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Essays on Decision Making Under Uncertainty

Dissertation

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Abstract

This thesis examines several aspects of decision making under uncertainty.

In the first chapter, coauthored with Ondřej Rydval, Andreas Ortmann, and Ralph Hertwig, we replicate three pricing tasks of Gneezy, List, and Wu (2006) for which they document the so-called uncertainty effect, namely, that people value a binary lottery over non-monetary outcomes less than other people value the lottery's worse outcome. While the authors implemented a verbal lottery description, we use a physical lottery format, which makes misinterpretation of the lottery structure highly unlikely. We also provide subjects with complete information about the goods they are to value (book gift certificates and one-year deferred payments). Contrary to Gneezy, List, and Wu (2006), we observe for all three pricing tasks that subjects' willingness to pay for the lottery is significantly higher than other subjects' willingness to pay for the lottery's worse outcome.

In the second chapter, I investigate the relationship between attitudes towards ambiguity and the ability to reduce compound risks. The evidence from an experiment on adolescents shows that patterns identified in the previous literature are susceptible to experimental implementation and the characteristics of the subjects. Cognitive skills and the way lotteries are presented affect reduction of compound risks differently to ambiguity neutrality. My results suggest that theoretical studies which model ambiguity preferences by relaxing the assumption of compound risk reduction should be viewed with caution.

In the third chapter, I review recent experimental studies on decision making under ambiguity and identify the main determinants related to intrinsic characteristics of a subject *(static contexts)* and related to interaction between a subject and ambiguous reality *(dynamic contexts)*. Significantly fewer papers address robustness under dynamic contexts. Moreover, several studies report contradictory results and shifts to ambiguity-neutrality under certain conditions. I suggest that, if we aim to predict behavior under ambiguity, then we ought to focus on robustness of attitudes toward ambiguity, specifically in dynamic contexts.

Abstrakt

Tato disertační práce zkoumá několik aspektů rozhodování při nejistotě.

V první kapitole, jejímiž spoluatory jsou Ondřej Rydval, Andreas Ortmann, a Ralph Hertwig, replikujeme tři experimenty, ve kterých Gneezy, List a Wu (2006) ukazují takzvaný vliv nejistoty, totiž že účastníci oceňují binární loterii s nepeněžními výsledky méně, než jiní účastníci oceňují nejhorší výsledek dané loterie. Narozdíl od těchto autorů, kteří popsali strukturu loterie pouze slovně, používáme fyzický formát loterie, kterým se stává špatné pochopení loterie vysoce nepravděpodobným. Navíc účastníkům podáváme kompletní informace o oceňovaných statcích (knižních dárkových poukazech a rok odložených platbách). Narozdíl od výše zmíněných autorů shledáváme ve všech třech experimentech, že účastníci oceňují loterii významně výše, než jiní účastnící oceňují nejhorší výsledek dané loterie.

Ve druhé kapitole zkoumám vztah mezi preferencemi jedinců k nejednoznačnosti a jejich schopnosti redukovat kombinovaná rizika. Experiment provedený se žáky druhého stupně poukazuje na to, že výsledky popisované v předešlých studiích jsou náchylné na změny v experimentalním designu a charakteristikách jedinců. Tento vztah dále záleží na kognitivních schopnostech účastníků a formě prezentace loterií. Mé výsledky nabádají k obezřetnosti při interpretaci teoretických studií modelujících preference k nejednoznačnosti pomocí uvolnění podmínky redukce složených loterií.

Ve třetí kapitole vyhodnocuji poslední experimentální studie týkající se rozhodování při nejednoznačnosti, a dále určuji klíčové faktory vztahující se k vnitřní charakteristice subjektů (stabilní kontexty) a těm, které souvisejí s interakcí subjektů a nejednoznačné reality (dynamické kontexty). Zjistila jsem, že podstatně méně článků se zabývá dynamickými kontexty. Porovnáme-li navíc výsledky několika studií, zjistíme, že jejich výsledky jsou protichůdné a za určitých podmínek se posouvají k neutralitě k nejednoznačnosti. Je-li našim cílem předpovědět chování při nejednoznačnosti, navrhuji, abychom se soustředili na zkoumání postojů k nejednoznačnosti v dynamických kontextech.

Contents

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Preface

Decision theory is both among the most established and the most challenged areas in economics. The fundamental Expected Utility Theory (EUT) by von Neumann and Morgenstern and Savage's Subjective Expected Utility Theory (SEUT) are dated from around the 1950s and continue to prevail as normative standard for researchers. However, substantial and growing empirical evidence from experiments, and closely related disciplines, has been undermining the descriptive validity of EUT and SEUT and has left controversies and unanswered questions. In my dissertation, I experimentally study two such controversies, the *uncertainty effect* and *preferences towards ambiguity.* I further contribute to the ongoing debate about experimental methodology.

Since there is some ambiguity about the meaning of terms such as *risk* and *uncertainty* in the literature I here define *risk* as "an uncertain situation in which the decision-maker knows the probability distribution of outcomes", *ambiguity* as "an uncertain situation in which probabilities are not known", and *uncertainty* as "an umbrella term, which refers to any decision problem with uncertain parameters".

In the first chapter, which is a joint work with Ondřej Rydval, Andreas Ortmann, and Ralph Hertwig, we investigate the robustness of the *uncertainty effect.* Gneezy, List and Wu (2006) introduced this term to describe a situation where individuals valued an uncertain prospect less than its worst possible outcome. Valuation of an uncertain prospect between the values of its lowest and highest outcomes is the common implicit assumption for most deterministic models of risky choice. Gneezy *et al.* observed the *uncertainty effect* in both their lab and field treatments. They explained it by extreme risk-aversion, which may exist for between-subject comparison and non-monetary outcomes for lotteries (which seems not to exist for within-subject design). In contrast, we conjectured that subjects might have had problems understanding the tasks and not enough motivation to provide true valuations. We used a physical lottery format and complete information about the uncertain prospects, thus eliminating possibilities for misinterpretations. Using real and hypothetical willingness-to-pay tasks and hypothetical evaluation of deferred payments, we were not able to replicate the results of Gneezy *et al.* Our paper was published in *Experimental Economics* in 2009, and since then the study by Gneezy *et al.* has been challenged by several other groups of researchers. In Table 1, I summarize published papers on the *uncertainty effect*. Half of the studies reported only hypothetical treatments. According to our paper, the lack of incentives in hypothetical tasks might produce biased results; in this sense the *uncertainty effect* is an artifact of the features of experimental design and implementation. Turning to experiments with real stakes, Sonsino (2008) provided low financial incentives and Drichoutis *et al.* (2012) showed that overbidding in their experiments (valuing a prospect more than its best outcome) was correlated with competitiveness. Highly competitive subjects might have been more interested in the possibility of winning than in providing true valuations. Therefore, for these two papers the observed *uncertainty effect* could be a result of low stakes and a competitive setup. Given evidence from the experiments shown in Table 1, the *uncertainty effect* does not seem prevalent. The alleged *uncertainty effect* illustrates the importance of methodology and systematic analysis when studying violations of any decision theory.

In the second and third chapters, I analyze preferences towards ambiguity, yet another behavior incompatible with SEUT. Since Ellsberg (1961), experimentalists have demonstrated numerous examples when individuals (dis)liked ambiguity and theorists have tried to accommodate these preferences in new models. So far, it is unclear which of those models are the most appealing empirically.

To contribute to this discussion, in the second chapter I tested theories which present ambiguity as a two-stage (compound) lottery. In these models, ambiguity-sensitive behavior is defined as non-reduction of compound lotteries. Thus, individuals who exhibit different values for a compound lottery and a corresponding simple lottery are those who are either ambiguity-averse or ambiguity-seeking. In my experiment, I used middle-school adolescents and controlled for cognitive and non-cognitive abilities. For a significant part of my subject sample I did not observe a significant relationship between ambiguity-neutral behavior and reduction of compound lotteries. Moreover, the results are susceptible to experimental design and subjects' characteristics. Therefore, I caution about modeling ambiguity by relaxing the reduction-of-compound-lotteries assumption and using compound lotteries to represent ambiguity in experiments.

Despite a large and growing number of theories describing preferences for ambiguity, several recent experimental papers reported inconsistency of ambiguity-sensitive behavior. Should we consider preferences towards ambiguity as a rational behavior, and thus model it as a normative improvement to current theories, or as an anomaly, and attempt to accommodate it only for descriptive purposes? To answer this question we need to understand the nature of preferences to ambiguity and their robustness under different contexts. In the third chapter, I review experimental literature to identify, classify and analyze determinants of preferences to ambiguity. I create a database and categorize all studies depending on whether determinants are related to intrinsic characteristics of a decision maker (*static contexts*) or interaction between a decision maker and ambiguity (*dynamic contexts*). I discover a disproportional focus in research attention on static rather than dynamic contexts. Moreover, most authors who focused on dynamic contexts reported controversial results and indeed revealed inconsistent behavior under ambiguity. I propose suggestions for further research.

Even though the frontier in decision making research moves rather quickly, in my thesis I attempted to emphasize the importance of timely testing of the so-called "paradoxes" which challenge the existing theories, as well as the new theories which appear as descriptive improvements.

Table 1: Literature on *uncertainty effect*

Study	Sample size[a]			Framing	Elicitation	UE?
	Pricing	Choice	Field			
	L/Lottery/H	L/Lottery	L/Lottery/H			
Gneezy *et al.* (2006)	20/20/20 (98/226/99)	37/43 (180/182)	50/150/50 (43/140/46)	Book gift certificates, sport cards market	BDM (Pricing & Choice), SPA (Field)	yes
Sonsino (2008)	120, within-subject design			Three objects and lotteries over pairs	Web SPA, 5% chance for real play	yes (27% of total)
Keren and Willemsen (2009)	(54/118/53)	(151/314)		Only hypothetical questions		no[b]
Rydval *et al.* (2009)	53/53/- (15/89/-)			Book gift certificates	MPL, physical representation	no
Simonsohn (2009)	(140/135/140)			Only hypothetical questions		yes
Drichoutis *et al.* (2012)	-/71/-	-/71		P-bet and $-bet lotteries	Second-price auction	yes[c]
Newman and Mochon (2012)	(155/164/155)			Only hypothetical questions		yes
Benzion *et al.* (2013)	66/143/66			Several objects and lotteries over pairs	Both WTP & WTA, SPA	no

[a]Sample size in corresponding tasks: Pricing = willingness to pay for gift certificates; Choice = choice between a lottery and cash; Field = field study. L = low outcome valuation task; Lottery = lottery valuation task; H = high outcome valuation task. Numbers without parenthesis refer to treatments with real stakes, numbers in parenthesis refer to hypothetical treatments;

[b]For those who comprehended the lottery instructions correctly;

[c]Overbidding behavior was correlated with competitiveness.

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Chapter 1

How Certain is the Uncertainty Effect?[1]

(co-authored with Ondřej Rydval, Andreas Ortmann, and Ralph Hertwig)

We replicate three pricing tasks of Gneezy *et al.* (2006) for which they document the so-called uncertainty effect, namely, that people value a binary lottery over non-monetary outcomes less than other people value the lottery's worse outcome. While the authors implemented a verbal lottery description, we use a physical lottery format which makes misinterpretation of the lottery structure highly unlikely. We also provide subjects with complete information about the goods they are to value (book gift certificates and one-year deferred payments). Contrary to Gneezy *et al.* (2006), we observe for all three pricing tasks that subjects' willingness to pay for the lottery is significantly higher than other subjects' willingness to pay for the lottery's worse outcome.

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[1]An earlier version of this work has been published in Ortmann, Prokosheva, Rydval, and Hertwig (2007) "Valuing a Risky Prospect Less than Its Worst Outcome: Uncertainty Effect or Task Ambiguity?", *CERGE-EI WP series*, No. 334; in Rydval, Ortmann, Prokosheva, and Hertwig (2009) "How Certain Is the Uncertainty Effect?", *CERGE-EI WP series*, No. 385 and (a shorter version) in Rydval, Ortmann, Prokosheva, and Hertwig (2009) "How Certain Is the Uncertainty Effect?", *Experimental Economics*, 12, 473–487. We thank Pavlo Blavatskyy, Jordi Brandts, John Duffy, Uri Gneezy, Glenn Harrison, John List, Doron Sonsino, Tim Salmon, Lise Vesterlund, George Wu, participants of the ESA World Meeting 2007 at LUISS in Rome, and an anonymous referee for comments. This research was partly supported by a research center grant No. LC542, grant No. MSM0021620846 from the Czech Ministry of Education, Youth, and Sports, and by Swiss National Science Foundation Grant 100014-118283. All errors are my own.

1.1 Introduction

Most theories of decision under risk require that the value of any risky prospect lie between the value of the prospect's best and worst outcomes. Gneezy *et al.* (2006, henceforth GLW) term this requirement the *internality axiom* (henceforth IA) and document its systematic violations.[2] For various valuation goods (book gift certificates, one-year deferred payments, work effort, and sports cards), elicitation modes (pricing and choice), and implementation variants (hypothetical and real-stakes, laboratory and field experiments), GLW demonstrate that people value binary lotteries with intermediate probability mixes less than other people value the lotteries' worse outcomes.

GLW propose that IA violation is caused by what they call the *uncertainty effect*, attributable to two lottery design features that obstruct the IA. First, the lotteries involve non- monetary outcomes, the valuation of which may induce higher cognitive demands or perception of uncertainty. Second, the between-subjects design does not prompt subjects to value the lotteries based on valuing their outcomes. GLW argue that these two design features jointly trigger a "risk and return" lottery valuation process incompatible with most theories of decision under risk: Rather than valuing the lottery outcomes, subjects are hypothesized to value the expectation of the outcomes' face values and subsequently discount it for the risk involved in the lottery. This valuation process can indeed explain the observed IA violation if a high risk premium is levied on the lotteries with intermediate probability mixes.[3]

Our study of the *uncertainty effect* has two stages. In an initial study (reported fully in Ortmann *et al.*, 2007), we examine whether GLW's experimental instructions – using a verbal and possibly ambiguous lottery description – could have contributed to IA violation that these authors report. For hypothetical pricing of book gift certificates, we show that rewording GLW's lottery instructions increases the lottery's valuation to an extent that essentially eliminates the possibility of IA violation. Nevertheless, our initial study still uses a verbal lottery description, which could in principle lead to misinterpretation. Sharing our concerns with GLW's lottery instructions, Keren and Willemsen (2009) demonstrate that verbally describing the same lottery in terms of a coin flip or a spinner wheel helps alleviate IA violation, though some subjects still fail a lottery comprehension test.

[2]In their footnote 1, GLW discuss theories of decision under risk that in principle permit IA violation. While the IA is seemingly derived from deterministic theories, its empirical tests have implications for stochastic theories as well.

[3]GLW in fact always observe IA violation for equiprobable binary lotteries, except for the hypothetical pricing of gift certificates, where IA violation also occurs for lotteries with the probability mixes of (0.6, 0.4) and (0.4, 0.6).

Our main, more extensive study aims to further enhance the transparency of GLW's tasks, while retaining the design features that the authors regard as essential for observing the *uncertainty effect*. We systematically replicate three of GLW's tasks involving hypothetical and real-stakes pricing of gift certificates and hypothetical pricing of deferred payments. Unlike the authors of previous studies, we implement a "physical" lottery format. Using equiprobable binary lotteries, we elicit subjects' willingness to pay for the opportunity of drawing a good (a gift certificate or a deferred payment form) from a closed bag containing two goods which are identical except for their face value. This lottery structure is physically demonstrated by the experimenter while reading aloud the instructions, which arguably rules out any misinterpretation. Furthermore, unlike previous studies except for Sonsino's (2008) web-based experiment, we provide subjects with complete information about the goods they are to value.

Following GLW, we use a between-subjects design. GLW document previous, as well as their own, results suggesting that neither between-subjects design nor non-monetary outcomes can separately induce IA violation. As detailed in the next section, however, Sonsino (2008) documents some (relatively minor) IA violation even in a within-subjects design, despite making the IA principle transparent to subjects.

We find no evidence for the *uncertainty effect*. Contrary to GLW, and in line with the IA, we observe for all three pricing tasks that subjects' willingness to pay for the lotteries is significantly higher than other subjects' willingness to pay for the lotteries' worse outcomes. In the next section, we outline our design and implementation and relate them to other studies that have, in various forms, replicated GLW's experiment. Section 3 presents the results, paying special attention to the between-subjects nature of the data. The concluding section discusses which implementation differences most likely lie behind the systematic discrepancy between our and GLW's findings.

1.2 Design and implementation

1.2.1 The valuation tasks and lottery implementation

We study three pricing tasks for which GLW document IA violation: hypothetical pricing of book gift certificates, real-stakes pricing of book gift certificates, and hypothetical pricing of deferred payments. For each task, we run a lottery treatment eliciting subjects' willingness to pay for an equiprobable binary lottery featuring two gift certificates (deferred payments) with a face value of x and $2x$, and a baseline treatment eliciting other subjects' willingness to pay for the worse gift certificate (deferred payment) with a face value of x. Hence the IA is violated whenever the lottery-baseline between-subjects treatment effect is negative[4] In several instances, we in fact run more than one lottery or baseline treatment for a given task, as will become clear from the following description and from the design matrix in Table 1.1. Experimental instructions for the various treatments are included in Appendix 1.

Our initial study is motivated by our conjecture that GLW's lottery instructions – by describing the lottery purely verbally and by making a conceptual divide between the to-be- valued lottery ticket and the lottery outcomes – might have led to misinterpretation of the lottery structure. For the hypothetical pricing of gift certificates, we first conduct a lottery treatment T1 using GLW's instructions. In another lottery treatment T2, we reword GLW's lottery instructions in a way that assigns the lottery probabilities directly to the gift certificates' face values. We use gift certificates for the Luxor Book Palace (Neoluxor), which is one of the largest bookstores in the Czech Republic located within walking distance of the experimental site. As in GLW, the gift certificates are valid for the next two weeks.

To preview the results, the rewording of GLW's lottery instructions in T2 is associated with a strong upward shift in lottery valuations compared to T1. More importantly, the rewording essentially eliminates IA violation, since 84% of lottery valuations in T2 are at or above the face value of the worst gift certificate. Nevertheless, we admittedly cannot exclude the possibility that our rewording induces other types of misinterpretation of the lottery. To tackle this issue, our main study uses a physical lottery format which – even without explicitly mentioning the lottery at all – makes lottery misrepresentation highly unlikely.[5]

The following physical format is implemented in all lottery treatments of our main study (i.e., in all treatments in the middle row of Table 1.1 except for T1 and T2). While reading the lottery instructions aloud, the experimenter presents a bag into which he places the two gift certificates (or deferred payment forms) and demonstrates how one certificate (form) is to be randomly drawn from the closed bag. The instructions explain that the two certificates (forms) are identical except for their value – which is also apparent to subjects when inspecting the circulated certificates (forms) as explained right below – and thus the chances of drawing either the better or the worse certificate (form) are equal. Subjects are

[4] Our design does not address the possibility that lotteries are valued more than their best outcomes, though Sonsino (2008) demonstrates this can happen in very rare cases (in 0.75% of observations).

[5] Using a physical lottery format to study sources of risky decision anomalies is of course nothing novel and goes back a long way. See, for instance, Grether and Plott (1979, Section II) where a physical lottery demonstration does not help alleviate preference reversals, and Gigerenzer *et al.* (1988, Experiment 1) where a physical demonstration of random sampling lessens base rate neglect significantly.

then asked to state their willingness to pay for the opportunity of drawing one gift certificate (deferred payment form) from the bag.[6]

Our main study also uses gift certificates for the Luxor Book Palace (Neoluxor), but we now provide subjects with more information about the certificates. The instructions explain that the certificates are valid for the next three months and enable in-store and online purchase of books (including CD and DVD formats), maps, stationery, etc.[7] In all gift certificates treatments (T3-T8), the experimenter circulates among subjects several gift certificates of the appropriate face value in order to ensure common knowledge and enhance credibility. While reading the instructions aloud, the experimenter also mentions other conditions of use of the certificates, such as the characteristic that no cash is returned if one's purchase falls below the certificates' face value.

In the hypothetical pricing of deferred payments, we use hypothetical payment forms guaranteeing cash payment one year from the date of the experiment. The instructions explain that the deferred payment would (in a real-stakes scenario) be guaranteed by the research organization that finances the experiment. In all deferred payments treatments (TP3-TP8), we again circulate several (hypothetically filled out) payment forms of the research organization for subjects' inspection. While reading the instructions aloud, the experimenter also mentions that the deferred cash payment would (in a real-stakes scenario) be made by one of the experimenters at the experimental site.

In the hypothetical pricing of gift certificates, $x = 500$CZK (about \$25), roughly matching the (student) purchasing power of $x = \$50$ used in GLW. In the hypothetical pricing of deferred payments, we again use $x = 500$CZK to make the two hypothetical pricing tasks comparable (GLW used $x = \$100$). In the real-stakes pricing of gift certificates, $x = 200$CZK (about \$10) and all subjects' decisions are played out, while GLW used $x = \$50$ and played out the decisions of 5% (1 in 20) of the subjects. Note that $x = 200$CZK is still a substantial amount of money: it would be sufficient to purchase just under half of all books and textbooks and a much larger proportion of stationery items currently sold at the Neoluxor online shop (and the certificates can of course co-finance purchases exceeding their face value).

We next relate our design and implementation to other studies of the *uncertainty effect*. In a replication of GLW's hypothetical pricing of gift certificates, Keren and Willemsen

[6]For the real-stakes pricing of gift certificates, we were unable to obtain gift certificates worth $2x$, so we used two identical table tennis balls marked x and $2x$ to represent the real gift certificates. Subjects were informed that if they were to draw a ball marked $2x$, they would receive two gift certificates worth x.

[7]The three-month validity of the gift certificates is longer than the two-week validity in GLW, our initial study, and Keren and Willemsen (2009). The discrepancy is not our choice but rather due to the bookstore's current policy. For completeness, Sonsino's (2008) gift certificates were valid for 6 months.

(2009) find that when the equiprobable binary lottery is described in terms of a coin flip or a spinner wheel (though only verbally), no IA violation occurs, whereas when the lottery structure is described purely verbally though explicitly, the IA is violated.[8] Furthermore, a considerably higher proportion of subjects pass a lottery comprehension test in the former implementation (69–87%) compared to the latter implementation (29–43%). In general, IA violation seems to occur only for the group of subjects who fail the lottery comprehension test. While these findings are illuminating, one should note that GLW also use a (verbally described) coin-flip lottery implementation in their real-stakes gift certificates tasks but still observe IA violation. Also, Keren and Willemsen's lottery description retains the lottery ticket (or lottery participation) terminology and the lottery itself is not demonstrated to subjects physically, which could be a reason why the proportion of subjects failing the lottery comprehension test is non-negligible.

In another related but *within*-subjects study, Sonsino (2008) runs a web-based experiment where subjects first value three gift certificates with widely different (undisclosed) market prices, featuring a luxurious weekend vacation, a gourmet dinner, and a choice between a fine bottle of wine and a box of gourmet chocolate. Subjects then value binary lotteries featuring pairs of the certificates while observing their own previous valuations of the certificates themselves. The valuations are elicited using a sequence of six- bidder Vickrey auctions (the probability of an auction being played out is about 5%). The lotteries (involving various probability mixes) are described verbally using the lottery ticket terminology and pie charts. Subjects are invited to ex-post participate in the actual lottery draw, where lottery outcomes are determined by volunteer subjects secretly choosing numbers which are then compared to randomly generated numbers.

To the best of our knowledge, Sonsino (2008) is the first study to demonstrate that IA violation can occur in a within-subjects design. In almost 12% of cases, subjects value a lottery less than they value either of the lottery's outcomes, and 27% of subjects do so at least once. IA violation gets more frequent as the probability of winning the lotteries' better outcomes decreases, contrary to GLW where IA violation occurs only for lotteries with intermediate probability mixes. One may only speculate about the reasons behind these results, including the web-based nature of Sonsino's experiment (potentially leading to loss of control), the auction-based elicitation mechanism, the lotteries featuring different goods (rather than goods that are identical except for their face value), and the sequential nature

[8] The latter result is also observed by Simonsohn (2009), who implements the hypothetical pricing of gift certificates with an explicitly worded lottery as part of a series of surveys and experiments.

of the valuations possibly generating valuation order effects. Besides these potential reasons, it is once again possible that the verbal (though explicit) lottery description – involving the lottery ticket terminology and a rather lengthy explanation of the lottery draw – was misinterpreted by some subjects and contributed to what Sonsino calls "lottery aversion".[9]

1.2.2 Eliciting willingness to pay (WTP)

In the hypothetical pricing of gift certificates and deferred payments, we follow GLW in that the instructions simply ask subjects to state the highest price they would be willing to pay. One can of course imagine a procedurally more incentive-compatible mechanism for the hypothetical WTP elicitation, such as a hypothetical-stakes version of the elicitation mechanism we use for the real-stakes pricing task. However, for the sake of replication, we wished to retain the key features of GLW's hypothetical-stakes design for which the authors document IA violation. We hope – as, implicitly, did the authors of the previous hypothetical studies of the *uncertainty effect* – that the (potential) hypothetical bias does not interact with the lottery and baseline treatments in a way that biases the treatment effect towards or away from IA violation.

In the real-stakes pricing of gift certificates, we use the multiple price list (MPL) mechanism for eliciting WTP. In the lottery and baseline treatments T5 and T7, respectively, subjects receive $2x$ for participating. They are asked to indicate (by circling either Yes or No) their willingness to pay various prices listed in the MPL, where the prices range from $0.1x$ to $2x$ and rise in $0.1x$ increments. Subjects know that only one of their 20 decisions is payoff-relevant: after making all 20 decisions, each subject randomly draws a card from a box with cards numbered 1 to 20 to determine her payoff-relevant row. If she circled Yes in that row, she pays the price and gets the gift certificate worth x (in T7) or randomly draws a certificate from a bag containing two certificates worth x and $2x$ (in T5). If she circled No in the payoff-relevant row, she earns the participation fee.

We also run a supplementary baseline treatment T8 with a narrower MPL price range of $0.1x$ to x; hence the participation fee is x and subjects make only 10 Yes-or-No decisions. This would normally be a standard MPL procedure for valuing the gift certificate worth x, but we wish to guard ourselves against the possibility of a "mid-table" effect: A potential

[9] Sonsino (2008) argues that IA violation could be triggered solely by subjects' aversion to the presented lotteries per se. In the author's post-experimental questionnaire, aversion to lotteries is the most frequently chosen explanation for IA violation. Subjects were shown an example of within-subjects IA violation and, if admitting to the (hypothetical) possibility of exhibiting such behavior, were prompted to choose their favorite explanation for the behavior from a list of three options, the other two being "noise distraction" and "other explanations.".

caveat of the MPL method is that subjects may be naturally drawn towards the middle of the MPL. Thus if the lottery treatment T5, with the wide price range, were only compared with the baseline treatment T8, with the narrow price range, the mid-table effect could work against the occurrence of IA violation. We circumvent this problem by implementing the lottery and baseline treatments T5 and T7, respectively, with the same (wide) price range. In addition, comparing WTP valuations in T7 and T8 allows us to assess whether the mid-table effect is actually present in the baseline treatments.

To further guard ourselves against the possibility that the mid-table effect works against the occurrence of IA violation, we conduct a supplementary lottery treatment T6 where, similar to Andersen *et al.* (2007) and Harrison *et al.* (2007), the MPL is asymmetric: it is "skewed low" in the sense that the mid-row price is well below the mean of the MPL (in fact below 0.8x). If the mid-table effect is indeed present, the asymmetric MPL should (*ceteris paribus*) induce lower WTP valuations and hence favor the occurrence of IA violation. Furthermore, since the asymmetric MPL in T6 shares all prices up to $1.4x$ with the symmetric MPL in T5, we can directly compare WTP valuations in T5 and T6 to see whether the mid-table effect is actually present in the lottery treatments (in the price region up to $1.4x$ where most lottery valuations can be expected to fall).

In sum, to give IA violation a fair chance to occur in our data, we run the baseline treatment T7, with the wide MPL price range, and the lottery treatment T6, with the asymmetric MPL, both of which favor the occurrence of IA violation if the mid-table effect is indeed present. While one can think of other behavioral effects being induced by our variation of the lottery and baseline treatments, such as changing the effective power of financial incentives, we cannot find a reason why any such effect should work against the occurrence of IA violation.

GLW used the Becker-DeGroot-Marschak (BDM) mechanism to elicit real-stakes WTP. We use the MPL elicitation mechanism as an alternative incentive-compatible elicitation mechanism. We do not wish to contrast the potential advantages and disadvantages of the MPL and BDM elicitation mechanisms. Our main goal is to assess the *direction* of the lottery-baseline treatment effect (i.e., the occurrence of IA violation or lack thereof) rather than obtaining WTP point estimates. Any incentive-compatible elicitation mechanism should serve that goal, unless the mechanism interacts with the lottery and baseline treatments in a way that biases the treatment effect towards or away from IA violation. In this respect, given the well-known concerns with the BDM mechanism (e.g., Karni and Safra, 1987; Harrison, 1992; Horowitz, 2006), and having explicitly accounted for the mid-table effect that potentially induces treatment interactions when using the MPL mechanism, we feel more confident using the MPL mechanism to study the lottery-baseline treatment effect.

There are of course refinements of the basic MPL procedure which, for example, allow subjects to express indifference in their WTP, or elicit more precise WTP valuations by iteratively decreasing the MPL price increments (e.g., Andersen *et al.*, 2007). Once again, however, given our focus on the direction of the lottery-baseline treatment effect rather than on WTP point estimates, the benefits of such refinements, in our view, do not outweigh their potential costs associated with increased complexity of the elicitation procedure. Our MPL price increments of $0.1x = 20$CZK (about $1) seem fine enough to lessen concerns related to the interval-censored nature of MPL responses, which we in any case address statistically (see footnote 15).[10] Also, our subjects can anytime make decisions in a non-monotonic manner, which may well indicate indifference. We tackle these (very rare) cases by using wider price intervals to represent the concerned subjects' WTP, and we always do so in a way that favors the occurrence of IA violation.

1.2.3 Other design and implementation details

As already mentioned above, the lottery and baseline treatments for each pricing task were conducted in a between-subjects design. However, there is a within-subjects component in our design in that the hypothetical pricing of deferred payments was run as a "surprise" task following the hypothetical and real-stakes pricing of gift certificates. In particular, a lottery (baseline) treatment of the deferred payments task always followed a lottery (baseline) treatment of the gift certificates tasks. In Table 1.1 and Table 1.4, TP3 for instance denotes the deferred payments lottery treatment which followed the gift certificates lottery treatment T3. The deferred payments lottery treatments (TP3, TP5 and TP6) are identical except that each of them is preceded by a different gift certificates lottery treatment, and similarly for the deferred payments baseline treatments (TP4, TP7 and TP8).

While the instructions for the deferred payments task reminded subjects that they faced a new task unrelated to the gift certificates task just finished, the gift certificates valuation undoubtedly influenced the subsequent deferred payments valuation in some manner. However, given the variety of gift certificates lottery (baseline) treatments preceding the deferred payments lottery (baseline) treatments, we can study this influence in a systematic manner

[10] There is also some controversy regarding whether WTP can actually be elicited as precisely as required by mechanisms that elicit point-estimate responses, such as the BDM mechanism or the simplistic hypothetical elicitation mechanism that we use in the hypothetical pricing tasks. See, for example, Andersen *et al.* (2007) for further discussion.

when assessing the lottery-baseline treatment effect for the deferred payments task, as outlined in section 3.3. For this reason, we do not view the deferred payments valuations as less informative than the gift certificates valuations.

Both our initial and main studies were run in a pen-and-paper format as in GLW, and all sessions were conducted by the first author in Czech (the experimental instructions in the Appendix 1 are translations of the original Czech instructions). All parts of the experiment were anonymous and the payments as well as lottery draws (if any) were done privately at the end of a session. Including an initial demographic questionnaire, hypothetical sessions lasted about 20 minutes while sessions involving the real-stakes pricing task lasted slightly longer. Subjects earned 100CZK (about $5) for participating in the hypothetical sessions, while the participation fee was 400CZK or 200CZK in sessions involving the real-stakes pricing task (see section 2.2).

The initial study was conducted in early April 2007 while the main study in early December 2008. Subjects in our initial study were 64 students from the Faculty of Social Sciences of the Charles University in Prague, recruited using posters. Subjects in our main study were 150 students from various Prague universities recruited online using ORSEE (Greiner 2004). Just over a third of them were students from various branches of engineering, one quarter were students of finance, business, management or accounting, another quarter were economists, and the remainder came from other fields. Subjects were 18 to 30 years old with the mean and median age of 22 years, and 67% of them were males.

1.3 Results

Before reporting our results, a (perhaps obvious) note of caution is in order.[11] In any between-subjects study of this kind, one would hope that the subject pool is reasonably homogenous (or treatments properly randomized) in relevant aspects, in order to permit unconditional lottery-baseline treatment comparisons; or that observable demographic characteristics can account for relevant across-treatment differences in subject pool composition and hence permit conditional treatment comparisons. One should nevertheless be open to the possibility that, even if the uncertainty effect as described by GLW is nonexistent, genuinely lower gift certificates valuation (real-stakes or hypothetical) in the lottery treatment compared to the baseline treatment could *in principle* generate IA violation. By a similar

[11] This cautionary note is not meant to imply that GLW and other studies of the *uncertainty effect* are unaware of the potential caveats of using a between-subjects design.

token, genuinely higher time discounting of the deferred payments in the lottery treatment compared to the baseline treatment could *in principle* generate IA violation. Also, high risk aversion of subjects in the lottery treatments could contribute to – though by itself not generate – IA violation.[12]

We report both unconditional tests for the lottery-baseline treatment effect (t-test, Wilcoxon rank-sum test and Kolmogorov-Smirnov test) as well as t-tests (or Wald tests) that condition on the collected demographic characteristics (age, gender, year and field of study, and a wealth proxy related to family car ownership).[13] However, we remain cautious that the reported treatment effects might be affected by across-treatment differences in gift certificates valuation, time preferences or other relevant individual characteristics that we do not account for. What gives us some confidence in our results is that, across all three pricing tasks, we observe a systematic lottery-baseline treatment effect in the direction of the IA (contrary to the systematic IA violation documented by GLW).

Since the stakes (real or hypothetical) are substantial across the three pricing tasks, we report all WTP figures as percentages of x, i.e., the face value of the worse gift certificate or deferred payment. This permits a clearer comparison of WTP valuations across tasks and vis-à-vis previous studies. Any such between-subjects comparison should naturally be interpreted with the above cautionary note in mind. In sections 3.1-3.3 below, treatments are numbered as in Table 1.1 and additionally have a short verbal description in accordance with the earlier discussion.

1.3.1 Hypothetical pricing of gift certificates

Table 1.2 displays summary statistics and beneath them statistical tests for the hypothetical pricing of gift certificates, jointly for our initial and main study.[14] Focusing first on the three lottery treatments T1-T3, WTP valuations are very similar in T3 (using the physical lottery format) and T1 (replicating GLW's lottery instructions). On the other hand, WTP valuations are considerably higher in T2 (rewording GLW's lottery instructions), and significantly so as shown in the first three test rows. This discrepancy may be due to the different

[12] Here we mean a standard concept of risk aversion, broadly defined but quite different from Sonsino's (2008) "lottery aversion" concept discussed in our footnote 9.

[13] Some of the tests may be deemed more appropriate than others depending on how one views the nature of the data – see also footnote 10.

[14] In the OLS estimation yielding the conditional t-test statistics, the collected demographic characteristics are jointly significant at the 5% level and subjects' year of study is individually significant at the 5% level (other controls including session dummies are individually insignificant at the 10% level). We omit the wealth proxy from the final estimation since it could be viewed as a controversial indicator of subjects' wealth and is in any case highly insignificant.

presentation of the lottery structure in T2, but as noted above, it may be at least partly due to across- treatment differences in subject pool composition that we do not account for. For further comparison, all three of our lottery treatments have higher WTP valuations compared to GLW, where the corresponding 95% confidence interval for WTP mean is merely (18.83, 45.66), and also compared to the corresponding 95% confidence interval of (50.00, 94.80) in Keren and Willemsen (2009, Experiment 2, taking only subjects passing their lottery comprehension test).

Turning next to our baseline treatment T4, the 95% confidence interval for WTP mean is (59.18, 79.22), while GLW's corresponding confidence interval is (41.38, 63.02) and Keren and Willemsen's (2008; Experiment 2) confidence interval is (45.20, 60.00). Although our sample size in T4 is relatively small, this comparison of baseline treatments seems to indicate higher "genuine" (though hypothetical) valuations of gift certificates in our study compared to the other two studies. Other things equal, this would work in favor of us finding IA violation, but we observe even higher WTP valuations in the lottery treatments, as detailed next.

In particular, the most appropriate lottery-baseline comparison is between T3 and T4, which share the implementation features of our main study. The treatment effect is clearly in the direction of the IA: as the fourth test row shows, WTP valuations are significantly higher in the lottery treatment T3 than in the baseline treatment T4. Not reported in Table 1.2, comparing the baseline treatment T4 with the initial study's lottery treatments, T1 and T2, yields statistically even stronger support for the IA.

1.3.2 Real-stakes pricing of gift certificates

Table 1.3 displays summary statistics and beneath them statistical tests for the real-stakes pricing of gift certificates.[15] Out of the 109 subjects completing the task, we detect three incomplete responses which clearly signal misunderstanding of the MPL valuation procedure (one in a lottery treatment and two in a baseline treatment) and consequently exclude these subjects from the analyzed sample. We further observe two subjects entering a non-monotonic response: a single No response preceded and followed by Yes responses. We treat these one-off cases of non-monotonicity by recoding the non-monotonic No responses

[15] In the interval regression estimation that takes into account the interval-censored nature of the MPL responses and yields the conditional Wald test statistics, the collected demographic characteristics are jointly significant at the 10% level and subjects' age is individually significant at the 5% level (other controls including session dummies are individually insignificant at the 10% level). We again omit the wealth proxy from the final estimation, for reasons explained in footnote 14.

as Yes responses, which favors IA violation, since both subjects happen to be in a baseline treatment.

We start with assessing the extent of the mid-table effect. The lottery treatment T6, with the asymmetric MPL, has slightly lower WTP valuations compared to the lottery treatment T5, with the symmetric MPL, which is in the direction of the mid-table effect. However, the across-treatment differential is small and far from significant, as shown in the first test row. There is stronger evidence for the mid-table effect in the baseline treatments, where T8, with the narrow MPL price range, has considerably lower WTP valuations than does T7, with the wide MPL price range. The across-treatment differential is statistically significant, as shown in the second test row.

We next turn to the lottery-baseline treatment effect, which can be evaluated in several ways. One can pool the lottery treatments T5 and T6 and the baseline treatments T7 and T8, as done in columns (5) and (6) of Table 1.3, respectively. This yields 95% confidence intervals for WTP mean of (74.83, 92.91) for the lottery treatments and (56.57, 69.85) for the baseline treatments. Hence the treatment effect is clearly in the direction of the IA, as is also confirmed in the third test row. Alternatively, one can make stricter treatment comparisons that favor IA violation, for example by excluding the baseline treatment T8, with the narrow MPL price range (see the fourth test row), or by contrasting only the lottery treatment T6, with the asymmetric MPL, and the baseline treatment T7, with the wide MPL price range (see the fifth test row). Even these stricter comparisons provide clear support for the IA.

For comparison, GLW's lottery treatment yields much lower WTP valuations compared to ours: the corresponding 95% confidence interval for WTP mean is (40.34, 71.66). On the other hand, GLW's baseline treatment yields a 95% confidence interval for WTP mean of (66.77, 85.23), which is higher than in our case but not so much higher than in our baseline treatment T7. This latter comparison seems to suggest comparable "genuine" valuations of gift certificates in our and GLW's study.

Bearing in mind the design and implementation differences, one can further compare the hypothetical and real-stakes WTP valuations of gift certificates in our Table 1.2 and Table 1.3, respectively. Casual comparison suggests that there is a minor upward hypothetical bias in both the lottery and baseline treatments (excluding for now the exceptionally high valuations in T2). By contrast, similar comparison of GLW's hypothetical and real-stakes WTP valuations suggests a downward hypothetical bias in both the lottery and baseline treatments.

1.3.3 Hypothetical pricing of deferred payments

Table 1.4 displays summary statistics, and beneath them statistical tests for the hypothetical pricing of deferred payments.[16] As explained in section 2.3, the lottery (baseline) treatments of this task followed the lottery (baseline) treatments of the gift certificates pricing tasks. Spearman's rank correlation coefficient between WTP valuations in the gift certificates and deferred payments treatments ranges between 0.27 and 0.60. The correlation is generally higher for the lottery treatments than for the corresponding baseline treatments, which is likely due to subjects' risk preferences affecting both of their valuations in the lottery treatments, whereas the baseline treatments lack this common valuation factor.

The lottery-baseline treatment effect can again be evaluated in several ways. One can pool all lottery treatments and all baseline treatments, as done in columns (1) and (2) of Table 1.4, respectively. This yields 95% confidence intervals for WTP mean of (70.28, 87.66) for the lottery treatments and (41.62, 54.89) for the baseline treatments. Hence the overall treatment effect is clearly in the direction of the IA, as confirmed in the first test row. Alternatively, one can evaluate the treatment effect separately for the treatments following the real-stakes gift certificates treatments (as done in columns (3) and (4) and tested in the second test row), and for the treatments following the hypothetical gift certificates treatments (as done in columns (5) and (6) and tested in the third test row). These separate comparisons both provide clear support for the IA.

For comparison, GLW's lottery treatment yields much lower WTP valuations than ours: the corresponding 95% confidence interval for WTP mean is merely (19.51, 45.49). GLW's baseline treatment yields a 95% confidence interval of (34.02, 53.18), which is only marginally lower than in our case (see columns (2), (4) and (6) of Table 1.4). Thus "genuine" valuations of deferred payments are quite comparable in our and GLW's study.

We next address in more detail how subjects' WTP valuations in the deferred payments task are influenced by their previous gift certificates valuations. Comparing column (3) with column (5) and column (4) with column (6), and inspecting the fourth test row, one can see that WTP valuations of deferred payments are significantly higher when preceded by hypothetical rather than real-stakes gift certificates valuations. This could indicate a kind of anchoring effect related to the upward hypothetical bias of the gift certificates valuations reported in section 3.2. Alternatively, it could also indicate a "disciplining" effect of the

[16] In the OLS estimation yielding the conditional t-test statistics, the collected demographic characteristics are jointly significant at the 10% level and subjects' field of study is individually significant at the 5% level (other controls including session dummies are individually insignificant at the 10% level). We again omit the wealth proxy from the final estimation for reasons explained in footnote 14.

real- stakes gift certificates elicitation mechanism, which is fully incentive-compatible and thus may give subjects who experience it a better idea of how to approach the subsequent hypothetical pricing of deferred payments.

To further investigate this issue, we compare WTP valuations in the deferred payments lottery treatments TP5 and TP6, which were preceded by the real-stakes gift certificates lottery treatments. The fifth test row shows that WTP valuations in TP5 are only slightly higher than in TP6, which matches the results for the preceding gift certificates treatments T5 and T6. We further compare WTP valuations in the deferred payments baseline treatments TP7 and TP8, which were preceded by the real-stakes gift certificates baseline treatments. As the sixth test row shows, WTP valuations in TP7 are only slightly higher than in TP8, which contrasts with the much larger difference in WTP valuations between the preceding gift certificates treatments T7 and T8. This last result suggests that the anchoring effect is not so strong and that the disciplining effect (or some alternative effect) may play a role.

1.4 Discussion and conclusion

We systematically observe that the internality axiom is *not* violated. Subjects' willingness to pay for equiprobable binary lotteries is significantly higher than other subjects' willingness to pay for the lotteries' worst outcomes, regardless of whether the WTP valuation is real-stakes or hypothetical and whether the outcomes are gift certificates or deferred payments.

We do not wish to draw any overreaching conclusions about the reality of IA violation (and hence the *uncertainty effect*) systematically documented by GLW. As noted in section 3, under between-subjects design, genuinely higher gift certificates or deferred payments valuation in a baseline treatment compared to a corresponding lottery treatment could in principle produce IA violation, even if GLW's uncertainty effect is nonexistent. Nevertheless, we observe that, with our implementation of the pricing tasks and in our subject pool, IA violation is (statistically) extremely unlikely.

The implementation in our main study rests on using a physical lottery format that arguably renders misinterpretation of the lottery structure highly unlikely, and on providing subjects with complete information about the goods they are to value. Either of these features could have contributed to our results differing dramatically from GLW's. The physical lottery format likely plays a role, given Keren and Willemsen's (2008) finding that verbally presenting the lottery in terms of a coin flip or a spinner wheel helps alleviate IA violation (though 13-31% of subjects still misunderstand the lottery structure). Providing complete information about the to-be-valued goods is unlikely to play a critical role, given that Sonsino (2008) uses this implementation feature but still observes some IA violation even in a within-subjects design.[17]

Our results could of course be specific to our subject pool – Prague students with varied academic background and other demographic characteristics. Nevertheless, conditioning on the observable characteristics when evaluating the lottery-baseline treatment effect leaves our results qualitatively unchanged. While it is difficult to judge whether our subject pool differs from that of GLW (University of Chicago students), we observe that the two subject pools mostly have comparable genuine (baseline-treatment) valuations of gift certificates and deferred payments. Naturally, replicating our implementation of GLW's tasks with other subject pools would add to our understanding of the external validity of our results.

We do not wish to dispute that GLW's lottery design may involve high cognitive demands or perception of uncertainty or both, which may trigger a valuation process incompatible with risky decision theories. After all, like GLW, we did not trace the valuation process that our subjects actually used. However, our results suggest that at least part of what GLW call the uncertainty effect might be triggered by their verbal lottery instructions obstructing the application of the IA. The relatively low lottery valuations might, for instance, stem from subjects' aversion to undetermined lottery probabilities, in the spirit of Sonsino's (2008) lottery aversion explanation (see above) and a common interpretation of Ellsberg-type paradoxes (see, e.g., Nau, 2007).

In psychology, verbally described tasks similar to GLW's are ubiquitously used in research on probabilistic or logical reasoning. Many psychologists have emphasized that a primary source of ambiguity in word problems stems from the use of ambiguous natural language terms and experimenters' violations of conversational norms (e.g., Adler, 1991; Hilton, 1995; Evans, 2002; Mellers *et al.*, 2001; Schwarz *et al.*, 1991). To interpret people's behavior as violating rules of logic, probability theory or axioms of rational choice theory, the experimenter needs to assume that the word problem represents nothing more than instantiations of normative rules. Yet in inferring the intended meaning of words or utterances, experimental subjects may arrive at interpretations that diverge from those of experimenters (e.g., Evans, 2002; Gigerenzer *et al.*, 1988; Hertwig and Gigerenzer, 1999; Politzer and Noveck, 1991). Thus experimenters, in equating their own and their subjects' understanding of the task, may erroneously interpret subjects' behavior as irrational (see also Harrison, 2005).

[17] Unfortunately, other design and implementation features of Sonsino's and our study differ too widely to draw any firm conclusions about the discrepancy in the results.

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Tables

Table 1.1: Design matrix of pricing tasks and treatments

	Hypothetical pricing of gift certificates	Real-stakes pricing of gift certificates	Hypothetical pricing of deferred payments
Lottery treatments	T1, T2, T3	T5, T6	TP3, TP5, TP6
Baseline treatments	T4	T7, T8	TP4, TP7, TP8

Table 1.2: Willingness to pay in the hypothetical pricing of gift certificates[a]

Column Treatment	(1) T1 (replication)	(2) T2 (reworded lottery)	(3) T3 (physical lottery)	(4) T4 (baseline)
Number of subjects	32	32	26	15
Mean WTP (standard deviation)	90.31 (32.89)	111.03 (25.87)	88.22 (34.00)	69.20 (18.09)
Median WTP	100.00	105.00	98.90	70.00
95% C.I. for WTP mean	(78.46, 102.17)	(101.70, 120.35)	(74.49, 101.95)	(59.18, 79.22)
95% binomial exact C.I. for WTP median	(60.00, 100.00)	(100.00, 130.00)	(80.00, 100.00)	(60.00, 84.00)
Null hypothesis tested	t-test	t-test with controls	Wilcoxon rank-sum test	Kolmogorov-Smirnov test
H_0: T1–T3=0	$t = 0.24$, $p = 0.8135$	$t = -0.07$, $p = 0.945$	$Z = 0.31$, $p = 0.7575$	$M = 0.11$, $p = 0.980$
H_0: T2–T1=0	$t = 2.80$, $p = 0.0068$	$t = 2.93$, $p = 0.004$	$Z = 2.56$, $p = 0.0105$	$M = 0.34$, $p = 0.045$
H_0: T2–T3=0	$t = 2.90$, $p = 0.0053$	$t = 2.21$, $p = 0.030$	$Z = 2.91$, $p = 0.0036$	$M = 0.38$, $p = 0.020$
H_0: T3–T4=0	$t = 2.34$, $p = 0.0247$	$t = 2.47$, $p = 0.015$	$Z = 2.25$, $p = 0.0243$	$M = 0.47$, $p = 0.018$

[a]The top part of the table displays summary statistics for various treatments while the bottom part displays tests for across-treatment differences (H_0 and the corresponding tests to the right of it). All WTP figures are percentages of $x = 500$CZK. All tests are two-sided. Test statistics are rounded to 2 decimal places and p-values are left at the precision reported by *Stata*. t-tests with demographic controls are based on heteroskedasticity-robust standard errors. Exact p-values are reported for the Kolmogorov-Smirnov test.

Table 1.3: Willingness to pay in the real-stakes pricing of gift certificates[a]

Column	(1)	(2)	(3)	(4)	(5)	(6)
Treatment(s)	T5 (lottery with symmetric MPL)	T6 (lottery with asymmetric MPL)	T7 (baseline with wide price range)	T8 (baseline with narrow price range)	T5,6 (pooled lottery treatments)	T7,8 (pooled baseline treatments)
Number of subjects	35	18	39	14	53	53
Mean WTP (st. dev.)	86.57 (36.21)	78.61 (24.96)	66.67 (23.32)	53.57 (24.37)	83.87 (32.80)	63.21 (24.08)
Median WTP	100.00	90.00	70.00	55.00	100.00	60.00
95% C.I. for WTP mean	(74.13, 99.01)	(66.20, 91.02)	(59.11, 74.23)	(39.50, 67.64)	(74.83, 92.91)	(56.57, 69.85)
95% binomial exact C.I. for WTP median	(80.00, 100.00)	(60.00, 100.00)	(50.00, 80.00)	(30.00, 80.00)	(75.00, 100.00)	(50.00, 70.00)
Null hypothesis tested	t-test	Wald test with controls	Wilcoxon rank-sum test	Kolmog.-Smirnov test		
H_0: T5–T6=0	$t = 0.83$, $p = 0.4080$	$t = 0.98$, $p = 0.329$	$Z = 1.40$, $p = 0.1625$	$M = 0.23$, $p = 0.485$		
H_0: T7–T8=0	$t = 1.78$, $p = 0.0808$	$t = 1.49$, $p = 0.137$	$Z = 1.67$, $p = 0.0950$	$M = 0.28$, $p = 0.322$		
H_0: T5,6–T7,8=0	$t = 3.70$, $p = 0.0004$	$t = 4.06$, $p = 0.000$	$Z = 3.90$, $p = 0.0001$	$M = 0.45$, $p = 0.000$		
H_0: T5,6–T7=0	$t = 2.94$, $p = 0.0042$	$t = 3.45$, $p = 0.001$	$Z = 3.18$, $p = 0.0015$	$M = 0.43$, $p = 0.001$		
H_0: T6–T7=0	$t = 1.76$, $p = 0.0843$	$t = 1.99$, $p = 0.047$	$Z = 1.87$, $p = 0.0613$	$M = 0.30$, $p = 0.165$		

[a] The top part of the table displays summary statistics for various treatments while the bottom part displays tests for across-treatment differences (H_0 and the corresponding tests to the right of it). All WTP figures are percentages of $x = 200$CZK. All tests are two-sided. Test statistics are rounded to 2 decimal places and p-values are left at the precision reported by *Stata*. Wald tests with demographic controls are based on heteroskedasticity-robust standard errors. Exact p-values are reported for the Kolmogorov-Smirnov test.

Table 1.4: Willingness to pay in the hypothetical pricing of deferred payments[a]

Column	(1)	(2)	(3)	(4)	(5)	(6)
Treatment(s)	TP3,5,6 (pooled lottery treatments)	TP4,7,8 (pooled baseline treatments)	TP5,6 (lotteries after real certificates)	TP7,8 (baselines after real certificates)	TP3 (lottery after hyp. certificates)	TP4 (baseline after hyp. certificates)
Number of subjects	80	70	54	55	26	15
Mean WTP (st. dev.)	78.97 (39.04)	48.25 (27.83)	75.65 (38.94)	44.70 (27.57)	85.87 (39.10)	61.27 (25.60)
Median WTP	80.00	50.00	80.00	40.00	85.00	70.00
95% C.I. for WTP mean	(70.28, 87.66)	(41.62, 54.89)	(65.02, 86.28)	(37.25, 52.16)	(70.07, 101.66)	(47.09, 75.44)
95% binomial exact C.I. for WTP median	(60.00, 99.00)	(40.00, 60.00)	(60.00, 99.00)	(36.00, 60.00)	(60.00, 120.00)	(50.00, 80.00)
Null hypothesis tested	*t*-test	Wald test with controls	Wilcoxon rank-sum test	Kolmog.-Smirnov test		
H_0: TP3,5,6–TP4,7,8=0	$t = 5.60$, $p = 0.0000$	$t = 6.65$, $p = 0.000$	$Z = 4.97$, $p = 0.0000$	$M = 0.43$, $p = 0.000$		
H_0: TP5,6–TP7,8=0	$t = 4.78$, $p = 0.0000$	$t = 5.37$, $p = 0.000$	$Z = 4.29$, $p = 0.0000$	$M = 0.43$, $p = 0.000$		
H_0: TP3–TP4=0	$t = 2.18$, $p = 0.0357$	$t = 3.17$, $p = 0.002$	$Z = 1.95$, $p = 0.0515$	$M = 0.43$, $p = 0.037$		
H_0: TP3,4–TP5,6,7,8=0	$t = 2.50$, $p = 0.0137$	$t = 2.30$, $p = 0.023$	$Z = 2.35$, $p = 0.0190$	$M = 0.21$, $p = 0.108$		
H_0: TP5–TP6=0	$t = 0.47$, $p = 0.6414$	$t = 0.73$, $p = 0.467$	$Z = 0.72$, $p = 0.4724$	$M = 0.22$, $p = 0.577$		
H_0: TP7–TP8=0	$t = 0.46$, $p = 0.6455$	$t = 0.30$, $p = 0.764$	$Z = 0.42$, $p = 0.6755$	$M = 0.14$, $p = 0.953$		

[a] The top part of the table displays summary statistics for various treatments while the bottom part displays tests for across-treatment differences (H_0 and the corresponding tests to the right of it). All WTP figures are percentages of x = 500CZK. All tests are two-sided. Test statistics are rounded to 2 decimal places and p-values are left at the precision reported by Stata. t-tests with demographic controls are based on heteroskedasticity-robust standard errors. Exact p-values are reported for the Kolmogorov-Smirnov test.

Appendix 1: Experimental instructions

A: Experimental instructions for the hypothetical pricing of gift certificates, lottery treatment T1

Instructions for the experiment

Imagine that we offer you a lottery ticket that gives you a 50 percent chance at a 500 CZK gift certificate for the Luxor Book Palace at Wenceslas Square, and a 50 percent chance at a 1,000 CZK gift certificate for the Luxor Book Palace. Whichever gift certificate you win is good for use for the next two weeks. What is the highest amount of money you would be willing to pay for this lottery ticket?

Your answer: _______________CZK

B: Experimental instructions for the hypothetical pricing of gift certificates, lottery treatment T2

Instructions for the experiment

Imagine that we offer you a gift certificate for the Luxor Book Palace at Wenceslas Square. There is a 50 percent chance that it is a certificate worth 500 CZK, and a 50 percent chance it is a certificate worth 1,000 CZK. Whether the gift certificate is worth 500 CZK or 1,000 CZK is determined by flipping a fair coin. Whichever gift certificate you receive will be good for use for the next two weeks. What is the highest amount of money you would be willing to pay for this gift certificate?

Your answer: ________________CZK

C: Experimental instructions for the hypothetical pricing of gift certificates, lottery treatment T3

Instructions for the experiment

In this experiment, we will ask you a hypothetical question. Regardless of your answer, you will earn 100 CZK for participating. Please read the instructions carefully and then write your answer at the end of the instructions. If you have any queries, please raise your hand. The experimenter will come to you and answer your query privately.

Your task is as follows. The bag on the table in front of the experimenter contains two gift certificates for purchase of goods at the Luxor Book Palace (Neoluxor) at Wenceslas square. **The two gift certificates are identical, except that one of them has a value of 500 CZK while the other has a value of 1000 CZK.** Each certificate is valid for the next three months and entitles the owner to purchase goods for up to the value of the certificate, for example, various kinds of books including CD and DVD formats, maps, stationery, and so on. The certificates can also be used to make purchases in the Neoluxor internet shop.

Now imagine you had an opportunity to draw **one** gift certificate from the bag. You would not be able to look into the bag while drawing, and since the two certificates in the bag are identical (except for their value), you would have equal (50-50) chances of drawing either the 500 CZK or the 1000 CZK certificate.

Our question is as follows: **What is the highest price (in CZK) you would be willing to pay for the opportunity of drawing one certificate from the bag?** Please write your answer here:

____

D: Experimental instructions for the hypothetical pricing of gift certificates, baseline treatment T4

Instructions for the experiment

In this experiment, we will ask you a hypothetical question. Regardless of your answer, you will earn 100 CZK for participating. Please read the instructions carefully and then write your answer at the end of the instructions. If you have any queries, please raise your hand. The experimenter will come to you and answer your query privately.

Your task is as follows. The experimenter will show you a **500 CZK gift certificate for purchase of goods at the Luxor Book Palace (Neoluxor) at Wenceslas square**. The gift certificate is valid for the next three months and entitles the owner to purchase goods for up to 500 CZK, for example, various kinds of books including CD and DVD formats, maps, stationery, and so on. The certificates can also be used to make purchases in the Neoluxor internet shop.

Our question is as follows: **What is the highest price (in CZK) you would be willing to pay for this certificate?** Please write your answer here:

____

E: Experimental instructions for the real-stakes pricing of gift certificates, lottery treatment T6 with the asymmetric MPL (lottery treatment T5 has identical instructions except for the symmetric MPL)

Instructions for the experiment

In this experiment, we give you **400 CZK for participating**. How much you earn in total will depend on your decisions. Please read the instructions carefully and then indicate your decisions on the attached ANSWER SHEET. If you have any queries, please raise your hand. The experimenter will come to you and answer your query privately.

Your task is as follows. The bag on the table in front of the experimenter contains two gift certificates for purchase of goods at the Luxor Book Palace (Neoluxor) at Wenceslas square. **The two gift certificates are identical, except that one of them has a value of 200 CZK while the other has a value of 400 CZK.** Each certificate is valid for the next three months and entitles the owner to purchase goods for up to the value of the certificate, for example, various kinds of books including CD and DVD formats, maps, stationery, and so on. The certificates can also be used to make purchases in the Neoluxor internet shop.

Now imagine you have an opportunity to draw **one** gift certificate from the bag. You would not be able to look into the bag while drawing, and since the two certificates in the bag are identical (except for their value), you would have equal (50-50) chances of drawing either the 200 CZK or the 400 CZK certificate.

Our question is as follows: **What is the highest price (in CZK) you are willing to pay for the opportunity of drawing one certificate from the bag?**

Please answer our question by filling out the attached ANSWER SHEET. In the ANSWER SHEET, we ask you to make 20 decisions. In each row, we ask you whether you are willing to pay the displayed price for the opportunity of drawing one certificate from the bag. In Row 1, for example, we ask you whether you are willing to pay 20 CZK for the opportunity of drawing one certificate from the bag. If you circle YES, you are saying that you are willing to pay 20 CZK, whereas if you circle NO, you are saying that you are not willing to pay 20 CZK. You will make similar decisions in all the remaining rows, except that the displayed price increases as you move down the ANSWER SHEET. Thus by circling YES or NO in each row, you will indicate the highest price you are willing to pay for the opportunity of drawing one certificate from the bag.

As we said at the beginning, we give you 400 CZK for participating. You can use this amount to pay for the opportunity of drawing one certificate from the bag. Note that 400 CZK would be enough to pay even the highest price displayed in the last row of the ANSWER SHEET.

After you will have made all 20 decisions, we will collect your ANSWER SHEET. At the end of this experimental session, the following procedure will follow:

1. The experimenter will invite you individually to another room and will find your ANSWER SHEET based on your anonymous ID number.

2. Then you will select randomly (without looking) one card from a box with 20 cards numbered 1 to 20. The number on the selected card will determine the row in your ANSWER SHEET that will be relevant for your earnings in this experiment. You of course do not know which row you will select, but you know that each row is equally likely to be selected. It is therefore important that you make a careful decision in **each** of the 20 rows.

3. Depending on your decision in the selected row, one of the following two situations, A or B, will happen:

A. If you circled YES in the selected row, you will pay us the price displayed in that row. We will simply subtract the price from the 400 CZK you get for participating. The remainder of the 400 CZK (after subtracting the price) will be paid to you by the experimenter in cash. Then you will have the opportunity of drawing one certificate from the bag, as described above. The experimenter will place a 200 CZK and a 400 CZK certificate in the bag and you will draw one of them (without looking into the bag). The certificate that you draw will be yours to keep.

B. If you circled NO in the selected row, you are saying that you are not willing to pay the price displayed in that row. Thus you will not pay us anything, but you will also not have the opportunity of drawing a certificate from the bag. The 400 CZK for participating will be paid to you by the experimenter in cash.

Now please fill out the attached ANSWER SHEET.

ANSWER SHEET

Please circle <u>either</u> YES <u>or</u> NO in each row. This will indicate whether you are willing to pay the displayed price for the opportunity of drawing one certificate from the bag. As explained above, the bag contains two gift certificates for purchase of goods at the Luxor Book Palace. The certificates are identical except that one of them has a value of 200 CZK while the other has a value of 400 CZK.

Row 1	I am willing to pay **20 CZK**	YES	NO
Row 2	I am willing to pay **40 CZK**	YES	NO
Row 3	I am willing to pay **50 CZK**	YES	NO
Row 4	I am willing to pay **60 CZK**	YES	NO
Row 5	I am willing to pay **80 CZK**	YES	NO
Row 6	I am willing to pay **90 CZK**	YES	NO
Row 7	I am willing to pay **100 CZK**	YES	NO
Row 8	I am willing to pay **120 CZK**	YES	NO
Row 9	I am willing to pay **140 CZK**	YES	NO
Row 10	I am willing to pay **150 CZK**	YES	NO
Row 11	I am willing to pay **160 CZK**	YES	NO
Row 12	I am willing to pay **180 CZK**	YES	NO
Row 13	I am willing to pay **200 CZK**	YES	NO
Row 14	I am willing to pay **220 CZK**	YES	NO
Row 15	I am willing to pay **240 CZK**	YES	NO
Row 16	I am willing to pay **260 CZK**	YES	NO
Row 17	I am willing to pay **280 CZK**	YES	NO
Row 18	I am willing to pay **320 CZK**	YES	NO
Row 19	I am willing to pay **360 CZK**	YES	NO
Row 20	I am willing to pay **400 CZK**	YES	NO

F: Experimental instructions for the real-stakes pricing of gift certificates, baseline treatment T7 with the wide MPL price range (baseline treatment T8 has identical instructions except for the narrow MPL price range, 10 decisions made, and 200 CZK for participating)

Instructions for the experiment

In this experiment, we give you **400 CZK for participating**. How much you earn in total will depend on your decisions. Please read the instructions carefully and then indicate your decisions on the attached ANSWER SHEET. If you have any queries, please raise your hand. The experimenter will come to you and answer your query privately.

Your task is as follows. The experimenter has at his disposal **200 CZK gift certificates** for the purchase of goods at the Luxor Book Palace (Neoluxor) at Wenceslas square. Each certificate is valid for the next three months and entitles the owner to purchase goods for up to 200CZK, for example, various kinds of books including CD and DVD formats, maps, stationery, and so on. The certificates can also be used to make purchases in the Neoluxor internet shop.

Our question is as follows: **What is the highest price (in CZK) you are willing to pay for the gift certificate?**

Please answer our question by filling out the attached ANSWER SHEET. In the ANSWER SHEET, we are asking you to make 20 decisions. In each row, we are asking you whether you are willing to pay the displayed price for the gift certificate. In Row 1, for example, we are asking you whether you are willing to pay 20 CZK for the gift certificate. If you circle YES, you are saying that you are willing to pay 20 CZK, whereas if you circle NO, you are saying that you are not willing to pay 20 CZK. You will make similar decisions in all the remaining rows, except that the displayed price increases as you move down the ANSWER SHEET. Thus by circling YES or NO in each row, you will indicate the highest price you are willing to pay for the gift certificate.

As we said at the beginning, we give you 400 CZK for participating. You can use this amount to pay for the gift certificate. Note that 400 CZK would be enough to pay even the highest price displayed in the last row of the ANSWER SHEET.

After you have made all 20 decisions, we will collect your ANSWER SHEET. At the end of this experimental session, the following procedure will follow:

1. The experimenter will invite you individually to another room and will find your ANSWER SHEET based on your anonymous ID number.

2. Then you will select randomly (without looking) one card from a box with 20 cards numbered 1 to 20. The number on the selected card will determine the row in your ANSWER SHEET that will be relevant for your earnings in this experiment. You of course do not know which row you will select, but you know that each row is equally likely to be selected. It is therefore important that you make a careful decision in **each** of the 20 rows.

3. Depending on your decision in the selected row, one of the following two situations, A or B, will happen:

A. If you circled YES in the selected row, you will pay us the price displayed in that row. We will simply subtract the price from the 400 CZK you get for participating. The remainder of the 400 CZK (after subtracting the price) will be paid to you by the experimenter in cash, and you will also receive the 200 CZK gift certificate.

B. If you circled NO in the selected row, you are saying that you are not willing to pay the price displayed in that row. Thus you will not pay us anything, but you will also not receive the gift certificate. The 400 CZK for participating will be paid to you by the experimenter in cash.

Now please fill out the attached ANSWER SHEET.

ANSWER SHEET

Please circle either YES or NO in each row. This will indicate whether you are willing to pay the displayed price for the gift certificate. As explained above, this is a 200 CZK gift certificate for purchase of goods at the Luxor Book Palace.

Row 1	I am willing to pay **20 CZK**	YES	NO
Row 2	I am willing to pay **40 CZK**	YES	NO
Row 3	I am willing to pay **60 CZK**	YES	NO
Row 4	I am willing to pay **80 CZK**	YES	NO
Row 5	I am willing to pay **100 CZK**	YES	NO
Row 6	I am willing to pay **120 CZK**	YES	NO
Row 7	I am willing to pay **140 CZK**	YES	NO
Row 8	I am willing to pay **160 CZK**	YES	NO
Row 9	I am willing to pay **180 CZK**	YES	NO
Row 10	I am willing to pay **200 CZK**	YES	NO
Row 11	I am willing to pay **220 CZK**	YES	NO
Row 12	I am willing to pay **240 CZK**	YES	NO
Row 13	I am willing to pay **260 CZK**	YES	NO
Row 14	I am willing to pay **280 CZK**	YES	NO
Row 15	I am willing to pay **300 CZK**	YES	NO
Row 16	I am willing to pay **320 CZK**	YES	NO
Row 17	I am willing to pay **340 CZK**	YES	NO
Row 18	I am willing to pay **360 CZK**	YES	NO
Row 19	I am willing to pay **380 CZK**	YES	NO
Row 20	I am willing to pay **400 CZK**	YES	NO

G: Experimental instructions for the hypothetical pricing of deferred payments, lottery treatments TP3, TP5 and TP6

Instructions for the experiment

In this part of today's experiment, we will ask you a hypothetical question which is in no way related to the part just finished. Your answer will not affect your earnings in today's experiment but is a precondition for completing the whole experiment. Please read the instructions carefully and then write your answer at the end of the instructions. If you have any queries, please raise your hand. The experimenter will come to you and answer your query privately.

Your task is as follows. The bag on the table in front of the experimenter contains two cheques which guarantee a payment in cash one year from now. **The two cheques are identical, except that one of them guarantees a payment of 500 CZK one year from now, while the other one guarantees a payment of 1000 CZK one year from now.** For both cheques, the payment is guaranteed by the Max-Planck-Gesellschaft – the research organization that finances this experiment.

Now imagine you had an opportunity to draw **one** cheque from the bag. You would not be able to look into the bag while drawing, and since the two cheques in the bag are identical (except for their value), you would have equal (50-50) chances of drawing either the 500 CZK or the 1000 CZK cheque.

Our question is as follows: **What is the highest price (in CZK) you would be willing to pay for the opportunity of drawing one cheque from the bag?** Please write your answer here:

____

H: Experimental instructions for the hypothetical pricing of deferred payments, baseline treatments TP4, TP7 and TP8

Instructions for the experiment

In this part of today's experiment, we will ask you a hypothetical question which is in no way related to the part just finished. Your answer will not affect your earnings in today's experiment but is a precondition for completing the whole experiment. Please read the instructions carefully and then write your answer at the end of the instructions. If you have any queries, please raise your hand. The experimenter will come to you and answer your query privately.

Your task is as follows. The experimenter will show you a **cheque which guarantees a payment of 500 CZK in cash one year from**. The payment is guaranteed by the Max- Planck-Gesellschaft – the research organization that finances this experiment.

Our question is as follows: **What is the highest price (in CZK) you would be willing to pay for the cheque?** Please write your answer here:

____

Chapter 2

Comparing Decisions Under Compound Risk and Ambiguity: the Importance of Cognitive Skills[1]

I investigate the relationship between attitudes towards ambiguity and the ability to reduce compound risks. The evidence from an experiment on adolescents shows that patterns identified in the previous literature are susceptible to experimental implementation and the characteristics of the subjects. Cognitive skills and the way lotteries are presented affect reduction of compound risks differently to ambiguity neutrality. My results suggest that theoretical studies which model ambiguity preferences by relaxing the assumption of compound risk reduction should be viewed with caution, and I add to the evidence against the use of compound lotteries to represent ambiguous environments in experiments.

JEL Classification: C91 D81

Keywords: Ambiguity, Cognitive ability, Reduction of compound lotteries

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2.1 Introduction

Ellsberg (1961) described situations under uncertainty when unknown probabilities of outcomes induced behavior violating Subjective Expected Utility theory (SEU; Savage, 1954). Since then many studies[2] have tried to explain and accommodate this inconsistency, generally termed *attitude towards ambiguity*. Several prominent theories (Segal, 1987, 1990; Halevy and Feltkamp, 2005; Seo, 2009) model attitudes towards ambiguity by introducing second-order probabilities and relaxing the usual assumption of reduction of compound lotteries (RoCL). Consider a decision maker who bets on an ambiguous lottery $(x, A; 0, A^C)$. She gets x in state A and 0 if the state is not A, but she is not aware of the probability distribution over the state space. In this situation, Segal (1987) and those who have since applied the same approach assume that the decision maker imagines betting on a two-stage lottery. During the first stage, Nature defines the probability distribution over states, for example $\hat{p}$, out of all possible distributions. During the second stage, the decision maker participates in the lottery $(x, \hat{p}; 0, 1 - \hat{p})$. The decision maker does not know the exact value of $\hat{p}$ but knows (forms subjective beliefs about) its distribution F. Under the RoCL assumption, the decision maker would be indifferent between betting in this imaginary two-stage (ambiguous) lottery and the corresponding simple risky lottery. Thus, non-reduction between first- and second-stage probabilities becomes the main source of ambiguity non-neutral behavior in Segal (1987) and in the strand of literature that follows his work.[3]

Is relating ambiguity to compound risk a valid behavioral assumption? Thus far, the results from the experimental literature answering this first question seem inconclusive. A number of studies have investigated the predictive power of two-stage theories by running tests of one or several models. Attanasi, Gollier, Montesano and Pace (2014), Conte and Hey (2013), and Qiu and Weitzel (2015) report supportive results for the smooth-ambiguity model by Klibanoff *et al.* (2005). Ahn, Choi, Gale and Kariv (2014), on the other hand, conclude that decisions in their experiment were better described by SEU theory.[4] In none of these papers, however, did researchers specifically test for differences in behavior under

[2] See reviews by Camerer and Weber (1992); Etner, Jeleva and Tallon (2012); Machina and Siniscalchi (2014); Trautmann and van de Kuilen (2014).

[3] The models by Ergin and Gul (2009), Klibanoff, Marinacci and Mukerji (2005), and Nau (2006) can be classified into the literature in which ambiguity is described by second-order probabilities. Formally, however, the models do not require violation of reduction of *objective* compound lotteries to generate attitudes to ambiguity, instead requiring violation of reduction of *subjective* second-order *acts* or *structures*. As Halevy (2007, p. 515) states: “lotteries and second-order acts (even when the second-order distribution is objective) are different mathematical concepts”. For a theoretical overview of ambiguity models with second-order probabilities see Halevy (2007, pp. 512–519) and Conte and Hey (2013, pp. 115–118).

[4] See also Armantier and Treich (2015) who introduce the notion of *complex risk* and show that two-stage theories cannot capture behavior towards *complex risk*.

compound risk and ambiguity. This has been done in a separate group of studies employing a within-subject experimental design. Whereas Halevy (2007) and Dean and Ortoleva (2015) found a close relationship between RoCL and attitudes to ambiguity, Bernasconi and Loomes (1992) and Abdellaoui, Klibanoff and Placido (2015) provided results showing a much weaker relationship between these two behavioral patterns. Interestingly, when Abdellaoui *et al.* (2015) juxtaposed two groups of subjects, engineering and non-engineering students, the quantitatively more advanced engineers exhibited even less association between ambiguity neutrality and RoCL. This leads to the second question: to what extent does the observed behavioral relationship between ambiguity and compound risk depend on cognitive abilities of the experimental participants as well as the ways the experiment is implemented.

In this study I have tested how behavior under ambiguity was related to behavior under compound risk and how cognitive skills contributed to this relationship. Previous literature employed university students with relatively homogenous characteristics and who likely had similar experiences with uncertain events. Moreover, they might have studied the notion of probability during their coursework, which could have led them to evaluate lotteries in ways that fit their existing knowledge. To avoid this possible bias, I used a subject sample of students with no formal education on probability. Middle-school adolescents participated in my experiment as a part of a longitudinal study on education outcomes in the Czech Republic. In a within-subject design, the subjects valued three lotteries (one risky, one compound, and one ambiguous). To ensure maximum transparency, all lotteries were implemented in a novel physical format and subjects were incentivized with monetary rewards. To address my first question as to whether relating ambiguity to compound risk is a valid behavioral assumption, I investigated the robustness of the relationship by varying the lottery presentation and prizes for the lotteries. Because each subject evaluated all three lotteries, the comparative context could have influenced the valuation outcomes (see, for example, Fox and Tversky, 1995; Chow and Sarin, 2001). Therefore, in my first treatment all the lotteries were presented at once and subjects were asked to evaluate the lotteries jointly. In the second treatment the subjects were shown lotteries in different orders and evaluated them separately. To answer the second question about the impact of cognitive abilities, I ran several cognitive and non-cognitive tests to track subjects' background and skills characteristics.

My findings reveal a significant number of subjects who do not comply with the patterns reported in Halevy (2007) or Abdellaoui *et al.* (2015). I observe discrepancy in behavior under ambiguity and compound risk, in part explained by whether lotteries were evaluated jointly or separately and in part by the background characteristics of the subject sample.

This work attempts to contribute to the broader discussion of whether and how to model ambiguity preferences. A recent collection of articles in the journal *Economics and Philosophy* (2009, vol. 25) reflects this controversy. Critics like Al-Najjar and Weinstein (2009) strongly advocate for considering ambiguity non-neutral preferences as a deviation from normative behavior which is not worth modeling, even for a descriptive purpose. At the same time, numerous empirical studies, both lab and field, provide evidence of the existence of ambiguity non-neutral behavior (for an overview, see Camerer and Weber, 1992; Trautmann and van de Kuilen, 2014). The results of many of these studies cannot be explained only by aversion to possible manipulation of probabilities against the subject. However, it is not clear whether non-neutral attitudes toward ambiguity are related to manifestation of some personality characteristics or to a lack of sophistication in the source of ambiguity. In my experiment, I observe a correlation between ambiguity neutral behavior and cognitive skills under certain experimental conditions, therefore adding to the explanation that sophistication might play some role. I discuss related papers in the literature review section.

In terms of experimental methodology, it is important to understand the relationship between RoCL and ambiguity neutrality, and whether it works through background characteristics, because a lottery with second-order probabilities could be a convenient way to design ambiguity in the lab (Maafi, 2011; Di Mauro and Maffioletti, 2004). If background characteristics play a different role for these behaviors, or if there is a weak relationship between RoCL and ambiguity neutrality, then perhaps researchers should try to find other ways to represent ambiguity (see, for example, Abdellaoui, Baillon, Placido and Wakker, 2011) and control for the background characteristics that appear most relevant.

2.2 Related experimental literature

To my knowledge, Bernasconi and Loomes (1992) was the first paper to test the equivalence between ambiguous lotteries and two-stage (compound) lotteries. The authors ran a compound risk version of Ellsberg's three urn experiment and observed a lower proportion of subjects behaving as expected in experiments with ambiguous urns. Bernasconi and Loomes (1992) did not, however, compare the decisions on an individual level. It is unclear how subjects with comparable characteristics would behave under the same experimental conditions but with ambiguous urns. The experiment considers only hypothetical answers, which may induce additional biases in valuation (Camerer and Hogarth, 1999; Ortmann and Hertwig, 2006).

More recent studies by Halevy (2007), Abdellaoui *et al.* (2015), and Dean and Ortoleva (2015) investigated both behavior under ambiguity and compound risk on an individual level, and therefore are the most relevant to my study. All three papers employed a similar within-subject design (see Table 2.1).

Note that all three studies used student subjects, and all, except Halevy's (2007) Robustness Round, were computerized, which arguably might create subjects' suspicion towards ambiguous lotteries. In Halevy's (2007) experiment, all lotteries were presented at once, and subjects were then asked to evaluate them. This differs from the procedure in Dean and Ortoleva (2015) and Abdellaoui *et al.* (2015), in which subjects evaluated lotteries one by one, so they did not know in advance either the types or the order of the compound lotteries. On the one hand, Halevy's (2007) setup might lead to anchoring and interval evaluation, when subjects choose the most preferable and the least preferable lottery and distribute their valuation of other lotteries in-between (see the comparative ignorance hypothesis by Fox and Tversky, 1995). On the other hand, while the joint evaluation presumably induces subjects to concentrate on the difference between the lotteries, it allows them to notice the elements that are identical, such as lottery prize and expected probability levels. This may cause some subjects to evaluate lotteries identically, which could be true especially for those acquainted with the notion of probability or with some natural understanding of it. Sequential evaluation of lotteries, however, is more vulnerable to mistakes by inattentive subjects. For example, when evaluating the final lottery in a sequence, a subject may erroneously think it has a different maximum prize than the first lottery and assign it a different value based on this belief.

Halevy (2007) showed that subjects who reduced compound lotteries were predominantly ambiguity neutral. Further, conditional on ambiguity neutrality, most of the subjects were able to reduce compound lotteries. Dean and Ortoleva (2015) report similar results to Halevy (2007), but they ran approximately 50 different tasks, estimating various behaviors under uncertainty, of which only two were played for real payment. The authors themselves mentioned the possible impact of insufficient incentives. Abdellaoui *et al.* (2015) attempted to replicate Halevy's (2007) experiment but distinguished between two kinds of subjects: engineering and non-engineering students. Their results revealed a weaker relationship between RoCL and ambiguity neutrality than those of Halevy (2007); this was especially apparent for engineers. None of these three papers, however, explicitly tried to measure the impact of cognitive skills. Abdellaoui *et al.* (2015) conjectured that the differences between their results and Halevy's could be susceptible to the differences in subjects' background characteristics and their quantitative skills. Since their subject sample consisted only of university students, there was likely a low variation in cognitive skills. Likewise, Dean and Ortoleva (2015) ran additional personality tests but point out the homogeneity of their student subject sample as a possible explanation for their insignificant results.

The idea to connect individual characteristics to preferences stems from psychology and has recently been studied by behavioral economists. In Table 2.2, I compare the most recent relevant papers, highlight the important experimental design features and list the results, specifically whether cognitive or non-cognitive skills are found to be related to risk[5] or ambiguity preferences.

The evidence for a link between skills and attitudes to risk and ambiguity is mixed. A group of papers has shown that risk preferences are related to cognitive skills. People with better results on cognitive tests tend to be less risk averse (Burks *et al.*, 2009; Dohmen *et al.*, 2010; Benjamin *et al.*, 2013). On the contrary, other papers do not support this observation and report a non-significant relationship (Borghans *et al.*, 2009; Booth and Nolen, 2012; Eckel *et al.*, 2012; Sutter *et al.*, 2013; Taylor, 2013). Whereas Borghans *et al.* (2009) and Dohmen *et al.* (2010) did not find any significant correlation between ambiguity aversion and cognitive skills, Rustichini *et al.* (2012), Sutter *et al.* (2013) and Dean and Ortoleva (2015) found correlation between some non-cognitive skills and attitudes to ambiguity.

There seems no clear relationship between the magnitude of ambiguity aversion and background characteristics. Yet, there is growing evidence that individual confidence in dealing with probabilities or observation of (confident) others may be related to ambiguity neutral behavior. Psychological literature shows how individuals with different cognitive abilities can have different ways of dealing with situations involving probabilistic choice tasks. While a majority may choose non-normative heuristics that do not require high cognitive capacity, the more intelligent minority may opt for normative decisions, which might be more complex but more efficient in the end (Hogarth, 1975; West and Stanovich, 2003). Chew, Ratchford and Sagi (2013b) in their recent study divided subjects who correctly comprehended ambiguity tasks into probabilistically-minded (those who were able to attach probabilities for the ambiguous event) and ambiguity-minded (those who were not able to specify unknown probabilities). The latter group represented the vast majority and exhibited significantly higher ambiguity-averse attitudes than the former group. Thus, people who can more easily quantify ambiguity are perhaps more ambiguity neutral.

[5] I review papers on risk attitudes because compound risk in general represents risky situations but with a more complex decision tree. Thus, similar background characteristics may impact the decision making process when dealing with compound lotteries.

2.3 Experimental design

2.3.1 Treatments

Since the way lotteries are presented can interact with cognitive skills and thus impact the evaluation process differently, I randomly assigned all subjects to two treatments, *Joint evaluation* and *Separate evaluation* (see Table 2.3).

Whereas in *Joint evaluation* all lotteries were presented to the subjects at once, in the treatment *Separate evaluation*, the lotteries were presented sequentially, without subjects knowing what lotteries they would evaluate later in the sequence. Importantly, to make the sequential presentation even more salient and distinguishable from the simultaneous presentation, I alternated every lottery evaluation with a short unrelated task. The objective was to test sequential evaluation in a within-subject design with minimum possible anchoring on the previously presented lotteries. However, I acknowledge that this treatment might differ from a sequential treatment without any alternating tasks.[6] Additionally, I control for the lottery prize size, non-cognitive skills, and background characteristics.

2.3.2 Measures of preferences

To measure risk, ambiguity and compound risk preferences I elicited certainty equivalents for corresponding lotteries. A common feature of many papers employing non-student subject samples is to use the Multiple Pricing List elicitation procedure (MPL; for examples and discussion see Andersen *et al.*, 2006, 2007; Holt and Laury, 2002) rather than the Becker-DeGroot-Marschak method (BDM, Becker *et al.*, 1964). For subjects from the general public, the MPL procedure appears more transparent and easier to explain than the BDM method.

To evaluate the lotteries, each participant was given a simplified version of MPL with ten rows, where one column offered a bet on a lottery and the other column offered some sure amount of money, sorted in ascending order (see an example MPL in Appendix 2.1). Whenever a subject switched from one column to another, I calculated a midpoint between switching values as a certainty equivalent for the corresponding lottery. In the explanation given prior to the subjects filling in the MPL, they were shown a bag representing the corresponding lottery and all relevant details were explained. Afterwards, they were asked

[6] I tried to eliminate any undesirable noise associated with the unrelated tasks. Before distributing every answer form, the experimenters explicitly re-iterated the instructions for the specific task to follow. Additionally, I control for whether the difference of order between the tasks in my experiment and the unrelated tasks had any impact on a) evaluations of all lotteries and b) differences between the evaluations. I do not find any significant differences. The results are reported in Appendix 2.2B.

to fill in the answer form. The experiment assistants did not explicitly ask subjects to make only one switch, because that could have interfered with the subject's evaluation process. Since switching between columns was not restricted, I found 22.7% of subjects (57 of 233) made inconsistent MPL choices. Their answer forms contained at least two jumps between columns on at least one MPL task. Importantly, for the whole subject sample, I observe a negative relationship between inconsistent choices in MPL tasks and cognitive abilities (reminiscent of the findings in Moon and Martin, 1996). Thus, I can partially attribute inconsistent choices to possible misunderstanding of the task or lower levels of attention.[7]

To minimize any fears of manipulation on the part of the experimenters, all tasks were demonstrated using identical containers with screw caps and colors hidden under the caps.[8] Accordingly, instead of the usual colored balls or chips in opaque bags, I used transparent bags with identical containers which were different only when opened. Henceforth, I will use "red containers" and "blue containers" to refer to containers with red and blue hidden colors, respectively.

In this way, the risky lottery was presented as a bag with two red and two blue containers. The compound lottery was presented as two bags, one with one blue and three red and the other with one red and three blue containers. The containers were opened, then closed, and inserted into the bags in front of the subjects. To create an ambiguous lottery, the experimenter took four red and four blue containers, opened them and showed them to participants. The experimenter then closed each container, put them into one bag and asked four different subjects to draw one container each so that only four were left in the bag. As a result, neither experimenter nor subjects could know the actual distribution of colors. Then the subjects were asked aloud what kind of distribution they expected to see in the bag. Subjects responded orally that any combination was possible and the experimenters confirmed aloud that indeed any combination of blue and red containers could be there.[9] All bags were assembled in front of the subjects before they were given the MPL forms to fill in and before how to fill in the forms was explained. Thus, subjects were asked to evaluate

[7] See Bettinger and Slonim (2007) for a discussion of how demographic characteristics influence switching behavior.

[8] See Appendix Figure 2.5.1 for a picture of containers used in the experiment.

[9] The representation of ambiguous lotteries is a widely discussed design feature of experiments measuring ambiguity preferences (Al-Najjar and Weinstein, 2009; Hey *et al.*, 2010). Since my subjects were adolescents, I aimed to make the task as clear and transparent as possible by assembling the ambiguous bag in front of them. Hence subjects could have perceived the ambiguous bag similar to a compound-risk bag. This could weaken the interpretation of the main results had I discovered a strong relationship between behavior under compound risk and ambiguity, which I did not. Still, a possible design extension would be to add a treatment where no (or less) information is given on how the bags are assembled. This could, however, trigger various additional layers of uncertainty aversion possibly related not to the lotteries but to the experiment itself.

bags only after these bags were ready in front of them.

2.3.3 Measures of cognitive and non-cognitive skills

In the experimental literature described in Table 2.2, researchers mainly use IQ and school test results to measure cognitive skills, and variants of the Big5 test to measure non-cognitive skills. For my experiment I ran two tests on cognitive skills, an Arithmetic Test (AT) and a Working Memory Test (WMT), and a test on personality characteristics (PCT).[10] During the AT, subjects were asked to solve simple arithmetic problems grouped by four (one per each arithmetic sign) in a limited time. The points were assigned only for correctly solved groups, in order to induce subjects to work through every problem and thereby to ensure compatibility of results. While AT tests for abilities to make calculations with accuracy and speed, WMT tests for the ability to keep information accessible in the memory. Since every subject had to evaluate three different lotteries, working memory capacity could be an important determinant. The WMT used in the experiment was a computerized version of a working memory test (operation span) widely used in psychological literature (Turner and Engle, 1989; Engle *et al.*, 1999). Subjects were shown different letters on a screen, one by one, and in-between they had to solve simple arithmetic problems. Afterwards, subjects had to report the letters in the order they were shown. For my final analysis I exclude 41 subjects who made too many mistakes in the arithmetic calculations in WMT (above a certain threshold usually used in the literature).[11]

2.4 Procedures

The investigation was a part of the Czech Longitudinal Study in Education (CLoSE). Schools were chosen from the pool of schools participating in CLoSE. In total eleven classes of 6th graders agreed to participate in our study.[12] On average, a class consisted of 21 subjects. To unify conditions for all subjects, the experiment was performed during normal school time

[10]See Rydval (2011, pp. 11–15) for an overview of literature on cognitive and non-cognitive measurements.

[11]In the Appendix 2.2E, I replicate the main results for a less strict accuracy rate in WMT. In general, the accuracy rate is used to avoid situations when people do not concentrate much on math problems, thus mostly using short-memory to remember the sequence of letters. Note that subjects know of the math accuracy requirement and their accuracy rate is shown on the screen during the WMT test. Since my subjects were adolescents, the arithmetic problems should have required more attention and processing resources compared to university students who typically participate in WMT experiments. Thus, probably the undesirable diversion of cognitive resources would be less frequent, which could justify a somewhat less strict accuracy rate cut-off when analyzing the data.

[12]Five classes were from Prague and six were from three other regions. The 6th grade is the first year of secondary school. On average, the subjects were 12 years old.

and in CERGE-EI classrooms.

Each session consisted of only one class, thus in total we ran 11 separate sessions during November 2013 – June 2014. In the morning, participants and their class teachers arrived at CERGE-EI and remained for the duration of the experimental session. All sessions were conducted in Czech by native speakers of the language. On arrival every subject received a unique number and used only this number for identification during the entire session. The experimenters explicitly explained that all the data was anonymous and would not be used to track any individual answers. Further, experimenters randomly divided all participants into two roughly equal groups. While one group worked on experimental tasks (Lotteries), the other group performed skills tests (Tests) in a different room; when finished, the groups changed to the other task set (see Table 2.3). Except for the WMT test, all tasks were administered in a pen-and-paper format. On average each session lasted 2.5 hours with one break; all participants received a small snack between the Lotteries and Tests.

All tasks were incentivized with real money. Everyone was paid a participation fee (100 CZK $\approx$ \$5 at the time of the experiment), which also served as an incentive for completing the Tests. Additionally, one random task from Lotteries (one out of three lotteries for everyone) was selected and paid out for real at the end of the experiment. Specifically, a randomly chosen subject randomly drew the lottery to be paid out for all participants within a group. Then every subject randomly drew a line in his or her MPL to be played for real – a container from a bag with ten containers numbered one to ten. For every subject in a private room, the experimenter checked the choice (lottery or money) in the answer form and in the case of "prefer lottery", the subject first bet the color and then drew a container from the corresponding bag, in the case of "prefer money", he or she was given money. The actual money was distributed by class teachers after the experimental session.

2.5 Results

In total 233 subjects participated in the experiment. Each individual observation consists of data from three lotteries, three tests (two cognitive skills tests and one test on personality traits) and background characteristics. As described in the sections *Measures of preferences* and *Measures of cognitive and non-cognitive skills*, I dropped several subjects with inconsistent answers in MPL tasks and with too low accuracy rates in the working memory test, therefore the remaining analysis is based on the data from 135 subjects.

2.5.1 Lotteries and tests statistics

To relate my results to other findings in the literature, I analyze the valuations of lotteries and the corresponding attitudes to risk, compound risk, and ambiguity. Following Borghans *et al.* (2009) and Sutter *et al.* (2013), I define attitudes to risk (risk aversion) as a weighted certainty equivalent of a risky lottery:

$$RA = 1 - L_R/prize$$

where L_R equals a certainty equivalent of risky lottery and $prize$ is the maximum prize in the corresponding lottery. Further, I define attitudes to compound risk (ambiguity) relative to risk as a normalized difference between risky and compound (ambiguous) lottery valuations:

$$CA = (L_R - L_C)/(L_R + L_C), \quad AA = (L_R - L_A)/(L_R + L_A)$$

where L_C (L_A) equals a certainty equivalent of compound (ambiguous) lottery. The larger the value of the difference (CA or AA) the more averse the subject is; whenever the difference is zero, $L_R = L_C$ ($L_R = L_A$), the subject is indifferent to compound risk (ambiguity neutral). On average, my subject sample is risk, ambiguity, and compound risk averse,[13] which is in line with findings in other studies described in Table 2.2.

Although there is evidence of subjects being more risk averse (mean RA_{200} > mean RA_{100}) in the tasks with a higher lottery prize (as in Holt and Laury, 2002), there is no significant difference in attitudes towards ambiguity (AA) or RoCL (CA) depending on the prize amount. In the Appendix 2.2, I perform several robustness checks and determine that neither order of tasks, nor the mid-list problem[14] have any significant impact on certainty equivalents for the lotteries.

Similar to Halevy (2007), valuations of risk, compound risk and ambiguity lotteries are positively correlated (see Table 2.4). The reason could be that subjects found it difficult to evaluate lotteries separately and resorted to making comparisons among the lotteries. The ambiguous lottery was presumably the most complex of all lotteries to evaluate and the risky lottery was presumably the easiest. Comparing correlations between treatments and prizes, we notice that the pair L_R and L_C has lower correlations for the higher prize, but there are no differences across treatments. One reason might be that the increase in lottery size led to

[13] For attitudes to risk, I test H_0: RA=0.5; t-test rejects H_0 at 5% level, mean RA = 0.52, p-value = 0.0351. For attitudes to ambiguity, I test H_0: AA=0; Wilcoxon signed-ranks test (due to non-normality of sample) rejects H_0 at 5% level, mean AA = 0.062, p-value = 0.0012. For attitudes to compound risk, I test H_0: CA=0; Wilcoxon signed-ranks test (due to non-normality of sample) rejects H_0 at 5% level, mean CA = 0.039, p-value = 0.0139.

[14] Tendency to provide focal values; for MPLs, two middle rows can be a focal point for switching.

more independent valuations for these less complex lotteries. For the pair-wise correlations including an ambiguous lottery, the prize has effect only in *Joint Evaluation* (L_C and L_A have different correlations in *Joint Evaluation*; L_R and L_A have lower correlation for the higher-prize lottery in *Joint Evaluation*, although significant only at the 8% level). Since the joint context allows easier comparison between lotteries, subjects may pay more attention to the size of the lottery, and thus exhibit less association between lotteries. When we look at between-treatment comparison, we see that the correlations between L_R and L_A are lower for *Separate Evaluation*. Possibly, *Joint Evaluation* induces subjects to relate the ambiguous lottery (the most complex) to the least complex risky lottery. Thus, the subjects use a similar model of evaluation. At the same time, in *Separate Evaluation* the correlation between L_C and L_A is higher compared to *Joint Evalution*. Both compound and ambiguous lotteries are harder to evaluate, which might lead to subjects using similar models of evaluation, especially in *Separate Evaluation*. When presented with both lotteries at once, they might focus on the difference between them, thus decreasing the level of association in valuations.

The correlations between the main test measures are presented in Table 2.5. Cognitive skills measurements are positively correlated. Math Anxiety, as expected, is negatively correlated with AT scores – the less subjects are anxious about mathematical tasks, the higher results they achieve on the test. On contrary, WMT results are not correlated with Math Anxiety, in line with the assumption that the AT and the WMT capture different attributes of cognitive skills. Other psychological characteristics and background variables are not significantly correlated with cognitive test scores.

2.5.2 Relationship between ambiguity neutrality and RoCL

Table 2.6 compares data from existing literature and from the current study. For my subject sample we can see a significant relationship between ambiguity neutral behavior and RoCL.

Though this pattern supports the findings from the previous literature, the results of my experiment differ in two important ways. First, my subject sample includes a considerably higher proportion of subjects both reducing compound lotteries and being ambiguity neutral, 41% of subjects assigned the same values to the compound lottery as to the risky lottery and 44% of subjects were ambiguity neutral (as compared to 16%/20% in Halevy (2007), 15%/26% in Abdellaoui *et al.* (2015), and 20%/19% in Dean and Ortoleva (2015)). Similarly, Chew *et al.* (2013b) observed a low level of ambiguity aversion when considering their whole subject sample. However, for the group that passed a comprehension task above a certain measure, the level of ambiguity aversion was higher. Chew *et al.* conjecture that complexity might drive people to make inattentive choices, thus they exhibit lower levels of ambiguity aversion. It is possible that in my experiment, some subjects were less attentive and therefore set identical values for all three urns. If so, we should expect subjects with lower cognitive skills to be in this group; however, the results discussed in the next subsection reveal that this is not the case.

As to the second difference in my findings, 17 of 22 subjects in Halevy (2007), who set the same values for all lotteries, chose the focal (mid-list) values. For example, for the binary lottery with probabilities 1/2 and outcomes 0 and 2, subjects chose 1 for all lotteries. We can only guess about the reasoning applied, but if indeed the choice was driven by mid-list value, the data pattern reported by Halevy becomes less evident. In my paper I observe neither a mid-list problem for any lottery valuations, nor any focal point in data for this group (see Appendix 2.2D for more discussion).

Importantly, Halevy (2007) and Abdellaoui *et al.* (2015) use several compound lotteries, and thus their condition for reduction of compound lotteries was stricter. To comply with the RoCL condition, the subjects in their experiments had to provide the same value for a risky lottery and *all* compound lotteries. In contrast, Dean and Ortoleva (2015) and my study use only one compound lottery. I analyzed how the results from Halevy (2007) would change if the condition of compound lottery reduction is based on only one lottery. Given ambiguity neutral behavior, approximately the same proportion of subjects would be able to reduce a compound lottery, regardless of which compound lottery is analyzed. However, for those who would follow RoCL, the proportion of ambiguity neutral and ambiguity non-neutral subjects seems to depend on the number and type of compound lotteries under consideration. For example, when using a compound lottery that is easier to comprehend (like the degenerate lottery V4 in Halevy, 2007), we would observe more subjects reducing compound risk but remaining ambiguity non-neutral. In general it is not clear what impact the evaluation of additional compound lotteries would have on the findings in experiments *à la* Halevy (2007).[15] Thus, this feature of experimental design might have significant impact on the interpretation.

[15] I report the data used for the analysis in the Appendix 2.3.

2.5.3 The impact of cognitive skills and evaluation treatments

To facilitate comparison to the previous studies, I have divided the entire subject sample into two groups by WMT score and AT score results; see Table 2.7. Additionally, I used data from Halevy (2007) and created a similar division for his subject sample. In the group *Major 0* I have included all subjects majoring in humanities or social sciences, whereas group *Major 1* includes subjects with majors requiring more exposure to mathematics. 27 subjects did not report their major. 22 of them were in the group with "*Ambiguity Neutrality = no*" and "$RoCL = no$". We can observe for my data, similar to Halevy, that being on a diagonal in the table (stronger relationship between RoCL and ambiguity neutrality) becomes more salient for subjects with better mathematical skills, namely subjects with higher AT scores in my sample and subjects in group *Major 1* in Halevy's sample. There is no such pattern for WMT scores or for the data from Abdellaoui *et al.* (2015).

To investigate the impact of cognitive skills, I estimate probit models for RoCL and for ambiguity neutrality as dependent variables. I include both treatments (lottery prize and evaluation type), their interaction, and cognitive measurements as main explanatory variables, and background characteristics and non-cognitive skills as additional controls. Since the way lotteries were presented to the subjects could directly influence the evaluation process, which is affected by cognitive abilities, I also include interaction terms between AT and WMT scores and evaluation treatment.[16] Tables 2.10-2.11 present the estimates. In Table 2.10, columns (1) and (4) show the effect of treatment and cognitive skills without controls, the results in columns (2) and (5) include gender and whether the school attended by the subject was located in Prague, a crude proxy for socioeconomic status,[17] and the results in columns (3) and (6) additionally include non-cognitive skills as controls. In Table 2.11, I demonstrate the impact of different levels of cognitive skills on marginal effects at different percentiles.

First, for subjects with higher WMT scores who evaluated lotteries jointly (as opposed to separately) there was a significantly higher probability of them providing the same values for risky and compound lotteries. The effect is not significant for subjects with low scores in WMT (see Table 2.11 and Figure 2.1). Psychology literature connects better working memory capacity to superior decision making under risk and acting more in line with expected-value calculations (Cokely and Kelley, 2009). Perhaps, given the young age of my

[16] Tables 2.8 and 2.9 show that the random assignment to both prize and evaluation treatments ensured that all differences in means for control variables are insignificant at 5% level.

[17] The average wage in Prague is approximately 30% higher than in other regions of the Czech Republic, including those where schools were sampled from.

subjects, the possibility to observe all lotteries at once was an additional necessary condition to relate a compound lottery to a risky lottery. Moving on to preferences towards ambiguity, note that the impact of *Joint Evaluation* is much weaker, though in the same direction, for ambiguity neutrality and is significant only for the 25th percentile of WMT scores (at a 10% level).

Second, there is a significant relationship between RoCL and AT scores, whereas there is no such relationship between ambiguity neutral behavior and AT scores. Halevy (2007) reports that in his experiment those who had more training in mathematics set equal (focal) values for all lotteries. My subject sample has no training in advanced mathematics and I did not ask subjects to explain their choices; therefore I can only conjecture about their underlying reasoning. Still, it seems that the differences in Halevy's (2007) design and that of Abdellaoui *et al.* (2015) could lead to differences in results: presenting all lotteries at once in Halevy's experiment might make it more likely for math-inclined subjects to value them identically.

For both RoCL and ambiguity-neutral behavior I observe a significant relationship with gender: being female leads to a lower probability of a subject reducing compound lotteries or being ambiguity-neutral. Contrary to many studies relating ambiguity to personality traits, I do not observe any significant relationship between measures of non-cognitive skills and ambiguity neutrality or ability to reduce compound lotteries.

2.6 Conclusion

The main finding of my experiment is that, although the relationship between attitudes to ambiguity and compound risk may be significant in some implementations, it is highly susceptible to experimental design and background characteristics of the subject sample.

First, the estimation results demonstrate that those performing better on the arithmetic test are more likely to reduce compound lotteries; however, this does not hold for working memory measurement. Importantly, cognitive tests results are not significant when evaluating behavior under ambiguity. These findings do not support the observation of Abdellaoui *et al.* (2015) that more quantitatively sophisticated subjects perform less in line with Halevy's (2007) results.

Second, when considering the impact of how the lotteries were presented to the subjects (all at once or one at a time), I find evidence that joint evaluation of lotteries had a significant impact on the probability that subjects with high WMT scores would reduce compound lotteries. The effect for ambiguity neutrality was weaker and insignificant on average. Therefore, differences in the designs of experiments by Halevy (2007) and Abdellaoui *et al.* (2015) could partially explain the differences in their results.

This experiment suggests that behavior under compound risk and ambiguity might be driven by different background characteristics. Therefore models or experimental designs that equalize these two notions may provide misleading results.

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Table 2.1: Literature studying relationship between RoCL and attitudes to ambiguity at a within-subject level[a]

	H 2007, ME	H 2007, RR	A 2015, ST2	A 2015, ST3	D&O 2015
Sample size	104	38	115	64	190
Sample structure	All students who signed-up for slots	Students, proportional sampling within cohorts	Students, 75 engineering, 40 all other fields	Students, 51 engineering, 13 quantitative economics	All students who signed-up for slots
Implementation	Computerized	Physical	Computerized	Computerized (subject verbalized, experimenter entered choices)	Computerized
Framing	Boxes and balls	Boxes and chips	Balls in urns drawn on the screen		Bags and chips
Elicitation	BDM	BDM	Iterative choice list procedure[b]		MPLs
Probability levels	1/2 for all	1/2 for all	1/2 for all	1/12, 1/2, 11/12	1/2 for all
Order of tasks[c]	4 tasks in order: R, A, C1, C2. Lotteries presented at once	4 tasks, different orders: R, A, C1, C2. Lotteries presented at once	5 tasks, different orders: R, A, C1, var of C1, C2	32 tasks in order: R(4), A(7), C(21)	9 tasks (3 per each prize) in order: R(3), A(3), C2(3)
Lottery prize	$2	$20	€50	€50	$6, $8, $10
Payments	Every lottery was played and paid		One random task was played and paid		2 lotteries of 50 were played for real

[a]H 2007, ME and RR = Halevy (2007), Main Experiment and Robustness Round with higher stakes; D&O 2015 = Dean and Ortoleva (2015); A 2015, ST2 and ST3 = Abdellaoui *et al.* (2015), Study 2 and Study 3

[b]Computerized version of MPL, see Abdellaoui *et al.* (2011);

[c]R = Risky lottery; A = Ambiguous lottery, C1 = Compound lottery, 2 colors, uniform distribution; C2 = Compound lottery, either all of one color or all of the other color.

Table 2.2: Literature studying relationship between personality traits and attitudes to risk/ambiguity

Study	Sample size[a] / Age	Measured attitudes[b]	Framing / Elicitation[e]	Tests Cogn[c] / Tests NCogn[f]	Results[d]: majority RA/AA[g]	Results[d]: related to Cogn/NCogn
Becker *et al.* 2012	ST 489 students	Risk	Lottery MPLs	– versBig5, LoC	–/–	–/mixed
	R 902 > 17 y.o.	Risk	Lottery MPLs	– versBig5	–/–	–/mixed
	R 14,243 SOEP	Risk	Self-assessment	– versBig5, LoC	–/–	–/yes
Benjamin *et al.* 2013	R 92 + R 81 high school seniors	Risk	Lottery MPLs	Standardized test (similar to SAT) –	–/–	yes/–
Booth and Katic 2013	R 1586 18 y.o.	Risk Hyp	Lottery, Self-assessment	Ranking for university entrance –	yes/–	NS/–
Booth and Nolen 2012	R 260 ≈ 15 y.o.	Risk	Lottery Choice, Self-assessment	Mazes –	yes/–	NS/–
Borghans *et al.* 2009	R 347 15-16 y.o.	Risk, Ambiguity	Urns BDM	RavenIQ Big5, Ambition, SelfCtrl, FlexTh	yes/yes	R: NS/mixed A: NS/NS
Burks *et al.* 2009	R 1066 trainee truckers	Risk	Lottery MPLs	RavenIQ, Hit15, NumETS MPQ	–/–	yes/mixed
Dean and Ortoleva 2015	ST 190 students	Risk, Ambiguity, Cpd risk	Lottery, bags MPLs	RavenIQ, SAT Overconfidence, Overplacement	yes/yes	NS/mixed
Dohmen *et al.* 2010	R 902 > 17 y.o.	Risk	Lottery MPLs	similar to WAIS Big5	yes/–	yes/NS
Eckel *et al.* 2012	R 490 9th-11th graders	Risk	Visual, choice among circles	NumETS Psychometric scales	yes/–	NS/mixed
Rustichini *et al.* 2012	R 1065 trainee truckers	Risk, Ambiguity	Lottery MPLs	RavenIQ, Hit15, NumETS MPQ	–	R: yes/mixed A: NS/mixed
Sutter *et al.* 2013	R 661 10-18 y.o.	Risk, Ambiguity	Lottery MPLs	German/Math grade –	yes/yes	R: NS/– A: mixed/–
Taylor 2013	ST 98 undergrad	Risk, Risk Hyp	Lottery MPLs	CRT, Numeracy test –	yes/–	Real R: NS/– Hyp R: yes/–

[a]R = representative; ST = students; SOEP = German Socio-Economic Panel Study;
[b]Risk, Ambiguity, Cpd risk (compound risk) = tasks with real incentives; Risk Hyp = hypothetical;
[c]RavenIQ = Raven Progressive Matrices; Hit15 = test of backward reasoning; NumETS = test of quantitative literacy from the Educational Testing Service; WAIS = Wechsler Adult Intelligence Scale; SAT = SAT math scores; Mazes = mazes similar to http://games.yahoo.com/games/maze.html; CRT = cognitive reflective test adapted from Frederick (2005);
[d]– = no information; NS = non-significant relationship;
[e]BDM = Becker-DeGroot-Marschak method (Becker *et al.*, 1964); MPL = Multiple Pricing List (Holt and Laury, 2002); Choice = choice between a lottery and sure amount; Self-assessment = answer to a survey question;
[f](vers)Big5 = (version of) Big Five measures (openness to experience, neuroticism, extraversion, agreeableness, conscientiousness); SelfCtrl = Self-control; FlexTh = Flexible thinking; MPQ = Multidimensional Personality Questionnaire; LoC = Locus of Control;
[g]majority RA/AA = sample is risk/ambiguity averse on average (yes/no)

Table 2.3: Experimental treatments

Treatment	Max lottery prize	Stage I	Stage II	No. of groups
Separate Evaluation of lotteries	100 CZK	Lotteries	Tests	3
Separate Evaluation of lotteries	100 CZK	Tests	Lotteries	3
Separate Evaluation of lotteries	200 CZK	Lotteries	Tests	3
Separate Evaluation of lotteries	200 CZK	Tests	Lotteries	3
Total *Separate Evaluation*				12 groups
Joint Evaluation of lotteries	100 CZK	Lotteries	Tests	3
Joint Evaluation of lotteries	100 CZK	Tests	Lotteries	2
Joint Evaluation of lotteries	200 CZK	Lotteries	Tests	2
Joint Evaluation of lotteries	200 CZK	Tests	Lotteries	2
Total *Joint Evaluation*				9 groups

Table 2.4: Correlations between lotteries valuations, by prize and evaluation treatment

	Joint Evaluation			Separate Evaluation			**H**$_0$: r$_{JEval}$=r$_{SEval}$		
Max Lottery Prize = 100 CZK		L$_R$	L$_C$		L$_R$	L$_C$		L$_R$	L$_C$
	L$_C$	**0.8830** (<0.0001)		L$_C$	**0.7543** (<0.0001)		L$_C$	*	
	L$_A$	**0.8019** (<0.0001)	**0.7799** (<0.0001)	L$_A$	**0.4602** (0.0042)	**0.7982** (<0.0001)	L$_A$	**	
Max Lottery Prize = 200 CZK		L$_R$	L$_C$		L$_R$	L$_C$		L$_R$	L$_C$
	L$_C$	**0.1748** (0.5174)		L$_C$	**0.2561** (0.1016)		L$_C$		
	L$_A$	**0.5669** (0.0220)	**0.2670** (0.3175)	L$_A$	**0.3942** (0.0098)	**0.6126** (<0.0001)	L$_A$		
H$_0$: r$_{P100}$=r$_{P200}$		L$_R$	L$_C$		L$_R$	L$_C$			
	L$_C$	* * *		L$_C$	* * *				
	L$_A$	*	**	L$_A$					

Notes: Correlation coefficients are in bold, p-values are in parentheses.
Tests for both H$_0$ are multivariate tests on correlations. For example, for a *Joint Evaluation* treatment we can reject at 1% level the hypothesis of equality of correlations between L$_R$ and L$_C$ across prizes.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2.5: Correlations among tests and background variables

	AT score	WMT	JudgC	MathA	NeedC	Persev	Premed	Sens	Girl
WMT score	**0.1863**								
	(0.0305)								
Judgmental Confidence	0.0880	0.0539							
	(0.3103)	(0.5348)							
Math Anxiety	**0.2674**	-0.0515	**0.5656**						
	(0.0017)	(0.5530)	(0.0000)						
Need for Cognition	0.0812	0.0351	**0.3986**	**0.5072**					
	(0.3492)	(0.6865)	(0.0000)	(0.0000)					
Perseverance	0.0491	0.1224	**0.3743**	**0.2881**	**0.5177**				
	(0.5714)	(0.1573)	(0.0000)	(0.0007)	(0.0000)				
Premeditation	0.0185	0.0449	-0.0370	0.1102	**0.3479**	**0.5218**			
	(0.8314)	(0.6051)	(0.6700)	(0.2034)	(0.0000)	(0.0000)			
Sensation Seeking	-0.0114	-0.0786	0.1255	-0.0118	0.1551	0.1201	-0.0894		
	(0.8952)	(0.3650)	(0.1471)	(0.8922)	(0.0725)	(0.1652)	(0.3025)		
Girl	-0.0730	0.1204	**-0.1897**	**-0.2091**	-0.0680	-0.0392	-0.0336	0.0868	
	(0.4000)	(0.1642)	(0.0275)	(0.0149)	(0.4334)	(0.6516)	(0.6990)	(0.3169)	
School is in Prague	0.1514	0.0309	0.0021	0.1017	0.0077	-0.0486	-0.0015	**-0.1980**	0.0704
	(0.0795)	(0.7217)	(0.9803)	(0.2406)	(0.9298)	(0.5758)	(0.9866)	(0.0213)	(0.4170)

Notes: Coefficients in bold are significant at 5% level.

Table 2.6: Relationship between RoCL and attitudes towards ambiguity

Study	Halevy (2007)			Abdellaoui *et al.* (2015)			Dean and Ortoleva (2015)			This paper		
	Reduce compound lotteries											
Ambiguity neutral	yes	no	Total	yes	no	Total	yes	no	Total	yes	no	Total
yes	**22**[a] (4.5[b]) 16%	**6** (23.5) 4%	**28** 20%	**13** (4.4) 11%	**17** (25.6) 15%	**30** 26%	**27** (5.7) 18%	**1** (22.3) 1%	**28** 19%	**46** (24.5) 34%	**13** (34.5) 10%	**59** 44%
no	**1** (18.5) 1%	**113** (95.5) 79%	**114** 80%	**4** (12.6) 4%	**81** (72.4) 70%	**85** 74%	**3** (24.3) 2%	**117** (95.7) 79%	**120** 81%	**10** (31.5) 7%	**66** (44.5) 49%	**76** 56%
Total	**23** 16%	**119** 84%	**142** 100%	**17** 15%	**98** 85%	**115** 100%	**30** 20%	**118** 80%	**148** 100%	**56** 41%	**79** 59%	**135** 100%
Fisher's exact test, 2-sided	$p < 0.001$			$p < 0.001$			$p < 0.001$			$p < 0.001$		

[a]Number of subjects;
[b]Expected number equals to number of subjects given independence between RoCL and ambiguity neutrality, for example, *This paper*, yes/yes: $24.5 = (59 \div 135) \times (56 \div 135) \times 135$;

Table 2.7: Variation with background, cognitive tests

Study	Halevy (2007)				Abdellaoui *et al.* (2015)				This paper							
	Reduce compound lotteries															
Ambiguity neutral	Major 0		Major 1		Non-engineers		Engineers		AT low[a]		WMT low[b]		AT high		WMT high	
	yes	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes	no
yes	**4**[c] 9%[d]	**1** 2%	**14** 20%	**4** 6%	**6** 15%	**9** 23%	**7** 9%	**8** 11%	**18** 24%	**11** 15%	**21** 31%	**9** 13%	**28** 46%	**2** 3%	**25** 37%	**4** 6%
no	**1** 2%	**38** 87%	**0** 0%	**53** 74%	**0** 0%	**25** 62%	**4** 5%	**56** 75%	**6** 8%	**39** 53%	**4** 6%	**34** 50%	**4** 7%	**27** 44%	**6** 9%	**32** 48%
Fisher's exact test, 2-sided	$p < 0.001$		$p < 0.001$		$p = 0.001$		$p = 0.001$		$p < 0.001$		$p < 0.001$		$p < 0.001$		$p < 0.001$	

[a]Median AT score = 5; AT low = 0-5; AT high = 6-11;
[b]Median WMT score = 51; WMT low = 15-51; WMT high = 52-73;
[c]Number of subjects;
[d]% of total within a subgroup (e.g. Major 0);

Table 2.8: Sample characteristics, by evaluation treatment

	Joint Evaluation			Separate Evaluation			t-test	
Variable	Mean	Med	SD	Mean	Med	SD	Diff	p-value
Girl	0.393	0	0.493	0.443	0	0.500	−0.050	0.564
School is in Prague	0.554	1	0.502	0.557	1	0.500	−0.003	0.969
Arithmetic Test score	4.732	5	2.347	5.506	6	2.370	−0.774*	0.063
Working Memory Test score	50.268	50.5	12.114	52.354	53	11.514	−2.087	0.312
Judgmental Confidence	2.505	2.475	0.513	2.504	2.5	0.430	0.002	0.985
Math Anxiety	2.554	2.45	0.772	2.619	2.6	0.822	−0.065	0.649
Need for Cognition	2.406	2.417	0.462	2.407	2.417	0.417	0.000	0.996
Perseverance	2.871	2.95	0.500	2.891	2.85	0.388	−0.020	0.794
Premeditation	2.934	3	0.526	2.896	2.909	0.418	0.038	0.643
Sensation Seeking	2.990	3.208	0.680	3.041	3	0.474	−0.051	0.608
Observations		56			79		135	

Notes: ** $p < 0.05$, * $p < 0.1$

Table 2.9: Sample characteristics, by lottery prize

	100 ZK			200 CZK			t-test	
Variable	Mean	Med	SD	Mean	Med	SD	Diff	p-value
Girl	0.377	0	0.488	0.483	0	0.500	−0.106	0.220
School is in Prague	0.571	1	0.498	0.534	1	0.500	0.037	0.672
Arithmetic Test score	5.026	5	2.242	5.397	6	2.370	−0.371	0.373
Working Memory Test score	52.000	53	11.528	50.810	50.5	11.514	1.190	0.563
Judgmental Confidence	2.494	2.5	0.443	2.519	2.55	0.430	−0.025	0.754
Math Anxiety	2.511	2.5	0.739	2.700	2.6	0.822	−0.189	0.175
Need for Cognition	2.397	2.417	0.433	2.419	2.375	0.417	−0.022	0.776
Perseverance	2.828	2.85	0.461	2.954	2.95	0.388	−0.126	0.096*
Premeditation	2.880	2.909	0.520	2.955	3	0.418	−0.074	0.359
Sensation Seeking	2.996	3.083	0.607	3.051	3	0.474	−0.055	0.576
Observations		77			58		135	

Notes: ** $p < 0.05$, * $p < 0.1$

Table 2.10: Effects of lotteries presentation and cognitive skills on RoCL and ambiguity neutrality

Dependent var	RoCL			Ambiguity neutrality		
	(1)	(2)	(3)	(4)	(5)	(6)
Evaluation (Joint=1)	-3.451***	-3.504***	-3.707***	-3.775***	-3.656***	-3.775***
	(1.281)	(1.324)	(1.423)	(1.246)	(1.286)	(1.356)
Working Memory Test score	-0.012	-0.009	-0.004	-0.019	-0.018	-0.015
	(0.013)	(0.013)	(0.014)	(0.013)	(0.013)	(0.014)
Arithmetic Test score	0.082	0.087	0.058	0.059	0.052	0.035
	(0.065)	(0.066)	(0.071)	(0.063)	(0.064)	(0.068)
Max Lottery Prize (100 CZK=1)	0.035	-0.032	-0.029	0.012	0.097	0.089
	(0.295)	(0.319)	(0.326)	(0.291)	(0.313)	(0.318)
Evaluation × WMT score	0.049**	0.050**	0.051**	0.051**	0.054**	0.055**
	(0.021)	(0.021)	(0.022)	(0.020)	(0.021)	(0.022)
Evaluation × AT score	0.158	0.139	0.182	0.124	0.111	0.135
	(0.115)	(0.117)	(0.129)	(0.110)	(0.113)	(0.122)
Evaluation × Prize	0.873*	0.991	0.973	0.974*	0.620	0.583
	(0.528)	(0.639)	(0.666)	(0.529)	(0.625)	(0.647)
Girl, School is in Prague		yes	yes		yes	yes
Non-cognitive skills			yes			yes
Pseudo-R^2	0.121	0.145	0.171	0.092	0.116	0.127
LR (p-value)	22.2(0.002)	26.5(0.002)	31.3(0.008)	17.0(0.017)	21.4(0.011)	23.4(0.076)
Observations	135	135	135	135	135	135

Notes: Reported coefficients are from probit model, standard errors are in parentheses;
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$;
Evaluation: dummy variable, Joint = 1, Separate = 0;
Max Lottery Prize: dummy variable, 100 CZK = 1, 200 CZK = 0.

Table 2.11: Marginal effects of treatments and cognitive skills

	RoCL			Ambiguity neutrality		
	(1)	(2)	(3)	(4)	(5)	(6)
WMT and AT scores are at:	(25th pc)	(50th pc)	(75th pc)	(25th pc)	(50th pc)	(75th pc)
Evaluation: Joint	-0.043	0.128	0.380***	-0.161	0.015	0.280**
	(0.101)	(0.088)	(0.118)	(0.106)	(0.091)	(0.128)
Working Memory Test score	0.005	0.006	0.004	0.002	0.002	0.002
	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)	(0.003)
Arithmetic Test score	0.043**	0.045**	0.036**	0.030	0.032	0.028
	(0.019)	(0.020)	(0.017)	(0.019)	(0.020)	(0.018)
Max Lottery Prize: 100 CZK	0.109	0.133	0.106	0.105	0.121	0.113
	(0.080)	(0.091)	(0.090)	(0.085)	(0.095)	(0.095)
Other controls (Girl, School is in Prague, Non-cognitive skills)	yes	yes	yes	yes	yes	yes
Observations	135	135	135	135	135	135

Notes: Results (marginal effects) are from probit model, standard errors are in parentheses, all variables are at means *except* for WMT and AT scores;
(1) and (4) WMT and AT scores are at 25th percentile: 44 for WMT, 4 for AT;
(2) and (5) WMT and AT scores are at 50th percentile: 51 for WMT, 5 for AT;
(3) and (6) WMT and AT scores are at 75th percentile: 60 for WMT, 7 for AT;
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Figures

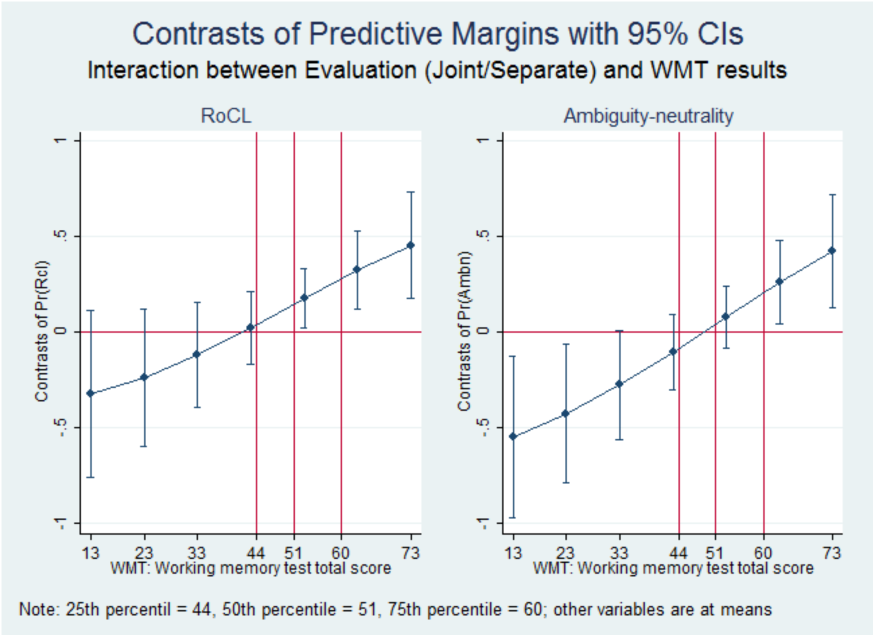

Figure 2.1: Working Memory Test

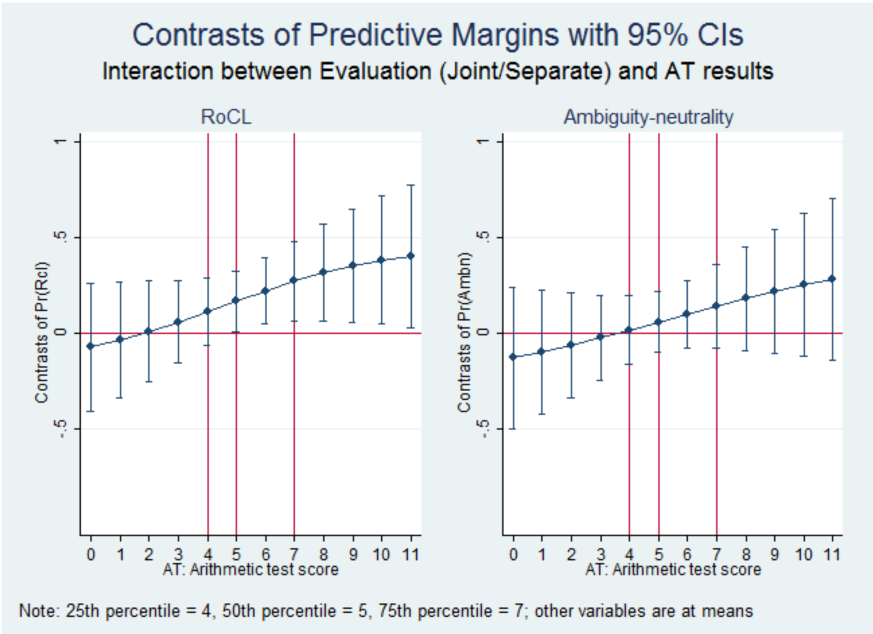

Figure 2.2: Arithmetic Test

Appendix 1: Instructions (MPL form for a lottery with 100 CZK prize)[18]

STUDENT ID: _______________

INSTRUCTIONS

In every row, please, make a cross in either the left or right square.
– If you cross the **left** square, then it means you would like to **draw a container** from the bag. In case you guess the color correctly, you will win **100 CZK**.
– If you cross the **right** square, then it means for this row you would like to receive a **sure amount of money** stated there.

Remember that you do not know yet what row will be played in the end of the experiment. Your final reward will depend on which row you draw and what choice you make there.

Row [1]	Draw a container	☐	|	Receive money, 10 CZK	☐
Row [2]	Draw a container	☐	|	Receive money, 20 CZK	☐
Row [3]	Draw a container	☐	|	Receive money, 30 CZK	☐
Row [4]	Draw a container	☐	|	Receive money, 40 CZK	☐
Row [5]	Draw a container	☐	|	Receive money, 50 CZK	☐
Row [6]	Draw a container	☐	|	Receive money, 60 CZK	☐
Row [7]	Draw a container	☐	|	Receive money, 70 CZK	☐
Row [8]	Draw a container	☐	|	Receive money, 80 CZK	☐
Row [9]	Draw a container	☐	|	Receive money, 90 CZK	☐
Row [10]	Draw a container	☐	|	Receive money, 100 CZK	☐

[18]Translation from Czech

Appendix 2: Sensitivity analysis

A: Inconsistent MPLs

Table 2.2A.1 reports the results from estimating the effect of cognitive skills on whether the subject made inconsistent choices in MPL. The coefficient for AT score is positive and significant at the 5 percent level. The probability of making inconsistent choices is less for those with higher scores in AT.

Table 2.2A.1: Inconsistent choices in MPLs and cognitive skills[a]

Dependent var	Inconsistent MPL	
	(1)	(2)
AT score	-0.009**	-0.010**
	(0.004)	(0.004)
WMT score		0.001
		(0.002)
Preuso-R^2	0.025	0.026
LR (p-value)	6.51(0.011)	6.74(0.034)
Observations	233	231

[a]Results (marginal effects) are from probit model, standard errors are in parentheses; **Significant at the 5% level

B: Order effect

To control for order effects, the lotteries were presented in different orders (differed by classes). Harrison *et al.* (2005) show that a lottery which comes later in a series of lottery valuations (the lotteries were of different scale) may be valued less than when it comes first. Thus, subjects exhibit more risk aversion in later tasks. Halevy (2007), in contrast, finds an opposite effect in his data. It is possible that some subjects might be more cautious when seeing the lottery task for the first time, and thus assign lower values. Further valuation tasks may already seem familiar, and therefore the valuations would be shifted upwards. Table 2.2B.1 shows mean valuations for each lottery in my experiment depending on the order it was presented. There is a slight decrease on average for valuations of 100 CZK lotteries (in contrast to Halevy, 2007); however, there is no clear trend for the 200 CZK lotteries. To test for order effects on valuation, I investigate both the impact of being presented before (versus after) some other lottery and the impact of order relative to other lotteries.

Table 2.2B.1: Mean lottery valuations, by prize

	Lottery 100 CZK				Lottery 200 CZK			
	Task #1	Task #2	Task #3	All	Task #1	Task #2	Task #3	All
L_R	48.8	53.1	44.4	49.7	88.9	72.2	95.4	87.6
L_C	48.8	44.6	47.3	46.8	91.8	81.4	81.5	83.4
L_A	47.7	46.1	41.4	45.0	70	84.3	84.3	82.1

1) *Does lottery valuation depend on the absolute order of presentation (whether the lottery was presented first versus second or third)?*

To test for this possible bias, I compare MPL_R (MPL_C or MPL_A) value when a lottery was valued first out of all three lotteries to MPL_R (MPL_C or MPL_A) when a lottery was valued second or third. Table 2.2B.2 shows that the order of presentation did not impact the results: For all lotteries I cannot reject the test of difference in means at 5% level.

Table 2.2B.2: Does lottery valuation depend on the absolute order of presentation?[a]

Lottery 100 CZK	
H_0: L_R (R is 1st) = L_R (R is 2nd or 3rd),	p=0.6857, n_1=26, n_2=51
H_0: L_C (C is 1st) = L_C (C is 2nd or 3rd),	p=0.2848, n_1=21, n_2=56
H_0: L_A (A is 1st) = L_A (A is 2nd or 3rd),	p=0.3156, n_1=30, n_2=47
Lottery 200 CZK	
H_0: L_R (R is 1st) = L_R (R is 2nd or 3rd),	p=0.8570, n_1=38, n_2=20
H_0: L_C (C is 1st) = L_C (C is 2nd or 3rd),	p=0.3281, n_1=11, n_2=47
H_0: L_A (A is 1st) = L_A (A is 2nd or 3rd),	p=0.3046, n_1=9, n_2=49

[a] All tests are Wilcoxon-Mann-Whitney tests due to small sub-samples.

2) *Does lottery valuation depend on the relative order of presentation?*

Table 2.2B.3 presents the results of tests performed separately by prize. For example, in the first line of the table I test whether the mean value of the risky lottery differs for groups that were presented this lottery before (as opposed to after) the compound lottery. Similar to the first question, all differences in means are insignificant, with marginal significance for the ambiguous lottery in the 100 CZK task.

3) *Does lottery valuation depend on the order relative to unrelated experimental tasks?*

Table 2.2B.4 shows all tests are insignificant; therefore we cannot reject the hypothesis of differences between valuations depending on order with unrelated experimental tasks.

Table 2.2B.3: Does lottery valuation depend on the relative order of presentation?[a]

Lottery 100 CZK	
H_0: L_R (R before C) = L_R (R after C),	p=0.8957, n_1=38, n_2=39
H_0: L_R (R before A) = L_R (R after A),	p=0.5363, n_1=47, n_2=30
H_0: L_C (C before R) = L_C (C after R),	p=0.5598, n_1=39, n_2=38
H_0: L_C (C before A) = L_C (C after A),	p=0.5593, n_1=28, n_2=49
H_0: L_A (A before R) = L_A (A after R),	p=0.3156, n_1=30, n_2=47
H_0: L_A (A before C) = L_A (A after C),	p=0.2673, n_1=49, n_2=28
Lottery 200 CZK	
H_0: L_R (R before C) = L_R (R after C),	p=0.2276, n_1=47, n_2=11
H_0: L_R (R before A) = L_R (R after A),	p=0.8570, n_1=38, n_2=20
H_0: L_C (C before R) = L_C (C after R),	p=0.3281, n_1=11, n_2=47
H_0: L_C (C before A) = L_C (C after A),	p=0.4536, n_1=18, n_2=40
H_0: L_A (A before R) = L_A (A after R),	p=0.8552, n_1=20, n_2=38
H_0: L_A (A before C) = L_A (A after C),	p=0.4426, n_1=40, n_2=18

[a]All tests are Wilcoxon-Mann-Whitney tests due to small sub-samples.

Table 2.2B.4: Does lottery valuation depend on the order relative to unrelated experimental task?[a]

Lottery 200 CZK	
H_0: L_R (before exp) = L_R (after exp),	p=0.8097, n_1=7, n_2=36
H_0: L_C (before exp) = L_C (after exp),	p=0.9583, n_1=7, n_2=36
H_0: L_A (before exp) = L_A (after exp),	p=0.6215, n_1=7, n_2=36

[a]All tests are Wilcoxon-Mann-Whitney tests due to small sub-samples.

C: Size of the lottery prize

Does lottery prize amount have any effect on CA and AA (attitudes towards compound risk and ambiguity relative to risk)? For all the lottery valuations, the Wilcoxon-Mann-Whitney test cannot be rejected (see Table 2.2C.1).

Table 2.2C.1: Do relative attitudes towards compound risk/ambiguity depend on prize amount?[a]

H_0: CA (prize=100) = CA (prize=200),	p=0.6444, n_1=77, n_2=58
H_0: AA (prize=100) = AA (prize=200),	p=0.5490, n_1=77, n_2=58

[a]All tests are Wilcoxon-Mann-Whitney tests due to non-normality of sub-samples.

D: Mid-list problem

A potential problem for MPL procedure is that some subjects might switch from the lottery column to the sure amount column exactly in the middle of the pricing list because they might subconsciously wish to make their answer look symmetric and not because this is their true value (see a discussion in Andersen *et al.*, 2006). I test whether the mean differs from the mid-list value for each lottery valuation. For all variables, t-tests reject the null hypothesis of equality between mid-list value and mean lottery valuation (see Table 2.2D.1).

Table 2.2D.1: Lottery valuations comparing to mid-list values[a]

Lottery 100 CZK		Lottery 200 CZK	
H_0: L_R=55,	p=0.0055, n = 77	H_0: L_R=110,	p<0.0001, n = 58
H_0: L_C=55,	p=0.0001, n = 77	H_0: L_C=110,	p<0.0001, n = 58
H_0: L_A=55,	p<0.0001, n = 77	H_0: L_A=110,	p<0.0001, n = 58

[a]All tests are one sample t-tests.

Figure 2.2D.1 shows distributions of valuations versus mean values.

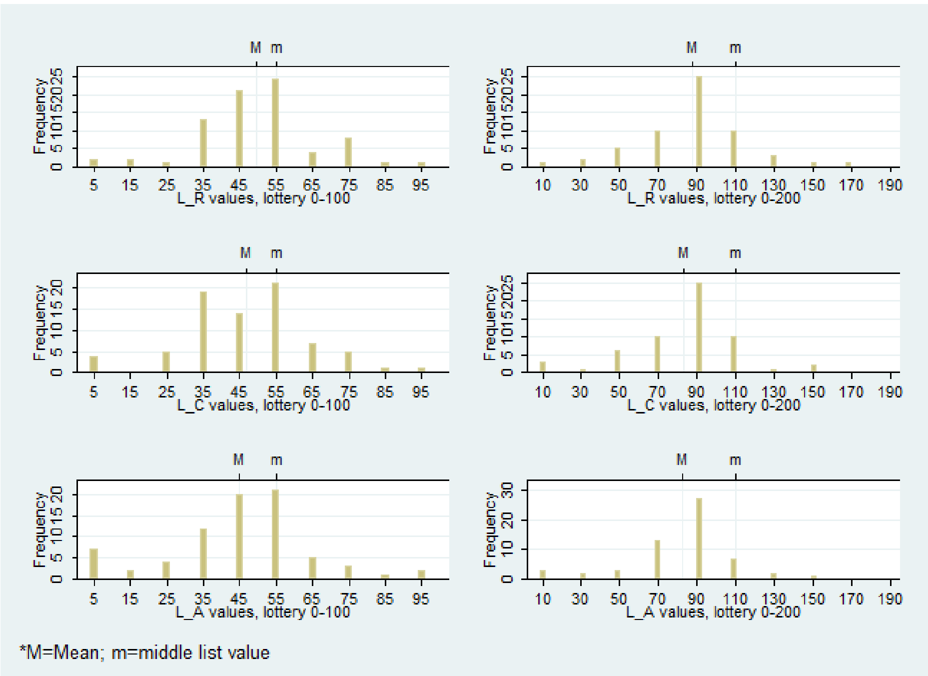

Figure 2.2D.1: Distributions of lottery valuations against mid-list value, by lottery prize

E: Low accuracy on the math operations in WMT

Following the literature in psychology (Engle *et al.*, 1999; De Neys *et al.*, 2002; Unsworth *et al.*, 2005), I excluded from analysis subjects who could not accurately solve mathematical equations during the WM test. Usually, studies require an accuracy rate above 85%. Durette (2011) in his thesis argues that by dropping too many subjects due to an 85% threshold we may create an unnecessary bias in our subject sample: to eliminte those with high rates of math anxiety instead of those who use only short memory or have low motivation in performing the test.

Since I am dealing with adolescents (and as explained in the footnote 11, p.45 in the main text), in this section I replicate the main tables (Table 2.10 and 2.11) for a WMT accuracy rate 70% (I exclude only those who make more than 23 errors, compared to 12 in the main analysis), which corresponds to the 95th percentile for total accuracy errors in WMT (see Figure 2.2E.1). The relationships between the main variables (ambiguity neutrality and RoCL) and cognitive skills, as well as treatment effect (*Joint Evaluation*), remain significant as in the main analysis.

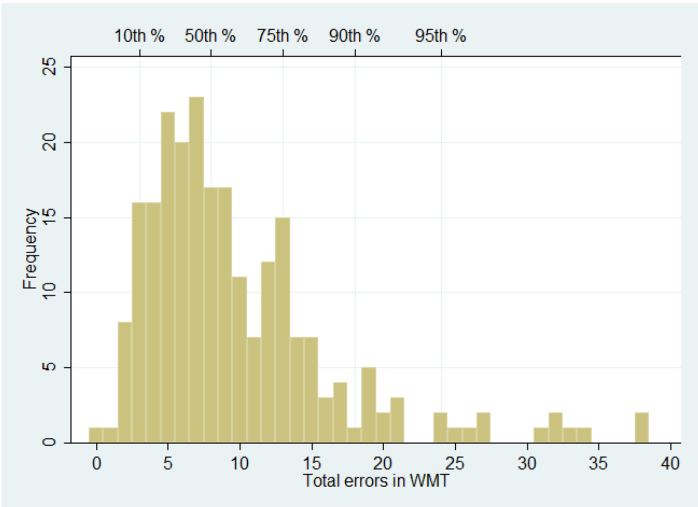

Figure 2.2E.1: Distribution of total errors in Working Memory test (for all participants, $N = 233$)

Table 2.2E.1: Effects of lotteries presentation and cognitive skillson on RoCL and ambiguity neutrality

Dependent var	RoCL			Ambiguity neutrality		
	(1)	(2)	(3)	(4)	(5)	(6)
Evaluation (Joint=1)	-4.000***	-3.858***	-4.128***	-3.390***	-3.016***	-3.137***
	(1.120)	(1.170)	(1.243)	(1.021)	(1.059)	(1.091)
Working Memory Test score	-0.020	-0.016	-0.012	-0.021*	-0.018	-0.018
	(0.012)	(0.012)	(0.012)	(0.011)	(0.012)	(0.012)
Arithmetic Test score	0.089	0.079	0.053	0.048	0.034	0.031
	(0.058)	(0.059)	(0.063)	(0.057)	(0.057)	(0.060)
Max Lottery Prize (100 CZK=1)	0.044	0.0288	0.041	0.027	0.122	0.097
	(0.273)	(0.291)	(0.297)	(0.269)	(0.287)	(0.292)
Evaluation × WMT score	0.056***	0.056***	0.057***	0.044***	0.044**	0.047**
	(0.018)	(0.019)	(0.019)	(0.018)	(0.018)	(0.019)
Evaluation × AT score	0.210**	0.191*	0.235*	0.124	0.105	0.110
	(0.107)	(0.110)	(0.120)	(0.096)	(0.099)	(0.104)
Evaluation × Prize	0.794	0.705	0.675	0.878	0.460	0.390
	(0.488)	(0.577)	(0.591)	(0.473)	(0.549)	(0.560)
Girl, School is in Prague		yes	yes		yes	yes
Non-cognitive skills			yes			yes
Pseudo-R^2	0.133	0.160	0.177	0.076	0.105	0.116
LR (p-value)	29.7($<$ 0.001)	35.7.5($<$ 0.001)	39.6(0.001)	17.1(0.017)	23.6(0.005)	26.0(0.038)
Observations	164	164	164	164	164	164

Notes: Reported coefficients are from probit model, standard errors are in parentheses;
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$;
Evaluation: dummy variable, Joint = 1, Separate = 0;
Max Lottery Prize: dummy variable, 100 CZK = 1, 200 CZK = 0.

Table 2.2E.2: Marginal effects of treatments and cognitive skills

	RoCL			Ambiguity neutrality		
	(1)	(2)	(3)	(4)	(5)	(6)
WMT and AT scores are at:	(25th pc)	(50th pc)	(75th pc)	(25th pc)	(50th pc)	(75th pc)
Evaluation: Joint	-0.122	0.107	0.419***	-0.176*	-0.008	0.223*
	(0.093)	(0.083)	(0.104)	(0.094)	(0.082)	(0.117)
Working Memory Test score	0.003	0.004	0.002	0.000	0.000	0.000
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Arithmetic Test score	0.049***	0.054***	0.039***	0.026	0.028	0.026
	(0.018)	(0.019)	(0.015)	(0.017)	(0.018)	(0.016)
Max Lottery Prize: 100 CZK	0.098	0.121	0.090	0.087	0.097	0.095
	(0.077)	(0.089)	(0.080)	(0.080)	(0.087)	(0.087)
Other controls (Girl, School is in Prague, Non-cognitive skills)	yes	yes	yes	yes	yes	yes
Observations	164	164	164	164	164	164

Notes: Results (marginal effects) are from probit model, standard errors are in parentheses, all variables are at means *except* for WMT and AT scores;
(1) and (4) WMT and AT scores are at 25th percentile: 42 for WMT, 4 for AT;
(2) and (5) WMT and AT scores are at 50th percentile: 50 for WMT, 5 for AT;
(3) and (6) WMT and AT scores are at 75th percentile: 59 for WMT, 7 for AT;
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Appendix 3: Impact of compound lottery presentation

Table 2.3.1 shows results from Halevy (2007) when the condition of compound lottery reduction is based on only one lottery. Data was taken from Halevy (2007). The calculations are my own.

Table 2.3.1: Impact of compound lottery presentation (Halevy, 2007)

	Reduce compound lotteries								
	V1=V3=V4			V1=V3			V1=V4		
Ambiguity neutral	yes	no	Total	yes	no	Total	yes	no	Total
yes	**22**[a] 16%	**6** 4%	**28** 20%	**25** 18%	**3** 2%	**28** 20%	**24** 17%	**4** 3%	**28** 20%
no	**1** 1%	**113** 79%	**114** 80%	**7** 5%	**107** 75%	**114** 80%	**22** 15%	**92** 65%	**114** 80%
Total	**23** 17%	**119** 83%	**142** 100%	**32** 23%	**110** 77%	**142** 100%	**46** 32%	**96** 68%	**142** 100%

[a] Number of subjects.

Appendix 4: Relationship between RoCL and attitudes to ambiguity

Table 2.4.1 presents the experimental data, divided by two sub-samples depending on how the lotteries were presented to the subjects. If we compare count data, we can observe approximately the same distribution of subjects among groups. The only difference is a slight increase in the number of individuals who reduce compound lotteries in the treatment *Joint Evaluation*.

Table 2.4.1: Relationship between RoCL and attitudes towards ambiguity, by treatment

Treatment:	Joint Evaluation			Separate Evaluation		
	Reduce compound lotteries					
Ambiguity neutral	yes	no	Total	yes	no	Total
yes	**22**[a] (13.0[b]) 39%	**4** (13.0) 7%	**26** 46%	**24** (11.7) 30%	**9** (21.3) 12%	**33** 42%
no	**6** (15.0) 11%	**24** (15.0) 43%	**30** 54%	**4** (16.3) 5%	**42** (29.7) 53%	**46** 58%
Total	**28** 50%	**28** 50%	**56** 100%	**28** 35%	**51** 65%	**79** 100%
Fisher's exact test (2-sided)	$p < 0.001$			$p < 0.001$		

[a]Number of subjects
[b]Expected number equals to number of subjects given independence between RoCL and ambiguity neutrality

Appendix 5: Additional figures

Figure 2.5.1: Containers used in the experiment

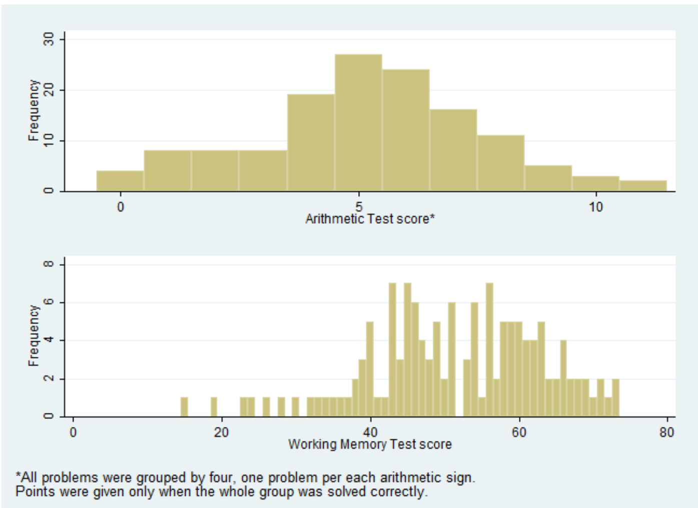

Figure 2.5.2: Cognitive tests distributions

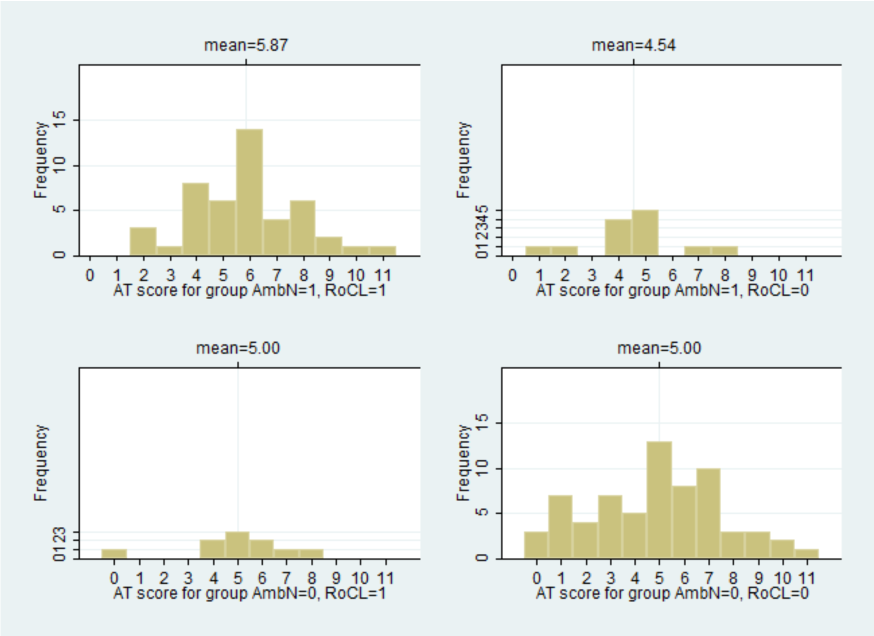

Figure 2.5.3: Distributions of AT scores within groups with different attitudes to ambiguity and RoCL

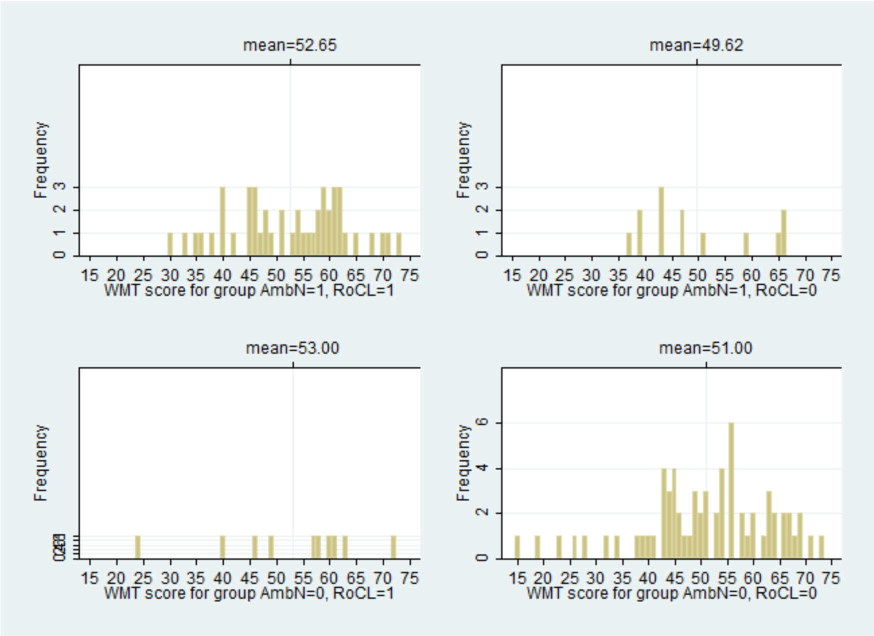

Figure 2.5.4: Distributions of WMT scores within groups with different attitudes to ambiguity and RoCL

Chapter 3

Determinants and Consistency of Behavior Under Ambiguity[1]

I review recent experimental studies on decision making under ambiguity and identify the main determinants related to intrinsic characteristics of a subject *(static contexts)* and to interaction between a subject and ambiguous reality *(dynamic contexts)*. Significantly fewer papers address robustness under dynamic contexts. Moreover, several studies report contradictory results and shifts to ambiguity-neutrality under certain conditions. I suggest that, if we aim to predict behavior under ambiguity, then we ought to focus on robustness of attitudes toward ambiguity, specifically in dynamic contexts.

JEL Classification: C91 D81
Keywords: Ambiguity, Ambiguity aversion, Uncertainty

[1] I thank Andreas Ortmann for many helpful comments. All errors are my own.

3.1 Introduction

It is more than 20 years since Camerer and Weber (1992) published the first widely cited literature review on developments in modeling preferences under ambiguity. Although a number of reviews have been published since then (the most recent being Gilboa and Marinacci, 2013; Etner *et al.*, 2012; Machina and Siniscalchi, 2014; Siniscalchi, 2008; Trautmann and van de Kuilen, 2014; Wakker, 2008), none of these papers extensively describes the literature on determinants of attitudes to ambiguity. So far, we have no explicit answer to the question as to whether ambiguity-sensitive behavior is a stand-alone and stable characteristic or if it becomes salient only in the presence of other factors and depends on specific contexts. Advocating the latter, Al-Najjar and Weinstein (2009) initiated a discussion in *Economics and Philosophy* about the usefulness of modeling preferences towards ambiguity. Al-Najjar and Weinstein opposed both normative and descriptive interpretations of ambiguity models because, as they claimed, non-neutral preferences towards ambiguity were irrational and ambiguity models had no apparent advantage in explaining this irrationality.

More recently a number of papers have highlighted a possible problem with consistency of attitudes to ambiguity in experiments. Charness *et al.* (2013), Duersch *et al.* (2013), Stahl (2014), and Voorhoeve *et al.* (2016) report significant numbers of subjects who exhibited inconsistent choices when evaluating several ambiguous lotteries or when asked the same question several times to reveal ambiguity preferences (within one session or with a time lag). In contrast, the overwhelming majority of subjects *with* consistent choices in their experiments turned out to be ambiguity-neutral. Thus, the authors question whether the Ellsberg paradox requires much attention from both theoretical and empirical points of view.

Behavioral economists list a variety of factors which can affect choices under ambiguity, additionally to fundamental preferences over outcomes. I classify the moderators of choices under ambiguity into three broad categories: related to the internal state of a decision maker; related to characteristics of an ambiguous situation itself; and related to interaction between a decision maker and ambiguity (see Table 3.1). Every subject can be described by a collection of intrinsic characteristics, which are state variables for a specific ambiguous situation. These characteristics (for example, gender, personality or wealth) can be considered as *static* determinants in the short term, even though they can possibly change over time. Analogously, we can identify static characteristics of any ambiguous situation; for example, the source of ambiguity or outcome domain (gains or losses). The subject constantly interacts with the ambiguous reality through learning new information. Thus, by interaction between

Table 3.1: Moderators of choices under ambiguity

SUBJECT	$\longleftrightarrow$	AMBIGUITY
Intrinsic characteristics [static]	*Interaction* [dynamic]	*Characteristics of ambiguity*
– Risk and time preferences	– Intertemporal stability	– Source of ambiguity
– Reduction of compound risk	– Social interactions	– Outcome domain (gains/losses)
– Demographic characteristics	– Learning	
– Competence	– Markets	

subject and ambiguity, I refer to *dynamic* situations where the level of ambiguity is endogenous. By determinants in dynamic contexts I refer to any factors which lead to acquisition of new information. The examples are intertemporal choices, peer effects, discussions with others, learning, and market interactions.

In this paper I review experimental studies focusing on the robustness of ambiguity attitudes to intrinsic static characteristics and dynamic contexts. A number of papers describe how characteristics of ambiguity impact individual choices (Trautmann and van de Kuilen, 2014, provide the most recent overview). In my survey, however, I would like to focus on the subject, and juxtapose the impact of internal static characteristics to the impact of external dynamic contexts. The objective is two-fold: First, to classify the experimental evidence on the determinants of preferences to ambiguity and analyze whether they impact robustness; second, to identify gaps in the literature on ambiguity and discuss possible directions for further research. Camerer and Weber (1992) focused on general concepts and models of ambiguity-sensitive behavior and their application. The authors discussed psychological determinants of ambiguity aversion and behavior in the experimental markets, albeit referring to few papers. Etner *et al.* (2012) mainly reviewed theoretical studies and only briefly touched upon experimental studies. Similarly, Gilboa and Marinacci (2013), Machina and Siniscalchi (2014), Siniscalchi (2008), and Wakker (2008) reviewed recent theoretical advances. Trautmann and van de Kuilen (2014) discuss extensively the implementation of ambiguity in the lab, the stylized facts observed in the experiments, and the evidence on external validity. However, to date there has been no complete overview of papers studying the determinants of ambiguity, specifically static characteristics of a subject and dynamic contexts.

I proceed as follows. In section 2, I describe the notion of *ambiguity* and selection criteria for the studies surveyed. In sections 3 and 4, I review the corresponding experimental papers for static and dynamic contexts. In section 5, I discuss possible directions for future research and provide conclusions.

3.2 Definitions

First appearing in the works of Knight (1921) and Keynes (1921, 1937), the difference between the terms “uncertainty” and “risk”[2] became widely accepted in economics only after Ellsberg (1961) published his work on how behavior under “uncertainty” (as opposed to behavior under “risk”) can violate Savage’s Subjective Expected Utility Theory (SEU; Savage, 1954). Ellsberg himself believed in the theory’s ability to predict outcomes in the majority of situations where uncertainty is involved. He showed, however, special cases where subjects’ deliberate actions contradict SEU axioms. Ellsberg termed those situations *ambiguity*:

> Let us imagine a situation in which so many of the probability judgments an individual can bring to bear upon a particular problem are either “vague” or “unsure” that his confidence in a particular assignment of probabilities, as opposed to some other of a set of “reasonable” distributions, is very low. We may define this as a situation of high ambiguity. (1961, p. 660)

Some studies still refer to situations with unknown probabilities as “uncertainty” (e.g. Frechette *et al.*, 2014), although today it is common practice to term those situations “ambiguity”. “Uncertainty” is instead used typically in a broad sense, to describe a state with any unknown parameters (for example, risk, ambiguity, or lotteries with unknown outcomes).

For my survey, I selected 64 studies, following a clearly defined identification process. I classified them into two main categories – static and dynamic contexts – and further into several subcategories (see Appendix 1 for the detailed description of selection criteria). Despite the growing number of publications related to ambiguity,[3] determinants of preferences related to intrinsic characteristics receive more attention relative to those in dynamic contexts. 48 of 64 studies fit the criteria for the static contexts part compared to 20 of 64 for the dynamics context part (four studies were included in both categories).

3.3 Static contexts

In Table 3.2, I present papers which relate ambiguity-sensitive behavior to intrinsic characteristics, grouped into four subcategories: risk and time preferences, relationship to reduction

[2] Risk is understood as a situation when the probability distribution of outcomes is *known*; uncertainty is understood as a situation when the probability of outcomes is *unknown*.

[3] A search request in the EBSCO EconLit database in April 2015 returned over 2300 results for the word “ambiguity”: 374 were published in 2000-2004, 621 in 2005-2009, and 842 in 2010-2014. The original paper by Ellsberg (1961) has over 5400 citations and the first extensive survey by Camerer and Weber (1992) has over 1250 citations in Google Scholar. In one of the unofficial ratings created in 2013, Ellsberg’s paper is the 6th in top papers in behavioral economics by citations in Web of Science and 7th in Google scholar, see http://economicspsychologypolicy.blogspot.com/2013/07/most-cited-papers-in-behavioral.html

of compound risk, demographic characteristics, and competence. It is impossible to mention every single characteristic that might have a relationship with attitudes to ambiguity, therefore the following table summarizes the main directions of research in this area.

3.3.1 Risk and time preferences

Risk and time preferences are the main characterizations in Expected Utility Theory and numerous experimental studies estimate both risk and time preferences, and relationships between them.[4] Dean and Ortoleva (2015), Cohen *et al.* (2011), and Sutter *et al.* (2013) analyzed behavior under all three domains – risk, time, and ambiguity. The authors elicited individual measures of the three attitudes in a model-free setting and looked at correlations among them. All three studies found no relationship between impatience and ambiguity aversion, but contradictory results for the relationship between risk- and ambiguity aversion (positive in Dean and Ortoleva, negative in Sutter *et al.*, and no relationship in Cohen *et al.*). Given that recent studies report biased results for discount rates estimated independently from risk preferences (see Cheung, 2015, pp. 2243–2245 and references therein), it is possible that the estimation strategy influenced the results of Dean and Ortoleva, Cohen *et al.*, and Sutter *et al.*

Chesson and Viscusi (2003) introduced the uncertainty in timing of outcomes. Their subjects had a choice between receiving money in a certain period of time or participating in a lottery which would randomly choose one of the two periods of time (expected mean time was the same). Those individuals who exhibited higher levels of aversion to ambiguity were more likely to be averse to the uncertain timing of outcomes. All the questions were hypothetical, therefore we should take the results with caution.

Trautmann and van de Kuilen (2014, pp. 22–24) provide an overview of the extensive literature studying the relationship between attitudes to risk and ambiguity. The evidence mostly supports a positive relationship between risk-aversion and ambiguity aversion; however, several studies do not report any relationship at all (Brown *et al.*, 2010; Levy *et al.*, 2010). Abdellaoui *et al.* (2011) and Weinstock and Sonsino (2014) suggest that optimism seems to explain part of the relationship between attitudes to risk and ambiguity. In their experiments individuals who were more tolerant to risk exhibited more optimism in uncertain (ambiguous) environments.

[4] For a survey see Jamison *et al.* (2012). Also see the most recent discussion on the estimation of risk and time preferences by Andreoni and Sprenger; Bin and Zhong; Cheung; and Epper and Fehr-Duda, in *American Economic Review*, **105**, 2015.

Some studies did not directly relate attitudes to risk and ambiguity, but instead showed how real life decisions under risk could differ from decisions under ambiguity.[5] The difference in behavior, depending on the type of uncertainty, provides an additional argument for considering attitudes to risk and ambiguity as separate preferences.

3.3.2 Reduction of compound risk

Several theories model ambiguity preferences by relaxing the Reduction of Compound Lotteries axiom.[6] For experimentalists, compound (two-stage) lotteries are a convenient way to design ambiguity in experiments. Therefore, a valid empirical question is whether and how ambiguity-sensitive behavior is related to reduction of compound prospects. Here I will consider only papers which investigate attitudes to compound risk and ambiguity on an individual level in a model-free setting. For an overview of experimental papers evaluating two-stage probability models see Attanasi *et al.* (2014, Section 5.2).

Bernasconi and Loomes (1992) were the first to show that the Ellsberg problem, presented as a two-stage lottery and in a between-subject design, produced many fewer violations of SEU than the usual Ellsberg-type experiments. Halevy (2007), in a within-subject experiment, evaluated preferences toward risk, ambiguity and different types of compound risk. He reported a relationship between violations of ambiguity-neutrality and compound risk reduction. In Prokosheva (2015), I observed a less significant relationship than Halevy and additional sensitivity to experimental implementation details and the cognitive skills of the subject sample. Similarly, Abdellaoui *et al.* (2015) did not find a relationship between ambiguity neutrality and reduction of compound lotteries for a subset of their subject sample. Overall, studies suggest that there is no clear-cut relationship between preferences to ambiguity and compound risk.

3.3.3 Demographic characteristics

Biological characteristics

[5] Alpizar *et al.* (2011) report that farmers in Costa Rica were more willing to accept changes in technologies when the risk was uknown (ambiguous) compared to known. In an experiment with Midwestern grain farmers, Barham *et al.* (2014) observed higher speed of adoption of new technologies for subjects who were more ambiguity-averse. Carbone and Infante (2014) investigated intertemporal consumption and found different results for choices under risk compared to choices under ambiguity.

[6] Harrison *et al.* (2015) explain and discuss different formulations of the Reduction of Compound Lotteries axiom (see pp. 1–5). A definition taken from there: "the Reduction of Compound Lotteries axiom states that a decision-maker is indifferent between a compound lottery and the actuarially-equivalent simple lottery in which the probabilities of the two stages of the compound lottery have been multiplied out".

Based on evidence from neuroscience,[7] Rustichini *et al.* (2005) hypothesized that choices under risk and ambiguity activate different mental processes. Their experiment revealed different cortex zones activated for ambiguous and risky environments, and a smaller response time for choices under ambiguity than for choices under risk. A smaller response time for a seemingly more difficult choice problem – when probabilities are unknown – might seem preposterous. The authors explain it by the trade-off between costs of attentional effort and effectiveness. Dickhaut *et al.* (2013) and Huettel *et al.* (2006) report the same result of shorter response time for the evaluation of ambiguous choice. Huettel *et al.* (2006) and Smith *et al.* (2002) also found that separate cortex zones were activated in risk and ambiguity tasks. Chew *et al.* (2012) discovered a connection between genes linked to anxiety-related traits and ambiguity aversion. We can conclude that studies investigating physiological characteristics in humans[8] agree on the existence of distinct factors which guide decision-making processes under ambiguity.

Numerous studies have related gender to risk preferences (the most recent examples include Booth and Katic, 2013; Charness and Gneezy, 2012; Eckel and Grossman, 2008) and have reported males as less risk-averse than females. This is probably one of the most stable and replicated results in the behavioral economics literature. Whereas Croson and Gneezy (2009) did not differentiate between "uncertainty" (ambiguity) and risk, the majority of studies aimed at differentiating between these notions did not find any significant relationship between gender and preferences to ambiguity (Borghans *et al.*, 2009; Butler *et al.*, 2013; Sutter *et al.*, 2013).[9] These results seem striking. The gender difference in risk attitudes is attributed to overconfidence: Risk-taking behavior in males is partially explained by their higher levels of confidence (Croson and Gneezy, 2009). We might expect overconfident people to feel more competent on average. Since several studies have found a negative relationship between ambiguity aversion and the feeling of competence (Heath and Tversky, 1991, and see subsection 3.3.4 in this chapter), then we might expect to see less ambiguity-averse males than females. The existing experimental evidence, however, does not support this logic.

Cardenas and Carpenter (2013) and Tymula *et al.* (2012) observed lower ambiguity

[7]See Taya (2012) for a review of neuroimaging studies, mostly published in natural sciences journals.

[8]Hayden *et al.* (2010) observed ambiguity-averse behavior in monkeys, therefore ambiguity-sensitive behavior is not an exclusively human characteristic.

[9]Brighetti and Lucarelli (2015) used only a psycho-physiological survey to evaluate attitudes to risk and ambiguity and also found no difference with respect to gender. Powell and Ansic (1997) studied risk-taking behavior in financial decisions and reported that ambiguity did not impact gender differences in risk strategies. Dimmock *et al.* (2015a) and Dimmock *et al.* (2016) found only weakly positive relationship between gender (male) and ambiguity-aversion using American Panel Survey. Interestingly, when the same authors (Dimmock *et al.*, 2015b) used the Dutch household survey, they did not find any relationship between gender and attitudes to ambiguity.

aversion in younger people. This corresponds to the results of Akay *et al.* (2012) who report a negative relationship between ambiguity aversion and health conditions.

Cognitive abilities and personality

If ambiguity-sensitive behavior is irrational, then we could expect to observe individuals with higher cognitive abilities exhibiting more ambiguity-neutral behavior. However, Borghans *et al.* (2009) report no relationship between cognitive abilities (measured by Raven's matrices) and elicited values of an ambiguous urn. Sutter *et al.* (2013) show that ambiguity aversion is (weakly significantly) increasing with better grades in German language. Both studies involved adolescents and young adults. Rustichini *et al.* (2012) used a large sample of trainee truck drivers and, similarly to Borghans et al., report no relationship between attitudes to ambiguity and cognitive skills.

A number of studies have proposed that attitudes to ambiguity could be influenced by psychological factors, for example, personality type, thought processes, or state of mood during the decision making process. Borghans *et al.* (2009) measured the Big Five psychological traits and found no relationship with ambiguity aversion. Butler *et al.* (2013) focus on how reliance on intuition reduces risk and ambiguity aversion. Retail investors and subjects in the lab, who based their decisions more on intuition than on reasoning, were less ambiguity-averse. Pulford (2009) and Feinberg and Yesuf (2013) report that more optimistic people were less ambiguity-averse in their experiments. Baillon *et al.* (2014) used weather conditions as a proxy for affective state and found that bad weather (sadness) led to more ambiguity-neutral decisions. In contrast, Pulford and Gill (2014), using a different affective state – feeling lucky – did not find any significant relationship with attitudes to ambiguity.

Another possible explanation related to the individual model of thinking is that subjects have no access to a fair randomization device, and thus cannot decide on exact probabilities and avoid choosing ambiguous prospects. Dominiak and Schnedler (2011) showed that this was not the case; in their experiment, most of the ambiguity-averse subjects were randomization-neutral.

Socio-economic background

Most theories in economics depend on the assumptions about relationship between risk preferences and wealth (Chiappori and Paiella, 2011; Guiso and Paiella, 2008; Paravisini *et al.*, 2010). Whereas literature addressing behavior under risk has established a common result of decreasing absolute risk aversion (negative relationship between wealth and absolute risk-aversion; see the overview in Chiappori and Paiella, 2011), there is no universal evidence of the relationship between preferences to ambiguity and wealth. Akay *et al.* (2012) investigated ambiguity (and risk) attitudes for the poor in Ethiopia and did not find any relationship to wealth. Similarly, in a recent study using a large representative sample and different elicitation method (source method), Dimmock *et al.* (2016) and Dimmock *et al.* (2015b) show an insignificant (weakly significant) relationship between attitudes to ambiguity and different demographic characteristics, including age, education, financial assets, health, and income. Butler *et al.* (2013), however, report a positive and significant correlation between ambiguity aversion and wealth. The authors stress the potential importance of this finding when explaining the stockholding puzzle and reconciling historical levels of equity premiums. In contrast, Li (2015) found a negative relationship between wealth and ambiguity aversion for adolescents in rural China, and Cardenas and Carpenter (2013) observed a negative relationship between economic well-being and wealth for a large representative sample from Latin America.

Societies whose citizens have experienced more adverse life conditions could be more conservative in evaluating ambiguous situations. Yet, both Akay *et al.* (2012) and Cardenas and Carpenter (2013) report that the heritage (ancestry) of experimental subjects did not affect their choices under ambiguity.

Trust

Given the set of actions, trust is always a decision with an uncertain outcome. Still, neither Eckel and Wilson (2004) nor Houser *et al.* (2010) observed a correlation between measures of risk-aversion and behavior in a trust game. Corcos *et al.* (2012), on the other hand, studied behavior under ambiguity and showed a negative correlation between ambiguity aversion and trust behavior in a one-shot trust game. Feinberg and Yesuf (2013) measured trust as a self-reported attitude towards other people and also revealed a negative correlation between ambiguity aversion and being trustful.

3.3.4 Competence: From intrinsic characteristics to context

Whereas preferences and demographic characteristics can be clearly classified as intrinsic characteristics, competence is a quality which combines the internal ability and the relation of a subject to the external context. Heath and Tversky (1991) were the first to conjecture that feeling the lack of being knowledgeable about a context can be a source of ambiguity-averse behavior (for a summary of recent literature see de Lara Resende and Wu, 2010, pp. 111–113). Successive studies, using a similar approach as Heath and Tversky, also found a negative relationship between ambiguity-averse behavior and competence (de Lara Resende and Wu, 2010; Di Mauro, 2008; Hogarth and Grieco, 2004; Keppe and Weber, 1995; Klein *et al.*, 2010).[10]

If competence impacts behavior under ambiguity, then subjects who are professionals should be less ambiguity-averse in decisions related to their competency. Surprisingly, Holm *et al.* (2013) did not find a significant difference in preferences to ambiguity for entrepreneurs (chief executive officers) and a control group of working class people in China. Similarly, Cabantous (2007) did not observe differences in premiums for insurers who were more familiar with the scenarios in experimental tasks. Koudstaal *et al.* (2015) reported comparable choices under ambiguity for a large set of entrepreneurs, managers and employees.

Various characteristics of a subject were shown to impact decision making under ambiguity, however, very few relationships seem robust under different experimental implementations. Competence appears to be one of the few traits to exhibit stable relationship with ambiguity-sensitive behavior. Since an increasing level of competence is usually subject to additional knowledge about ambiguous reality, the dynamic contexts might lead to lower levels of ambiguity aversion or, possibly, to ambiguity-neutral behavior.

3.4 Dynamic contexts

Almost every uncertain situation includes possibilities to learn additional information, observe others, or participate in a series of interactions with feedback, all of which might influence a decision maker's choices. Surprisingly, a much smaller strand of literature investigates the impact of these outside factors on attitudes to ambiguity compared to literature reviewed in the previous section. In Table 3.3, I classify all papers related to dynamic context by sources of new information and very briefly summarize how every paper answered the question, "Would preferences towards ambiguity remain robust in this context?"

The following subsections are arranged by increasing the level of interaction with the outside world. Self-contemplation over time is probably the most basic interaction, therefore

[10] I omit studies which report tests of the Comparative Ignorance Hypothesis (CIH) by Fox and Tversky (1995) because they focus on sources of ambiguity and not on a subject. For the most recent studies on CIH see Dolan and Jones (2004), Rubaltelli *et al.* (2010), and an overview by Trautmann and van de Kuilen (2014, 3.5)

the first subsection discusses intertemporal stability. Obtaining some new information is the next level of interaction. The general case refers to learning, when some new evidence about the ambiguous situation is revealed. As a special case of learning, I separate a discussion about social interactions with uninformative others, when subjects observe or participate in discussions with others, in a case where everyone possesses identical information and is aware of that. In the last subsection I review market situations, when agents are involved in two-sided interactions with feedback and the possibility to optimize choices.

3.4.1 Intertemporal stability

The concept of stability over time is an important assumption for descriptive and predictive purposes, for example when eliciting preferences or estimating parameters for the probability weighting function in Prospect Theory (for an overview of recent literature on preference stability over time see Zeisberger *et al.*, 2010). Yet, Duersch *et al.* (2013) are the only researchers to study the intertemporal stability of preferences to ambiguity. The authors conducted a three-color urn experiment with a time lag between tasks, measuring attitudes to ambiguity and risk in the intertemporal condition. The control condition elicits preferences with a minimum time lag – choices from the same urn within one experimental session. The authors report a decrease in consistency: Only 57% of choices under ambiguity were stable after a time lag of two months compared to 79% of choices within the same session. Interestingly, the time consistency of risk preferences elicited with the same time lag of two months was 85%, thus considerably higher than for ambiguity tasks. Two factors correlated with consistent intertemporal choices under ambiguity. Those who were able to recall their choices and/or were sure in their choices were more consistent in their decisions. However, Duersch et al. point out that the direction of causality is not clear. It is possible that either some subjects used the same strategy when evaluating urns, and therefore answered consistently, or that some subjects were consistent only because they recalled their previous answers.

Two recent studies report consistency of ambiguity preferences within one experimental session. Hey and Pace (2015) used the data from one of their previous experiments, in which subjects had to solve 76 tasks by allocating tokens among ambiguous prospects in every task. Hey and Pace divided all the tasks into two parts by the order of response and compared the parameters of the models they were estimating for each of the parts. The authors concluded that the parameters describing the perception of ambiguity did not change significantly and preferences on average did not become noisier.

Lahno (2014) investigated how observing preferences to ambiguity of another subject could change the decision maker's own preferences. Compared to 79% in Duersch *et al.* (2013), Lahno discovered 90% of her subjects exhibiting the same preferences in two tasks, independent of any peer information. Perhaps this relatively high percentage of consistency is due to her usage of a two-color/two-urn task, compared to a three-color urn task in Duersch et al. Trautmann and van de Kuilen (2014) concluded that the three-color task usually corresponded to lower levels of ambiguity aversion, and Chew *et al.* (2013b) explained this by task complexity. Chew *et al.* (2013b) report that in their experiment, those with low comprehension of tasks exhibited less ambiguity aversion. It is possible that inconsistency between tasks is connected to lesser understating of tasks, thus producing more noisy and randomized answers.

3.4.2 Social interactions with uninformative others

Social interactions or simple observations of others may impact individual decision making under uncertainty, first of all, because they add some additional sources of information. There is a vast amount of literature, originating in psychology and sociology, about the influence of the social context on decision processes. A number of studies report that groups should behave more rationally than individuals. This is especially true for situations in which a universally 'correct' decision exists, compared to judgment problems (see an overview of the related literature in Cooper and Kagel, 2005). Therefore, in strategic contexts, groups (or teams) are expected to behave more in line with the game-theoretic predictions. On the other hand, Kocher and Sutter (2007) show that when different motives in a game are at odds (for example, competition against profit maximization), group interactions will not necessarily lead to more strategic behavior. Thus, if we consider ambiguity-sensitive behavior as irrational, we could expect a shift towards neutrality in teams or after discussion with others.

In the next subsection, I review only studies in which all subjects had identical prior information about an ambiguous prospect, therefore I refer to 'uninformative others'. However, it would be interesting to test the stability of preferences to ambiguity when subjects observe others who have different private information or who have access to a different amount of information (for example, are considered experts).

Peer observation and/or concerns about their opinion

In a search for explanations for ambiguity-averse behavior, Curley *et al.* (1986) evaluated several competing hypotheses existing at that time. Only "other-evaluation", a fear of appearing incompetent due to negative emotions from being evaluated by others, was supported by the data in their experiment. The experimental design included a choice between two urns, one risky and one ambiguous, and the treatment group had to make those choices publicly, thus experience the effect of indirect evaluation. However, it is hard to estimate the extent of this effect, since when stating their preferences privately, subjects still may have some fear of being evaluated (even in anonymous tasks), and therefore may be more ambiguity-averse. Trautmann *et al.* (2008) introduced an experimental design in which they could assure students that no one could possible learn about their decisions. In their experiment subjects bet on two DVDs. In one treatment subjects could hide their real preferences for either of the DVDs, thus losing the lottery did not lead to this indirect evaluation. The results of this treatment revealed no ambiguity aversion among subjects, in contrast to usual betting when their preferences were observed. Thus, the study emphasized the importance of social factors for decisions under ambiguity.

Cooper and Rege (2011) studied how being aware of the choices of others between a risky and an ambiguous lottery could influence the choice of a given individual. Though the authors did not directly measure ambiguity preferences, they showed that the more often subjects observed disagreement between their own choices and the choices of others in an anonymous group, the higher was the probability that they switched their preferences between the lotteries. Cooper and Rage point out that their results are only consistent with a social regret explanation and not with pure imitation, tastes for conformity, or social learning. Thus, the results support the "other-evaluation" hypothesis of Curley *et al.* (1986) and Trautmann *et al.* (2008).

Groups and discussions

Keller *et al.* (2007) studied risk and ambiguity preferences in individual and dyad decisions. Decisions in dyads were performed in a face-to-face discussions, and thus had to be unanimous. The authors, despite their expectations, observed cautious shifts in dyads: joint decisions were more ambiguity-averse than individual. However, there were a number of weaknesses in the experimental design, therefore these results might be misleading. First, the tasks included only hypothetical questions, thus subjects had very low incentives to provide consistent willingness-to-pay between group and individual tasks. Second, the authors did not check for order effects; individual tasks were always in the first part of the experiment.

Brunette *et al.* (2014) report results on group shift effects under different decision rules, unanimity and majority. Groups were randomly formed of three members and there were no personal interactions. Though on average subjects were ambiguity-averse in individual tasks and within a group, the authors did not observe any significant group shift effect for ambiguity preferences. When considering group preferences by decision rule, Brunette et al. found that ambiguity aversion was significant only in groups with majority decision rule. For a unanimity decision rule, group behavior was not statistically different from ambiguity-neutral behavior.

The latter result of Brunette *et al.* is close to Charness *et al.* (2013) and Keck *et al.* (2014) who report subjects being more ambiguity-neutral in groups or after non-binding discussions with others. In a between-subject experiment, Charness *et al.* (2013) offered their subjects a choice between risky and ambiguous lotteries. The sessions varied depending on whether individual choices were made before or after non-binding discussions in person. The authors observed more ambiguity-neutral decisions after discussions. When subjects were motivated financially to persuade others, the ambiguity-neutral decisions were chosen even more frequently.

Keck *et al.* (2014) experimented with groups of three people. Similarly to Charness *et al.* (2013), group decisions and consultations were performed in person. The authors ran different orders of tasks and showed that group decisions and individual decisions after non-binding consultations were more ambiguity-neutral compared to individual decisions. Keck *et al.* (2014) explain this shift towards neutrality by two conjectures. First, group participation might decrease fear of being evaluated by others as discussed in Trautmann *et al.* (2008); however, it is not clear why ambiguity-seeking behavior was also reduced in their experiment. Second, there exists a “persuasive power of ambiguity neutrality”, thus ambiguity-sensitive behavior is less stable when confronted in a social context.

Whereas the shift towards neutrality between individual and group decisions supports Charness *et al.* (2013), Keck *et al.* (2014) did not observe any shift between individual preferences measured before group decisions compared to after group decisions. In individual sessions after non-binding consultations, subjects were more ambiguity-neutral, but in individual sessions after group decisions, subjects exhibited the same preferences to ambiguity as in individual sessions before group decisions. It is possible that in group tasks subjects with stronger leadership skills imposed their opinions without much explanation. In turn, individuals more inclined to conformism did not spend time listening to arguments and simply agreed. During the non-binding discussions, however, there was no pressure to come to a common decision, therefore everyone was just interested in the opinion of others and, perhaps, comprehended better. This explanation requires further testing.

Contrary to previous studies, Levati *et al.* (2015) did not find differences between group and individual decisions under ambiguity. In their experiment, subjects chose between two urns, one risky and one ambiguous. The decisions were made both individually and in groups of three. Decisions in groups varied by the decision rule imposed, majority or dictatorship of one or two members. On average participants exhibited ambiguity aversion when deciding individually and the vast majority of group decisions were identical to individual decisions. Among those who changed their opinion in groups, many were more ambiguity-averse, especially in the dictator treatments. The authors conjecture that fear of blame might have caused shifts towards more cautious decisions.

3.4.3 Learning

Very often real-life situations with ambiguity provide at least some opportunities for learning. Those would be rather rare exceptions, when individuals could not acquire supplementary information either through collecting it personally or addressing some knowledgeable sources. A number of theoretical papers model situations when agents differentiate between risk and ambiguity and have access to learning.[11] Notably, there are very few papers which empirically study questions around this topic. In the context of my paper, I investigate how learning might influence preferences to ambiguity.

It might seem logical that more information in the end will necessitate convergence to a risky situation with known probabilities and, thus, subjects with ambiguity-neutral preferences. As Zimper (2011) theoretically shows, this might not be the case when we deal with non-Bayesian learners. Under the usual assumption of Bayesian updating and additive probabilities, an individual's estimates of subjective probabilities eventually will converge upon or be around true probabilities. However, in a framework of non-additive probabilities, Zimper shows that updating rules[12] exist ,which will keep the decision maker in the situation of ambiguity, therefore enable the existence of ambiguity-sensitive preferences.

[11] For example, see Chateauneuf *et al.* (2007); Epstein and Schneider (2007); Epstein *et al.* (2010); Eichberger *et al.* (2007); Hanany and Klibanoff (2009), for a review see Machina and Siniscalchi (2014)

[12] Specifically, so-called *full Bayesian rules* by Pires (2002); Eichberger *et al.* (2007); Siniscalchi (2011), the optimistic and the pessimistic by Gilboa and Schmeidler (1993)

In an unpublished manuscript, Baillon *et al.* (2015) empirically investigate the effect of learning on ambiguity attitudes. The authors ran a lab experiment measuring prices of options depending on four real stocks which had recently entered the New York Stock Exchange. Baillon et al. elicit attitudes to ambiguity through two separate measures, pessimism (actual attitudes to ambiguity) and likelihood insensitivity (attitudes to extreme probabilities). The results revealed a significant decrease in likelihood insensitivity with more information and no impact of new information on pessimism. However, we cannot conclude that there was no relationship between learning and attitudes to ambiguity because the authors report very low levels of pessimism on average. Since the subjects in the study were majors in finance, it is possible that a feeling of competence diminished the feeling of ambiguity. It would be interesting to see the impact of competence on the results and whether both measurements maintain an unchanged relationship with learning for subjects who have low competence in the source of ambiguity.

When studying how sampling experience impacts preferences for ambiguity, Ert and Trautmann (2014) report evidence inconsistent with the results of Baillon *et al.* (2015). Ert and Trautmann (2014) divided subjects into two groups. Both groups chose between a risky lottery with different probabilities of success (from low to high) and an ambiguous lottery without any information about the underlying probability distribution (the true distribution for both lotteries was the same in every choice). But while the control group was just reporting their choices, the treatment group was given a possibility to sample (as long as they wished) the ambiguous lottery before making each choice. The subjects in the treatment group exhibited preferences reversal for low and high probabilities and, overall, sampling increased preferences to ambiguity. In an additional session, Ert and Trautmann (2014) rejected the hypothesis that these results were driven by too extreme beliefs updating after sampling. The authors proposed two explanations. The first explanation connected a positive relationship between sampling and ambiguity-attitudes to the effect of biases in processing sampled data. The second suggested the "motivational change": sampling might have induced too optimistic weighting of probabilities instead of the usual more pessimistic weighting.

The study by Ert and Trautmann (2014) is related to literature in psychology on differences in decision outcomes when probabilities are described versus experienced (for an overview see Rakow and Newell, 2010). This literature reports a similar fourfold pattern for decisions from experience – risk-aversion in gain domain and risk-seeking in loss-domain – as does economic literature for attitudes to ambiguity.

Two recent manuscripts by Frechette *et al.* (2014) and Bergheim (2014) investigated other factors which impact decisions under ambiguity through learning. Specifically, they analyzed which intrinsic characteristics may have induced subjects to engage in learning when the level of ambiguity was endogenized and could be decreased by subjects. Both studies report a significant effect of personality characteristics on information gathering which, possibly, could influence attitudes to ambiguity.

3.4.4 Markets

The financial literature uses ambiguity-sensitive behavior to explain many existing puzzles and paradoxes (see recent studies reviewed in Guidolin and Rinaldi, 2013); however, there is no unequivocal answer in ambiguity literature whether ambiguity-averse or ambiguity-seeking behavior can appear and survive in a market setting. Efficient markets have to eliminate any noise and behavioral inconsistencies, including any ambiguity-sensitive behavior (Fama, 1970). Whereas Camerer and Kunreuther (1989), Corgnet *et al.* (2013), Di Mauro and Maffioletti (2004), and Kocher and Trautmann (2013) found very limited or no presence of ambiguity aversion in experimental markets, Bossaerts *et al.* (2010), Di Mauro (2008), and Sarin and Weber (1993) reported that ambiguity aversion can be observed in competitive markets and can influence market outcomes.[13]

Füllbrunn *et al.* (2014) tested how intraperiod market feedback and the winning probability of the ambiguous asset can affect prices in experimental asset markets. Their most important conclusion says that ambiguity aversion stays in the markets only if market feedback is limited (call market) and there is a significant number of ambiguity-averse participants. The authors noted that those studies which reported ambiguity aversion were not allowing short sales constraints, thus they questioned to what extent short sales may influence ambiguity-sensitive behavior in the markets.

Huber *et al.* (2014) showed that notwithstanding the structure of the market, if the ambiguous assets started trading with a negative skew (low probability for a lower outcome), the market did not manage to correct the prices. Thus, ambiguous assets stayed underpriced. However, in treatments with a positive skew in prices (high probability for a lower outcome) and in treatments with probabilities learned by sampling, the assets were priced efficiently.

[13] Füllbrunn *et al.* (2014) provide a concise overview of these papers in the experimental background to their paper (p. 3).

3.5 Discussion and research directions

In this chapter, I have analyzed how different factors can impact behavior under ambiguity in static and dynamic contexts. Whereas some determinants under static contexts (such as risk and competence) exhibit a significant relationship to attitudes to ambiguity, we do not observe any robust relationships under dynamic contexts. Even though there are a number of papers studying markets and social interactions, the majority report either shifts towards neutrality (Charness *et al.*, 2013; Keck *et al.*, 2014; Trautmann *et al.*, 2008) or very limited conditions when attitudes to ambiguity survive (Füllbrunn *et al.*, 2014). If we aim to predict behavior under uncertainty, we need to understand better how dynamic characteristics influence consistency of ambiguity preferences. I identify several directions which could potentially contribute to such an understanding:

1) Intertemporal stability
How is consistency related to understanding of tasks and background characteristics? It is possible that the low consistency results in the study by Duersch *et al.* (2013) was caused by subjects who used some kind of randomization strategy. For example, they could have answered differently even within one session, and believed that this way they maximized their payoffs.

2) Peer effects
Do low/high expected (matching) probabilities have any effect on shifts in preferences to ambiguity? Under Prospect Theory, people are more sensitive to extreme probabilities, thus, they might be less sensitive to observations of others.

3) Learning
To date, there are two papers providing opposite evidence on the effect of learning on preferences to ambiguity. Does competence have any impact on the different results reached by Ert and Trautmann (2014) and Baillon *et al.* (2015)? Does the relationship between learning and ambiguity preferences work in the other direction: Would ambiguity-sensitive preferences impact learning? Trautmann and Zeckhauser (2013) showed that a significant number of experimental subjects did not use learning opportunities in choice under ambiguity. When offered two lotteries, one risky and one ambiguous with the possibility to learn through two repetitive draws, the majority preferred the former. Thus, on average they lost

the chance to increase their probabilities of success. Liu and Colman (2009) provided their subjects with a similar task but without the possibility to learn from an ambiguous urn (the urn was reset before every draw) and reported opposite results. Their subjects completed two tasks, a choice between a single bet on a risky or an ambiguous urn and a choice between repetitive bets from a risky or an ambiguous urn. A significantly larger number of subjects preferred the repeated bet on an ambiguous urn compared to a single bet. Liu and Colman (2009) explain their findings by loss-aversion of their subjects and by their beliefs that uncertainty was eliminated in the repeated choice, even though there was no possibility to learn. It is possible that subjects in Trautmann and Zeckhauser's experiment did not choose the ambiguous lottery because they had a feeling that to learn probability one needs a larger sample, thus could be averted by the limited number of draws (only two) and not by the ambiguous lottery itself.

Anderson (2012) reports two seemingly challenging notions from a multi-armed bandit experiment. Ambiguity-averse agents appeared to experiment less, and at the same time, they were willing to pay more than ambiguity-neutral subjects to get the exact payoff probability distribution of the ambiguous arm. Qiu and Weitzel (2015) and Moreno and Rosokha (2015) study the learning process uncer ambiguity. While Qiu and Weitzel (2015) did not observe strong deviations from Bayesian updating, Moreno and Rosokha (2015) found that in ambiguous contexts subjects significantly overweighted new signal. Given the importance of a learning context, more experimental evidence is definitely needed in this area.

Overall, there are notably fewer studies investigating behavior under ambiguity related to dynamic factors, therefore the evidence is very scarce. Historically, experimentalists have mainly focused on replicating Ellsberg's original experiment under static conditions. As Trautmann and van de Kuilen (2014) note, this happened probably due to its straightforward design implementation. The dynamic contexts, though more complicated to design in experiments, are more relevant to decisions in the 'wild'. Therefore, investigating behavior under ambiguity in dynamic contexts could provide additional perspectives on external validity.

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Tables

Table 3.2: Attitudes to ambiguity and intrinsic characteristics[a]

1. Preferences	
risk preferences	Abdellaoui *et al.* (2011) [RA+]; Akay *et al.* (2012) [RA–]; Bossaerts *et al.* (2010) [RA+]; Brown *et al.* (2010) [N]; Butler *et al.* (2013) [RA+]; Chakravarty and Roy (2008) [RA+ gains]; Charness and Gneezy (2010) [RA+]; Chew *et al.* (2013a) [RA+]; Cohen *et al.* (1987) [N]; Cohen *et al.* (2011) [RA+, driven by extreme subjects; N, without extreme subjects]; Di Mauro and Maffioletti (2004) [N]; Dimmock *et al.* (2015a) [RA+]; Dimmock *et al.* (2016) [RA+]; Dimmock *et al.* (2015b) [RA–]; Kocher and Trautmann (2013) [RA+]; Lauriola and Levin (2001) [RA+ losses]; Lauriola *et al.* (2007) [RA+]; Levy *et al.* (2010) [N]; Mukerji *et al.* (2014) [RA–]; Potamites and Zhang (2012) [RA+ weak]; Qiu and Weitzel (2012) [RA+]; Sutter *et al.* (2013) [RA–]
time preferences	Chesson and Viscusi (2003) [uncertain timing of outcome, +]; Cohen *et al.* (2011) [N]; Sutter *et al.* (2013) [N]
2. Reduction of compound risk	
	Halevy (2007) [AN]; Abdellaoui *et al.* (2015) [AN*]
3. Demographic characteristics	
biological	***age***: Butler *et al.* (2013) [N]; Cardenas and Carpenter (2013) [+]; Dimmock *et al.* (2015a) [–]; Dimmock *et al.* (2016) [–]; Dimmock *et al.* (2015b) [N]; Tymula *et al.* (2012) [+] ***gender (male)***: Borghans *et al.* (2009) [N/+]; Butler *et al.* (2013) [N]; Cardenas and Carpenter (2013) [+]; Dimmock *et al.* (2015a) [+]; Dimmock *et al.* (2016) [+]; Dimmock *et al.* (2015b) [N]; Sutter *et al.* (2013) [N]; ***health***: Akay *et al.* (2012) [–]; Dimmock *et al.* (2016) [N]; ***neuro/genetic***: Chew *et al.* (2012); Carpenter *et al.* (2011); Dickhaut *et al.* (2013); Rustichini *et al.* (2005); Smith *et al.* (2002)
cognitive	***IQ***: Borghans *et al.* (2009)[N]; Sutter *et al.* (2013) [+/–]
personality	***dealing with probabilities***: Dominiak and Schnedler (2011) [randomization, N] ***intuition***: Butler *et al.* (2013) [–]; ***mood (affective state)***: Baillon *et al.* (2014) [sadness/bad weather, AN]; Feinberg and Yesuf (2013) [self-reported level of happiness in life, N]; Pulford and Gill (2014) ["feeling lucky", N] ***non-cognitive traits***: Borghans *et al.* (2009)[N]; Feinberg and Yesuf (2013) [optimism, –]; Pulford (2009) [optimism, –]
socio-economic	***local heritage***: Akay *et al.* (2012) [Ethiopia, N]; Cardenas and Carpenter (2013) [Latin America, N]; ***wealth***: Akay *et al.* (2012) [Ethiopia, N]; Butler *et al.* (2013) [+]; Cardenas and Carpenter (2013) [economic well-being, –]; Dimmock *et al.* (2016) [N]; Dimmock *et al.* (2015b) [N]
trust	Corcos *et al.* (2012) [–]; Feinberg and Yesuf (2013) [–]
4. Competence	
	Cabantous (2007) [N]; de Lara Resende and Wu (2010) [gains, –; losses, N]; Di Mauro (2008) [self-assessed, –]; Heath and Tversky (1991) [–]; Hogarth and Grieco (2004) [–]; Holm *et al.* (2013) [N]; Keppe and Weber (1995) [self-assessed, –]; Klein *et al.* (2010) [–]; Koudstaal *et al.* (2015) [N]

[a] [+]/[−]/[+/−] = positive/negative/depends on specific characteristic relationship with ambiguity aversion;
[RA+]/[RA−] = positive/negative relationship between risk-aversion and ambiguity aversion;
N = Non-significant relationship; AN = more ambiguity-neutral;
AN* = more ambiguity-neutral, though not robust to type of compound risk and/or background characteristics

Table 3.3: Robustness of attitudes to ambiguity in dynamic contexts[a]

1. Intertemporal stability	
	Duersch *et al.* (2013) [NO]; Hey and Pace (2015) [YES]
2. Social interactions with uninformative others	
peers	Curley *et al.* (1986) [NO]; Muthukrishnan *et al.* (2009) [NO/YES]; Trautmann *et al.* (2008) [NO]
groups	Brunette *et al.* (2014) [YES/NO]; Charness *et al.* (2013) [NO]; Keck *et al.* (2014) [NO]; Keller *et al.* (2007) [YES]
3. Learning	
	Baillon *et al.* (2015) [YES]; Ert and Trautmann (2014) [NO]
4. Markets	
	Bossaerts *et al.* (2010) [YES]; Camerer and Kunreuther (1989) [NO]; Corgnet *et al.* (2013) [NO]; Di Mauro and Maffioletti (2004) [NO]; Di Mauro (2008) [YES]; Füllbrunn *et al.* (2014) [NO/YES]; Huber *et al.* (2014) [YES/NO]; Kocher and Trautmann (2013) [NO]; Sarin and Weber (1993) [YES]

[a]YES = consistent under dynamic context;
NO = not consistent under dynamic context;
YES/NO = consistency depends on specific conditions

Table 3.4: Alphabetical list of selected papers, by groups

Group	Papers
1 [static]	Abdellaoui *et al.* (2011); Abdellaoui *et al.* (2015); Akay *et al.* (2012); Baillon *et al.* (2014); Borghans *et al.* (2009); Bossaerts *et al.* (2010); Brown *et al.* (2010); Butler *et al.* (2013); Cabantous (2007); Cardenas and Carpenter (2013); Carpenter *et al.* (2011); Chakravarty and Roy (2008); Charness and Gneezy (2010); Chesson and Viscusi (2003); Chew *et al.* (2012); Chew *et al.* (2013a); Cohen *et al.* (1987); Cohen *et al.* (2011); Corcos *et al.* (2012); de Lara Resende and Wu (2010); Di Mauro (2008); Di Mauro and Maffioletti (2004); Dickhaut *et al.* (2013); Dimmock *et al.* (2015a); Dimmock *et al.* (2016); Dimmock *et al.* (2015b); Dominiak and Schnedler (2011); Feinberg and Yesuf (2013); Halevy (2007); Heath and Tversky (1991); Hogarth and Grieco (2004); Holm *et al.* (2013); Keppe and Weber (1995); Klein *et al.* (2010); Kocher and Trautmann (2013); Koudstaal *et al.* (2015); Lauriola and Levin (2001); Lauriola *et al.* (2007); Levy *et al.* (2010); Mukerji *et al.* (2014); Potamites and Zhang (2012); Pulford (2009); Pulford and Gill (2014); Qiu and Weitzel (2012); Rustichini *et al.* (2005); Smith *et al.* (2002); Sutter *et al.* (2013); Tymula *et al.* (2012);
2 [dynamic]	Baillon *et al.* (2015); Bossaerts *et al.* (2010); Brunette *et al.* (2014); Camerer and Kunreuther (1989); Charness *et al.* (2013); Corgnet *et al.* (2013); Curley *et al.* (1986); Di Mauro (2008); Di Mauro and Maffioletti (2004); Duersch *et al.* (2013); Ert and Trautmann (2014); Füllbrunn *et al.* (2014); Hey and Pace (2015); Huber *et al.* (2014); Keck *et al.* (2014); Keller *et al.* (2007); Kocher and Trautmann (2013); Muthukrishnan *et al.* (2009); Sarin and Weber (1993); Trautmann *et al.* (2008).

Appendix 1: Selection of references

The references for this review have been collected up to April 2015. All studies were retrieved from three sources:

- *Scopus* reference database returned 650 results for a search line: "TITLE-ABS-KEY(ambig* AND avers*) OR TITLE-ABS-KEY(ambig* AND attitude*) OR TITLE-ABS-KEY(ambig* AND experiment*) AND SUBJAREA(ECON)";
- *EBSCO EconLit with full text* database returned 632 results for a search line: "ambiguity AND (averse OR aversion OR attitude OR experiment)";
- 129 references were taken from Trautmann and van de Kuilen (2014).

The procedure for final selection was the following:

1. I merged all references into one database (available upon request);
2. I eliminated earlier versions of published papers;
3. Based on the abstract, I eliminated all unrelated and/or non-experimental papers;
4. The remaining 124 studies were classified into 3 groups: *Group 0* = to be possibly used in the discussion, *static* = related to static contexts, *dynamic* = related to dynamic contexts. Four studies were classified into both groups, for example, Bossaerts *et al.* (2010) studied the impact of ambiguity on equilibrium in financial markets and measured the correlation between attitudes to risk and ambiguity;
5. Finally, each group was further divided into subgroups (e.g. 1.1 = related to risk and time preferences, see Table 3.5). Some studies were classified into complimentary subgroups to be possibly used in the discussion (e.g. 1.1* = studies, in which authors do not directly relate risk/time preferences to preferences towards ambiguity but which could be used in the discussion in the corresponding subsection). In total, 64 of 124 studies were classified as related to static/dynamic contexts (listed in Tables 3.2, 3.3, and 3.4), 24 of 124 studies were classified as complimentary, and 36 of 124 as *Group 0*. I mention in the text 19 of 24 complimentary studies and 11 of 36 *Group 0* studies.

Additionally, I am aware of several related studies (either unpublished or published in non-economic journals, see Table 3.6), which did not appear in the search results from the three sources mentioned above. I did not include these studies in the final tables on static and dynamic contexts (Table 3.2 and Table 3.3) but I refer to some of them in the discussion.

Table 3.5: Groups of references

Group	Description	Number of studies
1.1	Risk and time preferences	23
1.2	Reduction of compound risk	2
1.3	Demographic characteristics	20
1.4	Competence	9
2.1	Intertemporal stability	2
2.2	Social interactions	7
2.3	Learning	2
2.4	Markets	9

Table 3.6: Other related papers

Study	Group to be
Burks *et al.* (2009)	1.1
Chew *et al.* (2013b)	1.3
Dean and Ortoleva (2015)	1.1, 1.2, 1.3
Henrich and McElreath (2002)	1.3
Huettel *et al.* (2006)	1.3
Lahno (2014)	2.2
Levati *et al.* (2015)	2.2
Li (2015)	1.3
Prokosheva (2015)	1.2
Rustichini *et al.* (2012)	1.3
Shyti (2013)	1.3