ZHIHUA LI

Irrationality: What, Why and How

Irrationality: What, Why and How

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Irrationeel: Wat, Waarom, en Hoe

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Preface

I pictured myself working as a middle level manager in the Chinese division of a transnational corporation, when I was an undergraduate majoring in Business Administration. My plan changed when I started my Mphil training at the Rotterdam School of Management. I thought a consulting company might be a better placement for me. I was wrong again. Almost five years after a course I took from Prof. Peter Wakker, I am now writing my PhD dissertation on behavioral economics. This story tells us that deciding your major before you enter college is actually not that important. Sometimes, we just overweight the stakes of the decision.

I first became acquainted with behavioral economics at Peter's lecture. I was fascinated by the anomalies behavioral economists found in people's decisions. One year later, I fortunately became a PhD student of Prof. Han Bleichrodt and Prof. Peter Wakker. Even today I am still grateful for their confidence in me when my only training in economics was two courses on Microeconomics and Macroeconomics.

Starting as an outsider, I benefited much from the excellent research environment Han and Peter provided. I cannot thank them enough for that. Han and Peter somehow make a perfect match for supervising PhD students. Peter tends to attend everything personally and take care of every detail of our research and life. Han is always there for us and supports us from behind the scene, setting up the environment. Besides, this thesis would not have come into existence without my daily supervisor Prof. Kirsten Rohde's help either. She was always there for me whenever I had some questions, work-related or not. My thanks also go to Prof. Aurelien Baillon for his generosity in sharing his experiences and wise suggestions. I also thank all of our group members: Martijn van den Assem, Arthur Attema, Ilke Aydogan, Dennie van Dolder, Yu Gao, Zhenxing Huang, Umut Keskin, Chen Li, Ning Liu, Rogier Potter van Loon, Julia Muller, Asli Selim, Jan Stoop, Uyanga Turmunkh, Tong Wang, and

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Chapter 1

Introduction

Before behavioral economics, people held the general impression that economics research is about utility, supply-demand, profits and markets. Economics is often associated with politics but nothing close to our everyday life. This changed when several books on behavioral economics made it to the non-fiction bestseller list. The merit of behavioral economics is that it stayes real. Behavioral economics studies how human beings actually behave without assuming that people are rational. The key tool of behavioral economics is decision-making.

We are facing various decisions in our life, from choice of career or life partner, to choice of travelling destination, to how much to save or which medical treatment to receive. The consequences of these decisions are closely related to the quality of our life. However, the truth is that even professional decision analysts do not have the universal model to get the perfect answer for every decision problem we have. One reason is that the dynamic world is way too complex to model. The other reason is that there are just too many different kinds of decisions. Some decisions are trivial daily life decisions such as which item on a menu to choose. Some decisions are of life changing importance such as career choices. The criteria of a good decision can heavily depend on your own tastes and preferences. However, behavioral economists don not care whether you decide to buy an iPhone 6 or a Samsung S5. But we do care if on the one hand you prefer iPhone 6 over Samsung S5 and iPhone 5s and on the other hand you choose Samsung S5 when it is compared with iPhone 6 and Samsung S3.

1.1 Rationality

It is all about rationality as far as behavioral economists are concerned. The topic of rationality will never lose its popularity among behavioral economists no matter how trite it may sound. The ultimate aim of behavioral economists is to guide people towards rationality and to improve their decision-making abilities. As with most popular phenomena, there is no lack of debates over rationality. Commonly speaking, a rational decision essentially means the optimal action or the best strategy for achieving a goal in a certain situation. However, there are different standards or criteria to evaluate optimality. Complex dynamic situations and underlying assumptions leave rationality even more difficult to define on general grounds. A consensus has not yet been reached among behavioral scientists (both economists and psychologists) as to what is rational. Not to mention the possible interpretations of rationality in sociology, political science, or philosophy.

According to classical economics, from value theory to utilitarianism, rationality may refer to the tradeoff between the value of a good perceived by consumers and the associated costs. Daniel Bernoulli (1738)'s theory of diminishing marginal utility to explain the St. Petersburg paradox facilitated the development of expected utility theory (EU). Later, EU was rationalized by von Neumann-Morgenstern's utility theorem(Neumann & Morgenstern (1947)), which together with its extension through Savage (1954)'s axioms provides the preference conditions for a rational agent who acts in a manner isomorphic to subjective expected-utility (SEU) maximizers when facing risk and uncertainty. SEU defines a rational agent by seven axioms, among which completeness, transitivity, independence and continuity are often used to evaluate the rationality of people's behavior when under risk. Any deviations from the preference axioms are deemed to be irrational. The term 'homo economicus' is often used to refer to a rational decision maker.

Still, in real life decisions people tend to consistently deviate from EU's predictions. It was not for long before the advanced concept of rationality was subject to more and more doubts raised by both economists and psychologists. The main issue is about the descriptive power of EU. Empirical tests were conducted and consistent violations of EU were found (see for example Starmer (2000) and Trautmann and van de Kuilen (2014)). Bounded rationality was proposed by Herbert A. Simon (1955) to capture and justify human being's limited cognitive ability and available resources in the decision-making process. With bounded rationality, the person who makes the optimal decision with given limited information is also considered to be rational.

Prospect theory was proposed to describe real life choices (Kahneman & Tversky (1979)). In this theory, people are allowed to violate the expected utility axioms.

Following this line of thought, some economists take transitivity and monotonicity as the borderline to assess rationality. However, even transitivity has been questioned by some researchers (Loomes & Sugden (1982)), who argued that intransitivity can be rational.

1.2 Irrationality

With the unsettled controversies over rationality, it may help to see it from a different angle. Many behaviors are generally accepted and agreed to be irrational, despite the continuing debates on rationality. Psychological researchers are excellent at identifying anomalies in our decision-making processes, such as the status quo bias, the confirmation bias, and the small sample bias, just to name a few. This thesis investigates three research questions of irrationality.

1.2.1 What

What to be recognized as irrationality?

Before we set hands to correct irrationality, we first need proper tools to detect it. The general method that we will use to observe irrationality from people's behavior is called revealed preference. Revealed preference theory assumes that the preference of a consumer can be observed from their purchasing behavior (Samuelson (1938)). It defines utility by observing behavior. In 1954, Savage suggested that real decision situations for a decision maker to choose from should be used to derive the decision maker's real preferences. This view has led to the implementation of real incentives in economic experiments. Real incentives are, unfortunately, not omnipotent. When real incentives are incorrectly implemented, the preferences derived are no longer accurate. Identification of irrationality from such revealed preferences is meaningless, since the irrationality could be due to the improper incentives used.

Chapter 2 introduces the prior incentive system (Prince), a new system for implementing real incentives in choice experiments. Prince combines the efficiency and tractability of matching questions with the transparency and validity of binary choice questions. Thus Prince revives matching, mainly by making the Becker-DeGroot-Marschak mechanism(Becker et al. (1964)) transparent. By reconciling matching and choice, Prince resolves the classical preference reversals. It reduces and resolves a number of problems of current incentive systems: (a) The income effect; (b) the reliance on isolation; (c) strategic behavior for adaptive experiments. Its incentive compatibility is clearer to subjects than was possible before. Not only do we avoid any deception of subjects, but, moreover, every subject verifies so during the experiment. We demonstrate the general implementability of Prince by applying it to standard preference measurements.

What to be corrected of irrationality?

Even though irrationality is better recognized than rationality, there are still a number of disagreements about it. One of the main research interests in behavioral economics today concerns decisions under ambiguity, that is, decisions in which probabilities of uncertain events are unknown. Some researchers believe in a normative status for decisions under ambiguity that deviate from expected utility, and seek to adopt normative models to explain such decisions under ambiguity. In Chapter 3, we show that ambiguity is not just about unknown probability but it is also associated with beliefs and emotions. Ambiguity aversion cannot be normatively modeled. We investigate ambiguity attitudes in five different contexts, systematically investigating the dependence on outcomes and events and comparing these dependencies. Our findings support event dependence of ambiguity attitudes over outcome dependence, thus supporting event-based theories such as multiple priors, Choquet expected utility, and prospect theory over outcome-based theories such as the smooth model. Besides aversion, insensitivity plays a big role in ambiguity attitudes. We further compared the suitedness of parametric families to capture ambiguity attitudes. For ambiguity more than for risk, families work best if they incorporate insensitivity (inverse-S) properly.

1.2.2 Why

Why steer people away from irrationality?

This question may sound trivial, because being more rational is naturally desired by most of us. Attempts at prescriptive improvements of decisions can lead to paternalism though. Proponents of consumer sovereignty will argue to leave people alone who behave irrationally. And, consequently, ethical and moral objections can be raised against paternalism. A central question in many debates on paternalism is whether a decision analyst can ever go against the stated preference of a client, even if merely intending to improve the decisions of the client. Using four gedanken-experiments, Chapter 4 shows that this central question, so cleverly and aptly avoided by libertarian paternalism (nudge), cannot always be avoided. The four thought experiments, while purely hypothetical, serve to raise and specify the critical arguments in a maximally clear and pure manner. The first purpose of Chapter 4 is, accordingly, to provide a litmus test on the readers' stance on paternalism. We also survey and organize the various stances in the literature. The secondary purpose of Chapter 2 is to argue that paternalism cannot always be avoided and that consumer sovereignty cannot always be respected. However, this argument will remain controversial.

1.2.3 How

How to correct irrationality?

Chapter 5 investigates the effects of predicting choices made by others on own choices, following up on promising first results in the literature that suggested improvements of rationality and, hence, new tools for nudging. We find improvements of strong rationality (risk neutrality) for losses, but not for gains. There are no improvements of weak rationality (avoiding preference reversals). Overall, risk aversion increases. As for the effects of own choices on predictions, the risk aversion predicted in others' choices is reduced if preceded by own choices, both for gains and losses. We consider four psychological theories of risk: learning, construal level theory, risk-as-feelings, and risk-as-value (combined with anchoring). Our results support risk-as-value combined with anchoring and can be reconciled with risk-as-feelings. Relative to preceding studies, we add real incentives, obtain pure framing effects, and use simple stimuli that were maximally targeted towards our research questions.

Chapter 2

Prince: An Improved Method for Measuring Incentivized Preferences[1]

2.1 Introduction and background

Behavioral economics posed many challenges to the classic revealed preference paradigm in economics. Many challenges were handled by incorporating irrationalities in decision models, as for instance in Tversky & Kahneman's (1992) prospect theory. Experimental economists initially pointed to flaws in the original experiments conducted by behavioral economists: lack of real incentives, insufficient learning opportunities, and sometimes deception, to defend the revealed preference paradigm.

Preference reversals (Lichtenstein & Slovic (1971)) however, were a more fundamental challenge - the first signs in the literature of a discrepancy between choice and matching, calling into question the very existence of preferences. And these fundamental challenges (Grether & Plott (1979)) were endemic in more carefully conducted experiments.

Although some authors blame choice-based procedures (Fischer et al. (1999)). preference reversals more often are blamed on matching techniques, in which subjects directly indicate indifference values (reviewed by Attema & Brouwer (2013)). As a result, binary choices are now the most common way to measure preferences, with indifferences derived indirectly from switching values in choice lists. Binary choices

[1]This chapter is based on the paper "Prince: An Improved Method for Measuring Incentivized Preferences" co-authored with Cathleen Johnson, Aurélien Baillon, Han Bleichrodt, Dennie van Dolder, and Peter P. Wakker

also have drawbacks. They are more cumbersome to administer and they give interval rather than point estimates. They also have their own biases.[2]

A perennial difficulty in economic experiments is that real incentives as implemented in the laboratory are decontextualized and accordingly hard to understand for subjects. The problem is greatest for matching, where the Becker-DeGroot-Marschak (1964) mechanism (BDM) is often criticized for this reason.[3] Both choice and matching experiments commonly involve more than one decision. Incentivizing all decisions leads to income effects. For this reason the random incentive system (RIS; first proposed by Savage (1954, p. 29)) is now commonly employed. In this system, only one of the experimental decisions, randomly selected at the end, is implemented for real. If subjects, for each experimental decision, condition on this being the only real one (isolation), then incentive compatibility follows. However, subjects may conceive of the set of decisions as a meta-lottery (Holt (1986)) where, for instance, some decisions can be used to hedge others, and spillover effects can result.

The prior incentive method (Prince), introduced in this paper, reduces and avoids the aforementioned problems by combining and improving a number of features from existing incentive systems, particularly the random incentive system (RIS), the BDM, and Bardsley's (2000) conditional information system. In brief, the choice question (rather than a choice option) implemented for real is randomly selected *before* rather than after the experiment, is provided to the subjects in a tangible form (for example in a sealed envelope), and subjects' answers are framed as instructions to the experimenter about the real choice implemented at the end. Incentive compatibility can now be crystal clear, not only to homo economicus but also to homo sapiens, and isolation is maximally salient.

Prince combines the tractability and precision of matching with improvements to binary choice's clarity and validity. Thus, Prince reinvents matching as the superior mechanism for measuring preferences. Further, for adaptive experiments (where the sequence of questions is path dependent) subjects cannot answer strategically, and subjects know this. We thus resolve the incentive compatibility problem for adaptive experiments. Wakker & Deneffe's 1996 tradeoff method (TO) for measuring utility under ambiguity now becomes available to experimental economists in properly incentivized form.[4] Finally, not only does Prince avoid deception, but, moreover,

[2]These biases have an older history in psychophysics (Gescheider (1997, Ch. 3)). From the beginning (Fechner (1860)), psychophysicists used binary comparisons besides matching to measure subjective values. The Nobel laureate Békésy (1947) introduced bisection ("the staircase method"), to avoid the biases in choice lists ("limiting methods").

[3]See, for instance, Bardsley et al. (2010, p. 271 ff.), Camerer (1995, p. 695 ff.), Noussair et al. (2004), and Seidl (2002, p. 630 ff.).

[4]An advantage of the tradeoff method is that utility is not affected by probability weighting,

nondeception is verifiably transparent to the subjects. Throughout this paper, to illustrate Prince's novelty, we use standard experiments with classical stimuli.

For non-experimentalists, our improved measurements of preferences shed new light on general economic concepts. First, utility is closer to linear than traditionally thought. Second, ambiguity attitudes display likelihood insensitivity besides the well-known aversion. Third, Prince helps reveal which known choice anomalies reflect genuine deviations from homo economicus and which are artifacts of measurement problems. Our tests confirm that preference reversals were due to measurement problems. They do not reflect genuine intransitivities. Prince does not, however, resolve the endowment effect, which may reflect genuine preference and, hence, a robust discrepancy between homo sapiens and homo economicus that is no mere artifact. Here we disagree with Plott & Zeiler (2006). We emphasize that Prince improves measurements of preferences but not preferences themselves. The endowment effect may be irrational but is not an appearance created by Prince.

All stimuli and material of our experiments not presented in the main text or appendix is in the Web Appendix, in particular in part WE there.

2.2 Prince explained

This section introduces the Prince system. We explain its principles in the first two subsections, and define them formally in section 2.2.3. Discussion is in section 2.6 and section 2.7.

2.2.1 Prince defined

The experiment begins with a *real choice situation* (RCS) selected from a set of possible choice situations for each subject. In our experiments the RCS is written on a slip of paper and put in a sealed envelope (following Bardsley (2000, p. 224)). The RCS describes a number of choice options (two in our experiments). The subject will receive one of these options and her goal in the experiment is to get the most preferred one. Although the subject does not know her particular RCS, she does receive some information such as the average, maximum, and/or minimum outcomes. The partial description about the RCS is constructed so that each choice situation considered during the experiment can possibly be the RCS. The subject need not know the exact probabilities of the latter possibility, and such probabilities need not be uniform, but they should be salient enough to motivate subjects to truthfully answer the experimental questions(Bardsley et al. (2010, p. 220)). It is important

so that the measurements are valid for virtually all (non)expected utility theories. In particular, collinearities between utility and probabilities are avoided.

that the slip of paper in the selected prior envelope describes the entire RCS, with *all* choice options available (section 2.7), not just one choice option.

During the experiment, various possible choice situations (candidates to be the RCS) are presented to the subject. We explicitly ask subjects to give ”instructions” about the real choice to be implemented at the conclusion of the experiment. This real choice is concrete with the envelope in hand. At the end of the experiment, the experimenter opens the prior envelope and uses the instructions provided by the subject to select the desired option. We never ask “what would you do if” referring to unspecified choice situations. A script with statements such as “If you write what you want then you get what you want,” or “If you give wrong instructions, then you don’t get what you want” further emphasize the connection between decision and outcome. Hence, incentive compatibility is crystal clear to the subjects.

2.2.2 Prince for adaptive experiments: Problems and solutions

In adaptive experiments, stimuli depend on subject responses to previous stimuli. If traditional RISs are used, subjects may benefit (or think they benefit) from answering a question untruthfully so as to improve future stimuli. Such gaming is impossible with Prince, and this is obvious to the subjects, because the RCS has been determined prior to the experiment.

For adaptive experiments, experimenters will not know exactly which choice situations will occur during the experiment. This raises two *overlap problems.*

1. The *indeterminacy overlap problem* entails the possibility that none of the instructions from the subject pertain to the RCS, leaving the choice from the RCS unspecified. This solution is simple: subjects may choose between the options in the prior envelope on the spot.

2. The *exclusion overlap problem* arises if the partial information about the RCS excludes some choice situations generated during the experiment, thereby reducing salience and motivation for truthfulness in these excluded choice situations. To combat the exclusion overlap problem, experimenters must frame the partial information concerning the RCS by anticipating the range of possible choice situations generated in the experiment. They do this by using descriptive theory and pilots. For example, in our adaptive experiment (section 2.5) we informed subjects about a large possible outcome (>€3000). Choice situations with very large monetary amounts could arise in our experiment, depending on subjects’ answers.

2.2.3 Prince summarized

We now formally list the principles that define Prince.

1. [**Priority**] The RCS is determined at the start, *before the subject made any decision.*

2. [**Tangibility**] A description of the RCS is handed out to the subject in *tangible* form, such as in a sealed envelope (the *prior envelope*).

3. [**Wholeness**] The description handed out to the subjects describes the entire RCS and not just one option (such as an option is for instance a random price in BDM).

4. [**Concreteness of situation and procedure**] During the experiment, we never refer to abstract hypothetical choice situations in an unspecified situation or procedure ("what would you prefer if . . . "). We always refer to the RCS in the envelope and to the action of the experimenter ("which option should the experimenter select from your envelope . . . ").

5. [**Instructions to experimenter**] We explicitly request "instructions" from the subjects, asking them about what to select from their envelope, rather than asking the vaguer "what would you prefer if."

6. [**No indeterminacy**] For adaptive experiments: If subjects do not give instructions during the course of the experiment regarding the RCS, then they can choose on the spot, after the envelope has been opened.

7. [**No exclusion**] For adaptive experiments: The initial description about the RCS should be framed so as not to exclude potential choice situations faced during the experiment.

While parts of Prince have been used before (section 2.6), their integration into Prince is new, and is necessary to ensure the proper conditioning and transparency of incentive compatibility. For example, all BDM implementations that we are aware of violate Principle 3 [wholeness], leading subjects to condition the wrong way (enhancing rather than avoiding meta-lottery perceptions; section 2.7), which is arguably the main reason for BDM's bad performance.

In our experiments, not only the prior envelopes, but all stimuli are physical. We use no computers although this not essential for Prince, which is why it was not listed as a principle. Other researchers may prefer computerized implementations of Prince. The physical availability of the RCS to every subject such as in a prior envelope is essential though, which is why it is listed as (2) above.

We avoid deception and, hence, the partial information about the RCS provided must be true. Although it is not a defining principle of Prince, in our implementation subjects could completely verify the absence of deception. First, before they received the real payment, they could verify the correctness of the information provided about the stimuli (Web Appendix E). Second, unlike computer randomizations, our physically generated randomizations were fully verifiable and carried out by the subjects themselves.

2.3 Experiment 1: Reviving matching (WTA)

Experiment 1 implements PRINCE for one of the most used value concepts for non-market goods: willingness to accept (WTA). We measured WTA for a university mug that could be bought on campus for €5.95. Mugs are suited to test the endowment effect (tested later) because people quickly develop attachments to them. WTA measures how much money a subject would accept in lieu of the mug, which according to traditional theories should be the mug's cash equivalent.

N = 30 subjects (40% female), recruited from undergraduates in the School of Economics, Erasmus University Rotterdam, the Netherlands, participated in one classroom session. Advertisement of the study promised a €10 show-up fee plus either a mug or additional money. Participants immediately received a mug (*endowment*) along with the show-up fee.

Next, the experimenter presented 50 sealed envelopes, visibly numbered 1-50. These were separated into 5 piles of 10 each (1-10, ..., 41-50). Five subjects each checked one pile to verify that each number between 1 and 50 occurred once. The subjects placed the envelopes into a large opaque bag, shuffled them, and randomly redistributed them over a number of smaller bags (one for each row in the classroom). Each subject, in turn, randomly took one envelope, the prior envelope, from a bag (without replacement). Subjects were told that their envelope described two options, and that at the end of the session we would give them one of those two, based on instructions that subjects would give us.

Subjects received a questionnaire reproduced in Figure 2.1, and were given a short written explanation along with a PowerPoint presentation on the procedure. They were told that they could give up the mug for a price: "You will write instructions, for each possible content of your envelope (for each money amount), which of the two options you want. At the end, we will give you what you instructed. ...If you write what you want, then you get what you want!" We call the question in Figure 2.1 Question 1 for later comparisons with Experiment 2.

At the end of the experiment, subjects handed in their questionnaire (instructions

In each of the 50 envelopes, one option is to keep your mug, and the other option is to give up your mug for a money amount. The note in each envelope is as follows.

Option 1: Keep your mug
Option 2: Give up your mug for €x

The money amount x varies between €0 and €10 in different envelopes. Five of the envelopes contain a randomly generated amount between €0 and €1, five envelopes contain a randomly generated amount between €1 and €2, five contain a randomly generated amount between €2 and €3, and so on, with finally five envelopes containing a randomly generated amount between €9 and €10. Thus the amount in your envelope can be any amount, in cents, between €0 and €10.

Please give us instructions, for each possible envelope that your envelope may be, whether we should let you keep your mug, or we should give you that money amount in exchange for your mug. Do so by specifying a threshold (in cents).

My threshold is € ……,……

If the money amount x in my envelope is equal to or above the threshold, then give me that money amount in exchange for my mug.

If the money amount x in my envelope is below the threshold then let me keep my mug.

Figure 2.1 Instructions for WTA with matching and prior endowment (Question 1)

to experimenter). An experimenter opened their envelope, observes the real choice situation specified in the envelope, and follows the instructions in the questionnaire.

Result. The average WTA was 4.99 (SD 2.41). Further results are in section 2.4.3.

Discussion of providing range 0-10 for answers. Whereas specifying a range cannot be avoided for choice lists, it is optional for matching. We chose to specify it here, but for comparison will not specify it later in Questions 5 and 6 in section 2.4.5. There are pros and cons (Birnbaum (1992)). We chose the range to facilitate comparability with choice lists presented later.

2.4 Experiment 2: Prince implemented in a large experiment

In Prince, only one choice is implemented for real. Experiment 2 shows how Prince can nevertheless be used in large experiments with many different measurements.

2.4.1 General procedure

N = 80 subjects (41.2% female) recruited from undergraduate students in School of Economics, Erasmus University Rotterdam, the Netherlands, were randomly divided into two groups. Each group participated in one classroom session. They received a €10 show-up fee and could gain an additional offering: money, mug, or chocolate. Experimental instructions including a short presentation were given by the experimenter (Web Appendix WE). For each of the two sessions there were 90 envelopes, numbered 1-90 in random order. As with Experiment 1, these envelopes were separated into piles of 10, checked by subjects, shuffled, explained, and randomly distributed without replacement.

The two groups of subjects received different versions of the first question, *1-match* or *1-choice* (see Figures 2.2 and 2.3). These questions were part of a between-subject test in this large experiment. Question 1 appears in Experiment 1. The remaining eight questions, 2-9, were asked in randomized orders to all subjects in the two groups. Each of the nine questions corresponded to a type (the term used with subjects) of envelope, and there were 10 envelopes of each type (one for each question asked of the subjects). The numbering (1-match/choice, 2,3,. . . ,9) of types/questions used in this paper was not communicated to subjects. Thus each subject randomly drew an envelope, their prior envelope containing their RCS, from 90 envelopes and then gave 9 instructions in response to 9 types/questions.

At the end of the experiment, each subject handed in their questionnaire bundle and prior envelope. An experimenter opened the envelope, searched for the instruction in the questionnaire made operational by the RCS, and carried it out.

2.4.2 The endowment effect

Question 1-match measured subjects' WTA for a mug, now without endowment. It was asked of 41 of the 80 subjects. Figure 2.2 presents Question 1-match. Results and discussion are at the end of section 2.4.3.

Instructions for envelopes of type γ

In each of the 10 envelopes of type γ, one option is the mug you just saw, and the other option is a money amount. The note in each envelope of type γ is as follows.

Type γ

Option 1: The mug

Option 2: €x

The money amount x varies between €0 and €10 in different envelopes. One of the envelopes contains a randomly generated amount between €0 and €1, one envelope contains a randomly generated amount between €1 and €2, one contains a randomly generated amount between €2 and €3, and so on, with finally one envelope containing a randomly generated amount between €9 and €10. Thus the amount in your envelope can be any amount, in cents, between €0 and €10.

Please give us instructions, for each possible envelope of type γ that your envelope may be, whether we should give you the money amount or the mug. Do so by specifying a threshold (in cents).

My threshold is € ......,......

If the money amount x in my envelope is equal to or above the threshold, then give me that money amount.
If the money amount x in my envelope is below the threshold then give me the mug.

Figure 2.2 Instructions for cash equivalent with matching and no prior endowment (Question 1-match)

2.4.3 Matching versus choice lists between subjects

Question 1-choice repeated Question 1-match, again with no endowment, but now using choice lists instead of matching, for the remaining 39 subjects. Figure 2.3 Question 1-match. The sure amount of money (the alternative to the mug) increases with each option presented. At first, nearly all subjects preferred the mug, but by the end nearly all subjects preferred the money. Somewhere, they switched, and the midpoint between the two money amounts where they switched was taken as their indifference point.

An inconsistency results if a subject takes the money when the money offer is small but then switches to the mug when more money is offered. We allowed such inconsistencies so as to be able to detect subjects' misunderstandings, providing information about the transparency of Prince.

Result of Questions 1, 1-match, and 1-choice. In the 119 choice lists presented in this experiment (39 subjects here and all 80 subjects in section 2.4.4), there was only one inconsistency-that is, only one switch in the wrong direction (by subject 59). In otherwise comparable studies, typically 10% of subjects have inconsistent switches (Holt & Laury (2002)). Because this one subject exhibited other anomalies as well (violating stochastic dominance in a later question), we removed her from our analyses. Leaving her in would not alter our results. Table 2.1reports some statistics, and Table 2.2 reports tests.

	Groups	N	Mean	Std. Deviation
Experiment 1	Question 1	30	4.99	2.41
Experiment 2	Question 1-match	41	3.19	1.96
	Question 1-choice	39	3.61	2.51

Table 2.1 Statistics for Questions 1 (matching with prior endowment), 1a (matching without prior endowment), and 1b (choice list without prior endowment).

Questions	Treatment	Mean difference	t	Df.	p-Value
1—1-match	prior endowment or not	1.81	3.48	69	0.001
1-match—1-choice	matching versus choice	-0.43	-0.86	78	0.40

Table 2.2 Test of equality of means.

Discussion. Prince confirms the endowment effect.[5] Rational or not, it reflects a genuine property of preference(Brosnan et al. (2012); Korobkin (2003, p. 1244)), not

[5]See Camerer (1995, p. 666 ff.) and Schmidt & Traub (2009) and its references.

Instructions for envelopes of type δ

In each of the 10 envelopes of type δ, one option is the mug you just saw, and the other option is a money amount. The money amount x varies between €0.50 and €9.50 in different envelopes (see below).

The note in each envelope of type δ is as follows.

Type δ

Option 1: the mug

Option 2: €x

In the following list, each line describes the content of one envelope of type δ. On each line, cross out the square before the option that we should give you if that line describes the two options in your envelope.

1.	□MUG	□€0.50
2.	□MUG	□€1.50
3.	□MUG	□€2.50
4.	□MUG	□€3.50
5.	□MUG	□€4.50
6.	□MUG	□€5.50
7.	□MUG	□€6.50
8.	□MUG	□€7.50
9.	□MUG	□€8.50
10.	□MUG	□€9.50

Figure 2.3 Instructions for cash equivalent with choice list and no prior endowment (Question 1-choice)

a bias in measurement.

Prince corrects errors of measurement and, thus, resolves the discrepancy between choice and matching. Our matching questions are very similar to the choice questions, directly referring to the choice in the prior envelope held in hand. Accordingly, their equality is no surprise. Our contribution here is of a methodological nature: we made matching look like choice, combining the virtues of both.

The test of choice versus matching presented here was between subjects. For its result, based on a null accepted, to be convincing, statistical power should be sufficient. It is, because, first, the endowment effect is very significant. Second, in section 2.4.4 we confirm our finding in a within-subject test for 80 subjects, increasing power. Several tests reported later confirm the null accepted.

2.4.4 Matching versus choice lists within-subjectss

Questions 2 and 3 replicate Questions 1-match and 1-choice with chocolate (price €6.25) instead of a mug. Chocolates and mugs were used by Kahneman et al. (1990), and many follow-up studies. Here we follow suit. Questions 2 and 3 were asked to each subject, allowing within-subject comparisons. The stimuli are in Web Appendix WB. The average cash equivalent was 3.31 for matching and 3.26 for the choice list (t_{79} = 0.28, p = 0.78), confirming the null hypothesis of equality.

2.4.5 Testing preference reversals

We used Prince to test the classical preference reversal of Lichtenstein & Slovic (1971). Details are in Web Appendix WA. For Question 4, the choice question, we used an analog of Figure 4.1 without the description of x. Option 1 was $4_{0.97}0$ (receiving €4 with probability 0.97 and €0 otherwise), called *P-bet* in the literature because the gain probability is high. Option 2 was $16_{0.3}10$ called the *\$-bet* because it has a high minimum possible gain (in dollars when receiving its name; Lichtenstein & Slovic (1971)). We also measured their cash equivalents in Questions 5 and 6, again using analogs of Figure 2.2, but without ranges for amount x, writing only "The amount x varies between the envelopes." Although in consequence almost nothing is known about x's randomness, that affects neither the compatibility nor the transparency of incentives.

Normal preference reversals (higher CE of the \$ bet but, paradoxically, choosing the P bet) occurred for 11% of the subjects, and the opposite preference reversals (higher CE of the P bet but choosing the \$ bet) happened for 7% of the subjects. These percentages are not significantly different (p = 0.55) and are infrequent enough to be explained as random choice inconsistencies (Schmidt & Hey (2004)). We find no evidence of genuine preference reversal.

Our finding deviates from other studies of preference reversals, where normal preference reversals are found in large majorities (surveyed by Seidl (2002)). Preference reversals reflect errors in the measurement of preferences (procedural variance) rather than genuine properties of preferences such as intransitivities (Tversky et al. (1990)). Prince restores consistency between choice and matching, thus resolving preference reversals.

2.4.6 Measuring subjective probabilities and ambiguity attitudes

Using questions 7, 8, and 9 we replicate the measurements of subjective probabilities and ambiguity attitudes by Baillon & Bleichrodt (2011, Study 1). They used classical choice lists, but we use Prince and matching. Details are in Web Appendix WC. We measured the probability p such that

$$10_E0 \sim 10_p0, \tag{2.1}$$

E denotes an event explained as an observation from the Dutch AEX stock index, and 10_E0 means that the subject receives €10 if E happens, and nothing otherwise. 10_p0 means that the subject received €10 with objective probability p. The probability p giving the preceding indifference is called the *matching probability* of event E, denoted m(E). We measured it for three events:

E = A (Question 7): The Dutch AEX stock index increases or decreases by no more than 0.5% during the experiment.

E = B (Question 8): The Dutch AEX stock index increases by more than 0.5% during the experiment.

E = A∪B (Question 9): the AEX stock index decreases by no more than 0.5% during the experiment.

Our presentation of questions was similar to Figure 2.2, with option 1 being 10_E0 and option 2 being 10_p0, requesting that a threshold for p (instead of x) be specified. Baillon and Bleichrodt showed how we can use these observations to analyze ambiguity attitudes, using a nonadditivity index $m(A) + m(B) - m(A \cup B)$. We replicated all their findings. In particular, the nonadditivity index was mostly positive, rejecting expected utility, and confirming Tversky & Fox (1995) subadditivity and Abdellaoui et al. (2011) a(mbiguity-generated likelihood)-insensitivity. These properties are genuine properties of preferences and not artifacts of measurement. Hence Prince did not remove them. Validity is confirmed because we found the same phenomena on subjective probabilities as other experimental studies did. Here, as

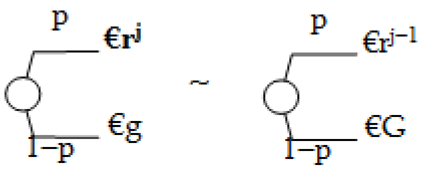


Figure 2.4 Tradeoff method indifferences

throughout, the advantage of Prince is that we obtained our results more quickly (using matching instead of choice) and more precisely than preceding papers did.

2.5 Experiment 3: Prince implemented in an adaptive experiment; measuring utility

We use an adaptive method to measure utility and show how Prince can resolve incentive compatibility problems by ruling out strategic answering. Exact stimuli, instructions, and details are in Web Appendix WE. We first piloted the following procedures in two sessions, each with approximately 10 graduate students from ERIM research institute of Erasmus University. These students had considerable exposure to decision theory. After the pilot, as an assignment, they were tasked with criticizing the procedures, especially in ways the experimenter could deceive or manipulate. They were unsuccessful in their attempts to find weaknesses in the procedures.[6] These students, as well as colleagues in informal pilots, confirmed procedural transparency and absence of biases.

We will use Wakker & Deneffe (1996) adaptive tradeoff (TO) method to measure utility. This method is robust to violations of expected utility and provides a correct utility function irrespective of whether a subject maximizes expected utility, prospect theory, or most other nonexpected utility theories. Implementations were as yet not incentive-compatible, and adding incentive compatibility is our contribution here. Integrating incentive compatibility makes the method suited for economics.

2.5.1 The preferences to be elicited for the tradeoff method

We measure indifferences $r^j_p g \sim r^{j-1}_p G$, j = 1, . . . 4 (Figure 2.4, with the conventional notation for bets using circles as chance nodes). Consistent with the notation used in the stimuli of the experiment, superscripts indicate the outcomes r^j.

[6]There were humorous suggestions such as "pull the fire alarm just when you have to pay €3000."

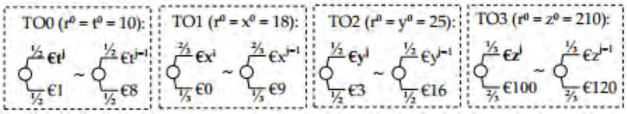


Figure 2.5 The values used for $TO_0 - TO_3$; j=1,. . . , 4.

The experimenter chooses some pre-set values $0 < p < 1, G > g > 0$ (gauge outcomes), and $r^0 > G$. Then the bold-printed $\mathbf{r^1}, ...\mathbf{r^4}$ are elicited sequentially from each subject over four stages. The experiment is adaptive because r^1, r^2, and r^3, after having been elicited, serve as input to the next question in stages 2, 3, and 4. We assume a *weighted utility* model:

$$\text{for } x \geq y, x_p y \text{ is evaluated by } \pi U(x) + \rho U(y)(\pi > 0, \rho > 0) \tag{2.2}$$

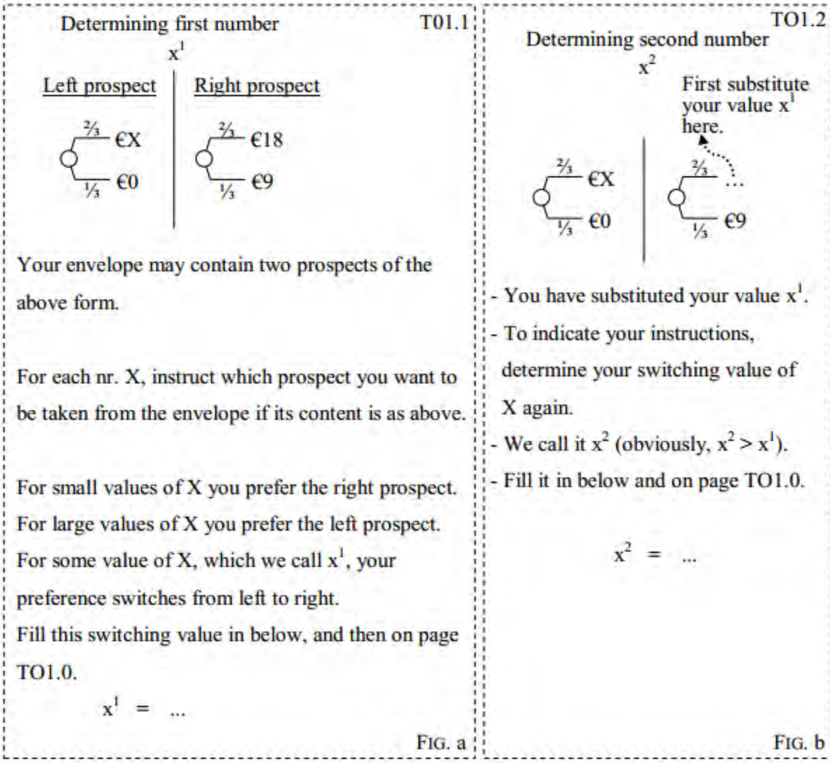
This model includes expected utility, prospect theory for gains Tversky & Kahneman (1992), and most other generalizations of expected utility (Wakker (2010, §7.11)). Algebraic manipulations show that the $r^j s$ are equally spaced in utility units under Eq. 2.2 (Wakker (2010, §4.3, §7.11, §10.6)):

$$U(r^4) - U(r^3) = U(r^3) - U(r^2) = U(r^2) - U(r^1) = U(r^1) - U(r^0). \tag{2.3}$$

A nonparametric measurement of utility results (section 2.5.5, section 2.5.7) that is valid for virtually all risky choice theories. The observations can be used for parametric fitting (section 2.5.6, section 2.5.8). The TO method avoids collinearity between utility U and probability weighting (π and ρ in Eq. 2.2): Eq. 2.3 is not affected by the probability weights π and ρ, and we need not even estimate them. For other measurements of prospect theory in the literature, collinearity is a serious problem (demonstrated by Zeisberger et al. (2012, p. 366 ff.)).

We carried out the TO measurement with four sets of pre-determined values, one training set and three observational sets: TO0 (with t^j playing the role of r^j, t means training), TO1 (with x^j for r^j), TO2 (with y^j for r^j), and TO3 (with z^j for r^j) depicted in Figure 2.5.Wakker & Deneffe (1996) used the same stimuli but scaled up and choices were hypothetical.

The Figure 2.6 displays the first two questions, TO1.1 and TO1.2, of the TO1 quadruple, as presented to the subjects. Question TO1.2 immediately followed TO1.1 on a separate page. Not only is the experiment adaptive, but also it is obviously so to subjects. Each subjects had to impute the answer they gave to the first question $x^1 (= r^1)$ before answering the next question (determining r^2). The third and fourth questions were like the second, requesting information of the previous answer.



Determining first number x^1 T01.1

Left prospect | Right prospect

⅔ €X, ⅓ €0 | ⅔ €18, ⅓ €9

Your envelope may contain two prospects of the above form.

For each nr. X, instruct which prospect you want to be taken from the envelope if its content is as above.

For small values of X you prefer the right prospect.
For large values of X you prefer the left prospect.
For some value of X, which we call x^1, your preference switches from left to right.
Fill this switching value in below, and then on page TO1.0.

x^1 = ...

FIG. a

TO1.2

Determining second number x^2

First substitute your value x^1 here.

⅔ €X, ⅓ €0 | ⅔ ..., ⅓ €9

- You have substituted your value x^1.
- To indicate your instructions, determine your switching value of X again.
- We call it x^2 (obviously, $x^2 > x^1$).
- Fill it in below and on page TO1.0.

x^2 = ...

FIG. b

Figure 2.6 Figures used in the tradeoff method.

2.5.2 Procedure and real incentives

We used Prince in two one-hour, pen and paper sessions (25 and 55 subjects) with performance-contingent incentives. Subjects were undergraduate students of Erasmus University in Rotterdam who were enrolled in an economics class. They received a €5 show-up fee in addition to their performance-based payoff. They first chose a sealed envelope with their RCS. Then they received written instructions, accompanied by an explanatory PowerPoint presentation.

Subjects filled out the training questions of TO0, jointly and simultaneously, exactly as in Wakker & Deneffe (1996), guided by the PowerPoint presentation. Subjects wrote their answers on pp. TO0.1-TO0.3, which they kept, but also on the front page TO0.0, which they tore off and gave to the experimenter at the end of the experiment. We explained how the performance payment procedure worked, and how subjects' answers to the questionnaire would operationalize the selection from the RCS in their envelope. Only then did subjects receive the three sets of questions TO1, TO2, TO3 (ordered randomly, subject-dependent), which they completed at their own pace. Three subjects in the first group, and six in the second, were randomly selected for real play at the end.

2.5.3 Construction and use of envelopes for real incentives, and avoiding the two overlap problems

In preparation for each session, we constructed 100 envelopes, from which each subject would randomly choose one (without replacement). Each envelope contained a slip with two bets written on it (the RCS). We used popular theories of risky choice, mostly expected value and prospect theory, and pilot studies to determine the contents of the envelopes that minimize both overlap problems. Because the details depend on particularities of the experiment, we present them in Appendix A.

2.5.4 Experiment with hypothetical choice

We did two sessions (10 and 44 subjects) with hypothetical choice. Subjects were unaware that other subjects played for real incentives. There was no role for Prince techniques here, as no incentives needed explaining or implementing. We only describe the differences with the incentivized experiment. Subjects received €10 for participation. They made less on average than the real incentive condition but the session took less time. The results that follow concern the incentivized sessions, unless stated otherwise.

2.5.5 Non-parametric analysis

An advantage of the TO method is that we can infer the utility function nonparametrically, i.e., without a commitment to any family or shape of utility functions (Wakker (2010, §9.4.2)), using Eq. 2.3. Figure 2.7 will present utility graphs generated by average answers.

To develop a nonparametric test of concavity, note that for strictly concave utility we have (with r = x, y, or z, respectively)

$$r^{i+2} - r^{i+1} > r^{i+1} - r^i \tag{2.4}$$

for all i, and for strictly convex utility we have

$$r^{i+2} - r^{i+1} < r^{i+1} - r^i \tag{2.5}$$

for all i.
We classified a subject's utility as concave if Eq. 2.4 was satisfied more often than Eq. 2.5, and as convex if the opposite held, with Eq. 2.5 satisfied more often than Eq. 2.4. The remaining subjects were irregular or linear.

2.5.6 Parametric analysis

Whereas the TO method allows nonparametric utility analyses, it can also be used for parametric analyses. We used Eq. 2.2 with the two most common parametric utility families, CARA (constant absolute risk aversion, or linear-exponential) and CRRA (constant relative risk aversion, or log-power) utility. They are defined as follows.
CARA utility:

$$for\, \alpha > 0, U(r) = 1 - e^{-\alpha r}; \tag{2.6}$$

$$for\, \alpha = 0, U(r) = r; \tag{2.7}$$

$$for\, \alpha < 0, U(r) = e^{-\alpha r} - 1. \tag{2.8}$$

CRRA utility:

$$for\, \rho > 0, U(r) = r^{\rho}; \tag{2.9}$$

$$for\, \rho = 0, U(r) = ln(r); \tag{2.10}$$

$$for\,\rho < 0, U(r) = -r^{\rho}. \tag{2.11}$$

We used Eq. 2.3 as the basis of our parametric analysis, rewriting it as

$$r^j = U^{-1}(2U(r^{j-1}) - U(r^{j-2})) \text{ for j = 2, 3, 4.} \tag{2.12}$$

We assumed a Fechnerian error model with the error term directly imposed on the answers r_j that subjects produced:

$$r^j = U^{-1}(2U(r^{j-1}) - U(r^{j-2})) + \epsilon_R \text{ for j = 2, 3, 4.} \tag{2.13}$$

We chose to impose error terms upon the direct measured values, being r^j 2.12, rather than upon utilities as often done in the literature. Our error model is similar to Wilcox (2011) contextual approach. The error term ϵ_R has expectation 0 and standard deviaton $\delta > 0$ per €. That is, δ is to be interpreted as the standard deviation when differences $r^j - r^{j-1}$ are in the order of magnitude of €1. Our error model is thus close to the thinking process of subjects, because the TO method enhances the direct comparison of $r^j - r^{j-1}$ during the experiment. Given that $x^j - x^{j-1} = 4.5$ under expected value maximization, we chose the standard deviation ϵ_x for $r^j = x^j$ equal to 4.5δ. We similarly used expected value approximations to set $\epsilon_y = 13\delta$ and $\epsilon_z = 40\delta$. We estimate δ and the utility parameter so as to maximize likelihood.

2.5.7 Results of the non-parametric analysis

As regards the overlap problems of section 2.2.2, for eight out of the nine envelopes opened during our experiment, the questionnaire answers determined the choice from the envelope, which was implemented. For the indeterminate case, the subject choose on the spot.

Figure 2.7 depicts the utility graphs resulting from average answers to the x, y, and z questions, based on Eq. 2.3, with utility normalized to be 0 at outcomes x^0, y^0, and z^0, and to be 1 at x^4, y^4, and z^4. Note that these graphs do not involve any parametric assumption. They can also be produced for every individual. We can use overlaps of the x, y, and z regions to combine such curves into one overall curve. As one would expect from the overall concavity of curves in Figure 2.7, most participants exhibit concave utility (Eq. 2.4) versus convexity over outcomes (Eq. 2.5): 37 versus 13 for the x's, 29 versus 12for the y's, and 21 versus 14 for the z's. This is significant for both x and y ($p \leq 0.01$). Thus our findings confirm concavity of utility, although the concavity is not pronounced. For the x's, about 20% of our subjects had equality for all i in Eqs. 2.4 and 2.5, giving perfectly linear utility and the same held for the y's and z's. The subjects not classified exhibited irregular (or

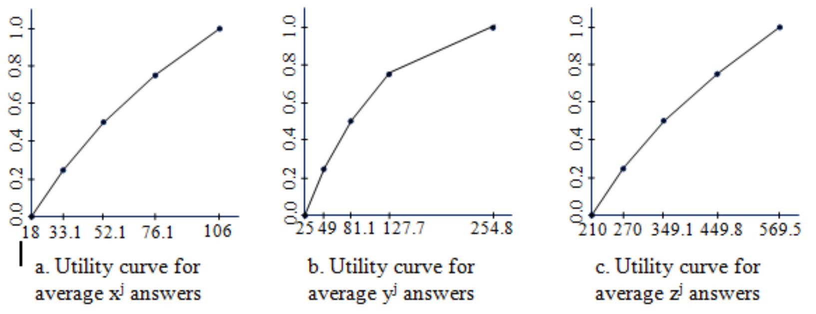


Figure 2.7 Nonparametric utility curves for the TO method.

linear with noise) utilities. The hypothetical choice groups' results were in line, but with more concavity for x and y stimuli, and not for z stimuli, than for incentivized groups.

2.5.8 Results of the parametric analysis

Our main analysis is at the individual level, finding the best utility parameter and error variance for all observations x^j, y^j, and z^j simultaneously. Normal distributions of the parameters are rejected and, hence, we report medians, Wilcoxon signed rank, Mann-Whitney, and Kruskal-Wallis tests.

The median CARA parameter α is 0.0001 (more precisely, $\alpha = 0.000099$), yielding risk tolerance $1/\alpha =$€10000. It suggests concave utility, but the value is not significantly different from linearity ($\alpha = 0$). The median power ρ is 0.96, again suggesting weak concavity but not deviating significantly from linearity ($\rho = 1$).

The standard deviation δ is larger for CARA utility than for CRRA utility (p = 0.001).[7] Thus CRRA utility better fits the data than CARA utility. We also fitted parameters to the x stimuli, y stimuli, and z stimuli separately, and compared them. Kruskal-Wallis tests reveal that CARA's α marginally depends on x, y, and z (p = 0.09), being higher for x than for y stimuli (p = 0.03), marginally higher for x than for z stimuli (p = 0.09), and not different between y and z stimuli. These results confirm the commonly assumed decreasing absolute risk aversion (references in Wakker (2010, p. 83)). A reason that we find no differences for the z stimuli may be because the z stimuli, while at higher levels of wealth, also involve bigger differences, which in itself will enhance risk aversion. CRRA's ρ does depend on x, y, and z (p = 0.01), being smaller for the x than for the y and z stimuli ($p \leq 0.01$), and

[7]Their medians happen to be the same (0.88), due to different skewnesses. Note that the standard deviations have the same unit (1 per €) and, hence, can be compared.

not different between the y and z stimuli. This finding implies decreasing relative risk aversion. Although most authors have conjectured increasing, rather than decreasing relative risk aversion, several studies found the opposite (Gollier (2001, §4); Ogaki & Zhang (2001)) and no consensus has been reached on this point.

Our findings comparing incentivized and hypothetical choices agree with common findings. The hypothetical data were noisier and contained more outliers. Further: (1) There was some more risk seeking for hypothetical choice than for real incentives in the z stimuli, reaching marginal significance ($0.05 < p < 0.10$) both for CARA and CRRA utility. (2) No significant differences were found in risk attitudes or standard deviations of parameters.

2.5.9 Discussion of the adaptive utility measurement

As regards the problem of strategic answering in adaptive experiments, Toubia et al. (2013, p. 629) and Wang et al. (2010) provide suggestions alternative to ours for mitigating this problem. One of these suggestions, deriving a preference functional from the experimental answers and implementing this functional in the RCS, was implemented by Ding (2007). Then subjects cannot directly understand the effects of their answers on the RCS during the experiment, and have to trust the relevance of the derived functional.

In experiments where subjects cannot really influence stimuli, they may mistakenly think they can, e.g. due to magical thinking (Rothbart & Snyder (1970)) or illusions of control (Stefan & David (2013)). Such distortions are more likely with future than with past uncertainties. Thus Prince helps to avoid such distortions.

By classical economic standards it may be surprising that we find near-linear utility, whereas classical estimates, based on expected utility, usually find more concavity. Recent studies find that risk aversion is mostly generated by factors other than utility for the moderate stakes considered in our experiment. With these factors filtered out, as in Eq. 2.3, utility turns out to be almost linear. Epper et al. (2011) who, like us, corrected for deviations from expected utility, argued for the reasonableness of this finding.

Unlike most measurements of utility in the literature, our analysis does not need to correct for deviations from expected utility. The TO stimuli were carefully devised such that those deviations have no bearing on our analysis, giving the same Eq. 2.3 under expected utility and nonexpected utility. The deviations are avoided rather than corrected for. This further supports our claim of high internal and external validity for our utility findings.

2.6 Parts and ingredients of Prince used in preceding studies

Virtually all choice experiments using the RIS randomly select the RCS at the conclusion of the experiment, thus violating our Principle 1 (priority), and then also 2 (tangibility) and mostly 4 (concreteness). All (to our knowledge) violate Principle 5 (instructions). Many usually satisfy Principle 3 (wholeness), randomly selecting, for instance, a row in a choice list, which constitutes the whole choice situation. Virtually all matching experiments (mostly using BDM) similarly violate Principles 1, 2, 4, and 5, and none that we know of satisfy Principle 3. The remainder of this section focuses on studies satisfying Principle 1.

To our knowledge, Bardsley (2000) was the first to satisfy Principle 1.[8] His purpose was to implement lying without lying, so to say. Bardsley could not determine the choice options in the RCS for a given subject beforehand because the latter depended on choices made by other subjects during the experiment. Thus Bardsley could not satisfy our Principles 3-5. He did not satisfy Principle 2 (tangibility) either but recommended it for future studies (last para of his §7).

In Schade et al. (2012, first version 2001), options were determined a priori in an envelope (lying on a desk in the front of the room), but not whole choice situations. What was real (sculpture/painting) was determined only at the end of the experiment, and with a small probability. Hence Principle 1 was satisfied, and Principle 2 was approximately, but Principles 3-5 were not. Wang et al. (2007) also used Schade, Kunreuther, & Koellinger's (2012) design, referring to Schade et al.'s (2001) working paper.

Bohnet et al. (2008) determined the RCS a priori. One choice option was inserted in an envelope that was visibly posted on a blackboard while subjects answered the experimental questions. Thus Principle 1 is satisfied, and Principle 2 is approximately so. Principles 3-4 were not satisfied. The authors first asked subjects what subjects would "pick," but later formulated these as instructions to the experimenters, thus partly satisfying Principle 5. Hao & Houser (2012) satisfy our Principles 1 and 2. They also used a formulation in the spirit of Principle 5. However, to optimize other goals in their research, they deviate from Principles 3-4. They present a meta-

[8]The first experiment with a prior envelope may have occurred earlier, by Johann Wolfgang von Goethe (January 16, 1797, letter cited by Mandelkow (8 ed, p. 254)). Goether wrote: "I am inclined to offer Mr. Vieweg from Berlin an epic poem, Hermann and Dorothea ... Concerning the royalty we will proceed as follows: I will hand over to Mr. Counsel Böttiger a sealed note which contains my demand, and I wait for what Mr. Vieweg will suggest to offer for my work. If his offer is lower than my demand, then I take my note back, unopened, and the negotiation is broken. If, however, his offer is higher, then I will not ask for more than what is written in the note to be opened by Mr. Böttiger". We thank Uyanga Turmunkh for this citation.

lottery B before explaining choices, then present a single strategy-choice between meta-lotteries, explicitly deviating from isolated binary choices.

Bleichrodt et al. (2013) considered case-based decision theory. Their questions concern varying memories for the same two choice options, rather than varying choice options as in classical revealed-preference experiments. They used Bardsley's (2000) method, satisfying Principles 1-2. They did not satisfy Principles 3-5. Their experiment was adaptive, but such that the two overlap problems (indeterminacy and exclusion) did not arise, thus avoiding strategic behavior.

In Camerer (1989), subjects, when selected for real play, could choose their preferred option only then, on the spot. We did the same but only in the special cases where the experimental choices did not specify the choice in the RCS, which can happen in adaptive experiments. Camerer's experiment contained no adaptive questions, and he offered the ex-post choice to all subjects, thus testing isolation. His test involved revising choices, which entailed a slight deviation from the prior announcement that experimental choices would be outcome relevant, involving a mild form of deception.

2.7 General discussion

The principles of Prince listed in section 2.2.3, and in general every detail of Prince, serve to enhance isolation by enhancing psychological conditioning upon the RCS. Although Starmer & Sugden (1991) found isolation satisfied in the RIS, violations have been found. In fact, any finding of learning, order effect, or spillover effect (Cox et al. (2014)) entails a violation of isolation. Hence improvements of isolation are desirable.

Regarding Principle 1 (priority), many studies show that conditioning works better for events determined in the past, even if yet uncertain, than for events to be determined in the future.[9] In the case of future determination, a meta-lottery is realistically perceived because the situation is still unsolved. More generally, we want

[9]See Bardsley et al. (2010, p. 277), Cubitt et al. (1998b), Cubitt et al. (1998a), Hey & Lee (2005), and Shafir & Tversky (1992, p. 463). In Bardsley et al.'s (2010) terminology, Prince uses the direct decision approach and avoids the strategy method. TThat the timing of the resolution of uncertainty, even if of no strategic or informational relevance, still affects subjects, has been demonstrated in many studies (Bosman & Van Winden (2010); Halevy (2007); Kreps & Porteus (1979)), and plays a role in time inconsistencies. In particular, prediction and postdiction are perceived differently (Brun & Teigen (1990); Heath & Tversky (1991); Rothbart & Snyder (1970)). Importantly for Prince, people more readily condition on uncertainties determined in the past than in the future, and take future uncertainty more as a meta-lottery (Keren (1991)). This phenomenon underlies several findings in game theory (Weber et al. (2004): virtual observability). Techniques for enhancing conditional isolated thinking are commonly used in insurance and marketing. For instance, pseudo-certainty is generated by making clients ignore background risks and exceptions to reimbursements (Tversky & Kahneman (1981, p. 456)).

the RCS to be felt as realistically as possible.

There have been several implementations of real incentives using prior envelopes (section 2.6) after Bardsley (2000), but all describe only one choice option in the envelope. If the randomization concerns the whole choice situation as with Prince (Principle 3), then subjects immediately condition on it, serving isolation. BDM randomizes a choice option (the price) rather than the choice situation, leading subjects to condition the wrong way. It obfuscates the choice situation, with the random price draw enhancing the undesirable perception of meta-lotteries. Principle 3 (wholeness) is crucial for Prince.

Researchers in decision theory will immediately see that Prince is strategically equivalent to RIS, asking for real preferences. Homo ecnomomicus will behave the same in both procedures. However, as Bardsley et al. (2010, pp. 270-271) wrote: "the effects of incentive mechanisms can depend on features of their implementation which are irrelevant from a conventional choice-theoretical point of view." Prince minimizes the biases generated by those features. It targets homo sapiens.

Throughout the history of preference measurement, there have been discussions of the pros and cons of matching versus choice.[10] Choice is less precise. It takes more time to elicit preferences, requires a specification of range and initial values which generates biases, and it enhances the use of qualitative noncompensatory heuristics (lexicographic choice and misperception of dominance). Matching is harder for subjects to understand, as are its incentive compatible implementations. Further, the matching environment can lead subjects to ignore qualitative information and to resort to inappropriate arithmetical operations.

Prince avoids an important misperception of matching: subjects may misperceive matching as bargaining.[11] In Prince, with the choice situation (the price therein being one option) specified beforehand in an envelope held in hand, it is perfectly obvious that this price is not subject to bargaining or any other influence.

Several experimental economists have implemented more than one choice situation for real, which is acceptable if the distortions due to the income effect are smaller than other distortions.[12] A systematic study is Cox et al. (2011), which is close in spirit to our study in seeking to reduce distortions in the RIS. It considers alternative

[10] These discussions include Bostic et al. (1990), Noussair et al. (2004), and Poulton (1989). There is also extensive literature in the health domain (Stevens et al. (2007); Weinstein et al. (1996a)) and psychophysics (Gescheider (1997, Ch. 3).)

[11] Because the link to the RCS is not clear in classical implementations, subjects think of what is closest from their everyday life, and this is probably bargaining. See Engelmann, & Hollard's (2010, trade uncertainty), Korobkin (2003, p. 1234), and Sayman & Öncüler (2005, §2.2); also see Bardsley et al. (2010, p. 273).

[12] Repeated payment is common in game and market experiments. In individual choice it is not very common, but still has been used in several studies, including Epper et al. (2011), and Mosteller & Nogee (2006).

incentive systems that imply particular income effects, and investigates circumstances in which these income effects generate smaller distortions than the regular RIS does. Our study seeks to improve the RIS while avoiding any income effect, thus preserving incentive compatibility for homo economicus, rather than replacing RIS by another system with some income effect.

Our claims of the high quality of Prince preference measurements are based on the theoretical arguments underlying Prince, exceptionally high consistency of our data (only one of 119 choice lists was inconsistent), debriefings and discussions in pilots, and confirmation of all well-established preference findings. Comparisons with gold standards of true preference would be desirable, but there are no gold standards here. Further supporting the quality of data by showing out-of-sample predictive power, especially regarding real-life decisions, extensive consistency checks to assess noise separated from between-individual and between-stimuli variation, manipulations of Prince with separate principles turned on and off, and comparisons with existing preference measurements are also desirable. Given the size of this paper, we prefer to leave such tests to future studies.

A drawback of Prince is that its implementation may take more preparation time: Numbered envelopes with different choice situations have to be prepared for every session of an experiment.

2.8 Conclusion

The prior incentive system (Prince) is an improvement of the random incentive system, the Becker-DeGroot-Marschak system, and Bardsley's (2000) conditional information system. Prince improves measurements of preferences without affecting those preferences themselves. Our subjects understand that there is only one real choice situation: the one they hold in hand. Prince resolves a number of problems: (a) violations of isolation; (b) misperceptions of bargaining; (c) strategic answering in adaptive experiments. Incentive compatability is completely transparent to subjects. Hence there were virtually no irrational preference switches in choice lists.

Prince reconciles choice and matching, combining the efficiency and precision of matching with an improved clarity and validity of choice. This resolves the classical preference reversals and reinvents matching. Prince corrects the major mistake of BDM: to randomize choice options rather than choice situations. Thus Prince leads subjects to condition properly. Prince provides more valid and transparent measurements of preferences, including WTA, subjective probabilities (section 2.4.6), and utilities (section 2.5), and ambiguity attitudes (section 2.4.6). The endowment effect and nonadditivity of subjective probabilities are genuine properties of preferences, entailing genuine deviations from classical principles. As with aversion to ambiguity, insensitivity is also real, and plays a role. Preference reversals, to the contrary, are due to errors in measurements. Utility is closer to linear than commonly thought, and decreasing absolute risk aversion is confirmed.

Every incentivized experimental measurement of preference or value can be improved using Prince. Prince sheds new light on which phenomena are to be incorporated in behavioral models.

2.A Appendix: Procedures for the TO measurement

The x stimuli contain an outcome 0, at which utility is $-\infty$ for $\rho \leq 0$. Because this outcome cancels from the equations, we still allow for power $\rho \leq 0$ in our data fitting. Only five subjects had $\rho \leq 0$ (minimum -0.75), and they do not affect any result.

Preceding studies and real incentives

Wakker & Deneffe (1996) introduced the TO method. If real incentives are implemented using the traditional BDM method and RIS, then subjects can obviously answer strategically and may rationally overstate the values of r^1, r^2, and r^3. Hence most experiments using the method were hypothetical. Four exceptions are Abdellaoui (2000), Bleichrodt et al. (2010), Schunk & Betsch (2006), and van de Kuilen & Wakker (2011) where the RIS was used with real incentives but subjects apparently did not notice the chance to answer strategically. The latter study used a strategy-check question to verify so (end of their §4). Toubia et al. (2013) also used the RIS in an adaptive experiment, but here it was, unlike with the TO method, impossible for subjects to notice the possibility to answer strategically.

We use Prince to implement real incentives. Then the RCS has been randomly selected for each subject before decision making. Subjects possess the RCS but with details remaining unknown. Subjects then obviously do not have any opportunity to influence the RCS and, furthermore, this is perfectly clear to them. Wang et al. (2007, p. 203) pointed out that providing the RCS prior to the experiment also rules out any illusion of strategic answering in a nonadaptive experiment.

Construction and use of envelopes for real incentives

For the 100 envelopes constructed before the experiment, we chose 12 types of envelopes, one for each question from TO1.1 - TO3.4. The envelope of type TOi.j (i = 1-3; j = 1-4) contains the two bets of Question TOi,j with values substituted as indicated in Table 2.3. The values $\overline{x^0} = x^0 = 18$, $\overline{y^0} = y^0 = 18$ and $\overline{z^0} = z^0 = 210$ are as in Figure 2.5. For example, the envelopes of type TO1.2 results from Figure 2.6b and contains the bets $32_{\frac{2}{3}}0$ and $27_{\frac{2}{3}}9$. The only exception is type TO3.4, which contains $\overline{z^{3'}} = 3050$ instead of $z^3 = 342$ for r^3. The # numbers in Table 2.3 indicate how many of the 100 envelopes were of the particular type. For example, there were three envelopes of type TO1.2.

The stimuli of TO1 were used by Abdellaoui (2000) and those of TO2 were used by Booij et al. (2010), both scaled up. Those of TO3 were not used before. They

	j=1	j=2	j=3	j=4
TO1	$\overline{x^1} = 23$ for x^1 (#33)	$\overline{x^2} = 27$ for x^2 (#3)	$\overline{x^3} = 32$ for x^3 (#3)	$\overline{x^4} = 36$ for x^4 (#3)
TO2	$\overline{y^1} = 46$ for y^1 (#33)	$\overline{y^2} = 69$ for y^2 (#3)	$\overline{y^3} = 92$ for y^3 (#3)	$\overline{y^4} = 116$ for y^4 (#3)
TO3	$\overline{z^1} = 253$ for z^1 (#9)	$\overline{z^2} = 297$ for z^2 (#3)	$\overline{z^3} = 342$ for z^3 (#3)	$\overline{z^4} = 3100$ for z^4 (#1)

* In this choice question (Figure 2.5, TO3, j = 4), we used $\overline{z^{3\prime}} = 3050$ (instead of $\overline{z^3} = 342$) for z^3.

Table 2.3 The pre-set TO values

serve to study high outcomes.

Subjects were informed that: (a) at least one of every ten of them would play for real; (b) if playing for real, the average gain under random ("blind") choosing is 53.27; (c) at least one prize exceeded 3000. They were told that the types of envelope did not occur equally often, which should be obvious with 100 envelopes of 12 types.

In other respects the organization was as with the Prince experiments reported before. For example, the questionnaire asked subjects to give us instructions about which option from their own envelope to give them at the end of the experiment. One more difference was as follows. In the previous experiments we demonstrated how Prince can be used with a regular performance-contingent real payment for every subject. We now implement Prince with performance-contingent real payments only for some subjects, but then having these payments large. Abdellaoui et al. (2011, Web Appendix) suggested that such payment schemes work best to motivate subjects. For this purpose, at the end we collected the three front pages numbered TOj.0 of each subject that contained their answers, whereas the subjects kept the rest of their questionnaires that also contained their answers.[13] The front pages of each subject were folded together so as to be unrecognizable, and were put in an opaque case and shuffled. Then we let one subject draw some (three in the first session and six in the second) triples of questionnaires from this case, after which the corresponding subjects played for real.

Maximizing overlap with experimental questions

The numbers x^j for TO1 in Table 2.3 result from expected value maximization, and those for TO2 and TO3 (except TO3.4) result from Tversky & Kahneman's (1992) prospect theory and their parameters. We rounded the $x^j s$. We obviously constructed the 100 envelopes, in particular the frequency of each type of envelope among the 100 envelopes. so as to generate an appropriate overall expected value of the game for the subjects.

[13]The subjects filled out all answers twice: once on the page containing the question, and once on the front page numbered TOj.0.

We chose most envelopes of type TOj.1 because here three rather than two outcomes were known beforehand. Hence, each answer x^1 by the subject implied a choice from the corresponding envelope, depending on whether x^1 exceeded the corresponding value from the envelope or not.[14] The same observations hold for y^1 and z^1. We thus obtain a high overlap between experimental answers and RCSs.

In the explanations given at the beginning of the experiment an example was used that occurred in a pilot: In TO1.1 a subject in a pilot had answered $x^1 = 50$, implying an indifference $10_{\frac{1}{2}}8 \sim 50_{\frac{1}{2}}1$. Then, if the choice in the envelope had been between $10_{\frac{1}{2}}8$ and $32_{\frac{1}{2}}1$, the former, $10_{\frac{1}{2}}8$, would be given to the subject. We explained that the subject's preference would only be reinforced if outcome 10 in his preferred bet had been increased to 24, and he would obviously want the former even more from the pair $\{24_{\frac{1}{2}}8,\ 32_{\frac{1}{2}}1\}$. This pair was actually contained in the envelope of this subject, who indeed received $24_{\frac{1}{2}}8$. In general, if the value $\overline{r^j}$ in the envelope exceeded the answer r^j given by the subject and *the value* $\overline{j^{i-1}}$ in the envelope was *below* rj^{i-1}(reinforcing the preference), then we would give the bet with outcome rj to the subject. Conversely, if the value $\overline{r^j}$ in the envelope was below the answer r^j given by the subject and *the value* $\overline{j^{i-1}}$ *in the envelope exceeded* j^{i-1} (reinforcing the preference), then we would give the lottery with outcome r^{j-1} to the subject. Thus the answers given during the experiment pertained to many possible envelopes, as we explained to the subjects.

Verifiable absence of deception

As in the previous experiments, subjects could verify that there was no deception. Again, at the beginning they verified, through sets of 10 numbered envelopes, that all numbered envelopes were present. Subjects collected, shuffled, and selected envelopes from bags themselves. At the end, when subjects who had been selected for payment came to the front of the room, a list describing the content of all 100 envelopes was handed out to the other subjects, with calculations showing that our information about expected value under random play and maximal amounts was correct. These subjects were asked to open their envelope and verify that the list correctly described its content. For the subjects who were in front of the class, everyone in the class could see that the description of their envelopes was correct.

One of the experimenters carried over €3000 in cash with him, showing to the subjects that this amount could and would be paid on the spot if the lucky case arose. The random selection of at least one per 10 subjects to play for real (three out of 25 in the first session and 6 out of 55 in the second) was done by letting the

[14]If indifference, subjects could choose on the spot, as always if the experimental answers did not specify a choice.

subjects put their questionnaires in an opaque bag, after which the questionnaires to be implemented for real were randomly selected by a subject. Lotteries were generated using a six-sided die, thrown by a subject selected for this role by the subject who was playing for real.

Chapter 3

The Rich Domain of Ambiguity Explored[1]

3.1 Introduction

This paper experimentally measures ambiguity attitudes for different sources of uncertainty, different outcomes, and their combinations. We thus provide the first systematic study of these dependencies, and comparisons between them. In particular, we investigate the following (not-exclusive) topics of debate in the current literature: (1) Does ambiguity attitude depend on the source of uncertainty (source-dependence)? (2) Is ambiguity attitude better captured by outcome functions outcomes, or by event functions? (3) To what extent is ambiguity aversion universal? (4) Are the findings of the widely studied Ellsberg urns representative for natural uncertainties, or are they due to particular negative emotions with little general validity? These questions lead to more fundamental questions: (5) Is there a general ambiguity attitude, reflecting an attitude towards probabilities being unknown, or are ambiguity attitudes driven by context-dependent emotions generated by the uncertain events considered? (6) Can deviations from Savage's (1954) subjective expected utility, as in the modern ambiguity models, be rational?

Following up on a positive answer to the first question, we also investigate which parametric families of (subjective-)probability transformation functions are best suited to capture ambiguity attitudes and their variations. The related question has been studied extensively for risk, but we are the first to investigate it for ambiguity. Hey et al. (2010) were the first to systematically test many ambiguity theories, consid-

[1]This chapter is based on the paper "The Rich Domain of Ambiguity Explored" co-authored with Julia Müller, Tong Wang and Peter P. Wakker.

ering an ambiguity triangle domain with a bingo blower as source of uncertainty. Kothiyala et al. (2012) supplemented their study. Our paper is a follow-up study, focusing on the winner of the above comparisons - prospect theory with the source method - and testing which submodel best captures variations in ambiguity attitudes in a richer domain.

3.2 Two topics of debate

This section provides literature and background on two topics of debate investigated in this paper.

3.2.1 Ambiguity through outcome-functions in the smooth model

Ellsberg (1961) initiated the study of ambiguity, where decisions under uncertainty have to be made without knowledge of underlying probabilities. One current topic of debate is whether ambiguity attitudes are better modeled through outcome functions or event functions. Most decision models use event functions, generalizing subjective probabilities.[2] They involve generalizations of integrals in their definitions, and they allow for kinks in indifference curves.

Recently, alternative models, using outcome functions, became popular. We start with the clearest model of this kind: source-dependent expected utility by Chew et al. (2008). Here expected utility holds within each source of uncertainty, but different sources may involve different utility functions. Ambiguity aversion is captured by utility being more concave for ambiguous sources than for risky sources. Thus an outcome function (utility) is used to model ambiguity attitudes, and the ambiguity attitude depends not only on the source of uncertainty but also on the range of outcomes considered.

The most popular outcome-based model is the smooth model by Klibanoff et al. (2005), which we next discuss in some detail.[3] In the smooth model, uncertainty about the true (risky) probability measure on the state space is expressed by a second-stage probability measure. With respect to first-stage probabilities, expected utility is maximized. In the second stage, backward induction is used and again expected utility is maximized, but a concave transformation φ is applied to risky expected

[2]See Gajdos et al. (2008), Ghirardato et al. (2004), Gilboa (1987), Gilboa & Schmeidler (1989), Hansen & Sargent (2001), Jaffray(1989, 1994), Schmeidler (1989), Tversky & Kahneman (1992), Wakker (2010). Recent generalizations include Chateauneuf & Faro (2009) and Maccheroni et al. (2006), with Strzalecki (2011) connecting event- and outcome-based models.

[3]models and generalizations include Chew et al. (2008), Dobbs (1991), Ergin & Gul (2009), Kahneman & Tversky (1979, p. 30 ff.), Nau (2006b), Neilson (2010), and Seo (2009).

utility, generating ambiguity aversion. A big pro of the smooth model is its analytical tractability, using only conventional integrals and having no kinks in preferences, so that traditional optimization techniques can readily be used. Ambiguity attitudes are different for different outcome domains with different utility curvature.

An empirical difficulty of outcome-function models is that ambiguity attitudes cannot change if we go from likely to unlikely gain-events whereas empirically they do, changing from ambiguity averse to ambiguity seeking. Another empirical difficulty is that these models assume expected utility for risk, which is needed for their tractability, but which is violated empirically (Allais (1953); Kahneman & Tversky (1979); Starmer (2000)).

In the smooth model, utility could be different for different sources of uncertainty, capturing source dependence of ambiguity attitudes, in the same way as it is different for the (risky) first stage than for the ambiguous second stage. Whereas such source dependence can be investigated empirically, it was not the intention of the inventors of the smooth model. They assumed a Savage state space in the first stage, which captures all uncertainties. This means that their utility transformation φ applies to all sources of uncertainty, and then cannot capture source dependence. It should capture the ambiguity attitude of the decision maker and should apply to all sources of ambiguity. The smooth model can differentiate between different uncertain sources through different second-stage probability distributions, which are assumed to capture the ambiguity in events and to be endogenous. To the best of our knowledge, this endogenous aspect of the smooth model has not yet been investigated. Applications of the smooth model have taken the two-stage decomposition as exogenous, determined by the researcher. Source dependence of ambiguity aversion may possibly be captured through different dispersions of the two-stage probability decompositions, but such dispersions cannot capture insensitivity nor its source dependence. Given that there are many two-stage decompositions, and that this subjective parameter of the smooth model is of a high dimensionality, its analysis will be a complex topic of future research, and we will not discuss it further.

3.2.2 External validity of Ellsberg urns

Another point of debate in the current literature concerns the relevance of the Ellsberg urn events for general uncertainty. These events have been the by far most used events to study ambiguity in the literature up to today. Several authors have argued for broadening the domain of study. In our everyday decisions, we almost never know probabilities of the uncertain events faced, but these uncertainties come naturally, and not through information deliberately concealed as with the ambiguous Ellsberg urn. This urn will, unlike the known urn, generate negative social emotions (Tversky & Fox (1995)). Hence, natural uncertainties deserve closer examination. In a review paper of over 20 years ago, Camerer & Weber (1992) wrote: "There are diminishing returns to studying urns." The same view motivated the work on ambiguity by Amos Tversky. He often emphasized the importance of source dependence (Heath & Tversky (1991); Tversky & Kahneman (1992); Tversky & Fox (1995); Tversky & Wakker (1995)).

Many other recent studies have casted further doubts on the general validity of the phenomena found for Ellsberg urns (reviewed by Trautmann & Van De Kuilen (2013)). To clarify the role of emotions generated by particularities of events, we will add two sources of uncertainty. The first concerns the district of origin of a child in charity schooling in India. While there still is concealed information differentiating the ambiguous events (now about districts) from unambiguous events as with Ellsberg urns, this case will generate additional positive social ("feel-good") emotions. Those will counterbalance the negative emotions about the concealed information. The second additional source that we consider concerns health and will not generate asymmetric emotions. Unlike with the other sources, there is no information deliberately concealed by us and differentiating the ambiguous from the unambiguous events. This example will be more characteristic of natural uncertainties and ambiguities.

3.3 Our analysis of ambiguity

Our analysis is based on binary rank-dependent utility (binary RDU), and its specification in Abdellaoui et al. (2011) source method. Binary RDU specifies evaluations of two-outcome prospects, and comprises many ambiguity theories including multiple priors, α maxmin, prospect theory, and Choquet expected utility Wakker (2010, §10.6). Thus our analysis is valid for all these theories. We will use the simplified implementation of the source method introduced by Dimmock et al. (2012). Those authors deliberately minimized the number of measurements and the experimental time per subject so as to demonstrate the tractability of their method. We will use more detailed and thorough measurements and more time per subject so as to obtain better reliability and validity.

The three sources of uncertainty that we consider are: (1) which of 10 possible colors a ball drawn from an Ellsberg urn has; (2) which of 10 possible viruses caused a disease; (3) which of 10 possible districts a child from India came from. We will also consider three kinds of outcomes: money, waiting time, and life duration.

Subjects consider gambles $\alpha_E\beta$ on events E, yielding a favorable outcome α if event E happens and an unfavorable outcome β otherwise. We explain how we measured ambiguity indexes for the Ellsberg urn. For the other two sources it was done the same way. We considered events E_j of j winning colors for j = 1, 3, 5, 7, and 9, where higher j's give more favorable events. We call j/10 the *Bayes-probability* of event E_j, because a Bayesian decision maker would assign this subjective probability to event E_j. For each event E_j we determined the *matching probability* m(j/10), being the objective probability such that a subject considered gaining α under event E_j equivalent to gaining α with this objective probability m(j/10). The function m depends on the source of uncertainty, which can be expressed by adding the subscript in ms_o.

For all sources, subjects had the same information about all events E_j, and they had no reason to consider any event E_i preferable to any other event E_j.[4] Hence an ambiguity neutral (Bayesian) decision maker assigns subjective probability j/10 to each event E_j and has m(j/10) = j/10 for all j. For each event E_j, j/10 - m(j/10) serves as *event-dependent ambiguity aversion index*, and we have:

$$j/10 - m(j/10) > 0 : \text{ambiguity aversion for} E_j; \tag{3.1}$$

$$j/10 - m(j/10) = 0 : \text{ambiguity neutrality for } E_j; \tag{3.2}$$

$$j/10 - m(j/10) < 0 : \text{ambiguity seeking for } E_j; \tag{3.3}$$

For example, ambiguity averse subjects dislike the ambiguity comprised in E_j and a smaller objective probability m(j/10) will then be equivalent to E_j, implying Eq. 3.1. Thus the matching probabilities m(j/10) provide an easy tool to measure ambiguity attitudes. Dimmock et al. (2012) give theoretical justifications for this claim, showing that matching probabilities easily and completely capture ambiguity attitudes for binary RDU and all theories comprised by it. DKW derived global indexes of ambiguity attitudes as follows. First, for the five data points (j/10, m(j/10)) the best-fitting (by quadratic distance) line

$$p \longmapsto c + sp; \tag{3.4}$$

(truncated at 0 and 1; i.e., it should not be negative or exceed 1) is determined. We emphasize that this line only serves as an intermediate step in a mathematical calculation of the indexes, that is, in recoding the data. The line should not be taken as a statistical estimation. It is natural that ambiguity aversion is higher as the points m(j/10) are lower, analogously to Gilboa and & Schimeidler's (1989, pp.

[4]This condition amounts to Chew & Sagi (2008) exchangeability condition. They provided the main theorem showing that this condition justifies using the source method.

572, 574) and Dow & da Costa Werlang's (1992) index of ambiguity aversion (see Eq. 3.11). We thus define:

$$b = 1 - s - 2c \text{ is the index of } \textit{ambiguity aversion.} \tag{3.5}$$

This component is motivational, reflecting an overall like or dislike of ambiguity. Under expected utility (ambiguity neutrality) c = 0 and s = 1, and the index has value 0. Positive values indicate ambiguity aversion, with 1 the maximum, and negative values indicate ambiguity seeking, with -1 the minimum (occurring if the value in Eq. 3.5 at p = $\frac{1}{2}$ is 1). Further

$$a = 1 - s \text{ is the index of } \textit{(ambiguity-generated likelihood)- insensitivity.} \tag{3.6}$$

This index reflects the shallowness of m and, hence, insensitivity towards changes in likelihood of the E events. Insensitivity is a cognitive component of ambiguity, reflecting general (lack of) understanding. It is prior to any liking or disliking, and means that people simply do not understand ambiguity well, taking it too much as fifty-fifty. Under expected utility, a = 0, reflecting optimal sensitivity. The index a usually is positive, reflecting imperfect sensitivity. On exceptional occasions it can be negative, reflecting oversensitivity in the middle region. Then rare events are not overweighted but ignored. For further explanations and theoretical background, see Dimmock et al. (2012). We will investigate how ambiguity attitudes, measured through the indexes, depend on the outcomes and the sources of uncertainty. We also used parametric families of subjective-probability transformation functions to fit the matching probabilities. These functions have commonly been used for decision under risk, capturing risk attitudes. We use them for matching probabilities m(j/10)), capturing ambiguity attitudes.

Neo-additive:

$$m(0) = 0; m(1) = 1; m(p) = c + sp \text{ for } 0 < p < 1; s \geq 0, c \leq 1; \text{m is truncated at 0,1.} \tag{3.7}$$

Indexes of a-insensitivity and ambiguity aversion were defined in Eqs. 3.5 and 3.6.

Goldstein & Einhorn:

$$m(p) = \frac{bp^a}{bp^a + (1-p)^a}; a > 0, b > 0 \tag{3.8}$$

with a an index of a-insensitivity and b an index of ambiguity aversion.

Prelec (1998) 2-parameter

$$m(p) = (exp(-(ln(p))^a))^b; a > 0, b > 0 \tag{3.9}$$

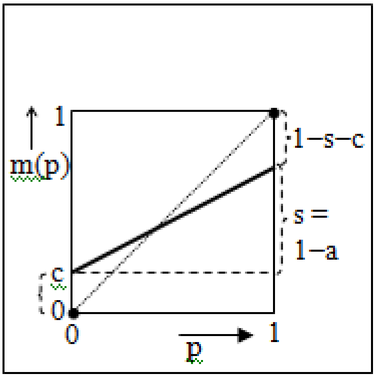


Figure 3.1 The neo-additive family

with, again, a an index of a-insensitivity and b an index of ambiguity aversion.

Prelec (1998) 1-parameter 3.9 with b = 1.

Tversky & Kahneman (1992):

$$m(p) = \frac{p^c}{(p^c + (1-p)^c)^{1/c}} \text{ for } c \geq 0.28 \tag{3.10}$$

where c is both an index of a-insensitivity and of ambiguity aversion.

3.4 Experimental Design

3.4.1 The basic treatment

We considered five treatments, that is, five combinations of sources and outcomes, displayed in Table 3.1. Exact wordings of the instructions for subjects are in the appendix. We partially randomized the order of presentation of the treatments by using two different orderings: basic, week, year, health, kid, and the other ordering was reversed: kid, health, year, week, basic.

This subsection presents the first treatment, the basic treatment, which concerns a standard Ellsberg experiment. Two urns contained 100 balls with ten different colors: yellow, orange, red, dark-pink, light-pink, purple, dark-blue, light-blue, light-green and dark-green. For urn K the composition of balls was known, while for urn U the composition was unknown. The unknown urn U was prepared beforehand by a third party. Therefore the experimenters themselves did not know its composition during the experiment.

treatment	**source of uncertainty**	**outcome**
basic	Ellsberg urn	money
week	Ellsberg urn	waiting time (weeks)
year	Ellsberg urn	waiting time (years)
kid	districts	money
health	viruses	life time

Table 3.1 The five treatments

For each j = 1 or 3 or 5, subjects first had to choose j winning colors, which determined event E_j for urn U. It was next announced how many balls of these winning colors urn K would contain, where this number did not have to be j/10 and was different in different choice situations. Then the subject would choose if a ball was drawn from urn U or urn K. If the color of the ball was a winning color, then the subject would receive a good outcome (€500), and otherwise a bad outcome (€0) would result. If the implementation of real incentives involved urn K, i.e. if the subject had chosen a draw from urn K, then this urn was prepared with the proper composition by the experimenters in front of the participants of that session.[5]

<table>
<tr><th colspan="2">number of balls in urn K</th><th>K</th><th>U</th><th colspan="2">number of balls in urn U</th></tr>
<tr><td>€500 if ball is yellow, orange, or red</td><td>€0 for other colors</td><td></td><td></td><td>€500 if ball is yellow, orange, or red</td><td>€0 for other colors</td></tr>
<tr><td>0</td><td>100</td><td></td><td>X</td><td rowspan="11">unkown</td><td rowspan="11">unkown</td></tr>
<tr><td>10</td><td>90</td><td></td><td>X</td></tr>
<tr><td>20</td><td>80</td><td></td><td>X</td></tr>
<tr><td>30</td><td>70</td><td>X</td><td></td></tr>
<tr><td>40</td><td>60</td><td>X</td><td></td></tr>
<tr><td>50</td><td>50</td><td>X</td><td></td></tr>
<tr><td>60</td><td>40</td><td>X</td><td></td></tr>
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<tr><td>80</td><td>20</td><td>X</td><td></td></tr>
<tr><td>90</td><td>10</td><td>X</td><td></td></tr>
<tr><td>100</td><td>0</td><td>X</td><td></td></tr>
</table>

Table 3.2 The five treatments

For each E_j, we elicited choices for all 101 compositions of winning balls in urn K using the incentive compatible implementation of refined choice lists introduced

[5]For implementing the composition of urn K swiftly, for every color, groups of ten balls were stringed (the balls had holes), thus enabling to quickly and reliably prepare any amount of balls between 0 and 100 in front of the participants, verifiable for all.

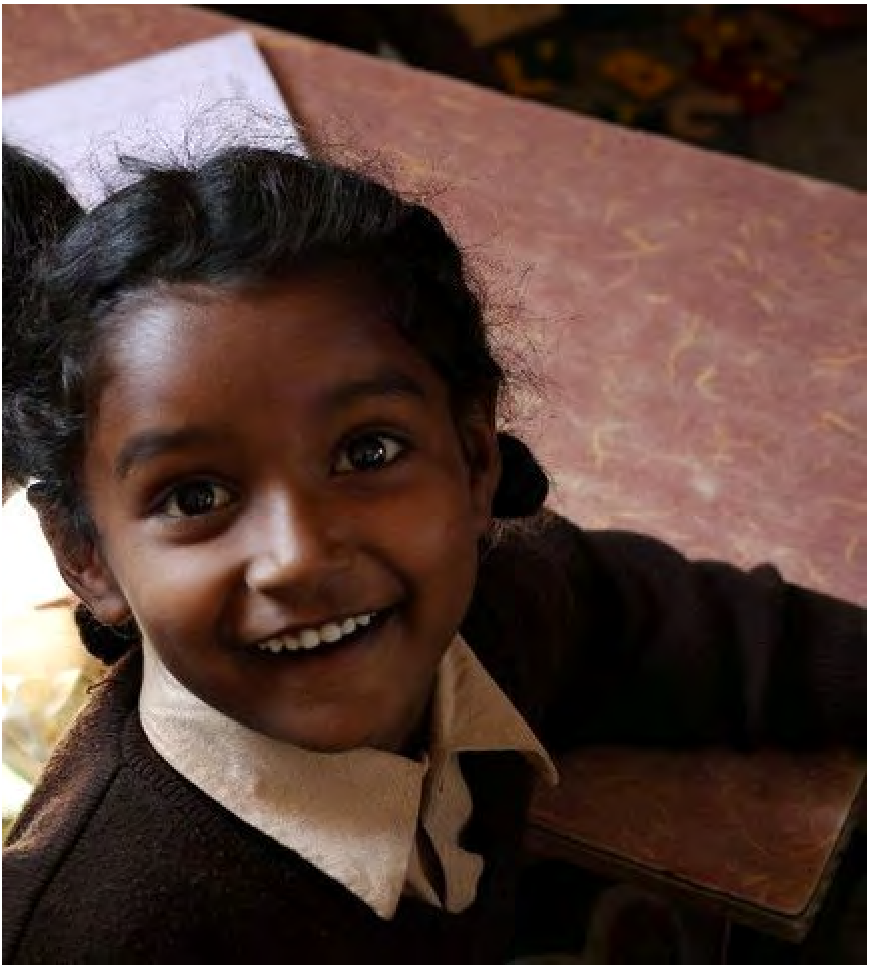
by Abdellaoui et al. (2011). For low numbers of winning balls in K, subjects should prefer urn U, and for high numbers urn K. Somewhere in between preferences switch, and this switching point we measured, as follows. A first choice list (Table 3.2) included 11 choices between urn K and urn U, with 0, 10, ..., 100 balls of the winning color(s) in urn K. The second choice list would then be refined between the two values where the switching happened. In Table 3.2, this happens between 20 and 30, and the next choice list would include the choices for 21, 22, ... to 29 balls of winning color(s) in urn K. From this we are able to infer the choices of the subject for all 101 compositions of urn K (for 0,1, ... to 100 balls of the winning color(s)). If for j winning balls in U, preferences switch between i and i + 1 winning balls in K, then we estimated the matching probability m(j/10) to be $(i+\frac{1}{2})/100$. If there are no switches, then m(j/10) is 0 or 1 as the case may be. The program enforced monotonicity and did not allow for multiple switches. We would randomly select one from the 101 compositions of urn K implemented, and not only from the choices actually asked in the two choice lists, to ensure incentive compatibility.

For each determination of m(j/10) as just described and j = 1, 3, 5, we would immediately after consider the same set of j colors, but now these were the losing colors, and the other 10 - j colors were the winning colors. This way we would determine m(1-j/10). We thus obtained two measurements of m(5/10) who were not statistically different for any treatment, suggesting that there were no different framing effects. In several following analyses we used the average of the two observations of m(5/10). Only in fittings they were treated as two separate observations.

3.4.2 Alternative treatment

In the second treatment week, the outcome was changed and was waiting time instead of money. Subjects did receive money, €250, and did so with certainty. But now the uncertainty concerned the time of receipt. The favorable outcome was receiving the money immediately, and the unfavorable outcome was receiving it in eight weeks. The third treatment year was like the treatment week, only the money to be won with certainty was €5000 and the time of receipt was either immediately or in 10 years. Choices in this treatment were hypothetical, and subjects received an immediate flat payment of €250 if this treatment was selected for implementation. In all else the two treatments week and year were the same as the basic treatment, concerning the same Ellsberg urns and the same way to measure matching probabilities m(j/10).

In the fourth treatment, the kid treatment, we did not change the outcomes (€500 or €0) relative to the basic treatment, but instead the source of uncertainty. The source involved a charitable program in rural India, paying for school education of children. We showed our subjects a photo from one of the children whose lives have been transformed by this charitable program.



The child came from one of 100 villages that were distributed over 10 possible districts. Subjects could now gamble on the district where the child's village belongs to. They could now choose which winning districts (instead of colors) to gamble on. Our subjects could not be expected to have any geographic knowledge of these villages or districts, or their sizes. Thus the 10 districts were equally likely to our subjects in the same way as the 10 colors in the Ellsberg urn were equally likely. The 100 villages are analogous to the 100 balls for Ellsberg, neither being outcome-relevant beyond their district. Both the photo and the charitable context related to education can be expected to arouse positive emotions, which may offset the negative emotions generated by us concealing information about the districts to our subjects. Hence, this treatment could have been called the feel-good treatment. Matching probabilities were measured using the same known urn K as before. Now each district was coupled with a color, so that gambling on three districts corresponded with gambling on three colors in the known urn, and so on.

The fifth and final treatment was a health treatment, which deviated more from the basic treatment than the other treatments did. We now changed both the outcomes and the source of uncertainty. This treatment was again hypothetical, and subjects received an immediate flat payment of €250 if this treatment was selected for implementation. For the source of uncertainty, we employed a virus story. The subjects were asked to imagine that they were diagnosed with a certain disease and that they would have to receive a treatment against it. It would furthermore be known that there are ten possible mutually exclusive viruses (numbered from 1 to 10) causing the exact same disease. There would be no way to diagnose which virus is causing the disease, but the disease would only be cured if the real virus was treated. In the case of recovery (disease cured), the subjects would live 50 years longer in good health, and otherwise one year longer in good health. That is, the outcome now was life duration.

Subjects were asked to choose between Treatment K and Treatment U. Treatment K would use a broad-spectrum antiviral supplement with a Known success rate (given in %). Treatment U was said to be new and would use specific supplements (numbered from 1 to 10) which would only be effective for the virus with the corresponding number. Only if the right supplement for the real virus would be chosen, the disease would be cured. Because the subjects were told that there is no way to diagnose which virus is causing the disease, the 10 viruses were equally likely to them in the same way as the Ellsberg colors or the districts were. Event E_j now meant that only j supplements could be provided. To measure matching probabilities m(j/10), we now did not use a known urn, but the treatment K with success rates specified for 0%, 1%, ..., 100%. Because this fifth health treatment was hypothetical, and because within the hypothetical story the experimenter could not know about the true treatment, subjects did not choose the j treatments provided, but the the first j treatments were offered. This avoids both suspicion and illusion of control, the common confounds in Ellsberg experiments.

3.4.3 Further experimental details

Subjects

N=66 subjects (73% male, 27% female), bachelor and master students from various fields were recruited online from the EconLab website of Erasmus School of Economics.

Procedure

The experiment was conducted at the ESE-econlab of the Erasmus University Rotterdam. There were three sessions, all at the same day.[6] They lasted 1.5 to 2 hours.

Incentives

Subjects received a show-up fee of €5. For further payment we used a random incentive system. Total average earnings were €16.36. One randomly chosen subject in each of the three sessions received an additional payment on top of the show-up fee. For this subject we first randomly determined which of the five treatments would be implemented. Two treatments were hypothetical, for which a fixed payment of €250 was given. If one of the other three treatments was chosen, then we would next randomly select the Bayes pobability j/10 (out of six, with j = 5/10 asked twice). Then we would randomly determine one of the 101 objective probabilities in urn K, and implement the prospect chosen by the subject for this K. The payoff was determined by a draw from the urn chosen by the subject, K or U (referring to districts in the kid treatment). Urn U had been determined beforehand by an outside party, with its composition unknown to the experimenters. Urn K, if implemented, was prepared in front of the subjects. The subject playing for real would draw a ball from the respective urn. If the color of the ball (or district) was included in the winning colors, then the subject would receive the good outcome, and otherwise the bad outcome. All implementations of randomness were non-computerized, and verifiable to the subjects. We implemented all random choices by drawing from bags: For the subject to be chosen from all subjects in the session we used ID cards, for the choices of treatment (out of five), Bayes probability (out of six), and composition of urn K (out of 101) we prepared boxes with numbered table-tennis-balls.

3.4.4 Analysis

Data fitting was done by likelihood maximization, assuming Fechnerian normally distributed error terms Besides data fitting based on maximum likelihood, we also did data fitting minimizing squared distances, but here all results were virtually identical, differing only in fourth digits, and they will not be reported.

[6] All sessions were scheduled the same day to avoid that participants could learn beforehand that a charitable program in rural India were involved, and could have gathered information about it.

Wilcoxon signed rank tests and t-tests gave the same conclusions throughout. We report the former because normal distributions of our variables were mostly rejected. We both did overall fitting and fittings at the individual levels. For the overall fitting, we assume that all subjects have the same matching probabilities. We then fit an overall matching probability function with each parameter containing dummy variables for different treatments using the basic treatment as the baseline. To compare across treatments, we use the Wald test to see whether the coefficients for different treatments differ. For the individual fitting, we fitted the functions for each subject individually in each treatment. We report the results of the overall fittings. Individual fittings gave the same results (see section 3.A.2).

3.5 Results

We used two orders of presenting the stimuli, but found no differences in results and, hence, pool all data (see section 3.A.2).

3.5.1 Descriptive results regarding ambiguity aversion, a-insensitivity, and outcome- versus event-dependence

Figure 3.2 plots the mean matching probabilities m(j/10) and displays the main phenomena, which will later be confirmed by statistical tests. The curves are on average somewhat below 0.5, meaning that there is more ambiguity aversion than ambiguity seeking. But ambiguity aversion is not strong, and for low likelihoods there is prevailing ambiguity seeking. Thus the prevailing phenomenon is a-insensitivity. The curves are almost linear in the interior, suggesting that neo-additive functions will fit the data well.

As for comparisons between treatments, the curves of the three Ellsberg treatments are very similar and, hence, outcomes do not affect ambiguity attitudes. Changes in the source of uncertainty, in the kid and health treatments, do affect ambiguity attitudes. In particular, sensitivity becomes much better.

3.5.2 Statistical tests of ambiguity aversion, a-insensitivity, and outcome- versus event-dependence

Table 3.3 presents estimations of the indexes of Eqs. 3.5 and 3.6 resulting from overall estimations. There is some ambiguity aversion, but it is close to the neutral level of 0, and for the health treatment it is only marginally significant. A-insensitivity is strong. Comparing across treatments, changes of outcomes do not affect the indexes, which are the same for the treatments basic, week, and year, as confirmed by ANOVA

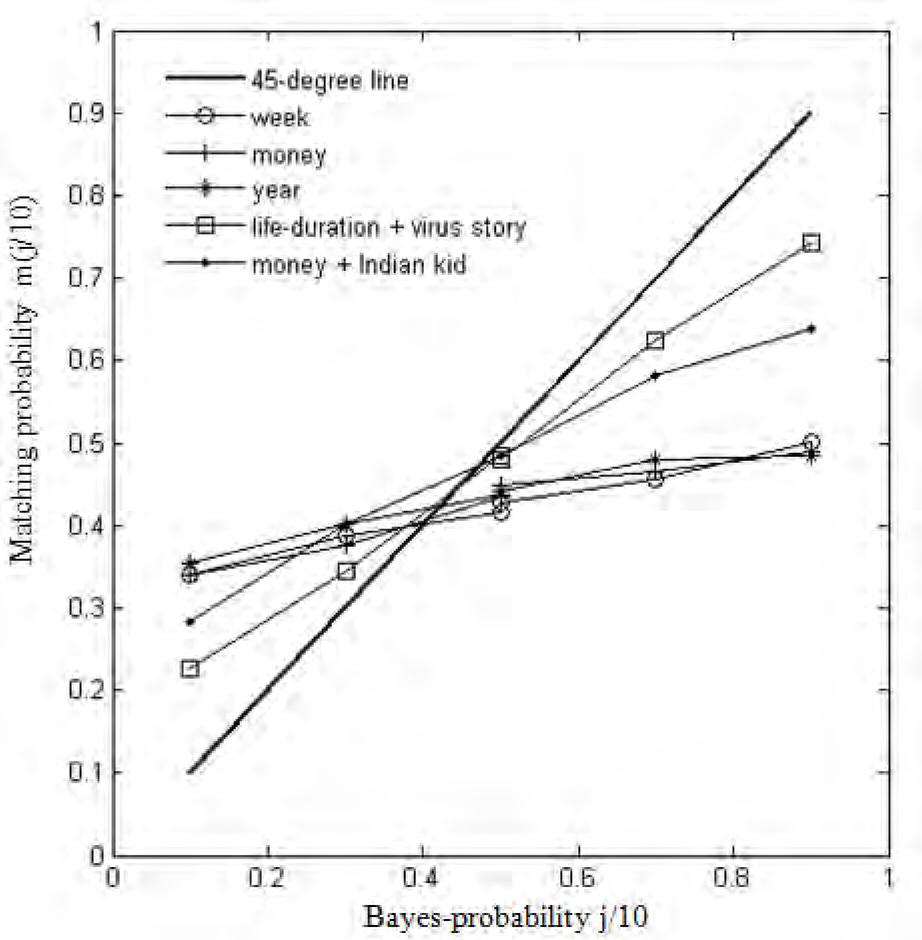


Figure 3.2 Mean matching probability

(p = 0.98 and 0.88). Changing the source of uncertainty in the kid treatment gives lower ambiguity aversion ($p < 0.001$) and much better sensitivity ($p < 0.001$) than in the basic treatment. The health treatment has yet more sensitivity than the kid treatment ($p < 0.001$), and seemingly more ambiguity aversion that is, however not significantly different (p = 0.7).

	basic	week	year	kid	health
ambiguity aversion index b	0.15**	0.16**	0.13**	0.04*	0.03^{ms}
a-insensitivity index a	0.81**	0.8**	0.83**	0.55**	0.34**

p-value of Wald test against H_0: $= 0$: ms: $p < 0.1$; *: $p < 0.05$; **: $p < 0.0001$.

Table 3.3 Estimations of ambiguity indices

Ambiguity attitudes	Bayes probability	ambiguity aversion index = Bayes probability j/10 - median matching probability m(j/10) [p-value of Wilcoxon rank sum testing if ambiguity aversion index is 0] (percentages of subjects: ambiguity seeking/averse)				
		basic	week	year	kid	health
ambiguity seeking (j/10-m(j/10<0)	0.1	-0.245 [0.000] (**87.88%**/12.12%)	-0.245 [0.000] (**89.39%**/10.61%)	-0.280 [0.000] (**84.84%**/15.15%)	-0.085 [0.000] (**84.85%**/15.15%)	-0.005 [0.000] (**81.82%**/18.18%)
	0.3	-0.065 [0.000] (**80.30%**/19.70%)	-0.100 [0.000] (**72.24%**/25.76%)	-0.105 [0.000] (**71.21%**/28.79%)	-0.050 [0.000] (**80.30%**/19.70%)	-0.005 [0.000] (**74.24%**/25.76%)
ambiguity averse (j/10-m(j/10>0)	0.5	0.035 [0.000] (12.12%/**87.88%**)	0.005 [0.000] (19.70%/**80.30%**)	0.005 [0.000] (24.24%/**75.76%**)	0.005 [0.000] (33.33%/**66.67%**)	-0.005 [0.000] (66.67%/**33.33%**)
	0.7	0.230 [0.000] (4.55%/**95.45%**)	0.205 [0.000] (3.03%/**96.97%**)	0.205 [0.000] (10.61%/**89.39%**)	0.100 [0.000] (28.79%/**71.21%**)	-0.005 [0.200] (56.06%/**43.94%**)
	0.9	0.430 [0.000] (3.03%/**96.97%**)	0.405 [0.000] (6.06%/**93.94%**)	0.410 [0.000] (1.52%/**98.48%**)	0.205 [0.000] (24.24%/**75.76%**)	-0.005 [0.200] (51.52%/**48.48%**)

Table 3.4 Ambiguity attitudes per event

Table 3.4 analyses ambiguity attitudes per event (Eqs. 3.1-3.3), presenting the median event-dependent ambiguity aversion index per event and treatment. It shows that we have ambiguity seeking for the unlikely events E_1 and E_3 and ambiguity aversion for all other events, including E_5, except for E_7 and E_9 in the health treatment, where we have neutrality.

3.5.3 Parametric fitting

Table 3.5 shows that the ordering of goodess of fit is: (1) neo-additive; (2) Goldstein & Einhorn; (3) Prelec 2-parameter; (4) Prelec 1-parameter; (5) Tversky & Kahneman, both by the variance explained and by the AIC criterion. The AIC criterion corrects for the number of parameters used, but still the two-parameter families perform best. It is apparently important to consider both the aversion and the insensivitity component to study ambiguity, and focusing on one (Prelec 1-parameter considers only insensitivity) or combining the two (Tversky & Kahneman) loses too much explanatory power. Other than this, the ordering found is different than for risk (Stott (2006); Balcombe & Fraser (2013)). The reason is that insensitivity plays a more central role for ambiguity than for risk. Hence the neo-additive family, which can readily handle extreme degrees of insensitivity, fares best, and hence Prelec's 1-parameter family fares better here than Tversky & Kahneman's. That Goldstein & Einhorn fare some better than Prelec's two-parameter family may be because the former better separates the two parameters. In Prelec's family, the insensitivity parameter a overlaps some with the b parameter in also capturing some aversion. An advantage of the Prelec 2-parameter family is that it is more tractable analytically, and that it received a preference foundation (Prelec (1998)).

For all families, we repeated the analyses of the aversion and insensitivity parameters as done with our indexes. All analyses confirm the findings of our analyses in terms of the indexes.

	neo-additive	Goldstein-Einhorn	Prelec 2-parameter	Prelec 1-parameter	Tversky & Kahneman
least square R^2	0.234	0.232	0.229	0.202	0.083
maximum likelihood:AIC	-0.521	-0.518	-0.513	-0.484	-0.345

Table 3.5 Fit of parametric families

3.5.4 Further analysis

By monotonicity, m(j/10) should be increasing in j. We found that there is much insensitivity, with m only weakly increasing with a shallow slope. Because of this, and because of the randomness that is common in decision experiments, we can expect many violations of monotonicity at the individual level. We tested monotonicity in all cases possible. The second row in Table 3.6 gives the percentages of violations for the five treatments. These relatively high percentages, higher than are commonly found in experiments for decision under risk, confirm that there is much insensitivity. They also confirm that choices are most rational in the health treatment, second-most in the kid treatment, and they are least so, and about equal, in the remaining three treatments.

	basic	week	year	kid	health
violations of monotonicity	25%	28%	27%	19%	14%
correlations of indices	0.41**	0.35**	0.35**	0.14	0.35**

** $p < 0.004$

Table 3.6 Violations of monotonicity, and correlations between indexes

Although ambiguity aversion and a-insensitivity are conceptually distinct and orthogonal, they may well be empirically correlated. A positive correlation is natural because the indexes both concern deviations from Bayesianism and, according to many, deviations from rationality. The third row in Table 3.6 gives the correlations of the two indexes for each of the five treatments. The correlations all are significantly positive except in the kid treatment.

3.5.5 Discussion of experimental details

For the basic treatment with the unknown Ellsberg urn and also for the kid treatment, subjects could choose the colors/districts to gamble on. Otherwise, subjects might suspect that the experimenters had rigged the urns or chosen bad districts so as to pay less to subjects. A drawback of this implementation is that subjects can have an illusion of control (which can lead to over- but also to underestimation of likelihood, effects that may average out). Despite this drawback, our implementation is the favored one in the literature today because it avoids suspicion. That, immediately after having gambled on an event, the subjects gamble on the complementary event serves as a further measure against suspicion, making clear to subjects that we had no interest in rigging urns or districts.

We grouped events and their complements together to make likelihoods clearer to subjects and thus obtain replies of higher quality. This way we can directly measure Schmeidler (1989)'s indexes of ambiguity aversion. There, the matching probabilities m(j/10) that we measure are the capacities of the events E_j. Schmeidler (1989) and Dow & da Costa Werlang (1992) proposed the violations of additivity captured by

$$1 - m(j/10) - m(1 - j/10) \tag{3.11}$$

as indexes of ambiguity aversion. They are the sum of the event-dependent ambiguity indexes (Eq. 3.1) of E_j and its complement E_{10-j}, and they have been widely used since. Our index b of ambiguity aversion is an aggregation of Schmeidler's indexes for the pairs (E_1,E_9), (E_3,E_7), and (E_5,E_{5c}).

Two treatments in our design are hypothetical, even though there are many reasons to prefer real incentives to hypothetical choice. That the health treatment had to be hypothetical, and that this cannot be avoided due to the artificial nature of the Ellsberg experiment, which is one of the main messages of this paper, will be explained in section 3.6. One of the purposes of the third treatment was to test the hypothetical bias. That we found no differences between this treatment and the second, incentivized, treatment suggests that there is no hypothetical bias in our design. The good quality of the results in the health treatment, better than in the other treatments, and with subjects apparently well motivated, further suggests that we have no hypothetical bias.

Because of the high insensitivity that we found, with many violations of monotonicity, the behavior of our subjects in the first three treatments comes close to models of complete ignorance, where all ambiguous events are treated alike, leading to a maximal insensitivity index of 1. Such behavior was axiomatized by Cohen & Jaffray (1980), and underlies the modeling of ambiguity in Jaffray (1989). Gul & Pesendorfer (2014) developed an extended version of Jaffray's model, avoiding the use of exogenous objective probabilities. Their Axiom 3 for diffuse events, and the explanation following it, allow for complete insensitivity and violations of strict monotonicity.

3.6 General discussion

We first discuss the basic treatment with the classical Ellsberg urns (10 colors) and monetary outcomes. Here we find the usual prevailing ambiguity aversion as appearing from our a-index. In particular, for the ambiguous fifty-fifty event of five colors, which is similar to the two-color Ellsberg paradox, 79% of the subjects exhibited ambiguity aversion.[7] For unlikely events (one and three colors), a-insensitivity has an

[7] The events of the three-color Ellsberg paradox cannot be readily compared to our events of similar likelihood (with three or seven colors). The three-color Ellsberg paradox has one unambiguous

effect opposite to ambiguity aversion, giving ambiguity seeking. This is what we find, with over 80% of the subjects exhibiting ambiguity seeking. This finding confirms Ellsberg's prediction of the 1960s(Ellsberg (2001, p. 203 ll. 12-14; pp. 205-206)) and agrees with common empirical findings. Combined with ambiguity aversion for likely events it gives an estimated insensitivity index of 0.80, showing that this component is also present in the traditional Ellsberg setting.

The variation of outcomes, keeping the Ellsberg urn of the basic treatment but using short incentivized waiting times as outcomes, or using long and unincentivized waiting times, did not have any effect on the indexes or on the ambiguity aversion for any event. Changing the source of uncertainty from Ellsberg urns to districts of children while keeping the monetary outcomes of the basic treatment greatly affected ambiguity attitudes.

The aforementioned findings answer the questions raised in the introduction. Ambiguity attitudes do depend on events, and more so than on outcomes. Hence modeling ambiguity attitudes through event functions is more natural than through outcome functions. Although ambiguity aversion is prevailing, it is not strong and for unlikely events we even find the opposite. Hence, ambiguity aversion cannot be treated as a universal phenomenon.

Our deviating findings for the kid treatment are unsurprising given that we deliberately induced positive emotions for these events. In this sense our finding is similar to Tversky & Fox (1995), whose finding of ambiguity seeking for ambiguity related to basket ball under basket ball fans is similarly unsurprising. Readers may want to criticize these findings for being generated by specific details of events and not being representative of general ambiguity. Let us first agree with the latter point. However, and this is in fact our point, ambiguity attitudes have always been generated by particular event-specific emotions. The uncertainty in the Ellsberg urns, and many related experiments, is particular with relevant information deliberately kept secret for no clear reason and contrasted with other events for which no such secrets are kept. We face natural uncertainties with no probabilities known in our decisions every day, but they rarely involve Ellsberg-type aversive secrets.

The great preference of our subjects for the district uncertainty over the Ellsberg urns can be criticized for being irrational. The only outcomes resulting for our subjects are money amounts, and only the likelihoods of these obtaining are relevant. Those likelihoods are equivalent when generated by 10 balls or when generated by 10 districts of a nice child. No outcome, neither the fates of our subjects nor the fate of the child will be different in the two situations. Hence the two decision situations should be treated the same. Yet our subjects let the pleasant thoughts about the

event, whereas in our case all events within one source are uniformly ambiguous.

charity for the child leak into positive evaluations of prospects. They will also let the negative thoughts about concealed information leak into negative evaluations of prospects. We agree that such leaking is irrational. Such irrational interaction between the events and the value of the prospect reflects a violation of Savage (1954) independence of tastes and beliefs. Yet such interactions underly every nonexpected utility model.

Our findings show that events rather than outcomes influence ambiguity attitudes. It could be conjectured that ambiguity attitudes could then still be modeled through utility functions, by using different utility functions of the same outcomes for different sources of uncertainty (§2). However, several studies tested such dependencies of utility and did not find it (Abdellaoui et al. (2011); Abdellaoui et al. (2013); Abdellaoui et al. (2014)). Another problem of outcome-function models is that they are not homeomorphic, meaning that they cannot reflect natural psychological processes. Ambiguity concerns events and how people feel about those, and not how they feel about outcomes. At any rate, based on our empirical findings we conclude that the outcome based ambiguity models do not work well for empirical purposes.

The health treatment with life duration depending on unknown viruses can less readily be compared to the other treatments because both the source of uncertainty and the outcomes differ from the other treatments. This treatment serves to avoid the emotions generated by unnatural sources of uncertainty in the other treatments, and in this sense to be more representative of natural uncertainties. Accordingly, a crucial feature of this treatment is that the uncertainties with known and unknown probabilities refer to similar kinds of uncertainties, having to do with viruses and fighting them. There are no emotional asymmetries between known and unknown probabilities where one is associated with positive or negative social emotions and the other is not. The only real difference between the two uncertainties in the health treatment is that one deals with new risks for which no statistics are available yet, and the other deals with known risks for which we do have statistics. This is the essence of the difference between ambiguity and risk. Hence we feel that the health treatment is closest to natural uncertainties and ambiguities.

In one respect the health treatment cannot be realistic, which forced us to resort to a hypothetical gedanken experiment and hypothetical choice. It is that we should have a number of uncertainties given beforehand that are completely exchangeable and symmetric in every respect. We need this feature for direct comparability with the Ellsberg urn which is the topic of this paper, and where such symmetry is central. That such symmetries are virtually absent from practice, and that it is virtually impossible to come up with a realistic example of this kind with real incentives, can be held against the representativeness of the Ellsberg urns for applications. It may be more interesting to study general sources of uncertainty and their general characteristics without trying to directly relate to the ambiguity attitudes of Ellsberg urns. Abdellaoui et al. (2011, second experiment) showed how the source method can be used in such cases. In the present paper, however, we focused on the ambiguity attitudes mostly studied in the literature as yet, to shed light on the general validity of the Ellsberg experiments.

The high sensitivity in the health treatment, and absence of ambiguity aversion, are remarkable. Subjects discriminated different levels of likelihood considerably better than in the other treatments. It suggests greater interest and better motivation on the part of subjects, even though this treatment could not satisfy the real incentive principle of experimental economics. It has been observed before that subjects are well motivated to answer questions about health, even if hypothetical (Bleichrodt & Pinto Prades (2009, end of §2)). In the same spirit, many people voluntarily donate money to support medical investigations.

That ambiguity attitudes found in Ellsberg urns are not very representative of natural ambiguities, and that ambiguity attitudes can depend much on the source of uncertainty involved, can be taken as bad news. It increases generality at the cost of predictability. We think that the variety of uncertain events that we face is too rich and varied to expect simple laws to hold in great generality There are too many kinds of uncertainties, and non-Bayesian (irrational!?) ways to think. In this sense, ambiguity, i.e. nonprobabilized events, can be compared to commodities, say nonmonetary outcomes. One cannot expect one kind of utility function to hold for all nonmonetary commodities, or utility to be more concave for all nonmonetary outcomes than for monetary outcomes. Our empirical domain is, simply, too rich. General attitudes towards uncertainties, and relations between them, have to be studied so as to maximize predictability and to reduce generality as much as possible.

We chose waiting time as outcome because there is much interest in the effect of ambiguity on optimal stopping times. See Della Seta et al. (2013), Miao & Wang (2011), Nishimura & Ozaki (2004), Nishimura & Ozaki (2007), and Riedel (2009). The results of these studies could be distorted if ambiguity attitudes towards waiting time were different from other ambiguity attitudes. It may then be reassuring that ambiguity attitudes do not change for waiting times. We chose life duration as third outcome because this is a central outcome in the health domain.

3.7 Conclusion

We find no support for outcome dependence of ambiguity attitudes in between-outcome comparisons, i.e., when changing outcomes while keeping the source of uncertainty fixed. This adds to the same finding in the literature keeping outcomes fixed while changing sources. We do find support for event-dependence of ambiguity attitudes between sources, i.e. when changing the source of uncertainty. This evidence supports event-based theories of ambiguity such as prospect theory and multiple priors against outcome-based theories such as the smooth model.

We also find support for event-dependence of ambiguity aversion within sources, where aversion changes to seeking if events change from likely to unlikely. This finding again supports event-based ambiguity theories against outcome-based theories. It also supports the importance of insensitivity besides aversion to ambiguity. For parametric families to analyze ambiguity, more than for risk, it is important to capture insensitivity (inverse-S) properly. Hence parametric families that can properly model insensitivity will be most suited to analyze ambiguity attitudes.

3.A Appendix

3.A.1 Instructions

The virus story

Imagine that you are diagnosed with a certain disease. You have to receive a treatment against the disease. There is no possibility to abstain from treatment. The only choice you have is which treatment you will receive. Research on the disease all over the world has revealed the following facts: There are ten possible viruses causing the disease (i.e. virus 1, virus 2, virus 3, virus 4, virus 5, virus 6, virus 7, virus 8, virus 9, and virus 10). The prevalence (the rate of occurrence) of the viruses causing the disease is unknown. And there is no way to diagnose which virus you have; they all lead to the same disease. (The viruses are mutually exclusive; you will always have just one virus). Only if the real virus is treated will the disease be totally cured.

Treatment K

Treatment K treats the disease with a Known success rate. The success rate is known from experiences with previous patients. It uses a broad-spectrum antiviral supplement, which is not specific to any one of the viruses, but is generally effective for all viruses alike. For example, for treatment K, the success rate can be 10%. We will also consider other possible success rates. If you are cured by treatment K, you will live 50 years longer from now on in good health; otherwise you will live only 1 year longer from now on in good health.

Treatment U

Treatment U is new. It uses ten different supplements. We name the ten supplements S1, S2, S3, S4, S5, S6, S7, S8, S9, and S10 respectively. Each supplement is effective for the corresponding virus. (For example, supplement S7 is only effective for virus 7.) However, different supplements are not always available. You will therefore be treated only with the available supplements. Remember that there is no way to tell which virus causes your disease. If the right supplement for the real virus is chosen, then you will be cured and live 50 years longer from now on in good health; otherwise you will live only 1 year longer from now on in good health.

Indian Districts

One bets not only on urns but also on an option of Children's welfare (Option C). Option C concerns school-building work in rural India. The Satya Bharti School Program aims to make available free, high quality primary and secondary education to poor children in the rural areas of India. The kid in the photo is one of the kids whose lives have been transformed because of this program. The program helped 100 villages in 10 districts in India and the kid is from one of the 100 villages. You can bet on the district where the kid's village belongs.

3.A.2 Web Appendix

See `http://people.few.eur.nl/wakker/pdf/webapp_indian_kids.pdf`

Chapter 4

If Nudge Cannot Be Applied: A Litmus Test of the Readers' Stance on Paternalism[1]

4.1 Introduction

Descriptive models in behavioral economics were devised to capture empirical deviations from classical decision principles. Studying these descriptive models helps us to better understand certain empirical patterns of inconsistencies and/or biases of human behavior in decision making. Further, taking the classical principles as normative, the empirical deviations from them, captured by descriptive models, leave space for improving the empirically observed decisions. Paradoxically, the prescriptive[2] value of normative models depends on the existence of such deviations; as Raiffa (1961) put it: "We do not have to teach people what comes naturally."

Attempts at prescriptive improvements of decisions can lead to paternalism, and consequently ethical and moral objections can be raised. There have indeed been many debates about paternalism. A central issue is whether one (for instance, a decision analyst) can ever go against the stated preferences of the people affected. Critics of paternalism emphasize that one should never impose a choice on people against their own will.

[1]This chapter is based on the paper "If Nudge Cannot Be Applied: A Litmus Test of the Readers' Stance on Paternalism" co-authored with Chen Li and Peter P. Wakker, and published in Theory and Decision

[2]Prescriptive applications use normative models to improve decisions but reckon with descriptive limitations.

A new impulse for decision theory came from libertarian paternalism, often referred to as nudge, following Thaler & Sunstein (2003). A clever way was found to improve empirical decisions using the techniques of behavioral economics while never going against the will of the people affected. People are nudged in the right direction of choice, while left with full freedom of choice. This approach can be used in situations where people have no clear preferences, and it has led to a variety of applications (Jones et al. (2011) and Thaler & Sunstein (2008)).

In practice, situations arise where people's preferences are explicitly stated while at variance with normative principles. Then the *central question*, critical to debates on paternalism, is whether a decision analyst can seek to improve decisions while going against the stated preferences of a client, or whether one should respect stated preferences and forego purported improvements. Libertarian paternalism avoids addressing this central question. We use four thought experiments to show that this question cannot always be avoided. In each of these four experiments, a dilemma is faced, and a paternalistic decision should be made or forgo ne, with no possibility of avoiding the dilemma comprised in the central question.

The exact details of our four thought experiments are unlikely to arise in practice, and they were not developed for this purpose. They constitute gedanken-experiments common in philosophy, serving to maximally clarify the relevant issues. These issues are central in many practical decisions, playing a role in all decisions that affect other people. The four thought experiments represent real decision situations where the dilemma in the central question cannot be avoided, and a decision has to be made, one way or another. This leads to our main purpose: we provide a litmus test on the readers' stance on paternalism, and a way to organize stances in the literature. Our secondary purpose is to convince the readers that paternalism itself is sometimes appropriate, and consumer sovereignty should not always be respected . However, this argument is controversial and no consensus is likely to come soon.

4.2 Views on paternalism

Paternalistic interventions often improve people's well-being. Taxation policies are used to regulate addictive behaviors such as smoking. The "no drink driving" regulation that helps reduce traffic accidents is another example. Yet many objections to paternalistic interventions can still be raised. One such objection is based on a desire to avoid power manipulations. Paternalistic interventions give decision analysts/policy makers the power to influence the decisions of others, and may be misused for self-interest rather than for the well -being of the people affected. Although this is a strong argument against paternalism, it will not be considered in this paper for reasons of simplicity. We focus on what we called the central question and assume that the only purpose of a decision analyst is to optimize others' wellbeing.

A second objection against paternalism takes issue with the assumption that particular decision principles can be qualified as normative. In reply, our analysis will only assume stochastic dominance and transitivity as normative principles, whose normative status is uncontroversial, although some doubts have still been expressed (Bell (1982); Loomes & Sugden (1982) and Mandler (2005)). We emphasize that our discussion needs no commitment to a normative status of expected utility or any other specific optimization theory. Our examples also pose a dilemma for people who advocate using heuristics rather than optimization theories (Berg & Gigerenzer (2010)).

A third objection extends the second one. The argument is that individual decision makers' preferences reflect their true values which they know better than any outsider can. Therefore, it is best for individual decision makers to decide for themselves what is best for them. This objection is sometimes supported by Hume's famous citation "reason is, and ought only to be the slave of the passions", where preferences are taken as passions that should never be overruled by paternalistic reasons (Hume (1740)).[3]

A fourth objection is that people should always have the freedom to follow their own preferences, irrespective of whether these preferences are optimal in some sense or not (McQuillin & Sugden (2012, p. 556)). In a similar spirit, it can be argued that people need not get what others think to be best; instead, they should get what they choose for themselves. In defense of this objection, some may advance an evolutionary interpretation, claiming that the best preferences will then survive in the long run. Others may base their arguments on direct ethical principles, assigning an intrinsic value to freedom of choice (Sugden (2004)).

For later discussion, we group these four objections, while raised from different perspectives, under the name anti-paternalism. Other terms used in the literature for anti-paternalistic positions are the Humean view on preference, or consumer sovereignty.

Several moderate positions have been advanced. Cautious paternalism (O'Donoghue & Rabin (1999)) and libertarian paternalism are similar, combined in soft paternalism (McQuillin & Sugden (2012, p. 560)). They advocate interventions, such as smartly designed choice structures, education, and/or incentive schemes, which nudge people to better choices, while keeping the ultimate decision power with individuals. In this way, people without a stable and consistent preference will be influenced by the

[3]Our interpretation of Hume's citation is different, and we think that it does not preclude paternalism. Broome (1993) and Sugden (1998, p. 48) present critical discussions.

structure and therefore go for the choice that is most probably the best for them. On the other hand, those who choose differently from the paternalistic suggestion are more likely the ones who understand themselves better and opt for a genuinely different preference.

Asymmetric paternalism (Camerer et al. (2003)) gives a special importance to rational individuals. It purports that rational individuals, who can independently make the best decisions, should be harmed as little as possible. Therefore, in cases where a paternalistic decision needs to be made for a group of people, but a separation between rational and irrational individuals is impossible, priority should be given to ensure that no harm is done to rational individuals. Paternalistic interventions should be restrained even if, on average, they benefit ed the majority of people.

Moderate positions make paternalistic interventions less controversial as they take into account an individual's own will. However, their applicability is limited. In many situations, they cannot provide us with guidance, as our thought experiments will demonstrate.

The most far-reaching form of paternalism is optimal paternalism, advocated by Zamir (1998) and O'Donoghue & Rabin (2003). They argue against the overweighting of rational individuals in asymmetric paternalism, and favor occasional coercive paternalistic interventions. The latter can be justified in situations where the cost of allowing irrational individuals to make errors significantly exceeds the harm brought to rational individuals by paternalistic interventions. These authors recommend using cost benefit analyses to find optimal policies, thus taking a purely economic perspective.

4.3 The decision to be taken, and two quality-of-life measurements

In the thought experiments that we now consider, you can imagine yourself to be a decision analyst (or a doctor) who has to decide on a medical treatment for a client. Your client's status quo is that he suffers from impaired vision. There is only one treatment available that may cure him and bring back perfect vision, but there is a risk of failure, leading to complete blindness. You must choose between treatment and the status quo (no treatment) for your client.

Unfortunately, in addition to impaired vision, your client is now unconscious, which is only temporary, has no serious consequences, and does not affect vision or health. This means that you cannot communicate with him now. You cannot wait until the client regains consciousness, because t he treatment works only if carried out immediately . Hence arguments for patient autonomy play no role here. For simplicity, we assume that the treatment is cheap, simple, and has no side effects. We assume that medical arguments give no clear verdict. The subjective quality-of-life perception of your client regarding the vision levels is the deciding factor.

As explained before, we assume that your only interest is to choose what is best for your client. You do not have to account for your decision to any outside party, but you do account to your own conscience. Legal considerations play no role, but moral ones do. Note that both a choice of treatment and a choice of the status quo are your choice, and morally you are equally responsible for both.

Your decision is based on information about the quality of life evaluation of the status quo (called status quo quality from now on) as relative to the quality of treatment (called treatment quality from now on). To determine the former, we consider two quality of life measurement methods commonly used in the health domain. We assume that, besides these two measurements, no other relevant information about the status quo quality is available. In particular, you cannot make your own assessment of this quality and must go entirely by the information from the quality of life measurements.

The first, traditional, measurement method is the *probability equivalent* (PE) method, often called the standard gamble method in the health domain (Drummond et al. (1997)). In the measurement survey, participants are asked to choose between the status quo (indicated by a neutral face symbol in Figure 4.1) and a hypothetical treatment. All treatments considered either result in complete recovery with perfect vision (indicated by a smiley symbol) or the worst possible outcome of complete blindness (indicated by a frownie symbol). The treatments differ only in their success ratio - the probability of complete recovery (p in Figure 4.1). Participants are asked the question in Figure 4.1.

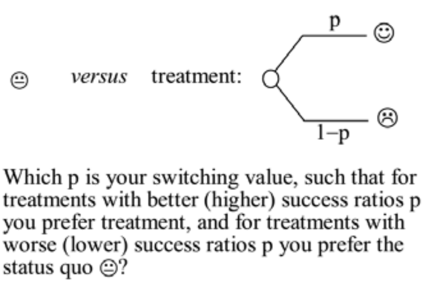


Figure 4.1 Probability equivalent question

The switching value p is an index of the quality of life of the status quo. We call p the *status quo quality*, abbreviated *sqq*. The higher sqq is, the better the status quo is judged to be by the client. Although this method is often analyzed assuming expected utility, we do not need this assumption in what follows. We will never assume more than transitivity and stochastic dominance.[4]Given the switching value sqq of the client, we have the following indifference:

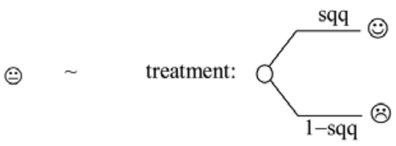


Figure 4.2 Status quo quality (probability sqq)

The second measurement method is the *certainty equivalent* (CE) method, which is indirect and more laborious. Each survey participant is presented with a rich set of vision levels varying from perfect vision to blindness. The status quo is contained in this set. Initially, some treatment with success ratio p_1 is chosen. Participants are asked the question in Figure 4.3:

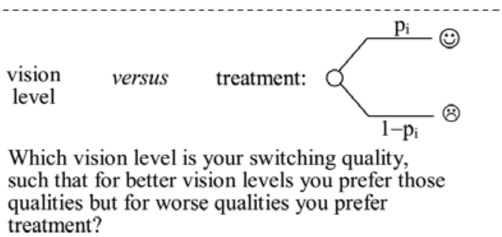


Figure 4.3 Certainty equivalent question

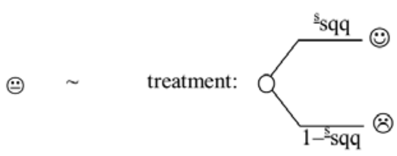


Figure 4.4 Second status quo quality (probability $^{\underline{s}}sqq$)

If the switching vision level given by the client is better than the status quo, then the success ratio is decreased, and if it is worse, then the success ratio is increased. The choice question is repeated with the new success ratio. This process goes on

[4]Our assumption of indifference at the switching value amounts to a kind of continuity, but is not essential to our arguments and is only made to simplify the presentation. Strict preferences at either side of the switching value suffice for our analysis. The fact that a choice must be made in each decision situation does not reflect a completeness assumption, but is intrinsic to the decision situation. This is discussed further in section 4.5.3.

until a success ratio p_i is found for which the switching vision level is the status quo. The resulting success ratio is again an index of quality of life of the status quo. We abbreviate it as the *second status quo quality*, or ${}^{\underline{s}}sqq$. The higher ${}^{\underline{s}}sqq$ is, the better the status quo is judged to be by the client. We now have the following indifference in Figure 4.4.

It is natural to expect that $sqq = {}^{\underline{s}}sqq$, which follows from the classical decision principles of transitivity and stochastic dominance. The equality is especially natural if the interviews were conducted carefully, with clients understanding the stakes well and (if possible...) being proficient in decision theory. It would also help if interviewers could interact with the subjects and influence answers, which is useful in prescriptive as opposed to descriptive applications (Keeney & Raiffa (1976, §5.8) and Payne et al. (1999) and Edwards & Elwyn (2006)). However, we assume that the measurements were conducted as is common in most descriptive and applied studies, where there is only limited time for the measurements and explanations, where there can be no interaction to influence answers, and where interviewers themselves are often not well versed in decision theory. These limitations are unavoidable in large-scale quality-of-life measurements, and obviously preclude any interactions to discover or correct violations of decision principles. In this case, violations of the equality $sqq = {}^{\underline{s}}sqq$ can result.

4.4 The four cases

This section presents the four thought experiments used to identify stances on paternalism. Henceforth, we will call them four cases. From now on, imagine that the only treatment available for your client has a success ratio 0.90. Because the success ratio is an index of the quality of the treatment, we call it the *treatment quality*, or *tq*, in analogy to sqq. The decision situation faced by you is depicted in Figure 4.5.

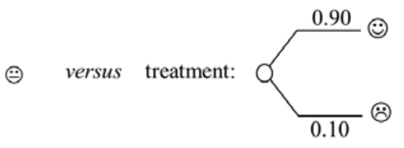


Figure 4.5 The real treatment decision

In each of the following four cases, our question to the readers will be: given that the tq of the treatment is 0.90, would you choose treatment or the status quo for your client? The readers may want to make up their own mind immediately after reading each case, and before reading the pro and con arguments put forward by others.

CASE 4.1S [Statistical information]. You do not know the sqq of your client. But you do know the sqq, based on the PE method, of 10,000 similar clients with the same status quo vision level as your client. The mean, median, and modus sqq over these 10,000 clients are all 0.91, slightly exceeding the tq of 0.90.□

In Case 4.1S, although the sqq of your client is unknown, you probably consider 0.91 to be the best estimate of your client's sqq, based on the information gathered from 10,000 similar clients. You will probably choose for the status quo, which follows naturally from the analysis in Figure 4.6, which assumes only transitivity and stochastic dominance.[5] We expect this to be the answer of most readers (when first reading).

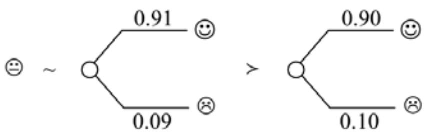


Figure 4.6 The analysis of Case 4.1S

CASE 4.2DS [Double Statistical information]. In this case, besides the sqqs of Case 4.1S, you also have information about the $sqq \stackrel{s}{=} sqq$ of 10,000 similar clients. The mean, median, and modus of $sqq \stackrel{s}{=} sqq$ are, however, 0.85, which is considerably lower than the sqq of 0.91 found before, and the tq of 0.90.□

In Case 4.2DS you face a large inconsistency in the data. At least one of the two measurements is incorrect and cannot reflect a genuine maximization of happiness. A natural first reaction is that the data are of poor quality, and that no decision should be based on such poor data. You would want to obtain better information. This could be done by interacting with 10,000 similar clients, or at least with some of them, to reconcile the inconsistencies. You could also communicate with the client or people close to him. However, in all of the four thought experiments, we assume that you have to make a decision now, with no other information available, nor any possibility to interact. Such situations are common in practice, especially in policy decisions. Facing the incoherence in revealed preferences, our thought experiment requires that you, as a decision analyst, devise a strategy to resolve it. Bear in mind

[5]The left indifference follows the PE measurement (Figure 4.2) with sqq = 0.91. The strict preference follows from stochastic dominance. The preference of status quo over treatment then follows from transitivity.

that a refusal to take any action is not possible. Doing nothing means choosing the status quo. As an sqq of 0.91 suggests a preference for the status quo, whereas an $sqq =^{\underline{s}} sqq$ of 0.85 suggests the opposite, the two methods give contradictory suggestions. You may reason that, if the truth is in the middle between 0.91 and 0.85 in Case 4.2DS, then 0.88 is the best estimate of the status quo quality. It is below the treatment quality tq = 0.90, suggesting that treatment is better. This answer is enhanced by the observation that a wrong treatment decision is off by only 0.01 if the sqq of 0.91 reflects the true preference, whereas a wrong choice of the status quo is off by 0.05 if the $sqq =^{\underline{s}} sqq$ of 0.85 reveals the truth. Hence, we expect most readers to choose treatment in Case 4.2DS.

CASE 4.3SI [Statistical and Individual information]. In this case, besides the sqqs and $sqq =^{\underline{s}} sqq$ of the previous cases, you also know the sqq of your client, and it is 0.91.□

Here we expect the readers to be divided. Although the information about 10,000 clients reveals large inconsistencies, the information about the preference of the client himself does not. His sqq exceeds the tq, suggesting a preference of the status quo over treatment. It is natural to think that the client's own stated preference should be more relevant than evaluations by other similar clients, further supporting your choice of the status quo. Yet an argument for treatment can also be supported. Although there is no direct evidence of inconsistent preference of your client, you can expect that he is probably like the 10,000 others, and is probably subject to the same inconsistencies.Taking possible inconsistencies into account, you can still adhere to the treatment decision of the previous Case 4.2DS, and also choose treatment in the present Case 4.3SI.

CASE 4.4I [Individual information]. In this case, you do not have the information on similar clients. You only know the sqq of your client, which is 0.91.□

Case 4.4I seems to be even clearer than Case 4.1S. Unlike Case 4.2DS and Case 4.3SI, there are no apparent inconsistencies in the data available on this case. Given that your client's sqq exceeds tq, we expect most readers to prefer the status quo here.

4.5 Using the cases as a litmus test on paternalistic views

Table 4.1 lists the most common stances, ordered from least to most paternalistic, with the number of tr (treatment) choices increasing.

4.5.1 Invalid stances

Before discussing valid stances, we first discuss some invalid ones. *Refusal stance.* This stance entails the refusal to take any decision in Case 4.2DS. Strictly speaking, this stance is not possible in our thought experiments, but we know from experience that some readers will still want to consider it. The argument for refusal can be that no decision can be taken on such poor information, and one really has to get better information. Expressing doubts about decisions without committing to any may be possible in philosophical debates, but is not possible in our thought experiments, as it is usually not in practice. A purpose of our thought experiments is to show that taking a position on reconciling inconsistencies is unavoidable, contrary to what the refusal stance entails. People sympathetic to the refusal stance may include practitioners reluctant to involve in gedanken-experiments, and experimenters reluctant to consider hypothetical choice, even though the latter is essential in prescriptive applications (Keeney & Raiffa (1976, §1.4.3)). We will not consider the refusal stance any further.

The ostrich stance [row 1 or 2]. A widespread misunderstanding is to think that if your data do not show any violation of a model, then you may use that model for data analysis, even though other studies designed to test the model did find violations.[6] The data of Cases 4.1S and 4.4I do not show a violation of expected utility and, hence, according to the ostrich stance, one may use expected utility to analyze these cases. Then the status quo is chosen, as easily follows from transitivity and stochastic dominance (conditions verified by expected utility). Thus weak anti-paternalism in row 2 may result, or, more likely, anti-paternalism in row 1.

Many applications of expected utility and, for instance, many analyses of PE and CE measurements, are based on this misunderstanding. We disagree with the ostrich stance. In the Cases 4.1S and 4.4I, no violations of expected utility showed up only because the data did not test for them. Assuming absence of violations is like a doctor who declares a disease non-existent simply because he did not test for it. Inconsistencies are not the real problem, but they are a symptom of the real problem.

[6]See Birnbaum (1992, p. 21, 2nd column, 2nd para), Cohen & Einav (2007, pp. 746-747) and Diamond (2008, p. 1860 1st para)). A related problem is that many medical applications use PE measurements as the gold standard, based only on the normative expected utility foundation, without concern about the many descriptive biases that have been documented (Drummond et al. (1997); Torrance & Feeny (1989, p. 560))

	4.1S (Statistical) sqq=0.91	4.2DS (Double Statistical) sqq=0.91; $\underline{{}^{s}}sqq$=0.85	4.3SI (Statistical and Individual) sqq=0.91; $\underline{{}^{s}}sqq$=0.85; individual sqq=0.91	4.4I (Individual) individual sqq= 0.91
1. Antipaternalism	sq	tr	sq	sq
2. Weak antipaternalism 1	sq	tr	tr	sq
3. Weak antipaternalism 2	tr	tr	sq	sq
4. Weak paternalism	tr	tr	tr	sq
5. Strong paternalism	tr	tr	tr	tr

tr: choose treatment; sq: choose status quo

Table 4.1 The most common stances on paternalism

The real problem concerns the biases in people's preferences. They will be present in Cases 4.1S and 4.4I as much as in the other two cases. Evidence for general violations of expected utility is reviewed by Camerer (1995) and Starmer (2000). Violations for the types of questions considered in our thought experiments are referenced later.

Non-statistical stance [row 1 or 3]. Many people erroneously think that statistical information is not relevant to individual cases. Therefore, the data of the 10,000 similar subjects supposedly should play no role in the treatment decision of your client. This stance will choose the status quo in Cases 4.3SI and 4.4I, only paying uncritical attention to the information coming from one individual client there. This stance can lead to anti-paternalism in row 1 or weak anti-paternalism in row 3. The non-statistical stance is surprisingly widespread (Steiner (1999)). In the health domain, it is often based on the Hippocratic Oath that doctors take, swearing to do what is best for their patient (Fuchs (1974)). Many doctors erroneously take this to imply that they can ignore not only the interests of patients other than their own, but also relevant information from patients other than their own. In an historical study, Murphy (1981) discusses the non-statistical stance in medicine in France in the 19th century. This stance also underlies the distinction often made between statistical lives and identified lives. Society commonly weighs identified lives higher than statistical lives (Schelling (1968)), leading, for instance, to overspending on the treatment of rare diseases.

Some extreme advocates of the frequentist interpretation of probability, and critics of the concept of subjective probability, are also open to the non-statistical stance (Gigerenzer (1991, pp. 260-261, Examples 1 and 2); Lopes (1981) and Shackle (1949, p. 71)). Such views also appear in discussions of the Monty Hall problem. For instance, Baumann (2005) wrote, supporting the non-statistical stance: "If the best argument so far for switching in an isolated individual case (not in a series of cases) fails, then one might wonder whether probabilistic arguments say anything at all about isolated individual cases."

Incoherence stance [row 1]. Berg & Gigerenzer (2010, p. 148) argue that there is no irrationality in inconsistent preferences, writing "No studies we are aware of show that deviators from rational choice earn less money, live shorter lives, or are less happy." They argue for ecological rationality, with decision heuristics adapted to environments. Their ecological argument is not useful for the dilemma presented here and, in general, is tangential to the problems of inconsistency. Our gedanken-experiment requires a position to be taken regarding the inconsistency, and environments and ecologies provide no escape from the dilemma. Other arguments in favor of outcome-oriented ("correspondence") policies, with no direct concern for internal coherence, have been advanced (Friedman (1953); Hammond (2006) and Smith (2008)).

These authors do not reject the usefulness of resolving inconsistencies explicitly as Berg & Gigerenzer (2010) do.

4.5.2 Valid stances

Strong paternalism [row 5; our stance]. To position our upcoming discussion of various valid stances, we start with our own. We recommend treatment in all cases. In Case 4.2DS, we expect that virtually all readers will choose treatment. The information, poor as it is, directs to this decision, as discussed before. In Case 4.3 SI, the individual information added suggests that the client is like the 10,000 others, making it likely that he is subject to the same inconsistencies as the others. Hence, we recommend treatment here as we do in Case 4.2DS.

In Cases 4.1S and 4.4I, our data contain no inconsistencies. Yet, following up on our criticism of the ostrich stance, many studies have demonstrated that discrepancies as found in the other two cases do occur. General discrepancies of such a fundamental nature, called preference reversals, were first demonstrated by Lichtenstein & Slovic (1971), and have been extensively confirmed since (reviewed by Slovic (1995)). Comparisons of PE and CE measurements invariably found higher PE values, as in our example.[7] Such discrepancies show that at least one of the measurements concerned contains biases, and the million dollar question then is what these biases are and how to correct for them.

Behavioral economics can serve to provide diagnostic tools to identify the biases underlying the above discrepancy. Bleichrodt (2002) showed that most biases occur for the PE measurement, which generates serious biases upward.[8] This further supports our recommendation of deviating from the status quo decision suggested by the sqq = 0.91 observation, and of choosing treatment instead. Hence we also recommend treatment in Cases 4.1S and 4.4I, even though they do not directly reveal inconsistencies.

We expect that most readers did not know about the literature just cited when they started reading this paper, and hence favored the status quo in Cases 4.1S and 4.4I. We hope that now, after learning about this literature, they side with us and favor treatment in all cases. We similarly hope that readers who knew about this literature beforehand, favored treatment in all cases from the beginning. At any rate,

[7] See for instance Baron (1994, §17.1.4), Bleichrodt et al. (2001, p. 1505), Bleichrodt et al. (2007), Delquié (1993), Hershey & Schoemaker (1985), Johnson & Schkade (1989), Morrison (2000), Seidl & Traub (1999), Slovic et al. (1990, Study 5), Stiggelbout et al. (1994, p. 87).

[8] Other studies confirming the poor performance of PE measurements include Chilton & Spencer (2001), Doctor et al. (2010), Dolan (2000, p. 1745), Hershey & Baron (1987, p. 208), Jansen et al. (1998), Officer & Halter (1968, p. 260), Revicki & Kaplan (1993), Richardson (1994), Stalmeier (2002), Stiggelbout & de Haes (2001, p. 224), and Torrance (1987)

from now on, we assume the cited literature on biases to be known in the discussions that follow.

Weak paternalism [row 4]. This stance may be taken by readers who find an argument based only on related literature too weak to overrule the stated preference of the affected client. Then the status quo may be chosen in Case 4.4 I. On the other hand, paternalism is still considered necessary when no preference of the affected individual is clearly stated (Case 4.1S) or when there is clear evidence of inconsistencies in similar cases (Cases 4.2DS and 4.3SI).

Individual autonomy [rows 1 or 3]. Other readers will take the dividing line between cases where the individual preference is clearly stated (Cases 4.3SI and 4.4I) and those where it is not (Cases 4.1S and 4.2DS). They will not want to overrule the preference of an individual client based on inconsistencies among other clients (Molenaar et al. (2004, p. 2129)). It leads to the same decisions as the non-statistical stance but for more valid ethical reasons. Redelmeier & Tversky (1990, p. 1162) argued for the opposite: "Physicians and policy makers may wish to examine problems from both perspectives to ensure that treatment decisions are appropriate whether applied to one or to many patients."

Asymmetric paternalism [rows 1 or 3]. Going against the stated preference of the client means harming him if he is fully rational (in the sense of satisfying the classical normative decision-theory principles). Asymmetric paternalism wants to avoid harming rational clients, and recommends against treatment in Cases 4.3SI and 4.4I. We acknowledge the harm of choosing treatment for a rational client in the two cases, but think that it is far more likely that the client violates classical decision principles and, in this sense, is irrational. Asymmetric paternalism leads to the same conclusions as the individual autonomy stance whenever the level of rationality of our clients is unknown.

Strict sampling [row 2]. This stance concerns readers who find arguments based on the literature too weak. They are willing to overrule preferences that themselves contain inconsistencies as in Case 4.2DS, and they are even willing to do so based on similar observations as in Case 4.3SI. However, they find the inconsistencies in the literature, based on more remote samples and stimuli, too unconvincing. Libertarian paternalism. This stance may suggest, if possible, using only the CE measurements yielding $\underline{{}^{s}}sqq$, and avoiding the PE measurements yielding sqqs, in the above cases. However, such an escape from the dilemma is not possible for us, with PE measurements already available. Libertarian paternalism seeks to avoid dilemmas and offers no guidance for cases as those considered here.

Cautious and soft paternalism. These terms combine various positions that avoid coercion, and best correspond with the middle rows.

Anti-paternalism [row 1]. Row 1 combines the two versions of weak anti-paternalism. This stance holds that preferences are only overruled if they are inconsistent within themselves, so that overruling one way or the other in Case 4.2DS cannot be avoided anyhow. It also implies that one then never relies on indirect suggestions of inconsistencies, and hence favors the status quo in the other three cases. In this spirit, Molenaar et al. (2004, p. 2129) wrote: "The use of a decision aid did not influence the kind of treatment selected. This is a desirable outcome as the aim of the decision aid is to assist patients in the decision-making process, and not to prescribe a course of action." Bernheim & Rangel (2009) introduced a revealed preference model where preference, in general, can depend on the context ("ancillaries"), and on no information other than directly revealed choices. This model accepts preferences as non-suspect only in the special case where they are not context -dependent; i.e., they are not part of an inconsistency. This model will formally recommend the status quo in Cases 4.1S and 4.4I, and probably also in Case 4.3SI if it does not consider outside information, leading to strong anti-paternalism. Stances that avoid the responsibility of choosing treatment , for instance, in Cases 4.1S and 4.4I, could be considered to be forms of paternalism. Similarly, strict adherence to consumer sovereignty can be interpreted as paternalism. Own assessments of non-commitment are then given priority over assessments of the client's best interests. We will, however, adhere to the most common interpretations, where these stances are taken as what they aim to be: non-paternalistic.

4.5.3 Some related views on the (non-)existence of preference in the literature

We start with views that lead to strong paternalism. Throughout the history of decision theory , empirical studies have found inconsistencies in preferences that signal that biases are effective, and have argued for the desirability to debias (Edwards (1962); Loewenstein & Ubel (2008, §6.1.2)). One of the most notorious cases concerns the discrepancy between willingness to pay and willingness to accept (Schmidt & Traub (2009)). Many decision analysts recommended measuring utilities and other relevant quantities in several manners, and resolving inconsistencies using cross checks (Keeney & Raiffa (1976, §5.8.3); Kriegler et al. (2009, p. 5046); McCord & de Neufville (1985); Weinstein et al. (1996b, p. 1257).[9]

In one form of debiasing, one develops measurement tools that avoid biases.[10]

[9] A remarkable study is Elstein et al. (1986), who found a case where more than half of the physicians' judgments deviated from the recommendation of a decision analysis.

[10] The numerous references include Baron et al. (2001), Huber et al. (2001), Lefebvre et al. (2011), McFadden (2006), Tversky & Kahneman (1981). Loewenstein & Ubel (2008, pp. 1806-1807) discuss the health domain.

Such debiasing would, like libertarian paternalism, suggest using CE measurements rather than the more biased PE measurements, if possible. In our cases, such avoidance is not possible since the biased PE measurements have already been implemented. A second form of debiasing is still possible in our cases, because it is carried out ex post: If we have estimates of the biases, we can correct the measurements already obtained (Anderson & Hobbs (2002); Bleichrodt et al. (2001); Kahneman (2003, p. 1468); Viscusi (1995, last paragraph)).

Avoiding and correcting biases is, of course, common in all empirical sciences. It becomes delicate in decision theory if biases are interpreted as human irrationalities. In this case, no clear objectively correct gold standard is available (Tversky & Kahneman (1981, p. 437 3rd column)), and ethical complications may arise. We expect that authors working on debiasing are close to our paternalistic stance in row 5. Many authors have argued, as we do, that behavioral economics gives tools to improve human decisions (Diamond (2008); Kahneman (2003); McFadden (1999); and Oliver (2013)), which also suggests opposing anti-paternalism.

The constructive view of preference (Payne et al. (1999)) entails that preferences obtained in measurements are constructed on the spot by participants, just so as to answer unfamiliar questions. These constructed preferences have little or even no relation to the underlying true preferences. Some constructivists will conclude that we should dig deeper to find true preferences (McFadden (1999); Slovic (1995); Tversky (1996)). In our four cases, where we cannot dig deeper, we would have to accept that there are biases, and correct for them to our best ability , leading to our recommendations of treatment.

We now turn to a number of views of preference that lead to anti-paternalism. We start with constructivists who go in a direction opposite to the one considered above. They argue that true underlying preferences are a meaningless and non-existing concept (Güth (1995, p. 342); Starmer (1996); discussed by Camerer (1995, p.673)). As a result, they will probably not search for the implementation of true preferences, but prefer to minimize intervention in any form, leading to the anti -paternalistic stance. Closely related to the second constructivist stance just discussed is the view that people may not be able to have any preference at all in some situations, leading to incompleteness of preference.[11] This may, for instance, happen if different values are considered to be incommensurable. Incompleteness has often been defended in descriptive applications. In the prescriptive context considered here, this position is invalid, amounting to the refusal stance. Valid prescriptive variations of this stance will probably lead to recommendations that coincide with anti-paternalism.

[11] See Baucells & Shapley (2008), Bewley (1986), Danan (2008), Dubra et al. (2004), Gilboa et al. (2008, pp. 179-180), Mandler (2005), Nau (2006a).

For some authors, the volatility of observed preferences has led to a position between the existence and non-existence of preference: they consider preferences to be fundamentally random (Hey (2005); Regenwetter et al. (2011); Wilcox (2008)). We take their position as merely descriptive, but expect advocates of random preference not to seek for true preferences and, hence, to be sympathetic to anti-paternalism in our cases. However, random preferences are untenable in prescriptive applications: No-one would want a doctor, judge, or waiter in a restaurant to decide by a coin toss.[12]

Loewenstein & Ubel (2008) argue that many discussions of paternalistic interventions require speculations about genuine welfare. Examples are the speculations that people save too little for retirement, overeat, or lack discipline to quit smoking. The authors point out that such speculations, whether based on decision utility or experienced utility, are always problematic. In our gedanken-experiments, the choice inconsistency problem arises independently of any such speculation. Our recommendation of treatment in all four cases is based on the following two arguments. First, an underestimation of 0.90 relative to 0.91 is less serious than an overestimation of 0.90 relative to 0.86. Second, extensive literature suggests great overestimations of probabilities in PE questions. Neither of these two arguments entails direct speculations on genuine welfare.

There have been many other relevant debates in the literature on choice inconsistencies and ways to reconcile and improve them. A complete review of this literature is beyond the scope of this paper. We only briefly mention debates in intertemporal choice, where present and future selves can disagree, leading to intertemporal inconsistencies and self-control problems (Strotz (1956)). Some have argued that even for addictions, no outsiders should intervene ("rational addiction"; Becker & Murphy (1988)), which entails an anti-paternalistic stance. Others have favored interventions of benevolent social planners (Gruber & Köszegi (2001); Heil et al. (2003)).

4.6 Conclusion

To discuss the central question concerning paternalism that plays a role in many practical situations, we used a hypothetical gedanken-experiment to maximally clarify the relevant issues (mainly our Case 4.4I). The gedanken-experiment provides a litmus test for the readers' stance on paternalism. Providing this test was the primary purpose of this paper. It shows that one cannot avoid taking a position (disproving many claims to the contrary in the literature). Our thought experiments involved only minimal rationality conditions, being transitivity and stochastic domi-

[12] Randomization in game theory only serves to be unpredictable by opponents.

nance. Thus, we separated debates about paternalism from debates about rationality of expected utility or other theories.

We further argued for deviating from what at first sight seems to be the true and consistent preference of a client in some situations (but on closer inspection need not be, as we argued). Although it is easy to cast doubt on our stance, as sometimes it can lead to wrong decisions, it is not easy to suggest better stances. Every other stance can also lead to wrong decisions, especially if ignoring a large literature on welldocumented biases, or when serving only to avoid responsibility. The latter is impossible in our gedanken-experiment as it often is impossible in practice. For example, medical treatment decisions have to be taken one way or the other , and money spent on one treatment cannot be spent on another. Our analysis draws on the vast literature on biases in behavioral economics, and further clarifies how behavioral economics can be of use in prescriptive decision making.

Chapter 5

Improving One's Choices by Putting Oneself in Others' Shoes - An Experimental Analysis[1]

5.1 Introduction

This paper examines the well-known advice: "When deciding, imagine what others would do in your place." In an experiment, we investigate how predictions of others' choices made prior to own choices affect own choices. We, similarly, investigate how own choices made prior to predictions of others' choices affect those predictions. Our study is inspired by Faro & Rottenstreich (2006). In their Experiment 3 they primarily found that predictions move choices towards risk neutrality for a wide range of stimuli incorporating gains and losses and very small and very large probabilities. Under the strong rationality assumption that risk neutrality is normative for the moderate amounts considered (Tversky & Kahneman (1981); Kahneman & Lovallo (1993)), the latter effects are desirable. These results are promising. There is a renewed interest in procedures for debiasing and improving decisions in behavioral economics (Thaler & Sunstein (2008, nudge)). Thus, Faro & Rottenstreich (2006) conclude: "Our work suggests one such simple procedure: when choosing for yourself, start by making predictions of what others would do."

[1]This chapter is based on the paper "Improving One's Choices by Putting Oneself in Others' Shoes - An Experimental Analysis" co-authored with Kirsten I.M. Rohde and Peter P. Wakker

Our paper follows up on the suggestions made, adding the following contributions. First, we use real incentives throughout, agreeing with the requirements of experimental economics. Second, we add a clearer test of rationality, being reduction of preference reversals (weak rationality). In particular, our losses will concern the same final outcomes as our gains, and will only be different in terms of framing, as with the original Asian disease problem . Thus we test pure reference dependence while ruling out income effects. Third, we test a number of predictions of theories advanced in the literature: learning (Camerer & Ho (1999); Erev & Roth (1998)), construal-level theory (Trope & Liberman (2010)), risk-as-feelings (Hsee & Weber (1997)), and risk-as-value (Brown (1965)) combined with anchoring on previous answers.

To focus on the aforementioned questions, we keep our design as simple as possible in other respects. Thus, following Hsee & Weber (1997, p. 46), we only ask simple choice questions. We use exactly the choice stimuli of Vieider (2011). These test framing and reference dependence in their purest form, and use simple probabilities and moderate outcomes suited for salient real incentives.

We find risk aversion for gains, risk seeking for losses, and preference reversals, as common in the literature. Regarding the effects of prediction on choice, prediction enhances risk aversion, reaching significance for losses. There it enhances strong rationality by reducing risk seeking. Unfortunately, we do not find any effect on weak rationality, and preference reversals in choice are unaffected by prior prediction. Regarding the effects of prior choice on prediction, prior choice enhances predicted risk seeking. Our results agree with, primarily, strong anchoring (Faro & Rottenstreich (2006, p. 537)) and, secondarily, risk-as-value. Our results are also consistent with risk-as-feelings, because reducing the empathy gap as we did reduced the strength of risk-as-feelings.

5.2 Experiment

We analyze a monetary version of the Asian Disease problem. The instructions for participants are in the Appendix.

5.2.1 Stimuli

To ensure anonymity and avoid communication during the experiment, participants were seated in sight-shielded cubicles. The experiment was conducted using paper and pencil. The experimental questions are displayed in Figures 5.1 and 5.2. There always is a choice between €25 for sure, or a 25% chance at gaining €40 and a 75% chance at gaining €20, in terms of final outcomes. However, the figures frame the

The experimenter gives you a cash gift of €20. These €20 are yours to dispose of. Additionally, you are given the choice between the following two options:

- ☐ Option A: Winning €5 for sure.
- ☐ Option B: Winning €20 with a 25% probability, and winning nothing with a 75% probability.

Which option do you choose?

(a) The choice-gain question

The experimenter gives you a cash gift of €40. These €40 are yours to dispose of. Additionally, you are given the choice between the following two options:

- ☐ Option A: Losing €15 for sure.
- ☐ Option B: Losing €20 with a 75% probability, and losing nothing with a 25% probability.

Which option do you choose?

(b) The choice-loss question

Figure 5.1 The choice questions

options differently, with Figs. 5.1a/5.2a using gain terms and Figs. 5.1b/5.2b using loss terms. Figure 5.1 asks for direct choices, and Figure 5.2 asks for predicting the choice of another participant, randomly and anonymously selected from the other participants in the experiment.

5.2.2 Participants

115 participants from Erasmus University Rotterdam were recruited. The average age was 21.2 years, 60% being male. These participants were divided into 9 experimental sessions with between 10 and 19 participants.

5.2.3 Treatments

In the choice-prediction (CP) treatment participants first made choices (Figure 5.1) and then predictions (Figure 5.2), and in the prediction-choice (PC) treatment it was the other way around. Participants always first answered the gain question and then the loss question.

The experimenter gives the other participant a cash gift of €20. These €20 are his/hers to dispose of.
Additionally, the participant is given the choice between the following two options:

- ☐ Option A: Winning €5 for sure.
- ☐ Option B: Winning €20 with a 25% probability, and winning nothing with a 75% probability.

Which option do you predict the participant to choose?

(a) The prediction-gain question

The experimenter gives the other participant a cash gift of €40. These €40 are his/hers to dispose of.
Additionally, the participant is given the choice between the following two options:

- ☐ Option A: Losing €15 for sure.
- ☐ Option B: Losing €20 with a 75% probability, and losing nothing with a 25% probability.

Which option do you predict the participant to choose?

(b) The prediction-loss question

Figure 5.2 The prediction questions

5.2.4 Incentives

This experiment was joined with another, unrelated, experiment. Every participant received a €5 flat fee for participating. In each session one of the participants was randomly selected to play one of the questions (out of both experiments), randomly selected, for real. Choices for oneself were implemented as they are. In case of prediction, a correct prediction was awarded €15. The participant who was selected to play for real was paid in private, when all other participants had left the room. For a prediction question, the participant would not know whose choice he/she had to predict. Thus, participants were informed at the beginning of the experiment that their choices would be anonymous in the sense that other participants would not observe them.

5.2.5 Discussion of stimuli

The stimuli in Figure 5.1 were taken from Vieider (2011), using the exact same wordings. They were especially designed for tractable incentive-compatible pure tests of preference reversals, using moderate payoffs and probabilities. There are several reasons for choosing this task. First, it is easy. Second, it can be used to analyze both preference reversals and risk attitudes. Third, it has been widely studied and is well known. We only measure overall risk aversion or risk seeking as simply as possible, and do not seek to decompose these into components such as utility and probability weighting (Qiu & Steiger (2011)).

In Figure 5.2, we let the "other" refer to an anonymously chosen single individual from the other participants in the experiment. McCauley et al. (1971) let the other refer to the average participant of a group, but average participant may easily be confounded with average payment, which is why we preferred to avoid this framing. Faro & Rottenstreich (2006) and Hsee & Weber (1997) studied several framings of others in detail, and found that an empathy gap that generates differences would disappear if the other were too familiar to the participant. We chose the formulation that seemed best suited for our purposes.

5.3 Hypotheses

Choice questions as in Figure 5.1, being monetary versions of the Asian disease problem, have been extensively studied (Kühberger (1998); Kühberger & Gradl (2013)). The common finding is risk aversion for gains in Fig. 5.1a, and risk seeking for losses in Fig. 5.1b, as predicted by prospect theory (Tversky & Kahneman (1992)).

How people predict others' choices, as in Figure 5.2, and how these predictions are related to own choices, has also been widely studied (Faro and Rottenstreich, 2006, Hsee and Weber, 1997, and their references). The common finding is that predictions of choices are closer to risk neutrality than own choices (Daruvala (2007); Faro & Rottenstreich (2006)). Our main interest is the effect of prior predictions on posterior choice. We also investigate the reversed effect. The literature is not unanimous on what we can expect. We will compare four theories, each with different predictions for our stimuli. Table 5.4 in the results section summarizes the predictions of the theories. Consulting this table may be convenient at this stage.

Because we did not directly ask for predictions of preference reversals, and obviously did not reward correct predictions (indirectly derivable from predicted choices) of those, we will not consider preference reversals in predictions. Preference reversals will always concern choices.

5.3.1 Learning

Learning entails that participants get to understand the stimuli better as the experiment proceeds, which will enhance rationality. As regards weak rationality, learning will reduce preference reversals. Thus, for instance, there will be fewer preference reversals under PC than under CP. As regards strong rationality, there will be more risk neutral choices in PC than in CP.

The optimal predictions in our choice experiment are to always predict the majority choices. Thus, the increased learning for predictions in CP relative to PC suggests more predictions of risk aversion for gains, and fewer for losses.

5.3.2 Construal Level Theory

Construal-level theory (Trope & Liberman (2010)) predicts that objects that are psychologically more distant are construed in a more abstract way. Increased distance induces a stronger focus on central aspects of the object and a weaker focus on peripheral aspects. In our experiment, it will increase rationality. We operationalize psychological distance through social distance. Predicting others' choices will be more remote than making own choices. Hence predicted choices of others will be more rational than own choices. Having prediction precede choice can be expected to increase the perceived distance and abstract way of viewing, increasing rationality of choices, resulting in fewer preference reversals, and more risk neutrality, as with learning. Similarly, having choices precede predictions will generate more risk aversion predicted for gains, and less for losses, which is the same as with learning but for different reasons.

5.3.3 Risk-as-value

Two theories often put forward when comparing choices to predictions are risk-as-value and risk-asfeelings. Risk-as-value assumes that people find risk seeking an admirable characteristic and believe that they possess more of it than others (Clark et al. (1971), Levinger & Schneider (1969), McCauley et al. (1971), Wallach & Wing (1968), Willems (1969)). Risk-as-value accordingly predicts that choices are more risk seeking than predictions. As emphasized by McCauley et al. (1971), risk-as-value has no direct predictions for the effect of prior prediction on posterior choice or vice versa. We therefore combine risk-as-value with the plausible hypothesis of anchoring (Faro & Rottenstreich (2006, p. 537)).

For our main research interest, the effect of prior prediction on choice, the combined effect of risk-as-value and anchoring is as follows. First, because of risk-as-value, predictions in PC will be more risk averse than choices in CP. Next, because of anchoring, this extra risk aversion will be extended to choices in PC. The end result is that prior prediction will make choices more risk averse, both for gains and for losses. Similarly, predictions in CP will be less risk averse than in PC, again both for gains and for losses. These effects will not affect the prevalence of preference reversals systematically.

5.3.4 Risk-as-feelings

The risk-as-feelings hypothesis states that people predict others to have a similar risk attitude as themselves, but, due to an empathy gap, less pronounced so, moving all predictions of others' choices in the direction of risk neutrality (Slovic (2010)). Faro & Rottenstreich (2006, "methods" on p. 537 and "results and discussion" on pp. 537-538) find the following effect of prior prediction on posterior choice and vice versa. In the PC treatment, when choosing, participants implicitly acknowledge the empathy gap and adjust for it, leading to an increased discrepancy between choice and prediction. This effect is opposite to anchoring. For gains we then get more risk averse choices for PC than for CP, and for losses we get fewer risk averse choices. Similarly, CP will yield fewer risk averse predictions than PC for gains and more risk averse predictions for losses. Moves away from risk neutrality increase preference reversals and, hence, risk-as-feelings suggests more preference reversals under PC than under CP.

5.4 Results

Table 5.1 shows the percentages of safe choices and safe-choice predictions for every treatment and ask. Table 5.2 shows percentages of preference reversals. Eyeballing the data suggests that we have the usual risk aversion for gains, risk seeking for losses, and preference reversals. Further, we have anchoring, with second answers close to first answers: choices close to predictions in PC and predictions close to choices in CP. Risk aversion is always higher in PC than in CP, and preference reversals are not affected by treatment. These observations are confirmed by the following statistical analyses.

	Safe Choices		p-value
	CP(n=58)	PC(n=57)	(2-Tailed)
Choic-Gain	59%	67%	0.375
Choice-Loss	17%	33%	0.048
Prediction-Gain	52%	72%	0.027
PRediction-Loss	16%	28%	0.104

Table 5.1 percentage of safe choices; p-values of Mann-Whitney U tests.

Logit regression

	CP(n=58)	PC(n=57)
Choice	47%, 5%	42%, 9%

Table 5.2 Percentages of participants exhibiting preference reversals in choices [common, uncommon]

	Coefficient	p-Value(2-Tailed)
Constant	0.22	0.330
Loss	-1.73	0.000***
Prediction	-0.11	0.503
PC	0.70	0.006***

Table 5.3 Logit regression

Table 5.3 shows the results of a logit regression with answer (=1 if safe) as dependent variable, and dummy-variables loss, prediction, and PC as independent variables, clustered on subjects. The results confirm the sign (loss) and treatment (PC) effects observed in Table 5.1. They also confirm the similarity between choices and predictions consistent with anchoring. Interaction effects between the independent variables were insignificant and therefore omitted from the regression. These results will be confirmed by the non-parametric analyses given next.

Risk attitudes and preference reversals

The risk aversion for gains and risk seeking for losses, both for choices and predictions, are significant at $p = 0.01$ except for gains under CP. For preference reversals, we consider two types (Bardsley et al. (2010, p. 131); Maafi (2011)) . Common preference reversals concern a safe choice for gains and a risky choice for losses, which is the common finding. Uncommon preference reversals concern the opposite choices. At least 50% of the participants exhibit preference reversals, mostly in the common direction.

Comparison of choices and predictions

Within treatment PC, 67% of risk averse choices is not significantly different from 72% predicted risk averse choices.[2] We similarly find no differences between the other predictions and choices within treatments. Both for PC and CP, the correlations between predictions and choices always exceed 0.3 and are always significant at $p = 0.01$, whereas none of the other variables in Table 5.1 are significantly correlated.

Treatment effects

All observations under PC reveal more risk aversion than under CP, twice signif-

[2] It may be argued that the predictions in PC should be compared with the choices in CP (which are a priori choices not following after a prediction task). Here the differences are not significant either.

icant at p = 0.05 and once marginally significant (Table 5.1). The total number of preference reversals does not differ significantly between treatments. Common preference reversals are reduced somewhat when going from CP to PC, but uncommon preference reversals are increased.

5.5 Summary, implications for theories, and related literature

Our results on risk attitudes and preference reversals agree with common findings in the literature. The within-treatment comparisons of choice and prediction confirm anchoring, with second answers in a treatment always close to first answers. This anchoring is consistent with cognitive dissonance (Festinger (1957)) and false consensus (Ross et al. (1977)). Given anchoring, the higher risk aversion in PC than in CP confirms risk-as-value. The absence of interaction between independent variables in the logit regression further supports a general increase of risk aversion due to prediction, for gains and losses alike, in agreement with risk-as-value. Prediction does not improve weak rationality (avoiding preference reversals), and neither does it improve strong rationality (risk neutrality) for gain choices. It only improves strong rationality for loss choices. Table 5.4 summarizes our results and the predictions of the theories considered.

Our experiment was designed to be maximally simple and, hence, we only measured binary choices and predictions (risk averse or risk seeking), and not indifferences as in Faro & Rottenstreich (2006, p. 539) or Hsee & Weber (1997). In the terminology of Faro & Rottenstreich (2006, p. 539), our questions concern the type of emotions (risk averse or risk seeking), and not the intensity. In our design, if each participant had been a perfect predictor, maximizing expected payoff, then each would predict the majority choices, which is risk aversion for gains and risk seeking for losses. In other words, then we would have had 100% risk aversion predictions for gains and 100% risk seeking predictions for losses. In reality, participants' predictions were close to their own choices within each treatment, as confirmed by the statistical analyses. At the group level, the percentages of risk aversion were close for choice and prediction in both treatments. If we could treat the group of subjects in each treatment as one decision maker, then our finding would entail probability matching (individuals not choosing best in all situations, but having their overall choice percentages match real probability distributions; Bitterman (1965); Vulkan (2002)). That is, we have found a kind of probability matching at the group level.

Many papers have studied the discrepancies between prediction and choice, and we will not seek to completely review this literature. McCauley et al. (1971) compared the risk aversion in choices that participants make for themselves with the predicted risk attitudes of others as we did, with a CP treatment and a PC treatment. Unlike us, they did not consider losses and did not employ real incentives. Moreover, there was an average-other/average-payoff confound in their design. They found support for risk-as-value, but no anchoring.

Risk-as-feelings was introduced in a careful study by Hsee & Weber (1997). They found the opposite of McCauley et al. (1971), being less risk aversion in predictions, and supporting risk-asfeelings. They found that the empathy gap underlying risk-as-feelings becomes smaller as the others to be predicted are closer, reducing the effects of risk-as-feelings. This was confirmed by Hsee & Weber (1999) and Faro & Rottenstreich (2006). The latter added a refinement. They noted that less risk aversion for predictions than choices in Hsee & Weber (1997) always coincided with more risk neutrality. Faro and Rottenstreich found that the latter effect was the general one, leading to more risk aversion for predictions than choices whenever choices are risk seeking. Their finding was confirmed by Daruvala (2007). Combined with our finding of anchoring, it suggests that prior prediction can improve strong rationality. Our findings confirm so for losses, but not for gains.

Our findings can be reconciled with the findings on risk-as-feelings because the others whose choices were to be predicted by our participants were closer and more similar in our study than in previous studies. They were participants in the same group and experiment, being in the exact same situation as the participants during the experiment, which reduced the empathy gap. Combining our findings with those of Faro & Rottenstreich (2006) or Hsee & Weber (1997) suggests that prior predictions improve rationality best if there is an empathy gap, but loses most of its force if there is no empathy gap. Another difference is that the preceding studies concerned intensities of emotions, which are underestimated in predictions, whereas our study concerned types of emotions. The latter are subject to false consensus, leading to anchoring (Faro & Rottenstreich (2006, p. 539)).

We find no support for construal level theory or learning. The latter may be because there was only limited opportunity to learn, and learning may only become effective after more repetitions of the same task.

5.6 Conclusion

We investigated the saying "When deciding, imagine what others would do in your place," following up on promising first results (Faro & Rottenstreich (2006) or Hsee & Weber (1997)). Those results suggested that first imagining what others do may increase rationality of choice and, thus, provide a useful addition to modern nudging

	Risk Aversion for Gains	Risk Aversion for Losses	Preference Reversals
Learning	↓	↑	↓
Construal Level Theory	↓	↑	↓
Achoring&Risk-as-Value	↑	↑	-
Risk-as-Feeling	↑	↓	↑
Our results	↗	↑	-

(a) Predicted changes in choices when going from CP to PC

	Risk Aversion for Gains	Risk Aversion for Losses
Learning	↑	↓
Construal Level Theory	↑	↓
Achoring&Risk-as-Value	↓	↓
Risk-as-Feeling	↓	↑
Our results	↓	↘

(b) Predicted changes in predictions when going from PC to CP

↑ increase; ↓ decrease; ↗ increasing trend but not significant; ↘ decreasing trend but not significant; - no effect

Table 5.4 Summary of results and predictions of theories

techniques. Our experiment used simple and well-known stimuli with real incentives and careful framing, all targeted towards testing the aforementioned hypothesis. We found an improvement of strong rationality (risk neutrality) for losses, but not for gains, and we found no improvement of weak rationality (avoiding framing effects and preference reversals). Conversely, predictions for gains were closer to risk neutrality when preceded by choice. Predictions for losses were not affected by preceding choices.

Our results best fit with, primarily, strong anchoring and, secondarily, risk-as-value. They can also be reconciled with findings on risk-as-feelings. Thus prior prediction can improve rationality if there is an empathy gap, as shown by preceding studies, but loses much of its force if the empathy gap is reduced, as shown by our study. Prior prediction is still useful when people face losses. Predictions are subject to a kind of probability matching at the group level.

Chapter 6

Conclusion

This thesis has made a special effort to explore some relevant issues on (ir)rationality. Chapter 2 and Chapter 3 deal with ways to rekon with irrationalites if they cannot be avoided. Chapter 2 improves the methodology to measure preferences by proposing a new incentive system on individual decision-making: the prior incentive system (Prince). Chapter 3 addresses the issue of irrationality in decisions under ambiguity. Chapter 4 answers the question of why we steer people away from irrationality. Chapter 4 discusses whether we should correct people's irrationality by imposing a better decision when freedom of choice cannot be realized. Chapter 4 concludes with recommending strong paternalism and provides a litmus test for people's views on paternalism. Chapter 5 answers the question how to make people less irrational. The chapter studies the social influences on people's decision-making processes and offers possible approaches to nudge people away from irrationality. In the present chapter, the main conclusions and results are summarized. Furthermore, at the end of this chapter, some ideas for future research are discussed.

Chapter 2 introduces Prince (prior incentive system), a new incentive system preferable to current popular incentive systems for individual decision-making. Prince improves measurements of preferences without affecting those preferences themselves. In the experiment, we demonstrated that the implementation of the tangible real choice situation (RCS) makes the incentive compatibility completely transparent to subjects. This improves the weakness of Becker-DeGroot-Marschak mechanism, which is difficult for subjects to understand. Additionally, with Prince, we show that the preference reversals disappear. When decisions elicited by choice list and by matching are compared, both using Prince, no significant differences are found between matching and choice list. Prince resolves a number of other problems: (a) violations of isolation; (b) misperceptions of bargaining; (c) strategic answering in adaptive experiments. Not only do we avoid deception, but furthermore this is completely clear and verifiable for the subjects. Thus Prince provides more valid and transparent measurements of preferences.

Chapter 3 finds no support for outcome dependence of ambiguity attitudes in between-outcome comparisons, i.e., when changing outcomes while keeping the source of uncertainty fixed. This adds to the same finding in the literature keeping outcomes fixed while changing sources. We do find support for event-dependence of ambiguity attitudes between sources, i.e. when changing the source of uncertainty. This evidence supports event-based theories of ambiguity such as prospect theory and multiple priors against outcome-based theories such as the smooth model. We also find support for event-dependence of ambiguity aversion within sources, where aversion changes to seeking if events change from likely to unlikely. This finding again supports event-based ambiguity theories against outcome-based theories. It also supports the empirical importance of insensitivity besides aversion to ambiguity. For parametric families to analyze ambiguity, more than for risk, it is important to capture insensitivity (inverse-S) properly. Hence parametric families that can properly model insensitivity will be most suited to analyze ambiguity attitudes.

In Chapter 4, to discuss the central question concerning paternalism that plays a role in many practical situations, we used a hypothetical gedanken-experiment to maximally clarify the relevant issues (mainly our Case 4.4I). The gedanken-experiment provides a litmus test for the readers' stance on paternalism. Providing this test was the primary purpose of this paper. It shows that one cannot avoid taking a position (disproving many claims to the contrary in the literature). Our thought experiments involved only minimal rationality conditions, being transitivity and stochastic dominance. Thus, we separated debates about paternalism from debates about rationality of expected utility or other theories. We further argued for deviating from what at first sight seems to be the true and consistent preference of a client in some situations (but on closer inspection need not be, as we argued).

Whereas it is easy to cast doubt on our stance, as it can sometimes lead to wrong decisions, it is less easy to suggest better stances. Every other stance can also lead to wrong decisions, especially if ignoring a large body of literature on well-documented biases, or when serving only to avoid responsibility. The latter is impossible in our gedanken-experiment as it often is impossible in practice. For example, medical treatment decisions have to be taken one way or the other, and money spent on one treatment cannot be spent on another. Our analysis draws on the vast literature on biases in behavioral economics, and further clarifies how behavioral economics can be of use in prescriptive decision making.

Chapter 5 investigates the saying "When deciding, imagine what others would do in your place” following up on promising first results (Faro and Rottenstreich 2006; Hsee and Weber 1997). Those results suggested that first imagining what others do may increase rationality of choice and, thus, provides a useful addition to modern nudging techniques. Our experiment used simple and well-known stimuli with real incentives and careful framing, all targeted towards testing the hypothesis suggested above. We found an improvement of strong rationality (risk neutrality) for losses, but not for gains, and we found no improvement of weak rationality (avoiding framing effects and preference reversals). Conversely, predictions for gains were closer to risk neutrality when preceded by choice. Predictions for losses were not affected by preceding choices. Our results best fit with, primarily, strong anchoring and, secondarily, risk-as-value. They can also be reconciled with findings on risk-as-feelings. Thus prior prediction can improve rationality if there is an empathy gap, as shown by preceding studies, but loses much of its force if the empathy gap is reduced, as shown by our study. Prior prediction is still useful when people face losses. Predictions are subject to a kind of probability matching at the group level.

Investigations of (ir)rationality have many implications both theoretically and practically. This thesis has investigated some issues associated with irrationality. Much remains to be explored. Nevertheless, investigations of (ir)rationality will mainly focus on the three questions what, why, and how. The why question has been widely discussed and to a large extent consensus has been reached to improve people’s decision-making. Opinions diverge only when it comes to whether we should limit people’s choice liberty. It is still debated as to what is rational. In addition to what has been studied in this thesis, further research should provide more insights into the identification of (ir)rationalities in this dynamic and complex world. For instance, we need better models to describe the role of reference points in people’s behavior. Discovering anomalies from people’s decisions is the first step. Investigating the underlying causes of these anomalies comes next, and can go hand in hand with research on how to correct the anomalies. Answers to the how question have the most direct implications for our daily life. Future research efforts are worthwhile in directing people towards rationality.

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Irrationeel: Wat, Waarom, en Hoe

Samenvatting

Dit proefschrift onderzoekt enige relevante kwesties over (ir)rationaliteiten. Hoofdstukken 2 en 3 gaan over manieren om met irrationaliteiten rekening te houden als ze niet vermeden kunnen worden. Hoofdstuk 2 presenteert een methodologische verbetering om preferenties te meten door een nieuw incentive systeem voor te stellen voor individuele beslissingen: het prior incentive system (Prince). Hoofdtuk 3 behandelt irrationaliteiten in beslissen onder ambiguïteit. Hoofdstuk 4 beantwoordt de vraag waarom we mensen proberen te behoeden voor irrationaliteiten. Het bediscussieert of we de irrationaliteiten van mensen moeten corrigeren door een betere beslissing op te leggen wanneer keuzevrijheid niet gerealiseerd kan worden. Het hoofdstuk sluit af met het aanbevelen van sterk paternalisme en verschaft een lakmoestest voor visies op paternalisme. Hoofdtsuk 5 beantwoordt de vraag hoe mensen minder irrationeel te maken. Het hoofdstuk bestudeert de sociale invloeden op beslissings-processen and biedt mogelijke benaderingen om mensen van irrationaliteiten weg te "nudgen". In dit hoofdstuk worden de belangrijkste conclusies en resultaten samengevat. Tenslotte worden aan het einde enige ideeÃ«n voor toekomstig onderzoek gepresenteerd.

Hoofdstuk 2 introduceert Prince (prior incentive system), een nieuw incentive systeem dat te prefereren is boven tegenwoordig populaire incentive s ystemen voor individuele beslissingen. Prince verbetert de metingen van preferenties zonder ze te beÃ¯nvloeden In een experiment tonen we dat het gebruik van een tastbare werkelijke keuzesituatie (RCS: real choice situation) de incentive compatibility geheel duidelijk maakt voor subjecten. Dit verbetert een zwakte van het Becker-DeGroot-Marschak mechanisme, hetgeen moeilijk voor subjecten te begrijpen is. We tonen verder dat preference reversals verdwijnen als we Prince gbruiken. Wanneer choice lists en matching procedures onder Prince worden vergeleken, worden geen significante verschillen gevonden. Prince lost een aantal andere problemen op: (a) schendingen van isolatie; (b) mispercepties van onderhandelingen; (c) strategisch antwooren in adaptieve experimentens. Niet alleen vermijden we deceptie, maar, meer dan dat, dit is geheel duidelijk en verifieerbaar voor de subjecten. Zo verschaft Prince beter geldige en duidelijkere metingen van preferenties.

In Hoofdstuk 3 vinden we geen steun voor de hypothese dat ambiguïteitshoudingen afhangen van uitkomsten in een tussen-uitkomsten vergelijking, dwz wanneer uitkomsten veranderen terwijl de bron van onzekerheid vast gehouden wordt. Dit bevestigt dezelfde bevindingen in de literatuur waarbij uitkomsten vast worden gehouden terwijl bronnen van onzekerheid worden gevarieerd. We vinden wel steun voor de afhankelijkheid van ambiguïteits-attituden van gebeurtenissen wanneer de bron van onzekerheid varieert. Deze evidentie steunt theorieën van ambiguïteit die op gebeurtenissen gebeurd zijn, zoals prospect theorie en multiple priors theorieën, tegen theorieën gebaseerd op uitkomsten, zoals het welbekende smooth model. We vinden ook steun voor de afhankelijkheid van ambiguïteitsafkeer van gebeurtenissen, waarbij afkeer van ambiguïteit verandert in voorkeur als gebeurtenissen veranderen van waarschijnlijk in onwaarschijnlijk. Deze bevinding steunt andermaal gebeurtenisgebaseerde theorieën tegenover uitkomst-gebaseerde theorieën. Het onderschrijft ook het empirische belang van ongevoeligheid naast afkeer. Voor parametrische families voor het analyseren van ambiguïteit, meer nog dan bij risico, is het belangrijk om ongevoeligheid (inverse-S) goed te modelleren. Daarom zijn parametrische families die ongevoeligheid goed behandelen het meest geschikt voor de analyse van ambiguïteits-attituden.

In Hoofdstuk 4 gebruiken we een hypothetisch gedachten-experiment om een centrale vraag over paternalisme aan de orde te stellen die een rol speelt in vele practische situaties. We gebruiken het gedachten-experiment om de relevante kwesties zo duidelijk mogelijk te maken (vooral Case 4.4I). Het gedachten-experiment verschaft een lakmoes test voor het standpunt van de lezer aangaande paternalisme. Deze test verschaffen is het hoofddoel van dit paper. Het toont dat men niet kan vermijden om stelling te nemen (waarmee vele tegengestelde beweringen uit de literatuur worden ontkracht). Onze gedachten-experimenten gebruiken slechts minimale rationaliteits condities, namelijk transitiviteit en stochastische dominantie. Zo kunnen we over paternalisme debatteren zonder een standpunt in te nemen over de rationaliteit van verwacht nut of andere theorieën. We pleiten ervoor in sommige situaties af te wijken van wat op het eerste gezicht de ware en consistente preferentie van een cliënt lijkt te zijn (maar dat bij nadere beschouwing niet was, zoals we beargumenteren).

Terwijl het gemakkelijk is ons standpunt in twijfel te trekken, omdat het soms tot verkeerde beslissingen leidt, is het niet zo gemakkelijk om een beter standpunt aan te geven. Ieder ander standpunt kan namelijk ook tot verkeerde beslissingen leiden, vooral als men daarbij een uitgebreide en wel-gedocumenteerde literatuur over biases negeert, of als het alleen maar dient om verantwoordelijkheid te ontlopen. Dat laatste is onmogelijk in ons gedachten-experiment, zoals het vaak onmogelijk is in praktische situaties. Bijvoorbeeld, medische behandelingen moeten gedaan worden op de eenof andere manier, en geld dat is uitgegeven aan een behandeling kan niet uitgegeven worden aan een andere behandeling. Onze analyse is gebaseerd op een uitgebreide literatuur over biases in behavioral economics, en maakt verder duidelijk hoe behavioral economics gebruikt kan worden voor prescriptieve beslissingen.

Hoofdstuk 5 onderzoekt het gezegde "Bij het nemen van beslissingen, stel je voor wat een ander in jouw plaats zou doen,"voortbordurend op veelbelovende resultaten van Faro and Rottenstreich (2006) en Hsee and Weber (1997). Die resultaten suggereerden dat het zich eerst inbeelden wat een ander zou doen de rationaliteit van keuzen bevordert en, dus, een nuttige toevoeging zijn van moderne nudging technieken. Ons experiment gebruikt simpele en wel-bekende stimuli met werkelijke betalingen en zorgvuldige framing, allemaal gericht op het testen van de boven gesuggereerde hypothese. We vinden een verbetering van sterke rationaliteit (risico neutraliteit) voor verliezen, maar niet voor winsten, en we vinden geen verbetering van zwakke rationaliteit (het vermijden van framing effects en preference reversals). Van de andere kant, voorspellingen voor winsten liggen dichter bij risico neutraliteit wanneer ze vooraf gegaan werden door keuzen. Voorspellingen voor verliezen worden niet beïnvloed door voorafgaande keuzen. Onze resultaten passen het beste bij, in de eerste plaats, een sterk anker-effect en dan, ten tweede, risico-als-waarde. Ze kunnen ook verklaard wrden met risk-as-feelings. Daardoor kan vooraf-voorspellen rationaliteit verbeteren als er een empathie-gap is, zoals getoond door eerdere studies, maar het verliest zijn kracht als de emphatie-gap gereduceerd is, zoals aangetoond door onze studie. Vooraf voorspellen is wel nuttig wanneer mensen met verliezen te maken hebben. Voorspellingen zijn onderhevig aan een soort van kans-matching op groeps niveau.

Onderzoeking naar (ir)rationaliteiten hebben veel implicaties, zowel theoretisch als practisch. Dit proefschrift heeft enige van die implicaties onderzocht. Onderzoekingen van (ir)rationaliteit zullen zich voornamelijk richten op de drie vragen waarom, wat, en hoe. De waarom vraag is uitgebreid bediscussieerd en in grote mate is overeenstemming bereikt over hoe beslissingen van mensen te verbeteren. De meningen lopen uiteen over de vraag hoe we de vrijheid van mensen moeten beperken. Discussies duren voort over de vraag wat rationaliteit is. In toevoeging tot wat onderzocht is in dit proefschrift, moet verder onderzoek meer inzicht geven in de identificatie vn (ir)rationaliteien in deze dynamische en complexe wereld. Bijvoorbeeld hebben we betere modellen nodig om de rol van referentiepunten in menselijk gedrag te beschrijven. Anomalieën ontdekken in menselijke beslissingen is de eerste stap. De onderliggende oorzaken onderzoeken is de volgende stap, en deze kan hand in hand gaan met onderzoek naar hoe de anomalieën te corrigeren. Antwoorden op de hoe vraag hebben het meest direct implicaties voor ons dagelijkse leven. Onderzoek is gewenst naar methoden om mensen meer tot rationaliteit te brengen.

About the Author

Zhihua Li was born on August 14, 1986 in Xinxiang, China. She holds a Bachelor's degree in Business Administration from China University of Geosciences (CUG) and a second Bachelor's degree in Biology Science from Wuhan University. Her grades ranked 1st among graduates of year 2008 at School of Management, CUG. She holds a Research Master's degree with highest distinction in Organizational Behaviour from Erasmus Research Institute of Management, Rotterdam School of Management.

In October 2010, Zhihua was promoted by Prof. Han Bleichrodt and Prof. Peter P. Wakker as a PhD candidate in Behavioural Economics and joined Department of Applied Economics, Erasmus School of Economics. Her daily supervisor is Prof. Kirsten I.M. Rohde. With the interest in contributing to a deeper understanding of decision-making and judgements, she carried out reseaches testing economic theories in the enviroment of imperfect decision makers, as well as applying rational theories to improve decision-making quality. She also takes a special interest in bridging advancements in decision theory and improvements of real life decisions.

As part of her PhD training, Zhihua spent time as a visiting researcher at National University of Singapore. She has presented her work at different conferences and worshops such as Foundations of Utility and Risk (FUR), Subjective Probability, Utility and Decision Making (SPUDM), TIBER/Erasmus Behavioral Economics - workshop, Learning, Decision and Bounded Rationality Workshop. Her work has also been presented at conferences and leading research institutes such as Decision Theory Forum (DT), London School of Economics, National University of Singapore, Nanyang Techonological University, University of Zurich. Currently, she works as a research fellow at the University of Warwick.

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DISSERTATIONS LAST FIVE YEARS

Abbink, E., Crew Management in Passenger Rail Transport, Promotor(s): Prof.dr. L.G. Kroon & Prof.dr. A.P.M. Wagelmans, EPS-2014-325-LIS, `http://hdl.handle.net/1765/1`

Acar, O.A., Crowdsourcing for innovation: Unpacking Motivational, Knowledge and Relational Mechanisms of Innovative Behavior in Crowdsourcing Platforms, Promotor: Prof.dr. J.C.M. van den Ende, EPS-2014-321-LIS, `http://hdl.handle.net/1765/1`

Acciaro, M., Bundling Strategies in Global Supply Chains, Promotor(s): Prof.dr. H.E. Haralambides, EPS-2010-197-LIS, `http://hdl.handle.net/1765/19742`

Akpinar, E., Consumer Information Sharing; Understanding Psychological Drivers of Social Transmission, Promotor(s): Prof.dr.ir. A. Smidts, EPS-2013-297-MKT, `http://hdl.handle.net/1765/50140`

Alexiev, A., Exploratory Innovation: The Role of Organizational and Top Management Team Social Capital, Promotor(s): Prof.dr. F.A.J. van den Bosch & Prof.dr. H.W. Volberda, EPS-2010-208-STR, `http://hdl.handle.net/1765/20632`

Akin Ates, M., Purchasing and Supply Management at the Purchase Category Level: Strategy, Structure, and Performance, Promotor: Prof.dr. J.Y.F. Wynstra, EPS-2014-300-LIS, http://hdl.handle.net/1765/1

Almeida, R.J.de, Conditional Density Models Integrating Fuzzy and Probabilistic Representations of Uncertainty, Promotor Prof.dr.ir. Uzay Kaymak, EPS-2014-310-LIS, http://hdl.net/1765/1

Bannouh, K., Measuring and Forecasting Financial Market Volatility using High-Frequency Data, Promotor: Prof.dr.D.J.C. van Dijk, EPS-2013-273-F&A, http://hdl.handle.net/1765/38240

Benning, T.M., A Consumer Perspective on Flexibility in Health Care: Priority Access Pricing and Customized Care, Promotor: Prof.dr.ir. B.G.C. Dellaert, EPS-2011-241-MKT, http://hdl.handle.net/1765/23670

Ben-Menahem, S.M., Strategic Timing and Proactiveness of Organizations, Promotor(s): Prof.dr. H.W. Volberda & Prof.dr.ing. F.A.J. van den Bosch, EPS-2013-278-S&E, http://hdl.handle.net/1765/39128

Berg, W.E. van den, Understanding Salesforce Behavior Using Genetic Association Studies, Promotor: Prof.dr. W.J.M.I. Verbeke, EPS-2014-311-MKT, http://hdl.handle.net/1765/51440

Betancourt, N.E., Typical Atypicality: Formal and Informal Institutional Conformity, Deviance, and Dynamics, Promotor: Prof.dr. B. Krug, EPS-2012-262-ORG, http://hdl.handle.net/1765/32345

Binken, J.L.G., System Markets: Indirect Network Effects in Action, or Inaction, Promotor: Prof.dr. S. Stremersch, EPS-2010-213-MKT, http://hdl.handle.net/1765/21186

Blitz, D.C., Benchmarking Benchmarks, Promotor(s): Prof.dr. A.G.Z. Kemna & Prof.dr. W.F.C. Verschoor, EPS-2011-225-F&A, http://hdl.handle.net/1765/226244

Boons, M., Working Together Alone in the Online Crowd: The Effects of Social Motivations and Individual Knowledge Backgrounds on the Participation and Per-

formance of Members of Online Crowdsourcing Platforms, Promotor: Prof.dr. H.G. Barkema, EPS-2014-306-S&E, `http://hdl.net/1765/50711`

Borst, W.A.M., Understanding Crowdsourcing: Effects of Motivation and Rewards on Participation and Performance in Voluntary Online Activities, Promotor(s): Prof.dr.ir. J.C.M. van den Ende & Prof.dr.ir. H.W.G.M. van Heck, EPS-2010-221-LIS, `http://hdl.handle.net/1765/21914`

Budiono, D.P., The Analysis of Mutual Fund Performance: Evidence from U.S. Equity Mutual Funds, Promotor: Prof.dr. M.J.C.M. Verbeek, EPS-2010-185-F&A, `http://hdl.handle.net/1765/18126`

Burger, M.J., Structure and Cooptition in Urban Networks, Promotor(s): Prof.dr. G.A. van der Knaap & Prof.dr. H.R. Commandeur, EPS-2011-243-ORG, `http://hdl.handle.net/1765/26178`

Byington, E., Exploring Coworker Relationships: Antecedents and Dimensions of Interpersonal Fit, Coworker Satisfaction, and Relational Models, Promotor: Prof.dr. D.L. van Knippenberg, EPS-2013-292-ORG, `http://hdl.handle.net/1765/41508`

Camacho, N.M., Health and Marketing; Essays on Physician and Patient Decision-making, Promotor: Prof.dr. S. Stremersch, EPS-2011-237-MKT, `http://hdl.handle.net/1765/23604`

Cankurtaran, P. Essays On Accelerated Product Development, Promotor: Prof.dr.ir. G.H. van Bruggen, EPS-2014-317-MKT, `http://hdl.handle.net/1765/1`

Caron, E.A.M., Explanation of Exceptional Values in Multi-dimensional Business Databases, Promotor(s): Prof.dr.ir. H.A.M. Daniels & Prof.dr. G.W.J. Hendrikse, EPS-2013-296-LIS, `http://hdl.handle.net/1765/50005`

Carvalho, L., Knowledge Locations in Cities; Emergence and Development Dynamics, Promotor: Prof.dr. L. van den Berg, EPS-2013-274-S&E, `http://hdl.handle.net/1765/38449`

Carvalho de Mesquita Ferreira, L., Attention Mosaics: Studies of Organizational Attention, Promotor(s): Prof.dr. P.M.A.R. Heugens & Prof.dr. J. van Oosterhout, EPS-2010-205-ORG, `http://hdl.handle.net/1765/19882`

Cox, R.H.G.M., To Own, To Finance, and to Insure; Residential Real Estate Revealed, Promotor: Prof.dr. D. Brounen, EPS-2013-290-F&A, http://hdl.handle.net/1765/40964

Defilippi Angeldonis, E.F., Access Regulation for Naturally Monopolistic Port Terminals: Lessons from Regulated Network Industries, Promotor: Prof.dr. H.E. Haralambides, EPS-2010-204-LIS, http://hdl.handle.net/1765/19881

Deichmann, D., Idea Management: Perspectives from Leadership, Learning, and Network Theory, Promotor: Prof.dr.ir. J.C.M. van den Ende, EPS-2012-255-ORG, http://hdl.handle.net/1765/31174

Desmet, P.T.M., In Money we Trust? Trust Repair and the Psychology of Financial Compensations, Promotor: Prof.dr. D. De Cremer & Prof.dr. E. van Dijk, EPS-2011-232-ORG, http://hdl.handle.net/1765/23268

Dietvorst, R.C., Neural Mechanisms Underlying Social Intelligence and Their Relationship with the Performance of Sales Managers, Promotor: Prof.dr. W.J.M.I. Verbeke, EPS-2010-215-MKT, http://hdl.handle.net/1765/21188

Dollevoet, T.A.B., Delay Management and Dispatching in Railways, Promotor: Prof.dr. A.P.M. Wagelmans, EPS-2013-272-LIS, http://hdl.handle.net/1765/38241

Doorn, S. van, Managing Entrepreneurial Orientation, Promotor(s): Prof.dr. J.J.P. Jansen, Prof.dr.ing. F.A.J. van den Bosch & Prof.dr. H.W. Volberda, EPS-2012-258-STR, http://hdl.handle.net/1765/32166

Douwens-Zonneveld, M.G., Animal Spirits and Extreme Confidence: No Guts, No Glory, Promotor: Prof.dr. W.F.C. Verschoor, EPS-2012-257-F&A, http://hdl.handle.net/1765/31914

Duca, E., The Impact of Investor Demand on Security Offerings, Promotor: Prof.dr. A. de Jong, EPS-2011-240-F&A, http://hdl.handle.net/1765/26041

Duursema, H., Strategic Leadership; Moving Beyond the Leader-follower Dyad, Promotor: Prof.dr. R.J.M. van Tulder, EPS-2013-279-ORG, http://hdl.handle.net/1765/39129

Eck, N.J. van, Methodological Advances in Bibliometric Mapping of Science, Promotor: Prof.dr.ir. R. Dekker, EPS-2011-247-LIS, http://hdl.handle.net/1765/26509

Essen, M. van, An Institution-Based View of Ownership, Promotor(s): Prof.dr. J. van Oosterhout & Prof.dr. G.M.H. Mertens, EPS-2011-226-ORG, http://hdl.handle.net/1765/22643

Feng, L., Motivation, Coordination and Cognition in Cooperatives, Promotor: Prof.dr. G.W.J. Hendrikse, EPS-2010-220-ORG, http://hdl.handle.net/1765/21680

Fournë S. P. L. Managing Organizational Tensions: A Multi-level Perspective on Exploration, Exploitation, and Ambidexterity, Promotor(s): Prof.dr. J.J.P. Jansen, Prof.dr. S.J. Magala & dr. T.J.M.Mom, EPS-2014-318-S&E, http://hdl.handle.net/1765/21680

Gharehgozli, A.H., Developing New Methods for Efficient Container Stacking Operations, Promotor: Prof.dr.ir. M.B.M. de Koster, EPS-2012-269-LIS, http://hdl.handle.net/1765/37779

Gils, S. van, Morality in Interactions: On the Display of Moral Behavior by Leaders and Employees, Promotor: Prof.dr. D.L. van Knippenberg, EPS-2012-270-ORG, http://hdl.handle.net/1765/38028

Ginkel-Bieshaar, M.N.G. van, The Impact of Abstract versus Concrete Product Communications on Consumer Decision-making Processes, Promotor: Prof.dr.ir. B.G.C. Dellaert, EPS-2012-256-MKT, http://hdl.handle.net/1765/31913

Gkougkousi, X., Empirical Studies in Financial Accounting, Promotor(s): Prof.dr. G.M.H. Mertens & Prof.dr. E. Peek, EPS-2012-264-F&A, http://hdl.handle.net/1765/37170

Glorie, K.M., Clearing Barter Exchange Markets: Kidney Exchange and Beyond, Promotor(s): Prof.dr. A.P.M. Wagelmans & Prof.dr. J.J. van de Klundert, EPS-2014-329-LIS, http://hdl.handle.net/1765/1

Hakimi, N.A, Leader Empowering Behaviour: The Leader's Perspective: Understanding the Motivation behind Leader Empowering Behaviour, Promotor: Prof.dr. D.L. van Knippenberg, EPS-2010-184-ORG, http://hdl.handle.net/1765/17701

Hensmans, M., A Republican Settlement Theory of the Firm: Applied to Retail Banks in England and the Netherlands (1830-2007), Promotor(s): Prof.dr. A. Jolink & Prof.dr. S.J. Magala, EPS-2010-193-ORG, http://hdl.handle.net/1765/19494

Hernandez Mireles, C., Marketing Modeling for New Products, Promotor: Prof.dr. P.H. Franses, EPS-2010-202-MKT, http://hdl.handle.net/1765/19878

Heyde Fernandes, D. von der, The Functions and Dysfunctions of Reminders, Promotor: Prof.dr. S.M.J. van Osselaer, EPS-2013-295-MKT, http://hdl.handle.net/1765/41514

Heyden, M.L.M., Essays on Upper Echelons & Strategic Renewal: A Multilevel Contingency Approach, Promotor(s): Prof.dr. F.A.J. van den Bosch & Prof.dr. H.W. Volberda, EPS-2012-259-STR, http://hdl.handle.net/1765/32167

Hoever, I.J., Diversity and Creativity: In Search of Synergy, Promotor(s): Prof.dr. D.L. van Knippenberg, EPS-2012-267-ORG, http://hdl.handle.net/1765/37392

Hoogendoorn, B., Social Entrepreneurship in the Modern Economy: Warm Glow, Cold Feet, Promotor(s): Prof.dr. H.P.G. Pennings & Prof.dr. A.R. Thurik, EPS-2011-246-STR, http://hdl.handle.net/1765/26447

Hoogervorst, N., On The Psychology of Displaying Ethical Leadership: A Behavioral Ethics Approach, Promotor(s): Prof.dr. D. De Cremer & Dr. M. van Dijke, EPS-2011-244-ORG, http://hdl.handle.net/1765/26228

Huang, X., An Analysis of Occupational Pension Provision: From Evaluation to Redesign, Promotor(s): Prof.dr. M.J.C.M. Verbeek & Prof.dr. R.J. Mahieu, EPS-2010-196-F&A, http://hdl.handle.net/1765/19674

Hytöen, K.A. Context Effects in Valuation, Judgment and Choice, Promotor(s): Prof.dr.ir. A. Smidts, EPS-2011-252-MKT, http://hdl.handle.net/1765/30668

Iseger, P. den, Fourier and Laplace Transform Inversion with Application in Finance, Promotor: Prof.dr.ir. R.Dekker, EPS-2014-322-LIS, http://hdl.handle.

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Jaarsveld, W.L. van, Maintenance Centered Service Parts Inventory Control, Promotor(s): Prof.dr.ir. R. Dekker, EPS-2013-288-LIS, http://hdl.handle.net/1765/39933

Jalil, M.N., Customer Information Driven After Sales Service Management: Lessons from Spare Parts Logistics, Promotor(s): Prof.dr. L.G. Kroon, EPS-2011-222-LIS, http://hdl.handle.net/1765/22156

Kagie, M., Advances in Online Shopping Interfaces: Product Catalog Maps and Recommender Systems, Promotor(s): Prof.dr. P.J.F. Groenen, EPS-2010-195-MKT, http://hdl.handle.net/1765/19532

Kappe, E.R., The Effectiveness of Pharmaceutical Marketing, Promotor(s): Prof.dr. S. Stremersch, EPS-2011-239-MKT, http://hdl.handle.net/1765/23610

Karreman, B., Financial Services and Emerging Markets, Promotor(s): Prof.dr. G.A. van der Knaap & Prof.dr. H.P.G. Pennings, EPS-2011-223-ORG, http://hdl.handle.net/1765/22280

Kil, J.C.M., Acquisitions Through a Behavioral and Real Options Lens, Promotor(s): Prof.dr. H.T.J. Smit, EPS-2013-298-F&A, http://hdl.handle.net/1765/50142

Klooster, E. van't, Travel to Learn: The Influence of Cultural Distance on Competence Development in Educational Travel, Promotors: Prof.dr. F.M. Go & Prof.dr. P.J. van Baalen, EPS-2014-312-MKT, http://hdl.handle.net/1765/151460

Koendjbiharie, S.R., The Information-Based View on Business Network Performance Revealing the Performance of Interorganizational Networks, Promotors: Prof.dr.ir. H.W.G.M. van Heck & Prof.mr.dr. P.H.M. Vervest, EPS-2014-315-LIS, http://hdl.handle.net/1765/1

Koning, M., The Financial Reporting Environment: taking into account the media, international relations and auditors, Promotor(s): Prof.dr. P.G.J.Roosenboom & Prof.dr. G.M.H. Mertens, EPS-2014-330-F&A, http://hdl.handle.net/1765/1

Konter, D.J., Crossing borders with HRM: An inquiry of the influence of contextual differences in the adaption and effectiveness of HRM, Promotor: Prof.dr. J. Paauwe, EPS-2014-305-ORG, http://hdl.handle.net/1765/1

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Lam, K.Y., Reliability and Rankings, Promotor(s): Prof.dr. P.H.B.F. Franses, EPS-2011-230-MKT, http://hdl.handle.net/1765/22977

Lander, M.W., Profits or Professionalism? On Designing Professional Service Firms, Promotor(s): Prof.dr. J. van Oosterhout & Prof.dr. P.P.M.A.R. Heugens, EPS-2012-253-ORG, http://hdl.handle.net/1765/30682

Langhe, B. de, Contingencies: Learning Numerical and Emotional Associations in an Uncertain World, Promotor(s): Prof.dr.ir. B. Wierenga & Prof.dr. S.M.J. van Osselaer, EPS-2011-236-MKT, http://hdl.handle.net/1765/23504

Larco Martinelli, J.A., Incorporating Worker-Specific Factors in Operations Management Models, Promotor(s): Prof.dr.ir. J. Dul & Prof.dr. M.B.M. de Koster, EPS-2010-217-LIS, http://hdl.handle.net/1765/21527

Leunissen, J.M., All Apologies: On the Willingness of Perpetrators to Apoligize, Promotor: Prof.dr. D. De Cremer, EPS-2014-301-ORG, http://hdl.handle.net/1765/1

Liang, Q., Governance, CEO Indentity, and Quality Provision of Farmer Cooperatives, Promotor: Prof.dr. G.W.J. Hendrikse, EPS-2013-281-ORG, http://hdl.handle.net/1765/1

Liket, K.C., Why 'Doing Good' is not Good Enough: Essays on Social Impact Measurement, Promotor: Prof.dr. H.R. Commandeur, EPS-2014-307-S&E, http://hdl.handle.net/1765/51130

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Mees, H., Changing Fortunes: How China's Boom Caused the Financial Crisis, Promotor: Prof.dr. Ph.H.B.F. Franses, EPS-2012-266-MKT, http://hdl.handle.net/1765/34930

Meuer, J., Configurations of Inter-Firm Relations in Management Innovation: A Study in China's Biopharmaceutical Industry, Promotor: Prof.dr. B. Krug, EPS-2011-228-ORG, http://hdl.handle.net/1765/22745

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Kroon, EPS-2011-224-LIS, http://hdl.handle.net/1765/22444

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Oosterhout, M., van, Business Agility and Information Technology in Service Organizations, Promotor: Prof,dr.ir. H.W.G.M. van Heck, EPS-2010-198-LIS, http://hdl.handle.net/1765/19805

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Otgaar, A.H.J., Industrial Tourism: Where the Public Meets the Private, Promotor: Prof.dr. L. van den Berg, EPS-2010-219-ORG, http://hdl.handle.net/1765/21585

Ozdemir, M.N., Project-level Governance, Monetary Incentives and Performance in Strategic R&D Alliances, Promotor: Prof.dr.ir. J.C.M. van den Ende, EPS-2011-235-LIS, http://hdl.handle.net/1765/23550

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Porck, J.P., No Team is an Island, Promotor: Prof.dr. P.J.F. Groenen & Prof.dr.

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Pourakbar, M. End-of-Life Inventory Decisions of Service Parts, Promotor: Prof.dr.ir. R. Dekker, EPS-2011-249-LIS, `http://hdl.handle.net/1765/30584`

Pronker, E.S., Innovation Paradox in Vaccine Target Selection, Promotor(s): Prof.dr. H.R. Commandeur & Prof.dr. H.J.H.M. Claassen, EPS-2013-282-S&E, `http://hdl.handle.net/1765/39654`

Retel Helmrich, M.J., Green Lot-Sizing, Promotor: Prof.dr. A.P.M. Wagelmans, EPS-2013-291-LIS, `http://hdl.handle.net/1765/41330`

Rietveld, C.A., Essays on the Intersection of Economics and Biology, Promotor(s): Prof.dr. P.J.F. Groenen, Prof.dr. A. Hofman, Prof.dr. A.R. Thurik, Prof.dr. P.D. Koellinger, EPS-2014-320-S&E, `http://hdl.handle.net/1765/1`

Rijsenbilt, J.A., CEO Narcissism; Measurement and Impact, Promotor: Prof.dr. A.G.Z. Kemna & Prof.dr. H.R. Commandeur, EPS-2011-238-STR, `http://hdl.handle.net/1765/23554`

Roelofsen, E.M., The Role of Analyst Conference Calls in Capital Markets, Promotor(s): Prof.dr. G.M.H. Mertens & Prof.dr. L.G. van der Tas RA, EPS-2010-190-F&A, `http://hdl.handle.net/1765/18013`

Roza, M.W., The Relationship between Offshoring Strategies and Firm Performance: Impact of Innovation, Absorptive Capacity and Firm Size, Promotor(s): Prof.dr.

H.W. Volberda & Prof.dr.ing. F.A.J. van den Bosch, EPS-2011-214-STR, http://hdl.handle.net/1765/22155

Rubbaniy, G., Investment Behavior of Institutional Investors, Promotor: Prof.dr. W.F.C. Verschoor, EPS-2013-284-F&A, http://hdl.handle.net/1765/40068

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Shahzad, K., Credit Rating Agencies, Financial Regulations and the Capital Markets, Promotor: Prof.dr. G.M.H. Mertens, EPS-2013-283-F&A, http://hdl.handle.net/1765/39655

Sotgiu, F., Not All Promotions are Made Equal: From the Effects of a Price War to Cross-chain Cannibalization, Promotor(s): Prof.dr. M.G. Dekimpe & Prof.dr.ir. B. Wierenga, EPS-2010-203-MKT, http://hdl.handle.net/1765/19714

Sousa, M., Servant Leadership to the Test: New Perspectives and Insight, Promotors: Prof.dr. D. van Knippenberg & Dr. D. van Dierendonck, EPS-2014-313-ORG, http://hdl.net/1765/1

Spliet, R., Vehicle Routing with Uncertain Demand, Promotor: Prof.dr.ir. R. Dekker, EPS-2013-293-LIS, http://hdl.handle.net/1765/41513

Srour, F.J., Dissecting Drayage: An Examination of Structure, Information, and Control in Drayage Operations, Promotor: Prof.dr. S.L. van de Velde, EPS-2010-186-LIS, http://hdl.handle.net/1765/18231

Staadt, J.L., Leading Public Housing Organisation in a Problematic Situation: A Critical Soft Systems Methodology Approach, Promotor: Prof.dr. S.J. Magala, EPS-2014-308-ORG, http://hdl.handle.net/1765/50712

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It is all about rationality as far as behavioural economics are concerned. However, no census can be reached as to what is rationality. With the unsettled controversies over rationality, it may help to see it from a different angle. This thesis has made a special effort to explore some relevant issues on (ir)rationality. Chapter 2 and Chapter 3 answer the question what is irrationality. Chapter 2 improves the methodology to measure irrationality by proposing a new incentive system on individual decision-making: the prior incentive system (Prince). Chapter 3 addresses the issue of irrationality in decisions under ambiguity. Chapter 4 answers the question of why we steer people away from irrationality. Chapter 4 discusses whether we should correct people's irrationality by imposing a better decision when freedom of choice cannot be realized. Chapter 4 concludes with recommending strong paternalism and provides a litmus test for people's views on paternalism. Chapter 5 answers the question how to make people less irrational. Chapter 5 studies the social influences on people's decision-making processes and offers possible approaches to nudge people away from irrationality.

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