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A behavioral approach of decision making under risk and uncertainty

Thomas Garcia

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Thomas GARCIA

**A behavioral approach of decision making
under risk and uncertainty.**

Devant le jury composé de :

Brice CORGNET, Professeur, EM Lyon Business School, Président

Jean-Christophe VERGNAUD, Directeur de recherches, Sorbonne Université, Rapporteur

Aurélien BAILLON, Professeur, Erasmus School of Economics, Rapporteur

Dulleck UWE, Professeur, QUT, Examineur

Marie-Claire VILLEVAL, Directrice de Recherche, C.N.R.S., Directeur de thèse

Sébastien MASSONI, Docteur, QUT, Co-Directeur de thèse

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QUEENSLAND UNIVERSITY OF TECHNOLOGY



GATE - UNIVERSITY LYON 2

PHD THESIS - DOCTOR OF PHILOSOPHY (ECONOMICS)

CANDIDATE: THOMAS GARCIA

A behavioral approach of decision making under risk and uncertainty

Supervisors:

DR SÉBASTIEN MASSONI, QUT

PROFESSOR MARIE CLAIRE VILLEVAL, GATE

Committee:

PROFESSOR AURÉLIEN BAILLON, ERASMUS UNIVERSITY — REVIEWER

PROFESSOR BRICE CORGNET, EM LYON & GATE

PROFESSOR UWE DULLECK, QUT

PROFESSOR JEAN-CHRISTOPHE VERGNAUD, PARIS 1 — REVIEWER

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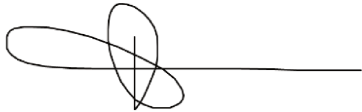
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Statement of Original Authorship

The work contained in this joint thesis undertaken between QUT and Université Lumière Lyon 2 has not been previously submitted to meet requirements for an award at these or any other higher education institution. To the best of my knowledge and belief, the thesis contains no material previously published or written by another person except where due reference is made.

01/12/2018

A handwritten signature consisting of a stylized, cursive letter 'L' followed by a horizontal line extending to the right.

Abstract

This thesis investigates how individuals make decisions under risk and uncertainty. It is composed of four essays that theoretically and experimentally investigate decision-making.

The first two essays study situations where a decision maker has to decide whether an event has occurred using uncertain evidence. Accurately identifying that this event has occurred is more rewarded than accurately identifying that it has not occurred. This decision problem induces a divergence between two qualities of a decision: optimality and accuracy. Both essays reproduce such situations in a laboratory experiment based on perceptual tasks and analyze behavior using Signal Detection Theory to study the optimality-accuracy trade-off. The first essay confirms the existence of the trade-off with a leading role of accuracy. It explains the trade-off by the concern of individuals for being right. The second chapter finds that presenting perceptual evidence last contributes to the existence of the optimality-accuracy trade-off.

The third essay studies how other-regarding preferences interact with attitude toward ambiguity. It reports the results of an experiment where subjects have to make donations to charities. Donations may have either ambiguous costs or ambiguous benefits. We find that other-regarding preferences are decreased under ambiguity. In other terms, we highlight that individual use ambiguity has an excuse not to give. This excuse-driven behavior is stronger for ambiguous costs than ambiguous benefits.

The fourth essay challenges the external validity of laboratory risk preference measures using behavior in experimental risk tasks and naturally occurring behavior under risk. We find that risk preference measures are related with the former but that they fail to explain the latter.

Keywords: signal detection theory; optimality-accuracy trade-off; ambiguity; risk preferences; excuse-driven behavior; elicitation methods; laboratory experiment; lab-in-the-field experiment.

Résumé détaillé en français¹

Il n'est pas certain que tout soit
incertain

Blaise Pascal, *Pensée*

L'incertitude est un élément incontournable de l'existence humaine. Nous ne savons pas de quoi notre avenir est fait, mais nous devons cependant nous y confronter. Chaque fois que nous choisissons un chemin pour aller travailler, que nous décidons d'aller à un deuxième rendez-vous ou que nous mettons de l'argent de côté pour les mauvais jours, nous avons affaire à des conséquences incertaines. Prenons pour exemple la situation suivante : un médecin doit choisir un traitement pour un patient. Il établit un diagnostic de l'état du patient. Le diagnostic ne peut pas déterminer avec certitude la maladie du patient. Il n'est donc pas certain de l'état de santé du patient. Cependant, il connaît l'état du patient après le traitement en fonction de son état de santé réel. Par exemple, si le patient souffre d'une maladie et que le médecin décide de ne pas le traiter, le patient ne pourra pas se rétablir et la maladie pourrait s'aggraver. Dans la situation décrite, la décision du médecin est motivée par son attitude face au risque ou à l'incertitude : comment il pondérera les différents états de santé post-traitement en fonction de leurs probabilités respectives. Cette attitude à l'égard du risque ou de l'incertitude peut être guidée par certaines normes professionnelles ou par des opinions personnelles. Mon objectif est d'étudier comment les individus se comportent dans ce cadre décisionnel risqué ou incertain.

Une caractéristique clé des décisions ayant des conséquences aléatoires est de caractériser ce que l'on sait de l'inconnu. Le décideur peut connaître la loi régissant le caractère aléatoire. En d'autres termes, il peut connaître la distribution probabiliste

¹This chapter is an extensive summary of this thesis written in French as required by Lyon 2.

des résultats possibles. Dans ce cas, nous définissons le caractère aléatoire comme un “risque”. Il est aussi possible que même la distribution des probabilités soit inconnue du décideur, c’est-à-dire qu’il n’y a pas de probabilités objectives pour guider la prise de décision. Dans ce cas, nous définissons le caractère aléatoire comme “incertain” (Knight, 1921).

Tous les chapitres de cette thèse sont consacrés à l’étude du comportement humain sous risque et incertitude. Ils sont tous basés sur l’analyse de comportements individuels. Ce comportement a été observé soit lors d’expériences en laboratoire, soit sur le terrain.

Le laboratoire est un outil important pour étudier la prise de décision car il permet un contrôle strict des environnements décisionnels. En effet, l’expérimentateur a un contrôle total sur l’information reçue par les sujets, et peut manipuler cette information pour étudier les changements de comportement. Les expériences de terrain diminuent le contrôle sur l’environnement expérimental mais augmentent la validité externe des résultats car les décisions ne sont pas prises dans un environnement artificiel. Les expériences en laboratoire et sur le terrain sont donc deux outils importants et complémentaires pour étudier le comportement humain.

En ce qui concerne les décisions à risque, les expériences en laboratoire fournissent aux chercheurs une information parfaite sur les conséquences et les probabilités associées qui sont prises en compte par un individu au moment de sa décision. De plus, les chercheurs peuvent choisir librement les conséquences et les probabilités qui sont les plus pertinents pour étudier une problématique. En comparaison, il est plus difficile de connaître les conséquences et les probabilités dans les décisions de la vie réelle et encore plus difficile de les manipuler. Les expériences en laboratoire sont donc bien adaptées à l’évaluation des théories sur la façon dont les individus prennent des décisions risquées. En particulier, ils permettent aux chercheurs d’estimer les paramètres de décision déterminant les prédictions théoriques. Je présente dans la partie “décision sous risque” les principales théories de décision en environnement risqué et les procédures expérimentales pour mesurer leurs paramètres respectifs.

Bien que la plupart des tâches expérimentales utilisées en risque soient basées sur des loteries où toutes les conséquences possibles ainsi que leurs probabilités associées sont connus, les types de tâches utilisées pour générer de l’incertitude sont nombreux. En effet, fournir des informations sur une situation aléatoire sans donner d’informations sur les probabilités donne beaucoup plus de latitude à l’expérimentateur. Une possibilité est

de fournir aux sujets un intervalle de probabilités. Ce type d'incertitude est appelé "ambiguïté". Une autre possibilité est d'induire de l'incertitude en utilisant des informations perceptuelles. Je présente dans la partie "décision sous incertitude", d'abord les principales conclusions sur les différences comportementales entre le risque et l'ambiguïté, puis différentes tâches perceptuelles utilisées pour étudier les décisions sous incertitude et les cadres théoriques sous-jacents utilisés pour analyser ces décisions. Enfin, la dernière partie donne les objectifs et les grandes lignes de cette thèse.

Décision sous risque

Deux cadres théoriques dominants sont largement utilisés pour modéliser les décisions à risque : la théorie de l'utilité espérée et la "Cumulative Prospect Theory".

La théorie de l'utilité espérée (ci-après EUT) suppose que la valeur d'une loterie risquée est donnée par la somme de l'utilité de chacune de ses conséquences, pondérée par leur probabilité correspondante. L'évaluation des conséquences selon l'utilité des gains au lieu des gains eux-mêmes suit le travail de von Neumann and Morgenstern (1945). Formellement, considérez la loterie P , de telle sorte que :

$$P = (x_1, p_1; \dots; x_n, p_n) \tag{1}$$

avec (x_1, \dots, x_n) l'ensemble des conséquences possibles et (p_1, \dots, p_n) leurs probabilités associées. L'utilité espérée est donnée par :

$$V_{EUT}(P) = \sum_{i=1}^n p_i \times U(x_i) \tag{2}$$

avec U défini comme une fonction d'utilité de von Neumann-Morgenstern. On suppose généralement que ces fonctions sont concaves.

Une autre théorie est celle de la "cumulative prospect theory" (Tversky and Kahneman, 1992, ci-après CPT). La CPT diffère de l'EUT sur trois points. Premièrement, l'utilité des conséquences possibles n'est pas pondérée par leurs probabilités correspondantes, mais par une fonction de ces probabilités. Elle permet la surpondération des petites probabilités et la sous-pondération des grandes probabilités. Deuxièmement, la fonction de pondération des probabilités n'est pas appliquée aux probabilités d'événements isolés, mais à leur fonction de répartition. Elle permet de ne surpondérer que les

conséquences à la fois extrêmes et improbables, plutôt que toutes les conséquences improbables. Enfin, la fonction de pondération probabiliste est différente pour les gains et les pertes (définis selon un point de référence endogène). Elle permet une pondération différente d'un gain ou d'une perte équivalente.

Formellement, considérons la loterie $P = (x_{-m}, p_{-m}; \dots; x_n, p_n)$ telle que les conséquences possibles soient classés par ordre croissant (c.-à-d. $x_{-m} \leq x_{-m+1} \leq \dots \leq x_n$) et $U(x_0) = 0$. L'utilité espérée sous CPT de cette loterie peut être écrite :

$$V_{CPT}(P) = \sum_{i=1}^n \pi_i^+ \times U(x_i) + \sum_{i=-m}^0 \pi_i^- \times U(x_i) \quad (3)$$

où :

$$\begin{cases} \pi_n^+ = w^+(p_n); \pi_{-m}^- = w^-(p_{-m}) \\ \pi_i^+ = w^+(p_i + \dots + p_n) - w^+(p_{i+1} + \dots + p_n), 0 \leq i \leq n-1 \\ \pi_i^- = w^-(p_{-m} + \dots + p_i) - w^-(p_{-m} + \dots + p_{i-1}), 1-m \leq i \leq 0 \end{cases} \quad (4)$$

où w^+ et w^- sont des fonctions strictement croissantes de l'intervalle unité dans lui-même.

Bien que ces théories fournissent un cadre conceptuel pour l'étude de la prise de décision en domaine risqué, il faut définir une forme fonctionnelle pour la fonction d'utilité (et la fonction de pondération probabiliste pour la CPT) afin de représenter le comportement. La famille de fonctions d'utilité la plus utilisée en économie expérimentale est celle des fonctions CRRA (constant relative risque aversion, e.g. Wakker, 2008). Un exemple simple est le choix d'un portefeuille qui peut contenir un actif risqué et un actif sûr. S'il connaît une augmentation de son patrimoine, une personne ayant des préférences de type CRRA choisira de maintenir inchangée la part détenue dans l'actif risqué. Formellement, la fonction d'utilité CRRA pour une loterie ne contenant que des gains peut être écrite comme suit :

$$\forall x \in \mathbb{R}^+, U(x) = \begin{cases} \frac{x^{(1-\gamma)}}{1-\gamma} & \text{if } \gamma \neq 1 \\ \ln(x) & \text{if } \gamma = 1 \end{cases} \quad (5)$$

avec x le gain et γ le coefficient d'aversion au risque. Les individus sont caractérisés comme "averses au risque" lorsque le paramètre d'aversion au risque est positif (fonction d'utilité concave). Ils sont caractérisés comme "amateurs du risque" si leur paramètre d'aversion au risque est négatif (fonction d'utilité convexe). Sinon, ils sont considéré

comme “neutres au risque”.

Pour la fonction de pondération probabiliste, la fonction la plus utilisée a été introduite par Prelec (1998). Elle est donnée par :

$$\pi(p) = \exp[-(-\ln p)^\alpha] \tag{6}$$

avec p la probabilité et α le paramètre de sensibilité à la probabilité.

L’un des objectifs de l’économie expérimentale est de mettre au point des procédures permettant d’estimer les paramètres fixés par un cadre théorique. De nombreuses procédures ont été développées. Elles varient en fonction de la complexité des choix possibles. Je décris ci-après quelques-unes des procédures les plus influentes.

Une des premières procédures a été introduite par Binswanger (1980). Les sujets doivent choisir entre sept loteries. Les six premières loteries ont une espérance et une variance croissante, i.e. une valeur espérée plus élevée mais plus de risque. La dernière loterie a la même espérance mais une variance supérieure à l’avant dernière, i.e. la même valeur prévue et plus de risque. Une procédure similaire a été développée par Eckel and Grossman (2008). Les deux procédures diffèrent concernant la relation entre le gain espéré et la variance (convexe pour Binswanger, 1980, et linéaire pour Eckel and Grossman, 2008). Gneezy and Potters (1997) ont aussi conçu une procédure similaire basée sur des choix augmentant en espérance et en variance, mais pour un ensemble de décision beaucoup plus étendu et sans l’utilisation de loteries. Dans ces procédures, les sujets doivent choisir comment investir dans un actif risqué en payant k fois le montant investi avec probabilité p (et le perdre autrement). Le montant qui n’est pas investi est conservé par le sujet. p et k sont choisis de sorte que $p \times k > 1$. Investir est donc en espérance plus lucratif que de ne pas investir. Une autre approche consiste à utiliser des listes de prix. Cette approche a été introduite par Holt and Laury (2002). Cette procédure est devenue courante dans les études d’économie expérimentale (Charness et al., 2013). Les sujets doivent faire 10 choix binaires entre deux loteries A et B. Les loteries A et B ont deux gains possibles. Les gains sont classés de telle sorte que l’un des gains de la loterie A est inférieur aux deux gains de la loterie B tandis que l’autre gain de la loterie A est supérieur aux deux gains de la loterie B. Les gains des loteries A et B sont constants, mais la probabilité associée à chaque gain change. Les individus choisissent, a priori, la loterie A pour les premières décisions puis ils choisissent loterie B à un moment donné. Tanaka et al. (2010) a également développé une méthode basée sur

des listes de prix comprenant trois listes différentes. Contrairement à Holt and Laury (2002), les probabilités associées aux résultats des loteries binaires A et B sont fixes mais les conséquences possibles des loteries changent à l'intérieur de chaque liste. Le fait d'avoir trois listes de prix permet d'estimer trois paramètres au lieu d'un seul.

D'autres procédures reposent sur des tâches plus visuelles imitant des situations réelles. Dans la procédure introduite par Lejuez et al. (2002), les sujets doivent gonfler un ballon qui peut exploser chaque fois qu'il est gonflé. La probabilité d'explosion augmente avec chaque fois que le ballon est gonflé. Les sujets gagnent de l'argent chaque fois qu'ils gonflent le ballon, mais perdent tous leurs gains si le ballon explose. La tâche de Crosetto and Filippin (2013) suit un principe similaire. Les sujets doivent collecter successivement des boîtes, dont l'une contient une bombe. Les gains augmentent linéairement avec le nombre de boîtes accumulées, mais sont nuls si la bombe est également collectée.

Enfin, certaines procédures non-incitées (basées sur les déclarations des sujets) ont aussi émergées. Ces méthodes sont utiles car elles peuvent être incluses dans une enquête, mais elles ne permettent pas d'estimer des paramètres. La plus influente est celle de Dohmen et al. (2005) qui consiste en une échelle de Likert sur la tolérance aux risques. Il a été constaté que cette mesure était corrélée à des mesures incitées obtenues en laboratoire (Dohmen et al., 2011).

A l'exception de la procédure de Tanaka et al. (2010), toutes ces procédures ont en commun que les décisions sont conçues de telle sorte que le coefficient d'aversion au risque de la EUT puisse être estimé une fois qu'une forme paramétrique de la fonction d'utilité est supposée (ou plus précisément une valeur minimale et maximale de ce paramètre). Une autre approche pour estimer le paramètre de la EUT consiste à estimer le coefficient d'aversion au risque au moyen d'enchères (Cox et al., 1982). D'un point de vue théorique, l'estimation de l'aversion au risque dans le cadre de la EUT est assez simple car il suffit d'estimer un seul paramètre. Par contre, l'estimation des paramètres de la CPT est plus complexe puisqu'elle repose sur trois paramètres (courbure de la fonction d'utilité, sensibilité à la probabilité et aversion aux pertes). La procédure de Tanaka et al. (2010) est conçu de manière à pouvoir estimer ces trois coefficients.

Pour conclure sur les mesures des préférences de risque, il convient de noter que, si la plupart des estimations sont paramétriques, certaines procédures diffèrent car elles permettent des estimations non paramétriques. Par exemple, la méthode du "trade-off" introduite par Wakker and Deneffe (1996) recherche l'indifférence entre des paris

avec deux conséquences possibles. Cette méthode a été utilisée par Wakker and Deneffe (1996) pour obtenir des préférences face au risque dans le cadre de l'EUT. Abdellaoui (2000) ajoute une deuxième étape après la méthode du "trade-off" pour obtenir aussi une séquence de probabilités également espacées en termes de pondération des probabilités et ainsi estimer des préférences face au risque dans le cadre de la CPT.

Les économistes disposent ainsi d'un grand nombre de procédures pour estimer l'aversion au risque. Dans un monde idéal, l'outil (ici la procédure) utilisé pour mesurer un phénomène (ici l'aversion au risque) ne devrait pas affecter la mesure observée. Toutefois, des études comparant les mesures obtenues avec les différentes procédures concluent systématiquement que l'aversion au risque mesurée dépend de la méthode d'estimation (voir He et al., 2018 pour une revue des principaux résultats). Cela souligne l'importance du choix de la procédure de risque.

Un aspect fondamental des expériences de laboratoire est leur généralisabilité, c'est-à-dire que les connaissances acquises en laboratoire peuvent être extrapolées au monde extérieur (Levitt and List, 2007). Les déterminants estimés des décisions risquées en laboratoire devraient donc également guider, au moins dans une certaine mesure, les décisions à l'extérieur du laboratoire. En suivant ce constat, plusieurs des procédures décrites précédemment se sont appuyées sur leur capacité à expliquer le comportement sur le terrain pour justifier leur bien-fondé (par exemple, Lejuez et al., 2002; Dohmen et al., 2005 et Tanaka et al., 2010). Cependant, aucune étude n'a encore mené une analyse systématique de la validité externe des procédures d'estimation de l'attitude face au risque. Le chapitre 5² a pour objectif d'analyser la validité externe de cinq des procédures d'estimation de l'attitude face au risque les plus influentes. C'est procédure sont: Gneezy and Potters (1997), Holt and Laury (2002), Dohmen et al. (2005), Eckel and Grossman (2008) et Tanaka et al. (2010). Leur validité externe est évaluée en fonction de deux ensembles de comportements : les décisions risquées en laboratoire et les décisions risquée dans la vie de tous les jours.

De plus, le chapitre 2 utilise la mesure d'estimation de l'attitude face au risque de Eckel and Grossman (2008) pour estimer la courbure de la fonction d'utilité des sujets. Il nous permet de définir des stratégies de décision optimales dans le cadre de la EUT.

²Le chapitre 5 est le premier à être mentionné dans ce résumé. J'ai choisi de classer les différents sous-chapitres par ordre chronologique afin de rendre compte de l'évolution de mes recherches.

Décision sous incertitude

Dans de nombreuses décisions ayant des conséquences aléatoires, la personne prenant la décision ne dispose que d'informations vagues sur les probabilités associées aux différentes conséquences. Pour toutes ces situations, le fait de présenter la décision comme une décision à risque ne rend pas compte de la complexité de la situation. La situation doit donc être représentée comme une décision sous l'incertitude où les sujets ne connaissent pas les probabilités des possibles conséquences. D'un point de vue expérimental, la mise en œuvre de décisions incertaines est associée à un défi plus important que pour les décisions à risque. En effet, le chercheur doit fournir de l'information probabiliste suffisamment complexe pour être vague mais pas trop complexe pour permettre de garder le contrôle sur l'information considérée par le décideur. Deux solutions sont mises en œuvre dans cette thèse. Le type d'incertitude le plus simple utilisé est appelé "ambiguïté". L'ambiguïté consiste à fournir aux sujets un intervalle de probabilité contenant la probabilité effective. Une façon plus complexe d'induire l'incertitude repose sur l'utilisation d'informations perceptuelles. L'objectif est de donner des informations perceptuelles qui fournissent des renseignements sur les probabilités de chaque conséquence. Cependant, ces informations doivent être suffisamment bruitées pour ne pas révéler totalement les probabilités. Je fais une revue des principaux résultats empiriques sur la différence entre risque et ambiguïté dans la partie "décision sous ambiguïté". Je décris les méthodes expérimentales et le cadre théorique associé qui sont utilisés dans cette thèse pour étudier la prise de décision sous incertitude perceptuelle dans la partie "décision sous incertitude perceptuelle".

Décision sous ambiguïté

Un point de départ naturel concernant la différence entre les décisions sous risque et sous ambiguïté est le travail d'Ellsberg qui développe le "paradoxe d'Ellsberg" (Ellsberg, 1961). Dans sa version plus simple, le décideur fait face à deux urnes de 100 billes dans chaque urne. La première urne A contient 50 boules rouges et 50 boules noires. La deuxième urne B contient des boules rouges et noires mais le nombre de boules de chaque couleur est inconnu. Les décideurs prennent deux décisions consécutives. Pour la première décision, le décideur gagne de l'argent si une boule rouge est tirée. Pour la deuxième décision, le décideur gagne de l'argent si une boule noire est tirée. Becker and

Brownson (1964) trouvent que la plupart des individus préfèrent miser sur l'urne A pour les deux décisions. C'est un paradoxe, car ils agissent comme si : (i) il est plus probable de tirer une boule dans l'urne A que dans l'urne B, (ii) il est également plus probable de tirer une boule noire dans l'urne A que dans l'urne B. Ces personnes sont appelées "averse à l'ambiguïté" puisqu'elles préfèrent choisir une alternative où la distribution de probabilité des conséquences est connue plutôt que de choisir une alternative où les probabilités sont inconnues. Oechssler and Roomets (2015) passe en revue 39 articles expérimentaux mettant en œuvre le problème de l'urne d'Ellsberg publiés entre 1964 et 2015 (dont celui de Becker and Brownson, 1964). Le pourcentage de sujets averses à l'ambiguïté est en moyenne de 56,6%, mais il y a beaucoup d'hétérogénéité entre les études (pourcentage variant de 8% à 93%). Cela semble confirmer que la plupart des individus sont averses à l'ambiguïté.

Cependant, l'étude de la différence entre le risque et l'ambiguïté basée sur des choix entre les urnes d'Ellsberg ne donne pas une vue d'ensemble de la question. En effet, dans de telles décisions, les options risquées et ambiguës sont considérées conjointement. Par opposition, Fox and Tversky (1995) étudie les attitudes à ambiguës dans les situations où le décideur n'évalue les décisions risquées et ambiguës que de manière isolée. Ils constatent que l'aversion à l'ambiguïté disparaît dans ce contexte. Ils concluent que l'aversion à l'ambiguïté observée avec les urnes d'Ellsberg est principalement due au contraste entre les deux urnes. Binmore et al. (2012) trouve un résultat comparable en utilisant la méthode de "matching probability" (Kahn and Sarin, 1988). Cette méthode consiste à obtenir la probabilité de gain de l'urne risquée de telle sorte que le décideur soit indifférent entre parier sur l'urne risquée ou l'urne ambiguë. Ils ne trouvent pas de preuve d'aversion à l'ambiguïté, mais ils constatent seulement que les décisions sont influencées par un certain niveau de bruit qui est modestement biaisé dans le sens de l'aversion à l'ambiguïté.

Des préférences hétérogènes ou des attitudes à ambiguïté non uniforme peuvent également expliquer l'hétérogénéité des résultats sur l'aversion à l'ambiguïté. Stahl (2014) trouve une grande hétérogénéité dans les attitudes à l'ambiguïté. Bien que le sujet représentatif de son étude soit averses à l'ambiguïté, seulement 12% des sujets sont en fait classés comme tels. Baillon and Bleichrodt (2015) étudie l'aversion à l'ambiguïté pour les gains et les pertes qui sont très probables ou peu probables. Ils mettent en évidence un modèle d'attitude ambiguë à quatre facettes: l'aversion à l'ambiguïté pour

des gains très probables et des pertes peu probables et la recherche de l’ambiguïté pour des gains peu probables et des pertes très probables.

Une question connexe est la relation entre le niveau d’ambiguïté (différence entre les probabilités les plus élevées et les plus faibles dans l’intervalle de probabilité) et l’aversion à l’ambiguïté. Becker and Brownson (1964) évalue cette question en mettant en œuvre 10 choix entre deux urnes avec différents niveaux d’ambiguïté et en incitant les sujets à parier sur leur urne préférée pour chacun des choix (les “primes d’ambiguïté”). Ils constatent que les primes d’ambiguïté diminuent avec le niveau d’ambiguïté. Ce résultat a été reproduit, par exemple, par MacCrimmon and Wehrung (1990); Curley and Yates (1989) et Kahn and Sarin (1988).

Les découvertes sur l’attitude face à l’ambiguïté sont donc globalement en faveur de l’aversion à l’ambiguïté même si la robustesse de l’aversion à l’ambiguïté à travers les contextes est faible (voir Trautmann and Van De Kuilen, 2015 pour un examen plus détaillé des travaux expérimentaux sur l’attitude à l’ambiguïté). Dans tous les contextes décrits précédemment, les personnes évaluent le risque et l’ambiguïté avec des conséquences uniquement pour elles-mêmes. Il n’y a donc pas de place pour une évaluation stratégique de l’ambiguïté. Si les individus peuvent utiliser le risque et l’ambiguïté de manière stratégique, par exemple pour justifier leurs comportements injustes, la différence de comportement en risque et en ambiguïté peut être renforcée. Le chapitre 4 étudie comment l’attitude à l’ambiguïté et l’attitude au risque diffèrent dans un contexte où le risque et l’ambiguïté peuvent servir d’excuse pour ne pas donner (comportement d’auto-justification). L’expérience n’implique pas la prise en compte conjointe des loteries sous risque et ambiguïté pour avoir une évaluation conservatrice de l’aversion à l’ambiguïté. En outre, différents niveaux d’ambiguïté sont mis en œuvre pour vérifier si le niveau d’ambiguïté a une incidence sur les comportements d’auto-justification. Elle contribue ainsi à mieux comprendre la différence entre la prise de décision sous risque et sous ambiguïté.

Décision sous l’incertitude perceptuelle

Il est intéressant de fournir aux personnes certaines probabilités (connues pour leur risque ou vagues pour leur ambiguïté), car cela donne au chercheur beaucoup de contrôle sur l’information disponible au moment de la décision. Cependant, ce contrôle se fait au prix de situations assez artificielles qui peuvent empêcher l’observation de certains

déterminants des décisions naturelles. L'étude des décisions dans des tâches qui fournissent des informations de manière plus naturelle est donc également importante pour avoir une approche globale des décisions dans un environnement aléatoire.

Certaines études utilisent des sources naturelles d'incertitude pour générer l'aléatoire (e.g. Heath and Tversky, 1991; de Lara Resende and Wu, 2010; Abdellaoui et al., 2011 et Chew et al., 2012). Ces sources naturelles sont souvent basées soit sur des événements climatiques, comme les températures, soit par l'évolution des marchés boursiers. Ils obtiennent que les individus sont moins réticents à l'incertitude lorsqu'ils se sentent familiers avec la source d'incertitude. Ce résultat met en évidence l'importance de la source d'incertitude.

Dans cette thèse, j'utilise des tâches perceptuelles pour induire l'incertitude et étudier différents aspects de la décision sous incertitude. Par opposition aux loteries, par exemple, les tâches perceptuelles reposent sur la limitation des capacités cognitives plutôt que sur le caractère aléatoire artificiel de la situation. Dans les situations mises en œuvre, le décideur doit identifier si un signal est présent ou absent. Le décideur observe si le signal est présent sur la base de certaines informations perceptuelles. Cependant, l'information n'est disponible que pour une courte période de temps, ce qui rend la présence du signal incertaine pour l'observateur (tout en étant certaine pour l'expérimentateur, d'où l'incertitude impliquée par les limitations cognitives). Il y a quatre cas possibles : deux types de succès et deux types d'erreurs. Le décideur signale que le signal est présent soit correctement ("détection correct") ou incorrectement ("fausse alarme"), il signale que le signal est absent correctement ("rejet correct") ou incorrectement ("omission"). Sa décision est donc un compromis entre ne pas manquer trop de signaux et ne pas interpréter trop de signaux absents comme présents.

Bien que les décisions fondées sur des informations perceptuelles soient moins artificielles que les décisions fondées sur les loteries, il est plus difficile pour le chercheur de connaître l'information considérée. Le recours à des modèles théoriques est donc nécessaire pour fournir un cadre analytique. L'un de ces cadres est la Théorie de la Détection du Signal (Green and Swets, 1966, SDT ci-après).³ Elle permet aux chercheurs de distinguer ce qui provient de la difficulté de la tâche — difficulté de distinguer entre

³Un autre cadre théorique repose sur les "Drift Diffusion Models" (Ratcliff, 1978, ci-après DDM). Il suppose que les preuves sont accumulées à la suite d'un processus de marche aléatoire ou plus généralement d'un mouvement brownien (Kramers, 1940), lorsque le niveau de preuve dépasse un certain seuil, cela déclenche la décision. Le temps de réaction est donc un aspect central des DDM. Ce cadre théorique sort du cadre d'intérêt de cette thèse.

la présence et l'absence du signal — et ce qui provient de la stratégie de décision — niveau de preuve minimal en faveur de la présence du signal nécessaire pour répondre signal. Dans sa forme la plus simple, la quantité de preuves reçues lorsque le signal est présent ou lorsque le signal est absent est supposée suivre des Gaussiennes dont les moyennes diffèrent, mais pas la variance. Le SDT indique que la décision est entièrement déterminée par l'application d'un critère de décision simple à la quantité de preuve, c'est-à-dire que si la quantité de preuves dépasse un seuil, l'observateur reporte que le signal est présent, sinon il reporte que le signal est absent.

Le SDT a un impact énorme sur de nombreuses branches de la psychologie comme l'interprétation des processus sensoriels, la recherche sur la mémoire ou le traitement des informations verbales (McNicol, 2005). Toutefois, peu d'études ont utilisé la SDT dans la littérature économique (par exemple Barkan et al., 1998 et Barkan, 2002 pour des applications aux problèmes de sécurité). Je pense que la SDT peut fournir un cadre approprié pour étudier de nombreuses situations d'intérêt général pour les économistes. En effet, les situations économiques, y compris les erreurs de type I et de type II (ou succès), sont nombreuses : par exemple, les décisions de négociation sur un marché volatil, les délibérations du jury sur la culpabilité d'un prévenu et les décisions médicales (voir exemple en introduction). Les chapitres 2 et 3 utilisent la SDT appliquée aux situations économiques pour mettre en évidence l'existence d'un "arbitrage entre l'optimalité et l'exactitude". La tension entre l'optimalité et l'exactitude est induite par la récompense asymétrique du succès lorsque le signal est présent et lorsque le signal est absent (les deux types d'erreurs étant récompensés également). Les sujets font donc face à un compromis entre essayer de maximiser leurs gains et essayer de donner le plus grand nombre de bonnes réponses. Elle nous permet d'évaluer si les individus tirent une utilité non monétaire du fait d'avoir raison. Cela nous permet également de nous interroger sur l'impact de l'ordre des informations sur de telles décisions.

Objectif et grandes lignes de cette thèse

Les deux premiers essais utilisent les tâches perceptuelles comme outil et la SDT comme modèle pour mettre en évidence l'existence d'un arbitrage entre l'optimalité et l'exactitude dans les décisions prises en incertitude et discuter de son origine. Bien qu'ils partagent une méthodologie et un sujet d'intérêt communs, ils diffèrent quant à leur public cible. Le premier chapitre se situe à la frontière entre l'économie et la psychologie car il vise à

introduire de nouvelles méthodes de la psychologie computationnelle en économie pour répondre à des questions économiques. Il s'adresse donc aux économistes. Le deuxième chapitre franchit cette frontière. Il prolonge les études antérieures en psychologie computationnelle et s'adresse donc aux psychologues. Ce chapitre diffère donc par le style de rédaction qui correspond aux normes de la littérature en psychologie. Les deux derniers chapitres se distinguent des deux premiers par le fait qu'ils s'appuient sur des tâches économiques plus standard que des tâches perceptives. Le troisième chapitre étudie la différence entre risque et ambiguïté en ciblant un aspect particulier de la décision : le comportement d'auto-justification. Enfin, le dernier chapitre combine des données de terrain et de laboratoire pour étudier si les méthodes d'évaluation de l'attitude face au risque sont capables d'expliquer le comportement risqué à l'intérieur et à l'extérieur du laboratoire.

Pour plus de détails, le chapitre 2 présente l'essai *Aiming to choose correctly or to choose wisely? The optimality-accuracy trade-off in decisions under uncertainty*, co-écrit avec Sébastien Massoni. Dans cet essai, nous étudions des situations où les individus doivent décider si un événement s'est produit en utilisant des preuves incertaines. Le fait de correctement déterminer que cet événement s'est produit est plus rémunéré que le fait de correctement déterminer qu'il ne s'est pas produit. Nous pouvons identifier deux stratégies décisionnelles. Premièrement, maximiser la récompense espérée de la décision que nous appelons la "stratégie optimale". Deuxièmement, maximiser la probabilité de correctement classifier la situation : la stratégie de "maximisation de l'exactitude". Viser l'optimalité réduira l'exactitude, tandis que viser l'exactitude diminuera le bénéfice espéré. Le décideur est donc confronté à un arbitrage entre optimalité et exactitude. Notre étude est composée de deux expériences en laboratoire ayant deux objectifs complémentaires. La première expérience permet d'évaluer si l'existence de l'arbitrage optimalité-exactitude est validée empiriquement et de tester sa robustesse dans différents contextes. La deuxième expérience permet d'évaluer dans quelle mesure l'arbitrage entre l'optimalité et l'exactitude peut s'expliquer par l'utilité dérivée du fait d'avoir raison. Pour bien analyser cet arbitrage, il faut mettre en œuvre les tâches expérimentales suffisamment complexes pour ne pas perdre l'essence même de la situation étudiée tout en étant en mesure d'identifier la décision des sujets et de la comparer aux prévisions théoriques. En réponse, nous avons fondé notre approche sur une tâche perceptuelle et un modèle théorique utilisé en sciences cognitives : La théorie de la détection

du signal. Etablir de liens entre les questions économiques et des modèles extérieurs à la littérature économique afin de répondre à des questions pertinentes pour les économistes est la principale contribution de ce chapitre. Les résultats de la première expérience confirment l'existence de l'arbitrage avec un rôle majeur de l'exactitude. Les résultats de la deuxième expérience expliquent l'arbitrage par le souci des individus d'avoir raison. Le chapitre 3 présente l'essai *Manipulating payoff processing order in perceptual decision-making* co-écrit avec Ismaël Rafai et Sébastien Massoni. Le chapitre 3 s'appuie sur les conclusions du chapitre 2 pour étudier une nouvelle explication potentielle de l'arbitrage optimalité-exactitude basée sur un biais de récence. Une façon d'interpréter le biais d'optimalité-exactitude consiste à conclure que les individus se fient trop aux preuves perceptuelles — qui leur donnent de l'information sur la bonne réponse — et pas assez sur la matrice des gains — qui leur donnent de l'information sur les gains. Cependant, dans toutes les études antérieures fondées sur la théorie de la détection du signal, les matrices de gains sont toujours affichées avant les preuves perceptuelles. Nous émettons l'hypothèse que l'ordre dans lequel l'information est fournie peut avoir de l'importance et que le fait de recevoir une information en dernier peut en accentuer l'importance dans le processus décisionnel. Nous manipulons donc l'ordre des informations entre les gains et la preuve perceptuelle dans une tâche de discrimination visuelle pour vérifier l'existence d'un effet d'ordre. Les résultats montrent un effet d'ordre entre les gains et les stimuli : les individus déplacent leur critère de décision vers l'optimalité lorsque l'information sur les gains est donnée en dernier et nous observons une tendance suggérant une meilleure discrimination lorsque les stimuli sont affichés en dernier. Même si ces effets ne sont pas assez importants pour atteindre l'optimalité, ils informent sur l'intégration des gains et des stimuli dans le processus décisionnel perceptuel.

Les chapitres 2 et 3 utilisent la prise de décision médicale comme illustration. En effet, le cadre de détection du signal est particulièrement pratique pour penser à la décision d'un médecin dans l'incertitude. Bien que la relation patient-médecin soit une relation à deux agents, ces chapitres simplifient la situation en résumant tous les coûts et avantages de la décision pour le patient et le médecin dans une fonction d'objectif unique que le médecin maximise. Pour aller plus loin, il est intéressant de distinguer les fonctions d'objectif du médecin et du patient pour étudier comment un médecin pondère les deux fonctions dans sa décision. Cette situation est plus complexe car elle est influencée à la fois par l'attitude à l'incertitude, les préférences vie-à-vie d'autrui

(si les deux fonctions d'objectif ne sont pas parfaitement alignées) et, peut-être, une interaction des deux. Cette interaction potentielle peut provenir du fait qu'un environnement incertain peut fournir aux individus une excuse pour se comporter de façon plus égoïste. Même si le chapitre 4 n'est pas directement motivé par la prise de décision médicale (pour laquelle le médecin pourrait penser qu'il doit agir en faveur du patient), il étudie comment l'incertitude et le comportement vie-à-vie d'autrui interagissent. Dans ce chapitre intitulé *Ambiguity and excuse-driven behavior in charitable giving* et co-écrit avec Sébastien Massoni et Marie Claire Villeval, nous étudions comment l'ambiguïté interagit avec le comportement de don en utilisant le risque comme référence. Nous nous appuyons sur l'étude de Exley (2015a) qui conclut que les individus utilisent le risque comme excuse pour ne pas donner aux associations caritatives. Nous étendons cette étude à l'ambiguïté pour deux raisons. La première ambiguïté est une représentation plus précise du monde réel. Deuxièmement, l'ambiguïté est associée à un processus décisionnel plus complexe et plus subjectif que le risque. Nous voulons donc tester si la différence peut être utilisée d'une manière égoïste. Dans une expérience de laboratoire, nous varions le type de hasard (risque, ambiguïté partielle ou totale) et l'agent dont les gains sont incertains (le donneur ou l'association caritative). Nous montrons que les individus utilisent l'ambiguïté stratégiquement comme une marge de manœuvre morale pour se comporter moins généreusement sans se sentir coupables. Un tel comportement d'auto-justification est plus prononcé lorsque les gains du donateur sont incertains. Cependant, l'intensité des comportements d'auto-justification sont comparable dans l'ambiguïté et dans le risque. Les individus exploitent donc n'importe quel type d'aléa comme une excuse pour ne pas donner, quelle que soit la nature de cet aléa.

Enfin, le chapitre 5, nommé *Do laboratory measures of risk attitude predict behavior under risk in and outside of the laboratory?* et co-écrit avec Gary Charness, Theo Offerman et Marie Claire Villeval étudie les mesures de préférence face au risque. Ces mesures sont d'une importance cruciale dans la littérature économique car elles permettent aux chercheurs de mesurer l'attitude face au risque. Plusieurs mesures ont vu le jour sur la base de différents protocoles. Nous évaluons la validité externe de certaines de ces mesures en fonction de leur capacité à établir une corrélation avec les comportements à risque en laboratoire ou sur le terrain. Nous constatons que les mesures de préférence pour le risque sont liées aux comportements dans d'autres tâches de laboratoire. De plus, les méthodes les plus complexes sont moins efficaces pour expliquer les comportements

dans ces autres tâches. Cependant, nous constatons que les mesures de la préférence pour le risque ne sont pas liées à la prise de risque sur le terrain. Nous concluons donc que même si la validité externe des mesures des préférences face au risque est avérée dans le laboratoire, cette validité est compromise en dehors du laboratoire. Conformément à la conclusion de Friedman et al. (2014), il semble que les mécanismes mis au point pour mesurer la préférence pour le risque ne reflètent pas exactement les comportements risqués sur le terrain. Cela suggère que d'autres recherches sont nécessaires pour trouver une meilleure adéquation entre les mécanismes de mesure et les décisions risquées.

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Chapter 1

General introduction

Il n'est pas certain que tout soit
incertain
(it is not certain that everything is
uncertain)

Blaise Pascal, *Pensée*

Uncertainty is an inescapable element of human existence. We do not know what our future is made off but we still need to face it. Every time we choose a route to work, decide whether to go on a second date, or set money aside for a rainy day, we are dealing with uncertain consequences.

Consider the following situation: a doctor has to choose a treatment for a patient. He will perform a diagnosis of the patient's condition. The diagnosis cannot determine with certainty the patient's disease. He is thus unsure of the patient's condition. However, he knows the patient's post-treatment states of health depending on the patient's actual condition. For example, if the patient suffers from a disease and the doctor choose not to treat him, the patient will not be able to recover and the disease may get worse. In the described situation, the doctor's decision is driven by his attitude toward risk or uncertainty: how he will weight the different post-treatment states of health based on their respective likelihood. This attitude towards risk or uncertainty might be guided by some professional standards or by some personal opinions. I am interested in studying how individuals would behave in such a risky or uncertain decision framework.

A key feature of decisions with random consequences is to characterize what is known about the unknown. The decision maker may know the law governing the randomness.

In other words, he might know the probabilistic distribution over the possible outcomes. In this case, we define the randomness as “risk”. Another possibility is that even the distribution probability is unknown to the decision maker i.e. there are no objective probabilities to guide decision making. In this case, we define the randomness as “uncertainty” (Knight, 1921).

All chapters of this thesis are dedicated to the study of human behavior under risk and uncertainty. All of them are based on the analysis of individual behavior. This behavior has been observed either during laboratory experiments or in the field.

The laboratory is an important tool to study decision-making as it enables tight control of decision environments. Indeed, the experimenter has total control on the information received by the subjects, and can manipulate this information to study behavioral changes. Studying decisions in the field decreases the control over the experimental environment but increases the external validity of the results as decisions are not made in an artificial environment. Laboratory and field experiments are thus two important and complementary tools to study human behavior.

Concerning the decisions under risk, laboratory experiments provide researchers with perfect information on the outcomes and the associated probabilities considered by an individual when making a decision. In addition, researchers can freely select the outcomes and probabilities that are the most relevant to study a problematic. In comparison, it is more difficult to know outcomes and probabilities in real life decisions and even more difficult to manipulate them. Laboratory experiments are thus well suited to the evaluation of theories on how individuals make decisions under risk. Especially, they enable researchers to estimate the decision parameters that drive theoretical predictions. I present in Section 1.1 the main theories of decision under risk and experimental procedures to elicit their respective parameters.

While under risk most experimental tasks are based on lotteries where all possible outcomes as well as their associated probabilities are known, types of tasks are numerous under uncertainty. Indeed, providing information on a random situation without giving information on probabilities give a lot more latitude to the experimenter. A possibility is to provide subjects with a probability range. This type of uncertainty is referred to as “ambiguity”. Another possibility is to induce uncertainty using perceptual information. I present in Section 1.2, first the main findings on behavioral differences between risk and ambiguity, then different perceptual tasks used to study decisions under uncertainty

and the underlying theoretical frameworks used to analyze these decisions. Finally, Section 1.3 gives the aim and outlines of this thesis.

1.1 Decision under risk

Two dominant theoretical frameworks are widely used to model decisions under risk: expected utility theory and cumulative prospect theory.

The expected utility theory (hereafter EUT) assumes that the value of a risky prospect is given by the sum of the utility of each of the outcomes weighted by its corresponding probabilities. The valuation of the outcomes in term of the utility of the payoffs instead of payoffs follows the work of von Neumann and Morgenstern (1945). Formally, consider lottery P , such that:

$$P = (x_1, p_1; \dots; x_n, p_n) \quad (1.1)$$

with (x_1, \dots, x_n) the set of outcomes and (p_1, \dots, p_n) the associated probabilities. The expected utility is given by:

$$V_{EUT}(P) = \sum_{i=1}^n p_i \times U(x_i) \quad (1.2)$$

with U defined as a von Neumann-Morgenstern utility function. These functions are typically assumed to be concave.

An alternative theory is the cumulative prospect theory (Tversky and Kahneman, 1992, hereafter CPT). CPT differs from EUT on three points. First, the utility of the outcomes is not weighted by corresponding probabilities but by a function of these probabilities. It allows for the over-weighting of small probabilities and the under-weighting of large probabilities. Second, the probability weighting function is applied, not to the probabilities of single events, but to the cumulative distribution function. It allows over-weighting of only unlikely extreme outcomes, rather than all unlikely outcomes. Finally, the probability weighting function is different for gains and losses (defined according to an endogenous reference point). It allows for a different weighting of a gain and an equivalent loss.

Formally, consider the lottery $P = (x_{-m}, p_{-m}; \dots; x_n, p_n)$ such that outcomes are ranked (i.e. $x_{-m} \leq x_{-m+1} \leq \dots \leq x_n$) and $U(x_0) = 0$. The expected utility under CPT

of this lottery can be written:

$$V_{CPT}(P) = \sum_{i=1}^n \pi_i^+ \times U(x_i) + \sum_{i=-m}^0 \pi_i^- \times U(x_i) \quad (1.3)$$

where:

$$\begin{cases} \pi_n^+ = w^+(p_n); \pi_{-m}^- = w^-(p_{-m}) \\ \pi_i^+ = w^+(p_i + \dots + p_n) - w^+(p_{i+1} + \dots + p_n), 0 \leq i \leq n-1 \\ \pi_i^- = w^-(p_{-m} + \dots + p_i) - w^-(p_{-m} + \dots + p_{i-1}), 1-m \leq i \leq 0 \end{cases} \quad (1.4)$$

and where w^+ and w^- are strictly increasing functions from the unit interval into itself.

While these theories provide a conceptual framework to study decision making under risk, one needs to assume a functional form for the utility function (and the probability weighting function for CPT) in order to fit actual behavior. The family of utility functions the most widely used in experimental economics is constant relative risk aversion (CRRA) functions (e.g. Wakker, 2008). A simple illustration is the choice of a portfolio that can contain a risky asset and a safe asset. If he experiences an increase in wealth, an individual with CRRA preferences will choose to keep unchanged the fraction held in the risky asset. Formally the CRRA utility function for a lottery with only gains can be written as follows:

$$\forall x \in \mathbb{R}^+, U(x) = \begin{cases} \frac{x^{1-\gamma}}{1-\gamma} & \text{if } \gamma \neq 1 \\ \ln(x) & \text{if } \gamma = 1 \end{cases} \quad (1.5)$$

with x the payoff and γ the coefficient of risk aversion. Individuals are referred to as “risk averse” if their risk aversion parameter is positive (concave utility function). They are referred to as “risk seeking” if their risk aversion parameter is negative (convex utility function). They are referred to as “risk neutral” otherwise.

Concerning the probability weighting function, the function the most widely used has been introduced by Prelec (1998). It is given by:

$$\pi(p) = \exp[-(-\ln p)^\alpha] \quad (1.6)$$

with p the probability and α the probability sensitivity parameter.

One objective of experimental economics is to develop procedures to elicit the free

parameters of a certain theoretical framework. Numerous procedures have been developed that vary regarding the complexity of the induced choice menu. I hereafter describe some of the most influential procedures.

One of the earliest procedure has been introduced by Binswanger (1980). Subjects have to choose between seven lotteries. The first six lotteries are mean-increasing and variance-increasing i.e. higher expected value but more risk. The last one is mean-preserving and variance-increasing i.e. same expected value and more risk. A similar procedure has been developed by Eckel and Grossman (2008). The two procedures differ regarding the relationship between expected payment and variance (convex for Binswanger, 1980, and linear for Eckel and Grossman, 2008). Gneezy and Potters (1997) also implement a comparable procedure based on mean-increasing and variance-increasing choices but for a choice set much larger and without the use of lotteries. In these procedures, subjects have to choose how to invest in a risky asset paying k times the invested amount with probability p (and loss it otherwise). The amount that is not invested is kept by the subject. p and k are chosen so that $p \times k > 1$, making the expected value of investing higher than the expected value of not investing.

Another approach consists in using price lists. This approach has been introduced by Holt and Laury (2002). This procedure has become prevalent in experimental economic studies (Charness et al., 2013). Subjects have to make 10 binary choices between two lotteries A and B. Lotteries A and B have two possible payoffs. The payoffs are ranked such that one of the payoff of Lottery A is lower than both payoffs of Lottery B while the other payoff of Lottery A is higher than both payoffs of Lottery B. The payoffs of lotteries A and B are constant, however the probability associated with each outcome changes. Subjects are expected to start selecting Lottery A for the first decisions then to switch to Lottery B at some point. Tanaka et al. (2010) also developed a method based on price lists that include three different lists. Contrary to Holt and Laury (2002), the probabilities associated with the outcomes of the binary lotteries A and B are fixed. Instead, the outcomes of the lotteries change within each list. Having three price list enables researchers to estimate three parameters instead of one.

Other procedures rely on more visual tasks that mimic real-life situations. In the procedure introduced by Lejuez et al. (2002), subjects have to inflate a balloon that might explode each time it is inflated. The probability of explosion increases with each pump. Subjects earn money each time they inflate the balloon but lose all earnings if

the balloon explodes. The task of Crosetto and Filippin (2013) follows a similar design. Subjects have to consecutively collect boxes, one of which contains a bomb. Earnings increase linearly with the number of boxes accumulated but drop to zero if the bomb is also collected.

Finally, some non-incentivized procedures (based on reported risk aversion) have also emerged. These methods are convenient as they can be included in a survey, however they do not enable eliciting parameters. The most influential is the one of Dohmen et al. (2005) that consists of a Likert scale on the willingness to take risk. This measure has been found to be related to incentivized laboratory measures (Dohmen et al., 2011).

At the exception of the procedure of Tanaka et al. (2010), all these procedures have in common that the decisions are designed in such a way that the coefficient of risk aversion of the EUT can be estimated once a parametric form of the utility function is assumed (or more precisely a minimal and a maximal value of this parameter). Another approach to estimate the EUT parameter consists in retrieving the coefficient of risk aversion through auction bids (Cox et al., 1982). From a theoretical point of view, estimating risk aversion under EUT is rather simple as it requires to estimate only one parameter. By opposition, estimating the parameters of CPT is more complex as it is based on three parameters (curvature of the utility function, probability sensitivity, and loss aversion). The procedure of Tanaka et al. (2010) is designed such that these three coefficients can be estimated.

To conclude on risk preference measures, it is worth noticing that, while most estimations are parametric, some procedures differ as they enable parameter-free estimations. As an example, the “trade-off” method introduced by Wakker and Deneffe (1996) search for indifference between two-outcome gambles. This method has been used by Wakker and Deneffe (1996) to elicit risk preferences under EUT. Abdellaoui (2000) adds a second step after the trade-off method to also obtain a sequence of probabilities equally spaced in terms of probability weighting and thus elicit risk preferences under CPT.

Economists thus have a large number of procedures available to estimate risk aversion. In an ideal world, the tool (here the procedure) used to measure a phenomenon (here risk aversion) should not affect the observed measure. However, studies comparing measures obtained with the different procedures consistently conclude that the measured risk aversion depends on the elicitation method (see He et al., 2018 for a review). It stresses the importance of the choice of the risk procedure.

A fundamental aspect of laboratory experiments is their generalizability i.e. that insights gained in the lab can be extrapolated to the world beyond (Levitt and List, 2007). Estimated drivers of decisions under risk in the laboratory should thus also drive, at least to a certain extent, decisions in the field. Based on similar reasoning, several of the procedures previously described have relied on their ability to explain field behavior to justify their relevance (e.g. Lejuez et al., 2002; Dohmen et al., 2005 and Tanaka et al., 2010). However, no study has yet lead a comprehensive analysis of the external validity of the risk elicitation procedures. Chapter 5¹ is dedicated to the systematic analysis of the external validity of five of the most influential risk elicitation procedures that are the procedures introduced by: Gneezy and Potters (1997), Holt and Laury (2002), Dohmen et al. (2005), Eckel and Grossman (2008), and Tanaka et al. (2010). Their external validity is assessed based on two sets of behavior: decisions under risk in the laboratory and decisions under risk in the field.

In addition, Chapter 2 uses the risk preference measure of Eckel and Grossman (2008) to estimate the curvature of the utility function of the subjects. It enables us to define optimal decision strategies under EUT.

1.2 Decision under uncertainty

In many decisions with random consequences, the decision maker has only vague information about the probabilities associated with the different outcomes. For all these situations, presenting the decision as a decision under risk does not capture the complexity of the situation. The situation thus needs to be presented as a decision under uncertainty where the subjects do not know the probabilities of the outcomes. From an experimental point of view, implementing decisions under uncertainty is associated with a bigger challenge than decisions under risk. Indeed, the researcher needs to provide some probabilistic information that is complex enough to be vague but not too complex to enable control on the information considered by the decision maker. Two solutions are implemented in this thesis. The simpler type of uncertainty used is referred to as “ambiguity”. Ambiguity consists of providing subjects with a probability interval that contains the actual probability. A more complex way of inducing uncertainty relies on the use of perceptual information. The objective is to give perceptual information that provides

¹Chapter 5 is the first to be mentioned in this introduction. I have chosen to rank the different chapters in a chronological order to capture the evolution of my research.

information on the probabilities of each outcome. However, the information should be noisy enough not to reveal totally the probabilities. I review in Subsection 1.2.1 the main empirical findings on the difference between risk and ambiguity. Subsection 1.2.2 describes experimental settings and associated theoretical framework used in this thesis to study decision making under perceptual uncertainty.

1.2.1 Decision under ambiguity

A natural starting point regarding the difference between decisions under risk and under ambiguity is the work of Ellsberg that develops the “Ellsberg Paradox” (Ellsberg, 1961). In its simpler version, the decision maker faces two urns with 100 balls in each urn. The first urn A contains 50 red balls and 50 black balls. The second urn B contains red and black balls but the number of balls of each color is unknown. The decision maker makes two consecutive decisions. For the first decision, the decision maker earns money if a red ball is drawn. For the second decision, the decision maker earns money if a black ball is drawn. Becker and Brownson (1964) find that most individuals prefer to bet on the urn A for both decisions. This is a paradox as they act as if: (i) it is more likely to draw a ball in urn A than in urn B, (ii) it is also more likely to draw a black ball in urn A than in urn B. Such individuals are referred to as “ambiguity averse” in the sense that they would rather choose an alternative where the probability distribution of the outcomes is known over one where the probabilities are unknown. Oechssler and Roomets (2015) review 39 experimental papers implementing the Ellsberg urn problem published between 1964 and 2015 (including the one of Becker and Brownson, 1964). The percentage of ambiguity averse subjects is on average 56.6% but there is a lot of heterogeneity between studies (percentage varying from 8% to 93%). It seems to confirm that most individuals are ambiguity averse.

However, studying the difference between risk and ambiguity using preferences between Ellsberg urns does not give a comprehensive view of the question. Indeed, in such decisions, risky and ambiguous options are considered jointly. By opposition, Fox and Tversky (1995) study ambiguity attitudes in situations where the decision maker evaluates risky and ambiguous decisions only in isolation. They find that ambiguity aversion disappears in this setting. They conclude that the ambiguity aversion observed with Ellsberg urns is mainly driven by the contrast between the two urns. Binmore et al. (2012) find a comparable result using “matching probability” (Kahn and Sarin, 1988).

This method consists in eliciting the winning probability of the risky urn such that the decision maker is indifferent between betting on the risky urn or the ambiguous urn. They do not find evidence of ambiguity aversion but they only find that decisions are affected by some noise that is modestly biased in the direction of ambiguity aversion.

Heterogeneous preferences or non-uniform ambiguity attitudes can also explain the heterogeneity in the results on ambiguity aversion. Stahl (2014) finds a large heterogeneity in ambiguity attitudes. While the representative subject of his study would be ambiguity averse, only 12% of the subjects are actually categorized as such. Baillon and Bleichrodt (2015) study ambiguity aversion for gains and losses that are either likely or unlikely. They highlight a fourfold pattern of ambiguity attitude: ambiguity aversion for likely gains and unlikely losses and ambiguity seeking for unlikely gains and likely losses.²

A related question is the relation between the level of ambiguity (difference between highest and lowest probabilities within the probability interval) and ambiguity aversion. Becker and Brownson (1964) assess this question by implementing 10 choices between two urns with different levels of ambiguity and eliciting subjects' willingness to pay to bet on their preferred urn for each of the choices (the "ambiguity premia"). They find that the ambiguity premia decreases with the level of ambiguity. This result has latter been replicated by, for example, MacCrimmon and Wehrung (1990); Curley and Yates (1989) and Kahn and Sarin (1988).

Evidence on ambiguity-attitude is thus globally in favor of ambiguity-aversion even if the robustness of ambiguity-aversion across contexts is weak (see Trautmann and Van De Kuilen, 2015 for a more detailed review of experimental work on ambiguity attitude). In all previously described settings, individuals evaluate risk and ambiguity with consequences only for themselves. There is thus no room for strategic evaluation of ambiguity. If individuals can use risk and ambiguity strategically, for example as a justification for their unfair behavior, the difference of behavior under risk and ambiguity may be strengthened. Chapter 4 studies how ambiguity attitude and risk attitude differ in a context where risk and ambiguity can be used as an excuse not to give. The design does not involve joint consideration of lotteries under risk and ambiguity to have a conservative evaluation of ambiguity aversion. In addition, different levels of ambiguity are implemented to test whether excuse-driven behavior under ambiguity is affected by

²Baillon and Bleichrodt (2015) use a natural source of uncertainty instead of probability range in their study.

the level of ambiguity. It thus contributes to better understand the difference between decision making under risk and under ambiguity.

1.2.2 Decision under perceptual uncertainty

Providing individuals with some probabilities (either known for risk or vague for ambiguity) is appealing as it gives to the researcher a lot of control on the information available at the decision time. However, this control comes at the cost of rather artificial situations that may prevent the observation of some drivers of naturally-occurring decisions. Studying decisions in tasks that provide information in a more natural way is thus also important to have a comprehensive approach to decisions in a random environment.

Some studies use natural sources of uncertainty to generate randomness (e.g. Heath and Tversky, 1991; de Lara Resende and Wu, 2010; Abdellaoui et al., 2011 and Chew et al., 2012). These natural sources are often given by either climatic events, as temperatures, or evolution of stock markets. A typical finding highlighting the importance of the source of uncertainty is that individuals are less averse to uncertainty when they feel familiar with the uncertainty source.

In this thesis, I use perceptual tasks to induce uncertainty and study different aspects of the decision under uncertainty. By opposition to lotteries, for example, perceptual tasks rely on the limitation of cognitive abilities rather than on the artificial randomness of the situation. In the implemented situations, the decision maker has to identify if a signal is present or absent. The decision maker observes whether the signal is present based on some perceptual information. However, the information is available only for a short period of time making the presence of the signal uncertain to the observer (while being certain for the experimenter, hence the uncertainty implied by cognitive limitations). There are four possible cases: two types of successes and two types of errors. The decision maker reports that signal is present either correctly (“hit”) or incorrectly (“false alarm”), he reports that signal is absent either correctly (“correct rejection”) or incorrectly (“miss”). His decision is thus a trade-off between not missing too many signals and not interpreting too many absent signals as present.

While decisions based on perceptual evidence are less artificial than decisions based on lotteries, it is more difficult for the researcher to know the information considered. Model-based approaches are thus used to provide an analytic framework. One of these

frameworks is the Signal Detection Theory (Green and Swets, 1966, SDT hereafter).³ It enables researches to disentangle what comes from the difficulty of the task — how hard it is to distinguish between the presence and the absence of the signal — and what comes from the decision strategy — minimum level of evidence in favor of the presence of the signal needed to answer signal. In its simplest form, the amount of evidence received when the signal is present or when the signal is absent are assumed to follow Gaussians that differ in means but not in variance. The SDT states that the decision is entirely determined by applying a simple decision criterion to the magnitude of evidence i.e. if the amount of evidence exceeds a threshold, the observer reports that the signal is present and otherwise he reports that the signal is absent.

SDT has an enormous impact on many branches of psychology as the interpretation of sensory processes, memory research or processing of verbal information (McNicol, 2005). However, only few studies have used SDT in the economic literature (e.g. Barkan et al., 1998 and Barkan, 2002 for applications to safety problems). I believe that SDT can provide an appropriate framework to study many situations of general interest for economists. Indeed, economic situations including Type I and Type II errors (or successes) are numerous: e.g. trading decisions on a volatile market, jury deliberations on the guilt of a defendant and medical decision making (see introducing example). Chapter 2 and Chapter 3 use SDT applied to economic situations to highlight the existence of an “optimality accuracy trade-off”. The tension between optimality and accuracy is induced by rewarding asymmetrically success when the signal is present and when the signal is absent (the two types of errors being rewarded equally). Subjects thus face a trade-off between trying to maximize earnings and trying to give the highest number of correct answers. It enables us to evaluate if individuals derive a non-monetary utility from the fact of being right. It also enables us to question the impact of the information order on such decisions.

³Another influential framework is given by Drift Diffusion Models (Ratcliff, 1978, hereafter DDM). It assumes that evidence is accumulated following a random walk process or more generally a Brownian motion (Kramers, 1940), when the level of evidence exceeds a certain threshold it triggers the decision. The reaction time is thus a central aspect of DDM. This framework is outside of the scope of this thesis.

1.3 Aim and outline of this thesis

The two first essays use perceptual tasks as a tool and SDT as a model to highlight the existence of an optimality-accuracy trade-off in decisions under uncertainty and discuss its origin. While they share a common methodology and subject of interest, they differ regarding their targeted audience. The first chapter is at the border between economics and psychology as it aims to introduce new methods from computational psychology in economics to answer economic questions. It is thus addressed to economists. The second chapter crosses this border. It extends previous studies in computational psychology and is thus addressed to psychologists. This chapter thus differs in the redaction style to fit the norms of the psychology literature. The two last chapters differ from the two formers as they rely on typical economic tasks rather than perceptive tasks. The third chapter studies the difference between risk and ambiguity by targeting a special aspect of the decision: self-excusing behavior. Finally, the last chapter combines field and laboratory data to study if risk elicitation methods are able to explain behavior under risk inside and outside of the laboratory.

To go into more details, Chapter 2 presents the essay *Aiming to choose correctly or to choose wisely? The optimality-accuracy trade-off in decisions under uncertainty*, co-authored with Sébastien Massoni. In this essay, we study situations where individuals must decide whether an event has occurred using uncertain evidence. Accurately identifying that this event has occurred is more rewarded than accurately identifying that it has not occurred. We can identify two decision strategies. First, to maximize the expected reward of the decision that we refer to as the “optimal strategy”. Second, to maximize the likelihood of accurately classifying the situation: the “accuracy-maximizing strategy”. Aiming for optimality will reduce accuracy while targeting accuracy will decrease expected benefits. The decision maker is thus facing a trade-off between optimality and accuracy. Our study is composed of two laboratory experiments pursuing two complementary objectives. The first experiment assesses whether the existence of the optimality-accuracy trade-off is empirically supported and tests its robustness in different settings. The second experiment evaluates to what extent the optimality-accuracy trade-off can be explained by the utility derived from being right. To properly analyze the trade-off one needs to implement in the laboratory a task complex enough not to lose the essence of the studied situation while still being able to identify subjects’ deci-

sion strategies and compare them to theoretical predictions. As an answer, we base our approach on a perceptual task and a computational model used in cognitive sciences: Signal Detection Theory. Linking economic questions with models outside of the economics literature to answers relevant questions for economists is the key contribution of this chapter. The results of the first experiment confirm the existence of the trade-off with a leading role of accuracy. The results of the second experiment explain the trade-off by the concern of individuals for being right.

Chapter 3 presents the essay *Manipulating payoff processing order in perceptual decision-making* co-authored with Ismaël Rafai and Sébastien Massoni. The Chapter 3 builds on the findings of Chapter 2 as it studies a new potential explanation of the optimality-accuracy trade-off based on a recency bias. A way of interpreting the optimality-accuracy bias is to conclude that individuals rely too much on perceptual evidence — giving them information on the correct answer — and not enough on the payoff matrix — giving them information on the payoffs. However, in all previous studies relying on Signal Detection Theory, payoff matrices are always displayed before perceptual evidence. We hypothesize that the order in which information is provided may matter and that receiving a piece of information last may emphasize its importance in the decision process. We thus manipulate the information order between payoffs and perceptual evidence in a visual discrimination task to test for the existence of an order effect. Results show an order effect between payoffs and stimuli: individuals shift their decision criterion in the direction of optimality when payoff information is given last and we observe a trend suggesting a better discriminability when stimuli are displayed last. Even if these effects are not large enough to reach optimality, they inform about the integration of payoffs and stimuli in the perceptual decision process.

Chapter 2 and Chapter 3 use medical decision making as an illustration. Indeed, the signal detection framework is particularly convenient to think of the decision of a doctor under uncertainty. While the patient/doctor relation is a two-agent relation, these chapters simplify the situation by summarizing all the costs and benefits of the decision for the patient and the doctor in a unique objective function maximized by the doctor. To go a step further, it is interesting to distinguish the objective functions of the doctor and the patient to study how a doctor weights both functions in his decision. This situation is more complex as it is influenced both by uncertainty attitude, other-regarding preferences (if both objective functions are not perfectly aligned) and, maybe,

an interaction of both. This potential interaction may come from the fact that an uncertain environment may provide individuals with an excuse to behave more selfishly. Even if Chapter 4 is not directly motivated by medical decision making (for which the doctor might feel that he must act in favor of the patient), it studies how uncertainty and other-regarding behavior interact. In this chapter named *Ambiguity and excuse-driven behavior in charitable giving* and co-authored with Sébastien Massoni and Marie Claire Villeval, we study how ambiguity interacts with giving behavior using risk as a baseline. We build on the study from Exley (2015a) that finds that individuals use risk as an excuse not to give to charities. We extend this study to ambiguity for two reasons. First ambiguity is a more accurate representation of the real world. Second, ambiguity is associated with a more complex and more subjective decision process than risk. We thus want to test if the difference might be used in a self-serving way. In a laboratory experiment, we vary the type of randomness (risk, partial ambiguity or full ambiguity) and the agent with uncertain payoffs (either the donor or the charity). We show that individuals use ambiguity strategically as a moral wiggle room to behave less generously without feeling guilty. Such excuse-driven behavior is more pronounced when the payoffs of the donor are uncertain. However, the importance of excuse-driven behavior is comparable under ambiguity and under risk. Individuals exploit any type of randomness as an excuse not to give, regardless of the nature of this randomness.

Finally, Chapter 5, named *Do laboratory measures of risk attitude predict behavior under risk in and outside of the laboratory?* and co-authored with Gary Charness, Theo Offerman and Marie Claire Villeval focuses on measures of risk attitude. These measures are of crucial importance in the economic literature as they enable researchers to measure risk attitude. Several measures have emerged based on different protocols. We evaluate the external validity of some of these measures based on their ability to correlate with behavior under risk either in the laboratory or in the field. We find that measures of risk attitude are related to behavior in other laboratory tasks. Moreover, the most complex methods are less closely related to behavior in risk tasks. However, we find that measures of risk attitude are not related to risk-taking in the field. We conclude that while the external validity of risk preference measures holds in closely related frameworks, this validity is compromised in more remote settings. In line with the conclusion in Friedman et al. (2014), it appears that the mechanisms developed to measure risk preference do not accurately reflect behavior under risk in the field. It

suggests that further research is needed to find a better match between measurement mechanisms and decisions under risk.

1.4 Contribution to each chapter

As the four following chapters have been co-authored, I state below my specific contributions to each of these chapters.

- **Chapter 2:** I proposed the research question, contributed to the design of the experiments, programmed the experiments, conducted the experiments, undertook the empirical analysis, and wrote the first draft. Sébastien Massoni provided me with guidance during these different steps and proposed the general framework.
- **Chapter 3:** Ismaël Rafai (Ph.D. student - GREDEG) and I proposed the research question, designed the experiment, conducted the experiment and undertook the empirical analysis. I programmed the experiment and wrote the first draft. Sébastien Massoni provided us with guidance during these different steps.
- **Chapter 4:** I proposed the research question, proposed the design of the experiment, programmed the experiment, conducted the experiment, undertook the empirical analysis and wrote the first draft. Sébastien Massoni provided and Marie Claire Villeval provided me with guidance during these different steps.
- **Chapter 5:** I undertook the empirical analysis and wrote the first draft. Gary Charness, Theo Offerman and Marie Claire Villeval provided me with guidance during these different steps. They also proposed the research question, proposed the design of the experiment, programmed the experiment and conducted the experiment.

Chapter 2

Aiming to choose correctly or to choose wisely? The optimality-accuracy trade-off in decisions under uncertainty¹

2.1 Introduction

Individuals regularly face situations where they must decide whether an event has occurred or not. However, evidence is often insufficient to determine with certainty the accurate answer. This lack of evidence can create a potential tension between two fundamental aspects of the decision: whether to maximize the expected consequences of the decision or to make the accurate decision. If accurately identifying that an event has occurred is more important than accurately identifying that it has not occurred, aiming at maximizing expected consequences will not maximize the likelihood of giving the accurate answer.

For example, consider a doctor who has to determine if a patient suffers from an illness. The doctor observes the patient's symptoms. However, these symptoms are not clear enough to determine with certainty whether the patient is sick. Moreover, the value of accurately diagnosing a sick patient is higher than the value of accurately

¹Co-authored with Sébastien Massoni.

diagnosing a sane patient (for instance because not treating a sick patient will cause its death). To account for the asymmetry in the decision consequences, a doctor who wants to maximize the expected impact of his decision on the patient's health should be more likely to diagnose sane patients as sick than to diagnose sick patients as sane. For an extreme asymmetry, the doctor could diagnose all patients as sick to guarantee that all sick patients are indeed treated at the expense of wrongly classifying all sane patients. By opposition, a doctor who wants to maximize accuracy should diagnose a patient as sick if he believes that it is more likely that the patient is sick than sane.

We define a first type of decision strategy: to maximize the expected value of the decision. This strategy corresponds to the classical value-based approach to the quality of a decision, and it is thus referred to as the "optimal strategy". We also define a second type of decision strategy: the "accuracy-maximizing strategy" that maximizes the likelihood of accurately classifying the situation. Aiming for optimality will reduce accuracy while targeting accuracy will decrease expected benefits. The decision maker is thus facing a trade-off between optimality and accuracy.

Our study is composed of two laboratory experiments pursuing two complementary objectives. The first experiment assesses whether the existence of the optimality-accuracy trade-off is empirically supported and tests its robustness in different settings. The second experiment evaluates to what extent the optimality-accuracy trade-off can be explained by the utility derived from being right.

The optimality-accuracy trade-off plays a crucial role in decisions under uncertainty involving asymmetric benefits between different types of successes and errors. Apart from medical decision-making, we can think about a response to a warning of danger, military decisions during conflicts, jury deliberations on the guilt of a defendant, or trading decisions on a volatile market. However, to the best of our knowledge and despite its important implications about efficiency, experts' behaviors and payment schemes, the trade-off has never been extensively analyzed in the economic literature. We argue that it results from a methodological difficulty. Indeed, to properly analyze the trade-off one needs to implement in the laboratory a task complex enough not to lose the essence of the studied situation while still being able to identify subjects' decision strategies and compare them to theoretical predictions. As an answer, we base our approach on a perceptual task and a computational model used in cognitive sciences. Individuals face a detection task in which they have to identify if a signal is present in a noisy

environment. The Signal Detection Theory (SDT hereafter; Green and Swets, 1966; Wickens, 2001) provides theoretical foundations for identifying the main components of the decision. It disentangles the impact of the stimulus itself (for example the symptoms of a patient) from the impact of the decision strategy (for example when to diagnose the patient as sick). Besides, SDT provides a powerful tool to make precise predictions of the expected behaviors for optimality or accuracy-maximization in our different settings. In the perspective of improving decision making, the ability of SDT to identify the decision strategy is crucial. It allows us to know whether, for example, poor performance is caused by low discrimination of the signal — in which case little can be done to improve performance — or by a decision strategy not suited to achieve the desired goal — in which case improved performance may be possible by influencing this strategy. For these reasons, we argue that this analytical and experimental framework is particularly appropriate to handle the optimality-accuracy question.

Our first experiment provides empirical findings on how people react to the optimality-accuracy trade-off. We vary the incentives to compare situations where optimal behavior and accuracy-maximizing behaviors are different (possible trade-off) to a baseline where they are similar (no trade-off). To capture a wide range of decisions settings, trade-off situations include gain situations (positive payoffs) and a pure-loss situations (negative payoffs). Besides, we study how risk aversion influences our results. We compute individuals risk aversion parameters to estimate the curvature of the utility function. Based on these parameters, we test the robustness of our results to defining optimality as maximizing the expected utility and no more as maximizing the expected value. The results of our first experiment show that observed decision strategies are included between optimal and accuracy-maximizing strategies. Furthermore, we find that decision strategies are closer to the accuracy-maximizing strategy than to the optimal strategy.

To establish the existence of an optimality-accuracy trade-off is a first step. However, it raises the question of which underlying processes are at its source. A first explanation of the trade-off could be that individuals carry out an actual trade-off by caring for monetary outcomes but also by caring for giving an accurate answer. A second explanation could be that, while caring only for monetary outcomes, individuals are unable to escape from a biased decision process leading to too much accuracy. To evaluate which explanation is the most likely, we introduce the concept of the “value of being right” in the second experiment. The value of being right corresponds to the utility derived

from being right outside of payoff considerations. Using the same setting than the first experiment, the second experiment compares behavior with and without monetary incentives to disentangle the value of being right from payoff motivations. We obtain that the value of being right plays a key role in the departure from optimality. Therefore, we conclude that the observed trade-off is not only the consequence of a difficulty to reach optimality but also of a search for accuracy.

The remainder of this paper is organized as follows: Section 2.2 gives a brief theoretical overview of SDT, presents the related literature and introduces our hypotheses. Section 2.3 describes the experimental design of the two experiments. The data analysis methodology and the results of our studies are reported in Section 2.4. Section 2.5 concludes.

2.2 Theoretical background and hypotheses

2.2.1 Signal Detection Theory

SDT provides a general framework to describe and analyze perceptual decisions made under uncertainty. All models of detection should deal with at least two components of the detection decision: the sensory process, which converts the stimuli into an internal representation; and the decision process, which decides based on this representation what to answer. SDT, by combining detection theory and statistical decision theory, permits us to disentangle the decision process from the perceptual process involved in the task itself. SDT is thus used as a statistical tool to describe how individuals have made their decisions. We will also rely on a second aspect of SDT in this study: its ability to provide a characterization of an ideal observer's decision process. Given the perceptual characteristics of the task, we can make predictions about the decision process of a hypothetical agent using all the information available in an optimal (Bayesian) way. Comparing actual behaviors with this ideal observer's behavior gives insight into the actual decision-making process of the individuals. We will first present the statistical aspect of SDT and show that we can infer the decision process based on actual choices. Then we will characterize how an ideal observer should behave and thus what is optimality in our decision framework.

Facing an uncertain situation, an observer has to detect if a noisy signal is present. SDT separates the decision-making process into two statistical elements: the discrimina-

tion (the perceptual properties of the task) and the criterion (the decision process used). The most natural setting of SDT is the following: a situation has two possible states of nature that are either “signal” or “noise”. In state “signal”, the signal adds up to some noise making its discrimination difficult. In state “noise”, only noise remains and no signal is present. The observer faces, and he gathers evidence on the state of nature. Based on the level of this internal accumulation of evidence, he answers either “signal absent” or “signal present”. There are four scenarios: to correctly report that the signal is present (“hit”), to incorrectly report that the signal is present (“false alarm”), to correctly report that the noise is present (“correct rejection”), and to incorrectly report that the noise is present (“miss”). Formally, the state of nature Θ takes values in $\{0, 1\}$ with $\Theta = 0$ meaning that the state of nature is “noise” and $\Theta = 1$ meaning that the state of nature is “signal”. The observer makes a decision D taking values in the same space ($D = 1$ meaning “signal present” and $D = 0$ meaning “signal absent”). Table 2.1 summarizes the four possible outcomes.

	Signal $\Theta = 1$	Noise $\Theta = 0$
Signal present $D = 1$	Hit	False Alarm
Signal absent $D = 0$	Miss	Correct Rejection

Table 2.1: Outcome of a decision (D) depending on the state of nature (Θ).

A continuous random variable represents the level of evidence, X , received by the observer on the state of nature. It is informative on the state of nature as its distribution depends on this state. A higher level of evidence indicates a higher likelihood that the state of nature is “signal”. The difficulty of the task depends on the shape of the densities associated with each state of nature (Figure 2.1 - d). The perceptual process is thus defined by this internal representation of evidence facing signal and noise situations. The distance between the means of these two density functions measures the discrimination abilities of individuals facing the task.² The level of discriminability between signal and noise impacts the different outcomes of the decision. For example, if there is a large overlap between the two conditional densities, the task is more difficult as the level of

²In the experiments we will control for individual heterogeneity of discrimination abilities. Thus the discrimination is a pure measure of the task difficulty.

evidence received in the state of nature “noise” has a high probability of being received in the state of nature “signal”. Note that this overlap implies that there will always be some errors in the decision process and explains why the choices are made under uncertainty. The information revealed by the level of evidence can be expressed as a likelihood ratio. This likelihood ratio is the ratio of the conditional probabilities of observing X , i.e. a single quantity expressing the strength of evidence associated with each state of nature.

$$\forall X \in \mathbb{R}, \mathcal{L}(X) = \frac{\mathbb{P}(X|\Theta = 1)}{\mathbb{P}(X|\Theta = 0)} \quad (2.1)$$

The observer has to decide when to answer “signal present”. The decision strategy is summarized by the minimal value of the likelihood ratio required to answer “signal present”. In other words, it represents the minimal strength of evidence in favor of the state of nature “signal” needed to answer “signal present”. This threshold, which is the statistical measure of the decision process itself, is called decision criterion and noted β (Figure 2.1 - a and b). The decision can thus be expressed as:

$$\forall (X, \beta) \in \mathbb{R}^2, D(X, \beta) = \begin{cases} 1 & \text{if } \mathcal{L}(X) \geq \beta \\ 0 & \text{if } \mathcal{L}(X) < \beta \end{cases} \quad (2.2)$$

When choosing his decision criterion, the observer faces a trade-off between the two types of correct decisions (hit and correct rejection). If the observer answers “signal present” more often, it increases his hit probability but decreases his correct rejection probability. The same mechanism applies if he sets his criterion according to the two types of errors (miss and false alarm), i.e. answering “signal present” more often decreases miss probability but increases false alarm probability. By observing the choices of subjects (and thus the proportions of each type of success and error), we are able to estimate the criterion used by individuals facing the decision problem.

In order to estimate the model, one needs to assume a distribution of perceived evidence by the observer. For each state of nature, standard SDT assumes that the level of evidence coming from a noise (X_n or $X|\Theta = 0$) or a signal (X_s or $X|\Theta = 1$) follow Gaussian distributions (Gold and Shadlen, 2001, 2007). The key parameter is the difference in means between these distributions: the discrimination, d' . The higher the

discrimination parameter, the easier it is to discriminate the signal from the noise.

$$X_n \sim \mathcal{N}(0, \sigma_n^2) \text{ and } X_s \sim \mathcal{N}(d', \sigma_s^2) \quad (2.3)$$

To decrease the number of parameters, it is possible to fix one of them. The convention is to fix the variance of the noise to 1. Besides, the model is commonly reduced to the equal variance SDT by assuming that the variances of the level of evidence in both states of nature are the same ($\sigma_s^2 = \sigma_n^2 = 1$). The specification has the advantage of being easily interpreted with one parameter referring to the difficulty of the task (the discrimination d') and the other referring to the decision strategy (the criterion β).

Overall, the statistical aspect of SDT allows for an estimation of the perceptual process via the discrimination ability and the decision process via the set of the decision criterion. As we consider that the observer is aware of the statistical properties of the signal and the noise but has no specific control over these properties, the perceptual process cannot be further investigated. On the contrary, the decision process can be studied in a normative way. Depending on the decision goals the relative valuation of each type of correct answer will vary and results in different decision rules. These decision rules may follow different optimization targets, and SDT describes the behavior of an ideal observer using these rules.³ We distinguish two decision rules that maximize either the expected payoff or the accuracy (Figure 2.1 - c).

We define the optimal observer as the observer maximizing the expected payoff of his decision. To maximize expected payoff, the relative valuation of a hit and a correct rejection must be equal to the relative payoffs' difference between a correct and an incorrect answer in both states of nature multiplied by the ratio of *prior* probabilities of each state. The decision criterion β^* maximizing the expected payoff of the decision is thus defined, with Π the payoff associated with each outcome, as:

$$\beta^* = \frac{\Pi(\text{correct rejection}) - \Pi(\text{false alarm})}{\Pi(\text{hit}) - \Pi(\text{miss})} \times \frac{\mathbb{P}(\Theta = 0)}{\mathbb{P}(\Theta = 1)} \quad (2.4)$$

An observer maximizing the probability of giving a correct answer will value equally a hit and a correct rejection. He will answer the more likely of the two states of nature. The decision criterion β^a maximizing the probability of success is thus equal to the ratio

³Note that all decision rules based on likelihood ratio are optimal according to the Neymann-Person criteria as they maximize the hit rate for a given correct rejection rate (Neyman and Pearson, 1933).

of *prior* probabilities of each state:

$$\beta^a = \frac{\mathbb{P}(\Theta = 0)}{\mathbb{P}(\Theta = 1)} \quad (2.5)$$

In our study, we focus on the opposition between accuracy and optimality induced by payoffs. The *prior* probabilities of both states of nature are chosen to be the same (equal base-rates; $\mathbb{P}(\Theta = 1) = \mathbb{P}(\Theta = 0)$). In equal base-rates, β^* and β^a are equal to:

$$\beta^* = \frac{\Pi(\text{correct rejection}) - \Pi(\text{false alarm})}{\Pi(\text{hit}) - \Pi(\text{miss})} \text{ and } \beta^a = 1 \quad (2.6)$$

Thus, SDT provides a normative framework for the decision process of an ideal observer using a criterion maximizing expected payoffs and a criterion maximizing success rates. While these two criteria can be defined as optimal in the sense that they come from an ideal observer, we define the optimal criterion as the one that maximizes the expected payoffs of the decision i.e. that provides optimal monetary performances facing the decision.⁴

In the present paper, we will use both aspects of SDT. The ideal observer theory will give predictions on how an individual maximizing expected payoffs or maximizing accuracy will behave. The statistical decision theory will be used to infer measurements on actual perceptual and decision processes of our subjects. By comparing ideal and observed behaviors, we will be able to see if individuals facing asymmetrical stakes decisions can reach the optimal performances in terms of payoffs or if they are more pulled toward accuracy.

2.2.2 Related literature

The optimality-accuracy trade-off has been extensively studied in cognitive sciences. Various methods to induce an asymmetrical situation have been proposed, as well as different modelizations of these situations. While most of the studies are in psychology, some attempts to explain the observed behavior have been proposed using an economic framework.

Using SDT, asymmetry is induced by manipulating either the probabilities of each state of nature (base-rates) or the payoffs. In the literature, payoff manipulations include

⁴See Rahnev and Denison (2017) for a consideration and a literature review about (sub)optimality of perceptual decision in Bayesian frameworks.

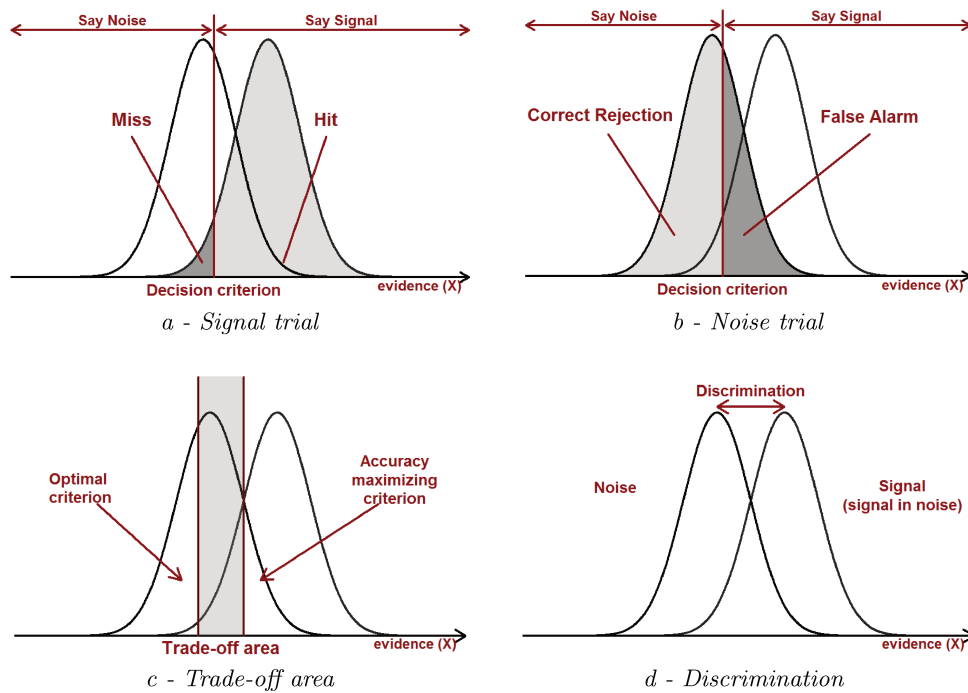


Figure 2.1: The different components of Gaussian equal variance SDT.

Reading note: Figure a and Figure b give the decision as a function of the criterion and the level of evidence. The shaded areas represent the corresponding probabilities of each outcome. Figure c plots, for an asymmetry toward signal, the trade-off area i.e. the set of criteria between the optimal and the accurate criterion. Figure d represents discrimination based on the distribution of the level of evidence coming from a noise and a signal.

the introduction of non-zero cost or a loss for some outcomes (Barkan et al., 1998; Maddox et al., 2003b; Maddox and Bohil, 2005), the multiplication of payoffs (Galanter and Holman, 1967; Maddox et al., 2003b) or combinations of the previous manipulations with different discriminations (Bohil and Maddox, 2001, 2003). Facing these changes in incentives, individuals use a decision strategy that is too close to the accuracy-maximizing decision strategy to reach optimality. When all payoffs are increased, the decision strategy is closer to optimality. Introducing non-positive payoffs or decreasing the discrimination has an opposite effect on the decision strategy. Facing asymmetry at the base-rate level, individuals also use decisions strategies that are between the accuracy-maximizing and the optimal decision strategy. Indeed, the probability that individuals report the more likely of both states of nature is lower than its *prior* probability. However, individuals tend to be closer to optimality when facing base-rate inequalities than equivalent

payoff asymmetries (Maddox and Bohil, 1998; Maddox and Dodd, 2001; Maddox and Bohil, 2005). The assertion that individuals use a decision strategy too close to the accuracy-maximizing decision strategy is dependant on how optimality is defined. Papers formerly mentioned are in line with SDT founding work (Green and Swets, 1966) as they consider optimality as the payoff-maximizing strategy. However, in a utility approach, the subjective distortion of payoffs and probabilities leads us to reconsider the optimal criterion. While Lynn and Barrett (2014) promote a utility-based SDT, Ackermann and Landy (2015) use individual weighting-functions to derive Prospect Theory optima. They conclude that the shift from expected-payoff to expected-utility maximization reduces the gap between optimality and accuracy. However, the shift is not strong enough to suppress the gap.

The present work complements existing researches on several points. First, we highlight the existence of the trade-off using a yes/no detection task while previous studies implement two-alternative forced choice (2AFC) tasks.⁵ In real-life situations, high stakes are caused by the signal (e.g., a disease for a doctor, or danger for soldiers). Contrary to a 2AFC setting, our framework enables us to reproduce the link between signal and high stakes, increasing the external validity of the results. Second, the impact of losses on the trade-off has been highlighted only in mixed gain-loss frameworks but never in a pure-loss frame. However, the non-separability between the valuation of gain and loss reveals the importance to isolate losses from gains to study the loss impact (Wu and Markle, 2008). As an answer, we implement an innovative pure-loss framework to link the impact of losses on decisions to the optimality-accuracy trade-off reaction. Third, using individual risk preferences estimations, we are able to challenge previous insights about the potential importance of a utility framework to assess the trade-off.

Different explanations have been investigated to sort out the origin of the optimality-accuracy trade-off. The first category of explanations is based on the idea that, as individuals face multiple consecutive decisions, what matters most is not the optimal solution *per se* but the path of optimization (Roth and Erev, 1995). Based on this idea, Maddox and Bohil (2005) argue that the maximization process does not converge to optimality as the slope of the reward function in a neighborhood of the optimal solution is not steep enough to enable individuals to reach optimality (flat-maxima hypothesis). If individuals initiate their maximization process at the equal-probability/equal-payoff solution, it will

⁵In yes/no detection tasks, the signal is either present or absent. In 2AFC tasks, the signal is always present, and individuals have to choose its location between two options.

converge to an overly accurate solution. The outcome bias may also affect the path of optimization. As the decision is taken under uncertainty, a correct answer is not always an optimal answer. Overestimating the meaning of a correct answer would result in an outcome bias (Alicke et al., 1994; Baron and Hershey, 1988; Gino et al., 2010). Erev (1998) states that the path of optimization does not converge to optimality, as positive rewards may reinforce a non-optimal strategy. In the same modelization framework, Myung and Busemeyer (1989) and Busemeyer and Myung (1992) find support for two different maximization processes, based either on a first or second order analysis of the criterion adjustment. The error correction model assumes that the magnitude of the stopping criterion is increased following an incorrect diagnosis while the hill climbing model states that the direction of adjustment changes after wrong answers.

The second category of explanations relies on the theory that subjects emphasize accuracy over optimality. Several studies have highlighted that individuals tend to give over-accurate answers (Maddox and Bohil, 2003; Bohil et al., 2004; Balci et al., 2011; Bogacz et al., 2006). Their approach is based on model comparisons with a goodness-of-fit analysis. As a result, Maddox and Bohil (2003); Bohil et al. (2004) argue that subjects attempt to maximize payoffs on each trial, but erroneously believe that maximizing accuracy fulfills this objective. This over-accuracy has also been interpreted as an induced value associated with successes. In a related setting based on unequal base-rates, Siegel (1959) concludes that individuals classifying correctly the less likely outcome benefit from an additional non-monetary reward compared to the correct classification of the more likely outcome. We base our approach on a novel design enabling a direct evaluation of the value of being right and thus challenges previous studies showing that over-accuracy results from a biased search of optimality.

2.2.3 Hypotheses testing

Based on the results of the literature, our two studies aim to test a set of five hypotheses.

Experiment 1 enables to test the following hypotheses:

H 1.1. *The observed decision criterion is lower bounded by the optimal decision criterion.*

H 1.2. *The observed decision criterion is upper bounded by the accuracy-maximizing decision criterion.*

Satisfying Hypothesis 1.1 and Hypothesis 1.2 gives the existence of the optimality-accuracy trade-off (cf. Figure 2.1 - c).⁶

H 1.3. *The observed decision criterion is closer to the accuracy-maximizing decision criterion than to the optimal decision criterion.*

Hypothesis 1.3 investigates whether one of the two concepts may have a leading role in the trade-off. Accuracy maximization is a natural behavior and requires only to assess information coming from the stimulus. An optimal behavior also requires to assess information from the payoff matrix and to aggregate them optimally with stimulus information. We thus hypothesize that optimality is harder to reach.

H 1.4. *The observed decision criterion and discrimination are affected by the decision under loss.*

Hypothesis 1.4 assesses the robustness of the previous hypotheses to decisions under loss.

H 2.1. *The optimality-accuracy trade-off is not only the consequence of a sub-optimal maximization process, but also a reaction to the intrinsic valuation of being right.*

Experiment 2 is designed to assess Hypothesis 2.1. More precisely, it investigates if the trade-off may take its roots in a mixed valuation of accuracy and optimality. To test this hypothesis, we need to disentangle the monetary incentives of giving a correct answer – as correct answers are more rewarded than errors – from the intrinsic motivation of being right. We implement two treatments: a treatment with higher payoffs for successes than for errors and a treatment without incentives on the quality of the decision. The incentivized treatment implements symmetric payoffs (same payoff for both types of successes and same payoff for both types of errors) such that accuracy and optimality are perfectly aligned. Thus maximizing only payoffs or a combination of accuracy and optimality lead to the same decision strategy ($\beta^* = \beta^a = 1$). In the flat payoff treatment, an individual valuing the fact of being right should maintain the same decision strategy as accuracy is still maximized by $\beta^a = 1$.⁷ In contrast, a pure-payoff maximizer should approach random guesses as all decisions give him the same level of satisfaction. Thus,

⁶The range of the bounds comes from the asymmetry toward signal; an asymmetry towards noise would reverse this range.

⁷Even if the decision strategy is kept constant, the effort invested may be different. The discrimination term captures it.

if we observe a change in behavior between both treatments, the optimality-accuracy trade-off is only the consequence of a maximization process unconsciously biased towards accuracy. Otherwise, the value of being right plays a role in the trade-off.

2.3 Design

Both experiments consisted of a series of individual perceptual decisions organized in a block/trial architecture. Each block is a treatment while each trial is a decision. To make a decision, subjects received two different types of information: a signal-in-noise stimulus and a payoff matrix. Facing the stimulus, they formed beliefs on whether it is a signal or a noise. The payoff matrix provided a matching rule between the four possible decision outcomes and the payoffs. By combining both information, they had to choose to answer either “signal present” or “signal absent”. Treatments corresponded to variations in the payoff matrix. Both studies used the same stimulus but differed mainly regarding the treatments implemented. In Experiment 1, before the main section, we elicited attitudes toward risk, ambiguity, and loss. This preferences elicitation was not present in Experiment 2 as risk and loss attitudes are not central to this experiment. At the end of both experiments, subjects had to answer a standard demographic questionnaire.

In this section, we present the main perceptual task, the preferences elicitation mechanism, the experimental procedures and our sample of subjects. Figure 2.2 gives an overview of a trial proceeding.

2.3.1 Stimuli

For both studies we used a variation of the perceptual task used by Massoni et al. (2014) and Hollard et al. (2016) to study subjective beliefs: a yes/no signal-in-noise stimulus. Subjects faced two circles with a certain number of dots in each one. They had to determine if there is a different number of dots in the circles (“signal present”) or if there is the same number of dots (“signal absent”). Dots were randomly displayed in two circles during 900 ms. When signal was the correct answer (“signal trial”), one circle always contained 50 dots while the other contained more dots. The position of the target circle (on the left or right) was randomly chosen on each trial. When noise was the correct answer (“noise trial”), both circles contained the same number of dots (50). The short display time and the randomness of the dots localization made counting dots impossible

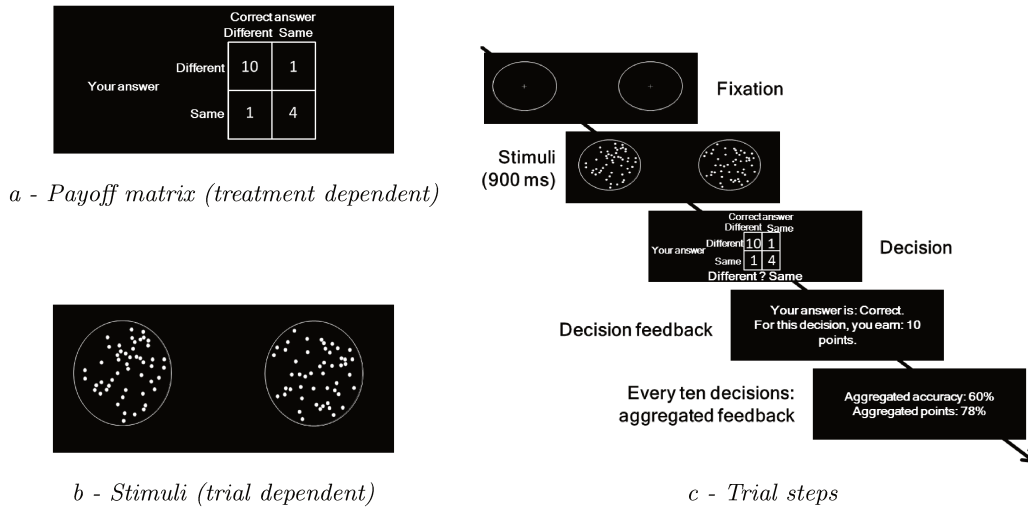


Figure 2.2: Trial proceedings.

Reading note: (a) Payoff matrix: Subjects were informed of the payoff matrix at the beginning of treatments and this matrix was then displayed on the screen when they made their decisions. (b) Example of stimuli. (c) Trial steps were: fixation, stimuli display (900 ms), decision and feedback. Every ten trials aggregated feedback constituted the last trial step.

for subjects. It caused uncertainty in the decision. To have a neutral framework, noise was referred to as “Same” and signal as “Different” during the experiment. Before and after the stimulus display, subjects faced a fixation screen (the fixation screen was composed of both circles without dots but with a cross in the middle of each one of them). The first fixation screen was used to direct subjects’ attention to the future location of the stimulus. The second one enhanced stimulus information processing after the end of the stimulus display. To increase control we i) calibrated the task difficulty at the subject level, and ii) randomly generated stimuli before the experiment by difficulty levels. The difference in the number of dots between the two circles in signal trials gives the stimuli difficulty (hereafter referred to as x_c). The calibration of the difficulty of the task was done using a psychophysical staircase at the subject level in order to control for individual heterogeneity of visual abilities (Levitt, 1971).⁸ In this non-incentivized

⁸The psychophysical staircase used was a one-up two-down staircase in which one dot was removed after two consecutive correct answers and one dot was added after one failure. The calibration ended when subjects had achieved 35 reversals in the staircase (a reversal means a

calibration period, subjects received only the stimulus information and had to determine if the stimulus were either in a noise or a signal state. Feedback on decision accuracy was provided after each decision. The targeted success rate was 71%. This level was chosen for subjects to outperform random guesses (relevance of the stimulus) and to avoid being too close to perfect discrimination (relevance of the payoff matrix). For the same level of calibration, we presented the same set of stimuli to subjects in each block for each difficulty level. Stimuli were randomly generated before the experiment under constraints concerning dots spacing.⁹ The frequency of signal and noise trials were controlled to be exactly equal, and the sequence was the same for all blocks.

2.3.2 Payoff matrices

We fixed base-rates but manipulated the payoff matrices to induce asymmetry in payoffs. Experiment 1 was based on 4 different matrices (one symmetric, three asymmetric) while Experiment 2 was based on two matrices (one symmetric and incentivized - same as Experiment 1 - and one with flat payoffs). Both studies were within-subject experiments. The order of treatments was randomized at a subject level.

Experiment 1 - Experiment 1 aimed to measure the difference in behavior resulting from asymmetrical stakes. Out of the four payoff matrices used, three generated asymmetric payoffs and one induced symmetric payoffs. All asymmetries were biased toward signal to increase external validity.¹⁰ Payoff matrices can be expressed using the two free parameters presented in Table 2.2. The a parameter introduces the optimality-accuracy trade-off by creating an asymmetry between both kinds of successes, and t translates the payoff matrix.¹¹ The “Symmetric Treatment” corresponded to $t = a = 0$. Subjects

change in the x_c 's value). The number of dots difference x_c used in the experiment was computed as the mean dots number across the two last reversals of the staircase.

⁹All dots were of the same size (diameter 0.4°) and the average distance between dots was kept constant. They appeared at random positions inside two outline circles (diameter 5.1°) first displayed with fixation crosses at their centers at eccentricities of $\pm 8.9^\circ$.

¹⁰As an example in cancer detection, security control or response to warnings of danger, the signal situation is respectively a tumor, a harmful individual or a threat. They are the high stakes situations.

¹¹In most real-world situations (e.g., medical decision-making), the asymmetry between answers' stakes is induced by an asymmetry of both successes and errors. Model predictions do not vary for an asymmetry induced by successes, errors or both, however the behavioral impact is an open question. As studying this phenomenon is not in the scope of this paper, we choose to induce asymmetry by varying only successes' payoffs to decrease the difficulty of the task and thus to reduce the noise in the evaluation of the trade-off.

earned the same payoff when the correct answer is signal or noise. The “Low Gain Treatment” induced an asymmetry while staying as close as possible from the Symmetric Treatment: $t = 0$, $a = 3$. The “Loss Treatment” was the minimal translation from Low Gain Treatment inducing losses for all outcomes: $t = -11$, $a = 3$. The “High Gain Treatment” was obtained from the Low Gain Treatment after a positive translation matching the norm of the translation between Loss and Low Gain Treatments: $t = 11$, $a = 3$. This design enabled us to implement a pure-loss treatment as even with only incorrect answers in loss decisions, subjects were covered by their decisions in gain and still obtained an overall positive payment.¹²

	Different No. of dots $\Theta = 1$	Same No. of dots $\Theta = 0$
“Different” $D = 1$	$7+t+a$	$1+t$
“Same” $D = 0$	$1+t$	$7+t-a$

Table 2.2: Experiment 1 - Payoff matrix (payoff of a decision, D , depending on the state of nature, Θ).

Treatments: “Symmetric Treatment” $(t, a) = (0, 0)$ i.e. $\begin{bmatrix} 7 & 1 \\ 1 & 7 \end{bmatrix}$; “Low Gain Treatment” $(t, a) = (0, 3)$ i.e. $\begin{bmatrix} 10 & 1 \\ 1 & 4 \end{bmatrix}$; “Loss Treatment” $(t, a) = (-11, 3)$ i.e. $\begin{bmatrix} -1 & -10 \\ -10 & -7 \end{bmatrix}$ and “High Gain Treatment” $(t, a) = (11, 3)$ i.e. $\begin{bmatrix} 21 & 12 \\ 12 & 15 \end{bmatrix}$.

Experiment 2 - Experiment 2 consisted of the previous Symmetric Treatment and a new flat payoff matrix as described in Table 2.3. This payoff matrix enabled us to have a flat payment with minimal departure from the Symmetric Treatment. For sake of clarity, Experiment 2 treatments are referred to as “Incentivized Treatment” and “Flat Treatment”.

¹²According to the payment rule detailed in Subsection 2.3.5, a subject giving only incorrect answers would have received: 20 points (5×1 point in the Symmetric Treatment, 5×1 point in the Low Gain Treatment, 5×-10 points in the Loss Treatment and 5×12 points in the High Gain Treatment).

	Different No. of dots $\Theta = 1$	Same No. of dots $\Theta = 0$		Different No. of dots $\Theta = 1$	Same No. of dots $\Theta = 0$
“Different” $D = 1$	7	1	“Different” $D = 1$	4	4
“Same” $D = 0$	1	7	“Same” $D = 0$	4	4

*a - Incentivized Treatment**b - Flat Treatment*Table 2.3: Experiment 2 - Payoff matrix (payoff of a decision, D , depending on the state of nature, Θ).

2.3.3 Feedback

To help subjects in their decision-making process, we provided them with two types of feedback. We aimed to not influence subjects to consider either more the stimulus or the payoff matrix. Thus, we gave them feedback on the stimulus - if their decisions were correct or incorrect - and feedback on the payoff matrix - payoff earned for the decision. In addition to trial-by-trial feedback, subjects received every 10 decisions two types of aggregated feedback. First, we showed their success rate for the last 10 decisions. Second, we provided aggregated payoff feedback indicating how close they were from optimality in terms of payoffs during the last 10 decisions.¹³ Our aggregated payoff feedback is based on the additional payoff earned for a correct answer to reflect the performance of subjects without being influenced by any endowment effect. Feedback effects have been highlighted in perceptual tasks using for example objective, optimal or delayed feedback (Maddox and Bohil, 2001; Maddox et al., 2003a; Ell et al., 2009). Including accuracy feedback was important to limit the formation of biased beliefs on performances. The optimality feedback motivation is two-fold. First, it balances accuracy and optimality regarding feedback. Second, as experiments are based on payoff manipulations, it was important to ensure that subjects were assessing correctly the impact of their decisions. Optimality feedback works as a recall on the payoffs implication of each decision.

¹³The aggregated payoff feedback (apf) is given by:

$$\forall(x, y, z) \in \mathbb{R}^3, \text{ apf}(x, y, z) = \begin{cases} \frac{x-y}{z-y} & \text{if } z > y \\ 1 & \text{if } z = y \end{cases} \quad (2.7)$$

With x actual payoffs for the last 10 decisions, y payoffs if all answers would have been incorrect, z payoffs if all answers would have been correct.

2.3.4 Preference elicitation: attitudes toward risk and ambiguity in the gain and loss domains.

We use the risk and ambiguity preferences elicitation task of Dai et al. (2017) which is based on Eckel and Grossman (2008) method with the presentation proposed by Eckel et al. (2012). This mechanism has the advantage of being cognitively simple for subjects and giving less noisy estimates than more complex measures (Dave et al., 2010). The preferences elicitation task was composed of four successive lottery decisions. Subjects had to select a two-outcome lottery out of six possibilities. Lottery sets differed from one another regarding the likeliness of each outcome and the fact that outcomes can induce a loss. For “Decisions 1” and “Decision 2” (see Chapter A of the Appendix, Figure 1 - a and b), each outcome was equally likely — *decisions under risk*. For “Decisions 3” and “Decision 4” (Chapter A of the Appendix, Figure 1 - c and d), the probability of each outcome varied between 30% and 70% — *decisions under ambiguity*. For Decisions 1 and 3, losses were impossible - *decisions under gain* — while some outcomes resulted in losses for Decisions 2 and 4 — *decisions under loss*. Lotteries were organized as follows: moving clockwise the expected payoff and its variance increased, exception made of the last lottery (A) for which only the variance of the payoff was higher compared to the previous one (F) (cf. Chapter A of the Appendix, Figure 1 for the visual design). Lotteries of decisions under loss are obtained after subtracting a fixed amount from lotteries of decisions under gain. This amount is allocated to subjects as an endowment for decisions under loss.

2.3.5 Experimental procedures

All sessions of both experiments were conducted at Queensland University of Technology (QUT) in September 2016. Subjects were students from QUT and were enrolled using ORSEE (Greiner, 2015). Experiments have been programmed using MATLAB with the Psychophysics Toolbox version 3 (Brainard, 1997) and have been achieved on computers of resolution 1920×1080. Concerning the main task, 10%¹⁴ of subjects’ decisions were converted into payments at a rate of 8 *points* = AUD 1.¹⁵ In Experiment 1 where the preference task was present, one decision was randomly selected and adds up to total

¹⁴There are 20 paid decisions out of 200 in Experiment 1 and 10 paid decisions out of 100 in Experiment 2. The number of decisions paid per block was controlled to be equal.

¹⁵Payoffs were referred to as “points” during both studies.

earnings (2 *ECU* = *AUD* 1).

The first experiment was 90 minutes long and paid on average *AUD* 27.0. The second experiment was half as long and paid on average 10.5 *AUD*. In total 84 subjects participated in these studies across 3 sessions per experiment. The 41 subjects involved in Experiment 1 were on average 22.5 years old, 56% of them were males. The 43 subjects involved in Experiment 2 were on average 23.2 years old, 63% of them were males.

2.4 Methodology and results

We present the methodology used to analyze experimental data in Subsection 2.4.1. For both studies, we combine SDT-based analyzes (Subsection 2.4.2.2, Subsection 2.4.3.2) and SDT-free analyzes (Subsection 2.4.2.1, Subsection 2.4.3.1) to provide an extensive discussion of our hypotheses set.

2.4.1 Methodology

SDT-based estimations and model predictions - Empirically, SDT allow us to compute estimators of the decision criterion and the discrimination based on the observer's percentage of hits (hit rate) and percentage of false alarms (false alarm rate). For n decisions, divided in n_s signal trials and n_n noise trials, the hit rate (HR) and the false alarm rate (FAR) are computed using the number of times subjects answer signal in both types of trials:

$$HR = \frac{\sum_{i=1}^{n_s} \mathbb{1}(D_i = 1)}{n_s} \text{ and } FAR = \frac{\sum_{i=1}^{n_n} (\mathbb{1}(D_i = 1))}{n_n} \quad (2.8)$$

The probabilities of hit (P_H) and false alarm (P_{FA}) are given by (proof in Chapter A A of the Appendix.):

$$P_H = \mathbb{P}(D = 1 | \Theta = 1) = 1 - \phi\left(\frac{\log(\beta)}{d'} - d'/2\right) \quad (2.9)$$

$$P_{FA} = \mathbb{P}(D = 1 | \Theta = 0) = 1 - \phi\left(\frac{\log(\beta)}{d'} + d'/2\right) \quad (2.10)$$

Hit or false alarm rates equal to 0 or 1 prevent robust estimations. Following the

recommendations from Brown and White (2005), we use the *log – linear* correction proposed by Hautus (1995) for estimations:

$$HR_C = \frac{0.5 + \sum_{i=1}^{n_s} \mathbf{1}(D_i = 1)}{n_s + 1} \quad \text{and} \quad FAR_C = \frac{0.5 + \sum_{i=1}^{n_n} (\mathbf{1}(D_i = 1))}{n_n + 1} \quad (2.11)$$

With HR_C the corrected hit rate and FAR_C the corrected false alarm rate.

By finding the couple of parameters $(\hat{\beta}, \hat{d}')$, the most likely to have produced the actual HR_C and FAR_C , we are able to predict SDT parameters based on observed data (left side of Figure 2.3).

We are interested in the effects of decision strategies' changes for an exact same level of perceptual abilities. We thus derive model predictions by replacing the decision strategy by either the optimal decision strategy or the accuracy maximizing decision strategy and by keeping constant the discrimination term. We use a two-step approach. We first estimate $(\hat{\beta}, \hat{d}')$ then replace $\hat{\beta}$ by their theoretical values from optimal or accurate behaviors and compute the couple $(\hat{P}_H, \hat{P}_{FA})$ (right side of Figure 2.3). Based on \hat{P}_H and \hat{P}_{FA} , we are able to derive the predicted probability of success (success rate; \hat{R}_S) and the expected payoff per trial (\hat{E}_P). Total payoffs for a series of trials are obtained by multiplying \hat{E}_P by the number of trials.

$$\hat{R}_S = \frac{\hat{P}_H + (1 - \hat{P}_{FA})}{2} \quad (2.12)$$

$$\begin{aligned} \hat{E}_P &= \hat{P}_H \times \Pi(hit) + (1 - \hat{P}_H) \times \Pi(miss) + (1 - \hat{P}_{FA}) \times \Pi(CR) + \hat{P}_{FA} \times \Pi(FA) \\ &= \hat{P}_H[\Pi(hit) - \Pi(miss)] + \hat{P}_{FA}[\Pi(FA) - \Pi(CR)] + \Pi(miss) + \Pi(CR) \end{aligned} \quad (2.13)$$

with Π the payoff associated with each outcome, FA the false alarm and CR the correct rejection.

We predict behaviors for the two opposed decision strategies involved in the optimality-accuracy trade-off: β^* , the optimal strategy and β^a , the accuracy-maximizing strategy (Subsection 2.2.1). For the symmetric treatment $\beta^* = 1$ and for asymmetric treatments $\beta^* = \frac{1}{3}$. For all treatments $\beta^a = 1$.

Estimations of attitudes toward risk and ambiguity in the gain and loss domains - We derive two types of estimations at a subject-level: model-free estimations based on

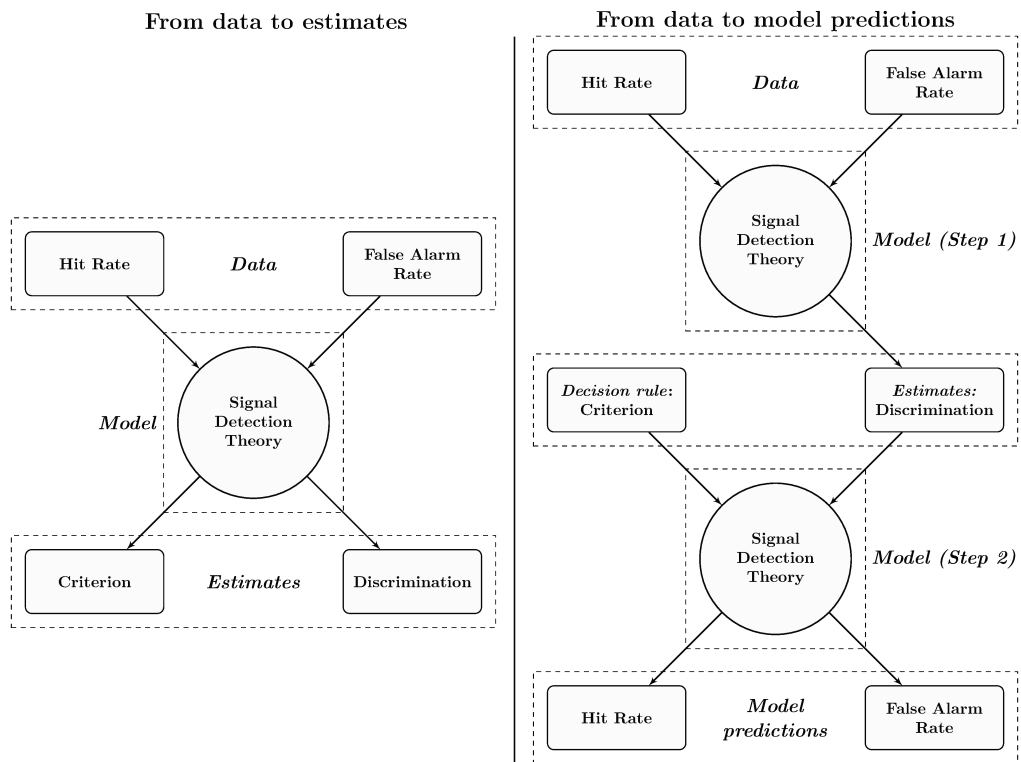


Figure 2.3: Discrimination and criterion estimations based on SDT modelisation.

two-by-two comparisons of decisions and model-based estimations based on the decision in gain under risk.

Ambiguity attitudes are defined by comparison to risk attitudes. Thus we compare the lotteries in gain under ambiguity and under risk to evaluate the ambiguity attitude under gain (Decision 3 and Decision 1). If subjects accept a lower level of ambiguity under gain (Decision 3) than the level of risk under gain (Decision 2), they are “ambiguity averse under gain”. If they accept a higher level of ambiguity under gain than the level of risk under gain, they are “ambiguity seeker under gain”. Subjects giving the same answer are “ambiguity neutral under gain”. Similarly, we evaluate the ambiguity attitude under loss by comparing decisions under loss (Decision 2 and 4). We also estimate if individuals are more risk averse and more ambiguity averse under gain compared to loss. We compare risky lotteries with and without losses to evaluate domain-dependant risk attitude (Decision 2 and Decision 1). If subjects accept a higher level of risk in the gain frame (Decision 1) than in loss frame (Decision 2), they are “more risk averse under loss”. If they accept a higher level of risk in the loss frame, they are “less risk averse under loss”. Subjects giving the same answer are “equally risk averse under loss”. The same method is used to evaluate domain-dependant ambiguity attitude (Decision 3 and 4).¹⁶

Model-based estimations consist of the estimation of constant relative risk aversion (CRRA) parameter intervals based on the decision in gain under risk. This specification of risk attitude has been chosen in compliance with influential experimental literature in risk preferences estimation (Andersen et al., 2008; Wakker, 2008; Dohmen et al., 2011). The CRRA utility function can be expressed as:

$$\forall x \in \mathbb{R}^+, U(x) = \begin{cases} \frac{x^{(1-\gamma_G)}}{1-\gamma_G} & \text{if } \gamma_G \neq 1 \\ \ln(x) & \text{if } \gamma_G = 1 \end{cases} \quad (2.14)$$

$$\forall x \in \mathbb{R}^-, U(x) = \begin{cases} -\lambda \frac{(-x)^{(1-\gamma_L)}}{1-\gamma_L} & \text{if } \gamma_L \neq 1 \\ -\lambda \ln(-x) & \text{if } \gamma_L = 1 \end{cases} \quad (2.15)$$

¹⁶Following the insights from Prospect Theory (Kahneman and Tversky, 1979), individuals are expected to be risk (or ambiguity) averse in the gain domain and risk (or ambiguity) seekers in the loss domain for probabilities around 0.5. We thus expect individuals to be less risk (or ambiguity) averse under loss compared to gain. However, we find that 43.9% of the subjects are more risk averse under loss and 58.5% of them are more ambiguity averse under loss. It might be explained by some framing effects.

with x the payoff, γ_G and γ_L the coefficients of risk aversion in the gain and the loss domains and λ the coefficient of loss aversion.

Using the decision in gain under risk (Decision 1), we estimate a parameter interval for each decision (Table 2.4).¹⁷ From left to right, individuals are less risk averse implying a decrease in their CRRA parameters.

Lottery	B	C	D	E	F	A
<i>Low Payoff</i>	7	6	5	4	3	0.5
<i>High Payoff</i>	7	9	11	13	15	17.5
$\gamma_G \in$]3.45 ; ∞ []1.16 ; 3.45[]0.7 ; 1.16[]0.49 ; 0.7[]0 ; 0.49[]- ∞ ; 0[
N (%)	2.4	26.8	24.3	4.9	17	24.6

Table 2.4: CRRA parameter (γ_G) intervals for the decision in gain under risk (Decision 1).

2.4.2 Results - Experiment 1

To investigate Hypothesis 1.1 to Hypothesis 1.4, Experiment 1 results are first assessed without using the SDT before estimating the quality of SDT predictions at a subjects-treatments level and finally at a decision level. SDT-free analyses are mainly dedicated to evaluating the SDT-free Hypothesis 1.4 on decisions under loss.

2.4.2.1 SDT-free analysis at a decision level

Table 2.5 presents the estimation results¹⁸ (cf. Chapter A Table 1 of the Appendix for descriptive statistics).

Dependent variables - Four variables are relevant for analyzing subjects' behaviors. The variable "success" is a dummy variable equal to 1 for correct decisions. Subjects have a 67.3% success rate validating the pre-task calibration (targeted value 71%). The variable "hit" is equal to success for signal trials while "false alarm" is a dummy variable equal to 1 for errors in noise trials. The last variable of interest is the response time. We measure the response time as the time between the beginning of the stimulus display (lasting 900 ms) and the decision.

¹⁷See Chapter A D.1 of the Appendix for details.

¹⁸Effects are estimated with and without control variables.

Independent variables - We use three different types of variables. For each treatment, we add an indicative variable (Symmetric, Low Gain, High Gain, and Loss) to estimate the impact of treatments on behavior. The second set of variables is given by individual characteristics (gender, age, and attitudes toward risk, loss, and ambiguity). Finally, we control for trials number within each treatment.

Results: weak supports for Hypothesis 1.1 and Hypothesis 1.2 - The treatment variable is relevant in explaining the four dependent variables (Fisher test; p-value: hit = 0.081, false alarm = 0.032, success = 0.086 and reaction times < 0.001). As predicted by Hypothesis 1.1 and Hypothesis 1.2, successes are positively affected by the Symmetric Treatment. Interestingly the effect is driven by the false alarm rate but not by the hit rate. The Low Gain Treatment is associated with more successes however the hit rate drives this effect. This low gain framework may involve less pressure on subjects as it does not involve losses and payoffs for successes are lowest than in the high gain framework.

Result: no support for Hypothesis 1.4 - The Loss Treatment has no influence on the decision outcomes (Fisher test; p-values: hit = 0.345, false alarm = 0.756 and success = 0.695).¹⁹ However in the Loss Treatment, subjects took more time to make their decisions (Fisher test, p-value = 0.006). It is in line with previous research that has suggested that polarity and numerical magnitudes of negative numbers are represented separately and thus associated with higher response times (Tzelgov et al., 2009; Blair et al., 2012).

Additional results - Analyzing attitudes toward risk and ambiguity, we note that only domain-specific ambiguity attitude explains performances. Subjects exhibiting more ambiguity aversion under loss have a lower false alarm rate and a higher success rate while subjects exhibiting less ambiguity aversion under loss have a lower hit rate. These better performances have been achieved as a result of higher response times.

It raises the question: does domain-specific ambiguity attitude induce different behaviors in the Loss Treatment? The cross effect of the Loss Treatment and domain-specific ambiguity attitude has a significant influence on the false alarms (Fisher test, p-value = 0.038), successes (Fisher test, p-value = 0.069) and response times (Fisher test, p-value = 0.029). Being more ambiguity averse under loss increases response times more in the Loss Treatment. Being less ambiguity averse under loss decreases the success

¹⁹This result is robust when reducing the analysis to only asymmetric treatments.

rate more in this treatment (Chapter A of the Appendix, Table 3). We conclude that when subjects are more ambiguity averse under loss and confronted to loss, ambiguity or both, they invest more cognitive resources in the task and also achieve higher performances. The fact that domain-specific attitudes toward ambiguity are significant while domain-specific attitudes toward risk are not is in line with the empirical differentiation of attitudes toward risk and ambiguity (Cohen et al., 2011). Finally, we do not observe a learning effect during the experiment as the trial number does not affect decisions. The significant negative trend on response times could be explained by the additional time needed by subjects to get used to the new payoff matrix in the first trials of each treatment.

2.4.2.2 SDT-based analysis

This subsection studies the three SDT-based hypotheses: Hypothesis 1.1, Hypothesis 1.2 and Hypothesis 1.3. To conclude, we oppose asymmetric treatments (trade-off existence) to the symmetric treatment (trade-off absence). We first provide an analysis of the distribution of SDT estimates, then analyze SDT predictions at a subject-treatment level and at a decision level.

SDT estimates

Level of analysis and SDT specification - The strength of SDT relies on the ability to disentangle the impact of the task difficulty from the decision rules. In theory, the task difficulty is the same for all treatments. Empirically, effort, learning, tiredness or some other random components of the decision process may induce differences in the subjects' capacity to discriminate. As the estimation of criteria relies on estimations of discrimination, if variations in discrimination are not totally random it may affect the quality of the criteria's estimations. In order to control for these variations, we compute criteria and discrimination for each subject and each treatment. Parameters are estimated by maximum-likelihood using the Gaussian specification of SDT and constraining parameters on their definition set ($d' \in \mathbb{R}^+$). This methodology is reproduced in all SDT-based analyses.

Results - Discriminations and criteria medians are lower for asymmetric treatments (Figure 2.4). However, we are unable to reject the hypothesis of equality between asymmetric and symmetric treatments for discriminations (p-value = 0.813) and criteria (p-

	<i>Dependent variable:</i>							
	Hit (1)	(2)	False Alarm (3)	(4)	Success (5)	(6)	Response time (7)	(8)
Treatments								
Symmetric	0.00001 (0.103)	0.0001 (0.103)	-0.279*** (0.095)	-0.280*** (0.095)	0.144** (0.068)	0.144** (0.068)	0.068* (0.038)	0.068* (0.038)
Low Gain	0.221** (0.105)	0.221** (0.105)	-0.115 (0.094)	-0.115 (0.094)	0.154** (0.068)	0.154** (0.068)	0.002 (0.038)	0.002 (0.038)
Loss	0.152 (0.104)	0.153 (0.104)	-0.106 (0.094)	-0.106 (0.094)	0.121* (0.068)	0.121* (0.068)	0.147*** (0.038)	0.147*** (0.038)
Individual Characteristics								
Loss Averse Under Risk		-0.015 (0.256)		0.021 (0.228)		-0.024 (0.199)		0.053 (0.167)
Loss Seeker Under Risk		0.266 (0.319)		0.421 (0.283)		-0.106 (0.246)		0.184 (0.206)
Loss Averse Under Ambiguity		0.409 (0.299)		-0.578** (0.266)		0.502** (0.232)		0.258 (0.194)
Loss Seeker Under Ambiguity		-0.553 (0.383)		0.173 (0.344)		-0.313 (0.299)		0.315 (0.252)
Ambiguity Averse Under Gain		-0.128 (0.297)		0.084 (0.266)		-0.084 (0.231)		-0.274 (0.194)
Ambiguity Seeker Under Gain		-0.212 (0.292)		-0.064 (0.260)		-0.057 (0.226)		-0.267 (0.189)
Ambiguity Averse Under Loss		0.117 (0.253)		-0.219 (0.226)		0.165 (0.196)		0.175 (0.165)
Ambiguity Seeker Under Loss		0.430 (0.319)		-0.072 (0.280)		0.214 (0.244)		-0.039 (0.204)
Male		0.603*** (0.215)		-0.018 (0.192)		0.291* (0.167)		-0.101 (0.139)
Age		-0.058*** (0.021)		0.018 (0.019)		-0.036** (0.017)		0.006 (0.014)
Other								
Trial Number		-0.001 (0.003)		0.0005 (0.002)		0.001 (0.002)		-0.004*** (0.001)
Constant	1.067*** (0.140)	1.885*** (0.562)	-0.364*** (0.121)	-0.465 (0.503)	0.680*** (0.101)	1.049** (0.435)	1.547*** (0.066)	1.475*** (0.364)
Observations	4,100	4,100	4,100	4,100	8,200	8,200	8,200	8,200
BIC	4,507	4,580	5,248	5,323	10,025	10,107	26,563	26,666

Note:

*p<0.1; **p<0.05; ***p<0.01

Table 2.5: Experiment 1 - SDT-free impacts of treatments, individual characteristics and trial number.

Reading note: Regression - random-effect models logistic (1,2,3,4,5,6) and Gaussian (7,8). Reference groups - High Gain for treatment, "neutral preferences" for preferences variables.

value = 0.402).²⁰

The direct analysis of criteria and discriminations does not allow us to conclude on Hypothesis 1.1, Hypothesis 1.2 or Hypothesis 1.3. This lack of direct results may come from different factors. First, this approach is conservative as by aggregating information

²⁰Throughout the paper, statistical tests used to compared two samples are two-sided Wilcoxon signed rank tests unless specified otherwise.

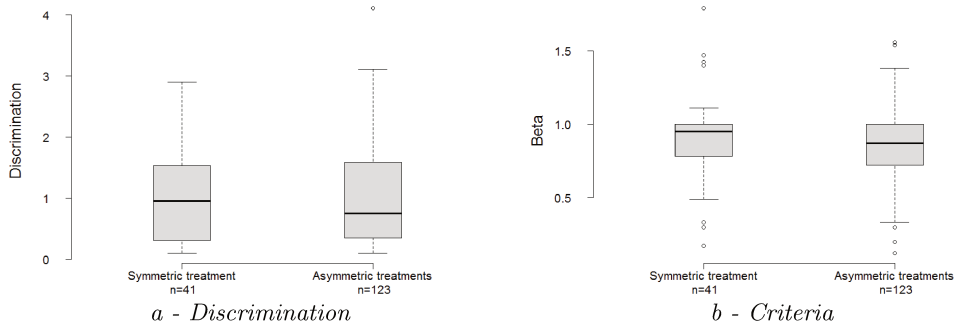


Figure 2.4: Experiment 1 - Estimated discriminations and criteria.

Reading note: (a) - distribution of the estimated discrimination (a) and β criteria (b) for symmetric and asymmetric treatments. Interquartile ranges are represented by the horizontal lines.

we decrease substantially our statistical power. Second, we do not use the full potential of SDT as we do not take into account its predictive power concerning accuracy maximization or optimality. To better understand the optimality-accuracy trade-off, we provide a comparison of SDT predictions and observed data.

Comparison to SDT predictions at a subject-treatment level

Level of analysis and specification - We first compute predictions of success rates and payoffs for an accuracy-maximizing and an optimal decision strategy at a subject-treatment level. We then compute mean ratios between these predictions and observed data. In addition, we tackle the question of a potential efficiency loss due to the optimality-accuracy trade-off. Results are summarized in Figure 2.5.

Result: support for Hypothesis 1.1 - Facing the optimality-accuracy trade-off, the observed success rate is 7.9% higher than the optimal success rate (p-value < 0.001). It induces a payoff decrease of 11.8% (p-value < 0.001).²¹ Opposing the deviations from predictions in symmetric and asymmetric treatments we observe that subjects are closer to optimality in the Symmetric Treatment for both the success rate and payoffs (p-values < 0.001). We thus conclude that, compared to the optimal behavior, subjects are over-accurate leading to an efficiency loss.

Result: support for Hypothesis 1.2 - For asymmetric payoff matrices, the observed success rate is 1.8% lower than the accuracy-maximizing success rate (p-values < 0.001).

²¹To control for the differences in performance-based payoffs from one treatment to another, we scale the payoffs by subtracting the minimal payoff (i.e., payoff for errors) from the actual payoff.

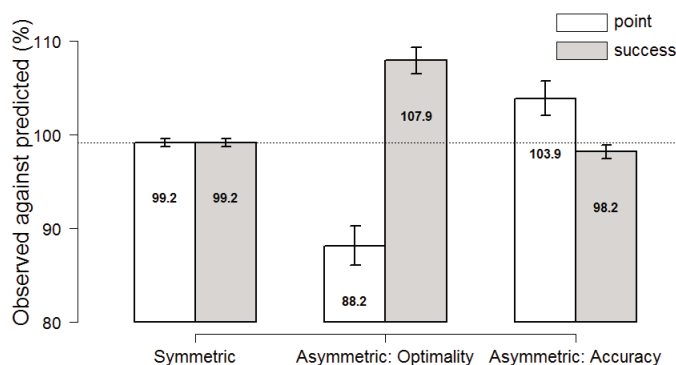


Figure 2.5: Deviations from accuracy-maximizing and optimal decision strategies.

Reading note: the mean ratio between observed and predicted decisions for the symmetric treatment and the asymmetric treatments are represented. White bars provide points comparisons and grey bars provide success rate comparisons. 95% confidence intervals are represented by the horizontal lines. By construction, deviations expressed in term of points and success rate are equal for the Symmetric Treatment.

This deviation from the accuracy-maximizing decision strategy induces a 3.9% increase of subjects' payoffs (p-values < 0.001). Again subjects are closer to predictions for payoffs when there is no trade-off (p-value < 0.001). Concerning success rates the difference is not significant (p-value = 0.156). These analyses globally support that subjects deviate from the accuracy-maximizing strategy toward the optimal strategy.

Result: support for Hypothesis 1.3 - Subjects are neither pure accuracy-maximizer nor optimal, however from which behavior do they differ the less? We observe that deviations from accuracy maximization are weaker than the deviations from optimality both in term of success rate and payoffs (p-values < 0.001).

Comparison to SDT predictions at a decision level

We assess if previous findings are reproduced at a decision level. We also investigate if SDT predictions are relevant in explaining data. Table 2.6 presents results for the four predicted variables based on the accuracy-maximizing criterion and the optimal criterion.²²

Level of analysis and specification - At a decision level, we test the predicted variables against the observed data including a dummy variable equal to one for asymmetric

²²All previous analysis are robust when adding controls in the regressions (SI Table 5)

treatments and a constant (c).

$$y_{obs} \sim y_{pred} + \mathbb{1}_{\{asymmetric\ treatment\}} + c \quad (2.16)$$

The dummy variable tells us whether there is an overestimation or an underestimation of the dependent variable for asymmetric treatments.²³ The significance of the prediction term assesses the relevance of SDT predictions.

Result: support for Hypothesis 1.1 - For asymmetric treatments, optimality-based predictions overestimate the hit rate, the false alarm rate, and payoffs. They underestimate the success rate. This over-accuracy ultimately results in sub-optimality.

Result: support for Hypothesis 1.2 - Accuracy-based predictions underestimate the number of hits, false alarms, and payoffs for asymmetric treatments. It means that subjects are not pure-accuracy maximizers; they are inclined to sacrifice some accuracy for the low stakes trials to increase their accuracy for high stakes trials.

Result: support for Hypothesis 1.3 - By comparing BIC between both predictions rules, we notice that accuracy-based predictions systematically give a better fit to observed data. Thus data reveal an actual criterion closer to the accuracy-maximizing one rather than the optimal one.

Additional result - Each prediction has a positive impact on its corresponding predicted variable at a 1% test level. Thus predictions are relevant in predicting observed data.

2.4.2.3 Attitudes toward risk

The utility-maximizing criterion - We defined optimality as the strategy aiming to maximize the expected value of the decision. However, individuals may evaluate the payoff matrices in terms of expected utility instead. As our design involves equal base-rates, there is no effect of weighted probabilities. Likewise, as our asymmetrical rewards are in gains or losses but not in mixed outcomes, we do not have an effect of loss aversion on the optimal criterion. Thus, all changes will come from the shapes of the utility functions under gains and losses. Based on a CRRA utility function, it is possible to

²³Including a second dummy variable equal to one for the Loss Treatment enables us to test if the rejection of Hypothesis 1.4 is confirmed by SDT-based analysis. For the eight models estimated this dummy variable has no impact on the dependent variable (Chapter A of the Appendix, Table 4). It confirms previous findings.

	<i>Dependent variable:</i>							
	Hit (1)	(2)	False Alarm (3)	(4)	Success (5)	(6)	Payoff (7)	(8)
Hit Optimal	1.303** (0.651)							
Hit Accuracy		5.472*** (0.478)						
False Alarm Optimal			2.283*** (0.198)					
False Alarm Accuracy				5.368*** (0.416)				
Success Optimal					4.960*** (0.212)			
Success Accuracy						5.216*** (0.217)		
Payoff Optimal							0.995*** (0.005)	
Payoff Accuracy								0.999*** (0.005)
Asymmetric	-0.283 (0.221)	0.184** (0.085)	-0.766*** (0.116)	0.178** (0.079)	0.284*** (0.059)	-0.013 (0.057)	-0.498*** (0.091)	0.185** (0.090)
Constant	0.202 (0.452)	-2.697*** (0.336)	-1.339*** (0.109)	-2.342*** (0.160)	-2.585*** (0.149)	-2.754*** (0.151)	-0.009 (0.089)	-0.029 (0.082)
Observations	4,100	4,100	4,100	4,100	8,200	8,200	8,200	8,200
BIC	4,499	4,399	5,125	5,104	9,758	9,741	44,124	44,052

Technical note: *p<0.1; **p<0.05; ***p<0.01

Table 2.6: Experiment 1 - Impacts of SDT predictions on observed behaviors.

Reading note: for each dependent variable, we present the causal effect of the corresponding predicted variable based on either optimal decision (odd numbers) or accuracy-maximizing (even numbers) strategies. Regressions estimate random-effect models logistic (1,2,3,4,5,6) and Gaussian (7,8).

define another criterion than the standard optimal criterion: the SDT criterion maximizing expected utility. This criterion is obtained like the optimal decision criterion (cf. Equation 2.4) with the difference that the utility of payoffs replaces their actual values. It is thus defined by:

$$\beta_U^* = \frac{U(\Pi(\text{correct rejection})) - U(\Pi(\text{false alarm}))}{U(\Pi(\text{hit})) - U(\Pi(\text{miss}))} \quad (2.17)$$

with U the CRRA utility function (cf. Equation 2.14 and Equation 2.15).

Assuming risk aversion in the gain domain, the new utility-maximizing criterion will be higher than the optimal criterion for positive asymmetrical rewards. On the contrary, assuming risk seeking in the loss domain, the utility-maximizing criterion will be lower than the optimal one for the negative payoffs. we assess the robustness of Hypothesis 1.1 and Hypothesis 1.3 using utility-maximizing decision criteria based on

individual estimations of risk preferences.²⁴

Estimation - The estimated β_U^* have a mean of 0.530 (*s.d.* = 0.280) for the Low Gain Treatment, a mean of 0.386 (*s.d.* = 0.086) for the High Gain Treatment and a mean of 0.234 (*s.d.* = 0.185) for the Loss Treatment. By definition, utility-maximizing criteria are equal to the optimal criterion for the Symmetric Treatment ($\beta_U^* = 1$).

Result: Hypothesis 1.1 is also supported for the utility-maximizing decision criterion - SDT predictions at a subject-treatment level highlight that the observed success rate is 8.8% higher than the utility-maximizing success rate (*p-value* < 0.001) for asymmetric treatments. It implies a payoff decrease of 10.30% (*p-value* = 0.011). SDT predictions at a decision level reveal that utility-maximizing predictions also lead subjects to overestimate the hit rate, the false alarm rate, and the payoffs while underestimating the success rate (Chapter A of the Appendix, Table 6).

Result: Hypothesis 1.3 is also supported for the utility-maximizing decision criterion - Deviations from accuracy maximization are lower than the deviations from utility maximization both in term of success rate and payoffs (*p-values*: success < 0.001 and payoff < 0.001).

Additional result - By comparing BIC of Chapter A Table 6 of the Appendix and Table 2.6, we notice that utility-maximizing predictions have a lower predictive power than optimality based predictions. It gives support to the payoff-maximization approach.

2.4.3 Results - Experiment 2

Experiment 2 tackles the question summarized in Hypothesis 2.1: does the value of being right contribute to the observed optimality-accuracy trade-off? The answers lie in the difference of behavior between the Incentivized Treatment and the Flat Treatment. If subjects are pure-payoff maximizers, their decisions should approach random guesses in the Flat Treatment. Otherwise, they should use comparable decision strategies in both treatments. In line with the progression undertaken to investigate Experiment 1 results, Experiment 2 results are first assessed on a SDT-free base before estimating the quality of SDT predictions at a subject-treatment level and finally at a decision level.

²⁴In Chapter A D.2 of the appendix, we detail our methodology of estimation. In addition, we show in Chapter A of the Appendix, D.3 that our results will hold using highly conservative estimations of utility-maximizing criteria.

2.4.3.1 SDT-free analysis at a decision level

Level of analysis and specification - We regress the treatment and controls on hits, false alarms, successes and response times. Control variables are the trial number, the gender and the age of subjects.²⁵ Results are given in Table 2.7, additional descriptive statistics are to be found in Chapter A of the Appendix, Table 2.

Result: subjects outperform random guesses but still decrease their success rate in the Flat Treatment - In the Flat Treatment subjects' success rates decrease. This result is driven both by a decreasing hit rate and an increasing false alarm rate. On average, the success rate decreases from 68.4% to 63.8% (p-value = 0.001). We reject the hypothesis that the true mean success rate in the Flat Treatment is equal to 50% at all common test levels (binomial-test, p-value < 0.001).

Result: response times decrease in the Flat Treatment - Individuals make their decisions more quickly in the Flat Treatment compared to the Incentivized Treatment (p-value = 0.007). It confirms that they are sensitive to change in payoff matrices. In addition, as in Experiment 1 individuals' response times decrease along treatments.

Conclusion: support for Hypothesis 2.1 - Subjects have on average outperformed random guesses. Thus we cannot find evidence for a pure-payoff maximization. However, their accuracy has decreased from the Incentivized Treatment to the Flat Treatment. It shows that when the monetary reward is withdrawn from the value of being right, this value is not strong enough to induce, by itself, the same level of cognitive investment. The analysis of response times confirms it. Based on these considerations, it is not possible to reject Hypothesis 2.1 as subjects are involved in an accuracy-seeking behavior in the Flat Treatment.

2.4.3.2 SDT-based analysis

SDT-based analysis at a cross subject-treatment level

Level of analysis and specification - Discrimination and decision parameters are computed at a cross subject-treatment level. If subjects were only maximizing rewards, their discrimination should be close to null, and their decision criteria should approach a random distribution.

Results: decision criteria are not different between treatments and do not reflect

²⁵Two subjects entered an inconsistent age (< 3), we assigned them the mean age of all other subjects.

	<i>Dependent variable:</i>							
	Hit (1)	(2)	False Alarm (3)	(4)	Success (5)	(6)	Response Time (7)	(8)
Treatments								
Flat Treatment	-0.215** (0.098)	-0.215** (0.098)	0.231** (0.092)	0.231** (0.092)	-0.218*** (0.066)	-0.218*** (0.066)	-0.218*** (0.022)	-0.218*** (0.022)
Controls								
Trial number		-0.004 (0.004)		-0.001 (0.003)		-0.0001 (0.002)		-0.005*** (0.001)
Male		-0.211 (0.268)		-0.135 (0.173)		-0.022 (0.172)		0.013 (0.121)
Age		0.0001 (0.027)		-0.001 (0.017)		0.001 (0.017)		-0.009 (0.012)
Constant	1.001*** (0.137)	1.226* (0.699)	-0.694*** (0.094)	-0.571 (0.460)	0.816*** (0.087)	0.812* (0.449)	1.730*** (0.057)	2.064*** (0.314)
Observations	2,150	2,150	2,150	2,150	4,300	4,300	4,300	4,300
BIC	2,561	2,582	2,797	2,819	5,400	5,425.222	9,549	9,553
<i>Note:</i>						*p<0.1; **p<0.05; ***p<0.01		

Table 2.7: Experiment 2 - SDT-free impacts of the Flat Treatment, demographics and trial number.

random-guesses - We cannot reject the equality of criteria between treatments (p-value = 0.491). In the Flat Treatment we notice much more extreme values than in the Incentivized Treatment; however more values are gathered around the median (Figure 2.6 - b). Using a Levene's test for equality of variance (Levene, 1960), we accept the treatments variance equality hypothesis (p-value = 0.718). We test criteria distribution using Kolmogorov-Smirnov tests. First, we compared criteria from both treatments. We do not reject the hypothesis of equal distribution at all common test levels (p-value = 0.303). Then, we test criteria from the Flat Treatment against a uniform distribution of same range. We reject the hypothesis of equal distribution at 1% (p-value < 0.001).

Result: discriminations decrease in the Flat Treatment but stay above null discrimination - The discrimination is lower in the Flat Treatment (p-value < 0.001, Figure 2.6 - a). The number of subjects with a null discrimination increases by one from the Incentivized to the Flat Treatment (14% against 16% of \hat{d}').

Conclusion: support for Hypothesis 2.1 - Once again, SDT enables us to disentangle the decision strategy from the discrimination. We find no difference in decisions strategies between treatments and a non-random strategy for the Flat Treatment. We explain the decreasing discrimination in the Flat Treatment compared to the Incentivized Treatment by a decrease in the effort level. Subjects invested less cognitive resources when monetary rewards were suppressed. However, the value of being right has kept discrimi-

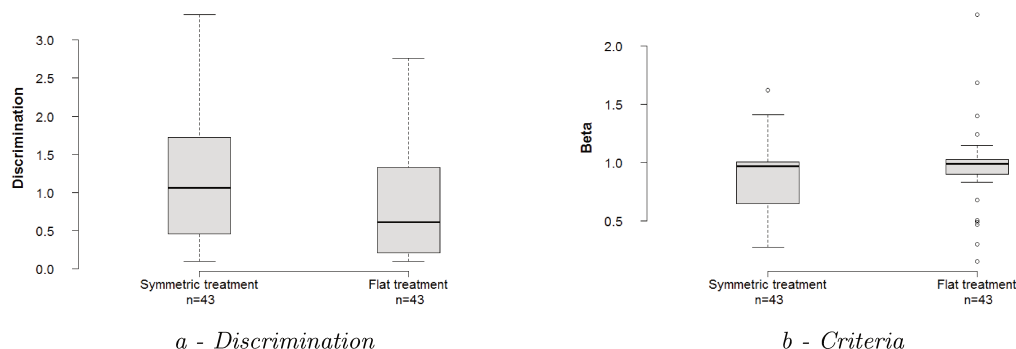


Figure 2.6: Experiment 2 - Estimated discrimination and criteria.

Reading note: Distribution of the estimated discrimination (a) and β criteria (b) for Incentivized and Flat Treatments. Interquartile ranges are represented by the horizontal lines.

nation above a random-guess level. To complete the spectrum of analysis, we assess the robustness of previous conclusions at a decision-level analysis.

SDT-based analysis at a decision level

Level of analysis and specification - We produce decision predictions based on estimated discrimination and theoretical β . In Experiment 2 only $\beta_a = 1$ can be used as reference.²⁶

Regressions of forecast variables against observed variables are given in Table 2.8.²⁷ We are mainly interested in studying to what extent the accuracy-maximizing criterion (β_a) is relevant for predicting behaviors in the Flat Treatment.

Result: accuracy-maximizing predictions explain behaviors in both treatments with a better fit for the Incentivized Treatment - We notice that all predictions are explaining actual behaviors. In addition, the Flat Treatment variable has no explanatory power. It means that the initial hypothesis made for predicting data ($\beta = \beta^a$) is relevant to explain data for treatments with and without incentives. Finally, we have computed the quality of fit using BIC by clustering data according to treatments. The Incentivized Treatment is better fitted than the Flat Treatment.²⁸

Conclusion: support for Hypothesis 2.1 - Data being explained by accuracy-maximization

²⁶Under the hypothesis of mixed valuation of optimality and accuracy subjects should use this criterion in both treatments. Under the hypothesis of pure-payoff maximization, it is impossible to compute a unique reference criterion as subjects are supposed to behave randomly.

²⁷Predictions are produced for hits, false alarms, and successes. Payoffs are not predicted as by definition observed and predicted data are the same for at least half of the sample.

²⁸All previously described results are robust to controls (Chapter A Table 7 of the appendix).

predictions reinforces previous findings on the role played by the value of being right in the optimality-accuracy trade-off. Higher deviations from predictions in the Flat Treatment are identified consistently with the decreasing level of effort in non-incentivized treatments.

	<i>Dependent variable:</i>		
	Hit (1)	False Alarm (2)	Success (3)
Predicted Hit	6.053*** (0.572)		
Predicted False Alarm		4.016*** (0.490)	
Predicted Success			4.843*** (0.306)
Flat Treatment	0.054 (0.101)	0.047 (0.094)	0.0005 (0.068)
Constant	-3.182*** (0.391)	-1.939*** (0.173)	-2.521*** (0.210)
Observations	2,150	2,150	4,300
BIC Flat	1,318	1,410	2,727
BIC Incentive	1,194	1,349	2,543
<i>Technical note:</i>	*p<0.1; **p<0.05; ***p<0.01		

Table 2.8: Experiment 2 - Impacts of SDT predictions on observed behavior.

Reading note: for each dependent variable, we present the causal effect of the corresponding predicted variable based on the accuracy-maximizing strategy. Regressions estimate logistic random-effect models. AIC correspond to regressions clustered per treatment.

2.5 Discussion and Conclusion

In most circumstances, seeking accuracy is relevant to achieving optimality. However, in a situation with different types of successes and errors rewarded asymmetrically, significant departures from accuracy must be achieved to approach optimality. As a consequence, an optimality-accuracy trade-off arises. Based on a multi-level analysis, the results of our first experiment confirms the existence of the optimality-accuracy trade-off using a signal-in-noise detection task. Actual behaviors exhibit a deviation from accuracy in the direction of optimality but do not approach the optimal solution. This tendency is so pronounced that we observe a leading role of accuracy in the trade-off.²⁹ In most of our analysis, we consider that the optimal decision strategy is the strategy maximizing

²⁹Subjects seem to be aware of this behavior. In a question at the end of Experiment 1, 80% of them report that they tried to maximize their success rates rather than their payoffs.

expected payoffs. In addition, we obtain that the optimality-accuracy trade-off is robust to a shift from expected payoff to expected utility by computing individual weighting functions based on CRRA utility. In that sense, we confirm part of Ackermann and Landy (2015) results; however our findings mitigate the importance of a utility-based approach. Individual preferences toward risk, ambiguity, and loss have a low explanatory power on observed behaviors, while we should observe important effects if the subjective utility was playing a key role in the decision process. Moreover, our design allows us to reject the impact of losses on the trade-off. It seems that in this perceptual task, individuals react less to reference-point manipulations than in preferences tasks (e.g., lotteries). This lack of effect is even more remarkable in that we do not implement a loss framing but real monetary losses in the experiment. This result, that extends previous findings based on a mixed gain-loss framework (Maddox et al., 2003b), goes against the relevance of applying the Prospect Theory framework to this context. The second experiment investigates to what extent the value of being right can explain the optimality-accuracy trade-off. Our approach, based on an original design enabling us to tackle this question directly, provides new insights and contrasts with previous results (Maddox and Bohil, 2003; Bohil et al., 2004). In line with the leading role of accuracy, our multi-level evaluation of the value of being right shows that this value plays a central role in the trade-off solution. When removing all monetary incentives, we obtain that a search for accuracy drives individuals.

The role of intrinsic motivations has been highlighted to play a significant role in individuals' decisions (Camerer and Hogarth, 1999; Gneezy and Rustichini, 2000; Benabou and Tirole, 2003). The importance of the value of being right gives evidence that the optimality-accuracy trade-off may find its foundations in an opposition between intrinsic and extrinsic motivations. Even if individuals are explicitly asked to maximize payoffs, they may intrinsically value to give accurate answers. Extrinsic incentives are not aligned with internal motivations for asymmetric incentives as they are maximal when accepting giving more false answers than necessary. This interpretation of the optimality-accuracy trade-off is in line with previous research obtaining that multiplying all payoffs by a constant and thus raising extrinsic incentives for a comparable level of intrinsic incentives leads to decisions closer to optimality (Bohil and Maddox, 2003). This internal valuation of success has been highlighted in other contexts. Thinking about how individuals behave in contests, this same mechanism of induced utility of success

has been obtained and referred to as the joy of winning (Price and Sheremeta, 2011; Mago et al., 2013). To extend such findings in an individual setting is interesting as this utility is not driven by competition and the seek for out-performing our counterparts but only by internal challenge and self-ego.

To obtain this result, we evaluate the role of intrinsic valuation in a flat payoff setting to cancel extrinsic motivations and thus isolate intrinsic motivations. However, one limitation is that extrinsic and intrinsic motivations may interact. Extrinsic motivation may crowd-out intrinsic motivations resulting in a decrease of effort or motivation when payoff incentives are implemented (Gneezy et al., 2011). We find no evidence of such a mechanism as, in our flat treatment, individuals' response time decreases compared to the Incentivized Treatment. It demonstrates that when suppressing external motivation, the effort level is not increasing. To go a step further in the evaluation of the value of being right, it is possible to oppose extrinsic and intrinsic valuations. For example, rewarding more errors than successes or allowing subjects to observe stimuli again at a cost would induce this opposition. If subjects continue to sacrifice payoffs for accuracy, even in such a salient framework, it will reinforce the importance of the value of being right. Another limitation is that we cannot exclude a potential experimenter demand effect (Zizzo, 2010). The search for accuracy might be linked to an internal pressure for subjects to perform a task in an experimental setting as it is commonly observed in non-incentivized psychology studies or surveys. While we do not have a control for this effect, we doubt that it drives our result. First, subjects performed the Incentivized Treatment and the Flat Treatment in the same session. It should decrease the pressure felt by subjects to perform the non-incentivized task by comparison of both treatments. We also argue that real-life situations are quite similar as they involve internal or external pressure to succeed even without rewards. Thus, we conclude in favor of our accuracy seeking hypothesis and that individuals intrinsically value the fact of being right.

From a methodological point of view, this paper argues in favor of the use of SDT to analyze our research question. This computational approach is widely used in cognitive sciences but still uncommon in economics. We think that understanding the optimality accuracy trade-off requires such a framework. Indeed, our study puts forward the three main aspects of SDT (Wickens, 2001). It provides a powerful tool to analyze the observed behaviors in terms of decision strategies and task properties. It allows for comparison between treatments to precisely identify the impacted aspects of the behaviors. Finally,

it defines what is optimality by a modelization of how an optimal individual should behave. Thus, we would have been unable to define the trade-off and to get insights of its main properties without the use of SDT. As what can be considered as a drawback, a SDT analysis implies the use of perceptual decisions. Instead, we believe that it can be a pertinent paradigm for further research in experimental economics. First, it allows for a control of the cognitive processes involved in the decision, while an economic task will always imply a lower control of what the subjects have really in mind. Second, we argue that our approach of SDT relies on both the perceptual and the economic components of a decision. The optimality accuracy trade-off may be based on the assessment of perceptual information (visual stimuli) and preferences (utility of payoffs). In that sense, more frequent use of perceptual tasks in experimental economics may allow a combined framework of perceptual and economic decision-making (Summerfield and Tsetsos, 2012; Lynn et al., 2015). Last, a perceptual task is not different from other tasks used in experimental economics in the sense that they all suffer from artificiality but are relevant for the theory tested, and necessary to observe and measure the outcomes of interest (Starmer, 1999). For all these reasons we hope that the use of computational models such as SDT will continue to develop in economic studies.

Identifying the optimality-accuracy trade-off and understanding its reasons, as this paper attempts, is a necessary prerequisite to change behaviors toward more optimality. Building on the findings of this chapter, [Chapter 3](#) thus propose a non-invasive method to try to correct behaviors toward more optimality in a framework similar. This method consists in manipulating the information order between payoff information and perceptual evidence.

Chapter 3

Manipulating Payoff Processing Order in Perceptual Decision-Making¹

3.1 Introduction

Perceptual decision-making is based on two types of information: perceptual evidence and valuation of various options. Combining these two sources of information leads to what we define as the optimal behavior: maximizing expected reward. However rewards are sometimes neglected by individuals: they base their decision mainly on the amount of perceptual evidence favoring one or the other alternative. Behavioral divergences between these two strategies depend on the structure of rewards. If reward differentials between successes and errors are constant across the possible options, maximizing accuracy or maximizing expected reward leads to the same behavior. However, introducing asymmetric rewards implies divergences in behavior. Maximizing accuracy will be no more optimal in terms of rewards and the need to process both sources of information becomes salient.

Since attaining accuracy is not necessarily equivalent to being optimal, individuals achieve optimality at the expense of accuracy. Indeed if some decisions lead to higher additional rewards than others, individuals have to concede lower accuracy for the low rewarded decisions compared to the highly rewarded ones. This ultimately results in a

¹Co-authored with Ismaël Rafai and Sébastien Massoni.

loss of accuracy to the benefit of maximal payoffs. As an example consider a physician facing a patient exhibiting ambiguous symptoms that may be related to either a serious disease or to a minor one. Correctly identifying that the patient suffers from the serious disease is associated with higher stakes than correctly classifying the disease as minor. To maximize her patient's welfare, the physician should sometimes diagnose the major disease even if the minor one is more likely.

Facing such asymmetrical decisions, a robust finding is that individuals fail to adjust their behaviors. They use a decision strategy leading to over-accurate but sub-optimal answers (e.g. Pitz and Downing, 1967; Maddox and Bohil, 1998; Bohil and Maddox, 2001 and Chapter 2). This inability to shift their behaviors away from accuracy-maximizing is referred as a conservative criterion placement.² Observing such conservatism in decision criteria placement shows that information about rewards is not enough weighted compared to perceptual evidence. In this case, how and when, both sources of information are conveyed may matter.

Experimental procedures are designed such as subjects almost always know the payoffs associated with the decisions before observing the stimuli. Within this sequence of information proceeding, we may assume an order effect between rewards information and perceptual evidence. If this is the case, conservative placement and order effect are confounded in the behavioral measurements. The purpose of this study is to investigate if and to what extent information order can explain conservative criterion placement. To tackle this question we use a two-alternative forced choice (2-AFC) visual task with asymmetrical payoffs and rely on a Signal Detection Theory (SDT) for the analytical framework. SDT (Green and Swets, 1966) allows us to understand perceptual decision-making by disentangling the impact of stimuli perception — the discriminability — from the impact of the decision strategy — the decision criterion. It also offers a normative benchmark to predict what will be an optimal behavior i.e. a perfect combination of perceptual evidence and rewards information. We apply this framework to a numerosity task in which subjects have to identify the largest magnitude in two visual stimuli. Decisions are associated with different payoff matrices linking the possible actions to asymmetric rewards. We vary payoff matrices and stimuli trial-by-trial while their presentation order is changed between blocks of trials. Note that even if presenting payoff

²This paper focuses on asymmetry induced by payoffs. Conservative criterion placement has also been highlighted for asymmetry in prior probabilities that each answer is correct (base-rate; Ell et al., 2009; Bohil and Wismer, 2015).

information first seems a natural way to proceed, nothing, from theoretical point of view nor evidence-based from real-life decision, gives a strong support for this assumption. SDT assumes that individuals decide on the basis of the likelihood of correctness and the payoffs of each decision. But it is agnostic on how these two types of information are proceed and on which one comes first. External validity does not impose such a sequence neither. To refine the example previously discussed, the order in which information is delivered reflects two possibilities available to the physician. The consequences of each treatment are established by a cost-benefit analysis based on the patient's health history. The likelihood of each disease is obtained after having examined the patient. The physician will examine the patient and investigate the consequences of each treatment but he is free to choose the order in which she performs these two tasks.

The existence of a potential order effect between rewards and perceptual information may find its roots in two distinct observed experimental facts: an established order effect on discriminability of sequential stimuli and evidence of a dual-decision process facing multiattribute choice alternatives. For 2-AFC discrimination tasks involving comparative judgments between two sequential stimuli, SDT postulates that the decision is taken only with respect to the internal magnitude difference between stimuli representation. While temporal order of stimuli presentation should not affect the decision, there are robust evidence contradicting this postulate. Indeed, when subjects are asked to compare the magnitude of a constant standard against the magnitude of a variable comparison, the discrimination sensitivity is higher when the standard precedes rather than follows the comparison (Dyjas and Ulrich, 2014). This so-called Type B effect goes against SDT neutrality of temporal information (Ulrich and Vorberg, 2009) and has been observed across multiple stimuli attributes and sensory modalities (Ellinghaus et al., 2018). Our study will investigate whether a similar order effect can be observed beyond pure perceptual comparisons in a situation where reward information interferes with perceptual evidence. Another approach to investigate a potential payoff order effect is to be found in a dynamic multiattribute choice approach. Breaking with the time-order neutrality, some multiattribute models show that the sources of information are processed separately in a dynamic way. Applying this method to the integration of payoffs and perceptual information, Diederich and Oswald (2014) propose a sequential sampling model that produces different predictions depending on the order in which attributes are considered. In a design related to ours, Diederich (2016) shows that this

modelization provides the best fit to observed behaviors. Even if this approach does not rely on SDT but on its dynamic extension, it provides experimental evidence in favor of a dynamic integration of payoffs and perceptual evidence in which the order of the processing affects the decision.

The current study assesses a payoff order effect on perceptual decision by inverting the order in which both types of information are given to subjects. By comparing discriminability and decision criteria placement for the different information orders, it contributes to understand how payoffs and perceptual evidence are processed over time. Our results show that individuals shift their decision criterion in the direction of optimality when payoff information is given last and we observe a trend suggesting a better discriminability when stimuli are displayed last. Although these effects confirm the investigated order effect, their impacts are insufficient to move away subjects from the conservative criterion placement. Overall our study highlights that the order of processing payoffs and perceptual information matters but human tendency to maximize accuracy over rewards is too strong for being vanished by such manipulation.

3.2 Method

Stimuli

We used 2-AFC stimuli previously used in Massoni et al. (2014) and Fleming et al. (2016). This consists of a 700ms long display of dots in two circles on either sides of the screen, one with 50 dots and the other with c more dots (Figure 3.1-A). Subjects evaluated which circle contained the most dots. c was calibrated at a subject level to control for heterogeneity in visual abilities and kept constant during the experiment.³

Task and Procedure

Participants — 58 students from Queensland University of Technology were enrolled in four 1-hour sessions (age: $M = 24.89$, $SD = 5.9$; 45% were females). They were paid on average AUD 22.8 ($SD = 4.83$) based on the number of points earned during the experiment.

Experiment phase — The experiment was a within-subject design consisting of series of individual perceptual decisions organized in 4 blocks of 72 trials. To make a decision, subjects received two different kinds of information: on stimuli and on payoffs. Payoffs

³Additional information about stimuli and their calibration are in Chapter B of the Appendix.

were communicated by a payoff matrix providing a matching rule between the four possible decision outcomes and the number of points earned in each case. By combining both kinds of information, participants answered either “Left” or “Right” for the chosen circle. To help subjects in their decision-making, we gave them feedback on accuracy and results for points after each trial as well as additional aggregated feedback on accuracy and points every 12 trials.⁴ 28 randomly selected decisions were paid (10 *points* = AUD 1). In the “Stimuli-First” treatment, subjects observed stimuli before the payoff matrix. In the “Payoff-First” treatment, the order was reversed (Figure 3.1-C&D). Treatments were varied between blocks (two blocks per treatment).⁵

Payoff Matrices — Payoff Matrices are 2×2 matrices that indicate, for each trial, the monetary reward (in points) associated with correct and incorrect decisions. Stimuli need to be varied trial-by-trial, we thus vary also payoff matrices trial-by-trial to limit artificial differences between stimuli information and information on payoffs. To make this manipulation relevant, we used a set of 3×2 payoff matrices that were invariant regarding the optimal decision criterion but varied regarding the payoff variance (difference between correct and incorrect payoffs). This consists of matrices with “Low” (M_{low}), “Intermediate” (M_{int}) and “High” (M_{high}) payoff variances with more points earned for successes either for “Left” or “Right” (see Table 3.1 and Figure 3.1-B). Values have been chosen to respect these theoretical constraints while being visually as close as possible and rewarding similarly perfect performances.⁶ We use negative and positive values as previous studies have obtained that pure-gain, pure-loss and mixed gain/loss frameworks do not affect decision criterion placement (Maddox et al., 2003b; Garcia and Massoni, 2017).

Data analysis

Based on our 2-AFC stimuli and the three different payoff variances, we specify our model as a Gaussian equal-variance SDT model with signal being “the highly rewarded

⁴Feedback on aggregated accuracy indicate the mean accuracy and feedback on aggregated points indicates how close subjects were from perfect performance. Additional information are in Chapter B of the Appendix.

⁵We randomly generated the same sequence of stimuli and payoff matrices for all the subjects. The target circle’s frequency, payoff matrices and the number of paid decisions were controlled such that they were equal in each block. The order of treatments was randomly generated.

⁶ M_{high} is obtained from M_{low} by reversing the two digits of the most rewarded success (12 to 21), suppressing the 0 of the less rewarded success (10 to 1) and adding minus to errors (9 to -9). M_{int} preserves a similar pattern with intermediate values. All matrices give the same expected payoff for perfect performance (11 points) but vary regarding the expected payoff for random guesses (1 point for M_{high} , 5 points for M_{int} and 10 points for M_{low}).

Payoff Matrix	Success Left	Success Right	Error Left	Error Right
Low (M_{low})	12	10	9	9
	10	12	9	9
Intermediate (M_{int})	17	5	-1	-1
	5	17	-1	-1
High (M_{high})	21	1	-9	-9
	1	21	-9	-9

Table 3.1: Set of payoff matrices.

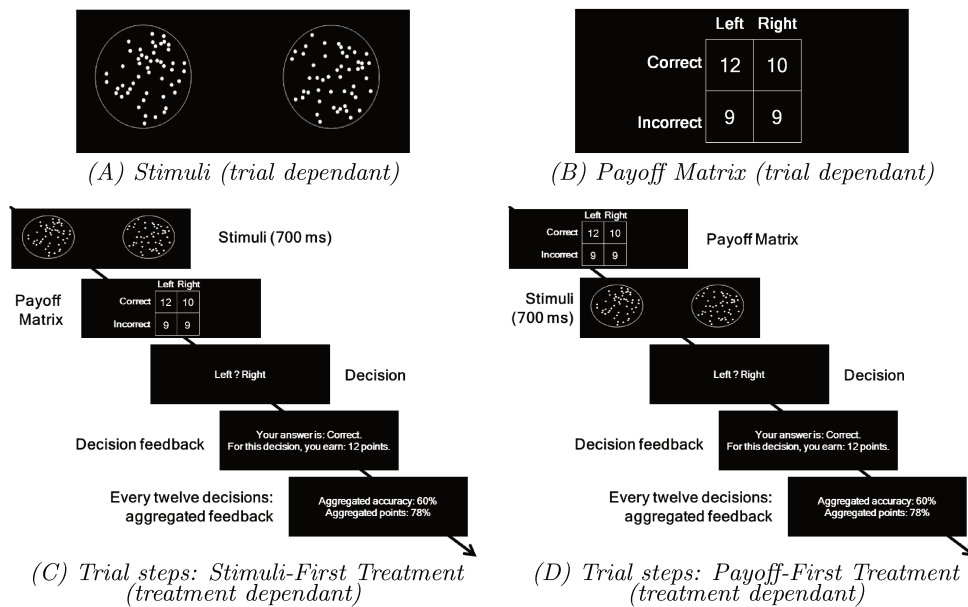


Figure 3.1: Experimental design. (A) Example of stimuli. Subjects evaluated which circle contained the most dots. (B) Example of a Payoff Matrix. (C) Timeline of a trial for the Stimuli-First treatment. Facing a fixation cross, subjects initiated the trial. Stimuli appeared for 700 ms before being replaced by the fixation cross. The payoff matrix was displayed with no time constraints. Subjects made their decisions (right or left). Feedback on the accuracy of the answer and the number of points earned was given. Every twelve trials, aggregated feedback on accuracy and points over these trials was provided. (D) Timeline of a trial for the Payoff-First Treatment. The display order of the payoff matrix and stimuli is reversed compared to the previous timeline.

circle contains more dots”. Reporting the “signal” thus consists in choosing the highly rewarded alternative.⁷ The optimal decision criterion is $\beta^* = \frac{1}{3}$ for all matrices.⁸

Gaussian equal-variance SDT provides a computational model to estimate both subject discriminability (d') and decision criterion (β). To evaluate treatments effects, we estimate one couple $(\hat{d}', \hat{\beta})$ for each order of information at a subject level (Estimation (1)). To assess the impact of each payoff matrix on the decision strategy, we estimated one decision criterion for each combination of matrices and information orders at a subject level. We estimated a single discriminability for each order of information. Estimation (2) thus consists in estimating a vector $(\hat{d}', \hat{\beta}_{low}, \hat{\beta}_{int}, \hat{\beta}_{high})$ for each order of information at a subject level. We compute estimators using maximum likelihood because the analytic form of the estimators is unknown for Estimation (2) and equivalent to the analytic form for Estimation (1) (cf. Ackermann and Landy (2015) for a similar estimation procedure).⁹

Paired t-tests are used to compare two balanced samples. Matrix effects across our 6 samples (3 matrices \times 2 treatments) are compared using repeated measures analysis of variance (rANOVA) followed by Tukey’s range tests for multiple comparisons of means. We report Cohen’s d_z and partial Eta-squared (η_p^2) as effect size measures.

3.3 Results

Estimated discriminabilities and decision criteria for each treatment based on Estimation (1) are presented in Figure 3.2. Estimated decision criteria for each matrix in each treatment based on Estimation (2) are presented in Figure 3.3.

We observe a mean decision criterion of 0.93 ($SD = 0.202$) for the Payoff-First

⁷The equivalence between the 2-AFC specification induced by the perceptive task and our framework is detailed in Chapter B of the Appendix.

⁸The optimal decision criterion is given by the ratio between the additional payoff of a success for the “noise” alternative and the additional payoff of a success for the “signal” alternative (Wickens, 2001, p.36):

$$\beta_{high}^* = \frac{1 - (-9)}{21 - (-9)} = \beta_{int}^* = \frac{5 - (-1)}{17 - (-1)} = \beta_{low}^* = \frac{10 - 9}{12 - 9} = \beta^* = \frac{1}{3} \quad (3.1)$$

⁹Estimation methods, comparison between both estimations types and reasons for assuming constant discriminability across payoff contrasts are detailed in Chapter B of the Appendix. The equivalent analytic forms of Estimation (1) are (Wickens, 2001, p.24 & p.30): $\hat{d}' = Z(HR) - Z(FAR)$ and $\hat{\beta} = \exp(\frac{1}{2}[Z(FAR)^2 - Z(HR)^2])$ with Z the inverse Gaussian distribution, HR the hit rate and FAR the false alarm rate.

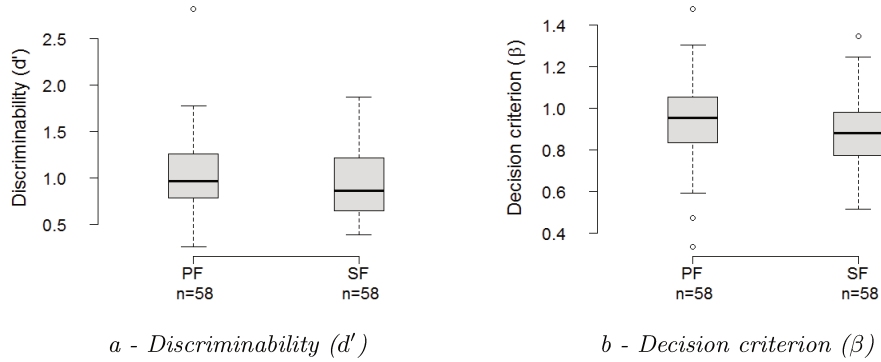


Figure 3.2: Tukey box-and-whisker plot - Discriminability and decision criteria for Payoff-First treatment (PF) and Stimuli-First treatment (SF).

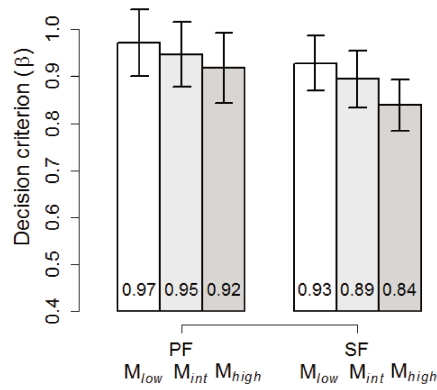


Figure 3.3: Estimated decision criteria for each matrix by treatment. M_{low} , M_{int} and M_{high} respectively correspond to “Low”, “Intermediate” and “High” payoff variances. PF and SF respectively correspond to Payoff-First treatment and Stimuli-First treatment. Error bars correspond to 95% confidence intervals. Bold figures inside bars are mean values.

treatment and 0.88 ($SD = 0.158$) for the Stimuli-First treatment. Both decision criteria are lower than the accuracy maximizing decision criterion, ($\beta^a = 1$), [$t(57) = -2.535, p = 0.014$] for the Payoff-First treatment and [$t(57) = -5.847, p < 0.001$] for the Stimuli-First treatment; and higher than the optimal decision criterion ($\beta^* = \frac{1}{3}$), [$t(57) = 22.58, p < 0.001$] for PF and [$t(57) = 26.31, p < 0.001$] for Stimuli-First treatment. We conclude that subjects use conservative decision criteria.

Subjects' decision criteria are lower in the Stimuli-First treatment ($M = 0.88, SD = 0.158$) than in the Payoff-First treatment ($M = 0.93, SD = 0.202$), [$t(57) = 2.280, p = 0.026, d_z = 0.299$]. To assess the robustness of this finding, we conduct a 2 (information order: "Stimuli First" and "Payoff First") \times 3 (payoff variance: "Low", "Intermediate" and "High") repeated measures analysis of variance (rANOVA) on the decision criterion (β). We find a statistically significant main effect of the information order [$F(1, 57) = 5.725, p = 0.020, \eta_p^2 = 0.091$]. We thus conclude that subjects' decision strategies shift in the direction of optimality in the Stimuli-First treatment compared to the Payoff-First treatment. Descriptively, the gap between optimal decision criterion and actual decision criterion is reduced by 9.0% in the Stimuli-First treatment.

Based on the rANOVA, we also find a statistically significant main effect of the payoff variance [$F(2, 114) = 3.144, p = 0.047, \eta_p^2 = 0.052$]. The presence of the "High" payoff variance (M_{high}) leads subjects to adopt a lower decision criterion compared to the "Low" payoff variance (M_{low}) resulting in a shift in the direction of optimality [$q = -2.496, p = 0.034, d_z = 0.328$]. Descriptively, the gap between optimal decision criterion and actual decision criterion is reduced by 11.7% for M_{high} compared to M_{low} . The decision criterion associated with the intermediate payoff variance (M_{int}) is included between decision criteria of the two other payoff variances. However, it is not statistically different from either of them [$M_{low} : q = -1.044, p = 0.549, d_z = 0.137; M_{high} : q = -1.453, p = 0.314, d_z = 0.191$]. We conclude that subjects' decision strategies shift in the direction of optimality in presence of higher payoff variances. Finally, we do not find a statistically significant effect of interaction between payoff variance and information order on strategies [$F(2, 114) = 0.37, p = 0.691, \eta_p^2 = 0.006$].

Analyzing discriminabilities, we find a trend suggesting that subjects exhibit greater discriminability in the Payoff-First treatment ($M = 1.02, SD = 0.415$) compared to the Stimuli-First ($M = 0.95, SD = 0.391$) treatment [$t(57) = 1.854, p = 0.069, d_z = 0.243$].

3.4 Discussion

The purpose of this study was to investigate a potential confound between the conservative criteria placement and a payoff order effect. Our results show that this order effect exists: displaying payoffs last shifts decision criterion in the direction of optimality while receiving visual stimuli last involves a trend suggesting a greater discriminability. But the magnitudes of the changes in behaviors are not large enough to explain the conservative criterion placement. We thus confirm the robustness of this bias to a payoff order effect and stay in line with previous studies showing the difficulty to change behaviors toward optimality (see Rahnev and Denison, 2018, for a review).

Even if we cannot shift the decisions in a large extent, we still observe an effect of processing order. Its existence informs on how payoffs and perceptual information are combined. In line with the evidence from the dynamic integration of multiattribute component of a choice, our results are in favor of a two-stage model in which both attributes (payoffs and perceptual evidence) are processed separately and sequentially over time. The differences we observed in terms of decision criteria (and to a lesser extent in discriminability) according to the order the information is displayed fit in the recent evidence supporting such dual-stage model to combine perceptual decision-making and payoffs (Diederich and Busemeyer, 2006; Diederich, 2008, 2016; Rorie et al., 2010; Gao et al., 2011). In particular, Diederich (2016) studies how providing information about payoffs before, after or at the same time than the stimuli affects choice probability and reaction time. While our design is almost identical to hers, both approaches are different. She performs sequential sampling models comparison and provides evidence for a dual-stage model while our main motivation is to disentangle the conservative criterion placement in static SDT from an information order effect. This divergence of analytical framework, allows us to conclude on different points of interest on the basis of a similar design.

Our results extend previous research on the Type B order effect to multiattribute choice. Type B order effect for comparison of consecutive stimuli is formalized based on the weighted difference model (Dyjas et al., 2014). According to this model, participants weight the sensory input from the first and second stimulus differently. Applying this model to experimental evidence (e.g. Nachmias, 2006; Ulrich, 2010; Bruno et al., 2012) typically leads to the conclusion that the weight associated with the second stimulus

is larger than the weight associated with the first stimulus (Dyjas et al., 2014). In our framework, the decision is not based on the difference between two stimuli but on the difference between a likelihood ratio (extracted from the stimuli) and a decision criterion (extracted from the payoff matrix). Weighted difference model can be applied by associating a weight to both the likelihood ratio and the decision criterion. Our results could thus be explained similarly to Type B effects by a larger weight on the first of two decision components. As detailed in Dyjas et al. (2014), this difference in weighting can be interpreted as an higher attention allocation to the second component of the decision. This attentional switch described by the dual-stage model of multiattribute choice would thus result in an unbalanced attention within attributes. This unbalanced attention may be explained by an optimal reaction to a memory decay. As suggested by Hellström and Rammsayer (2004), the quality of the information collected first may be altered due to forgetting. Adapting respective weights would thus correspond to giving more importance to the best quality information.

We observe that increasing the payoff variance leads to a shift of behaviors toward a less conservative criterion placement. This effect is in line with previous studies investigating how payoff matrix multiplication affects criterion learning (Bohil and Maddox, 2003; Maddox et al., 2003b). These studies increase payoff variance by multiplying all payoffs by a constant factor and find that decision criterion learning is improved for high variance payoff matrices. The fact that we observe the same effect, while in our design decision criterion learning effects are less salient, highlights the strength of the variance effect.¹⁰ Indeed, criteria adjustment is more difficult when the decision environment is changed trial-by-trial than block-by-block thus our experimental design gives a more conservative evaluation of the variance effect (Brown and Steyvers, 2005; Starns and Olchowski, 2015). Bohil and Maddox (2003); Maddox et al. (2003b) explain this improved criterion learning by a change in the slope of the objective reward function. The reduction of the learning component suggests that allocation of attention mechanisms may also play a role in the process. A higher payoff variance may direct the attention toward the payoff matrix which would positively shift the decision criterion. While we assume a direct effect of attention on criterion placement, we cannot exclude the existence of an indirect effect on discriminability. The attention allocated toward payoffs may take place at the expense of the perceptual information processing. This depletion

¹⁰In our experiment subjects made a smaller number of decision (288 against 1920 to 6480) and payoffs were varied at a decision level rather than at a block level (60 to 120 decisions).

of attention will result in a lower discriminability for higher payoff variances. We do not address this point for statistical reasons in the present study (see Chapter B of the Appendix for details).

The order in which payoffs are displayed in a perceptual task fails to explain the observed conservative criterion placement. Yet the sequence of information is not neutral on behaviors. We highlight a new order effect between perceptual information and payoffs. It opens questions on how payoff information and stimuli evidence are dynamically integrated to set the decision strategy. In particular, understanding how attention is allocated between both information sources might provide a deeper knowledge of the perceptual decision process.

Chapter 4

Ambiguity and excuse-driven behavior in charitable giving¹

4.1 Introduction

Donations are often subject to uncertainty, whether the latter consists of risk or ambiguity. First, the impact of donations on the beneficiaries may be uncertain as donations may not reach their recipients. Embezzlement is one cause of uncertainty. As an example, four cancer charities were charged with \$187 million in fraud in the US in 2015. Donors were told that their money would help cancer patients while the majority of donations benefited only the perpetrators, their families and friends, and fundraisers.² Mismanagement is another source of uncertainty. Humanitarian crises often involve sudden and large amounts of donations raised to help people in need. Managing supplies during such crises is difficult, hence supplies do not always reach recipients. For instance, the action of the Red Cross during hurricane Harvey has been pointed out as it had to throw out tens of thousands of meals, being unable to find the people who needed them.³ Second, the cost of donations may also be uncertain. Uncertainty is particularly salient in the case of living organ donations. For example, living kidney donors may incur costs for transportation, lodging, medical and medication expenses, lost wages and

¹Co-authored with Sébastien Massoni and Marie Claire Villeval.

²<https://www.ftc.gov/news-events/press-releases/2015/05/ftc-all-50-states-dc-charge-four-cancer-charities-bilking-over>

³<https://www.washingtonpost.com/news/post-nation/wp/2017/08/28/people-are-urging-donations-for-harvey-relief-efforts-just-not-to-the-red-cross>.

other incidentals. These costs are uncertain, as they vary substantially between donors and they cannot be accurately predicted due to their complexity (Reese et al., 2015). As an illustration, Klarenbach et al. (2014) observe that, out of 100 donors, more than half of them incurred costs lower than \$1000 while for a third of them they exceeded \$5000.

Uncertainty is expected to have a direct impact on donations: when the costs of the donation increase or when its benefits decrease, individuals are expected to give less. Moreover, an indirect effect may lead to an even further decrease: individuals may use uncertainty as an excuse not to give. Indeed, giving decisions may be motivated by a genuine concern for others that leads individuals to maximize utility over payoff for self and payoff for others (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Charness and Rabin, 2002; Andreoni and Miller, 2002). However, they may also be motivated by the individuals' desire to be *perceived as* being concerned by others' well being (Ariely et al., 2009). Individuals are thus more likely to behave selfishly when their social image is preserved from the judgment of others and from their own judgment (Hoffman et al., 1996; Dana et al., 2006; Hamman et al., 2010). Situations decreasing the guilt of not giving open a "moral wiggle room" to individuals for behaving less altruistically (Dana et al., 2007; Grossman and Van Der Weele, 2017; Bicchieri et al., 2018). When the direct link between an action (the donation) and its consequences (the cost or benefit of the donation) is blurred, individuals may use uncertainty strategically to make more selfish decisions.

A first type of uncertainty is based on known randomness: risk. Under risk, individuals know the probability distribution of outcomes. Krawczyk and Le Lec (2010); Brock et al. (2013) and Freundt and Lange (2017) use dictator games to highlight that generosity is reduced under risk. However, it does not provide definitive evidence of the existence of a moral wiggle room, as the direct, non-excuse driven, effect of risk on donations is not identified separately from the indirect, excuse-driven, effect. Exley (2015a) reveals the existence of a moral wiggle room under risk by measuring certainty equivalents to different types of lotteries. By controlling for individuals' own risk attitude and other-regarding risk attitude, she isolates an excuse-driven effect of risk on donations.

A second type of uncertainty involves unknown randomness: ambiguity. Under ambiguity, individuals have only imprecise information on the probability distribution of the outcomes. In most instances in real life, donors are more likely to face situations that are better described by ambiguity than by risk. The risks of embezzlement and

mismanagement are difficult to evaluate and the costs of organ donations are uneasy to anticipate due to their high volatility. Comparing decisions under risk and ambiguity usually reveals that most individuals prefer facing risk than comparable ambiguity. This behavior is known as ambiguity aversion (Ellsberg, 1961). Ambiguity increases the complexity of the decision process. Risk preferences express tastes over the solution of a trade-off between outcomes and probabilities. Ambiguity preferences are based on a similar trade-off, except that decisions request the subjective assessment of all available information (see Machina and Siniscalchi, 2014 for an extensive review). Such subjective evaluation may be a source of moral wiggle room because it gives an opportunity to individuals to manipulate their beliefs. For example, Akerlof and Dickens (1982) developed a theoretical model that allows individuals to alter their beliefs to justify their decisions. Based on probabilistic dictator games, Feiler (2014) highlights that situations allowing moral wiggle room are situations that allow individuals to manipulate their beliefs so as to increase the value of a selfish action. A similar belief manipulation may take place when individuals assess donations under ambiguity. It may lead individuals to be more pessimistic when evaluating a donation with ambiguous benefits than when evaluating ambiguity on their own earnings. It may also lead individuals to be more optimistic when evaluating a donation with ambiguous benefits than when evaluating ambiguity on their own earnings. Such strategic manipulations may enable individuals to choose not to donate without feeling guilty. While under risk individuals evaluate only objective information, we expect excuse-driven behavior to be reinforced under ambiguity. Our aim is measuring and comparing excuse-driven behavior under risk and under ambiguity to assess whether ambiguity aversion provides additional moral wiggle room. We address this question both when ambiguity affects the cost or the benefit of a donation. Indeed, first, the evaluation of uncertainty for oneself (cost) or for others (benefit) may differ (Reynolds et al., 2009; Eriksen and Kvaløy, 2010; Chakravarty et al., 2011). Second, excuse-driven behavior is expected to have opposite effects on the two types of ambiguity; thus, studying both of them allows us to highlight a strategic manipulation of ambiguity attitudes.

To the best of our knowledge, only two studies have focused on giving behavior under ambiguity. The most closely related study is the one by Haisley and Weber (2010) about self-serving interpretations of ambiguity in other-regarding behavior. They analyze binary dictator decisions when a recipient's payoff is determined by the outcome

of a lottery. The lottery can be either risky or ambiguous. They find that subjects are less generous under ambiguity compared to risk. However, this finding does not identify precisely excuse-driven behavior because individuals are usually ambiguity averse. The observed behavior can thus be caused either by a direct effect of ambiguity on donations—donations are perceived as less valuable, thus donors give less—or by an indirect excuse-driven behavior—individuals act as if they were more averse to ambiguity when asked to give. Asking each dictator to estimate the expected value received by the receiver, Haisley and Weber (2010) find that the difference between the actual value and the estimated value is larger under ambiguity compared to under risk. Both previous results vanish when subjects have previously made individual decisions under risk and ambiguity without a trade-off between self-interest and fairness (*i.e.*, when a decision only impacts the individual's payoff), or when recipients make decisions with hypothetical consequences. The second closely related study focuses on ex-post and ex-ante fairness (Cettolin et al., 2017). It implements dictator games with a lottery involving risk either for the recipient or the dictator. It shows that dictators have a similar behavior under ambiguity and under risk. Here, subjects have not previously made individual decisions under risk and ambiguity without a trade-off between self-interest and fairness.

The studies of Haisley and Weber (2010) and of Cettolin et al. (2017) on the impact of ambiguity on donations when the recipient's payoff is ambiguous deliver different conclusions. This lack of consistency calls for further research on this topic. Furthermore, Haisley and Weber (2010) identify a self-serving use of ambiguity to decrease donations based on the subjects' estimations of the impact of their own past decisions. However, recent studies have highlighted that past decisions may be recalled selectively and self-servingly. For example, Saucet and Villeval (2018) and Carlson et al. (2018) show that the memory of past selfish decisions is less accurate than the memory of altruistic decisions. Since subjects in Haisley and Weber (2010) give less under ambiguity than under risk, the differences in recalls between risk and ambiguity settings can reveal a self-serving use of ambiguity when deciding whether to donate, but it might also reveal self-serving recalls. It is therefore important to study excuse-driven behavior by other means than recalls to isolate excuse-driven behavior when deciding whether to donate. To that aim, instead of using the evaluation of past decisions we use the method introduced

by Exley (2015a) to identify excuse-driven behavior based solely on decisions.⁴ First, we replicate Exley (2015a) to confirm the existence of moral wiggle room under risk. Then, we use the same method to identify the existence of moral wiggle room under ambiguity. Finally, we compare moral wiggle room under risk and under ambiguity to test for reinforced excuse-driven behavior under ambiguity. We assess and compare excuse-driven behavior when evaluating “lotteries-for-self” (*i.e.*, when uncertainty affects the cost of the donation) and “lotteries-for-charity” (*i.e.*, when uncertainty affects the benefit of the donation).

Under risk, we find evidence of excuse-driven behavior when subjects have to value lotteries-for-self. We also find some evidence of excuse-driven behavior when subjects have to value lotteries-for-charity. These results hold under ambiguity. Our analysis indicates that self-serving behavior is more likely to emerge when individuals are evaluating uncertainty on their own payoffs than uncertainty on others’ payoffs. Donations differ under risk and under ambiguity: individuals give less in ambiguous than in risky settings. However, once we isolate the differences in non-strategic preferences from excuse-driven behavior, we find no evidence of an increased moral wiggle room under ambiguity compared to risk. These findings hold regardless of whether subjects face “full ambiguity” (probability completely unknown) or “partial ambiguity” (probability unknown within an interval). Overall, our findings contrast with previous findings as they suggest that some individuals exploit any type of uncertainty as an excuse not to give, and that the very nature of uncertainty does not matter in that respect.

The remainder of this paper is organized as follows: Section 4.2 details our framework and introduces our hypotheses. Section 4.3 presents our experimental design. The data analysis methodology and the results of our study are reported in Section 4.4. Section 4.5 concludes.

4.2 Framework and hypotheses

Consider a situation where an individual can donate to a charity, and where either the cost of the donation for the individual or the benefit of the donation for the charity is random. This randomness can be either risk or ambiguity. The evaluation of excuse-driven behavior for donations under risk or under ambiguity is based on the elicitation

⁴This method is also used in Exley (2015b) to document the use of charity performance metrics as an excuse not to give.

of the individuals' valuations of the donation in various scenarios.

We first define X as the monetary payoff such that the individual is indifferent between receiving €10 or donating € X to a charity. The objective is measuring altruistic preferences in the absence of risk and ambiguity. Formally:⁵

$$(10, 0) \sim (0, X) \quad (4.1)$$

Then, we introduce risk by means of two lotteries. “Lotteries-for-self” under risk, P_r^s , pay €10 to the subject with probability p and pay €0 to charity with certainty. “Lotteries-for-charity” under risk, P_r^c , pay € X to the charity with probability p and pay €0 to the subject with certainty. In other words, these two lotteries are obtained from the two allocations of $(10, 0)$ and $(0, X)$ by introducing a $(1 - p)$ probability of a €0 outcome for both agents.

$$\text{Under risk : } P_r^s = (p, (10, 0); 1 - p, (0, 0)) \text{ and } P_r^c = (p, (0, X); 1 - p, (0, 0)) \quad (4.2)$$

Under ambiguity, the probability of paying the non-null amount is not unique but defined over the interval $[p - a, p + a]$, and it is denoted \tilde{p} .

Under ambiguity :

$$P_a^s = (\tilde{p}, (10, 0); 1 - \tilde{p}, (0, 0)) \text{ and } P_a^c = (\tilde{p}, (0, X); 1 - \tilde{p}, (0, 0)) \text{ with } \tilde{p} \in [p - a, p + a] \quad (4.3)$$

We measure how much individuals value these lotteries both in terms of self-valuation (*i.e.*, the payoff-for-self equivalent to the lottery) and charity-valuation (*i.e.*, the donation equivalent to the lottery). The self-valuation of a lottery is denoted $Y^s(P)$. It means that individuals are indifferent between the lottery P and receiving € $Y^s(P)$ and it gives the certainty equivalent of the lottery. The charity-valuation of a lottery is denoted

⁵In the pair (x,y) the first parameter corresponds to self-payoff and the second to charity-payoff. X exists and is unique under the assumption that individuals strictly prefer more payoffs than less both for themselves and for the charity. First, it implies that they have altruistic motives in the sense that they give a positive value to charity-payoff. Second, it implies that they are not inequality averse regarding charity-payoff. The fact that individuals give money to charities that is wealthier than them supports this assumption.

$Y^c(P)$. It means that individuals are indifferent between the lottery P and donating $\in Y^c(P)$ to the charity and it gives the certain donation equivalent to the lottery.

There are two types of lotteries (lottery-for-self and lottery-for-charity) and two types of valuations (self-valuation and charity-valuation). It results in four types of lottery valuations, as summarized in Table 4.1. For lotteries-for-self, the two types of lottery valuations are: self-valuations of lotteries-for-self (“Self/Self” valuations) and charity-valuations of lotteries-for-self (“Self/Charity” valuations). For lotteries-for-charity, the two types of lottery valuations are: self-valuations of lotteries-for-charity (“Charity/Self” valuations) and charity-valuation of lotteries-for-charity (“Charity/Charity” valuations).

To introduce a common denomination between the two types of lotteries, we refer to the decisions that affect the payoffs of one agent only as “decisions without self *vs.* charity trade-off”. These decisions are the self-valuations of lotteries-for-self (when there is no possibility to give) and the charity-valuation of lotteries-for-charity (when there is obligation to give). In contrast, the other decisions are “decisions with a self *vs.* charity trade-off” as the lottery and its certainty equivalent are not associated to the same agent. They are the charity-valuations of lotteries-for-self and the self-valuations of lotteries-for-charity.

		Lottery:	
		Self	Charity
Valuation:	Self	Self/Self: $Y^s(P_u^s)$	Self/Charity: $Y^s(P_u^c)$
	Charity	Charity/Self: $Y^c(P_u^s)$	Charity/Charity: $Y^c(P_u^c)$

with $u = r$ for lotteries under risk and $u = a$ for lotteries under ambiguity

Table 4.1: Types of lottery valuations under uncertainty

Our hypotheses are based on comparisons between self-valuations and charity-valuations. To enable such comparisons, we scale valuations based on the donation $\in X$ equivalent to receiving $\in 10$. Self-valuations, $Y^s(P)$, are scaled as percentages of 10 being received by the subject. Charity valuations, $Y^c(P)$, are scaled as percentages of X being donated.⁶

Hypothesis 1-R and Hypothesis 2-R address the existence of moral wiggle room under risk (replication of Exley, 2015a). Hypothesis 1-A and Hypothesis 2-A address its existence under ambiguity (extension of Exley, 2015a to ambiguity). For an individual

⁶This rescaling is based on the assumption of a local linear utility in payoffs. It allows us to compare self-valuations and charity-valuations. This assumption is not crucial as most of our results do not rely on such comparisons.

with standard preferences (*i.e.*, someone who does not violate the independence axiom), the self-valuations and the charity-valuations of the exact same lotteries should be similar. In contrast, for an individual whose behavior is excuse-driven self-valuations and charity-valuations differ. The nature of this difference depends on whether uncertainty is borne by the individual or by the charity.

When evaluating lotteries-for-self, an increased charity-valuation ($Y^c(P_u^s)$) reflects a less altruistic behavior. Indeed, a low charity-valuation means that the individual accepts to forego his own potential payoff from the lottery against a modest donation. In contrast, a high charity valuation means that the individual is willing to forego his own potential payoff only if the donation is large enough. A higher charity-valuation compared to self-valuation identifies the presence of excuse-driven preferences for risk-for-self (respectively, ambiguity-for-self). Indeed, the individual behaves less altruistically in the presence of risk (respectively, ambiguity). Such behavior, as formalized in Hypothesis 1-R and Hypothesis 1-A, implies the existence of moral wiggle room either under risk or under ambiguity for lotteries-for-self:

H 1-R. *Individuals exhibit excuse-driven behavior under risk-for-self:*

$$Y^c(P_r^s) > Y^s(P_r^s)$$

H 1-A. *Individuals exhibit excuse-driven behavior under ambiguity-for-self:*

$$Y^c(P_a^s) > Y^s(P_a^s)$$

The same reasoning applies to the evaluation of lotteries-for-charity. When evaluating these lotteries, a decreased self-valuation ($Y^s(P_u^c)$) reflects a less altruistic behavior. Indeed, a high self-valuation means that the individual accepts to forego donating the outcome of the lottery to the charity only against a large payoff-for-self. In contrast, a low self-valuation means that the subject is willing to forego donating the outcome of the lottery to the charity only in exchange for a modest payoff-for-self. A higher charity-valuation compared to the self-valuation identifies the presence of excuse-driven behavior for risk-for-charity (respectively ambiguity-for-charity). Indeed, the subject behaves less altruistically in the presence of risk-for-charity (respectively ambiguity-for-charity). Such behavior, as formalized in Hypothesis 2-R and Hypothesis 2-A, implies the existence of moral wiggle room either under risk or under ambiguity for lotteries-for-charity.

H 2-R. *Individuals exhibit excuse-driven behavior under risk-for-charity:*

$$Y^c(P_r^c) > Y^s(P_r^c)$$

H 2-A. *Individuals exhibit excuse-driven behavior under ambiguity-for-charity:*

$$Y^c(P_a^c) > Y^s(P_a^c)$$

Next, we study whether ambiguity increases the size of the moral wiggle room (Hypothesis 3 and Hypothesis 4). Indeed, in contrast to risk, decision-making under ambiguity involves the subjective assessment of available information. Moreover, belief manipulation has been identified as a source of self-serving behavior and Haisley and Weber (2010) find a self-serving interpretation of ambiguity. We thus hypothesize that excuse-driven behavior is reinforced under ambiguity. Following the previous dichotomy, we distinguish excuse-driven behavior depending on who is the beneficiary of the lottery.

For risk-for-self or ambiguity-for-self, the size of the moral wiggle room is given by the difference between self-valuations of lotteries-for-self ($Y^c(P_u^s)$) and charity-valuations of lotteries-for-self ($Y^s(P_u^s)$). A larger difference between the two valuations indicates a larger moral wiggle room as formalized in Hypothesis 3:

H 3. *Individuals exhibit reinforced excuse-driven behavior under ambiguity-for-self compared to risk-for-self:*

$$Y^c(P_a^s) - Y^s(P_a^s) > Y^c(P_r^s) - Y^s(P_r^s)$$

The situation is symmetric for risk-for-charity *vs.* ambiguity-for-charity. The size of the moral wiggle room is given by the difference in valuations between charity-valuations of lotteries-for-charity ($Y^c(P_u^c)$) and self-valuations of lotteries-for-charity ($Y^s(P_u^c)$), as formalized in Hypothesis 4:

H 4. *Individuals exhibit reinforced excuse-driven behavior under ambiguity-for-charity compared to risk-for-charity:*

$$Y^c(P_a^c) - Y^s(P_a^c) > Y^c(P_r^c) - Y^s(P_r^c)$$

4.3 Experimental design

The experimental design consists in 13 price lists in which subjects have to make binary decisions to earn money for themselves and for a charity. The first price list serves as a calibration task used to estimate the charity-valuation equivalent of €10 for self-payoff. The remaining 12 price lists involve choices between a safe payoff and a lottery. These lotteries vary along two dimensions: the type of uncertainty (risk, partial ambiguity, and full ambiguity) and the beneficiary of each option (lotteries-for-self or lotteries-for-charity, safe payoffs for the subject or for the charity). All subjects face the 13 price lists. An example of price list is displayed in Figure 4.1. The other price lists are presented in Section C1.

4.3.1 Calibration

The calibration task allows us to estimate the charity-valuation equivalent of €10 of self-payoff (X as defined in Equation 4.1) for each individual.⁷ We use this estimation to calibrate price lists and lotteries in the main task at the subject level. Subjects are unaware that their choices in this price list are used for calibration to prevent strategic behavior. The price list is composed of 16 binary decisions between two options (labeled option A and option B).⁸ Option A always pays €10 to the subject, while option B pays an amount increasing from €0 to €30, by increments of €2, to the charity. When the value of the donation associated with option B increases, subjects should switch from option A to option B at some point if they prefer to donate €30 than to receive €10. Following Exley (2015a), X takes the value of option B at the switching point from option A to option B.⁹ X is an integer.

⁷In the calibration task, individuals can donate to a charity. We acknowledge that this situation may incorporate some natural uncertainty about the benefit of the donation. However, we assume that it involves less uncertainty than the main task because here we do not add artificial uncertainty. Hence, in the text we refer to this decision as “riskless” so as to differentiate it from the other decisions.

⁸In the calibration price list as well as in following price lists, we did not constrain decisions by implementing enforced-single-switching, nor did we preselect any option. Instead, we follow Andreoni and Sprenger (2011) and Exley (2015a) by informing subjects that most people start by selecting option A, and then switch to option B at some point in the price list (see the paragraphs “Successive decisions” of the instructions in Appendix Subsection C1.1 for the actual phrasing).

⁹The actual value of X is included between the value of option B at the switching point and its previous value in the list. For a given level of altruism, the value of $Y^s(P_u^c)$ is increasing in X while the value of $Y^c(P_u^s)$ is decreasing in X (with $u = r$ or $u = a$). We thus consider that X

4.3.2 Main task

The main task is composed of 12 price lists. Each price list is obtained by combining one type of uncertainty with one beneficiary combination, as detailed hereafter. The order of price lists is randomized at the individual level.

4.3.2.1 Beneficiaries

In all price lists, one option pays the outcome of a lottery while the other option pays a safe amount. The beneficiary of the lottery can be either the subject or the charity. Likewise, the beneficiary of the safe payoff may be either the subject or the charity. This 2×2 design gives four different configurations allowing us to estimate the certainty equivalents of lotteries-for-self and lotteries-for-charity, either in terms of self-payoff or in terms of charity-payoff, as presented in Table 4.1. Each price list is composed of 21 decisions. Lotteries-for-self pay €10 with probability p and €0 with probability $(1 - p)$. Lotteries-for-charity are obtained by replacing the potential self-payoff of €10 with the equivalent donation € X (as estimated at the individual level in the calibration task). Likewise, the safe payoff option is calibrated based on X . For the self-safe payoff, the payoff increases from €0 to €10 by increments of €0.5. For the charity-safe payoff, the payoff increases from €0 to € X by increments of € $\frac{X}{20}$. It means that price lists differ across subjects depending on the individual valuations in the calibration task.

4.3.2.2 Lotteries

We implement three types of uncertainty: one involving risk and two involving ambiguity. Under risk, the probability p is known and equal to 0.5. Giving the same likelihood to positive and null payoffs minimizes potential problems of understanding (Kahneman et al., 1982). Under ambiguity, the probability takes its value in an interval. The subject knows only the lower bound and the upper bound of this interval. For the two levels of ambiguity, the interval is centered in 0.5 to ensure comparability with the risky situation. Under “full ambiguity”, the probability is defined in the interval $[0, 1]$, *i.e.* the probability is entirely unknown. Under “partial ambiguity”, the probability is defined in the interval $[0.25, 0.75]$, reducing the interval size by half. Two levels of ambiguity are implemented because ambiguity aversion has been shown to increase with the size

is the maximum value possible to provide conservative testing of our hypotheses 1-R, 1-A, 2-R and 2-A.

of the probability interval (Chew et al., 2017). This variation allows us to test whether this behavioral difference also exists for excuse-driven behavior.

To facilitate understanding, lotteries are associated with a visual aid. Lotteries under risk are presented as a circle divided into two equal portions to indicate that both outcomes are equally likely. “0” is displayed in the left portion of the circle and the positive payoff is displayed in the right portion. Both payoffs have the same font size. Under ambiguity, the size of each portion of the circle is moving continuously to describe the probability set. The relative font size of each payoff also varies dynamically to reflect the difference in likelihood between the two outcomes. Additional information about the visual aid is given in Appendix Subsection C1.3.

We use the procedure introduced by Stecher et al. (2011) to implement ambiguity. This procedure is based on successive draws in a distribution with no finite quantiles or moments, the Cauchy distribution. Using a divergent distribution guarantees that no probability can be associated with each event and thus provides ambiguity.

4.3.3 Charity choice

Before the calibration decision, subjects started by selecting a charity among three. The three charities were major, well-known, French charities covering various domains: “La Ligue Contre le Cancer” supports research on cancer, “Les Restos du Coeur” provides food to the homeless, and “Médecins sans Frontières” organizes medical interventions in foreign countries. Giving subjects a choice between several charities aims at increasing their involvement in the experiment and the chance that they care about the charity.

4.3.4 Experimental procedures

The experiment was conducted at Gate-lab (Ecully, France). In total, 200 individuals were invited using the Hroot software (Bock et al., 2014) and participated in one of nine sessions. 91 subjects were males (45.5%), 176 were students from the local business, engineering and medical schools (88%), and their average age was 22.7 years (s.d. = 3.64). The experiment was programmed using the Java language. A session lasted approximately an hour.

Subjects received no feedback on the outcome of their decisions before the display of the payment screen at the very end of the session. Two decisions were randomly selected among the 273 decisions in the thirteen price lists for payment. We imposed that these

two decisions were drawn from different price lists. On average, the total payoff per subject was €25.8 (s.d. = 6.01) divided into €15.0 for personal earnings (including a €7 show-up fee) and €10.8 for donations. Subjects received their earnings privately in cash at the end of the session. Donations to charities were made immediately after using charities' respective online payment platforms.¹⁰ To guarantee to subjects that the donations were actually transferred, we offered to every subject the possibility to observe the bank transfers; this was made common knowledge at the beginning of the session. On average, 4.9 subjects per session volunteered to witness the transfers.

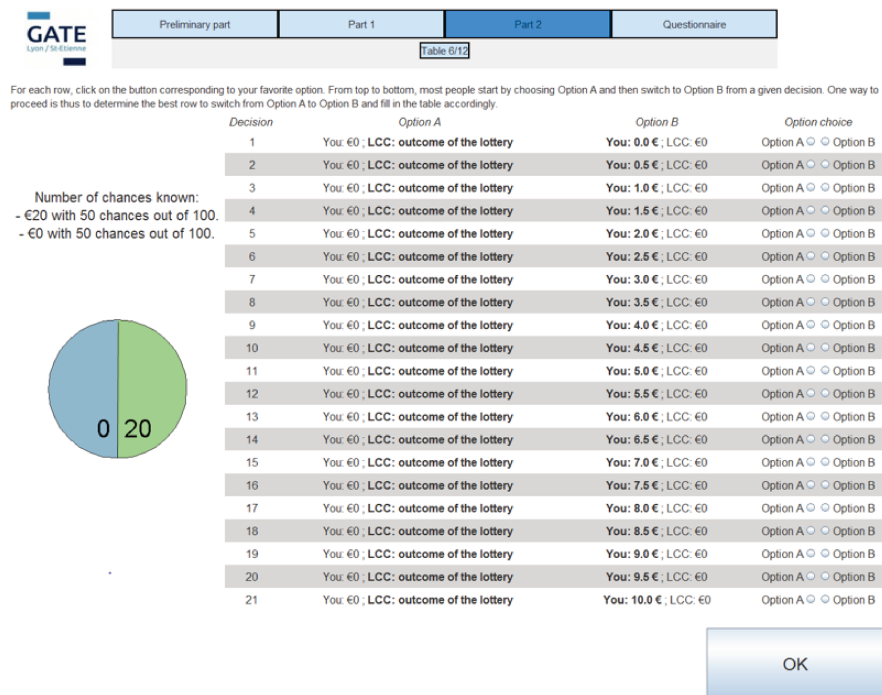


Figure 4.1: Decision example - self-valuation of lottery-for-charity (Self/Charity) under risk, charity: “Ligue Contre le Cancer” (LCC), $X = €20$.

Notes: Subjects have to choose between Option A and Option B in each row of the price list. Option A pays €20 to the “Ligue Contre le Cancer” with $p = 0.5$ and €0 otherwise. Option B pays a safe payoff to the subject that varies between €0 and €10.

¹⁰La Ligue Contre le Cancer: <https://don.ligue-cancer.net/b/mon-don>, Les Restos du Coeur: <https://dons.restosducoeur.org/b/mon-don>, Médecins sans Frontières: <https://soutenir.msf.fr/b/mon-don>.

4.4 Results

First, we present the methodology used to analyze the experimental data and the results of the calibration task. Then, we explore excuse-driven behavior in the main task.

4.4.1 Data analysis and calibration

We start by explaining how certainty equivalents are computed based on the price lists and how these certainty equivalents are scaled up to build measures that are easier to interpret.

When decisions within a given price list exhibit a unique switching point from option A to option B, it means that the valuation is included between the value of option B at the switching point and its previous value in the list. We thus determine valuation as the mean of both values.¹¹

Our treatment of multiple switching points is data-driven. In the calibration task, 22 subjects switch multiple times (11% of all subjects). Their average number of switches is 6.95 (out of the 15 possible switches). In the main task, 6.08% of all price lists contain more than one switching point. Note that subjects with multiple switching points in the calibration task are responsible for almost half of the multiple switches in the main task. They switched more than once in 24.2% of the price lists while it happened to other subjects in 3.8% of the price lists. Moreover, conditional on switching multiple times, they switched on average 7.51 times compared to 5.6 times for the remaining sample. These subjects' pronounced tendency for multiple switching is likely due to a misunderstanding of the task. Following the same rule than Exley (2015a), we exclude these 22 subjects from further analyses. For the remaining sample (178 subjects), we take the first switching point as the actual switching point (like, *e.g.*, Meier and Sprenger, 2015; Exley, 2015a).¹²

Lottery valuations are scaled up as a percentage of the corresponding riskless lottery valuation. Self-certainty equivalents are thus divided by 10 since riskless lottery-for-self would pay € 10. Charity-certainty equivalents are divided by X since lottery-for-charity

¹¹When a subject never switched to option B, the maximal value of option B was used (this case represents 316 observations out of 2600). When a subject never chose option A, the minimal value of option B was used (this case represents 89 observations out of 2600).

¹²Section C4 shows that our conclusions are robust to the inclusion of decisions by multiple switchers.

would pay $\in X$. For example, if the self-certainty equivalent of a lottery is $\in 8.25$, the valuation is $\frac{8.25}{10} = 82.5\%$. If the charity-certainty equivalent of a lottery is $\in 13.5$ and X equals $\in 20$, the valuation is $\frac{13.5}{20} = 67.5\%$. Both scaled certainty equivalents are comparable since donating $\in X$ to the charity is equivalent to receiving $\in 10$. For the sake of conciseness, lottery valuations scaled as a percentage of the corresponding riskless lottery valuation are referred to as “lottery valuations” thereafter.

The calibration task estimates, at the subject level, the value X such that the subject is indifferent between receiving $\in 10$ or donating $\in X$ to the charity. The distribution of these valuations is displayed in Figure 4.2. The average estimated charity-valuation of a self-payoff of $\in 10$, X , is 24.6 (*s.d.* = 7.2): to convince a subject to forego a payoff of $\in 10$, the charity has to receive at least $\in 24.6$. Five subjects exhibit pure pro-social motivation as they prefer to donate any positive amount to the charity than receiving $\in 10$. On the other hand, the estimated X is $\in 30$ for 94 subjects. Among these 94 subjects, 23 subjects forego the self-payoff of $\in 10$ if the charity receives $\in 30$ (12.9%) and 71 subjects never renounce to the $\in 10$ even if the charity receives the maximum transfer (39.9%, 71 subjects out of 178). For these 71 subjects, the estimation of X is censored since they exhibit choices consistent with pure selfish motivation. For these censored subjects, X is under-estimated.¹³ We acknowledge that this is problematic when comparing self-valuations and charity-valuations, as under-estimating X inclines conclusions toward finding excuse-driven behavior (see footnote 9). An alternative could be to exclude censored subjects from further analysis. However, this introduces a selection bias in the data. Indeed, more altruistic subjects (who have a lower value of X) are less likely to exhibit a self-excusing behavior (see Section C3). Excluding censored subjects thus corresponds to excluding those who are more prone to self-excusing behavior. Therefore, exclusion provides a very conservative test of the existence of excuse-driven behavior.

To address this issue, the analyses involving comparisons between self-payoffs and charity-payoffs are reported using both the very conservative approach (excluding the censored subjects) and the less conservative one (including them). The analysis excluding the censored subjects is reported in Appendix when it confirms the analysis that includes them. When it contradicts this analysis, it is reported and discussed in the

¹³Increasing the maximal transfer in the calibration task above $\in 30$ could reduce the number of censored subjects. However, censored subjects may have purely selfish preferences; moreover, increasing the maximal transfer would increase both the expected cost of the experiment and the number of decisions. Note that the percentage of censored subjects with selfish preferences is comparable with that found in Exley (2015a, 42%) and Engel (2011)’s meta-study (36%).

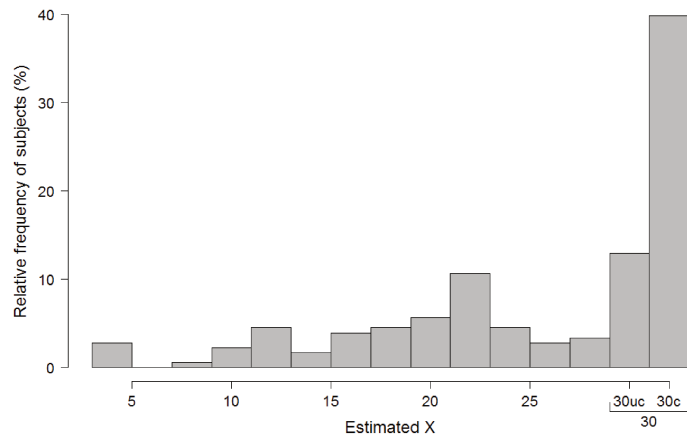


Figure 4.2: Distribution of estimated charity-valuation of €10 of self-payoff (X).

Notes: The histogram represents the percentage of the 178 subjects with a given X . “30uc” corresponds to subjects with an uncensored X equal to €30 and “30c” corresponds to subjects with a censored X equal to €30.

main document.

4.4.2 Moral wiggle room under uncertainty

We first analyze separately behavior under risk and behavior under ambiguity to test the existence of moral wiggle room in each setting. Second, we compare behavior under risk and under ambiguity to assess whether the size of the moral wiggle room increases under ambiguity compared to risk. While in the first two subsections ambiguity encompasses both partial and full ambiguity, the third subsection distinguishes between the two levels of ambiguity. Finally, as in Haisley and Weber (2010), we test whether self-excusing behavior is less likely to emerge when subjects have faced risk or ambiguity earlier in the experiment in a situation without a self *vs.* charity trade-off. Descriptive statistics are summarized in Table 4.2 and analyzed throughout the results section.

4.4.2.1 Excuse-driven behavior under risk and under ambiguity

To test for the existence of a moral wiggle room under risk and under ambiguity, we use linear regressions with errors clustered at the subject level since each subject makes 12 repeated decisions. The dependent variable is the lottery valuation (*i.e.*, how much the subject values the lottery, expressed as a percentage of the corresponding riskless lottery).

Decision type:	Self/Self	Charity/Charity	Charity/Self	Self/Charity	All
Including censored subjects (n=178):					
Risk	46.95 (20.28)	45.24 (22.70)	75.29 (28.44)	36.07 (26.81)	50.89 (28.76)
Ambiguity	43.02 (18.65)	39.66 (18.41)	72.84 (27.40)	32.82 (23.53)	47.09 (27.03)
<i>Partial ambiguity</i>	43.78 (22.42)	40.87 (21.24)	73.81 (29.44)	32.71 (23.87)	47.79 (28.94)
<i>Full ambiguity</i>	42.26 (21.96)	38.46 (20.75)	71.88 (31.01)	32.92 (25.73)	46.38 (29.32)
Excluding censored subjects (n=107):					
Risk	49.46 (19.27)	46.59 (20.54)	66.68 (28.56)	44 (26.30)	51.68 (25.49)
Ambiguity	43.06 (18.46)	41.87 (17.95)	63.74 (26.62)	40.68 (23.98)	47.34 (23.95)
<i>Partial ambiguity</i>	43.06 (21.40)	42.87 (20.72)	64.63 (28.89)	40.56 (24.33)	47.78 (25.89)
<i>Full ambiguity</i>	43.06 (21.43)	40.86 (20.30)	62.85 (30.62)	40.79 (26.90)	46.89 (26.73)

Table 4.2: Descriptive statistics - mean lottery valuations (expressed in terms of a percentage of the corresponding riskless lottery valuation) by decision type and by lottery type

Notes: Standard errors are in parentheses below corresponding means. Decision types are referred to following the norm Safe/Lottery, except for “All” that give values for all decision types pooled together. For ambiguity, values are given either for both ambiguity levels pooled (“ambiguity”) or by ambiguity level (“partial ambiguity” and “full ambiguity”). Censored subjects are included in the upper part of the table while they are excluded in the lower part.

The independent variables include three dummy variables. “*Charity*” is equal to 1 for lotteries-for-charity (symmetrically, “*Self*” is equal to 0 in this case). “*Ambiguity*” is equal to 1 for lotteries under ambiguity (symmetrically, “*Risk*” is equal to 0 in this case). “*Trade-off*” is equal to 1 if the safe payoff and the lottery outcome do not have the same beneficiary. The independent variables also include interaction terms.

Regressions are reported in Table 4.3. In this table, the coefficients associated with the “*Trade-off*” variable give the effect on lottery valuation of a self *vs.* charity trade-off for: lotteries-for-self under risk (model 1), lotteries-for-self under ambiguity (model 2), lotteries-for-charity under risk (models 3 and 5) and lotteries-for-charity under ambiguity (model 4 and 6). Models (5) and (6) are similar to models (3) and (4), respectively, except that they exclude censored subjects. Figure 4.3 displays a visual presentation of

the effect of the self *vs.* charity trade-off under risk (Figure 4.3-a) and under ambiguity (Figure 4.3-b).

We introduce our first result:

Result 1-R. *Subjects exhibit excuse-driven preferences under risk-for-self.*

Support for Result 1-R: the coefficient associated with the “Trade-off” variable in model (1) measures the difference between charity-valuations and self-valuations of lotteries-for-self under risk. We find that the charity-valuations of lotteries-for-self are significantly increased by 28.34 percentage points (p.p.) compared to the self-valuations ($p < 0.001$).¹⁴ It means that subjects are less risk-averse when the safe amount is donated to the charity than to themselves. This behavior that results in increased expected self-payoffs to the detriment of donations reveals a self-excusing behavior. This supports Hypothesis 1-R.¹⁵

Result 1-A. *Subjects exhibit excuse-driven preferences under ambiguity-for-self.*

Support for Result 1-A: the coefficient associated with the “Trade-off” variable in model (2) measures the difference between charity-valuations and self-valuations of lotteries-for-self under ambiguity. We find that charity-valuations of lotteries-for-self increase by 29.82 p.p. compared to self-valuations ($p < 0.001$). It means that subjects exhibit excuse-driven behavior also under ambiguity-for-self (Hypothesis 1-A).

Result 2-R. *There is some evidence that subjects exhibit excuse-driven preferences under risk-for-charity.*

Support for Result 2-R: the coefficients associated with the “Trade-off” variable in models (3) and (5) measure the difference, including and excluding censored subjects, between self-valuations and charity-valuations of lotteries-for-charity under risk. When including censored subjects, the self-valuations of lotteries-for-charity are significantly decreased by 9.17 p.p. compared to the charity-valuations of the same lotteries ($p < 0.001$). The analysis including censored subjects thus provide evidence in favor of the

¹⁴Throughout the paper, when discussing our regressions, we report the associated p-values. Otherwise, we report the p-values from two-sided t-tests when comparing one sample to a specific value and we report the p-values from two-sided paired t-tests when comparing two samples from repeated measures.

¹⁵As detailed in Section C2, Result 1-R and Result 1-A are robust to the exclusion of censored subjects.

existence of excuse-driven behavior under risk-for-charity. However, excluding censored subjects leads to a non-statistically significant decrease by 2.59 p.p. ($p = 0.379$). The increase in the p-value is not only due to the decrease in the sample size but it is also associated with an effect size more than three times smaller (for comparable standard errors) due to this selection. This very conservative approach does not support Hypothesis 2-R.

Result 2-A. *There is some evidence that subjects exhibit excuse-driven preferences under ambiguity-for-charity.*

Support for Result 2-A: the coefficients associated with the variable “Trade-off” in models (4) and (6) measure, including and excluding censored subjects, the difference between self-valuations and charity-valuations of lotteries-for-charity under ambiguity. When including censored subjects, the charity-valuations of lotteries-for-self decrease by 6.85 p.p. compared to the self-valuations of the same lotteries ($p < 0.001$). The analysis including censored subjects thus supports the existence of excuse-driven behavior under ambiguity-for-charity. However, excluding censored subjects leads to a non-statistically significant decrease by 1.19 p.p. ($p = 0.618$). The effect is divided by more than five when excluding these subjects (for comparable standard errors). As for risk, this very conservative approach does not support Hypothesis 2-A.

To go further, we compare the strength of excuse-driven behavior for lotteries-for-self *vs.* lotteries-for-charity, under risk or under ambiguity. As stated above, excuse-driven behavior causes a change in lotteries-for-self valuations of 28.34 p.p. under risk and 29.82 p.p. under ambiguity (when including censored subjects). This leads to a change in lotteries-for-charity valuation of 9.17 p.p. under risk and 6.85 p.p. under ambiguity (also when including censored subjects). We find a statistically significant increase of excuse-driven behavior when individuals value lotteries-for-self compared to lotteries-for-charity for both types of uncertainty ($p < 0.001$ under risk and under ambiguity—regardless of whether the censored subjects are included or not).

To conclude, the separate analysis of the trade-off between self-payoffs and donations under risk and under ambiguity provides evidence in favor of the existence of a moral wiggle room both under risk and under ambiguity. We observe that attitudes toward risk and ambiguity differ when there is a trade-off between self and charity. This difference results in a reduction of altruistic behavior compared to the baseline riskless situation captured in the calibration task. While we find only some evidence supporting the

existence of excuse-driven behavior for lotteries-for-charity, it is unequivocal that excuse-driven behavior is stronger when individuals value lotteries-for-self than when they value lotteries-for-charity.

	<i>Dependent variable: Lottery valuation (%)</i>					
	(1)	(2)	(3)	(4)	(5)	(6)
Trade-off	28.34*** (2.53)	29.82*** (2.32)	-9.17*** (2.41)	-6.85*** (1.90)	-2.59 (2.95)	-1.19 (2.39)
Ambiguity	-3.93*** (1.49)	-3.93*** (1.49)	-5.58*** (1.44)	-5.58*** (1.44)	-4.72*** (1.67)	-4.72*** (1.67)
Charity	-1.71 (1.77)	-3.36** (1.58)				
Self			1.71 (1.77)	3.36** (1.58)	2.87 (2.20)	1.19 (1.93)
Charity × Trade-off	-37.51*** (3.91)	-36.67*** (3.56)				
Self × Trade-off			37.51*** (3.91)	36.67*** (3.56)	19.81*** (5.00)	21.87*** (4.53)
Charity × Ambiguity	-1.64 (1.92)					
Self × Ambiguity			1.64 (1.92)		-1.68 (2.59)	
Charity × Risk		1.64 (1.92)				
Self × Risk				-1.64 (1.92)		1.68 (2.59)
Trade-off × Ambiguity	1.48 (2.13)		2.32 (2.12)		1.40 (2.74)	
Trade-off × Risk		-1.48 (2.13)		-2.32 (2.12)		-1.40 (2.74)
Charity × Trade-off × Ambiguity	0.84 (2.93)					
Charity × Trade-off × Risk		-0.84 (2.93)				
Self × Trade-off × Ambiguity			-0.84 (2.93)		2.06 (4.06)	
Self × Trade-off × Risk				0.84 (2.93)		-2.06 (4.06)
Intercept	46.95*** (1.52)	46.95*** (1.52)	45.24*** (1.70)	45.24*** (1.70)	46.59*** (1.99)	46.59*** (1.99)
Censored subjects	Yes	Yes	Yes	Yes	No	No
Num. obs.	2136	2136	2136	2136	1284	1284
Num. ind. obs.	178	178	178	178	107	107

Table 4.3: Lottery valuations.

*Notes: OLS regressions with errors clustered at the subject level. "Trade-off": Trade-off between self-payoff and donations. "Charity": Outcome of the lottery for the charity. "Self": Outcome of the lottery for the subject. "Ambiguity": Lottery under ambiguity. "Risk": Lottery under risk. The symbol "×" is used for interaction variables. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.*

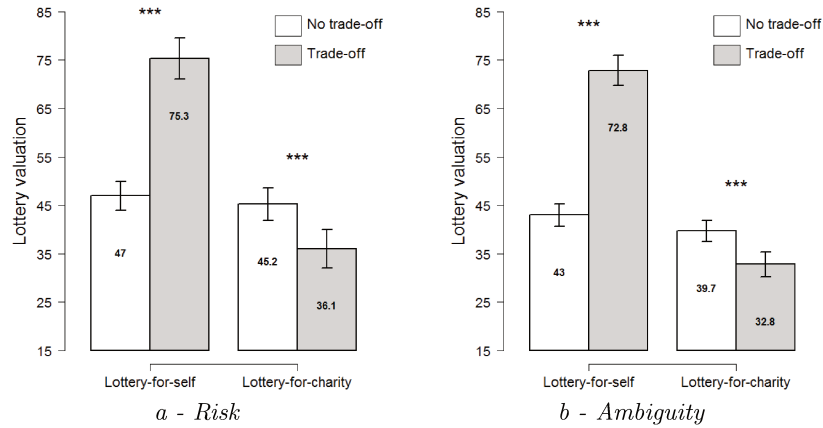


Figure 4.3: Comparison of lottery valuations (%) with and without a self *vs.* charity trade-off

*Notes: In trade-off situations both the lotteries-for-self and the lotteries-for-charity are valued differently highlighting excuse-driven behavior. Censored subjects are not excluded. Mean values are written within bars'. White bars provide valuations without a trade-off and grey bars provide valuations with a trade-off. The horizontal lines represent 95% confidence intervals. Stars represent the level of significance for paired t-tests at the subject level. *** $p < 0.01$.*

4.4.2.2 Excuse-driven behavior under ambiguity compared to risk

We next compare the lottery valuations under risk and under ambiguity, first by decision type, then by lottery type. Figure 4.4 provides a visual presentation of these comparisons.

For all types of decisions valuations under ambiguity are lower than valuations under risk (Figure 4.4-a). The mean self-valuation of the lotteries-for-self is €4.70 (47.0% of €10). Since risk neutral individuals would value this lottery €5 (since its outcome is €10 with one chance out of two), it indicates that subjects exhibit some risk aversion ($p = 0.046$). The valuation is even lower under ambiguity: €4.30 ($p = 0.009$), which reveals the presence of ambiguity aversion.

We observe a similar pattern for the charity-valuation of lotteries-for-charity. Subjects are averse to risk-for-charity and ambiguity-for-charity, as both valuations are significantly lower than 50% (respectively 45.2% and 39.7%, $p = 0.006$ and $p < 0.001$). Furthermore, the mean valuation decreases by 5.58 p.p. under ambiguity compared to risk ($p < 0.001$). The difference in valuation between ambiguity and risk is similar for lotteries-for-charity and for lotteries-for-self ($p = 0.391$). This reveals that there is no significant difference in ambiguity aversion when evaluating ambiguity-for-self or

ambiguity-for-charity.

The charity-valuation of lotteries-for-self is above 50% ($p < 0.001$). The mean valuation is €7.53 under risk and €7.28 under ambiguity. For this type of decision, a higher lottery valuation increases the number of decisions in which the subject receives the lottery and decreases the number of decisions in which the charity receives a safe payoff. A higher lottery valuation is thus associated with lower donations. However, valuation decreases under ambiguity compared to risk but this is only borderline significant ($p = 0.094$). Without controlling for risk and ambiguity preferences, there is a trend suggesting that ambiguity tends to reduce excuse-driven behavior instead of reinforcing it.

The last type of valuation involves self-valuations of lotteries-for-charity. For this type of decision, a lower lottery valuation decreases the number of decisions in which the charity receives the lottery and increases the number of decisions in which the subject receives a safe payoff. A lower lottery valuation is thus associated with less donations. The mean valuation is 36.07% under risk and 32.81% under ambiguity. Without controlling for risk and ambiguity preferences, ambiguity decreases valuations significantly ($p = 0.034$). It thus seems that for lotteries-for-charity ambiguity increases the moral wiggle room. However, without using as a baseline the charity-valuation of lotteries-for-charity, we cannot disentangle the effect of ambiguity aversion from the effect of excuse-driven behavior, as both effects tend to decrease lottery valuations. Thus, we now compare the difference in lottery valuations with and without a self *vs.* charity trade-off under risk and under ambiguity to test Hypothesis 3 and Hypothesis 4. These differences are presented in Figure 4.4-b. Their comparisons are given by the coefficients associated with the “*Trade-off* × *Ambiguity*” variable in Table 4.3 (model 1 for lotteries-for-self and in model 3 for lotteries-for-charity).

Result 3. *Excuse-driven behavior is not reinforced under ambiguity-for-self compared to risk-for-self.*

Support for Result 3: A larger difference between the self-valuations of lotteries-for-self (no self *vs.* charity trade-off) and the charity-valuations of lotteries-for-self (self *vs.* charity trade-off) corresponds to reinforced excuse-driven behavior. Figure 4.4-b shows that the difference between self-valuation and charity-valuation of lotteries-for-self is 28.34 p.p. under risk and 29.82 p.p. under ambiguity. Descriptively, an increased difference under ambiguity (1.48 p.p.) goes in the direction of reinforced excuse-driven

behavior under ambiguity. However, this difference is not statistically significant (Table 4.3, model (1) - effect associated with “*Trade-off* \times *Ambiguity*”: $p=0.486$). Therefore, there is no evidence in favor of Hypothesis 3. This result is robust to the exclusion of censored subjects (see Section C2).

Result 4. *Excuse-driven behavior is not reinforced under ambiguity-for-charity compared to risk-for-charity.*

Support for Result 4: A larger difference between charity-valuations of lotteries-for-charity (no self *vs.* charity trade-off) and self-valuations of lotteries-for-charity (self *vs.* charity trade-off) corresponds to reduced excuse-driven behavior. Figure 4.4-b shows that this difference is -9.17 p.p. under risk and -6.85 p.p. under ambiguity. Descriptively, a decreased difference under risk goes in the direction of reinforced excuse-driven behavior under risk. (Table 4.3, effect associated with “*Trade-off* \times *Ambiguity*”: $\beta = 2.32$, $p = 0.273$ in model 3 and $\beta = 1.40$, $p = 0.609$ in model 5). It rejects that ambiguity reinforces excuse-driven behavior (one-sided t-test, $p = 0.863$). We therefore conclude that Hypothesis 4 is not supported.

Thus, controlling for the direct effect of ambiguity on lottery valuations reverses potential conclusions. In fact, disentangling between direct effects and indirect excuse-driven effects reveals no evidence of reinforced excuse-driven behavior under ambiguity compared to risk, both for lotteries-for-self and for lotteries-for-charity. We conclude that any type of uncertainty is used as an excuse not to give and thus the type of uncertainty is not prevalent.

4.4.3 Excuse-driven behavior under partial and full ambiguity

In this section, we distinguish between the two levels of ambiguity to assess if the previous results hold for both. We study decisions under partial ambiguity ($\tilde{p} \in [0.25, 0.75]$) and decisions under full ambiguity ($\tilde{p} \in [0, 1]$) separately before comparing them with decisions under risk.

Hypothesis 1-A regarding excuse-driven behavior under ambiguity-for-self is supported both for partial ambiguity and for full ambiguity ($p < 0.001$ in both cases). Hypothesis 2-A regarding ambiguity-for-charity is supported for partial ambiguity ($p < 0.001$) and full ambiguity ($p = 0.009$) when censored subjects are included. However,

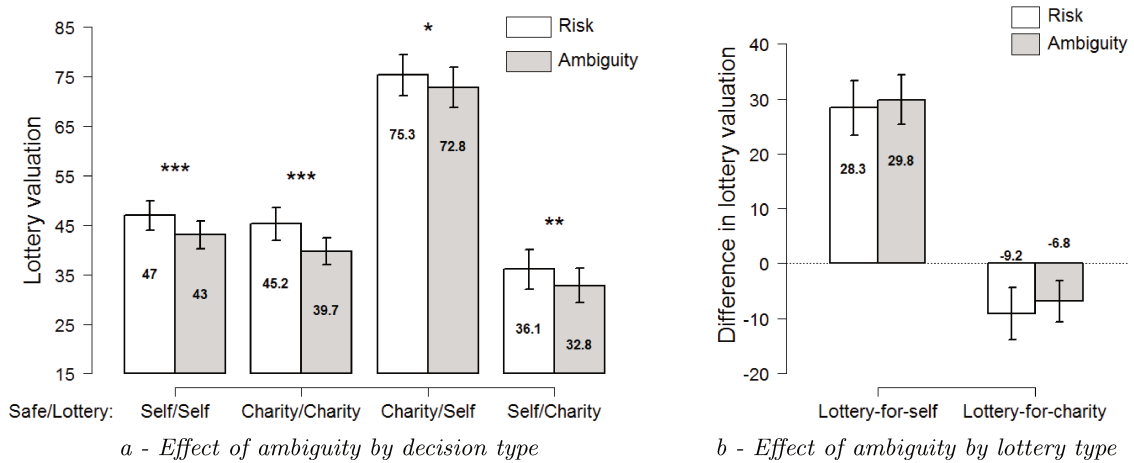


Figure 4.4: Comparison of lottery valuations (%) under risk and under ambiguity

Notes: (a) Mean lottery valuation for each decision. (b) Difference in mean lottery valuations between decisions with a self vs. charity and without this trade-off either for lotteries-for-self or lotteries-for-charity. While subjects behave differently under risk and ambiguity (a), excuse-driven behavior is not reinforced under ambiguity compared to risk (b). Censored subjects are not excluded. Mean values are written at bars' centers. White bars provide valuations under risk and grey bars provide valuations under ambiguity. The horizontal lines represent 95% confidence intervals. Stars represent the level of significance for paired *t*-tests. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

when excluding these subjects, Hypothesis 2-A is supported neither for partial ambiguity ($p = 0.383$), nor for full ambiguity ($p = 0.979$).

To compare behavior under risk vs. ambiguity by decision type, we regress “Partial ambiguity” (equal to 1 for lotteries under partial ambiguity) and “Full ambiguity” (equal to 1 for lotteries under full ambiguity) on the lottery valuation for each decision type. Errors are clustered at the subject level, as each subject makes three choices per decision type. Table 4.4 reports the estimates. Full ambiguity decreases valuations for all decision types. Partial ambiguity has a negative effect on valuations for all decision types at the exception of the self-valuation of lotteries-for-charity ($p = 0.297$). Our conclusions by decision type are thus largely confirmed for each ambiguity level. When comparing valuations within ambiguity levels, we find no statistical difference between valuations under partial ambiguity and under full ambiguity even if, descriptively, most valuations under full ambiguity are lower than corresponding valuations under partial ambiguity (except for Self/Charity decisions).

There is evidence for Result 3 for any level of ambiguity. Under partial ambiguity, charity-valuations of lotteries-for-self increase by 30.03 p.p. compared to self-valuations

of the same lotteries. Under full ambiguity, they increase by 29.62 p.p. The difference with the increase under risk (28.34 p.p.) is not significantly different either from the increase under partial ambiguity (1.68 p.p., $p = 0.403$) or for the increase under full ambiguity (1.27 p.p., $p = 0.667$). Likewise, Result 4 is supported for any level of ambiguity. Under partial ambiguity, self-valuations of the lotteries-for-charity decrease by 8.16 p.p. compared to charity-valuations of the same lotteries. Under full ambiguity, they decrease by 5.54 p.p. The difference with the decrease under risk (9.17 p.p.) is not significantly different from the decrease under partial ambiguity (1.01 p.p., $p = 0.667$) or from the decrease under full ambiguity (3.64 p.p., $p = 0.109$). In conclusion, previous findings hold for both levels of ambiguity.

Type of decision (Safe/Lottery):	<i>Dependent variable: Lottery valuation (%)</i>			
	Self/Self (1)	Charity/Charity (2)	Charity/Self (3)	Self/Charity (4)
Partial ambiguity	-3.17** (1.60)	-4.37*** (1.60)	-1.49 (1.43)	-3.36** (1.62)
Full ambiguity	-4.69** (1.87)	-6.78*** (1.64)	-3.41* (2.02)	-3.15* (1.65)
Intercept	46.95*** (1.52)	45.24*** (1.70)	75.29*** (2.14)	36.07*** (2.01)
Num. obs.	534	534	534	534
Num. ind. obs.	178	178	178	178

Table 4.4: Effect of partial ambiguity and full ambiguity by decision type.

*Notes: OLS regressions with errors clustered at the subject level. "Partial ambiguity": Lottery under partial ambiguity. "Full ambiguity": Lottery under full ambiguity. Lotteries under risk compose the reference category. Censored subjects are not excluded. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$*

4.4.4 Excuse-driven behavior and prior experience

Haisley and Weber (2010) differentiate between two types of subjects based on their previous experiences. They define "constrained" subjects as those that have made decisions under risk and ambiguity involving only themselves earlier in the experiment. In these decisions, there is no tension between self-interest and fairness. In contrast, the "unconstrained" subjects have not such experience. Based on this concept, Haisley and Weber (2010) conclude to a higher prevalence of self-excusing behavior under ambiguity compared to risk, but only for unconstrained subjects. In our experiment, some deci-

sions were also made by unconstrained subjects while others were made by constrained subjects. We can thus test whether Result 3 and Result 4 could be explained by the proportion of decisions made by constrained subjects.

Applying the definition of Haisley and Weber (2010), we consider that a decision is made by a constrained subject if he has previously made a decision without a self *vs.* charity trade-off.¹⁶ It gives 2004 constrained decisions out of the 2136 decisions. 76 different subjects made unconstrained decisions.

To test whether ambiguity has a different impact on unconstrained decisions compared to constrained decisions, we focus on decisions that are potentially different when constrained: decisions with a self *vs.* charity trade-off. We regress on the lottery valuation: two dummy variables, “*Ambiguity*” (equal to 1 if decisions are made under ambiguity) and “*Unconstrained*” (equal to 1 if decisions are unconstrained), their interaction (“*Ambiguity* × *Unconstrained*”), and a continuous variable “*Trial*” (equal to the number of the decision). This last variable controls for a possible time effect since unconstrained decisions were made before constrained decisions. Finally, we distinguish charity-valuations of lotteries-for-self (model 1) from self-valuations of lotteries-for-charity (model 2) since reinforced excuse-driven behavior under ambiguity has opposite effects on lotteries-for-self and lotteries-for-charity.

As reported in Table 4.5, ambiguity does not have a significantly different impact on unconstrained decisions and on constrained decisions on any decision types (Charity/Self decisions: $p = 0.719$, Self/Charity decisions: $p = 0.658$). Moreover, the coefficient signs are opposed to the expected ones, as reinforced excuse-driven behavior under ambiguity for unconstrained decisions was expected to be associated with a positive effect in model (1) and a negative effect in model (2).

In conclusion, the behavioral differences between constrained and unconstrained decisions do not explain why we find no support for Hypothesis 3 and Hypothesis 4.

¹⁶Other definitions could be used. For example, differentiating constrained decisions for lotteries-for-charity and lotteries-for-self or differentiating constrained decisions under risk and under ambiguity. The definition implemented favors the potential effect of being constrained because decisions are more likely to be classified as constrained.

Type of decision:	<i>Dependent variable: Lottery valuation (%)</i>	
	Charity/Self (1)	Self/Charity (2)
Ambiguity \times Unconstrained	-2.61 (7.26)	3.13 (7.25)
Ambiguity	-2.11 (1.63)	-3.56** (1.72)
Unconstrained	6.16 (6.26)	-9.80 (6.60)
Trial	-0.05 (0.46)	-0.59 (0.42)
Intercept	74.88*** (4.28)	41.15*** (3.76)
Num. obs.	534	534
Num. ind. obs.	178	178

Table 4.5: Effect of being unconstrained on lottery valuation under ambiguity in situations with a self *vs.* charity trade-off.

*Notes: OLS regressions with errors clustered at the subject level. "Ambiguity": Lottery under ambiguity. "Unconstrained": Decision made by an unconstrained subject. "Trial": Number of the decision. The symbol " \times " is used for interaction terms. Decision types are referred following the norm Safe/Lottery. Censored subjects are not excluded. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.*

4.5 Discussion and conclusion

Our laboratory experiment aims at disentangling between a direct, non-excuse driven, effect of risk and ambiguity on charitable giving and excuse-driven behavior. Our findings indicate that the valuation of lotteries for self is affected by self-excusing behavior: individuals use the pretext of risk to decrease their donations. The valuation of lotteries for self is more strongly affected by such behavior than the valuation of lotteries for others. Overall, we find no evidence that excuse-driven behavior is reinforced under ambiguity compared to risk. It suggests that individuals use any type of uncertainty as an excuse not to give but do not care about the type of uncertainty.

Regarding risk as an excuse not to give, our results are in line with those of Exley (2015a). The only difference is that Exley (2015a) finds evidence of excuse-driven behavior in the valuation of lotteries-for-charity both with and without censored subjects, while we only find evidence of such behavior when including censored subjects in the

analysis. Also, in Exley (2015a), the effect size is about four times larger than ours.¹⁷ We reject that these differences are driven by a difference in statistical power, but a lower number of decisions in our within-subject design may reduce behavioral changes between decisions.¹⁸ Indeed, subjects may have a better recollection of previous decisions that could lead to some anchoring effect. Also Exley (2015b) provides converging evidence of the existence of excuse-driven response to charity performance indicators: the low-rating of a charity gives individuals an excuse not to give. A low rating affects the benefit of the donation and is thus related to risk-for-charity. Overall, these different studies point to the existence of excuse-driven behavior under risk-for-charity.

Our extension of Exley (2015a) to ambiguity reveals no difference in the use of moral wiggle room under risk or ambiguity. This result contrasts with those of Haisley and Weber (2010) that conclude on reinforced self-serving behavior under ambiguity compared to risk for a group of subjects. As already pointed out, Haisley and Weber (2010) based their analysis on the evaluation of past decisions. Differences between risk and ambiguity can therefore be due to an excuse-driven use of ambiguity when deciding to donate, but also to self-serving selective recalls. In our settings, selective memory is excluded by design, and this may contribute to explain the difference between our results and those of Haisley and Weber (2010).

Our study highlights differences in excuse-driven behavior when individuals evaluate uncertainty for themselves or for a charity. It extends the findings of previous studies that have compared risk evaluation in both settings. While Reynolds et al. (2009) and Eriksen and Kvaløy (2010) find increased risk aversion when deciding on behalf of others, Chakravarty et al. (2011) conclude that individuals are more risk seeking. We do not find difference in either risk attitudes or ambiguity aversion between lotteries for self and lotteries for charity. This suggests that individuals assume that the charity values uncertainty like themselves. However, the evaluation of lotteries for self is more prone to excuse-driven behavior than the evaluation of lotteries for charity, both under risk and under ambiguity.

¹⁷In Exley (2015a) the effect size for excuse-driven behavior under risk-for-charity without censored subjects is -9.74 (Table B.7) while our effect size is -2.59 (Table 4.3, model 5).

¹⁸In Exley (2015a) subjects evaluate seven different lotteries that vary regarding their probability of paying the non-null amount, while our design includes a unique probability ($p = 0.5$). From a statistical point of view, we have collected more independent observations (180 subjects against 99 subjects) and the magnitude of excuse-driven behavior is globally stable for all probabilities in her study (except a decrease for probabilities smaller than 0.1).

Uncertainty on the costs of donations is quite rigid from the charity perspective as they are induced by the intrinsic characteristics of potential donors. Reducing uncertainty on the benefits of a donation is more feasible (e.g. by targeting possibilities of embezzlement and mismanagement), as this type of uncertainty is determined by the characteristics of the charity. Our findings can thus stress the importance for non-profit organizations of reducing uncertainty to increase donations. Indeed, reducing uncertainty would increase donations through a twofold process: it increases the donation value and it discourages excuse-driven behavior. We also inform on the gain of providing potential donors with some precise information on the costs and benefits of the donations. Indeed, our results show that more precise information increases the value of the donation but does not decrease excuse-driven behavior. As clarifying randomness usually comes at a cost, better understanding its gain can help design more efficient interventions for the collection of charitable donations.

Chapter 5

Do laboratory measures of risk attitude predict behavior under risk in and outside of the laboratory?¹

Risk is at the core of economic decisions. For example, risk preferences are an essential element in any discussion regarding finance, insurance and the asset markets. It is thus necessary to understand how individuals behave in risky environments in order to properly understand financial decision-making. To address this, numerous experimental methodologies dedicated to measuring individual risk attitudes have emerged (for a survey, see Harrison and Rutström, 2008). In this study, we assess the external validity of five of the most influential risk-preference-elicitation procedures by testing whether they can explain laboratory financial decisions and behavior in the field. A great deal of research has pursued the question of how to best measure risk preferences, yet one question that has received little attention is how well these attempts actually map into behavior by people in the field.

While the default assumption seems to be that these measurements are useful, this has been called into question by works such as Friedman et al. (2014). The first work on this topic was conducted by Binswanger (1980), which attempted to measure risk prefer-

¹Co-authored with Gary Charness, Theo Offerman and Marie Claire Villeval.

ences of farmers in India. The method he designed presented a choice of seven lotteries, which involved a coin flip and which varied the payoffs for heads and for tails. Regressions on choices made indicate that “the independent variable which most consistently correlated with the ordinal risk measure turned out to be ‘luck’ – that is, past coin flip realizations during earlier trials of the Binswanger procedure,” and so does not inspire confidence that a stable trait is being measured. Friedman et al. (2014) note: “Subsequent investigators, notably Jacobson et al. (2007), would have even greater difficulty getting estimates from the Binswanger procedure to predict out-of-sample data.” They also mention historical problems with the most common contemporary measure (Holt and Laury, 2002, hereafter HL), with pie-chart displays (Hey and Orme, 1994), and with physiological measures (Sapienza et al., 2009). Overall, they note: “The different ways of eliciting risk parameters in cash-motivated, controlled economics experiments yield different general results.” (see He et al., 2018 for a review). Loomes (1988) is one of the first studies to notice such inconsistencies when evaluating risk attitude using certainty equivalents.

More recently, Deck et al. (2013) find considerable within-subject variation in behavior between four measures of risk attitude : HL, Eckel and Grossman (2008, hereafter EG), “the deal or no deal” method from Deck et al. (2008) and the “balloon analogue” risk task from Lejuez et al. (2002, hereafter BART). Crosetto and Filippin (2016) confirm this finding by comparing behavior in EG, HL, Gneezy and Potters (1997, hereafter GP) and the procedure introduced by Crosetto and Filippin (2013). The inconsistency is robust when also considering measures from psychology and cognitive neuroscience as shown by the comparisons of decisions in HL, BART, Columbia card task, marbles tasks and two developed in-house measures conducted in Pedroni et al. (2017). Finally, Dulleck et al. (2015) reach a similar conclusion based on HL and the procedure introduced by Andreoni and Harbaugh (2009). These findings raise the question: Can one’s underlying (“true”) risk preferences be accurately measured in the laboratory?

A critical aspect of laboratory experiments is their generalizability, *i.e.*, that insights gained in the lab can be extrapolated to the world beyond (Levitt and List, 2007). However, the adequacy between some measures of risk attitude and other behavior under risk has been analyzed almost exclusively in studies focusing on a single risk measure. Based on a self-reported risk measure that they introduce, Dohmen et al. (2005, hereafter WTR for Willingness to Take Risks) report that investment in stocks, actively engaging

in sports, being self-employed, and smoking are related to risk attitude.² Using the same measure in rural Thailand, [Hardeweg et al. \(2013\)](#) confirm its relation to being self-employed and also find a relation to the purchase of lottery tickets. [Lusk and Coble \(2005\)](#) and [Andersen et al. \(2008\)](#) find that risk aversion measured with HL is negatively correlated with the consumption of genetically-modified food, cigarette smoking, heavy drinking, being overweight and seat belt non-use. [Guiso and Paiella \(2008\)](#) highlight a positive link between a direct measure of absolute risk aversion based on a willingness to pay and the likelihood to face income uncertainty or to become liquidity constrained. [Lejuez et al. \(2002\)](#) find that the measure of risk aversion introduced in their study correlates with the self-reported frequency of addictive, unsafe and unhealthy behavior. [Verschoor et al. \(2016\)](#) use GP on a sample of farmers and find that it correlates with some risky choices (*e.g.*, the purchase of fertilizer) but not others (*e.g.*, growing of cash crops). [Falk et al. \(2018\)](#) use an index of risk preferences based on certainty equivalents and WTR that correlates with self-employment and smoking. Finally, [Sutter et al. \(2013\)](#) study the impact of children and adolescents' risk aversion on smoking, drinking, the body mass index (BMI), savings, and conduct at school using an elicitation method based on certainty equivalents ([Wakker, 2010](#)). Risk aversion is only related to the BMI. Therefore, while consistency between measures has been extensively and systematically studied, an extensive and systematic analysis of their ability to explain risky behavior in other settings is missing in the literature.

To the best of our knowledge, the only other study aiming to provide a systematic evaluation of the measures of risk attitude is [Galizzi et al. \(2016\)](#).³ They test the relation between three measures (HL, GP, and WTR) and field behavior based on a UK representative sample. They find that none of these measures are related to smoking, junk-food consumption, regularly saving, or savings horizons. HL and EG are related respectively to the regular consumption of fruits and vegetables, and having a private pension fund. WTR is associated with heavy alcohol drinking. Overall, they thus find mixed evidence of a link between measures of risk attitude and field behavior. In comparison, we consider a larger range of measures of risk attitude. We use different types of behavior as a benchmark (risky financial decisions in the laboratory and field

²However, as described below, many studies in psychology have found major differences in risk attitude across such domains (*e.g.*, [Weber et al., 2002](#)).

³Both studies were developed at about the same time. We designed and conducted our experiment prior to the release of this working paper, without being aware of their project.

behavior). Finally, we also focus more on financial decisions as field behavior that can possibly be explained by risk aversion.

Two valuable characteristics of a measure of risk attitude can be identified: simplicity and theoretical compliance (Charness et al., 2013). Simplicity is thought to decrease measurement errors and misunderstanding. Relying on more elaborate theories is thought to permit measures to describe behavior more precisely. However, achieving both objectives is difficult, since compliance with advanced theory requires the implementation of complex procedures. Risk measures can thus be ranked according to this trade-off between simplicity and theoretical refinement. We select five of the most popular procedures currently in use in experimental economics, which vary regarding their level of complexity. At one end of the spectrum is the complex procedure described by Tanaka et al. (2010, hereafter TCN), which allows the researcher to identify the utility-curvature and probability-weighting parameters of prospect theory. At the other end of the spectrum, the non-incentivized survey questions introduced by Dohmen et al. (2011) have no specific relation to any particular economic theory. Between these two extremes, we also consider three incentivized methods: an investment task proposed by Gneezy and Potters (1997) and adapted by Charness and Gneezy (2010), a choice of one lottery out of six introduced by Eckel and Grossman (2008), and finally a more complicated procedure based on ten choices between paired lotteries proposed by Holt and Laury (2002).

We test the external validity of measures of risk attitude based on two different types of risk-related behavior. The first set of risky behavior is composed of laboratory financial decisions. They are composed of a portfolio task, an insurance task, and a mortgage task. In contrast to measures of risk attitude, their instructions are context-rich in the sense that the type of decision is explicitly mentioned when describing the tasks. These tasks thus constitute an intermediate step between standard laboratory measures and field behavior, since they introduce some context but are still artificial situations. The second set of risky behavior is composed of naturally-occurring field behavior that reflects the risk exposure that individuals are willing to bear in their everyday lives. The risk attitude in the field is assessed based on insurance decisions, employment decisions, and investment decisions that can be either monetary or in properties.

We collect decisions in both settings using a representative sample of the Dutch population: subjects of our experiment are part of the Longitudinal Internet Studies

for the Social sciences (LISS) panel. Studying a representative sample of the population increases the likelihood that subjects face major financial decisions under risk such as investing or purchasing insurance compared, for example, to a student sample. Moreover, more complex measures of risk attitude perform better for individuals with high numeracy skills. Thus using a representative sample makes it more likely that our conclusions are not biased by numeracy. [Noussair et al. \(2013\)](#) used this panel to study whether risk aversion, prudence, and temperance are related to six types of financial decisions. They implement a single measure of risk aversion based on five binary choices between a lottery and a safe amount. They find that owning real-estate, long-term insurance, or loans are unrelated to any of the risk measures. Individuals with higher temperance are more likely to have a savings account and are less likely to have unpaid balances on a credit card. Neither risk aversion, prudence nor temperance are related to real-estate investments, risky investments or having a loan. In comparison, we do not consider higher-order risk attitudes but we vary how risk aversion is measured. We find that measures of risk attitude are indeed related to behavior in laboratory financial decisions. We also find that more complex measures under-perform. However, the measures of risk attitude in the lab—either simple or complex—consistently fail to predict risk attitude in the field. Our conclusion highlights an apparent lack of external validity of these common measures of risk attitude.

The remainder of this paper is organized as follows: [Section 5.1](#) describes the experimental design. The data-analysis methodology and the results are reported in [Section 5.2](#). [Section 5.3](#) concludes.

5.1 Experimental design

In this section, we first present our sample of subjects and experimental procedures. Then, we describe our measures of risk attitude in the laboratory: risk-attitude elicitation procedures and framed laboratory financial decisions. Finally, we introduce our measures of risk attitude in the field.

5.1.1 Sample of subjects and experimental procedures

We conducted our experiment on a sample of the LISS panel composed of 1122 individuals from different households. The distributions of the age and income of our sample

confirm its diversity. Subjects are on average 51 years old (s.d. = 16.44). The youngest of our subjects is 18 years old while the oldest is 92 years old. Their net monthly incomes are on average €1473 (s.d. = 2291). Monthly incomes range from no revenue to a maximum of €69054. Finally, 46% of our sample is male.

Our experiment is composed of five measures of risk attitude, three laboratory financial decisions and six measures of risk exposure in the field. The risk attitude of each subject is measured using a single procedure (between-subject design). It enables us to guarantee that our main focus of interest is not affected by a carry-over between procedures (Charness et al., 2012).

All subjects make decisions for the three laboratory financial tasks. Instructions of the experimental measures are in Section D1. Subjects were paid based on their answers in one of these four parts. Their earnings were on average €9.03 (s.d. = 10.67). Subjects were paid by bank transfer at the end of the experiment. The survey questions used to assess risk attitude in the field were asked of all subjects.

5.1.2 Our measures of risk attitude

The five measures we use to measure risk attitude are: WTR, GP, EG, HL, and TCN.

Self-reported measure: Dohmen et al. (2011). The simplest of all procedures consists of asking subjects directly if they are willing to take risks. Subjects rank their willingness to take risks on a 0 to 10 scale with 0 being the lowest willingness and 10 the highest. The exact phrasing of the question is: “How do you see yourself: are you generally a person who is fully prepared to take risks or do you try to avoid taking risks?”. This question is completed by a similar question specifically targeting financial decisions: “How would you rate your willingness to take risks concerning financial matters?”. Subjects also answer on a 0 to 10 scale. The general question is referred to hereafter as “WTR G (for General)” while the specific question is referred to as “WTR S (for Specific)”. In contrast to the procedures below, this mechanism is not incentivized and is based on reported preferences rather than revealed preferences. It is thus impossible to estimate risk-attitude parameters based on these questions.

Investment task: Gneezy and Potters (1997). This procedure is perhaps the most straightforward procedure based on revealed preferences. Subjects receive an endowment

of €8. They are offered to invest in a lottery that pays 2.5 the amount invested with a 50% chance and that pays €0 otherwise. For practical issues, their investment must be divisible by 0.01 (*i.e.*, 801 different options). Whatever is not invested is kept. Formally, subjects choose an investment $k \in [0, 8]$ with $(100 \times k) \in \mathbb{N}$. They are paid according to the lottery $(8 - k, 0.5; 8 + 2.5 \times k, 0.5)$. The expected earning and the earning variance are thus increasing with the investment. Risk-neutral and risk-seeking subjects should invest all their endowments. Investment should decrease as risk aversion increases.

Ordered lottery selection: Eckel and Grossman (2008). This procedure is close to Binswanger (1980), and also comparable to that of Gneezy and Potters (1997) but with a more narrow decision space. Subjects select one two-outcome lottery out of six possibilities, as introduced in Table 5.1. The first lottery is a safe lottery paying €7. The next four lotteries are obtained by adding €2 to one outcome and deducting €1 from the other outcome. Both outcomes being equally likely, the expected value and the variance are increasing from one lottery to the next. Risk-averse subjects should select one of the five first lotteries depending on their degrees of risk-aversion. Only risk-neutral (or very slightly risk-averse/risk-seeking individuals) should select the fifth lottery. The last lottery is obtained by adding and deducting the same amount of €2.5 to the two outcomes of the fifth lottery. While it is impossible to discriminate between risk-neutral and risk-seeking people, the last lottery is the unique choice for people who are at least moderately risk-seeking.

Lottery	1	2	3	4	5	6
<i>Low Payoff</i> ($p=0.5$)	€7	€6	€5	€4	€3	€0.5
<i>High Payoff</i> ($p=0.5$)	€7	€9	€11	€13	€15	€17.5

Table 5.1: Our EG-style payoff matrix.

List of paired lotteries: Holt and Laury (2002). This procedure requires subjects to make 10 decisions. It is more complicated than previous procedures, but it does enable us to disentangle risk-seeking subjects from risk-neutral subjects. Ten ordered choices between two lotteries denoted A or B are presented to subjects (Table 5.2). Lottery A always pays either €8.0 or €6.4 while Lottery B pays €15.4 or €0.4. The probability

that both lotteries pay the high payoff is varied between choices from 0.1 to 0.9. Lottery A is safer than Lottery B, however, the expected value of lottery A increases from €6.56 to €8 while the expected value of Lottery B increases from €1.9 to €15.4. For the first four decisions, only risk-seeking subjects should choose Lottery B as this lottery has a lower expected value and more risk than Lottery A. After these decisions, risk-averse subjects might switch to Lottery B. The later they switch to Lottery B, the more risk averse they are. The last decision is singular, as no risk is involved. It tests if subjects have understood the instructions. If this procedure is selected for payment in our experiment, one decision is randomly selected for payment. Note that there should be (at most) one crossing from the left side to the right side. A serious issue is that there are often multiple crossings in the experimental population, particularly in rural areas of undeveloped nations, suggesting a lack of comprehension.

Lottery A				Lottery B			
$p(\text{€}8)$	Outcome	$p(\text{€}6.4)$	Outcome	$p(\text{€}15.4)$	Outcome	$p(\text{€}0.4)$	Outcome
0.1	€8	0.9	€6.4	0.1	€15.4	0.9	€0.4
0.2	€8	0.8	€6.4	0.2	€15.4	0.8	€0.4
0.3	€8	0.7	€6.4	0.3	€15.4	0.7	€0.4
0.4	€8	0.6	€6.4	0.4	€15.4	0.6	€0.4
0.5	€8	0.5	€6.4	0.5	€15.4	0.5	€0.4
0.6	€8	0.4	€6.4	0.6	€15.4	0.4	€0.4
0.7	€8	0.3	€6.4	0.7	€15.4	0.3	€0.4
0.8	€8	0.2	€6.4	0.8	€15.4	0.2	€0.4
0.9	€8	0.1	€6.4	0.9	€15.4	0.1	€0.4
1	€8	0	€6.4	1	€15.4	0	€0.4

Table 5.2: Our HL-style payoff matrix.

Multiple lists of paired lotteries: Tanaka et al. (2010). The procedure introduced by Tanaka et al. (2010) is the most complicated procedure implemented in this study, as it consists of two lists of 14 decisions each.⁴ This higher level of complexity is explained by the fact that this procedure relies on prospect theory as an alternative framework to expected utility. While the expected utility is characterized only by the concavity of a utility function, prospect theory is also characterized by a probability weighting parameter. Each combination of decisions in the two price lists determines a combination of prospect theory parameters.

⁴The original procedure is composed of three price lists. The additional price list is dedicated to estimating a loss aversion parameter. As loss attitude was not in the scope of our study, we have not implemented this last price list.

Both lists are composed of a constant lottery (Lottery A) and a lottery for which one outcome is increasing from one row to another (Lottery B). In the first list introduced in the upper part of Table 5.3, Lottery A always pays €8 with probability 0.3 and €2 with probability 0.7. Lottery B pays €1 with probability 0.9 and, with probability 0.1, an amount increasing from €13.6 (first decision) to €340 (last decision). In the second list introduced in the lower part of Table 5.3, the Lottery A always pays €8 with probability 0.9 and €6 with probability 0.1. The Lottery B pays €1 with probability 0.3 and, with probability 0.7, an amount increasing from €10.8 (first decision) to €26 (last decision). For both price lists, more subjects should choose Lottery B when proceeding down the list as the value of Lottery A is constant while the value of Lottery B increases. Contrary to HL, this procedure enforces monotonic switching by asking subjects at which question they want to switch from Lottery A to Lottery B in each list.⁵ If this procedure is selected for payment, one decision is randomly selected in one of the two lists for payment.

5.1.3 Laboratory financial decisions

The three laboratory financial decisions reproduce three types of financial decisions under risk in the laboratory: a portfolio decision, an insurance decision, and a mortgage decision.

Portfolio task: The portfolio task reproduces an investment decision. Subjects are told that they have to manage a fund of €100. To invest this money, they have the choice between three projects. The first project pays a safe amount of €0.6 for each euro invested. The second project pays, for each euro invested, €0.2 with probability 0.5 and €1.4 with probability 0.5. The last project pays, for each euro invested, €0.2 with probability 0.8 and €4.2 with probability 0.2. Subjects can freely divide the €100 between the projects, but they have to invest all the money. For practical issues, investments must be non-negative integers.

⁵Note that this by no means ensures comprehension.

<i>First price list - TCN 1</i>							
Lottery A				Lottery B			
$p(\text{€}8)$	Outcome	$p(\text{€}2)$	Outcome	$p(\text{€}X)$	Outcome	$p(\text{€}1)$	Outcome
0.3	€8	0.7	€2	0.1	€13.6	0.9	€1
0.3	€8	0.7	€2	0.1	€15	0.9	€1
0.3	€8	0.7	€2	0.1	€16.6	0.9	€1
0.3	€8	0.7	€2	0.1	€18.6	0.9	€1
0.3	€8	0.7	€2	0.1	€21.2	0.9	€1
0.3	€8	0.7	€2	0.1	€25	0.9	€1
0.3	€8	0.7	€2	0.1	€30	0.9	€1
0.3	€8	0.7	€2	0.1	€37	0.9	€1
0.3	€8	0.7	€2	0.1	€44	0.9	€1
0.3	€8	0.7	€2	0.1	€60	0.9	€1
0.3	€8	0.7	€2	0.1	€80	0.9	€1
0.3	€8	0.7	€2	0.1	€120	0.9	€1
0.3	€8	0.7	€2	0.1	€200	0.9	€1
0.3	€8	0.7	€2	0.1	€340	0.9	€1

<i>Second price list - TCN 2</i>							
Lottery A				Lottery B			
$p(\text{€}8)$	Outcome	$p(\text{€}6)$	Outcome	$p(\text{€}X)$	Outcome	$p(\text{€}1)$	Outcome
0.9	€8	0.1	€6	0.7	€13.6	0.3	€1
0.9	€8	0.1	€6	0.7	€15	0.3	€1
0.9	€8	0.1	€6	0.7	€16.6	0.3	€1
0.9	€8	0.1	€6	0.7	€18.6	0.3	€1
0.9	€8	0.1	€6	0.7	€21.2	0.3	€1
0.9	€8	0.1	€6	0.7	€25	0.3	€1
0.9	€8	0.1	€6	0.7	€30	0.3	€1
0.9	€8	0.1	€6	0.7	€37	0.3	€1
0.9	€8	0.1	€6	0.7	€44	0.3	€1
0.9	€8	0.1	€6	0.7	€60	0.3	€1
0.9	€8	0.1	€6	0.7	€80	0.3	€1
0.9	€8	0.1	€6	0.7	€120	0.3	€1
0.9	€8	0.1	€6	0.7	€200	0.3	€1
0.9	€8	0.1	€6	0.7	€340	0.3	€1

Table 5.3: Our representation of TCN's price lists.

Formally, they are paid according to the lottery $L_{j,k}$, defined as:

$$L_{j,k} = \begin{cases} 0.6 \times (100 - j - k) + 0.2 \times (j + k) & \text{with } p = 0.4 \\ 0.6 \times (100 - j - k) + 0.2 \times j + 4.2 \times k & \text{with } p = 0.1 \\ 0.6 \times (100 - j - k) + 1.4 \times j + 0.2 \times k & \text{with } p = 0.4 \\ 0.6 \times (100 - j - k) + 1.4 \times j + 4.2 \times k & \text{with } p = 0.1 \end{cases}$$

They choose $(j, k) \in \llbracket 0, 100 \rrbracket^2$ such that $j + k \leq 100$.

Projects are increasing in expected value (€0.6 for the first, €0.8 for the second, and €1 for the third), but are also increasing in their payoff variances. Thus, investments in the second and third projects should decrease as risk aversion increases. We summarize

the decision in a single measure given by the expected value of the lottery $L_{j,k}$. The expected value should decrease as risk aversion increases.

Insurance task: The insurance task captures how subjects cover risks. Subjects are given an endowment of €10. However, this endowment may be lost with probability 0.1. They can partially insure themselves against this risk. They choose one insurance scheme out of five possibilities. Insurance schemes cost either €0, €0.5, €1, €1.5, and €2.5. If the endowment is lost, the insurance pays three times the insurance fee. Subjects are thus paid according to one of the five lotteries described in Table 5.4. Risk-seeking and risk-neutral subjects should choose not to buy any insurance (Lottery 1). The chosen insurance fee should increase as risk aversion increases.

Lottery	1	2	3	4	5
<i>Endowment lost ($p=0.1$)</i>	€0	€1.5	€3	€4.5	€7.5
<i>Endowment kept ($p=0.9$)</i>	€10	€9.5	€9	€8.5	€7.5
<i>Expected value</i>	€9	€8.7	€8.1	€7.8	€7.5

Table 5.4: Payoff matrix of the insurance task.

Mortgage task: The mortgage task assesses the repayment profile that subjects would prefer when investing in real estate. Subjects are told that they have taken out a loan of €10 that must be repaid in 10 years. Every year, they receive an income of €1.5, and they have to pay the interest on the loan. They have the choice between three options that vary regarding the interest rate of the first year and the volatility of following interest rates. With the first option, the interest rate is fixed at 7%. They thus pay €0.7 per year ($€10 \times 7\%$). With the second option, the interest rate is at 6% for the first year. The first year, they thus pay €0.6. Any following year, this rate may vary, up to 2% below its value of the previous year and up to 2% above its value of the previous year. With the third option, the interest rate is at 5% for the first year. Any following year, this rate may vary, up to 4% below its value of the previous year and up to 4% above its value of the previous year. To facilitate understanding, a figure showing the interest rates over 100 years is part of the instructions. Options are increasing regarding the risk taken but decreasing regarding the expected total payment. The number of the

chosen option is thus decreasing as risk aversion increases.

5.1.4 Field behavior

We have six measures of risk exposure in the field. Three of them target investment decisions, two involve insurance choices, and one involves employment choice. Risk aversion is expected to have an unambiguous impact on four of these measures in the field (savings, risky investments, insurance, and deductible), as they are directly related to the variance of the final outcome and widely used to assess risk-taking in the field. For the remaining two measures (self-employed and owning real-estate) the expected relationship is less straightforward. However, some previous studies have shown that risk attitudes may influence the decision to become self-employed or to invest in real estate. We thus include these measures to diversify our investigation of financial domains as there may be potential specific risk attitudes across these. We discuss each of the field behavior in turn below.

The first measure gives the total balance that subjects have in their current accounts, savings accounts, term deposit accounts, savings bonds or savings certificates, and bank savings schemes. It is expressed in thousands of euros.⁶ One would expect that a more risk-averse individual would have a higher degree of (precautionary) savings, to guard against short-term financial reverses. Thus, we feel that savings will increase as risk aversion increases (for a given income) since savings are safe, and so this should be positively correlated with one's measured financial risk preferences. This measure has previously been used to link experimental and field behavior by, for example, Noussair et al. (2013), Sutter et al. (2013) and Galizzi et al. (2016).

The second measure tells us the percentage of earnings that is invested in risky accounts. Risky accounts include, but are not limited to, growth funds, share funds, bonds, debentures, stocks, options, or warrants. In general, we expect that the percentage of earnings invested in risky accounts will decrease with risk-aversion, so that one would expect risky financial investments in the field to be positively correlated with risky financial decisions in our own (smaller-stake) investment tasks. This measure has been used previously in Dohmen et al. (2011), Noussair et al. (2013) and Drerup et al. (2017).

The last investment measure concerns owning real-estate investment properties. It is

⁶The value of the savings can be negative to capture the position of an individual with more debts than deposits.

equal to one if subjects own real estate that is not used as their own home, second home or holiday home. While real estate typically increases in value over time (and so might be considered non-risky), many of us remember the collapse in prices in the late 2000's, with properties losing as much as 75% of their value, and the large losses in our own financial portfolios. One could also consider that the relative irreversibility of real-estate investment and its lack of liquidity make it riskier. So owning investment real estate, generally speaking, involves more risk than savings but less than stocks. These factors lead us to expect that owning investment real estate will be negatively correlated with risk aversion. This measure has been used previously in [Noussair et al. \(2013\)](#).

People who dislike risk are more likely to wish to insure against loss, even paying a substantial premium to do so. Our first insurance measure is related to financial insurance. It tells us if subjects have a single-premium insurance policy, a life annuity insurance, or endowment insurance (not linked to a mortgage). This measure is equal to one if the subject possesses any financial insurance. Since a physical calamity could be disastrous, leaving one's family without income, risk aversion would seem to be closely linked to the desire to purchase such insurance. We expect that the likelihood of being insured will increase as risk aversion increases. This measure has also been used previously in [Noussair et al. \(2013\)](#).

The second insurance measure concerns health insurance. It is equal to one if subjects have chosen a voluntary deductible for their health insurance. A higher deductible increases the variance of outcomes. We thus expect that the likelihood of choosing a deductible will decrease as risk aversion increases. While we are not aware of any studies using this measure to assess the performance of laboratory measures of risk attitude, some studies use insurance deductible choices directly to estimate risk attitude (e.g., [Cohen and Einav, 2007](#); [Sydnor, 2010](#) and [Barseghyan et al., 2013](#)).

Finally, we consider whether individuals are self-employed. This measure is equal to one if subjects are freelancers or have another independent profession. Since owning one's own business has considerably more uncertainty than receiving a regular paycheck, we would expect entrepreneurial people to be less risk averse than others. Indeed, [Dohmen et al. \(2005\)](#), [Hardeweg et al. \(2013\)](#) or [Falk et al. \(2018\)](#) found that self-employment decreases as risk aversion increases. However, studies on entrepreneurship have provided less clear findings on the link between entrepreneurship and risk taking, with many finding that risk attitudes between entrepreneurs and non-entrepreneurs differ

in surveys but not in measures elicited in lab experiments (Holm et al., 2013; Andersen et al., 2014; Koudstaal et al., 2015). We thus include this variable to study if we can identify a relationship between our risk attitudes and self-employment.

The field measures are statistically described in Table 5.5. The number of observations per measure shows that not all variables are measured for all subjects. Before answering each block of questions, subjects were given the option to answer if they were willing to answer. If subjects were not willing to answer, the measure is not available.

Statistic	Unit	Num. obs.	Mean	St. Dev.	Min	Max
<i>Savings</i>	k €	499	28.458	105.746	-600	1,513
<i>Risky investments</i>	%	529	6.957	18.142	0	100
<i>Real estate</i>	Yes (1) / No (0)	803	0.052	0.223	0	1
<i>Insurance</i>	Yes (1) / No (0)	803	0.153	0.360	0	1
<i>Deductible</i>	Yes (1) / No (0)	892	0.228	0.420	0	1
<i>Self-employed</i>	Yes (1) / No (0)	1,122	0.042	0.200	0	1

Table 5.5: Descriptive statistics - Field measures

Table 5.6 gives the Pearson's rank-correlation coefficient between pairwise combinations of field behavior. Each behavior is related to, at least, two other behaviors. Field behaviors are globally related to one another but correlation coefficients are far from perfect correlation (the highest coefficient is equal to 0.17). It means that each behavior has its own determinants and thus, it makes sense to study if risk aversion can explain each field behavior.

	<i>Savings</i>	<i>Risky Investments</i>	<i>Real estate</i>	<i>Insurance</i>	<i>Deductible</i>
<i>Savings</i>	-	-	-	-	-
<i>Risky Investments</i>	0.14**	-	-	-	-
<i>Real estate</i>	0.13***	0.06	-	-	-
<i>Insurance</i>	0.10**	0.05	0.17***	-	-
<i>Deductible</i>	-0.02	0.11**	0.06	-0.02	-
<i>Self-employed</i>	-0.01	0.06	0.09**	0.15***	0.04

Table 5.6: Field behavior - Correlation matrix

Notes: Pearson's rank correlation coefficients. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

5.2 Results

First, we present the methodology used to compute a risk-aversion parameter based on decisions in the tasks measuring risk attitude and we compare the value of this parameter between measures. Second, we study correlations between measures of risk attitude and laboratory financial decisions. Finally, we study correlations between measures of risk attitude and field behavior.

5.2.1 Aggregate risk-aversion parameter

Each measure of risk attitude is expressed on its own scale. To measure risk aversion on a common scale, we estimate a risk-preference parameter for all procedures measuring risk attitude (with revealed preferences). This enables us to make between-procedure comparisons and to create a single measure of risk preferences available for most of our sample. For all incentivized procedures, we use a CRRA specification for the utility function following influential literature in the estimation of risk attitude (Andersen et al., 2008; Wakker, 2008; Dohmen et al., 2011). The parameter r represents the concavity of the utility function. Risk aversion increases as the value of r decreases.

$$\forall x \in \mathbb{R}^+, U(x) = \begin{cases} x^r & \text{if } r > 0 \\ \ln(x) & \text{if } r = 0 \\ -x^r & \text{if } r < 0 \end{cases} \quad (5.1)$$

All incentivized procedures except Tanaka et al. (2010) measure risk aversion based on expected-utility maximization. Tanaka et al. (2010) is designed to allow for probability weighting. Instead of maximizing the expected value, individuals are modeled as maximizing the expected prospect value. We reproduce their approach by using the functional form of Prelec (1998) for the probability function: $\pi(p) = \exp[-(-\ln p)^\alpha]$. α gives the probability sensitivity.⁷

Estimation methods of the risk-aversion parameter are presented in Section D2. This parameter is computed for 872 subjects and its mean value is equal to 0.060 (s.d. = 1.40). We refer to this parameter as the “aggregated risk parameter” since it aggregates

⁷In following analyses, we only consider the curvature parameter estimated with TCN as all other measures only estimate this parameter. However, under prospect theory, the risk attitude is not driven only by this parameter. In Subsection D4.1, we also consider the probability-sensitivity parameter either independently or jointly with the curvature parameter.

risk-aversion parameters estimated with different methods (even if, for each subject, the risk aversion parameter is estimated with a single procedure). Estimated values for each procedure are presented in Table 5.7, along with statistics describing our laboratory measures. Bar plots of the decisions and risk-aversion parameters are available in Section D3. Note that our results are robust to the exclusion of subjects switching multiple times (see Subsection D4.3 of the Appendix).

Let us introduce our first result.

Result 1. *There is no consistency across incentivized measures of risk attitude.*

We compare the estimated risk parameter across the incentivized risk-elicitation procedures (thus excluding the WTR measure). We make pairwise comparisons using two-sided t-tests since the risk-elicitation procedures have been implemented on different subjects. TCN is significantly different from HL ($p < 0.001$) and from GP ($p=0.051$). The only measures that are not statistically different are HL and GP ($p=0.141$). We reject that EG is similar to the other measures at a 1% level for each pairwise comparison. This can be explained by the surprisingly high proportion (43%) of subjects that have chosen to take no risk in the EG task. This proportion is much higher than usually found in the literature (*e.g.*, in Eckel and Grossman, 2008 4.3% of the subjects choose the option with no risk). Overall, we find that measures of risk attitude are mainly inconsistent with each other.

	WTR G	WTR S	GP	EG	HL	TCN 1	TCN 2	Mor.	Por.	Ins.
Number of observations	250	250	213	241	203	215	215	1122	1122	1122
Decision range	[0,10]	[0,10]	[0,8]	[1,6]	[1,11]	[1,15]	[1,15]	[1,3]	[60,100]	[1,5]
Mean observed decision	4.12 (2.08)	3.48 (2.05)	3.72 (2.54)	2.46 (1.69)	6.92 (2.57)	7.32 (4.88)	7.79 (5.35)	1.67 (0.68)	76.59 (9.61)	2.68 (1.64)
CRRA r range	-	-	[-1,2]	[-3,2]	[-1,3]	[0,05,1,5]		-	-	-
Estimated mean CRRA r	-	-	0.56 (0.86)	-1.25 (1.77)	0.43 (0.96)	0.69 (0.46)		-	-	-
Abs. corr.: CRRA r / decision	-	-	0.95	0.97	0.99	0.97		-	-	-

Table 5.7: Descriptive statistics - Measures of risk attitude and laboratory financial decisions

Notes: *Mor.*, *Por.* and *Ins.* respectively refer to the mortgage task, the portfolio task and the insurance task. The decision is equal to the reported number for WTR G and WTR S, the lottery number for EG and the invested amount for GP. For TCN, the decision is equal to the number of the row at which they switch from option A to option B (it is equal to 15 if the subjects have always chosen option B). For HL, the decision is equal to the mean switching point. “Abs. corr.: CRRA r / decision” gives the absolute value of the correlation coefficient between the estimated risk-aversion parameters and the observed decisions (for TCN, we consider the mean of TCN 1 and TCN 2 previously reduced and centered). Standard deviations are in parentheses.

5.2.2 Measured risk attitude and financial laboratory decisions

In this subsection, we study whether our measures of risk attitude can explain laboratory financial decisions. In order to lead the analysis in a meaningful and intuitive way, we reverse-code the decisions of the insurance task so that decisions in all the three tasks are decreasing as risk aversion increases.

We regress the outcome of each measure of risk attitude and each laboratory financial decision. We also include demographic and income characteristics as controls. Regressions are described by the following model:

$$Lab\ Financial_i = \beta_0 + \beta_1 Risk\ Attitude_i + \beta_2 Age_i + \beta_3 Male_i + \beta_4 Income_i + \epsilon_i \quad (5.2)$$

Lab Financial is consecutively equal to the decision in the insurance task, the decision in the mortgage task or the expected value of the lottery in the portfolio task. *Risk Attitude* is consecutively equal to the answer to one of the Dohmen et al. (2011)’s questions (WTR G and WTR S), to the estimated aversion parameter with one of the procedures (HL, GP, Tanaka, EG) or to the estimated aversion parameter with any of the procedures (aggregated parameter).

Regressing independently the risk-aversion parameters of each incentivized procedure

on the laboratory financial decisions enables us to compare the performances of the different procedures. Regressing pooled estimations allows us to go beyond the specificity of each procedure to study if, overall, these procedures explain decisions in the laboratory financial decisions. Results are presented in Table 5.8. We only display the effect of the measure of risk attitude for each of the 21 regressions, since the other independent variables are controls.

Result 2. *Most measures of risk attitude correlate with the laboratory financial decisions.*

We first analyze which measures explain which laboratory financial decisions. Behavior in both the portfolio task and in the mortgage tasks is explained by almost all risk-elicitation procedures. There is no major difference in how well these tasks explain decisions. The insurance task is, however, singular. Behavior in this task is explained only by one measure (HL) and even the aggregated risk-aversion parameter cannot explain it ($p=0.294$). This task can be summarized as a single lottery choice among mean-decreasing and variance-decreasing lotteries. In that sense, it is closely related to the GP and EG procedures. However, the framing is different, as subjects are told that they can insure themselves against a loss. This loss framing combined with an insurance framing may explain the behavioral change. Finally, we find that all statistically-significant effects are in the expected direction as the value of the decision in the laboratory financial decisions has been ordered to decrease with risk-aversion.

Result 3. *The most sophisticated measures of risk attitude are less able to explain behavior in laboratory financial decisions.*

We then compare performances between risk-elicitation procedures. GP, WTR G, WTR S and EG perform equally well, as their impact on behavior in the mortgage and the portfolio tasks is significant at the 5% level. They also explain at least one of these tasks at a 1% level. However, they do not explain behavior in the insurance task and HL is the only measure that has a significant impact on this task ($p=0.034$). The HL measure also helps to explain behavior in the portfolio task at the 5% level ($p=0.033$), but not in the mortgage task ($p=0.527$) and it does not explain any laboratory financial decision at a 1% level. Based on this approach, TCN has the weakest performance of all measures; its impact on behavior in the portfolio task is only marginally significant ($p=0.070$).⁸

⁸The lack of explanatory power of TCN is confirmed when considering also the probability weighting parameter.

This analysis suggests that the most complex procedures are outperformed by simpler procedures. It is possible that the structure of the laboratory financial decisions is closer to the structure of the simpler procedures, which may also contribute to explain their higher performance.

Measures	WTR G	WTR S	GP (r)	EG (r)	HL (r)	TCN (r)	Aggregated (r)
<i>Dependent variable: Mortgage task</i>							
Risk attitude	0.377*** (0.074)	0.365*** (0.071)	0.350** (0.170)	0.405*** (0.079)	0.093 (0.147)	0.033 (0.305)	0.233*** (0.050)
<i>Dependent variable: Insurance task</i>							
Risk attitude	0.079 (0.062)	-0.005 (0.062)	-0.010 (0.156)	0.064 (0.069)	0.318** (0.150)	0.002 (0.299)	0.048 (0.046)
<i>Dependent variable: Portfolio task</i>							
Risk attitude	1.064*** (0.312)	1.354*** (0.309)	2.431*** (0.804)	1.653*** (0.354)	1.506** (0.703)	2.672* (1.466)	1.187*** (0.234)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	242	242	208	232	210	204	854
Log-Lik. (mortgage)	-218.345	-218.281	-191.859	-201.190	-194.607	-194.706	-794.939
Log-Lik. (insurance)	-335.232	-336.038	-296.152	-339.715	-276.087	-276.823	-1,201.564
R ² (portfolio)	0.083	0.111	0.067	0.110	0.063	0.019	0.042

Table 5.8: Laboratory financial decisions explained by measures of risk attitude

*Notes: The table reports the results of ordered logistic regressions when the dependent variable is “mortgage task” or “insurance task”, and of OLS regressions when the dependent variable is “portfolio task”. In each column, the independent variable “risk attitude” comes from a different measure of risk attitude. (r) indicates that the measure of risk attitude is an estimated parameter. Otherwise, it is the decision itself. The other independent variables are age, male, income and an intercept. In the upper part of the table, the dependent variable is the decision in the mortgage task. In the middle part, it is the decision in the insurance task reverse-coded. In the bottom part, it is the expected value of the decision in the portfolio task. Goodness of fit indices are R² for OLS regressions and Log-likelihood for ordered logistic regressions. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.*

5.2.3 Measures of risk attitude and field measures

We present whether our measures of risk attitude can explain field decisions. Following previous methodology, we regress the outcome of each measure of risk attitude on each

field measure:

$$\text{Field Measure}_i = \beta_0 + \beta_1 \text{Risk Attitude}_i + \beta_2 \text{Age}_i + \beta_3 \text{Male}_i + \beta_4 \text{Income}_i + \epsilon_i \quad (5.3)$$

Field Measure is consecutively equal to the amount of savings, the percentage of risky investments, having real estate, having financial insurance, having health insurance deductible and being self-employed. As in the previous subsection, *Risk Attitude* is consecutively equal to each measure of risk attitude or to the aggregated risk parameter.

Result 4. *None of the measures of risk attitude explain field behavior.*

We first analyze regressions of the different measures on field behavior reported in Table D2 in the Appendix. The lack of explanatory power of the different risk-elicitation procedures is striking: no measure of risk attitude is statistically significant at even a 10% level in any of the thirty-six regressions. As a result, we are not able to discriminate among procedures since they all consistently fail to explain field measures. Based on our sample, this analysis raises serious concerns about how well common measures of risk attitude explain field behavior.

To challenge these findings, we then focus on the aggregated risk parameter. This aggregated parameter is less affected by the specificity of each procedure, and it is estimated for a larger number of subjects than each individual procedure. Table 5.9 reports the results of regressions of the aggregated parameter on our different field measures. These regressions reveal a trend suggesting that being insured and owning real-estate investments are negatively impacted by the aggregated risk parameter ($p=0.084$ and $p=0.098$, respectively).⁹ This result for insurance goes in the expected direction: purchasing insurance allows people to decrease the risk, so that more risk-averse individuals should be more insured.

The aggregated risk parameter has no statistically-significant impact on the other field measures at any conventional levels. Thus, the estimated risk parameter has overall little explanatory power. Could it stem from a lack of statistical power? To address this point, we calculated the effect size and the statistical power of the risk parameter in the different regressions. The effect size is measured using marginal effects for logistic

⁹These p-values are obtained without controlling for multiple-comparisons to be conservative when assessing Result 4. If we adjust the p-values, no relation is statistically significant.

regressions, standardized coefficients and f^2 for OLS regressions.¹⁰ Reported power tests are the estimated statistical power and the estimated number of observations needed to find a statistically-significant effect at a 5% level with a statistical power of 80%.¹¹ Analyzing statistical power provides us with an estimation of how reliable our findings are. Our conclusion regarding the absence of effect of the aversion parameter on the probability of being self-employed is deeply rooted, since the statistical power is above the threshold of 80%. For the other variables, our statistical power is below this threshold. For these variables, obtaining a p-value under 5% with a power above 80% would require a great increase in the sample size (between 1763 and 142857 observations).¹²

In conclusion, we observe that the effects of the aggregated parameter on field behavior are small at best and are either statistically-insignificant or weakly significant. The inability of our standard measures of risk attitude to explain field behavior goes beyond the specificity of each procedure since even the aggregated risk-preference parameter does a poor job of explaining risky decisions in the field.

5.3 Discussion and conclusion

Based on a large-scale experiment, we evaluate if experimental measures of risk attitude are able to explain risky behavior in both experimental settings and naturally-occurring settings. First, we confirm previous findings on the inconsistency between measures of risk attitude. Second, we find that these measures have some predictive power on behavior in experimental settings, and that the most complex procedure (TCN) is outperformed by simpler procedures. Finally, we find no correlation between field behavior and measures of risk attitude. This finding is confirmed for all of the implemented mea-

¹⁰ f^2 is the variation of Cohen's f^2 associated with the aggregated risk parameter (Cohen, 1988). This measure reports the increase in the explained variance due to adding a given variable in the regression, divided by the unexplained variance for normalization.

¹¹ We used G*Power (Faul et al., 2009) to compute statistical-power measures. Thresholds for α and β were chosen following conventional standards for adequacy.

¹² The magnitudes of marginal effects range between 0.001 (for self-employment) and 0.017 (for being insured). An increase in one unit of the risk parameter (approximately two-thirds of its standard deviation) increases the odds of field behavior to between 1% and 2%. For the continuous measures, an increase of one standard deviation of the parameter (1.5 units) leads to an increase of respectively 4.2% and 3.2% of standard deviation for the amount of savings or the percentage of risky investments. A rather large variation in estimated risk aversion has thus a rather small impact on field measures. The variations in Cohen's f^2 classify the effects on continuous field measures as no more than that one-tenth of an effect characterized as small ($f_{small}^2=0.02$).

	<i>Dependent variable:</i>					
	Savings (1)	Risky Investments (2)	Real Estate (3)	Insurance (4)	Deductible (5)	Self-employed (6)
Aggregated risk parameter	-2.556 (2.726)	-0.424 (0.654)	-0.211* (0.127)	-0.136* (0.078)	0.056 (0.068)	-0.023 (0.123)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Constant	Yes	Yes	Yes	Yes	Yes	Yes
Observations	595	595	830	371	678	397
R ²	-	-	-	0.257	-	0.048
Akaike Inf. Crit.	514.111	216.423	293.640	-	740.136	-
Marginal effect	-	-	-0.006	-0.017	0.010	-0.001
Standardized coefficient	-0.042	-0.032	-	-	-	-
f ²	0.002	0.001	-	-	-	-
P-value	0.349	0.518	0.098	0.084	0.408	0.854
Statistical power	0.577	0.600	0.332	0.457	0.575	0.825
Observation $\alpha = 0.05, \beta = 0.2$	2619	7846	3112	1763	9565	142857

Table 5.9: Field measures explained by the aggregated risk parameter

*Notes: This table reports the results of OLS regressions (models (1) and (2)) and from Logistic regressions (models (3) to (6)). Controls include age, male, and income. Odd ratios, marginal effects, standardized coefficients, f², p-values, and statistical power are computed for the aggregated risk parameter. Marginal effects are computed at the mean. “Observation $\alpha = 0.05, \beta = 0.2$ ” gives an estimation of the sample size needed to obtain a statistically-significant effect at a 5% level and with a 80% power of the aggregated risk parameter on the dependent variable. ***p < 0.01, **p < 0.05, *p < 0.1.*

asures, either simple or complex. We thus conclude that while measures of risk attitude can explain behavior in the laboratory, they fail to explain behavior in external settings.

These findings may result from several potential explanations such as the domain-specific nature of risk attitudes, different drivers of risky behavior in the field, noisy measurements, and weaknesses of the expected-utility-theory paradigm on which most measures are built.

Regarding the first possible explanation, studies in experimental psychology approach risk attitudes as being content- and context- dependent. In particular, Weber et al. (2002) developed a scale measuring risk-taking across six domains: financial, health, ethical, recreational, investment and gambling decisions. Using this scale, several studies have found that risk-taking is indeed domain-specific (Weber et al., 2002; Hanoch et al., 2006). In the economics literature, Reynaud and Couture (2012) and Deck et al. (2013) investigated whether domain-dependence could explain the inconsistency between measures of risk attitude. While Reynaud and Couture (2012) conclude that domain-dependence may explain this inconsistency, the results from Deck et al. (2013) do not support this finding. In our study where we investigate decisions in various financial domains, one could claim that the experimental measures are perceived as gambling

decisions in contrast with field measures. However, the lack of explanatory power of the measures of risk attitude for any field financial behavior investigated—independently of the domain—and the absence of difference between the general and domain-specific questions of Dohmen et al. (2011) provide little evidence that our results are due to the domain-specific nature of risk attitudes.

A second possible explanation could be that behavior under risk in natural settings is mainly driven by other factors than risk preferences. A major difference between experimental measures and field behavior is that in the field the “perception of risk”—how risky one judges a situation—is more difficult to evaluate than in an experimental setting. In an experimental setting, probabilities are defined exogenously, whereas they are subjectively evaluated in the field and they arise endogenously. As an illustration of the difference between risk perception and risk attitude, the former has been found to differ widely between cultural backgrounds, while the latter was much more stable (Weber and Hsee, 1998). Differences in perceived risk may also be related to moral values, peer effects or external constraints. For instance, many important risky decisions in the field, like buying a house, investing in the stock market and choosing a pension plan are much more complex than the relatively simple lottery choices that subjects face in an experiment. For such complex problems in the field, decisions may result from household preferences more than individual ones, and people may also seek advice (for example 56% of the American households ask for advice to financial professionals, see Egan et al. (2019)) or copy the choices of people that they consider successful (social learning). By doing so, they may end up displaying a different risk attitude than they would if they were confronted with a simple problem where social sampling is not available (Offerman and Schotter, 2009). If the risk perceptions are the primary driver of risky decisions in the field and if they differ from objective risks, it might contribute to explain why risk preferences elicited experimentally fail to explain actual behavior. Similarly, Noussair et al. (2013) highlight that the complexity of field behavior might be better captured by higher-order risk attitudes than by the second order-risk attitudes.

A third possibility is that the link between risk attitude measurements and field measures of risk taking is weak because measurements are noisy. Notice that our various risk attitude measures may be noisy estimates of a stable underlying latent risk preference parameter. In addition, the measurements of the field behavior may also be noisy because they are self-reported. As a result, it can be that a correlation between two measures of

risk attitudes is low, even though the behavior that these measures capture is driven by the same latent preference parameter. Pure measurement error may also drive the lack of correlation between measured risk attitudes and risky behavior in the field (see also Einav et al., 2012).

Finally, most of the measures of risk attitude tested are based on expected-utility maximization. Perhaps such specifications may not be adequate, as probability weighting is important in guiding decision making (Kahneman and Tversky, 1979). However, in our study the only measure of risk attitude that relies on prospect theory (TCN) performs as badly as other measures concerning the explanation of field behavior. Furthermore, it has the weakest performance in explaining laboratory financial decisions. Our results thus do not provide evidence that prospect theory is an improvement over the expected-utility framework.

Our findings shed light on the existing gap between laboratory and field decisions under risk. Risk preferences seem to depend on the setting in which they are expressed. They are particularly difficult to evaluate for researchers, since both methods based on revealed preferences and methods based on the self-reported willingness to take risk do not seem to predict actual behavior. In line with the conclusions of Friedman et al. (2014), it appears that the mechanisms developed to measure risk preference do not accurately reflect financial behavior in the field. An ambitious research challenge will be to find a better match between measurement mechanisms and field behavior.

Chapter 6

General conclusion

6.1 Summary

The chapters of this thesis study a range of decisions under risk and uncertainty. This variety of behavior includes behavior under risk, ambiguity and perceptual uncertainty. Behavior under risk and uncertainty are studied alternatively in the laboratory and the field.

Chapter 2 focuses on situations where individuals must decide whether an event has occurred using uncertain evidence. Accurately identifying that this event has occurred is more rewarded than accurately identifying that it has not occurred. This decision problem induces a divergence between two types of decision strategy: to maximize the expected value of the decision (optimality) and to maximize the likelihood of making an accurate decision (accuracy). As both optimality and accuracy are relevant qualities of a decision, individuals may face a trade-off between optimality and accuracy. Using Signal Detection Theory as a normative benchmark, we highlight the existence of the optimality-accuracy trade-off and study its origins in two laboratory experiments on perceptual decision-making. The results of the first experiment confirm the existence of the trade-off with a leading role of accuracy. The results of the second experiment explain the trade-off by the concern of individuals for being right.

Chapter 3 aims at identifying the influence of the information order on the optimality-accuracy trade-off. Indeed, perceptual decision-making involves two types of information: perceptual evidence and payoff structure. When faced with perceptual decisions with asymmetric stakes, individuals face an optimality-accuracy trade-off. This trade-off

prevents them from reaching optimal decisions which maximize expected payoffs. We hypothesize that the order in which information is provided may matter on behaviors. Receiving a piece of information last emphasizes its importance in the decision process. In this experiment, we manipulate the information order between payoffs and perceptual evidence in a visual discrimination task. Results show an order effect between payoffs and stimuli: individuals shift their decision criterion in the direction of optimality when payoff information is given last, and we observe a trend suggesting a better discriminability when stimuli are displayed last. Even if these effects are not large enough to reach optimality, they inform about the integration of payoffs and stimuli in the perceptual decision process.

Chapter 4 studies how ambiguity interacts with giving behavior using risk as a baseline. A donation may have ambiguous costs or ambiguous benefits. In a laboratory experiment, we show that individuals use this ambiguity strategically as a moral wiggle room to behave less generously without feeling guilty. Such excuse-driven behavior is more pronounced when the costs of a donation — rather than its benefits — are ambiguous. However, the importance of excuse-driven behavior is comparable under ambiguity and under risk. Individuals exploit any type of uncertainty as an excuse not to give, regardless of the nature of this uncertainty.

Finally, Chapter 5 challenges the external validity of laboratory risk preference measures. Based on a large scale experiment using a representative panel of the Dutch population, we test if risk preference measures are able to explain two different types of behavior: i) behavior in experimental risk tasks, ii) behavior in naturally occurring field behavior under risk (financial, health and employment decisions). We find that risk preference measures are related to behavior in experimental risk tasks. Moreover, more complex measures underperform. However, we find that risk preference measures are not related to risk-taking in the field, calling into question the methods currently used for the purpose of measuring actual risk preferences. We conclude that while the external validity of risk preference measures holds in closely related frameworks, this validity is compromised in more remote settings.

6.2 Limits and extensions

Decisions under uncertainty with asymmetrical payoffs imply a trade-off between an optimal decision process and an accurate one. Previous results of the literature, as

well as the Study 1 of Chapter 2, have highlighted a consistent behavior leading the decision process toward accuracy over optimality. Understanding why individuals are not able to change their process of decision toward optimal behaviour remains an open question. Study 2 of Chapter 2 offers the first tentative of explanation: individuals are naturally validating the value of being right. Chapter 3 proposes another explanation: a recency bias causes individual to over-weight information on accuracy over information on payoffs. However, even when presenting payoff information last, individuals stay far from optimality. While many other explanations are serious candidates to explain the trade-off, it appears that one component should be crucial to understanding the decision process: the subjective beliefs. All decisions made under uncertainty or ambiguity imply that individuals form some subjective beliefs over the possible states of nature (Savage, 1954). SDT itself is based on an implicit representation of subjective beliefs through the likelihood-based evidence. Indeed, SDT offers to go beyond the decision process (type I decision) to go to the metacognitive process (type II decision). Using an extended theoretical framework, it allows an estimation of the stated beliefs as well as their quality (Galvin et al., 2003; Maniscalco and Lau, 2014).

A potential extension could thus aim to understand the importance of subjective beliefs in the optimality-accuracy trade-off. Indeed switching from the accuracy process to the optimal one can only be done if individuals have an accurate knowledge of the quality of their decisions. By asking their confidence in the accuracy of the decision, we can observe how good their metacognitive abilities are. SDT offers a normative framework to measure these abilities (Fleming and Lau, 2014). Calibration or overconfidence will reflect how their confidence matches their success rate on average. While discrimination or resolution will define how their confidence can fluctuate according to their level of success. We can expect that the quality of beliefs will be positively related to a more optimal decision process. In addition to this descriptive aspect, it is unclear whether asking subjective beliefs also change the decision process itself (see e.g. Nyarko and Schotter 2002 or Rutström and Wilcox 2009 in economic games and Moran et al. 2015; Navajas et al. 2016 in perceptual decisions.). If it is the case, by revealing their level of confidence, individuals may be more aware of the process of decision. We can hypothesize that accessing this knowledge will lead behaviors toward the optimal decision.

Other limitations and extensions of Chapter 2 and Chapter 3 are based on theoretical considerations. SDT does not take into account the timing of the decision. Drift-diffusion

models (Ratcliff, 1978, hereafter DDM) provide a similar framework that includes the dynamic of the decision as a key component. It assumes that evidence is accumulated following a random walk process or more generally a Brownian motion (Kramers, 1940). When the level of evidence exceeds a certain threshold it triggers the decision. Even if considering SDT as a static version of DDM is simplistic, taking into account a time component in the theory may strengthen the understanding of the decision process. Another avenue for future research would thus be to investigate the reasons for the optimality-accuracy trade-off based on DDM.

Chapter 4 finds that excuse-driven behavior are similar under risk and under ambiguity. A message of this thesis is that perceptual tasks are relevant to study economic problems. Based on this message, a natural extension could be to study excuse-driven behavior under perceptual uncertainty. The perception might indeed be also affected by self-excusing behavior, and may even reinforce it.

One of the objectives of Chapter 5 — to provide a guideline for the selection of risk reference measure — has not been reached. Thus, new studies should be designed to answer this question. Features as the number of subjects or the type of field behavior might lead to more conclusive findings. However, if the apparent lack of correlation between risk preference measures and field behavior holds, new studies should focus on finding methods for eliciting preferences that have improved external validity.

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Chapter A

Appendix of Chapter 2

A1 Hit and false alarm probabilities

We demonstrate Equation 6 and Equation 7. We start by introducing the criterion λ such that:

$$\forall (X, \lambda) \in \mathbb{R}^2, D(X, \lambda) = \begin{cases} 1 & \text{if } X \geq \lambda \\ 0 & \text{if } X < \lambda \end{cases} \quad (\text{A.1})$$

The criterion λ , we can easily write the probability of hit (P_H) and the probability of false alarm (P_{FA}):

$$\begin{aligned} P_H &= \mathbb{P}(X_s > \lambda) = 1 - \phi(\lambda - d') \\ P_{FA} &= \mathbb{P}(X_n > \lambda) = 1 - \phi(\lambda) \end{aligned} \quad (\text{A.2})$$

Using Equation 2, we can express β as a function of λ :

$$\beta = L(\lambda) = \frac{\mathbb{P}(\lambda|\Theta = 1)}{\mathbb{P}(\lambda|\Theta = 0)} = \frac{f_s(\lambda)}{f_n(\lambda)} \quad (\text{A.3})$$

Thus:

$$\begin{aligned} \log(\beta) &= \log\left(\frac{f_s(\lambda)}{f_n(\lambda)}\right) \\ &= \log\left(\frac{1}{\sqrt{2\pi}}\exp\left(-\frac{1}{2}(\lambda - d')^2\right)\right) - \log\left(\frac{1}{\sqrt{2\pi}}\exp\left(-\frac{1}{2}\lambda^2\right)\right) \\ &= d'(\lambda - \frac{1}{2}d') \end{aligned} \quad (\text{A.4})$$

Finally, we express P_H and P_{FA} as a function of the criterion β :

$$\begin{aligned} P_H &= 1 - \phi\left(\frac{\log(\beta)}{d'} - d'/2\right) \\ P_{FA} &= 1 - \phi\left(\frac{\log(\beta)}{d'} + d'/2\right) \end{aligned} \tag{A.5}$$

A2 Complementary results

A2.1 Descriptive statistics

Statistic	Treatment	Mean (% or ms)	St. Dev.	Min (% or ms)	Max (% or ms)
Hit	S	72	15.44	36	100
	LG	75.90	14.35	44	100
	L	74.73	15.96	28	100
	HG	72	17.55	40	100
False Alarm	S	35.61	13.88	12	68
	LG	39.12	18.84	8	100
	L	39.32	19.06	12	100
	HG	41.66	21.11	0	100
Success	S	68.20	11.86	50	92
	LG	68.39	12.30	50	92
	L	67.71	13.43	34	92
	HG	65.17	14.70	42	100
Response Time	S	1.62	0.42	0.89	3.00
	LG	1.55	0.42	0.44	2.44
	L	1.69	0.61	0.37	3.95
	HG	1.55	0.38	0.73	2.58

Note: Symmetric (S), Low Gain (LG), Loss (L) and High Gain (HG).

Table A1: Study 1 - Descriptive Statistics

Reading note: Each subject represents one observation.

Statistic	Treatment	Mean (% or ms)	St. Dev.	Min (% or ms)	Max (% or ms)
Hit	I	71.00	17.16	32	100
	F	65.95	15.53	32	100
False Alarm	I	33.95	12.75	8	56
	F	39.07	15.49	8	92
Success	I	68.42	12.82	42	96
	F	63.77	10.86	48	88
Response Time	I	1.73	0.45	1.16	3.11
	F	1.51	0.41	0.59	2.83

Note: Incentivized (I) and Flat (F).

Table A2: Experiment 2 - Descriptive Statistics

Reading note: Each subject represents one observation.

A2.2 SDT-free analysis

	<i>Dependent variable:</i>							
	Hit		False Alarm		Success		Response time	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cross-variables								
Loss × More Ambiguity Averse Under Loss	-0.211 (0.207)	-0.211 (0.207)	-0.103 (0.178)	-0.103 (0.178)	-0.023 (0.131)	-0.023 (0.131)	0.245*** (0.072)	0.245*** (0.072)
Loss × Less Ambiguity Averse Under Loss	-0.346 (0.268)	-0.346 (0.268)	0.472* (0.248)	0.476* (0.248)	-0.359** (0.172)	-0.359** (0.172)	-0.019 (0.100)	-0.019 (0.100)
Treatments								
Symmetric	-0.00000 (0.103)	0.00003 (0.103)	-0.278*** (0.095)	-0.278*** (0.095)	0.144** (0.068)	0.144** (0.068)	0.068* (0.038)	0.068* (0.038)
Low Gain	0.221** (0.105)	0.221** (0.105)	-0.115 (0.094)	-0.115 (0.094)	0.153** (0.068)	0.153** (0.068)	0.002 (0.038)	0.002 (0.038)
Loss	0.334* (0.183)	0.334* (0.183)	-0.120 (0.155)	-0.120 (0.155)	0.194* (0.113)	0.194* (0.113)	0.006 (0.063)	0.006 (0.063)
Individual Characteristics								
More Ambiguity Averse Under Loss	0.002 (0.288)	0.459 (0.303)	-0.550** (0.218)	-0.552** (0.270)	0.298 (0.199)	0.508** (0.234)	0.131 (0.145)	0.197 (0.195)
Less Ambiguity Averse Under Loss	-0.447 (0.397)	-0.469 (0.389)	0.190 (0.303)	0.054 (0.350)	-0.282 (0.276)	-0.222 (0.302)	0.180 (0.203)	0.320 (0.253)
More Risk Averse Under Loss		-0.015 (0.256)		0.021 (0.229)		-0.024 (0.199)		0.053 (0.167)
Less Risk Averse Under Loss		0.265 (0.319)		0.425 (0.283)		-0.107 (0.246)		0.184 (0.206)
Ambiguity Averse Under Gain		-0.129 (0.297)		0.084 (0.266)		-0.085 (0.231)		-0.274 (0.194)
Ambiguity Seeker Under Gain		-0.213 (0.292)		-0.065 (0.260)		-0.057 (0.226)		-0.267 (0.189)
Ambiguity Averse Under Loss		0.117 (0.253)		-0.221 (0.226)		0.165 (0.196)		0.175 (0.165)
Ambiguity Seeker Under Loss		0.431 (0.319)		-0.072 (0.281)		0.214 (0.244)		-0.039 (0.204)
Male		0.604*** (0.215)		-0.018 (0.192)		0.291* (0.167)		-0.101 (0.139)
Age		-0.058*** (0.021)		0.018 (0.019)		-0.036** (0.017)		0.006 (0.014)
Other								
Trial Number		-0.001 (0.003)		0.0005 (0.002)		0.001 (0.002)		-0.004*** (0.001)
Constant	1.130*** (0.245)	1.843*** (0.563)	-0.069 (0.188)	-0.464 (0.505)	0.546*** (0.169)	1.031** (0.437)	1.444*** (0.122)	1.510*** (0.364)
Observations	4,100	4,100	4,100	4,100	8,200	8,200	8,200	8,200
BIC	4,536	4,595	5,262	5,333	10,048	10,120	26,591	26,674

Note:

*p<0.1; **p<0.05; ***p<0.01

Table A3: Impacts of the cross-variable Loss Treatment × loss attitudes under ambiguity.

Reading note: Regression - random-effect models logistic (1,2,3,4,5,6) and Gaussian (7,8). Reference groups - High Gain for treatment, "neutral preferences" for preferences variables.

A2.3 SDT-based analysis

	<i>Dependent variable:</i>							
	Hit		False Alarm		Success		Payoff	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Hit Optimal	1.326** (0.650)							
Hit Accuracy		5.417*** (0.466)						
False Alarm Optimal			2.345*** (0.196)					
False Alarm Accuracy				5.508*** (0.401)				
Success Optimal					4.960*** (0.212)			
Success Accuracy						5.216*** (0.217)		
Payoff Optimal							0.994*** (0.005)	
Payoff Accuracy								0.999*** (0.005)
Asymmetric	-0.290 (0.221)	0.178** (0.085)	-0.794*** (0.115)	0.172** (0.079)	0.284*** (0.059)	-0.013 (0.057)	-0.498*** (0.090)	0.185** (0.090)
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Constant	YES	YES	YES	YES	YES	YES	YES	YES
Observations	4,100	4,100	4,100	4,100	8,200	8,200	8,200	8,200
BIC	4,572	4,469	5,195	5,171	9,758	9,741	44,217	44,161

Technical note:

*p<0.1; **p<0.05; ***p<0.01

Table A4: Experiment 1 - Impacts of SDT predictions on observed behaviors with a dummy variable Loss Treatment.

Reading note: for each dependent variable, we present the causal effect of the corresponding predicted variable based on either optimal decision (odd numbers) or accuracy-maximizing (even numbers) strategies with control variables. Regressions are random-effect models logistic (1,2,3,4,5,6) and Gaussian (7,8).

	<i>Dependent variable:</i>							
	Hit (1)	Hit (2)	False Alarm (3)	False Alarm (4)	Success (5)	Success (6)	Payoff (7)	Payoff (8)
Loss Treatment	0.050 (0.091)	-0.019 (0.093)	0.015 (0.083)	0.019 (0.083)	-0.013 (0.060)	-0.020 (0.060)	-0.189 (0.191)	-0.124 (0.190)
Hit Optimal	1.314** (0.653)							
Hit Accuracy		5.478*** (0.479)						
False Alarm Optimal			2.286*** (0.199)					
False Alarm Accuracy				5.374*** (0.417)				
Success Optimal					4.962*** (0.212)			
Success Accuracy						5.219*** (0.217)		
Point Optimal							0.986*** (0.010)	
Point Accuracy								0.993*** (0.010)
Asymmetric	-0.303 (0.224)	0.190** (0.091)	-0.773*** (0.120)	0.172** (0.084)	0.288*** (0.062)	-0.006 (0.060)	-0.430*** (0.114)	0.226** (0.110)
Constant	0.195 (0.453)	-2.701*** (0.336)	-1.340*** (0.109)	-2.344*** (0.160)	-2.586*** (0.149)	-2.756*** (0.152)	0.036 (0.100)	-0.0002 (0.093)
Observations	4,100	4,100	4,100	4,100	8,200	8,200	8,200	8,200
BIC	4,507	4,407	5,134	5,112	9,767	9,750	44,133	44,062

Technical note:

*p<0.1; **p<0.05; ***p<0.01

Table A5: Experiment 1 - Impacts of SDT predictions on observed behaviors with controls.

Reading note: for each dependent variable, we present the causal effect of the corresponding predicted variable based on either optimal decision (odd numbers) or accuracy-maximizing (even numbers) strategies with control variables. Regressions are random-effect models logistic (1,2,3,4,5,6) and Gaussian (7,8).

	<i>Dependent variable:</i>							
	Hit (1)	Hit (2)	False Alarm (3)	False Alarm (4)	Success (5)	Success (6)	Payoff (7)	Payoff (8)
Hit CRRA	1.001** (0.460)	1.018** (0.459)						
False Alarm CRRA			1.560*** (0.177)	1.590*** (0.180)				
Success CRRA					4.719*** (0.212)	4.581*** (0.240)		
Point CRRA							0.991*** (0.005)	0.991*** (0.005)
Asymmetric	-0.164 (0.157)	-0.169 (0.157)	-0.435*** (0.107)	-0.448*** (0.108)	0.283*** (0.059)	0.273*** (0.059)	-0.422*** (0.091)	-0.421*** (0.090)
Controls	NO	YES	NO	YES	NO	YES	NO	YES
Constant	YES	YES	YES	YES	YES	YES	YES	YES
Observations	4,100	4,100	4,100	4,100	8,200	8,200	8,200	8,200
BIC	4,499	4,571	5,167	5,241	9,825	9,915	44,138	44,231

Technical note:

*p<0.1; **p<0.05; ***p<0.01

Table A6: Experiment 1 - Impacts of utility-maximizing predictions on observed behaviors.

Reading note: for each dependent variable, we present the causal effect of the corresponding utility-maximizing predicted variables with (even numbers) and without control variables (odd numbers). Regressions estimate random-effect models logistic (1,2,3,4,5,6) and Gaussian (7,8).

	<i>Dependent variable:</i>		
	Hit (1)	False Alarm (2)	Success (3)
Predicted Hit	6.034*** (0.565)		
Predicted False Alarm		4.035*** (0.480)	
Predicted Success			4.852*** (0.308)
Flat Treatment	0.055 (0.101)	0.047 (0.094)	0.001 (0.068)
Controls	YES	YES	YES
Constant	YES	YES	YES
Observations	2,150	2,150	4,300
BIC Flat	1,338	1,430	2,750
BIC Incentive	1,210	1,367	2,565

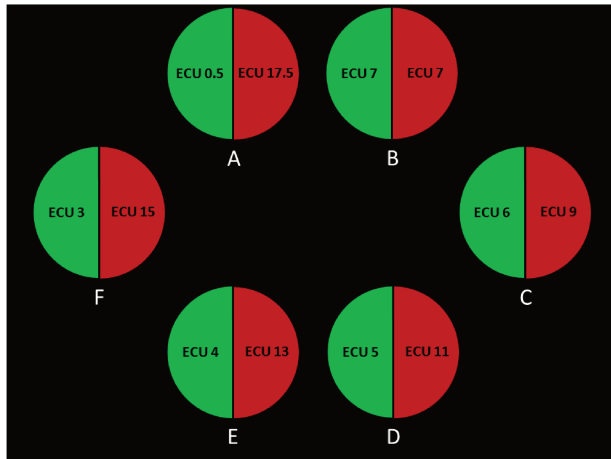
Technical note:

*p<0.1; **p<0.05; ***p<0.01

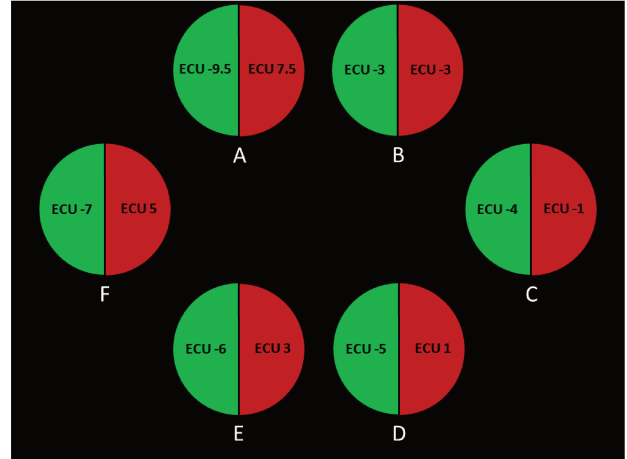
Table A7: Experiment 2 - Impacts of SDT predictions on observed behaviors with controls.

Reading note: for each dependent variable, we present the causal effect of the corresponding predicted variable based on the accuracy-maximizing strategy with control variables. Regressions estimate logistic random-effect models. AIC correspond to regressions clustered per treatment.

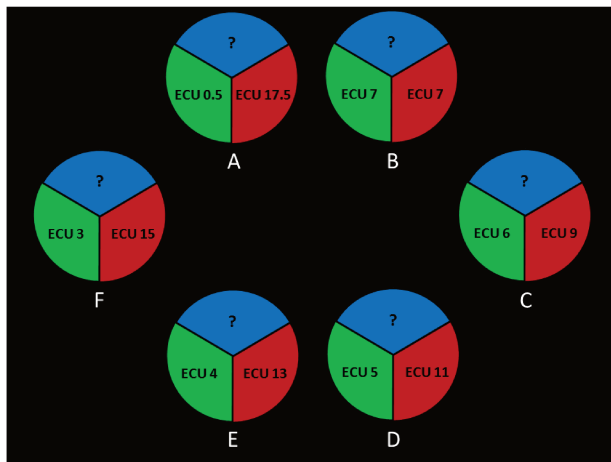
A3 Elicitation of attitudes toward risk, ambiguity, and losses



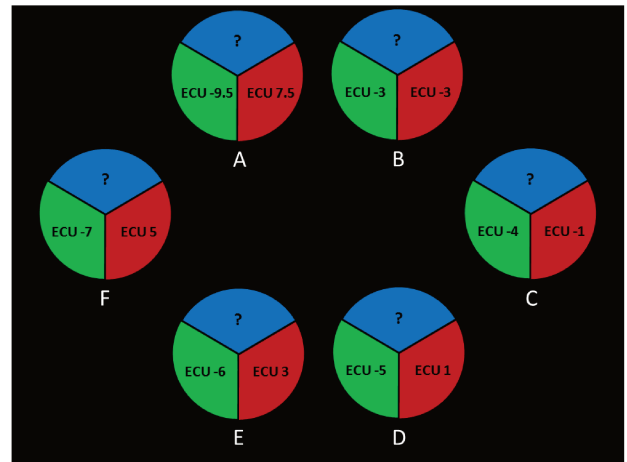
a - Decision 1 (gain, risk)



b - Decision 2 (loss, risk)



c - Decision 3 (gain, ambiguity)



d - Decision 4 (loss, ambiguity)

Figure A1: Lottery-based decisions used to elicit attitudes toward risk, ambiguity and losses.

Reading note: Each circle represents a lottery. Lotteries' earnings are displayed within circles. Under risk, circles are divided into two equal shares as each outcome is equally likely. The uncertainty is graphically depicted by a "?" displayed on part of the circle to express the fact that the true probability is unknown.

A4 Estimation of the utility-maximizing criterion using standard or conservative methodologies

A4.1 CRRA parameters estimation

CRRA parameter under gain (γ_G) - The elicitation of γ_G is based on the mechanism from Eckel and Grossman (2008) (Decision 1). A parameter interval is associated with each lottery. The upper (respectively lower) bound of the parameter interval associated with a lottery is the parameter implying indifference between this lottery and the next lottery (respectively the previous one).

CRRA parameter under loss (γ_L) - As Decision 2 involves both loss lotteries and mixed lotteries, we are unable to produce a direct estimation of γ_L based on our elicitation task. We thus need to rely on previous research to estimate this parameter.

A4.2 Standard methodology

Our approximation of β_U^* is based on two assumptions. In the gain domain, following Reynaud and Couture (2012), we measure γ_G as the centroids of each interval.¹ In the loss domain, we assume that the diminishing sensitivity is equivalent to the one in gain, i.e. $\gamma_G = \gamma_L$.² We classify 31 subjects as risk-averse in gain and risk seeker in loss, while 10 subjects exhibit the opposite pattern.³

Using these assumptions, we obtained that Hypothesis 1.1 and Hypothesis 1.3 were robust regarding risk attitude. While these assumptions were necessary to produce parameters estimations that have, in our view, the best likelihood of describing the true risk attitudes of subjects, it is important to assess if our results on utility-maximization are robust to changes in hypotheses. In a conservative approach, we thus use assumptions that challenge the most our results and prove that they are still valid.

¹For decisions B and A, the corresponding parameter intervals are half-bounded. We take the closest integer value from the existing bound that is included in the interval: 4 for $]3.45 ; \infty[$ and -1 for $]-\infty ; 0[$

²This reflection effect was empirically found by Tversky and Kahneman (1992). But evidence of the whole literature is more mitigated (cf. Note 1 in Wakker et al. (2007) for extensive references on the question).

³Note that we may overestimate the number of risk-averse subjects in the loss domain as only 2 subjects took the lottery B in Decision 2 which can be seen as a risk-averse behavior under loss.

A4.3 Conservative methodology

Hypothesis 1.1 is verified for the utility-maximizing criterion if subjects' criteria are lower than the utility-maximizing criteria. Hypothesis 1.3 is verified if subjects' criteria are closer to the accuracy-maximizing criteria than to the utility-maximizing decision criteria. For higher estimated values of the utility-maximizing criteria, it is more difficult to conclude in favor of Hypothesis 1.1 and Hypothesis 1.3. We thus use the assumptions leading to the highest values of the utility-maximizing criteria, referred to as conservative utility-maximizing criterion (β_U^c), to assess the robustness of previous results.

Assumptions - Concerning risk parameters in the gain domain, we select the CRRA parameter within each parameter interval giving the highest value of β_c^* . As the utility-maximizing decision criterion increases with the level of risk aversion (γ_G), we use the upper bound of each interval.

Individuals are generally risk-seeking when confronted to losses (Fishburn and Kochenberger, 1979; Kahneman and Tversky, 1979). By including this risk-seeking attitude in the CRRA utility function, we would obtain that utility-maximizing criteria are lower than optimal criteria in the loss domain. We relax this assumption by assuming that subjects are risk neutral in the loss domain ($\gamma_L=1$). Utility-maximizing criterion is thus considered equal to the optimal criterion for the loss treatment.

Estimation - The estimated β_U^c have a mean of 0.627 (*s.d.* = 0.251) for the Low Gain Treatment and a mean of 0.438 (*s.d.* = 0.128) for the High Gain Treatment. By definition conservative utility-maximizing criteria are equal to the optimal criterion for the Symmetric Treatment and the Loss Treatment (respectively 1 and $\frac{1}{3}$).

Result: Hypothesis 1.1 is also supported for the conservative utility-maximizing decision criterion - SDT predictions at a subject-treatment level highlight that the observed success rate is 5.5% higher than the utility-maximizing success rate (*p* - value < 0.001) for asymmetric treatments. It implies a payoff decrease of 10.2% (*p* - value < 0.001). SDT predictions at a decision level reveal that conservative utility-maximizing predictions also lead to overestimate the hit rate, the false alarm rate, and the payoffs while underestimating the success rate (SI Table A8).

Result: Hypothesis 1.3 is also supported for the conservative utility-maximizing decision criterion - Deviations from accuracy maximization are lower than deviations from conservative utility maximization both in term of success rate and payoffs (*p*-values: success < 0.001 and payoff = 0.012).

	<i>Dependent variable:</i>							
	Hit		False Alarm		Success		Payoff	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Hit CRRA	0.698*	0.721*						
	(0.412)	(0.411)						
False Alarm CRRA			1.773***	1.804***				
			(0.178)	(0.180)				
Success CRRA					4.857***	4.802***		
					(0.207)	(0.236)		
Payoff CRRA							0.995***	0.995***
							(0.005)	(0.005)
Asymmetric	-0.064	-0.070	-0.426***	-0.436***	0.209***	0.206***	-0.423***	-0.423***
	(0.139)	(0.138)	(0.101)	(0.101)	(0.058)	(0.058)	(0.091)	(0.091)
Controls	NO	YES	NO	YES	NO	YES	NO	YES
Constant	YES	YES	YES	YES	YES	YES	YES	YES
Observations	4,100	4,100	4,100	4,100	8,200	8,200	8,200	8,200
BIC	4,500	4,573	5,151	5,225	9,765	9,858	44,134	44,227

Technical note:

*p<0.1; **p<0.05; ***p<0.01

Table A8: Experiment 1 - Impacts of utility-maximizing predictions on observed behaviors.

Reading note: for each dependent variable, we present the causal effect of the corresponding utility-maximizing predicted variables with (even numbers) and without control variables (odd numbers). Regressions estimate random-effect models logistic (1,2,3,4,5,6) and Gaussian (7,8).

A5 Experimental instructions

A5.1 Experiment 1

Task 1

General information:

- Four sets of lotteries will be presented to you. For each set, you will have to choose one lottery out of six possibilities.
- Each lottery is composed of two different payoffs.
- When you select a lottery, you earn either the payoff on the left or on the right.

Proceeding:

- First lottery: No losses possible (no endowment), 50% of chance for the two different payoffs.
- Second lottery: Losses possible (endowment), 50% of chance for the two different payoffs.

- Third lottery: No losses possible (no endowment), between 30% and 70% of chance for the two different payoffs.
- Fourth lottery: Losses possible (endowment), between 30% and 70% of chance for the two different payoffs.

Task 2

General information:

- Determine if both circles contain either the same or a different number of dots.
- If you think that they contain the same number of dots, press the key on your keyboard.
- If you think that they contain a different number of dots, press the key on your keyboard.

For each decision, four outcomes are possible:

		Correct answer	
		<i>Different</i>	<i>Same</i>
Decision	<i>Different</i>	Success when different	Failure when similar
	<i>Same</i>	Failure when different	Success when similar

- To each possible outcome is associated a certain number of points.
- Your objective is to maximize the number of points earned.

Feedback:

- After each decision you will know if you were right or wrong and the number of points earned for this decision.
- Every 10 decisions, you will have:
 - An aggregated accuracy feedback: the percentage of correct answer during the last 10 decisions.
 - An aggregated point feedback: how close you are from the highest number of points compared to the lowest number of points accumulated during the last 10 decisions.

Steps:

- First training period without points.
- Second training period with points.
- Paid part: 4 blocks of 50 decisions. Points pattern are fixed within each block.

Earnings

Additionally to the AUD5 of show-up fee, you will receive:

First part:

- One of the chosen lottery will be randomly selected. You will be paid according to its result.
- Payoffs are in ECU.
- The conversion rate between ECU and AUD is:

$$2 \text{ ECU} = \text{AUD}1$$

Second part:

- Five decisions randomly selected in each block.
- Your payment is the sum of all points accumulated during the 4*5 selected decisions.
- Losses will occur but they will be more than compensated by your gains even in the worst case scenario.
- The conversion rate between points and AUD is:

$$8 \text{ points} = \text{AUD}1$$

A5.2 Experiment 2

Task

General information:

- Determine if both circles contain either the same or a different number of dots.
- If you think that they contain the same number of dots, press the key on your keyboard.
- If you think that they contain a different number of dots, press the key on your keyboard.

For each decision, four outcomes are possible:

- To each possible outcome is associated a certain number of points.
- Your objective is to maximize the number of points earned.

		Correct answer	
		<i>Different</i>	<i>Same</i>
Decision	<i>Different</i>	Success	Failure
	<i>Same</i>	Failure	Success

Feedback:

- After each decision you will know if you were right or wrong and the number of points earned for this decision.
- Every 10 decisions, you will have:
 - An aggregated accuracy feedback: the percentage of correct answer during the last 10 decisions.
 - An aggregated point feedback: how close you were from the highest number of points during the last 10 decisions.

Steps:

- First training period without point.
- Second training period with points.
- Paid part: 2 blocks of 50 decisions. Points patterns are fixed within each block.

Earnings

Additionally to the AUD5 of show-up fee, you will receive:

- Five decisions randomly selected in each block.
- Your variable payment is the sum of all points accumulated during the 2*5 selected decisions.
- The conversion rate between points and AUD is:

$$8 \text{ points} = AUD1$$

Chapter B

Appendix of Chapter 3

B1 Procedure

This section details the stimuli characteristics and the feedback system. The study was programmed using MATLAB with the Psychophysics Toolbox version 3 (Brainard, 1997) and was run on 1920×1080 resolution computers. Subjects were enrolled using ORSEE (Greiner, 2015).

B1.1 Stimuli

Calibration — Stimuli difficulty was given by the difference in number of dots between the two circles (hereafter referred as c). Calibration for task difficulty was done using a psychophysical staircase at the subject level in order to control for individual heterogeneity regarding vision acuity (Levitt, 1971). This one-up two-down staircase consists of removing one dot after two consecutive correct answers and adding one dot after one failure. The calibration ended when subjects had achieved 35 reversals in the staircase (a reversal means a change in the c 's value). The number of dots difference c used in the experiment was computed as the mean number of dots across the last two staircase reversals. In this non-incentivized calibration period, subjects received only the stimuli information and had to determine which circle contains the most dots. Feedback on decision accuracy was provided after each decision. The targeted accuracy rate was 71%. This level was chosen for subjects to outperform random guesses (relevance of stimuli) and to avoid being too close to perfect discriminability (relevance of the payoff matrix).

Stimuli generation — To insure the same level of calibration, we presented the exact

same set of stimuli in each block to participants with the same difficulty level. Stimuli were randomly generated prior to the experiment under constraints concerning dot spacing. All dots were of the same size (diameter 0.4°) and the average distance between dots was kept constant. They appeared in random positions inside two outlined circles (diameter 5.1°). Before and after dot appearance, fixation crosses were displayed in the center of both circles at eccentricities of $\pm 8.9^\circ$.

B1.2 Feedback

The feedback system was composed of trial feedback and aggregated feedback. We aimed not to influence participants to consider more stimuli or payoff matrices. Thus, we gave them feedback on stimuli — if their decisions were correct or incorrect — and feedback on the payoff matrix — the number of points earned for the decision. In addition to this trial-by-trial feedback, every 12 decisions subjects received two types of aggregated feedback. First, we showed their accuracy rate for the last 12 decisions. Second, we provided an aggregated point feedback indicating how close they were from perfect performances. Formally the aggregated point feedback (apf) was given by the following formula:

$$apf = \frac{x - y}{z - y} \quad (\text{B.1})$$

With x the actual number of points earned, y the number of points earned if all answers had been incorrect, z the number of points earned if all answers had been correct. We provide subjects with this kind of aggregated feedback i) to induce a match between subjective beliefs and actual performances, ii) to allow non-decision periods within blocks.

B1.3 Model specification: Gaussian equal-variance SDT

This section presents how we specify our model as a Gaussian equal-variance SDT. We start by modelling the perceptual part of the decision based on our 2AFC stimuli. Then, we use the payoffs provided by the payoff matrix to distinguish the four different outcomes of SDT.

The perceptual task was a 2AFC task in which subjects faced two circles with dots inside them and aimed to identify the circle with the most dots. The SDT theory specifies that evidence received from each circle follows a normal distribution. For a larger number of dots, the amount of evidence is in average larger. The variance parameters represent

a subject's visual capacity to evaluate stimuli. The difference in dot number being relatively small, we make the usual assumption that both variance parameters are equal.

We note X_s the perception of the circle with the most dots and X_n the perception of the circle with the least dots:

$$X_n \sim \mathcal{N}(\mu_n, \sigma^2) \text{ and } X_s \sim \mathcal{N}(\mu_s, \sigma^2) \quad (\text{B.2})$$

with $\mu_n < \mu_s$.

In addition to containing relatively greater or fewer dots, each answer is either associated with high or low payoffs when correct. To include this dimension, we note X_H the perception of the circle associated with higher rewards and X_L the perception of the circle associated with lower rewards. We note e the perceived difference between these two circles:

$$e = X_H - X_L = \begin{cases} X_s - X_n \sim \mathcal{N}(\mu_s - \mu_n, 2 \times \sigma^2) & \text{if the highly rewarded circle contains more dots} \\ X_n - X_s \sim \mathcal{N}(\mu_n - \mu_s, 2 \times \sigma^2) & \text{if the highly rewarded circle contains less dots} \end{cases} \quad (\text{B.3})$$

The model can be specified as a Gaussian equal-variance Signal detection task by defining “signal” the event “the highly rewarded circle contains more dots” and “noise” the opposite event “the highly rewarded circle contains less dots”. In fact, e can be written as:

$$e \sim \begin{cases} \mathcal{N}(0, 1) & \text{if “noise”} \\ \mathcal{N}(d', 1) & \text{if “signal”} \end{cases} \quad (\text{B.4})$$

$$\text{with } d' = \frac{2 \times (\mu_s - \mu_n)}{\sqrt{2} \times \sigma} \text{ and } \epsilon = \frac{Y + (\mu_s - \mu_n)}{\sqrt{2} \times \sigma}.$$

It is thus equivalent to the Gaussian equal variance SDT in its general specification. Under this framework, the four different outcomes are defined as:

- Hit: Reporting the highly rewarded circle when it contains more dots.
- Correct rejection: Reporting the lowly rewarded circle when it contains more dots.

- Miss: Reporting the lowly rewarded circle when it contains less dots.
- False alarm: Reporting the highly rewarded circle when it contains less dots.

Based on e , information extracted from stimuli is given by the likelihood function:

$$\mathcal{L}(e) = \frac{\mathbb{P}(e|S)}{\mathbb{P}(e|N)} \quad (\text{B.5})$$

The decision strategy β is summarized by the minimal value of the likelihood function required to answer signal (reporting the circle associated with higher rewards). This value has to be interpreted as follows: an individual with a decision strategy β reports the circle associated with the higher rewards when it is (at least) β times more likely than the other circle to contain more dots.

The optimal decision strategy is defined by reporting the presence of the signal if and only if:

$$\mathcal{L}(e) > \frac{\Pi(\text{correct rejection}) - \Pi(\text{false alarm})}{\Pi(\text{hit}) - \Pi(\text{miss})} \times \frac{\mathbb{P}(N)}{\mathbb{P}(S)} \equiv \beta^* \quad (\text{B.6})$$

Where Π refers to the payoff function. In our study subjects are informed that we implement equal base rates i.e. signal and noise are equally likely ($\mathbb{P}(S) = \mathbb{P}(N) = \frac{1}{2}$).

To conclude, we address the question of potential position bias. Position biases are biases to choose either “Left” or “Right”. Our modelization is orthogonal to spatial location as it takes as its reference the circle with the most dots and not its position. However, this does not affect our results as the experiment is designed to cancel out these potential biases as target circles and highly rewarded circles are balanced on both sides.

B2 SDT estimation

This section details how we compute the descriptive statistics and how we estimate SDT parameters.

Descriptive statistics - The hit rate (HR) is equal to the number of hits (n_H) divided by the number of signal trials (n_s). Likewise, false alarm rate (FAR) is equal to the

number of false alarms (n_{FA}) divided by the number of noise trials (n_n)

$$HR = \frac{n_H}{n_s} \text{ and } FAR = \frac{n_{FA}}{n_n} \quad (\text{B.7})$$

Estimations - To estimate a couple $(\hat{d}', \hat{\beta})$ for each information order:

- (i) We express the probabilities of hit (P_H) and false alarm (P_{FA}) as functions of (d', β) :

$$P_H(d', \beta) = 1 - \phi\left(\frac{\log(\beta)}{d'} - d'/2\right) \quad (\text{B.8})$$

$$P_{FA}(d', \beta) = 1 - \phi\left(\frac{\log(\beta)}{d'} + d'/2\right) \quad (\text{B.9})$$

Where ϕ refers to the cumulative distribution function of the standard normal distribution.

- (ii) We compute the probability that, for a couple (P_H, P_{FA}) , we obtain the observed number of hits and false alarms using binomial distributions.

$$f(n_H; n_s; P_H) = \binom{n_s}{n_H} (P_H)^{n_H} (1 - P_H)^{n_s - n_H} \quad (\text{B.10})$$

$$f(n_{FA}; n_n; P_{FA}) = \binom{n_n}{n_{FA}} (P_{FA})^{n_{FA}} (1 - P_{FA})^{n_n - n_{FA}} \quad (\text{B.11})$$

- (iii) We compute the couple (d', β) that maximizes the joint probability of $f(n_H; n_s; P_H)$ and $f(n_{FA}; n_n; P_{FA})$ by maximum likelihood:

$$(\hat{d}', \hat{\beta}) = \operatorname{argmax} (\log(f(n_H; n_s; P_H(d', \beta))) + \log(f(n_{FA}; n_n; P_{FA}(d', \beta)))) \quad (\text{B.12})$$

We use a similar approach to compute one discriminability and three matrix-specific decision criteria $(\hat{d}', \hat{\beta}_{low}, \hat{\beta}_{int}, \hat{\beta}_{high})$ for each order of information:

- (i) We compute three couples (P_H, P_{FA}) :

$$P_H(d', \beta_{low}), P_{FA}(d', \beta_{low}), P_H(d', \beta_{int}), P_{FA}(d', \beta_{int}), P_H(d', \beta_{high}) \text{ and } P_{FA}(d', \beta_{high}).$$

- (ii) We compute the associated binomial distributions based on the number of hits and false alarms per matrix.

- (iii) We compute the vector $(\hat{d}', \hat{\beta}_{low}, \hat{\beta}_{int}, \hat{\beta}_{high})$ that maximizes the joint probability of the binomial distributions by maximum likelihood.

B3 Robustness test of decision criterion estimation

In this paper, we present an estimation of one couple $(\hat{d}', \hat{\beta})$ for each information order at a participant level (Estimation (1)) and another estimation of one discriminability and three matrix-specific decision criteria $(\hat{d}', \hat{\beta}_{low}, \hat{\beta}_{int}, \hat{\beta}_{high})$ for each information order at a subject level (Estimation (2)). Estimation (1) leads to slightly higher \hat{d}' (d' were in average 1.39% higher when estimated with (2) than with (1) [$t(115) = -11.672, p < 0.0001$]. The maximum difference was 6.08%.) compared to Estimation (2). Another method consists in a two-step estimation. Estimate d' as in Estimation (1) then estimate the three matrix-specific decision criteria using \hat{d}' previously estimated.

The choice could impact the existence of a payoff contrast effect. This section produces results obtained with this second methodology and concludes that this result is robust to the methodology change.

The repeated measures ANOVA rejects the hypothesis that matrix-specific mean decision strategies are equal [$F(2, 114) = 3.168, p = 0.0458, \eta_p^2 = 0.05265$]. The Tukey's test reveals that the only significant difference is between matrix M_{high} and M_{low} [$q = -2.505, p = 0.0329, d_z = 0.329$].

B4 Constant discriminability across payoff variance

In our data treatment, we assume that the discriminability is independent from payoff variances while decision criteria are allowed to vary within payoff variances. This specification has been selected based on general considerations and data analysis.

As detailed in the paper, the effect of payoff variances on decision criteria is analyzed through the scope of attention allocation: individuals pay more attention to higher payoff variances. There is thus a direct effect of attention on decision criteria. This increasing attention toward payoff matrices may take place at the expense of the processing of stimuli information. This indirect effect would lead to a decreased discriminability for higher payoff variances. Choosing not to address this indirect effect provides a better statistical power to test the direct effect. In fact, estimating six parameters at the subject level out of 288 trials (144 signal trials and 144 noise trials) would lead to unreliable

estimations. For example, 5 different subjects have a higher number of false alarms than their number of hits for one of the combinations of [information order \times payoff variances] which implies an unreasonable negative discriminability.

The hypothesis of independence is also supported by our data. We compute discriminabilities for each combination of information order (Stimuli First and Payoff First) and payoff variance (Low, Intermediate, High) by maximum likelihood. We conduct a 3 (payoff variances: “Low”, “Int”, “High”) repeated measures analysis of variance (rANOVA) on the discriminability. We find no statistically significant main effect of the payoff variance [$F(2, 114) = 0.454, p = 0.636, \eta_p^2 = 0.008$].

Finally, our specification follows existing studies on the influence of payoff variances on decisions based on SDT (Maddox et al., 2003b; Bohil and Maddox, 2003). Indeed, these studies also compute a discriminability for each task resulting in discriminabilities independent from payoff information.

To conclude, we believe that the estimation implemented is the best suited for our experiment and enhances comparability with previous studies. However new studies may explore further the impact of different payoff variances on SDT parameters as well as the underlying attentional process by allowing discriminability to vary within payoff variances.

B5 Descriptive Statistics

Table B1 describes subject hit rates and false alarm rates for each payoff matrix in each treatment. It also gives these rates at a treatment level (referred as “All”).

The order effect obtained in the paper is driven mainly by the false alarm rate. In fact, the hit rate of both treatments is not different [$t(57) = -0.8967, p = 0.374, d_z = 0.118$] while the false alarm rate is higher in the Stimuli-First treatment compared to the Payoff-First treatment [$t(57) = -2.886, p = < 0.0055, d_z = 0.379$].

The matrix effect is salient for both hit rates [$F(2, 114) = 4.758, p = 0.0104, \eta_p^2 = 0.077$] and false alarm rates [$F(2, 114) = 5.694, p = 0.0044, \eta_p^2 = 0.091$]. M_{high} is the matrix inducing this effect. With M_{high} subjects achieve more hits than with M_{int} [$q = 3.076, p = 0.0059, d_z = 0.404$] and more false alarms than with M_{low} [$q = 3.277, p = 0.0032, d_z = 0.430$]. Additionally, we also observe slightly more false alarms with M_{int} than with M_{low} [$q = 2.337, p = 0.051, d_z = 0.307$].

Statistic n=58	Treatment	Payoff matrix	M (%)	SD	Min (%)	Max (%)
Hit Rate	<i>PF</i>	M_{low}	72.56	9.17	50.00	91.67
		M_{int}	71.05	14.32	37.50	100.00
		M_{high}	74.57	10.37	50.00	95.83
		<i>All</i>	72.73	8.49	52.78	94.44
	<i>SF</i>	M_{low}	74.21	9.32	54.17	95.83
		M_{int}	70.69	15.69	29.17	100.00
		M_{high}	76.08	9.12	54.17	95.83
		<i>All</i>	73.66	8.62	52.78	95.83
False Alarm Rate	<i>PF</i>	M_{low}	33.91	12.74	8.33	62.50
		M_{int}	35.56	15.06	4.17	79.17
		M_{high}	36.85	16.01	8.33	79.17
		<i>All</i>	35.44	13.00	11.11	70.83
	<i>SF</i>	M_{low}	35.78	14.24	4.17	87.50
		M_{int}	40.01	16.18	12.50	91.67
		M_{high}	41.09	15.97	8.33	91.67
		<i>All</i>	38.96	13.56	15.28	90.28

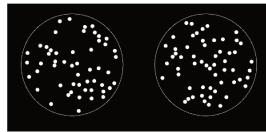
Table B1: Mean performances in terms of hit rate and false alarm rate. Treatments are Payoff-First (PF) and Stimuli-First (SF). M_{low} , M_{int} and M_{high} respectively correspond to “Low”, “Intermediate” and “High” payoff variances.

B6 Instruction

Task

In this experiment, you will play a total of 4 blocks of 72 trials. In each trial, you will receive two pieces of information.

- Stimuli: two circles containing different numbers of dots. Each circle has a 50% chance of containing more dots than the other :



- Payoff Table: number of points earned according to the outcome of your decision. The Payoff Table will be randomly selected among one of the six following tables.

Decision	<i>Left</i>	<i>Right</i>	Decision	<i>Left</i>	<i>Right</i>	Decision	<i>Left</i>	<i>Right</i>
<i>Correct</i>	12	10	<i>Correct</i>	21	1	<i>Correct</i>	17	5
<i>Incorrect</i>	9	9	<i>Incorrect</i>	- 9	- 9	<i>Incorrect</i>	- 1	- 1
Decision	<i>Left</i>	<i>Right</i>	Decision	<i>Left</i>	<i>Right</i>	Decision	<i>Left</i>	<i>Right</i>
<i>Correct</i>	10	12	<i>Correct</i>	1	21	<i>Correct</i>	5	17
<i>Incorrect</i>	9	9	<i>Incorrect</i>	- 9	- 9	<i>Incorrect</i>	- 1	- 1

Decision

In each trial, once you have received both kinds of information, you need to select one of the stimuli circles.

- If the selected circle is the one with the most dots, it is **correct**. Otherwise, it is **incorrect**.
- According to your decision and the Payoff Table, a different number of points is associated with correct and incorrect decisions.
- To select the left circle, press the key on your keyboard.
- To select the right circle, press the key on your keyboard.

Blocks

- From one block to another, the information is not displayed in the same order:
 - In “Type A blocks”, you first receive the **Payoff Table** then the **Stimuli** before the decision.
 - In “Type 1 blocks”, you first receive the **Stimuli** then the **Payoff Table** before the decision.
- You will play a total of 4 blocks alternating between the two types.

Feedback

- After each trial you will know if you were correct or not and the number of points earned.
- Every 12 trials, you will receive:
 - Aggregated accuracy feedback: the percentage of correct answers in the last 12 decisions.
 - Aggregated points feedback: how close you were from the highest number of points compared to the lowest number of points in the last 12 decisions.

Earnings

Additionally to the AUD 5 show-up reward, your variable payment will be based on :

- 6 trials randomly selected in each of the 4 blocks.
- Your variable payment is the sum of all points accumulated during the 24 selected decisions (equal proportion of each Payoff Table).
- The conversion rate between points and AUD is:

$$10 \text{ points} = \text{AUD}1$$

Chapter C

Appendix of Chapter 4

C1 Experimental design

C1.1 Instructions (original in French)

Instructions for Part 1 were distributed after the choice of the charity; instructions for Part 2 were distributed after the calibration task.

Instructions

Welcome to this experiment on decision making. Please turn off your phone and put it away. You are not allowed to communicate with other participants for the duration of the experiment, otherwise you will be excluded from the session without receiving your gains.

All the decisions you make during the session are anonymous: you will never be asked to enter your name into the computer.

During this session, you can make money for yourself and for a charity that you can choose among three. The amount you will earn depends on your decisions. Please read the instructions carefully.

This session consists of two parts in which you will make decisions grouped in tables. At the end of the session, two decisions will be randomly selected by the computer program in two different tables. The two selected tables may or may not belong to the same part. Your earnings and the earnings of the charity for these two parts will be the sum of the amounts respectively earned in these two decisions. Your total earnings consist of your payoff in these two parts, €5 for participating in this session and an

additional amount for completing a questionnaire at the end of the session. The total earnings of the charity are equal to its payoff in these two decisions. To summarize:

$$\begin{aligned} \text{Your earnings} &= \\ \text{your payoff in Decision A (part 1 or 2)} &+ \text{your payoff in Decision B (part 1 or 2)} + \text{€5} \\ &+ \text{payoff from the questionnaire} \\ \text{Charity earnings} &= \\ \text{charity earnings in Decision A (part 1 or 2)} &+ \text{charity earnings in Decision B (part 1 or} \\ &2) \end{aligned}$$

Your earnings will be paid in cash, in a separate room, privately and confidentially at the end of the session. The earnings of each charity will be paid to these charities by an experimentalist also at the end of the session. The earnings written on your receipt of payment will be equal to the sum of your earnings and the earnings of the charity. We remind you that our ethical rules are strict: all information that we communicate to you during the session is true and we commit to transfer the sums earned by the charities to these charities. We wish that at the end of the session at least one of you stays a few minutes to attend the transfer of the payment to the charities. We will make a call for volunteers at the end of the session.

We first present the instructions for the preliminary part. You will receive instructions for each new part at the end of the previous part.

Preliminary part

During this part, you will have to select which charity your decisions will affect. You will have to choose between three charities. The names of these charities and a brief description of their fields of action are listed below, in alphabetical order:

- Ligue Contre le Cancer: this charity aims to support medical research, information and public awareness as well as the fight for the respect of the sick person and his family.
- Médecins Sans Frontières: this charity provides medical assistance to populations whose lives or health are threatened in the event of armed conflicts, epidemics, natural disasters or exclusion of care.
- Restos du Coeur: this charity provides volunteer assistance to poor people by

promoting their access to free meals, their social and economic integration, and by fighting against poverty.

When you have selected a charity, please press OK. When all participants have made their decision, the instructions for Part 1 will be distributed.

Please read these instructions again. During the whole session if you have any questions, please raise your hand or press the red button on the side of your desk. We will answer your questions in private.

Part 1

During this part, you will have to make 16 decisions presented in a single table. Each decision consists in choosing between two options that influence your payoff and the payoff of the chosen charity if this decision is drawn for payment at the end of the session.

The decisions are presented in a table displaying 4 columns:

- The first column indicates the number of the decision.
- The second column indicates your payoff and the charity payoff with Option A.
- The third column indicates your payoff and the charity payoff with Option B.
- In the fourth column you enter your choice between Option A and Option B.

You must choose between the two options in each row of the table:

- With Option A your payoff is €10 and the charity payoff is €0.
- With Option B your payoff is €0 and the charity payoff is a certain amount between €0 and €30.

Consider for example Decision n°10.

- With Option A your payoff is €10 and the charity payoff is €0.
- With Option B your payoff is €0 and the charity payoff is €18.

The corresponding table row is shown below:

<i>Decision</i>	<i>Option A</i>	<i>Option B</i>	<i>Option choice</i>
10	You €10 ; Charity €0	You €0 ; Charity €18	Option A <input type="radio"/> <input type="radio"/> Option B

If you choose Option A and this decision is selected for payment, you earn €10 for you and €0 for the charity. If you wish to choose this option, select the button to the right of “Option A” as shown below:

<i>Decision</i>	<i>Option A</i>	<i>Option B</i>	<i>Option choice</i>
10	You €10 ; Charity €0	You €0 ; Charity €18	Option A <input checked="" type="radio"/> <input type="radio"/> Option B

If you choose Option B and this decision is selected for payment, you earn €0 for you and €18 for the charity. If you wish to choose this option, select the button to the left of “Option B” as shown below:

<i>Decision</i>	<i>Option A</i>	<i>Option B</i>	<i>Option choice</i>
10	You €10 ; Charity €0	You €0 ; Charity €18	Option A <input type="radio"/> <input checked="" type="radio"/> Option B

Following decisions

In the table, the amount earned by the charity increases from one row to another. From top to bottom, most people start by choosing Option A and then switch to Option B from a given decision. One way to proceed is thus to determine the best row to switch from Option A to Option B and fill in the table accordingly.

Once you have made your decisions, please press OK. You can validate your choices only once you have made all your decisions. When all participants have completed Part 1, the instructions for Part 2 will be distributed.

Please read these instructions again.

Part 2

This part is composed of 12 tables which will appear successively. In each table, you must make 21 decisions.

As in Part 1, in each row of each table you have to choose between two options. But there are two differences from the table in Part 1:

- First, the beneficiary of Option A and the beneficiary of Option B vary from one table to another. There are 4 cases:
 - You are the beneficiary of both Option A and Option B.
 - The charity is the beneficiary of both Option A and Option B.
 - You are the beneficiary of Option A and the charity is the beneficiary of Option B.
 - The charity is the beneficiary of Option A and you are the beneficiary of Option B.

You will be informed of the beneficiary of each option in each table.

- Second, the beneficiary of Option B payoff is still certain while the beneficiary of Option A payoff is not certain: it depends on the outcome of the lottery. The outcome of a lottery can take two values. Each value is associated with a number of chances that this value is drawn at random to be the outcome of the lottery. For example: €20 with 80 chances out of 100 and €0 with 20 chances out of 100. You will always be informed of the two possible payoffs. However, the number of chances of each payoff is not always known. There are 2 possible cases:
 - The number of chances is known. You will then be informed about the lottery itself. For example: €20 with 80 chances out of 100 and €0 with 20 chances out of 100.
 - the number of chances is unknown. You will then be informed of only the minimum number and the maximum number of chances for each of the two values. For example: €20 with a number of chances between 50 and 100 and €0 with a number of chances between 50 and 0. In this example, this means that €20 can have 50, 51, 52, and so on up to 100 possible chances, while €0 can have 50, 49, 48, and so on, respectively, up to 0 possible chances.

Here is another example: €20 with a number of chances between 25 and 0 and €0 with a number of chances between 75 and 100. In this example, this means that €20 can have 25, 24, 23, and so on up to 0 possible chances, while €0 can have 75, 76, 77, and so on up to 100 possible chances.

Lotteries can change from one table to another. Before discovering each table, the characteristics of the lottery will be communicated to you using numbers, like in the examples above, and graphics. The outcome of the lottery is drawn at random by the program. When the number of chances is unknown, it is selected at random among the possible numbers of chances. In this case, it is not possible to determine the probability that each number of chances will be selected.

Description of the task

In each table, for each of the 21 decisions you have to choose between:

- Option A that may pay the outcome of the lottery, either to you or to the charity;
- and Option B that pays for sure a certain amount either to you or to the charity.

As in Part 1, you have to select an option in each row of the table and validate your decisions at the end of each table.

Following decisions

As in Part 1, all the choices in each table are ordered. The amount associated with Option B increases from one row to another. From top to bottom, most people start by choosing Option A and then switch to Option B from a given decision. One way to proceed is thus to determine the best row to switch from Option A to Option B and fill in the table accordingly.

Once you have completed Part 2, you will have to complete a brief questionnaire. After the questionnaire, you will be informed of the decisions drawn by the program, of your earnings and those of your chosen charity. Then, you will be called to receive your earnings in a separate room. Please carry out your label and your pre-filled payment receipt, and leave the instructions on your desk.

Please read these instructions again.

C1.2 Screenshot

We report below decisions as they appeared on the subjects' computer screens. The text has been translated from French into English.


For each row, click on the button corresponding to your favorite option. From top to bottom, most people start by choosing Option A and then switch to Option B from a given decision. One way to proceed is thus to determine the best row to switch from Option A to Option B and fill in the table accordingly.

Decision	Option A	Option B	Option choice
1	You: €10 ; LCC: €0	You: €0 ; LCC: €0.0	Option A <input type="radio"/> Option B <input type="radio"/>
2	You: €10 ; LCC: €0	You: €0 ; LCC: €2.0	Option A <input type="radio"/> Option B <input type="radio"/>
3	You: €10 ; LCC: €0	You: €0 ; LCC: €4.0	Option A <input type="radio"/> Option B <input type="radio"/>
4	You: €10 ; LCC: €0	You: €0 ; LCC: €6.0	Option A <input type="radio"/> Option B <input type="radio"/>
5	You: €10 ; LCC: €0	You: €0 ; LCC: €8.0	Option A <input type="radio"/> Option B <input type="radio"/>
6	You: €10 ; LCC: €0	You: €0 ; LCC: €10.0	Option A <input type="radio"/> Option B <input type="radio"/>
7	You: €10 ; LCC: €0	You: €0 ; LCC: €12.0	Option A <input type="radio"/> Option B <input type="radio"/>
8	You: €10 ; LCC: €0	You: €0 ; LCC: €14.0	Option A <input type="radio"/> Option B <input type="radio"/>
9	You: €10 ; LCC: €0	You: €0 ; LCC: €16.0	Option A <input type="radio"/> Option B <input type="radio"/>
10	You: €10 ; LCC: €0	You: €0 ; LCC: €18.0	Option A <input type="radio"/> Option B <input type="radio"/>
11	You: €10 ; LCC: €0	You: €0 ; LCC: €20.0	Option A <input type="radio"/> Option B <input type="radio"/>
12	You: €10 ; LCC: €0	You: €0 ; LCC: €22.0	Option A <input type="radio"/> Option B <input type="radio"/>
13	You: €10 ; LCC: €0	You: €0 ; LCC: €24.0	Option A <input type="radio"/> Option B <input type="radio"/>
14	You: €10 ; LCC: €0	You: €0 ; LCC: €26.0	Option A <input type="radio"/> Option B <input type="radio"/>
15	You: €10 ; LCC: €0	You: €0 ; LCC: €28.0	Option A <input type="radio"/> Option B <input type="radio"/>
16	You: €10 ; LCC: €0	You: €0 ; LCC: €30.0	Option A <input type="radio"/> Option B <input type="radio"/>

OK

Figure C1: Calibration task, charity: “Ligue Contre le Cancer” (LCC).

Notes: Subjects have to choose between Option A and Option B for each row of the price list. Option A pays €10 to the subject. Option B pays a payoff to the “Ligue Contre le Cancer” that varies between €0 and €30.



Preliminary part
Part 1
Part 2
Questionnaire

Table 11/12

For each row, click on the button corresponding to your favorite option. From top to bottom, most people start by choosing Option A and then switch to Option B from a given decision. One way to proceed is thus to determine the best row to switch from Option A to Option B and fill in the table accordingly.

Decision	Option A	Option B	Option choice
1	You: €0 ; LCC: outcome of the lottery	You: €0 ; LCC: €0.0	Option A <input type="radio"/> Option B <input type="radio"/>
2	You: €0 ; LCC: outcome of the lottery	You: €0 ; LCC: €1.0	Option A <input type="radio"/> Option B <input type="radio"/>
3	You: €0 ; LCC: outcome of the lottery	You: €0 ; LCC: €2.0	Option A <input type="radio"/> Option B <input type="radio"/>
4	You: €0 ; LCC: outcome of the lottery	You: €0 ; LCC: €3.0	Option A <input type="radio"/> Option B <input type="radio"/>
5	You: €0 ; LCC: outcome of the lottery	You: €0 ; LCC: €4.0	Option A <input type="radio"/> Option B <input type="radio"/>
6	You: €0 ; LCC: outcome of the lottery	You: €0 ; LCC: €5.0	Option A <input type="radio"/> Option B <input type="radio"/>
7	You: €0 ; LCC: outcome of the lottery	You: €0 ; LCC: €6.0	Option A <input type="radio"/> Option B <input type="radio"/>
8	You: €0 ; LCC: outcome of the lottery	You: €0 ; LCC: €7.0	Option A <input type="radio"/> Option B <input type="radio"/>
9	You: €0 ; LCC: outcome of the lottery	You: €0 ; LCC: €8.0	Option A <input type="radio"/> Option B <input type="radio"/>
10	You: €0 ; LCC: outcome of the lottery	You: €0 ; LCC: €9.0	Option A <input type="radio"/> Option B <input type="radio"/>
11	You: €0 ; LCC: outcome of the lottery	You: €0 ; LCC: €10.0	Option A <input type="radio"/> Option B <input type="radio"/>
12	You: €0 ; LCC: outcome of the lottery	You: €0 ; LCC: €11.0	Option A <input type="radio"/> Option B <input type="radio"/>
13	You: €0 ; LCC: outcome of the lottery	You: €0 ; LCC: €12.0	Option A <input type="radio"/> Option B <input type="radio"/>
14	You: €0 ; LCC: outcome of the lottery	You: €0 ; LCC: €13.0	Option A <input type="radio"/> Option B <input type="radio"/>
15	You: €0 ; LCC: outcome of the lottery	You: €0 ; LCC: €14.0	Option A <input type="radio"/> Option B <input type="radio"/>
16	You: €0 ; LCC: outcome of the lottery	You: €0 ; LCC: €15.0	Option A <input type="radio"/> Option B <input type="radio"/>
17	You: €0 ; LCC: outcome of the lottery	You: €0 ; LCC: €16.0	Option A <input type="radio"/> Option B <input type="radio"/>
18	You: €0 ; LCC: outcome of the lottery	You: €0 ; LCC: €17.0	Option A <input type="radio"/> Option B <input type="radio"/>
19	You: €0 ; LCC: outcome of the lottery	You: €0 ; LCC: €18.0	Option A <input type="radio"/> Option B <input type="radio"/>
20	You: €0 ; LCC: outcome of the lottery	You: €0 ; LCC: €19.0	Option A <input type="radio"/> Option B <input type="radio"/>
21	You: €0 ; LCC: outcome of the lottery	You: €0 ; LCC: €20.0	Option A <input type="radio"/> Option B <input type="radio"/>

OK

Number of chances unknown:
 - €20 with a number of chances between 0 and 100 out of 100.
 - €0 with a number of chances between 100 and 0 out of 100.





Figure C2: Decision “charity-valuation of lottery-for-charity” (Charity/Charity) under full ambiguity, charity: “Ligue Contre le Cancer” (LCC), $X = €20$.

Notes: Subjects have to choose between Option A and Option B for each row of the price list. Option A pays €20 to the “Ligue Contre le Cancer” with $p \in [0, 1]$ and €0 otherwise. Option B pays a safe payoff to the “Ligue Contre le Cancer” that varies between €0 and €20.



Preliminary part
Part 1
Part 2
Questionnaire

Table 1/12

For each row, click on the button corresponding to your favorite option. From top to bottom, most people start by choosing Option A and then switch to Option B from a given decision. One way to proceed is thus to determine the best row to switch from Option A to Option B and fill in the table accordingly.

Decision	Option A	Option B	Option choice
1	You: outcome of the lottery ; LCC: €0	You: €0 ; LCC: €0.0	Option A <input type="radio"/> Option B <input type="radio"/>
2	You: outcome of the lottery ; LCC: €0	You: €0 ; LCC: €1.0	Option A <input type="radio"/> Option B <input type="radio"/>
3	You: outcome of the lottery ; LCC: €0	You: €0 ; LCC: €2.0	Option A <input type="radio"/> Option B <input type="radio"/>
4	You: outcome of the lottery ; LCC: €0	You: €0 ; LCC: €3.0	Option A <input type="radio"/> Option B <input type="radio"/>
5	You: outcome of the lottery ; LCC: €0	You: €0 ; LCC: €4.0	Option A <input type="radio"/> Option B <input type="radio"/>
6	You: outcome of the lottery ; LCC: €0	You: €0 ; LCC: €5.0	Option A <input type="radio"/> Option B <input type="radio"/>
7	You: outcome of the lottery ; LCC: €0	You: €0 ; LCC: €6.0	Option A <input type="radio"/> Option B <input type="radio"/>
8	You: outcome of the lottery ; LCC: €0	You: €0 ; LCC: €7.0	Option A <input type="radio"/> Option B <input type="radio"/>
9	You: outcome of the lottery ; LCC: €0	You: €0 ; LCC: €8.0	Option A <input type="radio"/> Option B <input type="radio"/>
10	You: outcome of the lottery ; LCC: €0	You: €0 ; LCC: €9.0	Option A <input type="radio"/> Option B <input type="radio"/>
11	You: outcome of the lottery ; LCC: €0	You: €0 ; LCC: €10.0	Option A <input type="radio"/> Option B <input type="radio"/>
12	You: outcome of the lottery ; LCC: €0	You: €0 ; LCC: €11.0	Option A <input type="radio"/> Option B <input type="radio"/>
13	You: outcome of the lottery ; LCC: €0	You: €0 ; LCC: €12.0	Option A <input type="radio"/> Option B <input type="radio"/>
14	You: outcome of the lottery ; LCC: €0	You: €0 ; LCC: €13.0	Option A <input type="radio"/> Option B <input type="radio"/>
15	You: outcome of the lottery ; LCC: €0	You: €0 ; LCC: €14.0	Option A <input type="radio"/> Option B <input type="radio"/>
16	You: outcome of the lottery ; LCC: €0	You: €0 ; LCC: €15.0	Option A <input type="radio"/> Option B <input type="radio"/>
17	You: outcome of the lottery ; LCC: €0	You: €0 ; LCC: €16.0	Option A <input type="radio"/> Option B <input type="radio"/>
18	You: outcome of the lottery ; LCC: €0	You: €0 ; LCC: €17.0	Option A <input type="radio"/> Option B <input type="radio"/>
19	You: outcome of the lottery ; LCC: €0	You: €0 ; LCC: €18.0	Option A <input type="radio"/> Option B <input type="radio"/>
20	You: outcome of the lottery ; LCC: €0	You: €0 ; LCC: €19.0	Option A <input type="radio"/> Option B <input type="radio"/>
21	You: outcome of the lottery ; LCC: €0	You: €0 ; LCC: €20.0	Option A <input type="radio"/> Option B <input type="radio"/>

OK

Number of chances unknown:
 - €10 with a number of chances between 25 and 75 out of 100.
 - €0 with a number of chances between 75 and 25 out of 100.

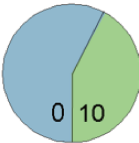


Figure C3: Decision “charity-valuation of lottery-for-self” (Charity/Self) under partial ambiguity, charity: “Ligue Contre le Cancer” (LCC), $X = €20$.

Notes: Subjects have to choose between Option A and Option B for each row of the price list. Option A pays €10 to the subject with $p \in [0.25, 0.75]$ and €0 otherwise. Option B pays a safe payoff to the “Ligue Contre le Cancer” that varies between €0 and €20.

C1.3 Lottery visual aid

We describe the visual aid for the lotteries. We use as an example the lottery-for-charity of a subject with $X = 20$. Under risk the lottery is $(20, 0.5; 0, 0.5)$. Its presentation is stationary (Figure C4-a). Under partial ambiguity, the lottery is $(20, p; 0, p)$ with $p \in [0.25, 0.75]$. The limit between the two portions of the circle is moving back and forth between one extreme situation *i.e.* $(20, 0.75; 0, 0.25)$, and the other *i.e.* $(20, 0.25; 0, 0.75)$ (Figure C4-b). Under full ambiguity, the lottery is $(20, p; 0, p)$ with $p \in [0, 1]$. The limit between the two portions of the circle is thus moving back and forth between one extreme situation *i.e.* $(20, 1; 0, 0)$, and the other *i.e.* $(20, 0; 0, 1)$ (Figure C4-c). Under ambiguity, the font size of each payoff (“0” or “20”) increases with its likelihood. Finally, the time needed to describe all possible situations, *i.e.* return to the same initial situation, is controlled to be equal to 10 seconds.

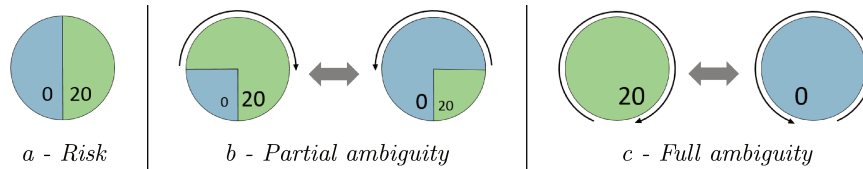


Figure C4: Schematic presentation of the lottery visual aid (potential gain of €20).

Notes: Thin black arrows represent movements of the limit between both parts of the circle. Thick grey arrows indicate the constant repetition of these movements.

C2 Robustness analysis with exclusion of censored subjects

X is defined as the monetary payoff such that a subject is indifferent between receiving €10 or donating € X to the charity. For 71 subjects out of 178 the estimation of X is censored because these subjects have always chosen the selfish option in the calibration task. For these subjects, we consider in the main analysis that X is equal to 30 because 30 is the maximal value of X that can be identified with our task.

Some analyses are based on comparisons between self-payoffs and charity-payoffs that use X to express both measures on the same scale. These results may thus be impacted by censorship as charity-valuations are then over-estimated when compared to

self-valuations. In this section, we replicate these analyses after excluding the censored subjects to check whether censorship impacts our conclusions. First, we replicate the analysis that aggregates partial ambiguity and full ambiguity and second, the analysis that differentiates between these two levels of ambiguity.

C2.1 Risk and ambiguity

Table A-1 reproduces model (1) and model (2) of Table 4.3 after excluding censored subjects. The coefficient associated to the “Trade-off” dummy (equal to 1 if the safe amount and the lottery outcome do not have the same beneficiary) gives the effect of the trade-off between lotteries-for-self and donations under risk (model 1) and under ambiguity (model 2). We find an increase of the lottery valuation by 17.22 p.p. under risk ($p < 0.001$) and 20.68 p.p. under ambiguity ($p < 0.001$). Result 1-R and Result 1-A are thus robust to the exclusion of censored subjects. These two increases are not significantly different from each other ($p = 0.243$, Table A-1 model (1): effect associated with “Trade-off \times Risk”). Result 4 is thus also robust to the exclusion of censored subjects.

C2.2 Risk, partial ambiguity, and full ambiguity

When differentiating between ambiguity levels, Result 2-A regarding excuse-driven behavior under ambiguity-for-self is supported both for partial ambiguity and for full ambiguity ($p < 0.001$ in both cases). Result 3 is also supported for any level of ambiguity. Under partial ambiguity, the charity-valuations of lotteries-for-self increases by 21.57 p.p. compared to self-valuations. Under full ambiguity, it increases by 19.79 p.p. The difference with the increase under risk (17.22 p.p.) is not significantly different either from the increase under partial ambiguity (4.34 p.p., $p = 0.102$) or the increase under full ambiguity (2.57 p.p., $p = 0.524$). Likewise, Result 4 is supported for any level of ambiguity. Under partial ambiguity, the self-valuation of lottery-for-charity decreases by 2.31 p.p. compared to the charity-valuation of the same lottery. Under full ambiguity, it decreases by 0.07 p.p. The difference with the decrease under risk (2.59 p.p.) is not statistically significantly different from the decrease under partial ambiguity (0.28 p.p., $p = 0.928$) or the decrease under full ambiguity (2.52 p.p., $p = 0.384$). In conclusion, previous findings are supported for each level of ambiguity when excluding censored subjects.

	<i>Dependent variable: Lottery valuation (%)</i>	
	(1)	(2)
Trade-off	17.22*** (3.30)	20.68*** (3.01)
Charity	-2.87 (2.20)	-1.19 (1.93)
Ambiguity	-6.40*** (1.94)	-6.40*** (1.94)
Charity \times Trade-off	-19.81*** (5.00)	-21.87*** (4.53)
Charity \times Ambiguity	1.68 (2.59)	
Charity \times Risk		-1.68 (2.59)
Trade-off \times Ambiguity	3.46 (2.96)	
Trade-off \times Risk		-3.46 (2.96)
Charity \times Trade-off \times Ambiguity	-2.06 (4.06)	
Charity \times Trade-off \times Risk		2.06 (4.06)
Intercept	49.46*** (1.87)	49.46*** (1.87)
Censored subjects	No	No
Num. obs.	1284	1284
Num. ind. obs.	107	107

Table A-1: Lottery valuation excluding censored subjects

*Notes: OLS regressions with errors clustered at the subject level. “Trade-off”: Trade-off between self-payoff and donations. “Charity”: Outcome of the lottery for the charity. “Self”: Outcome of the lottery for the subject. “Ambiguity”: Lottery under ambiguity. “Risk”: Lottery under risk. The symbol “ \times ” is used for interaction variables. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.*

C3 Censorship and selection bias

In this section we study whether excluding censored subjects from the analysis introduces a selection bias that biases results against excuse-driven behavior. Indeed, recall that censored subjects are those who have more selfish preferences in the calibration task.

To test this assumption, we evaluate whether increased altruism is associated with reduced excuse-driven behavior. The level of altruism is given by X (the value of the donation equivalent to receiving €10). The size of excuse-driven behavior is given by the difference in lottery valuation in situations with a self *vs.* charity trade-off and in situations without such trade-off. Excuse-driven behavior in the valuation of lotteries-for-self is identified by a positive difference while excuse-driven behavior in the valuation of lotteries-for-charity is identified by a negative difference.

We report the results of four linear regressions with errors clustered at the subject

level. Models (1) and (3) include all subjects, whereas models (2) and (4) exclude the censored subjects. The dependent variable is the lottery valuation. The independent variables include three dummy variables. “*Charity*” is equal to 1 for lotteries-for-charity (symmetrically “*Self*” is equal to 0 in this case). “*Trade-off*” is equal to 1 if the safe payoff and the lottery outcome do not have the same beneficiary. “ $(X - \bar{X})$ ” is equal to the centered calibration value X . We also interact the previous variables to generate interaction terms. The coefficient associated with the variable “*Trade-off* \times $(X - \bar{X})$ ” gives the effect of an increase in X in trade-off situations on: lottery-for-self valuation in models (1) and (3), and on lottery-for-charity valuation in models (2) and (4). Regressions are reported in Table A-2.

We find that excuse-driven behavior is reinforced for subjects with a high value of X for both types of lotteries. Conclusions are similar when considering all subjects or only uncensored subjects ($p < 0.001$ in the four models). We conclude that excluding censored subjects corresponds to excluding subjects that are more prone to behave in a self-excusing manner. It supports previous findings from Exley (2015a).

	<i>Dependent variable: Lottery valuation (%)</i>			
	(1)	(2)	(3)	(4)
Trade-off	29.33*** (1.76)	-7.62*** (1.62)	19.52*** (2.33)	-1.66 (1.96)
$(X - \bar{X})$	1.72*** (0.40)	-2.71*** (0.43)	1.72*** (0.48)	-2.21*** (0.59)
Charity	-2.81** (1.37)		-1.75 (1.61)	
Self		2.81** (1.37)		1.75 (1.61)
Charity \times Trade-off	-36.95*** (2.67)		-21.18*** (3.40)	
Self \times trade-off		36.95*** (2.67)		21.18*** (3.40)
Trade-off $\times (X - \bar{X})$	2.33*** (0.25)	-1.57*** (0.25)	2.09*** (0.30)	-1.52*** (0.32)
Charity $\times (X - \bar{X})$	-0.18 (0.17)		-0.11 (0.22)	
Self $\times (X - \bar{X})$		0.18 (0.17)		0.11 (0.22)
Charity \times Trade-off $\times (X - \bar{X})$	-3.90*** (0.37)		-3.61*** (0.42)	
Self \times Trade-off $\times (X - \bar{X})$		3.90*** (0.37)		3.61*** (0.42)
Intercept	44.33*** (1.26)	41.52*** (1.33)	45.19*** (1.57)	43.44*** (1.65)
Censored subjects	Yes	Yes	No	No
Num. obs.	2136	2136	1284	1284
Num. ind. obs.	178	178	107	107

Table A-2: Effect of the level of altruism (X) on excuse-driven behavior

Notes: OLS regressions with errors clustered at the subject level. “Trade-off”: Trade-off between self-payoff and donations. “Charity”: Outcome of the lottery for the charity. “Self”: Outcome of the lottery for the subject. “ $(X - \bar{X})$ ”: centered calibration value X . The symbol “ \times ” is used for interaction variables. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

C4 Robustness analysis with exclusion of multiple switchers

38 subjects out of 178 (21%) have switched multiple times in one of the twelve price lists of the main task. In total, these decisions represent 3.8% of all decisions. In the main analysis, we have included these decisions by considering the first switching point as being the true switching point. This section replicates this analysis after excluding the subjects who switched more than once to test whether our results are impacted by multiple switchers.¹

¹Alternatively, we could have excluded only the price lists in which subjects switched more than once. However, since we used a within-subject design, excluding some decisions made by a

Table A-3 replicates the regressions displayed in Table 4.3 excluding multiple switchers (models (1), (2), (5), and (6)), or excluding both multiple switchers and censored subjects (models (3), (4), (7), and (8)).

Excuse-driven behavior under risk-for-self is confirmed, as the coefficients associated with “*Trade-off*” in models (1) and (3) are significant ($p < 0.001$). Result 1-R is thus robust to the exclusion of multiple switchers. Likewise, excuse-driven behavior under ambiguity-for-self is also confirmed, as the coefficients associated with “*Trade-off*” in models (2) and (4) are significant ($p < 0.001$). It confirms the robustness of Result 1-A.

We find evidence of excuse-driven behavior under risk-for-charity only when including the censored subjects (coefficient associated with “*Trade-off*”: model (5) $p < 0.001$, model (7) $p = 0.585$). Likewise, excuse-driven behavior under ambiguity-for-charity is supported only when including censored subjects (coefficient associated with “*Trade-off*”: model (6) $p < 0.001$, model (8) $p = 0.961$). Result 2-R and Result 2-A are thus robust to the exclusion of multiple switchers.

The coefficients associated with “Trade-off \times Risk” in models (1) and (3) are not significant ($p = 0.630$ and $p = 0.149$, respectively). Result 3 on the absence of reinforced excuse-driven behavior under ambiguity-for-self compared to risk-for-self is supported when excluding multiple switchers.

Finally, the coefficients associated with “Trade-off \times Risk” in models (5) and (7) are also not significant ($p = 0.163$ and $p = 0.458$, respectively). Result 4 on the absence of reinforced excuse-driven behavior under ambiguity-for-charity compared to risk-for-charity is also supported when excluding multiple switchers.

subject but not others could bias our results.

	<i>Dependent variable: Lottery valuation</i>							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Trade-off	30.80*** (2.59)	31.68*** (2.47)	19.53*** (3.37)	22.56*** (3.34)	-9.65*** (2.62)	-6.76*** (2.08)	-1.69 (3.09)	0.13 (2.57)
Ambiguity	-2.47* (1.37)	-2.47* (1.37)	-4.58*** (1.44)	-4.58*** (1.44)	-6.48*** (1.57)	-6.48*** (1.57)	-5.92*** (1.80)	-5.92*** (1.80)
Charity	0.66 (1.74)	-3.36** (1.53)	-0.09 (2.07)	-1.44 (1.75)				
Self					-0.66 (1.74)	3.36** (1.53)	0.09 (2.07)	1.44 (1.75)
Charity × Trade-off	-40.45*** (4.24)	-38.45*** (3.92)	-21.22*** (5.55)	-22.44*** (5.17)				
Self × Trade-off					40.45*** (4.24)	38.45*** (3.92)	21.22*** (5.55)	22.44*** (5.17)
Charity × Ambiguity	-4.02** (1.74)		-1.34 (2.08)					
Self × Ambiguity					4.02** (1.74)		1.34 (2.08)	
Charity × Risk		4.02** (1.74)		1.34 (2.08)				
Self × Risk						-4.02** (1.74)		-1.34 (2.08)
Trade-off × Ambiguity	0.89 (1.84)		3.03 (2.10)		2.89 (2.07)		1.81 (2.44)	
Trade-off × Risk		-0.89 (1.84)		-3.03 (2.10)		-2.89 (2.07)		-1.81 (2.44)
Charity × Trade-off × Ambiguity	2.01 (2.55)		-1.22 (3.04)					
Charity × Trade-off × Risk		-2.01 (2.55)		1.22 (3.04)				
Self × Trade-off × Ambiguity					-2.01 (2.55)		1.22 (3.04)	
Self × Trade-off × Risk						2.01 (2.55)		-1.22 (3.04)
Intercept	46.77*** (1.56)	46.77*** (1.56)	49.84*** (1.72)	49.84*** (1.72)	47.43*** (1.81)	47.43*** (1.81)	49.75*** (1.94)	49.75*** (1.94)
Num. obs.	1728	1728	960	960	1728	1728	960	960
Num. ind. obs.	144	144	80	80	144	144	80	80
Sub. Mult. Switch	No	No	No	No	No	No	No	No
Censored subjects	Yes	Yes	No	No	Yes	Yes	No	No

Table A-3: Lottery valuation without multiple switchers

Notes: OLS regressions with errors clustered at the subject level. “Trade-off”: Trade-off between self-payoff and donations. “Charity”: Outcome of the lottery for the charity. “Self”: Outcome of the lottery for the subject. “Ambiguity”: Lottery under ambiguity. “Risk”: Lottery under risk. The symbol “×” is used for interaction variables. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Chapter D

Appendix of Chapter 5

D1 Instructions (original in Dutch)

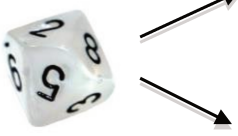
We thank you for participating in this experiment. This experiment consists of five parts in which we ask you to make decisions. Please read the instructions carefully as the earnings you will make depend mainly on your decisions and partly on chance. This questionnaire is about your own preferences. Always choose the option that you prefer.

To determine your earnings in this experiment, we will randomly draw one of the first four parts. Each of these parts has the same chance to be selected. Your earnings will be transferred to your bank account.


Part 1

You have to manage a fund of **100 Euros**. You can invest the 100 Euros in a single project, in two projects or in three projects. Each project is characterized by various opportunities for earnings.

- Project 1: You will earn **6 Euro-cents** for each Euro invested.
- Project 2: A fair ten-sided die is rolled. If the roll of the die is 1, 2, 3, 4, or 5 you will earn **2 Euro-cents** for each Euro invested. If the roll of the die is 6, 7, 8, 9 or 10, you will earn **14 Euro-cents** for each Euro invested.

	1,2,3,4,5	You earn 2 Euro-cents * number of Euros invested
	6,7,8,9,10	You earn 14 Euro-cents * number of Euros invested

- Project 3: A fair ten-sided die is rolled. If the roll of the die is 1, 2, 3, 4, 5, 6, 7, or 8 you will earn **2 Euro-cents** for each Euro invested. If the roll of the die is 9 or 10, you will earn **42 Euro-cents** for each Euro invested.

	1,2,3,4,5,6,7,8	You earn 2 Euro-cents * number of Euros invested
	9, 10	You earn 42 Euro-cents * number of Euros invested

If this part is drawn for actual payment, you will earn **the sum of the returns** earned on the investments made in the three projects.

Please indicate below how many Euros you invest in each project. Put 0 if you do not invest in a specific project. The total amount invested must equal 100 Euros.

I invest _____ Euros in Project 1


I invest _____ Euros in Project 2

I invest _____ Euros in Project 3

TOTAL: 100 Euros


Part 2

You receive 10 Euros. A fair ten-sided die is rolled. If the roll of the die is 1, 2, 3, 4, 5, 6, 7, 8, or 9 you will keep these **10 Euros**. If the roll of the die is 10, you will lose your **10 Euros**.


	↗	1,2,3,4,5,6,7,8,9	You keep your 10 Euros
	↘	10	You lose your 10 Euros

However, you can insure yourself against this risk. Five different options are offered to you with varying degrees of protection. Please choose your favorite option.


- **Option 1:** you do not buy any insurance. If the roll of the die is 1, 2, 3, 4, 5, 6, 7, 8, or 9, you earn **10 Euros**. If the roll of the die is 10, you earn **0 Euro**.

	↗	1,2,3,4,5,6,7,8,9	You earn 10 Euros
	↘	10	You earn 0 Euro


- **Option 2:** this insurance costs you **0.5 Euros**. If the roll of the die is 1, 2, 3, 4, 5, 6, 7, 8, or 9, you earn **9.5 Euros**. If the roll of the die is 10, you earn **1.5 Euros**.

	↗	1,2,3,4,5,6,7,8,9	You earn 9.5 Euros
	↘	10	You earn 1.5 Euro


- **Option 3:** this insurance costs you **1 Euro**. If the roll of the die is 1, 2, 3, 4, 5, 6, 7, 8, or 9, you earn **9 Euros**. If the roll of the die is 10, you earn **3 Euros**.

	↗	1,2,3,4,5,6,7,8,9	You earn 9 Euros
	↘	10	You earn 3 Euros

- **Option 4:** this insurance costs you **1.5 Euro**. If the roll of the die is 1, 2, 3, 4, 5, 6, 7, 8, or 9, you earn **8.5 Euros**. If the roll of the die is 10, you earn **4.5 Euros**.

	↗	1,2,3,4,5,6,7,8,9	You earn 8.5 Euros
	↘	10 184	You earn 4.5 Euros

- **Option 5:** this insurance costs you **2.5** Euros. If the roll of the die is 1, 2, 3, 4, 5, 6, 7, 8, or 9, you earn **7.5 Euros**. If the roll of the die is 10, you earn **7.5** Euros.

	↗	1,2,3,4,5,6,7,8,9	You earn 7.5 Euros
	↘	10	You earn 7.5 Euros

At the end of the experiment, if this part is drawn for actual payment, you will earn the money corresponding to your insurance choice and to the roll of the die.

Please choose your preferred option.

- Option 1
- Option 2
- Option 3
- Option 4
- Option 5

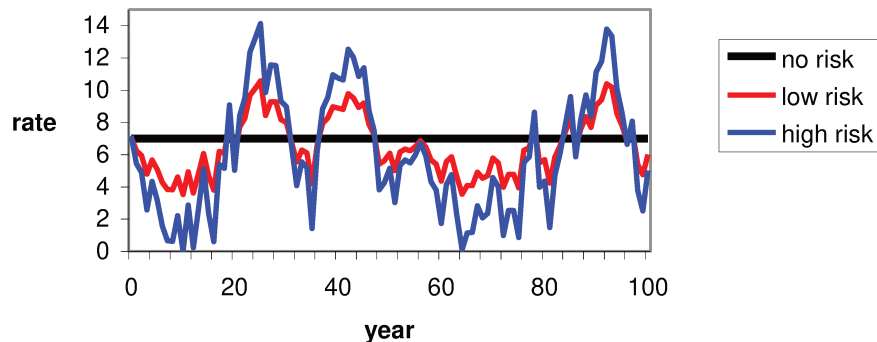
Part 3

Imagine the following scenario: You borrow 10 Euros that must be repaid after ten ‘years’.
Every year you receive an income of 1.5 Euros and you have to pay the interest on your loan.
You have to choose one of three interest-rate options for your loan.

Please choose your favorite option among the three following options:

- **Option 1:** You pay a fixed interest rate.
Every year, the interest rate amounts to **7%** of your loan, i.e. 70 Euro-cents are deducted from your income.
- **Option 2:** You pay an annually adjustable interest rate that can change slightly from year to year. The first year, the interest rate amounts to **6%**, i.e. 60 Euro-cents are deducted from your income. Any following year, this rate may vary, **up to 2% below** its value of the previous year and **up to 2% above** its value of the previous year.
- **Option 3:** You pay an annually adjustable interest rate that can change more substantially from year to year, but has a lower initial rate. The first year, the interest rate amounts to **5%**, i.e. 50 Euro-cents are deducted from your income. Any following year, this rate may vary, **up to 4% below** its value of the previous year and **up to 4% above** its value of the previous year.

The following figure shows how the interest rates developed in the previous 100 years (this year you are in year 100). The black line corresponds to Option 1, the red line to Option 2, and the blue line to Option 3.



Please choose your preferred option.

- Option 1
Option 2
Option 3

At the end of the experiment, if this part is drawn for actual payment, you will earn the sum of (1,5 Euros – interest) for each of the ten years.

Part 4a

In the Table below, you make 10 choices between “option A” and “option B”.



















- Option A is a lottery that pays you either **8 Euros or 6.4 Euros**.
- Option B is a lottery that pays you either **15.4 Euros or 0.4 Euro**.

Look at Decision 1. A ten-sided die is rolled. If the roll of the die is 1, you earn 8 Euros with option A and 15.4 Euros with option B. If the roll of the die is 2,3,4,5,6,7,8,9, or 10, you earn 6.4 Euros with option A and 0,4 Euro with option B.

Look at Decision 2. If the roll of the die is 1 or 2, you earn 8 Euros with option A and 15.4 Euros with option B. If the roll of the die is 3,4,5,6,7,8,9, or 10, you earn 6.4 Euros with option A and 0.4 Euro with option B.

As you move down the table, the chances of the higher prize for each option increase.

Please decide if you prefer option A or option B for each decision.

Decision number	If the roll of the die is	You earn in Euros		Your choice	
		Option A	Option B	Option A	Option B
1	 1	8	15,4	O	O
	 2,3,4,5,6,7,8,9,10	6,4	0,4		
2	 1,2	8	15,4	O	O
	 3,4,5,6,7,8,9,10	6,4	0,4		
3	 1,2,3	8	15,4	O	O
	 4,5,6,7,8,9,10	6,4	0,4		
4	 1,2,3,4	8	15,4	O	O
	 4,5,6,7,8,9,10	6,4	0,4		
5	 1,2,3,4,5	8	15,4	O	O
	 6,7,8,9,10	6,4	0,4		
6	 1,2,3,4,5,6	8	15,4	O	O
	 7,8,9,10	6,4	0,4		
7	 1,2,3,4,5,6,7	8	15,4	O	O
	 8,9,10	6,4	0,4		
8	 1,2,3,4,5,6,7,8	8	15,4	O	O
	 9,10	6,4	0,4		
9	 1,2,3,4,5,6,7,8,9	8	15,4	O	O
	 10	6,4	0,4		
10	All cases	8	15,4	O	O

At the end of the experiment, if this part is drawn for actual payment, the computer program will randomly select one of the 10 decisions. For the option chosen in this decision, the “roll of a die” by the program will determine your earnings.

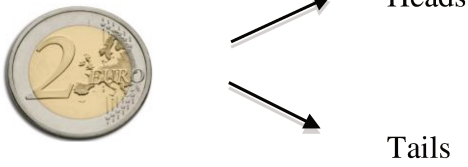
Part 4b

You receive 800 Euro-cents (8 Euros). You decide how many of these Euro-cents (between 0 and 800, inclusive) to invest. Those Euro-cents not invested are yours to keep.

The investment has a 50% chance of success. To determine if your investment is a success, a fair coin is tossed.

- If the coin comes up heads, your investment pays 2.5 times the amount you invested.
- If the coin comes up tails, you lose the amount invested.

At the end of the experiment, if this part is drawn for actual payment, your earnings are determined as indicated in the following figure.

	<p>You earn: 800 – the Euro-cents invested + 2,5 * the Euro-cents invested</p> <p>You earn: 800 – the Euro-cents invested</p>
---	--




Please indicate how many Euro-cents you want to invest: _____ Euro-cents.

Part 4c




We display two tables successively. If this part is drawn for payment at the end of the experiment, the program will select randomly one decision in one of the two tables for payment.

In **Table 1**, you make 14 choices between “option A” and “option B”.

- **Option A** is a lottery that pays you **8 Euros** if the roll of a ten-sided die is 1, 2, or 3 or **2 Euros** if the roll of the die is 4, 5, 6, 7, 8, 9, or 10. **Option A is identical in the 14 decisions.**



		1,2,3	You earn 8 Euros
		4,5,6,7,8,9,10	You earn 2 Euros

- **Option B** is a lottery that pays you either **1 Euro** if the roll of the die is 2, 3, 4, 5, 6, 7, 8, 9, or 10, or **an amount increasing from 13.6 Euros to 340 Euros along the 14 decisions** if the roll of the die is 1. For example, in decision 1, if the roll of the die is 1, it pays 13.6 Euros; in decision 2, it pays 15 Euros; finally in decision 14, it pays 340 Euros.

		1	You earn x Euros (with x=13.6 Euros in decision 1 to 340 Euros in decision 14)
		2,3,4,5,6,7,8,9,10	You earn 1 Euro

Please decide if you prefer option A or option B for each of the 14 following decisions. As soon as you click option B in one decision, the option A is automatically selected in all the decisions up to the top of the table and the option B is automatically selected in all the following decisions down to the bottom of the table. In other words, **you simply need to identify the decision from which you prefer option B.**


Table 1

Decision Number	Option A		Option B		You choose	
	 If the die indicates	You earn in Euros	 If the die indicates	You earn in Euros	Option A	Option B
1	1,2,3	8	1	13.6	O	O
	4,5,6,7,8,9,10	2	2,3,4,5,6,7,8,9,10	1		
2	1,2,3	8	1	15	O	O
	4,5,6,7,8,9,10	2	2,3,4,5,6,7,8,9,10	1		
3	1,2,3	8	1	16.6	O	O
	4,5,6,7,8,9,10	2	2,3,4,5,6,7,8,9,10	1		
4	1,2,3	8	1	18.6	O	O
	4,5,6,7,8,9,10	2	2,3,4,5,6,7,8,9,10	1		
5	1,2,3	8	1	21.2	O	O
	4,5,6,7,8,9,10	2	2,3,4,5,6,7,8,9,10	1		
6	1,2,3	8	1	25	O	O
	4,5,6,7,8,9,10	2	2,3,4,5,6,7,8,9,10	1		
7	1,2,3	8	1	30	O	O
	4,5,6,7,8,9,10	2	2,3,4,5,6,7,8,9,10	1		
8	1,2,3	8	1	37	O	O
	4,5,6,7,8,9,10	2	2,3,4,5,6,7,8,9,10	1		
9	1,2,3	8	1	44	O	O
	4,5,6,7,8,9,10	2	2,3,4,5,6,7,8,9,10	1		
10	1,2,3	8	1	60	O	O
	4,5,6,7,8,9,10	2	2,3,4,5,6,7,8,9,10	1		
11	1,2,3	8	1	80	O	O
	4,5,6,7,8,9,10	2	2,3,4,5,6,7,8,9,10	1		
12	1,2,3	8	1	120	O	O
	4,5,6,7,8,9,10	2	2,3,4,5,6,7,8,9,10	1		
13	1,2,3	8	1	200	O	O
	4,5,6,7,8,9,10	2	2,3,4,5,6,7,8,9,10	1		
14	1,2,3	8	1	340	O	O
	4,5,6,7,8,9,10	2	2,3,4,5,6,7,8,9,10	1		


(After decisions have been made in table 1)

In Table 2, you make also 14 choices between “option A” and “option B”.

- **Option A** is a lottery that pays you **8 Euros** if the roll of a fair ten-sided die is 1, 2, 3, 4, 5, 6, 7, 8, or 9 and it pays **6 Euros** if the roll of the die is 10. **Option A is identical in the 14 decisions.**



	1,2,3,4,5,6,7,8,9	You earn 8 Euros
	10	You earn 6 Euros

- **Option B** is a lottery that pays you either **1 Euro** if the roll of the die is 8, 9, or 10, and it pays **an amount increasing from 10,8 Euros to 26 Euros along the 14 decisions** if the roll of the die is 1, 2, 3, 4, 5, 6, or 7. For example, in decision 1, it pays 10.8 Euros; in decision 2, it pays 11.2 Euros; finally in decision 14, it pays 26 Euros.

	1,2,3,4,5,6,7	You earn x Euros (with x=10.8 Euros in decision 1 to 26 Euros in decision 14)
	8,9,10	You earn 1 Euro

Please decide if you prefer option A or option B for each of the 14 following decisions. As for Table 1, you simply need to identify the decision from which you prefer option B.

Table 2

Decision Number	Option A		Option B		You choose	
	 If the die indicates	You earn in Euros	 If the die indicates	You earn in Euros	Option A	Option B
1	1,2,3,4,5,6,7,8,9	8	1,2,3,4,5,6,7	10.8	O	O
	10	6	8,9,10	1		
2	1,2,3,4,5,6,7,8,9	8	1,2,3,4,5,6,7	11.2	O	O
	10	6	8,9,10	1		
3	1,2,3,4,5,6,7,8,9	8	1,2,3,4,5,6,7	11.6	O	O
	10	6	8,9,10	1		
4	1,2,3,4,5,6,7,8,9	8	1,2,3,4,5,6,7	12	O	O
	10	6	8,9,10	1		
5	1,2,3,4,5,6,7,8,9	8	1,2,3,4,5,6,7	12.4	O	O
	10	6	8,9,10	1		
6	1,2,3,4,5,6,7,8,9	8	1,2,3,4,5,6,7	13	O	O
	10	6	8,9,10	1		
7	1,2,3,4,5,6,7,8,9	8	1,2,3,4,5,6,7	13.6	O	O
	10	6	8,9,10	1		
8	1,2,3,4,5,6,7,8,9	8	1,2,3,4,5,6,7	14.4	O	O
	10	6	8,9,10	1		
9	1,2,3,4,5,6,7,8,9	8	1,2,3,4,5,6,7	15.4	O	O
	10	6	8,9,10	1		
10	1,2,3,4,5,6,7,8,9	8	1,2,3,4,5,6,7	16.6	O	O
	10	6	8,9,10	1		
11	1,2,3,4,5,6,7,8,9	8	1,2,3,4,5,6,7	18	O	O
	10	6	8,9,10	1		
12	1,2,3,4,5,6,7,8,9	8	1,2,3,4,5,6,7	20	O	O
	10	6	8,9,10	1		
13	1,2,3,4,5,6,7,8,9	8	1,2,3,4,5,6,7	22	O	O
	10	6	8,9,10	1		
14	1,2,3,4,5,6,7,8,9	8	1,2,3,4,5,6,7	26	O	O
	10	6	8,9,10,94	1		

Part 4d

- How do you see yourself: are you generally a person who is fully prepared to take risks or do you try to avoid taking risks?

Please give a value between 0 and 10, with 0 for “not at all willing to take risks” and 10 for “very willing to take risks”.









- How would you rate your willingness to take risks concerning financial matters?



Part 4e

Please choose your favorite option among the six options displayed in the table below.

In each option, a fair coin is tossed. Each option pays two possible payoffs depending on the outcome. The only exception is option 1 in which the payoff is always 7.

Option	If the coin indicates	You earn in Euros	Your choice
1	 Head Tail	7 7	O
2	 Head Tail	9 6	O
3	 Head Tail	11 5	O
4	 Head Tail	13 4	O
5	 Head Tail	15 3	O
6	 Head Tail	17.5 0.5	O

At the end of the experiment, if this part is drawn for actual payment, you will receive the amount stated for the option you have chosen, depending on the coin toss by the computer.

Part 5

Please answer the three following questions.

(1) A bat and a ball cost 1.10 Euro in total. The bat costs 1 Euro more than the ball.
How much does the ball cost?

_____ Euro-cents

(2) If it takes 5 machines 5 minutes to make 5 widgets, how long would it take 100 machines to make 100 widgets?

_____ minutes

(3) In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half of the lake?

_____ days

D2 Estimation of CRRA parameters

We provide details on how we estimate risk-aversion. We first describe the general methodology. Then, we discuss special cases.

D2.1 General methodology

For each decision, a parameter interval is associated with each decision. The upper (respectively lower) bound of the parameter interval associated with the decision is the parameter implying indifference between this decision and the choice with a risk level just below (respectively above). When the decision is the least risky (alternatively riskiest) of the risk-elicitation procedure, there is no choice with a risk level just below (respectively above). The interval is thus unbounded. To estimate a single parameter, we consider the centroids of each interval when the interval is bounded following Reynaud and Couture (2012). When the interval is unbounded, we take the closest integer value from the existing bound that is included in the interval.

D2.2 Special cases

For two measures of risk attitude (GP and HL), we apply corrections to some of the decisions to increase the quality of our estimations.

GP: Rounding correction

For GP, the number of decisions available is much higher than for the other incentivized preferences (801 compared to 10 for HL, 14 for each question of TCN and 6 for EG). Facing such a large decision set, participants may have used heuristics to reduce the decision set (Heiner, 1983; Simon, 1955). It is essential to identify such reductions of the decision set as estimated parameters rely not only on the chosen decision but also on the decisions with a risk level just above and just below in the decision set. When analyzing untreated decisions, we find that most subjects have not considered the whole decision space but have rounded their answers to the nearest 50 cents. Indeed, 93.2% of the subjects' answers are multiple of 0.5. In order to have the highest likelihood of estimating the risk-aversion accurately, we thus apply the previously described method by rounding possible decisions to the nearest 50 cents. Without this correction, estimated parameters lie between -69 and 2. It is difficult to impute such extreme levels

of risk-aversion to something else than a methodological problem. With the rounding correction, estimated parameters lie between -1 and 2. This interval is comparable to the parameter range of the other measures of risk attitude.

HL: multiple switching points

For HL, subjects' decisions are not constrained such that they should have a unique switching point. In our experiment, however, 23.1% of the subjects switch multiple times. For these subjects, we consider that their true switching point is their mean switching point. Not excluding these subjects enables us to have a comparable sample size for the different measures of risk attitude. We test the robustness of our results in Section D4.

D3 Data plots

We give the histograms of the measures of risk attitude and the laboratory financial decisions. We report histograms of decisions in risk-elicitation procedures before reporting histograms of the estimated parameter.

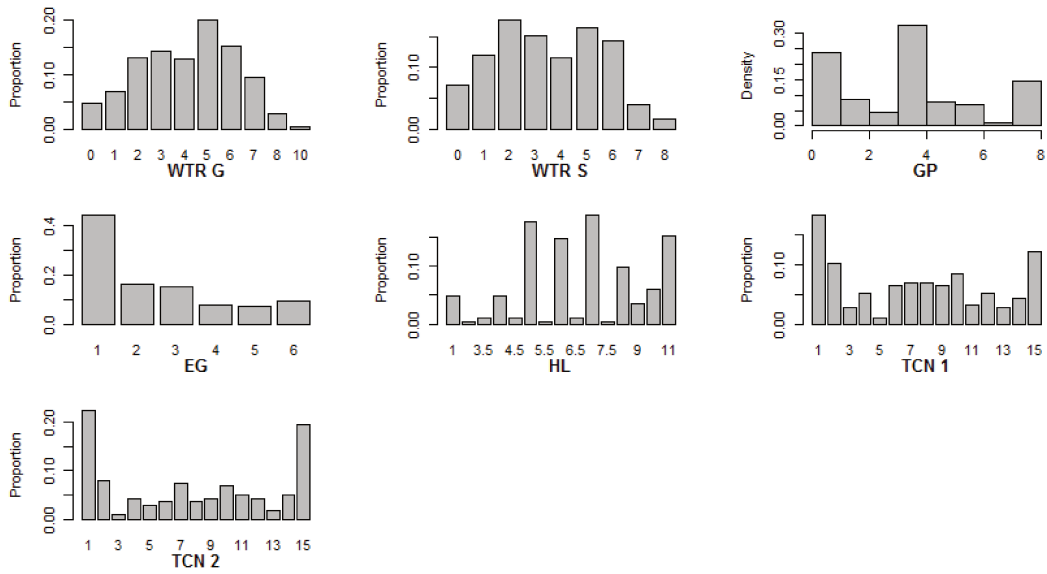


Figure D1: Measures of risk attitude - Observed decisions

Notes: These graphs plot the distribution of decisions for each measure of risk attitude. The decision is equal to the reported number for WTR G and WTR S, the lottery number for EG and the invested amount for GP. For TCN, the decision is equal to the number of the row at which they switch from option A to option B (it is equal to 15 if the subjects have always chosen option B). For HL, the decision is equal to the mean switching point. For all measures except GP, we consider that the decision is a categorical variable and bars present the proportion of each category. For GP, the decision is considered as continuous and each bar presents the density of observations within the corresponding range.

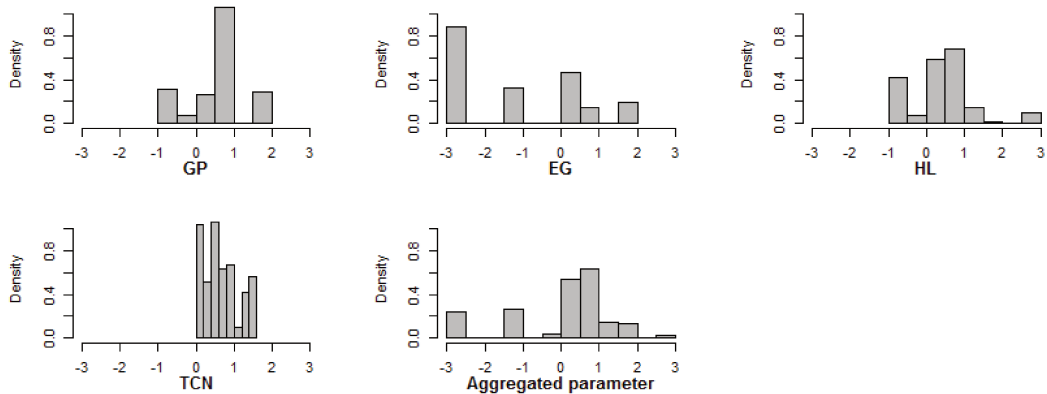


Figure D2: Measures of risk attitude - estimated risk-aversion parameter

Notes: These histograms plot the distribution of estimated risk-aversion parameters for each incentivized measure of risk attitude independently (GP, EG, HL and TCN) and jointly (aggregated parameter).

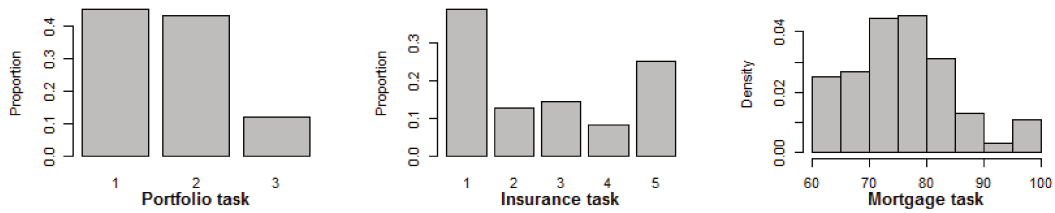


Figure D3: Laboratory financial decisions

Notes: These graphs plot the distribution of decisions for each laboratory financial decision. The decision is equal to the chosen option for the portfolio task and the insurance task. Bars present the proportion of each category. The decision is equal to the expected value of the decision in the mortgage task. Bars present the density of observations within each range.

D4 Robustness

D4.1 Probability sensitivity in TCN

In the core of the paper, we analyze how decisions in TCN explain other behavior under risk using only the utility curvature parameter. However, this method is richer and allows us to measure the sensitivity to probabilities. This sensitivity is associated, in the gain domain, with risk-seeking for low-probabilities and risk-aversion for high-probabilities (Tversky and Kahneman, 1992). This distortion decreases with the value of α .

In this appendix, we also consider the probability-sensitivity parameter to test the robustness of our previous findings. We first assess whether the probability sensitivity parameter is related to behavior under risk (independently of the utility curvature). Then, we assess whether both parameters can jointly explain this behavior using a predictive approach based on cumulative prospect theory. Neither of the two methods identifies a significant relationship between decisions in TCN and behavior under risk. We conclude that the independence between these two types of behavior is not due to the lack of consideration of the probability sensitivity.

We thus follow the same methodology than the one used in Section 5.2 to test the probability sensitivity against behavior under risk. Results are reported in Table D1. We find that the probability sensitivity is not related to behavior in laboratory financial decisions or to field behavior.

The theory predicts that the decision is driven by both the utility-curvature and the probability-sensitivity parameters. In order to assess jointly these two parameters, we

	<i>Dependent variable: Laboratory financial decisions</i>			<i>Dependent variable: Field measures</i>					
	Mor. (1)	Ins. (2)	Por. (3)	Sav. (4)	Ris. Inv. (5)	Real Est. (6)	Ins. (7)	Ded. (8)	Self-emp. (9)
Prob. sensitivity	-0.533 (0.489)	-0.583 (0.455)	-1.831 (2.320)	-13.892 (12.415)	-4.498 (5.175)	-3.112 (2.327)	-0.647 (0.828)	0.632 (0.693)	0.500 (1.217)
Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	200	200	200	91	96	142	142	165	200
R ²	-	-	0.006	0.144	0.046	-	-	-	-
Log Likelihood	-194.115	-276.000	-	-	-	-15.104	-69.470	-76.990	-27.439

Table D1: Laboratory financial decisions and field measures explained by the probability sensitivity estimated with TCN

*Notes: Ordered logistic regressions: (1) and (2). OLS regressions: (3) to (5). Logistic regressions (6) to (9). “Mor.”: mortgage tasks, “Ins.”: insurance task, “Por.”: portfolio task, “Sav.”: savings, “Ris. Inv.”: risky investments, “Real. est.”: real estate, “Ins.”: insurance, “Ded.”: deductibles, and “Self-emp.”: self-employed. Controls are “age”, “male”, “income”. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.*

use a predictive approach.

We first compute the value of the other tasks’ different options based on the parameters estimated in TCN. This value is computed following cumulative prospect theory, consistently with the approach of Tanaka et al. (2010). Formally, for the lottery $P = (x_1, p_1; \dots; x_n, p_n)$ such that outcomes are ranked, i.e., $x_1 \leq x_2 \leq \dots \leq x_n$:

$$V(P) = \sum_{i=1}^n h_i(p_i) \times U(x_i)$$

with:

$$h_i(p) = \pi\left(\sum_{j=1}^i p_j\right) - \pi\left(\sum_{j=1}^{i-1} p_j\right)$$

Second, we order all options from the one with the highest value (rank of 1) to the one with the lower value (maximal rank). Finally, we compute the rank of the chosen option. The quality of the prediction is decreasing with the rank of the chosen option.

We apply this method to the insurance task and the portfolio task. Indeed, it can be applied only to lottery decisions; so, neither the mortgage task nor, obviously, field behavior can be written under a lottery form.

There are five options in the insurance task and 5151 options in the portfolio task (maximum number of non-zero elements in a triangular 101×101 matrix). To estimate the quality of the prediction, we report two performance indices. First, we report the

average number of options with a higher value than the chosen option (rank of the chosen option minus one). Second, we report the proportion of decisions precisely predicted. For the insurance task, we consider that the value is precisely predicted if the observed decision has the highest value. For the portfolio task, we allow a less conservative definition of precisely predicted decisions since the number of possible decisions is much larger (5151 in portfolio task against five in the insurance task). We thus consider that a decision is precisely predicted if the chosen option is in the top 5% of the options with the higher value.

For the insurance task, we find that, on average, 2.4 decisions are better ranked than the chosen option. In addition, 20.94% of the decisions are correctly predicted. For the portfolio task, we find that, on average, 2657 options have a higher value than the chosen option. 6.51% of all chosen options are ranked in the top 5% of the options with the highest value. We use binomial tests to evaluate if the proportions of precisely predicted decisions are higher than random guesses (5% for the portfolio task and 20% for the insurance task). We find that TCN does not perform better than random guesses ($p=0.733$ for the insurance task and $p=0.274$ for the portfolio task).

The results reported in Section 5.2 are thus robust to the analysis of the probability sensitivity parameter. We conclude that behavior in TCN is not related to behavior in the laboratory financial decisions or to field behavior.

D4.2 Independent regressions of measures of risk attitude on field measures

We regress the risk parameter obtained in each of the measures of risk attitude on field behavior. We do not find any statistically-significant relation at a 10% level. The lack of explanatory power of measures of risk attitude is thus confirmed when regressing measures independently on field behavior.

D4.3 HL: Exclusion of subjects with multiple switching points

Previous analyses include subjects with multiple switching points by considering their mean switching point. To evaluate if this methodology influences our results, we also analyze the data after having excluded subjects with multiple switching points. We report regressions in Table D3. Regressions (1) to (3) reproduce regressions of Table 5.8 concerning the influence of the measures of risk attitude on the laboratory financial

	HL (r)	GP (r)	TCN (r)	EG (r)	WTR G	WTR S
<i>Dependent variable: Savings</i>						
Risk attitude	-16.336 (11.816)	2.258 (4.523)	-3.850 (8.289)	-1.430 (1.614)	4.757 (6.525)	-3.040 (6.705)
<i>Dependent variable: Risky investments</i>						
Risk attitude	-3.060 (2.127)	-3.624 (2.538)	0.482 (3.317)	0.074 (0.997)	0.072 (0.798)	0.668 (0.780)
<i>Dependent variable: Real estate</i>						
Risk attitude	-0.097 (0.669)	0.303 (0.419)	0.227 (1.110)	-0.247 (0.189)	0.063 (0.220)	0.126 (0.219)
<i>Dependent variable: Insurance</i>						
Risk attitude	-0.073 (0.352)	-0.223 (0.283)	-0.679 (0.510)	-0.102 (0.115)	-0.030 (0.134)	-0.054 (0.133)
<i>Dependent variable: Deductible</i>						
Risk attitude	-0.031 (0.200)	0.100 (0.232)	0.198 (0.255)	0.104 (0.096)	0.045 (0.099)	0.160 (0.101)
<i>Dependent variable: Self-employed</i>						
Risk attitude	0.140 (0.363)	-0.117 (0.370)	0.271 (0.981)	-0.495 (0.310)	0.174 (0.193)	0.033 (0.184)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Constant	Yes	Yes	Yes	Yes	Yes	Yes
Observations (savings)	90	86	91	104	117	117
R ² (savings)	0.507	0.174	0.134	0.278	0.136	0.134
Observations (risk inv.)	100	95	96	106	110	110
R ² (risk inv.)	0.112	0.102	0.038	0.021	0.079	0.086
Observations (real estate)	141	147	142	165	165	165
Akaike Inf. Crit. (real estate)	34.605	74.674	41.903	83.298	59.729	59.482
Observations (insurance)	141	147	142	165	165	165
Akaike Inf. Crit. (insurance)	80.492	131.887	147.716	165.312	121.010	120.895
Observations (deductible)	157	168	165	188	174	174
Akaike Inf. Crit. (deductible)	189.050	186.239	164.209	211.776	173.595	171.267
Observations (self-emp.)	198	201	200	231	234	234
Akaike Inf. Crit. (self-emp.)	82.667	90.484	64.967	64.590	78.108	78.912

Table D2: Field measures explained by measures of risk attitude

Notes: For each column, the independent variable “Risk attitude” comes from a different measure of risk attitude. (r) means that the “Risk attitude” is an estimated parameter. Otherwise, it is the decision itself. The other independent variables are age, male, income and an intercept. The dependent variables vary in each block of the Table. When the dependent variable is continuous, we use OLS regressions (savings and investments). Otherwise, we use logistic regressions. The number of observations, R² for OLS regressions and Akaike Information criterion for logistic regressions are displayed in the last block of the Table and referred to by the name of the corresponding dependent variable. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

decisions. We find that with or without subjects switching multiple times, HL explains decisions in the insurance task and the portfolio task but not in the mortgage task. Regressions (4) to (9) reproduce regressions of Table D2 concerning the influence of the measures of risk attitude on field behavior. We also find that excluding multiple switchers does not change our results since there is no statistically-significant relationship between HL and any of the field measures.

We conclude that our results are robust to the exclusion of subjects with multiple switching points.

	<i>Dependent variable: Lab fin. decisions</i>			<i>Dependent variable: Field measures</i>					
	Mor. (1)	Ins. (2)	Por. (3)	Sav. (4)	Ris. Inv. (5)	Real Est. (6)	Ins. (7)	Ded. (8)	Self-emp. (9)
HL parameter	0.080 (0.152)	0.344** (0.163)	1.514* (0.774)	-0.006 (0.017)	-2.540 (2.069)	-0.010 (0.629)	-0.101 (0.345)	-0.020 (0.200)	0.268 (0.336)
Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	153	153	153	109	88	109	109	120	153
R ²	-	-	0.054	0.226	0.121	-	-	-	-
Log Likelihood	-147.770	-202.561	-	-	-	-11.771	-32.168	-73.633	-31.002
Mul. Swi.	No	No	No	No	No	No	No	No	No

Table D3: Laboratory financial decisions and field measures explained by HL - Subjects switching multiple times are excluded.

*Notes: Ordered logistic regressions: (1) and (2). OLS regressions: (3) to (5). Logistic regressions (6) to (9). "Mor.": mortgage tasks, "Ins.": insurance task, "Por.": portfolio task, "Sav.": savings, "Ris. Inv.": risky investments, "Real. est.": real estate, "Ins.": insurance, "Ded.": deductibles, and "Self-emp.": self-employed. Controls are "age", "male", "income". *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.*