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Decision-making under uncertainty: contributions to elicitation methods with real-world applications

Chris Ouangraoua

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THÈSE DE DOCTORAT

Prise de décision en situation d'incertitude : Contributions aux méthodes d'élicitation avec applications en contextes réels

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Groupe de Recherche en Droit, Economie et Gestion

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du grade de docteur en Sciences
économiques
d'Université Côte d'Azur**

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Prise de décision en situation d'incertitude : Contributions aux méthodes d'élicitation avec applications en contextes réels

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Prise de décision en situation d'incertitude : Contributions aux méthodes d'élicitation avec applications en contextes réels

Résumé

L'incertitude est un concept central dans l'étude de la prise de décision économique. L'ambiguïté est une notion sous-jacente de l'incertitude définie comme la perception subjective de l'absence d'information sur les probabilités des événements. Dans cette thèse, nous avons cherché à comprendre comment les attitudes individuelles face à l'incertitude influencent les décisions dans des contextes réels.

Pour atteindre cet objectif, nous nous sommes livrés à deux exercices. D'abord, nous avons étudié l'impact des méthodes expérimentales d'élicitation sur les mesures des attitudes envers l'ambiguïté. Ensuite, nous avons examiné comment ces mesures prédisent les comportements des individus confrontés à des problèmes de décision en situations réelles. Nous nous sommes particulièrement concentrés d'une part sur l'adoption de technologies dans le contexte de l'assurance automobile et d'autre part sur les comportements délétères.

Cette thèse contient trois chapitres. Le premier chapitre examine les relations entre les attitudes face au risque et à l'ambiguïté. Nous avons effectué une méta-analyse sur cinquante-cinq études expérimentales pour lesquelles nous avons analysé l'influence de leurs caractéristiques respectives sur le signe de la corrélation entre les deux attitudes. Nous avons montré que lorsque les études ont des échantillons de petite taille, il est plus probable d'observer une corrélation nulle qu'une corrélation

positive. Dans le deuxième chapitre, nous nous sommes concentrés sur le contexte de l'assurance automobile. Nous avons expérimentalement étudié le rôle des attitudes face à l'ambiguïté dans le choix entre une procédure de résolution des litiges basée sur le jugement humain et une procédure automatisée. Nos résultats révèlent que l'expérience automobile des utilisateurs façonne leur perception de l'incertitude, influençant leurs préférences pour les différentes procédures. Dans le troisième chapitre, nous avons proposé d'explorer la validité interne et externe de trois mesures des attitudes face à l'ambiguïté. En particulier, nous proposons d'examiner leur cohérence, leur stabilité et leur relation avec des mesures de quatre comportements délétères que sont l'alcoolisme, le tabagisme, les jeux d'argent et l'utilisation problématique des smartphones.

Mots clés : Prise de décision en situation d'incertitude, Attitudes face à l'ambiguïté, Méthodes d'élicitation, Économie expérimentale

Decision-Making under Uncertainty: Contributions to Elicitation Methods with Real-World Applications

Abstract

Uncertainty is a central concept in economic decision-making analysis. Ambiguity is an underlying notion of uncertainty referring to the subjective perception of missing information about the probabilities of events.

In this thesis, we aimed to contribute to understanding how individual attitudes towards uncertainty influence decisions in real-world contexts. To achieve this objective, we conducted two investigations. First, we explored how experimental elicitation methods impact the measurements of ambiguity attitudes. Second, we examined how these measurements predict individuals' behavior when facing decision problems in real-world situations. We specifically focused on technology adoption in the car insurance context on the one hand and on harmful behaviors on the other hand.

This thesis contains three chapters. The first chapter scrutinizes the relationships between risk and ambiguity attitudes. We conducted a meta-analysis on fifty-five experimental studies for which we analyzed the influence of their respective features on the sign of the correlation between risk and ambiguity attitudes. We provided evidence that when the studies have small sample sizes, it is more probable to observe a zero correlation over a positive one.

In the second chapter, we focused on the context of car insurance. We exper-

imentally investigated the role of ambiguity attitudes in choosing between a dispute resolution procedure driven by human judgment versus an automated one. Our results revealed that users' car experience shapes their perception of uncertainty, influencing their preferences for the procedures.

In the third chapter, we proposed to explore the internal and external validity of three ambiguity attitudes measurement methods. We specifically proposed to examine their consistency, stability, and relationship with measures of four harmful behaviors that are alcoholism, smoking, gambling, and problematic smartphone use.

Keywords : Decision-making under uncertainty, Ambiguity attitudes, Elicitation methods, Experimental economics

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There is an unresolved ambiguity about the real purpose of this thesis. Indeed, it is not clear to me whether it was about economics or about myself. On reflection, this thesis could also have been a way of knowing myself better. The method would be the guidance of my supervisors and the support of my loved ones, friends, and colleagues. The result? Another version of myself freed of a few flaws and filled with new dreams. A finding that life will test the robustness of. So, even if these future lines of acknowledgment seem somewhat cliché, or if I forget someone (inevitably, there will be), please bear in mind that my gratefulness continues beyond these words. I want to express my sincere and deep gratitude:

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General Introduction

Uncertainty in economics

Economic life is inherently intertwined with *uncertainty*, a concept emphasized by eminent economists Knight and Keynes. As philosophers and economists, they convincingly demonstrated that economic phenomena cannot be predicted in mechanical terms due to an intrinsic characteristic making them non-deterministic. Their message states that the existence of uncertainty imposes a rule: any simplification is dangerous. Hence, there is no magic tool for grasping the full complexity of an uncertain world, and economic agents should not be given divine clairvoyance. In contrast, until the 1960s, neoclassical economists were slower to realize the profound implications of allowing uncertainty into their reasoning. For example, by removing the assumption of perfect information, whether concerning prices, quality, or behavior, theorists opened the Pandora's box of neoclassical analysis and questioned the notion that the market equilibrates naturally (Stiglitz, 2002). For instance, what are the consequences when consumers lack complete price information? George Stigler (1961) claimed that the spread of prices on a market is partially due to differences in offered services but also to the diverse costs of information-seeking that consumers face (the search). However, estimating these costs is challenging due to an infinite regression process. This shows that the traditional concept of equilibrium, which relies on market coordination through the price system, was no longer evident. The uncertainty about prices is amplified when there is also information asymmetry information about the quality of a good,

which can lead the market to collapse entirely as demonstrated by Akerlof (1978) on the used car market. Additionally, individual conduct in contractual relationships can be uncertain. Can coordination be achieved even when parties exhibit unpredictable behavior? An important issue arises as opportunism assumptions cannot be confirmed or disproven, ultimately rendering complete contract design ineffective (Arrow, 1978).

Uncertainty has clearly become a critical challenge to the economic analysis of competitive markets, disrupting the mechanics of prices and competition in search of the best equilibrium. The proposed solutions to address the provoked disorders include information acquisition strategies and excessively costly contractual or incentive procedures (Riley, 2001).

However, it is important to note that the neoclassical approach to uncertainty was cautious. It was only introduced as a sophistication of their models under certainty through the inclusion of known probability distributions on world events and futures markets. Thus, the lack of information borne by individuals does not necessarily mean total ignorance. The uninformed agents often know the probability distribution of the unknown variable, putting them in a situation of risk. Moreover, decision-makers are often attributed with exceptional cognitive abilities, allowing them to perfectly compensate their lack of information and calculate and specify parameter values to optimize utility. In the end, the weakened neoclassical analysis took refuge in a form of uncertainty that can always be characterized and calculated. Nevertheless, the rationalist cocoon that economists have tried to build has not resisted a deeper analysis of economic agents' complex psychological decision-making processes.

Decision-making under uncertainty

Uncertainty takes multiple forms and suggests numerous interpretations. For example, economists agree that the uncertainty inherent in a roll of a dice, the suc-

cess of a new technology, and even the ignorance of a deterministic fact (such as the thousandth decimal of pi) are different notions. Nevertheless, how individuals resolve these uncertainties is still a matter of debate. For example, is it always possible to rationalize decisions in these situations? The multiplicity of answers to this question makes decision-making under uncertainty one of the most inconclusive research programs in economics.

The dominant approach in this domain has been the Expected Utility Theory, proposed in response to the Saint Petersburg paradox of Daniel Bernoulli. The theory allows individuals to make choices based on known or unknown probabilities. If these probabilities are known, decisions are considered risky and can be modeled using the expected utility model from John von Neumann et Oskar Morgenstern (Von Neumann & Morgenstern, 1944). When probabilities are unknown, decision-making relies on subjective probabilities, modeled by Leonard Savage's subjective utility expectation (L. Savage, 1954). By maximizing utility expectation, agents can make optimal decisions. Yet, expected utility models' validity has been widely criticized and proved insufficient as a descriptive framework, with experiments showing that it may not accurately reflect actual human decision-making processes. Such discrepancies are known as experimental paradoxes, where theoretical predictions are incompatible with real-life results despite the internal consistency of models and adequate testing. A notorious paradox in risk situations is that of Allais (Allais, 1953) which challenges the independence axiom from Von Neumann and Morgenstern (1944) (presumed invariance of preferences for two lotteries following linear combination with a third). Economists have two options when faced with paradoxes: the normative approach which blames agents' irrational decision-making, or the descriptive approach which questions the model's predictive power and calls for an overhaul of its axioms. The latter is currently dominant in the expected utility model's revision.

An underlying notion of uncertainty conceptualized by Ellsberg (1961), "ambiguity," has been the focus of considerable theoretical and empirical development

in recent decades. Ironically, the concept of ambiguity does not fall under a single definition, and the attitudes toward it do not have a clear interpretation. Frisch and Baron (1988) define it as the subjective perception generated by a lack of relevant information when this information could be known. This definition includes all manifestations of missing information, including novelty, complexity, or vagueness. Ilut and Schneider (2022) simply asserts that "ambiguity refers to uncertainty when the odds are not known." Abdellaoui, Baillon, Placido, and Wakker (2011) explain that "Ambiguity reflects what uncertainty comprises beyond risk [...]", concerning "[...] the differences between decisions and beliefs for unknown probabilities versus those for known probabilities." This raises the question of how agents perceive ambiguity and behave when facing it. Thus, ambiguity aversion refers to agents' preference for probable and precise over probable but imprecise information. Ambiguity seeking and neutrality are defined accordingly.

Ellsberg proposed two thought experiments to observe ambiguity aversion: the "three-color Ellsberg paradox" and the "two-urn Ellsberg paradox." With these examples, he highlights the complex nature of decisions under ambiguity, which may no longer obey probabilistic logic and even violate basic principles of rationality. However, a retrospective look is necessary to fully capture the essence of Ellsberg's conception and how it later influenced the literature.

A historical perspective of the concept of ambiguity

The philosophical exploration of pioneering economists in the 1920s on uncertainty reveals numerous similarities. They collectively rejected the assumption that agents possess omniscience. Instead, they emphasize individuals' challenges regarding knowledge, prediction, complexity, and novelty. Interestingly, how the other economists will later tackle uncertainty-related issues reveals a significant schism in their differing notions of rationality.

Knight (1921)'s approach remains a leading reference in the literature. A com-

mon misconception is attributing to him the conceptual distinction between "risk" and "uncertainty" to designate probabilistic and nonprobabilistic beliefs, respectively (LeRoy & Singell Jr, 1987). However, Knight used "risk" to describe situations in which probabilities could be deduced or determined through empirical frequencies, whereas "uncertainty" referred to situations where there was no basis for objective probability measurement. Hence, under "Knightian uncertainty," neither "a priori probabilities" nor "statistical probabilities" can constitute a rationale for decision-making. It refers to unique situations that cannot be reduced to a group of similar cases, making them non-probabilizable. When faced with these singular situations, which are unprecedented, complex, or poorly structured, prediction cannot solely rely on probabilistic models. Forecasts in uncertain situations are based on personal judgment (estimates) and the individuals' confidence in their judgment, known as epistemic probability. It also requires a balance for decision-makers between simplifying and referring to general categories to enable objective prediction and grasping singularity by perceiving new traits or privileging complexity. Knight was also the first to introduce the term "subjective uncertainty." During the same year, John Maynard Keynes added further complexity to Knight's dichotomous conception.

Keynesian uncertainty is conceptually close to Knight's, to the point that economists often refer to it as Knight-Keynes uncertainty (Hodgson, 2011). They differ, however, in their starting point. Keynes (1921) begins from the concept of probability, defining it as the "rational degree of belief" that should be attached to a proposition, given a set of premises. Thus, he did not view the notion of "degree of belief" as subjective or personal but as a rational means to quantify the probability of an event. Indeed, decision-makers can weigh the consequences of different alternatives and decide to best serve their interests. In Keynes's vision, probabilities ranged from impossibility to certainty, and specific subsets of probabilities formed an "ordered series" of mutually comparable elements. Therefore, Keynes recognized that some probabilities may take on numerical values in a limited set

of cases, largely dependent on evidence that supports the calculation. Indeed, as they require knowledge of all factors for accurate determination of relative importance, Keynes observed that scholars overestimate the significance of this type of probabilities, likely due to its compatibility with mathematical manipulability. The second set of probabilities may not be numerically measurable and only be ranked in order of magnitude. Finally, the last type of probabilities can neither be ranked ordinally nor cardinally. This uncertainty *à la* Keynes emerges from the conflict between different bodies of knowledge, leading to the inability to rank potential arguments. This conception conceals a philosophical subtlety that the source of uncertainty lies not in the future's unpredictability but rather in the complexity of the present. Keynes provided one of the first attempts at a characterization of non-probabilistic beliefs, allowing belief structures that cannot be expressed numerically.

Continuing the work of Keynes, another notable later contribution to non-probabilistic beliefs was by George Shackle (1949). Shackle's theory presents a distinct approach to uncertainty. The central concept is "potential surprise," a measure reflecting the level of surprise expected upon learning of a particular event's occurrence or a hypothesis's truth. This concept differs from standard probability, as the degree of potential surprise depends on the relative likelihoods of known alternatives and the set of alternatives that might occur. To illustrate this distinction, Shackle presents the example of four equally qualified candidates for a job appointment. A probabilistic representation of such a situation would assign each candidate a probability of $1/4$ and view any candidate's appointment as equally likely. However, this "symmetric uncertainty" does not produce potential surprise and cannot capture the full complexity of decision-making under uncertainty. Instead, Shackle proposes measuring attitudes towards uncertainty using a concept of "acceptance," which allows individuals to assign degrees of acceptance to new rival hypotheses that were not initially considered. This theory also departs from the traditional additive expected value/expected utility approach. Instead, when

evaluating alternative courses of action, an individual must reduce the array of hypotheses about the relevant consequences of each action to some compact and vivid statement. To do so, the individual determines the degree to which each possible gain in action can "stimulate him agreeably." This stimulation level is an increasing function of the value of the gain and a decreasing function of its potential surprise. However, the power of mutually exclusive hypotheses of success to afford enjoyment by imagination is not additive, and the entire positive stimulation of an action is defined by its most stimulating possible gain. Shackle's work is a pioneering effort to develop a new mathematical framework of decision-making under uncertainty although several authors criticized some of its assumptions as unrealistic.

Moreover, among the authors after Keynes (1921), the literature has probably retained more of the contributions of Frank Ramsey, who played a critical role in the development of probabilistically sophisticated beliefs. Like Keynes, Ramsey (1926) affected a certain consistency of reasoning on beliefs but shows that it can be measured, leading to the computational decision theory under uncertainty. Frank Ramsey (1926) established a set of assumptions on choice behavior, which implied the existence of a classical probability measure over events. Unlike prior attempts to measure probabilistic beliefs using betting odds, Ramsey did not assume actual risk neutrality. Instead, he attributed the Bernoullian principle of expected utility maximization to agents and worked directly with the utilities or values of various outcomes. Ramsey defined the degree of belief in a manner that presupposed the use of mathematical expectation. While this approach was noteworthy for its pioneering efforts in measuring subjective probabilities, L. Savage (1954) criticized that Ramsey imposed the property of expected utility maximization on agents rather than jointly axiomatizing it.

L. Savage (1954)'s book is a remarkable decision theory achievement. It provides the classic representation of preferences through subjective expected utility, based on the concept of subjective probability advocated by Ramsey (1926) and

De Finetti (1937) and the idea of expected utility derived by Von Neumann and Morgenstern (1944). Contrary to Ramsey, Savage imposed reasonable restrictions in the form of "axioms" and derived tractable representations from them. Unfortunately, despite its elegance, his approach is not without its share of problems. His main conclusion suggests that decision-maker behavior can be modeled as the maximization of subjective expected utility, with the only requirement being that a probability distribution represents beliefs. This allows for including strange or unreasonable beliefs as long as they are probabilistic. One could argue that these strange beliefs may converge to "true probabilities" through learning and Bayesian updating. However, this does not resolve the problem as learning can take considerable time, and Bayes' law is silent when conditioning on zero probability events. Therefore, a more refined understanding of belief formation among individuals would undoubtedly yield valuable insights, especially in scenarios devoid of frequentist evidence but still possessing relevant information. This challenge may be particularly daunting for emerging risks, where data might be scarce. Consider the "emerging risks" associated with climate change or new technologies like blockchain. At one extreme, if no information is available about the occurrence of a new risk, decision-makers still need to construct a probability distribution. In this case, their behavior would be indistinguishable from their behavior when facing a well-documented risk with the same distribution. Still, this approach was unsatisfying.

Daniel Ellsberg (1961), a Ph.D. student of Savage, questioned the subjective expected utility model and the representation of information in terms of probability. He introduced two thought experiments to show that this representation becomes distorted in specific contexts. Simply summarized, the agents were presented with the choice of betting on an urn containing a number of balls with a known color distribution (a risky urn) or betting on an urn with an unknown proportion of balls (an ambiguous urn). Most agents preferred to bet on the former, revealing ambiguity aversion. A *prima facie* analysis of the decision problem suggests that

individuals cannot be assumed to act according to a probability distribution, regardless of the decision criterion involved. Hence, the nature of the information available for formulating probabilities under uncertainty affects individuals' confidence in their judgment and, consequently, their preferences. This highlights the role of confidence in judgment discussed by Knight (1921). The intuition emanating from Ellsberg's paradox was a direct provocation to neoclassical economists, who circumvented the problem of the absence of objective probabilities by simply attributing subjective probabilities to agents as the basis for all calculations. This may explain decision theorists' mixed initial reactions to the Ellsberg paradox. Ellsberg's summarized diverse perspectives on these matters unfolded through discussions. For example, he explained that Dalkey and Jacob Marschak admitted to violating the rational axioms. Other researchers, like Howard Raiffa, first violated the axioms but felt guilty and reconsidered analysis immediately afterward. Paul Samuelson, Gerard Debreu, and Robert Schlaifer adhered strictly to the rational axioms over relying on intuition or resorted to the Principle of Insufficient Reason. Despite its initial timid acceptance, ambiguity attitudes have been subject of considerable subsequent development. After Ellsberg (1961), economists and psychologists have conducted numerous experimental studies that have presented the thought experiments differently to observe ambiguity attitudes from different angles. Anecdotally, John Chipman conducted the earliest known experiments of this nature in 1958 and 1960 (Ellsberg cites Chipman (1958, 1960)'s "nearly identical experiment" on the Two-Color Paradox).

Theoretical developments have also provided interesting insights into the very definition of ambiguous events. The Maxmin Expected Utility model (Gilboa & Schmeidler, 1989) considers that individuals cannot assign a unique probability to certain events. The Smooth Ambiguity Preferences model (Klibanoff, Marinacci, & Mukerji, 2005) insists on the representations of second-order priors. For the Choquet Expected Utility model (Schmeidler, 2004), agents would be violating additivity, expressing a lack of confidence in their likelihood assessment. For Epstein

and Zhang (2001), ambiguity is seen as a violation of probabilistic sophistication. Thus, an ambiguity-averse attitude would contain features inconsistent with probabilistic beliefs.

The resulting insights provided invaluable richness to microeconomic and macroeconomic analysis beyond the use of objective risk (see, for extensive review Ilut & Schneider, 2022).

Methodology

Over the past four decades, economic methods and practices have undergone an empirical turn (Backhouse & Cherrier, 2017). Reports of experiments in economic literature were scarce throughout most of the twentieth century, with economists predominantly regarding economics as a non-experimental science. Indeed, relying on naturally occurring phenomena to study and establish generalizations was customary. Since the 1980s, researchers have increasingly used experimental techniques in economics to explore economic behavior, make precise inferences, and test hypotheses. Specifically, experimental methods allow economists to isolate one variable from others and manipulate it while holding everything else constant. They can then achieve a clean measurement of the impact of that variable on economic outcomes.

However, experiments often have different conditions compared to naturally occurring situations, leading to skepticism about the reliability and generalizability of results. For example, experimental results are sensitive to various factors that are difficult to control, such as supposedly irrelevant factors (Thaler, 2015). From one experiment to another with the same research question, a slight variation in the elicitation task or subjects demographics could lead to a plurality of insights on the same topic, making conclusions challenging. The cost of implementing an experiment and its complexity can also lead researchers to oversimplify, often by imposing experimental conditions on participants or through insufficient replica-

tion. Simplicity gains may result in information loss, as sometimes the results disconnect from real-life situations. Furthermore, experimenters are cautious about contextualization, which could lead to heterogeneous representations in participants' minds. Methodologically-oriented research is focusing on filling these gaps.

The elicitation of ambiguity attitudes is not exempt from the general methodological problems of experimental methods in economics. During the last three decades, considerable studies revealed some inconsistencies. For example, the fact that ambiguity attitudes are defined in relation to risk raises issues when eliciting them. Fox and Tversky (1995) and Fox and Weber (2002) noted that aversion is higher when ambiguous and risky options are evaluated together than when ambiguous and risky options are presented separately. This aspect is crucial in experiments that involve direct comparisons of risky and ambiguous bets, as opposed to real-world decisions where unambiguous options are scarce. Counterbalancing of tasks can also affect evaluations significantly, as ambiguity aversion is less pronounced when ambiguous options are evaluated first (Dimmock, Kouwenberg, Mitchell, & Peijnenburg, 2013; Fox & Weber, 2002). As a result, the representativeness of laboratory measurements in non-comparative real-world settings remains unclear. Recent experimental results also suggest that individuals' choices are noisy and that they likely perceive ambiguity simply as an equal-chance lottery (Binmore, Stewart, & Voorhoeve, 2012). In addition, a large proportion of individuals seem to make decisions almost at random (Stahl, 2014), and they can exhibit ambiguity aversion or seeking depending if the experimenter chooses a valuation or a choice task (Maffioletti, Schmidt, & Schroder, 2009; Trautmann, Vieider, & Wakker, 2011). Even when the same elicitation task was repeated, the consistency of decisions was not guaranteed (Duersch, Römer, & Roth, 2013). Specific configurations such as group decisions or market interaction also appear important in moderating ambiguity attitudes. In Charness, Karni, and Levin (2013), mixed groups (composed of individuals with different ambiguity attitudes) tend to favor ambiguity neutrality even though neither ambiguity aversion nor seeking were

more justifiable than the other. In market settings, ambiguity seems to affect both market prices and asset holdings, with evidence suggesting that ambiguity averters influence market outcomes and that they are not excluded from the market (Bossaerts, Ghirardato, Guarnaschelli, & Zame, 2010). However, the extent of these results is still limited, as more research is needed to drive robust conclusions. Outside the laboratory setting, few authors were able to establish the connection between ambiguity attitudes and real-life behaviors (Sutter, Kocher, Glätzle-Rützler, & Trautmann, 2013). These results reveal significant weaknesses in the theoretical conceptualization of ambiguity aversion and elicitation methods. Adequate characterization of ambiguity and ambiguity attitudes thus remains a central inquiry in economics. Other approaches are also increasingly being used, either in combination with or as a complement to the experimental method.

Researchers often encounter significant variations among calculations of crucial economic parameters. These variations arise from human interactions in diverse and evolving historical, cultural, and institutional environments. Unbiased and rigorous summaries of studies are crucial for understanding the actual consequences of variations in key parameters. To complement experimental methods in economics, meta-analysis has become increasingly important for synthesizing and interpreting findings across studies. This statistical approach combines the results of multiple studies to arrive at a more comprehensive and reliable assessment of the effect of an intervention or treatment. It can also contribute significantly to the research process by aiding in the design of new studies. Nevertheless, the researcher must be aware that meta-analysis may not be informative as it sometimes fails to highlight univariate effects, has restricted coverage, may include inappropriate studies, achieves heterogeneous data summarizations, groups different causal factors, lead to inaccurate estimates, and may obscure discrepancies with a theory-directed approach (Eysenck, 1994). Yet, it remains a privileged tool for synthesizing data from prior studies.

Despite the scattered results of the literature on ambiguity and ambiguity at-

titudes, the latter contains rare meta-analyses, such as Krain, Wilson, Arbuckle, Castellanos, and Milham (2006). However, it could benefit from this approach to, for example, assess the generalizability of preferences elicited by a specific experimental method, or identify gaps in the literature for future research.

This thesis independently used controlled laboratory experiments and meta-analysis, as explained in the following section.

Overview of the chapters

In this thesis, we aimed to contribute to understanding how individual attitudes towards uncertainty shape decisions in real-world contexts. To this end, we investigated i) how elicitation methods affect measures of ambiguity attitudes and ii) how these measures predict individual behavior for real-world decision problems. We were first interested in the decision problem of technology adoption in the insurance context, and second, we scrutinized four harmful behaviors (alcoholism, smoking, gambling, and problematic smartphone use). As we were interested in applied domains, it was important to identify the elicitation methods that best represent the uncertainty inherent to the decision problem and that elicit preferences with minimal bias. The literature on risk and risk attitudes has provided considerable theoretical and methodological resources to address these challenges (see, e.g., Barseghyan, Molinari, O'Donoghue, & Teitelbaum, 2018; Fehr-Duda & Epper, 2012; Meyer, 2014). However, real-world situations rarely involve objective probabilities available to decision-makers, making the use of ambiguity the most appropriate approach. Yet, techniques for characterizing ambiguity and eliciting ambiguity attitudes are still very scattered despite decades of development (Trautmann & Van De Kuilen, 2015). Nevertheless, this would not be a concern if individuals behave similarly in situations of risk and ambiguity. Consequently, risk attitudes could be used as a proxy for ambiguity attitudes. Since risk is a

special case of ambiguity, it cannot be ruled out that the associated attitudes are strongly related. Unfortunately, the literature is inconclusive on this point. Moreover, correlation tests between the two attitudes show that, in general, there are as many positive as zero correlations. Therefore, this thesis's starting point was to understand the origins of this heterogeneity. Such an inquiry would enable understanding whether or not the associations between risk and ambiguity attitudes were related to the characteristics of the experimental tasks. For example, Fox and Tversky (1995) and Fox and Weber (2002) argued that null correlations between the two attitudes are due to contrast effects, which emerge when the attitudes are jointly elicited. Having to evaluate risky and ambiguous options simultaneously, individuals report biased estimates, which in turn bias the correlation to zero. If this effect is confirmed, the choice of an experimental task to study the real-world decision problem must take account of these features to elicit preferences properly.

In the first chapter of this thesis, we investigated the coincidence conditions of the frequency of risk and ambiguity attitudes. We have opted for the meta-analysis method to make our approach more global and avoid restricting to a specific experimental method. In addition, conducting an experiment would have required considerable experimental conditions on a large population. We gathered 68 correlation signs from 55 research studies and associated features, including presentation of risk and ambiguity options, outcome domain, types of incentives distributed, elicitation format, sample size, implementation of ambiguity, and experimental population. As we operated with a small dataset (68 observations), we relied on exact statistical methods for accurate and reliable estimations. Results suggested that with the data available, a low sample size from the considered studies drives the likelihood of zero over positive correlation. There was also weak evidence that the contrast effects influence the probability of observing a null correlation. The nature of the measure used might explain negative correlations, but no conclusions could be drawn, given the scarcity of data. Nevertheless, these results did not clarify the actual nature of the relationship between risk and ambiguity atti-

tudes. They did indicate the conditions under which these relationships could be observed. However, by highlighting the importance of sample size, our analysis suggested that the two attitudes under scrutiny are indeed positively correlated (as a large enough sample size should be the norm for any experimental study).

In the second chapter, we focused on investigating the role of ambiguity attitudes in a specific real-world decision problem. We opted for the simple case of an individual choice between a dispute resolution procedure driven by human judgment (deemed ambiguous) versus an automated procedure (deemed unambiguous) in the context of car insurance. This automated procedure is based on a smart contract, a technology that automatically executes the terms of a contract (insurance contract, in our context). The algorithms underlying this execution draw their data from a blockchain, a distributed data ledger that eliminates the need for third parties (Ali, Jaradat, Kulakli, & Abuhlimeh, 2021; Buterin, 2014). Given the novelty of these elements involved in the decision context, we were faced with the challenge of finding the experimental task that would best represent our decision problem. As the literature does not offer an answer to this case, the construction of a task that would mimic the characteristics of the decision problem was necessary. Specifically, in the second chapter of this thesis, we examined how attitudes towards ambiguity, risk, and reduction of compound risks influence the choice between a blockchain-based smart contract and an ambiguous expert-based one. Compound risks, less known in the literature, correspond to another implementation of ambiguity involving second-order probabilities (see, e.g., Halevy, 2007). We first designed a simplified framework to represent the context by relying on noteworthy works on blockchain and smart contracts (Eenmaa-Dimitrieva & Schmidt-Kessen, 2019; Hans, Zuber, Rizk, & Steinmetz, 2017). Then, we conducted a laboratory experiment using an adaptation of the Bomb Risk Elicitation Task (Crosetto & Filippin, 2013) as our ambiguity elicitation task. Finally, we correlated the elicited preferences with participants' demographics, personality traits, and car use experience. This last variable is an important element of context since

the car driving experience is likely to modify users' perception of associated services such as insurance (A. Cohen, 2005). We found that users' car experience shapes their perception of uncertainty and influence their choice of procedure. In particular, the ambiguity averse users with no car experience were more willing to opt for blockchain-based smart contracts, probably due to their dislike of unpredictable situations. Furthermore, we highlighted the significant role of generalized trust and optimism in potential users' decision-making when adopting blockchain technology-based smart contracts in the insurance sector. We found that social factors like trust in other humans can influence users' decisions towards human-based procedures. In contrast, optimism positively affects openness to smart contracts, making users more willing to test new technologies even though they may not entirely understand them. However, it was challenging to assess the external validity of our study mainly because the chosen decision problem is relatively new, therefore, real-world data is currently lacking. Indeed, for further study, we decided to rely on more common decision problems like those underlying harmful behaviors (for example, alcoholism, smoking, gambling, and problematic smartphone use). Nevertheless, despite being extensively documented, the literature does not explicitly inform about the most appropriate experimental method to elicit the individual ambiguity attitudes associated with them. We thus had to i) scrutinize the available experimental methods to select the most suitable ones and ii) test the extent to which their measures correlate with the different harmful behaviors. Going through the first stage was already challenging because no objective selection criteria based on the internal consistency of the experimental methods' measures were available. It was therefore crucial for further study to address this issue as well.

Traditionally, constructing an experimental task had to meet the requirement of validity, i.e., containing premises consistent with the conclusions sought. The most popular forms of validity are internal (measuring behavior with minimum bias) and external (generalizing results to an uncontrolled environment). The latter re-

mains a general issue when using controlled laboratory experiments (List, 2007). Regarding risk and ambiguity elicitation in particular, the difficulty of measures to remain internally valid stems from the complexity of the observed behavior and certain conditions imposed on agents, either to reduce experimentation costs or to have readily analyzable data. This is the case, for example, when the researcher must decide whether or not to impose a single switching point when the task contains a choice list. On the one hand, allowing agents to choose freely could lead to high levels of multiple switching, rendering the data unworkable with current models. On the other hand, forcing them to switch once preserves the sample size of participants, which is already generally small. This example is just one of the dilemmas that experimenters have to deal with, such as the non-repeated measurement of behavior and the systematic use of Random Incentive System (Baltussen, Post, Van Den Assem, & Wakker, 2012).

In the third chapter of our thesis, we proposed to examine the internal and external validity of measures of individual ambiguity attitudes. We plan to replicate three well-known ambiguity attitude measurement methods (Abdellaoui et al., 2011; Baillon, Huang, Selim, & Wakker, 2018; Chakravarty & Roy, 2009). In Abdellaoui et al. (2011), ambiguity and risk attitudes are quantified through the use of two indexes: pessimism and likelihood insensitivity. The difference in pessimism between risky and ambiguous choices is computed to determine ambiguity attitudes. Baillon, Huang, et al. (2018) use particular indexes for obtaining probability equivalents (matching probabilities) for a partition of three mutually exclusive events. Their indexes are also directly observable, correct for subjective likelihoods, and work for all artificial and natural events. Additionally, they remain valid even if the expected utility for risk is violated and remain valid across various ambiguity theories. In Chakravarty and Roy (2009), "KMM representation" (Klibanoff et al., 2005) describe ambiguity attitudes. The resulting "KMM value" reflects the subjective expected utility of the prospect, conditional on the assumption of expected utility under risk and the decision maker's assignment of

subjective probabilities over the occurrence of two events. Internal validity will be determined by examining the consistency and stability of the measure when the task is repeated. We have chosen not to delve into the causes of potential inconsistencies as it would require a complex analysis incorporating both theoretical and methodological elements. Our focus is solely on the evaluation of different methods for ambiguity attitude measurements. For external validity, we will evaluate how these measures accurately reflect real-world harmful behaviors like alcoholism, smoking, gambling, and problematic smartphone use.

Main contributions of the thesis

This thesis first provides empirically grounded insights contributing to understanding the heterogeneity in the relationship between risk and ambiguity attitudes. This question has remained unanswered in the literature, and authors have only speculated on the reasons behind a positive, negative, or null relationship. The first study to openly address the issue is Boun My, Brunette, Couture, and Van Driessche (2022). The authors have, however, opted for a specific experimental design and, in so doing, have contributed more to the questioning than to providing generalizable insights. We favored a meta-analysis approach, identifying key variables to help better understand the determinants of correlations. Based on these insights, further experimental studies could scrutinize these determinants and improve the understanding of the relationship between risk and ambiguity attitudes. This will have numerous implications, including for learning under uncertainty (Jia, Furlong, Gao, Santos, & Levy, 2020) and for certain phenomena still insufficiently understood in financial markets, such as the value effect (Bossaerts, Guarnaschelli, Ghirardato, & Zame, 2009).

In addition, we have contributed to the literature on the behavioral determinants of technology adoption traditionally addressed by management or market-

ing sciences. Indeed, by bringing behavioral economics perspectives to this topic, we contributed with methods that are new to these issues. We also confirm the need to incorporate individuals' perceptions of uncertainty and that this perception could be related to other variables, such as car use.

Finally, by proposing a replication of popular experimental tasks and by examining the rate of inconsistencies and the stability of the measures, we aim to provide the literature with additional objective criteria for ambiguity attitudes elicitation method selection. An examination of the exportability of the ambiguity attitude measures will enrich this analysis.

Chapter 1

**Why are risk and ambiguity attitudes
(not) correlated? Insights from a
meta-analysis**

1.1 Introduction

This study contributes to understanding the correlation sources between risk and ambiguity attitudes in the individual decision-making literature.

Risk and ambiguity have been extensively studied in economics over decades. They characterize the uncertainty inherent to financial and insurance markets or in simple individual consumption decision problems. Under ambiguity, the uncertainty perceived by the decision-maker cannot be represented by a probability distribution. This conception thus englobes the concept of risk, which characterizes a measurable uncertainty.¹ Risk aversion is the preference for a “safe” lottery over its risky alternative. Ambiguity aversion reflects the preference for a risky lottery over its ambiguous counterpart. In practice, the presence of complexity, imprecision, vagueness, insolubility, or novelty may be sufficient to invoke the notion of ambiguity (Cavatorta & Schröder, 2019).

Although risk attitudes and ambiguity attitudes are conceptually considered different, there is no consensus on their empirical link. This relationship is generally assumed to be captured by correlation tests.² Since the early experiments reporting this statistic (M. Cohen, Jaffray, & Said, 1985, 1987), positive, negative, and null association results have been found in the literature (see Table 1.1). How can this heterogeneity be explained? Addressing this question involves investigating the conditions under which the frequency of risk averse behavior coincides or not with the frequency of ambiguity averse behavior.

Theoretical models provide little or no information about the precise nature of the relationship between these two attitudes. For instance, the paradigm of Halevy and Feltkamp (2005) and the Reference Dependence Model of Qiu and Weitzel (2011) suggest a positive relationship between risk and ambiguity prefer-

¹Bergheim and Roos (2013): “[...] risk can be thought of as a special case of ambiguity. [...] Risky lotteries are less uncertain than ambiguous ones because they have fewer unknown elements.” Abdellaoui et al. (2011): “Ambiguity reflects what uncertainty comprises beyond risk.”

²Correlation tests may not be the most relevant for accurately capturing the relationship between these two preferences. For example, they do not consider the influence of socio-demographic characteristics, as regression does. We do not discuss the suitability of these tests in this article.

Table 1.1: Summary of the signs of correlation observed in the literature

Note: Statistics based on the data used in this study.

Correlation Sign	Frequency	Percent
Negative	6	8.80
Positive	31	45.60
Zero (uncorrelated)	31	45.60
Total	68	100.00

ences while some axiomatic models remain entirely silent (Gilboa & Schmeidler, 1989; Schmeidler, 2004). Therefore, it is challenging for a researcher to comment on the correlation result observed after an experiment without precise theoretical foundations. As a result, most authors do not even report the computed correlation, and those who do generally do not further interpret it.

Nevertheless, there are numerous reasons for being concerned about the correlation between risk and ambiguity attitudes (henceforth CRAA). The presence of correlation suggests that there would be a linear and/or monotonic relationship between risk and ambiguity preferences. In the case of a positive (negative) correlation, it would imply that risk-averse individuals are (not) also ambiguity-averse, for example. Risk attitudes can be used as proxies for ambiguity attitudes when the correlation value is high (Dimmock et al., 2013). It would also be advantageous when measuring one of the two (mostly ambiguity) is costly and time-consuming, especially in field studies. However, because these preferences would be redundant, using them jointly in linear regression as explanatory variables could lead to multicollinearity and bias the estimated effects. Curley, Yates, and Abrams (1986) states that detecting a correlation is indicative of a common mechanism linking the two preferences. Chapman, Dean, Ortoleva, Snowberg, and Camerer (2018) suggests that a moderate correlation (e.g., 0.5) points that an underlying factor moderates both variables, such as decision mode (Butler, Guiso, & Jappelli, 2014). For example, reasoning individuals' preferences may reveal a positive CRAA, as they can cope better with uncertainty. For Jia et al. (2020), a positive CRAA makes

it more likely that individual learning in risky environments can generalize to ambiguous ones. Bossaerts et al. (2009) it might explain the value effect in financial markets, a phenomenon documented by Fama and French (1998) where value stocks have higher returns than growth stocks since only investors who are both ambiguity and risk-averse will be inclined to hold growth stocks.³ In the case of zero correlation, individuals' choices within risky environments would not be related to their choices within ambiguous ones. Therefore, the two behaviors can be considered complementary and can be analyzed separately as it captures distinct behavioral aspects (Koudstaal, Sloof, & Van Praag, 2016).⁴

As we have explained, the consequences of the actual nature of the relationship between risk and ambiguity are not trivial. We investigated the reasons underlying the heterogeneity in correlation signs observed in the literature. Correlation values were not of interest because they are highly sensitive to elicitation features and to the statistics used to calculate them. Only few authors have commented on CRAA causes, the most common explanation being measurement error (Camerer & Weber, 1992; Trautmann & Van De Kuilen, 2015), and the nature of the index used (Fairley & Sanfey, 2020).

The novelty of this work resides in that it approaches the issue of CRAA from a global perspective. This study provides generalizable findings by relying on a meta-analysis, i.e., integrating several results associated with the methodological elements employed by articles in the literature. To conduct the meta-analysis, we collected 68 correlation signs from 55 research studies, to which we associated their respective elicitation format, sample size, outcome domain, presentation of ambiguous options, implementation of ambiguity, distributed incentives, and experimental population type. We were not interested in correlation values as they

³Growth stocks are stocks of companies that are expected to grow at a faster rate than the overall market or their industry peers, and may have higher price-to-earnings ratios. Value stocks refer to stocks of companies that are undervalued in the market and offer greater potential returns.

⁴The notion of independence of the variables is not mentioned here, as it depends on the metric used to capture the relationship. When correlation is measured by Bravais-Pearson's r , a zero correlation does not imply independence, only that the two variables are not linearly related. Independence can, however, be asserted when Spearman's $\rho = 0$.

are highly sensitive to elicitation features and to the metrics used to capture the correlation. We used exact statistical methods to analyze which experimental features influence the chances of observing a positive or zero correlation. Negative correlations were excluded from the statistical analysis as they are likely to be statistical artifacts. We first considered the full dataset and 3 subsets based on the gain domain, the real monetary incentives, and their intersection. Estimations suggest that the probability of observing a positive correlation in the literature is driven by the study's sample size that computed it. The effect is also observed in the real monetary incentives subset, and we found weak evidence on the gain domain and the intersection of gain domain and real monetary incentives. We checked the robustness of these results by conducting Monte Carlo permutation tests (1000 simulations) to estimate p-values. Additionally, there is weak evidence of the influence of contrast effects induced by the joint/separate presentation of risk and ambiguous options in the elicitation task (Camerer & Weber, 1992; Fox & Tversky, 1995; Trautmann & Van De Kuilen, 2015). This finding is also robust. The results are reproducible using the statistical code and data on an Open Science Framework (OSF) repository ([link here](#)).

The remainder of this paper is structured as follows. Section 1.2 explains the literature's interpretation of the CRAA issue. Section 1.3 describes the data collection process and the statistical method. In Section 1.4, we presented the results of the statistical analyses that are discussed in Section 1.5.

1.2 Literature

This section presents various observations drawn from the literature on the issue we are investigating. Few authors comment on the causes of the presence or absence of CRAA. Most seem to favor a positive CRAA, attributing its absence to measurement problems rather than individual preferences. Two explanations recur: the contrast effect induced by the joint presentation of risky and ambiguous

options, and the use of a normalized measure of their valuations.

Among the earliest commentators, Camerer and Weber (1992) interpreted the null CRAA found in M. Cohen et al. (1985), Curley et al. (1986) and Hogarth and Einhorn (1990) as arising from a measurement error.⁵ More precisely, Camerer and Weber highlighted the joint evaluation feature of their elicitation task. Joint evaluation refers to the practice of not eliciting preferences in independent tasks. Indeed, by simultaneously considering two options, the individual tends to value each option in terms of the other, which could lead to biased measures of preferences (Bazerman, Moore, Tenbrunsel, Wade-Benzoni, & Blount, 1999). Considering ambiguity attitudes, their conceptual definitions have prompted a long-standing methodological tradition of measuring them to be measured in comparison with other measures of risk attitudes. However, elicitation methods that allow such a comparison without compromising accuracy are still under development in the literature.⁶ Therefore, it cannot be ruled out that several studies use features unfavorable to the independent perception of risky and ambiguous options, which subsequently have an impact on their correlation metrics. Kocher and Trautmann (2013) and Trautmann and Van De Kuilen (2015) clarify that the underlying mechanism concerns the measurement of risk rather than ambiguity : since the risky option might seem more attractive than the ambiguous option, the individual would display biased degrees of risk attitudes while ambiguity attitudes may remain unchanged. This contrast effect is more salient when the individual is extremely risk and ambiguity averse. Trautmann and Van De Kuilen (2015) noted that it could also arise when the tasks (and by extension, options) are counterbalanced (alternated) (Dimmock et al., 2013). It also corresponds to comparative ignorance, when ambiguity aversion is more pronounced when ambiguous events are compared with familiar events (here, risk) (Fox & Weber, 2002). The joint presentation (or

⁵To our best knowledge, M. Cohen et al. (1985) and its republication M. Cohen et al. (1987) were the earlier studies to test the CRAA.

⁶For example, Baillon, Huang, et al. (2018) has recently proposed preference capture indices for ambiguity that correct for subjective likelihoods and remain valid even when individuals violate expected utility for risk.

counterbalancing) of risk and ambiguity options would, therefore, be a moderator of ambiguity attitudes. We found 15 CRAA calculated with preference measures elicited in this way.

The other measurement problem affecting CRAA relates to the computation of the ambiguity attitudes index. Fairley and Sanfey (2020) and Trautmann and Van De Kuilen (2015) observed that measuring ambiguity aversion by the normalized difference between risky and ambiguous certainty equivalents could lead to null or even negative CRAA.⁷ Normalization controls that the effect of a difference in outcome between two options may be stronger for a category of subjects, especially for those who are most averse. Consequently, the use of such a metric implies that an individual cannot simultaneously be categorized as extremely risk averse and ambiguity averse. This could lead to negative or null CRAA. Indeed, the effect of normalization might suggest that a negative CRAA is simply a statistical artifact. However, (Attanasi, Gollier, Montesano, & Pace, 2014; Dimmock, Kouwenberg, & Wakker, 2016) found a negative association but did not use such a measure. The authors did not comment this result. Nevertheless, the small number of negative correlations is insufficient to generalize an intuition about their causes or test them statistically. Moreover, even considering that they are mostly statistical artifacts, it is unclear what sign (positive or zero) they would have had if calculated with non-normalized measurements.

Marginal comments on CRAA include Koch and Schunk (2013) who stated that a positive correlation should be observed and explained that many studies did not find this result because the lotteries' stakes were low and participants could not actually lose their own money. Consequently, the authors suggested that i) CRAA is domain-dependent, ii) the size of the prizes is crucial, and iii) a real loss condition is preferable. Unfortunately, few CRAA are available in the loss domain, and the perception of the payoff's size depends on the experimental population.

⁷It is given by $a = CE_R - CE_A / (CE_R + CE_A)$, with (CE) the certainty equivalent of the risky (R) and the ambiguous option (A).

Additionally, taking money from participants is not a commonly accepted practice in experimental economics and psychology.

The last minor comment concerns Butler et al. (2014), who proposed the role of decision mode as a mediator between risk and ambiguity attitudes. They showed that decision mode (reasoning or intuition) influences risk and ambiguity attitudes. These decision modes fit into the cognitive architecture based on a dual system (System 1 and 2) advocated by Kahneman (2003). Reasoned individuals tend to systematically compare potential alternatives after processing all available information. Intuitive individuals address challenging or complex problems by selecting the most satisfying solutions. Butler et al. (2014) considered that reasoned individuals are better at coping with uncertainty. Hence, their preferences are likely to reveal a positive CRAA.

1.3 Research Design

Understanding the heterogeneity of CRAA in the literature required collecting correlation information directly from research studies that computed them. Table 1.2 contains the correlation signs and the associated features of the studies in which they are reported. As each study is unique and complex, common reference points were needed to avoid focusing on subtle differences that could make the statistical analysis tedious or impossible.

1.3.1 Collection of research studies and selection of information

The scope of the search was limited to individual decision-making experiments in economics and psychology.⁸ The preferences considered were restricted to simple attitudes and did not concern high-order preferences or insensitivity measures. We

⁸An exception was however made for Calford (2020). Although it is a game theory study, the source of uncertainty was not the other players but the urns.

only included studies treating ambiguity as event-driven.⁹ No study was excluded based on publication status, number of citations, or any bibliometric index.

Two parallel pathways were used to find research studies computing CRAA. First, we exploited the references reported in literature reviews, notably Camerer and Weber (1992); Trautmann and Van De Kuilen (2015) and Etner, Jeleva, and Tallon (2012). From these references, we also drew other references and so on. More than half of the articles were identified using this approach. Second, we used Harzing's Publish or Perish Graphical User Interface, a large-scale search tool allowing for complex sorting of articles listed in portals like Google Scholar and Scopus. Using such a search engine for our research question is not time-efficient, as it requires screening thousands of results for a small number of relevant articles. In addition to the term "ambiguity," the keywords used in the engine included other terminology, such as "uncertainty" in association with "preferences," "attitudes," or "aversion." At the end of this process, a sample of 55 *papers* was collected. The complete data used for the statistical analysis in this study are reported in Table 1.2.¹⁰

It was also challenging to identify the desired information in some studies. In some cases, the information was not mentioned in the study itself but in another study.¹¹ For instance, the review of Trautmann and Van De Kuilen (2015) directly computed two CRAA from Akay, Martinsson, Medhin, and Trautmann (2012) and from Sutter et al. (2013). The same applies to Butler et al. (2014), whose correlation value is given by Guiso and Jappelli (2008).

When information was explicitly reported but ambivalent, we imposed some

⁹We did not analyze outcome-driven ambiguity as it does not receive much attention from the literature relative to event-driven ambiguity.

¹⁰Trautmann and Van De Kuilen (2015) explains that the scarcity of papers that computed CRAA (relative to the extensive literature on risk and ambiguity attitudes) might be due to some authors' indifference to mention a zero correlation as it does not bring any additional information. If this is the case, it will imply that the actual number of zero correlations in the literature would exceed the number of positive and negative correlations. However, it is also plausible that other authors may be tempted not to report a positive correlation value (especially if it is high) as they use the two preferences as explanatory variables within a linear regression (which could lead to multicollinearity).

¹¹The supplementary materials provided by a study were also a valuable source of information.

information selection rules to minimize bias when reporting. They also serve as a reference for readers who might find that some data we reported in Table 1.2 conflicts with what they expected.¹² In particular, we prioritized the “uniqueness of information” when a study contained several CRAAs. We examined the respective experimental conditions on which these were based to determine whether they differed significantly from one another. If not, only one CRAA was reported. This criterion led to the inclusion of only one of the CRAA from Prokosheva (2016) among those for which preferences were elicited with substantially close payoffs (100 CZK \approx 5\$ and 200 CZK \approx 10\$ at the moment of the experiment). Boun My et al. (2022) was subject to the criterion for the same reasons. The same applied to Koch and Schunk (2013), who used a special experimental condition where participants could lose their own money in one of their experiments. The CRAA associated with the “standard” condition was retained. In addition, we prioritized the “objective” meaning of information over its author’s interpretation. For example, we did not follow Dimmock et al. (2013), who, despite the correlation test’s significance, interpreted the statistic’s low value ($\rho = 0.16$) as null. In such cases, we preferred to report a positive CRAA, as did other studies that found low values in the same order of magnitude (Fairley & Weitzel, 2017; Zhang, 2022). Guiso and Jappelli (2008) and Butler et al. (2014) even reported $\rho = 0.07$ as positive nonetheless. Moreover, M. Cohen et al. (2011) indicated that the significance of their correlation test was entirely attributable to subjects with extreme risk and ambiguity attitudes and did not reflect their population’s average. To ensure comparability with other studies, we reported a null CRAA. Finally, we did not report anything when some information was not mentioned, when it was impossible to find it in other sources, and when there was no indication helping to infer it.

¹²Subtleties can be found throughout the text of a study. For example, the final sample size used in correlation calculations may differ from the number of participants initially announced due, for instance, to the withdrawal of some participants. Attention should also be paid to the simplifications some authors made, such as M. Cohen, Tallon, and Vergnaud (2011), who visually presented a choice list to the reader but specified in a sentence that their experimental participants chose the lotteries one after the other. Akay et al. (2012) even used the term “choice list” while implementing sequential lottery choices.

Table 1.2: Full dataset containing CRAA and experimental features of the studies
Note: "Seq. Lott. Choice" is for Sequential Lottery Choice. In the Options Presentation variable, we assigned the "joint" category to studies alternating between presenting risky and ambiguous lottery options (see Section 1.2).

Study	id	id	Ambiguity	Correlation	Correlation	Outcome	Risk	Ambiguity	Sample	Options	Distributed	Population
Author	Correlation	Implementation	Sign	Value	Domain	Format	Format	Size	Presentation	Incentives		
Abdelloui et al. (2011)	1	1	total	positive	0,53	gain	Choice List	Choice List	66	separate	real	students
Abdelloui et al. (2011)	1	2	natural	positive	0,82	gain	Seq. Lott. Choice	Seq. Lott. Choice	29	separate	real	students
Abdelloui et al. (2011)	1	3	natural	positive	0,64	gain	Seq. Lott. Choice	Seq. Lott. Choice	29	separate	hypothetical	students
Ahsanzaman, Palm-Förster, and Suter (2022)	2	4	total	positive	0,38	gain	Choice List	Choice List	318	separate	real	students
Akay et al. (2012)	3	5	total	negative	-	gain	Seq. Lott. Choice	Seq. Lott. Choice	93	separate	real	non students
Alevy (2013)	4	6	total	uncorrelated	0,00	gain	Choice List	Choice List	176	separate	real	students
Anantanasuwong, Kouwenberg, Mitchell, and Peijnenberg (2019)	5	7	natural	positive	0,49	gain	Choice List	Choice List	289	separate	real	non students
Anderson, Gibson, Luchtenberg, and Seiler (2022)	6	8	total	positive	0,40	gain	Choice List	Choice List	1817	separate	real	non students
Attanasi et al. (2014)	7	9	total	negative	-	gain	Seq. Lott. Choice	Seq. Lott. Choice	35	separate	real	students
Attanasi, Festré, Chessa, Ballatore, and Ouangraoua (2021)	8	10	imprecision	uncorrelated	0,00	gain	Choice List	Choice List	144	joint	real	students
Baillon, Schlesinger, and van de Kuilen (2018)	9	11	total	positive	0,22	gain	Seq. Lott. Choice	Seq. Lott. Choice	199	joint	real	students
Berghem and Roos (2013)	10	12	total	positive	0,27	gain	Choice List	Choice List	77	separate	real	students
Bianchi and Talion (2019)	11	13	total	uncorrelated	0,00	gain	Seq. Lott. Choice	Seq. Lott. Choice	511	separate	hypothetical	non students
Blankenstein, Peper, Crone, and van Duijvenvoorde (2017)	12	14	imprecision	uncorrelated	0,00	gain	Seq. Lott. Choice	Seq. Lott. Choice	50	separate	real	non students
Boun My et al. (2022)	13	15	total	positive	0,39	gain	Choice List	Choice List	209	separate	real	students
Brown, Erdman, Ling, and Santos (2010)	14	16	total	uncorrelated	0,00	gain	Seq. Lott. Choice	Seq. Lott. Choice	24	separate	real	students
Brunette, Cabantous, and Couture (2011)	15	17	total	positive	0,39	gain	Choice List	Choice List	60	separate	real	students
Burks, Carpenter, Götte, and Rustichini (2008)	16	18	imprecision	positive	-	gain	Choice List	Choice List	892	separate	real	non students
Butler et al. (2014)	17	19	total	positive	0,07	gain	Choice List	Seq. Lott. Choice	1306	separate	hypothetical	non students
Calford (2020)	18	20	total	positive	0,30	gain	Seq. Lott. Choice	Seq. Lott. Choice	116	separate	real	students
Chakravarty and Roy (2009)	19	21	total	positive	0,36	gain	Choice List	Choice List	85	separate	real	students
Chakravarty and Roy (2009)	19	22	total	uncorrelated	0,00	loss	Choice List	Choice List	85	separate	real	students
Chapman et al. (2016)	20	23	total	uncorrelated	0,00	gain	Choice List	Choice List	1000	separate	real	non students
Chew, Miao, and Zhong (2017)	21	24	total	uncorrelated	0,00	gain	Choice List	Choice List	56	separate	real	students
Chew et al. (2017)	21	25	imprecision	positive	0,36	gain	Choice List	Choice List	56	separate	real	students
M. Cohen et al. (1987)	22	26	total	uncorrelated	0,00	gain	Choice List	Choice List	134	joint	real	students
M. Cohen et al. (1987)	22	27	total	uncorrelated	0,00	loss	Choice List	Choice List	134	joint	real	students
M. Cohen et al. (2011)	23	28	imprecision	uncorrelated	0,00	gain	Seq. Lott. Choice	Seq. Lott. Choice	400	separate	real	non students
M. Cohen et al. (2011)	23	29	imprecision	uncorrelated	0,00	gain	Seq. Lott. Choice	Seq. Lott. Choice	104	separate	real	students
Corcos, Pannese, and Bourgeois-Gironde (2012)	24	30	total	uncorrelated	0,00	gain	Choice List	Choice List	93	separate	real	students
Cubitt, van de Kuilen, and Mukerji (2018)	25	31	total	negative	-	gain	Choice List	Choice List	84	separate	real	students
Curley et al. (1986)	26	32	total	uncorrelated	0,00	gain	Seq. Lott. Choice	Seq. Lott. Choice	63	joint	real	students
d'Albis, Attanasi, and Thibault (2020)	27	33	total	uncorrelated	0,00	gain	Choice List	Choice List	100	separate	real	students
Dean and Ortoleva (2019)	28	34	total	positive	0,46	gain	Choice List	Choice List	159	separate	real	students
Dimmock et al. (2013)	29	35	total	positive	0,16	gain	Seq. Lott. Choice	Seq. Lott. Choice	3158	separate	real	non students
Dimmock et al. (2016)	30	36	total	negative	-0,18	gain	Seq. Lott. Choice	Seq. Lott. Choice	756	separate	hypothetical	non students
Drouvelis and Jamison (2015)	31	37	total	positive	0,40	gain	Choice List	Choice List	192	separate	real	students
Drouvelis and Jamison (2015)	31	38	total	positive	0,61	loss	Choice List	Choice List	192	separate	real	students
Duersch et al. (2013)	32	39	total	positive	-	gain	Seq. Lott. Choice	Choice List	110	separate	real	students
Fairley and Wetzell (2017)	33	40	total	positive	0,13	gain	Choice List	Choice List	233	separate	real	students
Fairley and Sanfey (2020)	34	41	total	negative	-0,27	gain	Choice List	Choice List	172	separate	real	students
Guo, Chen, and Liu (2022)	35	42	imprecision	uncorrelated	0,00	loss	Seq. Lott. Choice	Seq. Lott. Choice	117	separate	real	students
Hogarth and Einhorn (1990)	36	43	total	uncorrelated	0,00	gain	Seq. Lott. Choice	Seq. Lott. Choice	146	joint	hypothetical	students
Hogarth and Einhorn (1990)	36	44	total	uncorrelated	0,00	gain	Seq. Lott. Choice	Seq. Lott. Choice	49	joint	real	students
Huettel, Stowe, Gordon, Warner, and Platt (2006)	37	45	total	uncorrelated	0,00	gain	Seq. Lott. Choice	Seq. Lott. Choice	12	joint	real	non students
Jia et al. (2020)	38	46	imprecision	positive	0,49	gain	Seq. Lott. Choice	Seq. Lott. Choice	80	joint	real	students
Jordán Martín et al. (2016)	39	47	total	uncorrelated	0,00	gain	Seq. Lott. Choice	Seq. Lott. Choice	104	separate	real	students
Koch and Schunk (2013)	40	48	total	uncorrelated	0,00	loss	Choice List	Choice List	73	separate	real	students
Kocher and Trautmann (2013)	41	49	total	positive	-	gain	Seq. Lott. Choice	Seq. Lott. Choice	176	separate	real	students
Kocher, Lahno, and Trautmann (2018)	42	50	total	uncorrelated	0,00	gain	Choice List	Choice List	501	separate	real	students
Koudstaal et al. (2016)	43	51	total	uncorrelated	0,00	gain	Choice List	Choice List	2288	separate	real	non students
Lauriola and Levin (2001)	44	52	total	uncorrelated	0,00	gain	Seq. Lott. Choice	Seq. Lott. Choice	76	joint	hypothetical	students
Lauriola and Levin (2001)	44	53	total	uncorrelated	0,00	loss	Seq. Lott. Choice	Seq. Lott. Choice	76	joint	hypothetical	students
Levy, Snell, Nelson, Rustichini, and Glimcher (2010)	45	54	imprecision	uncorrelated	0,00	gain	Seq. Lott. Choice	Seq. Lott. Choice	18	joint	real	non students
Potamites and Zhang (2012)	46	55	natural	uncorrelated	0,00	gain	Seq. Lott. Choice	Seq. Lott. Choice	292	joint	real	non students
Prokoshcheva (2016)	47	56	total	positive	0,56	gain	Choice List	Choice List	135	joint	real	non students
Prokoshcheva (2016)	47	57	total	positive	0,36	gain	Seq. Lott. Choice	Seq. Lott. Choice	135	separate	real	non students
Qu and Wetzell (2011)	48	58	total	positive	0,49	gain	Seq. Lott. Choice	Seq. Lott. Choice	208	joint	real	students
Stanton et al. (2011)	49	59	total	positive	0,42	gain	Seq. Lott. Choice	Seq. Lott. Choice	298	separate	real	non students
Sutter et al. (2013)	50	60	total	negative	-	gain	Choice List	Choice List	487	separate	real	non students
Tevenart and Brunette (2021)	51	61	total	uncorrelated	0,00	gain	Choice List	Choice List	45	separate	hypothetical	non students
Tymula, Glimcher, Levy, and Belmaker (2012)	52	62	imprecision	positive	0,39	gain	Seq. Lott. Choice	Seq. Lott. Choice	135	separate	real	non students
Tymula et al. (2012)	52	63	imprecision	uncorrelated	0,00	loss	Seq. Lott. Choice	Seq. Lott. Choice	135	separate	real	non students
van den Bos and Hertwig (2017)	53	64	imprecision	uncorrelated	0,00	gain	Seq. Lott. Choice	Seq. Lott. Choice	105	separate	real	non students
van den Bos and Hertwig (2017)	53	65	imprecision	uncorrelated	0,00	loss	Seq. Lott. Choice	Seq. Lott. Choice	105	separate	real	non students
Vieider et al. (2015)	54	66	total	positive	0,68	gain	Choice List	Choice List	2879	separate	real	students
Vieider et al. (2015)	54	67	total	positive	0,66	loss	Choice List	Choice List	2879	separate	real	students
Zhang (2022)	55	68	total	positive	0,14	gain	Seq. Lott. Choice	Seq. Lott. Choice	693	separate	real	non students

1.3.2 Variables construction

Variable names will henceforth be *italicized*. We reported the signs and the values of the CRAA in the variables named *Correlation Sign* and *Correlation Value*, respectively. However, the CRAA values are not of interest for the rest of the study as they are highly sensitive to elicitation features and to the statistics used to capture the correlation (for example, Pearson's r , Spearman's ρ , or Kendall's τ). Figure 1.1 displays the distribution of correlation values available in the literature. Since they are not numerous and extremely heterogeneous, we did not use them in a regression analysis.

Among the causes of null CRAA proposed by the literature, only the contrast effects/comparative ignorance (*Options Presentation*) and outcome domain (*Out-*

come Domain) can be observed for all elicitation tasks.¹³ As this study is exploratory, we considered other variables for further analysis. They were chosen not based on comments from the literature but as salient features directly comparable from one study to another. These variables refer to ambiguity implementation (*Ambiguity Implementation*), the format of risk and ambiguity attitudes elicitation tasks (*Risk Format* and *Ambiguity Format*), incentives distributed (*Incentives*), sample size of the study (*Sample Size*), and population (*Population*).¹⁴ Table 1.3 provides their descriptions and categories. Since the authors discussing CRAA did not mention them, we made no assumptions about the sign or magnitude of their effect on CRAA. However, it is worth noting that they may also be sources of bias in risk and ambiguity attitudes measures, extending the intuition made by Camerer and Weber (1992) (see Section 1.2). For example, total ambiguity or imprecise probability implementation of ambiguity do not necessarily lead to the same degrees of ambiguity attitudes (Chew et al., 2017). Choice list formats can lead to middle bias (Andersen, Harrison, Lau, & Rutström, 2006). Hypothetical payments may be insufficiently attractive to accurately measure preferences (Camerer, 1995). A sample size inadequate to the research question may lead to erroneous statistical tests (Faber & Fonseca, 2014). An experimental population of students will not necessarily have the same preferences as a non-student population (Levitt & List, 2007). The literature is not unanimous on the potential bias they lead to, and we did not address them in this study. Additionally, we did not report information such as the elicitation task or the theoretical model used in the studies because they differed from one study to another, and no pattern emerged.

¹³In *Options Presentation*, we assigned the “joint” category to studies alternating between presenting risky and ambiguous lottery options (see Section 1.2).

¹⁴Concerning the experimental population, some articles do not mention whether it is student or non-student on average. Thus, we used the heuristic of considering the average age as an indicator. For example, an average age of 14 (van den Bos & Hertwig, 2017) does not reflect a student population.

Figure 1.1: Distribution of available CRAA values

Note: The numbers associated with the dots in the graph are based on id Author variable from Table 1.2.

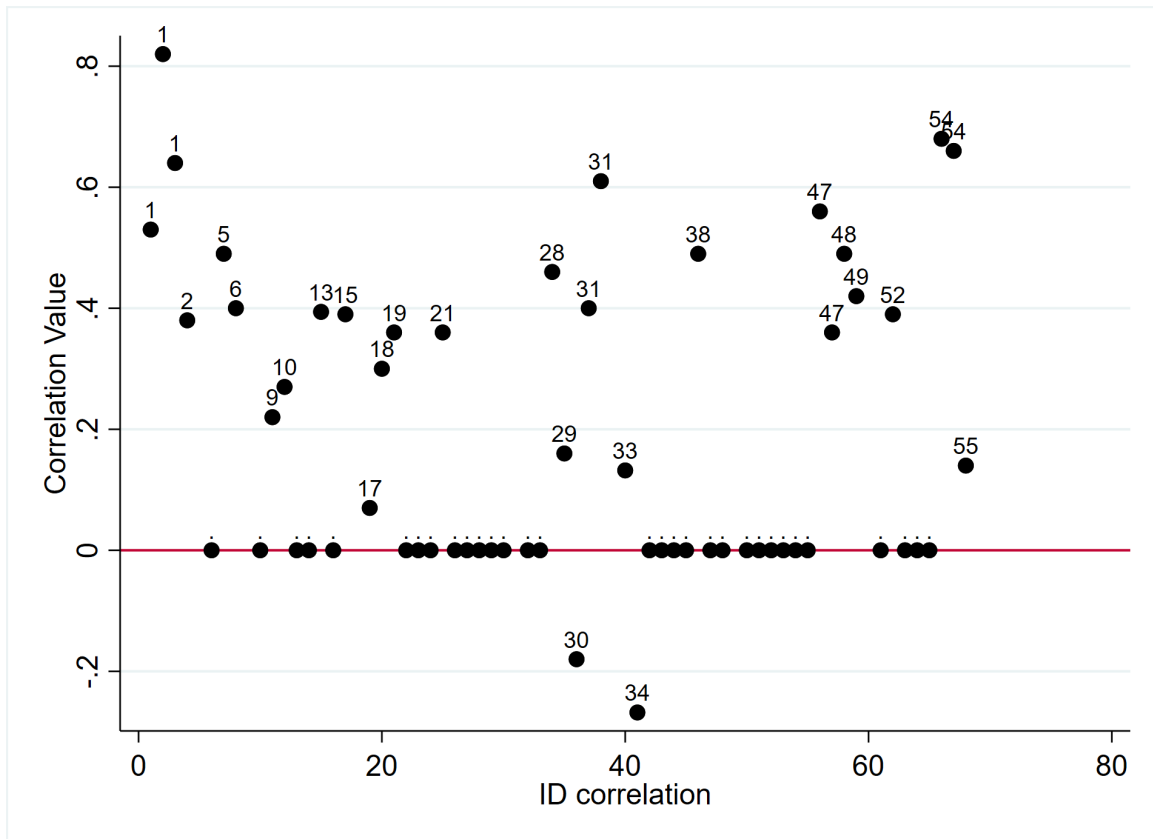


Table 1.3: Summary of the variables

Note: ^aThe negative CRAA were not included in Correlation Sign.

^bThe reference category is marked with (*).

Variable	Description	Status	Categories	N
Correlation Sign ^a	The sign of the observed correlation	Response variable	Positive ^{a,b} Zero	31 31
Ambiguity Elicitation Format	The format in which lotteries were displayed in the ambiguity elicitation task	Explanatory variable	Choice List* Sequential Lottery Choice	33 35
Options Presentation	Joint or separate displaying of risk and ambiguity options	Explanatory variable	Separate* Joint	53 15
Population	The type of population who participated in the experiment	Explanatory variable	Students* Non Students	43 25
Sample Size	The number of participants of the experiment(s) in the considered study	Explanatory variable	-	-
Ambiguity Implementation	How ambiguity were presented to the subjects: total ambiguity, where no probability information were given, imprecision where an interval of probability was given, natural, where probability information depended on a non-artificial phenomenon	Explanatory variable	Total* Imprecision Natural	51 13 4
Incentive	How subjects were paid: according to the lotteries' outcomes (Real monetary incentives) or not (Hypothetical)	Subset (Real Payoffs)	Real Payoffs* Hypothetical Payoffs	60 8
Outcome Domain	The sign of lotteries payoffs in the experiments	Subset (Gain Domain)	Gain Domain* Loss Domain	59 9

1.3.3 Analysis

We further specified exact logistic regression models with the *Correlation Sign* as the dependent variable. We excluded negative correlations due to the interpretation difficulties associated with them (see Section 1.2). The independent variables were *Options Presentation*, *Ambiguity Implementation*, *Ambiguity Format*, *Sample Size*, and *Population*. They are all binary, except for *Ambiguity Implementation* and *Sample Size* (considered as ordinal). The *Risk Format* variable was not considered due to its near-perfect correlation with *Ambiguity Format*.

Exact logistic regression is a method for estimating parameters in the logistic model. It utilizes the conditional distribution of parameters sufficient statistics and produces conditional maximum likelihood estimates. Cox and Snell (1989) introduced this approach as an alternative to maximum likelihood estimation, which can be unreliable in small sample sizes or when the data are sparse or skewed. In traditional logistic regression, Z statistics are based on asymptotics. However, the p-values generated by exact logistic regression are based on the conditional distribution of sufficient statistics for individual parameters given the values of the other parameters. This results in accurate sufficient statistics (Mehta & Patel, 1995).

We performed the exact logistics estimations on the full data set and on 3 subsets based on the most frequent categories of *Outcome Domain* (**Gain Domain**), *Incentive* (**Real Payoffs**), and their intersection (henceforth represented by \cap). The subsets names will henceforth be in **bold**. The intersection of subsets corresponds to experimental studies featuring the following characteristics: **Gain Domain** \cap **Real Payoffs**. The subsets were chosen because numerous experimental studies in the literature generally have these features.

All observations of *Correlation Sign* have been treated as independent, although some originate from the same study (see Table 1.2). This is because we only reported several correlations from the same study when the experimental conditions

involved in its calculation were strictly different (see Section 1.3.2). We therefore assumed that the common experimental conditions of the study did not influence the different correlation signs computed within the study. Exact logistic estimations are also demanding on computing memory. For this reason, we divided the sample size variable (ordinal) into 20-quantiles (ventiles). Such a transformation minimizes information loss while lightening calculations. In the next section, we examined the stability of the results following this manipulation.

To check the robustness of the results, we conducted permutation tests using Monte Carlo permutations. Permutation tests are used to determine the significance of a test statistic by rearranging the order of observed values in a variable. It involves comparing the observed statistic with a distribution generated by randomly permuting the values N times. For instance, let's consider the above-mentioned relationship $Pr(Y_i | x_i)$. By holding the x order fixed, the y order is rearranged in all possible ways. $P^* = Pr(Y_i^* | x_i)$ is computed for each permutation of Y_i^* , with P^* the permutation distribution of the relationship. The permutation is made under the weak assumption of exchangeability, i.e., that the ordering of the elements of y is independent of the ordering of the elements of x , given the observed values of both. 1000 permutations were conducted for this study. The key advantage of a permutation test is the ability to control for Type I errors (false positives).

1.4 Results

This section provides the statistical analysis results related to the research question. Unless stated otherwise, the following statistical tests have **0.10** as level of significance.¹⁵ The results are reproducible using the data and statistical code on an OSF repository ([link here](#)).

¹⁵The significance level was chosen according to the low data volume we operate with. A detailed discussion on the rationale underlying this approach can be found in J. Kim (2015).

Before proceeding with the estimations, we first conducted Fischer exact tests to check for nonrandom associations between *Correlation Sign* and the other variables. Only *Option Presentation* and *Sample Size* are significantly associated with *Correlation Sign* (Fisher's exact = 0.07 and 0.02, respectively). The association is weak for *Option Presentation*. We reproduced these results with exact median tests.

We then estimated the exact logistics models by integrating the other variables. It also allows checking whether the effects identified by Fischer exact tests persist when introducing other variables. We considered the full dataset and the 3 subsets detailed in Section 1.3. Table 1.4 provides the estimation results. The response variable is *Correlation Sign*. Constant terms do not add information to these estimates. They have therefore not been included in the table.

Table 1.4: Exact Logistic Estimation Tables (subsets included)

	<i>Correlation Sign</i> (Full dataset)	<i>Correlation Sign</i> (Gain Domain)	<i>Correlation Sign</i> (Real Payoffs)	<i>Correlation Sign</i> (Gain Domain \cap Real Payoffs)
<i>Options Presentation</i>	3,34* (0,08)	3,45* (0,09)	2,24 (0,27)	2,44 (0,23)
<i>Ambiguity Implementation</i>	0,73 (0,2)	0,69 (0,46)	1,07 (0,89)	0,96 (0,95)
<i>Ambiguity Format</i>	0,91 (0,88)	0,84 (0,80)	0,82 (0,77)	0,79 (0,74)
<i>Sample Size</i>	1,14** (0,01)	1,11* (0,05)	1,13** (0,02)	1,10* (0,09)
<i>Population</i>	3,18 (0,11)	2,94 (0,15)	2,44 (0,26)	2,28 (0,31)
Observations	62	53	55	47

Odd ratios reported
Sufficient Statistics (p-values) in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Results 1 to 3 concern the effect of the sample size. Results 4 and 5 concern the effect of risky and ambiguous options presentation. The last result concerns the null effects of the other variables considered in the analysis.

Result 1: The probability of observing a positive CRAA increases as the sample size used by the studies increases. Consequently, the probability of observing a null CRAA increases as the sample size decreases.

Result 2: The probability of observing a positive CRAA increases when the sample size used by the studies increases on the **Real Payoffs** subset.

Result 3: There is weak evidence that the probability of observing a positive CRAA increases as the sample size used by the studies increases on the **Gain Domain** and **Gain Domain** \cap **Real Payoffs** subsets.

Result 4: There is weak evidence that the probability of observing a positive CRAA increases when risk and ambiguous options are presented separately.

Result 5: There is weak evidence that the probability of observing a positive CRAA increases when risk and ambiguous options are presented separately on the **Gain Domain** subset.

Result 6: There is no evidence that the other variables considered have an effect on the probability of observing a positive CRAA, whatever the subset considered.

The effect of the *Sample Size* variable is robust irrespective of the number of quantiles considered.¹⁶ The odds ratios are not high, fluctuating between 1.10 and 1.14 for the sample size effect and between 1.10 and 3.14 for the option presentation effect.

We finally checked the robustness of the results by conducting permutation tests with 1000 repetitions. The effect of Sample Size persists on sets where it was already identified, except on **Gain Domain** \cap **Real Payoffs** where the permutation test failed to confirm the effect. The effect of options presentation is not confirmed on **Gain Domain** subset. The confirmed results of permutation tests are reported in Table 1.5.

¹⁶We have additionally estimated the linear models and then verified by Variance Inflation Factors tests that the models do not suffer from multicollinearity ($1.16 \lesssim VIF \lesssim 1.54$). The signs of the estimated effects also remain unchanged.

Table 1.5: Permutation Test Results

Variables	P*	Repetitions	p-value	[95% Conf. Interval]
<i>Sample Size</i> (Full dataset)	0,13	1000	0,01	[0.008419, 0.0246197]
<i>Sample Size</i> (Real Payoffs)	0,12	1000	0,02	[0.0195059, 0.0413847]
<i>Sample Size</i> (Gain Domain)	0,1	1000	0,06	[0.0478618, 0.0787799]
<i>Options Presentation</i> (Full dataset)	1,2	1000	0,07	[0.0576536, 0.0909142]

1.5 Discussion

This study aimed to investigate the underlying causes of CRAA. To achieve this, 68 correlation data from 55 research studies were reviewed and analyzed, considering characteristics such as elicitation format, sample size, outcome domain, presentation of ambiguous options, implementation of ambiguity, distributed incentives, and experimental population type. Upon analyzing the results of exact logistic estimations, it was observed that a small sample size has a greater probability of producing null correlations rather than positive ones. We weakly corroborated the suspicion of an effect of simultaneously presenting risky and ambiguous options (contrast effects) on the sign of CRAA, which mitigates but does not invalidate the intuition of Camerer and Weber (1992); Kocher and Trautmann (2013); Trautmann and Van De Kuilen (2015). The former result is not surprising since the greater the sample size used to conduct a correlation test, the greater its statistical power. Indeed, a sufficiently large sample size better represents the population from which it is drawn because its parameters are less affected by randomness (Halsey, Curran-Everett, Vowler, & Drummond, 2015).

Obviously, the present study may also be subject to the power issues induced

by the use of a small sample (our dataset contains 68 observations). Although the use of exact statistical methods and robustness tests can reduce these issues, the weak evidence found for the effect of contrast effects (Option Presentation) will require further investigation with a larger study subset to reveal stronger evidence or an experiment specifically designed to investigate this issue. Other variables that could bias the measures of risk measures and ambiguity attitudes (non-monotonic preferences, etc.) will also need to be considered.

The study could benefit from the progress made in the field of neuro-economics. For example, Huettel et al. (2006) shows that risk and ambiguity activate two distinct brain regions. However, little is known about these regions' simultaneous or sequential activation conditions. To address this issue, it will be necessary to expose the subjects to different stimuli (different experimental tasks, formats, domains, payments, etc.) and observe which stimuli their brain regions are more sensitive to. Such a study's results significantly improve our understanding of the nature of the relationships between risk and ambiguity attitudes.

Chapter 2

Ambiguity in human-based vs. automated dispute resolution procedures¹²

¹This chapter has been co-authored with Agnès Festré (Université Côte d'Azur), Michela Chessa (Université Côte d'Azur), Giuseppe Attanasi (Sapienza University of Rome) and Marta Ballatore (Université Bourgogne Franche-Comté).

²This study results from an interdisciplinary collaborative project involving academics in economics, management, law, and computer science, as well as industrialists. The objective of the project is to assess the potential changes for end-users when introducing a Blockchain-based Smart Contract in the context of connected vehicles.

2.1 Introduction

Our research is originally motivated by the emergence of blockchain-based smart contracts (hence, BCT-based SC) in the context of insure-tech and digitalization in the insurance sector (Bauer, Tyler Leverty, Schmit, & Sydnor, 2021). Several economic, informational, and organizational advantages of BCT have been highlighted in the literature (see, e.g., Ali et al., 2021), among which the fact that BCT can provide new governance mechanisms to organize collaborations (Lumineau, Wang, & Schilke, 2021). Research on BCT also shows that the most appealing benefits offered by BCT are not entirely related to its functionalities but also come from associated elements such as the *smart contract* (Halaburda, 2018).

Szabo (1997) defines the Smart Contract (henceforth, SC) as a computerized transaction protocol that “self-executes” the terms of a contract. Overall, a SC provides a wide range of opportunities for economic activities, from lower transaction costs to more objective and transparent procedures. For instance, Buterin (2014) underlines the potential of a SC in reducing downtime, censorship, or fraud in several transaction scenarios. Since a SC leverages the BCT environment to verify, validate, and execute the terms and clauses of an agreement, BCT and SC can be seen as complementary technologies for disruptive use cases, from financial services to Internet of Things (henceforth IoT) -based offerings. BCT can solve security, maintenance, and privacy issues of IoT applications (Hassan, Rehmani, & Chen, 2019).

The extant literature includes several studies on the positive effect on economic exchanges of BCT-based SCs (see, e.g. Davidson, De Filippi, & Potts, 2016; Eenmaa-Dimitrieva & Schmidt-Kessen, 2019; Suliman, Husain, Abououf, Alblooshi, & Salah, 2019) resulting in a digital transformation at the internal and ecosystem levels. However, until now, an important issue for the successful implementation of such technologies, namely, end-users’ perception, has not received much attention. Users’ willingness to embrace new innovation and technology has been shown to be

a critical factor (Kleijnen, Lee, & Wetzels, 2009; Taherdoost, 2019). Studying the needs of individuals and the drivers of adoption is the first step for companies to determine an appropriate way to develop the technology system. Without a user-centric perspective, the risk of technology rejection is high. This is particularly relevant in the case of BCT-based SCs, where adoption implies a disruptive change in end-users' habits and reference points. Indeed, BCT-based SCs are supposed to replace human intermediation services, by substituting a computer program-based procedure for an expert-based one.

An extensive literature on algorithms has already tackled related issues. Several studies have demonstrated the influence of several determinants on algorithm aversion in the case of expert replacement (e.g., Dietvorst, Simmons, and Massey (2015), Prahl and Van Swol (2017)). Research has also evidenced potential ways of fostering the acceptability and adoption of algorithms (e.g., by training (Green & Hughes, 1986) or providing the right incentives (Alexander, Blinder, & Zak, 2018)). However, this literature presupposes the existence of already existing prototypes or established technologies. This is the case in the financial sector, where, for example, the adoption of the BCT-based cryptocurrency Bitcoin has already been largely discussed (see, e.g. Catalini & Tucker, 2017). However, it is difficult to replicate these studies in other sectors, such as the car insurance sector, in which a stable use or a suitable prototype of BCT-based SCs does not exist yet. In a nutshell, in many sectors, the desirable conditions seem not to be in place to assess the technological, social, and economic factors of BCT-based SC adoption with traditional methodologies (e.g., questionnaires and interviews).

By contrast, we claim that behavioral and experimental economics represents an interesting candidate for analysis beforehand (i.e., without having a prototype or a usage to test), the willingness to accept disruptive technologies such as BCT-based SCs by potential users. As put forward by Gupta, Kannan, and Sanyal (2018) the method of experimental economics can potentially add tremendous value in the information systems domain (i.e., in studies on electronic commerce and busi-

ness, IS use/adoption, human-computer interaction, and digital transformation, even though in a somewhat provocative way, he also identifies the limitations of already existing experimental studies in the field). In particular, one added value of lab experiments is that they allow to measure usually non-observable or hardly observable individual features such as preferences toward risk or social preferences. For instance, within the algorithm adoption literature related to information systems (see, e.g., Hashim, Kannan, & Maximiano, 2017), some behavioral economics studies have shed light on attitudes toward algorithms by investigating the conditions under which context-specific behavioral designs can improve algorithmic decision (Burton, Stein, & Jensen, 2020). For reasons that will be developed below, we argue that a laboratory experiment investigating the choice determinants of the substitution of a BCT-based SC for an expert-based one may be particularly useful in the context of our case study of a car insurance contract. Studies in laboratory experiments have already investigated whether humans prefer to depend on the decisions of others or states generated by a computer in non-strategic uncertain settings (Farjam, 2019), showing how often humans may prefer a so-called computerized uncertainty.

More specifically, our paper aims to investigate the following research questions: Which typology of end-users may be more open to digital services based on BCTs and SCs? Or more importantly, can we disentangle the effect of uncertainty preferences in the degree of end-users' openness to disintermediation? While our initial motivation was the insurance industry, the questions raised here also relate, in an abstract way, to other digital services for connected vehicles (e.g., vehicle rental or repair). Even if we narrowly focus on the case of a BCT-based SC for a one-sided car accident (see Section 2.2 for a detailed description of our case study), other possible applications are manifold.

We address these questions by relying on a non-contextualized laboratory experiment. We proposed to our subjects a decision task representing the key features of the decision-making process provided by a BCT-based SC. In particular,

we borrowed elements from H. Kim and Laskowski (2017). They describe BCT as a technology-based procedure reducing uncertainty in economic decision-making, and SC as a key feature that takes distributed ledgers a step further in reducing uncertainty since the integration of SCs mitigates the complexity of BCT-based value exchanges. In practice, the real-world decision-making problem under scrutiny – adopting or not a BCT-based SC – is characterized by the simultaneous presence of risky prospects and ambiguous probabilities. Accordingly to the one-sided accident case study, in the status quo situation, the decision-maker has to rely on a third party, the expert, whose moves are unpredictable. This makes the probability of occurrence of the risky event ambiguous, i.e., not objective and/or not known a priori and/or not commonly agreed upon, according to the usual definition of the concept of ambiguity in economics (see, e.g., the experimental papers by Attanasi & Montesano, 2012; Liu & Colman, 2009). Indeed, the interaction with an expert is modeled as ambiguous since the end-user cannot know exactly what the expert takes into account when evaluating the accident dynamics. While excluding that the expert acts strategically, the expert's final assessment is a black box for the insured end-user, since he/she does not know the expert's objective function. Instead, with the introduction of a SC, whose terms are stored in the BCT before its self-execution and cannot be changed by anyone, the probability of the risky event becomes unambiguous, both because it becomes known a priori by the user and because a third party's action no longer being requested, the user's decision making loses complexity. The insured car user can anticipate how the BCT-based SC will perform under the insurance contract terms using the driving data sent by the sensors, which makes him/her aware of the specific probability of a refund after a specific car accident. Thus, our analysis is consistent with the literature that has addressed the economic benefits of BCT-based SCs to the insurance industry (see, e.g., Hans et al., 2017).

A questionnaire was sent one week before to integrate experimental data with experimental participants' demographics, personality traits, and car use experi-

ence with owning and/or using a car. The main purpose of the pre-experimental questionnaire was threefold. First, we aimed to understand the effect of standard idiosyncratic features (e.g., gender, age, education) on the propensity to move from an ambiguous system (third party) to an unambiguous one (BCT- based SC) in our experimental design. Second, given that BCT is defined as a “trust machine” (Berkeley, 2015), to have a “trustless environment”, the pre-experimental questionnaire helped us identify the effect of individual traits related to the case study, such as trust in others and in technology, and past experience with car insurance companies. Third, the pre-experimental questionnaire allowed us to compare two groups of potential end-users: those with experience with vehicles and those without. The latter point is crucial in our analysis, as different traits may have a different role in the scenario depending on the users’ experience. For example, Ma, Zhu, Hu, and Chiu (2018) shows that driving experience impacts driving behaviors such that those with more experience are less likely to have an accident. However, the link between experience and the propensity to prefer a particular claims assessment system is more elusive. One explanation is that experienced drivers were potentially more likely to interact with insurance providers and thus to encounter (or hear about) negative experiences, which may lead them to prefer in the future a less complex, less ambiguous system with minimal human intervention such as a SC. On the other hand, drivers with no experience may still perceive a traditional human-based system as the most suitable, despite its complexity. In fact and as predicted, we find that the role played by the behavioral traits in influencing user’s openness to BCT-based SCs depends primarily on the user’s experience of owning or using a car. In particular, for subjects with no experience, we find a significant effect on their openness to SCs of their ambiguity preferences. For experienced users, ambiguity preferences do not have a significant impact on SC adoption, while risk preferences, generalized trust, and optimism do. This indicates that an experience with a given status quo will necessarily impact the user’s openness to a new alternative digital technology and that the behavioral traits underlying this openness

are status quo dependent. Our results will open a multidisciplinary discussion for an end-user-centric approach to BCT-based SCs development. On the one hand, our results may be exploited for policies aiming to make end-users understand the changes these unfamiliar technologies bring. On the other hand, they might be employed in the different testing phases of a BCT-based SC design.

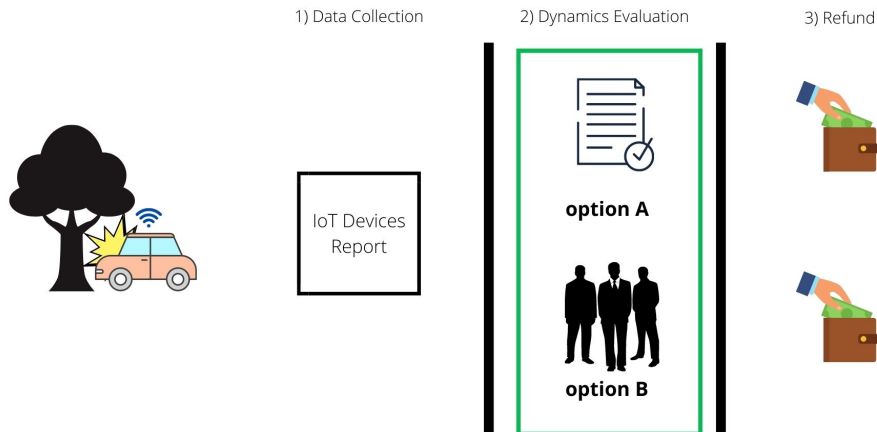
The rest of the paper is organized as follows. Section 2.2 illustrates our case study. In Section 2.3 we present our empirical methodology. The data analysis strategy and the results are detailed in Section 2.4. Section 2.5 concludes by discussing the implications of our results on policy development for blockchain-based technologies.

2.2 Case study

We compare automation provided by a BCT-based SC with a human expert-based procedure to assess individual behavior. Transposed to the insurance case study (see Figure 2.1), we propose to our subjects 2 systems to assess driving behavior at the time of a car accident: a BCT-based SC (option A), and a traditional expert-based insurance procedure (option B). Both procedures start with a one-sided car crash and data sent from the connected car sensors (step 1) and end with a potential refund (step 3).

The experimental design of our lab experiment is a simplified representation (also called a toy model) of Step 2 - Dynamics Evaluation. Since our goal is not to study the effectiveness of one procedure over the other but to focus on behavioral traits influencing end-users' openness to SC, the two options are simplified as follows to elicit individual preferences. Option A - the SC - is considered a risky solution. All parameters of the SC are stored in the BCT before the accident occurs, and the evaluation of the driving behavior and of its consequences on the refunding process is objective. The expert-based procedure (option B), on the other hand, refers to an ambiguous solution, mainly because the judgment of an insur-

Figure 2.1: The case study: Insurance process after a car crash



ance expert is subjective and cannot be predicted a priori. For example, it is almost impossible to calculate the probability that he/she will discover the driver's bad driving behavior even if IoT sensors send data.

2.3 Methodology

This section contains a general presentation of our research procedure, followed by a detailed illustration of the two parts of our empirical study, namely an online questionnaire and a laboratory experiment.

2.3.1 Procedure

Our empirical study consisted of two parts: an online questionnaire and a laboratory experiment. It involved a sample of 157 subjects, 66 percent females and 34 percent males, aged between 18 and 42, with an average age of 23 years old (see the Appendix for more details about our sample). 130 participants were students from different departments of the Côte d'Azur University, while 27 subjects had already finished their university education. The representativeness of the sample

was controlled through the repetition of experimental sessions, where a statistical analysis revealed no significant differences between the different groups.

Although still debatable in the community of experimentalists, the use of a predominantly student population is well suited to this study: they correspond to the future users of the technology under investigation in this paper and allow for better control of the basic misunderstandings (language, perception of probabilities, etc.) that could strongly bias the results if applied to a more general population.

The experiment was programmed in z-Tree (Fischbacher, 2007), and subjects were recruited through ORSEE (Greiner, 2015). The Ethical Committee of our university approved the recruitment and experiment processes under protocol # 2019-17-10.

Simple participation in the questionnaire and the experiment were rewarded with a participation fee of €8, to which a variable amount was added depending on the subject's decision and on the results of some random draws, ranging from a minimum of €4 to a maximum of €32. On average, each participant received a final payment (including the participation fee) of €21.08.

As far as some random draws were concerned, we opted for a mechanical technology through cards drawn by a monitor. The monitor acted independently from the experimenter and was drawn at random among the subjects at the beginning of each session of the experiment by the experimenter. The monitor was added to convince subjects that the results of the random draws by which payments were determined were not controlled by the experimenter or predetermined. We refer to this monitor in the following as the "neutral subject". The neutral subject participated as all the others in the online questionnaire, but then he/she remained passive during all the experimental tasks and was rewarded a fixed amount of €32, corresponding to the largest possible gain, plus the €8 of the participation fee. The choice of rewarding the neutral subject with the highest payment possible was in order not to make him/her feel disappointed by having lost an opportunity to win a larger gain because of a fully random choice in which he/she had no voice.

Moreover, the high payment was to reward the high responsibility of his/her work and to ensure he/she took his/her role seriously. In fact, the neutral subject was in charge of all the random draws for the payments of subjects in the session. These draws were done individually and anonymously in a separate room.

Since there was one passive neutral subject per session, and since we had a total of 13 sessions, our study is based on observations from the remaining population of 144 subjects. The Appendix details how the payments were determined.

2.3.2 The questionnaire

In the first part of our empirical research, the subjects participated in an online exploratory closed questions survey e-mailed one week before the laboratory session (see the Appendix). The survey included elements designed to measure 3 main sets of constructs: (i) demographics, (ii) personality traits, (iii) car use experience with owning and/or using a car.

Demographic questions were constructed to collect information about *gender*, *age*, and *education* level.

Personality traits questions were intended to check for perceived dishonesty, attitude toward ambiguity, optimism, attitude toward risk, generalized trust, and trust in technology. The question about *perceived dishonesty* was elicited *à la* Attanasi, Bucciol, Cicognani, Montinari, et al. (2017). The question was phrased as “How do you consider the level of dishonesty in the following contexts?” and then a list of 9 contexts was proposed, including, e.g., car repair shops, Amazon, or the Banking sector. A subject could check a perceived level of dishonesty on a scale from “Low” to “High”. An “I’m not sure” option was also included. Attitudes toward *ambiguity* and *optimism* were elicited *à la* Cavatorta and Schröder (2019) by asking the subject how much he/she agrees with a given set of statements. Possible answers ranged from “Totally disagree” to “Totally agree”, with a “I don’t know” option. Between the large set of questions proposed in the paper of Ca-

vatorta and Schröder, we chose the ones that were, in our opinion, closer to the real-world experience of a general subject who participated in the survey. In particular, we retained the response to the following statement, “In uncertain times, I usually expect the best” as a measure of *optimism* (Chang, Maydeu-Olivares, & D’Zurilla, 1997). Attitude toward *risk* was elicited *à la* Dohmen et al. (2011) by directly asking the subject to declare, from a scale from 1 to 10, how much he/she loves risk. *Generalized trust* questions were taken from the Master questionnaire (2017-2020, wave 7) of World Values Survey and included two questions, the first one asking whether the subject thinks that the others should be trusted, with a “yes-no” response, and the second one asking the subject how much he/she trusts others on a scale from 0 to 10. Finally, as the last personality trait, we considered what we call *trust in technology*. This trait was intended to investigate faith in general technology *à la* Mcknight, Carter, Thatcher, and Clay (2011), i.e., individuals’ beliefs about attributes of Information Technology (IT). We defined two symmetric questions to the ones used for generalized trust, then asked a subject if he/she trusts digital technologies, with a “yes-no” answer, and how much he/she trusts digital technologies from a scale from 0 to 10.

User’s *car experience* with owning or using a car was addressed by asking the subjects if they have a car, or not, and if they use a car, or not.

2.3.3 The lab experiment

In the second part of our empirical research, the subjects participated in a laboratory experiment. The experiment was run at the Laboratory of Experimental Economics of Côte d’Azur University (LEEN-NiceLab) in December 2019.

In total 13 sessions were run with an average of 12 subjects each, including the neutral subject. At the beginning of each session, all subjects except the neutral one were randomly allocated to a computer terminal, and they received instructions. Instructions showed a numerical example of computing the payoff for each of the

4 parts of the experiment to make sure that subjects understood the game correctly (see the Appendix).

Experimental Design

The lab experiment consisted of 4 independent parts. The set of instructions for the 4 parts is provided in the Appendix.

In all the 4 parts, to make the information about the random draws (from now on, lotteries) more directly accessible to the subjects, the implementation of the lotteries was made by drawing some cards from a deck of 20 cards, 19 blue and 1 red (from now on, labeled RC). A subject's decision consisted of choosing between some (eventually compounded) lotteries. A lottery provided a given number of cards to be drawn from the deck and then, consequently, a different probability of drawing the RC. Drawing the RC led to a low return of €4. Not drawing the RC led to a return in euros equal to twice the number of drawn cards (from which the maximum payoff equals €32).

PART 1 of the experiment aimed at eliciting *behavioral risk* preferences of subjects. It implemented a risk task, i.e., a task in which the probabilities of incurring a bad outcome in the different lotteries were known. As we have already explained in the Introduction, a risky lottery was assumed, in our study, to mime a BCT-based procedure, i.e., the SC. Our risk elicitation task was conceived as a task whereby every subject had to choose his/her favorite among the 7 lotteries in Table 2.1.

Table 2.1: Behavioral risk preference elicitation.

Lottery	# of cards to be flipped over	% changes of getting the RC	Return	Expected value
1	4	20%	8€ if no RC / 4€ if RC	7.2
2	6	30%	12€ if no RC / 4€ if RC	9.6
3	8	40%	16€ if no RC / 4€ if RC	11.2
4	10	50%	20€ if no RC / 4€ if RC	12
5	12	60%	24€ if no RC / 4€ if RC	12
6	14	70%	28€ if no RC / 4€ if RC	11.2
7	16	80%	32€ if no RC / 4€ if RC	9.6

These lotteries are characterized by a clear trade-off between the amount of money that could be earned and the likelihood of obtaining it. We chose not to offer the subjects too extreme choices (e.g., drawing less than 4 cards or more than 16) to avoid situations where the corresponding probability of getting the RC was too close to 0 or to 1, respectively. Choosing a high number of cards to draw from the deck increased not only the potential payoff but also the probability of drawing the RC and thus losing part of this potential payoff. Lines of Table 2.1 correspond to an increasing number of cards that will be flipped over (indicated in column 2), therefore increasing the probability of getting the RC, in column 3. Column 4 gives the payoff a subject obtained when he/she did not get the RC and when he/she got it, respectively. The last column of Table 2.1 gives the expected payoff of each lottery. The information about the probability of getting the RC and the expected value for each lottery are presented here to better explain the task to the reader. However, such information was not provided to the subjects.

Note that this task differs from the traditional Holt and Laury (2002) lottery choice task. In fact, we did not intend to measure subjects' risk aversion but rather to evaluate their risk attitudes in terms of a threshold of risk acceptance in line with the "Bomb" Risk Elicitation Task (BRET) by Crosetto and Filippin (2013). In our setting, a risk-neutral subject (i.e., an expected utility maximizer) should choose Lottery 4 or Lottery 5. Choosing Lottery 6 or 7 denotes a risk-loving subject, while lottery 1-3 is typical of risk-averse individuals.

PART 2 of the experiment aimed at eliciting *behavioral ambiguity* preferences. While the BCT-based procedure (SC) corresponds to the risky option, as in PART 1, the expert-based procedure is assumed to contain an ambiguous component due to human error.

We, therefore, investigated how subjects behaved when facing the choice between a risky lottery (Option A) and an ambiguous one (Option B), i.e., a lottery in which the probability of incurring a bad outcome is unknown.

Formally, the risky lotteries of this PART 2 were the same 7 lotteries of PART

1. In the ambiguous ones, on the other side, a subject was facing analogous corresponding lotteries, having the same outcomes as the risky ones, but for which he/she could not know exactly the number of drawn cards and, consequently, for which it was impossible for the subject to compute the probability of drawing the RC, thus simulating the assessment process using human cognition assumed to be sufficiently complex to be deterministic.

In Table 2.2 below, the second column (option A) contains the same 7 lotteries that we had in PART 1 (within brackets the information about the corresponding probability of getting the RC). Column 3 (option B) presents the equivalent ambiguous lotteries. Column 3 indicates the interval of the number of cards to draw in the ambiguous lotteries, each corresponding to three different lotteries that yield different probabilities of obtaining the RC and from which the subject could not know which one he/she would have really faced. Differently from PART 1, in PART 2 a subject had to make a choice, for each line, between the risky lottery and the ambiguous one, thus making in total 7 different choices. Observe that, for each line, assuming an equal probability of facing one of the three lotteries of option B (what is usually referred to as a uniform second-order probability), the expected number of cards to be drawn is identical between option A and option B, and so is the probability of drawing the RC. However, the ambiguity was given by the fact that the second-order hidden probability was in fact unknown. As a result, a subject could only know that by choosing option B he/she would have faced a lottery with a probability of obtaining the red card that lay in an interval around the probability of the risky choice in the corresponding option A, but without knowing its value with certainty.

Observe how, according to some of the most well-known theoretical models to describe an attitude toward ambiguity, the behavior of a subject facing such a task is expected to be extremely regular. With a smooth-ambiguity model (Klibanoff et al., 2005), for example, an ambiguity-averse subject should always choose option

Table 2.2: Behavioral ambiguity preference elicitation.

Lottery	# of cards to be flipped over	% changes of getting the RC	Return	Expected value
1	4 (20%)	Btw 3 and 5 cards	8€ if no RC / 4€ if RC	7.2
2	6 (30%)	Btw 5 and 7 cards	12€ if no RC / 4€ if RC	9.6
3	8 (40%)	Btw 7 and 9 cards	16€ if no RC / 4€ if RC	11.2
4	10 (50%)	Btw 9 and 11 cards	20€ if no RC / 4€ if RC	12
5	12 (60%)	Btw 11 and 13 cards	24€ if no RC / 4€ if RC	12
6	14 (70%)	Btw 13 and 15 cards	28€ if no RC / 4€ if RC	11.2
7	16 (80%)	Btw 15 and 17 cards	32€ if no RC / 4€ if RC	9.6

RC denotes the red card. Under the assumption that in Option B the distribution between the three lotteries is uniform, the expected value of Option A and Option B is the same for each line.

A, while an ambiguity-lover would prefer option B. Evidence shows that the real behavior of the subjects is instead much more complex. For this reason, in the data analysis in Section 2.4, we computed the number of option B choices to indicate a subject's ambiguity-loving attitude.

PART 3 was introduced as an intermediary step to isolate a well-known attitude in behavioral sciences that could bias decisions in our experimental design: aversion to Reduction of Compound Lotteries (RoCL), first described by Segal (1987). Indeed, with equal probabilities, some individuals would tend to prefer random processes in single stages rather than those consisting of several stages. For example, drawing one by one up to 10 out of 20 cards gives the same probability of finding the RC as drawing 10 out of 20 cards at once, but may not be equally interpreted by individuals. Then, the expert-based procedure in option B in PART 3 was formalized as a compounded lottery which did not alter the probability of finding the RC when compared to option A, but that could however be perceived by the subjects otherwise. Contextualized to the case study, this was meant to identify within the population of future users those who would choose a BCT-based SC, not because of its deterministic process, but because it does not involve a third party. This has strong implications when it comes to forming sales arguments, for example.

Table 2.3 gives the two options over which each subject expressed his/her pref-

erence for each line. Option A still corresponds to the risky lottery (as in option A of PART 2) whereas option B describes a compound lottery, i.e., the same lottery as in option A plus an additional second-order lottery whereby a given number of cards is flipped over if the RC is returned or another given number of cards to be flipped over in case the RC is not returned. In options A and B, all the probabilities were known to the subjects. Lotteries in options A and B have been set to yield the same probability of getting the RC and therefore, the same expected return (last column) so that after reducing option B to simple lotteries, options A and B become identical from a probabilistic point of view.

Table 2.3: Behavioral RoCL preferences elicitation.

Lottery	# of cards to be flipped over	% changes of getting the RC	Return
1	4 (20%)	4 cards and if RC then another 10 cards 4 cards and if not RC then another 2 cards	8€ if no RC / 4€ if RC
2	6 (30%)	6 cards and if RC then another 10 cards 6 cards and if not RC then another 4 cards	12€ if no RC / 4€ if RC
3	8 (40%)	8 cards and if RC then another 10 cards 8 cards and if not RC then another 7 cards	12€ if no RC / 4€ if RC
4	10 (50%)	10 cards and if RC then another 10 cards 10 cards and if not RC then another 10 cards	16€ if no RC / 4€ if RC
5	12 (60%)	12 cards and if RC then another 13 cards 12 cards and if not RC then another 10 cards	24€ if no RC / 4€ if RC
6	14 (70%)	14 cards and if RC then another 16 cards 14 cards and if not RC then another 10 cards	28€ if no RC / 4€ if RC
7	16 (80%)	16 cards and if RC then another 18 cards 16 cards and if not RC then another 10 cards	32€ if no RC / 4€ if RC

RC denotes the red card.

Finally, **PART 4** of the experiment resumed our case study, namely, a situation where subjects chose between the BCT-based SC (the risky lottery in option A as in PART 2 and PART 3) and the expert-based procedure (option B) which consisted of a compound lottery with unknown probabilities in one of the two stages, then a combination of the options B previously present in PART 2 and PART 3, respectively. The 7 choices of this last part of the experiment are summarized in Table 2.4.

Table 2.4: Smart Contract vs. Expert-based Procedure preferences elicitation.

Lottery	# of cards to be flipped over	% changes of getting the RC	Return
1	4 (20%)	4 cards and if RC then another btw 3 and 5 cards 4 cards and if not RC then another btw 1 and 3 cards	8€ if no RC / 4€ if RC
2	6 (30%)	6 cards and if RC then another btw 5 and 7 cards 6 cards and if not RC then another btw 2 and 4 cards	12€ if no RC / 4€ if RC
3	8 (40%)	8 cards and if RC then another btw 7 and 9 cards 8 cards and if not RC then another btw 3 and 5 cards	12€ if no RC / 4€ if RC
4	10 (50%)	10 cards and if RC then another btw 8 and 11 cards 10 cards and if not RC then another btw 4 and 6 cards	16€ if no RC / 4€ if RC
5	12 (60%)	12 cards and if RC then another btw 11 and 13 cards 12 cards and if not RC then another btw 5 and 7 cards	24€ if no RC / 4€ if RC
6	14 (70%)	14 cards and if RC then another btw 13 and 15 cards 14 cards and if not RC then btw 6 and 8 cards	28€ if no RC / 4€ if RC
7	16 (80%)	16 cards and if RC then another btw 15 and 17 cards 16 cards and if not RC then another btw 7 and 9 cards	32€ if no RC / 4€ if RC

RC denotes the red card.

2.4 Results

This section examines which personality and behavioral factors play a role in the potential adoption of SCs. First, we define our explanatory variables and our dependent variable, and we provide some preliminary statistics (Section 2.4.1). Then, we illustrate our regression analysis and present the results of our investigation (Section 2.4.2).

2.4.1 Variables definition and preliminary descriptive statistics

From the responses to the questionnaire (see the Appendix), we define the explanatory variables that describe the constructs we presented in Section 2.3.2. The corresponding variables are *Gender*, *Age*, *Car experience*, *Perceived dishonesty*, *Optimism*, *Risk attitude*, *Generalized trust* and *Trust in technology*. From the laboratory experiment data, we define the experimental variables we presented in Section 2.3.3. The corresponding variables are *Risk loving*, *Ambiguity loving*, *RoCL loving*, which are all still explanatory variables, and, finally, the dependent variable of our analysis, *Openness to SC*.

In our analysis, we wish to put the accent on the role of the current experience of potential users with cars. The motivation is twofold. First, we based our model on the insurance reimbursement process after a car accident. The experience of owning or using a car for young end-users constitutes already an experience of the classical uncertain environment around vehicles (e.g., the relationship with a trusted third party, the informational asymmetry in selling a second-hand car, etc.). Therefore, we suppose that the effect of the aforementioned traits could be heterogeneous between these two different groups of users. The second motivation is connected to the former. Our investigation aims to provide insights for SC adoption policies in the insurance sector. From the practical point of view, it is important to identify the personality and behavioral traits on which an *ad hoc* policy might affect the different groups of potential end-users. Hence, we split our subject pool into 2 groups:

- i. Subjects with *No experience*: represented by those subjects who do not own and do not use a car (as drivers) at the period of the study (*Car experience* = 0)
- ii. Subjects with *Experience*: represented by those subjects who do own and/or use a car at the period of the study (*Car experience* = 1)

Table 5 presents the summary descriptive statistics of our panel of variables for our total sample, for the sample restricted to users with *No experience* and for the sample restricted to users with *Experience*.

Table 2.5: Descriptive Statistics

	All subjects					No experience					Experience				
	Obs.	Mean	SD	Min	Max	Obs.	Mean	SD	Min	Max	Obs.	Mean	SD	Min	Max
Open. to SC	144	2.743	2.091	0	7	81	3.086	2.075	0	7	63	2.302	2.0449	0	7
Risk loving	144	4.375	1.443	2	7	81	4.407	1.464	2	7	63	4.333	1.426	2	7
RoCL loving	144	3.444	1.753	0	7	81	3.432	1.910	0	7	63	3.460	1.543	0	7
Ambiguity loving	144	3.611	1.743	0	7	81	3.679	1.836	0	7	63	3.524	1.625	0	7
Age	141	22.660	3.770	18	38	79	22.899	3.901	18	38	62	22.354	3.604	18	37
Gender	144	.340	.475	0	1	81	.358	.482	0	1	63	.317	.469	0	1
Education	144	.458	.5	0	1	81	.420	.497	0	1	63	.508	.504	0	1
Risk attitude	144	5.514	2.119	1	10	81	5.321	1.993	1	10	63	5.762	2.263	1	10
Generalized trust	144	4.389	2.300	0	10	81	4.259	2.355	0	10	63	4.556	2.234	0	9
Trust in tech	144	4.792	2.326	0	10	81	4.556	2.414	0	10	63	5.095	2.190	0	9
Perc. dishonesty	144	1.757	1.033	0	3	81	1.728	1.073	0	3	63	1.794	.986	0	3
Optimism	144	1.319	.973	0	3	81	1.173	.933	0	3	63	1.508	.998	0	3
Car Experience	144	0.437	0.497	0	1	81	0	0	0	0	63	1	0	1	1
<i>N</i>	144					81					63				

When performing the two-sample Wilcoxon rank-sum (Mann-Whitney) test, we observe no significant difference for the control variables such as *Gender*, *Education*, *Age*, and between the two groups of users with *Experience* and users with *No experience*. Therefore, the two sub-samples are comparable in terms of our control variables.

2.4.2 Main results

As expected, *Education* is statistically positively correlated to subjects' *Age* (Spearman's Rank Correlation: $\rho=0.620$, $p<.001$). Therefore, to avoid multicollinearity issues in our models, we retain the variable *Education*. We also retain this variable to catch the role in our regression model of the higher experience with probabilities the subjects potentially gained during university years.

In line with evidence from the literature (see, Halevy, 2007), we found that *Ambiguity loving* and *RoCL loving* are significantly correlated (Spearman rank correlation: $\rho = 0.363$, $p<.001$). Risk Loving and Ambiguity Loving are however not correlated, in line with the results of Bianchi and Tallon (2019), M. Cohen et al. (2011), and Corcos et al. (2012). Not surprisingly, the Wilcoxon matched-pairs signed-rank test reveals equality of distributions between *Ambiguity Loving* and *RoCL loving* and different distributions between *RoCL loving* and *Openness* ($z = -3.006$ and $p=.002$). Finally, running a power analysis between *RoCL loving* and *Openness* returns a high power of 0.820 . Then, Table 6 reports the results from four Ordered Logit regressions with *RoCL loving*. We also provide in the Appendix the ordered Logit regressions with *Ambiguity loving*, OLS regressions, and longitudinal logit regressions with *RoCL loving* and *Ambiguity loving*. Our results are robust irrespective of the econometric model implemented, and the empirical analyses use session-clustered standard errors. We also tested for multicollinearity in our OLS models using Variance Inflation Factors (VIF). Results show no multicollinearity that justifies further investigation.

Table 2.6: Ordered Logit regressions with RoCL explaining openness to SC

	(1) No Experience Openness to SC	(2) No experience Openness to SC	(3) Experience Openness to SC	(4) Experience Openness to SC
Risk loving	1.183 (0.184)	1.215 (0.227)	1.466** (0.190)	1.520** (0.216)
RoCL loving	0.623* (0.132)	0.598* (0.129)	1.236 (0.299)	1.162 (0.293)
Gender		0.821 (0.350)		0.664 (0.429)
Education		1.594 (0.798)		1.0178 (0.511)
Risk attitude (survey)		1.046 (0.112)		0.993 (0.147)
Generalized trust		1.163 (0.141)		0.721* (0.101)
Trust in tech		0.876 (0.0971)		1.092 (0.126)
Perceived dishonesty		1.132 (0.198)		0.723 (0.199)
Optimism		0.701 (0.218)		2.079* (0.640)
<i>N</i>	81	81	63	63

Exponentiated coefficients; Standard errors in parentheses

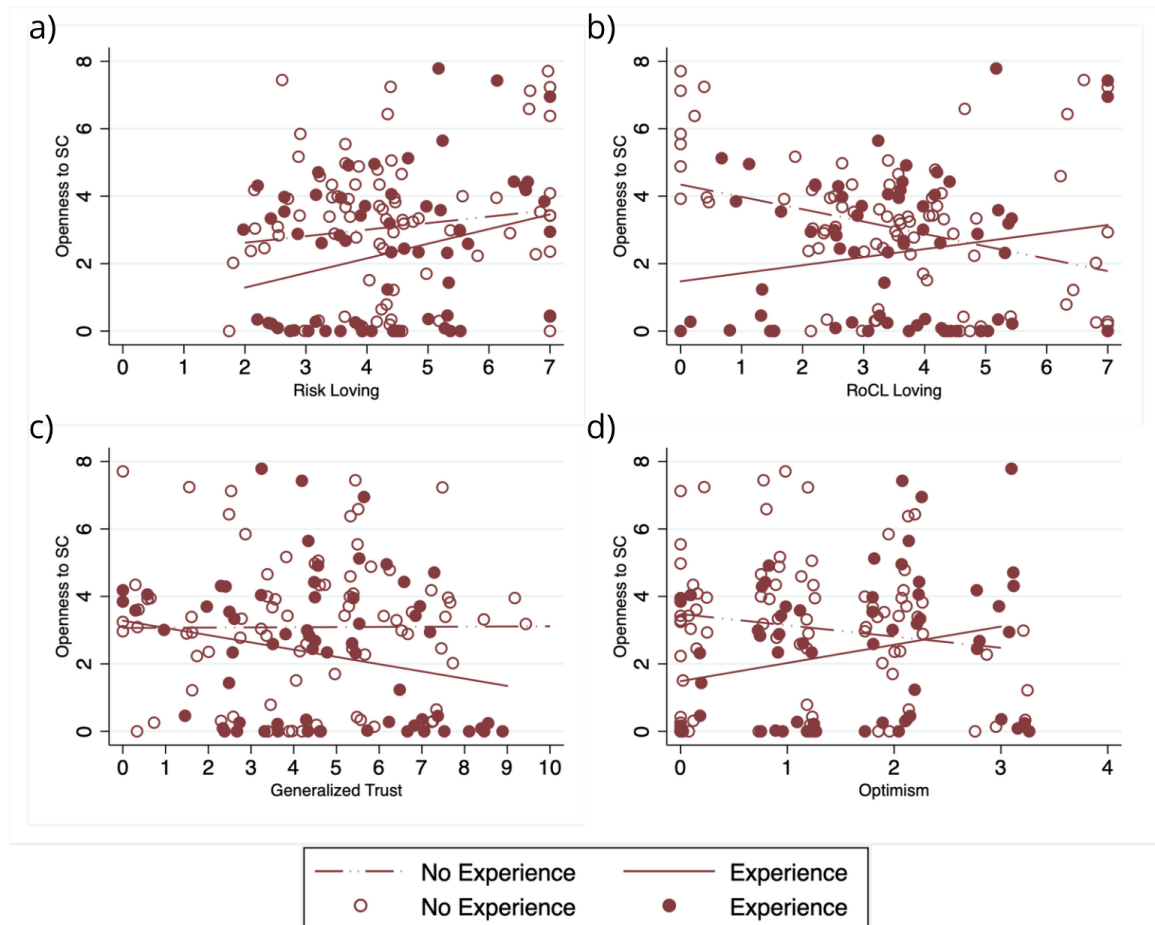
Standard Errors adjusted for 13 clusters in session

* $p < .05$, ** $p < .01$, *** $p < .001$

In Table 2.6, Ordered Logit regressions detail the odds ratios for each explanatory variable, and when we consider our pool of subjects divided between the ones with *No experience* and the ones with *Experience*. The regression tables show that the results remain robust regardless of including the survey variables alongside the experimental variables.

Figure 2.2 provides a visual representation of the relationship between the dependent variable *Openness to SC* and the significant variables from the regressions, *Risk loving*, *RoCL loving*, *Generalized trust* and *Optimism*, by showing linear correlations. The dashed lines refer to subjects with *No experience* of owning or using a car, while the continuous line refers to subjects with *Experience*.

Figure 2.2: Linear correlations between Openness to SC and significant variables from regressions



Looking at the two behavioral traits investigated through the lab experiment, *Risk loving* and *RoCL loving*, we notice that they both have a significant effect on *Openness to SC*, but each of them only on a specific group of users. *Risk loving* plays a significant positive effect in the case of subjects with *Experience*. The higher their risk preferences, the more likely they will choose a SC instead of the expert-based procedure. We can see the positive effect on Figure 2.2-(a). *RoCL loving* has a significant negative effect in the group of users with *No experience*. All of this leads to the following results:

Result 1: *Potential users with car experience and with risk-loving preferences are more willing to embrace a SC.*

Result 2: *Potential users with no experience and RoCL-averse (ambiguity-averse) preferences are more willing to embrace a SC.*

Generalized Trust and *Optimism* are the two variables from the survey that play a significant effect on *Openness to SC* for potential users with *Experience*. No variable from the questionnaire significantly affects end-users with *No experience*. *Generalized Trust* has a negative effect (see Figure 2.2-(c)): the higher the level of users' perception that most people can be trusted, the lower his/her openness to SC. Put differently, these subjects are more willing to choose an expert-based procedure instead of a SC. This leads to our third result:

Result 3: *Potential users with car experience and with high social beliefs are more willing to prefer the current system based on human activity.*

On the other hand, *Optimism* plays a significant positive role (see Figure 2.2-(d)): the more experienced individuals are optimistic, the more likely they are open to SC. This leads to our fourth and last result:

Result 4: *Potential users with car experience and with a high level of optimism are more willing to embrace a SC.*

As for the other variables defined from the questionnaire, none significantly affects *Openness to SC*. To be more precise, we see that *Perceived dishonesty* in the insurance sector has no significant effect on both groups' *Openness to SC*. This may be due primarily to two reasons. First, this variable is defined as a self-assessment of the perceived level of dishonesty in the insurance sector. Users may tend to declare something not representative of their own experience. Second, the question is about the insurance sector in general. Responses could be biased by other forms of insurance the subjects are more familiar too, e.g., medical private insurance that

is common in France. Also, *Trust in technology* does not play a significant role in explaining potential users' *Openness to SC*. A possible motivation can be linked to the fact that both groups are composed of young people who are used to using technology in daily life and would not be against a SC simply because of a fear of the technology behind it (namely, the BCT). Finally, another self-reported behavioral trait, *Risk attitude*, does not play a significant role in explaining *Openness to SC*.

2.5 Discussion and conclusion

According to the extant literature, the main disruptive value provided by a BCT-based SC lies in the avoidance of relying on trusted third parties. In many sectors such as the insurance sector, this may result in increased objectivity and transparency. In fact, as pointed out in the Introduction, a BCT-based SC tends to clarify the probability of occurrence of an event, whereas when the decision context involves a third trusted party, such as an insurance expert, the event remains hardly predictable. In this paper, we presented a lab experiment that, complemented by a questionnaire, can help our understanding of the choice determinants of a BCT-based SC (as compared to an ambiguous expert-based procedure) that drive the potential openness of end-users toward this new technology. In addition, to increase external validity, we designed it to be consistent with the context of a specific case study: the insurance process after a one-sided car accident.

Eliciting uncertainty preferences (*Risk loving*, *Ambiguity loving* and *Reduction of Compound Lottery (RoCl) loving*), we found consistent results in our comprehension of a BCT-based SC. In fact, we find that the role played by the behavioral traits in influencing user's openness to BCT-based SCs depends primarily on the user's experience of owning and/or using a car. In particular, experienced users may be prone to opt for a BCT-based SC instead of an expert-based procedure when they have higher *Risk loving* preferences (see **Result 1**). We interpret this result

as a propensity of risk-loving subjects to depart from a known status quo to test emergent technologies that would radically change their habits. *Ambiguity loving* and *RoCl loving* do not play a role in their choice, as, by being experienced, they are used to the current environment that is affected by high unpredictability and complexity of outcomes. Instead, a greater aversion to *Ambiguity* and *RoCL* (unpredictable and complex situations) among potential users with no experience may refer to a greater *Openness to SC* (see **Result 2**). In the specific case of high ambiguity aversion, users with no experience may prefer a clearer procedure evaluating a car accident: the coding of each step being stored in advance in the blockchain, driving behaviors, and contract clauses become the inputs for a more objective and more transparent outcome (e.g., a refunding process). Consistently, ambiguity-loving people may prefer the expert-based procedure, as the expert judgment is hardly predictable. In the specific case of RoCL preferences, individuals with a higher aversion to complex situations may prefer the SC because it provides a reduction in the evaluation steps by removing the expert involvement.

Regarding the traits measured with the questionnaire, *Generalized Trust* and *Optimism* are the two significant variables that affect the *Openness to SC* of potential users with experience. The first one plays a negative role. We believe that social factors can influence the decision of experienced users toward an expertise-based procedure because of the comforting human interaction of the traditional expert-based refunding process. For example, their past experience with humans may encourage users with a high level of social trust to be open to unpredictable choices (see **Result 3**), but that preserves human interaction. On the contrary, *Optimism* plays a positive effect on *Openness to SC* (see **Result 4**). This result may seem counterintuitive since people who feel optimistic when uncertain situations have a higher *Openness to SC*. However, this may refer to a propensity to test new technologies that they have not yet truly understood. In this sense, such users may be considered potential early adopters of technologies for SC policies.

To sum up, we report heterogeneous effects of behavioral traits of potential

end-users, conditional on owning or using a car. The openness to SC is affected by different traits depending on whether a subject owns/uses a car or not. In the case of potential end-users with experience, risk preferences positively affect openness to SC. Accordingly, a policy enhancing the adoption of SCs by these users should emphasize SC ability to clarify the evaluation procedure of the accident dynamics (e.g., the objectivity of evaluation). Similarly, when addressing users with experience and who have low general trust in others, a policy should underline that a SC is intended to replace the human activity of a third-trusted party. In the case of users with no experience, an adoption policy could lean on users' aversion to unpredictable situations (ambiguous and/or composed of several steps) by showing SC potential of reducing/eliminating third trusted parties.

Since at the time of the experiment, a prototype of a BCT-based SC at the service of the car insurance sector did not exist yet, we translated our case study into a toy model and focused on the behavioral attitudes of potential future users. The experimental methodology allowed us to provide a first overview of the openness of potential users to BCT-based SCs, without waiting for the release of a prototype. Our research does not focus on the effect of technology features. Instead, the experimental methodology helps us to focus on end-users' behavioral traits and openness to the main benefits for the economic decision environment (namely, the redundancy of trusted third parties).

To the best of our knowledge, this study represents a first step in the behavioral analysis of SC adoption in the insurance and connected vehicles sectors. From a methodological and policy perspective, our study adds to the literature by developing a framework that provides insights on how a smart contract should be explained to non-experts. For this reason, this paper tries to associate results with possible policy measures. Moreover, these results are based on methodologically accurate literature on risk and ambiguity. Therefore, we achieve high internal validity with adequate control and rigor in the experimental design.

A natural follow-up is to analyze the effect on openness to SC of other behav-

ioral traits that the experimental literature shows to be crucial for decision-making: social, cognitive, and emotional factors or cooperative behaviors in the presence of externalities in the technology adoption. As far as external validity is concerned, this may be improved through an experimental analysis conducted to test if our results are confirmed in a more contextualized experimental setting.

Chapter 3

Registered Report: Internal and External Validity of Ambiguity Attitudes Measures¹

¹This chapter has been co-authored with Ismaël Rafäi (Aix-Marseille School of Economics) and Guilhem Lecouteux (Université Côte d'Azur).

3.1 Introduction

Many economic decisions are taken in uncertain environments, where a single action may induce several possible and mutually exclusive outcomes, depending on the realization of the true state of the world. When the distribution of states of the world is determined *a priori* by objective probabilities, two cases arise: (i) decisions under *risk*, for which the decision maker knows the objective probabilities, and (ii) decisions under *ambiguity*, for which the decision maker ignores the objective probabilities and has to form some subjective probabilities about the occurrence of the states of the world (Machina & Siniscalchi, 2014).

The Ellsberg paradox (1961) suggests that decision-makers tend to prefer risky prospects to similar ambiguous ones, which can be interpreted as the formation of mutually inconsistent subjective probabilities about the distribution of states of the world. This common phenomenon is referred to as *ambiguity aversion* and constitutes a violation of the standard model of decision *à la* L. J. Savage (1972). In other contexts (e.g., health choices involving losses, Attema, Bleichrodt, and L'Haridon (2018)), decision-makers can also exhibit ambiguity-seeking attitudes, resulting in a preference for ambiguous over risky prospects. Furthermore, the literature suggests that attitudes towards risk and ambiguity could be correlated (see, for discussion, Ouangraoua, 2023), meaning that it both risk preferences and attitudes towards ambiguity must be measured properly. While risk preferences are relatively easy to measure in the lab (see, e.g., Galizzi, Machado, & Miniaci, 2016), this is less the case for attitudes towards ambiguity. Even though several methods have been proposed so far to measure them with different laboratory tasks (see, for extensive reviews, Etner et al., 2012; Trautmann & Van De Kuilen, 2015), none imposed itself as the gold standard, and researchers lack a clear framework to compare the different methods.

This paper aims to offer a systematic comparison of the replication of three established ambiguity attitudes measurement methods (AAMM hereafter), namely

(in alphabetical order) the ones proposed by Abdellaoui et al. (2011) (henceforth “Method A”), Baillon, Huang, et al. (2018) (henceforth “Method B”), and Chakravarty and Roy (2009) (henceforth “Method C”). Our main criteria to compare those methods are the internal and external validity of the instruments.² To do so, we compare (i) the consistency, (ii) the stability, and (iii) the predictive power of the obtained measures. More specifically, we measure for each method (i) the proportion of inconsistent choices, preventing the computation of the ambiguity attitude measure; (ii) the changes in the measures between two identical close moments of measurement, and (iii) the relationship between the obtained measure, and self-assessed risky behaviors. Side measures such as individuals’ perceived complexity level and response times distribution will also be collected, analyzed, and discussed.

We limit our study to comparing methods regarding consistency, stability, and exportability of ambiguity attitude measures. Thus, we do not focus, for example, on the causes generating the potential inconsistencies because this would require a sophisticated analysis combining theoretical elements and methodological subtleties. Thus, we do not compare the degrees of the ambiguity attitude elicited due to the differences in the theoretical paradigms studied.

The remainder of this report is structured as follows. Section 3.2 presents the tasks and ambiguity attitudes indexes of the three AAMM. Section 3.3 states the three hypotheses regarding the consistency, stability, and predictive power of the AAMM. Section 3.4 describes our replication strategy of the three methods and our planned statistical analysis. The future results will be presented and discussed in the sections 3.5 and 3.6.

²A measure is internally valid if it captures the behavior or its variations with the least amount of error (Roe & Just, 2009). This definition also includes the concept of “construct validity” in psychometrics. It is externally valid if the relationships observed generalize to different uncontrolled environments or different types of populations (List, 2007).

3.2 Description of the Ambiguity Attitude Measurement Methods

3.2.1 Overview of the three methods

We choose to replicate the methods of Abdellaoui et al. (2011), Baillon, Huang, et al. (2018), and Chakravarty and Roy (2009) because of their popularity and also of their conceptual diversity. Although the original three tasks share the same choice list format, they differ in several features, summarized in Table 3.1. Method A allows the researchers to obtain a precise measure based on certainty equivalents while keeping relatively simple instructions to the participants. The main drawbacks are the task length, which could increase participants' cognitive burden over time, and the relative technicality of the parametric estimation, which requires a more elaborated (and thus, empirically disputable) theoretical framework. Method B proposes a more direct way to measure ambiguity attitudes without requiring the estimation of a more elaborated theoretical background with probability weightings. However, the matching probabilities to be elicited require a high level of cognitive effort from the participants. Finally, Method C is easily and rapidly implementable and is based on Klibanoff et al. (2005)'s characterization of ambiguity attitudes. Unlike Methods A and B, however, it requires expected utility to hold on the set of prospects, which cannot accommodate the possibility of different probability weightings for subjective beliefs over the set of states of the world.

3.2.2 Method A: The Source Method

Description of the task

The AAMM proposed by Abdellaoui et al. (2011) consists in measuring the attitude towards ambiguity as the difference between the *source functions* (i.e. a subjective

Table 3.1: Differences between the chosen measurement methods

Notes: ^a Number of citations from Google Scholar ^b Number of choice lists (CL) excluding independent risky prospects and controls. ^c Indicates the position of the most moderate risky option. It may induce a middle bias. ^d Valuation from the Klibanoff et al. (2005) theoretical framework. ^e Ease of understanding required valuations. Certainty Equivalents/Matching probabilities comparisons based on Baillon, Huang, et al. (2018).

	Method A	Method B	Method C
Study	Abdellaoui et al. (2011)	Baillon, Huang, et al. (2018)	Chakravarty and Roy (2009)
#citations ^a	687	136	157
Valuation	Certainty equ.	Matching prob.	KMM value ^d
# CL ^b	26	6	1
#Decisions per CL	51	20	10
Skewness of CL ^c	No	Yes	No
#parameters	2	2	1
Cognitive Effort ^e	Lower	Higher	Moderate
Duration	Higher	Moderate	Lower

weighting probability function)³ of the decision maker when facing choices under risk or under ambiguity. The certainty equivalents (henceforth CE) required for the parametric estimation of the source function are elicited by presenting a series of iterative multiple price lists⁴ with a list of ambiguous and a list of risky prospects. Note that in the initial procedure, the use of iterative multiple price lists enforces monotonicity.

The participants are asked to choose between a lottery paying 0€ or 25€ and a set of increasing degenerate lotteries paying from 0€ to 25€. The position of the switching point determines the CE of the lottery. The lottery is an urn containing eight different colored balls, knowing that each ball has the same probability of being drawn. The exact composition of the urn can be known (urn K), meaning we have a risky lottery, or it remains unknown (urn U) with an ambiguous lottery. Abdellaoui et al. (2011) presents the notion of ambiguity to the subject as the possibility that some colors may be repeated or absent in the urn.

Overall, the initial design is composed of 32 different events for the ambiguous lottery (see *Table A.3.* in the Web-Appendix of Abdellaoui et al. (2011)).

³See Abdellaoui et al. (2011) for a detailed discussion on sources of uncertainty.

⁴For a given prospect, several choice lists were successively presented to the subjects to get closer to the exact value of its certainty equivalent. They could then achieve a 2% precision.

Determination of the ambiguity attitudes index: differences between source functions

Ambiguity and risk attitudes in Abdellaoui et al. (2011) are captured by two indexes, pessimism and likelihood insensitivity. Ambiguity aversion is then measured as the difference in terms of pessimism between a risky and an ambiguous choice. It is assumed that individuals evaluate a prospect $x_E y$ associated with the urns S ($S = K$ or $S = U$) as follows:

$$x_E y \mapsto w_S(p)u(x) + (1 - w_S(p))u(y) \quad (3.1)$$

with $u(x)$ the utility function, and $w_S(p)$ the source function, transforming the probability p of event E into a weight $w_S(p)$. The ambiguity index is computed by assuming the following parametric forms:⁵

$$\begin{cases} u(x) = \left(\frac{x}{25}\right)^{\rho_S} \\ w_S(p) = \gamma + \varsigma p \end{cases} \quad (3.2)$$

Abdellaoui et al. (2011) assume here a CRRA utility function, normalized between 0 and 1 for payoffs ranging from 0 to 25, with a parameter ρ_S , which can depend on the type of urn (known or unknown composition). The source function takes a neoaddivitive form Chateauneuf, Eichberger, and Grant (2007): it is continuous and strictly increasing on $(0; 1)$, with $w_S(0) = 0$ and $w_S(1) = 1$. The distance to the identity function at $p = 0$ is γ , ς is the intercept, and the distance at $p = 1$ is $\delta = 1 - \gamma - \varsigma$. The difference between those two values corresponds to an index of pessimism:⁶

$$\bar{b} = \delta - \gamma \Leftrightarrow \bar{b} = 1 - \varsigma - 2\gamma \quad (3.3)$$

⁵We have changed several notations to avoid confusion with the other indexes. For example, we further write "a" for the ambiguity aversion index to facilitate identification with Method A.

⁶Abdellaoui et al. (2011) also use a Prelec function to fit their data, though the linear form is sufficient to build the index capturing ambiguity attitudes. We will also provide indexes computed through the Prelec function.

Abdellaoui et al. (2011) propose to define their index of ambiguity aversion as the difference between the levels of pessimism in an uncertain and a risky environment:

$$a = \bar{b}_U - \bar{b}_K. \quad (3.4)$$

The parameters are elicited from the CE of the participants. ρ_S is derived from the CE with $j = 4$ (out of 8) winning colors, with a subjective probability of 0.5 for each urn. The source function is then estimated by fitting $w_S(j/8) = (CE/25)^{\rho_S}$, for $p \neq 0.5$ (i.e. $j \neq 4$). We can then deduce the values $\hat{\gamma}_S$ and $\hat{\zeta}_S$. The index for ambiguity aversion is thus:

$$\hat{a} = (\hat{\zeta}_U - \hat{\zeta}_K) - 2(\hat{\gamma}_U - \hat{\gamma}_K) \quad (3.5)$$

3.2.3 Method B: The Matching Probability Method

Description of the task

Baillon, Huang, et al. (2018) proposes to elicit matching probabilities (i.e., probability equivalents) for a partition of three mutually exclusive events E_i generated by the same source, e.g., the variations of the AEX (Amsterdam stock exchange) index. The task consists, for each event or conjunction of events, of eliciting the probability m that makes the individual indifferent between playing the ambiguous lottery (paying 20€ or 0€ depending on the realization of the single or composite event) and playing a risky lottery (paying 20€ with probability m , and 0€ with probability $1 - m$). When eliciting the matching probabilities for each part of the partition, their sum should add up to 1 if the individuals are ambiguity neutral. A sum below 1 would suggest ambiguity aversion, and above 1 an ambiguity-seeking attitude. Each prospect $20E_i0$ or $20E_{ij}0$, is presented in a choice list (resulting in a total of 6 choice lists, the number of possible combinations with 3 events), and the position of the switching point determines the matching probability for the

prospect. A particular design feature of (Baillon, Huang, et al., 2018) is using unequally spaced choice lists to mitigate potential middle bias. Overall, the initial design is composed of 16 different events for the ambiguous lottery (see *Table A.I.* in the Appendix of Baillon, Huang, et al. (2018))

Matching probabilities can thus offer a direct way to measure ambiguity attitudes without further specifying a theoretical model describing how individuals weigh subjective probabilities and their risk preferences. In Baillon, Huang, et al. (2018), single switching was imposed to minimize inconsistent choices.

Determination of the ambiguity attitudes index: sum of complementary matching probabilities

For each single events $E_i, i \in (1, 2, 3)$ the participants indicate their matching probabilities $m_i = m(E_i) = P(E_i)$. Similarly, they then indicate their matching probabilities for each composite event $E_{ij} = E_i \cup E_j, i, j \in (1, 2, 3), i \neq j$. If an individual is ambiguity neutral, we should have $m_i + m_{jk} = 1$. Attitudes towards ambiguity are therefore measured by looking at the *average* values of m_i and m_{jk} , with the following ambiguity aversion index:

$$b = 1 - \bar{m}_c - \bar{m}_s, \quad (3.6)$$

where $\bar{m}_s = (m_1 + m_2 + m_3) / 3$ denotes the average probability for single events, and $\bar{m}_c = (m_{12} + m_{13} + m_{23}) / 3$ denotes the average probability for composite events. Under ambiguity neutrality, we should have $1/3$ for single events and $2/3$ for composite events, and $b = 0$. In case of an extreme aversion to ambiguity, the matching probabilities for all events should be close to 0, with an index of $b = 1$, while an extreme preference for ambiguity would lead to matching probabilities of 1, with an index of $b = -1$.

3.2.4 Method C: Chakravarty and Roy Method

Description of the task

Chakravarty and Roy (2009) method is inspired by Holt and Laury's Multiple Price List (Holt & Laury, 2002). Participants face a set of 10 choices between a risky and an ambiguous lottery presented in parallel within a choice list. The risky lotteries only vary in outcomes (0 INR with probability 0,5 or an amount from 20 to 140 INR with probability 0,5),⁷ and the outcome of the ambiguous lottery remains constant (0 INR or 100 INR). The position of the switching point reflects individual attitudes towards ambiguity — if the switch happens for low amounts of the risky lottery, we have an ambiguity aversion, while ambiguity neutrality implies a switching point when the amounts are identical between the two lotteries, and a switch for high values reveal a preference for ambiguity.

The lotteries are two urns filled with colored balls (2 colors). The risky urn contains 5 balls of each color, while the ambiguous urn contains 10 balls whose color is unknown to the participant. Participants are asked to bet on one of the colors and win if and only if the ball drawn is the same color.

A special feature of the ambiguous urn in Method C is that the balls are all the same color, with only two possible configurations.⁸

Determination of the ambiguity attitudes index: KMM value

Ambiguity attitude in Method C is described under the KMM representation (Klibanoff et al., 2005), representing the ambiguous prospect as “subjective expected utility over expected utilities.” We denote $(x, 0; p)$ the risky prospect that pays $x > 0$ with probability p (and 0 otherwise) and $(x, 0; 0, 1)$ the ambiguous prospect for

⁷1 EUR \approx 1.43 USD \approx 66.83 INR for the year 2009.

⁸Experimenters usually construct the ambiguous urn so that all combinations of colors of the items used can be represented, but Chakravarty and Roy approached it differently by borrowing their construction of the urn from Halevy (2007), who sought to highlight the link between ambiguity aversion and violations of reduction of compound lotteries. This project does not address whether such a presentation of the ambiguous urn to participants will induce a different perception of ambiguity.

which either $p = 0$ or $p = 1$, the true value of p being unknown to the decision maker. If we assume that expected utility under risk holds, we can define a utility function for money $u : \{0, x\} \rightarrow \mathbb{R}$, e.g., a CRRA function: $u(x) = x^r$. When facing an ambiguous prospect, the KMM value corresponds to the subjective expected utility of the prospect — under the condition that expected utility under uncertainty holds — where the decision maker assigns subjective probabilities $\sigma = (\sigma_{E_1}, \sigma_{E_2})$, $\sigma_{E_1} \geq 0, \sigma_{E_2} \geq 0, \sigma_{E_1} + \sigma_{E_2} = 1$ over the occurrence of the two events (E_1 leading to $p = 1$, and E_2 leading to $p = 0$). The utility function associated with the KMM value is denoted $v : \{0, x\} \rightarrow \mathbb{R}$, which we also estimate by a CRRA function, with $v(x) = x^c$. The KMM value of the ambiguous prospect is thus:

$$KMM(x, 0; 1, 0) = \sigma_{E_1} v(1 * u(x) + 0 * (u(0))) + \sigma_{E_2} v(0 * u(x) + 1 * (u(0))) \quad (3.7)$$

$$KMM(x, 0; 1, 0) = \sigma_{E_1} v(u(x)) \quad (3.8)$$

$$KMM(x, 0; 1, 0) = \sigma_{E_1} x^{rc} \quad (3.9)$$

The value of the parameter c then serves as an index for ambiguity attitude: a value of $c = 1$ means ambiguity neutrality, while ambiguity aversion corresponds to $c < 1$ and ambiguity-seeking to $c > 1$. Given the task of the experiment (the balls are all of one color or of the other), we assume, following Chakravarty and Roy (2009), that $\sigma = (0.5, 0.5)$. We can easily check that this specification allows us to estimate the value of c directly without requiring the elicitation of risk preferences beforehand (we must indeed compare the KMM value of the ambiguous prospect with the expected utility of the risky prospect at the switching point, and can thus directly simplify by r).

3.3 Hypotheses

We now present the null hypotheses to be tested.

3.3.1 Internal validity: Consistency of choices

Our focus on the prevalence of inconsistent choices as a test of internal validity is motivated by their occurrence in practically all experiments in choice list format as soon as the task does not enforce consistent single switching. Moreover, to our knowledge, the correlation between the elicitation method and the probability of inconsistent choices has not been investigated. If such a relationship exists, then the choice of a specific experimental design could favor their occurrence, irrespective of the underlying psychological mechanisms. Therefore, if for a given AAMM, their actual unobserved rate is substantial, imposing consistency (e.g., by enforcing monotonicity) would systematically bias the collected data (see, e.g., Engel & Kirchkamp, 2019). Early prediction of their rate could be a subsidiary argument for choosing a method, or highlight the need for a preventive adjustment of the sample size to capture a significant number of consistent choices.

For each method, the opportunity set takes the format of a menu of ordered lotteries presented simultaneously in lists and jointly with their respective alternatives.⁹ All the AAMM we presented are computed within a specific theoretical framework described above in Section 3.2. Whenever a participant falsifies the underlying assumptions, the AAMM cannot reveal his or her ambiguity attitude. In particular, the decision-makers are expected to reveal a consistent single switching point for each choice list from the ambiguous to the risky prospect (since risky prospects are incrementally more favorable, either in terms of payoff or probability of success), which excludes from data analysis all choices patterns not respecting

⁹The simultaneous and orderly presentation is not necessary, although popular. Numerous studies present the lotteries sequentially and randomly to minimize any influence of adjacent lotteries in the participant's evaluation. Whether such an approach generates fewer or more inconsistent decisions is an interesting question for future research.

this condition.

Formally, each choice list contains a constant ambiguous prospect L_0 , to compare with a series of risky prospects $(L_i)_{i \in [1, n]}$ with increasing values for the outcomes and/or probabilities of success. Revealing a consistent single switching implies that $\exists s \in 0, n$, such that:

$$\begin{cases} L_0 \succ L_i & \text{if } i \leq s \\ L_i \succ L_0 & \text{if } i > s \end{cases} \quad (3.10)$$

For a given measurement method $m \in \{A, B, C\}$, let $1 - \mu_m$ denote the probability that a participant reveals a consistent single switching point. Thus, μ_m represents the probability that a participant exhibits an inconsistent choice pattern. The first hypothesis we will test ($H1$) is the following :

$$H1 : \mu_A = \mu_B = \mu_C \quad (3.11)$$

That is, the violation of the underlying theoretical model is equally likely across methods.

3.3.2 Internal validity: Stability of ambiguity attitude measures

We consider the stability of measures as a second test of internal validity. Our approach is close to Duersch et al. (2013), who studied temporal stability of ambiguity attitudes and found a 30% mismatch between two identical measurements separated by two months.¹⁰ We decided to consider a shorter time lag of about ten minutes between our measurements. By reducing the time lag between the two measurements, we minimize the influence of external factors likely to alter individuals' preferences. This allows us to attribute potential change in the ambiguity

¹⁰One might question whether having measures of preferences mismatches between several repetitions reveals inconsistency. This will not be the case if preferences are thought time-invariant for the same probability-generating event. Thus variability can be admitted without necessarily indicating inconsistency since it concedes possible influences of unrelated events or factors.

attitude measure to the imprecision of the measurement methods rather than a change in individuals' preferences.

For a given measurement method $m \in \{A, B, C\}$, let $D_m = \frac{\bar{A}_m^2 - \bar{A}_m^1}{\hat{\sigma}_m}$ the normalized average difference of the ambiguity attitude measures between its second and first iterations (with $\hat{\sigma}_m$ the estimated standard deviation of the measure). We will take the absolute value of D_m . The second hypothesis we will test ($H2$) is the following :

$$H2 : D_A = D_B = D_C \quad (3.12)$$

That is, no method is more stable than another.

3.3.3 External validity: predictive power of AAMM

AAMM are expected to reveal the individual's preferences when facing ambiguous environments and, therefore, could predict behaviors made in real-life ambiguous environments. Therefore, the external validity of the AAMM will be assessed by investigating the possible correlations between the ambiguity attitudes indexes and the participants' scores computed from a questionnaire about self-reported attitudes towards risky behaviors. The questionnaire is taken from Gaucher, Lecoutoux, and Rafai (2023) and will be administered online to participants at least one week before the laboratory experiment.

For a given individual i , let $(A_m)_i$ denote his or her normalized measure of ambiguity attitude, revealed by a measurement method $m \in \{A, B, C\}$, Y_i a set of behaviors made in ambiguous environments and X_i a set of controls and individual characteristics. We estimate the relationship between the measure in the lab and real-life behaviors by the vector β_m in the following equation:

$$Y_i = \beta_m \times (A_m)_i + \beta_0 \times X_i + \epsilon_i \quad (3.13)$$

With ϵ_i an error term. The behaviors we consider are risky behaviors for which there exist epidemiological data about the possible health costs: from the perspective of health authorities, such behaviors can be considered as *risky* behaviors with known probabilities (e.g., smoking will procure a certain benefit to the user, combined with an increased probability of being sick), though users are not necessarily aware of the actual level of risks associated to those behaviors (they may therefore see such choices as *ambiguous* prospects). We intend to test whether ambiguity attitudes toward money in the lab can be related to attitudes toward risky behaviors outside the lab.

We will collect attitudes towards real-life risky behaviors with the questionnaire designed by Gaucher et al. (2023), with sections about alcohol consumption, smoking, gambling, smartphone use, and socio-demographic characteristics. The section on alcohol dependence is a combination of the Alcohol Use Disorders Identification Test (AUDIT) (Babor et al., 2001) and questions from Santé Publique France (2017). The section on cigarette dependence is based on the Cigarette Dependence Scale (Etter, Le Houezec, & Perneger, 2003) and questions from Santé Publique France (2021). Questions on gambling are based on the Canadian Problem Gambling Index (CPGI) (Ferris & Wynne, 2001) and additional questions from Gaucher et al. (2023). A short version of the Smartphone Addiction Scale (Kwon, Kim, Cho, & Yang, 2013) constitutes the section on problematic smartphone use. Finally, the socio-demographic questionnaire captures variables such as gender, age, socio-professional status, revenue, or place of residence. We will add a self-assessed risk aversion question from Dohmen et al. (2011) to the section on socio-demographic characteristics. Each test targeting a risky behavior, in particular, gives a score that will be used in the regression (3.13). We report the questionnaire in the Appendix.

The third hypothesis we will test ($H3$) is the following :

$$H3 : \beta_A = \beta_B = \beta_C \tag{3.14}$$

That is, each method is equally predictive of the real-life (risky) behaviors described above.

3.4 Design and Analysis Plan

3.4.1 Procedure

The experiment is scheduled for 2023 at the Laboratory of Experimental Economics of Côte d'Azur University (LEEN-NiceLab) (France). We plan to recruit 240 participants with ORSEE.¹¹ The expected length of the experiments is 45 minutes on average, and the expected payment is XX €, including a fixed fee of 7 € for showing up. Answering the questionnaire is incentivized by 3 € and is a condition to participate in the experiment. It takes an average of 20 minutes to answer. To link the answers to the questionnaire with the decisions taken in the experiment, we will ask participants at the end of the questionnaire to generate and write down a confidential password which they will have to enter again on the computers on the day of the experiment. The Ethical Committee of our university approved the procedure in June 2022.

Each session is assigned a single AAMM, and all participants within a session complete the same tasks. A session is divided into 4 parts, as described in Figure 3.1. Participants are told that parts 1, 2, and 3 are incentivized and will be randomly selected for payment. In Part 1, participants complete one of the AAMM assigned to the session. In Part 2, we measure controls, including risk aversion through the Bomb Risk Elicitation Task (Crosetto & Filippin, 2013, see Section 3.4.3) and a working memory task, the Delayed Matching to Sample test (Hartman, Dumas, & Nielsen, 2001). We implement this task to redirect participants' attention to another task and thus minimize reminders of their past decisions, which could interfere with their decisions when we will repeat the AAMM task in part 3. Part 2

¹¹This sample size is sufficient to meet the desired statistical power of 0.80 for this study and was calculated based on data from the pilot experiments.

will last around 10 minutes on average. Part 3 is an exact repetition of Part 1 (with the same AAMM). Finally, in Part 4, we propose an ending questionnaire to collect participants' perceived level of task complexity and then proceed to individual payments.

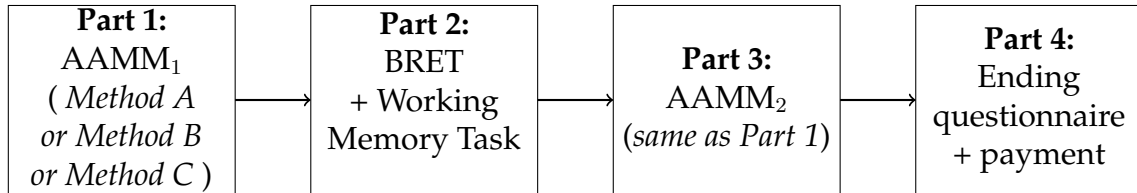


Figure 3.1: Structure of an experimental session

The experiment is computerized and programmed through z-tree (Fischbacher, 2007). We use a two step-Random Incentive System to determine: 1) Which part, 2) Which choice within the part will be incentivized. In part 2, only the BRET task is considered for payment, not the working memory task.¹² Participants will be told the computer does all randomizations. Then, the chosen lottery will be played physically. Experimental instructions and screenshots are given in the Appendix.

3.4.2 Implementation of the Ambiguity Attitudes Measurement Methods

We will replicate the AAMM presented in Section 3.2, matching the original methods as much as possible. However, some changes are required from the original studies to allow better comparability between the methods. In particular, we changed the stakes of the original methods to propose comparable incentives between methods. We thus assign all ambiguous prospects winning pay-offs of 15€ and 0€ otherwise. The pay-offs of risky prospects will be adjusted accordingly.

To test our first hypothesis, the major change we will implement is not to force

¹²We are not incentivizing the memory test because participants' performance on it is not a crucial measure for our study. Furthermore, rewarding this task in addition to the BRET task, which is also in Part 2, would add further complexity to the experiment. We will nevertheless use these data to explore our hypotheses in greater depth (see Section 3.4.5).

single switching, thus allowing participants to freely choose their preferred lotteries within the choice lists.

Modifications made in method A

In the replication of method A, the eight colors we will use to generate events within the urns K and U are $\{Black(B), Purple(P), Orange(O), Cyan(C), Walnut(W), Green(G), \text{ and } Azur(A)\}$, each with the same probability of being drawn. We will not control for uniformity because of the length of the experiment; we assume it as given by Abdellaoui et al. (2011). We will not use iterative multiple price lists, which would not allow testing Hypothesis 1. Finally, to shorten the length of the experiment, We will choose a lower precision of the certainty equivalent measure than the original article, so the total number of decisions per choice list will be limited to 14. Table 3.2 describes the list of prospects in this method.

Table 3.2: Prospects presented in Method A's choice lists

Note: In each choice list, the participant makes 14 decisions between a prospect option (that does not change within the choice list) and a sure amount that always ranges from 1€ (decision 1) to 14€ (decision 14). (U) / (K) indicate respectively the choice lists where the urn composition of the prospect is unknown/known.

	Choice List	Prospect				Sure amount
		# Winning colors	p	Winning	Otherwise	
ElicitationofSource Functions	1 (U) & 8 (K)	1 (Black)	0,125			from 1€ to 14€(1€ increment)
	2 (U) & 9 (K)	2 (+ Purple)	0,25			
	3 (U) & 10 (K)	3 (+ Orange)	0,375			
	4 (U) & 11 (K)	4 (+ Cyan)	0,5	15€	0€	
	5 (U) & 12 (K)	5 (+ Walnut)	0,625			
	6 (U) & 13 (K)	6 (+ Green)	0,75			
	7 (U) & 14 (K)	7 (+ Azur)	0,875			
ElicitationofUtility	15 (U) & 21 (K)	4 (Black, Purple, Orange or Cyan)	0,5	10€	0€	from 1€ to 14€(1€ increment)
	16 (U) & 22 (K)			15€	8€	
	17 (U) & 23 (K)			8€	10€	
	18 (U) & 24 (K)			10€	5€	
	19 (U) & 25 (K)			12€	8€	
	20 (U) & 26 (K)			15€	10€	

Modifications made in method B

For method B, we will settle for artificial events generated with urns and balls.¹³

The 3 colors we will use are *Red(R)*, *Azur(A)*, and *Green(G)*. We will keep the implementation of (Baillon, Huang, et al., 2018) consisting in building unequally

¹³Baillon, Huang, et al. (2018) originally used natural events, but their method is not limited to them, so we decided to use urn and balls for the sake of comparability.

spaced choice lists to mitigate potential middle bias. Table 3.3 describes the set of prospects in this method.

Table 3.3: Prospects presented in Method B's choice lists

Note: In each choice list, the participant makes 20 decisions between an ambiguous prospect (that does not change within the choice list) and 20 risky prospects with winning probabilities varying from 1% (decision 1) to 99% (decision 20).

Choice List	Ambiguous Prospect			Risky Prospects		
	# Winning colors	Winning	Otherwise	p(Winning)	Winning	Otherwise
1	1 (Red)					
2	1 (Azur)					
3	1 (Green)	15€	0€			
4	2 (Red \cup Azur)			from 1% to 99%(unequal increment)	15€	0€
5	2 (Red \cup Green)					
6	2 (Green \cup Azur)					

Modifications made in method C

Finally, concerning method C, the risky urn will contain 5 red (R) and 5 blue (B) balls, while the ambiguous urn will contain 10 red and blue colored balls with unknown content. We will keep the particular construction of the ambiguous urn, presenting it as containing either all red or all blue balls. Similar to Chakravarty and Roy (2009), we will take precautions to minimize any strategic motivation that might emanate from participants and experimenters. We will allow participants to choose their winning color, and the experimenter will randomly choose the composition of the ambiguous urn before the experiment starts. Table 3.4 describes the prospects in this procedure.

Table 3.4: Prospects presented in Method C choice lists

Note: In a single choice list, the participant makes 10 decisions between a fixed ambiguous prospect and 10 risky prospects with unchanging probabilities (0.5) but winning outcomes varying from 2€ (decision 1) to 15€ (decision 10).

	Risky Prospects				Ambiguous Prospect				Bounds for c
	Composition of the urn	Winning	Otherwise	p(winning)	Composition of the urn	Winning	Otherwise	p(winning)	
1		2€							{0,30 ; 0,60}
2		4€							{0,60 ; 0,84}
3		7€							{0,84 ; 0,90}
4		8€							{0,90 ; 0,95}
5	5 Red & 5 Blue	9€			10 Red-0 Blue			(1/0)	{0,95 ; 1}
6		10€	0€	0,5	or	10€	0€	or	{1 ; 1,04}
7		11€			10 Blue-0 Red			(0/1)	{1,04 ; 1,07}
8		12€							{1,07 ; 1,11}
9		13€							{1,11 ; 1,17}
10		15€							{1,17 ; +∞}

3.4.3 Control: the Bomb Risk Elicitation Task

We must control for risk aversion to assess the correlation between ambiguity attitudes and real-life decisions under uncertainty. We will use the same measure of risk aversion for all the AAMM, i.e. the Bomb Risk Elicitation (BRET) (Crosetto & Filippin, 2013). The BRET is indeed simple to understand and consists of a single decision, preventing the issue of multiple switching points. To decrease the number of items (hence prospects) to be considered by the participants, we adopt a transformation of the task format where we reduce the number of items to 10, and we use an urn and balls to generate events.¹⁴ In this task, we will present to the participant a set of 9 white balls plus one hidden “bomb” (here, a pink ball). The participant will be asked to collect the number of balls she wants, knowing that each white ball collected earns 1.5 euros, but gains are nullified when the bomb is collected. A risk-neutral participant is expected to choose $k^* = 5$ balls.

Table 3.5: Prospects presented in the BRET task

No. balls drawn	Chances to draw the bomb (pink ball)	Winning outcome	Expected value	Bounds for r
1	0,1	1,5	1,35	$\{-\infty ; 0,16\}$
2	0,2	3	2,4	$\{0,16 ; 0,7\}$
3	0,3	4,5	3,15	$\{0,7 ; 0,9\}$
4	0,4	6	3,6	$\{0,9 ; 0,97\}$
5	0,5	7,5	3,75	$\{0,97 ; 1,02\}$
6	0,6	9	3,6	$\{1,02 ; 1,06\}$
7	0,7	10,5	3,15	$\{1,06 ; 1,13\}$
8	0,8	12	2,4	$\{1,13 ; 1,26\}$
9	0,9	13,5	1,35	$\{1,26 ; +\infty\}$

3.4.4 Memory test: The Delayed Matching to Sample test

The Delayed Matching to Sample test (Hartman et al., 2001) evaluates an individual’s capability to maintain and manipulate visual information in mind for a short period while other cognitive processes are being performed. The test involves presenting a sample stimulus, which is then removed after a short delay, followed

¹⁴In the original task, participants were given a grid of 100 boxes, presenting a set of 101 lotteries.

by other comparison stimuli. The participant must select the comparison stimulus that matches the sample stimulus. For this study, the test last 10 minutes. In order to reduce interference with decisions during the repeated AAMM task in part 3, we employ this task as a means to redirect participants' attention away from previous decisions, effectively minimizing reminders of said decisions.

Since participants' performance on the memory test is not a critical measure for our study, we will not incentivize it. Moreover, adding rewards for this task, alongside the BRET task in Part 2, would unnecessarily complicate the experiment's payment phase (Part 4).

3.4.5 Other exploratory measures

In addition to the main measures, we will collect:

- Confidence in the randomness of drawings ;
- Response time ;
- Participants' perceived level of complexity for each task.

Supplementary analyses not registered in this report and based on those variables will be performed and presented as exploratory. For example, we will perform several segmentation analyses to investigate whether some methods could be more or less appropriate for some sub-population.

3.4.6 Data analysis strategy

Once collected, data will be openly available on the following OSF repository: <https://osf.io/mq2fc/>. A simulated dataset (see the Appendix) is available on the same repository to test the statistical script for planned analyses. Another statistical script that we will use to perform further exploratory analyses will be added to the repository.

We will compute the ambiguity attitude measures for each participant as described in Section 3.2. Then, we will test the hypotheses presented in Section 3.3. As socio-demographic variables are included in the estimates, we will check for interactions between them and multicollinearity.

Table 3.6 summarizes the main planned analyses.

Table 3.6: Planned analyses

Element	Technique
Consistency of choices	Test of Proportions
Stability of measures	z-test
External validity	Wald-test
index a	1 - Utility : Non-linear least-square estimation of (3.1) (dep. var = CE)
	2 - Source functions: Non-parametric estimation of $w_s(j/8)$, ($j \neq 4$)
	3 - Pessimism indexes for Unknown urn and Known urn: estimation of (3.2)
	4 - a index: Difference between pessimism of Unknown urn and Known urn
index b	Direct computation of b through (3.6)
index c	Maximum Likelihood estimation, no socio-demographic variables (dep. var = consistent binary choices in C)
	Maximum Likelihood estimation, with socio-demographic variables (dep. var = consistent binary choices in C)

3.5 Results

This section will be completed after data collection.

3.6 Discussion

This section will be modified/completed after data collection. However, regardless of the future results, it is already possible to discuss some of the limitations of our study.

We identify two main limitations at this stage of the study.

The first concerns the dependence of our hypotheses 2 and 3 on the observed rate of consistency of choices. Indeed, if we observe a high rate of inconsistent choices, hypothesis 1 will not be affected, while the test of the last two hypotheses may suffer from low power due to the small size of the consistent data sample. Method A is particularly likely to cause this problem because of its length. However, although it is a significant limitation, we anticipate the problem by proposing

strategies for dealing with erroneous data, thus still allowing the calculation of our indices. They are detailed in the Appendix.

The second limitation concerns the potential presence of confounders. We have chosen to restrict the level of analysis to the preferences measured through the three methods, paying minimal attention to the perception of the intrinsic characteristics of these methods. These are, for example, the differences in perception between valuation methods (A) and choice methods (B and C) that can lead to preference reversals (Maffioletti et al., 2009), the length of the tasks, or even the size of the choice list. They are likely to act as confounding variables and reduce the significance of our tests. For example, the experimental tasks being of unequal length, the rate of inconsistencies from one task to another can be attributed to the length of the task. To control the effect of this type of variable, we will collect additional information proposed in Section 3.4.5 from a questionnaire after the experiment.

General Conclusion

The study of decision-making under uncertainty is a rich and complex research area that continues to yield fruitful directions for exploration. Ambiguity, a notion underlying uncertainty referring to the subjective perception of missing information, continues to generate growing interest. Despite significant advances in the field over the past six decades, some important issues still remain to be answered. For instance, we need more conclusive evidence to support a comprehensive theory on how ambiguity attitudes interplay with other attitudes such as risk attitudes. Additionally, to better understand the actual nature of preferences and help refine theories, research must also emphasize comparing the measures from different elicitation methods and assessing the relative advantages of each method that accurately captures ambiguity attitudes outside the laboratory setting. To contribute to addressing these challenges, we have undertaken three research projects corresponding to the chapters of this thesis.

The relationship between risk attitudes and ambiguity attitudes is not clearly understood, and the literature has reported mixed results in terms of correlation. In the first chapter of this thesis, we examined the causes underlying the correlation between these two attitudes by conducting a meta-analysis. To avoid relying on a specific experimental method and to provide generalizable results, a meta-analysis was conducted on 68 correlation signs from 55 research studies, taking into account their elicitation format, sample size, outcome domain, presentation of risky and ambiguous options, implementation of ambiguity, distributed incentives, and experimental population type. The meta-analysis found that sample size was a significant driver of the likelihood of observing a positive correlation, and this effect was also observed in the subset of studies distributing real monetary incentives to their participants. There was also weak evidence that the joint or separate presentation of risky and ambiguous options influenced the relationship. The latter finding mitigates the conception of Fox and Tversky (1995), Camerer and Weber (1992), Kocher and Trautmann (2013), Trautmann and Van De Kuilen (2015). Negative correlations were excluded from the analysis as they were deemed to be

statistical artifacts.

The second chapter of this thesis investigated the role of ambiguity attitudes in a real-world decision problem in the context of car insurance. The decision problem involved choosing between a dispute resolution procedure driven by human judgment (deemed ambiguous) and an automated procedure (deemed unambiguous) based on smart contracts and algorithms. To represent this decision problem, a laboratory experiment was conducted using an adapted version of the Bomb Risk Elicitation Task (Crosetto & Filippin, 2013) to elicit participants' attitudes towards ambiguity, risk, and reduction of compound risks. Additionally, we collected the participants' demographics, personality traits, and car use experience through a questionnaire. Car driving experience was expected to affect their perception of uncertainty (A. Cohen, 2005). The findings confirmed this impact on our pool of participants, as we showed that users who are ambiguity averse and have no car experience are more likely to opt for the automated procedure. Their dislike of unpredictable situations probably makes them more inclined towards the automated procedure. Their decision also depends on generalized trust and optimism levels. The users' trust in other humans positively affects their decision towards human-based procedures. In contrast, optimism positively affects the users' willingness to adopt automated procedures, making them more open to testing new technologies, even if they do not completely understand them.

The external validity of our second study was difficult to assess because the decision problem was new and lacked real-world data. For the further study, we chose to rely on more common decision problems related to harmful behaviors like alcoholism, smoking, gambling, and problematic smartphone use. However, the literature does not provide clear guidance on the suitable experimental methods to elicit ambiguity attitudes associated with these behaviors. Therefore, we decided to scrutinize the available methods to select the most appropriate one and test the extent to which their measures correlate with different harmful behaviors.

The third chapter of the thesis then focused on examining the internal and ex-

ternal validity of measures used to assess individual ambiguity attitudes. We proposed to replicate three well-known ambiguity attitude measurement methods. The first method (Abdellaoui et al., 2011) involves quantifying ambiguity and risk attitudes through two indexes: pessimism and likelihood insensitivity. The difference in pessimism between risky and ambiguous choices is used to determine ambiguity aversion. The second method (Baillon, Huang, et al., 2018) uses specific indexes to obtain probability equivalents for a partition of three mutually exclusive events. These indexes are directly observable, correct for subjective likelihoods, and remain valid across various ambiguity theories, even if the expected utility for risk is violated. The third method (Chakravarty & Roy, 2009) involves the "KMM representation" (Klibanoff et al., 2005) that describes ambiguity attitudes. The resulting "KMM value" is the subjective expected utility of the prospect, conditional on the assumption of expected utility under risk and the decision maker's assignment of subjective probabilities over the occurrence of two events. We proposed to examine the internal validity of the measures by evaluating their consistency and stability when the task is repeated. The focus is solely on the evaluation of different methods for ambiguity attitude measurements, and the causes of potential inconsistencies will not be explored as it would require a complex analysis of theoretical and methodological elements. The external validity will be determined by assessing how the measures reflect real-world harmful behaviors such as alcoholism, smoking, gambling, and problematic smartphone use. We will capture them through a questionnaire from Gaucher et al. (2023).

The contributions we have made, nonetheless, need to be completed. Our research question for the first study could be enriched by confirming or supplementing it with other methodological approaches. We plan in particular to directly collect primary databases from studies that have elicited both within-subject risk and ambiguity aversion (whether or not they have calculated their correlations) and to calculate the correlations from these data ourselves. There are at least four advantages to be expected: i) obtaining more data; ii) standardizing the calcula-

tion of the correlations with the same statistic (for example, Spearman) and being able to explain the value of the correlation as well as the sign; iii) including quantitative variables such as risk and ambiguity premia; and iv) the manipulation of the types of measures used (parametric or non-parametric, for example). In addition, we could control for the effect of certain practices, such as the inclusion or exclusion of extreme subjects or multiple switchers. Obtaining primary data will also enable us to go beyond correlation since we could perform regressions with uniform socio-demographic variables to obtain a higher level of analysis of the relationship between risk and ambiguity aversion. Furthermore, it could enable exploring more diverse relationships of ambiguity attitudes and, for example, attitudes toward reduction of compound lotteries, ambiguity insensitivity, high-order ambiguity attitudes, and, more interestingly, outcome-based ambiguity attitudes, which are becoming increasingly popular in the literature but about which little is yet known. This could extend to an attempt to draw up a global map of the links between different types of behavior in situations of uncertainty. It could be operationalized not only through meta-analysis but also through within-subjects experimentation. Such experimentation could be both expensive and time-consuming. We are confident that future methodological advances will make it feasible.

Extending the second study will require even greater interdisciplinarity to understand the determinants of adoption and even appropriation of the technologies associated with the dispute resolution procedure we have studied. We plan to incorporate specific determinants highlighted in other social science disciplines (for example, management sciences and sociology) concerned with smart contracts into the analysis. The subsequent study could then mix laboratory experiments with discrete choice experiments to quantify the relative importance attributed by individuals to each feature of the technology.

Finally, in addition to the results that will confirm or refute our hypotheses, the last study could also be augmented by a more specific analysis of the underlying causes of multiple switching in choice lists and preference instability. For example,

a better understanding of inconsistencies can be achieved by calculating multiple switching probabilities as a function of the number of lotteries in the choice list, the number of choice lists in the task, or the size of the lottery stakes. By conducting this analysis with a very large sample size, we could implement machine learning-based techniques to achieve more accurate predictions. We also plan to repeat our measurements a larger number of times over several different time intervals (for example 1 day, 1 month, 1 year) to investigate whether a stable trend emerges in the elicited preferences. It could also answer the question of how many repetitions are needed to reach the "actual preference" of the individual elicited by the same experimental method, which would be a significant methodological contribution.

Appendix

A.1 Appendix A

A.1.1 Additional Tables and Figures

Table A.1: Description of Lab Experiment Variables.

Variable	Definition
Risk Loving	Variable measuring subjects' risk preferences (PART 1). It assumes a value from 1 to 7, where 0 indicates risk-averse subjects. 7 indicates risk-loving subjects.
Ambiguity Loving	Variable measuring subjects' ambiguity preferences (PART 2). The choices of subjects in PART 2 are accumulated. It assumes a value from 0 to 7, where 0 indicates ambiguity-averse subjects. 7 indicates ambiguity-loving subjects.
RoCL Loving	Variable measuring subjects' RoCL preferences (PART 3). The choices of subjects in PART 3 are accumulated. It assumes a value from 0 to 7, where 0 indicates compound lotteries-averse subjects. 7 indicates compound lotteries-loving subjects.
Openness to SC	Variable measuring subjects' openness to smart contracts (PART 4). It refers to the case study explored in the paper. The choices of subjects in PART 4 are accumulated. It assumes a value from 0 to 7, where 0 indicates subjects with "no openness". In this case, they always selected the "expert procedure (option A)". 7 indicates subjects with "higher openness". In this case, they always selected the "SC procedure (option B)".

Table A.2: Description of Questionnaire Variables.

Variable	Definition
Gender	Dummy variable taking of one (=1) if the respondent is Male (question A).
Age	Subjects' age (question B)
Education	Dummy variable taking of one (=1) if Graduate Student (question C).
Car Experience	Dummy variable taking of one (=1) if at least use or ownership of a car (question D).
Perceived Dishonesty	Variable measuring perceived dishonesty in the insurance sector (question E1). It assumes a value from 0 to 3, where 0 indicates "low" and 3 indicates "high". The option "I don't know" is taken as 0.
Optimism Level	Variable measuring subjects' optimism level (question F3). It assumes a value from 0 to 3, where 0 indicates "Totally disagree". 3 indicates "Totally agree". The option "I don't know" is taken as 0.
Risk Attitude	Variable measuring subjects' declared risk attitude (question G). It assumes a value from 1 to 10, where 1 indicates "risk-averse person". 10 indicates "risk-loving person".
Generalized Trust	Variable measuring trust that subjects have in others in general (question I). It assumes a value from 1 to 10, where 1 indicates "It is better not to trust". 10 indicates "It is better to fully trust"
Trust in Technology	Variable measuring trust that subjects have in technology in general (question K). It assumes a value from 1 to 10, where 1 indicates "It is better not to trust". 10 indicates "It is better to fully trust"

Table A.3: Draws for payments and average individual payoff per session

Session nb.	# subjects	PART nb., Line nb.	average payoff
Session 1	13	PART 4 Line 5	21.84
Session 2	13	PART 2 Line 6	21.69
Session 3	11	PART 2 Line 5	19.82
Session 4	9	PART 3 Line 4	24
Session 5	13	PART 1	20
Session 6	11	PART 1	19.27
Session 7	13	PART 3 Line 2	20.31
Session 8	13	PART 1	20.92
Session 9	10	PART 4 Line 7	23.20
Session 10	10	PART 2 Line 6	18.80
Session 11	14	PART 3 Line 2	16.57
Session 12	13	PART 1	25.62
Session 13	14	PART 4 Line 3	22.57

Table A.4: Socio-demographic characteristics

		# of subjects (%)
Gender	<i>Male</i>	57 (34%)
	<i>Female</i>	103 (66%)
	Total	157
Education	<i>Bachelor student</i>	84 (53,5%)
	<i>Master student</i>	46 (29%)}
	<i>Master's degree</i>	27 (17,5%)}
	Total	157

Table A.5: Ordered Logit regressions with ambiguity loving explaining openness to SC

	(1) No Experience Openness to SC	(2) No experience Openness to SC	(3) Experience Openness to SC	(4) Experience Openness to SC
Risk loving	1.141 (0.179)	1.175 (0.215)	1.428** (0.184)	1.496** (0.210)
Ambiguity loving	0.730* (0.108)	0.748* (0.105)	1.035 (0.185)	0.952 (0.209)
Gender		1.110 (0.384)		0.626 (0.393)
Education level		1.237 (0.804)		1.009 (0.584)
Risk attitude (survey)		1.012 (0.0941)		1.025 (0.160)
Generalized trust		1.109 (0.149)		0.735* (0.0899)
Trust in technology		0.956 (0.122)		1.084 (0.137)
Perceived dishonesty		1.082 (0.176)		0.682 (0.190)
Optimism		0.727 (0.236)		2.134* (0.757)
<i>N</i>	81	81	63	63

Exponentiated coefficients; Standard errors in parentheses

Standard Errors adjusted for 13 clusters in session

* $p < .05$, ** $p < .01$, *** $p < .001$

Table A.6: OLS regressions with RoCL Loving explaining openness to SC

	(1) No Experience Openness to SC	(2) No experience Openness to SC	(3) Experience Openness to SC	(4) Experience Openness to SC
Risk loving	0.196 (0.155)	0.210 (0.197)	0.429** (0.131)	0.417* (0.136)
RoCL loving	-0.367* (0.167)	-0.397* (0.159)	0.231 (0.231)	0.140 (0.195)
Gender		-0.190 (0.371)		-0.461 (0.580)
Education		0.618 (0.631)		-0.0879 (0.455)
Risk attitude (survey)		-0.00181 (0.110)		0.0128 (0.126)
Generalized trust		0.164 (0.136)		-0.313* (0.111)
Trust in technology		-0.134 (0.128)		0.0794 (0.113)
Perceived dishonesty		0.062 (0.186)		-0.327 (0.233)
Optimism		-0.378 (0.314)		0.651* (0.261)
Constant	3.480** (0.759)	3.592** (1.103)	-0.362 (0.999)	0.755 (2.230)
<i>N</i>	81	81	63	63

Standard errors in parentheses

Standard Errors adjusted for 13 clusters in session

* $p < .05$, ** $p < .01$, *** $p < .001$

Table A.7: OLS regressions with Ambiguity Loving explaining openness to SC

	(1) No Experience Openness to SC	(2) No experience Openness to SC	(3) Experience Openness to SC	(4) Experience Openness to SC
Risk loving	0.196 (0.163)	0.207 (0.201)	0.435* (0.144)	0.413* (0.140)
Ambiguity loving	-0.308* (0.132)	-0.279 (0.129)	0.0502 (0.175)	-0.00900 (0.174)
Gender		0.0311 (0.346)		-0.518 (0.505)
Education		0.386 (0.686)		-0.0555 (0.497)
Risk attitude (survey)		-0.00713 (0.0982)		0.0408 (0.132)
Generalized trust		0.114 (0.150)		-0.303* (0.0991)
Trust in technology		-0.0599 (0.136)		0.0735 (0.121)
Perceived dishonesty		0.0612 (0.155)		-0.381 (0.192)
Optimism		-0.364 (0.318)		0.665* (0.285)
Constant	3.355** (0.735)	3.170** (0.845)	0.235 (0.817)	1.185 (2.227)
<i>N</i>	81	81	63	63

Standard errors in parentheses

Standard Errors adjusted for 13 clusters in session

* $p < .05$, ** $p < .01$, *** $p < .001$

Table A.8: Longitudinal Logit regressions with RoCL Loving explaining openness to SC

	(1) No Experience Openness to SC	(2) No experience Openness to SC	(3) Experience Openness to SC	(4) Experience Openness to SC
Risk loving	1.196 (0.152)	1.214 (0.202)	1.557** (0.245)	1.519** (0.200)
RoCL loving	0.221*** (0.0733)	0.217*** (0.0788)	0.566 (0.274)	0.552 (0.285)
Gender		0.905 (0.289)		0.549 (0.260)
Education		1.773 (0.911)		1.004 (0.334)
Risk attitude (survey)		1.016 (0.088)		1.050 (0.155)
Generalized trust		1.129 (0.100)		0.722* (0.093)
Trust in tech		0.902 (0.085)		1.094 (0.0988)
Perceived dishonesty		1.133 (0.174)		0.682* (0.104)
Optimism		0.698 (0.207)		1.959* (0.548)
Constant	0.656 (0.424)	0.539 (0.569)	0.0630*** (0.0530)	0.132 (0.247)
<i>N</i>	567	567	441	441

Exponentiated coefficients; Standard errors in parentheses

Standard Errors adjusted for 13 clusters in session

* $p < .05$, ** $p < .01$, *** $p < .001$

Table A.9: Longitudinal Logit regressions with Ambiguity Loving explaining openness to SC

	(1) No Experience Openness to SC	(2) No experience Openness to SC	(3) Experience Openness to SC	(4) Experience Openness to SC
Risk loving	1.182 (0.173)	1.200 (0.203)	1.538** (0.241)	1.507** (0.209)
Ambiguity loving	0.427* (0.150)	0.438* (0.151)	0.846 (0.294)	0.828 (0.293)
Gender		1.035 (0.268)		0.575 (0.278)
Education		1.553 (0.860)		0.969 (0.388)
Risk attitude (survey)		1.017 (0.0765)		1.035 (0.140)
Generalized trust		1.091 (0.134)		0.726* (0.105)
Trust in tech		0.943 (0.109)		1.094 (0.100)
Perceived dishonesty		1.130 (0.142)		0.709** (0.127)
Optimism		0.712 (0.186)		1.942* (0.560)
Constant	0.529 (0.325)	0.397 (0.355)	0.0563*** (0.0491)	0.115 (0.207)
<i>N</i>	567	567	441	441

Exponentiated coefficients; Standard errors in parentheses

Standard Errors adjusted for 13 clusters in session

* $p < .05$, ** $p < .01$, *** $p < .001$

A.1.2 Questionnaire

Consent to participate in this research

Your participation in this survey implies your understanding of the following information.

- i. **Tasks and time:** You will first need to complete an online questionnaire (which takes about 15/20 minutes) to access the lab experiment (which takes about 1.5 hours).
- ii. **Confidentiality:** Participation in this study is anonymous by default. The data we collect will be used in our research papers. The data we collect will not be sold.
- iii. **Compensation and voluntary participation:** You will be compensated for participating in the study. You are free to refuse to participate in this study or to terminate your participation at any time during the study. However, only full participation (completed questionnaire and participation in the laboratory experiment) will be eligible for payment. Completion of the questionnaire is required to gain access to the laboratory experiment. You will only be able to complete the questionnaire once, and you will not have the option of stopping the questionnaire and starting it again later.

(A) Gender:

Female

Male

(B) Your Age:

Enter your age:

(C) Education Level:

Baccalaureate or equivalent

- 1st year bachelor degree or equivalent
- 2nd year bachelor degree or equivalent
- 3rd year bachelor degree or equivalent
- 1st year Master degree or equivalent
- 2nd year Master degree or equivalent
- Others

(D) You:

- have a car and you use it
- have a car and you don't use it
- don't have a car and you use it
- don't have a car and you don't use it

(E) CONSIDER THE FOLLOWING DEFINITION OF DISHONESTY: "Lack of integrity and honesty to the detriment of a counterparty and/or the citizens". How do you consider the level of dishonesty in the following contexts? Take as a reference the country in which you live.

1) Insurance sector:

Low Medium-Low Medium-high High I'm not sure, I have no information, I am not aware

2) Car repair shops:

Low Medium-Low Medium-high High I'm not sure, I have no information, I am not aware

3) Public administrations (tax office, university services, etc.):

Low Medium-Low Medium-high High I'm not sure, I have no information, I am not aware

4) Bitcoin financial system:

Low Medium-Low Medium-high High I'm not sure, I have no information, I am not aware

5) Supermarket:

Low Medium-Low Medium-high High I'm not sure, I have no information, I am not aware

6) Amazon:

Low Medium-Low Medium-high High I'm not sure, I have no information, I am not aware

7) Social Network (Facebook):

Low Medium-Low Medium-high High I'm not sure, I have no information, I am not aware

8) Banking sector:

Low Medium-Low Medium-high High I'm not sure, I have no information, I am not aware

9) Betting sector (soccer, basketball):

Low Medium-Low Medium-high High I'm not sure, I have no information, I am not aware

(F) How much do you agree with the following statements:

1) Vague and impressionistic pictures really have little appeal for me:

Totally disagree Somewhat disagree Somewhat agree Totally agree
 I don't know

2) Before an examination, I feel much less anxious if I know how many questions there will be:

Totally disagree Somewhat disagree Somewhat agree Totally agree
 I don't know

3) In uncertain times, I usually expect the best:

Totally disagree Somewhat disagree Somewhat agree Totally agree
 I don't know

4) Vague and impressionistic pictures appeal to me more than realistic pictures:

Totally disagree Somewhat disagree Somewhat agree Totally agree

I don't know

5) When I undertake something new, I expect to succeed:

Totally disagree Somewhat disagree Somewhat agree Totally agree

I don't know

6) I try to avoid situations that are ambiguous:

Totally disagree Somewhat disagree Somewhat agree Totally agree

I don't know

7) I feel relieved when an ambiguous situation suddenly becomes clear:

Totally disagree Somewhat disagree Somewhat agree Totally agree

I don't know

8) When a situation is uncertain, I generally expect the worst to happen:

Totally disagree Somewhat disagree Somewhat agree Totally agree

I don't know

9) I find it hard to make a choice when the outcome is uncertain:

Totally disagree Somewhat disagree Somewhat agree Totally agree

I don't know

(G) On a scale of 1 to 10, how would you rate your attitude toward risk: are you a risk-averse person (1) or do you like to take risks (10)?

1 2 3 4 5 6 7 8 9 10

(H) Generally speaking, do you think that most of the people can be trusted or that it is better not to trust others? (Yes, most of the people can be trusted, No it is better not to trust)

yes

no

(I) On a 0-10 scale, how much do you trust others in general, where 0 indicates that it is better not to trust and 10 indicates that is better to fully trust?

0 1 2 3 4 5 6 7 8 9 10

(J) **Generally speaking, do you think that most of the new digital technologies can be trusted or that it is better not to trust them?** (Yes, most new digital technologies can be trusted, No, it is better not to trust them)

yes

no

(K) **On a 0-10 scale, how much do you trust new digital technologies in general, where 0 indicates that it is better not to trust and 10 indicates that is better to fully trust?**

0 1 2 3 4 5 6 7 8 9 10

A.1.3 Instructions

Welcome to the Laboratory of Experimental Economics of Nice. You are about to participate in an experiment where your decisions will be anonymous and will partly determine your final payment, so please read the following instructions carefully.

In addition to the earnings collected in the experiment and independently of your decisions, a fixed amount of 8 euros will be given to you to cover your travel expenses and your participation in the questionnaire previously distributed by email. A variable amount will be added according to the decisions taken during the experiment. The total amount of your winnings will be distributed to you individually and confidentially at the end of the experience after you have completed a short questionnaire.

To avoid distorting the results of the experience, we will ask you not to communicate or embarrass other participants. We also ask that you please turn off your mobile phones and not use them for the duration of the experience. Failure to comply with these rules will result in the termination of the experience, and any winnings will be forfeited.

If you encounter a technical problem, all we ask is that you raise your hand silently

and wait for the experimenter to come to you.

Everyone in this room has access to the same instructions and will participate in the same experiment.

Finally, this experiment should last an hour and a half.

Overview

The experiment consists of 4 independent parts. Only one of the 4 parts will be randomly selected at the end of the experiment with a 4-sided die to determine your variable payment. Each party has an equal chance of being selected. Each game lasts a maximum of 180 seconds = 3 minutes. The remaining time will be displayed in the top right corner of the screen. Please respect this time so as not to delay the other participants. For example, if PART3 is selected and you have won 32 euros in this game, we will add to this amount the fixed amount of 8 euros to determine your final payment: $32 + 8 = 40$ euros in total, paid anonymously at the end of the experience in a separate room.

At the entrance, are arranged [as shown in Figure A.2]:

- A set of 20 cards composed of nineteen (19) blue cards and one (01) red card;
- An automatic card shuffler;
- Dice;
- A hat containing marked balls of three different colors.

These objects will be used during the experiment.

In this experiment you will be asked to choose during each game how many cards you would prefer to have returned. The number and color of cards returned will determine your variable output. Note that each draw in this experiment will be done physically. Computers are only used to record your choices.

Figure A.2: Game table



One participant will be randomly selected to lay out the cards on a table and turn them over based on the decisions you make during the experiment. This participant will be called the “neutral subject”. He/she will be chosen at the beginning of the game and will remain inactive until the end. At the end of the experiment, the neutral subject will be in charge of making the draws for each participant in the following manner:

- All 20 cards will be shuffled 3 times in an automatic shuffler by the neutral subject;
- The set of cards will be handed to you face down by the neutral subject;
- You will place on your right the cards you want the neutral subject to turn over and on your left the cards you do not want him/her to turn over;
- The neutral subject will simultaneously turn over all the cards to your right.

In order to guarantee his/her neutrality, the neutral subject will be paid the maximum amount that can be obtained by taking part in this experiment, i.e. 40 euros (8 euros participation + 32 euros maximum variable gain).

Please re-read these instructions carefully. If you have any questions, raise your hand silently, an experimenter will come to your table. Otherwise, please wait until the draw for the neutral subject.

PART 1

During this part, you will be asked to choose the number of cards you wish to have returned by the neutral subject at the end of the experiment if PART 1 is drawn.

Each card returned by the neutral subject will bring you 2 euros. However, if the red card is returned, you will get a total amount of 4 euros. The fixed remuneration will not be affected and will always be added to your winnings at the end of the experiment. The chances (in percentage) of returning the red card, depending on the number of cards returned is given in the table.

In this section, you can only make one choice among the 7 that will be presented to you on the screen in the table below:

Ligne numéro	Nombre de cartes que le sujet neutre retournera :	Vous pouvez gagner :
1	4 cartes (20% de chances de retourner la CR)	Pas CR = 8 € CR = 4 €
2	6 cartes (30% de chances de retourner la CR)	Pas CR = 12 € CR = 4 €
3	8 cartes (40% de chances de retourner la CR)	Pas CR = 16 € CR = 4 €
4	10 cartes (50% de chances de retourner la CR)	Pas CR = 20 € CR = 4 €
5	12 cartes (60% de chances de retourner la CR)	Pas CR = 24 € CR = 4 €
6	14 cartes (70% de chances de retourner la CR)	Pas CR = 28 € CR = 4 €
7	16 cartes (80% de chances de retourner la CR)	Pas CR = 32 € CR = 4 €

Veillez sélectionner ci-dessous le numéro de la ligne que vous avez choisi

C 1 C 2 C 3 C 4 C 5 C 6 C 7

CR = CARTE ROUGE (RED CARD)

The first column of the table (reading from left to right) is the row numbers, the second is the number of cards you would like to see turned over, and the third is the amount you can win.

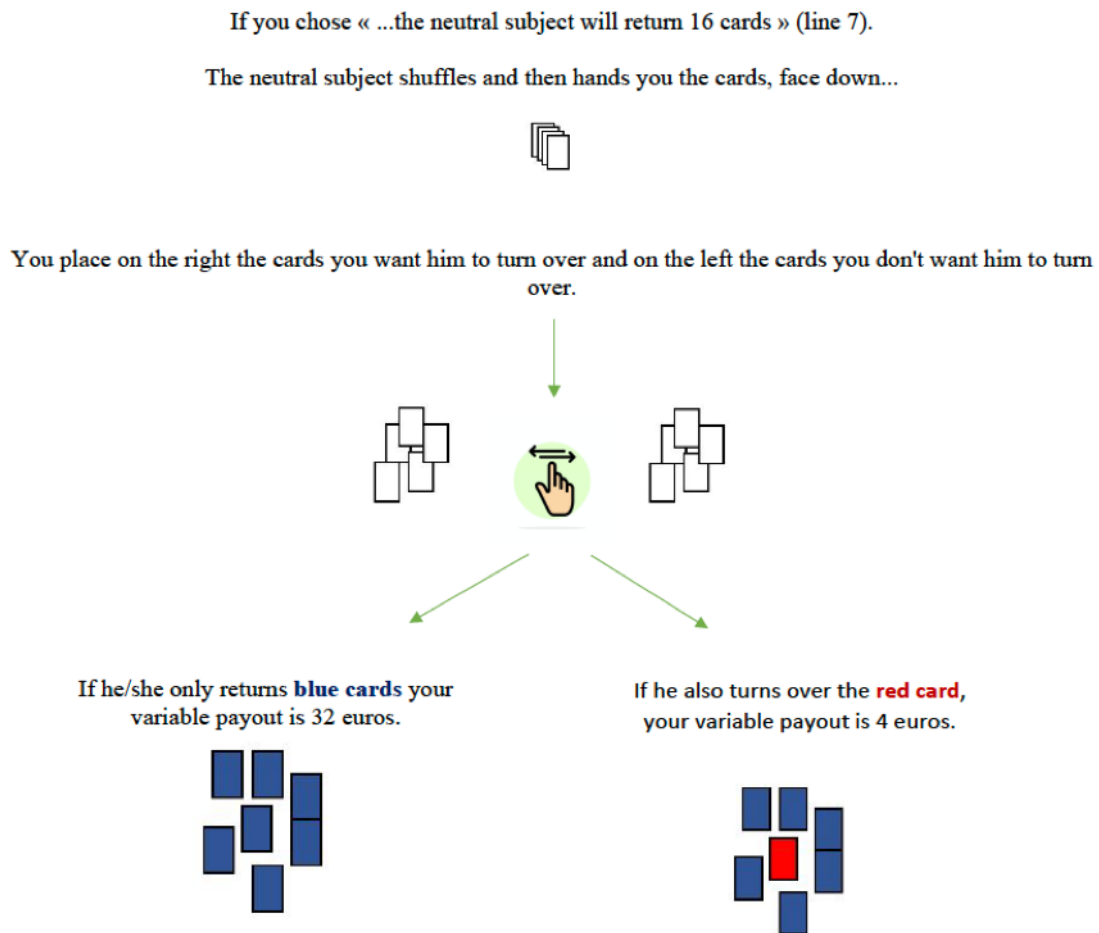
The list of choices that will be presented to you indicates the number of cards that

can be turned over for the first game. You can only choose one of the 7 choices by selecting the number of the corresponding line displayed at the bottom of the table, then validate by clicking on the “Continuer” (Continue) button. A confirmation message will then be displayed.

If you wish to change your choices, click on “Revoir” (Review). After clicking on “Confirmer” (Confirm), your choices will no longer be editable.

Please re-read these instructions carefully. If you have any questions, raise your hand silently, an experimenter will come to your table. Otherwise, please wait for the game to begin.

Figure A.3: PART 1. EXAMPLE



PART 2

In this part, you must choose the number of cards you wish to have returned by the neutral subject at the end of the experiment, if Part 2 is selected. For each of the choices that were proposed in Part 1, you will be asked whether you prefer that choice (OPTION A) or an alternative choice (OPTION B).

Thus, for each of the 7 lines, you must choose whether you prefer to have OPTION A or OPTION B completed at the end of the experiment. At the end of the experiment, if Part 2 is selected, the neutral subject will roll a 7-sided die and the option you have chosen for the corresponding line will be completed. The proposed options are presented in the table below:

Ligne numéro ____ :	OPTION A – Nombre de cartes que le sujet neutre retournera :	OPTION B – Nombre de cartes que le sujet neutre retournera :	Vous pouvez gagner pour une même ligne (quelque soit votre choix entre l'OPTION A ou l'OPTION B) :	Votre choix :
1	4 cartes (20% de chances de retourner la CR)	Entre 3 et 5 cartes	Pas CR = 8 € CR = 4 €	<input type="radio"/> Option A <input type="radio"/> Option B
2	6 cartes (30% de chances de retourner la CR)	Entre 5 et 7 cartes	Pas CR = 12 € CR = 4 €	<input type="radio"/> Option A <input type="radio"/> Option B
3	8 cartes (40% de chances de retourner la CR)	Entre 7 et 9 cartes	Pas CR = 16 € CR = 4 €	<input type="radio"/> Option A <input type="radio"/> Option B
4	10 cartes (50% de chances de retourner la CR)	Entre 9 et 11 cartes	Pas CR = 20 € CR = 4 €	<input type="radio"/> Option A <input type="radio"/> Option B
5	12 cartes (60% de chances de retourner la CR)	Entre 11 et 13 cartes	Pas CR = 24 € CR = 4 €	<input type="radio"/> Option A <input type="radio"/> Option B
6	14 cartes (70% de chances de retourner la CR)	Entre 13 et 15 cartes	Pas CR = 28 € CR = 4 €	<input type="radio"/> Option A <input type="radio"/> Option B
7	16 cartes (80% de chances de retourner la CR)	Entre 15 et 17 cartes	Pas CR = 32 € CR = 4 €	<input type="radio"/> Option A <input type="radio"/> Option B

CR = CARTE ROUGE (RED CARD)

The first column and the second column (named OPTION A) of the table correspond exactly to the line numbers and the list of choices proposed in Part 1. The third column (OPTION B) shows the alternative choice to the one proposed in OPTION A and the fourth column shows the amount you can win, which is the same for both options. Two buttons are provided for this purpose in the fifth column. For each row, the amount you can win is independent of whether you have chosen OPTION A or OPTION B. In fact, only the amounts indicated in the fourth column determine your win. Once you have chosen an option for each row in the table,

please click “Continuer” (Continue).

At the end of the experiment, if Part 2 is selected, the neutral subject will roll a 7-sided dice to determine which line to play.

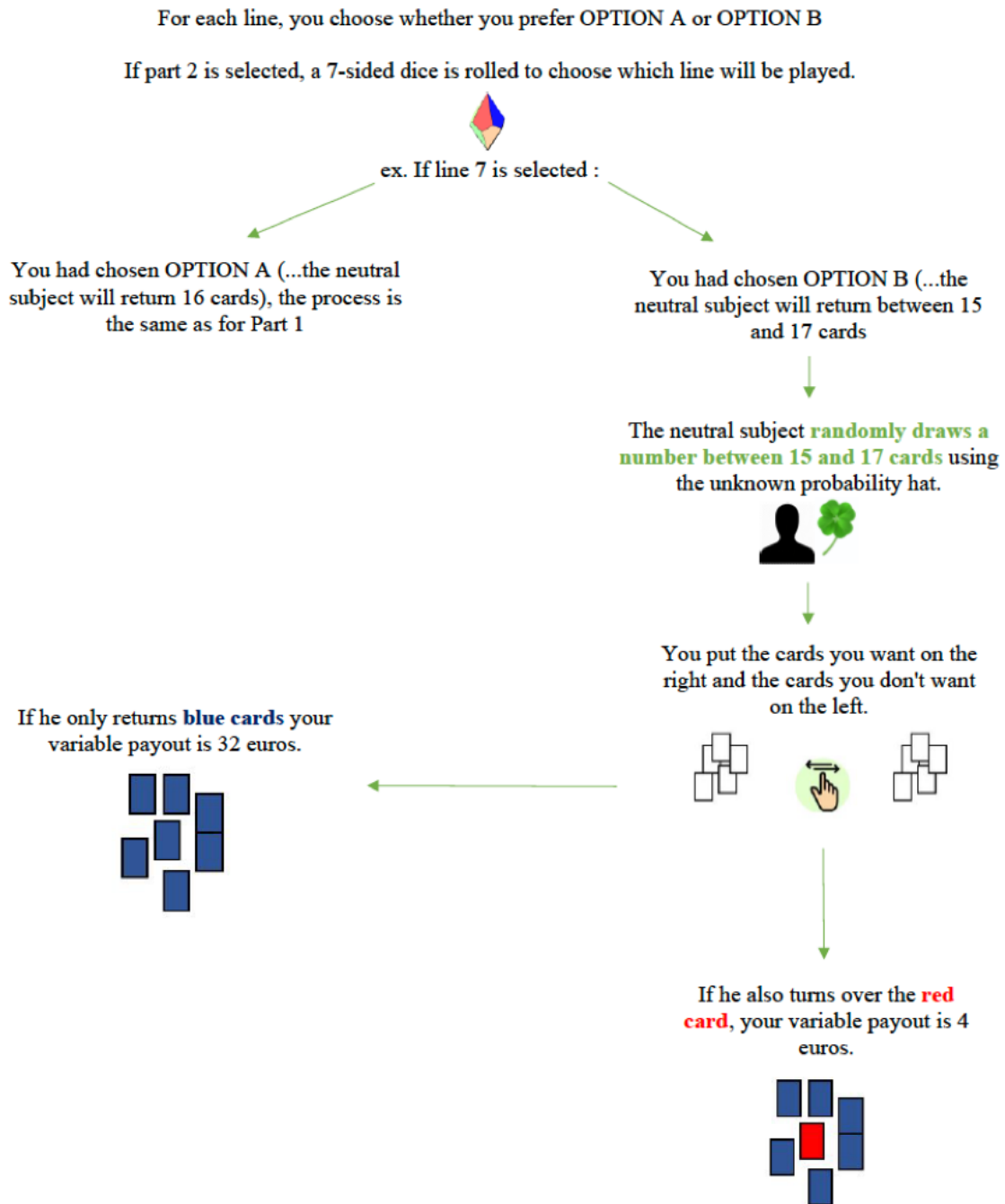
Then, for example, **if you chose OPTION A in the selected line, your winnings are determined in the same way as in Part 1 (see Figure 7).** If, on the other hand, **you have chosen OPTION B in the selected row:**

- i. The neutral subject will draw the number of cards to be turned over within the range indicated in the corresponding box using the unknown probability hat, and will ask you to place the cards you wish to be turned over to your right.
- ii. If none of the returned cards are red, your variable payout corresponds to the “PAS CR” case in the 4th column.
- iii. If one of the returned cards is red, your variable output is 4 euros.

Finally, to draw the exact number of cards you must choose if you have chosen option B, the neutral subject will have the hat of unknown probabilities containing 50 marked balls of 3 different colors: pink, blue and green. The exact number of each color in the hat is unknown. If he/she shoots a pink ball, he/she uses the smallest number in the range; if he/she shoots a blue ball, he/she uses the number in the middle, and finally, if he/she shoots a green ball, he/she uses the largest number in the range.

Please re-read these instructions carefully. If you have any questions, please raise your hand silently, and an experimenter will come to your table. Otherwise, please wait for the game to begin.

Figure A.4: PART 2. EXAMPLE



PART 3

In this part, you must choose the number of cards you wish to have returned by the neutral subject at the end of the experiment if Part 3 is selected.

As in the previous part, you will have to choose, for each row of the table, between two options. OPTION A, similar to the options presented in PART 1, and OPTION B. The potential earnings are the same for both options.

OPTION B is a two-step draw. Only the second draw indicates your variable winnings. Your variable win is only 4 euros if the red card is drawn in the second draw. The first draw indicates the number of cards that will be returned in the second draw.

So, if the red card is returned in the first draw, more cards will be returned in the second draw. The choices will be presented in the following table:

Ligne numéro ...	OPTION A ... nombre de cartes que le sujet veut retourner	OPTION B ... nombre de cartes que le sujet retournera	Vous pouvez choisir pour une même ligne (quelque soit votre choix entre l'OPTION A ou l'OPTION B)	Votre choix
1	4 cartes (20% de chances de retourner la CR)	4 cartes ET si la CR apparaît il en retournera 10 OU 4 cartes ET si la CR n'apparaît pas il en retournera 2	Pas CR = 8 € CR = 4 €	<input type="radio"/> Option A <input type="radio"/> Option B
2	6 cartes (30% de chances de retourner la CR)	6 cartes ET si la CR apparaît il en retournera 10 OU 6 cartes ET si la CR n'apparaît pas il en retournera 4	Pas CR = 12 € CR = 4 €	<input type="radio"/> Option A <input type="radio"/> Option B
3	8 cartes (40% de chances de retourner la CR)	8 cartes ET si la CR apparaît il en retournera 10 OU 8 cartes ET si la CR n'apparaît pas il en retournera 7	Pas CR = 16 € CR = 4 €	<input type="radio"/> Option A <input type="radio"/> Option B
4	10 cartes (50% de chances de retourner la CR)	10 cartes ET si la CR apparaît il en retournera 10 OU 10 cartes ET si la CR n'apparaît pas il en retournera 10	Pas CR = 20 € CR = 4 €	<input type="radio"/> Option A <input type="radio"/> Option B
5	12 cartes (60% de chances de retourner la CR)	12 cartes ET si la CR apparaît il en retournera 13 OU 12 cartes ET si la CR n'apparaît pas il en retournera 10	Pas CR = 24 € CR = 4 €	<input type="radio"/> Option A <input type="radio"/> Option B
6	14 cartes (70% de chances de retourner la CR)	14 cartes ET si la CR apparaît il en retournera 16 OU 14 cartes ET si la CR n'apparaît pas il en retournera 10	Pas CR = 28 € CR = 4 €	<input type="radio"/> Option A <input type="radio"/> Option B
7	16 cartes (80% de chances de retourner la CR)	16 cartes ET si la CR apparaît il en retournera 18 OU 16 cartes ET si la CR n'apparaît pas il en retournera 10	Pas CR = 32 € CR = 4 €	<input type="radio"/> Option A <input type="radio"/> Option B

CR = CARTE ROUGE (RED CARD)

For each line, you will therefore choose whether you prefer OPTION A or OPTION B.

If PART 3 has been selected to be played at the end of the experiment, the neutral subject will roll a 7-sided die to determine which line to play.

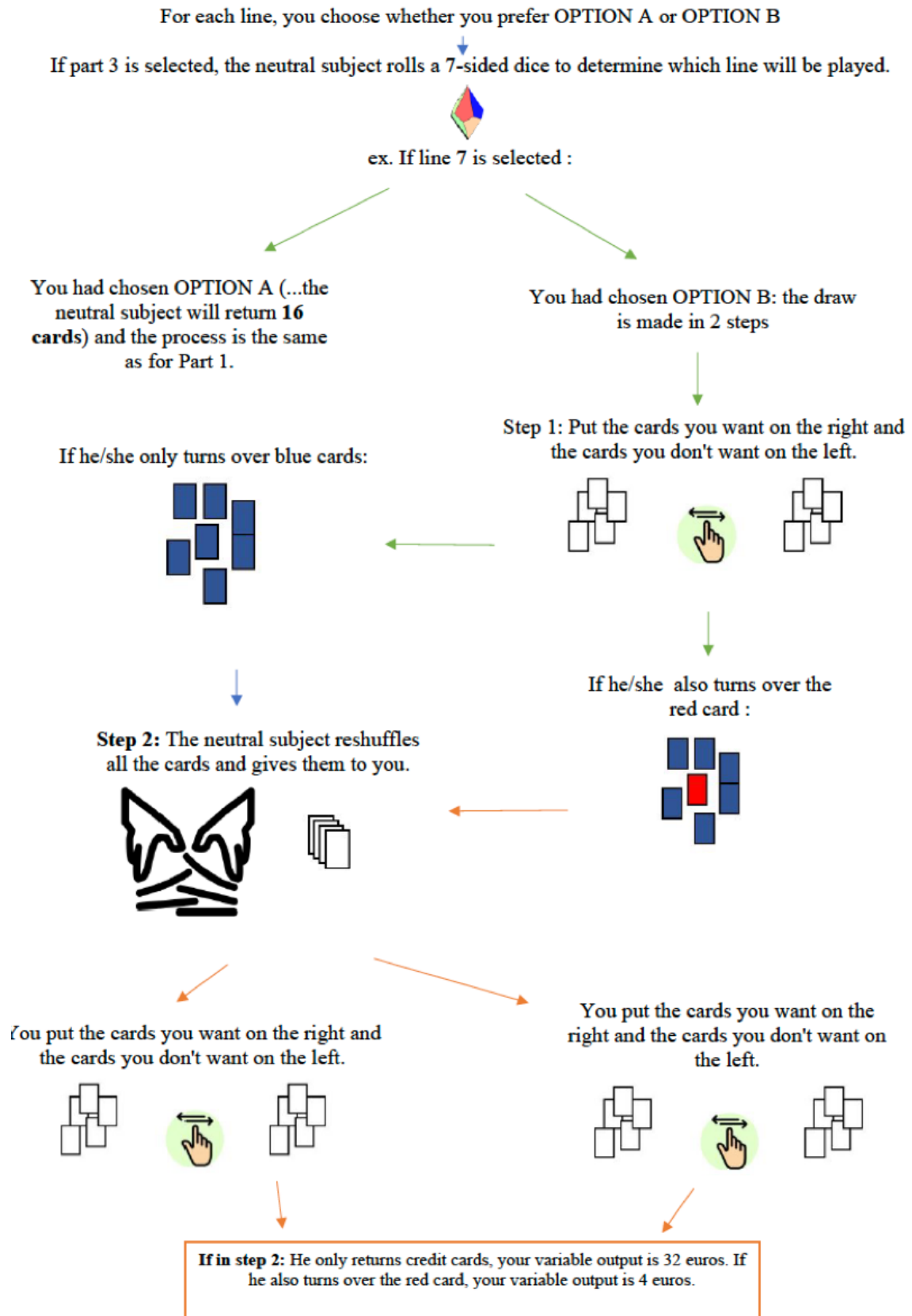
For example, if you chose **OPTION A** in the selected line, your winnings are determined in the same way as in **PART 1** (see Figure A.5).

If, on the other hand, you have chosen **OPTION B** in the selected line, the draw takes place in 2 steps:

- i. You choose a precise number of cards (defined according to the line) by placing them on your right, then the neutral subject turns them over and observes the result;
- ii. Depending on whether the red card has been turned over or not, the table shows you the number of cards that the neutral subject will turn over for the second step. The neutral subject shuffles and then hands you the set of cards again so that you choose again by placing on the right the cards you want him to turn over and on the left the ones you do not want him to turn over. Finally, he/she turns over the selected cards at the same time. If none of the cards turned over in this second draw are red, your variable winnings correspond to the "PAS CR" case in the 4th column. If one of the cards turned over in this second draw is red, your variable winnings are 4 euros.

Please re-read these instructions carefully. If you have any questions, raise your hand silently, and an experimenter will come to your table. Otherwise, please wait for the game to begin.

Figure A.5: PART 3. EXAMPLE



PART 4

In this part, you must choose the number of cards you wish to have returned by the neutral subject at the end of the experiment if PART 4 is selected.

As in the previous part, you will then be presented with a table with the same number of rows and columns. Once again, for each row of the table below, you must choose whether you prefer OPTION A or OPTION B.

Ligne à choisir	OPTION A – Nombre de cartes que le sujet souhaite retourner	OPTION B – Nombre de cartes que le sujet souhaite retourner	Vous pouvez choisir pour une même ligne également vos deux choix entre l'OPTION A ou l'OPTION B	Votre choix
1	4 cartes (20% de chances de retourner la CR)	4 cartes ET si la CR apparaît, il retournera entre 3 et 5 cartes OU 4 cartes ET si la CR n'apparaît pas, il retournera entre 1 et 3 cartes	Pas CR = 8 € CR = 4 €	<input type="radio"/> Option A <input type="radio"/> Option B
2	6 cartes (30% de chances de retourner la CR)	6 cartes ET si la CR apparaît, il retournera entre 5 et 7 cartes OU 6 cartes ET si la CR n'apparaît pas, il retournera entre 2 et 4 cartes	Pas CR = 12 € CR = 4 €	<input type="radio"/> Option A <input type="radio"/> Option B
3	8 cartes (40% de chances de retourner la CR)	8 cartes ET si la CR apparaît, il retournera entre 7 et 9 cartes OU 8 cartes ET si la CR n'apparaît pas, il retournera entre 3 et 5 cartes	Pas CR = 16 € CR = 4 €	<input type="radio"/> Option A <input type="radio"/> Option B
4	10 cartes (50% de chances de retourner la CR)	10 cartes ET si la CR apparaît, il retournera entre 8 et 11 cartes OU 10 cartes ET si la CR n'apparaît pas, il retournera entre 4 et 6 cartes	Pas CR = 20 € CR = 4 €	<input type="radio"/> Option A <input type="radio"/> Option B
5	12 cartes (60% de chances de retourner la CR)	12 cartes ET si la CR apparaît, il retournera entre 11 et 13 cartes OU 12 cartes ET si la CR n'apparaît pas, il retournera entre 5 et 7 cartes	Pas CR = 24 € CR = 4 €	<input type="radio"/> Option A <input type="radio"/> Option B
6	14 cartes (70% de chances de retourner la CR)	14 cartes ET si la CR apparaît, il retournera entre 13 et 15 cartes OU 14 cartes ET si la CR n'apparaît pas, il retournera entre 6 et 8 cartes	Pas CR = 28 € CR = 4 €	<input type="radio"/> Option A <input type="radio"/> Option B
7	16 cartes (80% de chances de retourner la CR)	16 cartes ET si la CR apparaît, il retournera entre 15 et 17 cartes OU 16 cartes ET si la CR n'apparaît pas, il retournera entre 7 et 9 cartes	Pas CR = 32 € CR = 4 €	<input type="radio"/> Option A <input type="radio"/> Option B

CR = CARTE ROUGE (RED CARD)

When you have reached the end of the table, please click on “Continuer” (Continue).

For each line you will therefore choose whether you prefer OPTION A or OPTION B. If PART 4 has been selected to be played at the end of the experiment, the neutral subject will roll a 7-sided dice to determine which line to play.

For example, if you chose **OPTION A** in the selected line, your winnings are determined in the same way as in PART 1 (see Figure A.6).

However, if you have chosen **OPTION B** in the selected line, the draw takes place in 2 steps:

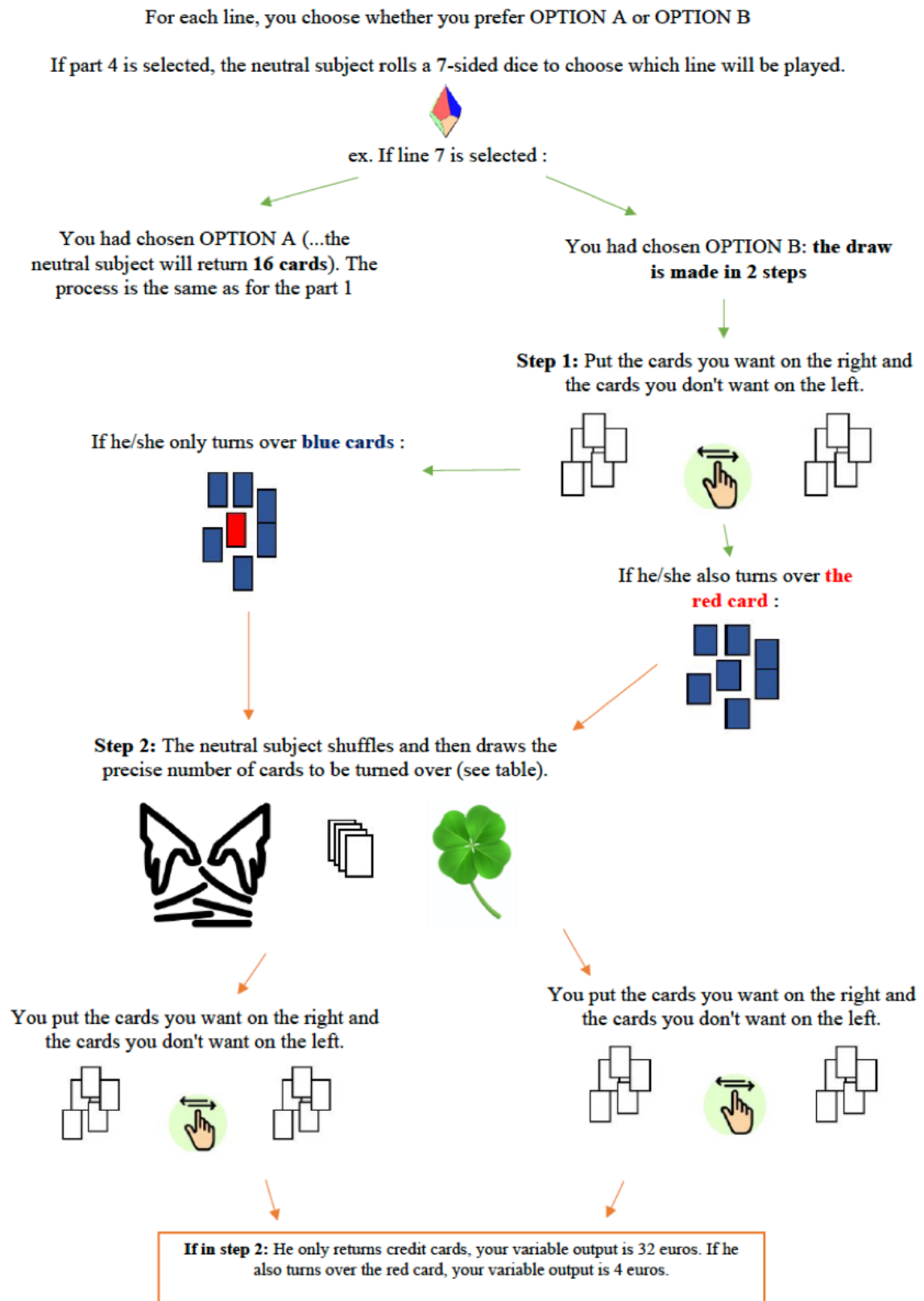
- i. You choose a precise number of cards (defined according to the line) by placing them on your right, then the neutral subject turns them over and observes

the result;

- ii. Depending on whether the red card has been turned over or not, the neutral player draws with the unknown probability hat and then tells you how many cards he/she will turn over for the second draw. He/she then shuffles and gives you all the cards again so that you choose again by placing the cards you want him to turn over to the right and the cards you don't want to turn over to the left. Finally, he/she turns over the selected cards at the same time. To draw the exact number of cards you must choose if you chose option B, the process is the same as in PART 2: it draws from the unknown probability hat with 50 balls marked with three different colors (pink, blue, green). If he/she draws a pink ball, he/she uses the smallest number in the range; if he/she shoots a blue ball, he/she uses the number in the middle, and finally, if he/she shoots a green ball, he/she uses the largest number in the range. If none of the cards returned in this second draw are red, your variable winnings correspond to the "PAS CR" case in the 4th column. If one of the cards turned over in this second draw is red, your variable payout is 4 euros.

Please read these instructions carefully. If you have any questions, raise your hand silently, and an experimenter will come to your table. Otherwise, please wait until the game starts.

Figure A.6: PART 4. EXAMPLE



End of the experiment

Thank you for participating in this experiment on economic decision-making. The experiment is now over.

We will now post a short questionnaire. Please fill it out accurately.

Please stay in your place until the experimenter calls you individually to make the draws and payments. Before that, we will draw the game and the line that will be played.

B.1 Appendix B

B.1.1 Dealing with inconsistent choices

Inconsistent observations generated by multiple switching usually lead researchers to discard them. Some authors have developed heuristic strategies to deal with the missing information and to avoid losing observations. For methods eliciting valuations such as Abdellaoui et al. (2011) and Baillon, Huang, et al. (2018), one technique involves calculating the CE or MP for each inconsistent individual by taking the midpoint of the interval between the first and the last switching point. This calculation can only be done if the first decision of the list is an ambiguous lottery (Fairley & Sanfey, 2020; Kocher et al., 2018). The values thus recovered can be included in the model. We will apply this technique for methods A and B. For example, if a participant reveals three MP of 0.15, 0.20 and 0.5 in method B, the MP of the participant is calculated as $0.15 + 0.5/2 = 0.325$.

For choice tasks like (Chakravarty & Roy, 2009), some authors used the simple strategy of counting the number of risky choices relative to the ambiguous ones (see, e.g., Attanasi et al., 2021; M. Cohen et al., 2011; Drouvelis & Jamison, 2015; Koch & Schunk, 2013; Tymula et al., 2012). This constitutes a model-free characterization of ambiguity attitude, which despite its simplicity, is imprecise by construction. Indeed, by relying only on the proportion of ambiguous choices relative to risky ones, it equally treats individuals who switched at the top of the list and those who switched at the bottom, thereby disconnecting the ambiguity attitude degree from the attractiveness of the lottery. We will check whether there is a strong correlation between the model-based and the model-free measures in method C.

B.1.2 Experimental Instructions

Participants will receive instructions in French (see Supplementary Materials). We have made the following translation. The headlines at the beginning of the instructions will not be shown to the participants. We will insert the control and working memory tasks (in that order) between the first and second measures of ambiguity aversion. The usual precautions required for the effective conduct of an individual decision-making experiment will be taken and notified to the participants (isolated computers, no communication, etc.).

General Instructions (for all participants)

Welcome to the Laboratory of Experimental Economics of Côte d'Azur University (LEEN-NiceLab). By agreeing to participate in this experiment, you signify your agreement with the Laboratory rules, available on the website or by request. You are about to participate in an experiment where your decisions will be anonymous and partly determine your final payment, so please read the following instructions carefully. In addition to the winnings collected in the experiment and independently of your decisions, you will be given a fixed amount of 7€ to cover your travel expenses. A variable amount will be added according to your decisions during the experiment. The total amount of your winnings will be distributed to you individually and confidentially at the end of the experiment after you have completed a questionnaire. To avoid distorting the experiment's results, we ask you not to communicate with or disturb other participants. We also ask you to switch off your cell phones and not use them for the experiment. In the event of a breach of these rules, the experiment will be interrupted, and winnings forfeited.

The experiment consists of several independent rounds. Each round lasts a maximum of 5 minutes. Instructions from each round will be sent to you as they

become available. Your decisions and chance will determine your variable winnings. To determine your variable payoff, only one of the rounds will be randomly selected at the end of the experiment. In this way, each round has an equal chance of being selected. You'll make decisions from the computer before you for each task. You can reread these instructions at any time. If you have questions, raise your hand silently, and an experimenter will come to your table. Otherwise, please wait for the rounds to start.

Instructions for Method A (title not shown to participants)

The rounds about to start are all independent from each other. They each comprise 16 decisions. If one of the following rounds is drawn at the end of the experiment, only one of the 16 decisions will be drawn randomly by the computer for payment. In each round and for each decision, you must choose between receiving a sure win or drawing a ball from a URN.

Rounds 1 to 7:

Not shown to participants: Instructions also apply to the repetition phase for rounds 16 to 22

The urn contains 8 balls of different colors. You must choose between receiving a sure win or drawing a ball from this urn. If you choose to draw a ball from the URN, you'll have the opportunity to win €15 if the ball of the color(s) shown on the screen is (are) drawn, and €0 otherwise.

Rounds 8 to 14:

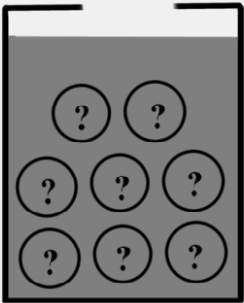
Not shown to participants: Instructions also apply to the repetition phase for rounds 23 to 29

The urn contains 8 balls, which may have the same or different colors or the colors may be repeated several times in the same urn. You, therefore, do not know the distribution of the different colors in the urn. Its exact composition will be revealed once we receive your payment. You must choose between receiving a sure win or drawing a ball from the urn. If you choose to draw a ball from the URN, you can win 15€ if the ball of the color(s) displayed on the screen is drawn, and 0€ otherwise.

To make your choices, a table will be displayed (read from left to right) with several rows, and on each of them, two options (in columns), OPTION 1 and OPTION 2. For each line of the table, you must decide whether you prefer OPTION 1

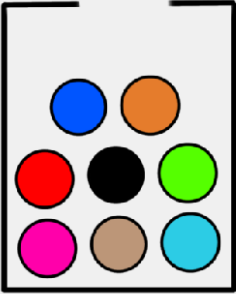








or OPTION 2. Click the "Continue" button to confirm your choices and move on to the next round. Once you've confirmed your choices for a round, you can't go back. At the end of the experiment, if round 15 and line 1 are selected, for example, and you have chosen the URN (OPTION 1) for this line, you will draw a ball from the urn. If you have questions, raise your hand silently, and an experimenter will come to your table. Otherwise, please wait for the rounds to start.

Figure B.1: Screenshot of a choice list in method A (Unknown urn, 4 winning colors)

Option 1 Tirer une boule dans l'Urne	1 2	Option 2 Recevoir cette somme :
 <p>Gagner 15 € si : ● ou ● ou ● ou ●</p> <p>Gagner 0€ si : ● ou ● ou ● ou ●</p>	<input type="radio"/> <input type="radio"/>	0€
	<input type="radio"/> <input type="radio"/>	1€
	<input type="radio"/> <input type="radio"/>	2€
	<input type="radio"/> <input type="radio"/>	3€
	<input type="radio"/> <input type="radio"/>	4€
	<input type="radio"/> <input type="radio"/>	5€
	<input type="radio"/> <input type="radio"/>	6€
	<input type="radio"/> <input type="radio"/>	7€
	<input type="radio"/> <input type="radio"/>	8€
	<input type="radio"/> <input type="radio"/>	9€
	<input type="radio"/> <input type="radio"/>	10€
	<input type="radio"/> <input type="radio"/>	11€
	<input type="radio"/> <input type="radio"/>	12€
	<input type="radio"/> <input type="radio"/>	13€
	<input type="radio"/> <input type="radio"/>	14€
<input type="radio"/> <input type="radio"/>	15€	

Continuer

Figure B.2: Screenshot of a choice list in method A (Known urn, 4 winning colors)

Option 1 Tirer une boule dans l'Urne	1	2	Option 2 Recevoir cette somme :
 <p data-bbox="236 674 655 712">Gagner 15 € si :  ou  ou  ou </p> <p data-bbox="236 750 624 788">Gagner 0€ si :  ou  ou  ou </p>	<input type="radio"/>	<input type="radio"/>	0€
	<input type="radio"/>	<input type="radio"/>	1€
	<input type="radio"/>	<input type="radio"/>	2€
	<input type="radio"/>	<input type="radio"/>	3€
	<input type="radio"/>	<input type="radio"/>	4€
	<input type="radio"/>	<input type="radio"/>	5€
	<input type="radio"/>	<input type="radio"/>	6€
	<input type="radio"/>	<input type="radio"/>	7€
	<input type="radio"/>	<input type="radio"/>	8€
	<input type="radio"/>	<input type="radio"/>	9€
	<input type="radio"/>	<input type="radio"/>	10€
	<input type="radio"/>	<input type="radio"/>	11€
	<input type="radio"/>	<input type="radio"/>	12€
	<input type="radio"/>	<input type="radio"/>	13€
	<input type="radio"/>	<input type="radio"/>	14€
<input type="radio"/>	<input type="radio"/>	15€	

Instructions for Method B (title not shown to participants)

Each round is independent of the others. They each comprise 20 decisions. If one of the following rounds is drawn at the end of the experiment, only one of the 20 decisions will be drawn randomly by the computer for payment.

Rounds 1 to 6 :

Not shown to participants: Instructions also apply to the repetition phase for rounds 8 to 13

In each round and for each decision, you must choose between the following two options:

- Win €15 by shooting a ball into an urn containing 9 balls. These balls can be RED, BLUE, and GREEN. However, you don't know the color distribution of the balls in the urn. The exact contents of the urn will be revealed to you upon receipt of your payment.
- Win €15 by rolling two 10-sided dice. The different chances of winning will be displayed on the screen.

Please note that this is the same ballot box used for all rounds 1 to 6. To make your choice between the two options, a table will be displayed (read from left to right) with several rows and on each of them two options (in columns), OPTION 1 and OPTION 2. For each line of the table, you must decide whether you prefer OPTION 1 or OPTION 2. Click on the "Continue" button to confirm your choices and move on to the next round. Once you've confirmed your choices for a round, you can't go back. At the end of the experiment, if round 5 and line 5 are selected, for example, and you have chosen the URN (OPTION 1) for this line, you will draw a ball from the URN. Otherwise, you'll roll the two 10-sided dice. If you have questions, raise your hand silently, and an experimenter will come to your table. Otherwise, please wait for the rounds to start.

Figure B.3: Screenshot of a choice list in method B (Single Event)

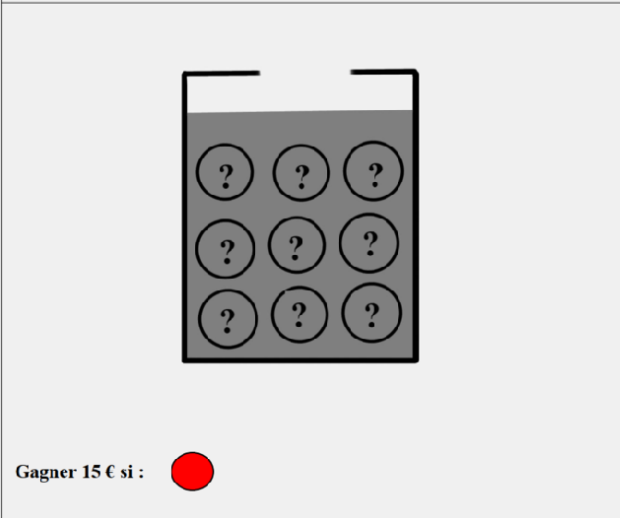
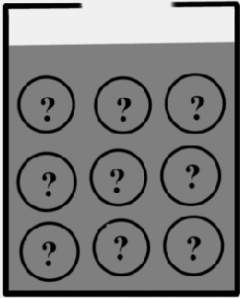


<p>Option 1 Vous gagnez 15€ si vous tirez une <i>boule rouge</i> (et rien sinon)</p>	<p>1</p>	<p>2</p>	<p>Option 2 Vous gagnez 15€ avec les probabilités suivantes : (et rien sinon)</p>
	<input type="radio"/>	<input type="radio"/>	0%
	<input type="radio"/>	<input type="radio"/>	1%
	<input type="radio"/>	<input type="radio"/>	2%
	<input type="radio"/>	<input type="radio"/>	5%
	<input type="radio"/>	<input type="radio"/>	10%
	<input type="radio"/>	<input type="radio"/>	15%
	<input type="radio"/>	<input type="radio"/>	20%
	<input type="radio"/>	<input type="radio"/>	25%
	<input type="radio"/>	<input type="radio"/>	30%
	<input type="radio"/>	<input type="radio"/>	35%
	<input type="radio"/>	<input type="radio"/>	40%
	<input type="radio"/>	<input type="radio"/>	45%
	<input type="radio"/>	<input type="radio"/>	50%
	<input type="radio"/>	<input type="radio"/>	55%
	<input type="radio"/>	<input type="radio"/>	60%
	<input type="radio"/>	<input type="radio"/>	65%
	<input type="radio"/>	<input type="radio"/>	70%
<input type="radio"/>	<input type="radio"/>	75%	
<input type="radio"/>	<input type="radio"/>	85%	
<input type="radio"/>	<input type="radio"/>	100%	

Figure B.4: Screenshot of a choice list in method B (Composite Event)

Option 1 Vous gagnez 15€ si vous tirez une <i>boule rouge</i> ou une <i>boule verte</i> (et rien sinon)	1	2	Option 2 Vous gagnez 15€ avec les probabilités suivantes : (et rien sinon)
 <p>Gagner 15 € si :  ou </p>	<input type="radio"/>	<input type="radio"/>	0%
	<input type="radio"/>	<input type="radio"/>	20%
	<input type="radio"/>	<input type="radio"/>	35%
	<input type="radio"/>	<input type="radio"/>	40%
	<input type="radio"/>	<input type="radio"/>	45%
	<input type="radio"/>	<input type="radio"/>	50%
	<input type="radio"/>	<input type="radio"/>	55%
	<input type="radio"/>	<input type="radio"/>	60%
	<input type="radio"/>	<input type="radio"/>	65%
	<input type="radio"/>	<input type="radio"/>	70%
	<input type="radio"/>	<input type="radio"/>	75%
	<input type="radio"/>	<input type="radio"/>	80%
	<input type="radio"/>	<input type="radio"/>	85%
	<input type="radio"/>	<input type="radio"/>	90%
	<input type="radio"/>	<input type="radio"/>	93%
	<input type="radio"/>	<input type="radio"/>	95%
	<input type="radio"/>	<input type="radio"/>	97%
<input type="radio"/>	<input type="radio"/>	98%	
<input type="radio"/>	<input type="radio"/>	99%	
<input type="radio"/>	<input type="radio"/>	100%	
			Continuer

Instructions for Method C (title not shown to participants)

Round 1

Not shown to participants: Instructions also apply to the repetition phase for round 2

The following round contains 10 decisions. If this round is drawn at the end of the experiment, only one of the 10 decisions will also be drawn randomly by the computer for payment. At the start of the round, we'll ask you to choose a color: RED or BLUE. For each decision, you must then choose to draw a ball from one of the two following urns:

- URN A, containing 5 RED balls and 5 BLUE balls.
- URN B contains 10 balls that are either all RED or all BLUE.

So you don't know the color of the balls in URN B (nor does the experimenter). How the URN B will be filled, and its exact contents will be revealed to you at the end of the experiment.

To make your choice between the two urns, a table will be displayed (read from left to right) with several rows and two options (in columns), URN A and URN B. For each line of the table, you must decide whether you prefer URN A or URN B.

Click on the "Validate" button to confirm your choices. You will not be able to go back. At the end of the experiment, if round 1 and line 5 are selected, for example, and you have chosen URN B for this line, you will draw a ball from this urn. You can reread these instructions at any time. If you have questions, raise your hand silently, and an experimenter will come to your table. Otherwise, please wait for the round to start. You do not know the color of the balls in urn B (nor does the experimenter). The process of filling urn B and its exact contents will be revealed to you at the end of the experiment. An illustration of the contents of the two urns has also been shared with you.

Figure B.5: Screenshot of the choice list in method C

Numéro de ligne:	URNE A		URNE B		Votre choix
	Si vous tirez une boule d'une couleur <i>que vous avez choisi</i> , vous gagnez :	Si vous tirez une boule d'une couleur <i>que vous n'avez pas choisi</i> , vous gagnez :	Si vous tirez une boule d'une couleur <i>que vous avez choisi</i> , vous gagnez :	Si vous tirez une boule d'une couleur <i>que vous n'avez pas choisi</i> , vous gagnez :	
-					-
1	2 €	0 €	10 €	0 €	<input type="radio"/> <input type="radio"/>
2	4 €	0 €	10 €	0 €	<input type="radio"/> <input type="radio"/>
3	7 €	0 €	10 €	0 €	<input type="radio"/> <input type="radio"/>
4	8 €	0 €	10 €	0 €	<input type="radio"/> <input type="radio"/>
5	9 €	0 €	10 €	0 €	<input type="radio"/> <input type="radio"/>
6	10 €	0 €	10 €	0 €	<input type="radio"/> <input type="radio"/>
7	11 €	0 €	10 €	0 €	<input type="radio"/> <input type="radio"/>
8	12 €	0 €	10 €	0 €	<input type="radio"/> <input type="radio"/>
9	13 €	0 €	10 €	0 €	<input type="radio"/> <input type="radio"/>
10	15 €	0 €	10 €	0 €	<input type="radio"/> <input type="radio"/>

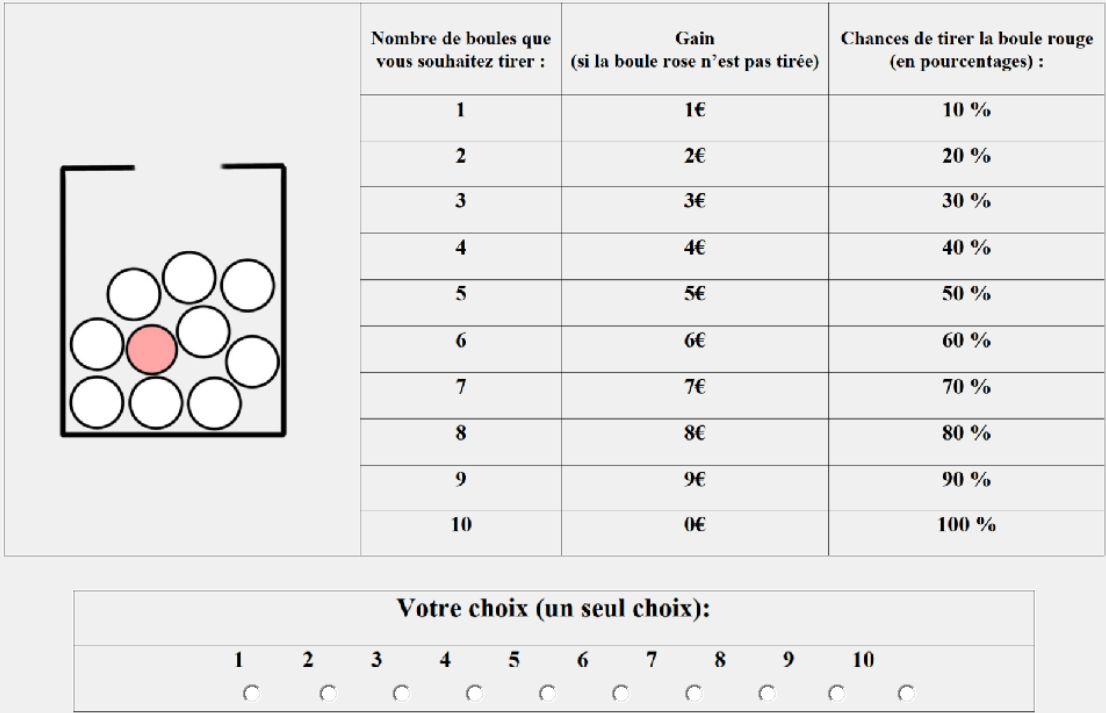
Instructions for control task (title not shown to participants)

This task is presented between methods A, B, or C repetitions.

As this round begins, you'll be asked to choose the number of balls you'd like to draw from a HAT at the end of the experiment if round 15 is selected. First, you'll

fill the HAT with 10 balls, including 9 white balls and 1 pink ball. Each white ball you shoot from the HAT will earn you 1 euro. However, if the pink ball is drawn, your collected winnings will be canceled. The fixed winnings remain unaffected. To help you choose, a table will be displayed (read from left to right) with all the relevant information about your potential winnings and the chances of drawing the pink ball.

Figure B.6: Screenshot of the control task (adaptation of the Bomb Risk Elicitation Task)



Nombre de boules que vous souhaitez tirer :	Gain (si la boule rose n'est pas tirée)	Chances de tirer la boule rouge (en pourcentages) :
1	1€	10 %
2	2€	20 %
3	3€	30 %
4	4€	40 %
5	5€	50 %
6	6€	60 %
7	7€	70 %
8	8€	80 %
9	9€	90 %
10	0€	100 %

Votre choix (un seul choix):

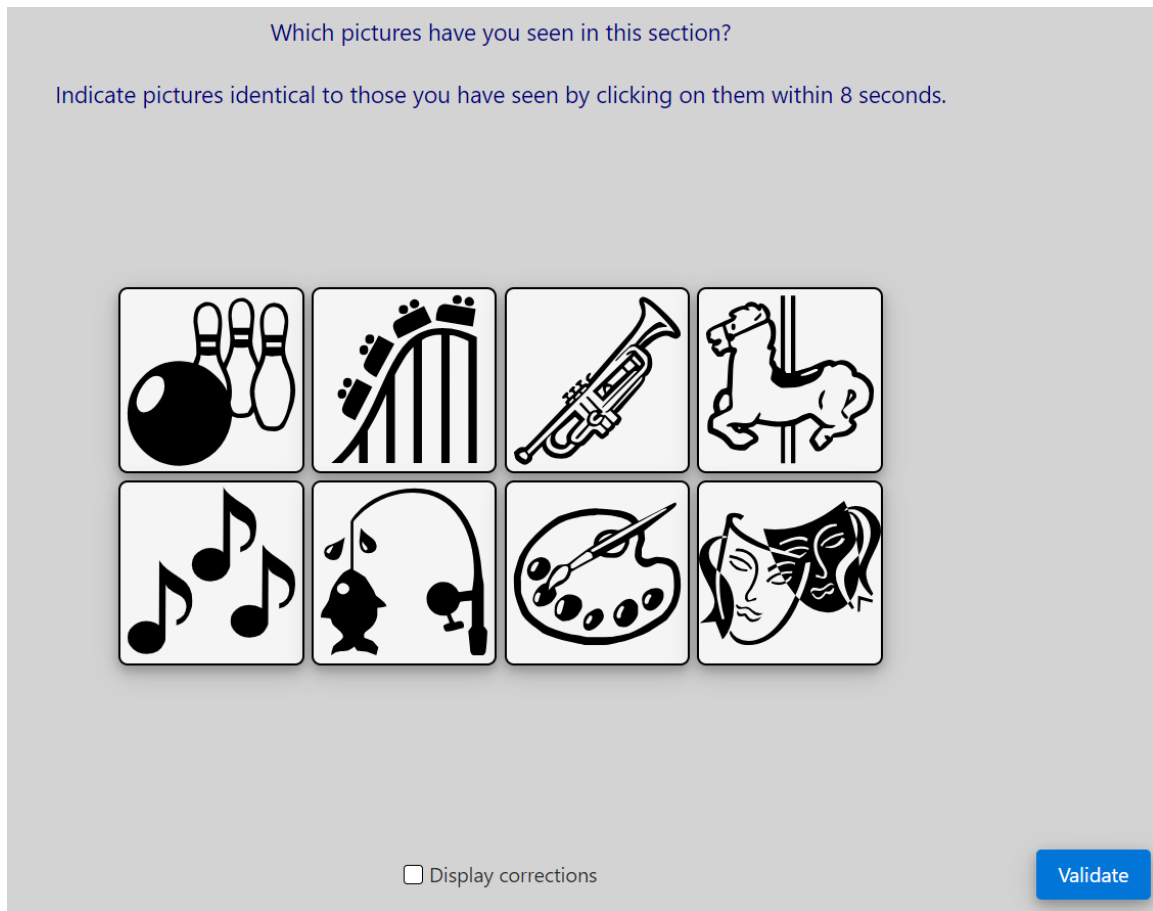
1 2 3 4 5 6 7 8 9 10

Additional task

This task is presented between the repetitions of methods A, B, or C after the control task.

At the end last round, we'll propose you an extra task. This is a popular memory test that you can find on the Internet. This memory test is also used for airplane pilots. Your performance in the memory test is not paid for. It's all about entertainment before the experiment continues. So do your best and have fun. At the end of the task, please enter your score in the box provided on your screen.

Figure B.7: Screenshot of an on going Delayed Matching to Sample test



B.1.3 Questionnaire (for all participants)

The questionnaire includes filter questions to separate individuals who have never been in contact with the source of the risk.

The section on alcohol dependence is a combination of the Alcohol Use Disorders Identification Test (AUDIT) (Babor et al., 2001) and questions from Santé Publique France (2017). The AUDIT allows detecting alcohol harmful drinking patterns. The test comprises 10 questions on alcohol consumption, dependence, and related harms. To help respondents, we will also display several alcohol types and their equivalents in quantity and concentration. The other questions in the section mainly concern binge drinking. The section on alcohol contains a total of 18 questions.

The section on cigarette dependence is based on the Cigarette Dependence

Scale (Etter et al., 2003) and questions from Santé Publique France (2021). The Cigarette Dependence Scale (CDS) is a self-report tool designed to assess the severity of nicotine dependence. It contains 12 items that address several elements of tobacco dependence, including consumption, craving, withdrawal symptoms, and failure to quit. The other questions in the section mainly concern the age of first smoking and questions on alternative forms of smoking, such as electronic cigarettes. The section on smoking contains a total of 22 questions.

Questions on gambling are based on the Canadian Problem Gambling Index (CPGI) (Ferris & Wynne, 2001) and additional questions from (Gaucher et al., 2023). The CPGI is a Canadian national screening tool that helps identify individuals at risk of experiencing problems related to excessive gambling. It is built on the answers to a set of 9 questions. The other questions in the section are mainly about investment in crypto-currencies. The section on smoking contains a total of 13 questions.

A short version of the Smartphone Addiction Scale (Kwon et al., 2013) constitutes the section on problematic smartphone use. The Smartphone Addiction Scale (SAS) is a standardized tool designed to assess an individual's smartphone addiction level. The scale comprises 10 questions evaluating various indicators of smartphone addiction, such as the frequency of smartphone use, anxiety, discomfort when the phone is not accessible, and the impact of smartphone usage on daily life and relationships.

The French translations of the AUDIT, CDS, CPGI, and SAS are provided by Gaucher et al. (2023).

Finally, the socio-demographic questionnaire captures variables such as gender, age, socio-professional status, revenue, or place of residence. We will add a self-assessed risk aversion question from Dohmen et al. (2011) to the section on socio-demographic characteristics.

Socio-demographic variables

- Gender :
 1. Woman
 2. Men
 3. Non binary / Do not wish to answer

- Age :

- What is your current situation?
 1. You work full-time
 2. You work part-time
 3. You work intermittently
 4. Looking for work (including unemployment)
 5. You are a student
 6. You are retired or pre-retired
 7. You have no professional activity

If several situations, take the most time-consuming activity

- What is the highest diploma you have ever obtained?
 1. No diploma
 2. Primary school certificate
 3. Certificate of professional competence
 4. First Cycle Certificate
 5. Professional Training Certificate

6. Professional Certificate
 7. Technical or vocational bacculaureate
 8. General bacculaureate
 9. Bacculaureate + 2 years
 10. Bacculaureate + 3 years (Bachelor's degree), DEUST, diploma in social or health professions, nursing diploma
 11. Bacculaureate + 4 years (master's degree, master 1)
 12. Bacculaureate + 5 or higher (DEA, DESS, master 2, MBA, doctorate, medicine, pharmacy, dentistry), engineering diploma, grande école, doctorate, etc.
 13. Other
- What is this diploma domain?
 1. Sciences (physics, computer science, biology, biochemistry, etc.)
 2. Mathematics
 3. Human Sciences (languages, history, geography, psychology, sociology, etc.)
 4. Law
 5. Economics
 6. Management Sciences, Economic and Social Administration (AES)
 7. Sports science (STAPS)
 8. Medical sector (pharmacy, medicine, dentistry, etc.)
 9. Arts and entertainment
 10. Other

- In which intervall does the sum of your **net** monthly income fall (including social benefits, scholarships, money received each month from relatives, pensions and other types of benefits)?
 1. Less than 230 euros per month
 2. From 230 to less than 380 euros per month
 3. From 380 to less than 600 euros per month
 4. From 600 to less than 1000 euros per month
 5. From 1,000 to less than 1,200 euros per month
 6. From 1,200 to less than 1,500 euros per month
 7. From 1,500 to less than 2,000 euros per month
 8. From 2000 to less than 2400 euros per month
 9. From 2400 to less than 3000 euros per month
 10. From 3,000 to less than 4,500 euros per month
 11. From 4500 to less than 7600 euros per month
 12. More than 7,600 euros per month

- Overall, would you say that your income is rather variable or rather stable from one month to the next?
 1. Rather stable
 2. Rather variable
 3. Don't know

- Over the next few months, do you think your income will...
 1. rather increase?
 2. rather decrease?

3. remain stable?
 4. don't know?
- Currently, would you say that within your household/family, financially...
 1. are you comfortable?
 2. How are you?
 3. is that right?
 4. do you find it difficult?
 5. you can't do it without going into debt (or using consumer credit)?
 - Has your financial situation changed since last year?
 1. Yes, it has deteriorated
 2. Yes, it has improved
 3. No, she hasn't changed
 4. Don't know
 - What type of commune do you live in?
 1. Rural
 2. Less than 20,000 inhabitants
 3. From 20,000 to 99,999 inhabitants
 4. 100,000 inhabitants and over
 5. Greater Paris

Tobacco

Smoking status *Before assessing tobacco dependence: distinguish between different statuses (smokers; ex-smokers; non-smokers) to avoid administering a dependence assessment questionnaire to a non-smoker.*

A daily smoker is an individual who declares that he or she smokes every day, or declares that he or she consumes a certain number of cigarettes (manufactured or rolled), cigars, cigarillos or chichas per day.

An occasional smoker is an individual who declares that he or she smokes, but not on a daily basis.

The term "smoker" (and by extension "smoking") is used loosely to refer to any individual who smokes, whether daily or occasionally.

An "ex-smoker" is a person who has smoked in the past, either occasionally or daily, and who declares that he or she does not smoke at the time of the survey.

A person who declares having smoked only once or twice to try it is considered never to have smoked.

*The quantities of tobacco smoked were calculated using the following equivalents used in Santé publique France's Barometers: 1 cigar = 1 cigarillo = 2 cigarettes; 1 manufactured cigarette = 1 rolled cigarette; shisha smokers are only occasional smokers (**shisha/narguilé smoker no CDS-12 because no equivalence, like electronic cigarette**).*

- Do you currently smoke, even occasionally?
 1. Yes (pack cigarettes, roll-your-own tobacco, cigars/cigarillos)
 2. Yes, but only chicha/narguilé
 3. Yes, but only electronic cigarettes
 4. No

- [If no] Have you ever tried smoking in your life?
 1. Yes
 2. No

- [If yes] Have you smoked...
 1. just once or twice to give it a try?

2. Occasionally (for any length of time)?
 3. daily for less than six months?
 4. daily for more than six months?
- *[If yes to question 1]* Do you smoke every day (even just one cigarette)?
 1. Yes
 2. No

Tobacco dependence assessment (excluding electronic cigarettes and chicha/narguilé)

[If smoker] At what age did you smoke your first cigarette?

1. before the age of 15?
2. between the ages of 15 and 17?
3. between the ages of 18 and 20?
4. between the ages of 21 and 30?
5. after 30 years?

If [daily smoker] At what age did you start smoking daily?

1. before the age of 15?
2. between the ages of 15 and 17?
3. between the ages of 18 and 20?
4. between the ages of 21 and 30?
5. after 30 years?

Table B.1: The Cigarette Dependence Scale (12-items)

Questions	Response options	Recoding
1. Please rate your addiction to cigarettes on a scale of 0 to 100: - I am NOT addicted to cigarettes at all = 0 - I am extremely addicted to cigarettes = 100	Addiction	0-20 = 1 21-40 = 2 41-60 = 3 61-80 = 4 81-100 = 5
2. On average, how many cigarettes do you smoke per day?	Cigarettes / day	0-5 = 1 6-10 = 2 11-20 = 3 21-29 = 4 30+ = 5
3. Usually, how soon after waking up do you smoke your first cigarette?	Minutes	0-5 = 5 6-15 = 4 16-30 = 3 31-60 = 2 61+ = 1
4. For you, quitting smoking for good would be:	Impossible = 5 Very difficult = 4 Fairly difficult = 3 Fairly easy = 2 Very easy = 1	No recoding
Please indicate whether you agree with each of the following statements:		
5. After a few hours without smoking, I feel an irresistible urge to smoke	Totally disagree = 1 Somewhat disagree = 2 Neither agree nor disagree = 3 Somewhat agree = 4 Fully agree = 5	
6. The idea of not having any cigarettes causes me stress	Totally disagree = 1 Somewhat disagree = 2 Neither agree nor disagree = 3 Somewhat agree = 4 Fully agree = 5	
7. Before going out, I always make sure that I have cigarettes with me	Totally disagree = 1 Somewhat disagree = 2 Neither agree nor disagree = 3 Somewhat agree = 4 Fully agree = 5	
8. I am a prisoner of cigarettes	Totally disagree = 1 Somewhat disagree = 2 Neither agree nor disagree = 3 Somewhat agree = 4 Fully agree = 5	
9. I smoke too much	Totally disagree = 1 Somewhat disagree = 2 Neither agree nor disagree = 3 Somewhat agree = 4 Fully agree = 5	
10. Sometimes I drop everything to go out and buy cigarettes	Totally disagree = 1 Somewhat disagree = 2 Neither agree nor disagree = 3 Somewhat agree = 4 Fully agree = 5	
11. I smoke all the time	Totally disagree = 1 Somewhat disagree = 2 Neither agree nor disagree = 3 Somewhat agree = 4 Fully agree = 5	
12. I smoke despite the risks to my health	Totally disagree = 1 Somewhat disagree = 2 Neither agree nor disagree = 3 Somewhat agree = 4 Fully agree = 5	

Supplementary questions : defining smoking status for electronic cigarettes *Vaping status is defined in terms of four modalities: vapoteur (declares daily or occasional vaping at the time of the survey) / exvapoteur -(has vaped daily for at least a month) / vapoteur expérimentateur (has just tried or -occasional -ex-vapoteur-) / jamais vapoteur.*

Non-smokers include experimental smokers, -daily smokers -and those who have never smoked.

Daily vapoteurs are current vapoteurs who declare that they vapot every day.

Vapo-smokers are people who declare they are vapo-smokers at the time of the -survey and who also smoke (daily or occasionally).

- Have you ever tried an electronic cigarette?

1. Yes

2. No

- Have you ever taken even a whiff?

1. Yes

2. No

- Do you currently use electronic cigarettes?

1. Yes

2. No

- How often do you use electronic cigarettes?

1. Every day

2. At least once a week

3. Less than once a week

- Have you ever used an electronic cigarette daily for at least a month?

1. Yes

2. No

Alcohol

Problem with AUDIT questionnaire no module on binge drinking: requires separate questions

Alcohol dependence assessment

- In the course of your life, have you ever drunk alcoholic beverages, i.e. beer, wine or any other type of alcohol?

1. Yes

2. No

No: no dependency questionnaire

If *yes*, then AUDIT questionnaire: 10 questions

Figure B.8: Definition of standard glass



Table B.2: The AUDIT questionnaire

Questions	Frequency				
	Never	Monthly or less	2-4 times a month	2-3 times a week	4 or more times a week
1. How often do you have a drink containing alcohol?	Never	Monthly or less	2-4 times a month	2-3 times a week	4 or more times a week
2. How many drinks containing alcohol do you have on a typical day when you are drinking ?	1 or 2	3 or 4	5 or 6	7 to 9	10 or more
3. How often do you have six or more drinks on one occasion?	Never	Less than monthly	Monthly	Weekly	Daily or almost daily
4. How often during the last year have you found that you were not able to stop drinking once you had started ?	Never	Less than monthly	Monthly	Weekly	Daily or almost daily
5. How often during the last year have you failed to do what was normally expected of you because of drinking ?	Never	Less than monthly	Monthly	Weekly	Daily or almost daily
6. How often during the last year have you needed a first drink in the morning to get yourself going after a heavy drinking session ?	Never	Less than monthly	Monthly	Weekly	Daily or almost daily
7. How often during the last year have you had a feeling of guilt or remorse after drinking ?	Never	Less than monthly	Monthly	Weekly	Daily or almost daily
8. How often during the last year have you been unable to remember what happened the night before because of your drinking?	Never	Less than monthly	Monthly	Weekly	Daily or almost daily
9. Have you or someone else been injured because of your drinking ?	No	-	Yes, but not in the last year	-	Yes, during the last year
10. Has a relative, friend, doctor, or other health care worker been concerned about your drinking or suggested you cut down ?	No	-	Yes, but not in the last year	-	Yes, during the last year

Binge drinking

- *[If you have ever drunk alcohol]* In the past 12 months, have you ever (even once) consumed 6 or more standard drinks on a day when you drink?

1. Yes
2. No

- *[If yes]* Have you ever been drunk?

1. Yes
2. No

If no, stop.

- At what age was it the first time (even approximately)?
- In the last twelve months, how many times have you been drunk?
- Have you ever consumed large quantities of alcoholic beverages in search of intoxication?

1. Yes
2. No

- *[If yes, previous question]* In the last twelve months, how often have you consumed alcoholic beverages to seek intoxication?
 1. Less than once a month
 2. Once a month
 3. Once a week
 4. Almost every day
 5. It only happened a few times / Very occasionally

- Since what age have you been drinking alcoholic beverages to seek intoxication?

Gambling addiction

- In the last twelve months, have you ever bet on gambling (lotteries, horse/sports betting including e-sport, scratch cards, pokers, casino, etc.), whether at a physical outlet or online (**excluding cryptocurrencies**)?
 1. Yes
 2. No

If no, end of the section. If yes, then dependency questionnaire

Evaluation of gambling addiction Canadian Excessive Gambling Index (CEGI):
9 items; following response modalities (associated score):

- Never (0)

- Sometimes (1)

- Most of the time (2)

- Almost always (3)

Over the last twelve months, in relation to your gambling activities...

1. did you bet more money than you could afford to lose?
2. did you need to stake more and more money to get the same excitement?
3. did you play another day to recover the money you lost playing?
4. have you sold anything or borrowed to get money to play?
5. have you ever felt that you might have a problem with the game?
6. Has gambling caused you any health problems, including stress or anxiety?
7. have people criticized your gambling habits or said you had a problem with gambling?
8. Have your gambling habits caused financial difficulties for you or those around you?
9. Do you feel guilty about your gambling habits or about what happens to you when you gamble?

Questions about investing in cryptocurrencies *The following questions will focus solely on cryptocurrencies.*

- Have you ever invested in cryptocurrencies (even just once)?

1. Yes
2. No

No fine

- *[If yes]* Which cryptocurrency(ies) have you invested in?

1. Bitcoin

2. Ethereum
 3. Altcoins / Other cryptocurrencies
- [If yes] For what reason(s) have you invested in cryptocurrencies?
 1. Long-term investment (to grow your capital while diversifying it)
 2. (Very) short-term investment (speculation, *i.e.* betting on a cryptocurrency in the hope that its value will increase very quickly)
 3. Advice from a third party (family member, influencer, etc.)
 4. To try / Out of curiosity
 5. Because I fundamentally believe in this technology/project
 6. Distrust of the traditional financial system
 7. Other
 - How much have you invested in cryptocurrencies, even approximately (in gross investment, this doesn't include money earned and reinvested afterwards)?

Problematic smartphone use

- Do you personally own or use a smartphone?
 1. Yes
 2. No

If no, then stop.

Assessing problematic smartphone use *Smartphone Addiction Scale - Short Version*

The following response modes (associated score) :

- Strongly disagree (1)
- Disagree (2)
- Somewhat disagree (3)
- Somewhat agree (4)
- Agreed (5)
- Strongly agree (6)

Max score = 60; Cut-offs: 31 for men; 33 for women "excessive smartphone use"
/ "smartphone addiction"

- For each of the following proposals, please tell me whether you strongly agree, agree, somewhat agree, somewhat disagree, disagree or strongly disagree:
 1. Missing planned work due to smartphone use
 2. Having a hard time concentrating in class, while doing assignments, or while working due to smartphone use
 3. Feeling pain in the wrists or at the back of the neck while using a smartphone
 4. Won't be able to stand not having a smartphone
 5. Feeling impatient and fretful when I am not holding
 6. Having my smartphone in my mind even when I am not using it
 7. I will never give up using my smartphone even when my daily life is already greatly affected by it
 8. Constantly checking my smartphone so as not to miss conversations between other people on Twitter or Facebook
 9. Using my smartphone longer than I had intended
 10. The people around me tell me that I use my smartphone too much.

B.1.4 Simulated Dataset

To test our statistical code, we propose simulated datasets. They are in CSV format and are available on <https://osf.io/mq2fc>. We have also deposited the statistical codes for the transformation and analysis of the data (using STATA software).

Participant	Session	ba111	ba112	...
1	1	0	1	...
2	2	1	0	...
...

Table B.3: Simulated dataset (Method B)

Participant	<i>AAMM</i>	<i>Index</i>	<i>Consistent_A</i>	<i>RiskBret</i>	<i>AuditScore</i>	<i>CDSScore</i>	Male	Age	...
1	A	0.99	1	0.2	11	21	0	22	...
2	A	0.64	0	0.4	10	31	1	21	...
...

Table B.4: Simulated final dataset (merged)

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