of a normal distribution.

Average results are consistent with the literature on risk attitudes in the laboratory. The majority of the subjects are risk averse (average n lower than 32) and very few subjects have an average higher than 48 (theoretical equivalent to an r of 3). On the other hand, with a median standard deviation of 11.43 the variability of the data is extremely high. This variability makes us think that the behavior of the subjects does not come down to a constant choice with a low variability around this choice. In any case, this variability has a major impact on the estimates of a risk aversion parameter from these data. Indeed, if we calculate the confidence interval for the parameter r per subject from the average choice and the standard deviation observed under the assumption of normality of the data, we obtain extremely wide intervals as shown in the corresponding column of table [4].

This high variability also makes the hypothesis that the data is approximately normally distributed aberrant. For at least half of the subjects we would have under the hypothesis of normality more than 4.19% of the data which would not be included between 0 and 64[5]. To finish with the hypothesis of normality of the individual choices, a Kolmogorov-Smirnov[6] test was carried out by individual on their choice. For 52.33% of the subjects the test rejects the hypothesis of normality of the data at a threshold alpha of 5%. We are therefore confident that a normal approximation is a poor representation of the behavior of our subjects, especially for some of them.

Can the behavior of individuals be described using a central value?
To test this we calculate for each individual their mean choice 5000 times, drawing 75 out of the 100 choices and then we test if these different values of the mean are normally distributed. This method consists in testing the central limit the-

---

[4]Note that as under the assumption of normality it is possible to observe values of n lower than 0 (and higher than 64) the values for the parameter r were brought back to 0 (and to 64) for the extreme cases and that this reduces the size of the reported confidence interval.

[5]To claim that our results can be described with a normal distribution would be similar to saying that a model that predicts that the height of 4.19%% of humans is less than 0 cm or greater than 3m accurately describes reality.

[6]As normality test are sensitive to ties, and our observations are integer and this contain ties, we add an uniform noise to observation before running the test.

Table 3.2: Share of subject who choice pass normality test

KS	SW	AD	KS & SW & AD	KS or SW or AD
69.67	54.33	60.67	43.33	79.33

orem on the individual bootstrapped mean. We use 3 different normality tests to ensure the reliability of the results obtained. The tests used here are the Kolmogorov-Smirnov (KS), the Shapiro-Wilk (SW) and the Anderson-Darling test (AD). Each test is based on a different criterion and we expect to have different but consistent results. In the table 3.2, we report for each test and their different combination the percentage of subjects for whom the test is not considered significant at the 5% alpha level.

The normality tests show that for a significant proportion of subjects, the mean is not normally distributed. The proportion of subjects varies according to the test, from 30.33% for the Kolmogorov-Smirnov test to 45.67% for the Shapiro-Wilk test.

This difference is explained by the statistics used, the Kolmogorov-Smirnov test takes into account the maximum deviation between the empirical distribution and the theoretical distribution while the Shapiro-Wilk test tests the difference between the ordered values and the observed values. We can say that the share of the subjects concerned is between 56.67% and 20.67% but it is difficult to give an exact value.

Beyond the fact that this confirms that individual data are not normally distributed for a significant number of subjects, it raises an even more important issue. Indeed, if the mean of the individual data itself is not normally distributed, this indicates that we are in a situation where the central limit theorem does not apply. This may have two causes, first, our sample is too small to allow the mean to converge. This would be problematic given that the number of data per individual that we have is much higher than what is usually done in experimental economics. Moreover, it would suggest that the data are distributed according to a probability distribution for which it is necessary to observe a large number of data to obtain a reliable estimate of the mean, which excludes most of the distributions usually used to model this type of data. Second, the decision process of the subjects is of a type that does not admit first and second order moments. This result shows us that random choice models such as Gul and Pesendorfer [2006], Gul et al. [2014] or Cerreia-Vioglio et al. [2019] are not suitable to describe the choices of our subjects. These models have in common that even if the choices of the subjects can vary between 2 iterations they should be distributed around a central value in this situation.

While the results reported above might seem like technicalities, we will show that they have implications for the methods used and the results reported in

risk elicitation studies. The first impact of the way individuals choose is on the statistical methods used to test hypotheses in experimental economics. The commonly used approach is to separate the observations that will be available according to the application or not of a treatment. We distinguish here three approaches. The first one, which is called in between, consists in applying the treatment to a part of the subjects and not applying it to another part, and in comparing the two groups. The second approach, called within, consists of applying the treatment to all subjects but on a subset of the observations collected by subjects, and comparing the results between the periods when the treatment was applied or not. The last one, the difference in difference (diffDiff) approach, consists in combining the two other approaches. The subjects are separated in two groups and the treatment is applied only to a part of the observations of one group, which allows to compare the differences of variations between the two groups for the periods when the treatment is applied or not. The comparison is then generally made using a test of equality of means between the groups.

We propose here to study ghost treatment. That is to say that we randomly group our observations as if our subjects had been subjected to a treatment when in fact they were not. To simulate a between-subjects treatment, the same number of subjects were randomly drawn from each group. To simulate a within-subjects treatment we draw a period and we take for each subject the same number of observations before and after this period. To simulate a difference-in-difference treatment we apply the same method used for within-subjects treatments but with two randomly selected groups as done for between-subjects treatments. For the first type, we compare the means of the n between the 2 groups with the help of a Student's t test. For the difference in difference simulations we compare the difference in means between the groups for the differences between the periods using the same test. For each of the presented approaches we test the results with a bootstrap method on individuals and periods. For each of the parameters we performed 5000 draws. For each draw we performed a test with a type I risk threshold of 5%. In the figure 3.2 we present the share of the draws for which the differences in behavior were judged as statistically significant (note that the ordinate axis is presented as a % of the maximum choice made by the subjects which is 100 in between and 50 for the other methods).

Since the groups are randomized, it is expected that the rate of significant cases will be equal to the type I error, that is 5%. Especially in within as no or little individual variation is assume in the experimental litterature. But we notice that only the difference-in-difference method gives the expected result. The two other methods present a rate of significant cases much higher than what is expected, higher than 75% when we use all the observations we have. This rate of significant cases seems to be little affected by the number of subjects, in the case of the between-subjects method the number of subjects does not even seem to have any impact On the other hand, this rate seems to increase with the number of observations per subject.
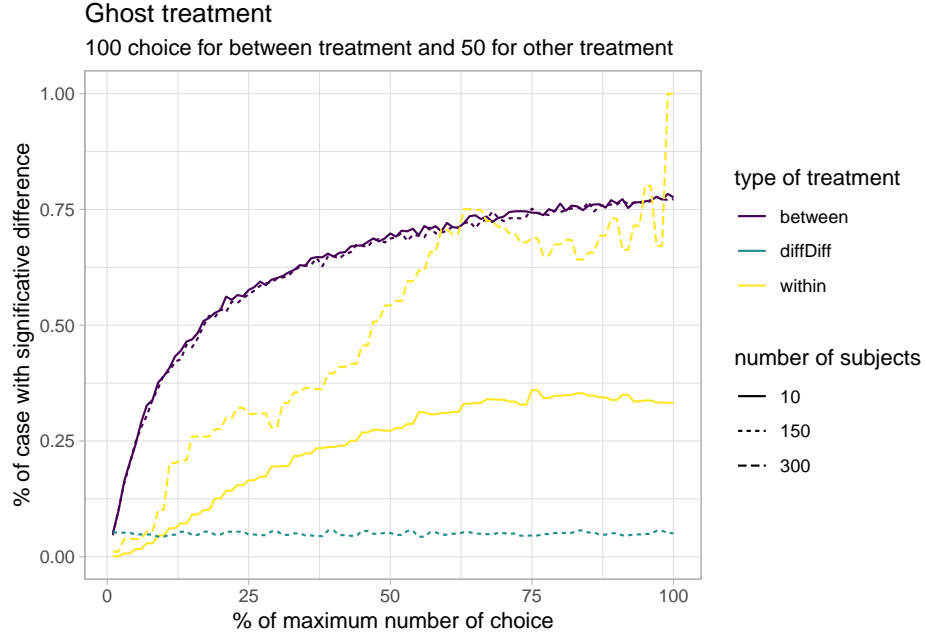
Figure 3.2: Bootstrap t.test at 5% levels for different design

This situation is particularly problematic if we wish to test a hypothesis on individual behavior, because taking too few observations leads to low power and increasing the number of observations without using adequate methods leads to a high risk of wrongly detecting an effect.

Another element in data is the low correlation of risk elicitation task with itself. We tested the correlation of the observations between different periods. The periods were selected using three different methods:

1. *random*: the periods were randomly drawn without replacement to form two groups of equal size.
2. *ordered*: the periods were randomly drawn without replacement before and after a value randomly drawn to form 2 groups of equal size.
3. *consecutive*: the same number of consecutive periods were drawn before and after a randomly drawn value. This method is the closest to an experiment consisting in testing the correlation of the elicitation task with itself.

For each of these methods we used a bootstrap method on the number of periods and we made for each value 5000 draws. The figure 3.3 shows the evolution of the average correlation by method as a function of the number of periods.

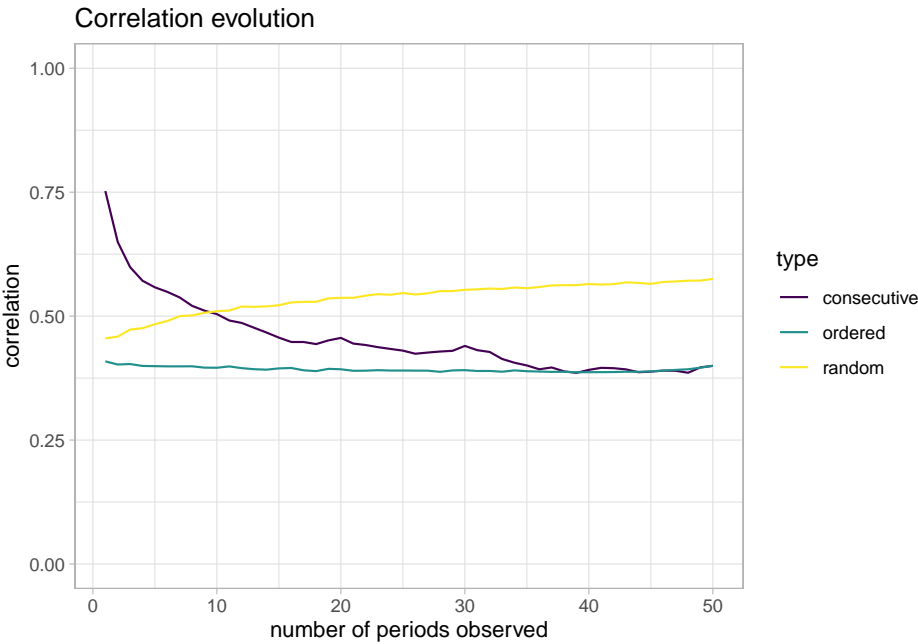As in theory we measure the correlation between independent and identically

Figure 3.3: Pearson corelation between subject choice.

distributed observations, we expect the measured correlation to be close to 1, only subject to random sampling variations and identical for the 3 methods. But we observe that the measured correlation varies between the three methods. Except for the ordered method whose result seems to be independent of the number of periods, the two other methods show a clear trend with the increase of the number of periods considered. The average correlation measured by the consecutive method is decreasing with the number of periods, while it is increasing for the random method. Moreover, the average correlation remains relatively low compared to what is theoretically expected. Indeed, all methods and number of observations taken together, it is between [0.39, 0.75]. And for the consecutive method with 50 observations per subject it is only 0.4. The observed correlation is therefore much lower than 1. The risk elicitation measure is therefore less correlated with itself than one would expect. This must be taken into account in studies where different risk measurement methods are compared as in Crosetto and Filippin [2016]. In this kind of exercise the correlation observed between two methods can be low compared to the theoretical value of 1 but be quite close to the correlation that a method has with itself. Beyond these practical considerations, this correlation of 0.4 tells us that our method of eliciting risk preferences is not reliable to indicate that an individual is more risk averse than another; indeed the ranking between individuals is likely to be different between 2 measures.

An issue related to the question of correlation is the question of the predictability of future behavior. This question has to our knowledge been little studied, and never on the same set of choices as the one used for the learning of behaviors. We will therefore compare different estimators according to the quality of their prediction on a set of observations different from the training set. This is cross validation, a commonly used method in machine learning. It allows to compare models while avoiding overfitting problems. In the figure 3.4 we show the results of different models according to the number of periods devoted to learning (the number of test periods and 100 minus the number of observations devoted to learning) in terms of mean square error.

We see that the models that perform the worst are the models that can be generalized to other choice sets. The r_irr model always performs worse than the dummy model and the r_local model performs worse as soon as the number of observations per subject becomes larger than 10. These two models perform poorer as the number of observations in the learning sample increases. In general these two models perform worse than the dummy model and do not seem to be relevant for predicting individual behavior. The mode-based model performs a little better than the dummy model when the number of training observations is higher than 14 but less otherwise. The best performing models are the median- and mean-based models. Both have a similar behavior in performing better when the training period is long. In the end, these 2 models perform much better than the dummy model. The mean-based model performs a little better than the median-based model. But even with 99 training periods the mean based model displays an MSE of more than 155.
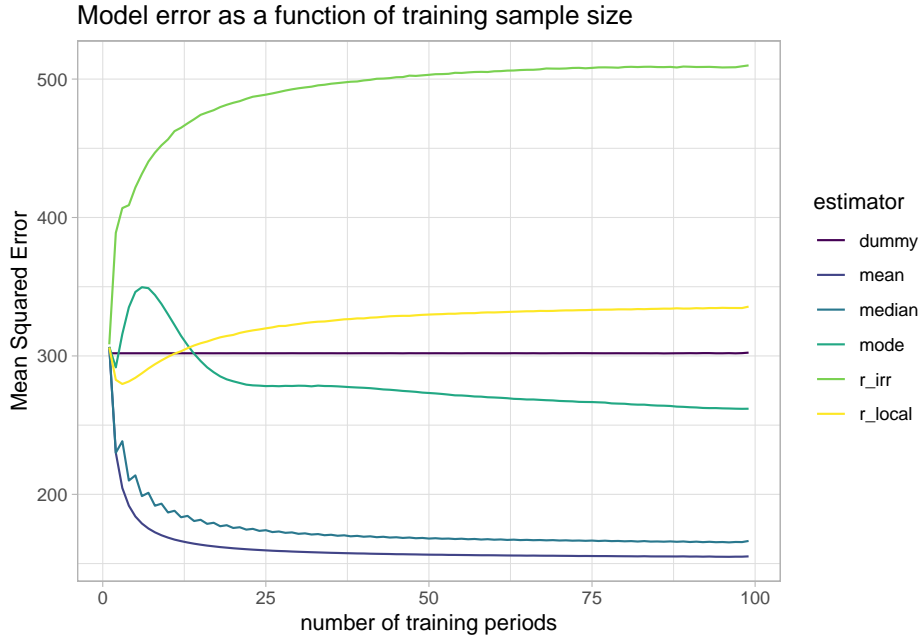
Figure 3.4: Descriptives power of different models.

In order to test the ability of the different models to predict the future behavior of the subjects, we use a similar approach to the previous one, but we separate the training and test samples according to their order. We create a training sample of size m for a subject by selecting the first m observations. Then we compute the MSE on the next 100-m observations, for each subject. By doing so we effectively test the ability of the models to predict future behavior by respecting the serial nature of the data. We can therefore construct a linear regression model for each subject that includes as an explanatory variable for a choice the choices made in previous periods. For each subject the model includes up to the last 5 choices in order to predict the choice of the current period, note that for some subjects the choices of the previous periods can be perfectly correlated between them and that in this case the number of previous choices included is reduced in order not to include two variables perfectly correlated between them. In order to be able to compare the performances of the different models we have used in addition to the linear regression some of the models used previously. In the figure 3.5 we present the performance in terms of MSE of the different models for different learning sample sizes.

As before, the least efficient models are those based on the calculation of a risk aversion parameter. This type of model offers a lower predictive power than the dummy model which always predicts 32. The mean-based model still offers
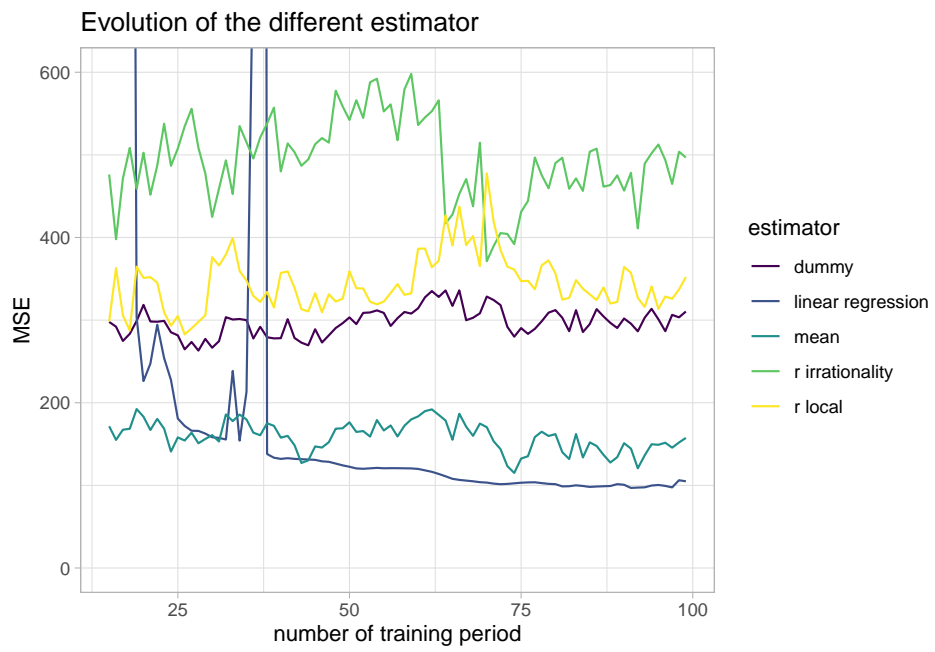
Figure 3.5: Predictives power of different models.

better results than the dummy model. It is also the best performing model when the number of observations in the training sample is low (about less than 40). Finally, the linear regression model offers extremely variable results for a small number of observations (about 40) but when the number of observations in the training sample is sufficient it is this type of model that gives the best results. Moreover, it seems that the regression model learns more than the mean-based model when the number of observations increases, which can lead us to hope that with a larger number of observations we can obtain good predictions.

By comparing the predictions of linear and mean models, we show the importance of a subject's previous choices on his decisions. If linear regressions offer better results it is because in some way the individual choices are not independent and it is possible to build models taking into account this dependence. Insofar as the linear regressions we have built here were built in a rudimentary way and that no model selection tools or even interaction terms or variables of order different from 1 were tested, it would be possible to have better results for this type of model. Another possibility of improvement would be the use of other classes of models like random forests.

This shows that to predict individual behavior, models based on a risk aversion parameter perform poorly on the simplest possible task. That when it comes to summarize these predictions using a single parameter or when we have few observation, the best parameter are the average of observations. But that the predictions obtained in this way are likely to be mediocre. But it should be possible to obtain better quality predictions if we use the right models and have a large number of observations.

3.4 Conclusion

In this article we have tried to provide some answers to the following question: When an individual is confronted several times with the same situation, does he always make the same choice? To do this, we proposed an experimental protocol in which our subjects were repeatedly asked the same question. The repetitions are done one after the other and the subjects receive no feedback before the end of the experiment. The question asked concerns risk preferences. Risk preferences are central to many decisions and their elicitation has already been widely studied in experimental economics. We therefore chose to adapt the BART method used in economics and psychology. This allowed us to repeatedly ask a question with the following properties:

1. The answer to the question is simple (choose a number between 0 and 64) and fast, especially since the subject chooses to always answer in the same way.
2. Subjects are encouraged to answer according to their preferences.
3. A subject with fixed preferences is encouraged to answer always in the same way.

4. The set of possible responses is large and therefore allows for variations in responses of different magnitudes.

With this question, we expected subjects to respond in the same way on all occasions.

But our results show that the subjects act in a varied way and that only a 6.67% of them always make the same choice. So even if the average value chosen by the subjects is coherent with the literature on the subject, the variability of the choices is very important between the subjects and high for an important part of the subjects. We show that this variability in the answers has important consequences on the modeling and estimation of the preferences for the risk. First of all, the estimation of a risk aversion parameter is extremely imprecise with a coefficient of variation for the parameter r of roughly 429 for CRRA function. The distribution of the answers is not normal for 52.33% of the subjects and for 20.67% of the subjects we cannot even be confident that their answer can be described with a central value.

Beyond the theoretical considerations for modeling that our results raise, we also show that the way individuals choose has practical consequences both on the methods used in experimental economics to test hypotheses and on the reliability of the conclusions that can be deduced from the estimation of a risk aversion parameter. Indeed, our results show that in a situation of repeated choice with between- or within-subjects experimental designs the chances of concluding wrongly that there is a significant effect are significantly higher than the significance threshold chosen for the tests. The difference-in-difference designs do not seem to suffer from this effect. We also show that the correlation of risk aversion measures for the same task is only about 0.4. This value is much lower than what one would expect (a correlation of 1) and may explain the low level of correlation observed between the various methods of measuring risk preferences. Moreover, we show that models based on the evaluation of a risk aversion parameter that can be generalized to different sets are very poor models for describing individual behavior. The best alternative to describe individual behaviors with a parameter is to use the mean observation. This paradoxical conclusion with our first conclusion led us to test an alternative model, individual linear regression. We compared this new model with the others according to their predictive power. We show that the predictive power of models based on a risk aversion parameter is as bad as their descriptive power. But we also show that when the number of observations is sufficient, the model based on linear regressions provides better results than the average. This shows us that it is easy to achieve better results than those proposed by the economic models.

Our results suggest that a static model based on a risk aversion parameter is not suitable to describe individual behavior. Indeed, we have shown that this type of model obtains poor results both from a descriptive and predictive point of view even in the simplest task. This is consistent with the work of Wilcox [2007] which shows the weaknesses of this type of model in a slightly more complex task. However, it seems possible to learn more about individual behavior by

having enough observations on repeated choices to train statistical models that could highlight behavioral regularities.

Finally, we can answer our question by saying that the elements at our disposal lead us to say that confronted with the same situation several times, individuals will act in different ways. But this question would deserve more work and results than the few elements we have brought here.

Conclusion

This thesis has been for me the opportunity to shape my understanding of economic research; in particular the impact of different approaches to experiments in economics. When I started my thesis, I was focused on developing experimental protocols that would allow me to measure subjects' preferences as accurately as possible. To design these protocols I relied heavily on theoretical work concerning the models I wanted to test. My goal was to place my subjects in a situation as close as possible to the theoretical model while exploring as much as possible the impact of the different parameters of the model on individual preferences. The experiment presented in chapter 2 is an example of this approach and it suffers from this intention to be as close as possible to the theory which limits the robustness of the analysis of the data in this chapter. My collaboration with Rustam Romaniuc, Dimitri Dubois and Paolo Crosetto encouraged me to develop an experimental design for the temptation model of Gul and Pesendorfer [2001] but which aimed at highlighting the demand for commitment and the capacity of individuals to exercise self-control. This approach that tests the behavioral predictions of the models is the common approach in the literature. This approach allows to document the specificities of individual behaviors and thus guide the development of theories. But identifying behaviors consistent with a theory does not test its validity. As pointed out by Karl Popper in Popper [2005] a given behavior can be predicted by a large number of different models. Moreover, there is an asymmetry between confirmation and refutation of a theory: no matter how many observations in favor of a theory we have, it only takes one observation to invalidate it. This must however be relativized by the imprecision and the possible errors of observations. But this idea encouraged me to adopt a different approach from the standard approach in experimental economics in the second half of my thesis. The idea is to construct the experimental design in such a way as to repeatedly observe a situation in which we know the theoretically expected behavior. We then compare the observed behavior to the behavior predicted by one or more theories. This approach, even if it does not allow us to falsify a theory in the sense of Karl Popper, allows us to compare empirically different models. By applying this approach and comparing economic models to simple statistical models I have shown in this thesis that the temptation model proposed by Gul and Pesendorfer [2001] predicts behaviors that are actually observed in our experience. This model

represents a more accurate description of subjects' choices for menus than the standard expected utility model. But Gul and Pesendorfer [2001]'s model does not describe behaviors more accurately than a model that ignores the composition of a menu. This lack of descriptive and predictive ability is also found with the expectation utility model for lottery choices. It has been shown that individual choices in the area of risk preferences are highly variable. We have also shown that even if the expected utility models and the Gul and Pesendorfer [2001] model do not correctly account for the choices of the subjects, it is possible to propose models that better describe the individual choices than the constant response models that perform as well as our economic models.

To conclude, I would like to propose a way to improve risk preference models. The idea comes from the foreword Aliprantis and Border [2006]: "*It has become clear in the last couple of decades that economic models capable of addressing real policy questions must be both stochastic and dynamic. There are fundamental aspects of the economy that static models cannot capture. Deterministic models, even chaotically deterministic models, seem unable to explain our observations of the world*". I think that looking for models in decsion theory that allow individual choices to be both dynamic and random could improve the predictive and descriptive capacity of models in economics.

Appendix A

Experimental instruction for chapter 2

Round 1

Read again the instructions

Please enter the number of air injections you wish to send into the balloon (max 64):

Ok

Round 1

22

Please enter the number of air injections you wish to send into the balloon (max 64):

```
40
```

The balloon exploded after 22 injections, you win 0.00 €

Next

Welcome

You are going to participate in a game that lasts about 15 minutes. At the end of the game, you will earn €5.00 and a bonus that will depend on your decisions. Your earnings will be sent to you by email as an Amazon gift certificate a few days after the end of the game.

The game has 10 round. In each round, you start with a deflated ball. Each time you inject air into the balloon, you win euros. The more the balloon is inflated, the more money you earn.

The balloon can withstand a maximum of 64 injections but may explode with each injection. The computer chooses at random the number of the injection at which the balloon explodes, i.e. a number between 1 and 64. The time at which the balloon explodes is therefore determined randomly.

You have to decide how many air injections you want to send into the balloon. Once your decision is made the balloon inflates automatically, injection by injection.

You earn €0.15 per injection if the balloon does not explode before the number of injections you choose. If the balloon explodes before, you earn €0.00.

At the end of the game, one of the 10 round will be randomly selected and your payoff for this round will be your payoff for the game.

Next

You can decide on a limit for the maximum number of injections you can choose in each of the 5 remaining rounds. To do this, simply enter a number between 1 and 64.

If, for example, you enter 40, then for each of the remaining 5 rounds you will not be able to choose more than 40 injections.

Whatever limit you choose, you have 75% chances to do the remaining rounds without limit, i.e. the set limit does not apply.

Do you want to set a limit before playing or do you want to play right away?

○ Set a limite   ○ Don't set a limit

Next

Limit

Please enter the maximum number of injections you will be able to choose for the remaining rounds (there is a 75% chance that this limit does not apply):

Next

Appendix B

Experimental instruction for chapter 3

Instruction of the experiment are display as follow:

Welcome

We invite you to read the following explanations carefully. This will ensure that the study takes place in the best possible conditions and that you can be sure that you can finish it as soon as possible. Do not hesitate to reread the following explanations several times.

This study will proceed as follows:

1. After reading these instructions, you will be asked to complete a small set of control questions.
2. You will then have the opportunity to experience the different stages of the study to familiarize yourself with them. This discovery stage will also be the opportunity for you to win your first bonus.
3. You will then be asked to complete a second set of control questions.
4. Finally, you will begin the main part of the study.
5. At the end of the study, you will receive a code that you will have to fill in on the platform. to be able to receive your bonus. A summary of your earnings will be posted at that time.

Your bonus

At the end of this study, you will receive a bonus that will depend on chance and your choices. This bonus will mainly depend on the lotteries you have chosen. A lottery is a bet that will increase your bonus by a certain amount or by zero based on a number randomly selected by the computer at the end of the study.

For example, the lottery: "1 in 7 chance of winning $7" will increase your bonus by $7 one time out of seven, and won't increase it six times out of seven.

You will have 6 opportunities to choose lotteries (one during the Discovery Stage and 5 others in the main part of the study).

You will choose each lottery from a menu. **A menu is a set of one or more lotteries in which you can choose one of the following only lottery.** Here is an example of choosing a lottery from a menu:

Your choice :

○ Having 1/3 of a chance to win $3
○ Having 1/9 of a chance to win $9

[I validate !]

lottery choice

Your preference for a menu

In this exercise, you will have to choose **under which conditions you would agree to exchange one menu for another**.

A first menu has been assigned to you. On the interface, this menu is displayed on the left.

Your task is to indicate from which amount you would accept to exchange this menu for another menu. On the interface, this other menu is displayed on the right.

Left Menu
- Having 1/3 of a chance to win $3
- Having 1/5 of a chance to win $5

Right Menu
- Having 1/7 of a chance to win $7
- Having 1/9 of a chance to win $9

I validate !

menus choice

You will find yourself in one of these 3 situations:

1. You think that the 2 menus are **equally good**. In this case, just place the cursor on **0** between the 2 menus.

2. You prefer to **keep** the menu that has been assigned to you. You can then move the cursor to the **right** to indicate the **minimum** amount. that you would like to **receive** to accept the exchange.

3. You prefer to **exchange** the menu assigned to you with the other menu. You can then move the cursor to the **left** to indicate the **maximum** amount. that you are willing to **pay** to exchange your assigned menu with the other menu.

After you validate your choice, the computer will choose a **random value**. This value will be used to find out if the computer agrees to make the exchange and for what amount. The value chosen by the computer is sufficient to make the exchange when :

- The computer is **ready to pay you more than you ask for**. You then receive the amount that the computer is willing to give you.
- the computer **ready to accept an amount smaller than you're willing to pay** to make the switch. You then pay the amount that the computer asks for.

In these two situations, **the exchange will take place** under conditions that are favourable to you. Indeed either you receive at least what you asked for the exchange – but you can also receive more – or you pay the maximum amount you indicated to keep the original menu – but you can also pay less.

Some examples:

- You have indicated that you would only agree to make the exchange if you receive at least $0,30 and the computer agrees to pay you $0,50 to make the exchange. In this case, the exchange will take place and you will receive $0,50.
- You have indicated that you would only agree to make the exchange if you receive at least $0,50 and the computer agrees to pay you $0,20 to make the exchange. In this case, the exchange doesn't take place, you keep your menu and you don't receive any money.
- You have indicated that you would only agree to make the exchange if you receive at least $0,40 and the computer agrees to make the exchange if you pay him $0,10. In this case, the exchange doesn't take place, you keep your menu and you don't receive any money.
- You have indicated that you would accept to pay a maximum of $0,20 to make the exchange and the computer agrees to make the exchange if you pay him $0,10. In this case, the exchange takes place and you lose $0.10 of your winnings.
- You have indicated that you would be willing to pay a maximum of $0.40 to make the exchange and the computer wants to receive 0,50€ to accept to exchange. In this case, the exchange does not take place, you keep your menu and you don't pay anything.
- You indicate that you would accept to pay a maximum of $0,30 to make the exchange and the computer agrees to pay $0,40 to make the exchange. In this case, the exchange will take place and you will receive $0,40.

With these examples you can make sure that if there is an exchange of menus, this exchange is done for an amount of money at least as interesting to you as the one you indicated.

- **If you prefer to keep your menu :** you will receive an amount higher or equal to the one you indicated if the exchange takes place.
- **If you prefer to exchange menu :** you will pay an amount lower or equal to the one you indicated if the exchange takes place (you can even receive money).

The amount you indicate is the amount you are willing to make the exchange for. If the exchange takes place, it will be at least for this amount, but it may be more advantageous for you.

The menus you will receive

During this study, you will receive 6 menus. The menus you will receive will depend on chance and the preferences you indicate:

- In the study, 42 pairs of menus will be offered to you (7 during the discovery stage and 35 in the main part of the study).
- Of these 42 pairs, 6 will be drawn at random (1 of the 7 pairs during discovery and 5 of the 35 choices during the main part of the study).
- For each of these 6 pairs of menus, you will receive a menu according to the preferences that you have indicated.
- In each of the 6 menus you will have received, you will choose a lottery.

Your total bonus will therefore be equal to the amount you will earn from your 6 lotteries plus or minus what you will have earned with the 6 pairs of menus that will have been selected.

Appendix C

Experimental instruction for chapter 4

Instruction of the experiment are display as follow:

Instruction

Welcome,

Please read the following instructions carefully. This task can make you win a bonus from 0$ to more than 30$.

Bonus

Your bonus will be calculated according to the lotteries that you will choose. You will choose 100 lotteries on a screen and one of them will be randomly picked by the computer. The randomly picked lottery will be played out and determine your bonus.

Lottery

You choose your preferred lottery by choosing a number between 0 and 64 in the corresponding box.

This number represents simultaneously the amount you can win and the probability you have to win.

The amount you can win will be the number you choose multiplied by 0.5$. The probability you have to win is (64 - n)/64, with n the number you choose. The higher the amount you can win, the lower the probability that you win. In order to help you, your chances of winning for the selected amount will be clearly indicated on the right hand side of your screen, next to the place where you input your choice.

An example is shown below:

your lottery   you have 73.44% chance to win 8.5$

| 17 |

lottery choice

This lottery, if drawn for payment, will give you the chance of winning $8.5 in 73.44% of cases and $0 in 16.56% of cases.

Task timeline

The task will proceed as follows:

1. **Questionnaire** We ask you to answer few question.
2. **Testing** You can then test how you will choose your lotteries and how the software interface will present the results on 10 lotteries. This test phase will not be relevant for the bonus.
3. **Bonus** You will then be able to choose the 100 lotteries that will allow you to increase your bonus.
4. **Result** You have to answer some additional questions and your results will be displayed along with instructions on how to fill in the necessary information to complete the Mechanical Turk Task.

We remind you that in case of disconnection, you can go back to where you were by entering your login and password on the login page.

If the instructions are clear to you, you can proceed to the next step by pressing the button below.

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