L'économie comportementale pour une meilleure conception des politiques : investigations expérimentales

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Présentée par **Sophie Clot**

BEHAVIOURAL ECONOMICS FOR BETTER POLICY DESIGN
EXPERIMENTAL INVESTIGATIONS

L'ECONOMIE COMPORTEMENTALE POUR UNE MEILLEURE CONCEPTION DES POLITIQUES
INVESTIGATIONS EXPERIMENTALES

Soutenue le 18 décembre 2014 devant le jury composé de

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Abstract

Behavioural economics for better policy design: Experimental investigations

The PhD dissertation explores some behavioural biases for improved policy design and contributes to a better understanding of the moral self-licensing phenomenon through the effect of potential mediators (voluntary vs. mandatory policies, interaction with financial incentives), the extent to which licensing might lead to behavioural change (from less cooperative behaviour to moral transgression), and the robustness of results (from classroom experiments to field experiments in developing countries). The PhD dissertation also includes laboratory experiments and the study of intertwined biases through complementary researches (mental accounting, hyperbolic discounting, risk aversion) for an increased openness and understanding of the behavioural approach as well as potential interactions among biases.

Keywords Behavioural economics; experimental economics; public policies; environmental policies; moral self-licensing

Résumé

L'économie comportementale pour une meilleure conception des politiques : Investigations expérimentales

Cette thèse de doctorat vise à explorer certains biais comportementaux pouvant conduire à une meilleure conception des politiques. La thèse contribue notamment à améliorer notre compréhension du phénomène de compensation morale ('Moral self-licensing effect') par l'étude de médiateurs potentiels (politiques volontaires vs. obligatoires, interaction avec les incitations financières), la mesure dans laquelle la compensation morale pourrait conduire à une altération des comportements (allant des comportements moins coopératifs jusqu'à la transgression de la morale), et la robustesse des résultats (via la réalisation d'expériences conventionnelles auprès d'étudiants mais aussi d'expériences de terrain dans des pays en voie de développement). Cette thèse inclut également des expériences en laboratoire ainsi que l'étude d'autres biais étroitement imbriqués et complémentaires ('mental accounting'[a], 'hyperbolic discounting'[b], aversion au risque) pour une plus grande ouverture et une meilleure compréhension de l'approche comportementale ainsi que la prise en compte des potentielles interactions entre biais.

[a] 'comptabilité mentale', [b] 'actualisation hyperbolique'

Mots Clés Economies comportementale; économie expérimentale; politique publique; politique environnementale; compensation morale

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'Most economists recognize that some of the people are not fully rational some of the time and some of the time that matters.'

Richard Thaler

Introduction Générale

Des paradoxes empiriques remettent en cause le cadre de l'économie traditionnelle pour concevoir les politiques publiques. 'Des baisses d'impôts qui ne parviennent pas à stimuler l'économie; des incitations financières qui ne parviennent pas à augmenter les taux de participation...' sont quelques-uns des résultats contre-intuitifs illustrant la nécessité d'adapter ou d'affiner les approches conventionnelles.

Ces effets à priori imprévisibles soulignent certaines limites de l'hypothèse liée à '*l'homo economicus*' présente dans les modèles théoriques néo-classiques: un agent rationnel, égoïste, maximisateur d'utilité, avec des capacités cognitives illimitées, une volonté parfaite et aucune considération pour autrui. Le grand avantage de ce portrait, représentant un '*homo economicus*' largement utilisé en économie, est son universalité, sa traçabilité et sa prévisibilité. Dans ce cas, pourquoi les politiques ne parviennent-elles pas forcément à atteindre leurs objectifs? Pourquoi les individus ne réagissent pas toujours comme prévu théoriquement? Comprendre les raisons qui sous-tendent les comportements individuels semble être essentiel pour une meilleure conception des politiques publiques.

L'économie comportementale offre une approche originale, se fondant sur la psychologie cognitive, relâchant les hypothèses traditionnelles en raison de la 'rationalité limitée', et acceptant les limites des capacités intellectuelles, de la volonté, de la mémoire, etc.

Les humains font des 'erreurs', et certaines d'entre elles sont prévisibles. Ces 'erreurs' prévisibles du comportement humain sont appelées 'biais comportementaux'.

Les individus ne se comportent pas toujours comme prévu par la théorie du choix rationnel. L'économie comportementale offre une voie prometteuse pour l'interprétation de ces écarts. L'objectif principal de cette thèse est d'explorer certains de ces biais comportementaux qui peuvent interférer avec les politiques, proposant une approche nouvelle, mais néanmoins complémentaire, visant à anticiper les effets néfastes qui pourraient éventuellement nuire à l'atteinte d'objectifs socialement désirables.

Une volonté limitée

Dans notre vie quotidienne, nous pourrions nous surprendre à agir de manière 'non rationnelle'. Nous achetons une glace alors que nous souhaitons perdre du poids (mais nous avons fait beaucoup de sport aujourd'hui), nous avons accepté une invitation à une soirée alors que nous avions prévu de travailler dur le lendemain matin (mais nous n'avons pas fait de pause-café cet après-midi), nous achetons des chaussures de marque alors que nous avions prévu d'économiser pour acheter une nouvelle voiture (mais nous venons de recevoir un remboursement des impôts), nous prenons notre voiture pour aller chez notre voisin alors que nous prétendons être 'écolo' (mais nous donnons 2.5€ à Greenpeace chaque mois), et parfois, nous oublions de poinçonner notre ticket de bus (mais nous le faisons correctement habituellement...), etc.

In fine, nous savons ce qui *devrait* être fait, mais n'y parvenons pas toujours.

L'un des principaux apports de la théorie des 'prospect'[1] (Kahneman et Tversky, 1979) a été de montrer empiriquement l'endogénéité des préférences. Cependant, la relation entre les actions passées et les décisions ultérieures reste largement inexplorée. En effet, des recherches récentes soulèvent des preuves contradictoires menant au paradoxe d'incohérence temporelle où les

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[1] *Prospect theory ou 'théorie des perspectives'*

décisions au fil du temps relèveraient plus de la substitution que de la complémentarité (cohérence). Cette tendance des individus à réagir face à un règlement, un contrat ou une norme en adoptant des comportements compensatoires (ou 'adaptation négative') pourrait potentiellement annuler tout ou partie des avantages de la mesure adoptée. Si cet effet de *'compensation morale[2]'* n'était pas pris en compte, il pourrait directement réduire, voir annuler, l'efficacité des politiques publiques.

Cadre de réflexion

Les externalités négatives restent un problème majeur des politiques publiques au sein desquelles le comportement individuel est considéré comme central. Depuis les instruments de marché jusqu'aux approches volontaires, les outils politiques ciblant les changements comportementaux ont connu un grand essor, malgré des résultats encore incertains. Dans ce contexte, l'économie comportementale semble offrir une voie prometteuse pour traiter des questions de politique environnementale.

Le Chapitre I aborde l'étude des comportements pro-environnementaux et pro-sociaux dans une optique d'approche volontaire. Des travaux récents sur l'effet de compensation morale offrent de nouvelles perspectives pour l'étude des mécanismes fondateurs des comportements pro sociaux. Ces travaux (Khan et Dhar 2006; Sachdeva et al, 2009;. Branas-Garza et al, 2013) soutiennent qu'un acte vertueux initial pourrait influencer les décisions futures, en diminuant la volonté d'agir vertueusement par la suite et vice versa (Le choix d'une entrée saine nous déculpabiliserait d'opter pour un dessert calorique, l'engagement volontaire dans des programmes de réduction des émissions de carbone nous libèrerait de l'utilisation de moyens de transports polluants, les dons aux associations caritatives nous déculpabiliseraient de l'achat d'articles superflus en période de fêtes, etc).

En conséquence, des politiques prônant de bonnes conduites pourraient mener à des effets globaux contre-productifs en légitimant l'adoption de comportements peu vertueux par la suite. Le Chapitre I

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[2] Traduit de l'anglais 'moral self-licensing effect'

étudie ce modèle d'autorégulation, via la réalisation d'un ensemble de quatre expériences. Dans une première sous-section, l'objectif est d'étudier la manière dont les politiques incitatives aggravent ou diminuent l'effet de compensation morale. Une première expérience (section I.1) met l'accent sur l'effet des politiques obligatoires, et la seconde s'intéresse aux incitations financières (section I.2). La section II est une tentative d'évaluation des retombées négatives potentiellement induites par l'effet de compensation morale; allant des actions non éthiques (section II.1)) aux actions transgressives (section II.2).

Dans le Chapitre II, nous élargissons les réflexions du Chapitre I à un contexte plus global impliquant davantage l'économie comportementale, afin de nous rapprocher de conditions réelles et d'augmenter la validité de ce travail pour les décideurs politiques, en utilisant des expériences de terrain. Le Chapitre II remplit deux objectifs. Au sein d'une première section, nous proposons d'étudier les retombées non anticipées liées aux contrats d'incitation ('*compensation morale*' et '*comptabilité mentale[3]*'). Dans une seconde section, nous nous interrogeons sur les biais comportementaux qui pourraient interférer en amont, soit lors de l'adoption de contrats d'incitation via la réalisation sur le terrain d'expériences visant à mesurer divers éléments du comportement individuel (altruisme, aversion au risque, confiance, patience, réciprocité).

Le Chapitre III a pour objectif de répondre à certaines questions méthodologiques soulevées dans les chapitres I et II. Tout d'abord, nous testons la validité du système des incitations aléatoires, sachant que trois sur quatre des expériences du Chapitre I y ont recours. Le système des incitations aléatoires bénéficie d'une popularité croissante au sein de l'économie expérimentale. Avec ce système, tous les sujets participent à une expérience, mais seulement une partie d'entre eux est rémunérée. Ce système offre une solution potentiellement intéressante pour tester des idées nouvelles devant mobiliser des échantillons de taille importante sans les contraintes logistiques liées aux paiements individuels et pouvant également permettre de réduire significativement le coût global. Cependant, cette méthode soulève de fortes critiques parmi les scientifiques qui lui reprochent de ne pas offrir

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[3] Traduit de l'anglais 'mental accounting'

une incitation suffisante. Dans la première section du Chapitre III, nous avons mené une expérience de laboratoire pour confronter les résultats obtenus par le système des incitations aléatoires à ceux obtenus via la méthode des paiements individuels conventionnelle.

Par ailleurs, dans le Chapitre II, nous avons conçu une version «portable» de la méthode du 'Convex Time Budget'[4] de Andreoni et Sprenger (2012), afin d'être à même de mesurer les préférences temporelles dans un contexte particulier de terrain. Cette version simplifiée n'a jamais été utilisée auparavant et des doutes pourraient surgir quant à sa fiabilité. Une évaluation plus poussée de cette méthode constitue donc le deuxième objectif du Chapitre III. Dans cette deuxième section du Chapitre III, nous comparons nos données de terrain à celles d'Andreoni et Sprenger (2012) afin de vérifier l'adéquation de cette version simplifiée.

La thèse est organisée comme décrit dans la figure 1. Le Chapitre I étudie l'effet de compensation morale; le Chapitre II élargit les apports du Chapitre I et confère une validité externe accrue par l'étude de l'interférence des comportements avec les politiques incitatives en situation réelle. Dans le Chapitre III, nous abordons des questions d'ordre méthodologique, soulevées dans les chapitres I et II.

Méthode

L'économie expérimentale offre un important sous-ensemble de solutions pour l'étude des questions de comportements mentionnées plus haut. Nous avons utilisé trois différents types d'expériences, en fonction de la problématique étudiée.

Le caractère exploratoire du Chapitre I nous a encouragé à réaliser des expériences de type 'papier-crayon'. Ce type d'expérience offre l'avantage de recueillir un grand nombre d'information en un laps de temps limité. La nature politique des questions abordées dans le Chapitre II, nous a conduit à réaliser des expériences sur le terrain, garantes de validité externe et de pertinence accrue pour les

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[4] Ou 'Budget temps convexe' traduit de l'anglais 'Convex Time Budget'

décideurs. Dans le Chapitre III, nous avons notamment utilisé des expériences de laboratoire, l'objectif étant de répondre à des questions méthodologiques, avec comme priorité la validité interne des résultats.

Figure 1 - Schémas de réflexion

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General Introduction

Empirical paradoxes challenge traditional economics framework for policy design. '*Tax rebate failing to boost the economy; financial incentives failing to increase participation …*' are some of the counter intuitive policy outcomes illustrating the need to adapt or refine conventional research approach.

Apparently unpredictable outcomes underline some limits of the '*Homo Economicus'* hypothesis linked to traditional theoretical models: a rational, self-interested, utility-maximizing agent, with unlimited cognitive abilities, perfect willpower and no concern for other people. The great advantage of this portrait, depicting an *Economic man* widely used in economics, is notably its universal applicability, tractability and predictability.

So, why policies often fail achieving their goal? Why sometimes individuals do not react as theoretically predicted? Understanding the reasons underpinning individual behaviour appears to be essential for better policy design.

Behavioural economics has put to the fore an original approach, relying on cognitive-psychology research, which relaxes traditional assumptions because of '*bounded rationality'*, and accepts limited cognitive ability, limited willpower, limited memory, etc.

Humans make '*errors*', and some of them are predictable. Those predictable '*errors'* in human behaviour are called '*behavioural biases*'.

Individuals do not always behave as predicted by rational choice theory and behavioural economics offers a great premise to study given deviations. The main objective of this dissertation is to explore

some behavioural biases that may interfere with policies and instruments, as a fresh but complementary approach, to anticipate detrimental effects that could possibly harness policies' power to reach socially desirable goals.

On bounded willpower

In our everyday life, we might surprise ourselves doing things that are not optimal for us. We eat ice-cream while we planed to loose weight (but we exercised a lot today), we party this evening while we planned working hard tomorrow morning (but we skipped coffee break this afternoon), we buy luxury shoes while we planned to save cash for a new car (but we received money back from a tax rebate), we take our car to drive next door while we claim to be a '*green*' person (but we give 2.5$ to Greenpeace each month), and sometimes, we forget to punch our bus ticket (but we usually do it right…) etc.

In fine, we know what is good, but we sometimes fail doing it.

One of the key contributions from prospect theory (Kahneman and Tversky, 1979) has been to empirically prove that preferences are endogenous. However, the relation between passed actions and subsequent decisions remains largely unexplored. Indeed, emerging researches and daily facts raise conflicting evidence leading to the paradox of temporal inconsistency where decisions over time reveal more substitutions (i.e. compensation) than complementarities (i.e. consistency). This hypothesized tendency of people to react to a regulation, a contract or a norm by increasing other compensatory (or 'negative adaptive') behaviour, potentially offsetting some or all of the benefits of the measure, has been called 'moral licensing'. If not taken into account, moral licensing could directly mitigate policies or contract's efficiency.

Reflexion framework

Environmental negative externalities remain a major public policy concern where individual behaviour is given as central. From market-based instruments to voluntary approaches, policy tools targeting behavioural change have been flourishing in the past decades, despite uncertain outcomes. In such context, behavioural economics seems to offer a promising way to deal with environmental policy issues. Most –but not all- of the dissertation will dedicate a special attention to environmental policies.

Chapter I starts by studying pro-environmental and pro-social behaviour in a voluntary approach perspective. Emerging work on moral licensing offers novel prospects to study moral behaviour's underpinning mechanism. Recent researches (Khan and Dhar 2006; Sachdeva et al., 2009; Branas-Garza et al., 2013) support that an initial virtuous act might impact future decisions, by decreasing the willingness to act virtuously in subsequent circumstances and vice versa (i.e., healthy starters free us to opt for big deserts; voluntary commitment to carbon reduction program releases the use of polluting transportation; donations to charity remove the guilt from buying unnecessary items at Christmas time, etc). As a result, well-intentioned policies can lead to overall counter-productive effects by licensing people to misbehave later on. Chapter I explores this self-regulation pattern, running a set of four 'paper and pencil' experiments. In a first sub-section, the aim is to investigate how incentive-based policies worsen or lessen moral licensing. A first experiment (Section I.1) focuses on the effect of mandatory enforcement of policies, and the second one on financial incentives to adopt policies (Section I.2). Chapter I Subsection II is an attempt to evaluate potential negative spillovers induced by moral licensing; either being unethical (Section II.1)) or being transgressive (Section II.2).

In Chapter II, we extend findings from Chapter I to a more general context involving additional insights from behavioural economics, in the perspective to get closer to real life settings and increase the accuracy of this work for policy makers, using field experiments. Chapter II is twofold. First, we

propose to study unintended spillovers due to Incentive contract's behavioural response (i.e., 'Moral licensing' and 'Mental accounting') in a natural field experiment, established on a real life situation. In a second part, we question behavioural biases that could interfere with incentive contract's compliance (i.e., 'Present bias'). For this second field study, we set up an 'Artefactual field experiment' composed of various games (trust game, time preference game, risk game, dictator game and triple dictator game).

Chapter III aims at addressing some methodological issues raised in both Chapter I and II. First, we try to test the validity of the 'Between subject Random Incentivized System' as three out of four of the experiments of Chapter I rely on this method. The Between subject Random Incentivized System (BRIS) is getting more and more popular among experimentalists. With this system, all subjects play one game, but only a percentage of them get paid. It offers a potential solution to test ideas among large sample of subjects without logistical constraints due to individual payment and can also reduce the overall cost. However, this method raises strong criticisms among scientists. In the first section of Chapter III, we ran a laboratory experiment to test whether results get under the Between subject Random Incentivized System are consistent with results under conventional individual payments. Second, in Chapter II, we ran a 'portable' version of Andreoni and Sprenger's (2012) Convex Time Budget method, in order to measure time preference in a particular context. This simplified version has never been used in field settings and doubts about its reliability call for further assessment, which is Chapter III section II motive. In this second section of Chapter III, we compare our field data to Andreoni and Sprenger (2012)'s data, checking for this simplified version adequacy.

The rest of the dissertation is organised as described in Figure 1. Chapter I explores the moral self licensing effect ; Chapter II enlarges the work and brings external validity by studying behavioural interference with incentive based policies in real life settings. In Chapter III, we address methodological issues raised in Chapter I & II.

Method

Experimental economics offer an important subset of solutions to address the behavioural issues mentioned earlier. We used three different kinds of methods, depending on the problematic.

The exploratory features of *Chapter I* encouraged us to run paper & pencil experiments. This type of experiment offers the advantage to collect a lot of information in a limited time period. *Chapter II* addresses policy issues, so we run field experiments, warrant of both increased external validity and relevance for policy makers. In Chapter III, we used laboratory experiments since the objective was to rigorously address a methodological issue, setting internal validity as a priority.

Figure 1 – Reflexion scheme

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Chapter I – On Bounded Willpower : Moral Licensing effect

Introduction

A growing literature on moral licensing opens new directions to understand the forces underlying individuals' moral behaviour. Khan and Dhar (2006) defined moral licensing effect as a « non conscious effect that operates by providing a moral boost in the self-concept, which increases the preference for a relative immoral action subsequently by dampening the negative self-attributions associated with such behaviour ». In other words, committing to a virtuous act in a preceding choice may lead to behave less virtuously in the succeeding decision. Consequently, well-intentioned policies can lead to overall counter-productive effects by licensing people to behave badly in related behaviours.

Traditionally, moral behaviour and social preferences have been considered as exogenous and consistent over time (Narloch et al, 2012; Carpenter and Seki, 2010; Castillo and Saysel, 2005; Henrich, 2000). But recent researches support that an initial virtuous act might impact future decisions, by decreasing the willingness to act pro-socially in subsequent circumstances and vice versa (Khan and Dhar, 2006; Sachdeva et al., 2009; Mazar and Zhong, 2010; Chiou and al., 2011). Moreover, anecdotal evidence shows that Prius Hybrid drivers are more likely to break crosswalk laws and get fines, green buyers are more likely to steal, people who wrote positive stories about themselves are more likely to act selfishly, etc. (Woodyard, 2009; Mazar and Zhong, 2010, Sachdeva et al., 2009).

Several empirical studies found support for moral licensing effect. Khan, Dhar and Schmidt (2010) explained that voluntary commitment to carbon reduction could promote the use of polluting transportation. In fact, airlines' programs offering customers the option of buying carbon offsets to counter the planet-warming emissions generated by their airplane flights have been effective in easing customer's guilt at using airline transportation, questioning the overall impact of the program.

Tiefenbeck et al. (2013) showed that targeted environmental campaigns on reducing water consumption, increased at the same time the electricity consumption compared to a control group. In the domain of health care, Chiou et al. (2011) found that smokers who take a dietary supplement smoke more cigarettes than others. Performing an economic experiment in which subjects played a sequence of giving decisions Branas-Garza et al. (2011) show that a deviation from a 'normal state of being' is balanced with a subsequent action that compensates for prior behaviour. Participants' donations in each round were negatively correlated to the amount they donated in the previous period. The authors therefore interpret donations over time as the result of a pattern of self-regulation: moral licensing (being selfish after being generous) and cleansing (being generous after being selfish).

This emerging literature provides good support for the existence of a moral licensing bias. However, various policy relevant issues remain uncovered. The first chapter of my thesis aims to get better insight on this phenomenon under different policy perspectives. The analysis of moral licensing and its implications in policy-making is divided into two sections. A first section focuses on the role of policies on the occurrence of moral licensing. A second section concerns the outreach of moral licensing and consequences of soliciting virtuous behaviour on a more global level.

More specifically, Section I relates to moral licensing interactions with distinct political approaches for behavioural change (i.e., 1) mandatory enforcement, 2) financial incentives).

First, moral licensing literature has paid little attention on how the circumstances under which the good deed has been generated influences the effect. To address negative externalities such as environmental problems, policymakers have traditionally relied on command and control and market-based instruments (Hahn, 1989) and more recently on voluntary approaches (Alberini and Segerson, 2002; Thaler and Sunstein, 2008). These instruments share the common feature to target behavioural changes. Little is known on whether the way by which behavioural changes has been generated influences subsequent behaviours. Does an identical change generated either by

regulatory or by voluntary approaches influence in the same way the likelihood of self-licensing in future behaviours? This is the question we propose to address in the first section. We investigate experimentally the behavioural consequences of a similar change generated either by regulatory means or voluntary means. In the environmental realm people frequently face either an obligation to adopt some behaviours (e.g., speed limits to reduce pollution under the threat of financial penalties) or are simply encouraged to adopt others (e.g., sorting wastes). Second, we investigated the interactions between moral licensing and monetary incentives. Regulatory instruments such as financial (dis)incentives have been criticized, notably because of their cost and possible crowding-out of intrinsic motivation (Frey and Jegen, 2001; Bénabou and Tirole, 2006; Gneezy et al, 2011). Will a good deed performed either for free or in exchange for payment will impact subsequent pro-social behaviour in the same way? To test this statement, we run a between subjects experiment comprised of an adapted dictator game preceded by paid and unpaid pro-environmental tasks.

Section II aims at getting more insight on moral licensing scope. Section I tests if moral licensing reduces pro social behaviour under certain circumstances. In section II, we propose to analyze whether this self-regulation process is strong enough to uninhibit morally dubious behaviour, in a two-step framework: 1) Being unethical and 2) Being transgressive. We further test for moral licensing external validity by running experiments in a developing country context free of the Western, Educated, Industrialized, Rich, and Democratic (WEIRD) effect.

Section II first part tests to what extent moral self-licensing may encourage anti-social behaviour such as damaging a social surplus for private benefits. To capture an anti-social behaviour, we used an 'appropriation game' (Cox et al., 2013), which closely relates to a dictator game except that it manipulates the endowment's allocation, and mimics a social dilemma situation where private and public benefits diverge. In a second part, we use a between-subject design with a cheating game to compare the level of cheating between the control group (no previous deed) and the group who just imagined performing a good deed. Those two experiments differ from the previous ones (section I) in

at least two things: different experimental procedures are used (appropriation game and cheating game instead of dictator game) and samples have distinctive characteristics (Madagascar students vs. French students). In that sense, section II also enables to check whether the moral licensing effect remains robust to various experimental settings.

The remainder of the chapter is organized as follows. The next section introduces the related literature and discusses moral licensing interactions with different policy orientations. Section II deals with moral licensing scope and contextual validity.

Section I – Moral licensing in the perspective of behavioural change

Part I. Do Good Deeds Make Bad People? Moral licensing in regulatory vs. non-regulatory scenarios[5]

I.I.1. Introduction

Recent behavioural research reveals that performing a good deed can license individuals to behave less virtuously in subsequent actions. Khan and Dhar (2006) defined this moral licensing effect as a *« non conscious effect that operates by providing a moral boost in the self-concept, which increases the preference for a relative immoral action subsequently by dampening the negative self-attributions associated with such behaviour ».* A study about the impact of companies that promote hybrid cars within their employees shows that hybrid drivers get more tickets and accidents than drivers of conventional cars (Woodyard, 2009). Several empirical studies found support for this self-licensing effect. Khan, Dhar and Schmidt (2010) explained that voluntary commitment to carbon reduction could promote the use of polluting transportation. In fact, airlines' programs offering customers the option of buying carbon offsets to counter the planet-warming emissions generated by their airplane flights have been effective in easing customer's guilt at using airline transportation, questioning the overall impact of the program. Tiefenbeck et al. (2013) showed that targeted environmental campaigns on reducing water consumption, increased at the same time the electricity consumption compared to a control group. In the domain of health care, Chiou et al. (2011) found that smokers who take a dietary supplement smoke more cigarettes than others.

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[5] Part I has been published as: Clot, S., Grolleau, G., Ibanez, L. (2014), Do Good Deeds Make Bad People? A Self-licensing experiment to revisit the norm enforcement debate, *European Journal of Law and Economics, Forthcoming.*

Nevertheless, this literature has paid little attention on whether the way the good deed has been generated influences the self-licensing effect. To address negative externalities such as environmental problems, policymakers have traditionally relied on command and control and market-based instruments (Hahn, 1989) and more recently on voluntary approaches (Alberini and Segerson, 2002; Thaler and Sunstein, 2008). These instruments share the common feature to target behavioural changes. Regulatory instruments such as financial (dis)incentives have been criticized, notably because of their cost and possible crowding-out of intrinsic motivation (Frey and Jegen, 2001; Bénabou and Tirole, 2006; Gneezy et al, 2011). Moreover, little is known on whether the way by which behavioural changes has been generated influences subsequent behaviours. Does an identical change generated either by regulatory or by voluntary approaches influence in the same way the likelihood of self-licensing in future behaviours? To fill this gap, we investigate experimentally the behavioural consequences of a similar change generated either by regulatory means or voluntary means. These issues fit well the raising field of 'behavioural law and economics' (Jolls et al., 1998; Jolls, 2007; Engel, 2013) and can contribute to the debate regarding the existence of substitutabilities or complementarities between law and morals. In the environmental realm people frequently face either an obligation to adopt some behaviours (e.g., speed limits to reduce pollution under the threat of financial penalties) or are simply encouraged to adopt others (e.g., sorting wastes).

To investigate whether a regulatory framework to incentivize individuals to adopt pro-environmental behaviour generate similar spillover effects in terms of licensing than a non-regulatory framework, we design a classroom experiment that aims to study the licensing effect in the environmental realm. As far as we know, the link between licensing and the way an environmental good deed is generated has not been studied. We also examine the licensing effect according to the students' major which can provide an indication of preexisting motivation regarding pro-environmental choices. We get several interesting results. Overall, the way the good deed is generated does not seem to influence the licensing effect. Nevertheless, we found that business- and environmental-orientated majors

react adversely to the regulatory framework. Further investigations lead us to explain this phenomenon by the intrinsic motivations of individuals. We show that environmental-orientated students exhibit higher intrinsic motivations regarding pro-environmental actions than business-orientated ones. This finding suggests that, for non-intrinsically (respectively intrinsically) motivated individuals the licensing effect is more likely to arise when the preceding 'virtuous' act is freely chosen (respectively regulatory).

The remainder of the paper is organized as follows. The next section overviews the related literature and motivates our research question. Section 3 exposes the empirical strategy. The results are presented and discussed in section 4. Section 5 provides some empirical evidence and further explanations for the interpretation of our results. And finally, section 6 highlights some policy implications and concludes.

I.I.2. Literature review

Individuals are not as selfish as traditional economics assumes but behave often pro-socially, contribute to charities and engage in pro-environmental behaviour even if this is costly for them. Contributing to public benefits allows individuals to feel better (i.e. warm-glow effect) and might be explained by different motivations such as conformity to social norms, altruism, fairness, or status seeking (Croson, 2007).

Modeling and understanding pro-social behaviour remain difficult. The model of moral behaviour developed by Bénabou and Tirole (2011) is a recent illustration of how theory works on addressing the challenge of moral behaviour complexity. Their model was designed to solve the temporal inconsistency observed in many empirical studies. More concretely, it includes a representation of pro social choices based on a strategic substitutability (good deed at "t" leading to a bad deed at "t + 1") whereas the idea of strategic complementarities prevailed in prior works. This theoretical

framework for moral behaviour enables to model behaviour that might be made of substitutable or complementary decisions over time (i.e. whether people license themselves to behave badly subsequently or keep consistent).

Nevertheless, most of the literature on licensing effect belongs to psychology and marketing[6]. Those works converge to show that a substitution might happen when moral capital has been earned, or said differently when an initial deed activates positively the self-concept. Khan and Dhar (2006) studied individuals' decision process in terms of luxury products consumption. They first found out in a pretest that luxury products are associated with guilt-feelings. Then, they showed how an initial situation referring to a charity act, could influence preferences for unnecessary or extravagant items in subsequent decisions. The results of their experience show that preference for luxury items was significantly higher in the case of a preceding charity action (license condition), than in the case where no prior charity action (control condition) had to be undertaken first (i.e. 57.4% selected a luxury item in the license condition vs. 27.7% in the control condition). Also, participants rated themselves significantly more positively on a 7 points scale within four attributes[7] (i.e. "I am compassionate", "I am sympathetic", "I am warm", and "I am helpful") in the licensing condition, meaning that an initial altruistic intent boosts the self-concept and may liberate people to choose more indulgent options (i.e. average of feelings was 5.76 in the license condition vs. 4.79 in the control condition). In a second experiment, they replicated a similar protocol but added an additional condition. The prior charity action was implemented as being a punishment ("punishment" condition) instead of a good deed ("volunteering" condition). In this experiment, presenting the good deed as a punishment dampened the licensing effect and participants were not more likely to select the luxury item compared to the license group (45% in the punishment condition vs. 40% in control, p=not significant), while the licensing effect still happened in the volunteering condition (62%

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[6] Not directly in line with our study but in the domain of law and economics, Cain, Loewenstein and Moore (2005) refer to moral licensing to explain the negative side effects of disclosure of conflict of interest as it may lead to an increase in biased information or advice.

[7] These items were utilized because they indicated a high degree of reliability in terms of coefficient alpha (Cronbach's α=0.84)

selected the luxury item vs. 40% in the control scenario, p<0.05). Interestingly, this supports that the licensing effect does not necessarily occur after a "good deed". Instead, the discriminating feature seems to be the way the good deed is generated. Sachdeva et al. (2009) showed that the compensation effect also exists in the reverse order, when a previous bad deed increases the likelihood of the magnitude of a subsequent good deed. They called it 'moral cleansing'. Also, when a high level of moral capital is 'credited', it can even lead to dishonesty and encourage clear moral transgressions (Jordan et al., 2011).

Mazar and Zhong (2010) examined the moral licensing effect in the environmental realm. The authors addressed two main questions: 1) the impact of exposure vs. purchase on moral licensing effect and 2) how far the regulation process may lead people to behave unethically. First, the results show that participants who were merely exposed to the green store shared more money in the dictator game than those who were merely exposed to the conventional store (average amount shared was $2.18 over $6 in the green store exposure condition vs. $1.59 in the conventional store exposure condition), whereas participants who had purchased in the green store shared less money than those who purchased in the conventional store (average amount shared was $1.76 over $6 in the green store purchase condition vs. $2.12 in the conventional store purchase condition). Second, the results also support that participants who chose to buy products from the green array were more likely to purposefully behave dishonestly such as cheating and stealing in a subsequent task. Mazar and Zong (2010) concluded that green products can establish enough moral capital to encourage clear transgressions such as lying and stealing[8].

At the same time, recent models of moral decision-making suggest that because of the complex interferences with moral motivations, performances of policy instruments depend on how those

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[8] However, subjects were constrained to buy green products because of their prior random assignment to either the conventional or the green store.

instruments are perceived by regulated entities (Nyborg and al, 2006). Indeed, rewarding or imposing constraints on individuals can push them to adopt behaviours that will not be adopted otherwise. In plausible circumstances, demonstration of authority such as rules and laws could build norms, by suggesting that an event is important enough to justify a costly intervention (Nyborg, 1999). Nevertheless, if intrinsic motivations preexist, introducing additional external incentives (e.g. authoritarian decision, monetary rewards) to reinforce the intrinsically motivated behaviour can backfire (Frey and Oberholzer-Gee, 1997; Frey and Jegen, 2001; Bowles, 2008; Gneezy et al., 2011). A growing literature argues that external interventions crowd out intrinsic motivation (Bénabou and Tirole, 2006) and some supportive empirical evidence has been accumulated in various domains (e.g., Gneezy and Rustichini, 2000; Vollan, 2008; Bowles, 2008).

Interestingly, the literature on crowding-out frequently assumes homogeneous agents whereas anecdotal evidence supports that individuals are heterogeneous in terms of intrinsic motivations. Consequently, the observed overall effect (e.g. a reduction of the desirable behaviour after the introduction of a monetary reward) can occult underlying forces where some individuals increased the encouraged behaviour while others decreased it (Beretti et al., 2011). Strengthening this assumption, some experimental works have provided evidence that the heterogeneity of motivation crowding effects depends on individuals' intrinsic motivation (Charness and Gneezy 2009, Adda 2011). In their survey, Charness and Gneezy (2009) found that monetary incentives have a different impact on gym attendance depending on individuals' motivation level before the introduction of the reward: the crowding in effect of incentives prevails for individuals with low intrinsic motivation (i.e. measured by the level of gym attendance before the incentive was introduced), while the effect is reversed for highly motivated subjects. The group of motivated individuals attending the gym on average 2.433 visits a week, decreases their attendance to 1.706 after the introduction of the incentive, whereas gym attendance increases from 0.250 to 1.405 visits in the other group. Similarly,

Adda (2011) found in a field experiment that the consequences of priming (i.e. through a set of treatments based either on making information public, giving information on what others do, or punishment) differ according to the initial level of intrinsic motivation (i.e. captured by the number of days of community work performed over the previous year, and by the belief that autonomy is one of the most important value to transmit to a child). Priming crowds-in pro-social behaviour of less intrinsically motivated individuals who show higher contribution level to the public good and crowds-out pro-social behaviour of more intrinsically motivated individuals who show lower level of contribution afterwards.

We believe that policy strategies that drive pro-environmental behaviour could create interferences with moral motivations and that the willingness to compensate in later pro-social behaviour is influenced by enforcement rules of virtuous behaviour. When a virtuous act is imposed by a third party, it can be perceived by intrinsically motivated people as a no-acknowledgment of their intrinsic motivations and as an attempt to control their behaviour, which elicits a sense of psychological reactance (Brehm and Brehm, 1981; Zhang et al., 2011; see also Clary and Snyder, 1999). On the other hand, in an unregulated context, when intrinsically motivated individuals face the suggestion to perform a good deed they were already doing or considering, they will find the intervention rather supportive. When the good deed is a habit, doing it again is unlikely to boost the relevant self-concept (Cornelissen et al., 2008). Or the good deed might even be seen as a pleasant activity in itself, and involves no moral cost (Bruvoll et al, 2002) such that there are no longer any motives for later lenient choices.

Considering people that are less intrinsically motivated, the reasons driving voluntary contributions might be supported by other motives than those mentioned in the case of intrinsically motivated individuals (i.e. see Nyborg et al, 2006 for a more comprehensive review). The virtuous deed is not performed for pleasure or because of internalized norms, but more likely to maintain one self-image

as socially responsible to others (Brekke et al, 2003). In those cases, performing the good deed voluntarily serves other's moral recognition. Once the positive self-image has been established, the need for doing another good deed subsequently decreases. In the case a good deed is imposed by laws, it does not support one's self-image since the virtuous act is misattributed (i.e. it is not attributed to the individual anymore, but rather to the institution that made it mandatory, Khan and Dhar, 2006). No moral credit is earned, and licensing is less likely to occur. In one of their experiments, Khan and Dhar (2006) demonstrated that people 'who imagined doing community service as court-ordered punishment for a traffic violation did not indulge themselves later'.

Our work is an extension on licensing to better understand the way environmental policies should be designed and the unexpected effects of regulatory constraints within the environmental domain. It constitutes a first step to explore whether environmental policy design (commitment to a pro-environmental action within an unregulated framework compared to a regulated framework) shapes the extent of licensing effect, knowing that individuals are heterogeneous with respect to intrinsic motivations. More precisely, our research question explores whether environmental regulation (compared to non-regulation) changes the way individuals compensate their moral behaviour and the possible moderating effect of individual characteristics.

I.I.3. Experimental design[9]

In the spring 2011, we conducted a set of classroom experiments with students from different study areas within Montpellier Universities (South of France). A total of 185 Master students participated to the experiment, including 123 students from business-related majors (Mean age= 22.70, SEM=0.20) and 62 students from environmental-related majors (Mean age= 20.77, SEM=0.11). All subjects were unfamiliar with experimental economics. Students were not informed previously that

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[9] See Appendix 1 for detailed instructions.

they would participate in an experiment to avoid any selection bias. They were already there for their lectures and the experiment was presented as a classroom activity at the end of the lecture.

Experiments lasted less than 10 minutes. Participants were not informed about the nature of the experiment or the treatment to which they would be assigned. In each 30 students group, students were told that there would be one 30€ prize for each 30 participants, attributed at the end of experiment by drawing lots[10]. Even if the expected prize for participants was only 1€, we introduced the randomized payment on the basis of previous works and for practical reasons. In an Ultimatum game, Bolle (1990) showed that deterministic and randomized reward structures lead to similar behaviour of subjects.

The aim of our experimental design is to better understand the licensing effect in the context of pro-environmental behaviour and more precisely, to what extent, the way the previous good deed is implemented, re-enforces or relaxes the motivation to morally compensate virtuous behaviour later on. We tested two policy scenarios on sequential pro environmental motivation: the regulatory scenario, where the previous good deed was compulsory, and the non-regulatory scenario where subjects could refuse to invest in a previous good deed[11].

This experimental protocol followed three steps[12]. First, the monitor read the instructions aloud and all subjects received closed envelopes containing a questionnaire corresponding to one of the three treatments described below. Second, subjects were invited to make their decisions after being asked whether they had well understood the rules of the game. Then, participants were given some minutes to fill in the questionnaires anonymously. Third, after the time elapsed, sheets were collected and the winning number was chosen by drawing lots and publicly announced. The amount corresponding to the winner's decision was put inside an empty envelope and given to the winner by the lecturer at the end of the lecture, which allowed participant's decision to remain anonymous.

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[10] A similar method is used in Exadaktylos et al. (2013).
[11] The good deed was hypothetical for practical reasons. A similar method was used in Khan and Dhar (2006), who show that the imagination of engaging in a good deed is sufficient to boost the self-esteem.
[12] See instructions in Appendix I.

Three different treatments were randomly used in each session. In all treatments, subjects played a dictator game where they were asked the amount of money they wanted to share with a program involved in environmental conservation, if they were given a 30€ prize. To limit biases, on perception and trust in particular Environmental NGO (such as Greenpeace or WWF), no particular information was given on the environmental program[13].

In the control treatment subjects only played the adapted dictator game. In treatment 1, before playing the adapted dictator game, subjects were given the possibility to commit voluntarily to an environmentally friendly act (non-regulatory condition). And in treatment 2, the environmentally friendly act was compulsory. Both regulatory and non-regulatory frameworks were based on priming.

More precisely:

- The non-regulatory framing states: << 1/ **You have the opportunity to get involved in a pro environmental program** one hour per week during a month. **Do you wish to engage? => Yes or No.** 2/ On a 1 to 9 scale, select the satisfaction level that best describes yours after that decision>>

- The regulatory framing states: << 1/ **Your University decides to settle a compulsory pro environmental program in which you have to get involved** one hour per week during a month. 2/ On a 1 to 9 scale, select the satisfaction level that best describes yours after that decision>>.

The satisfaction scale's records aimed mostly at making sure that subjects put some attention on the imagined act. Since both conditions (regulatory and non-regulatory) are based on cheap talk, it should theoretically not make any difference in participants' willingness to donate. Nevertheless, we believe that just imagining committing to a virtuous act is sufficient to induce a licensing effect.

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[13] See Chartier (2002), for instance, for divergences of policies in terms of objectives or communication strategies of Greenpeace and WWF.

Beside, everything was done to avoid attracting the attention of subjects regarding questionnaire variations (e.g., identical envelopes, similar questionnaire size, and identical questionnaires on a given row). Table 3 resumes our experimental design.

Table 3. Experimental Design

Control	Treatment 1 **Regulatory condition** A pro environmental deed <u>has to be done</u>	Treatment 2 **Non-regulatory condition** A pro environmental deed is <u>proposed</u> (1=Accept; 2=Refuse)

Dictator Game. (*Measuring the Willingness to Donate*) Part of the potential earnings to be given in favor of a pro environmental project

I.I.4. Results

I.I.4.1. Data sample

We collected 185 observations. Table 4 shows that our sample is relatively well balanced across treatment groups.

Table 4 – Sample characteristics.

(N=185)	(1) Control group (N=61)	(2) Treatment 1 (N=63)	(3) Treatment 2 (N=61)
Gender ratio (% of male)	36.1	31.8	37.7
Age	22.2	22.2	21.8
University major (% of business)	67.2	66.7	65.6
Low income ; < 500 euros / month (%)	44.26	47.62	49.18

We can notice that environmental-orientated students declared to have lower incomes than business orientated students. In the non-regulatory condition (treatment 2), 59.02% of the

participants choose to commit to the environmental activity. This ratio fails to 47.5% when considering business majors only, whereas they were 80.95% in the group of environmental majors. This difference is significant at the 5% level (Wilcoxon test: z = 2.503, p =0.0123). The lower implication in volunteering encouraged us to explore the intrinsic motivations of students having chosen different majors (see section 5).

I.I.4.2. Impact of policy scenarios on sequential pro-environmental behaviour

Overall data (see Figure 1) doesn't allow us to conclude on a significant difference between regulatory and non-regulatory environmental policies on pro-environmental behaviour later on, even if students donated slightly less, on average, in treatment 2 (good deed with no regulation) (see results of Wilcoxon rank-sum tests on all sample in table 7). Moreover, the difference between the three treatments (control, regulatory and non-regulatory treatments) is not significant (Kruskal Wallis test, p=0.2598)

Figure 1. Average willingness to donate to the environmental union under different conditions (N=185)
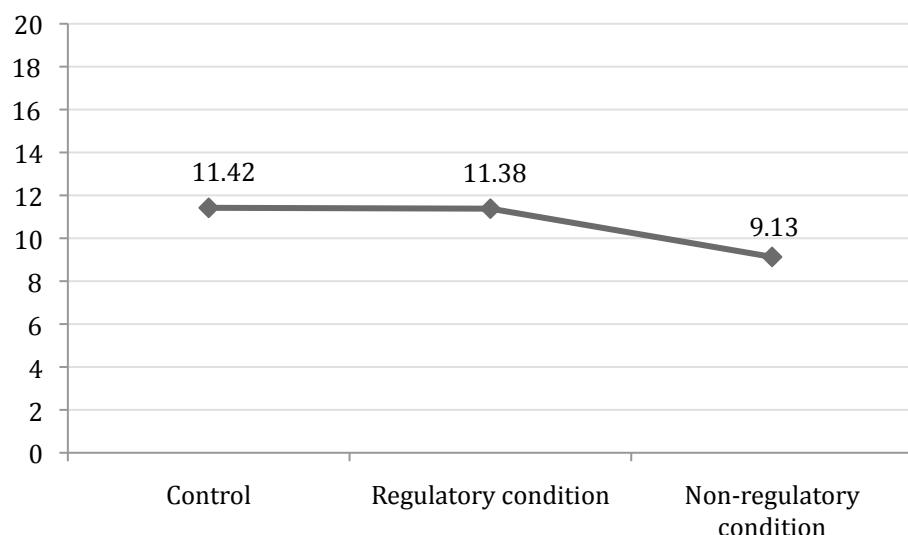
However if we distinguish our two sub-groups, we find that students from environmental-related and business-related majors reacted adversely to the two policy designs and shown in tables 6 and 7.

The two first columns in table 6 correspond to the willingness to donate, each of which is recorded separately among environmental and business majors. The third column shows the Wilcoxon statistical test of significance within the two subgroups (intergroup comparisons). The top row reports means for the control group (no virtuous act in a first stage). The next two rows explore means for the treatment one (non-regulatory condition), and for the treatment two (regulatory condition). When business-orientated students voluntarily engage into a virtuous act, their subsequent prosocial behaviour is highly reduced, i.e. they will donate 7.01€ less compared to those who haven't done a previous good deed (control group). This is not true for environmental-orientated students. The last row gives the statistical significance of the difference between the three samples (intragroup comparison), using a Kruskall Wallis test[14].

Table 6 – Motivation effect – Willingness to donate (WTD) and statistical differences (Wilcoxon rank-sum and Kruskal Wallis tests)

	All sample (N=185)	Environmental majors	Business majors	Wilcoxon test
Control *No virtuous act*	11.42 (N=61)	9.8 $^\Delta$ (N=20)	12.22 $^\Delta$ (N=41)	z=0.693; p =0.4886
Treatment 1 *Regulatory condition*	11.38 (N=63)	7.04 $^{\Delta,***}$ (N=21)	13.55$^{\Delta,***}$ (N=42)	z =2.612; p=0.0090
Treatment 2 *Non-regulatory condition*	9.13 (N=61)	10.88 $^{\Delta,**}$ (N=21)	5.21 $^{\Delta,**}$ (N=40)	z=-2.563; p=0.0104
Kruskal Wallis test	$\aleph^2$=2.696; p=0.2598	*$\aleph^2$=5.868; p=0.0532*	*$\aleph^2$=8.469; p=0.0145*	

$^\Delta$ Indicates statistical significance at the 5 percent level considering Kruskall Wallis test.
***, **, Indicates statistical significance at the 1 and 5 percent levels respectively, considering Wilcoxon test.

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[14] Non-parametric tests such as Wilcoxon (for two samples comparison) and Kruskal Wallis (for more than two samples comparison) were preferred to more common parametric Student t-test and ANOVA (one-way analysis of variance) because of the size of our samples.

Table 7 explores the intragroup comparison giving the statistical difference for each pair of samples (environmental vs. business majors). The first column indicates that the average WTD in treatment one (Regulatory condition) is significantly different at the 10% level from the WTD in the control group (no virtuous act first) for environmental-orientated students, but not for business-orientated students. Column two illustrates the opposite: WTD in treatment two (Non-regulatory condition) is significantly different from the control group at the 5% level in the case of business majors. For the two subgroups WTD in treatments one and two are statistically different from each other.

Table 7 – Treatment effect - Differences in means (Wilcoxon rank-sum test)

	Control/Treatment 1	Control/ Treatment 2	Treatment 1/ Treatment 2
Whole sample	z =1.306; p=0.1917	z =1.529; p=0.1262	z =-0.069; p=0.9453
Environmental majors	z =1.697; p=0.0896[*]	z =-0.350; p=0.7260	z =-1.986; p=0.0471[**]
Business majors	z =-0.871; p=0.3835	z =2.429; p=0.0151[**]	z=3.334; p=0.0009[***]

***, **, * Indicates statistical significance at the 1, 5 and 10 percent levels respectively, considering Wilcoxon test.

From these findings we can highlight the following results:

Our first result points out that environmental-orientated students donated significantly less than business-orientated students in the regulatory condition. Difference is significant at the 1% level (Wilcoxon test: z=2.612 p=0.0090).

Inversely, our second result states that environmental-orientated students donated significantly more than business-orientated students in the non-regulatory condition. Difference is significant at the 5% level (z=2.563, p= 0.0104).

So the licensing effect happened in the regulatory scenario with environmental-orientated students. The average donation for this subgroup is 2.76€ lower in the regulatory scenario than in the control (z=1.697, p=0.0896), and 3.84€ lower than in the non-regulatory scenario (z =-1.986, p=0.0471). And inversely, licensing appears to be more pronounced in the non-regulatory scenario with business-orientated students. Average donation for this subgroup is 7.01€ lower in the non-regulatory

scenario than in control (z=2.429, p=0.0151), and 8.33€ lower than in the regulatory scenario (z=3.33, p=0.0009). Figure 2 illustrates our findings.

Figure 2. Average willingness to donate to the environmental union under different conditions
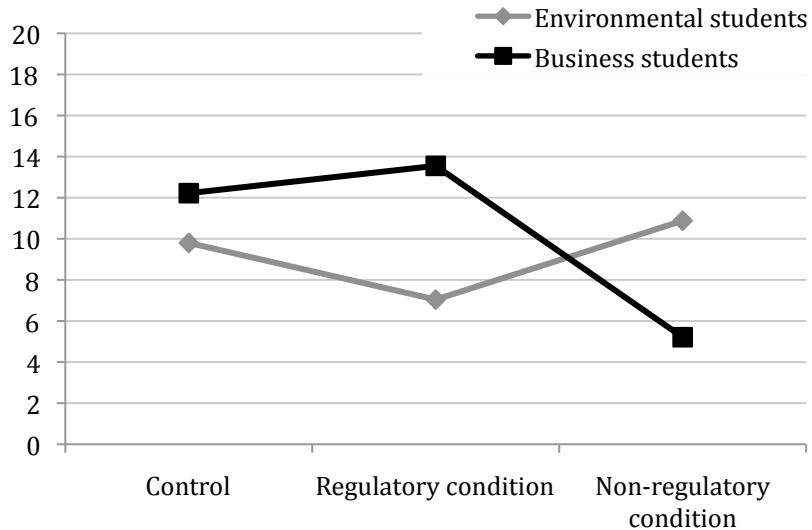
The difference in the donation amount between the two groups in the control condition (9.8€ vs 12.219€; p=not significant) is surprising. Within the literature, the numerous surveys that have been studying donations generally report that participants give a third of their initial endowment (Bolton, Katok, Zwick, 1998), which would mean than theoretically, average contribution should have been around 9€. In our case, business students gave 3.2€ more, and environmental students 0.8€ more than this theoretical amount. Even if this difference is not significant, we might want to discuss some potential explanations. Following a first intuition, we could think of justifying this difference by a revenue effect. In fact, business related majors have a greater part of high-income individuals than environmental related majors (72% vs. 15%). But we observe no significant revenue effect on donation amount (see regression results in table 8). A second intuition is that being involved in a non-environmentally related major (but rather in a major promoting economic enhancement) could

create a need to compensate ('compensation effect'). By choosing to study business, individuals have made a choice that could be perceived as self-oriented or 'not environmentally friendly', which might lead to a greater need to do something nice for the environment afterwards. Another intuition is derived from considering different contribution's philosophies. Given that environmental students are actively involved (i.e. through their studies but also in volunteering as shown in table 5), they might consider that contributing to the environment cannot be reduced to a monetary contribution, whereas business students could be more inclined to 'purchase' their environmental duty.

In order to capture the crossed effects on donation amounts in a regression analysis, we created four dummy variables (X_{11}, X_{12}, X_{21} and X_{22}) where X_{11} is a dummy variable equal to one when treatment one (regulatory condition) applies to group one (environmental majors), X_{12} the dummy variable equal to one when treatment one applies to group two (business majors), X_{21} when treatment 2 (non-regulatory condition) applies to group one and so on. Table 8 presents the linear regression results (by Ordinary Least Square method) that control for whether the participants belong to the environmental majors group or not, whether the good dead was generated mandatorily or voluntarily (to capture crossed effects of motivation and policies), gender and resources. First of all, the regression shows that gender and revenue do not influence the donation amount. Second, the coefficients on the X_{11} and X_{22} dummies are statistically significant, showing that crossing the study area orientation and a good initial deed generated regulatory (resp. non-regulatory) leads to a significant decrease in participants' subsequent contributions for the environment. This is consistent with the results of the Wilcoxon test presented in table 7.

Table 8: Factors affecting the donated amount (OLS)

Independent variable: Donation amount	Coefficient estimated	Standard Error
Gender (0=women; 1=men)	1.595	1.470
Revenue (0= <=500€ per month; 1=>500€ per month)	0.860	1.547
X_{11} (Environmental major * Regulatory condition)	-4.363 **	2.338
X_{12} (Environmental major * Non-regulatory condition)	-0.372	2.41398
X_{21} (Business major * Regulatory condition)	2.053	1.758097
X_{22} (Business major * Non-regulatory condition)	-6.015 ***	2.276
Constant	8.330 ***	2.842
Observations	185	
R^2	0.1050	
p	0.0086	

***, **, Indicates statistical significance at the 1 and 5 percent significance levels, respectively.

Results about what happened when business majors refused to commit to the virtuous act also deserve some attention[15]. A proportion of 52.5 % refused to engage for the environmental cause in the no regulation scenario (i.e. N=21), with an average donation of 10.52€ (more than twice the amount donated in the case where people agreed to volunteer). These amounts are significantly different with a Student test (t=-1.7647, p=0.0857) but not with the Wilcoxon test (z =-1.331, p=0.1832). This result could be another illustration of a compensation effect, as mentioned quite a few times in this work, where under some circumstances, paying could help to balance moral responsibility. However, given the number of observations, we remain cautious and cannot provide a clear cut conclusion. This calls for further investigations through an appropriate research design.

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[15] They were only 4 people in the intrinsically motivated group to decline the option of committing to the virtuous act, and gave an average of 13€. It could be interpreted as a compensation effect, but statistical power is too weak for further deduction.

I.I.5. Heterogeneity between environmental and business majors:
some further explanations

Our experimental design involves two subgroups (environmental-orientated students and business-orientated students) that behave in a significant different manner in terms of moral self-licensing related to regulatory or non-regulatory conditions. To better understand the rationale behind this, we investigate further on intrinsic motivations for pro-environmental behaviour that might constitute a plausible explanation for the emergence of moral self-licensing.

I.I.5.1. Measuring intrinsic motivations – NEP Scale and Lifestyle survey

It is well-known that students self-select and choose their majors at least partly because of their interests for the studied domains[16]. Moreover, it is important to emphasize some specificities of the French educational context. In France, students who enroll in business schools or in environmental-related schools make a very strong choice that frequently lock them in a given trajectory for several years[17]. Future engineers and business schools graduates have to succeed in a highly competitive selection, which borders on elitism after two years of intensive preparation. Engineering training then follows for three years at a given school. Recent publications stress that students enrolled in environment-related schools "elect by plebiscite" environment-related jobs (Véleine, 2007). Interestingly, recent research showed (Bauman and Rose, 2011; see also Frey and Meier, 2003) that economists are less pro-social individuals by selection. For those reasons, we hypothesized that students enrolled in environmental-related majors are more intrinsically motivated regarding

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[16] This point is consistent with Frank's finding (2003). Frank's (2003) survey on Cornell graduates show that 88 percent of socially concerned respondents would prefer a job for the American Cancer Society rather than for Camel Cigarettes with an average compensating wage premium of about $ 24,000.00 per year. Cornell graduates were invited to choose between pairs of hypothetical jobs where the job nature was the same but the employers' social responsibility reputation was different.

[17] http://www.cefi.org/MODE_B/BCU_OVER.HTM

environmental issues whereas students enrolled in business-related majors are less intrinsically motivated regarding the same issues[18].

To test this assumption, we carried out, *a posteriori,* a survey based on the New Ecological Paradigm (NEP) Scale (Dunlap et al., 2000), which is a revision of the scale developed by Dunlap and Van Liere in 1978. This scale (i.e. a 15-item questionnaire) consists of 8 pro-NEP and 7 anti-NEP items developed to measure strength of endorsement (from low to high) of an ecological worldview. It thus characterizes an individual's environmental concern based on the extent to which he agrees or disagrees with various statements on environmental issues, such as: the reality of limits to growth (questions 1,6,11), anti-anthropocentrism (2,7,12), the fragility of nature's balance (3,8,13), rejection of the idea that humans are exempt from the constraints of nature (4,9,14), and the possibility of an ecocrisis (5,10,15). The response categories range between 1 and 5 so that high scores correspond to a stronger pro-environmental view in the eight odd numbered items while this is reversed for the seven even numbered (low scores indicating a stronger pro environmental view).

After extensive application across a diverse range of studies[19], a broad consensus is emerging in the environmental psychology literature that the NEP represents a valid and reliable scale for measuring levels of ecological beliefs and behaviours (Cordano and al, 2003), as long as the 15–item version of the scale is used (Hawcroft and Milfont, 2010). We replicated this 15-item version of the NEP scale among our two subgroups of students. We decided to complete this scale by another questionnaire dealing with lifestyle to cross check for consistency. Questions about green habits (e.g., waste sorting, transportation, organic food consumption) as well as environmental/political engagement (e.g., involvement in environmental association, membership in political parties) were asked to strengthen our analysis about those two subgroups characteristics. This would also enable to see whereas the NEP scale is a good indicator of green habits and commitment.

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[18] For sake of exposition, we distinguish intrinsically motivated and non-intrinsically motivated but we are conscious that the reality is more nuanced. In short, we contend that business-students also truly care about the environment, but maybe not as strongly as environment-students.

[19] Dunlap and al's (2000) survey holds 477 citations.

I.I.5.2. Survey results: Measuring intrinsic motivation

Individuals who participated in the survey for the evaluation of intrinsic motivations regarding environmental issues were 98 graduated students from Montpellier Universities, belonging either to environmental related majors or business related majors. Table 4 summarizes the results for the 15 items NEP scales and table 5 presents data resulting from questions related to lifestyle.

Table 4 shows significant statistical differences for 8 over the 15 items of the NEP Scale, illustrating a stronger pro-environmental view for environmental related majors. Results are significantly different at 1% for questions 1, 5, 7, 8, 10, 11, 12 and at 5% for question 14. This clearly points out a distinct pro environmental orientation between environmental students and business students. This result is confirmed by questions about lifestyle. Table 5 demonstrates that environmental students have greener habits and are more engaged in environmental activities. Indeed, 41.38% of environmental students have already been involved in an environmental association versus 5.56% for business students (Significant difference at 1%[20]). Appraising environmental behaviour through daily habits, on a scale from 0 (=Never) to 3 (=Very often), we find that environmental students use more their bicycle for transportation (1.57 vs 0.97, $p < 0.01$), sort waste more regularly (2.45 vs 1.89, $p < 0.01$), go for organic products more often (1.72 vs 1.08, $p < 0.01$) and use less plastic bags (0.93 vs 1.32, $p < 0.01$). From our survey, we found support that students in environmental related majors have higher intrinsically motivations than students in business related majors.

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[20] A non-parametric Wilcoxon test has been used.

Table 4. Responses to the New Environmental Paradigm Scale items with Means, Standard Errors, Statistical significance of Environmental related and Business related majors' differences from Student's test and Wilcoxon's test[21].

Items	Environmental related majors (n=58)		Business related majors (n=40)		Statistical Significance of Difference between Environment and Business	
Do you agree or disagree that: *(1= Strongly Disagree; 2= Mildly Disagree; 3= Unsure; 4= Mildly Agree; 5= Strongly Agree)*	Mean	SE	Mean	SE	Student	Wilcoxon
1. We are approaching the limit of the number of people the earth can support	3.741379	0.1243503	2.925	0.1576571	0.0001***	0.0002***
2. Humans have the right to modify the natural environment to suit their needs	3	0.1369444	2.825	0.1890852	0.4439	0.3380
3. When humans interfere with nature it often produces disastrous consequences	3.327586	0.1399765	3.35	0.2104635	0.9266	0.7232
4. Human ingenuity will insure that we do NOT make the earth unlivable	3.241379	0.1212716	3.175	0.1334335	0.7181	0.7385
5. Humans are severely abusing the environment	4.327586	0.0964836	3.75	0.1631029	0.0016***	0.0020***
6. The earth has plenty of natural resources if we just learn how to develop them	3.793103	0.1272317	3.725	0.1753659	0.7479	0.9355
7. Plants and animals have as much right as humans to exist	4.637931	0.0725404	4.1	0.1467599	0.0005***	0.0019***
8. The balance of nature is strong enough to cope with the impacts of modern industrial nations	1.807018	0.1103611	2.375	0.1372229	0.0016***	0.0008***
9. Despite our special abilities humans are still subject to the laws of nature	4.245614	0.0945929	4.175	0.123452	0.6457	0.6214
10. The so-called "ecological crisis" facing humankind has been greatly exaggerated	2	0.1147079	2.675	0.1907727	0.0018***	0.0070***
11. The earth is like a spaceship with very limited room and resources	3.578947	0.1436792	2.875	0.1900995	0.0034***	0.0044***
12. Humans were meant to rule over the rest of nature	1.714286	0.1266816	2.675	0.1941038	0.0000***	0.0001***
13. The balance of nature is very delicate and easily upset	4.087719	0.1207763	3.875	0.1527001	0.2725	0.2723
14. Humans will eventually learn enough about how nature works to be able to control it	2.122807	0.1278073	2.625	0.1667468	0.0171**	0.0250**
15. If things continue on their present course, we will soon experience a major ecological catastrophe	4.122807	0.1121396	3.825	0.1636424	0.1233	0.1779

***, **, Indicates statistical significance at the 1 and 5 percent significance levels, respectively.

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[21] Given that our sample size is at the limit for the use of parametric test, we decided to run both parametric (Student t-test) and non-parametric (Wilcoxon test) tests.

Table 5. Lifestyle with Means, Standard Errors, Statistical significance of Environmental related and Business related majors' differences from Student's test and Wilcoxon's test.

Items	Environmental related majors (n=58)		Business related majors (n=40)		Statistical Significance of Difference between Environment and Business	
	Mean	SE	Mean	SE	Student	Wilcoxon
1. Have you ever been a member of an environmental association? (1=Yes; 0=No)	0.4310345	0.0700534	0.0555556	0.0387184	0.0001***	0.0002*****
2. Do you have a political party membership? (1=Yes; 0=No)	0.0701754	0.0491765	0.027027	0.027027	0.5078	0.8092
3. Have you ever undertaken voluntary work for the environment?(1=Yes; 0=No)	0.3508772	0.0729405	0.1081081	0.0517528	0.0169**	0.0190**
4. How often do you get involved in protests? (0=Never; 1=Sometimes; 2=Quite often; 3=Very often)	0.9298246	0.0862024	0.2702703	0.0740166	0.0000***	0.0000***
5. How often do you attend political debates? (0=Never; 1=Sometimes; 2=Quite often; 3=Very often)	0.6140351	0.0961489	0.6388889	0.1388889	0.8797	0.9163
6. How often do you ride a bike for transportation? (0=Never; 1=Sometimes; 2=Quite often; 3=Very often)	1.568966	0.1371918	0.972973	0.1128826	0.0028***	0.0054***
7. How often do you sort your waste? (0=Never; 1=Sometimes; 2=Quite often; 3=Very often)	2.448276	0. 0858128	1.891892	0.1766547	0.0023***	0.0138**
8. Between two similar products, do you choose the organic one? (0=Never; 1=Sometimes; 2=Quite often; 3=Very often)	1.724138	0.1199745	1.081081	0.1519561	0.0012***	0.0012***
9. Do you leave the water running unnecessarily? (0=Never; 1=Sometimes; 2=Quite often; 3=Very often)	0.6034483	0.0813515	0.8108108	0.1492616	0.1888	0.4287
10. Do you let lights on when you leave a room? (0=Never; 1=Sometimes; 2=Quite often; 3=Very often)	0.4482759	0.0703135	0.7837838	0.1507496	0.0267**	0.1234
11. Do you use plastic bags when shopping? (0=Never; 1=Sometimes; 2=Quite often; 3=Very often)	0.9310345	0.0644304	1.324324	0.1503452	0.0077***	0.0264**

***, **, Indicates statistical significance at the 1 and 5 percent significance levels, respectively.

I.I.6. Discussion and Conclusions

First of all, our results show that after people declared they engaged in a pro social act (e.g. doing something good for the environment), they feel licensed to behave more selfishly subsequently (e.g. donating less to an environmental program). Promoting virtuous behaviour through non-monetary incentives could therefore have some implications on further acts such as licensing people to misbehave.

Second, our contribution is an additional stone supporting the fact that actions must not be considered in isolation but as influencing each other. The influence is not only related to the nature of the action (good versus bad deed) but also the way it is generated. We have shown that the licensing effect is influenced by the way the 'virtuous' act is generated according to whether individuals are more or less intrinsically motivated.

The study aimed to experimentally test for conditions that are assumed to influence the licensing effect. We conclude that the presence of intrinsic motivation and the way the virtuous act is generated (regulation vs. no regulation) are two important conditions explaining the occurrence of the licensing effect. We found that more intrinsically motivated individuals donated significantly less than less intrinsically motivated individuals under regulation. Conversely, more intrinsically motivated individuals donated significantly more than less intrinsically motivated individuals under no regulation. The licensing effect arises when combining intrinsically (non-intrinsically) motivated individuals and regulatory (non regulatory) conditions. Overall, more intrinsically and less intrinsically motivated individuals reacted adversely to the treatment variables. Regulatory condition does not work well with intrinsically motivated individuals but it does work well with non-intrinsically motivated individuals. The non-regulatory condition performs better with intrinsically motivated individuals but licenses non-intrinsically motivated individuals. In short, these insights contribute to

broader understanding of when and why consistency and licensing emerge in the real world (see also Gneezy et al., 2012).

The main implication of our findings is to call for extreme care when using non regulatory approaches to incentivize individuals to behave pro-environmentally. They could help achieving significant impacts at lesser cost but are not immune to counter-productive effects. A plausible extension to this work would be to explore to what extent and in what ways monetary incentives would also interfere with the licensing effect (*See Section II*). At the margin, our results start exploring this idea by suggesting the need to target policies according to population subgroups and avoid 'one-size-fits-all' policies in the environmental realm. Indeed, it seems necessary to characterize and elicit whether subgroups of the population are more or less intrinsically motivated to tailor policy instruments accordingly. Interventions designed to change the behaviour of one group may not work equally to change the behaviour of another. Even worth, interventions can have unintended or detrimental effects if they are incongruent with the characteristics or beliefs of certain group members. In support to this concluding remark, Costa and Khan (2010) have shown that while the electricity conservation "nudge" of providing feedback to households on own and peers' home electricity usage works with liberals, it can backfire with conservatives. Moreover, this challenging point may raise equity issues where subgroups would face different instruments. Further researches may not only suggest strategies to reduce the likelihood of the licensing effect, but also investigate alternative policy designs that could improve overall environmental performance.

Part II. Self-Licensing and Financial Rewards: Is Morality For Sale[22]?

I.II.1. Introduction

In Hollywood movies (as in most movies), the good guys and the bad guys are easily recognized. In the typical economic landscape, the Good Samaritan and the Free Rider are similarly well distinguished. In both cases, we expect the good guys to act generously and the bad guys, or free riders, to act selfishly. If decisions were consistent, these "exogenous preferences" would not matter very much for economists. Emerging research and daily observations, however, raise conflicting evidence that suggests a temporal inconsistency wherein the decision-making process exhibits more substitutions than complements. From food consumption habits to pro-social attitudes, illustrations of this phenomenon exist in a wide range of domains, as suggested by the following cases: soaring sales of Big Macs after McDonald's added healthier items to its menu, Prius Hybrid drivers being more likely to break crosswalk laws, get into accidents, and receive fines, green buyers being more likely to steal, etc. (Woodyard, 2009; Mazar and Zhong, 2010). Several contributions from marketing and psychology attribute these types of behaviours to a "self-licensing effect", a situation wherein a good deed might excuse subsequent dubious behaviour (Khan and Dhar 2006; Sachdeva et al., 2009).

Until now, analysis of the licensing effect remains scarce in the economic literature. A notable exception is the contribution of Branas-Garza et al. (2011), in which the authors show that a deviation from a 'normal state of being' is balanced with a subsequent action that compensates for prior behaviour. Performing an economic experiment in which subjects played a sequence of giving decisions, they found that participants' donations in each round were negatively correlated to the amount they donated in the previous period. The authors therefore interpret donations over time as the result of a pattern of self-regulation: moral licensing (being selfish after being generous) and

cleansing (being generous after being selfish). Our analysis also pertains to a recent contribution by Tiefenbeck et al. (2013), in which the authors discuss the positive or negative potential side effects of environmental behaviour change. They found evidence that the behavioural change studied (decrease of water consumption due to a targeted campaign) generates negative, rather than positive, spillover in a related domain (increase in electricity consumption). They also indicated that their results were consistent with the predictions of self-licensing theory. Additionally, in the nudges literature, some researchers have found that informing people they are contributing more than others to a social good (e.g., by lower electricity consumption) can decrease contributions (e.g., lead to increased electricity consumption; Schultz et al., 2007). Even if alternative explanations have been suggested, such as conformity to a social norm, another possible explanation could well be the existence of a moral-licensing effect.

I.II.2. Experimental design[23]

This work examines the licensing effect in the context of pro-social behaviour. More precisely, we investigated whether a good deed performed either for free or in exchange for payment will impact subsequent pro-social behaviour in the same way. We use an adapted dictator game with one control and two treatments. In both treatments, players are first asked to imagine that they could engage in a pro-environmental activity, which consists in cleaning riverbanks near their home. We ask them to choose the number of bags they would be willing to fill with litter, from 0 to 20. In treatment one, respondents receive no payment for the activity. In treatment two, respondents are paid 2€ for each bag they fill. The control treatment does not include any prior neutral task, but we contend that this design does not fundamentally change our results.[24] The second step of the

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[23] See Appendix 2 for detailed instructions.

[24] Khan and Dhar (2006) tested the impact of a neutral task (identifying words that are misspelled in a passage of a text) vs. a positive task (indicating whether or not to help a foreign student). Their results show that the licensing effect is robust to this framing, such that subjects are more likely to behave self-indulgently after having performed a positive task than those having performed a neutral task (X2 = 7.95, p<0.05). In another

experiment is common to all groups and consists in asking participants to allocate an amount (30€) between themselves and an environmental charity (either World Wildlife Fund or Greenpeace).[25] Previous literature has shown that moral licensing can arise in the same domain (Monin and Miller, 2001, Sachdeva et al., 2009) or extend into unrelated domains (Mazar and Zhong, 2010). We select the initial activity and the pro-social behaviour from the same domain, i.e., removing debris from riverbanks and voluntary contribution to environmental protection (see Tiefenbeck et al., 2013 for another application of two environmental issues). While this issue is beyond the scope of our contribution, it may be useful to consider the suggestion made by Merritt et al. (2010) that 'prior behaviour may license a misdeed in the same domain via moral credentials (…), but license a misdeed in a different domain via moral credits'.

Before we ran the experiment, we hypothesized that establishing moral credential through doing a good deed for the environment will reduce subsequent pro-social intentions as measured through a charity donation game. In an experiment, Khan and Dhar (2006) showed that people who initially imagined doing a good deed were less inclined to behave ethically later on. In other studies, it has been shown that subjects who wrote something positive about themselves later gave one fifth of the amount given by those who wrote a story referring to their negative traits (Sachdeva, Lliev and Medin, 2009). Interestingly, another experiment demonstrated that performing a good deed under external motives (doing community service as a court-ordered punishment) dampened the licensing effect (Khan and Dhar, 2006). We test the effect of an external reward in the form of a financial

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experiment, Khan and Dhar (2006) compared mean self-assessment in license (positive task) and control (neutral task) conditions. Consistent with their previous results, participants rated themselves significantly more positively (over 4 attributes on a scale from 1 to 7) in the license condition than in the control condition. They replicated a parallel experiment with positive vs. no prior task and obtained similar results, proving that licensing effect is due to the positive attribute of the task and not the task itself. In a similar vein, Sachdeva, Lliev and Medin (2009) found a significant decrease in donations among subjects who previously wrote a positive story about themselves compared to those who wrote a neutral story. Also, Mazar and Zong (2010) showed that people who bought green products were then significantly more likely to cheat than people who bought regular products. Overall, it has been well established in this literature that the positive attributes of a given task are the factor underlying moral behavior change.

[25] Those two environmental charities were chosen because of their high popularity among French people.

incentive on self-licensing to determine whether paid and unpaid good deeds induce self-licensing in the same way.

The between-subjects experiment was performed among 290 students (40.5% male) at universities in Montpellier, a major city in southern France. Three sessions following identical instructions took place during student lecture periods, which reduced potential selection bias. The instructor asked participants to arrange themselves in exam formation (one student for every two places) to ensure anonymity. Students' earnings were based on their own responses. The dictator game per se lasted an average of five minutes, and the potential gain ranged from 0€ to 30€. The instructions indicated that one student in thirty would be selected by drawing lots at the end of the session to determine who would receive effective payments. Earnings were distributed in a closed envelope to participants upon presentation of their identification number. Detailed instructions for the experiment are provided in the appendix.

I.II.3. Results

Donation behaviour between the two organizations mentioned in the questionnaires was nearly equivalent (48.71% chose Greenpeace, giving an average amount of 9.82€ vs. 9.99€ for WWF). Figure 1 shows the selfishness rate (i.e. donation is equal to zero) for the control and treatments. In the control group, one in eight subjects (12.37%) exhibits selfish behaviour. This rate increases to more than one in four (27.37%) in treatment one (unpaid good deed) and is significantly different from the control group at the 1% level (Student's t-test: t=2.6413, p=0.0089). This difference is less salient between the control group and treatment two (good deed in exchange for payment): 17.53% of the subjects give nothing after a hypothetical paid good deed, which is not significantly different from the control group (t=0.9738, p=0.3314). These results suggest that performing an imaginary good deed for free allows more people to act selfishly when compared to the case where no good deed is performed (as in the control group) or when the good deed is paid (as in treatment two). The

difference in selfishness rates between treatments one and two is significant at the 5% level using a

binomial test (p=0.014142), which suggests that offering financial rewards for a good deed dampens

the licensing effect.

Figure 1 - Share of selfish subjects (=subject's donation is zero)
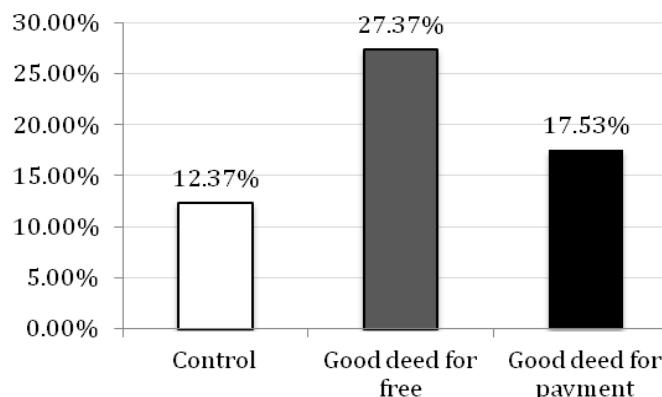
Figure 2 shows the average donation and filled bag rates in the control and treatment groups.

Overall, we observe that introducing payment led to a greater increase in the good deed than in the

subsequent charity donation. Among all observations (including when donation is null), subjects gave

an average of 31.62% of their endowment in the control group, equal to about 9.5€. Among

participants who imagined themselves as having performed an unpaid good deed, fewer chose to

donate at all. The decrease is slightly mediated in the case of a paid good deed (-1.42 points)

compared to an unpaid good deed (-1.95 points). The difference between the control group and

treatment one (unpaid good deed) is statistically significant at the 5% level using the Student's t-test

(t=190, p=0.0375). The difference between the control group and treatment two (paid good deed) is

not statistically different (t=192, p=0.1222). Among respondents who donated a positive amount, the

licensing effect appears to be softened, as differences in the amount donated between the control

and treatments groups are not statistically significant. Table 1 summarizes these results.

Figure 2 - Mean of donations (0-30€) and bags (0-20 bags) filled in the control and treatment groups
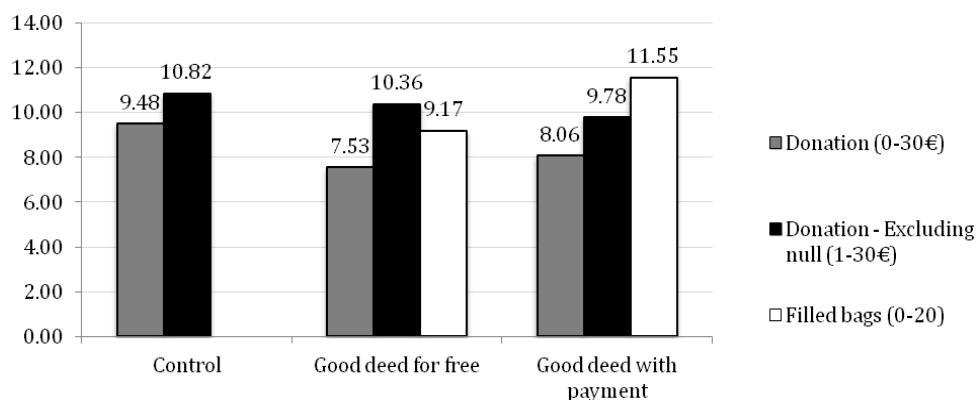
Table 1 - Statistical differences between control and treatment groups using the Student's t-test

	Student's t-test
All subjects	
Control (n=97)**/ Treatment 1** (n=95) *(Good deed, No payment)*	t= 2.0952/ p=0.0375**
Control (n=97)**/ Treatment 2** (n=98) *(Good deed, Payment)*	t= 1.5525/ p=0.1222
Treatment 1 (n=95)*(Good deed, No payment) /* **Treatment 2** (n=98)*(Good deed, Payment)*	t=-0.5029/ p=0.6156
Donors only (donation >0)	
Control (n=85)**/ Treatment 1** (n=69) *(Good deed, No payment)*	t=0.5115/ p=0.6097
Control (n=85)**/ Treatment 2** (n=80) *(Good deed, Payment)*	t=1.1863/ p= 0.2372
Treatment 1 (n=69)*(Good deed, No payment) /* **Treatment 2** (n=80)*(Good deed, Payment)*	t=0.5190/ p= 0.6045

***, **, * Indicates statistical significance at the 1, 5 and 10 percent significance levels, respectively.

Figure 3 provides further insights into the distributions of donations between control and treatment groups. In the control group, positive donations are less dispersed from the average (mean= 10.82353, sd=4.212161) than in the unpaid good deed (mean=10.36232, sd=6.87701) and paid good deed (mean= 9.775, sd=6.897027) treatments. In addition to the higher proportion of selfish subjects in the two treatments than in the control group (as mentioned earlier in the text), we also observe that positive donations are marked by an increase in non-selfish responses (i.e. donations greater than 10€). There are significantly more donations above 10€ among respondents who performed paid (Wilcoxon–Mann–Whitney test: z = -2.202, p=0.0277) and unpaid (z = -2.244, p=0.0248) good deeds than in the control group.[26,27]

Figure 3 - Distributions of donations (0€-30€) in control and the two treatments

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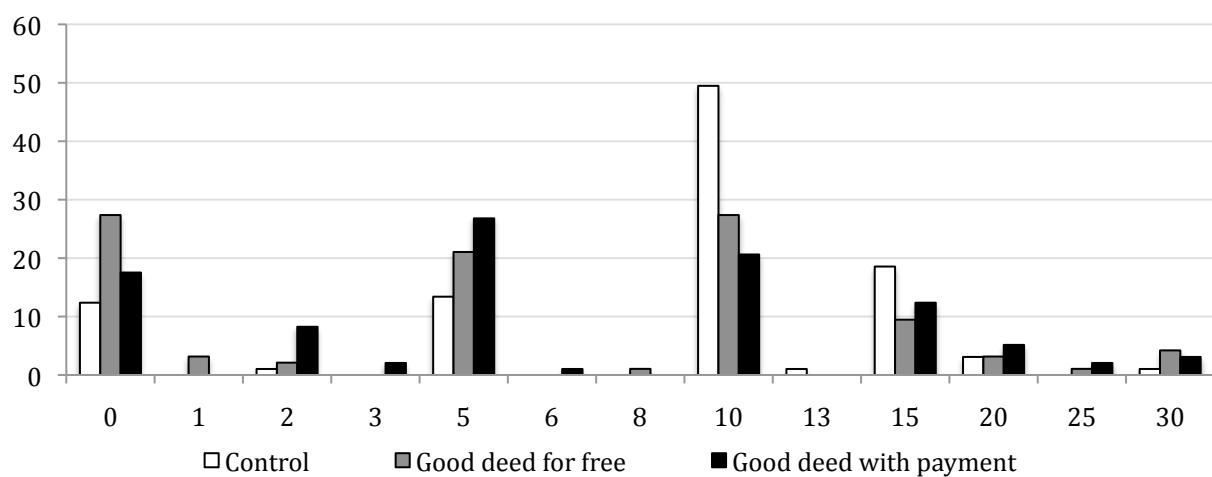
[26] Non-parametric tests are preferred when at least one of the two samples involved in the comparison is below 60.

[27] Assuming that treatments had heterogeneous effect among subjects, a possible interpretation is that a 'foot in the door' effect applies to intrinsically motivated individuals (doing a good deed also encourages subsequent good behaviour) while a 'licensing effect' applies to less intrinsically motivated individuals (doing a good deed licenses subsequent bad behaviour).

Research in economics indicates substantial and consistent differences in behaviour between men and women, indicating that women tend to be more socially-oriented while men tend to be more individually-oriented (Eckel and Grossman, 1998; see also Aguiar et al., 2009; Gneezy and Croson, 2008). Furthermore, some studies suggest that this gender difference is more pronounced when the recipient is a charity rather than another anonymous participant (e.g., Eckel and Grossman, 2003). In a meta-analysis of dictator games, Engel (2011) confirms that women tend to donate more money than men. Figures 4 and 5 present a gender analysis of our experiment. Results show that, as is consistent with existing literature (Engel, 2011), women gave more to charity than men in all groups. This difference in donation behaviour between men and women is statistically significant for all groups combined (Student's t-test: t=276, p=0.0175) as well as for treatment two (z = -1.745, p=0.0810). Moreover, men and women appear to react to financial incentives in opposite ways: whereas payments for a good deed tend to attenuate the licensing effect among women, they tend to strengthen the effect among men. When considering gender and treatments, the only subgroups where the licensing effect remains statistically significant is for women, between control and treatment one (z=2.562, p=0.0104). The lack of significance among subgroups crossing gender and treatments may also be linked to the sample size. In further support of the established gender effect, women declare a willingness to fill more bags than men in both treatment groups. This difference is significant at the 10% level in the no payment treatment (z =-1.747, p=0.0807) and not significant in the payment treatment[28].

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[28] As mentioned by a referee, income levels of participants and their average donations outside the experiment could constitute good control variables. Unfortunately, income data are very rough and average donations outside the experiment have not been gathered.

Figures 4 - Average donation across treatments, by gender
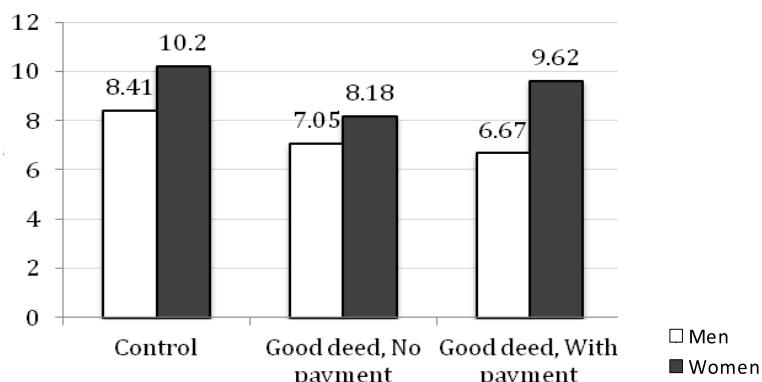
Figures 5 - Average bags filled across treatments, by gender
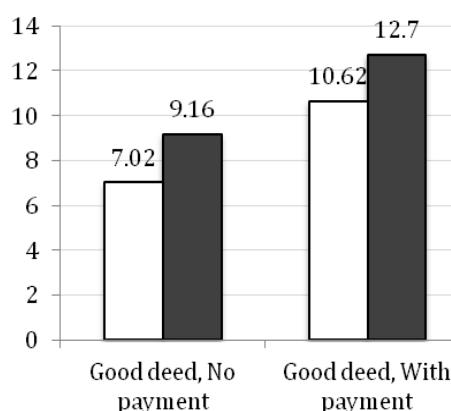
We conclude this analysis by performing a hurdle model, which separates the decision-making process regarding whether or not to give from the decision-making process regarding how much to give. The first part of the model consists of running a logit regression to explain the binary outcome of a zero versus positive donation. This regression confirms that the main explanatory variable in the decision to give is the unpaid good deed. Models 2 and 3 corroborate the robustness of this result.

Table 2 - Logit model regression – Donations (0/1)

	Model 1		Model 2		Model 3	
Good Deed Free	-0.946**	(-2.42)	-0.982**	(-2.55)	-0.763**	(-2.49)
Good Deed Paid	-0.335	(-0.80)	-0.409	(-1.00)		
Gender	0.223	(0.70)				
Constant	1.606***	(2.76)	1.958***	(6.35)	1.739***	(8.63)
Observations	278		289		289	
BIC	280.5		291.2		286.5	
Chi2	7.024		7.143		6.125	
p>chi2	0.0711		0.0281		0.0133	

T statistics in parentheses

* p<0.10, ** p<0.05, *** p<0.01

Table 3 presents results from the truncated linear regression, which seeks to explain positive donations under 30€ (the maximum donation amount was restricted to 30€). In line with our previous analysis, the linear regression shows that treatments do not impact the amount donated. However, it does confirm a crossed effect of gender and treatments on donations.

Table 3 - Truncated linear regression – Donations (1-30)

	Model 1		Model 2	
Good Deed Free	-0.234	(-0.17)	1.227	(1.625)
Good Deed Paid	-1.039	(-0.77)	0.331	(1.519)
Gender	2.917**	(2.42)		
Male * Good Deed Free			-3.957*	(2.174)
Male * Good Deed Paid			-3.868*	(2.134)
Constant	4.808**	(2.10)	9.602***	(0.985)
Observations	227		227	
BIC	1451.8		1446.2	
Chi2	6.438		6.379	
p>chi2	0.0921		0.0412	

T statistics in parentheses

* p<0.10, ** p<0.05, *** p<0.01

I.II.4. Discussion and Conclusions

In sum, our experiment reveals that the establishment of 'moral rectitude' through the imagination of performing an unpaid good deed may license subsequent selfish behaviour. Our results also suggest that paid good deeds are accordingly less likely to induce self-licensing. This study also provides evidence that the paid or unpaid characteristic of a good deed has a stronger impact on the decision regarding whether or not to donate than on the decision regarding how much to donate. In general, we find that engaging in an unpaid good deed encourages purely selfish behaviour (i.e. a donation of zero), but has little effect in determining the value of positive donations. Regarding gender considerations, our results indicate that women are more willing to donate to charity and to volunteer. The results of our dictator game are very similar to those of Engel (2011). Our results also suggest that men and women may react differently to payments for good deeds,

though confirming this would call for further research. Overall, the key contribution of this work is to present payments as a mediator of the licensing effect. An interesting extension would be to investigate whether the results would still hold if the hypothetical and real money tasks were made simultaneously rather than sequentially. More broadly, this work calls into question the image of the typical economic world as comprised of immutable Good Samaritans and Free Riders, raising evidence that is instead suggestive of a more nuanced vision of humanity wherein context plays a major role in determining whether people will behave as good guys or bad guys.

Section II – The impact of moral licensing on immoral behaviour

Part I. Moral self-licensing and social dilemmas: An experimental analysis from an appropriation game in Madagascar[29]

II.I.1. Introduction

A growing literature on moral self-licensing opens new directions to understand the forces underlying individuals' moral behaviour. Traditionally, moral behaviour and social preferences have been considered as exogenous and consistent over time (Narloch et al, 2012; Carpenter and Seki, 2010; Castillo and Saysel, 2005; Henrich, 2000). But recent researches support that an initial virtuous act might impact future decisions, by decreasing the willingness to act pro-socially in subsequent circumstances and vice versa (Khan and Dhar, 2006; Sachdeva et al., 2009; Mazar and Zhong, 2010; Chiou and al., 2011). For example, anecdotal and empirical evidence shows that Prius Hybrid drivers are more likely to break crosswalk laws and get fines, green buyers are more likely to steal, people who wrote positive stories about themselves are more likely to act selfishly, etc. (Woodyard, 2009; Mazar and Zhong, 2010, Sachdeva et al., 2009). In a similar vein, we run an experiment to test the prediction of the 'moral credit model' (i.e. good deeds establish moral credits like deposits in a bank account that can be 'withdrawn' to 'purchase' the right to perform bad deeds) in a developing country context and used a social dilemma situation where private and public benefits diverge.

If studies on social surplus creation and pro-social preferences have proliferated those last decades, much less is known about the motives underpinning surplus destruction's behaviour (Cox et al 2013).

We thus offer a new approach to study anti-social behaviour by mobilizing recent work in psychology (Khan and Dhar, 2006; Sachdeva et al., 2009; Mazar and Zhong, 2010; Chiou and al., 2011). A second originality of our paper is to test the moral credit model in a developing country context. Indeed, all researches on moral self-licensing effect have been run in Western countries, where a WEIRD[30] effect can be suspected (Heinrich et al., 2010). Indeed, Heinrich et al. (2010) argue that using samples drawn entirely from Western, Educated, Industrialized, Rich, and Democratic (WEIRD) societies to study human nature can be misleading. Cardenas and Carpenter (2008) state that "many developing countries rely on local norms and rules of conduct to provide public goods and regulate extraction from common pool resources" and argue that these informal norms for fairness and justice depend on the existing formal institution (laws in particular). Moreover, equity and fairness concerns differ according to wealth (Armantier, 2006). For these reasons, it seems important to us to investigate on other-regarding preferences in developing countries. The reminder of this paper is as follows. A literature overview is exposed in section 2. We then present the experimental design and procedures used to test our predictions in section 3. Section 4 provides the results and discusses them. Section 5 concludes.

II.I.2. Literature review

An emerging literature suggests that policies designed to create Pareto-Superior outcome may in fact impact individual behaviour in counterintuitive ways (Cardenas et al 2000, Vollan 2008, Narloch et al 2012). A possible explanation for this paradox comes from the divergence between standard economic theory and empirical evidence. For instance, standard economic theory depicts individuals as Homo Oeconomicus, who pursue their self-interests and free ride on others. However, ample evidence from behavioural economics demonstrates that individuals have pro-social preferences and

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[30] WEIRD effect : Western, Educated, Industrialized, Rich, Democratic

may succeed in overcoming social dilemmas under some circumstances (Ostrom 2000, Velez et al 2009, Cardenas 2004).

Since then, individuals have been increasingly described as a combination of self-interest and social preferences, tending to strike a balance between self and group interests. In this literature, social preferences are still considered as endogenous (Narloch et al, 2012, Carpenter and Seki, 2010; Castillo and Saysel, 2005; Henrich, 2000) and therefore determined by socio-economic and cultural characteristics while potentially crowded out by exogenous mediator such as external regulation (Cardenas et al, 2000) and external rewards (Vollan, 2008; Narloch et al, 2012). In parallel, the idea of moral self-licensing has emerged recently from the marketing and psychological literature, arguing that social preferences might be influenced by past behaviour.

'Moral self-licensing' refers to a situation where being 'good' leads to more self-indulgent option afterwards (the reverse situation would be better referred as moral cleansing). Khan and Dhar (2006) proposed to define moral self-licensing as a non-conscious effect that operates by providing a moral boost in the self-concept, which increases the preference for a relative immoral action subsequently by dampening the negative self-attributions associated with such behaviour. In other words, this mechanism corresponds to a process of moral accounting where good deeds are assimilated as moral credit and bad deeds as moral debit.

We review hereinafter a few studies that have explored the moral self-licensing effect. Sachdeva et al (2009) demonstrated how moral self-licensing could impact negatively pro-social preferences. In their experiment, participants who wrote a positive story about themselves were significantly less generous and keep more for themselves in a dictator game than those participants who just wrote a neutral story. In another study about green consumers, Mazar and Zong (2010) reported that consumers from a green store shared less money than consumers from a conventional store. Those consumers were also more likely to adopt dubious behaviour such as cheating and stealing. This study supports the existence of a self-regulation process, but also brings evidence that licensing may

lead up to moral transgressions. In the environmental domain, Panzone et al (2012) found that consumers are less sustainable in the food market once they have shown their environmental sensitivity in another domain. Tiefenbeck et al (2013) showed that residents who received weekly feedback on their water consumption lowered their water use (6% on average), but at the same time increased their electricity consumption by 5.6% compared with control subjects. In another vein, Clot et al (2013) studied the impact of financially rewarding good deeds on self-licensing. The authors found that prefacing the dictator game with an unpaid good deed licenses subsequent selfish behaviour, whereas a paid good deed dampens the effect. Their experiment was based on hypothetical good deeds, which further show that even good deeds that are imaginary have the power to license a decrease in social behaviour later on.

In parallel, anecdotal evidence illustrates further this moral paradox: a former director of a nonprofit psychotherapy organization stole more than $2.5 million from the group and used the money to pay off loans, credit cards and other expenses (Times - February 2, 2012), a property manager for a low-income co-op in the Bowery stole more than $260,000.00 from the building (Times - January 26, 2012), or even a Church worker who embezzled more than $1 million over seven years from the Roman Catholic Archdiocese (Times - January 30, 2012) , etc. Both experimental and experimental evidence support our main behavioural hypothesis, which is that *people who previously established their moral credentials are then more likely to act anti-socially*.

Interestingly, the nature of the good deed (whether it is for real or remains hypothetical) does not change substantially the licensing effect. Indeed, the literature review shows that licensing effect occurs both after a real good deed (e.g. buying green products) as well as after the 'hypothetical' establishment of moral rectitude (e.g. writing a positive story about oneself or doing an imaginary good deed). According to us, the impact of a hypothetical good deed deserves special attention since it brings direct implications on a real outcome (i.e. hypothetical good deed vs. real anti-social

behaviour). We therefore decide to focus on the impact of a good will statement (e.g. imaginary good deed) on subsequent social preferences.

Hypothesis: Given a situation of arbitration between private and public benefits, *stating* good intentions for 'others' (at the detriment of oneself) will push individuals to *be more anti-social* later on.

If the idea of a moral regulation process is not entirely new (i.e. transgression-compliance effect from Carlsmith and Gross, 1969), the recent expansion of the related literature (mostly in psychology and marketing) associated to an emerging consistency with economic model (Bénabou and Tirole, 2010), increases significantly the relevance of this effect for economic research. In line with these recent advances, we test to what extent moral self-licensing may encourage anti-social behaviour such as damaging a social surplus for private benefits. To capture an anti-social behaviour, we used an 'appropriation game'[31] (Cox et al., 2013), which closely relates to a dictator game except that it manipulates the endowment's allocation. The receiver has the money, and the dictator decides how much he takes from it. In our work, we implement a slightly different version of this game. In Bardsley (2008), each player is paired with a receiver played by an anonymous student. In our version, we have only one receiver, represented by a public institution. In sum, participants have the opportunity to take a part of an amount initially endowed to their belonging institution. A more detailed description of our design follows in the next section.

II.I.3. Experimental Design[32]

In our experiment, we address the question of how previous good deeds may license anti-social behaviour such as taking from a social surplus. Neither punishment nor repressive measures are part

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[31] Similar games could also be found in the literature under different labels such as the 'gangster game' (Eichenberger and Oberholzer-Gee, 1998) or 'taking game' (Bardsley,, 2008).

of the design to avoid the interference with risk issues. Moreover, we run a game that involved no strategic behaviour in terms of monetary outcome (participants individual payoffs are independent from other participants decisions). The game is designed to isolate the temptation to misappropriate a common fund for private benefits. We designed a two steps lab experiment for students, with one control and three treatments. The first step (for treatment groups) consists in a task that enables subjects to earn moral credit. The control group just has a neutral task (i.e. unscrambling sentences). In the second step, subjects are given the possibility to take an amount from a fund allocated to their University[33].

II.I.3.1. Qualitative background

Pretests were conducted in order to correctly design the first step of the experiment. We performed informal interviews to characterize 'good deeds' in Madagascar context and checked whether those "good deeds" really induce a moral boost (using a self-assessment scale).

First, two different good deeds have been selected through informal interviews. The first one refers to the University's environment. Indeed, the campus is settled on the hills of Antananarivo and enjoys very pleasant green settings. Each department's surroundings are most of the time well maintained, even though, there are always litters and papers flying around. Students are concerned by the look of their department and proposed the cleaning of their department's surroundings as a good deed. The second good deed that emerged from discussions consists in the creation of a guidebook dedicated to newly arrived students. In fact, many students are coming from rural areas and are therefore unfamiliar with the campus and its environment. They would highly benefit from a book hosting all kind of useful information about the campus.

Next, a pretest to validate the selection of those two good deeds was run on a focus group of 53 students from the University of Antananarivo. Based on a method used in Khan and Dhar (2006),

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[33] In Madagascar, a developing country, it is common to receive grants from institutions in the context of foreign aid.

measuring self-assessment, we asked subjects to indicate the extent to which they agree or disagree on a scale from 1 to 7 (1="Strongly disagree", 7="Strongly agree") with the following four statements "I am compassionate", "I am sympathetic", "I am generous" and "I am helpful". Khan and Dhar (2006) used these items because they exhibited a high degree of reliability in terms of coefficient alpha (Cronbach's α = 0.84). Students were randomly assigned to a license or a control condition. In the license condition, subjects were asked to imagine they engage into volunteering, choosing one of the two good deeds. Students in the control group just had to unscramble sentences. Results of the self-assessment scale show that participants evaluated themselves significantly more positively on the four attributes if they had imagined performing a good deed (license group) than if they had not (control group) (see table 1), regardless of the chosen good deed.

Table 1. Mean self-assessment in license and control conditions.

I am…	Licence (N=37)		Control (N=16)		Ranksum test[34]
Compassionate	5.216216	(0.2743683)	4.1875	(0.3442232)	z = -2.251, p = 0.0244
Sympathetic	5.459459	(0.264503)	4.375	(0.4643544)	z = -1.956, p = 0.0505
Generous	5.447368	(0.2376311)	4.3125	(0.3619248)	z = -2.382, p = 0.0172
Helpful	5.351351	(0.2853169	4.375	(0.4552929)	z = -1.860, p = 0.0628

Note: The standard error of the mean is in brackets.

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[34] Non parametrical Ranksum tests were used due to the small sample size.

II.I.3.2. Experimental procedure[35]

A total of 367 subjects from Antananarivo's University participate in the (paper-pencil) experiment[36].

All participants are unfamiliar with experimental games. We conduct 3 sessions. In all aspects, we

carefully follow identical procedures in each session based on a written protocol to minimize

context's biases (See the Appendix for detailed instructions). Participants are randomly assigned to 4

groups (one control and three treatments). Within each session, subjects are allowed to talk only to

administrators. There is 1 administrator for each 20 participants. Students are placed as in exam

conditions to ensure that decisions are made in private. Sessions are run among 3 different

departments of the university to minimize discussion and contamination between sessions.

Questionnaires are anonymous and identified by a unique number. The instructions state that there

are no right or wrong answers and that the experimenter aims to collect sincere answers. Also,

participant's professors were not involved in the experiments' administration to not influence

individuals' decisions.

Table 2. Game design

Groups	Stage 1	Stage 2	Stage 3	Money at stake (MGA)
Control	Neutral task			
License condition		Filler task	Appropriation game	30,000.00
Treatment 1	Good deed			
Treatment 2	Good deed, with option to refuse			
Treatment 3	Good deed, with option to select the level of participation			

The experiment design includes three stages, which are illustrated in table 2. In the first stage, the

purpose of treatments 1, 2 and 3 is to induce a moral boost ('moral self-license condition') by

referring to a situation where the participant is given the opportunity to establish a pro-social self-

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[35] See Appendix 3 for detailed instructions.

[36] We made sure that none of the students who had participated to either of the two focus groups took part in the experiment.

concept. In the control condition, this part is replaced by a dummy task, which involves unscrambling four sentences. Thanks to this procedure, the questionnaire length was similar among all groups, not to arouse suspicion among participants. This method was also used in Khan and Dhar (2006) who proved that unscrambling sentences do not affect individual's self-concept.

In the license condition, we asked participants to imagine they could volunteer to University service by either 1) 'cleaning buildings' surroundings' or 2) 'creating a guide book for new students'). All treatments propose the same volunteering activities. However, we introduced different options to reply. In treatment 1, subjects could choose between the two good deeds. In treatment 2, subjects could also opt out of the task (Khan and Dhar, 2006[37]). In treatment 3, subjects could select the time they want to dedicate to the good deed (1 hour, 2 hours, 3 hours, half day or a full day). Those three treatments have in common to include a prior licensing task, but they vary in the way subjects signal their good will.

In the second stage, subjects were asked to describe the impact the decision had on their selves (i.e. filler task). After completing stages 1 and 2, participants turned to the next sheet of the questionnaire. In stage 3, they are told that an institution grants money to their University for infrastructure improvement (no specific information is given on the total amount granted). Participants are told that they have the possibility to take money out of this public fund up to a certain amount. A choice table is given, specifying for each option, the distribution between the amount taken and what remains for the University. The instructions state in capital and bolds letters that decisions are real and confidential. It is also clearly mentioned that any sum taken would reduce from the same amount the grant intended to the University. Two checkboxes were proposed: "I do take in the common fund", followed by a free space to specify the amount (multiple of 1,000.00, up

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[37] Khan and Dhar (2006) used both manipulation options (with and without opt-out clause) in two different experiments. They found licensing effect in both experiments, but no direct comparison of the degree of licensing could be deducted.

to a limit of 30,000.00 MGA[38]), versus "I do not take in the common fund". After the time elapsed, questionnaires were collected and a lottery determined participants actually being paid. There was one winning number for every 10 participants. The amount at stake (30,000.00 MGA) is the equivalent of 15 meals (2,000.00 MGA is the average price for a meal proposed in one of the many popular restaurant located nearby the Campus).

II.I.3.3. Control variables

Demographic and more general questions were left to the end, in order not to influence the decisions during the experiment (referred as a stereotype bias; Lepore and Brown, 1997). Basic data on resources, gender, origin, and religion were collected. Also, additional questions assessing risk and positional attitudes were included to control for any potential cross effect with moral self-licensing.

Experimental literature suggests that women behave less selfishly than men (Engel, 2011; Eckel and Grossman, 1996) and are less competitive (Gneezy, 2003). Thus, we expect women to appropriate less money than men. We distinguished participant's origin in two different sub groups: those coming from the capital city Antananarivo and those coming from rural areas. In fact, coming from a rural area in general is associated with harder living conditions and lower incomes. We also expect people from rural area ('strangers') to be less concerned about the local public good and to have a greater willingness to take from the common pot (Habyarimana et al., 2007). Participants who are members of a religion that emphasizes the equality of human beings should play more fairly and cooperatively. Christianity has a strong emphasis on sharing, with certain Protestant movements being particularly critical of gathering wealth (Hayo and Vollan, 2012). Risk preferences and positional attitudes have been shown to affect cooperation as well (Bougherara et al., 2009, Solnick and Hemenway, 1998). We build an index for risk preferences based on two questions from the

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[38] MGA : Malagasy Ariary, the currency of Madagascar.

survey. In the first question, participants had to indicate which option they would prefer between a sure gain of 3,000.00 MGA (=0 point) and 80% of probability to earn 4,500.00 MGA (=1 point). In the second question, participants had to choose between a sure gain of 24,000.00 MGA (=0 point) and a 25% probability to earn 100,000.00 MGA (= 2 points). The index for risk preferences consists in adding points earned in both questions. The index varied from highly risk averse with 0 to risk taker with 3. To measure positional preference, we used a binary option, equal to one when participants indicated they would prefer having a lower monthly salary but higher than others' salary (500,000.00 MGA / 400,000.00 MGA) rather than a higher amount equal to others (600,000.00 MGA / 600,000.00 MGA).

II.I.4. Results

The characteristics of our data sample (of 367 subjects) are presented in table 3. The sample is well balanced across conditions, with an average of 51% male subjects, 21.64 years old. 36 subjects did not answer the appropriation game (the amount to take in the common pot). Results are based on the remaining 331 observations.

Table 3. Characteristics of participants (Percentage or mean)

	Control (n=175)	License (n=192)	Overall (n=367)
Age	21.63	21.64	21.64
Male	50%	52%	51%
Rural	27%	23%	25%
Ressource			
<50 000 Ar/month	45%	36%	41%
<100 000 Ar/month ; >50 000Ar/month	31%	36%	33%
<100 000 Ar/month	24%	28%	26%
Religion			
Catholic	40%	43%	42%
Protestant	55%	53%	54%
Others [39]	5%	4%	4%
Risk Preferences	0.642	0.646	0.644
Church attendance	4.2	3.6	3.9
Positional attitude	0.31	0.33	0.32

Finding 1: Anti-social behaviour increases under moral self-licensing.

Our first main result shows that participants in the moral self-licensing condition are more inclined to take in the common fund than participants in control. First, the number of participants who take money from the University's fund is greater in the moral self-licensing condition (81.66% versus 71.74% in the control group). Subjects who declare they would engage in a good deed for their University's department, are 9.92 points more inclined to take from the University's common fund subsequently. This difference is statistically significant using a binomial test (p|k|=0.00043). Second, among those participants who decide to take in the common fund, participants in the license group

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[39] Others includes: Muslims, Hindus and Jews.

take significantly more than participants from the control group (8.92 points more, t=-2.2308, p|t|=0.0266). The taken amount is on average 9,528.926 MGA (over the 30,000.00 available in the common fund) in the control group (SEM[40]= 906.2132) *versus* 13,838.1 MGA in the license group (SEM=704.3868), significantly different at 1% (t=-3.7304, p|t|=0.0002)[41]. Figures 1, 2 and 3 illustrate those findings.
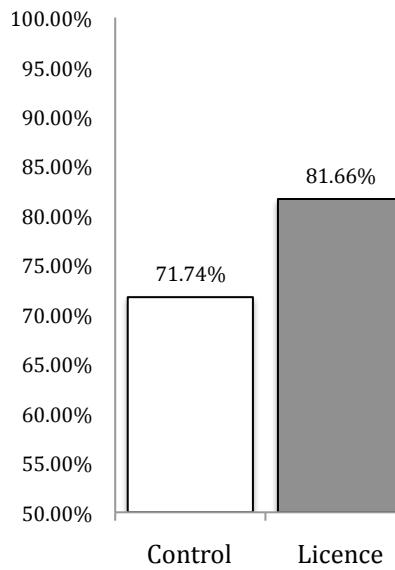
Figure 1. *Share of takers within the two scenarios.*
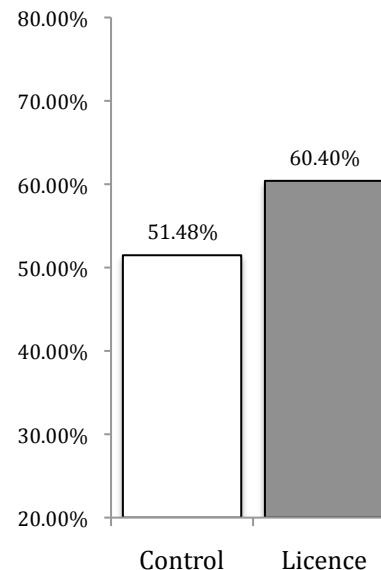
Figure 2. *Share of taken amount among takers within the two scenarios.*
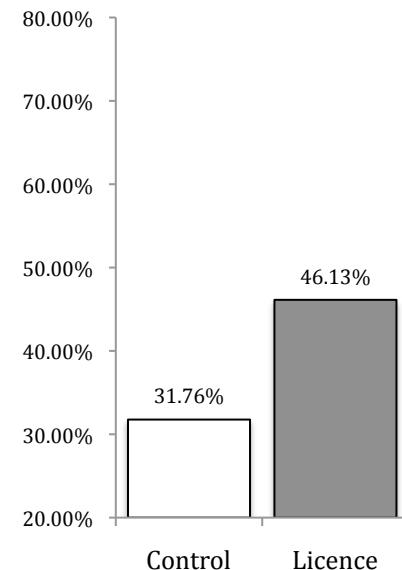
Figure 3. *Share of taken amount within the two scenarios.*

Altogether, 44% of respondents select the cleaning activity and 54.67% choose the guidebook. Only 3 subjects (over 76) decline the activity in the treatment with the opt-out clause. People who select the cleaning activity take on average 14,388.89, which is not statistically different from those who select the guidebook activity, who take on average 13,269.57 (Figure 4).

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[40] Standard Error of the Mean
[41] Among the 37 subjects randomly selected for payment, 4 had invalid forms (no answer in the appropriation game). The total amount at stake was distributed as follows: 353,000.00 MGA for the students and 757,000.00 MGA for the University.
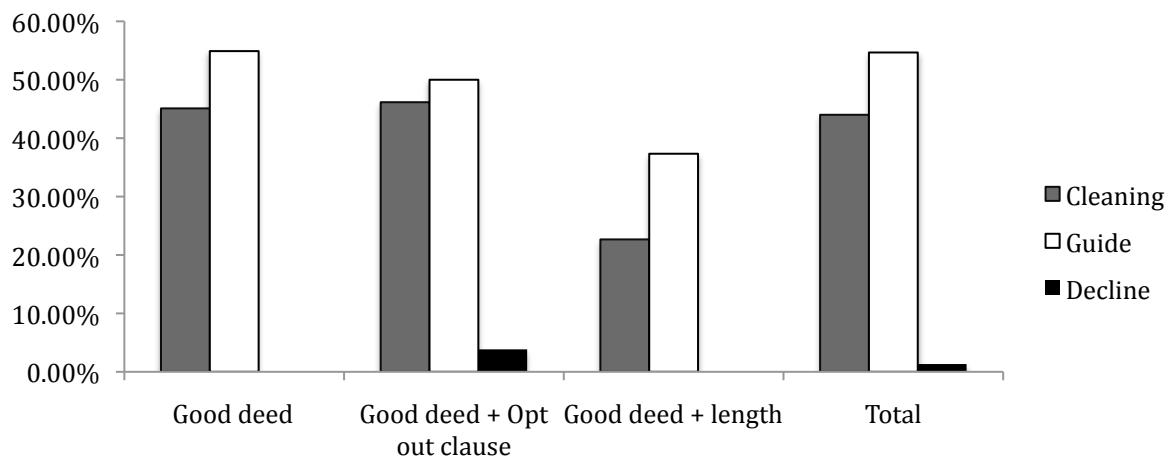
Figure 4. *Subjects' choice in stage 1 for treatments 1, 2 and 3.*

In the following paragraph, we discuss the way the good deed is implemented, and how it impacts subsequent anti-social behaviour.

Finding 2: Giving the possibility to opt-out volunteering has no impact on anti-social behaviour. However, low implication in the good deed leads to greater anti-social behaviour.

Looking at the different treatments designed to manipulate the licensing effect, we find that treatment 2 (good deed with opt out statement) and treatment 3 (good deed with free degree of implication) led to slightly greater licensing than treatment 2 (good deed with no options). Subjects in treatment 2 took approximately 1,000.00 MGA less than in the two other treatments, but this difference is not statistically significant. All treatments, taken one by one, are significantly different from control, using both parametric and non-parametric test. Tests are summarized in table 4. Figure 5 reviews the average taken amount for each treatment.
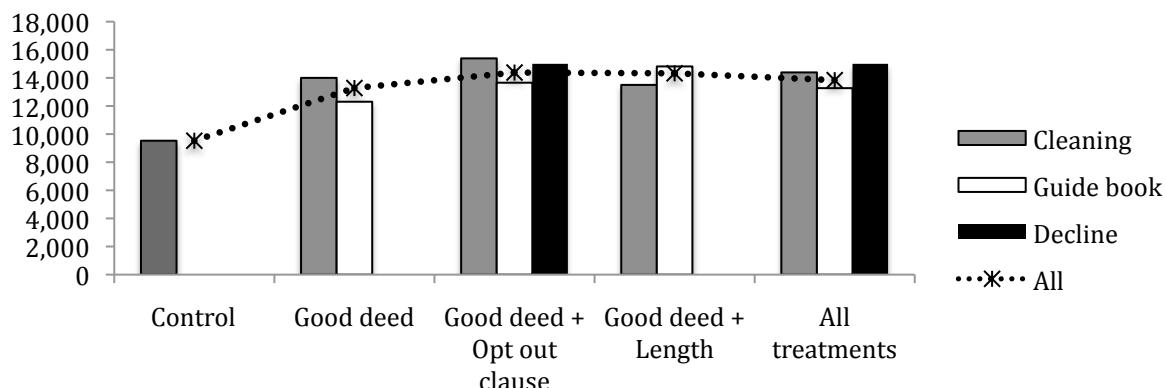
Figure 5. Mean of taken amount across Treatments 1, 2 & 3 and choices.

Table 4. *Difference in means between treatments 1, 2 & 3 and control (Student test and Wilcoxon test)*

Control n=175	T1 (Good deed), n=73	P-value		T2 (Good deed + Opt out clause), n=76	P-value		T3 (Good deed+ Length), n=43	P-value	
		t-test	wilcoxon		t-test	wilcoxon		t-test	wilcoxon
9528.926	13,277.23	0.0083	0.0145	14,378.79	0.0015	0.0006	14,325.58	0.0066	0.0031

Looking closer at treatment 3, we find that the degree of implication impacts significantly the moral self-licensing effect. Subjects that participate more than a half-day take 11,821 MGA, which is significantly less, at the 5% level[42] (z= 2.5482, p|z|=0.0147) than people who choose to participate less than a half-day, who appropriate 19,000 MGA. While participants who select less than a ½ day appropriate more than participants in control (z= 3.477, p|z|=0.0005), participants who select more than a ½ day do not take more than control (z=-1.422, p|z|=0.1549). Figure 6 shows the average amount taken, depending on the length dedicated to the University duty, corresponding to treatment 3.

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[42] 'Wilcoxon–Mann–Whitney' non-parametric test is preferred when at least one of the two samples involved in the comparison is below 60.
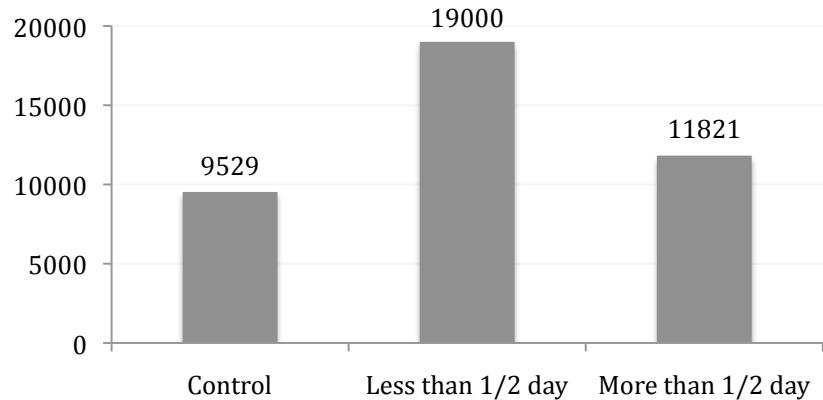
Figure 6. Average amount taken, by time of investment for the good deed.

Finding 3: Men behave more anti-socially than women. Moral self-licensing exists both for men and women, but is more pronounced for men (Figure 7).

Our data reveals that men appropriate significantly more than women. The average taken amount for men is 13,630.95 MGA versus 10,734.57 for women, significantly different at 1% (t=2.5790, p|t|=0.0103). The gender effect persists in treatments (15,336.45 vs. 12,107.84, t = 2.3181, p|t|=0.0214) but not in control (10,639.34 vs. 8,400.00, t=1.2383, p|t|=0.2181). We may conclude that men are more subject to moral self-licensing effect than women. Controlling for gender, licensing is present for both men and women. In fact, men took more in license condition than in control (15,336.45 vs. 10,639.34, t=-2.6971, p|t|=0.0077) as well as women (12,107.84 vs. 8,400.00, t=-2.5108, p|t|=0.0130). The gender effect persists even when controlling for risk preference in the regression (further discussed in the next paragraph) and a Spearman correlation test confirms that collinearity between those two variables can be rejected (p=0.8517).
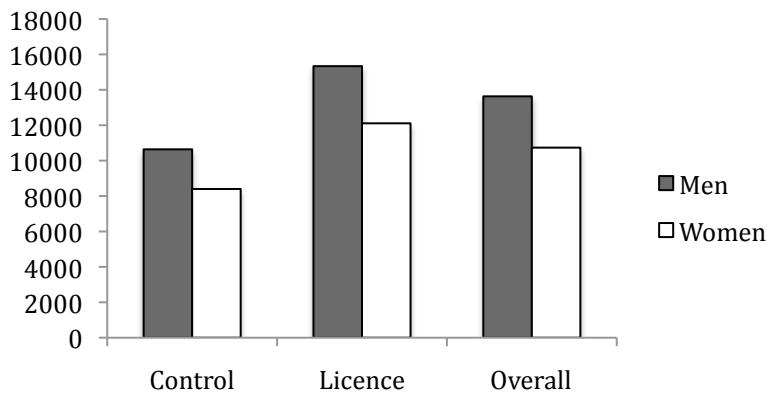
Figure 7. Average amount taken in control and license conditions, by gender.

We perform a set of regressions to get further understanding on the determinants of anti-social behaviour. First, we run a linear regression (table 5) of the taken amount (whole sample, control group, license group) and a probit model for the binary option 'take versus no take' (table 6).

Finding 4: Resource level, origin, religion, risk and positional preferences do not to affect anti-social behaviour.

Regression results of taken amount support our preceding findings. In this model, subjects in license condition take on average 4,076.00 MGA more than others in control with a probability below 1%. The regression also supports the existence of a gender effect with women taking on average 2,847.30 MGA less than men (p<5%). The regression does not support the existence of a relationship between taking behaviour and resource level, origin, religion, risk or positional preferences. Given that the licensing effect happens below the radar of consciousness, these results are not too surprising. It leads us to contend that licensing effect could be above all inherent to most individuals and might not rely on socio economic characteristics. The linear regression over the two sub samples (license and control conditions) supports the idea that gender effect plays a role in the license treatment but not in the control, with women taking on average 2,883.7 MGA less than men (p<5%) in the license condition.

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The probit analysis in table 6 supports that license condition plays a role in whether subjects take or not in the University's common fund. Being in the license group increases by a coefficient of 0.09 the probability of appropriating the money (p<5%). Gender has no effect on the tendency to take, meaning that gender affects rather the amount taken by subjects than the act of taking *per se*.

Table 5: OLS regression of taken amount

	(1)		(2)		(3)	
	All sample		Control sample		License sample	
License condition	4076.0***	(3.45)				
Gender	-2847.3**	(-2.50)	-2643.5	(-1.39)	-2883.7**	(-2.00)
Resource	120.2	(0.16)	90.11	(0.08)	125.8	(0.13)
Rural	-792.7	(-0.56)	-2497.0	(-1.07)	344.7	(0.19)
Catholic	983.1	(0.62)	3701.3	(1.30)	-704.8	(-0.36)
Protestant	979.6	(0.66)	929.4	(0.36)	649.7	(0.35)
Risk taker	2128.9	(1.37)	1672.4	(0.65)	2801.7	(1.42)
Positional seeker	1718.8	(1.50)	897.3	(0.47)	2340.9	(1.60)
Constant	9907.6***	(3.06)	10311.9**	(2.01)	13478.7***	(3.41)
Observations	331		163		168	
BIC	6869.5		2473.8		4419.7	
p	0.00311		0.574		0.190	

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Probit model (0=No take / 1=Take)

	(1)		(2)	
	Model 1		Model 2	
Licence condition	0.317**	(2.05)		
Gender	0.0962	(0.63)		
Licence condition*Gender			0.0978	(1.41)
Ressource	0.146	(1.46)	0.143	(1.44)
Rural	-0.0547	(-0.30)	-0.0643	(-0.35)
Catholic	0.0661	(0.30)	0.0750	(0.34)
Protestant	-0.0647	(-0.32)	-0.0764	(-0.38)
Risk taker	0.168	(0.76)	0.170	(0.77)
Positional seeker	0.0793	(0.51)	0.0789	(0.51)
Constant	-0.0277	(-0.06)	0.0640	(0.16)
Observations	331		331	
BIC	418.1		414.7	
chi2	8.120		5.601	
p	0.422		0.587	

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Another interesting finding of our experiment is related to the global amount of money remaining in the common fund. In our experiment, we found that people appropriate on average 40.88% (12,262.84 MGA), meaning that they leave or share 59.12%. When studying pro-social preferences, the most commonly used game is the dictator game, based on the measure of the willingness to donate to a receiver. Meta studies reports that people generally share 30% of the pie (Engel, 2010). Similar games have been played in developing countries, revealing equivalent results (individuals sharing 31% of the pie, Henrich et al., 2005). With people sharing more than half of the pie, the appropriation game seems to result in higher pro-social behaviour. Even if not directly comparable, such divergence between the two games indicates that manipulating framing could lead people to adopt different sharing rules.

II.I.5. Discussion and Conclusions

We have sought to illustrate that previous good deeds may license anti-social behaviour such as damaging a social surplus for private benefits in a developing country context. This work provides additional cues to understand why people may behave anti-socially and how social surplus creation might be threatened under some circumstances. Our results feed the debate on whether social preferences should be considered as exogenous or not. We clearly demonstrate through this experiment, that social preferences and moral behaviour also depend on external factors and could therefore vary over time as presented in the moral credit model.

While previous works on moral credit have shown that pro-social behaviour might be tempered due to the moral self-licensing effect, we provide evidence that it might also encourage anti-social

behaviour and contribute to explain social surplus decline. The licensing effect not only decreases the likeliness to be pro-social, but it also increases the likeliness to be anti-social. Having tested experimentally this effect in a developing country context, this work confers increased external validity to moral self-licensing behaviour, adding empirical evidence corroborating previous results found in the literature with WEIRD samples. Further evidence from the field would be an interesting extension of this work to dispel doubts on the 'education' and 'welfare' components of this effect.

More specifically, we find that the self-boost manipulation through the opt-out clause had no impact on further licensing effect, which suggests that a very simple signal is enough to induce subsequent anti-social behaviour. Meanwhile, the manipulation through the degree of engagement in the good deed seems to enlighten two types of behaviour: a low degree of engagement among people with a higher degree of moral self-licensing and a high degree of engagement among people with a lower degree of moral self-licensing.

Our results also reveal a gender effect as a second mediator of licensing. Men behave more anti-socially than women. Licensing effect is present for both, but seems more pronounced for men. Extra socio-economic variables such as resources, origin, religion, risk and positional preferences, that were part of the analysis, have shown no impact on the decision to cooperate nor on the licensing behaviour. To some extent, this confers to the licensing effect properties that make it robust to individual's socio economic attributes and somewhat generalizable to human's behaviour pattern.

Part II. Smug Alert! Exploring self-licensing behaviour in a cheating game.

II.II.1. Introduction

Prosocial and antisocial behaviours by the same individuals are ubiquitous in the real world. The same people perform good deeds such as helping others or preserving the environment while performing bad deeds such as lying and stealing. While prosocial behaviours have attracted a lot of attention among behavioural economists, antisocial behaviours are far less studied. Given that the same individuals are likely to perform the two kinds of behaviour, we examine whether performing a good deed can license individuals to cheat more. According to the moral model of the self-licensing theory, performing a good deed earns moral credits that reduce the discomfort of performing bad deeds afterwards, because they ensure a balanced moral account. In the moral credits model, good deeds establish moral credits like deposits in a bank account that can be 'withdrawn' to 'purchase' the right to perform bad deeds (Merritt et al., 2010; Krumm and Corning, 2008 ; Effron and Monin, 2010 ; Miller and Effron, 2010). For instance, Mazar and Zhong (2010) showed that U.S participants who chose to buy products from a green array were more likely to behave dishonestly in a subsequent task, presumably because green products establish enough moral capital to encourage moral transgressions. In an economic experiment, Branas-Garza et al. (2013) show that a deviation from a 'normal state of being' is balanced with a subsequent action that compensates for prior behaviour. Performing repeated one-shot dictator games, with subjects playing a series of giving decisions, the authors found that participants' donations in each round were negatively correlated to the amount they donated in the previous period. The authors therefore interpret donations over

time as the result of a pattern of self-regulation: moral licensing (being selfish after being generous) and cleansing (being generous after being selfish).

We use a between design with a cheating game to compare the level of cheating between the control group (no previous deed) and the group who just imagined performing a good deed. As far as we know our study is the first one to investigate the licensing effect in a developing country context. This extension of previous experiments is very important because it partially addresses the issue related to the use of Western, Educated, Industrialized, Rich, and Democratic (WEIRD) participants (Heinrich et al., 2010)[43]. Our experiment also allows to test whether the licensing effect remains robust to various experimental settings and to investigate the possible effect of other variables such as gender or origin on cheating. Indeed, literature about cheating reports inconsistent results about gender. In the meta-analysis of Ford and Richardson (1994), half studies found that women behave more ethically, while the other half found no significant relationship. Moreover, given that Madagascar has several different ethnic groups, we explore whether participants' origin plays a role in their ethical behaviour. Among the very few studies that have been looking at the relationship between ethics and ethnics' origin, Anderman et al. (1998) found no significant difference in cheating. However, minority students report more tolerance and consider that cheating is more acceptable than non-minority students.

The remainder of the paper is organized as follows. The next section exposes the empirical strategy. Section 3 provides the main results and discusses them. Section 4 concludes and suggests directions for further research.

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[43] Heinrich et al. (2010) argue that using samples drawn entirely from Western, Educated, Industrialized, Rich, and Democratic (WEIRD) societies to study human nature can be misleading.

II.II.2. Experimental design[44]

The study happened in Antananarivo's University, the capital and largest city of Madagascar. We modified a simple game designed by Fischbacher and Föllmi-Heusi (2013) where participants have ample opportunity to cheat without incurring any cost, except a moral cost if any. Participants are simply informed that they must roll a die in private, which will determine their payoff. The payoff equals 1,000.00, 2,000.00, 3,000.00, 4,000.00, 5,000.00 MGA for the corresponding die number rolled (respectively 1, 2, 3, 4, and 5) and zero if the number rolled is 6. To get an accurate idea of these amounts, the average price for a meal proposed in one of the many popular restaurants located nearby the campus is 2,000.00 MGA. Given that only the participant can observe the number that was actually rolled[45], participants have a clear interest to report 5, which corresponds to the highest gain. Even if we cannot observe cheating at the individual level, the comparison of the reported numbers' distributions to the theoretical distribution (each number has an odds to be rolled of 1/6) enables to measure cheating at the group level.

We use a between design with two treatments: one where participants just play the cheating game (control treatment) and another where participants just imagine that they have previously performed a good deed and play the cheating game (good deed treatment). Some preliminary work was necessary beforehand in order to determine what kind of activity University students would value as a "good deed" in a Malagasy cultural context.

The preliminary investigation was performed in two steps, involving students that were not part of our experimental sample. In the first step, informal discussions revealed that cleaning the University's green areas could be perceived as a good deed as well as writing a Campus' guidebook for new students. In a second step, a pretest to validate the selection of those two good deeds was run on a sample of 53 students. Following Khan and Dhar's (2006) method, we asked participants to indicate the extent to which they agree or disagree on a scale from 1 to 7 (1="Strongly disagree",

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[44] See Appendix 4 for detailed instructions.
[45] To ensure a high level of anonymity, each participant received an opaque plastic cup to roll the dice, with a small hole on the top in order to check for the outcome in private.

7="Strongly agree") with the following four statements "I am compassionate", "I am sympathetic", "I am generous" and "I am helpful". Khan and Dhar (2006) used these statements because they exhibited a high degree of reliability in terms of coefficient alpha (Cronbach's α = 0.84)[46]. Students were randomly assigned to a license or a control condition. In the license condition, subjects had to imagine volunteering, choosing one of the two good deeds. Results show that participants evaluated themselves significantly more positively on the four attributes in the license group than in the control group (see table 1) for the two good deeds. This preliminary test validates the selection of the two good deeds (i.e. cleaning the University's green areas and writing a Campus' guidebook for new students) as a way to boost individual's self-esteem.

Table 1 – Mean self-assessment in license and control condition with non-parametric Ranksum test[47] for statistical difference of means.

I am…	Licence (N=37)	Control (N=16)	Ranksum test
Compassionate	5.216216 (0.2743683)	4.1875 (0.3442232)	z = -2.251, p > |z| = 0.0244
Sympathetic	5.459459 (0.264503)	4.375 (0.4643544)	z = -1.956, p > |z| = 0.0505
Generous	5.447368 (0.2376311)	4.3125 (0.3619248)	z = -2.382, p > |z| = 0.0172
Helpful	5.351351 (0.2853169	4.375 (0.4552929)	z = -1.860, p > |z| = 0.0628

Standard error of the mean is in parenthesis.

The experiment was conducted with 100 students of the University of Antananarivo with 50 students in each treatment. The session took place during their lecture and the experiment was presented as a classroom activity at the end of the lecture[48]. Participants were not informed previously that they would participate in an experiment to avoid any selection bias. The experiment lasted about 20 minutes. Participants were not informed about the nature of the experiment or the treatment to which they would be assigned. Each student was paid anonymously at the end of the experiment. The average gain was 2,857.143 MGA. Participants' characteristics are provided in table 2. The

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[46] Statistical measure of internal consistency.
[47] Non parametrical Ranksum tests were used due to the sample size (N<60).
[48] Recent evidence supports that student subjects are an adequate pool for the study of social behavior (Exadaktylos el al 2013; Falk et al 2013).

sample appears to be well balanced across treatment and control, with no statistical differences between the characteristics of the two groups regarding gender, age, resources or origin.

Table 2 – Sample characteristics - Kruskal-Wallis H test of between group differences across participants assigned to control and treatment groups.

(N=100)	(1) Control group	(2) Treatment group	(3) H-statistics	(4) P-value
Socio-demographic characteristics				
Gender ratio (% of male)	60	52	0.643	0.4227
Age	21.4	21.32	0.014	0.9047
Monthly resources (%)				
< 50,000.00 Ar/month	42	58	0.000	1.000
> 50,000.00 Ar/month; < 100,000.00 Ar/month	50	26	0.000	1.000
> 100,000.00 Ar/month	8	16	0.000	1.000
Origin (% from Antananarivo)	74	80	0.503	0.4781

II.II.3. Results and discussion

We found that people in the licensing condition earns significantly more than in the control condition (22.21% more; p=0.086) as shown in Graph 1. In other words, by comparing theoretical gains with reported gains, we found that someone just imagining himself performing a good deed is more likely to cheat. Our results are consistent with the few available studies performed in Western countries where a WEIRD effect could be suspected (Mazar and Zhong, 2010). Strengthening this result, the share of participants reporting a dice's outcome equal to 5 (corresponding to the highest earning) is significantly higher in the license condition than in the control treatment (0.31 in license versus 0.15 in control - or 0.17 theoretically).

Looking at gender, interesting results arise. First, women appear to cheat more than men. Women won on average 24.30% more than men (t=-1.8932, p=0.0613). Our results are partly consistent with Wiltermuth (2011) who found experimental evidence that women cheat more than men when cheating benefits a third party, except that in our case cheating does not benefits to a third party. Second, licensing effect seems to be greater for men than for women. Women took 13.04% more in license condition than in control while men took 28.44% more. This last point could mean that self-licensing plays a greater role in cheating for men than for women. Nevertheless, it is premature to draw clear-cut conclusions on these crossed effects because of the sample size (N<60). Figures are presented in table 3.

Graph 1 - Earnings distribution among control and treatment
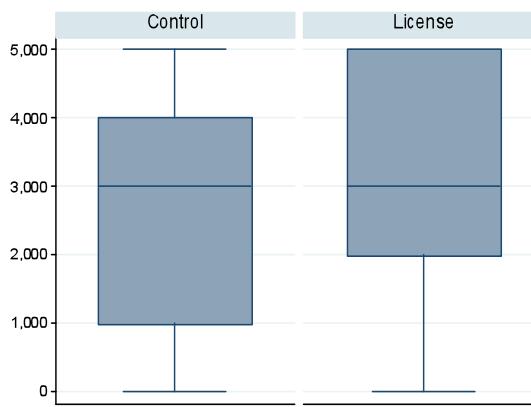
Graph 2 – Dice outcome among control and treatment
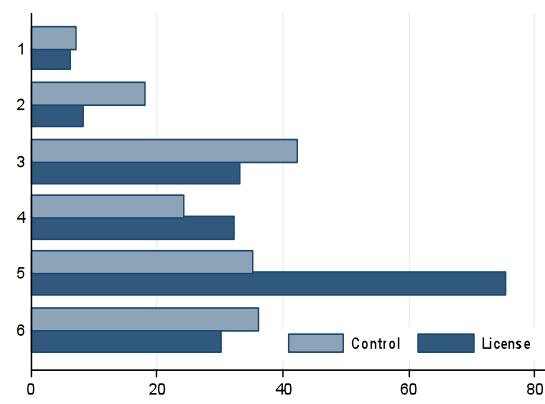
Graph 3 – Earnings distribution among control and treatment, by gender
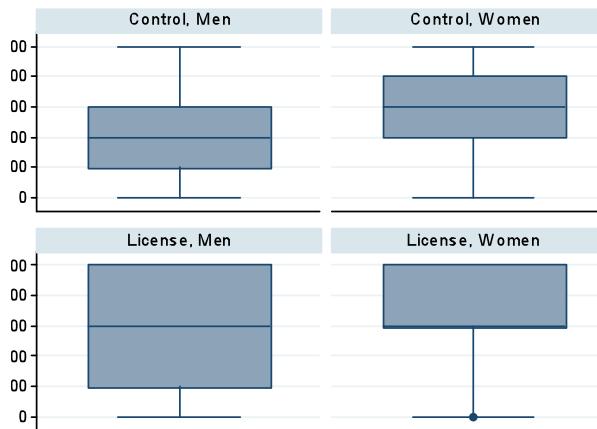
Graph 4 – Earnings distribution among control and treatment, by origin (From vs. Outside Antananarivo)
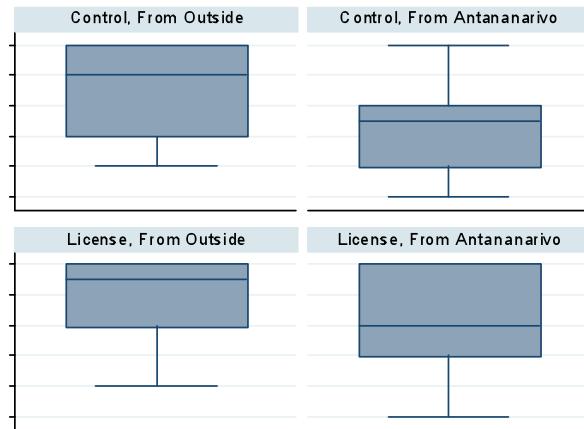
Another finding regarding 'outsiders' (people from provincial areas) deserves attention. As suggested in Graph 4, outsiders won 32.72% more than people from Antananarivo (t=-2.2544, p=0.0264). The effect persists significantly in the control group (2,305.556 vs. 3,307.692; t=2.0583, p=0.0451) but not in the license group (2,974.359 vs. 3,800.00; t=1.3766, p=0.1752). However, licensing effect appears to be more pronounced for people from Antananarivo (29.01% more; t=-1.8098, p=0.0744) than for outsiders (14.88% more; t=-0.7274, p= 0.4750). Table 3 summarizes those results. Again, these results must be considered with caution, given the sample size (N<60) and deserve further investigation.

Table 3 – Differences in earnings across treatment and control, for entire sample and sub samples.

(Student parametric test and Mann-Whitney non-parametric test[49].)

Comparisons	Average of earnings	Student test (t)	p-value	Mann-Whitney test (z)	p-value
Control vs. Treatment					
All sample (N=100)	2,571.429 / 3,142.857	-1.7321	0.0865	-1.846	0.0649
Proportion of high outcome (N=100)	0.28 / 0.48	-2.0842	0.0397	-2.050	0.0404
Men (N=55)	2,275.862 / 2,923.077	-1.3474	0.1836	-1.336	0.1817
Women (N=43)	3,000.000 / 3,391.304	-0.9192	0.3633	-1.081	0.2797
Antananarivo (N=75)	2,305.556 / 2,974.359	-1.8098	0.0744	-1.895-	0.0582
Outside (N=23))	3,307.692/ 3,800.000	-0.7274	0.4750	-0.841	0.4003
Men vs. Women					
Control (N=50)	2,275.862 / 3,000.000	-1.6300	0.1098	-1.618	0.1056
Licence (N=50)	2,923.077 / 3,391.304	-0.9569	0.3435	-0.718	0.4725
Antananarivo vs. Outside					
Control (N=50)	2,305.556 / 3,307.692	2.0583	0.0451	1.952	0.0510
Licence (N=50)	2,974.359 / 3,800.000	1.3766	0.1752	1.500	0.1337

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[49] Both parametric and non-parametric tests are used due to the critical sample's size.

II.II.4. Conclusions

This work corroborates existing literature on licensing effect. Further, we found support that hypothetical good deeds can license cheating. Not only the license effect might decrease the likelihood to act morally, but it might also increase the likelihood to act immorally. Consequently, good deeds are not always good news because they can lead people to less virtuous behaviours.

In addition, our experiment adds empirical evidence in a developing country context that good deeds can create moral credits that license people to perform later immoral behaviour such as lying or cheating.

A remaining and crucial issue is what circumstances make previous good deeds conducive to moral consistency or to moral compensation. Recent research indicates that people with a rule-based mindset or with an abstract mindset (e.g. recalling a past deed in the distant past) are more likely to exhibit behavioural consistency while people with an outcome-based mindset or with a concrete mindset (e.g. recalling a past deed in the recent past) are more likely to exhibit compensatory behaviour (Conway et Peetz, 2013; Cornelissen et al., 2013). Nevertheless more research is needed to support these findings. Along the same line, it would be useful to explore the reverse relationship, that is whether performing a bad deed makes people more likely to behave honestly or to perform good deeds.

Discussion

The four experiments developed in this first chapter converge to support the occurrence of a moral licensing effect under different circumstances: establishing goodwill through a hypothetical virtuous statement offers one the excuse to decrease pro social attitude later on.

We discuss below main outcomes and implications.

May we dampen the effect? Moral licensing mediators in a policy perspective for behavioural change

Our work reveals that moral licensing effect holds at least two relevant mediators for policy makers: the way the good deed is generated, and whether the good deed is paid or not.

We learned that the presence of intrinsic motivation and the way the virtuous act is generated (regulation vs. no regulation) are two important conditions explaining the occurrence of the licensing effect. Moral licensing is more likely to arise when the preceding 'virtuous' act is freely chosen (respectively regulatory caused) for non-intrinsically (respectively intrinsically) motivated individuals.

In addition, we found that offering financial rewards for a good deed dampens the licensing effect. Engaging in an unpaid good deed encourages purely selfish behaviour (i.e. null donation), but has little effect in determining the value of positive donations, whereas paid good deeds are less likely to induce self-licensing.

To what extent might moral licensing backfire? Moral licensing and immoral consequences

While previous works on moral licensing have shown that pro-social behaviour might be tempered due to the moral licensing effect, we provide evidence that it might also encourage both anti-social behaviour (misappropriating a common fund for private benefit) and transgressive behaviour (cheating). Not only the moral licensing effect might decrease the likelihood to act morally, but it might also increase the likelihood to act immorally.

More specifically, our results suggest that even a low signal is enough to induce subsequent anti-social behaviour: a low degree of implication in a hypothetical virtuous act corresponds to a higher level of misappropriation.

Gender considerations

Our results in terms of pro social behaviour are in line with existing literature: on average, women behave more pro socially than men (they give more in the dictator games and take less in the appropriation game).

Regarding interactions between moral licensing and financial rewards, men and women appear to react in opposite ways: whereas payments for a good deed tend to dampen the licensing effect among women, the effect is strengthened among men.

When analyzing moral licensing's scope, in the second part of this chapter, we found that men cheated and misappropriated more under moral licensing condition than women. Moral licensing is present for both, but appears to be more pronounced for men.

External validity & Robustness

This work confers increased external validity to moral licensing behaviour, adding empirical evidence corroborating previous results found in the literature with WEIRD samples. Further evidence from the field would be an interesting extension of this work to dispel doubts on the 'education' and 'welfare' components of this effect. In addition, testing moral licensing in real life situation instead of hypothetical condition would be of great interest for robustness purpose (cf. Chapter II).

Chapter I provides important insights about moral licensing. The selected methodology (lab-type experiments in classroom with random pay in 3 out of 4 of these experiments) is due to the exploratory nature of the research question. Such 'paper-pencil' experiments with random pay solve the logistical challenge faced by traditional laboratory experiments. In the same time, the incitation scheme might raise some concerns about results reliability since not all participants are paid. This methodological issue is further discussed in Chapter III.

We added empirical evidence through these experiments, that social preferences and moral behaviour also depend on external factors and could therefore vary over time. Moral licensing introduces dynamic considerations in the theory of social preferences. An important implication of our findings is to call for extreme care when using given behavioural approaches to incentivize individuals to act pro-environmentally. They could help achieving significant impacts at lesser cost but are not immune to counter-productive effects.

An interesting path for future work is to explore moral licensing generalization to individuals. Recent researches suggest that people with a rule-based mindset or with an abstract mindset (e.g. recalling

a past deed in the distant past) are more likely to exhibit behavioural consistency while people with an outcome-based mindset or with a concrete mindset (e.g. recalling a past deed in the recent past) are more likely to exhibit compensatory behaviour (Conway et Peetz, 2013; Cornelissen et al., 2013).

Mechanisms driving moral compensation remain another open question. Are individuals reacting to a 'Halo effect' (i.e., as in the moral credential model: the good deed impact individual's perception on the subsequent task) or are they in a moral credit model where good and bad deeds are substituted as in a self-regulation mechanism (i.e., moral credit model)?

Also, moral compensation could be embedded in a more general homeostasis frame. Indeed, beside the moral factor, some of the evidences include a risky component (i.e., increasing smoking while taking dietary supplements) as well as an economic component (i.e., being less charitable to compensate the time spent for a good cause). Risk homeostasis, a concept mainly developed by psychologists, refers to compensation in risky decisions, or a situation such as an increase in safety measures would lead to riskier attitudes. For instance, a study shows that drivers give less room to cyclists wearing helmets (Walker, 2006). An other study, compiling 3 years of cabs' data in Munich concluded that cabs with ABS have higher crash rates and take more risks, assuming that ABS would take care of them (Wilde, 1994).

Overall, this work raises numerous questions and calls for further research. It is crucial to get better understanding of such compensatory mechanisms. If not taken into account, the pre-cited negative adaptive behaviours may directly impact policies and contract's efficiency.

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Appendix

Appendix 1

Instructions

The following instructions were originally written in French. We document the instructions we used in an experiment involving 30 participants. The instructions in the other experiments are exactly the same except that they were adapted according to the number of participants (e.g. One prize of 30€ was given for each group of 30 participants. So if we had 60 participants, we gave two prizes, 90 participants, 3 prizes and so forth).

1. General instructions – First step

"You are now taking part in a survey referring to environmental issues. The envelopes you have been given contain the questionnaires. Please, wait for our signal before opening those envelopes. This survey will last about 5 minutes in total. Questionnaires are anonymous. At the end of the survey, one of you will win up to 30€. Drawing lots will designate the winner. Later on, when you will open your envelopes, you will see a numbered tag stapled to your questionnaire. You should leave it, because it will be used for the drawing lots. Your envelopes are numbered. You must keep them up to the end of the survey in order to collect your prize if your get the winning number. If you have any doubts or questions, please ask one of the experimenters. But we remind you that you must keep silent for the whole duration of the survey. Anyone that will not keep silent will be excluded from the drawing lots. Please fill in those questionnaires as accurately as possible. Notice that questionnaires not properly filled in will not be part of the drawing lots."

2. General instructions – Second step

"You can open your envelope. You have 1 minute and 30 seconds from now to fill in your questionnaire."

2.1 Instructions for control's sample

Anonymous questionnaire N° X.
(We are only interested in your opinion. There is no right or wrong answer.
Thanks to answer <u>as sincerely as possible.</u>

   A. Please provide the following information:

1. **Age** : _____ 2. **Sexe** : M. ☐ W. ☐	3. **Your monthly resources** : a) Less than 500€/month ☐ b) Between 500 and 1000 €/month ☐ c) Between 1000 and 1500 €/month ☐ d) More than 1500 € /month ☐

   B. After the survey, one of you will win 30€ (by drawing lots). If you are the winner, which part of this amount would you like to give to a programme involved in environmental conservation? (*Please indicate the amont of your choice*)
-   Amount (From 0 to 30 €) for the programme : [_____] €

The winner will be designated by drawing lots after the survey. An envelope containing the amount that corresponds to the participant's decision will be available by the professor at the end of the lecture.

The envelope's content, as well as collected data, will remain strictly confidential.

2.2 Instructions for treatment 1

Anonymous questionnaire N° X.
(We are only interested in your opinion. There is no right or wrong answer.
Thanks to answer <u>as sincerely as possible.</u>

   A. Please provide the following information:

1. **Age** : _____ 2. **Sexe** : M. ☐ W. ☐	3. **Your monthly resources** : a) Less than 500€/month ☐ b) Between 500 and 1000 €/month ☐ c) Between 1000 and 1500 €/month ☐ d) More than 1500 € /month ☐

   B. Your University decides to settle a compulsory pro environmental program in which you have to get involved one hour per week during a month.

On a 1 to 9 scale, select the satisfaction level that best describes yours after that decision:

 − (Not at all satisfied)   (Very satisfied) **✚**

1	2	3	4	5	6	7	8	9

   C. After the survey, one of you will win 30€ (by drawing lots). If you are the winner, which part of this amount would you like to give to a programme involved in environmental conservation? (*Please indicate the amont of your choice*)
-   Amount (From 0 to 30 €) for the programme : [_____] €

The winner will be designated by drawing lots after the survey. An envelope containing the amount that corresponds to the participant's decision will be available by the professor at the end of the lecture.

The enveloppe's content as well as the collected data will remain strictly confidential.

2.3 Instructions for treatment 2

Anonymous questionnaire N° X.
(We are only interested in your opinion. There is no right or wrong answer.
Thanks to answer <u>as sincerely as possible.</u>

A. Please provide the following information :

1. **Age** : _____ 2. **Sexe** : M. ☐ W. ☐	3. **Your monthly resources** : a) Less than 500€/month ☐ b) Between 500 and 1000 €/month ☐ c) Between 1000 and 1500 €/month ☐ d) More than 1500 € /month ☐

B. You have the opportunity to get involved in a pro environmental program one hour per week during a month

   a. Do you wish to engage? ?

      1. ☐ Yes   2. ☐ No

   b. On a 1 to 9 scale, select the satisfaction level that best describes yours after that decision :

− (Not satisfied at all)   (Very satisfied) **✚**

1	2	3	4	5	6	7	8	9

C. After the survey, one of you will win 30€ (by drawing lots). If you are the winner, which part of this amount would you like to give to a programme involved in environmental conservation? (*Please indicate the amont of your choice*)

- Amount (From 0 to 30 €) for the programme : [_____] €

The winner will be designated by drawing lots after the survey. An envelope containing the amount that corresponds to the participant's decision will be available by the professor at the end of the lecture.
The envelope's content as well as the collected data will remain strictly confidential.

3. General instructions – third step

« Time is over. We will now get back the questionnaires. Keep you envelope up to the drawing lots. We thank in advance the winner to keep silent in order to maintain the anonymous conditions of the survey. The winner is number xxx. . The winner may collect his/her prize by the professor at the end of the lecture. Thanks for your participation.»

Appendix 2

A. First step

The following instructions were originally written in French.

"You are now taking part in a survey referring to environmental issues. The envelopes you have been given contain the questionnaires. Please, wait for our signal before opening those envelopes. This survey will last about 2 minutes in total. Questionnaires are anonymous. At the end of the survey, you may win up to 30€. Drawing lots will designate the winner. There will be 1 winner every 30 participants. Later on, when you will open your envelopes, you will see a numbered tag stapled to your questionnaire. You should leave it, because it will be used for the drawing lots. Your envelopes are numbered. You must keep them up to the end of the survey in order to collect your prize if your get the winning number. If you have any doubts or questions, please ask one of the experimenters. But we remind you that you must keep silent for the whole duration of the survey. Anyone that will not keep silent will be excluded from the drawing lots. Please fill in those questionnaires as accurately as possible. Notice that questionnaires not properly filled in will not be part of the drawing lots. You can open your envelope. You have 2 minutes from now to fill in your questionnaire."

B. Second step

- Answering form - Control

Anonymous questionnaire N° X.

(We are only interested in your opinion. There is no right or wrong answer.
Thanks to answer as sincerely as possible.)

---

*In the part below, earnings are **real**.*

A. After the survey, one of you will win 30€ (by drawing lots). If you are the winner, would you like to give to a programme involved in environmental conservation? (If yes, please choose the programme and indicate the amount of your choice). Remember that you gain, if you get a winning number, will correspond to the 30 € minus the amount you chose to give to the programme.

WWF ☐   Greenpeace ☐

- Amount (From 0 to 30 €) for the programme : [_____] €.

➔ If you win at the drawing lot, you will receive your gain in an envelope. Your gain will be the 30€ minus the amount you gave to

the programme, which means (*please fill in the blank cells bellow*):

30€ - [_____] € (*Amount for the programme*) =   [_____] € (*Your gain*).

B. Please provide the following information :

1. **Age** : _____	3. **Your monthly resources** :	
	a) Less than 500€/month ☐	b) Between 500 and 1000 €/month ☐
2. **Sexe** : M. ☐ W. ☐	c) Between 1000 and 1500 €/month ☐	d) More than 1500 € /month ☐

The winner will be designated by drawing lots after the survey. An envelope containing the amount that corresponds to the participant's decision will be available by the professor at the end of the lecture.

The envelope's content as well as the collected data will remain strictly confidential.

---

2) Answering form - Treatment 1

Anonymous questionnaire N° X.

(We are only interested in your opinion. There is no right or wrong answer.
Thanks to answer as sincerely as possible.)

A. Suppose that an environmental organisation set up an activity to clean up river banks in your leaving area for a period of one month. A call for participation is opened and 20 litters bags can be obtained to that end. How many bags would you wish to fill in over this month time ?

Number of bags (From 0 to 20) : [_____] bag(s).

B. If you decided to participate in this activity, how do you feel? Could you describe your state of mind using three adjectives of your choice ?

1._____ 2._____ 3._____

---

*In the part below, earnings are **real**.*

C. After the survey, one of you will win 30€ (by drawing lots). If you are the winner, would you like to give to a programme involved in environmental conservation? (*If yes, please choose the programme and indicate the amount of your choice*). Remember that you gain, if you get a winning number, will correspond to the 30 € *minus the amount you chose to give to the programme.*

WWF [ ]   Greenpeace [ ]

Amount (From 0 to 30 €) for the programme : [_____] €.

➔ If you win at the drawing lot, you will receive your gain in an envelope. Your gain will be the 30€ minus the amount you gave to the programme, which means (*please fill in the blank cells bellow*):

30€ - [_____] € (*Amount for the programme*) =   [_____] € (*Your gain*).

D. Please provide the following information :

1. **Age** : _____	3. **Your monthly resources** :
2. **Sexe** : M. [ ] W. [ ]	a) Less than 500€/month [ ] b) Between 500 and 1000 €/month [ ] c) Between 1000 and 1500 €/month [ ] d) More than 1500 € /month [ ]

The winner will be designated by drawing lots after the survey. An envelope containing the amount that corresponds to the participant's decision will be available by the professor at the end of the lecture.

The envelope's content as well as the collected data will remain strictly confidential.

3) Answering form - Treatment 2

Anonymous questionnaire N° X.

(We are only interested in your opinion. There is no right or wrong answer.
Thanks to answer as sincerely as possible.)

---

*In the part below, earnings are **hypothetical**.*

A. Suppose that an environmental organisation set up an activity to clean up river banks in your leaving area for a period of one month. A call for participation is opened and 20 litters bags can be obtained to that end. Participants are paid 2 € per bag. How many bags would you wish to fill in over this month time ?

Number of bags (From 0 to 20) : [_____] bag(s).

➔ It means the following payment for you:

[_____] bag(s) (number of bags) x 2 € = [_____] € (Payments you get after the activity).

B. If you decided to participate in this activity, how do you feel? Could you describe your state of mind using three adjectives of your choice?

1._____   2._____   3._____

---

*In the part below, earnings are **real**.*

C. After the survey, one of you will win 30€ (by drawing lots). If you are the winner, would you like to give to a programme involved in environmental conservation? (*If yes, please choose the programme and indicate the amount of your choice*). Remember that you gain, if you get a winning number, will correspond to the 30 € *minus the amount you chose to give to the programme.*

WWF ☐   Greenpeace ☐

Amount (From 0 to 30 €) for the programme : [_____] €.

➔ **If you win at the drawing lot, you will receive your gain in an envelope**. Your gain will be the 30€ minus the amount you gave to the programme, which means (*please fill in the blank cells bellow*):

30€ - [_____] € (*Amount for the programme*) =   [_____] € (*Your gain*).

D. Please provide the following information :

1. **Age** : _____ 2. **Sexe** : M. ☐ W. ☐	3. **Your monthly resources** : a) Less than 500€/month ☐ b) Between 500 and 1000 €/month ☐ c) Between 1000 and 1500 €/month ☐ d) More than 1500 € /month ☐

The winner will be designated by drawing lots after the survey. An envelope containing the amount that corresponds to the participant's decision will be available by the professor at the end of the lecture.

The envelope's content as well as the collected data will remain strictly confidential.

C. Third step

"Time is over. We will now get back the questionnaires. Keep you envelope up to the drawing lots. We thank in advance the winner to keep silent in order to maintain the anonymous conditions of the survey. The winner is number xxx. . The winner may collect his/her prize by the professor at the end of the lecture. Thanks for your participation."

Appendix 3

JOUEUR A

I/ [INTRO]

Bonjour. Vous allez participer à un projet d'étude dans le cadre d'une réalisation de mémoires universitaires. Notez bien que les questionnaires sont entièrement anonymes. Ils sont juste identifiés par un numéro unique. Nous vous remercions par avance de répondre le plus sincèrement possible.

Cette étude va durer une heure environ. Pendant une heure, nous allons vous demander de remplir une série de questionnaires concernant la prise de décision ainsi que vos perceptions vis à vis de l'environnement.

Les questionnaires que nous allons vous distribuer sont à usage personnel. ***Il est interdit à partir de maintenant de communiquer avec les autres participants pendant toute la durée de l'enquête. L'enquête doit se dérouler dans le silence total. Toute personne qui ne respectera pas ce silence ne pourra plus participer.***

Préparer vos stylos, et vous pouvez répondre en malgache.

Nous allons à présent commencer avec le premier questionnaire. Vous verrez que dans ce questionnaire nous faisons référence à une désignation au hasard et à une somme d'argent. Nous souhaitons vous confirmer que c'est bien réel. En effet, à la fin de cette séance 5 personnes seront tirées au hasard. Ces 5 personnes resteront anonymes. Leurs décisions seront réellement appliquées.

Vous pouvez poser des questions s'il y a des phrases incompréhensibles.

Distribution des questionnaires.

Vous disposez de 15 minutes.

Les 15 minutes sont écoulées.

Nous allons à présent ramasser les premiers questionnaires. La désignation au hasard aura lieu à la fin de la séance. Détacher votre numéro qui se trouve sur les questionnaires car ceci vous permettra de collecter votre enveloppe à la fin du jeu avec votre argent si vous êtes la personne désignée. Merci de garder le silence jusqu'au bout de la séance.

Pendant ce temps, un autre expérimentateur distribue aux joueurs A les seconds questionnaires.

Voici à présent un deuxième questionnaire. (Distribuer le questionnaire socio environnemental aux joueurs). N'oubliez surtout pas de bien indiquer votre numéro, un emplacement est prévu sur chaque page du questionnaire. Merci de garder le silence et de faire appel à nous uniquement, si vous avez des questions de compréhension.

Vous disposez de 40 minutes.

Les 40 minutes sont écoulées.

Nous allons ramasser les deuxièmes questionnaires.

Nous allons à présent déterminer les 5 personnes désignées. Pour cela, nous allons tirer au hasard 5 numéros. Nous remercions par avance les personnes désignées de garder le silence afin de respecter les conditions d'anonymat de cette étude.

Il s'agit des numéros xxxxx.

Les personnes possédant ces numéros pourront venir chercher leurs enveloppes au secrétariat du C3EDM du département économie de la Faculté DEGS à partir de 16 h à 17h.

Veuillez répondre le plus précisément possible aux questions. **Il n'y a pas de réponses vraies ou fausses.** Seul votre avis nous intéresse.

Ce questionnaire est <u>STRICTEMENT ANONYME</u> - Merci de ne pas communiquer entre vous.

1^{ère}Partie.

Imaginez la situation suivante :

A/ Pendant la journée de votre département, vous avez la possibilité de faire une bonne action en sa faveur. Deux actions différentes vous sont proposées.

1. La première consiste à faire un grand nettoyage de l'entourage des bâtiments, afin d'améliorer l'image de votre département.

2. La deuxième action consiste à faire un guide pour les nouveaux étudiants afin de faciliter leur intégration dans votre département.

Au lieu de prendre cette journée pour vous détendre, vous choisissez donc de faire une bonne action en faveur de votre département. A laquelle de ces deux activités en faveur de votre département choisissez-vous de participer?

1. Je participe au grand nettoyage de l'entourage ☐

2. Je participe au guide pour les nouveaux étudiants ☐

B/ Combien de temps consacrerez-vous à effectuer la bonne action de votre choix ? *(cochez la case correspondante à votre choix)*

Durée	1h	2h	3h	Demi-journée	Toute la journée
Choix					

Les deux activités sont une réussite et le chef du département est très reconnaissant.

2ème Partie

Pouvez-vous exprimer en quelques phrases ce que vous ressentez suite à votre participation à cette activité ? *(Vous pouvez répondre en malgache)*

……

……

……

……

……

……

……

_____

1 – P/JA/I/ T1

3ème Partie

Indiquez sur une échelle de 1 (=pas du tout) à 7(= très) comment vous vous trouvez : *(Cochez la case correspondante à votre choix)*

	- (Pas du tout)						+ (Très)
	1	2	3	4	5	6	7
Je suis sympathique							
Je suis généreux							
Je suis serviable							
Je suis compatissant (= be fiantrana)							

4ᵉᵐᵉPartie

ATTENTION, DANS CETTE PARTIE, IL S'AGIT D'UNE DECISION REELLE. CETTE DECISION SERA EFFECTIVEMENT REALISEE A LA FIN DE L'ENQUETE POUR CEUX D'ENTRE VOUS QUI SERONT DESIGNES AU HASARD.

A travers un projet de réhabilitation, un organisme met à disposition de l'Université une cagnotte d'un certain montant. L'Université disposant de cette cagnotte va investir dans l'amélioration des infrastructures.

A la fin de ce questionnaire quinze numéros vont être désignés au hasard. Les personnes qui possèdent les numéros désignés auront le droit de prélever dans cette cagnotte (dans la limite de 30 000 Ariary chacun). Les sommes prélevées réduiront le montant de la cagnotte destinée à l'Université.

LES DECISIONS DES PERSONNES DESIGNEES RESTERONT ENTIEREMENT ANONYMES. LES ORGANISATEURS GARANTISSENT LA CONFIDENTIALITE DES RESULTATS. NOUS REMERCIONS PAR AVANCE LES PERSONNES POSSEDANT LES NUMEROS QUI SERONT DESIGNES A LA FIN DE LA SEANCE DE GARDER LE SILENCE AFIN D'ASSURER LES CONDITIONS D'ANONYMAT DE L'ENQUETE. LES SOMMES SERONT REMISES EN PRIVE DANS UNE ENVELOPPE FERMEE CONTRE PRESENTATION DU NUMERO AU SECRETARIAT DU C3EDM.

Votre décision :

=> Si vous êtes désigné, que faites-vous? **NOTEZ BIEN QUE CETTE DECISION EST REELLE ET SERA EFFECTIVEMENT APPLIQUEE.**

Cochez la case correspondant à votre choix.

☐ Je prélève dans la cagnotte.
Si oui, combien? Montant : _____ (Multiple de 1000, de 0 à 30 000 Ariary ; *cf : tableau ci-dessous*)

Pour moi	1000Ar	2000Ar	3000Ar	4000Ar	5000Ar	6000Ar	7000Ar	8000Ar	9000Ar	10000Ar
Pour la cagnotte	29000Ar	28000Ar	27000Ar	26000Ar	25000Ar	24000Ar	23000Ar	22000Ar	21000Ar	20000Ar

Pour moi	11000Ar	12000Ar	13000Ar	14000Ar	15000Ar	16000Ar	17000Ar	18000Ar	19000Ar	20000Ar
Pour la cagnotte	19000Ar	18000Ar	17000Ar	16000Ar	15000Ar	14000Ar	13000Ar	12000Ar	11000Ar	10000Ar

Pour moi	21000Ar	22000Ar	23000Ar	24000Ar	25000Ar	26000Ar	27000Ar	28000Ar	29000Ar	30000Ar
Pour la cagnotte	9000Ar	8000Ar	7000Ar	6000Ar	5000Ar	4000Ar	3000Ar	2000Ar	1000Ar	0Ar

☐ Je ne prélève pas dans la cagnotte.

****

Merci d'avoir répondu à ce questionnaire. Les personnes possédant les numéros désignés pourront retirer leurs gains contre présentation de leurs numéros au secrétariat du C3EDM.

2 – P/JA/I/ T1

1ère Partie

Veuillez mettre dans l'ordre les mots suivants afin de former des phrases :

1. débuté grande 2012 La de a le 31 au Palais mai Sports braderie Madagascar des.

……

……

2. La d'Exposition de ou FIM a le 31 Madagascar mai 2012 au Internationale Centre Futura Foire débuté.

……

……

3. à totale de est Madagascar 20 environs population millions La estimée d'habitants.

……

……

4. Madagascar nationale est compagnie aérienne la Air.

……

……

2ème Partie

Indiquez sur une échelle de 1 (=pas du tout) à 7(= très) comment vous vous trouvez : *(Cochez la case correspondante à votre choix)*

	− (Pas du tout) ——————————————————→ **+** (Très)						
	1	2	3	4	5	6	7
Je suis sympathique							
Je suis généreux							
Je suis serviable							
Je suis compatissant (= be fiantrana)							

1 – P/JA/P/ T2

3^{ème}Partie

ATTENTION, DANS CETTE PARTIE, IL S'AGIT D'UNE DECISION REELLE. CETTE DECISION SERA EFFECTIVEMENT REALISEE A LA FIN DE L'ENQUETE POUR CEUX D'ENTRE VOUS QUI SERONT DESIGNES AU HASARD.

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Votre décision :

=> Si vous êtes désigné, que faites-vous? **NOTEZ BIEN QUE CETTE DECISION EST REELLE ET SERA EFFECTIVEMENT APPLIQUEE.**

Cochez la case correspondant à votre choix.

☐ Je prélève dans la cagnotte.
Si oui, combien? Montant : _____ (Multiple de 1000, de 0 à 30 000 Ariary ; *cf : tableau ci-dessous*)

Pour moi	1000Ar	2000Ar	3000Ar	4000Ar	5000Ar	6000Ar	7000Ar	8000Ar	9000Ar	10000Ar
Pour la cagnotte	29000Ar	28000Ar	27000Ar	26000Ar	25000Ar	24000Ar	23000Ar	22000Ar	21000Ar	20000Ar

Pour moi	11000Ar	12000Ar	13000Ar	14000Ar	15000Ar	16000Ar	17000Ar	18000Ar	19000Ar	20000Ar
Pour la cagnotte	19000Ar	18000Ar	17000Ar	16000Ar	15000Ar	14000Ar	13000Ar	12000Ar	11000Ar	10000Ar

Pour moi	21000Ar	22000Ar	23000Ar	24000Ar	25000Ar	26000Ar	27000Ar	28000Ar	29000Ar	30000Ar
Pour la cagnotte	9000Ar	8000Ar	7000Ar	6000Ar	5000Ar	4000Ar	3000Ar	2000Ar	1000Ar	0Ar

☐ Je ne prélève pas dans la cagnotte.

****

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2 – P/JA/P/ T2

Veuillez répondre le plus précisément possible aux questions. **Il n'y a pas de réponses vraies ou fausses.** Seul votre avis nous intéresse.

Ce questionnaire est <u>STRICTEMENT ANONYME</u> - Merci de ne pas communiquer entre vous.

1^{ère}Partie.

Imaginez la situation suivante :

Pendant la journée de votre département, vous avez la possibilité de faire une bonne action en sa faveur. Deux actions différentes vous sont proposées.

1. La première consiste à faire un grand nettoyage de l'entourage des bâtiments, afin d'améliorer l'image de votre département.

2. La deuxième action consiste à faire un guide pour les nouveaux étudiants afin de faciliter leur intégration dans votre département.

Au lieu de prendre cette journée pour vous détendre, vous avez la possibilité de faire une bonne action en faveur de votre département. Choisissez-vous de participer à l'une de ces deux activités en faveur de votre département?

1. Oui, je participe au grand nettoyage de l'entourage ☐

2. Oui, je participe au guide pour les nouveaux étudiants ☐

3. Non, je ne participe pas ☐

Les deux activités sont une réussite et le chef du département est très reconnaissant.

2ème Partie

Pouvez-vous exprimer en quelques phrases ce que vous ressentez suite à votre participation à cette activité ? *(Vous pouvez répondre en malgache)*

……

……

……

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……

_____

1 – P/JA/I/T3

3ème Partie

Indiquez sur une échelle de 1 (=pas du tout) à 7(= très) comment vous vous trouvez : *(Cochez la case correspondante à votre choix)*

	− (Pas du tout) ⟶ + (Très)						
	1	2	3	4	5	6	7
Je suis sympathique							
Je suis généreux							
Je suis serviable							
Je suis compatissant (= be fiantrana)							

4^{ème}Partie

ATTENTION, DANS CETTE PARTIE, IL S'AGIT D'UNE DECISION REELLE. CETTE DECISION SERA EFFECTIVEMENT REALISEE A LA FIN DE L'ENQUETE POUR CEUX D'ENTRE VOUS QUI SERONT DESIGNES AU HASARD.

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Votre décision :

=> Si vous êtes désigné, que faites-vous? **NOTEZ BIEN QUE CETTE DECISION EST REELLE ET SERA EFFECTIVEMENT APPLIQUEE.**

Cochez la case correspondant à votre choix.

☐ Je prélève dans la cagnotte.
Si oui, combien? Montant : _____ (Multiple de 1000, de 0 à 30 000 Ariary ; *cf : tableau ci-dessous*)

Pour moi	1000Ar	2000Ar	3000Ar	4000Ar	5000Ar	6000Ar	7000Ar	8000Ar	9000Ar	10000Ar
Pour la cagnotte	29000Ar	28000Ar	27000Ar	26000Ar	25000Ar	24000Ar	23000Ar	22000Ar	21000Ar	20000Ar

Pour moi	11000Ar	12000Ar	13000Ar	14000Ar	15000Ar	16000Ar	17000Ar	18000Ar	19000Ar	20000Ar
Pour la cagnotte	19000Ar	18000Ar	17000Ar	16000Ar	15000Ar	14000Ar	13000Ar	12000Ar	11000Ar	10000Ar

Pour moi	21000Ar	22000Ar	23000Ar	24000Ar	25000Ar	26000Ar	27000Ar	28000Ar	29000Ar	30000Ar
Pour la cagnotte	9000Ar	8000Ar	7000Ar	6000Ar	5000Ar	4000Ar	3000Ar	2000Ar	1000Ar	0Ar

☐ Je ne prélève pas dans la cagnotte.

****

Merci d'avoir répondu à ce questionnaire. Les personnes possédant les numéros désignés pourront retirer leurs gains contre présentation de leurs numéros au secrétariat du C3EDM.

2 – P/JA/I/ T3

Veuillez répondre le plus précisément possible aux questions. **Il n'y a pas de réponses vraies ou fausses.** Seul votre avis nous intéresse.

Ce questionnaire est <u>STRICTEMENT ANONYME</u> - Merci de ne pas communiquer entre vous.

1^{ère}Partie.

Imaginez la situation suivante :

Pendant la journée de votre département, vous devez faire une bonne action en sa faveur. Deux actions différentes vous sont proposées.

1. La première consiste à faire un grand nettoyage de l'entourage des bâtiments, afin d'améliorer l'image de votre département.

2. La deuxième action consiste à faire un guide pour les nouveaux étudiants afin de faciliter leur intégration dans votre département.

Au lieu de prendre cette journée pour vous détendre, vous devez faire une bonne action en faveur de votre département. A laquelle de ces deux activités en faveur de votre département participez-vous?

1. Je participe au grand nettoyage de l'entourage ☐

2. Je participe au guide pour les nouveaux étudiants ☐

Les deux activités sont une réussite et le chef du département est très reconnaissant.

2ème Partie

Pouvez-vous exprimer en quelques phrases ce que vous ressentez suite à votre participation à cette activité ? *(Vous pouvez répondre en malgache)*

……

……

……

……

……

……

……

_____

1 – P/JA/I/ T4

3ème Partie

Indiquez sur une échelle de 1 (=pas du tout) à 7(= très) comment vous vous trouvez : *(Cochez la case correspondante à votre choix)*

	− (Pas du tout) ——————————————→ + (Très)						
	1	2	3	4	5	6	7
Je suis sympathique							
Je suis généreux							
Je suis serviable							
Je suis compatissant (= be fiantrana)							

4^{ème}Partie

ATTENTION, DANS CETTE PARTIE, IL S'AGIT D'UNE DECISION REELLE. CETTE DECISION SERA EFFECTIVEMENT REALISEE A LA FIN DE L'ENQUETE POUR CEUX D'ENTRE VOUS QUI SERONT DESIGNES AU HASARD.

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Votre décision :

=> Si vous êtes désigné, que faites-vous? **NOTEZ BIEN QUE CETTE DECISION EST REELLE ET SERA EFFECTIVEMENT APPLIQUEE.**

Cochez la case correspondant à votre choix.

☐ Je prélève dans la cagnotte.
Si oui, combien? Montant : _____ (Multiple de 1000, de 0 à 30 000 Ariary ; *cf : tableau ci-dessous*)

Pour moi	1000Ar	2000Ar	3000Ar	4000Ar	5000Ar	6000Ar	7000Ar	8000Ar	9000Ar	10000Ar
Pour la cagnotte	29000Ar	28000Ar	27000Ar	26000Ar	25000Ar	24000Ar	23000Ar	22000Ar	21000Ar	20000Ar

Pour moi	11000Ar	12000Ar	13000Ar	14000Ar	15000Ar	16000Ar	17000Ar	18000Ar	19000Ar	20000Ar
Pour la cagnotte	19000Ar	18000Ar	17000Ar	16000Ar	15000Ar	14000Ar	13000Ar	12000Ar	11000Ar	10000Ar

Pour moi	21000Ar	22000Ar	23000Ar	24000Ar	25000Ar	26000Ar	27000Ar	28000Ar	29000Ar	30000Ar
Pour la cagnotte	9000Ar	8000Ar	7000Ar	6000Ar	5000Ar	4000Ar	3000Ar	2000Ar	1000Ar	0Ar

☐ Je ne prélève pas dans la cagnotte.

****

Merci d'avoir répondu à ce questionnaire. Les personnes possédant les numéros désignés pourront retirer leurs gains contre présentation de leurs numéros au secrétariat du C3EDM.

2 – P/JA/I/ T4

Appendix 4

Bonjour. Vous allez participer à un projet d'étude dans le cadre d'une réalisation de mémoires universitaires. Notez bien que les questionnaires sont entièrement anonymes.Ils sont justeidentifiés par un numéro unique. Nous vous remerciions par avance de répondre le plus sincèrement possible.

Cette étude va durer 1 heure environs. Pendantcette heure, nous allons vous demander de remplir une série de questionnaires concernant la prise de décision ainsi que vos perceptions vis à vis de l'environnement. Vous participerez également à un jeu de hasard consistant en un petit jeu de dé. Le principe est le suivant : vous disposerez d'un montant initial de 1000 Ariary. Vous secouerez le dé dans le gobelet et vous le retournerez sur votre table, une seule fois. Vous le ferez tous en même temps après notre signal de départ. Vous regarderez le résultat à travers le trou du gobelet. Selon le numéro obtenu sur le dé, vos 1000 Ariary seront multipliés ou perdus.Nous souhaitons vous confirmer que c'est bien réel. Les détails sont donnés dans le questionnaire.

Les questionnaires que nous allons vous distribuer sont à usage personnel. Il est interdit à partir de maintenant de communiquer avec les autres participants pendant toute la durée de l'enquête. L'enquête doit se dérouler dans le silence total. Toute personne qui ne respectera pas ce silence ne pourra plus participer.

Préparer vos stylos, et vous pouvez répondre en malgache.

Nous allons à présent commencer avec le premier questionnaire.

Vous pouvez poser des questions s'il y a des phrases incompréhensibles.

Vous disposez de 10 minutes.

Après 5 minutes environs, une fois que la majorité a fini, on leur annonce le début du jeu de dé :

Vous pouvez à présent lancer le dé.Secouez le dé dans le gobelet et retournez-le sur votre table, une seule fois. Regardez le résultat à travers le trou du gobelet. Notez-le sur votre questionnaire et continuez.

(………)

Les 10 minutes sont écoulées. Nous allons récupérer les questionnaires. Détacher votre numéro qui se trouve sur les questionnaires car ceci vous permettra de collecter votre gain à la fin du jeu.

Voici à présent un deuxième questionnaire. (Distribuer le questionnaire socio environnemental).

N'oubliez surtout pas de bien indiquer votre numéro, un emplacement est prévu sur chaque page du questionnaire. Merci de garder le silence et de faire appel à nous uniquement, si vous avez des questions de compréhension.

Vous disposez de 40 minutes.

Pendant que les participants remplissent le deuxième questionnaire, préparer les enveloppes contenant les gains individuels en fonctions des premiers questionnaires.

Les 40 minutes sont écoulées, nous allons ramasser les questionnaires.

Fin de la séance : récupérer les questionnaires socio environnementaux et remettre les enveloppes contenant les gains sur présentation des numéros.

[FIN]

1 – F/I

Questionnaire anonyme n° ___.

Veuillez répondre le plus précisément possible aux questions. **Il n'y a pas de réponses vraies ou fausses.** Seul votre avis nous intéresse.

Ce questionnaire est <u>STRICTEMENT ANONYME</u> - Merci de ne pas communiquer entre vous.

1^{ère}Partie.

Imaginez la situation suivante :

Pendant la journée de votre département, vous avez la possibilité de faire une bonne action en sa faveur. Deux actions différentes vous sont proposées.

1. La première consiste à faire un grand nettoyage de l'entourage des bâtiments, afin d'améliorer l'image de votre département.

2. La deuxième action consisteà faire un guide pour les nouveaux étudiants afin de faciliter leur intégration dans votre département.

Au lieu de prendre cette journée pour vous détendre, vous choisissez donc de faire une bonne action en faveur de votre département.A laquelle de ces deux activités en faveur de votre département choisissez-vous de participer?

1. Grand nettoyage de l'entourage ☐

2. Guide pour les nouveaux étudiants ☐

Les deux activités sont une réussite et le chef du département est très reconnaissant.

2ème Partie

Pouvez-vous exprimer en quelques phrases ce que vous ressentez suite à votre participation à cette activité ?*(Vous pouvez répondre en malgache)*

………
………
………
………
………
………
………
………
_____

3ème Partie

Indiquez sur une échelle de 1 (=pas du tout) à 7(= très) comment vous vous trouvez : *(Cochez la case correspondante à votre choix)*

	− (Pas du tout) —————————————————→ + (Très)						
	1	2	3	4	5	6	7
Je suis sympathique							
Je suis généreux							
Je suis serviable							
Je suis compatissant (=be fiantrana)							

4ème Partie

A présent, nous allons jouer au dé. **DANS CE JEU, VOUS AVEZ LA POSSIBILITE DE GAGNER UNE SOMME REELLE**. Pour cela, il vous suffit de lancer le dé.

Vous devez secouer votre gobelet contenant le dé. Vous identifierez le numéro obtenu à travers le trou du gobelet. Les gains correspondant à chaque numéro sont présentés dans le tableau suivant.

Lancez le dé (*une seule fois*) et entourez le résultat que vous avez obtenu.

Résultat du dé	1	2	3	4	5	6
Gain	1000Ar	2000Ar	3000Ar	4000Ar	5000Ar	0Ar

****

Merci pour vos réponses. Les informations récoltées resteront strictement confidentielles. Vous pourrez récupérer l'enveloppe contenant votre gain à la fin de la séance, sur présentation de votre numéro.

2 – F/I/n°___

Questionnaire anonyme n° ___.

Veuillez répondre le plus précisément possible aux questions. **Il n'y a pas de réponses vraies ou fausses.** Seul votre avis nous intéresse.

Ce questionnaire est <u>STRICTEMENT ANONYME</u> - Merci de ne pas communiquer entre vous.

1ère Partie

Veuillez mettre dans l'ordre les mots suivants afin de former des phrases :

1. débuté grande 2012 La de a le 31 au Palais mai Sportsbraderie Madagascardes.

……

……

2. La d'Exposition de ou FIM a le 31 Madagascar mai 2012 au Internationale Centre Futura Foire débuté.

……

……

3. à totale de est Madagascar 20 environs population millions Laestiméed'habitants.

……

……

4. Madagascar nationale est compagnie aérienne la Air.

……

……

2ème Partie

Indiquez sur une échelle de 1 (=pas du tout) à 7(= très) comment vous vous trouvez :*(Cochez la case correspondante à votre choix)*

	− (Pas du tout) ⟶ + (Très)						
	1	2	3	4	5	6	7
Je suis sympathique							
Je suis généreux							
Je suis serviable							
Je suis compatissant (=be fiantrana)							

3ème Partie

A présent, nous allons jouer au dé. **DANS CE JEU, VOUS AVEZ LA POSSIBILITE DE GAGNER UNE SOMME REELLE**. Pour cela, il vous suffit de lancer le dé.

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Lancez le dé (*une seule fois*) et entourez le résultat que vous avez obtenu.

Résultat du dé	1	2	3	4	5	6
Gain	1000Ar	2000Ar	3000Ar	4000Ar	5000Ar	0Ar

****

Merci pour vos réponses. Les informations récoltées resteront strictement confidentielles. Vous pourrez récupérer l'enveloppe contenant votre gain à la fin de la séance, sur présentation de votre numéro.

Abstract

A growing literature on moral licensing opens new directions to understand the forces underlying individuals' moral behaviour. Traditionally, moral behaviour and social preferences have been considered as exogenous and consistent over time. But recent researches support that an initial virtuous act might impact future decisions, by decreasing the willingness to act pro-socially in subsequent circumstances and vice versa. For example, anecdotal evidence shows that Prius Hybrid drivers are more likely to break crosswalk laws and get fines, green buyers are more likely to steal, people who wrote positive stories about themselves are more likely to act selfishly, etc. In this first chapter, we propose to run a set of four experiments within two sections to explore the moral licensing effect under different policy perspectives. Section I introduce the related literature and discuss moral licensing interactions with different policy scenarios. Section II deals with moral licensing scope and contextual validity.

Résumé

Une littérature en forte expansion permet d'appréhender différemment les mécanismes sous-jacents au comportement moral des individus. Traditionnellement, le comportement moral et les préférences sociales ont été considérés comme exogènes et stables dans le temps. Cependant, des recherches récentes démontrent qu'une action vertueuse peut influencer des décisions futures, en diminuant la volonté de 'bien agir' ultérieurement et vice versa. Par exemple, les conducteurs de voitures hybrides seraient plus enclins à commettre des infractions au code de la route, les consommateurs de produits verts seraient plus susceptibles de voler que les autres, ou encore, les individus ayant relaté une histoire positive les concernant agiraient de manière plus égoïste par la suite, etc. Dans ce premier chapitre, nous présentons un ensemble de 4 expériences réparties en 2 sections visant à explorer cet effet d'ordre compensatoire. La première section introduit la littérature relative à l'effet de licence morale et discute des interactions possibles avec différents outils politiques. La deuxième section traite de la portée de l'effet de licence morale et aborde également la question de sa validité contextuelle.

Chapter II –Preference Consistency and Incentive-Based Policies

Improving contract's compliance, Mitigating unintended spillovers. Evidence from field experiments.

Introduction

Incentive-based policies appear as a popular tool to foster behavioural change. But most of the time, such policies are designed under the assumption that individuals react as given by the rational choice theory. Behavioural economics provides evidence of several deviations from this theory and offer an interesting vision to understand why policies might fail achieving their goal. Our motive in Chapter II is to study unintended behavioural responses to incentive-based mechanism that might mitigate policy design, using field experiments, in a 'Econs' vs. 'Humans' fashion[50] (Thaler and Sunstein, 2008).

This chapter extends findings from Chapter I to a more general context involving additional insights from behavioural economics that are relevant for the given incentive-based policy, namely Compensation and Rewards for Environmental Services, in the perspective to get closer to real life settings and increase the accuracy of this work for policy makers. Field experiments are particularly well suited to address the research question: how behavioural biases affect policies efficiency? Capturing as much of the context as possible, experiments in the field increase both external validity and policy relevance. List and Hofler (2004) revealed that pro social behaviours observed in laboratory settings were non existing or existing in a fewer extent in natural settings, questioning about the external validity delivered by laboratory experiments. Also, laboratory experiments are often observing student's behaviour, which undermines a possible extension of the results to the rest of the population.

Compensation and Rewards for Environmental Services are incentives provided to ecosystem providers to encourage them to enhance or maintain environmental services. Those tools are increasingly popular, especially in developing countries, holding both the highest pie of tropical forest and the highest rate of poverty (Ferraro, 2011; Pattanayak et al., 2010). The use of incentive-

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[50] 'Econs' are assumed to be rational while 'Humans' are rationally bounded.

based policies for protecting the environment has been supported since the end of the 1990s in international arenas and became the main innovative policy tools for biodiversity conservation worldwide.

Chapter II is twofold. First, we propose to study unintended spillovers of incentives-based contract designs due to behavioural response (i.e., 'Moral licensing' and 'Mental accounting'). In a second part, we question a behavioural bias that could interfere with compliance to such types of contracts (i.e., 'Present bias').

In sum, this chapter aims to study three behavioural biases. The first bias, named 'Mental accounting' studies the impact of income's source on individuals' consumption decisions. This bias is related to the compensation mechanism and studies whether money received from a working activity would be more likely to be considered as a current income and treated with high responsibility (i.e. utilitarian consumption) compared to money received as a compensation involving less or no effort, which would be more likely to be spent impulsively (i.e. self-indulgent, luxurious consumption). The second bias, named 'Moral licensing' studies the behavioural adaptation (i.e. moral compensation) that may take place when environmental conservation is morally oriented. It refers to a situation where doing something morally valuable in a first stage excuses to adopt dubious behaviour afterwards (see Chapter I). The third bias examines intertemporal decision-making (i.e., 'Present bias'). Behavioural economics research has shown that, in contexts where individuals must allocate a budget between a sooner date and a later date, the decision depends on temporal proximity. When the sooner date is in the distant future, individuals will often allocate a relatively smaller share to the sooner date, and a larger share to the later date. This reversal, referred to as hyperbolic discounting or present-bias, contradicts the standard economic assumption that individuals are consistent in their time preferences.

Designing public policies under the assumption that individuals are consistent in their preferences would therefore be problematic. This chapter tests, for the first time, whether those three biases

have external validity, running behavioural experiments in the field, and discusses their relevance for policy makers.

This Chapter is organized as follows: Part I focuses on unintended incentive's spillovers due to behavioural response, such as *Moral licensing* and *Mental accounting*, while Part II questions the behavioural biases that could intervene in incentive contract's compliance, being the *Present bias*.

Part I. Compensation and Rewards for Environmental Services (CRES) and efficient design of contracts in developing countries. Behavioural insights from a natural field experiment[51].

I.1. Introduction

Environmental mitigation policies are made of a set of tools including, among others, compensation and rewards mechanisms. The idea is quite straightforward: in exchange of an environmental service (e.g. fencing land to protect watershed, stopping cultivation to increase natural areas), individuals receive a compensation to offset foregone income, that could either take the form of money, training, in-kind, etc.... In the same vain, Rewards for Environmental Services are incentives provided to ecosystem providers to encourage them to enhance or maintain environmental services.

In a developing countries' context, those mechanisms are worthwhile for several reasons. To start with, developing countries hold the highest pie of tropical forest, which has the potential to provide several ecosystem services through species conservation, climate regulation, watershed protection, carbon sequestration and also pure aesthetic benefits. To end with, because developing countries characteristics have also to face high rate of poverty, the eventuality of a win-win approach which would enable both poverty alleviation and environmental conservation, makes Compensation and Rewards for Environmental Services (CRES) especially appealing for policy makers, program designers, and researchers.

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[51] Part I corresponds to: Clot, S., Andriamahefazafy, F., Grolleau, G., Ibanez, L., Méral, P. (2014), Payments for Ecosystem Services: Can we kill two birds with one stone? Insights from a Natural Field Experiment in Madagascar. *Lameta working paper 2014-1*. In revision with Ecological Economics.

When mechanisms are conferred so many attributes, negative effects are often neglected. As noted in Muradian *et al.* (2013) and Kinzig (2011, 2012), over reliance on win-win solutions might lead to ineffective outcomes, similar to earlier experience with integrated conservation and development projects. In this research, we use behavioural economics to enlighten potential biases that might affect tools designed for environmental conservation. Our motive is to study unintended behavioural adaptation that might mitigate policy design, in a 'Econs' vs. 'Humans' fashion (Thaler and Sunstein, 2008).

Our work aims to study two behavioural biases (i.e. predictable "error" in human behaviour, Kahneman, 2003). The first bias, named "mental accounting" studies the impact of income's source on individuals' consumption decision. The hypothesis behind mental accounting is that the way you obtain money impacts the way you spend it, by transiting through separate accounts (e.g. the same 100$ won't be spent the same way whether you found it on the street or earned it by working). This bias is related to the compensation mechanism and studies whether money received from a working activity would be more likely to be placed in a current income and treated with high responsibility (i.e. utilitarian consumption) compared to money received as a compensation involving less or no effort than the original activity, which would be more likely to be spent impulsively (i.e. self-indulgent, luxurious consumption).

The second bias, named "moral licensing" studies the behavioural adaptation (i.e. moral compensation) that may take place when environmental conservation is morally oriented. It refers to a situation where doing something morally valuable in a first stage excuse to adopt dubious behaviour afterwards. We therefore question the potential counterbalancing effect of rewards (i.e. promoting morally valuable behaviour) on prosocial motivation.

I.1.1. Compensation and mental accounting bias. Is one dollar always valued in the same manner?

Compensation and Rewards for Environmental Services (CRES) are defined as "Contractual arrangement and negotiated agreements among ecosystem stewards, environmental services beneficiaries, or intermediaries, for the purpose of enhancing, maintaining, reallocating or offsetting damage to environmental services." (Swallow *et al.*, 2009). This definition covers a wide array of cases depending on the ecosystem services provided (watershed protection, carbon sequestration, biodiversity conservation, etc.) and the contract's scheme (Type of work requested[52], cash vs. in-kind, direct vs. indirect payment etc). In this work, we will more specifically focus on the later aspect, payment scheme and degree of implication work requested. We want to explore the behavioural impact of the level of effort (i.e. amount of work) involved by the compensation mechanism, since it may significantly differ from the effort level required by more traditional land use.

The degree of implication depends on the programs. Some contracts mention the adoption of a sustainable management plan or an engagement in reforestation, while others are based on conservation exclusively. For instance, watershed protection includes cases where upstream communities are compensated to protect forests and renounce to their activity nearby the river bank (Pagiola *et al.*, 2008). Also, in given carbon sequestration programs, farmers are compensated to stop cutting trees (Börner *et al,.* 2010). Also in some biodiversity conservation projects, payments are offered in exchange of renouncing to hunt wildlife and limit expansion of crops and livestock on given lands (Frost and Bond, 2008).

One of the main focuses in research has been geared towards method to estimate monetary amounts that are at least equal to landowners' opportunity cost (Wunder 2008, Pagiola *et al.* 2005), but little attention has been given to how those payments are integrated in farmers' budgets. Under

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[52] Land conservation, land reforestation, sustainable management plan, etc

the general hypothesis of money fungibility, such payments should be substituted to traditional land use income.

In the meantime, researches in anthropology, psychology and behavioural economics suggest that human's reaction to economic instruments may differ from those predicted by models of rational choice. In sum, as well exposed by Thaler and Sunstein (2008) 'Humans' do not behave as 'Econs'. More specifically, a whole body of researches reports evidences that money is not treated as fungible by human's beings. The refutation of fungible money principle led a group of researcher to establish a theory of mental accounting (Kahneman and Tversky 1984, Thaler 1990, Thaler 1999), which posits that people value money differently depending on how the money is obtained and class it into categories. Such accounts would be meaningless if they were perfectly fungible, but experimental evidence proves that the way you get a payment or an in-kind determines the way you use it subsequently.

For instance, a research on prostitutes in Oslo demonstrated that they spend differently the money they earned from their clients than the one they received from the government. Money from the government will be used to spend on rent or food whereas money from their clients will be used to party (Sager, 2010). In Kenya, tribes make scrupulous differences between categories of money. Income from selling lands cannot be used to buy cattle or they believe the entire cattle would die (Shipton, 1990).

Mental accounting in developing countries remains widely underexplored. Even more, the absence of studies in natural context (i.e. outside the lab) casts doubt upon its external validity. Will money (or in kinds) received by farmers to stop working lands be used similarly to money obtained from working that lands? Is one dollar from a conserved land the same dollar from a cultivated land? This is one of the main questions we address in this work. We test how two different ways of obtaining an income influences consumption behaviour, using a natural field experiment in a developing country context.

The experimental purpose is to compare individual's choice between necessity vs. superior (i.e. luxury) goods, whether payments are obtained with little effort or by work. To the best of our knowledge, this is the first natural field experiment to test the impact of income's sources on spending behaviour. Mental accounting is an understudied effect within developing countries context, while it could directly impacts policies efficiency.

I.1.2. Rewards and moral licensing bias. Do good deeds make bad people?

Rewards tend to promote 'good behaviour' by recompensing action that goes into the desired way. It has the particularity to be embedded in a moral dilemma frame, where good deeds interact with bad deeds. In this context, environmental conservation becomes something 'good'. We can distinguish two kinds of rewards: financial rewards and non-financial rewards. For instance, existing works have demonstrated that financial rewards can crowd out pre-existing intrinsic motivation (Frey and Jegen, 2001; Bénabou and Tirole, 2006). On the other side, non-financial incentives have attracted increasing interest (Thaler and Sunstein, 2008), with still little evidence of behavioural responses. Gneezy *et al*. (2011) review the debate stating that the effects of incentives depend on how they are designed, the form in which they are given (especially monetary or nonmonetary), and how they interact with intrinsic motivations and social motivations.

Conducting an artefactual field experiment embedded in a reforestation project in Bolivia, Adda (2011) studied the trade-off between individual and social benefits in natural resource use and demonstrated that both financial and non-financial rewards could potentially crowd out intrinsic motivation. In parallel, an emerging literature on self-licensing demonstrates that doing something morally valuable in a first stage increases the likelihood to do something less morally valuable at a later stage. According to this theory, pro social preferences are not exogenous but context dependent (Khan and Dhar, 2006; Sachdeva *et al.*, 2009; Mazar and Zhong, 2010; Chiou *et al.*, 2011) and vary according to a moral regulation process where good deeds purchase the right to act more

selfishly afterwards. In other words, behaviours are highly related to the degree of self-esteem declared by participants.

Very little is known on this moral accounting process, better identified as a self-licensing effect, while it raises questions on specific interactions between monetary and non-monetary incentives for the effectiveness of ecosystems' conservation. Knowing more is thus essential when collective action institutions are involved and when the promoted task has an important pro-social component with moral consideration guiding people's actions such as it is in environmental conservation.

The reminding of this paper is organized as follows: the next section presents relevant contributions about mental accounting and self-licensing in the existing literature and introduces our main behavioural hypotheses. Section 3 presents the background work that contributed to design our study as well as the experimental procedure while section 4 analyses the results. In section 5, we discuss relevant implications for policy design.

I.2. Literature background, behavioural hypotheses and conceptual framework

I.2.1. Mental accounting

At least two ways of describing consumption behaviour are present in the literature. On one side, classic theory assumes that money is fungible. One dollar is one dollar, and it should be used independently of the way it has been created. This model assumes that when faced with a given income, individuals will use it according to their stated preferences. Within this viewpoint, human preferences are stable and exogenously determined. On the other side there is a distinct type literature, made of anthropologists, psychologists and behavioural economists, which inspiration are

mainly drawn by an empirical paradox. This literature argues the opposite: the way money is obtained matters.

I.2.1.1. From classic theory…

One of the fundamental assumptions in traditional economics is that money is fungible. All money is created equal and should be treated equal. This goes with the assumption that people tend to maintain preferences stable overtime disregarding money's framing[53]

However, evidence shows some deviations from those predictions (Shapiro and Slemrod 2003, Epley *et al*. 2006, Milkman and Beshears 2009), which are difficult to explain under this theoretical framework (e.g. tax rebate failing to boost the economy, coupons leading to unusual consumption, etc.).

I.2.1.2. …to empirical paradoxes

Empirical evidence suggests that humans create mental accounting procedures. People will track their financial activities using a set of cognitive labels depending to the context in which it was obtained, each label being associated with a different marginal propensity to consume. This principle has been developed by mental accounting theory (Kahneman and Tversky 1984, Thaler 1990).

Mental accounting refers to the tendency people have to classify their money into different accounts, similar to the way organizations create accounting procedures, to manage their financial decisions. Rather than making choices through a global optimization process over long-term horizon, individuals categorize their activities into 'mental accounts' and make their decisions independently. In the behavioural economics literature, Thaler (1999) mentions three components that characterize mental accounting. The first one captures how outcomes are perceived and experienced and how

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[53] Consistent consumption is predominant in the two economic theories of spending and saving, with the Life-cycle hypothesis (Modigliani, 1966) on one side, and Permanent income hypothesis (Friedman, 1957) on the other side. The Life-cycle hypothesis (LCH) argues that people maintain stable life style over time, while the Permanent Income hypothesis (PIH) defends that temporary change in income do not affect individual's spending behavior.

decisions are made and subsequently evaluated. The second component involves the assignment of activities to specific accounts. And the third one concerns the frequency with which accounts are evaluated.

In September 2001, the United States government gave back 38 billion dollars to the country in the form of tax rebates, with the objective of increasing consumer's expenses and stimulates the economy. American citizens received a check ranging from $300 to $600 according to their annual reported income. Later results based on macroeconomic data reported that government's expectations were not fulfilled, with low spending rates, partly softened by a 14% increase in saving (Shapiro and Slemrod, 2003). Additional data from Shapiro and Slemrod, analyzing 500 household survey reports that overall, only 21.8% of people answered that the tax rebate was used for spending. The tax rebate had a small impact on aggregate demand and therefore was not successful to provide the wanted short run stimulus. A follow up experiment was run in New York City's Grand Central Station a few months after the tax rebate occurred. Participants were first asked whether they remember receiving a check from the 2001 Tax Relief Act (they all did). The 76 participants were then assigned to two different conditions: one that presented the checks as an additional income resulting from a budget surplus returned as a bonus, or the other one presented it as a tax surplus that should be returned as withheld income (the framing in this condition was paraphrased from the real description of the rebate). Results indicate that participants in the bonus condition recalled spending more (M=76%) than those in the rebate condition (M=41%).

Although there are many reasons why people may choose to spend or save, Epley *et al*. (2006, 2007) suggests that the way people code these rebates may have a significant impact. Windfalls are more likely to be saved when coded as refund, and they are more likely to be spent were coded as bonus. To test this prediction, Epley *et al*. (2006, 2007) run an experiment involving 47 undergraduate students from Harvard University. For this experiment, participants received a $50 check as they arrived in the laboratory. While a first group was told that this money came from a 'fund's surplus',

the other group received the same amount as a 'tuition rebate'. A week after the experiment, participants received an email where they had to indicate how they spent this money. Results show that subjects in the bonus condition spent significantly more than people in the tuition rebate condition and corroborate the impact of incomes' framing (M=$22.04 vs M=$9.55, $\aleph^2$=6.34, p<0.05).

I.2.1.3. Playing with the 'house money'

A significant body of experimental economists have documented that when subjects play with standard laboratory endowment, they make less self-interested choice than when they use money they have earned through a laboratory task. It suggests that the impact of whether the monetary endowment is either a windfall gain ("house money") or a reward for a certain effort-related performance impacts subsequent behaviour, as documented below.

At risk games for instance, individuals are much more willing to take risk when the money is not really theirs, compared with when they had to earn it. Thaler and Johnson (1990) run a trading experiment involving 206 MBA students from Cornell University, demonstrating that traders who are given a higher windfall income at the start of a market session bid higher. In fact, 77% of subjects are risk takers when they experienced a gain in a first stage, versus 44% in single stage scenario including no prior gains. Ackert *et al*. (2006) found similar results: when endowed with house money, people become more risk taking.

Cherry *et al*. (2002) tested this 'house-money effect' (i.e. 'House money' versus 'Earned money') with about 300 undergraduates from University of Central Florida in a dictator game. It turned out that the percentage of selfish subjects (i.e. null offer) raises from 19% in the house money condition to 79% in the earned money condition, significant at the 1% level. Those results are supported by a charitable giving game from Reinstein and Riener (2011) where subjects are more willing to donate to a charity when the endowment is not linked to any previous effort.

I.2.1.4. And outside the lab?

Despite a lack of experimental evidence from the field, surveys based on quasi-experimental or empirical data offer worthy findings.

An interesting analysis of an online grocery store in North America reveals that customers increased their consumption of marginal products after they received a $10-off coupon. This research from Milkman *et al*. (2009) studied the effect of windfalls on people spending decisions by comparing the online purchase of groceries. Motivation for offering those $10 coupons was to thank customers who encouraged others to order from the online grocer. In total, between January 1st, 2005 and December 31st, 2005, 4,435 customers benefited from this $10-off discount coupon. Milkman et al. found that grocery spending increases by $1.59 when a $10-off coupon is redeemed (p<0.01), meaning that extra spending associated with coupon redemption is focused on groceries that a customer does not typically buy.

Carriker and Langemeier (1993) report the result of a study on the propensity to consume farm family disposable income from separate sources (farm operations, off-farm sources, and government payments. Their results, based on consumption expenditures from 165 Kansas' farms, indicate that the propensity to consume off-farm income and government payments is higher than the propensity to consume farm income.

In a developing countries context, a few works that studied savings behaviour endorse mental accounting principle. For example, researches that have been studying savings determinants found that in some developing countries (Guatemala and Malawi), remittances were not used in the same way than other income sources (Davies *et al*. 2009, Adams and Cuecuecha 2010). Adams and Cuecuecha (2010) found that money from remittances had greater propensity to be saved than money from other income sources, while Davis *et al*. (2009) report a positive impact of remittance specifically intended for education. Davies *et al*. (2009) results are based on a survey of 5,644 rural

households across Malawi. Their data include detailed income and consumption variables as well as a wide range of household characteristics. Qualitative questions reveal that households perceive remittances as distinct income from others and choose to use it differently. In support, a regression analysis supports that remittances from rural and urban Malawi exhibit a positive and significant impact on education expenses.

Dupas and Robinson (2012) document in their work a randomized control trial designed to test the impact of various saving's strategies on household capacity to cope with health problems. Within this work, interesting references are made to mental accounting[54]. For example, respondents said that once the money was set aside in a box, they were better able to avoid 'unplanned expenditures' (i.e. transfers to friends or relatives, luxury spending, etc). Indeed, people in the safe box group feel less constrained to share with relatives, reporting an average of 4.30 on a scale from 1 to 5 (1= High constraint; 5=Low constraint), versus 2.35 for the group with no safe box. A sizeable fraction (51%) reports that this is because the money in the box is for a specific goal. According to Dupas and Robinson (2012) money set aside was labeled as health savings and became non-fungible with other sources of cash, even though the money was still physically easily accessible.

On the behalf of this literature, we hypothesize that mental accounting matters, and compensation mechanism could have unintended consequences on economic behaviour. We formulate the following hypothesis:

Hypothesis 1: *Mental accounting affects economic (i.e. consumption) decision. Individuals who receive money tagged as 'low effort money' are more inclined to make self-indulgent choices subsequently than individuals who receive higher effort based money.*

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[54] In this illustration, mental accounting refers to the classification between two accounts: one for expenses vs. one for savings, by analogy with one account for unnecessary expenses vs. one account for necessary expenses. While slightly different from previous example, it confirms the general tendency people have to classify money into different accounts.

Both experimental and field experiments stress that the origin of income impacts on preferences and behaviour. 'Effort based money' vs. 'Low effort based money' leads people to behave differently and remittances are not treated as other income sources, or that money put in a safe box becomes non-fungible. While those outcomes partly dispel doubts on mental accounting's external validity, there is still no experimental evidence from the field that establishes the consequences of how income is obtained. We therefore believe that testing the impact of incomes' sources in field settings is highly valuable for researchers and policy makers involved in compensation program design.

In this work, we propose to investigate the effect of income's sources ('Low effort' based money vs. 'High effort' based money) on spending decisions (Self-indulgent vs. Utilitarian) within Madagascar rural areas, using a natural field experiment. This is the first time that the hypothesis for consistent vs. inconsistent consumption behaviour out of different income's framing is tested in real life settings.

I.2.2. Moral accounting

The forthcoming paragraph introduces a more unconventional question, while still inspired from mental accounting model. If the earlier focuses on financial transfers (between immediate consumption and savings, or classifying income among different groups, etc.), little attention has been given to moral transfers, while having a determinant role in common pool resources management and environmental conservation. Recent work, mainly coming from psychology, suggests that doing something morally valuable in a first stage might increase the likelihood to do something less morally valuable in a later stage or even morally dubious (Khan and Dhar, 2006; Sachdeva *et al.*, 2009; Mazar and Zhong, 2010; Chiou *et al.*, 2011).

Named 'Self licensing', the concept describes a situation where being 'good' leads to more self-indulgent option afterwards and vice et versa (the reverse situation would be better referred as moral cleansing). In other words, this describes a process of moral accounting where good deeds are

assimilated as moral credit and bad deed as moral debit. Experimental evidence shows that people who bought green product are then more likely to cheat (Mazar and Zhong, 2010), people who took vitamins are then increasing cigarette consumption (Chiou *et al*. 2011), or people who established non-racist preferences are then more likely to adopt racist attitudes (Merritt and Effron, 2010). In the environmental domain, Perino *et al*. (2012) report that consumers are less sustainable in the food market once they have shown their environmental sensitivity in another domain. Another research shows that residents who received weekly feedback on their water consumption lowered their water use (6.0% on average), but at the same time increased their electricity consumption by 5.6% compared with control subjects (Tiefenbeck *et al*., 2013).

Meritt *et al*. (2010) propose a moral credit model where credits are earned through good deeds and use a bank account metaphor: good deeds purchase 'moral credits' that diminish the responsibility of engaging in bad deeds in the future, permitting deviations from common 'self-expectancies'. A similar metaphor can be found in Hollander (1958) earlier work, where good deeds establish moral credits that can be 'withdrawn' to 'purchase' the right to do bad deeds with impunity from inside the individual. According to moral credit theory, when people feel licensed, they feel as if their past behaviours have earned them credit to stray some from the shining path while still retaining a positive balance in their moral bank account.

This raises questions on potential spillovers generated by programs relying on pro-social motivations. If nudges or pro-social preferences are increasingly referred as a convenient way to promote individuals' cooperation, potential externalities need to be considered.

Hypothesis 2: *Moral accounting (i.e. self-licensing) affects economic subsequent decision-making. Individuals who earn moral credits are more inclined to make less cooperative choices subsequently than individuals who do not earn moral credit.*

In the same domain, some researches established that hedonic consumption was associated with guilt and sentiment of responsibility (Dahl *et al*. 2003, Khan and Dhar 2006). The preference for luxuries is believed to produce subsequent negative self-attribution because such goods are harder to justify than necessary goods, and, as a matter of fact, are self-indulgent. Strahilevitz and Myers (1998) demonstrate that donations commitments to charity were more likely to lead to consumption of hedonic products than more necessary products. The other way round, the guilt linked to the preference for superior good might lead to the need for counter balancing those self-indulgent choices subsequently and explain greater contribution to a public good, so as a guilt-reduction mechanisms. This effect is the opposite of the moral licensing effect and is more commonly referred in the literature as 'moral cleansing' (Sachdeva *et al*. 2009, Brañas-Garza *et al*. 2012)

Hypothesis 2 bis: *Moral cleansing: individuals who lose moral credits through previous choices for self-indulgent products are more likely to make more cooperative choices subsequently than individuals who do not lose moral credits.*

I.3. Groundwork & experimental design

The next part presents the preliminary work that helped us to design our natural field experiment as well as the experimental design.

I.3.1. Initial test of mental accounting

Prior to implementing the natural field experiment designed to test this hypothesis, we run a behavioural experiment among students from the University of Antananarivo, as a first check[55].

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[55] We only carried out a test for mental accounting in order to make sure the existence of this bias in a developing country. Concerning the moral accounting, Clot *et al*. (2014) has shown that the phenomenon applies in Madagascar.

The four treatments are presented in table 1. The main objective of that design is to see whether on a hypothetical basis, people would state different economic decisions depending on how they got money (No effort money vs. Effort money). The other treatment (Yourself vs. Others) is a strategy that aims at minimizing declarative biases, under the assumption that individuals have excessively positive views of themselves but more accurate perception of their peers. This way, we could attempt to get more correct outcomes, controlling for answers that could be perceived as undesirable when referring to oneself, but not when referring to someone else (i.e. control for the social desirability bias). Indeed, preceding works have shown that more accurate answers can be collected by asking what people think about others' preferences (Epley and Dunning, 2000; Grolleau *et al.*, 2012).

Table 1 – Experimental design : 2 (No effort money vs. Effort money) x 2 (Yourself vs. Others) between subjects experiment (*Questions and possible answers within the 4 treatments*)

Treatments		Questions		Answers
1		Supposing that **you** get 10,000 MGA **by working in a restaurant**,	In your opinion, how would **you** use this money?	
2	*Yourself*	Supposing that **you** get 10,000 MGA **by chance in the street**,		1) Savings
				2) Necessary expenses
3		Supposing that **someone** gets 10,000 MGA **by working in a restaurant**,	In your opinion, how would **this person** use this money?	3) Leisure expenses
4	*Others*	Supposing that **someone** gets 10,000 MGA **by chance in the street**,		

A total of 746 undergraduates from Antananarivo's University enrolled in this survey, in May 2012. Results indicate the following: subjects were significantly more inclined to select the leisure option in the lucky (i.e. no effort) condition than in the working (i.e. effort) condition, when referring both to themselves and to others, which confirms our hypotheses. When referring to themselves, 30% chose the leisure option when getting the money by chance, which is 19 points higher than in the working condition, with difference significant at the 1% level (p=0.0000) using a Student t-test. The difference

is even greater when referring to others. They were 67% to state that others would choose the leisure option in the windfall condition, which is 48 points more than in the working condition, also significant at 1% level (p=0.0000). Figure 1 illustrates our findings. Those results strongly support our hypothesis that income framing has an impact on stated preferences.

Figure 1 – Money use among the 4 treatments
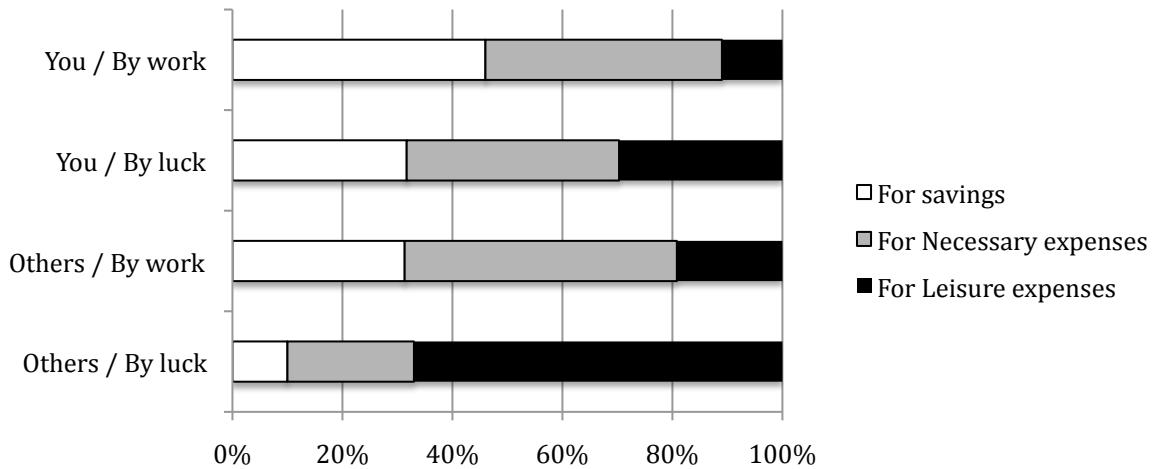
I.3.2. Preliminary work in the field

Taking a step further, we describe in the following part how we built our natural field experiment. The purpose of using natural field experiment is to capture as much of the context as possible and thus increase external validity. List and Hofler (2004) demonstrated that pro social behaviour observed in a laboratory settings were not happening anymore in natural setting, questioning about the external validity delivered by laboratory experiments. Also, laboratory experiments are often observing student's behaviour, which undermines a possible extension of the results to the rest of the population.

A natural field experiment consists in using a non-standard subject pool (as in artefactual field experiment) and in a field context (as in framed field experiment). The main specificity of this type of experiment lies in the fact that participants do not know that they are part of an experiment. It makes this kind of experiment very close to natural settings, limiting suspicion bias (knowing you are

participating in an experiment may eliminate spontaneous behaviour). This increased external validity is however offset by a set of logistical challenges, due to the constraint of ensuring high level of control during the experiment. In the next point, we describe how we overcame those constraints.

I.3.2.1. The field context

At a preliminary stage, we organized a focus group gathering the four VOI's representatives from each village we planned to visit as well as the VOI's president for the province. This focus group aimed at determining an activity that could be organized under the initiative of the VOI and linked to sustainable land use. This activity would build up our natural setting for the experiment. We wanted the activity to be the same among the four villages to reduce potential biases linked to the tasks' attributes.

Considering local characteristics, it turned out to be an activity based on Tapia's forest maintenance, including a set of duties such as path clearance and tree pruning. We also ensured we could schedule all the field experiment within the same week and we agreed on visiting the 4 villages named Ambohimanjaka, Tsarahonenana, Merinavaratra and Ambohijatovo, respectively from Monday to Thursday. The experiment had to be conducted before Friday's weekly market, where most of the villagers meet, in order to prevent contamination biases. Except on Fridays, villagers are unlikely to meet because of long walking distance within the four villages. The activity started early in the morning and lasted about 3 hours. We agreed with all VOI's representatives that the compensation for this half day activity will be an equivalent of 2,500 MGA in in-kind (the equivalent of a day wage for a low skilled worker). During that time, people in the 'Low effort' condition were given the product chosen and participated to other proposed activities[56].

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[56] As the non-effort group had the possibility to return to their farms more quickly and suffered from less foregone income than the effort group, we checked for possible income effects. Table 7 shows that there is no correlation between the choice for luxury goods and the income-related variables, i.e., monthly resources, weekly food expenses and land area.

Moreover, this setting is adequate to create the moral credit condition to test for our second hypothesis ('Moral accounting affects economic decision') while the kind of selected in-kinds (superior vs. necessity goods; see next paragraph for more details) would enable to test the mental accounting related hypothesis.

A month before the experiments, we run another set of studies in Arivonimamo's province to define the in-kinds that would be given to participants, which should symbolize either the hedonic consumption or the utilitarian consumption. Surveyors interviewed a total of 30 people[57] asking for the more necessary goods they were buying on a weekly basis versus the set of goods they do not buy commonly, but more likely for holidays or celebrations' purpose. Results shows that coffee and oil were the most cited items for necessary goods, both part of the weekly purchase. For both products, more than 80% of the respondents considered them as necessary. In the case of the superior good, surveyors asked for items that are not bought weekly, but more likely during holidays and celebration. Interviewed people mainly agreed on clothes and sodas (i.e. more specifically Fanta)[58], in more than 50% of the cases. We then kept the two goods that best suited our experimental constraints, therefore oil and soda[59], since both could be delivered in bottle format, which minimizes a potential packaging bias and helps control for decision anonymity.

I.3.2.2. Experimental strategy

We selected our participants from individuals, sharing a common pool resource and physically able to take part in the maintenance task. We worked in collaboration with the local organization in charge of land's conservation (locally called VOI or '*Vondron'Olona Ifototra*'). This organization supported our work by implementing a half-day activity linked to environmental management settling the basis of our experimentation. This activity was paid with in-kinds (superior vs. necessary

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[57] Other than those who participated to the field experiment.

[58] Clothes were more often cited (about 65% of participants) but less easy to accommodate with the experimental design compared to soda (about 50% of participants).

[59] 2,500 MGA corresponds to 1.5 litre of soda and 1 litre of oil.

goods) and participants were randomly assigned to two conditions, by drawing lots. A first group was constituted of participants that were exempted to work (also referred as the 'Low effort' money condition), while others had to comply with the task before getting their due (the 'High effort' money condition).

A week in advance, villagers were informed by their VOI's representative that a half day activity would take place to maintain Tapia's forest, with a meeting time set up at 6:45am. In exchange they would receive the equivalent of a day wage. Each VOI's representative had in charge to inform at least 40 individuals. The day of the activity, the experimental team met the VOI's representative first and then waited for participants. At 7am[60], the VOI's representative briefly introduced the team (our presence was explained as a punctual support to finance maintenance activities). The instructor then started reading the instructions aloud; thanking villagers for being there and confirming they would receive a 2,500 MGA compensation in in-kind (the equivalent of a day wage) after the activity.

After this brief introduction, we proceed to the randomization, which consisted in allocating people randomly between the two conditions ('Low effort' money vs. 'High effort' money). In order to do so, the audience was informed that due to a high number of participants, the group should be divided into two subgroups, and were distributed an individual number. The team then proceeded to the lottery. The subgroup constituted of non-selected numbers was told that they could start the activity and go to the Tapia's forest, while the other group was told to stay.

Once the group of workers left (they were accompanied by the VOI's representative and one member of the team), the instructor explained that not all participants were needed to work in the forest because they were too many, so they could luckily be exempted to work while still receiving their due wage. Those lucky participants were then in a condition where they got a day wage without any previous effort. The instructor explained that they could choose between two goods, which are

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[60] Participants were very punctual and most of them arrived before 7am. For those arriving later, they were told that unfortunately they could not participate anymore in the activity. For a few that insisted, they were included in the activity and received compensation but the corresponding observation was not included in our database.

oil and Fanta. The experimenter showed the litre of oil that was in a 1.5 litre bottle by taking it out of a black plastic bag and did the same for the Fanta (also in a 1.5 litre bottle). The instructor informed participants that they would receive their in-kind in a black plastic bag too. This setting aimed at avoiding emotional biases due to packaging[61], as well as social image biases. After they receive explanations of the two goods, they were given a paper with drawings of the two goods, and where they should mark the product they wanted. They had about one minute to mark their choice. After the minute elapsed, pencils were collected and they could go to collect their in-kind, individually, and the first experiment ended.

As a second activity, we proposed participants to take part to a socio economic survey. This was a way for us to collect extensive socio economic data. We told participants this survey will be paid 1,000.00 MGA and will last about 20 minutes. All participants agreed to take part in the survey. Questions were linked to their agricultural activities (culture type and farm size), their consumption habits in oil and soda, as well as more general data about their household and expenses.

Once they completed the survey, we gave them an envelope containing the 1,000.00 MGA (in 100 notes), mentioning they could give any part of this amount to their VOI by dropping the envelop in a box that was nearby. They were many options around to hide (houses, trees, etc.) so anyone could get a sense of privacy (what they all did) to split the 10 notes between themselves and the envelop they would then leave in the box.

This additional activity also aimed at testing the impact of the initial experiment on further economic decisions as presented in our second hypotheses. Making the payment separate between the two activities aimed at controlling for a potential remaining house money effect induced from the first activity.

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[61] A wide range of research in marketing has been studying the impact of packaging on consumer choice (Schifferstein et al, 2013).

About 4 hours later, while the other sub group came back from the Tapia's forest activity, the experimental team followed exactly the same procedure as the one just described above (cf. Instructions in Appendix 1 for more details).

I.4. Results

In this result's section, we first present a summary of our data and statistical tests. In a second stage, we proceed to a set of regressions to investigate the determinants of the observed behaviour. As we shall see, the level of hedonic preferences is significantly higher when money is obtained in the 'low effort' condition.

I.4.1. Data summary and statistical tests

I.4.1.1. Sample's characteristics

We collected in total 142 observations across the 4 villages. Participation rate in village 4 was lower than expected due to an unforeseen last minute circumcision's ceremony that happened the same day in a nearby village. Table 2 presents the summary data.

Participants were on average 39.8 years old, 23.94% reached secondary level and men represent 33.10% of the entire sample[62]. Most of participants (75.35%) have monthly resources[63] below 50,000.00 MGA. Weekly food expenses[64] per individual are on average 2,087.765 MGA. The average land size cultivated per farmer is 2.18 Acres. Half of participants were outsiders (i.e. people from Antananarivo province that settled in Arivonimamo since less than one generation). In terms of consumption habits, 100% are 'Oil's regular buyer' (i.e. someone who buys oil for regular purpose.),

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[62] Even if people in charge of inviting participants insisted on the importance to get men for this forest maintenance activity that requires strong physical abilities, men were still more reluctant to cancel their daily activity than women.

[63] Monthly resources were an estimate of how much a household gets per month, classified in four ranges, from low income (below 50,000.00 MGA) to high income (more than 200,000.00 MGA).

[64] Weekly food expenses divided by the number of members belonging to the household.

56.34% are Fanta's buyer' (i.e. someone who has bought Fanta at least once in the past) among which 65% are 'hedonic buyer' (i.e. someone who has bought Fanta for festive purpose -Mother's day, national holiday, birthdays etc.).

Table 2 – Sample characteristics for the four villages.

	TOTAL
Participants	N=142
Gender ratio (% of male)	**33.10%**
Age	**39.84507**
Monthly resources [1]	
<50 000	**75.35%**
50 000 – 100 000	**1.41%**
100 000 – 200 000	**22.54%**
>200 000	**0.70%**
Weekly food expenses / household unit	**2087.765**
Land size	**2.18**
Education level (% of secondary level)	**23.94%**
Outsiders	**50%**
Consumption's habits	
Oil's regular buyer	**100.00%**
Fanta's buyer	**56.34%**
Fanta's hedonic buyer	**36.62%**

[1] Monthly resource is in MGA. 1 Euro = 2.864,43 MGA.

In table 3, we test the presence of statistically significant differences between samples after randomization, controlling with the variables mentioned earlier, using the Kruskal-Wallis H test. This is the non-parametric equivalent of the one-way analysis of variance (ANOVA). Since all p-values are non-significant (column 4), we assume that this sample is equally distributed among control and treatment, which should enable adequate conditions to detect treatment effects. We also looked at correlation between control variables. Using Pearson correlation coefficient, we find strong

correlation between weekly food and monthly resources (r=0.2873, p=0.0005), as well as between weekly food and land size (r=0.1880, p=0.0302). Monthly resources are also correlated with gender and age (respectively r=0.1842, p=0.0282; r=-0.1897, p=0.0238).

Table 3 – Randomization table: Kruskal-Wallis H test of between group differences across participants from the four villages assigned to control and treatment groups.

(N=142)	(1) Control group	(2) Treatment group	(3) H-statistics	(4) P-value
Socio demographic characteristics				
Gender ratio (% of male)	36.98	28.98	0.676	0.4108
Age	40.38356	39.27536	0.136	0.7118
Monthly resources < 50 000 Ar	80.82	71.01	0.858	0.3542
Weekly food expenses per household unit	2098.233	2076.691	0.224	0.6359
Land size	2.9001	1.3801	1.357	0.2441
Education level (% of secondary level)	21.91	26.08	0.184	0.6682
Outsiders	52.05	47.82	0.189	0.6638
Consumption's habits				
Oil's regular buyer	100	100	0.000	1
Fanta's buyer	56.16	56.52	0.001	0.9705
Fanta's hedonic buyer	32.87	40.57	0.627	0.4285

I.4.1.2. Treatment's effects

Our first main result indicates that participants in the 'Low effort' condition were more inclined to choose a soda bottle than participants in the working (i.e. 'High effort') condition, which supports our main hypothesis. The data reports 13.7% of participants who selected the superior good in the 'Low effort' money condition against 2.9% in the 'High effort' money condition as illustrated in Figure 2. The difference is significant at the 5% level, using both Student's t-test (t=2.3407, p|t|=0.0207) and Ranksum test (z=2.304 p|z|=0.0212). Table 4 compares average choice for hedonic option across treatments, using a parametric and a non-parametric test.

Those first results apply to all participants, without considering their consumption's habits (whether they do consume oil and soda, and under which circumstances). As presented earlier, we gathered additional data (i.e. when people lastly bought oil and when they lastly bought Fanta), which facilitates classifying people among different category of consumption's habits.

For instance, looking participants that buy Fanta for 'Hedonic' purpose, the treatments difference is greater, with 29.17% of them choosing soda in the 'Low effort' money condition, versus 7.14% in the 'High effort' money condition, equal to 23 points more, significant at 5% using both tests (t=2.1444, p|t|=0,0369; z=2.073, p|z|=0.0382). Figure 2 illustrates those results. Also, in the 'Low effort' condition, 'Fanta's hedonic buyers' were significantly more likely to select the soda than 'Fanta's regular buyers' (t=-2.5804, p|t|=0.0137; z=-2.415, p|z|=0.0157), but not in the 'High effort' condition. It means that the income's framing had an impact on hedonic choice mainly on people who have a hedonic consumption of this good (i.e. Fanta's hedonic buyers). Taken as a whole, this strengthens the idea that 'Low effort' based money increases consumption of a superior good.

Figure 2 – Choice for a superior good across treatments and samples.
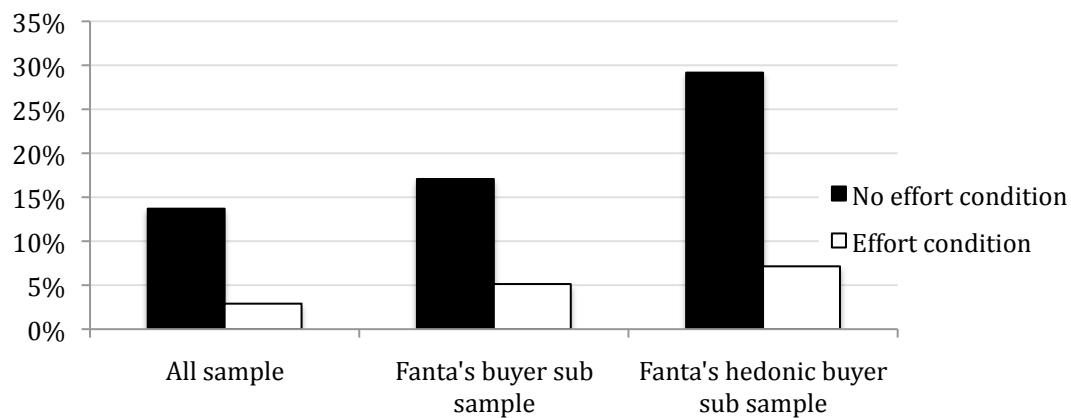
Table 4 – Differences in superior goods preferences across treatments and consumption category, for entire sample and sub samples.

(Student parametric test and Mann-Whitney non-parametric test.)

Comparisons	Percentage of participants selecting the superior good	Student test (t)	p-value	Mann-Whitney test (z)	p-value
Low effort condition vs. High effort condition					
All sample	13.7%/ 2,9%	2.3407	0.0207**	2.304	0.0212**
Buyers sub sample	17.1%/ 5.1%	1.6994	0.0932*	1.679	0.0931*
Festive buyers sub sample	29.2%/ 7.1%	2.1444	0.0369**	2.073	0.0382**
Fanta's festive buyer vs. regular buyer					
Low effort condition	29.1%/ 6.1%	-2.7951	0.0067***	-2.672	0.0076**
High effort condition	7.1%/ 0	-1.7500	0.0847*	-1.724	0.0847*

***, **, * Indicates statistical significance at the 1, 5 and 10 percent significance levels, respectively.

Looking at the public good contribution, happening at the second stage of this experiment, interesting results are rising.

First, positive contribution rate is 100%. Usual contribution rate (i.e. percentage of people who gives something) in standard dictator game are about 60% (Engel, 2011). All participants donated a positive amount, ranging from 100 MGA (48.59% of participants) to 900 MGA (<1% of participants). The average donation is 180.99 MGA, with a standard error of 9.84. Overall, participants shared 18.01% of the pie. Standard results in dictator game show that participants share about 30% of the pie (Engel, 2011). Similar games have been played in developing countries, revealing equivalent results (individuals sharing 31% of the pie, Henrich et al., 2005). Second, effort condition (i.e. 'High effort' condition) and consumption preferences seem both to affect the amount donated to the public good. Participant from the 'Low effort' condition gave on average 50 Ar more than participants from the 'High effort' condition, significant at the 5% level, while participants who chose the superior good gave close to 100 Ar more than people who chose the necessity good, significant at the 1% level. According to the statistical test presented in table 5, both effects appear to act

individually: among the participants who selected the necessity good, those in the 'Low effort' condition gave significantly more than those in the 'High effort' condition (188.89 vs. 158.21, p<0,10%), and among the participants belonging to the 'Low effort' condition, those who selected the superior good donated significantly more than those who selected the necessity good (290 vs. 188.89, p<0.05%). If that previous consumption effect seems stronger than the 'High effort' condition, this would have to be confirmed in the regression analysis.

Table 5 : Differences in pro social preferences across treatments and consumption choice, for entire sample and sub sample

Student parametric test and Mann-Whitney non-parametric test.

Comparisons	Average of donations	Student test (t)	p-value	Mann-Whitney test (z)	p-value
Low effort condition vs. High effort condition					
All sample	202.739/157.971	2.3086	0.0224**	2.686	0.0072***
Necessity good	188.888/ 158.209	1.7181	0.0882*	2.026	0.0427**
Superior good	290/150	0.8483	0.4161	1.390	0.1646
Superior good vs Necessity good					
All sample	266.666/173.076	-2.7040	0.0077***	-2.483	0.0130**
Low effort condition	290/188.888	-2.3127	0.0236**	-2.130	0.0332**
High effort condition	150/158.209	0.1201	0.9048	-0.144	0.8857

***, **, * Indicates statistical significance at the 1, 5 and 10 percent significance levels, respectively.

I.4.2. Regression analysis

We perform a Probit[65] regression for the determinants of luxury goods preferences, taking into account variables that could be expected to affect such behaviour. The two regressions for the

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[65] Because our dependant variable is binary, a Probit model is preferred.

determinants of luxury goods preferences are shown in Table 6[66]. Table 7 reports the marginal effects.

$$Pr\ (Y=1 \mid X_1, X_2,...,X_n) = F\ (\theta_0 + \theta_1 X_1 + \theta_2 X_2, ..., + \theta_n X_n)$$

The probit results support our findings. Being in the 'High effort' group decreases significantly by a coefficient of 0.127 the probability to choose the luxury good (p=0.000), while being a « Non regular buyer » increases significantly by a coefficient of 0.124 the probability to choose the superior good (p=0.014). We find no other effect relative to gender, age, education, income, or origin within the total sample data set. In model 2, we excluded correlated data (i.e. Monthly resources, land size and weekly food expenses) and kept only one indicator for income. Model 2 supports that the consumption decision is not due to a potential income effect. In addition, the hypothesis that consumption choice is linked to wealth is rejected at the 10% level for the 3 indicators using student t-test[67]. We also control for village effect. But the Kruskall-Wallis H test reports no difference in the distribution of the hedonic choice among the 4 villages. (χ^2=0.425, p=0.9350). In sum, the Probit model validates our first hypothesis, according to which mental accounting plays a role in economic decision.

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[66] Regression in the second model keeps only variables that were significant in model 1.
[67] Land size: 2.23 vs. 1.60, p= 0.5711; Weekly food expenses: 2,152.283 vs. 1,388.823, p=0.13 ; Monthly resources (<50,000 MGA) 0.083 vs. 0.253, p=0.19.

	(1) Model 1		(2) Model 2		(3) Model 3	
Superior good						
Effort condition	-1.035**	(-2.30)	-1.024**	(-2.46)	-1.417***	(-6.57)
Gender	0.206	(0.51)	0.140	(0.37)		
Age	0.000167	(0.01)	0.00190	(0.17)		
Monthly resources [less than 50 000 Ar]	-0.689	(-1.06)				
Weekly food expenses per household unit	-0.000181	(-0.94)	-0.000228	(-1.44)		
Land size	-0.0687	(-0.81)				
Education level [greater than secondary level]	0.355	(1.07)	0.235	(0.75)		
Outsiders	-0.163	(-0.40)	-0.126	(-0.35)		
Festive Buyer	1.116***	(2.78)	1.075***	(2.75)	1.032***	(2.89)
Constant	-0.692	(-0.53)	-0.699	(-0.61)		
Observations	132		141		142	
BIC	107.0		101.8		77.45	
chi2	22.30		19.83		55.66	
p	0.00798		0.00596		8.18e-13	

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Probit regression - Superior Good (0/1) - Marginal effects

	(1) Model 1		(2) Model 2		(3) Model 3	
Effort condition	-0.0791**	(-1.98)	-0.0846**	(-2.31)	-0.127***	(-3.75)
Gender	0.0157	(0.48)	0.0115	(0.36)		
Age	0.0000128	(0.01)	0.000157	(0.17)		
Monthly resources [less than 50 000 Ar] (d)	-0.0405	(-1.26)				
Weekly food expenses per household unit	-0.0000138	(-0.95)	-0.0000188	(-1.47)		
Land size	-0.00525	(-0.83)				
Education level [greater than secondary level]	0.0271	(1.02)	0.0194	(0.73)		
Outsiders (d)	-0.0124	(-0.41)	-0.0104	(-0.35)		
Festive Buyer (d)	0.119**	(2.10)	0.122**	(2.25)	0.124**	(2.46)
Observations	132		141		142	
BIC	107.0		101.8		77.45	
chi2	22.30		19.83		55.66	
p	0.00798		0.00596		8.18e-13	

Marginal effects; *t* statistics in parentheses

(d) for discrete change of dummy variable from 0 to 1

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

To understand further the determinant of donation, we performed a censored Tobit regression[68].

Table 8 reports the result. Various experimental works that studied cooperation provide cues for

control variables. To start with, Eckel and Grossman (1996) show that women behave more

cooperatively than men, suggesting we might expect a gender effect in donations. Second, we might

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[68] A censored regression model enables to take into account that the dependent variable can take any value from 0 up to 1,000.

expect different cooperation's level according to participant's origin. While the majority of our subjects' come from Arivonimamo's province, still some migrated from Antananarivo's province. We might therefore have a lower level of cooperation among people that are recently settled in the area. Age might also have a role to play. List and Karlan (2007) suggest that older people cooperate more than younger ones. In addition, it has been shown that donation increase with income and education (Hofmeyr et al 2007, Bekkers 2006).

First of all, the regression supports our second hypothesis. People who did an effort for their VOI's in a first stage (i.e. 'High effort' condition), contributed less than those who did not have to do a real effort (i.e. 'Low effort' condition). With a coefficient of 35.55, people in the 'High effort' condition decreased significantly their donations (p=0.059) compared to people that were in the 'Low effort' condition. However, the fact that individuals in the 'Low effort' group gave significantly more than individuals in the 'High effort' group (202.73 vs 157.97, p=0.0224) could also have other explanations. If the experiment was designed to isolate the 1,000 MGA endowment (corresponding to the interview) from the previous endowment (corresponding to the activity), one cannot exclude that participants considered it as one single endowment. Therefore, a house money effect might also be at play (i.e. people are more willing to donate when the endowment is not linked to any previous effort; Reinstein and Riener, 2011).

Interestingly, when restricting the analysis on the 'Low effort' group, we find that participants who selected the superior good gave significantly more than participants who selected the necessary good (290 vs. 188.88, p= 0.0236), which supports our hypothesis 2 bis. Individuals who lost moral credits through previous self-indulgent choices are more likely to make more cooperative choices subsequently than individuals who did not lose moral credits.

Age is significant in model 1, but the parameter became non-significant in the second model, that kept only significant variable from Model 1. We also find an income effect, indirectly measured through weekly food expenses, which appears to be robust across the two models.

Table 8: Censored Tobit regression - donation amount [0;1000]

	(1) Model 1		(2) Model 2		(3) Model 3	
Effort condition	-36.33*	(-1.77)	-37.71**	(-1.99)	-35.55*	(-1.90)
Gender	4.941	(0.23)	11.88	(0.59)		
Age	-1.057*	(-1.76)	-0.919	(-1.62)	-0.841	(-1.54)
Monthly resources [less than 50 000 Ar]	18.04	(0.73)				
Weekly food expenses per household unit	0.0123*	(1.92)	0.0142**	(2.52)	0.0134**	(2.39)
Land size	0.583	(0.20)				
Education level [greater than secondary level]	-8.453	(-0.49)	-5.771	(-0.35)		
Outsiders	-6.857	(-0.34)	-13.79	(-0.75)		
Superior good	96.94***	(2.68)	93.78***	(2.66)	91.28***	(2.69)
Festive Buyer	-3.678	(-0.18)	-7.850	(-0.39)		
Constant	244.0***	(3.67)	233.8***	(3.86)	231.6***	(5.71)
sigma						
Constant	109.2***	(16.25)	108.5***	(16.80)	109.1***	(16.85)
Observations	132		141		142	
BIC	1672.3		1771.4		1765.4	
chi2	21.21		21.32		19.38	
p	0.0197		0.00635		0.000663	

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

I.5. Conclusion

The use of economic incentives for biodiversity has been supported since the end of the 1990s in international arenas (CDB, UNEP, IUCN, OECD...). Since the emergence of the Ecosystem Services concept and mainly from Millennium Ecosystem Assessment and the TEEB process between 2005 and 2010, economic incentives (mostly Compensation and Reward for Environmental Services including Payment for ES) became the main innovative policy tools for biodiversity conservation worldwide.

However, efficiency of these measures is not well documented yet (Pattanayak, Wunder et al. 2010). This lack of feedback can be explained in two ways. On the one hand, we ignore long-term impacts on recipients' behaviours when they receive payment (compensation or reward) in cash. On the other hand, economic methods for assessing or measuring these behavioural changes are only now

starting to be explored by institutions or experts in charge of such assessments (Clements, John et al. 2010, d'Adda 2011, Travers, Clements et al. 2011, Narloch, Pascual et al. 2012).

Then, the key challenge for policy makers is the following: compensation and reward for environmental services are designed under the assumption that people would react as given in the rational choice theory, with an utility function based on profit maximization, associated to stable and exogenous preferences. Practitioners, donors, experts acknowledge that decision-making process is more complex but they don't know how to assess these behaviours. Natural field experiment can provide evidence that decision-making might, under some circumstances, follow different rules. Indeed, our work shows that the circumstances under which someone receives money and the motives behind involvement lead to different economic behaviours. Money obtained under low effort will lead to very different consumption patterns than money obtained under high efforts: luxury goods would be more salient in the case of windfall money. Moreover, money obtained under low effort also leads to subsequent higher pro social behaviour.

Enlarging the main focus of our investigations, this work leads us to a wider discussion on moral accounting, with results that raise evidence for both self-licensing and moral cleansing effect, in relation with the pro social motives and collective action dimension of conservation policies. But more than giving a clear and definite conclusion on how CRES should be implemented, this work mainly demonstrates that contextual environment and behavioural factors could challenge policies achievements at some point.

Finally the key output of our research should be that policy analysis which assumes money fungibility (i.e. a single marginal propensity to consume) may misestimate the impacts of compensation mechanisms.

Acknowledgement

Part II. Present bias predicts participation in payments for environmental services: Evidence from a behavioural experiment in Uganda[69]

II.1. Introduction

Farmers are necessary agents in global efforts to conserve the environment now that croplands and pastures together constitute the largest terrestrial system on Earth – covering some 48% of ice-free land surface (Ellis, 2011; Ellis, 2013). Recent evidence indicates that fine-scale forest elements interspersed with agricultural and grazing areas can confer a multitude of benefits such as building material, fuel, territory for crop pollinators and pest enemies, and biodiversity (Karp et al., 2013; Mendenhall et al., 2011). These forest elements also modulate hydrologic, sedimentary, and carbon cycles. To uproot them would eliminate each benefit and worsen such problems as flooding, infectious disease, and global warming. Accordingly, multimillion-dollar Payments for Environmental Services (PES) programs that subsidize farmers for keeping forest have traction around the world (Ferraro, 2011; Pattanayak et al., 2010). But why do farmers participate in PES?

Standard economic theory posits that individuals make decisions by optimizing a utility function in which certain constraints, principally limits on income and wealth, are balanced appropriately (Simon, 1959). Hence, most research concerning PES participation emphasizes the relative

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importance of income- and wealth-related variables, including debt, credit constraints, liquidity, land holdings, and property rights (Arriagada et al., 2012; Kosoy et al., 2007; Ma et al., 2012; Muradian et al., 2010; Pagiola et al., 2010; Vatn, 2010; Wunder, 2008; Zbinden and Lee, 2005). These studies assume that individuals will participate in PES if the payment offered for conservation exceeds the sum of their opportunity and transaction costs (Adams et al., 2010; Engel et al., 2008; Wunscher et al., 2011). Other studies examine nonmonetary correlates of participation such as age, education, social capital, and prior experience with environmentally beneficial practices, but these occupy a smaller portion of the literature (Gong et al., 2010; Ma et al., 2012; Pagiola et al., 2010). Still, much of the variation in PES participation decisions remains unexplained. Meanwhile, empirical findings from behavioural economics point to a number of normally unobservable preferences – distinct from (and yet conditioned by) the economic circumstances in which farmers find themselves – that merit attention (Venkatachalam, 2008).

Take trust, for example. Trust is one among several "slightly old-fashioned virtues" that Nobel Laureate Kenneth Arrow identified as intrinsic to the operation of the economic system (Arrow, 1972; Arrow, 1974). "Every commercial transaction," he wrote in 1972, "has within itself an element of trust." Many scholars have since substantiated Arrow's assertion that trust is an important "lubricant" in economic markets (Andreoni, 2005; Karlan et al., 2009; Ostrom and Walker, 2003; Putnam et al., 1993; Zak and Knack, 2001). Yet a prime, and understudied, subject is the importance of trust in the market for goods and services supplied by Nature – especially in developing countries. In these places, property rights are often tenuous. Without trust, farmers may fear that by leasing their land for conservation, they may be surrendering their tenure altogether. Although some PES studies claim that trust is a key input, quantitative evidence of this is sparse (Asquith et al., 2008; Tesfaye and Brouwer, 2012).

A second subject concerns intertemporal decision-making. Behavioural economics research has shown that, in contexts where individuals must allocate a budget between a sooner date and a later date, the decision depends on temporal proximity (Thaler, 1981). When the sooner date is in the distant future, individuals will often allocate a relatively smaller share to the sooner date, and a larger share to the later date. But when the timing of the payment moves closer to the present, these preferences reverse so that a larger share is allocated to the sooner date, and a smaller share is allocated to the later date (Figure 1). This reversal, referred to as hyperbolic discounting or present-bias, contradicts the standard economic assumption that individuals are consistent in their time preferences (Frederick et al., 2002; Laibson, 1997; O'Donoghue and Rabin, 1999). Recent evidence suggests that present-biased individuals may be prone to suboptimal financial behaviour. Meier and Sprenger (2010a), for instance, observe that present-biased individuals are more likely to have credit card debt, and to have significantly higher amounts of credit card debt, controlling for income, other socio-demographics, and credit constraints. Designing public programs under the assumption that beneficiaries are consistent in their time preferences is therefore problematic, particularly if heterogeneity in these preferences correlates with program participation. To date, however, the relationship between PES participation and individual time preferences has not been empirically examined despite circumstantial evidence that it may be important. Consider, for instance, Costa Rica's – oversubscribed – *Pago de Servicios Ambientales* reforestation program, which offers farmers a large upfront payment in the first year followed by smaller annual payments in later years, as compared to China's – under-subscribed – Guangxi Watershed reforestation program, which only offers annual payments (Gong et al., 2010).

This study tests, for the first time, whether heterogeneity in behavioural preferences correlates with PES participation decisions. We elicit the individual trust and time preferences of 263 farmers in rural Uganda using economic experiments that previous studies in rural settings have linked with real-world decisions (Ashraf et al., 2006b; Barr and Zeitlin, 2010; Karlan et al., 2009). We then match the

experimental measures to PES enrolment data and investigate their association with PES participation. We find that, among our sample of Ugandan farmers, experimentally measured trust preferences do not correlate with PES participation. By contrast, time preferences significantly predict PES participation. This result has implications for ongoing and planned PES programs involving farmers, particularly in Africa, and validates the use of behavioural experiments in explaining real-world decisions (Rustagi et al., 2010).

Figure 1. Illustration of Present-Biased Preferences
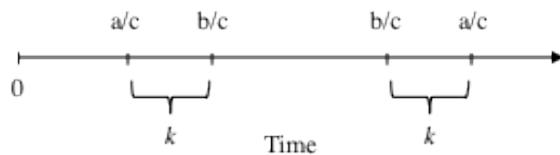
Notes: Here, k *is a period of time (for example, 5 weeks),* c *is a budget, and both* a *and* b *are budget shares such that* a + b = c *and* c > a > b.

II.2. Material and Methods

II.2.1. Data

II.2.1.1. Study Area

The study took place in Masindi District, western Uganda, where the Trees for Global Benefits program (TGB) is paying farmers to plant trees.

Uganda is located between the 29th and 35th longitudes east of the Prime Meridian in Equatorial East Africa. Its rainfall averages 900mm in the east to 2,000mm in the west (Grant, 1872). This ample supply of moisture sustains a substantial agricultural area, where farmers cultivate fast-growing cash crops, including bananas, beans, and sugar cane. Besides edibles, agricultural land contains trees and

forest remnants that store a quarter of the country's carbon stock (Avitabile et al., 2012; Drichi, 2003). Some farmers plant trees on their farms and then sell the sequestered carbon as credits to companies that wish to offset their greenhouse gas emissions. Though more than 200,000 farmers in 24 countries across Africa are selling such 'carbon offsets', Uganda has the fourth largest market share in the world (Molly Peters-Stanley et al., 2012). Carbon offset advocates trumpet the extra income and other benefits Ugandan farmers may collect from planting trees and selling carbon (Karuga, 2012; Planet, 2013). Critical observers cite an instance in which privately held forested land was forcibly appropriated by a large corporate carbon investor (Delingpole, 2011; Faris, 2007; Kron, 2011; McGroart, 2011) – a familiar experience to farmers previously dispossessed of their land by British colonists.

Notwithstanding these mixed views, TGB is generally well-regarded among Uganda's carbon offset programs (Masiga et al., 2012; Peskett et al., 2011; Reynolds, 2012). Since its 2003 inception with 33 farmers in Bushenyi District, the program has enlisted over 2,100 farmers, expanding north into Kasese, Hoima, and Masindi Districts. According to independent audits conducted by the Rainforest Alliance in 2009 and 2013, TGB is effectively and transparently governed by staff who are knowledgeable about the scientific and social aspects of large-scale farmer-based reforestation programs. The audits further confirm that program payments are traceable to farmers and that farmers understand the program's structure and requirements (Hayward et al., 2009; Masiga et al., 2012; Moore, 2014).

Throughout Masindi, TGB has been contracting private landholders to plant trees since 2008. The contracts stipulate the number and species of planted trees, the price of carbon, and the payment schedule (German et al., 2010; Nakakaawa et al., 2009; Peskett et al., 2011). Each seller must establish at least 400 seedlings. Seedlings must be indigenous African species or selected fruit trees. Common species are *Maesopsis eminii*, *Erythrina abyssinica*, *Ricinus communis*, *Markhamia*

platycalyx, *Persea americana* (avocado), and *Mangifera indica* (mango). Farmers are paid, on average, between 4 USD and 10 USD per ton of carbon. The precise price is negotiated between TGB, which acts on behalf of farmers, and buyers. Buyers include Standard Chartered Bank Uganda and the City of London, among others. Payments to farmers are staggered as follows: 30% upfront, 20% after the 1st year, 20% after the 3rd year, 20% after the 5th year, and 10% after the 10th year.

II.2.1.2. Study Sample

Individuals were recruited for the study according to three criteria: tribe, language, and land tenure. Tribal affiliation and linguistics are important considerations because Uganda has 51 tribes and 31 languages, several of which are mutually unintelligible (Ladefoged, 1992). Within Masindi, we selected 13 villages in 5 sub-counties where ethnic Banyoro are the predominant tribal group and Runyoro is the most widely spoken language. In each village, we asked local officials to invite between 20 and 24 landowners to take part in a voluntary study on economic decision-making. Potential subjects were told that the study would take 4 hours and they would earn approximately UGX 3,500.00 (USD 1.35) plus a UGX 1,500.00 thank-you payment, the average equivalent of two day's wages in rural Uganda (2006). In total, the study was conducted with 282 individuals, 94 of whom were TGB participants, 188 of whom were not. Because individuals owning less than 1 acre of land are effectively ineligible for the TGB, we removed 19 observations from the sample. The remaining 263 individuals constitute our primary study sample.[70]

The socio-demographic characteristics of individuals in our primary study sample were collected in a post-experiment survey and are presented in Table 1 Panel A. The average individual is 43 years old, has completed primary school, carries some household debt, and lives less than 1 km from a paved

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[70] The median daily wage of farmers in our primary study sample is 2,412 UGX. The study offered more than two full day wages for a half-day activity to increase the likelihood of obtaining a representative sample of landowners.

road (Column 1). The table divides the sample into TGB participants (Column 2; N=92) and non-participants (Column 3; N=161). T-tests show whether the two groups differ in any of the characteristic variables (Column 4). TGB participants have significantly more land (*p=0.00*; *t*-test) and longer tenure (*p=0.03*; *t*-test) than non-participants. Non-participants report significantly lower trust in NGOs (*p=0.06*; *t*-test).

II.2.2. Experimental Sessions

Critical to implementing field experiments, particularly in rural areas in developing countries, are subject comprehension and instructor training. We took 5 steps to ensure both. First, all instructions, oral and written, were translated from English into Runyoro and "back-translated" from Runyoro into English by a second translator without access to the original text. Second, 2 bilingual focus groups comprised of Makerere University students further polished these instructions. Third, a survey supervisor trained for 3 days with the authors, on both the English and Runyoro instructions. Fourth, 3 Runyoro-speaking instructors trained for 6 additional days with scripts that explained the decision tasks. Fifth, the supervisor and instructors administered mock experimental sessions in 3 villages outside the study area.

13 real experimental sessions were administered between March 22 and April 5, 2013.[71] No more than 1 session was conducted per day. To attract a representative sample of landowners in Masindi, all sessions took place in a central location like a school or community building that was scouted in advance.[72] The sessions typically began around 9 am so that subjects could attend to their farms and other obligations in the afternoon. Upon arrival, subjects were given a clipboard, a pencil, and a tag with an identification number on it. They were then escorted to their seats, which were spaced apart

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[71] If this period is a particularly food-insecure time of year for farmers, our results may be biased. According to the Famine Early Warning Systems Network, Uganda's food security outcomes for March 2013 were favorable due to an above-average harvest, fully stocked markets, and low retail prices.

[72] As in most consented experiments where subjects are allowed to select into the study, we cannot claim that our sample is perfectly representative of Masindi landowners.

such that no subject could see another subject's decisions. They were welcomed and read an introductory script detailing the experimental procedures and the method of payment, which utilized a 'within-subject random incentive system'.

Each experimental session encompassed 5 decision tasks: the Investment Game (trust and reciprocity), two adaptations of the Dictator Game (altruism), Convex Time Budget (time preferences), and Gamble-Choice (risk). The tasks were deployed in a fixed sequence with the individual games preceding the dyadic games. The survey supervisor administered all 5 tasks in all 13 sessions to eliminate experimenter effects. Each task was carried out like a standard experiment such that subjects were read scripted instructions, reminded of the method of payment, tested on their understanding of the task with examples, and invited to ask questions in private before making their decision. Questions were addressed by the instructors who would re-read the relevant parts of the scripted instructions. Instructors were trained to carefully explain the examples and re-read the scripted instructions so as not to lead subjects to a particular solution with suggestive language. Each instructor also carried a 18'' x 24'' laminated poster displaying the examples that allowed subjects to visualize each decision task.[73]

II.2.3. Experimental Procedures[74]

Importing laboratory experiments into rural settings is an important challenge. To the extent possible, we employed experimental procedures that other researchers have used to predict policy-relevant economic behaviour in developing countries (Ashraf et al., 2006b; Barr and Zeitlin, 2010; Karlan et al., 2009).

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[73] The employed methodology appears to have aided subject comprehension. One potential way of measuring miscomprehension is to measure the number of nonsensical decisions. For example, if a decision task asks subjects to choose an amount of money between 0 and 3,000.00 UGX, any response outside this range is nonsensical. Only 2 such responses occurred.

[74] See Appendix 2 for detailed instructions.

II.2.3.1. Trust Preferences

Trust preferences were elicited using the Investment Game, which is the standard decision task to estimate trust in behavioural economics (Andreoni, 2005; Berg et al., 1995; Guth et al., 1997; Johnson and Mislin, 2011; Karlan et al., 2009; Ostrom and Walker, 2003). The game consists of a sequential money transfer between two players – "Player 1" and "Player 2" – without assurance of external enforcement. Both players begin the game with equal endowments. Player 1 decides whether to transfer a portion of this endowment to Player 2 and how much to transfer. Whatever money Player 1 transfers is tripled by the experimenter and passed to Player 2, who then has the opportunity to return money to Player 1. To differentiate between transfers motivated by trust from transfers motivated by altruism, subjects separately play a modified version of the Dictator Game known as the Triple Dictator Game (TDG) (Ashraf et al., 2006a; Cox, 2004). The experimental setting in the TDG is identical to the Investment Game, except that in the TDG whatever amount Player 1 transfers to Player 2 determines the payoffs for both players. In keeping with the strategy method, all subjects play as Player 1 and as Player 2 in both games. Throughout, player identities are concealed to control for confounding factors, such as reputation outside the experiment, that may influence trust behaviour (Johnson and Mislin, 2011; Roth, 1995).

We compute two trust measures. The first is the money subjects' transfer as Player 1 in the Investment Game, expressed as a proportion of their initial endowment. The second is the difference between the money subjects transfer as Player 1 in the Investment Game and as Player 1 in the TDG, also expressed as a proportion of their initial endowment.

II.2.3.2. Time Preferences

Time preferences were elicited using the Convex Time Budget (CTB) task (Andreoni and Sprenger, 2012a, 2012b). In the pen-and-paper version of this game, subjects work through two forms, each

with five decisions, concerning payments over near- and long-term time frames. Each decision features three options offering a sooner payment at time t, at, and a later payment at time t + k, at+k, k > 0, at a fixed gross interest rate. In the first form, t is 1 day and k is 35 days; in the second form, t is 36 days and k is 35 days. All decisions have the same experimental budget, m, such that m/at is the gross interest rate, which varies in value from 1.0 to 1.43. This setup mitigates the concern that subjects might behave differently because some individuals have a better ability to calculate interest. The visual presentation of options does not require individuals to understand interest but instead simply offers them choices between different sums of money at different dates. Hence, while the interest rate is manipulated across decisions, subjects do not have to compute the interest rate themselves (Carvalho et al., 2013). Table 1 presents the CTB decisions. Figure 2 presents a sample CTB form. The forms display calendar dates for the sooner and later payments so that subjects can visualize the delay length for each decision.

We define two measures of time preferences, Budget Share 1 and Budget Share 2, which effectively estimate the average budget share that subjects allocate to sooner payment dates in the near- and long-term time frames, respectively. Note that these capture impatience such that a more impatient person allocates a larger budget share to sooner payment dates. We then define a measure of present bias as the difference between Budget Share 1 and Budget Share 2.

Because of the identified differences between time and risk preferences, we also elicited individual measures of risk preferences using a gamble-choice task (Eckel and Grossman, 2008). In this task, subjects choose between six gambles. Each gamble carries a 0.50 probability of a low outcome and a 0.50 probability of a high outcome. Gamble 1 offers a safe option involving a certain return with no risk. Gambles 2 to 5 increase linearly in both expected return and risk. Gamble 6 offers the same expected return as Gamble 5, but with more risk. We derive a measure of relative risk aversion by ranking subjects on a scale of 1 to 6 according to their selected gamble: subjects who select Gamble

1 are classified as risk-averse; subjects who select Gambles 2 to 5 are classified as risk-neutral;

subjects who select Gamble 6 are classified as risk-seeking.

Table 1. Convex Time Budget (CTB) Decisions

Decision	t	k	m	a_t	a_{t+k}	m/a_t
1	1	35	4000	4000	4000	1.00
2	1	35	4000	3800	4000	1.05
3	1	35	4000	3600	4000	1.11
4	1	35	4000	3200	4000	1.25
5	1	35	4000	2800	4000	1.43
6	36	35	4000	4000	4000	1.00
7	36	35	4000	3800	4000	1.05
8	36	35	4000	3600	4000	1.11
9	36	35	4000	3200	4000	1.25
10	36	35	4000	2800	4000	1.43

Notes: The table shows the basic CTB budgets: t *is the number of days until the sooner payment,* k *is the number of days between the sooner payment and the later payment,* m *is the experimental budget,* a_t *is the value of the sooner payment,* a_{t+k} *is the value of the later payment,* m/a_t *is the gross interest rate.*

Figure 2. Sample Convex Time Budget Decision Form

II.3. Results

II.3.1. Experimental Results

Table 2 Panel B presents the behavioural characteristics of individuals in our study sample as measured in the experimental sessions. In the following paragraphs, we evaluate the extent to which the measures that we collected correlate with TGB participation decisions. We also contextualize our measurements with findings from other studies.

Table 2. Summary Statistics of TGB participants and non-participants

		(1) All			(2) Participants			(3) Non-Participants		2 vs. 3
	N	Mean	(sd)	N	Mean	(sd)	N	Mean	(sd)	P-value
Panel A. Socio-Demographics										
Land Area[1/2]	263	2.67	(1.68)	92	3.33	(2.25)	171	2.31	(1.14)	0.00***
Income[1/2]	263	20.49	(22.77)	92	24.76	(33.19)	171	18.19	(13.95)	0.03**
Decision Power Over Land (years)	260	16.70	(13.88)	91	19.27	(13.87)	169	15.31	(13.72)	0.03**
Trust in NGO[†]	260	2.59	(1.08)	92	2.42	(1.07)	168	2.68	(1.08)	0.06*
Household Size	262	6.69	(3.53)	91	7.24	(3.58)	171	6.40	(3.47)	0.07*
Age	262	42.90	(13.94)	92	44.79	(13.28)	170	41.87	(14.21)	0.11
Gender (male= 1)	262	0.79	(0.41)	92	0.82	(0.39)	170	0.78	(0.42)	0.46
Education[††]	262	1.60	(0.83)	92	1.67	(0.88)	170	1.55	(0.81)	0.26
Distance to Market (km)	263	3.48	(2.68)	92	3.48	(2.68)	171	3.48	(2.63)	0.99
Distance to Paved Road (km)	263	0.84	(1.90)	92	0.73	(0.97)	171	0.89	(2.25)	0.49
Difficulty acquiring money[†††]	262	2.88	(0.89)	92	2.99	(0.82)	170	2.82	(0.92)	0.15
Outstanding loans (=1)	263	0.39	(0.49)	92	0.45	(0.50)	171	0.36	(0.48)	0.16
Savings (=1)	263	0.62	(0.49)	92	0.67	(0.47)	171	0.59	(0.49)	0.19
Trust in Family[†]	259	1.50	(0.64)	91	1.52	(0.62)	168	1.49	(0.65)	0.79
Trust in Community[†]	263	2.56	(0.95)	92	2.53	(0.99)	171	2.57	(0.93)	0.74
Trust in Government[†]	262	2.16	(0.87)	92	2.21	(0.94)	170	2.13	(0.83)	0.50
Panel B. Behavioral Preferences										
Budget Share 1: tomorrow & 5 weeks	262	0.26	(0.31)	91	0.30	(0.33)	171	0.23	(0.30)	0.08*
Budget Share 2: 5 weeks & 10 weeks	263	0.27	(0.34)	92	0.26	(0.33)	171	0.28	(0.34)	0.71
Present Bias (Budget Share 1 - Budget Share 2)	262	-0.01	(0.32)	91	0.05	(0.27)	171	-0.05	(0.34)	0.03**
Trust (proportion sent)	262	0.51	(0.29)	92	0.51	(0.29)	170	0.51	(0.30)	0.98
Reciprocity (proportion returned)	262	0.18	(0.08)	91	0.18	(0.08)	171	0.18	(0.08)	0.56
Altruism (proportion sent)	263	0.40	(0.30)	92	0.42	(0.29)	171	0.40	(0.31)	0.43
Risk Averse (=1)	263	0.62	(0.49)	92	0.57	(0.50)	171	0.64	(0.48)	0.32
Risk Neutral (=1)	263	0.12	(0.32)	92	0.16	(0.37)	171	0.09	(0.29)	0.10
Risk Seeking (=1)	263	0.27	(0.44)	92	0.27	(0.44)	171	0.26	(0.44)	0.88

Land area and income are square-root transformed. N is the number of farmer observations. Standard deviations of the mean are in parenthesis. Asterisks denote statistical significance levels: *** p<0.01, ** p<0.05, * p<0.1.

[†]*Trust estimates are coded as: completely (=1), a lot (=2), a moderate amount (=3), a little (=4), not at all (=5).*

[††]*Education is coded as: primary level (=1), secondary level (=2), tertiary level (=3), university level (=4).*

[†††]*Difficulty Acquiring Money is coded as: very difficult (=1), moderately difficult (=2), moderately easy (=3), easy (=4).*

First, we examine the relationship between trust and TGB participation. Considering only the data gathered in the Investment Game, trust behaviour between TGB participants and non-participants is statistically indistinguishable (p=0.98; t-test). Adjusting for altruism, by taking the difference between the money transferred in the Investment Game and the TDG, trust is still insignificant. Given that a large amount of prior work in behavioural economics has measured trust with procedures similar to ours, it is important to juxtapose our findings with those obtained in prior exercises. We do this by comparing our results with statistics compiled by Johnson and Mislin (2011) in their meta-analysis of 161 laboratory and field applications of the Investment Game. Importantly, the average trust

behaviour we observe, albeit more variable, corresponds closely to the trust behaviour previously collected in the behavioural economics literature. In our sample of 263 subjects, the mean amount transferred by Player 1s ("trustors") is 0.51 (SD=0.29). In Johnson and Mislin's aggregated sample of 23,000 subjects, the mean amount transferred by Player 1s is 0.50 (SD=0.12). This consistency gives us confidence that we correctly estimated trust as measured by the Investment Game, even though it did not deliver a correlation with TGB participation.

Next, we assess the association between time preferences and TGB participation. Overall, the sample appears time consistent, allocating, on average, 26.5% of the experimental budget to the sooner payment date in both the near- and long-term time frames (Table 2). Despite this overall trend, sub-sample inspection reveals remarkable variation. Almost a third (27%) of subjects is present-biased, a fraction found by similar behavioural economics field studies (Ashraf et al., 2006b; Meier and Sprenger, 2010a, b). Figure 3 shows the average budget share that TGB participants (gray) and non-participants (white) allocate to sooner payment dates against the gross interest rate, $1 + r$, for each CTB decision. The two experimental values of t, $t = 1, 36$ days, are shown in separate graphs (Figure 3 C and D). For both values of t, the budget share allocated to the sooner date declines monotonically with the interest rate. However, among TGB participants, when $t = 1$, the average budget share allocated to the sooner date is substantially higher as compared to when $t = 36$, indicating a largely present-biased population. In contrast, among non-participants, the average budget share allocated to the sooner date increases between the two time frames, indicating a largely future-biased population. Across gross interest rates, the difference in dynamic inconsistency between participants and non-participants is on the order of 4.24% of each budget (Figure 4; Table 2 Column 4 p=0.03).

Figure 3. Demand for Sooner Payments Among Participants and Non-Participants
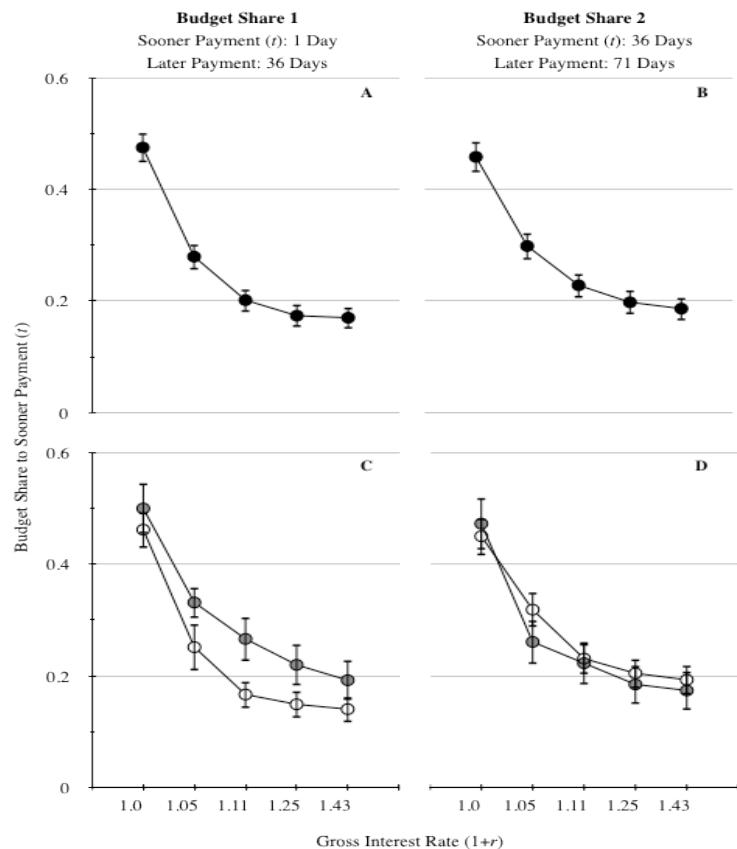
Notes: Circles are the mean budget share subjects allocate to sooner payment dates at each gross interest rate. All landowners (black), TGB participants (gray), non-participants (white). Error bars represent 95% confidence intervals.

Figure 4. Present Bias Among Participants and Non-Participants
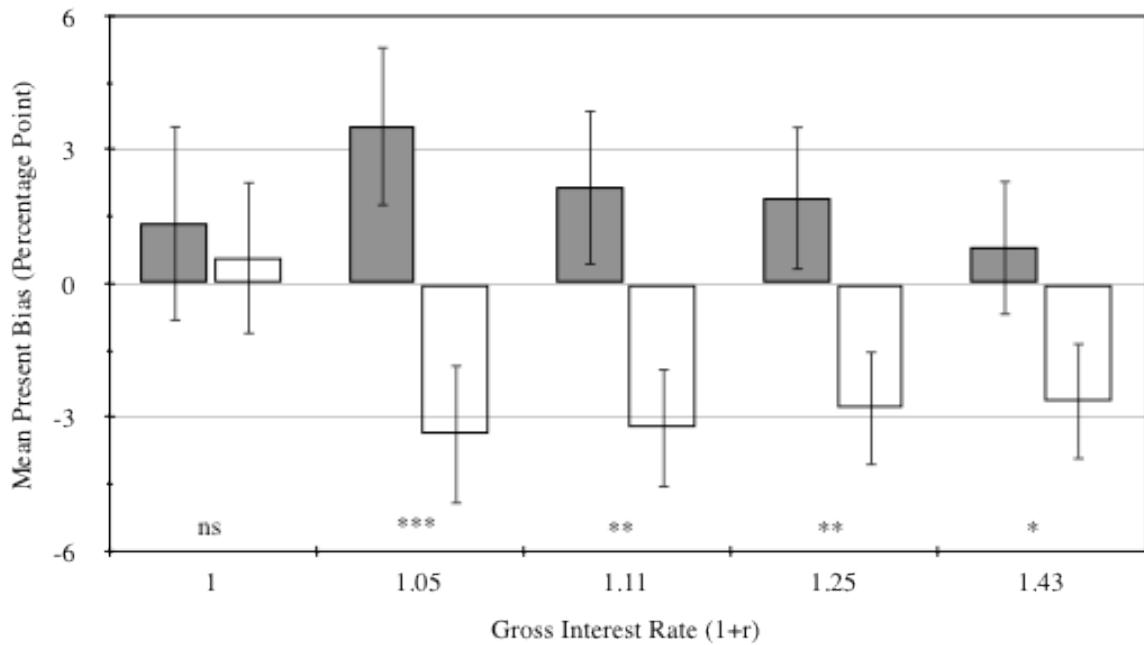
Notes: Present bias is the additive difference between Budget Share 1 and Budget Share 2. TGB participants (gray), non-participants (white). Asterisks denote statistical significance levels: *** p<0.01, ** p<0.05, * p<0.1.

II.3.2. Empirical Results

We investigate the relationship between heterogeneity in behavioural preferences and TGB participation decisions with logit models.

Beginning with trust, we encode TGB as a binary 1/0 variable that indicates whether an individual is or is not a TGB participant. We let i and v index the individual and village-level observations, respectively. We first estimate a regression of the form

$$\ln \left(\frac{P_{TGB}}{1 - P_{TGB}} \right)_{iv} = \alpha + \beta_1 (IT - TDT)_{iv} + \varepsilon_{iv} \tag{1}$$

The main explanatory variable, IT-TDT, is the difference between the money subjects transfer as Player 1 in the Investment Game and as Player 1 in the TDG, expressed as a proportion of their initial

endowment. The coefficient on this parameter estimates the change in the odds that individual i in village v is in TGB given greater trust. Surprisingly, the estimate is not statistically different from zero. We obtain similarly null estimates of trust when we control for potentially endogenous socio-demographic characteristics, (Table 3 Column 2) including land area, income, trust in NGOs, age, education level, savings (1/0), outstanding loans (1/0), liquidity, distance to a paved road, distance to a market, and land tenure. In the third specification, we further control for experimental measures of risk, time, reciprocity, and altruism (Table 3 Column 3). In our final specification we control for unobserved village-level variables that may influence TGB participation (Table 3 Column 4). In this, and all subsequent specifications, we report robust standard errors.

Using the same regression sequence, we assess whether present bias predicts TGB participation. This leads to

$$\ln \left(\frac{P_{TGB}}{1 - P_{TGB}} \right)_{iv} = \alpha + \beta_1 (BS1 - BS2)_{iv} + \beta_2 (BS2)_{iv} + \varepsilon_{iv} \tag{2}$$

We construct our main explanatory variable, BS1-BS2, as the difference in the budget shares that subjects allocate to the sooner payment date in the near- and long-term time frames. The coefficient on this parameter estimates the change in the odds that individual i in village v is in TGB given a greater preference for present consumption as the moment of consumption approaches. Notably, the estimate is large and significant: a 1 standard deviation increase in present bias predicts a 44.4 percentage point increase in the odds that an individual participates in TGB (p=0.04). Heterogeneity in individual discounting is captured by BS2, a continuous variable that indicates the long-term average budget share that individuals allocate to sooner payment dates. As in equation 1, we sequentially add a series of demographic and experimental control variables. With all control

variables, a 1 standard deviation increase in present bias predicts a 47.7 percentage point increase in the odds that an individual participates in TGB (p=0.02).

Table 3. Trust, time preferences and TGB participation.

Dependant variable: 1= participant; 0= non-participant								
	(1)		(2)		(3)		(4)	
	Coef.	(SE)	Coef.	(SE)	Coef.	(SE)	Coef.	(SE)
Panel A: trust								
Experimental measure of trust	-0.22	(0.34)	-0.31	(0.40)	-0.23	(0.41)	-0.08	(0.44)
Controls								
Socio-demographics	No		Yes		Yes		Yes	
Risk, trust, reciprocity, altruism preferences	No		No		Yes		Yes	
Village fixed effects	No		No		No		Yes	
Number of farmer observations	262		253		252		252	
Number of villages	13		13		13		13	
Pseudo R^2	0.00		0.11		0.14		0.17	
Panel B: time preferences								
Experimental measure of time preferences	*1.15***	*(0.50)*	*1.10***	*(0.54)*	*1.12***	*(0.56)*	*1.21***	*(0.53)*
Budget share 2	0.38	(0.46)	0.78	(0.49)	0.76	(0.52)	1.26**	(0.54)
Controls								
Socio-demographics	No		Yes		Yes		Yes	
Risk, trust, reciprocity, altruism preferences	No		No		Yes		Yes	
Village fixed effects	No		No		No		Yes	
Number of farmer observations	262		253		252		252	
Number of villages	13		13		13		13	
Pseudo R^2	0.02		0.12		0.13		0.17	

Logit regressions of experimental measures of trust (panel A) and time preferences (panel B) on TGB participation (=1). Robust standard errors are in parentheses. Asteriks denote statistical significance levels: ***p<0.01, **p<0.05, *p<0.1.

II.3.3. Robustness Tests

We test the robustness of these results to two potential concerns. The first concern may be that, among TGB participants, the program induced changes in their time preferences such that the experimental responses we observe are endogenous. Recent research by Meier and Sprenger demonstrates that intertemporal experimental decisions similar to ours are relatively stable over

time even after changes to economically relevant life circumstances, such as income, employment, and liquidity (Meier and Sprenger, 2010b). This work and other studies[75] analyzing the stability of time preferences are suggestive of stability, and hence, limited endogeneity. Unfortunately only one such study is conducted in a developing country, Bolivia, and no study examines whether PES participation induces a change in preferences (Chuang and Schechter, 2014). The second concern may be that the results are sensitive to sample selection criteria, which in this case is the 1-acre threshold at which we define TGB eligibility. To investigate this, we incrementally increase the minimum eligibility threshold and rerun the regressions. Importantly, the models are robust for threshold definitions up to 5 acres. Overall, these results convey a robust and stable correlation between experimentally measured present bias and TGB participation.

II.4. Discussion and Conclusions

Why do farmers participate in PES? Most research examining this question emphasizes the dominance of income, wealth, land, and other observable determinants. However, our results provide compelling evidence that normally unobservable *behavioural preferences* are also important in predicting participation. Specifically, we find that Ugandan farmers who exhibit present-biased preferences are *significantly* more likely to participate than those who show time-consistent or future-biased preferences. This positive relationship may be explained by the program's front-loaded payment structure, which offers farmers 30% of the total value of their carbon contract upfront and the other 70% via four subsequent instalments in later years.

The null relationship between TGB participation and trust is, by contrast, surprising. One explanation for this counterintuitive result is that the standard investment game, albeit a reliable measure of

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[75] The exception is a working paper from Mali on the stability of time preferences over 3 weeks, which reports evidence that experimental choices are correlated with income shocks (Dean and Sautmann, 2014).

interpersonal trust, may not be a useful proxy for individual trust in institutions. After all, the positive correlation between TGB participation and trust of NGOs identified in the post-experiment survey seems unambiguous. When asked, "In general, how much do you trust most NGOs operating in your sub-county?" TGB participants report trusting NGOs more than non-participants (*p=0.06*). Hence, we propose that future experimental research could usefully calibrate the standard investment game for measuring trust in institutions.

Future research could also explore behavioural factors associated with program attrition. Indeed, the presence in our sample of farmers who enrolled but subsequently exited the program facilitates an initial investigation. Comparing the preferences of these former participants (N=36) with current participants (N=92) indicates that risk aversion and future bias are positively associated with exiting TGB. A 1 standard deviation increase in risk aversion predicts a 194.6 percent increase in the odds that a farmer exits (*p=0.03*). Likewise, a 1 standard deviation increase in future bias predicts a 365.9 percent increase in the odds that a farmer exits (*p=0.01*). Without a larger sample of farmers who exited, these connections are speculative.

Several implications for PES programs emerge from this research. First, program planners are often interested in targeting program benefits to certain individuals, for example, the poor or otherwise disadvantaged. Successful targeting, however, requires knowledge of both the underlying behavioural mechanisms driving participation decisions and the program levers that influence such behaviour. Here we document, in one rural Ugandan setting, that heterogeneity in time preferences bias helps to explain PES participation decisions, and suggest that a front-loaded payment structure may be a program lever for attracting present-biased individuals. Whether or not front-loaded PES programs in other geographic and institutional settings disproportionately attract present-biased farmers requires further empirical investigation.

Beyond informing program design, this research has implications for program evaluation. Consider, for instance, cases in which individuals decide to participate in PES based on unobserved characteristics (such as present bias) that are also linked with their conservation behaviour. In such cases, it is difficult to attribute changes in their conservation behaviour to PES or the unobserved characteristics or both. Finally, we provide a field-tested technique that other researchers can adopt. Our approach adapts a best practice from the laboratory setting, the Convex Time Budget, to the field setting and, in this way, is a methodological contribution irrespective of our findings. Taken together, these implications underscore the usefulness of experimentally elicited behavioural preferences, and are important in light of progressive efforts to expand farmer participation in conservation programs.

Acknowledgements

Discussion

Part I and Part II converge demonstrating that individual's preferences are neither always consistent nor exogenous as it is traditionally assumed in economic theory.

Compensation and Reward for Environmental Services, or to a wider extent, incentive-based policies, are designed under the assumption that people would react as given in the rational choice theory, with an utility function based on profit maximization, associated to constant and exogenous preferences. Both field experiments provide evidence that decision-making might, under some circumstances, follow different rules.

Indeed, our work shows that the circumstances under which someone receive money leads to different economic behaviours. Money obtained under low effort will lead to very different consumption patterns than money obtained under high efforts, but also to subsequent higher pro social behaviour. Simultaneously, we obtained evidence for a moral compensation mechanism illustrated by both moral self-licensing and moral cleansing effect. In addition, our results demonstrate that studying behavioural biases is also important in predicting participation. Individuals who exhibit present-biased preferences are significantly more likely to participate than those who show time-consistent or future-biased preferences, when a program has a front-loaded payment structure. Whether those findings apply to other geographic and institutional settings requires further empirical investigations.

More specifically, Part I confirms the existence of a moral self-regulation in real life settings and brings external validity to the moral compensation theory studied in Chapter I. A moral cleansing effect aroused, but origins could be explained either by mental or moral compensation theories. Further research is necessary to disentangle both effects. In addition it could be interesting to investigate both the domain and the time frame in which self-regulation fits. While time horizon was limited to a few hours in our field experiment, it would be interesting to observe self-regulation

mechanism in real life settings on a longer time period as well as in different domains (i.e., health, education, etc).

Part II raises questions on methods for measuring individual preferences. It also points out the importance to better understand the articulation between preferences and choices. By eliciting preferences and linking such measures to observed behaviour, we contribute to better understand the underlying model explaining individuals' decision making. Surprisingly, we found a null relationship between contract's compliance and the experimental measure of trust, but a positive relationship with the survey measure of trust. This questions the accuracy of the standard investment game as an adequate proxy for measuring individual trust in institutions. Hence, we propose that future experimental research could adjust the standard investment game for measuring trust in institutions. Most importantly, our approach adapts a laboratory experimental design, the Convex Time Budget, to a field setting. However, this simplified version of a laboratory protocol would benefit from further investigation to validate its reliability in this particular context. We propose to address this question in Chapter III.

More research is needed to address interference between Incentive-based policies and behavioural biases. Whether those findings apply to other domains remains an open question (health, road safety, working environment, etc). More than giving a clear and definite conclusion on how incentive mechanisms should be implemented, this work mainly demonstrates that contextual environment and behavioural factors could challenge policies achievements at some point. Methods to capture true preferences in the field seem to be a promising path to further understand those interactions. From a political standpoint, preference heterogeneity, observed in both field experiments argues against 'one size fits all' policies. But it confers policy makers a powerful tool to implement targeted policies under the condition that behavioural characteristics are adequately assessed.

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Appendix

Appendix 1

I/ Introduction

Bonjour à tous et merci d'être venu. Nous sommes ici pour réaliser un travail de nettoyage de la forêt de tapia et entretenir les sentiers. En échange, vous recevrez une compensation d'une valeur de 2500 Ar. Vous êtes venus très nombreux. Nous allons vous diviser en deux groupes à l'aide d'un tirage au sort. Pour cela, nous allons vous distribuer des numéros. Nous allons tirer les 20 numéros au sort. Les personnes tirées au sort iront avec <<*Instructeur A*>> et <<*InstructeurB* >>, et les autres iront avec << *Représentant du VOI* >> et <<*Instructeur C*>>.

Procéder au tirage au sort et noter sur une feuille les numéros tirés au sort qui constituent le groupe A.

Les numéros gagnant (groupe A), vous restez ici avec nous. Pour les autres (groupe B), vous pouvez suivre << *Représentant du VOI* >> et <<*Instructeur C*>>.

2/ 1. Instructions pour le Groupe A *(A ENONCER UNE FOIS QUE LE GROUPE B EST PARTI)*

Bravo, vous venez d'être tiré au sort, ce qui vous donne la chance de pouvoir venir retirer votre bien dès maintenant. Vous avez le choix entre de l'huile ou du Fanta. Ces biens sont d'une valeur équivalente à 2500 Ar. Vous pouvez donc choisir entre de l'huile pour une valeur de 2500Ar ou du Fanta d'une valeur de 2500 Ar. Vos biens vous seront remis dans des sacs en plastique noir.

Montrer chacun des deux biens emballés.

Nous allons vous distribuer des coupons qui représentent les deux biens, mettez une croix sur le bien que vous voulez obtenir.

2/ 2.Instructions pour le Groupe B

Merci pour ce travail. Vous allez pouvoir venir retirer votre bien. Vous avez le choix entre de l'huile ou du Fanta. Ces biens sont d'une valeur équivalente à 2500 Ar. Vous pouvez donc choisir entre de

l'huile pour une valeur de 2500Ar ou du Fanta d'une valeur de 2500 Ar. Vos biens vous seront remis dans des sacs en plastique noir.

Montrer chacun des deux biens emballés.

Nous allons vous distribuer des coupons qui représentent les deux biens, mettez une croix sur le bien que vous voulez obtenir.

3/ Instructions communes Groupes A & B

Veuillez aller voir l'un des autres intervenants qui vont vous posez quelques questions. Après les questionnaires, vous aurez 1000 Ar pour vous remercier de votre participation.

A la fin de l'interview, remettre l'enveloppe et <u>noter le numéro du participant sur l'enveloppe</u> et les remercier pour leur participation.

Voici vos indemnités de 1000Ar. Si vous le souhaitez, vous pouvez en laisser une partie pour un programme qui protège la forêt. Pour cela mettez dans cette enveloppe la somme que vous voulez donnez et mettez-la dans cette urne. Bonne journée.

[FIN]

4/ Formulaire de décision

Appendix 2

[Instructor: Ensure each participant has a bottle of water, a pen, and 2 identical participant IDs. Also ensure that each participant is seated in the shade and at a reasonable distance from each other.]

Welcome and thank you for participating. Please turn off your cellphone.

To begin, we would like to make some general comments about the study we are doing today and explain the rules that you must follow.

This is a study of behaviour. In this study you will be asked to make a total of 26 decisions in the course of playing 5 games. Your participation will take approximately 4 hours. You must be willing to stay this long to participate in the study. If you think you may not be able to stay this long, please let us know now.

You will be given a thank-you payment of 1500 UGX, just for participating in this study! You will receive this thank-you payment at the end of the study. You will also earn an additional amount between 0 UGX and 7000 UGX from the games. Whatever additional amount you earn from the games will be added to this thank-you payment. The additional amount you earn will depend partly on your decisions and on the decisions of other participants and partly on chance. After you finish playing the 5 games, we will randomly draw a ball from a box to determine the game-that-counts. If the number on the ball is 1, everyone will be paid according to their decision in Game 1. If the number on the ball is 2, everyone will be paid according to their decision in Game 2. If the number on the ball is 3, everyone will be paid according to their decision in Game 3. If the number on the ball is 4, everyone will be paid according to their decision in Game 4. If the number on the ball is 5, everyone will be paid according to their decision in Game 5. Since all balls are equally likely to be drawn, you should play each game as if it will be the game-that-counts. The game-that-counts will determine your actual earnings. Information about your earnings will be kept strictly confidential.

Before we proceed further, let me emphasize something that is very important. It is important that you listen carefully to the game instructions because only people who understand the games will be allowed to play. We will present some examples to help you understand the games. Do not worry if you do not completely understand the games as we go through the examples here in the group. You will have a chance to ask questions to us in private to be sure that you understand how to play. However, while you may ask questions to us in private, you may not talk with other participants at

any point during the games. This is important. Please be sure that you follow this rule because it is possible for one participant to spoil the study for everyone.

You have been randomly assigned a participant ID. It is important that you do not misplace your participant ID. At the end of the study, you will need to present your participant ID to get your payment.

Remember that each game could be the game-that-counts! It is in your interest to treat each decision in each game as if it could be the decision that determines your earnings.

Game 1 Instructions

Now, I will read the instructions for Game 1.

The following form features 6 gambles, numbered 1 to 6. Each gamble has two possible outcomes: a low outcome and a high outcome. You are asked to select the gamble you like the most by marking the corresponding box.

If Game 1 is chosen as the game-that-counts, at the end of the study, I will draw a ball from this box. The colour of the ball will determine whether your earnings from the game will correspond to the low outcome or the high outcome in the gamble you select. If a yellow ball is drawn, your earnings will correspond to the low outcome. If a blue ball is drawn, your earnings will correspond to the high outcome. Since there is one yellow ball in the box and one blue ball in the box, the chance of drawing the yellow ball is equal to the chance of drawing the blue ball.

Remember, if Game 1 is chosen as the game-that-counts, your earnings will be determined both by the gamble you select <u>and</u> the color of the ball drawn from the box.

<u>Here is your decision form. Please write your Participant ID on this form. Let me know if you have any questions.</u>

 [Instructor: approach each participant individually to solicit questions.]

Game 1 Decision Form

Each gamble below (1 through 6) has two possible outcomes: a low outcome and a high outcome. If Game 1 is chosen as the game-that-counts, at the end of the study, I will draw a ball from this box. If a YELLOW ball is drawn, you will receive the LOW outcome. If a BLUE ball is drawn, you will receive the HIGH outcome. Select the GAMBLE you like the most by marking the corresponding box. *Remember to mark only 1 box on this form!*

	LOW Outcome	HIGH Outcome	
1	2800 UGX	2800 UGX	☐
2	2400 UGX	3600 UGX	☐
3	2000 UGX	4400 UGX	☐
4	1600 UGX	5200 UGX	☐
5	1200 UGX	6000 UGX	☐
6	200 UGX	7000 UGX	☐

Game 2 Instructions

Now, I will read the instructions for Game 2.

In Game 2, you are asked to make decisions involving payments over time. This game has 3 forms, each with 5 decisions, making 15 decisions in total. Each row in each form is a decision and is numbered from 1 to 15. Each row features a series of options consisting of a sooner payment AND a later payment. You are asked to select the option you like the most in each row by marking the box below it. You must **mark one box PER ROW**.

The decision process is best described by an example. Here is an example row:

payment TOMORROW ... *and* payment in 5 weeks	4 UGX 0 UGX ☐	2 UGX 3 UGX ☐	0 UGX 6 UGX ☐

In this example, someone is asked to select the combination of payment tomorrow AND payment in 5 weeks they like the most. As you can see, the sooner payment varies in value from 4 UGX to 0 UGX and the later payment varies in value from 0 UGX to 6 UGX. Note that there is a tradeoff between the sooner payment and the later payment across the options. **As the sooner payment goes down, the later payment goes up.**

In this example, suppose the option someone likes the most is 4 UGX tomorrow AND 0 UGX in 5 weeks, they would mark the left-most box as follows:

	payment TOMORROW ... *and* payment in 5 weeks	4 UGX 0 UGX ☒	2 UGX 3 UGX ☐	0 UGX 6 UGX ☐

Now, if the option someone likes the most is 2 UGX tomorrow AND 3 UGX in 10 weeks, which box should they mark?

	payment TOMORROW ... *and* payment in 10 weeks	4 UGX 0 UGX ☐	2 UGX 3 UGX ☐	0 UGX 6 UGX ☐

Now, if someone marked the right-most box as shown below. How much will this person earn in 5 weeks? How much will this person earn in 10 weeks?

	payment in 5 weeks... *and* payment in 10 weeks	4 UGX 0 UGX ☐	2 UGX 3 UGX ☐	0 UGX 6 UGX ☒

I would like to remind you that this game has 3 forms, each with 5 decisions, making 15 decisions in total. Each decision has a number from 1 to 15.

If Game 2 is chosen as the game-that-counts, at the end of the study, I will draw a ball from this box. There are 15 balls in this box, numbered 1 to 15. The number on the ball that I draw will determine the decision according to which your actual earnings will correspond. Since there is only one of each

number from 1 to 15 in the box, each decision has an equal chance of being drawn. This means it is in your interest to treat each decision as if it could be the one that determines your earnings!

IMPORTANT: If Game 2 is chosen as the game-that-counts, your earnings will be paid in airtime. That includes payments that you receive tomorrow as well as payments that you receive on later dates. On the scheduled day of payment, I will send airtime to your phone. As a reminder to you, the day before you are scheduled to receive your payment, I will send you a text message notifying you that the airtime is coming. If you do not have a phone I will send the airtime to your friend's phone.

Game 2 Decision Form: TOMORROW and in 5 WEEKS

For *each* row (1 to 5) below, decide how much money you would like for sure tomorrow AND in 5 weeks by marking the corresponding box. If this game is chosen as the game-that-counts, at the end of the study, I will draw a ball from this box. There are 15 balls in this box, numbered 1 to 15. The number on the ball that I draw will determine the decision according to which your actual earnings will correspond. If this game is chosen as the game-that-counts, your earnings will be paid in airtime.

Remember to mark 1 box PER ROW

1	payment TOMORROW …	4000 UGX	2000 UGX	0 UGX	
	and payment in 5 weeks	0 UGX ☐	2000 UGX ☐	4000 UGX ☐	
2	payment TOMORROW …	3800 UGX	1900 UGX	0 UGX	
	and payment in 5 weeks	0 UGX ☐	2000 UGX ☐	4000 UGX ☐	
3	payment TOMORROW …	3600 UGX	1800 UGX	0 UGX	
	and payment in 5 weeks	0 UGX ☐	2000 UGX ☐	4000 UGX ☐	
4	payment TOMORROW …	3200 UGX	1600 UGX	0 UGX	
	and payment in 5 weeks	0 UGX ☐	2000 UGX ☐	4000 UGX ☐	
5	payment TOMORROW …	2800 UGX	1400 UGX	0 UGX	
	and payment in 5 weeks	0 UGX ☐	2000 UGX ☐	4000 UGX ☐	

Game 2 Decision Form: TOMORROW and in 10 WEEKS

For *each* row (6 to 10) below, decide how much money you would like for sure tomorrow AND in 10 weeks by marking the corresponding box. If this game is chosen as the game-that-counts, at the end of the study, I will draw a ball from this box. There are 15 balls in this box, numbered 1 to 15. The number on the ball that I draw will determine the decision according to which your actual earnings will correspond. If this game is chosen as the game-that-counts, your earnings will be paid in airtime.
Remember to mark 1 box PER ROW!

Row				
6	payment TOMORROW ...	4000 UGX	2000 UGX	0 UGX
	and payment in 10 weeks	0 UGX ☐	2000 UGX ☐	4000 UGX ☐
7	payment TOMORROW ...	3800 UGX	1900 UGX	0 UGX
	and payment in 10 weeks	0 UGX ☐	2000 UGX ☐	4000 UGX ☐
8	payment TOMORROW ...	3600 UGX	1800 UGX	0 UGX
	and payment in 10 weeks	0 UGX ☐	2000 UGX ☐	4000 UGX ☐
9	payment TOMORROW ...	3200 UGX	1600 UGX	0 UGX
	and payment in 10 weeks	0 UGX ☐	2000 UGX ☐	4000 UGX ☐
10	payment TOMORROW ...	2800 UGX	1400 UGX	0 UGX
	and payment in 10 weeks	0 UGX ☐	2000 UGX ☐	4000 UGX ☐

Game 2 Decision Form: 5 WEEKS and 10 WEEKS

For *each* row (11 to 15) below, decide how much money you would like for sure in 5 weeks AND in 10 weeks by marking the corresponding box. If this game is chosen as the game-that-counts, at the end of the study, I will draw a ball from this box. There are 15 balls in this box, numbered 1 to 15. The number on the ball that I draw will determine the decision according to which your actual earnings will correspond. If this game is chosen as the game-that-counts, your earnings will be paid in airtime.
Remember to mark 1 box PER ROW

11	payment in 5 weeks …	4000 UGX	2000 UGX	0 UGX
	and payment in 10 weeks	0 UGX ☐	2000 UGX ☐	4000 UGX ☐
12	payment in 5 weeks …	3800 UGX	1900 UGX	0 UGX
	and payment in 10 weeks	0 UGX ☐	2000 UGX ☐	4000 UGX ☐
13	payment in 5 weeks …	3600 UGX	1800 UGX	0 UGX
	and payment in 10 weeks	0 UGX ☐	2000 UGX ☐	4000 UGX ☐
14	payment in 5 weeks …	3200 UGX	1600 UGX	0 UGX
	and payment in 10 weeks	0 UGX ☐	2000 UGX ☐	4000 UGX ☐
15	payment in 5 weeks …	2800 UGX	1400 UGX	0 UGX
	and payment in 10 weeks	0 UGX ☐	2000 UGX ☐	4000 UGX ☐

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Game 3 - Instructions for Player 1

Now, I will read the instructions for Game 3.

This game has two parts. In the first part you will play as Player 1. In the second part you will play as Player 2. You will be playing this game with someone from your village. This person will be chosen at random and will not necessarily be the same person with whom you will play in a future game. Neither of you will know exactly with whom you are playing. Only I know who plays with whom and I will not tell anyone.

Both Player 1 and Player 2 will start the game with 2000 UGX.

PLAYER 1 ROLE

As Player 1, you will start the game with 2000 UGX. As Player 1, you will decide whether to send any money to Player 2 and how much you would like to send. However much you decide to send, I will triple it and pass this tripled amount to Player 2. Player 2 will then decide whether to return any money to you and how much to return.

You will make your decision from the following five options:

(1) If you keep 2000 UGX and send 0 UGX to Player 2, the game will be over.

(2)   If you keep 1500 UGX and send 500 UGX to Player 2, I will increase this amount to 1500 UGX. Then, Player 2 will decide to return to you any amount between 0 UGX and 1500 UGX.

(3)   If you keep 1000 UGX and send 1000 UGX to Player 2, I will increase this amount to 3000 UGX. Then, Player 2 will decide to return to you any amount between 0 UGX and 3000 UGX.

(4)   If you keep 500 UGX and send 1500 UGX to Player 2, I will increase this amount to 4500 UGX. Then, Player 2 will decide to return to you any amount between 0 UGX and 4500 UGX.

(5) If you keep 0 UGX and send 2000 UGX to Player 2, I will increase this amount to 6000 UGX. Then, Player 2 will decide to return to you any amount between 0 UGX and 6000 UGX.

Let us consider a few examples. Suppose you decide to send 500 UGX to Player 2, how much will you have left? How much will Player 2 receive? Then, if Player 2 decides to return 100 UGX to you, how much will you earn from the game?

Now consider a different example. Suppose that you decide to send 1000 UGX to Player 2. How much will you have left? How much will Player 2 receive? If Player 2 decides to return 2000 UGX to you, how much will you earn from the game?

Please raise your hand if you have any questions and I will go through more examples with you individually. In the meantime, do not talk to anyone about this game. Even if you are not sure that you understand this game, do not talk to anyone about it. If you talk to anyone about this game, you will be disqualified.

Things to remember

• The decision to send money to Player 2 is entirely up to you.

• As long as you send money to Player 2, Player 2 has an opportunity to return money to you.

• The decision to return money to you is entirely up to Player 2. Player 2 may or may not return money to you. This means you could end the game with more or less money than when you began.

If Game 3 is chosen as the game-that-counts, at the end of the study, you will be randomly assigned to Group A or Group B. Then, I will randomly match each Player 1 from Group A with a Player 2 from Group B and I will randomly match each Player 1 from Group B with a Player 2 from Group A. Because the method is random, you will not necessarily be matched with the same person in both cases. Then, I will draw a ball from this box. The color of the ball will determine whether your earnings from this game will correspond to your decision as Player 1 or your decision as Player 2. If a yellow ball is drawn, participants assigned to Group A will earn the amount corresponding to their decision as Player 1 and participants assigned to Group B will earn the amount corresponding to their decision as Player 2. If a blue ball is drawn, participants assigned to Group B will earn the amount corresponding to their decision as Player 1 and participants assigned to Group A will earn the amount corresponding to their decision as Player 2. Since there is one yellow ball in the box and one blue ball in the box, the chance of drawing the yellow ball is equal to the chance of drawing the blue ball.

Remember, Game 3 has two parts. In this first part you are Player 1. You will decide the amount of money you would like to be tripled and passed to Player 2 by marking the corresponding box in the following form. You will send 0 UGX, 500 UGX, 1000 UGX, 1500 UGX, or 2000 UGX.

<u>Here is your decision form. Please write your Participant ID on this form. Let me know if you have any questions.</u>

Game 3 Decision Form for Player 1

From among the possible decisions below (1 through 5), decide how much money you would like to keep and send to Player 2. Then, Player 2 can decide to return money to you. If Game 3 is chosen as the game-that-counts, at the end of the study, I will randomly match each participant assigned to Group A with a participant assigned to Group B. Then, I will draw a ball from this box. The color of the ball will determine whether your earnings from this game will correspond to your decision as Player 1 or your decision as Player 2. If a yellow ball is drawn, participants assigned to Group A will earn the amount corresponding to their decision as Player 1 and participants assigned to Group B will earn the amount corresponding to their decision as Player 2. If a blue ball is drawn, participants assigned to Group B will earn the amount corresponding to their decision as Player 1 and participants assigned to Group A will earn the amount corresponding to their decision as Player 2. Since there is one yellow ball in the box and one blue ball in the box, the chance of drawing the yellow ball is equal to the chance of drawing the blue ball. *Remember, mark 1 box on this form!*

	If you KEEP	*and*	If you SEND	Player 2 will RECIEVE	
1	2000 UGX		0 UGX	0 UGX	☐
2	1500 UGX		500 UGX	1500 UGX	☐
3	1000 UGX		1000 UGX	3000 UGX	☐
4	500 UGX		1500 UGX	4500 UGX	☐
5	0 UGX		2000 UGX	6000 UGX	☐

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Game 3 – Instructions for Player 2

Remember, this game has two parts. In the first part you played as Player 1. Now, in the second part, you will play as Player 2. You will be playing this game with someone from your village. This person will be chosen at random and will not necessarily be the same person with whom you played in a previous game or will play in a future game. Neither of you will know exactly with whom you are playing. Only I know who plays with whom and I will not tell anyone.

Both Player 1 and Player 2 will start the game with 2000 UGX.

PLAYER 2 ROLE

As Player 2, you will start the game with 2000 UGX. Then, Player 1 will decide whether to send any money to you and how much they would like to send. However much Player 1 decides to send, I will triple it and pass this tripled amount to you. Then, you will decide whether to return any money to Player 1 and how much to return.

You will make **ALL** of the following four decisions:

(1)   If Player 1 sends you 500 UGX, I will increase this to 1500 UGX. Now, you will decide to return to Player 1 any amount between 0 UGX and 1500 UGX.

(2) If Player 1 sends you 1000 UGX, I will increase this to 3000 UGX. Now, you will decide to return to Player 1 any amount between 0 UGX and 3000 UGX.

(3) If Player 1 sends you 1500 UGX, I will increase this to 4500 UGX. Now, you will decide to return to Player 1 any amount between 0 UGX and 4500 UGX.

(4) If Player 1 sends you 2000 UGX, I will increase this to 6000 UGX. Now, you will decide to return to Player 1 any amount between 0 UGX and 6000 UGX.

Note: If Player 1 sends you 0 UGX you will not have an opportunity to return money to Player 1. The game will be over.

Let us consider a few examples. Suppose Player 1 decides to send 500 UGX to you, how much will Player 1 have left? How much will you receive from Player 1? Then, if you decide to return 100 UGX to Player 1, how much will Player 1 earn from the game? How much will you earn from the game?

Now consider a different example. Suppose that Player 1 decides to send 1000 UGX to you. How much will Player 1 have left? How much will you receive from Player 1? Then, if you decide to return 2000 UGX to Player 1, how much will Player 1 earn from the game? How much will you earn from the game?

Please raise your hand if you have any questions and I will go through more examples with you individually. In the meantime, do not talk to anyone about this game. Even if you are not sure that you understand this game, do not talk to anyone about it. If you talk to anyone about this game, you will be disqualified.

Things to remember

•   The decision to send money to you is entirely up to Player 1.

•   As long as Player 1 sends money to you, you have an opportunity to return money to Player 1.

•   The decision to return money to Player 1 is entirely up to you. You may or may not return money to Player 1. This means Player 1 could end the game with more or less money than when they began.

If Game 3 is chosen as the game-that-counts, at the end of the study, you will be randomly assigned to Group A or Group B. Then, I will randomly match each Player 1 from Group A with a Player 2 from Group B and I will randomly match each Player 1 from Group B with a Player 2 from Group A. Because the method is random, you will not necessarily be matched with the same person in both cases. Then, I will draw a ball from this box. The color of the ball will determine whether your earnings from this game will correspond to your decision as Player 1 or your decision as Player 2. If a yellow ball is drawn, participants assigned to Group A will earn the amount corresponding to their decision as Player 1 and participants assigned to Group B will earn the amount corresponding to their decision as Player 2. If a blue ball is drawn, participants assigned to Group B will earn the amount corresponding to their decision as Player 1 and participants assigned to Group A will earn the amount corresponding to their decision as Player 2. Since there is one yellow ball in the box and one blue ball in the box, the chance of drawing the yellow ball is equal to the chance of drawing the blue ball.

Remember, Game 3 has two parts. In this second part you are Player 2. As Player 2, you will make **ALL** four decisions on the following form **by writing the AMOUNTS you would like to return to Player 1 in the right-most box.**

Here is your decision form. Please write your Participant ID on this form. Let me know if you have any questions.

Game 3 Decision Form for Player 2

You are asked to make ALL 4 decisions on this form by writing the amounts you would like to return to Player 1 in the right-most box. If Game 3 is chosen as the game-that-counts, at the end of the study, I will randomly match each participant assigned to Group A with a participant assigned to Group B. Then, I will draw a ball from this box. The color of the ball will determine whether your earnings from this game will correspond to your decision as Player 1 or your decision as Player 2. If a yellow ball is drawn, participants assigned to Group A will earn the amount corresponding to their decision as Player 1 and participants assigned to Group B will earn the amount corresponding to their decision as Player 2. If a blue ball is drawn, participants assigned to Group B will earn the amount corresponding to their decision as Player 1 and participants assigned to Group A will earn the amount corresponding to their decision as Player 2. Since there is one yellow ball in the box and one blue ball in the box, the chance of drawing the yellow ball is equal to the chance of drawing the blue ball. *Remember, make ALL of the following 4 decisions!*

If Player 1 sent you 500 UGX, I increased this amount to 1500 UGX. Decide the amount you would like to return to Player 1. Write this amount in the right-most box.
Remember, you can decide to return any amount between 0 UGX and 1500 UGX!

| 1 | | If Player 1 kept 1500 UGX | *and* | If Player 1 SENT 500 UGX | | If you RECIVED 1500 UGX | | You will RETURN |
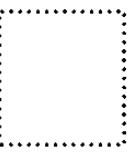
If Player 1 sent you 1000 UGX, I increased this amount to 3000 UGX. Decide the amount you would like to return to Player 1. Write this amount in the right-most box.
Remember, you can decide to return any amount between 0 UGX and 3000 UGX!

| 2 | | If Player 1 kept 1000 UGX | *and* | If Player 1 SENT 1000 UGX | | If you RECIVED 3000 UGX | | You will RETURN |

If Player 1 sent you 1500 UGX, I increased this amount to 4500 UGX. Decide the amount you would like to return to Player 1. Write this amount in the right-most box.
Remember, you can decide to return any amount between 0 UGX and 4500 UGX!

| 3 | | If Player 1 kept 500 UGX | *and* | If Player 1 SENT 1500 UGX | | If you RECIVED 4500 UGX | | You will RETURN |
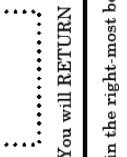
If Player 1 sent you 2000 UGX, I increased this amount to 6000 UGX. Decide the amount you would like to return to Player 1. Write this amount in the right-most box.
Remember, you can decide to return any amount between 0 UGX and 6000 UGX!

| 4 | | If Player 1 kept 0 UGX | *and* | If Player 1 SENT 2000 UGX | | If you RECIVED 6000 UGX | | You will RETURN |

NOTE: If Player 1 kept 2000 UGX and sent you 0 UGX you will receive 0 UGX and you will not have an opportunity to return money to Player 1. The game is over.

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Game 4 – Instructions for Player 1

Now, I will read the instructions for Game 4.

This game has two parts. In the first part you will <u>be</u> Player 1. In the second part you will <u>be</u> Player 2. You will be playing this game with someone from your village. This person will be chosen at random and will not necessarily be the same person with whom you played in a previous game or will play in a future game. Neither of you will know exactly with whom you are playing. Only I know who plays with whom and I will not tell anyone.

In this game, Player 1 has a decision to make. Player 2 does not have a decision to make.

PLAYER 1 ROLE

As Player 1, you may receive one of the following four endowments: 1500 UGX, 3000 UGX, 4500 UGX, or 6000 UGX. For each possible endowment, you will decide whether to send any money to Player 2 and how much you would like to send.

You will make **_ALL_** of the following four decisions:

(1)   If you receive 1500 UGX, you will decide to send to Player 2 any amount between 0 UGX and 1500 UGX. This means that your earnings from the game will be 1500 UGX minus the amount you send to Player 2.

(2)   If you receive 3000 UGX, you will decide to send to Player 2 any amount between 0 UGX and 3000 UGX. This means that your earnings from the game will be 3000 UGX minus the amount you send to Player 2.

(3)   If you receive 4500 UGX, you will decide to send to Player 2 any amount between 0 UGX and 4500 UGX. This means that your earnings from the game will be 4500 UGX minus the amount you send to Player 2.

(4)   If you receive 6000 UGX, you will decide to send to Player 2 any amount between 0 UGX and 6000 UGX. This means that your earnings from the game will be 6000 UGX minus the amount you send to Player 2.

Let us consider a few examples. Suppose you receive 4500 UGX and decide to send 500 UGX to Player 2. How much will Player 2 earn from the game? How much will you earn from the game?

Now consider a different example. Suppose you receive 3000 UGX and decide to send 100 UGX to Player 2. How much will Player 2 earn from the game? How much will you earn from the game?

Please raise your hand if you have any questions and I will go through more examples with you individually. In the meantime, do not talk to anyone about this game. Even if you are not sure that you understand this game, do not talk to anyone about it. If you talk to anyone about this game, you will be disqualified.

Things to remember

- The decision to send money to Player 2 is entirely up to you.
- Player 2 does not have a decision to make.

As Player 1, you are asked to make **ALL** four decisions on the following form **by writing the amounts you would like to send to Player 2 in the right-most box.** If Game 4 is chosen as the game-that-counts, at the end of the study, you will be randomly assigned to Group A or Group B. Then, I will randomly match each Player 1 from Group A with a Player 2 from Group B and I will randomly match each Player 1 from Group B with a Player 2 from Group A. Because the method is random, you will not necessarily be matched with the same person in both cases. Then, I will draw a ball from this box. The color of the ball will determine whether your earnings from this game will correspond to your decision as Player 1 or your role as Player 2. If a yellow ball is drawn, participants assigned to Group A will earn the amount corresponding to one of their four Player 1 decisions. The decision which will be implemented will be determined at random <u>by drawing a ball from this box.</u> In this case, participants assigned to Group B will earn the Player 2 amount corresponding to Player 1's decision. If a blue ball is drawn, participants assigned to Group B will earn the amount corresponding to one of their four Player 1 decisions. The decision which will be implemented will be determined at random <u>by drawing a ball from this box.</u> In this case, participants assigned to Group A will earn the Player 2 amount corresponding to Player 1's decision.

Remember, Game 4 has two parts. In this first part you are Player 1. As Player 1, you will make **ALL** four decisions on the following form **by writing the AMOUNTS you would like to send to Player 2 in the right-most box.**

<u>Here is your decision form. Please write your Participant ID on this form. Let me know if you have any questions.</u>

Game 4 Decision Form for Player 1

As Player 1, you are asked to make **ALL 4** decisions on this form **by writing the amounts you would like to send to Player 2 in the right-most box**. If Game 4 is chosen as the game-that-counts, at the end of the study, I will randomly match each participant assigned to Group A with a participant assigned to Group B. Then, I will draw a ball from this box. The color of the ball will determine whether your earnings from this game will correspond to your decision as Player 1 or your role as Player 2. If a yellow ball is drawn, participants assigned to Group A will earn the amount corresponding to a randomly chosen Player 1 decision and participants assigned to Group B will earn the Player 2 amount corresponding to Player 1's decision. If a blue ball is drawn, participants assigned to Group B will earn the amount corresponding to a randomly chosen Player 1 decision and participants assigned to Group A will earn the Player 2 amount corresponding to Player 1's decision. *Remember, make ALL of the following 4 decisions!*

1	If you received 1500 UGX, decide the amount you would like to SEND to Player 2. Write this amount in the right-most box. *Remember, you can decide to send any amount between 0 UGX and 1500 UGX!* If you RECIVED 1500 UGX You will SEND
2	If you received 3000 UGX, decide the amount you would like to SEND to Player 2. Write this amount in the right-most box. *Remember, you can decide to send any amount between 0 UGX and 3000 UGX!* If you RECIVED 3000 UGX You will SEND
3	If you received 1500 UGX, decide the amount you would like to SEND to Player 2. Write this amount in the right-most box. *Remember, you can decide to send any amount between 0 UGX and 4500 UGX!* If you RECIVED 4500 UGX You will SEND
4	If you received 1500 UGX, decide the amount you would like to SEND to Player 2. Write this amount in the right-most box. *Remember, you can decide to send any amount between 0 UGX and 6000 UGX!* If you RECIVED 6000 UGX You will SEND

Game 4 – Instructions for Player 2

This game has two parts. In the first part you will <u>be</u> Player 1. In the second part you will <u>be</u> Player 2. You will be playing this game with someone from your village. This person will be chosen at random and will not necessarily be the same person with whom you played in a previous game or will play in a future game. Neither of you will know exactly with whom you are playing. Only I know who plays with whom and I will not tell anyone.

PLAYER 2 ROLE

As Player 2, you do not have a decision to make in this game. You will earn whatever amount Player 1 decides to send you.

If Game 4 is chosen as the game-that-counts, at the end of the study, you will be randomly assigned to Group A or Group B. Then, I will randomly match each Player 1 from Group A with a Player 2 from Group B and I will randomly match each Player 1 from Group B with a Player 2 from Group A. Because the method is random, you will not necessarily be matched with the same person in both cases. Then, I will draw a ball from this box. The color of the ball will determine whether your earnings from this game will correspond to your decision as Player 1 or your role as Player 2. If a yellow ball is drawn, participants assigned to Group A will earn the amount corresponding to one of their four Player 1 decisions. The decision which will be implemented will be determined at random <u>by drawing a ball from this box</u>. In this case, participants assigned to Group B will earn the Player 2 amount corresponding to Player 1's decision. If a blue ball is drawn, participants assigned to Group B will earn the amount corresponding to one of their four Player 1 decisions. The decision which will be implemented will be determined at random <u>by drawing a ball from this box</u>. In this case, participants assigned to Group A will earn the Player 2 amount corresponding to Player 1's decision.

<u>Here is your decision form. Please write your Participant ID on this form. Let me know if you have any questions..</u>

Game 4 Decision Form for Player 2

As Player 2, you do not have a decision to make in this game. If Game 4 is chosen as the game-that-counts, at the end of the study, I will randomly match each participant assigned to Group A with a participant assigned to Group B. Then, I will draw a ball from this box. The color of the ball will determine whether your earnings from this game will correspond to your decision as Player 1 or your role as Player 2. If a yellow ball is drawn, participants assigned to Group A will earn the amount corresponding to a randomly chosen Player 1 decision and participants assigned to Group B will earn the Player 2 amount corresponding to Player 1's decision. If a blue ball is drawn, participants assigned to Group B will earn the amount corresponding to a randomly chosen Player 1 decision and participants assigned to Group A will earn the Player 2 amount corresponding to Player 1's decision.

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Game 5 – Instructions for Player 1

Now, I will read the instructions for Game 5.

This game has two parts. In the first part you will play as Player 1. In the second part you will play as Player 2. You will be playing this game with someone from your village. This person will be chosen at random and will not necessarily be the same person with whom you played in a previous game. Neither of you will know exactly with whom you are playing. Only I know who plays with whom and I will not tell anyone.

In this game, Player 1 has a decision to make. Player 2 does not have a decision to make.

PLAYER 1 ROLE

As Player 1, you may receive 2000 UGX. Then you will decide whether to send any money to Player 2 and how much you would like to send. However much you decide to send, I will triple it and pass this tripled amount to Player 2.

The following form features five possible decisions, numbered 1 to 5. Each possible decision allocates money between you and Player 2. Here are the five possible decisions:

(1) If you decide to keep 2000 UGX and send 0 UGX the game will be over. This means that your earnings from the game will be 2000 UGX.

(2)   If you decide to keep 1500 UGX and send 500 UGX to Player 2, I will increase this amount to 1500 UGX. This means that your earnings from the game will be 1500 UGX.

(3) If you decide to keep 1000 UGX and send 1000 UGX to Player 2, I will increase this amount to 3000 UGX. This means that your earnings from the game will be 1000 UGX.

(4)   If you decide to keep 500 UGX and send 1500 UGX to Player 2, I will increase this amount to 4500 UGX. This means that your earnings from the game will be 500 UGX.

(5) If you decide to keep 0 UGX and send 2000 UGX to Player 2, I will increase this amount to 6000 UGX. This means that your earnings from the game will be 0 UGX.

If Game 5 is chosen as the game-that-counts, at the end of the study, you will be randomly assigned to Group A or Group B. Then, I will randomly match each Player 1 from Group A with a Player 2 from Group B and I will randomly match each Player 1 from Group B with a Player 2 from Group A. Because the method is random, you will not necessarily be matched with the same person in both

cases. Then, I will draw a ball from this box. The color of the ball will determine whether your earnings from this game will correspond to your decision as Player 1 or your role as Player 2. If a yellow ball is drawn, participants assigned to Group A will earn the amount corresponding to their decision as Player 1 and participants assigned to Group B will earn the amount corresponding to their role as Player 2. If a blue ball is drawn, participants assigned to Group B will earn the amount corresponding to their decision as Player 1 and participants assigned to Group A will earn the amount corresponding to their role as Player 2. Since there is one yellow ball in the box and one blue ball in the box, the chance of drawing the yellow ball is equal to the chance of drawing the blue ball.

You are now asked to decide whether to send any money to Player 2 and how much you would like to send by marking the corresponding box in the following form.

Here is your decision form. Please write your Participant ID on this form. Let me know if you have any questions..

Game 5 Decision Form for Player 1

From among the possible decisions below (1 through 5), decide how much money you would like to keep and send to Player 2 by marking the corresponding box. After you make your decision, I will triple the amount you send to Player 2. If Game 5 is chosen as the game-that-counts, at the end of the study, I will draw a ball from this box. The color of the ball will determine whether your earnings from this game will correspond to your decision as Player 1 or your role as Player 2. If a yellow ball is drawn, participants assigned to Group A will earn the amount corresponding to their decision as Player 1 and participants assigned to Group B will earn the amount corresponding to their role as Player 2. If a blue ball is drawn, participants assigned to Group B will earn the amount corresponding to their decision as Player 1 and participants assigned to Group A will earn the amount corresponding to their role as Player 2. *Remember, mark 1 box on this form!*

	If you KEEP	*and*	If you SEND	Player 2 will RECIEVE	
1	2000 UGX		0 UGX	0 UGX	☐
2	1500 UGX		500 UGX	1500 UGX	☐
3	1000 UGX		1000 UGX	3000 UGX	☐
4	500 UGX		1500 UGX	4500 UGX	☐
5	0 UGX		2000 UGX	6000 UGX	☐

Game 5 – Instructions for Player 2

Remember, this game has two parts. In the first part you played as Player 1. Now, in the second part, you will play as Player 2. You will be playing this game with someone from your village. This person will be chosen at random and will not necessarily be the same person with whom you played in a previous game. Neither of you will know exactly with whom you are playing. Only I know who plays with whom and I will not tell anyone.

PLAYER 2 ROLE

As Player 2, you do not have a decision to make in this game. You will earn whatever amount Player 1 decides to send you. Player 1 may decide to send you 0 UGX, 500 UGX, 1000 UGX, 1500 UGX, or 2000 UGX. However much Player 1 decides to send, I will triple it and pass this tripled amount to you.

If Game 5 is chosen as the game-that-counts, at the end of the study, you will be randomly assigned to Group A or Group B. Then, I will randomly match each Player 1 from Group A with a Player 2 from Group B and I will randomly match each Player 1 from Group B with a Player 2 from Group A. Because the method is random, you will not necessarily be matched with the same person in both cases. Then, I will draw a ball from this box. The color of the ball will determine whether your earnings from this game will correspond to your decision as Player 1 or your role as Player 2. If a yellow ball is drawn, participants assigned to Group A will earn the amount corresponding to their decision as Player 1 and participants assigned to Group B will earn the amount corresponding to their role as Player 2. If a blue ball is drawn, participants assigned to Group B will earn the amount corresponding to their decision as Player 1 and participants assigned to Group A will earn the amount corresponding to their role as Player 2. Since there is one yellow ball in the box and one blue ball in the box, the chance of drawing the yellow ball is equal to the chance of drawing the blue ball.

Here is your decision form. Please write your Participant ID on this form. Let me know if you have any questions.

Game 5 Decision Form for Player 2

As Player 2, you do not have a decision to make in this game. As Player 2, you will earn whatever amount Player 1 decides to send you. Player 1 can decide to send you 0 UGX, 500 UGX, 1000 UGX, 1500 UGX, or 2000 UGX.

If Game 5 is chosen as the game-that-counts, at the end of the study, I will draw a ball from this box. The color of the ball will determine whether your earnings from this game will correspond to your decision as Player 1 or your role as Player 2. If a yellow ball is drawn, participants assigned to Group A will earn the amount corresponding to their decision as Player 1 and participants assigned to Group B will earn the amount corresponding to their role as Player 2. If a blue ball is drawn, participants assigned to Group B will earn the amount corresponding to their decision as Player 1 and participants assigned to Group A will earn the amount corresponding to their role as Player 2. Since there is one yellow ball in the box and one blue ball in the box, the chance of drawing the yellow ball is equal to the chance of drawing the blue ball.

Abstract

Chapter II studies unintended behavioural response to incentive-based mechanism that might mitigate policy design, using field experiment, in a 'Econs' vs. 'Humans' fashion. This chapter extends findings from Chapter I to a more general context involving additional insights from behavioural economics that are relevant for incentive-based policy. The purpose of using field experiment is to capture as much of the context as possible and thus increase our research's external validity and political relevance (List and Hofler, 2004). Chapter II is twofold. First, we propose to study unintended spillovers due to Incentive contract's behavioural response (i.e., '*Moral licensing*' and '*Mental accounting*'). In a second part, we question a behavioural bias that could interfere with incentive contract's compliance (i.e., '*Present bias*').

Résumé

Le Chapitre II se propose d'étudier les diverses interactions entre biais cognitifs et mécanismes incitatifs (i.e., contrat d'incitation), via la réalisation d'expériences de terrain. Ce chapitre prolonge le précédent en élargissant la recherche à d'autres biais comportementaux en lien avec les politiques incitatives. Par ailleurs, la réalisation d'expériences de terrain confère à ce travail une validité externe et une pertinence politique accrues, notamment via la contextualisation (List and Hofler, 2004). Le Chapitre II consiste en deux parties. Dans un premier temps, nous étudierons les potentiels effets d'entraînement non désirés provoqués par la réponse comportementale aux contrats d'incitation (i.e., '*Effet de licence morale*' et '*Comptabilité mentale*'). Une seconde section traitera de biais cognitifs intervenant en amont des contrats d'incitation (i.e., '*Préférence pour le présent*').

Chapter III – Methodological Issues

Introduction

Chapter III aims at addressing some methodological issues raised in Chapter I and II.

In Chapter I, we used a method called Between subject Random Incentivized System (BRIS) to explore the moral licensing bias. The Between subject Random Incentivized System (BRIS), increasingly used in experimental economics, has emerged as a potential solution to test ideas among large sample of subjects without logistical constraints due to individual payment. This type of incentivized payment scheme raises concern about results reliability since not all participants are paid and there is no evidence that preferences are properly assessed under this method. In the first section of Chapter III, we ran a laboratory experiment to test whether the results get under the Between subject Random Incentivized System (BRIS) are consistent with results under conventional individual payments.

In Chapter II, we brought to field settings a simplified version of Andreoni and Sprenger's (2012) Convex Time Budget method, in order to measure time preference in a real life context. We adapted their method to the field by restricting the set of feasible budget allocations between the sooner and the later date. Besides allowing for aggregate and individual estimates of subjective discount rates, the data generated by the CTB method can also be used to estimate the curvature of individuals' utility function. As a complement, we provided a second independent measure of risk preferences by relying on the method proposed by Binswanger (1980) and Eckel & Grossman (2008). However, this simplified version of a laboratory protocol would benefit from further research to validate its reliability. In the second section of Chapter III, we compare our field data to Andreoni and Sprenger (2012)'s laboratory data, checking for this simplified version adequacy. In addition, by combining the risk-preferences data traditionally measured and the alternative measure provided by the CTB

method, we can check for the accuracy of traditional risk measurement in determining utility function curvature.

Chapter III is organized as follows. Section I assess the reliability of Between subject Random Incentivized System and Section II focuses on the Convex Time Budget method portability.

Part I. Gambling with the Dictator - An experimental test of Random Incentivized Systems.

Experimental economics is a fast growing field, challenged by robustness and external validity. Ever-larger samples are preferred, which present researches with logistical and financial challenges (Charness *et al.* 2013). The Between-subject Random Incentivized System (BRIS) has emerged as a potential solution to test ideas among large sample of subjects without the logistical constraints that arise with individual payment systems. Additionally, it reduces the significant costs of recruiting subjects and eases the organizational constraints associated with large-scale experiments. With this mechanism, all subjects play one game, but only a percentage of subjects are paid. If experimenters maintain stakes at the initial standard level under random pay, expected rewards (i.e. an individual's statistical expectation of a gamble's outcome) are lowered, and the efficiency of the incentive becomes debatable from a theoretical standpoint. For instance, a 10€ reward with a one-in-ten probability of receiving the payment is equivalent to an expected reward of 1€. Traditional theory would recommend increasing the stakes in order to obtain an expected reward equivalent to the full pay experiment (i.e., 100€ in this example). However, insights from behavioral economics suggest that probabilities are frequently interpreted in ways that diverge from traditional economic theory. Specifically, people are particularly attracted to small probabilities of large rewards (Loewenstein et al., 2001) and generally tend to overweight small probabilities (Kahneman and Tversky 1979 ; Prelec 1998). Without such insights, how can one explain the fact that sales of the so-called Euromillion 2€ ticket reach up to 3 million purchases an hour at their peak, while the expected gain per ticket is only 0.08€? In a medical study by Volpp et al. (2008), lotteries used as incentives significantly and

positively altered health behaviors, showing that even small rewards have great incentive value if they occur immediately.

In this work, we ran a laboratory experiment to test whether results obtained under the Between-subject Random Incentivized System (BRIS) are consistent with results obtained under a conventional individual payment system. We found that subjects do not behave as predicted. Against the expectations of traditional economic theory, observed behaviors in a 10€ full pay vs. a 10€ random pay (i.e. one-in-ten probability) experiment are strikingly similar. Conversely, when the financial stake is increased to maintain a constant expected reward (i.e., the chance to win 100€ with a one-in-ten probability), small behavioral alterations appear.

I.1. Background

Despite its growing use (Clot *et al*. 2014, Langer and Weber 2008, Bettinger and Slonim 2007), no clear-cut evidence from the literature demonstrates that BRIS is statistically reliable. This paper uses a laboratory experiment based on a standard dictator game (Forsythe et al, 1988) to address the following question: how does the Between-subject Random Incentivized System (BRIS) impact individual decision making compared to the conventional incentive system? The dictator game is one of the most popular games among experimentalists. Using a very simple design, it is able to quantify systematic behavioral differences between populations. It has been run in more than one hundred published papers over the last three decades (Engel, 2011).

Studies about BRIS are scarce. A few exceptions apply to ultimatum game (Bolle, 1990), as well as less conventional experiments such as the 'deal or no deal' game (Baltussen *et al*., 2011). BRIS has also been studied under the Hybrid Random Incentivized System (HRIS) mechanism (Armantier 2006, Stahl and Haruvy 2006), which is a combination of BRIS and a Within-subject Random Incentivized

System (WRIS)[76]. However, those studies embed confounding factors (i.e., collateral results), clouding the issue. Additional probabilities inherent to either the experimental design (i.e., strategy method, risky choice) or the incentive mechanism (i.e., HRIS) make the full pay version of these experiments unsuitable as representations of baseline behaviour by distorting the elicitation of true preferences.

Bolle (1990) compared the outcomes of $10 ultimatum games under random pay and full pay procedures. In the random pay experiment, 2 out of 20 subjects were randomly selected to receive payment. In the full pay experiment, all 24 participants were paid. The hypothesis, that offers in experiments with random pay and full pay are the same, was accepted. However, the ultimatum game in this experiment was run in its strategic version (i.e., each pair of subjects played both roles and a lottery subsequently determined which player would be the proposer). Given this design, even the full pay experiment does not perfectly correspond to decision scenarios in which the potential gain is certain. The same criticism applies to Baltussen et al.'s (2011) study. Their experiment mimics the choice problems in the TV game show Deal or No Deal. In each game round, a subject must choose between a sure alternative and a risky lottery with known probabilities. Even in the full pay version of the game, however, probabilities and risky choice may interfere with the participant's observed behavior.

In order to correctly assess the impact of a random payment, the experimental design should not involve additional probabilities and/or strategic interactions. This is why the dictator game in its simplest version constitutes an appropriate design to test the Between-subject Random Incentives System (Sefton, 1992). Sefton (1992) compared data from random pay dictator games with full pay data from Forsythe et al. (1988) and concluded that subjects were more altruistic under uncertainty. However, this experiment has some limitations in terms of current methodological expectations.

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[76] Broadly defined, Random Incentivized Systems include two different categories: the Within-subject Random Incentivized System (WRIS) and the Between-subject Random Incentivized System (BRIS). The WRIS consists in randomly paying only one task over a set of multiple tasks. An often-cited argument in favor of this system is that the WRIS avoids the endowment effect (i.e. when a player's choice is influenced by the size of the potential gains the player made from previous games). The validity of the Within-subject RIS has been investigated in many studies. While past research has shown that decisions follow the rule of isolation (i.e., each decision is made independently) more than reduction (Beattie and Loomes 1997, Cubitt et al 1998, Camerer 1989), recent research from Cox et al. (2014) demonstrates that subjects do not in fact isolate each individual decision in multiple decision experiments.

First, the financial stake was moderate ($5 vs. a usual minimum of $10), and second, the sample was small (69 pooled pairs for the full pay experiment but only 24 pairs in the random pay experiment). In addition, the hypothesis of random pay under constant expected reward was not tested.

As the existing literature offers no clear conclusions regarding the validity of BRIS, we test this incentive system in the lab, with a large sample and standard stake, by comparing a full pay experiment with two random pay experiments.

Our experiment includes one control (full pay) and two treatments (random pay). Treatment one is exposed to the same stake as in the control treatment but faces a lottery for the payment (i.e. the chance to win 10€ with a one-in-ten probability). The second treatment faces a lottery with the same probability as in treatment one, however the stake in treatment two is increased so as to maintain the same expected reward as in the control treatment (i.e. the chance to win 100€ with a one-in-ten probability). The experimental procedures for each experiment are identical except that at the end of the random pay experiment, a lottery is conducted to select which pairs will be paid. We inform subjects about the payment procedure before starting the experiment.

I.2. Experimental design and data

I.2.1. Subjects

A total of 240 participants were recruited from a subject pool drawn from a database of people who had completed a form indicating their interest in participating in economic experiments. None of the subjects had previously participated in a dictator game. All subjects were paid a 6€ show-up fee. We conducted six lab sessions, each with 20 pairs, at the University of Montpellier[77] in March 2014. Participants were randomly assigned to two groups, split into rooms A and B. The two groups did not have any contact before, during or after the session. Participants were only allowed to talk to administrators.

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[77] Laboratoire d'Economie Experimentale de Montpellier

I.2.2. Experimental procedure[78]

The experiment consists in a standard Dictator Game as proposed by Forsythe et al. (1988). A dictator game is a two-player game in which a certain amount of money is to be divided and one of the players (the dictator) decides how to divide the money. Subjects in room A are randomly matched with subjects in room B. Only players in Room A (i.e., dictators) make a decision, by allocating a part or nothing of the endowment to player B. Before the experiment begins, subjects are given ample time to read a written version of instructions and to ask questions. Next, instructions for the dictator game are read aloud to both groups.

I.2.3. Treatments

Our design (Table 1) includes one control and two treatments. In the control session (i.e., 'full pay experiment'), subjects play for a 10€ real endowment[79]. In treatment 1, subjects play the game with the same stake as in control (i.e., 10€) but with one-in-ten chance of real payment ('random pay experiment'). In treatment 2, subjects play the game with a higher stake (i.e., 100€) and a one-in-ten chance of real payment. Control and treatment 2 have the same expected reward. In a 'full pay experiment' all subjects receive payments according to their decisions. This payment procedure elicits a truthful response to the question 'How would you divide the sum of money?' by actually confronting each dictator with the task. In a 'random pay experiment', a computerized lottery selects a subset of subjects to be paid according to their decisions at the end of the experiment.

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[78] See Appendix 1 for detailed instructions.
[79] Standard stakes in laboratory Dictator Games are usually $10 (Engel 2011).

Table 1. Between subject Random Incentive System - Experimental Design

	Endowment	Probability	Expected Reward
Control – 'Full Pay experiment'	10€	1/1	10€
Treatment 1 – 'Random Pay experiment – Standard stake'	10€	1/10	1€
Treatment 2 – 'Random Pay experiment – High stake'	100€	1/10	10€

I.3. Results and discussion

Subjects gave on average 2.7€ in the full pay experiment, 2.6€ in the Random Pay experiment – Standard stake (10€, p=0.1) and 26.6€ in the Random Pay experiment – High stake (100€, p=0.1).

R1 The distribution of dictators' offers among the 'Full pay experiment' and the 'Random Pay experiment – Standard stake' are significantly similar (Kolmogorov-Smirnov test reports no significant difference: D=0.05; p=1), as illustrated in Figure 1.

R2 The 'Random Pay experiment – High stake' produces a slightly different distribution, with fewer purely selfish subjects (i.e., 'zero offer': 10% vs. 25%, ns.), but more positive offers below 30% of the potential endowment (38% vs. 20%, ns.).

Samples are well balanced across control and treatments as shown in Table 2. No significant difference appears between groups for the main socio-demographic variables.

(N=120)	(1) Control	(2) Treatment 1	(3) Treatment 2	(4) H-statistics	(5) P-value
Socio demographic characteristics					
Gender ratio (% of male)	40%	50%	45%	0.59	0.74
Age	26.12	23.17	27.72	2.31	0.31
Student ratio	85%	90%	90%	0.19	0.9

Full Pay (10€) vs. Random Pay (10€, p=0.1)

Purely selfish subjects, as identified by 'zero offers', occurred in 25 percent of cases in the Full pay experiment, which is exactly the same proportion as in the Random pay version of the game. Fifty-fifty offers (i.e., egalitarian subjects) comprise 28% of the sample in both versions.

As the quantile-quantile plots below illustrate (Figure 2), the two distributions are strikingly similar. The majority of the points fall around the 45° line, indicating that the two samples have almost the same distribution. The two-sample Kolmogorov-Smirnov exact test for equality of distribution validates the hypothesis of similar distribution (No significant difference; D=0.05, p=1).

Full Pay (10€) vs. Random Pay (100€, p=0.1)

The donation ratios between the full pay experiment (10€) and the Random Pay experiment – High stake (100€, p=0.1) are more or less equivalent (27% vs. 26.6%, respectively). The two-sample Kolmogorov-Smirnov exact test supports the hypothesis of equality of distribution with the Full pay experiment, with non-significant difference between both samples (D=0.15, p=0.742). However, the Random Pay experiment – High stake is less likely to generate the same distribution according to Figure 2, where departures from the 45° line are small, but frequent.

The major discrepancy between the full pay and high stake random pay concerns both null and low offers. Zero offers are much less represented in the Random pay (100€, p=0.1) than in the full pay experiment. Running a rank sum test, the difference is significant, but only at the 10% level (z =1.754,

p=0.0794). The opposite happens for low offers: in the Random pay (100€, p=0.1) the offers ranging from 10% to 30% of the potential endowment are more numerous than in the Full pay experiment, and the two-sample Kolmogorov-Smirnov exact test rejects the hypothesis of equal distribution, finding a significant difference between both samples (D=0.44, p=0.05). Dictator games results of Cherry et al (2012) corroborates the 'zero offer' pattern as being a decreasing function of the stake ('zero offers' decrease as the stakes increase). Possible explanations include a rise of culpability associated with purely selfish behavior when stake is high. Most importantly, this is an additional argument in favor of the RIS incentive as a valid preference elicitation mechanism.

Figure 1 – Histograms of dictator's offer (part of endowment) among control (Full pay) and treatments (Random pay)
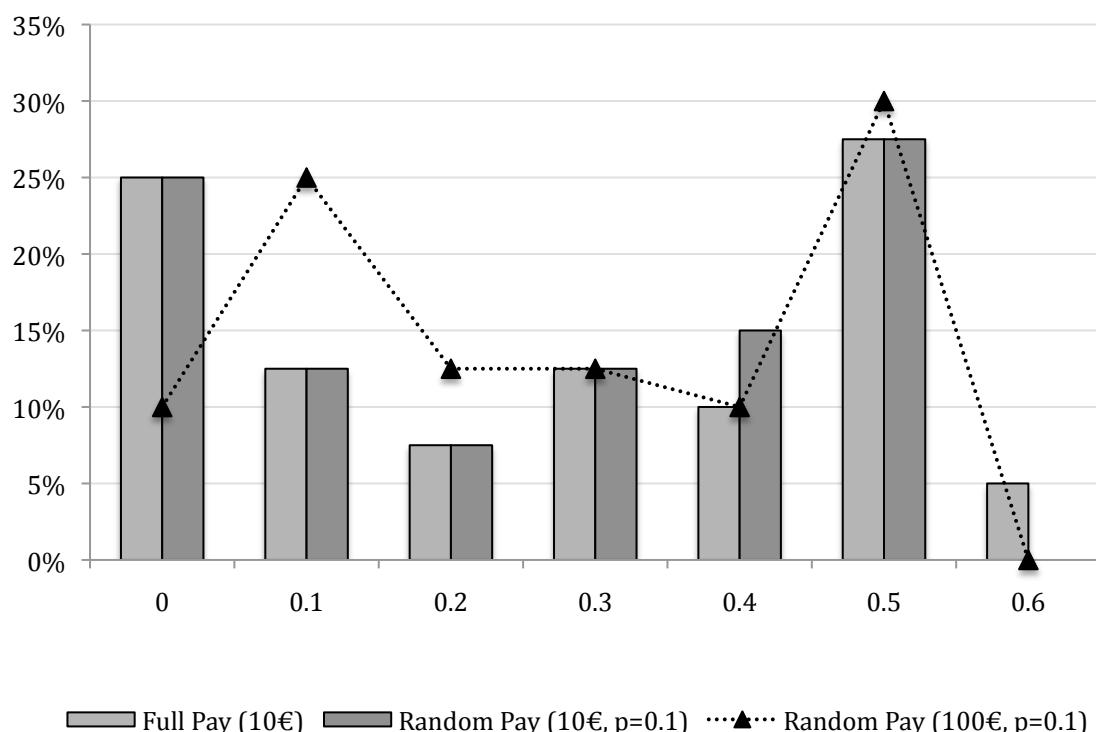
Figure 2 – Quantile-quantile plots of dictator's offers between control (Full pay) and treatments (Random pay)
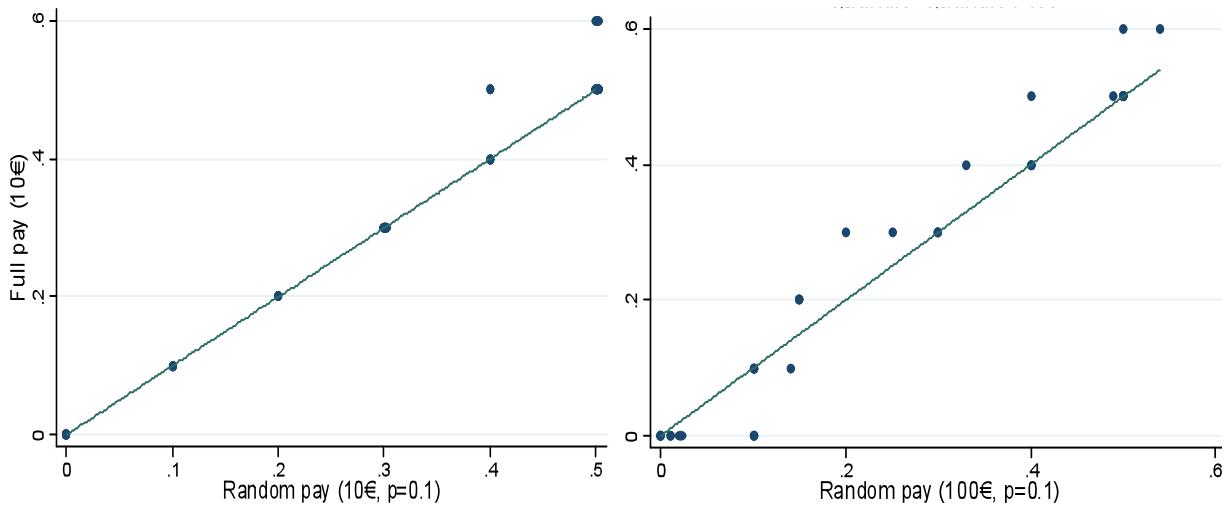
Our experiment shows that subjects tend to be more influenced by the amount of the reward itself than by the probability of receiving it. For an identical stake, behavior under random payment is surprisingly similar to behavior under regular incentive systems. For identical expected earnings, however, we observe significantly higher pure-egoistic behavior under certain payment than under random payment. Both findings contradict with theoretical predictions and may revolutionize experimental design and implementation. Further research could extend this work by testing the Between-subject Random Incentivized System with other games as well as with varying probabilities of payment.

This work widens the scope of methods available to researchers willing to test hypotheses under logistical and financial constraints. The validity of random incentive systems may also offer policy makers an opportunity to use financial incentives at lower cost when designing innovative policy measures that encourage adherence to better habits in an extensive variety of domains (e.g., environment, health, road security, etc). Because existing strategies to improve compliance with policies are frequently either too complex or too expensive to implement on large scales, random

(dis)incentive interventions could become a cost-effective approach to achieve political goals. Possible applications are numerous and wide-ranging, such as random rewards for proper waste management or random fines for not respecting mandatory health measures.

Part II. Are risk-preferences and time-preferences related? New evidence from a field experiment in rural Uganda.

II.1. Introduction

According to a recent literature risk preferences and time preferences are closely connected: individuals who are more risk-tolerant are also more patient (Anderhub *et al*., 2001, Burks *et al*., 2009, Dohmen *et al*., 2010, Carpenter *et al*., 2011, Benjamin et al., 2012). The reasons why these two facets of individual preferences are inter-related are unclear, but it seems that they have both common behavioural and genetic roots. For instance, Dohmen *et al*. (2009) found that patience and risk-tolerance are correlated with cognitive ability in their subjects (e.g. IQ score), and Carpenter *et al*. (2011) found that both risk preferences and time preferences are related to the 7-repeat allele of the DRD4 gene that regulates dopamine uptake in the brain. Indirect experimental evidence about the relation between risk and time preference was also provided recently by Halevy (2008), Epper *et al*. (2012), Andreoni and Sprenger (2012a) and Bchir *et al.* (2013).

It is unknown whether this particular link between risk preferences and time preferences is robust to methodological factors. All previous studies combined two independent methods to elicit risk-preferences and time preferences. Except Carpenter et al. (2011), they relied on certainty equivalents for risky lotteries to elicit risk-preferences and on variants of the multiple price list to elicit time-preferences. The method based on certainty equivalents has some well-known limitations. Depending on the nature of the subject pool it is sometimes preferable to rely on the single lottery choice task proposed in Binswanger (1980) and more recently in Eckel & Grossman (2008), rather than on the standard iterative methods usually implemented for narrowing down the certainty

equivalents. Similarly, the standard method for eliciting time preferences based on multiple price list has been criticized by Andreoni & Sprenger (2012) because it forces subjects to choose extreme budget allocations. Furthermore, while the available laboratory evidence suggests that risk-aversion and patience are correlated negatively, field data is sparse.

A relationship between time and risk preferences may have extensive implication for both policy and contract design enabling to take advantage of a self-selection process. For instance, insurance companies targeting risk averse individuals could favour front-loaded payment structures or inversely, companies seeking to recruit risk seekers could propose an advantageous back-loaded payment structures. In this paper we provide new evidence about the existence of a negative correlation between risk-tolerance and impatience, based on field data collected in rural Uganda for which we implemented a field version of the Convex Time Budget method devised by Andreoni & Sprenger (2012). The use of an innovative experimental tool with an unconventional sample where a WEIRD[80] effect (Henrich et al., 2010) cannot be suspected, increases both the robustness and the external validity of the risk and time relationship.

We measured time preferences by relying on a simplified version of Andreoni and Sprenger's (2012) CTB method. We adapted their method to the field by restricting the set of feasible budget allocations between the sooner and the later date. Besides allowing for aggregate and individual estimates of subjective discount rates, the data generated by the CTB method can also be used to estimate the curvature of individuals' utility function. We can therefore test directly our conjecture about the negative correlation between risk tolerance and impatience. In addition we provided a second independent measure of risk preferences by relying on the method proposed by Binswanger (1980) and Eckel & Grossman (2008) and which is particularly suited for field experiments (Dave et al., 2010). By combining the risk-preferences data of this alternative measure with the time-preferences data of the CTB method we can check for the robustness of the findings based on the CTB method alone.

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[80] WEIRD effect : Western, Educated, Industrialized, Rich, Democratic

Our main findings are the following: First, we provide new evidence on the negative correlation between risk-tolerance and impatience, which is robust to the CTB methodological factor. Second, we find that only risk averse individuals show significant time inconsistency.

The remainder of the paper is organized as follows: section 2 provides a brief overview of the literature, section 3 introduces the experimental design, section 4 presents the results and section 5 concludes.

II.2. Relation between risk-preferences and time-preferences

Anderhub et al. (2001) elicited risk-preferences by certainty equivalents for immediate lotteries and time preferences by certainty equivalents for delayed lottery. Similarly Dohmen et al. (2009) relied on certainty equivalents for a lottery to elicit risk-preferences and a multiple price list offering a choice between an immediate amount to a later amount.

II.2.1. Experimental design[81]

II.2.1.1. Risk-preferences

We elicited risk-preferences by relying on Eckel & Grossman (2008). Subjects were asked to select one gamble among the six available gambles reported in table 1. In table 1, the gambles are ranked according to their riskiness. Moving down the table from gamble 1 (a sure payoff) to gamble 6, both the expected payoff and the standard deviation increase. A subject who selects gamble 1 is categorized as *extremely risk-averse* while if he chooses gamble 6 he is considered as *risk-seeking*.

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[81] See Chapter 2 - Appendix 2 for detailed instructions.

Table 1 - Summary of gamble choices

Gamble	Low Payoff	High Payoff	Expected value	Sd	Implied CRRA[82] Range	Risk status	Fraction of Subjects (%)
1	2,800	2,800	2,800	0	3.46<r	Extremely risk-averse	16.25
2	2,400	3,600	3,000	600	1.16< r <3.46	Strongly risk-averse	16.61
3	2,000	4,400	3,200	1,200	0.71< r <1.16	Risk-averse	19.79
4	1,600	5,200	3,400	1,800	0.50< r <0.71	Slightly risk-averse	9.19
5	1,200	6,000	3,600	2,400	0< r <0.50	Risk Neutral	11.31
6	200	7,000	3,600	3,400	r <0	Risk Seeker	26.86

II.2.1.2. Time-preferences

For eliciting time-preferences, subjects were asked to choose an allocation of their endowment between a sooner date t and a later date $t+k$. Three possible options were available: they could choose to allocate the whole endowment at one of the two dates or they could choose to split it equally between these two dates. In some sense our design is a discrete version of Andreoni & Sprenger's CTB method. They found that 37% of their respondents always chose extreme allocations and the remaining ones chose on average corner allocations 50% of the time. Furthermore, those who choose interior allocations frequently choose the mid-point.

In this simplified version of the game, subjects answer three forms with 5 decisions each on allocation decisions over sooner and later time frames. In the first form, $t=1$ day and $k=35$ days; in the second form, $t=1$ day and $k=70$ days and in the third form, $t=36$ days and $k=35$ days.

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[82] Preferences follow a Constant Relative Risk Aversion functional form, this is calculated as the range of 'r' in the function $U=x^{(1-r)}/(1-r)$ for which each gamble is the utility-maximizing choice. (Eckel & Grossman, 2007)

Table 2 summarizes experimental choices and Figure 1 presents a sample CTB form.

Table 2 – Experimental parameters

Decision	t (sooner date)	k (delay)	P (Price ratios)
1	1	35	1.00
2	1	35	1.05
3	1	35	1.11
4	1	35	1.25
5	1	35	1.43
6	1	70	1.00
7	1	70	1.05
8	1	70	1.11
9	1	70	1.25
10	1	70	1.43
11	36	70	1.00
12	36	70	1.05
13	36	70	1.11
14	36	70	1.25
15	36	70	1.43

The series is designed such that participants will initially choose x_t, but at some points switches to x_{t+k}, providing the information on the utility function curvature.

In sum, variation in P gives the utility function curvature α, variation in k gives information on the discount factor δ and variation in t gives information on the present bias β.

TOMORROW and in 5 WEEKS   Participant ID: ....

For **_each_** row below (1 to 5), decide how much money you would like **tomorrow AND in 5 weeks** by marking the corresponding box. **_Remember to mark 1 box PER ROW!_**

If Game 2 is chosen as the game-that-counts, at the end of the study, I will draw a ball from this box. There are 15 balls in this box, numbered 1 to 15. The number on the ball that I draw will determine the decision according to which your actual earnings will correspond.

1	payment TOMORROW, 22/03/2013 _**and**_ payment on 26/04/2013	4000 UGX 0 UGX ☐	2000 UGX 2000 UGX ☐	0 UGX 4000 UGX ☐
2	payment TOMORROW, 22/03/2013 _**and**_ payment on 26/04/2013	3800 UGX 0 UGX ☐	1900 UGX 2000 UGX ☐	0 UGX 4000 UGX ☐
3	payment TOMORROW, 22/03/2013 _**and**_ payment on 26/04/2013	3600 UGX 0 UGX ☐	1800 UGX 2000 UGX ☐	0 UGX 4000 UGX ☐
4	payment TOMORROW, 22/03/2013 _**and**_ payment on 26/04/2013	3200 UGX 0 UGX ☐	1600 UGX 2000 UGX ☐	0 UGX 4000 UGX ☐
5	payment TOMORROW, 22/03/2013 _**and**_ payment on 26/04/2013	2800 UGX 0 UGX ☐	1400 UGX 2000 UGX ☐	0 UGX 4000 UGX ☐

II.2.2. Data

We run this field experiment in Masindi District, western Uganda (Figure 2), where Banyoro are the predominant tribal group and Runyoro the most widely spoken language. 51 tribes leave in Uganda, speaking 31 different languages. Masindi holds many Banyoro's villages that are easy to access. In addition, Banyoro are well represented in Kampala, Uganda's capital, where many students are

fluent in both Runyoro and English, which facilitates the coordination of focus group to test and translates our experimental protocol[83].

As simplified as it is from its lab version, our protocol still holds some characteristics that require minimum educational skills, such as writing and reading, in order to be tested without any external intervention. Participants were selected according to two criteria: language and land tenure. The land tenure criteria ensure that individuals have the minimum educational level required.

We run 13 sessions in 13 distinct villages within 5 sub-counties. Each session holds between 20 to 24 participants. Potential subjects were told that the study would take 4 hours and they would earn approximately UGX 3,500,00 (USD 1.35) plus a UGX 1,500.00 thank-you payment, the average equivalent of two day's wages in rural Uganda. In total, the study was conducted with 282 individuals.

Individual's socio-demographic characteristics were collected in a post-experiment survey (Table 3). Men represent 78% of the entire sample. The average individual is 42 years old, has completed primary school, owns 10 hectares of land and lives less than 1 km from a paved road.

Figure 2 – Location of Masindi district, Uganda

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[83] Protocol were adapted using double blind translation

Table 3 – Socio demographic characteristics

Village	1	2	3	4	5	6	7	8	9	10	11	12	13
N	19	22	21	22	20	23	22	23	23	20	24	23	21
Age	52.526	41.636	41.00	40.318	40.25	40.739	45.773	48.391	36.045	37.85	48.042	35.348	41.333
	(3.2)	(3.167)	(2.77)	(2.59)	(2.59)	(3.445)	(2.859)	(3.425)	(2.364)	(1.952)	(2.676)	(2.274)	(3.909)
Gender (Male=1)	1	0.773	0.857	0.727	0.7	0.826	0.955	0.696	0.818	0.75	0.833	0.826	0.571
	(0)	(0.091)	(0.078)	(0.097)	(0.105)	(0.081)	(0.045)	(0.098)	(0.084)	(0.099)	(0.078)	(0.081)	(0.111)
Education level†	1.842	1.591	1.4	1.864	1.5	1.7	2.00	1.81	1.381	1.789	1.696	1.652	1.476
	(0.138)	(0.142)	(0.134)	(0.274)	(0.136)	(0.147)	(0.186)	(0.264)	(0.161)	(0.211)	(0.183)	(0.162)	(0.131)
Land Area (Hectares)	23.444	14.00	13.25	10.813	4.425	7.429	8.024	5.87	5.262	4.363	6.083	8.522	16.881
	(10.315)	(5.589)	(3.722)	(2.933)	(0.58)	(1.963)	(1.27)	(0.77)	(0.907)	(0.648)	(1.584)	(1.635)	(12.178)
Income	23632.22	45170.47	28994.06	24742.05	73166.67	112934.8	50071.43	86188.41	70839.88	23708.33	36676.58	32206.15	42609.65
	(7963.59)	(13449.87)	(5357.876)	(5995.235)	(12034.7)	(32264.28)	(9352.197)	(31434.8)	(23026.34)	(4779.298)	(6653.92)	(11022.58)	(17607.29)
Household size	6.789	6.5	6.143	3.5	7.3	5.261	7.00	8.174	6.455	5.55	6.917	5.87	6.286
	(0.691)	(0.573)	(0.641)	(3.89)	(0.785)	(0.531)	(0.868)	(1.032)	(0.695)	(0.489)	(0.932)	(0.837)	(0.793)
Distance to market (Km)	2.516	3.75	2.643	0.98	3.205	5.122	2.414	3.326	4.518	1.181	2.85	7.413	4.838
	(0.424)	(0.492)	(0.356)	(0.235)	(0.475)	(0.697)	(0.349)	(0.275)	(0.533)	(0.268)	(0.362)	(0.705)	(0.482)
Distance to paved road (Km)	1.708	0.377	0.615	0.31	0.58	2.16	0.646	0.352	0.855	0.504	0.454	1.438	0.959
	(0.797)	(0.075)	(0.106)	(0.091)	(0.175)	(0.997)	(0.131)	(0.09)	(0.253)	(0.117)	(0.131)	(0.337)	(0.251)
Difficulty acquiring money††	2.316	2.909	2.286	2.818	2.8	3.13	3.273	2.652	2.909	2.78	3.083	3.261	2.857
	(0.154)	(0.112)	(0.22)	(0.215)	(0.213)	(0.192)	(0.15)	(0.184)	(0.196)	(3.988)	(0.19)	(0.157)	(0.21)
Outstanding loans (=1)	0.263	0.545	0.19	0.318	0.55	0.217	0.5	0.174	0.409	0.55	0.292	0.652	0.286
	(0.104)	(0.109)	(0.088)	(0.102)	(0.114)	(0.088)	(0.109)	(0.081)	(0.107)	(0.114)	(0.095)	(0.102)	(0.101)
Savings (=1)	0.526	0.727	0.286	0.409	0.6	0.478	0.864	0.478	0.636	0.85	0.625	0.739	0.619
	(0.118)	(0.097)	(0.101)	(0.107)	(0.112)	(0.106)	(0.075)	(0.106)	(0.105)	(0.082)	(0.101)	(0.094)	(0.109)

Notes: Standard errors are in parentheses. †Education is coded as: primary level (=1), secondary level (=2), tertiary level (=3), university level (=4). ††Difficulty Acquiring Money is coded as: very difficult (=1), moderately difficult (=2), moderately easy (=3), easy (=4). Asterisks denote statistical significance levels: *** p<0.01, ** p<0.05, * p<0.1.

II.3. Results

In this section we present our key findings. Subsection 4.1 is devoted to the comparison of our field data to Andreoni and Sprenger (2012)'s data (AS thereafter). There are two important differences between the two data sets: first our data was generated by a highly simplified version of the CTB method, adapted to the field and second, in contrast to AS who run controlled laboratory experiment with US students, we ran a framed field experiment in rural Uganda. In subsection 4.2 we assess the robustness of the relationship between risk and time preference, when other methods, such as CTB, are used.

II.3.1. Lab vs. Field CTB data

We estimated individual parameters for 203 of the 282 subjects of our sample[84]. Parameters were estimated as in Andreoni and Sprenger (2012). Allocations of experimental payments, xt and x_{t+k} between two periods, *t* and *t+k* are considered, and a quasi-hyperbolic structure for discounting is assumed (Laibson 1997, O'Donoghue and Rabin, 1999) leading to the following description of preferences.

$$U(x_t, x_{t+k}) = \begin{cases} x_t^\alpha + \beta\delta^k x_{t+k}^\alpha & if \quad t = 0 \\ x_t^\alpha + \delta^k x_{t+k}^\alpha & if \quad t > 0 \end{cases} \tag{1}$$

Given (1), the condition for utility maximization leads to equation (2).

$$MRS = \frac{x_t^{\alpha-1}}{\beta^{t_0}\delta^k x_{t+k}^{\alpha-1}} = P \tag{2}$$

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[84] They were 86 out of 97 in AS (2012)

Any variation in the gross interest rate P delivers the utility function curvature α. For a fixed interest rate, variation in delay length, k, delivers δ, and variation in whether the present, t=0, is considered, delivers β. Parameters are estimated by non-linear least square.

The 79 subjects for whom we could not provide meaningful estimates parameters are distributed as follows: 63 subjects chose "flat" allocations decisions[85], 3 reported confused answers[86] and for 13 of them the equations did not converge. The upper panel of table 4 summarizes the estimates of the preference parameters for the 203 usable observations (i.e., field sample). In the lower panel we report the AS estimates for comparison purposes (i.e., lab sample). In addition, we provide histograms of individual parameters for both samples (Figures 3 to figures 6).

Table 4: Individual parameter estimates

Parameter	N	Median	5th percentile	95th percentile
Field data				
α	203	0.949	-0.005	0.975
β	203	1	0.334	1.932
δ	203	1.004	0.991	1.031
r	203	-0.771	-0.999	22.58
Lab data (AS, 2012)				
α	86	0.9665	0.7076	0.9997
β	86	1.0011	0.9121	1.1075
δ	86	0.9991	0.9948	1.0005
r	86	0.4076	-0.1784	5.618

Broadly, the histograms reveal similar distributional patterns between lab and field samples. As in AS, the majority of our subjects have low discount rates, limited present bias and limited utility function curvature (i.e., both alpha and beta are mostly distributed around 1.)

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[85] No variations in participants' decisions over the 15 CTB choice sets.
[86] Inconsistent choices over time

The median curvature estimate is α = 0.949 (table 2), which is closer to the curvature estimate of AS than those reported in other papers which found CRRA estimates below 0.5 (Andersen et al., 2008).

The median time inconsistency parameter β is equal to 1, which is also very close to the AS, estimate. Consistent with their findings, the null hypothesis of no present bias (β=1) cannot be rejected (t-test, p =0.000). More specifically, 36.45% of the subjects from the field sample are categorized as 'time consistent'. Time consistent refers to the situation where the amount 'x' allocated to the sooner payment with t=0 and k=35 ($x_{1,35}$) is equal to the amount 'x' allocated to the sooner payment when t=35 and k=35 (i.e., $x_{1,35}$ =$x_{35,35}$). Within the remaining 63.55% of our sample, 36.45% are present biased (i.e., "Impatient now and patient later"; $x_{1,35}>x_{35,35}$) and 27.09% are future biased (i.e., "Patient now and impatient later"; $x_{1,35}<x_{35,35}$).

However, in contrast to AS (2012) our sample contains more time inconsistent individuals ($x_{1,35} \neq x_{35,35}$): AS found only 16.7% present biased subjects and 10.7% future biased. Our results are therefore closer to those of Ashraf et al. (2006), who ran their experiment in Philippines and found around 35% present biased and 19.8% future biased. Another difference with AS is the large proportion of subjects with negative discount rate (88.7%). We could suspect that this reveals a strong preference for commitment. Subjects prefer a certain amount in the future rather than the same amount tomorrow. It allows keeping money away from immediate consumption and secures future saving.

Both graphical and data comparison illustrate important similarities between data collected from the lab and the field. We underlined some specificities, such as a larger portion of future biased subjects and negative discount rates. Those are two characteristics most probably due to our field context (farmers from a southern country versus students from a northern country) as suggested by similarities found with Ashraf et al. (2006).

**Figure 2a - Estimated Annual Discount Rates
(Uganda sample)**

**Figure 2b - Estimated Annual Discount Rates
(AS 2012 sample)**

**Figure 3a - Estimated Daily Discount Factor (delta)
(Uganda sample)**
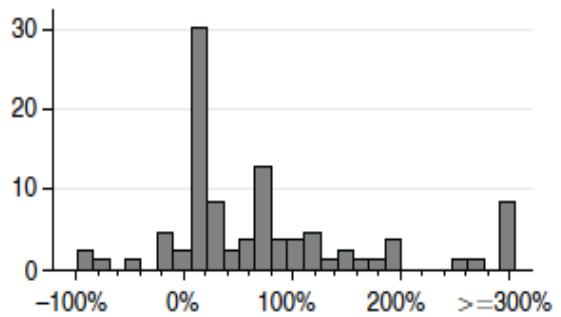
**Figure 3b - Estimated Daily Discount Factor (delta)
(AS 2012 sample)**

**Figure 4a - Estimated Present Bias (beta)
(Uganda sample)**
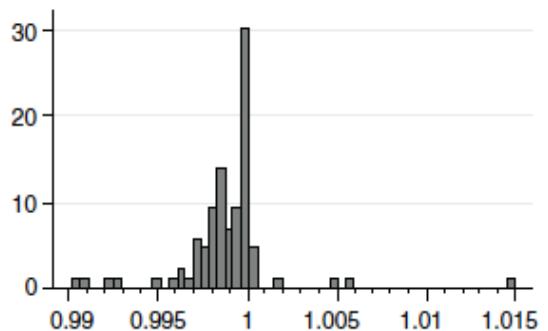
**Figure 4b - Estimated Present Bias (beta)
(AS 2012 sample)**

**Figure 5a - Estimated Curvature Parameter (alpha)
(Uganda sample)**
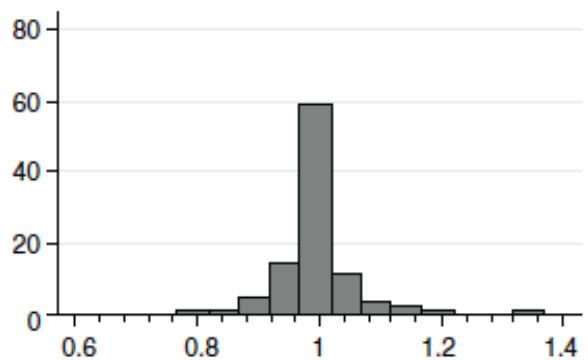
**Figure 5b - Estimated Curvature Parameter (alpha)
(AS 2012 sample)**
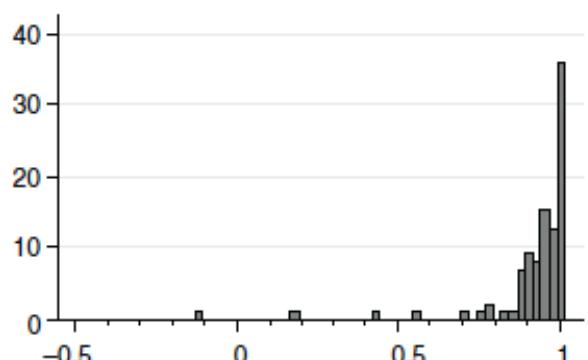
II.3.2. Correlation between risk preferences and time preferences

We classify individuals into risk categories from 1 to 6 using individual measures of risk preferences obtained by a gamble-choice task (Eckel and Grossman, 2008). In the gamble-choice task, subjects have to choose one out of six gambles. Each gamble carries a 0.50 probability of a low outcome and a 0.50 probability of a high outcome (see table 1). Moving down in table 1, from gamble 2 to 5, one can see that the low outcome is lowered while the high outcome is increased in such a way that both the expected return and the standard deviation increase simultaneously. Finally, gamble 6 is a mean-preserving spread of gamble 5.

We first present a graphical analysis and then proceed with regressions.

II.3.2.1. Graphical analysis

For the graphical analysis, we derive a measure of relative risk aversion by ranking subjects on a scale from 1 to 6 according to their selected gamble: subjects who select Gamble 1 are classified as risk-averse; subjects who select Gambles 2 to 5 are classified as risk-neutral; subjects who select Gamble 6 are classified as risk-seeking.

As illustrated in figure 7, risk-seekers are more patient than the other risk categories. The share of budget allocated to the earlier date is significantly lower for risk-seekers than for other risk-categories both for [k=35; t=1] at 10% significance level (Student t-test; t=1.6464, p=0.0999) as well as for [k=70 ; t=35] at 5% significance level (Student t-test; t=2.0787, p=0.0378). This result, illustrated by Figure 7, is in line with existing literature.

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Figure 7 – Mean experimental responses over time, by delay k and sooner payment t
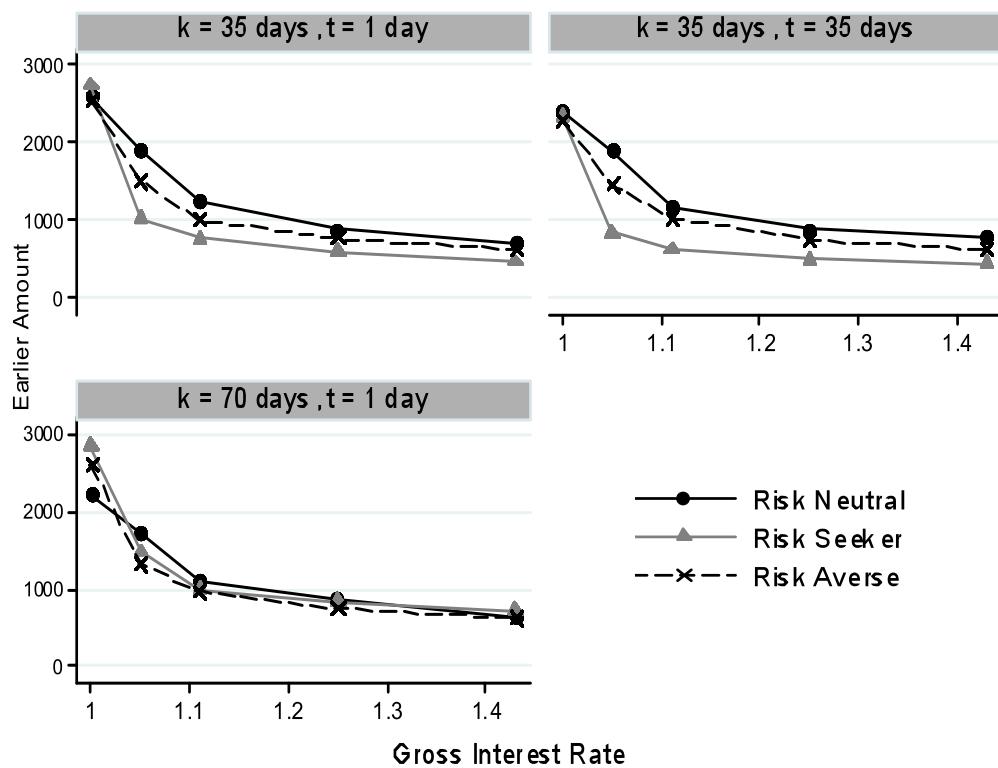
Figure 8 – Mean experimental responses over time, by risk profile
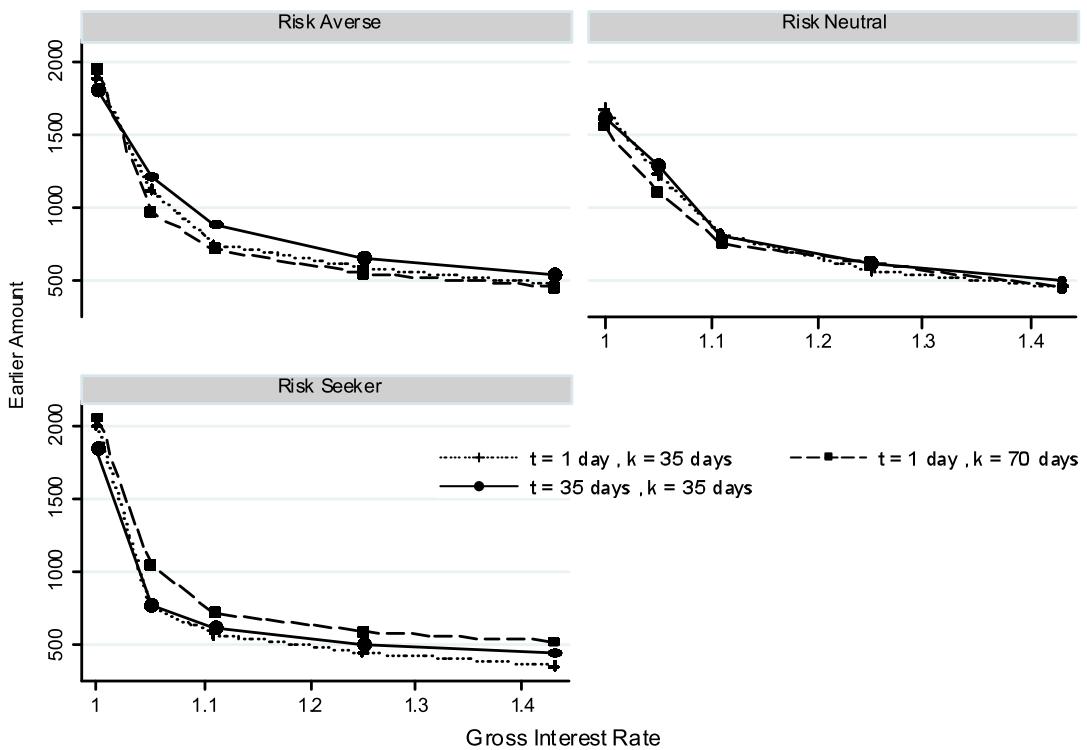
Figure 8 illustrates that risk-seekers and risk-neutral individuals exhibit time-consistent preferences ($x_{1,35}$ vs. $x_{35,35}$) while the opposite happens for risk-averse subjects. More specifically, risk-averse subjects are more likely to be future biased than other subjects. For t=1 day they choose a lower sooner amount than for t=35 days. Risk-seekers seem to be sensitive to increase in the delay k (all other things equal): they become less patient as k increases. They allocate lower amounts to the sooner date for k=35 days than for k=70 days. Interestingly, other risk categories are less sensitive to this parameter.

II.3.2.2. Regressions

Table 5 presents alpha (utility function curvature) linear regression. We ran several regressions, each of them including different control variables from our data set. The last model includes all control variables. The results show a strong negative correlation between the discount factor delta and utility function curvature alpha. This negative correlation is significant at 5% and robust to all models.

In table 6, we present an ordered probit regression, where the risk category (from 1 to 6) stands for the ordinal dependant variable. Here again, the regression supports a negative correlation between risk and time preferences (the more risk averse, the less patient).

We also find that individuals with more land are less risk averse, which is consistent with some literature linking welfare and risk. Researches found that people with higher income are less risk averse (Donkers et al., 2001; Hartog et al., 2002).

The regression reports that risk aversion increases with age. Tanaka et al (2010) found similar results. Education and gender are two more variables that seem to affect risk preferences (Tanaka et al., 2010, Croson and Gneezy, 2009). However, our sample, which is mainly compounded of men and educated people (for the purpose of testing the CTB method), does not allow testing these relationships.

Table 3: Alpha linear regression

	Critical variables	Risk exposure characteristics	Socio Economic characteristics	Land characteristics	All variables
beta	0.0128 (0.30)	0.0189 (0.43)	0.0223 (0.49)	0.0163 (0.35)	0.0368 (0.73)
delta	-1.600*** (-12.72)	-1.598*** (-12.56)	-1.584*** (-12.11)	-1.592*** (-12.45)	-1.566*** (-11.57)
drought		-0.0149 (-0.82)			-0.00984 (-0.46)
natural_risk_exp		-0.00903 (-1.06)			-0.00817 (-0.83)
attack		0.0280 (0.74)			0.0301 (0.68)
attack_exposure		0.00382 (0.33)			0.00468 (0.35)
robery		-0.0316 (-1.40)			-0.0409 (-1.59)
robery_exposure		-0.0167 (-1.58)			-0.0189 (-1.61)
gender			-0.00127 (-0.32)		0.0173 (0.29)
age			0.00161 (0.94)		0.00291 (1.16)
educ_level			-0.00380 (-0.36)		-0.00624 (-0.53)
hh-size			-0.00293 (-0.87)		-0.00371 (-1.04)
income_rough			-4.18e-08 (-0.18)		-0.000000110 (-0.42)
savings			0.0694 (1.33)		0.0731 (1.26)
borrowings			-0.0351 (-0.71)		-0.0480 (-0.88)
liquidity_diff			0.00130 (0.05)		-0.00118 (-0.04)
land_area				0.000843 (0.90)	0.000830 (0.84)
distance_market				0.00534 (0.66)	0.0107 (1.12)
distance_road				-0.00163 (-0.11)	-0.0108 (-0.44)
land_years				-0.00124 (-0.72)	-0.00189 (-0.70)
tenure_type_pos				-0.0663 (-1.23)	-0.0232 (-0.37)
_cons	2.413*** (15.68)	2.591*** (12.77)	2.301*** (10.70)	2.449*** (14.59)	2.426*** (8.04)
N	203	203	194	192	183
BIC	100.1	126.9	140.8	117.5	182.1
chi2					
p	1.01e-29	1.37e-25	7.00e-23	1.20e-25	3.83e-18

t statistics in parentheses

$* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Table 4: Oprobit Risk status (1-6)

	Critical variables		Risk exposure characteristics		Socio Economic characteristics		Land characteristics		All variables	
beta	-0.227	(-1.47)	-0.201	(-1.28)	-0.224	(-1.41)	-0.152	(-0.92)	-0.0902	(-0.52)
delta	-0.956**	(-2.00)	-0.945*	(-1.95)	-0.930*	(-1.91)	-0.867*	(-1.78)	-0.776	(-1.54)
drought			-0.0438	(-0.68)					0.00875	(0.12)
natural_risk_exp			-0.0592*	(-1.95)					-0.0466	(-1.38)
attack			-0.0223	(-0.16)					0.139	(0.86)
attack_exposure			0.0448	(1.07)					0.0577	(1.23)
robery			0.0763	(0.95)					0.0542	(0.61)
robery_exposure			-0.00230	(-0.06)					0.000133	(0.00)
gender					0.0122	(0.93)			-0.0692	(-0.33)
age					-0.0138**	(-2.35)			-0.0186**	(-2.16)
educ_level					0.00446	(0.11)			0.00654	(0.13)
hh_size					0.00357	(0.32)			-0.00310	(-0.26)
income_rough					0.000000801	(1.02)			0.00000102	(1.13)
savings					-0.199	(-1.10)			0.0336	(0.17)
borrowings					0.0973	(0.57)			-0.0276	(-0.15)
liquidity_diff					0.0953	(1.08)			0.0684	(0.71)
land_area							0.0111*	(1.72)	0.0118*	(1.79)
distance_market							0.00707	(0.24)	-0.0176	(-0.53)
distance_road							-0.0220	(-0.42)	-0.00929	(-0.11)
land_years							-0.0101	(-1.63)	0.00491	(0.53)
tenure_type_pos							0.418**	(2.18)	0.282	(1.28)
cut1										
_cons	-2.218***	(-3.79)	-2.253***	(-3.00)	-2.585***	(-3.32)	-1.785***	(-2.84)	-2.019*	(-1.90)
cut2										
_cons	-1.636***	(-2.83)	-1.654**	(-2.23)	-1.946**	(-2.52)	-1.206*	(-1.93)	-1.378	(-1.30)
cut3										
_cons	-1.105*	(-1.91)	-1.114	(-1.50)	-1.419*	(-1.84)	-0.666	(-1.07)	-0.847	(-0.80)
cut4										
_cons	-0.877	(-1.51)	-0.884	(-1.19)	-1.185	(-1.54)	-0.416	(-0.67)	-0.591	(-0.56)
cut5										
_cons	-0.596	(-1.03)	-0.599	(-0.81)	-0.883	(-1.15)	-0.125	(-0.20)	-0.276	(-0.26)
N	203		203		194		192		183	
BIC	732.1		756.6		729.2		711.4		738.4	
chi2	4.800		12.18	4	18.15		15.20		29.34	
p	0.0907		0.143		0.0525		0.0336		0.106	

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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II.4. Discussion

In this work, we used a portable version of the CTB experimental design to test the risk and time relationship in a developing country context.

The CTB method offers a unique way to elicit discounting factor, utility function curvature and time consistency through a single protocol while previous work relied on more complex experimental design. While traditionally the utility function curvature has been identified using risk elicitation method such as in the Double Multiple Price List (Andersen, Harrison, Lau, Rutstrom, 2008), the CTB method offers an innovative estimation tool.

Our estimates from a field experiment in western Uganda show to be coherent when comparing with AS 2012 data sample. We found no major differences except a few specificities most likely due to our field context.

Testing the risk and time relationship based on this novel utility function curvature estimate, we find the negative correlation rising from the literature to be robust to the CTB method. This relationship may have great implications from a policy design standpoint. An interesting path to explore could be the self-selection process in a targeted policy perspective.. In fact, incentive based policies could manipulate either the level of risk or patience to target their participants and thus reinforce their efficiency.

More specifically, we find that both risk seeker and risk neutral are time consistent. While a recent body of research argues on whether individuals are time consistent or not, risk preferences seem to be a promising way to explain this paradigm.

Discussion

Chapter III brings striking results.

By comparing Between subject Random Incentivized System and conventional individual payments, we found that subjects do not behave as theoretically predicted. Against all expectations, observed behaviours in a 10€ full pay vs. a 10€ random pay (i.e. one-in-ten probability) experiment are strongly similar. This demonstrates that all individuals are not expected utility maximizers under uncertainty. This finding contradicts with theoretical predictions and might revolutionize experimental design and implementation. Further research could extend this work by testing the Between subject Random Incentivized System with other games as well as different payments' probability.

Our work widens the scope of possibilities to all researchers willing to test ideas under logistical and financial constraints. To a greater extent, the validity of random incentive systems might offer policy makers an opportunity to use financial incentive at lower cost and set up innovative policy measures to drive behavioural changes, and encourage the adherence to better habits in a wide variety of domain (e.g., environment, health, road security, etc). Efforts to improve compliance with policies could be either too complex or too expensive to implement at large scale. As a solution, random (dis)incentive interventions (random rewards for right waste management, random fine for non respect of mandatory health measures, etc.) could be a cost-effective approach to achieve political goals.

In Section II, we used a portable version of the CTB experimental design to test the risk and time relationship in a developing country context. Section II presents the CTB method as a unique way to elicit discounting factor, utility function curvature and time consistency through a single protocol in field settings, while previous work relied on more complex experimental design. Our estimates from

field settings in rural Uganda show to be coherent when comparing with Andreoni and Sprenger's (2012) laboratory sample. However, we found no correlation between the risk measure and the CTB estimates of utility function curvature, which questions the use of risk experiments to identify and correct for curvature in discounting. Linearity is rejected, and a significant portion of our sample show time inconsistent preferences, being either present or future biased.

This Chapter (re)opens the debate on both how to incentivize subjects and how to measure preferences. It demonstrates that still a lot needs to be done from a methodological standpoint to improve scientific knowledge on how to accurately assess preferences.

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Appendix

Appendix 1

Instructions – Joueur A

Bonjour et bienvenue.

Vous avez été invité pour participer à une étude sur la prise de décision. Nous vous demandons de lire attentivement ces instructions, elles doivent vous permettre de bien comprendre le déroulement de cette étude. Lorsque vous aurez tous lu ces instructions, un examinateur les lira à son tour à voix haute.

Dans cette étude, on distingue deux rôles : participant A et participant B. Vous êtes désigné comme étant un participant A. Les participants B sont dans une autre salle.

L'ensemble des participants A et B va recevoir la somme de 6 euros correspondant aux frais de participation. Cette somme vous sera remise en espèce à la fin de l'expérience sur présentation de votre carte d'identité.

Lors de cette étude, vous allez peut être gagner une somme d'argent supplémentaire, qui vous sera également remise en espèce à la fin de la séance.

L'expérience se déroule de la manière suivante : L'ordinateur va associer chacun d'entre vous de manière aléatoire à un autre participant B qui se trouve en salle B. Vous ne saurez pas qui est cette personne, ni pendant, ni après la séance. Et les participants B ne sauront pas non plus qui vous êtes, ni pendant, ni après l'expérience.

Vous noterez qu'il y a d'autres participants dans la salle avec vous prenant part à l'étude. Vous ne serez pas associé à l'un de ces participants. Leurs décisions n'auront aucun effet sur vous et vos décisions n'auront aucun effet sur eux.

Durant cette étude, vos réponses seront traitées de façon entièrement anonyme. Vous indiquerez vos choix à l'ordinateur devant lequel vous êtes assis(e).

La somme de 10 euros **{T2 : 100 euros}** a été allouée à chaque paire de participants A et B. Les participants A peuvent décider la part de ce montant que le participant B va recevoir.

En tant que participant A, vous allez remplir un formulaire intitulé « Formulaire de décision» via l'ordinateur mis à votre disposition.

La première ligne du formulaire indique votre identifiant. L'identifiant du participant B auquel vous êtes associé figure en ligne 2. Le montant à diviser apparaît en ligne 3. Le participant A prend la décision. La décision consiste en un montant que le participant B va recevoir (ligne 4). Le montant que le participant A va recevoir (ligne 5) s'affiche alors automatiquement.

Le montant que la participant A va recevoir (ligne 5) est tout simplement le montant total, 10 euros {T2 : 100 euros}, moins le montant que le participant B va recevoir.

T1/T2 : Pour déterminer les paiements effectifs, l'ordinateur va tirer au sort 1 participant A pour 10 participants A. Seuls les participants A tirés au sort verront leur décision appliquée (et concerne donc le gain pour ce joueur A et le participant B associé). Les décisions des autres participants de seront pas appliquées et les gains à la fois pour les participants A et B seront nuls.

Vous êtes le participant A. Vous avez 5 minutes pour prendre votre décision. Seuls les participants A prennent une décision. Les participants B n'ont pas de décision à prendre. A la fin des 5 minutes, votre écran d'ordinateur va se verrouiller. Ne parlez pas aux autres participants de la salle tant que la session n'est pas terminée. Vous avez jusqu'à la sonnerie de l'alarme pour vous décider. {T1/T2 : Une fois les 5 minutes écoulées, l'ordinateur procèdera au tirage au sort. Votre gain s'affichera automatiquement à l'écran, à la fin de l'étude. }

Une fois le décompte lancé, vous aurez 5 minutes.

A la fin de l'étude, nous vous inviterons à l'appel de votre numéro à venir vous présenter afin de vous remettre en espèce la somme forfaitaire correspondant à votre participation ainsi que le gain éventuel lié à l'étude, sur présentation de votre carte d'étudiant. »

Instructions – Joueur B

Bonjour et bienvenue.

Vous avez été invité pour participer à une étude sur la prise de décision. Nous vous demandons de lire attentivement ces instructions, elles doivent vous permettre de bien comprendre le déroulement de cette étude. Lorsque vous aurez tous lu ces instructions, un examinateur les lira à son tour à voix haute.

Dans cette étude, on distingue deux rôles : participant A et participant B. Vous êtes désigné comme étant un participant B. Les participants A sont dans une autre salle.

L'ensemble des participants A et B va recevoir la somme de 6 euros correspondant aux frais de participation. Cette somme vous sera remise en espèce à la fin de l'expérience sur présentation de votre carte d'étudiant.

Lors de cette étude, vous allez peut être gagner une somme d'argent supplémentaire, qui vous sera également remise en espèces à la fin de la séance.

L'expérience se déroule de la manière suivante : L'ordinateur va associer chacun d'entre vous de manière aléatoire à un autre participant A qui se trouve en salle A. Vous ne saurez pas qui est cette personne, ni pendant, ni après la séance. Et les participants A ne sauront pas non plus qui vous êtes, ni pendant, ni après l'expérience.

La somme de 10 euros {T2 : 100 euros} a été allouée à chaque paire de participants A et B. Les participants A peuvent décider la part de ce montant que le participant B va recevoir. {T1/T2 : Pour déterminer les paiement effectifs, l'ordinateur tirera au sort 1 participant A pour 10 participants A. Seuls les participants A tirés au sort verront leur décision appliquée. Les décisions des autres participants ne seront pas appliquées et le gain sera nul. }

Seuls les participants A prennent une décision. En tant que participant B, vous n'avez pas de décision à prendre.

A la fin de l'étude, nous vous inviterons à l'appel de votre numéro à venir vous présenter afin de vous remettre en espèce la somme forfaitaire correspondant à votre participation ainsi que le gain éventuel lié à l'étude, sur présentation de votre carte d'étudiant.

Abstract

Chapter III offers a methodological contribution to the thesis. We propose to test the reliability of two innovative methods used in previous chapters, namely the *Between subject Random Incentivized System* (BRIS) and the Convex Time Budget (CTB). In the first section of Chapter III, we ran a laboratory experiment to test whether results get under the BRIS are consistent with results under conventional individual payments. In the second section, we compare field data to Andreoni and Sprenger (2012)'s data, checking for our CTB's simplified version adequacy. In addition, by combining the risk-preferences data traditionally measured and the alternative measure provided by the CTB method, we can check for the accuracy of traditional risk measurement in determining utility function curvature.

Résumé

Le Chapitre III offre une contribution d'ordre méthodologique à la thèse. Nous proposons de tester la fiabilité de deux méthodes novatrices utilisées dans de précédents chapitres, à savoir le 'Système d'Incitation Aléatoire entre Sujets[87]' (BRIS) et le Budget-Temps Convexe[88] (CTB). Dans une première section, nous réalisons une expérience en laboratoire pour tester la cohérence entre les résultats obtenus avec la méthode BRIS et ceux recueillis au travers de la méthode classique des paiements individuels. Dans une deuxième section, nous comparons des données récoltées sur le terrain aux données d'Andreoni et Sprenger (2012), afin de vérifier l'adéquation de notre protocole CTB simplifié. Par ailleurs, en combinant les données de préférences face au risque traditionnellement mesurées et les mesures alternatives obtenues par la méthode CTB, nous pouvons vérifier la pertinence de l'usage de la mesure traditionnelle du risque dans la détermination de la courbure de la fonction d'utilité.

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[87] Traduit de l'anglais 'Between subject Random Incentivized System'
[88] Traduit de l'anglais 'Convex Time Budget'

GENERAL DISCUSSION

Are we irrational or dynamically inconsistent?

This work reports several demonstrations of dynamic inconsistency in individual decision-making across Chapter I & II (moral self licensing, moral cleansing, time inconsistency). Most importantly, we argue that this inconsistency is not necessarily irrational and may result from cognitive biases.

Whether dynamic inconsistency is due to a self-regulation process, a halo effect or limited self control remains an open question. '*Limited self-control*' finds its roots in the '*Ego depletion*' theory, which considers willpower as a limited resource. According to this theory, performing a task requiring self-control may decrease the amount of self-control available for a subsequent task, potentially explaining why individual may fail to be consistent in their decisions over time. In parallel, the 'self-regulation' theory suggests that individuals behave as if they were managing a moral account seeking balance and compromise. This pattern may find biological support as stated by the homeostasis theory. Homeostasis consists in an internal regulation process to maintain conditions stable over time (such metabolic action includes sweating to maintain the body temperature constant, but also body reaction to exercising that prevents from loosing weight). This idea has mainly been developed by biologists and psychologists and mostly applied to risky behaviour. Risk homeostasis describes compensation in risky decisions and posits that a situation such as an increase in safety measures would lead to riskier attitudes (Drivers giving less room to cyclists wearing helmets). Moral compensation could therefore be embedded in a more general homeostasis frame. Indeed, beside the moral factor, some of the evidences include a risky component (i.e., increasing smoking while taking dietary supplements) as well as an economic component (i.e., being less charitable to compensate the time spent for a good cause).

In addition, it seems important to notice that we found heterogeneity in this inconsistency (indeed, some people are consistent!). Our results demonstrate that intrinsic motivation plays a role in the moral compensation mechanism (Chapter I). Our findings also suggest that risk preference could matter in time inconsistency (Chapter II). Those results require extensive research before drawing any clear-cut conclusion on inconsistency and individuals characteristics.

Further research may shed some lights on those different explanatory theories that may act either as substitute or complements in explaining dynamic inconsistency.

We initiated our investigation on pro social motivation inconsistency, and broaden the approach to inconsistency present in other behavioural domains (such as time inconsistency). As stated earlier, empirical evidence shows that risk preferences might be prone to inconsistency as well. Whether the compensation mechanism applies at a more general behavioural level and how it articulates between preferences offers both promising and exciting path for future research.

But above all, what seems important to underline is that under those circumstances, inconsistency becomes predictable. This idea strengthens the need for further investigations to better understand those biases and correct policies accordingly to improve their efficiency.

Lab vs. Field experiments

If a lab-type experiment offers the best solution when internal validity is needed, a field experiment overcomes external validity. It offers a great potential in policy perspective to test tools under real life settings where complexity of cognitive bias is restored, underlining the importance of studying interference between biases.

We also believe that lab-type experiments linked to observed behaviour offer promising avenue for research to improve experimental design. Indeed, we underlined several limits in game design that suggest further investigation. The investment game (Berg *et al.*, 1995) seems to have some limits in measuring trust for institution as suggested in Chapter II. In the same vein, but through a different

process, we found that the risk game (Eckel and Grossman, 2008) may not be the most adequate tool for measuring utility function curvature (Chapter III). This questions the internal validity of those games. Do we really measure what we think we do?

If the challenge regarding how to measure preferences in the field could be removed, it would offer a great potential to observe behaviour in a real life setting but also in a real life time frame (as opposed to lab experiment that are time restricted), to better understand decision over time, thus reinforcing the complementarities between field and lab experiments.

Further consideration for policy design: Controlled self-selection and random incentives

This dissertation illustrates many examples of preference heterogeneity, which may have important implications for designing innovative and efficient policies. Individuals react differently to regulation and financial incentives (as illustrated in Chapter I) as well as to commitment time frame (Chapter II). If incentive-based policies have mitigated effect according to individual's preferences, the policy maker may use data from preference elicitation to target a given intervention and take advantage of self-selection mechanisms. For instance, if a contract works better with present biased individuals, then the policy maker might offer a short-term contract and/or a frontload payment's structure. Considering the self-regulation issue, efforts to help individuals with their self-control must avoid burdening others with commitment they do not need.

Another important point for the policy maker concerns the use of Random Incentive Systems. Chapter III offers a methodological contribution on the validity of Random Incentivized System. But more than this, it appears to be an important finding for policy makers. This mechanism potentially offers a very innovative solution to incentive based policies that could be either too complex or too expensive to implement at large scale. In this sense, Random Incentivized Systems represent a cost-effective approach to achieve political goals.

In this dissertation, we discuss a panel of experiments that emphasize how behavioural economics may benefit to policy design. We explore what could be considered as 'irrationality', and propose ways to get better insight on human behaviour to control potential departure from theoretical prediction. Last but not least, with a better understanding on '*sub-optimal*' behavioural mechanism, policy makers may start to discover ways to control deviations from initial objectives and ultimately improve policy performance.

References

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Conclusion Générale

Sommes-nous irrationnels ou dynamiquement inconsistants?

Ce travail fait état de plusieurs manifestations d'inconsistance dynamique dans la prise de décision individuelle (effet de compensation morale, inconsistance temporelle…). Mais surtout, il apparaît que cette inconsistance n'est pas nécessairement irrationnelle et peut trouver son origine dans l'existence de biais cognitifs.

Que cette inconsistance dynamique soit liée à un processus d'autorégulation, à un effet de halo ou à une capacité de 'self-control' limitée, reste une question en suspend. Le concept de 'self-control' limité trouverait son origine dans la théorie de l' 'Ego depletion[89]', considérant la volonté comme une ressource limitée. Selon cette théorie, l'exécution d'une tâche nécessitant une certaine maîtrise de soi pourrait diminuer le niveau de 'self-control' disponible pour une tâche suivante, ceci pouvant potentiellement expliquer pourquoi des personnes peuvent manquer de cohérence au cours du temps. En parallèle, la théorie de l'autorégulation suggère que les individus se comportent comme si ils géraient un compte moral à la recherche de l'équilibre et du compromis. Cette théorie trouve un certain écho au sein du principe d'homéostasie. L'homéostasie consiste en un procédé de régulation interne visant à maintenir une stabilité dans le temps (par exemple, l'action métabolique comprend la transpiration pour maintenir constante la température du corps, mais aussi la réaction du corps à l'exercice qui engendre une résistance à la perte de poids). Cette idée a été principalement développée par les biologistes et les psychologues et elle a été plus spécialement appliquée aux comportements vis à vis du risque. L'homéostasie du risque postule qu'une situation telle qu'une augmentation des mesures de sécurité conduirait à des attitudes plus risquées (les conducteurs de véhicules donneraient moins de place aux

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[89] *Epuisement de l'égo*

cyclistes portant un casque). La compensation morale pourrait donc être intégrée dans un cadre plus général d'homéostasie. En effet, à côté du facteur moral, certains faits incluent un facteur risque (la prise de compléments alimentaires pouvant conduire à fumer davantage) ainsi qu'un facteur économique (être moins généreux pour compenser le temps passé lors d'une action caritative).

En outre, il semble important de noter que nous avons trouvé une certaine hétérogénéité dans cette inconsistance (certaines personnes sont consistantes!). Nos résultats illustrent notamment que la motivation intrinsèque joue un rôle dans le mécanisme de compensation morale (Chapitre I). Nos résultats suggèrent également que les préférences vis à vis du risque pourraient intervenir dans l'explication de l'inconsistance temporelle (Chapitre II). Cependant, ces éléments de conclusion invitent à davantage de recherches sur les caractéristiques individuelles liées à l'inconsistance dynamique ainsi que sur ces différentes théories explicatives pouvant être aussi bien substituables que complémentaires.

Mais surtout, ce qu'il semble important de souligner, c'est que, dans ces circonstances, l'inconsistance devient prévisible. Cette idée renforce la nécessité de poursuivre les recherches en économie comportementale afin de mieux appréhender ces biais et de permettre aux politiques de s'ajuster pour davantage d'efficacité.

Lab vs expériences de terrain

Si une expérience de laboratoire offre la meilleure solution lorsque la validité interne est nécessaire, une expérience de terrain répond aux enjeux lies à la validité externe. En effet, les expériences de terrain offrent un grand potentiel dans des perspectives d'applications politiques, en restituant le contexte de la vie réelle, en mettant en situation des sujets non conventionnels, avec une complexité liées aux interactions entre biais cognitifs rétablie.

Nous croyons également que le type d'expérience rapprochant les comportements mesurés à ceux observés dans la vie réelle (Chapitre II, section II) propose des pistes prometteuses pour améliorer la conception des outils expérimentaux. En effet, nous avons souligné plusieurs limites dans la conception de certains jeux qui suggèrent le besoin pour davantage de recherches dans ce domaine. Par exemple, le

jeu de l'investissement (Berg et al., 1995) semble avoir des limites dans la mesure de la confiance vis à vis des institutions comme suggéré dans le Chapitre II. Dans la même veine, mais par un processus différent, nous avons trouvé que le jeu de risque (Eckel et Grossman, 2008) peut ne pas être l'outil adéquat pour mesurer la courbure de la fonction d'utilité (Chapitre III). Ceci soulève un certain nombre de doutes sur la validité interne de ces jeux. Mesurons-nous vraiment ce que nous pensons mesurer?

Si le défi sur la façon de mesurer les préférences pouvait être levé, les techniques expérimentales offriraient un outil au fort potentiel pour l'observation des comportements en conditions réelles, mais aussi dans un cadre temps plus proche de la réalité (par opposition aux expériences de laboratoire qui sont réalisées dans un laps de temps limité), afin de mieux comprendre les processus décisionnels au fil du temps, renforçant ainsi les complémentarités entre les expériences sur le terrain et en laboratoire.

Quelques réflexions pour la conception des politiques: Auto-sélection et incitations aléatoires

Cette thèse illustre de nombreux exemples liés à l'hétérogénéité des préférences, ce qui peut avoir des implications importantes pour la conception de politiques novatrices et efficaces. Les individus réagissent différemment à la réglementation et aux incitations financières (comme illustré dans le Chapitre I) ainsi que vis à vis des délais d'engagement (Chapitre II). Si les politiques incitatives ont des effets hétérogènes selon les préférences de chacun, le décideur peut utiliser ces données pour cibler une intervention donnée et tirer parti d'un mécanisme d'auto-sélection. Par exemple, si un contrat fonctionne mieux avec les personnes manifestant une forte préférence pour le présent (*hyperbolic discounting*), le décideur pourrait offrir un contrat à court terme et / ou une structure de paiement à chargement frontal. Considérant l'effet de compensation morale, les incitations visant à aider les personnes pour lesquelles le 'self-control' fait défaut devraient éviter d'imposer à d'autres un engagement dont elles n'ont pas besoin.

Un autre point important pour le décideur politique concerne l'utilisation des systèmes d'incitations aléatoires. Le Chapitre III offre une contribution méthodologique sur la validité de ces systèmes d'incitations. Mais plus que cela, ce mécanisme offre potentiellement une solution très innovante pour

des politiques fondées sur les incitations qui pourraient sans cela se révéler trop complexes ou trop coûteuses à mettre en œuvre à grande échelle. En ce sens, les systèmes d'incitations aléatoires représentent une approche innovante et rentable pour l'atteinte d'objectifs politiques.

Dans cette thèse, nous avons discuté d'un ensemble d'expériences visant à souligner à quel point l'économie comportementale peut contribuer positivement à la conception des politiques. Après avoir exploré ce qui pourrait être considéré comme de '*l'irrationalité*', nous proposons des moyens visant à obtenir une meilleure compréhension des mécanismes sous-jacents à la prise de décision. Ainsi, grâce à cette conception novatrice visant à une appréhension corrigée de ces dispositifs 'sous-optimaux' du comportement, le décideur bénéficie d'outils innovants devant lui permettre de mieux contrôler les écarts avec les objectifs initialement fixés et, par conséquent, favoriser la mise en place de politiques innovantes et plus efficaces.

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'All generalizations are false, including this one.'

Albert Einstein

Behavioural economics for better policy design: Experimental investigations

The PhD dissertation explores some behavioural biases for improved policy design and contributes to a better understanding of the moral self-licensing phenomenon through the effect of potential mediators (voluntary vs. mandatory policies, interaction with financial incentives), the extent to which licensing might lead to behavioural change (from less cooperative behaviour to moral transgression), and the robustness of results (from classroom experiment to field experiments in developing countries). The PhD dissertation also includes laboratory experiments and the study of intertwined biases through complementary researches (mental accounting, hyperbolic discounting, risk aversion) for an increased openness and understanding of the behavioural approach as well as potential interactions among biases.

Keywords Behavioural economics; experimental economics; public policies; environmental policies; moral self-licensing

L'économie comportementale pour une meilleure conception des politiques : Investigations expérimentales

Cette thèse de doctorat vise à explorer certains biais comportementaux pouvant conduire à une meilleure conception des politiques. La thèse contribue notamment à améliorer notre compréhension du phénomène de compensation morale ('Moral self-licensing effect') par l'étude de médiateurs potentiels (politiques volontaires vs. obligatoires, interaction avec les incitations financières), la mesure dans laquelle la compensation morale pourrait conduire à une altération des comportements (allant des comportements moins coopératifs jusqu'à la transgression de la morale), et la robustesse des résultats (via la réalisation d'expériences conventionnelles auprès d'étudiants mais aussi d'expériences de terrain dans des pays en voie de développement). Cette thèse inclut également des expériences en laboratoire ainsi que l'étude d'autres biais étroitement imbriqués et complémentaires ('mental accounting'[a], 'hyperbolic discounting'[b-], aversion au risque) pour une plus grande ouverture et une meilleure compréhension de l'approche comportementale ainsi que la prise en compte des potentielles interactions entre biais.

[a] 'comptabilité mentale', [b] 'actualisation hyperbolique'

Mots Clés Economies comportementale; économie expérimentale; politique publique; politique environnementale; compensation morale

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