Adaptation Dynamics in Individual and Strategic Behavior

An Experimental Analysis

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I would like to dedicate this thesis to my amazing family and those who made this work possible and extremely fun.

Contents

List of Figures

List of Tables

Chapter 1

Introduction

When we look at the first rounds of an experiment, the large differences and variance among individual performances might be driven by a lack of experience and results tend to overestimate this heterogeneity. An experiment based on many rounds helps participants to adapt and improve decisions, both in strategic and non-strategic games; additionally, some individuals may be unaccustomed to a specific game and need some time to understand properly the task and their best (individual) strategy to apply.

This dissertation focuses, in four chapters, on adaptation dynamics and experience in strategic games and individual decision games. In particular, the role of experience through time that helps agents to improve their performances in accordance with their preferences.

Game repetition allows agents to fill the gap of experience in specific tasks, improving their performances and individual self-confidence. In this sense this dissertation aims to explore different experimental settings in which individuals, playing repeatedly the same task through the whole experiment, are able to become more sophisticated compared to early performances.

Two chapters focus on strategic behavior in a bargaining problem, in particular we compare how participants change their behavior through time. Chapter 2 analyzes the first round of a modified version of the Acquiring-a-Company game, where unexperienced individuals are asked to trade over a firm. We explore gender differences in "make- up" and "suspicion": we mainly find an effect of gender constellation such that when female sellers are aware to confront a female buyer, they "make-up" more.

Chapter 3 considers the same subjects after playing 31 rounds of the game. Participants are allowed to switch their payment method through which they will be paid at the end of the experiment. Gaining experience (the analysis was carry out in our companion paper Di Cagno et al. 2015) changes the strategic behavior of individuals yielding to a more homogeneous approach to the game , but the incentive scheme, endogenously selected, make them performing according to their individual preferences. We study the individuals' determinants of switching into a specific payment scheme taking advantage of the availability of the past history of behavior and performance in past rounds, as well as background information on individual characteristics. Second, we investigate whether the chosen payment scheme affects behavior and outcomes.

Gender differences, discussed both in Chapter 2 and 3 tend to disappear through time, women become more sophisticated improving their trading strategies and selecting efficiently the payment scheme.

Chapter 4 focuses on the individual adaptation dynamics in a Hybrid Public Good game, in particular the motivation of participants when contributing to a public good in the role of "leader" or "follower". In our design, each participant chooses an independent contribution as well as adjusted contributions depending on how the other's independent contribution qualitatively compares with the own one. We consider different adjust-probability levels: low ($p = 2.5\%$), middle ($p = 33\%$), and high ($p = 49\%$). When the probability p of adjustment is low, it is very unlikely that either contributor can adjust, hence the situation is close to a standard public good game with free riding being dominant. As p increases it becomes more likely that one will be able to adjust and the nature of the game gets closer to a trust game. We include two treatments differing in the way contributions can be adjusted: a Pure Adjustment treatment and a Contribution Choice treatment. In this work we distinguish between conditional cooperators and exploiters, which adapt differently to the game given the probability to adjust and the framing through which they adjust the independent contribution. When the probability of adjusting get larger, participants are more willing to behave as conditional cooperators, with independent contribution increasing through time.

Chapter 5 looks at individual decisions and experience effect in gambling games. We focus on a well-known cognitive bias, the almost-winning bias; agents misrepresenting the game are unable to distinguish between situations in which near misses signal ability and those in which no ability is involved. The experiment is aimed at checking players behavior given different set of information and two framings. Participant tend to change their individual behavior through time to a safer approach; they decrease their bets across rounds, although the almost-winning bias still resist for some agents. After playing several rounds, two different information settings were implemented in order to understand how to decrease the number of erroneous perceptions by warning subjects on almost-winning bias and independence of events or by disclosing the actual winning probabilities (nudging versus awareness). Both nudging and awareness are effective in reducing the willingness to choose the risky option but only the latter helps people to correctly interpret the game and the AW bias tends to disappear.

Chapter 2

Make-Up and Suspicion in bargaining with cheap talk. An experiment controlling for gender and gender constellation

with D. Di Cagno, W. Güth, N. Pace, L. Panaccione (Theory and Decision, 1-9, 2015)

Abstract

This paper explores gender differences in "make- up" and "suspicion" in a bargaining game in which the privately informed seller of a company sends a value message to the uninformed potential buyer who then proposes a price for the company. "Make-up" is measured by how much the true value is overstated, "suspicion" by how much the price offer differs from the value message. We run different computerized treatments varying in information about the gender (constellation) and in embeddedness of gender information. The asymmetry of the game and of information allows for a robust assessment of gender (constellation) effects. We report here the results from just one shot round decision since we expect such effects to be more pronounced for inexperienced participants. We mainly find an effect of gender constellation: when female sellers are aware to confront a female buyer, they overstate more, i.e. there is more "make-up". However, we cannot confirm gender (constellation) effects for suspicion.

Keywords: bargaining, cheap-talk, experiment, gender.
JEL: C78; C91; J16

2.1. Introduction

Much of gender research in experimental economics focus on differences in risk, delay, inequity, ... aversion to explain differences in subjects' choices. In this study, we are interested in the rather different hypothesis that, due to gender specific characteristics in bargaining, women are more suspicious than men and feel less obliged to tell the truth. This hypothesis is based on the evidence that persons belonging to groups that historically have been discriminated against (e.g., minorities and women) are less likely to trust and therefore behaving more strategically (Alesina and La Ferrara, 2002). Therefore, women could have evolved as relatively more risk averse (Eckel and Grossman, 2008), but also superior in detecting others' trustworthiness and in strategizing (Buss, 2005). So far the evidence for such hypotheses is inconclusive (Eagly and Wood, 1999).

We aim at verifying that women are more suspicious and strategizing than men by using a modified "Acquiring-a-Company" game (Samuelson and Bazerman, 1985), in which the seller of the company, after learning its value, can strategize when sending a value message to the potential buyer. The buyer then proposes a price after having received the message without, but knowing the value of the firm.[1] Since the message can be truthful or not, the price offer will reflect not only the desired share of surplus from trade but also the buyer's suspicion: for a more suspicious buyer the difference of the message received and the price proposed should be larger.

Since the experimental setting is one of bargaining whether to trade and, if so, at which price, our study is in line with those on gender differences in bargaining (Ayres and Siegelman 1995; Eckel and Grossman, 2001; Saad and Tripatl, 2001; Solnick, 2001; Riley and McGinn 2002; Gneezy and Rustichini, 2004; Niederle and Vesterlund, 2007; Sutter et al. 2009; García-Gallego 2012). We add new insights to this strand of gender research since we study how "make up", i.e. seller's overstatement of the firm's true value and "suspicion", i.e. buyer's underpricing of the value message, depend on gender and gender constellation. Furthermore, we study how seller's acceptance of price offers depends on gender and gender constellation.[2]

We expected women to "make-up" (strategize) more, to be more "suspicious" (underprice more), and to accept to trade more frequently when bargaining with female rather than with male partners.

Our results are based on the first paid round of an experiment which includes subsequent repeated rounds. We concentrate here on this set of data since we expect gender differences

[1] The "Acquiring-a-Company" game is often used as a simple environment to analyze the "winner's curse" (Kagel 1995, Thaler 1988). In a companion paper, we provide an analysis of this effect by using the data from repeated rounds (see Di Cagno et al. 2015).

[2] This step of the decision process resembles an ultimatum game. Therefore, our findings can be compared to the results of gender differences in that framework.

to emerge more clearly from choice behavior of inexperienced participants, for whom the offsetting effect of experience and learning are absent.

We find a significant effect of the female-female constellation for making up, i.e. sending value messages which overstate the true value of the firm. On the contrary, the experimental hypothesis that women are more suspicious than men is not confirmed by our analysis. However, the latter finding has to be interpreted with care since suspicion, measured as underpricing the value message, confounds the limited trust of that message with asking for a higher share from the surplus of trade.

2.2. The Game and the Experiment Setup

We consider a modified "Acquiring-a-Company" game in which the seller, after learning the firm's value v, can send a value message $\hat{v} = \hat{v}(v)$ to the potential buyer (Güth et al., 2014). The value of the firm is known only to the seller and is randomly generated according to the uniform density, concentrated on the interval $(0,1)$. The seller's evaluation of the firm is qv with $0 < q < 1$. The parameter q is exogenously given and commonly known. After receiving the value message $\widehat{v}$, the buyer proposes a price $p = p(\hat{v})$ for acquiring the company. If trade occurs, the gains from trade are $v - p$ for buyer and $p - qv$ for seller, hence the surplus amounts to $(1-q)v$, which is always positive. The seller can accept or reject the offer, after which the game ends. The payoff is $\delta(p - qv)$ for the seller and $\delta(v - p)$ for the buyer, where $\delta = \delta(p, \widehat{v}, v) = 1$ if the offer is accepted and $\delta = \delta(p, \widehat{v}, v) = 0$ if it is rejected. According to the benchmark solution, under the assumption of risk neutrality, the buyer offers $p^* = 0$ when $q > \frac{1}{2}$, which the seller rejects, and $p^* = q$ when $q \leq \frac{1}{2}$, which the seller accepts.

We run an experiment aimed at analyzing subjects' acceptance decisions, "make-up" – measured by the difference between the value message $\hat{v}$ and the true value v – and "suspicion" – measured by the difference between the value message $\hat{v}$ and the price offer p – by controlling not only for gender but also for gender constellation. To this aim, we ran twelve gender-balanced sessions at the laboratory of Max Planck Institute in Jena. A total of 376 students of different disciplines (11 sessions of 32 participants plus 1 session of 24) were recruited among the undergraduate population of Jena University using Orsee (Greiner, 2004). The experiment was fully computerized using z-Tree (Fischbacher, 2007).[3]

At the beginning of the experiment, half of participants were assigned to the role of seller and half to the role of buyer. In each session, male and female participants were evenly split in the two roles.

[3] After reading the instructions (see Appendix A) participants had to answer a few control questions before the experiment started.

The basic decisions were taken in the following order. First, the computer selects for each seller the value of the firm v according to a discrete uniform distribution on (0,100) and communicates it only to each seller. Second, the computer selects the value of q from a discrete uniform distribution concentrated on (0,1) and communicates it to both sellers and buyers. Third, the seller decides the value message $\widehat{v}$ to send to the buyer. Fourth, after receiving the value message, the buyer decides the price offer and communicate it to the seller. Finally, the seller decides whether to accept it or not. If she accepts, the firm will be sold at the offered price, while, if she does not, no trade takes place. At the end of the round, the payoffs of buyers and sellers are calculated by the computer and individually communicated.

We framed this decision process in three treatments differing in information only: in treatment U (Unknown), trading partners, randomly matched in pairs, are unaware of other's gender, which is known in treatment G (awareness of gender constellations). Finally, in treatment E (embedded information about the gender constellation) the field of study of both partners is added to information on gender in order to control for demand effects.

2.3. Main Findings

Proceeding as in backward induction, we begin with analyzing acceptance decisions δ by seller participants as depending on the profitability of the price offer.[4]

Observation 1. *One mainly observes the predicted theoretical rational behavior: $\delta = 1$ for $p \geq qv$ and $\delta = 0$ for $p < qv$. There exist no gender (constellation) effect in acceptance behavior of seller participants (see Table 2.1).*

These findings suggest that acceptance decision does not depend on the share of surplus that the seller aims to gain and question other-regarding concerns: at least for situations when own generosity would let the other gain whereas oneself suffers a (minor) loss, there is no evidence of pro-social behavior according to acceptance data (only 1.8% of sellers accepted to trade when $p < qv$). The seller accepts whenever he or she finds it convenient and his or her decision is not affected by a strategic behavior related to the price offer.

As far as "suspicion" is concerned, the price offer by the buyer could be influenced not only by suspiciousness about the value message but also by the desire to obtain a higher share of the surplus from trade. Thus a buyer who thinks the value message is truthful, i.e. expects $\hat{v}(v) = v$, may well propose a price $p < \hat{v}$. Actually, for ultimatum experiments, Eckel and

[4] What this neglets is a direct effect of the parameter q and the value v, which together determine the surplus from trade, as well as of $\widehat{v}$. We also checked the direct effect of q and v and the results do not change.

Table 2.1: Seller's acceptance δ by treatment and gender

Treatments	All	G & E		G		E	
Male	0.06						
	(0.05)						
Treatment G	-0.01	-0.08	-0.08				
	(0.08)	(0.07)	(0.07)				
Treatment E	0.07						
	(0.06)						
male seller-female buyer		-0.05	-0.02	-0.13	-0.05	-0.02	-0.00
		(0.09)	(0.08)	(0.20)	(0.24)	(0.10)	(0.08)
female seller-male buyer		-0.06	-0.03	0.15	0.23	-0.14	-0.11
		(0.09)	(0.09)	(0.14)	(0.17)	(0.12)	(0.10)
female seller-female buyer		-0.03		-0.08		-0.02	
		(0.09)		(0.22)		(0.10)	
male seller-male buyer			0.03		0.08		0.02
			(0.09)		(0.22)		(0.10)
profitability ($p \geq qv$)	0.71***	0.73***	0.73***	0.81***	0.81***	0.71***	0.71***
	(0.06)	(0.07)	(0.07)	(0.12)	(0.12)	(0.08)	(0.08)
Constant	0.07	0.20**	0.16**	0.02	-0.06	0.22**	0.19**
	(0.07)	(0.09)	(0.08)	(0.11)	(0.20)	(0.11)	(0.09)
Observations	188	128	128	32	32	96	96
R squared	0.43	0.46	0.35	0.46	0.46	0.49	0.49

Notes: OLS regressions. Coefficients and Huber-White robust standard errors (in parenthesis) are reported. Significance: * 0.1, ** 0.05, *** 0.01

Grossman (2001) find that women as proposer are more generous than men, that, in our set up, corresponds to offering a higher price. Thus more suspiciousness by female buyers could be compensated by more generous price offers. We do not claim to distinguish pure suspicion and underpricing to guarantee a satisfactory own share of the surplus but only maintain that more "suspicion" should increase $\hat{v} - p$.

Observation 2. *Male and female buyers do not differ in "suspicion", i.e. we cannot reject that $\hat{v} - p$ is homogeneously distributed for male and female buyer participants.*

Note, however, the significantly lower prices offered to male sellers in Treatment G (see Table 2.2).[5] This evidence could be explained by expecting that male sellers overstate more, contrary to our "make-up" hypothesis, or by discrimination of male sellers. Actually, Observation 3 below suggests no difference in overstating which supports the latter explanation.

Observation 2 as such does not question the hypothesis that women have evolved as more skeptical. The fact that we do not observe significant gender (constellation) differences in our measure of suspiciousness may be due to male buyers asking for a higher own share of the surplus from trade. This would suggest that male sellers reveal more ambition also by more make up so that the difference $\hat{v} - v$ is larger for them than for female sellers. This, however, can be rejected, as the following observation makes clear.

[5] This result is quite in contrast to the finding of Garcia-Gallego et al. (2012) in a field experiment.

Table 2.2: Buyer's offered price p by treatment and gender

Treatments	All	G & E		G		E	
Male	0.73	3.70		6.65		3.89	
	(7.18)	(8.35)		(22.38)		(9.24)	
Male $\times \hat{v}$	-0.02	-0.04		-0.06		-0.05	
	(0.14)	(0.16)		(0.40)		(0.18)	
Partner: Male			-4.28		-28.97*		2.49
			(8.59)		(15.64)		(9.54)
Partner Male $\times \hat{v}$			0.08		0.49*		-0.03
			(0.16)		(0.26)		(0.18)
Treatment G	2.39	2.14	2.51				
	(3.28)	(3.05)	(2.91)				
Treatemtn E	0.06						
	(2.49)						
q	0.15***	0.18***	0.18***	0.22**	0.21**	0.17***	0.17**
	(0.05)	(0.05)	(0.05)	(0.10)	(0.09)	(0.06)	(0.07)
$\hat{v}$	0.49***	0.47***	0.41***	0.57**	0.30	0.45***	0.45***
	(0.09)	(0.10)	(0.10)	(0.27)	(0.21)	(0.11)	(0.12)
Constant	-0.53	-1.65	2.39	-8.08	11.03	-0.35	0.28
	(4.91)	(5.65)	(4.55)	(19.50)	(12.51)	(6.00)	(4.93)
Observations	188	128	128	32	32	96	96
R-squared	0.327	0.362	0.362	0.411	0.474	0.344	0.342

Notes: OLS regressions. Coefficients and Huber-White robust standard errors (in parenthesis) are reported. Significance: * 0.1, ** 0.05, *** 0.01

Observation 3. *Male and female sellers do not differ in "make up", i.e. we cannot reject that $\hat{v} - v$ is homogeneously distributed for male and female seller participants (see Table 2.3).*

Even though there is no gender effect on making up, we find a gender constellation effect since, as stated by Observation 4, female sellers trading with female buyers "make-up" significantly more.

Observation 4. *In treatment G, there is more "make-up" in the female-female constellation, i.e. there is evidence that women are more strategizing by overstating more, quite surprisingly, confronting a female buyer (see Table 2.4).*

To further investigate the making up attitude, we report in Table 2.5 the probabilities of stating a value message equal, greater or lower than the true value of the firm for the pooled data from treatments G and E, which provide common knowledge of gender constellation.

There is quite some heterogeneity in value messages sent by sellers: 52.13% of them overstate ($\hat{v}(v) > v$), 26.60% understate ($\hat{v}(v) < v$), and 21.28% are truthful ($\hat{v}(v) = v$). Furthermore, consistently with Observation 4, we find a significant gender constellation effect on the probability of overstating ($P = 0.046$). Therefore, we conclude that average overstating and its probability are larger for the female-female constellation.

Table 2.3: Seller's "make-up" ($\hat{v} - v$) by treatment and gender

Treatments	**All**	**G & E**		**G**		**E**	
Male	1.49	1.53		-4.25		3.46	
	(2.70)	(3.43)		(6.85)		(3.97)	
Partner: Male			-2.50		-17.63***		2.54
			(3.43)		(6.09)		(3.97)
Treatment G	-1.54	0.81	0.81				
	(3.99)	(3.93)	(3.84)				
Treatment E	-2.35						
	(2.90)						
Constant	5.86**	3.48	5.50**	7.19	13.88***	2.52	2.98
	(2.38)	(2.53)	(2.74)	(5.32)	(4.00)	(2.68)	(2.94)
Observations	188	128	128	32	32	96	96
R-squared	0.005	0.002	0.005	0.013	0.218	0.008	0.004

Notes: OLS regressions. Coefficients and Huber-White robust standard errors (in parenthesis) are reported. Significance: * 0.1, ** 0.05, *** 0.01

Table 2.4: Seller's "make-up" ($\hat{v} - v$) by treatment and gender constellation

Treatments	**All**	**G & E**		**G**		**E**	
Treatment G	-1.54	0.81	0.81				
	(3.89)	(3.83)	(3.83)				
Treatment E	-2.35						
	(2.90)						
male seller-female buyer	-2.62	-2.88	-3.84	10.88	-11.00	-7.46	-1.46
	(3.94)	(4.91)	(4.97)	(8.43)	(7.73)	(5.81)	(5.94)
female seller-male buyer	-6.36*	-6.91	-7.88*	-2.50	-24.38**	-8.38	-2.38
	(3.68)	(4.69)	(4.75)	(9.50)	(8.89)	(5.26)	(5.41)
female seller-female buyer	0.77	0.97		21.88**		-6.00	
	(3.85)	(4.93)		(7.99)		(5.71)	
male seller-male buyer			-0.97		-21.87**		6.00
			(4.93)		(7.99)		(5.71)
Constant	8.65***	6.45*	7.42**	-2.50	19.38***	9.71**	3.71
	(3.24)	(3.58)	(3.71)	(6.12)	(5.13)	(3.93)	(4.14)
Observations	188	128	128	32	32	96	96
R-squared	0.026	0.026	0.026	0.263	0.263	0.029	0.029

Notes: OLS regressions. Coefficients and Huber-White robust standard errors (in parenthesis) are reported. Significance: * 0.1, ** 0.05, *** 0.01

Table 2.5: Truthtelling, overstating and understating the value message by gender and gender constellation

		female seller	male seller	P-value
Truthtelling		24.47	18.09	0.288
	female buyer	21.88	15.63	
	male buyer	31.25	21.88	
	P-value	*0.404*	*0.529*	
Overstating		50.00	54.26	0.562
	female buyer	59.38	46.88	
	male buyer	34.38	59.38	
	P-value	*0.046*	*0.324*	
Understating		25.53	27.66	0.743
	female buyer	18.75	37.50	
	male buyer	34.38	18.75	
	P-value	*0.162*	*0.098*	

Notes: This table considers data from Treatment G and Treatment E.

2.4. Conclusions

By a modification of the "Acquiring-a-Company" game, we studied in the lab how "make-up", "suspicion" and acceptance in bargaining depend on gender and gender constellation.

We find that female sellers make up significantly more and more frequently when matched with the same gender.

There is a surprising degree of truth-telling and an even higher degree of understating which, however, do not differ across gender and gender constellations. Moreover, we find no gender nor gender constellation effect on acceptance and no evidence of pro-social behavior, not even in those situations when generosity would let the other gain a lot at minor own loss. However, we can confirm that women are more strategizing by overstating more and more likely when confronting a female buyer.

Finally, at first sight it may seem that not being able to confirm that women are more suspicious is not consistent with the literature which finds significant, though conflicting, gender effects. Eckel and Grossman (2001) show that women are more generous as proposers in ultimatum experiments while Garcia-Gallego et al. (2012) argue that they are less generous, and also question the relevance of risk attitude.[6]

However, in our setting with asymmetric information and stochastic uncertainty "take-it-or-leave-it" price offers may not be gender (constellation) biased since trusting the value of the message is confounded with asking a higher share of surplus. Decoupling these two effects has been analyzed by assuming that more or less ambition in demanding a larger surplus share from trade should go along with more or less ambition in overstating. Since the latter is not

[6] According to our data, risk attitude does not affect the results.

significantly affected by gender (constellation) we could not confirm that women are more suspicious although we partly found them to be more often and to a larger extent strategizing.

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Appendix A

Translated Instructions

Introduction

Welcome to our experiment!

During this experiment you will be asked to make several decisions and so will the other participants.

Please read the instructions carefully. Your decisions, as well as the decisions of the other participants will determine your earnings according to some rules, which will be shortly explained later. In addition to your earnings from your decisions over the course of the experiment, you will receive a participation fee of 10 euro. Besides this amount, you can earn more euro. However, there is also a possibility of losing part of the participation fee, as it will be explained in the next section of these instructions. *But do not worry: you will never be asked to pay with your own money, as your losses during the tasks will be covered by the participation fee*. The participation fee and any additional amount of money you will earn during the experiment will be paid individually immediately at the end of the experiment; no other participant will know how much you earned. All monetary amounts in the experiment will be computed in ECU (Experimental Currency Units). At the end of the experiment, all earned in ECUs will be converted into euro using the following exchange rate:

30 ECU= 1 euro

You will be making your decisions by clicking on appropriate buttons on the screen. All the participants are reading the same instructions and taking part in this experiment for the first time, as you are.

Please note that hereafter any form of communication between the participants is strictly prohibited. If you violate this rule, you will be excluded from the experiment with no payment. If you have any questions, please raise your hand. The experimenter will come to you and answer your questions individually.

Description of the Experiment

This experiment is fully computerized. This experiment consists of the following **four phases, each composed by a different number of rounds**: Phase I of 1 round, Phase II of 30 rounds, Phase III of 12 rounds, and Phase IV of 10 rounds. After completing Phase I, you will proceed to Phase II; after completing Phase II, you will proceed to Phase III; after completing Phase III you will proceed to Phase IV. You can earn money in each phase of the experiment.

At the beginning and at the end of the Experiment, you are asked to reply to a short questionnaire.

At the beginning of the Experiment, each participant is randomly assigned one of two possible roles. Half the participants will be assigned the role of **Buyer**; the other half will be assigned the role of **Seller**. You will remain in the same role you have been assigned throughout the experiment.

In each of Phase I, II and III and in each of their rounds you will be matched with a different participant randomly assigned to you. In Phase IV you will decide individually and independently of your role.

Description of the Task – Phase I

In Phase I selling of a firm between a Seller, who owns the firm, and Buyer can take place. You will be told if you are Buyer or Seller, and will be matched with one of the other participant in the other role. For example, if you are selected as Buyer, then you will be randomly and anonymously matched with another participant who is a Seller.

The computer will randomly select the value of the firm among the following values: 5, 10, 15, 20, 25, 30, 35, 40, 45, 50, 55, 60, 65, 70, 75, 80, 85, 90 and 95 (all the values are equally likely). This value will be communicated only to the Seller. The Buyer will not learn the value of the firm selected randomly by the computer.

The Seller's evaluation of the firm is proportional to the value of the firm selected by the computer. This proportion will be randomly selected by the computer and can only take one of the following values: 10, 20, 30, 40, 50, 60, 70, 80 or 90 percent (all the values are equally likely). The Seller's evaluation is the value of the firm multiplied by the selected proportion. The proportion will be communicated to both, Buyer and Seller, whereas the value of the firm will be known only to the Seller. *Do not worry: the software will provide the information on the decision screen, depending on your role, Seller or Buyer.*

As an example, suppose that the computer selected a value of the firm equal to 90 and a proportion of 50 percent, so that the Seller's evaluation of the firm will be 45, corresponding to 50 percent of 90. *In this case, the Seller will find on the screen of the computer that the value of the firm is 90, the proportion is 50 percent and that the Seller's evaluation is 45*; *the Buyer will find on the screen only the proportion of 50 percent.* Another example: suppose that the computer selected a value of the firm equal to 90 and a proportion of 80 percent. In this case, the Seller's evaluation will be equal to 72, corresponding to 80 percent of 90. *In this case, the Seller will find on the screen of the computer that the value of the firm is 90, the proportion is 80 percent and that the Seller's evaluation is 72*; *the Buyer will find on the screen only the proportion of 80 percent.*

The Seller sends a value message to the Buyer about the value of the firm, which can be either true or false. Therefore, the value message is not necessarily equal to the firm value nor to the Seller's evaluation of the firm. The message consists of an integer value between 0 and 100.

After having received the message, the Buyer makes a take-it-or-leave-it offer to the Seller by proposing a price, an integer number between 0 and 100. When making this offer, the Buyer just knows the value message and by which proportion of the value the Seller evaluates the firm.

After having received the price offer of the Buyer, the Seller decides whether to accept it or not. If she accepts, the firm will be sold for the offered price to the Buyer. If she does not accept, no trade takes place. After the Seller has decided, the payoffs of Buyer and of Seller are calculated and individually communicated at the end of Phase I. These payoffs are calculated as explained below and they are paid to all participants at the end of the experiment.

Calculation of the payoff in Phase I

The payoff of the unique round in Phase I does not depend on the value message and is calculated as follows:

If the Seller has accepted the offered price, the payoffs are:

- The Buyer earns the difference between the value of the firm and the accepted price
- The Seller earns the difference between the accepted price and the Seller's evaluation of the firm

An example: suppose that the firm value is equal to 45 and that the proportion of the firm value is 80 percent, so that the Seller's evaluation of the firm is 36. Suppose the Buyer offer a price equal to 40, and that the Seller accepts it. In this case, the Buyer earns 45 – 40 = 5, and the Seller earns 40 – 36 = 4.

Another example: suppose that the firm value is equal to 45 and that the proportion of the firm value is 80 percent, so that the Seller's evaluation of the firm is 36. Suppose the Buyer offers a price equal to 55, and that the Seller accepts it. In this case, the Buyer earns 45 – 55 = - 10, and the Seller earns 55 – 36 = 19.

If the Seller does not accept the Buyer's offer, the payoffs are 0 for both Seller and Buyer.

Description of the Task – Phase II

In Phase II, you will face for 30 rounds the same situation as in Phase I. As in the previous Phase, in each of the rounds you will be matched with a different participant randomly assigned to you.

The same instructions as in Phase I apply to Phase II, also the calculation of the payoffs.

The payment from this Phase will consist of the payoff of **one of the 30 rounds randomly selected**. For example, if round number five is selected, your payment for Phase II will be the payoff you earned in that round.

<u>Calculation of the payoff in each round in Phase II</u>

The payoff of each round in Phase II does not depend on the value message and is calculated as follows:

If the Seller has accepted the offered price, the payoffs are:

- The Buyer earns the difference between the value of the firm and the accepted price
- The Seller earns the difference between the accepted price and the Seller's evaluation of the firm

An example: suppose that the firm value is equal to 45 and that the proportion of the firm value is 80 percent, so that the Seller's evaluation of the firm is 36. Suppose the Buyer offer a price equal to 40, and that the Seller accepts it. In this case, the Buyer earns 45 – 40 = 5, and the Seller earns 40 – 36 = 4.

Another example: suppose that the firm value is equal to 45 and that the proportion of the firm value is 80 percent, so that the Seller's evaluation of the firm is 36. Suppose the Buyer offers a price equal to 55, and that the Seller accepts it. In this case, the Buyer earns 45 – 55 = - 10, and the Seller earns 55 – 36 = 19.

If the Seller does not accept the Buyer's offer, the payoffs are 0 for both Seller and Buyer.

Description of the Task – Phase III

In Phase III, you will face for 12 rounds the same situation as in Phase I. As in the previous Phase, in each of the rounds you will be matched with a different participant randomly assigned to you.

The same instructions as in Phase I apply to Phase III.

At the beginning of the Phase you will be asked if you prefer to be paid on the basis of the payoff of **one of the 12 rounds randomly selected** *or* on the basis of **the average payoff of the 12 rounds**. On the basis of your choice, the computer will calculate your payoff for this Phase.

Calculation of the payoff in each round in Phase III

The payoff of each round in Phase II does not depend on the value message and is calculated as follows:

If the Seller has accepted the offered price, the payoffs are:

- The Buyer earns the difference between the value of the firm and the accepted price
- The Seller earns the difference between the accepted price and the Seller's evaluation of the firm

An example: suppose that the firm value is equal to 45 and that the proportion of the firm value is 80 percent, so that the Seller's evaluation of the firm is 36. Suppose the Buyer offer a price equal to 40, and that the Seller accepts it. In this case, the Buyer earns 45 – 40 = 5, and the Seller earns 40 – 36 = 4.

Another example: suppose that the firm value is equal to 45 and that the proportion of the firm value is 80 percent, so that the Seller's evaluation of the firm is 36. Suppose the Buyer offers a price equal to 55, and that the Seller accepts it. In this case, the Buyer earns 45 – 55 = - 10, and the Seller earns 55 – 36 = 19.

If the Seller does not accept the Buyer's offer, the payoffs are 0 for both Seller and Buyer.

Description of the Task – Phase IV

Phase IV consists of 10 rounds; during this Phase you won't interact with other participants. During this Phase you are asked to choose between pairs of lotteries. In particular, in each round for each lottery pair you have to assess which one you would prefer to play.

At the end of the experiment, **one round** will be randomly selected for payment, and the computer will play on your screen the lottery that you have preferred in this round. The payment of Phase IV is given by the result of this lottery.

Your Final Payment

Your final payment will be displayed on the screen at the end of the experiment. It is determined as the sum of:

- Payoff from the unique round in Phase I (in euro)
- Payoff from one randomly selected round in Phase II (in euro)
- Payoff from EITHER one randomly selected round OR an average payment between 12 rounds from Phase III (in euro)
- Payoff from one randomly selected round in Phase IV (in euro)
- Participation fee.

Chapter 3

To Switch or Not to Switch Payment Scheme? Determinants and Effects in a Bargaining Game

with N. Pace

Abstract

The incentive scheme selected in an experiment might trigger different type of behavior in participants. This paper is an attempt to screen the strategies adopted by agents in a bargaining game when buyer and seller have partly conflicting interests and are asymmetrically informed. We allow participants to choose the incentive scheme through which they will be paid at the end of the experiment controlling for past experience and individual characteristics. It is well known that payment method is highly correlated to the risk preferences shown by individuals, but little research is devoted to the analysis of the behavior induced by Random lottery Incentive scheme (RLI for short) and Cumulative Scheme payment (CS for short) both on individual and social results. This paper aims to fill the gap.
Keywords: bargaining, experiment, gender, payment scheme.
JEL: C78; C91; J16; J33

3.1. Introduction

In bargaining games strategic behavior of trading partners is essential for the final gains. What is generally underestimated is the importance of payment schemes in shaping strategic behavior and the heterogeneity of agents who may feel attracted by different incentive schemes that better suit their characteristics and preferences. Indeed, specific payment method in bargaining games are likely to affect individual strategies, but also tend to favor those subjects who perceived a specific incentive mechanism as more appropriate to the task and to their attitudes.

In this work we study the characteristics and strategic behaviors of experienced sellers and buyers who trade over the selling of a firm and have the option to change their payment-incentive scheme from a Random Lottery Incentive to a Cumulative Scheme (RLI and CS hereafter). In particular, we investigate whether past experience and individual characteristics affect the choice of switching payment scheme and whether this choice influences strategic behaviors. The key point is that agents are characterized by heterogeneous preferences and may behave differently when they have the option to sort themselves into payment incentive mechanisms associated to different levels of riskiness that better suit their intrinsic attitudes. So far methodological studies in the experimental context have focused on the validity of RLI and CS as unbiased and optimizing incentive schemes. Holt (1986) shows that responses to RLI might be biased by other tasks when subjects are not represented by expected utility preferences. Starmer and Sugden (1991) discuss Holt's hypothesis, rejecting his model but without ruling out the possibility that RLI is a bias-payment scheme. Although some critiques are moved to RLI, Cubitt et al. (1998) and Beattie and Loomes (1997) among others, restore the validity of RLI. [1]

In this sense, the number of experiments adopting RLI rather than CS incentive have decreased in the last decades and when comparing the two payment schemes, results appear quite mixed in the literature. Lee (2008) underlines that risk-averse subjects incentivized by CS tend to follow decreasing absolute risk aversion and behave more risk aversely while RLI is a method that can control for wealth effect and is considered a better incentive scheme. Laury (2005) focuses on eliciting choices under different payment schemes, including RLI and CS, finding out that no significant difference rises. [2]

Even though subjects might be consistent across incentive schemes when their choices affect their own payoff, things might change when they are asked to anticipate the strategy of another agent. To the best of our knowledge, current analysis explores individual decision making under different payment schemes in "games against nature," while changing incentives in games where individuals should reason and learn about other's behavior has not been explored (Beattie and Loomes, 1997).

This paper is an attempt to screen the strategies adopted by subjects in a bargaining game when buyer and seller have partly conflicting interests and asymmetric information. We are

[1] Since only one task will be paid for real, the RLI may encourage subjects to think about each task as if it were the only task faced and have the desirable effect of eliminating wealth effects (Bardsley 2010).

[2] Further discussion on incentive mechanisms to adopt in experimental settings is discussed by Azrieli et al. (2012), Cox et al. (2014), Harrison and Swarthout (2014).

particularly interested in the role of *incentives* and *motivations*. As described by Bardsley et al. (2009), *motivation* determines the behavior of subjects, although it is not controllable by experimenters because of the difficulty of knowing, for example, whether subjects preferences conform to the payment scheme imposed by the experimenter. This work tries to include both elements by allowing subjects to take their own design decision in light of their individual experience and preferences.

Previous contributions mostly focus on the effects of different incentive schemes on productivity and final payoff, without taking into account the fact that neglecting the importance of sorting into a particular scheme may lead to an overestimation of the role of incentives (Lazear, 2000). Only few empirical studies address this issue. In a controlled laboratory environment, Dohmen and Falk (2011) investigate which personal characteristics beyond individual productivity differences provoke workers to self-select into variables instead of fixed-pay contracts, and how relevant characteristics such as risk aversion, relative self-assessment, social preferences, gender, or personality shape the selection process. Their results reveal the importance of multidimensional sorting. Indeed, they find that output in the variable-payment schemes is higher than output under fixed-wage regime and they were able to attribute output differences to productivity sorting (more productive workers prefer the variable payment). Moreover, they find that women are less likely to choose a variable-payment scheme than men, supporting the idea that women tend to shy away from competition and select jobs that involve little or no competition.

The experiment of Eriksson et al. (2009) confirms the relevance of self-selection and the risk of overestimating variability of the effort exacerbated in experiments related to tournaments due to the fact that a competitive payment scheme is imposed on very risk-averse or under-confident subjects. In fact the choice on payment scheme, driven by risk preferences, reduces the variance of effort.

This paper focuses on a laboratory experimental setting which mimics a bargaining problem, using a modified version of Acquiring-a-Company game (Samuelson and Bazerman, 1985). An informed seller has to sell a company to an uninformed buyer, which offers a price based on a message (which can be true or can be false) on the value of the firm.

Experienced players, after playing 31 rounds of the bargaining problem, either assigned to the seller role or the buyer role, are asked to choose whether to switch from a RLI scheme, based on one random round selected at the end of the experiment, to a CS, consisting on the average payoff gained for the following stage, lasting 12 rounds.

The final goal of the paper is to understand the redistribution of the final outcomes and the social equality stemming from the incentive scheme chosen, and the matching between different payment schemes. In order to reach this goal, we structured the analysis in three steps.

First, we study the individuals' determinants of switching into a specific payment scheme, taking advantage of the availability of the past history of behavior and performance in the first stage of 31 rounds, as well as background information on individual characteristics. Second, we investigate whether the chosen payment scheme affects behavior and outcomes. Finally, we focus on the redistributive issue. In particular we study whether, under asymmetric information and different incentive schemes, sellers and buyers are able to share the total surplus of a single trade, and whether social equality is favored by switching incentive scheme.

Our results point out that sellers choose the payment scheme regardless of past history and that female sellers are more attracted by the CS scheme than male sellers, while buyers, who are actually facing the risky choice, prefer RLI where they moderate riskiness by playing more aggressively, but this is true only for female buyers. After choosing the payment scheme, sellers are generally more willing to accept the deals when paid according to CS, while buyers are more likely to earn more when choosing the RLI scheme, associated to lower price offers to the sellers.

Players are unaware of the payment selected by their trading partner, although the seller gets better deals when choosing CS and meeting a CS buyer; buyer improves his/her payoff by selecting RLI and meeting with a CS seller.
This paper is organized as follows. Section 3.2 describes the game model. Section 5.2 focuses on the experimental approach and Section 3.4 illustrates the results. The main conclusions of the paper are reported in Section 5.4.

3.2. Game Model

The game we adopted in this work is based on a modified version of the Acquiring-A-Company game proposed by Samuelson and Bazerman (1985). The firm owned by the seller has value v (known only by seller), randomly generated according to the uniform distribution $(0,1)$. However, for the seller the value of the firm is only qv, with $0 < q < 1$. The distribution of v and the value of q are common knowledge, while the value of the firm v is only known by seller. If trade occurs at price p, the buyer earns $v - p$ and the seller $p - qv$.[3] The decision process in each round is as follows:

(i) knowing v, the seller sends the value message $\hat{v} = \hat{v}(v)$ which might be true ($\hat{v} = v$) or false ($\hat{v} \neq v$);

(ii) after receiving message $\hat{v}$, the buyer proposes the price $p = p(\hat{v})$;

[3] Di Cagno et al. (2015b) is based on the same model

(iii) after receiving the price offer, the seller accepts it ($\delta(p) = 1$) or rejects it ($\delta(p) = 0$).

The seller earns $\delta(p)(p - qv)$ and the buyer $\delta(p)(v - p)$: when trading, i.e., when $\delta(p) = 1$, the total surplus $v(1 - q)$ is always positive. When not trading, i.e., when $\delta(p) = 0$, both buyer and seller earn nothing.

Since $\delta(p) = 1$ is only optimal for $p \geq qv$, a risk-neutral buyer expects to earn

$$\int_0^{p/q} (v - p)\, dv = (0.5 - q)\, \frac{p^2}{q^2} \tag{3.1}$$

which increases (decreases) with p for $q < 0.5$ ($q > 0.5$). Since $v < 1$ implies $vq < q$, it is never optimal for the buyer to offer a price higher than q: the price $p = q$ is optimal for $q \leq 0.5$ whereas trade is avoided by $p = 0$ for $q > 0.5$. This benchmark solution is not questioned by cheap talk, i.e. the value message $\hat{v}$.

Still one might want to speculate how behavior is affected when $-$ at least some $-$ seller participants are feeling obliged to tell the truth. When expecting this, buyer participants may believe the message $\hat{v}$ and suggest a price between $q\hat{v}$ and $\hat{v}$. Fairness-minded buyer participants might even propose the price $p(\hat{v}) = \frac{(1+q)\hat{v}}{2}$ splitting the surplus from trade $(1-q)\hat{v}$ equally split so that the Surplus Share (SS) gained by seller and buyer is $SS_{Buyer} = SS_{Seller} = \frac{(1-q)v}{2}$ which implies $\frac{p-qv}{(1-q)v} = \frac{v-p}{(1-q)v}$. Actually quite a number of seller participants feel obliged to choose $\hat{v}(v) = v$, and many price offers lay between $q\hat{v}$ and $\hat{v}$. However, cheap talk value messages more frequently induce opportunistic sellers to try to exploit buyers by "making up" via $\hat{v}(v) > v$ and this, in turn, questions buyers' trust in the message sent by the seller. We expect experienced buyers to be more skeptical and less trusting in order to avoid losses and the winner's curse.[4].

3.3. Experimental Protocol

We refer to the last stage results of a broader experimental project as Stage 1.[5] This stage consists of playing the bargaining game for 12 rounds and has been preceded by 31 rounds of the same game, which should allow our participants to fully understand the game (we call it Stage 0).

We ran 12 sessions with a total of 376 students (11 sessions with 32 participants each, plus one session with 24), recruited among the undergraduate population of Jena University using

[4]Winner's curse in the modified version of Acquiring-a-company game has been discussed by Di Cagno et al. (2015b)

[5]The English translation of the Instructions of the whole experiment is reported in AppendixA, where Phase III refers to what we name here Stage 1. We refer to previous stages as Stage 0.

Orsee (Greiner, 2004), at the laboratory of Max Planck Institute in Jena. The experiment was fully computerized using z-Tree (Fischbacher, 2007).

At the beginning of the experiment, before Stage 0, each participant is randomly assigned to one of the two possible roles (seller or buyer) and remains in this role throughout the whole experiment: Half of the participants are buyers, the other half sellers. Without being made aware of this, half of the sellers and buyers were males and the other half females. In each round, participants were randomly matched with a partner in the other role in order to possibly trade the firm owned by the seller. The value of the firm v, randomly selected for each seller-buyer pair according to a discrete uniform distribution concentrated on $(0,100)$, is told only to the seller (the actual values in the experiment, selected in steps of five, were 5, 10, ..., 95). Both (seller and buyer) are aware of the proportion (q), correlating the true evaluations v for buyer and qv for seller linearly. This proportion q is randomly selected from a discrete uniform distribution $(0,1)$; the actual values q in the experiment were rescaled in % and could only assume the following values: 10, 20, 30, 40, 50, 60, 70, 80, or 90 percent.

Table 3.1: Road map of game rounds

Step†	Seller	Buyer	Description
0	q, v known Partner information⋆	q known Partner information⋆	Initial information provided to buyers and sellers
1	Message $\hat{v}$	X	Seller sends message to Buyer
2	X	Price offer $p(\hat{v})$	Buyer makes price offer
3	Acceptance $\delta(p)$	X	Seller accepts or refuses price offer
4	Payoff $\delta(p)(p-qv)$	Payoff $\delta(p)(v-p)$	Seller and Buyer informed on payoff

† Each round involves four-steps.

⋆ Partner information depends on the treatment.

X Participants wait for partner's decision, i.e. they are inactive.

In each round (see Table 3.1) bargaining proceeds in the following way: The seller sends a value message ($\hat{v}$) to the buyer which can be true or false but not exceed 100. After receiving the message, the buyer proposes a price p which cannot exceed 100. Having received the price offer, the seller can accept it or not. If accepted, the firm is sold at the offered price; if not, no trade takes place. After each round, payoffs are calculated and privately communicated to buyer and seller.

Random matching between buyers and sellers was implemented to balance our sample by gender constellation. Pairs occurred in equal proportion: male buyer/female seller, male buyer/male seller, female buyer/male seller and female buyer/female seller. Participants were reminded in each round that they have been randomly paired and they received some initial information on their trading partner. We ran four treatments differing in information provided on the trading partner at the beginning of each round: In treatment *U* (Unknown), trading partners randomly matched in pairs, are unaware of the other's gender, which becomes known in treatment *G* (awareness of Gender constellation). Treatment *OC* (Other Confound) provides information about the field of study instead (Economics versus Non-Economics). Finally, treatment *E* (Embedded Gender Constellation) provides information about other's gender and field of study.

3.3.1. Payment Method

At the beginning of the experiment, subjects were instructed that the payment method adopted for the first part of the experiment was RLI scheme, in particular a round randomly selected at the end of the whole experiment was going to be truly paid.[6] At the beginning of Stage 1, participants are asked which payment they prefer to adopt for the following 12 rounds of Stage 1, either keeping the RLI scheme or switching to an average cumulative method. Immediately after Stage 1, in Stage 2[7] subjects played the Holt and Laury's (2002) lottery protocol to elicit risk preferences. Final gains were communicated privately at the end of the experiment, after Stage 2.

At the end of each round, participants received feedback about their final payoff for that round (in ECU). The conversion rate from experimental points to euro (1 euro=30 ECU) was announced in the instructions. If the seller accepted the offered price, the buyer earned the difference between the value of the firm and the price $(v-p)$ and the seller the difference between the accepted price and her evaluation of the firm $(p-qv)$. If the price was not accepted, the final gain from trade for both was zero due to no trade. Participants received an initial endowment of 300 ECU (10 euro) in order to avoid bankruptcy.

3.4. Result

The result section focuses on different aspects related to the bargaining problem: We consider both decision variables related to strategies adopted by buyers and sellers and their final outcomes. The decision variables we consider are:

- Seller's cheating propensity: the share $\frac{\hat{v}-v}{\hat{v}}$ for $\hat{v}>0$, the relative difference between the value stated and the true one;

[6]Stage 0 collects both Phase I and Phase II in the instructions, where Phase I lasted 1 incentivized round and the 30 rounds of Phase II were paid according to RLI payment method.

[7]Reported in AppendixA as Phase IV of instructions.

- Buyer's rentability: the share $\frac{\hat{v}-p}{\hat{v}}$, a measure of the gains sought by buyers given the message received and their trust on it. [8]

Both measures indicate the aggressiveness of seller and buyer in dealing with the trading partner. Then we consider the outcome of the trading process:

- Seller's surplus share ($\frac{p-qv}{(1-q)v}$) and buyer's surplus share ($\frac{(v-p)}{(1-q)v}$) when the deal is accepted.

- number of times the deal was refused; this variable is considered as an outcome from buyer's point of view and a decision variable when we consider the seller role.

The advantage of our experiment is the opportunity to study when/which subjects switch the payment mode, and whether they act more cautiously in one trading role compared to the other. This decision might be affected by inertia and wealth effect. In this sense, we design the experiment in order to account for switching from RLI to CS because (a.) subjects are not affected by wealth effect in the phase before switching payment and (b.) RLI is perceived as riskier compared to CS. We control whether switching is driven by individual characteristics such as gender and risk preferences.

We base the result analysis on (i) individual determinants and experience, the latter focusing on the last 10 rounds of Stage 0, which is the last phase of previous Stage where subjects have played enough rounds to become experienced players (Section 3.4.1). The following step (ii) is to evaluate how the payment selection changes the bargaining results from the seller and buyer point of view, in particular whether one group will end up better off than the other in terms of total surplus share gained in each period (Section 3.4.2). Finally, (iii) it is analyzed whether subjects are better off keeping or changing the payment scheme (Section 3.4.3). The final analysis is aimed to analyze whether switching improves the result for both trading partners or minimizes losses in the game (Section 3.4.4).

3.4.1. Why Do Players Switch?

At the beginning of Stage 1, the majority of sellers (81%) and buyers (73%) decide to be paid through the average payment scheme. When we look at the gender composition, we find a significant difference between female and male sellers i.e., female sellers choose (significantly) more than males the cumulative payment (see Table 3.2). This gender difference does not hold for buyers: Males choose the random payment around 25% more of the time than females do.

[8]Given the exogenous firm values v and q selected at each round, rentability and cheating are considered as percentages on the value stated by sellers

Table 3.2: Random payment choice in stage 1 by gender and role

Test on average selection of the random payment (RLI):				
	Female	Male	Female and Male	P value (F-M)
Sellers	0.117	0.255	0.186	0.015
	(94)	(94)	(188)	
Buyers	0.245	0.277	0.261	0.620
	(94)	(94)	(188)	
Sellers and Buyers	0.181	0.266	0.223	0.048
	(188)	(188)	(376)	
P value (S-B)	0.02	0.74	0.08	

Notes: Percentage of subjects choosing random lottery incentive scheme in stage 1 and test on payment scheme choice by gender and role. Number of observations in parenthesis.

Apparently, the group of female sellers chooses with higher frequency the cumulative payment when it is playing in the role that does not involve any risk.[9]

Past history plays a role in payment selection, as does role and gender. Figure 3.1 compares past choices (last ten rounds from Stage 0) with the payment scheme selected at the beginning of Stage 1 in order to control whether past decisions and outcomes drive sorting into payment scheme. Figure 3.1 considers both seller's decision and outcome on the left side and buyer's rentability and surplus share on the right side.
When we look at sellers, cheating propensity and final surplus share are not distributed differently if we control for those selecting CS rather RLI scheme. The cumulate distribution is alike for both groups with non-parametric test confirming the graphical intuition. Buyers seeking for higher rentability are more likely to select the RLI scheme in Stage 1 (p-value < 0.01), but when we decompose the effect across gender, the result is significant only for female buyers (p-value < 0.003).

Table 3.3 collects the regression analysis where we account for gender, role, and past experience (referring to both decisions and outcomes). In particular, from model (1) to (3), we focus on seller's likelihood to keep RLI payment by past cheating, by past surplus share of accepted deals (in both cases including average, standard deviation, minimum, and maximum), and by past acceptance rate (we consider the last 10 rounds of Stage 0). Female sellers are more likely to switch for the cumulative payment regardless of past decisions and outcomes. Risk preferences (weakly but) significantly affect the choice made by sellers; those showing risk-loving attitudes are more willing to keep the RLI scheme.

On the right side of Table 3.3, from model (4) to (6), we analyze the RLI scheme as a function of buyer's rentability, surplus share of accepted deals and acceptance rate in the last 10 rounds of Stage 0. Those offering lower prices (seeking higher rentability and trusting less) are more willing to maintain the RLI scheme until the end of the experiment: These buyers are characterized by more aggressiveness and are rejected more often (acceptance rate

[9] We test risk preferences distribution among roles and gender, without finding any relevant difference.

Figure 3.1: Past decisions and outcomes by payment scheme

Notes: We consider decisions and outcomes from the last 10 rounds of Stage 0, splitting the subjects by the payment decision made at the beginning of Stage 1

is significantly and negatively related to RLI scheme), and make higher profit when trades are accepted. The payment selections for buyers are greatly influenced by past choices and outcomes. Apparently their choices do not involve risk preferences.

Result 1. *While buyers are affected by past experience (rentability, acceptance and surplus share), the choices made by sellers are mainly related to individual characteristics. Risk preferences play no role for buyers while sellers are partially driven by them.*

3.4.2. Payment Scheme and New Strategies

At the beginning of Stage 1, we ask subjects if they prefer to keep RLI payment or switch to CS payment. Subjects sorting themselves in new payment scheme might also change their behavior. This section is aimed to describe the individual strategy after selecting the payment.

Table 3.3: Payment scheme: the role of past decisions and outcomes

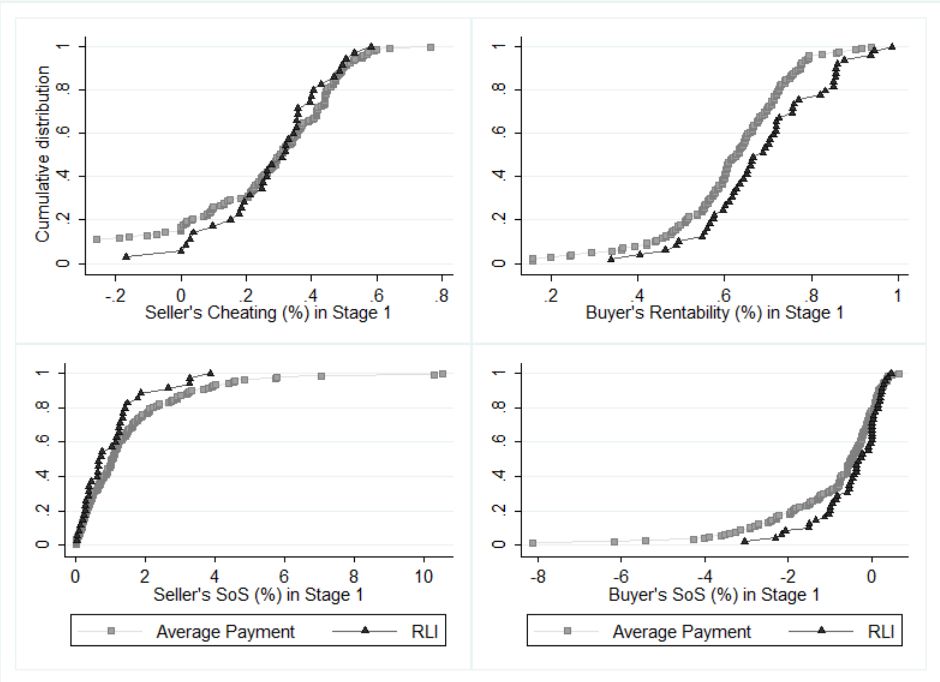
	Sellers			Buyers		
	Dependent: RLI dummy					
	(1)	(2)	(3)	(4)	(5)	(6)
	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)
Male	0.57**	0.55**	0.54**	-0.00	0.02	0.10
	(0.23)	(0.22)	(0.22)	(0.21)	(0.21)	(0.20)
Risk	0.09*	0.09*	0.09*	0.02	-0.05	0.00
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)
Average Cheat (%)	-0.51					
	(1.19)					
Sd Cheat	-2.04					
	(3.36)					
Min Cheat	-0.04					
	(1.02)					
Max Cheat	1.25					
	(1.40)					
Average Rentability (%)				3.68**		
				(1.48)		
Sd Rentability				1.63		
				(5.03)		
Min Rentability				0.11		
				(1.50)		
Max Rentability				-1.40		
				(2.31)		
Average SS (%)[1]		0.01			0.32*	
		(0.12)			(0.18)	
Sd SS		-0.17			0.14	
		(0.16)			(0.19)	
Min SS		0.28			-0.00	
		(0.27)			(0.05)	
Max SS		0.06			-0.86**	
		(0.05)			(0.40)	
Average Acceptance (%)			0.80			-1.33***
			(0.64)			(0.46)
Constant	-1.71***	-1.58***	-1.98***	-2.19***	0.12	-0.05
	(0.53)	(0.28)	(0.43)	(0.82)	(0.36)	(0.30)
Observations	188	185	188	188	174	188
Chi-squared	11.64	14.88	11.64	13.04	7.33	9.07
Pseudo R^2	0.09	0.07	0.06	0.06	0.05	0.04

Notes: Probit estimation and standard errors in parenthesis. All the decisions are based on individual average decision of the final 10 rounds of Stage 0. Risk measure goes from 0 (maximum risk averse) to 9 (maximum risk seeking).
[1] Surplus share (SS) include only the accepted deals: the analysis is consistent when we analyze the surplus share including not accepted contracts. * p<0.1, ** p<0.05, *** p<0.01.

We compare cumulative distribution of seller's cheating and surplus share of subjects switching to CS and those keeping the RLI scheme in Stage 1 (left side of Figure 3.2) and buyer's rentability and surplus share (right side of Figure 3.2). Results are now different from what we concluded looking at Figure 3.1: Seller's cheating distribution (graph top-left of Figure 3.2) does not differ across payment schemes, (non parametric test confirms this) but the rate of participants stating the true value of v is significantly higher among those keeping the RLI payment (see Appendix B Table B.1). Additionally, when we consider the seller's outcome, we find that sellers are making better deals when selecting the cumulative scheme although the non-parametric test does not reveal a significant difference.

Buyer's rentability after payment selection is consistent with the analysis of the last 10 rounds of Stage 0: Buyers who are more skeptical and seeking a larger surplus share are trying to offset the risk of having losses, and they are willing to select the RLI scheme because they use a game strategy based on a very low level of trust toward sellers. As we discussed in Section 3.4.1, the effect is mainly driven by female buyers.

Figure 3.2: Decision and outcomes in Stage 1 by payment scheme selection in Stage 1



Notes: We consider the average decision by subjects in the 12 rounds of Stage 1.

Even looking at the surplus share distribution after the payment selection we notice a similar path for the last 10 rounds of Stage 0. Buyer's surplus share improves when subjects sort themselves in the RLI. In fact, the probability of incurring losses strongly decreases (p-value <0.003).

This result resembles the conclusions from Stage 0, although the sorting effect amplifies it in Stage 1: While males are choosing the payment scheme regardless of their role and decisions, female participants sort themselves to the cumulative scheme when they are in the role of seller in order to avoid the payment mechanism perceived as riskier. Figure 3.2 justifies this choice because cumulative scheme seems more rewarding than the RLI scheme.

When we consider buyers, female subjects select RLI scheme as much as males even though the female subjects choosing it are also moderating the riskiness of the game by playing with a lower degree of trust toward seller, and offering lower prices.

The analysis in Table 3.4 focuses on three types of dependent variables representing results of Stage 1, in particular the acceptance rate, the surplus share gained (average and standard deviation) when deals are accepted. The model we implement is:

$$y_i = \alpha + \beta_1 Male \times CS_i + \beta_2 Male \times RLI_i + \beta_3 Female \times RLI_i + \gamma X_i + \delta Risk_i + \varepsilon_i \quad (3.2)$$

Where we account for the interaction between payment and gender (the benchmark is the fourth category Female×CS), X_i which is the average cheating (rentability) for seller (buyer) and risk measure. In the left columns of Table 3.4, we consider as dependent variables Seller's acceptance (model 1), average surplus share (model 2) and surplus share standard deviation (model 3). The analysis that underlines the acceptance rate is related to gender and payment scheme selected, in particular female sellers choosing CS are more likely to accept the price offered by buyers than female sellers choosing RLI, but the result can be extended also when we compare female sellers with CS incentive to the male sample (although the result is not significant). Surplus share is statistically higher (p-value<0.1) for women choosing CS but this is significant only when we compare with males selecting RLI. This implies that women are able to close more deals when sorting themselves in CS and also make the best of it. This result is confirmed by the robustness check in the last 10 rounds of Stage 0 (Appendix B, Table B.2): No interaction between contract scheme and gender is significant before choosing the payment contract. Females switching to CS in Stage 1 are able to perform on average better than other subjects, seeking a higher surplus share (on average) and closing more deals than the other groups.

In the right columns of Table 3.4, we focus on the likelihood that the buyer's offer will be accepted (model 4), with average surplus share (model 5) and surplus share standard deviation (model 6). Buyers are earning significantly more when sorting themselves in the RLI scheme, although there is no statistical difference in the coefficient "RLI*Female" and "RLI*Male" where we account for the gender effect. More aggressive buyers, seeking for larger shares of gains, are generally accepted less frequently but this effect seems stronger for male buyers rather than female ones. Standard deviation of surplus share through the 12 rounds of Stage 1 is significantly lower, both for sellers and buyers, when RLI payment is selected.

Result 2. *Female sellers choosing CS are more likely to accept the price offered by buyers and to get larger surplus share (significant only when comparing with males choosing RLI). Female buyers select RLI scheme as much as males but their strategy aims to moderate the risk, by playing with lower degree of trust toward seller and offering for lower prices.*

3.4.3. Matching Contracts

The total surplus in each deal is exogenously defined by the problem variables (q and v); players cannot change the available social surplus from trade. We investigate how payment scheme affects redistribution between agents and whether switching (or not) favors some agents.

Table 3.4: The role of payment scheme sorting on acceptance rate and surplus share in Stage 1

	Seller			Buyer		
	(1) Acceptance (Mean)	(2) SS accept[1] (Mean)	(3) SS accept[1] (SD)	(4) Acceptance (Mean)	(5) SS accept[1] (Mean)	(6) SS accept[1] (SD)
	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)
Cumulative Scheme*Male	-0.02	-0.53	-1.14	-0.05*	0.48	-0.88
	(0.03)	(0.46)	(0.91)	(0.03)	(0.48)	(1.00)
RLI*Male	-0.05	-0.98*	-1.82*	-0.06*	1.06**	-2.32**
	(0.04)	(0.52)	(0.99)	(0.04)	(0.52)	(0.96)
RLI*Female	-0.10*	-0.70	-1.94**	-0.03	1.08**	-2.00*
	(0.06)	(0.53)	(0.89)	(0.04)	(0.50)	(1.06)
Cumulative Scheme*Female	Benchmark					
Average Cheating (%)	0.04*	0.16	0.24			
	(0.02)	(0.17)	(0.29)			
Average Rentability (%)				-0.66***	1.79	-3.24
				(0.19)	(1.78)	(3.90)
Risk	0.01**	0.01	0.00	0.01*	-0.03	0.10
	(0.01)	(0.09)	(0.18)	(0.01)	(0.08)	(0.15)
Constant	0.51***	2.85***	4.74***	0.94***	-3.05***	6.65***
	(0.03)	(0.57)	(1.17)	(0.13)	(1.09)	(2.33)
Observations	188	187	185	188	184	183
R^2	0.06	0.02	0.02	0.32	0.04	0.04

Notes: Dependent and independent variables are based on individual average and standard deviation of the 12 rounds of Stage 1. Risk measure goes from 0 (maximum risk averse) to 9 (maximum risk seeking). Seller's cheat (%) is measured as difference $\frac{\hat{v}-v}{\hat{v}}$. Buyer's rentability (%) measures the distance between price offer and message received $\frac{\hat{v}-p}{\hat{v}}$.
[1] Surplus share (SS) include only the accepted deals: the analysis is consistent when we analyze the surplus share including not accepted contracts. * p<0.1, ** p<0.05, *** p<0.01.

Thanks to previous results, we concluded that switching the payment scheme is a signal of individual preferences for men and it is more related to strategizing for women. In this sense, we now discuss which payment scheme allows participants to pursue higher profits and via which one they better perform.

In the bimatrices of Figure 3.3 we compare the aggressiveness for sellers and buyers (cheating and rentability), acceptance and surplus share. Each matrix represents results for seller and buyer, in particular, we set the trade between buyer and seller choosing CS as the reference point. The other cells represent the other possible matches, and we check if these are significantly different from the reference group for sellers (first number in bracket) and buyers (second number).

The design excludes to be informed on the payment selected by the trade partner, although the results are significantly changing given the matching partner and the contract he or she has chosen.

Cheating and rentability are measures indicating the aggressiveness of players in getting larger profits: Buyer rentability (right entry in the upper left bimatrix) is significantly higher when buyers are sorting themselves in RLI scheme in Stage 1 (as well as late phase of Stage 0 see Appendix B, Figure B.1). The result is consistent to previous observations where buyers

Figure 3.3: Incentive scheme matching between seller and buyer on acceptance rate and surplus share

		Buyer RLI	Buyer CS
Seller	RLI	(0.285 , 0.092**)	(0.225 , 0.017)
Seller	CS	(0.127 , 0.079***)	Benchmark

Cheating/Rentability

		Buyer RLI	Buyer CS
Seller	RLI	(-0.363***, -0.436***)	(-0.067,-0.082)
Seller	CS	(-0.184***,-0.1773*)	Benchmark

Acceptance

		Buyer RLI	Buyer CS
Seller	RLI	(-1.130, 1.131)	(-0.831, 0.831)
Seller	CS	(-0.986*, 0.986*)	Benchmark

SS accepted trades

		Buyer RLI	Buyer CS
Seller	RLI	(-0.902* , 0.771)	(-0.518 , 0.487)
Seller	CS	(-0.699**, 0.613**)	Benchmark

SS (all plays)

Notes: Coefficients from panel regressions (probit for acceptance dummy and xtreg for the other variables). We consider here the 12 rounds of Stage 1. All results are considered as difference with seller (left entry) and buyer (right entry) choosing CS.
* p<0.1, ** p<0.05, *** p<0.01.

tend to sort themselves to RLI contract when more aggressive. When we control seller's cheating, left entry in the upper left bimatrix of Figure 3.3, no difference can be observed across groups.

Acceptance rate, in the upper right bimatrix, is significantly higher when seller and buyer choose to switch to CS contract, while the likelihood is lowest when both select the RLI scheme. When we double check for the last 10 rounds of Stage 0 (see Appendix B, Figure B.1), we confirm some sort of self-selection; sellers with RLI scheme meeting with future CS buyers in Stage 0 are trade partners with the highest probability to close the deal.
Looking at outcomes, we conclude that surplus share including only accepted deals (see lower left bimatrix in Figure 3.3) is significantly better (worse) for buyers (sellers) when choosing the RLI (CS). Buyer significantly improves his situation when meeting a CS seller, while seller is significantly worse off when meeting buyer with RLI scheme. When we consider the surplus share, including failed trades, the effect becomes even stronger (see lower right bimatrix in Figure 3.3). The effect is negligible when we look at outcomes in Stage 0 (see Appendix B, FigureB.1).

Result 3. *Sellers choosing CS are closing more deals and making larger profits, compared to buyers, which are better off when they keep the RLI scheme. In particular sellers matched with RLI buyers are making significantly less profits, whereas buyers are better off when choosing the RLI scheme, but the effect is significant only when matched with CS seller.*

3.4.4. Social (In)Equality

The Acquiring-a-Company game is a positive sum game allowing for social equality in the form of $\frac{p-qv}{(1-q)v} = \frac{v-p}{(1-q)v}$. This is relevant for our study because we want to assess here whether contracts affect the social best achieved in different trading groups, characterized by different decisions over payment scheme.

In particular we focus on the difference between results achieved by each couple of sellers and buyers in each round of Stage 1: $|SS_{Seller} - SS_{Buyer}|$ as a measure of social inequality: It takes value equal to zero in case of equality between partners.

Figure 3.4: Social inequality by trader's payment method

		Buyer RLI	Buyer CS
Seller	RLI	0.347	-1.164
	CS	-0.894	Benchmark

Social Inequality, Stage 0

		Buyer RLI	Buyer CS
Seller	RLI	-2.196	-1-653
	CS	-2.000**	Benchmark

Social Inequality, Stage 1

Notes: Marginal effects from interactions between contracts choices compared with seller and buyer choosing CS. The differences between surplus share include only the accepted deals: the analysis is consistent when we analyze the surplus share including not accepted contracts. * p<0.1, ** p<0.05, *** p<0.01.

Matrices in Figure 3.4 show social inequality both in last rounds of Stage 0 (on the left) and in Stage 1(on the right). Each matrix represents the difference between surplus share, in particular, we set the trade between buyer and seller both choosing the CS, as the reference point. The other cells represent the other possible matches, and we check if there is any significant difference from the reference group.

In Stage 0 we don't find any significant difference across contracts. When we look at Stage 1, the social inequality is significantly lower when CS sellers meet RLI buyers compared to the benchmark solution, where both seller and buyers selected CS contracts (benchmark cell is the lower right cell of each matrix). The CS seller trading with CS buyer drives more social inequality even when we compare with an RLI seller and buyer or an RLI seller meeting a CS buyer, but it is not significant. This implies that the probability to close a fair contract is higher when the seller selecting the CS scheme (associated to higher probability to accept, as discussed in Table 3.4) meets an RLI buyer (lower left of the right matrix in Figure 3.4), offering lower prices and being more skeptical.

Result 4. *Social (in)equality is (highest) lowest when CS sellers meet CS buyers but significantly (higher)lower only when we compare to CS sellers meeting RLI buyers.*

3.5. Final Remarks

The modified Acquiring-a-Company game admits two roles where only the uninformed side is actually experiencing risk: Although a seller's profit is a function of the price proposed by buyers, she/he always has the possibility to ultimately refuse the deal. We find that buyers selecting the RLI scheme are 25% of our sample regardless of gender. Women experiencing risky choice through many rounds, even if losses seems likely, become more tolerant toward risk and choose significantly more often the RLI scheme compared to female sellers. This might have two different explanations: On the one hand, it is commonly accepted that women shy away from competition (Niederle and Vesterlund, 2007) but in our case, experienced female buyers kept the payment scheme, perceived as riskier, because they adapted their behavior to the game.

As Casari et al. (2007) suggest, in repeated common value auction, women experience more often winner's curse at first than male participants. Because of the "shock therapy" at the beginning of the game, where they experience high losses, they adapt their strategies to avoid future failures (women turn from more aggressive bids to bid lower) maybe because they might have an initial lack of experience in strategic interactions compared to males (see Di Cagno et al., 2015a for further discussion on gender differences in the early phase of our experiment). In this sense, women improving their strategic interaction with more trials are able to select payment design preferred, consistently with their attitude during the game more than what males do.

On the other hand, when we consider female sellers choices, women shy away from riskier design selecting the CS, even though the decision does not involve a true risk. This result could be related to observations of gambling studies: Here women are more risk-averse in the gain-domain frame, although in the loss-domain, results are not conclusive (see Harbaugh et al. 2002, Schubert et al. 1999 and Eckel and Grossman, 2008). This could explain why women seem to be more risk-averse as sellers compared to when they are buyers.

In general, the payment scheme selected by subjects seem to favor the female attitude: Sellers with CS incentive improve their outcomes. We cannot rule out that the improvement in results is related to the effect of more experience, but we think that CS triggers a different behavior from participants which accept more often price offers and moderate the aggressiveness which characterize the RLI scheme. Subjects that are less aggressive sort themselves to the CS scheme improving consistently their final result because they are more confident in this new design.

When we look at the combination of the trade result, conclusions are indicating that the incentive mechanism selected by subjects creates four different groups, where two of them are characterized by aligned interests and two have different ones. The game, based on partly conflicting interests between seller and buyer, seems to emphasize that social optimal occurs when a less aggressive seller meets with a buyer that is more willing to take the risk of being rejected. This work proposes new light on bargaining, introducing the role of suitable payment for the involved partners. It shows that switching to a safer incentive leads to better deals for sellers, while buyers, which switch to try to offset the risk, were actually worse off because they let sellers take advantage of their lack of aggressiveness.

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Appendix B

Tables

Figure B.1: Incentive scheme matching in Stage 0, Late Phase by payment scheme

		Buyer: RLI	Buyer: CS
Seller	RLI	(0.271 , 0.069*)	(0.265 , 0.004)
	CS	(0.100 , 0.089***)	Benchmark

Cheating/Rentability

		Buyer: RLI	Buyer: CS
Seller	RLI	(-0.215, -0.275*)	(0.184*, 0.188*)
	CS	(-0.220***, -0.226**)	Benchmark

Acceptance

		Buyer: RLI	Buyer: CS
Seller	RLI	(-0.215, -0.275*)	(0.184*, 0.188*)
	CS	(-0.220***, -0.226**)	Benchmark

SS accepted trades

		Buyer: RLI	Buyer: CS
Seller	RLI	(-0.173 , 0.099)	(-0.101, 0.179)
	CS	(-0.459 , 0.368)	Benchmark

SS

Notes: Coefficients from panel regressions (probit for acceptance dummy and xtreg for the other variables). We consider here the last 10 rounds of Stage 0. All results are considered as difference with seller and buyer choosing CS.
* p<0.1, ** p<0.05, *** p<0.01.

Table B.1: The role of payment scheme sorting on sellers and buyers variables

Seller				Buyer			
Cheating (%) $\frac{\hat{v}-v}{\hat{v}}$				Rentability (%) $\frac{\hat{v}-p}{\hat{v}}$			
	Sellers switching	Sellers not switching	P value		Buyers switching	Bueyrs not switching	P value
Late Phase Stage 0	0.073	0.314	0.097	Late Phase Stage 0	0.601	0.685	0.000
Stage 1	0.075	0.285	0.186	Stage 1	0.601	0.689	0.000
P-value	0.986	0.266		P-value	0.601	0.806	
Truthtelling (v_hat=v)							
	Sellers switching	Sellers not switching	P value				
Late Phase Stage 0	0.175	0.180	0.808				
Stage 1	0.157	0.212	0.006				
P-value	0.170	0.269					
SS for sellers				SS for buyers			
	Sellers switching	Sellers not switching	P value		Buyers switching	Bueyrs not switching	P value
Late Phase Stage 0	1.471	1.463	0.982	Late Phase Stage 0	-1.031	-0.752	0.382
Stage 1	1.499	1.044	0.104	Stage 1	-1.024	-0.464	0.021
P-value	0.895	0.149		P-value	0.975	0.279	
Earnings for sellers				Earnings for buyers			
	Sellers switching	Sellers not switching	P value		Buyers switching	Bueyrs not switching	P value
Late Phase Stage 0	7.343	8.364	0.143	Late Phase Stage 0	4.050	3.759	0.772
Stage 1	7.387	6.663	0.217	Stage 1	5.106	4.925	0.843
P-value	0.910	0.039		P-value	0.135	0.285	
Seller's Acceptance							
	Sellers switching	Sellers not switching	P value				
Late Phase Stage 0	0.503	0.549	0.126				
Stage 1	0.544	0.502	0.127				
P-value	0.020	0.202					

Notes: Test on average bargaining variables by payment choice comparing the Late Phase of Stage 0 with 12 rounds of Stage 1.

Table B.2: The role of payment scheme sorting on acceptance rate and surplus share in Stage 0

	Seller			Buyer		
	(1) Acceptance (Mean)	(2) SS accept[1] (Mean)	(3) SS accept[1] (SD)	(4) Acceptance (Mean)	(5) SS accept[1] (Mean)	(6) SS accept[1] (SD)
	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)
Cumulative Scheme*Male	0.01	0.32	0.98	0.02	0.68	-1.49
	(0.03)	(0.70)	(1.33)	(0.03)	(0.73)	(1.34)
RLI*Male	0.04	-0.21	-0.61	-0.01	0.68	-0.80
	(0.04)	(0.68)	(1.21)	(0.04)	(0.89)	(2.09)
RLI*Female	0.03	-0.13	0.44	-0.02	0.47	-1.28
	(0.06)	(0.67)	(1.45)	(0.04)	(0.72)	(1.36)
Average Cheating (%)	0.05**	0.65***	1.06**			
	(0.02)	(0.24)	(0.41)			
Average Rentability (%)				-0.84***	3.04**	-5.48*
				(0.20)	(1.50)	(2.89)
Risk	-0.00	-0.17	-0.32	0.00	-0.20	0.43
	(0.01)	(0.12)	(0.21)	(0.01)	(0.15)	(0.28)
Constant	0.51***	3.30***	4.99***	1.02***	-3.24***	6.76***
	(0.03)	(0.69)	(1.20)	(0.13)	(0.96)	(1.79)
Observations	188	188	185	188	182	174
R^2	0.08	0.03	0.03	0.45	0.05	0.05

Notes: Dependent and independent variables are based on individual average and standard deviation of the final 10 rounds of Stage 0. Risk measure goes from 0 (maximum risk averse) to 9 (maximum risk seeking). Seller's cheat (%) is measured as difference $\frac{\hat{v}-v}{\hat{v}}$. Buyer's rentability (%) measures the distance between price offer and message received $\frac{\hat{v}-p}{\hat{v}}$.

[1] Surplus share (SS) include only the accepted deals: the analysis is consistent when we analyze the surplus share including not accepted contracts. * p<0.1, ** p<0.05, *** p<0.01.

Chapter 4

A Hybrid Public Good Experiment Eliciting Multi-Dimensional Choice Data

with D. Di Cagno, W. Güth, L. Panaccione

Abstract

Similar to Fischbacher and Gächter (2010) we suggest an elicitation method for exploring the motivation of participants when contributing to a public good in the role of "leader" or "follower". In the Hybrid Public Good experiment each of two interacting contributors chooses an independent contribution level as well as three adjusted contribution levels when (s)he, as the only adjusting player, learns that the other's independent contribution is smaller, equal or larger than the own one. To approximate the border cases of simultaneous contributing as well as sequential contributions we systematically vary the probability that one player can adjust, based on such qualitative information, but maintain that no adaptation at all and adaptation by only one occurs with positive probability. Adaptation is framed in two ways, once by additively changing the own independent contribution and once by stating new contribution levels. Surprisingly, the framing effect becomes stronger with experience. Reacting to coinciding independent contributions implies impressive conformity in contributing. Reacting to higher, respectively lower independent contributions implies average upward, and, more strongly, downward adaptation.

Keywords: Public goods, experiments, voluntary contribution mechanism.
JEL: C91, C72, H41

4.1. Introduction

Inspired by Revealed Preference theory (Samuelson 1938, Varian 2006), the Revealed Motive approach of experimental economics [1] tries to infer motives like preference relations, aspiration levels, inclinations towards risk and ambiguity, other-regarding concerns, etc. purely from experimental choice data. In this paper, we do not question this but suggest an elicitation technique to assess more directly the motives when contributing in a public good and demonstrate its potential by running an experiment with different treatments in order to show how its data can be more informative.

Recently, other scholars have attempted a similar goal. Fischbacher and Gächter (2010), for example, study experimentally a normal form version of a sequential public good game with one randomly determined first mover and three followers. All four participants are asked to choose an independent contribution and, as followers, for a response strategy specifying their reaction to the leader's choice. Here, the strategic uncertainty of the three followers, who do not know the other followers' response strategies, renders the interpretation of response behavior ambiguous. To avoid this problem, we consider a two-player game ruling out strategic uncertainty when "following"but maintain the advantage of Fischbacher and Gächter (2010) to elicit from each participant both the choice as "leader"as well as "follower".

In our design, each participant chooses an independent contribution as well as adjusted contributions depending on how the other's independent contribution qualitatively compares with the own one (see, related to this, Keser and van Winden, 2000 and, less closely, Kurzban and Houser, 2005): adjustment conditions only on whether the other's independent contribution is higher than, lower than, or equal to the own one. Either none or only one contributor can adapt the independent contribution according to a commonly known probability p, i.e. the probability of one of them being allowed to adjust is the unique "within subject"treatment variable.

We consider three different probability levels: low $(p = 2.5\%)$, middle $(p = 33\%)$, and high $(p = 49\%)$. When the probability p of adjustment is low, it is very unlikely that either contributor can adjust, hence the situation is close to a standard public good game with free riding being dominant. As p increases, the nature of the game changes[2]: while the same prediction for optimal choices follows from once repeated elimination of dominant strategies[3], it becomes more likely that one will be able to adapt; in this case the game gets closer to a trust

[1] See Di Cagno et al. (2015) for a more fundamental discussion of the (dis-)advantages of the Revealed Motive approach in experimental research.

[2] Another interpretation of the nature of our game refers to the leadership in voluntary contribution games, see e.g. Croson et al. (2005) and Levati et al. (2007).

[3] Fischbacher and Gächter (2010) rely on this stronger rationality postulate and thus somewhat weaken the dilemma aspect of their experimental game.

game. Indeed, suppose that the probability of adjustment is $p = 49\%$: since the probability that none can adapt is only 2%, contributors essentially face a symmetric trust game with both players assuming the trustor and the trustee role with 49% probability each.[4] In this case, the independent contribution can be interpreted as a trustor investing in trustworthiness, while the adapted contributions reveal trustworthiness of a trustee. Altogether we thus vary the crucial parameter p rather systematically but maintain its generality, i.e. we only approximate but never directly analyse the border cases of truly simultaneous, $p = 0$, respectively sequential contributions, $p = 1/2$. The obvious advantage of this is that by varying only one numerical parameter, p, we can approach very different games.

Our two between subjects treatments differ in the way how contributions are adjusted. In the Pure Adjustment (thereafter PA) treatment, subjects state an independent contribution and then are asked what to add to or subtract from the independent contribution in order to determine the final contribution. To maintain the independent contribution, the adjustment can be set equal to zero.

This way of asking for adjustment of (independent) contributions may trigger quite different reactions such as:

- opposition/inertia/resistance to change ("I want to maintain my independent contribution!");
- an obligation to change when new information is provided;
- a desire for flexibility allowing for both, positive and negative adjustments.

To control for this (demand) effect, we consider the Contribution Choice (thereafter CC) treatment: contributors choose an independent contribution and then the contributions by which they react to qualitative information. One basic reason for framing the contribution adjustments differently is that framing effects are often shown to influence behavior in one-off decision tasks and maybe used for nudging (see Thaler and Sunstein, 2008) at least inexperienced decision makers. However it is largely unexplored whether framing effects persist when decision makers become quite experienced. Our hypotheses that learning weakens the learning effect could, however, not be confirmed. Another reason is more subtle: having to adjust one's independent contribution seems slightly more cumbersome than deciding anew. Thus the two frames may not question the choice set but possibly their cognitive demands.

[4]Similarly to Berg et al. (1995) and the experiment of Fischbacher and Gächter (2010) trustor and trustee can choose among different contribution levels. However, trustees can condition their choice only on qualitative information.In this sense our setup resembles sequential Prisoners' Dilemma (PD) experiments with binary choice sets.

Rather than running our hybrid public game (HPG hereafter) just once, we wanted to explore how experience affects play without endangering the one-off character of the game. We thus let participants play recursively but with new randomly selected partners.

Given our experimental design, we are able to test how contributions react to the different frames and probabilities of adjustment across rounds of playing the HPG. To anticipate our findings: average independent and adapted contributions are generally higher in treatment CC and increasing with the probability of adaptation. The greater probability of adjustment sustains more voluntary cooperation across plays of HGP with this effect being stronger in treatment CC. The finding is surprising since we expect experience to render participants more immune to framing. In our view it is striking how participants react to the changing nature of the game due to different probabilities of adaptation.

Concerning whether and how contributions are adapted we can state the following: coinciding independent contributions are mostly maintained and one reacts to a higher independent contribution by the other, on average, by an increase but to a lower one by a quite stronger decrease. The first result confirms earlier findings, Croson et al. (2005) report that participants like to match the contributions of others which could be due to conformity seeking, as claimed by Carpenter (2004). However, Carpenter refers to conformity seeking as "copying the most relevant behavior in a population". In our set up with limited feedback information on others' behavior, conformity seeking can be interpreted in two ways: either as coincidence of independent contributions so that no adjustment establishes conformity or as tendencies to adjust in the direction of the other's independent contribution. In our context, as well as for Fischbacher and Gächter (2010), such conformity seeking could also be implied by let-down aversion or conditional cooperation.[5]

Our findings also suggest that conditional cooperation is affected by the salience of conditioning. In particular, higher probability of adaptation implies higher and more persistent cooperation, whereas for low probability of adaptation the dynamics of contributions are consistent with those of standard public good experiments, even with those using random strangers matching (see e.g. Fehr and Gächter, 2000). Similar to Fischbacher et al. (2001), we also consider different types of contributors, namely *conditional cooperators* and *exploiters*, or free-riders.[6] A conditional cooperator increases (decreases) her independent contribution if the other's independent contribution is larger (smaller) and does not adapt if it is equal to the own one. An exploiter, or free-rider, never contributes, either independently or when adjusting. Quite naturally, we expect a monotonic reaction to p and more free-riding behavior for small p due to the weaker trust game character.

[5]This might explain why Carpenter (2004) wants to define conformity seeking more distinctively.

[6]Even though we use the same, natural, terminology for types, there are differences rendering our findings not directly comparable to those of Fischbacher et al. (2001).

Finally, although we first of all want to propagate and demonstrate one elicitation method to collect more informative experimental choice data in order to more easily and profoundly assess their motives, one may ask whether our experimental scenario has any analogue in the field. Although many collective action tasks involve large numbers of interacting parties, the use of only two in a lab study seems less problematic as the - in the field quite frequent - role distinction of “leading”and “following”. Thus what is more crucial is that one can adapt only in the light of qualitative rather than quantitative information about “leader”choices by different participants. In our view, this could be due to a third party which intentionally provides only such information, for example, in order to inspire equal contributions what could weaken post-decisional regret. Actually allowing for adaptation, even more than just once as in our design, could be a rather useful and innovative way to avoid intragroup conflicts in collective action tasks. Otherwise we admit that it is hard to justify exact information when independent contributions are equal but rather qualitative information only when not. If at all information is vague, it would be more realistic to distinguish “pretty equal”and “considerably larger, respectively smaller”. To justify field relevance one then would have to find field situations with such vague comparison. Rather than simply referring to “noise”we admit this to be rare in the field.

The paper is organized as follows: Section 4.2 describes the experimental protocol. The descriptive and statistical data analysis is presented in section 4.3. We analyze the independent contributions and adjustments both from a static point of view across conditions as well as dynamically. Section 4.4 concludes.

4.2. Experimental protocol

In each experimental session, participants choose their contributions for 45 successive rounds, grouped in three phases of 15 rounds. Phases differ in the probability p of adjustment, which is either low ($p = 2.5\%$), middle ($p = 33.3\%$), or high ($p = 49\%$). Since each participant can adjust with probability p, the probability that none of them will adjust is equal to $1-2p$. The probability level in each phase is commonly known and occurs in increasing or decreasing sequences for a given session i.e. the increasing, respectively decreasing probability sequence is implemented “between subjects”. In all rounds of a given phase, subjects are randomly matched with a different anonymous partner. Participant were informed about this random rematching to weaken repeated game effects.

In each round, participants are endowed with 9 tokens (1 token = 0.5€) and state their independent contribution. Without knowing the other’s independent contribution, participants then adjust their independent contribution conditional on whether the other’s independent contribution is lower than, equal to, or greater than the own one. After the independent and the adjustment (for the three cases) choices, the computer $-$ according to p $-$ randomly selects whether one and, if so, which participant can adjust her independent contribution.

The payoffs are computed at the end of each round according to the standard public good linear payoff function:[7]

$$\pi_i = 9 - c_i + 0.8(c_i + c_j) \tag{4.1}$$

Here c_i (c_j) is the actual contribution of subject i (j), which is equal to the independent (adjusted) contribution if subject i does not (does) adapt. In the experiment all choices are restricted to integers. An MPCR of .8 may seem high but implies the same efficiency gains of individual contributions as in the standard 4-person games with one MPCR of .4; the 2-person interaction is imposed, as mentioned above, to exclude strategic uncertainty when following. Finally, in spite of most other studies, the (random) stranger matching protocol is used since we are interested in the motives guiding the contribution choices of "leader" and "follower" and not in those behind repeated play effects as typically studied by supergame experiments.

4.2.1. Treatments

The experiment distinguishes two framing treatments, differing in the way how contributions are adjusted:

- in the Pure Adjustment treatment (TR PA), each subject chooses her independent contribution c_i^0 $-$ with $0 \leqslant c_i^0 \leqslant 9$ $-$ and subsequently states the amount $\Delta_i^?$ $-$ with $? \in \{=,+,-\}$ and $-c_i^0 \leqslant \Delta_i^? \leqslant 9 - c_i^0$ $-$ to be added to (if positive) or subtracted from (if negative) the independent contribution in case the other's independent contribution is higher than $(+)$, lower than $(-)$ or equal to $(=)$ her own one.

- in the Contribution Choice treatment (TR CC), each subject chooses her independent contribution c_i^0 $-$ with $0 \leqslant c_i^0 \leqslant 9$ $-$ and subsequently states the adjusted contributions $c_i^?$, with $? \in \{+,-,=\}$ and $0 \leqslant c_i^? \leqslant 9$ in case the other's independent contribution is higher than $(+)$, lower than $(-)$ or equal to $(=)$ her own one.

In TR PA the actual contribution of subject i is $c_i^0 + \Delta_i^?$ if she is randomly selected to adjust, and c_i^0 otherwise. Similarly, in TR CC the actual contribution of subject i is $c_i^?$ if she is randomly selected to adjust, and c_i^0 otherwise.

[7]We use the term "project" in the experiment. This type of payoff function has been disseminated by Marvell and Ames (1979, 1980, 1981).

Observe that in both "between subjects" treatments the same final contributions are available since in TR PA any $c_i^?$ with $0 \leqslant c_i^? \leqslant 9$ can be realized via appropriate adaptations $\Delta_i^?$ with $-c_i^0 \leqslant \Delta_i^? \leqslant 9 - c_i^0$.

4.2.2. Sequences

As "within subjects treatments" the probability levels could differ across phases in the following way:

- in sequence A probability levels are increasing, hence $p = 2.5\%$ in phase 1, $p = 33.3\%$ in phase 2, and $p = 49\%$ in phase 3;
- in sequence B probability levels are decreasing, hence $p = 49\%$ in phase 1, $p = 33.3\%$ in phase 2, and $p = 2.5\%$ in phase 3.

The structure of the experiment is summarized in Table 4.1.

4.2.3. Feed-back information and payment

At the end of each round, participants are reminded of

- their independent contribution;
- their potential adaptations (either $\Delta_i^?$ in TR PA or $c_i^?$ in TR CC);
- the probability p of adapting in the current phase.

Moreover, they are informed about

- the random event (based on probability p) allowing possible adjustment of at most one independent contribution;
- their own payoff in the current round computed according to (1).

When one of the paired participants can adjust the independent contribution, the one who could adapt is told only whether the other's contribution was greater than, lower than or equal to the own one, while the other is only told that the partner could adapt. In addition, the own final payoff is communicated to either participant. When participants could not adapt, they are told that no adaptation has occurred and their own final payoff.

At the end of the session the computer randomly selects one round of each phase for payment. Each individual earns the sum of payoffs, corresponding to these (three) selected rounds. In addition, each participant received a show-up fee of €2.50. Subjects were paid in cash privately at the end of each session.

We ran 8 sessions at the laboratory of the Max Planck Institute in Jena. A total of 252 students (7 sessions of 32 participants plus 1 session of 28) were recruited among the undergraduate population of Jena University using Orsee (Greiner, 2004).

Subjects were provided with a hard copy of the instructions, which were read aloud by the experimental proctor (for an English translation of the instructions see Appendix). The experiment was fully computerized using z-Tree (Fischbacher, 2007).

Overall, subjects spent about 90 minutes in the laboratory, and earned on average €20.50, with slightly higher average payment in treatment CC, both sequences. Minimum earnings are higher in treatment PA, while maximum payments are higher in sequence B, in both treatments.

Table 4.1: The experimental protocol

Treatments	Pure Adjustment (PA): Adjustment via $\Delta_i^?$		Conditional Contribution (CC): Adjustment via $c_i^?$	
	Sequence A	Sequence B	Sequence A	Sequence B
Sessions	2	2	2	2
Phase 1	2.5%	49%	2.5%	49%
Phase 2	33.3%	33.3%	33.3%	33.3%
Phase 3	49%	2.5%	49%	2.5%
Rounds per phase	15	15	15	15
Subjects per session	32	32	32	32
Subjects	60(†)	64	64	64
Average earning (€)	20	20.19	20.36	20.31
Min. earning (€)	16.5	16.5	14.5	14.5
Max. earning (€)	23.5	26.5	24.5	25.5

† In one session of Treatment PA, sequence A: 28 subjects instead 32.
Participation fee: €2.5.

4.3. Hypotheses

We test several hypotheses concerning framing, sequence of probabilities, dynamics of contributing, and types of contributors.

- for *framing*, we test whether contributions differ due to the mode of adaptation. Although treatment PA and CC feature the same choice sets, we predict a stronger persistence of independent contributions when these are Δ-adapted as in treatment PA than when they are c-adapted as in treatment CC;

- for *sequence of probabilities*, we expect no persistent sequence effects but strong monotonic reactions of contributions to p with more free riding for smaller p due to the weaker trust game character;
- regarding *the contribution dynamics*, framing effects should disappear with experience and final contributions decrease across the 15 rounds within phases;
- for *types of contributors*, we expect, consistently with previous findings, more conditional contributors than freeriders.

4.3.1. Treatment and probability effects

Our first result confirms a significant framing effect: the way in which subjects state their adapted contributions (either via Δ-adaptation or via c-adaptation) affects the level of independent and adjusted contributions, even though choice sets are the same in both treatments.

Results 1: *Average independent contributions are significantly higher in treatment CC than in treatment PA. Average adapted contributions are generally higher in treatment CC than in treatment PA (see Table 4.2).*

Table 4.2: Contributions by sequences and treatments†

	Sequence A and Sequence B							
	c_i^0	Δ_i^+	$\Delta_i^=$	Δ_i^-	c_i^+	$c_i^=$	c_i^-	c_i
CC	**4.268**	**0.270**	-0.230	-1.594	4.537	**4.038**	**2.674**	**4.020**
PA	**4.035**	**0.502**	-0.205	-1.518	4.537	**3.830**	**2.517**	**3.762**
	0.00	0.00	0.35	0.09	0.99	0.00	0.00	0.00
	Sequence A							
	c_i^0	Δ_i^+	$\Delta_i^=$	Δ_i^-	c_i^+	$c_i^=$	c_i^-	c_i
CC	**4.277**	**0.275**	-0.173	-1.753	4.552	**4.104**	2.524	**3.997**
PA	**4.049**	**0.547**	-0.129	-1.626	4.596	**3.920**	2.423	**3.761**
	0.01	0.00	0.21	0.06	0.62	0.03	0.16	0.00
	Sequence B							
	c_i^0	Δ_i^+	$\Delta_i^=$	Δ_i^-	c_i^+	$c_i^=$	c_i^-	c_i
CC	**4.258**	**0.264**	-0.287	-1.435	4.522	**3.971**	**2.823**	**4.044**
PA	**4.022**	**0.459**	-0.276	-1.417	4.481	**3.746**	**2.605**	**3.763**
	0.00	0.00	0.79	0.76	0.64	0.01	0.00	0.00

†Test for difference in contribution means are reported in in the P-values line
TR CC is based on 2880 observations and TR PA counts 2770 observations.

Average independent contributions are significantly higher in treatment CC than in treatment PA when pooling the data of sequence A and sequence B as well as when considering them separately (see Table 4.2, column 1). The latter reveals that the order of probabilities (increasing vs. decreasing) does not affect average independent contributions.

Regarding adjusted contributions, the pattern is similar: in treatment CC average $c_i^=$ and c_i^- are significantly higher than in treatment PA, while c_i^+ is not statistically different across treatments (see Table 4.2, columns 5-7). This latter result is due to the fact that Δ_i^+ is significantly higher in treatment PA than in treatment CC and therefore offsets the lower level of the independent contribution observed in treatment PA.

Similarly to the independent contributions, the pattern of the adjusted contributions is independent of the sequence of probabilities. Evidently, anticipating whether and how one revises triggers more independent and final voluntary cooperation in treatment CC than in treatment PA. This, of course, questions all purely outcome-based social preferences and suggest that the mode of adaptation triggers partly specific reasons when and how to adapt, for example, inertia or resistance to change or an obligation to react to new information which could inspire c_i^0-choices allowing for flexibility in adaptation, e.g. via $c_i^0 = 4$ or 5.

According to the last column of Table 4.2 the actual final contribution is on average greater in treatment CC irrespective of sequence, a result consistent with those for independent and adjusted contributions.

Table 4.3 focuses on probability effects and confirms a significant and monotonic probability effect.

Results 2: *Average independent and adapted contributions are generally increasing with the probability of adjustment (see Table 4.3)*

When considering both (between subjects) treatments together, we find that independent as well as adapted contributions c_i^+ and $c_i^=$ are higher in phases with higher probability. In case of c_i^- the value remains constant across probabilities regardless of the frame, i.e. mode of adaptation. When considering each treatment separately, independent and adjusted contributions steadily increase from low to high probability (see Table 4.3, column 1 and columns 5-7). Moreover for $\Delta-$adjustments, Table 4.3 confirms that average Δ_i^+ is decreasing, while average $\Delta_i^=$ and Δ_i^- are increasing in absolute value with probability p (see Table 4.3, columns 2-4).

These results justify our arguments, put forward in the introduction: when the adjustment probability p is low the game perceived as a usual public good game while, when p is high, it is seen as a trust game. Subjects seem to realize this and modify their contributions accordingly. In particular, for $p = 49\%$, independent contributions are higher since as trustor one likes to invest more in trustworthiness. However, since for $p = 49\%$ average Δ_i^+ is lowest, using the jargon of trust games, we can say that rewarding such trusting is rather limited. Furthermore, since Δ_i^- is highest in absolute value, there is a strong negative reaction when the other's investment in trust is lower than the own one which suggests self-serving concerns: one reacts less strongly when adjustment is costly than when it is favorable.

To further investigate limited rewarding of trust and self-serving concerns, we introduce a measure of expected $\Delta-$adjustments of independent contribution which is defined as follows:[8]

$$d_i^j(c_j^0) = p\Delta_i^?(c_j^0) \text{ where } \Delta_i^?(c_0^j) = \begin{cases} \Delta_i^- & \text{if } c_i^0 > c_j^0 \\ \Delta_i^= & \text{if } c_i^0 = c_j^0 \\ \Delta_i^+ & \text{if } c_i^0 < c_j^0 \end{cases} \tag{4.2}$$

We consider the value of d_i^j using actual data and data from simulated pairs of subjects, obtained by pairing each subject with every other subject in the same round of the same sequence in a given treatment.[9,10]

Note that simulated data differ from actual ones since they neglect individual feedback effects stemming from the information received by subjects e.g. about their current payoff after each round. Therefore, this simulation provides a robustness check of our sample results.

Tables 4.4 and 4.5 report average actual and simulated d_i^j and additionally average values of final, i.e. after adjustment, total contribution both for actual and simulated data. Table 4.4 relates expected adjustments and public good provision to frame: actual expected adjustments do not vary across treatments, hence they are not related to frame, while the opposite is true for final public good provision, which is significantly affected by frame and significantly higher in treatment CC. Simulated expected adjustment is larger (in absolute value) in treatment CC, irrespective of sequences, and the same holds for public good provision.[11]

Table 4.5 relates expected adjustments and public good provision to probability level p. Irrespective of treatment, average d_i^j from actual data is negative and, in absolute value, increasing more than proportionally with adjustment probability. These results confirm our intuition that, when the game is closer to a trust game, rewarding trust is, on average, more dominated by self-serving concerns, whence the negative expected adjustment.

In the larger set of simulated data we find a similar pattern for d_i^j which, however, seems more pronounced in the CC treatment. This suggests that the trust game interpretation may

[8]Observe that $d_i^j(c_j^0)$ can be equivalently expressed as $c_i^0 - [(1-p)c_i^0 + p(c_i^?(c_j^0))]$, that is as the difference between the independent contribution and the expected adjusted contributions.

[9]For example, each individual in, say, the third round of phase 3 in the session with increasing probability under treatment PA is matched with every other subject in the third round of phase 3 in the session with increasing probability under treatment PA.

[10]We were able to implement this procedure thanks to the strategy (vector) method adopted in the experiment; it results in 703,620 observations regarding independent contributions and adapted contributions.

[11]We do not report simulated data on independent contributions and adjustments: their averages are the same as those in Tables 4.2 and 4.3. Since the same observations are used repeatedly, standard errors decrease drastically and the frame and probability effects appear as highly significant.

Table 4.3: Contributions by treatments and probabilities†

	Treatment PA and CC							
	c_i^0	Δ_i^+	$\Delta_i^=$	Δ_i^-	c_i^+	$c_i^=$	c_i^-	c_i
$p = 2.5\%$	**3.723**	**0.517**	**-0.051**	**-1.178**	**4.240**	**3.672**	2.545	**3.702**
	0.00	0.00	0.00	0.00	0.00	0.00	0.43	0.00
$p = 33.3\%$	**4.248**	**0.394**	**-0.268**	**-1.652**	**4.642**	**3.980**	2.596	**3.702**
	0.00	0.00	0.06	0.00	0.25	0.02	0.41	0.77
$p = 49\%$	**4.489**	**0.241**	-0.335	**-1.840**	4.730	**4.154**	2.649	3.979
	Treatment PA							
	c_i^0	Δ_i^+	$\Delta_i^=$	Δ_i^-	c_i^+	$c_i^=$	c_i^-	c_i
$p = 2.5\%$	**3.549**	0.590	**-0.056**	**-1.103**	**4.139**	**3.492**	2.446	**3.524**
	0.00	0.61	0.00	0.00	0.00	0.00	0.73	0.00
$p = 33.3\%$	**4.092**	**0.560**	**-0.287**	**-1.615**	**4.652**	**3.805**	2.477	**3.831**
	0.00	0.00	0.77	0.00	0.11	0.00	0.08	0.28
$p = 49\%$	**4.465**	**0.355**	-0.272	**-1.837**	4.820	**4.192**	2.627	3.932
	Treatment CC							
	c_i^0	Δ_i^+	$\Delta_i^=$	Δ_i^-	c_i^+	$c_i^=$	c_i^-	c_i
$p = 2.5\%$	**3.891**	**0.446**	**-0.046**	**-1.251**	**4.338**	**3.845**	2.640	**3.875**
	0.00	0.00	0.00	0.00	0.01	0.00	0.46	0.01
$p = 33.3\%$	**4.399**	**0.233**	**-0.249**	**-1.688**	**4.632**	**4.150**	2.711	**4.162**
	0.28	0.15	0.00	0.07	0.93	0.76	0.66	0.18
$p = 49\%$	4.513	0.130	**-0.395**	-1.843	4.642	4.117	2.669	4.023

†Test for difference in contribution means are reported in in the P-values line (between $p = 2.5\%$ vs. 33.3% and $p = 33.3\%$ vs. 49%).

Table 4.4: d_i^j and public good provision (PGP) by sequences and treatments†

	Sequence A and Sequence B			
Data	Actual	Simulated	Actual	Simulated
	d_i^j		PGP*	
CC	-0.269	**-0.270**	**8.040**	**8.010**
PA	-0.256	**-0.250**	**7.525**	**7.561**
	0.48	0.00	0.00	0.00
	Sequence A			
Data	Actual	Simulated	Actual	Simulated
	d_i^j		PGP*	
CC	-0.315	**-0.312**	**7.993**	**7.940**
PA	-0.294	**-0.287**	**7.523**	**7.507**
	0.47	0.00	0.00	0.00
	Sequence B			
Data	Actual	Simulated	Actual	Simulated
	d_i^j		PGP*	
CC	-0.223	**-0.228**	**8.088**	**8.080**
PA	-0.220	**-0.217**	**7.526**	**7.609**
	0.91	0.00	0.00	0.00

† Test for difference in contribution means are reported in in the P-values line for frame effect.
* Public good provision is final contribution to public good: PGP= $c_i + c_j$

be more salient for treatment CC featuring less rewarding of trust and more pronounced self-serving concerns.

Finally, public good provision has a similar pattern in simulated and actual data: it increases with probability p in treatment PA, while it decreases between $p = 33.3\%$ and $p = 49\%$ for treatment CC. As probability p increases, the independent contribution becomes larger whereas the magnitude of the adjustments Δ decreases. The final contributions reflect this double effect.

Table 4.5: d_i^j and total public good provision (PGP) by treatments and probabilities†

	Treatment PA and CC			
Data	Actual	Simulated	Actual	Simulated
	d_i^j		PGP*	
$p = 2.5\%$	**-0.014**	**-0.013**	7.405	**7.406**
	0.00	0.00	0.00	0.00
$p = 33.3\%$	**-0.272**	**-0.270**	**7.998**	**7.994**
	0.00	0.00	0.70	0.26
$p = 49\%$	**-0.501**	**-0.497**	7.957	7.978
	Treatment PA			
Data	Actual	Simulated	Actual	Simulated
	d_i^j		PGP*	
$p = 2.5\%$	**-0.012**	**-0.011**	**7.048**	**7.044**
	0.00	0.00	0.00	0.00
$p = 33.3\%$	**-0.263**	**-0.249**	**7.661**	**7.704**
	0.00	0.00	0.16	0.00
$p = 49\%$	**-0.494**	**-0.489**	7.865	**7.936**
	Treatment CC			
Data	Actual	Simulated	Actual	Simulated
	d_i^j		PGP*	
$p = 2.5\%$	**-0.017**	**-0.016**	**7.750**	**7.746**
	0.00	0.00	0.00	0.00
$p = 33.3\%$	**-0.281**	**-0.290**	**8.324**	**8.266**
	0.00	0.00	0.08	0.00
$p = 49\%$	**-0.509**	**-0.505**	8.047	**8.019**

† Test for difference in contribution means are reported in in the P-values line (2.5% vs. 33.3%; 33.3% vs. 49%).
* Total public good provision: PGP= $c_i + c_j$

4.3.2. Dynamics of contributions

In this section we analyze the trend of independent and adapted contributions over the 15 rounds of each phase. Recall that, due to the random stranger matching protocol, there are no reputation nor reciprocity effects across rounds. Therefore, the evolution of choices reveals mainly how individual intrinsic motivation to cooperate is affected by past play.

Figure 4.1 reports the dynamics of independent and adjusted contributions for each probability level without distinguishing between treatment PA and CC (as we do in Figure 4.2). The dynamics of contributions react significantly to the probability of adaptation since, when p is low, subjects tend to behave less cooperatively across rounds. With the help of Table 4.6 (columns 1-3), we can confirm that for $p = 2.5\%$ and $p = 33.3\%$ independent contributions c_i^0 steadily decline across rounds, while they remain constant when $p = 49\%$. The first finding is consistent with standard results on public good games, also – however weaker – for "random strangers", while the second is consistent with experimental findings on repeated trust game.[12] Conditional contributions(see Figure 4.1) for $p = 2.5\%$ and $p = 33.3\%$ steadily decline across rounds irrespective of the qualitative information regarding the other's contribution. When $p = 49\%$, the adapted contributions c_i^+ are essentially constant, while the contributions $c_i^=$ and c_i^- are slightly declining. These results are confirmed by Table 4.6 and are summarized in the following

Result 3: *Greater probability p sustains more voluntary cooperation, independently of the qualitative information regarding other's independent contribution.*

Figure 4.2 shows that the decline in independent contributions is more pronounced in treatment PA than in treatment CC when $p = 2.5\%$, while the dynamics of independent contributions are similar for the two treatments when $p = 33.3\%$. When $p = 49\%$, independent contributions slightly decline in treatment PA, while they remain essentially constant in treatment CC. Adapted contributions follows a similar trend: the decline, when significant, is more pronounced in treatment PA than in treatment CC, with the exception of c_i^-. This latter feature confirms the observation, put forward in the analysis of the expected adjustment, that treatment CC reinforces the trust game nature of high adaptation probability, while treatment PA reinforces the public good game nature of low adaptation probability. These results are confirmed by the data in Table 4.6 (columns 4-6 and columns 7-9) and summarized in the following

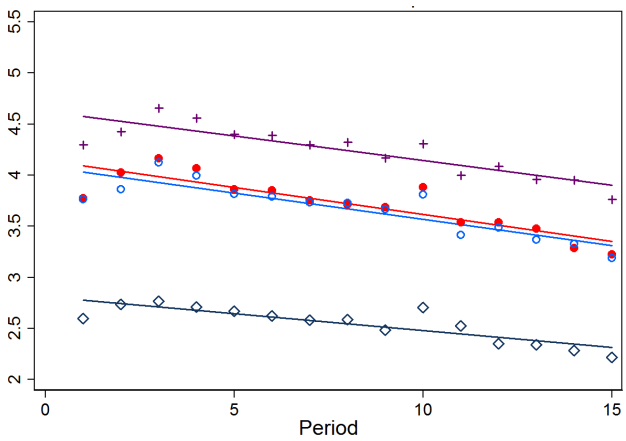
Result 4: *When the probability of adaptation is low, the decline in voluntary cooperation is more pronounced in treatment PA; when the probability of adaptation is high, the persistence of voluntary cooperation is higher in treatment CC.*

Figure 4.1 also reveals that adaptations Δ_i^+ are (positive and) higher when $p = 2.5\%$ than when $p = 49\%$, and higher in treatment PA than in treatment CC. Furthermore, adaptations Δ_i^- are (negative and) higher in absolute value when $p = 49\%$ than when $p = 2.5\%$. Overall, these results suggest that, when $p = 49\%$, reward of trust remains limited across rounds and that self-serving concerns are persistent.

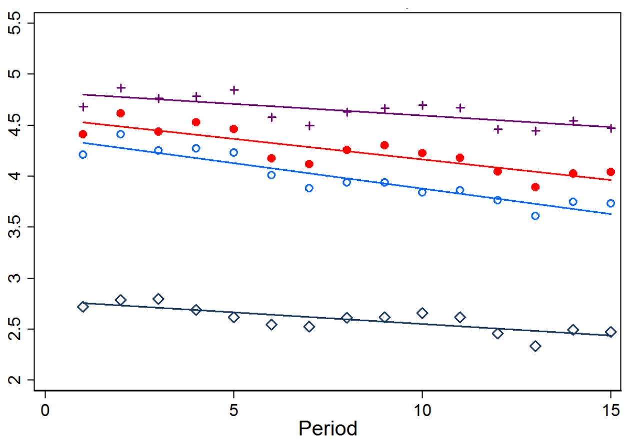
Table 4.7 analyzes the contribution dynamics as influenced by lagged contributions and lagged adjustments. The baseline specification is:

$$c_{it}^0 = \alpha Round_{it} + \beta_1 c_{it-1}^0 + \beta_2 (c_{it-1}^0)^2 + \beta_3 c_{-it-1}^0 + \gamma PO_{it-1} + \rho_1 \Delta_{i,t-1}^+ + \rho_2 \Delta_{i,t-1}^= + \rho_3 \Delta_{i,t-1}^- + \theta_i + v_{it}$$

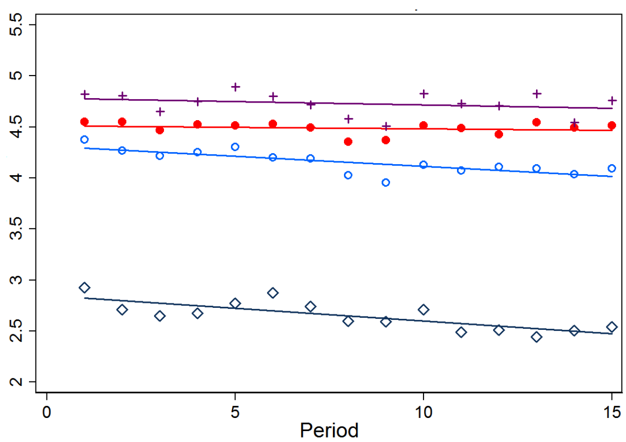
[12] See Ledyard (1995) for a survey on public good games. On trust games, see Berg et al. (1995) and Glaeser et al. (2000).



(a) Adjustment Probability $p = 2.5\%$



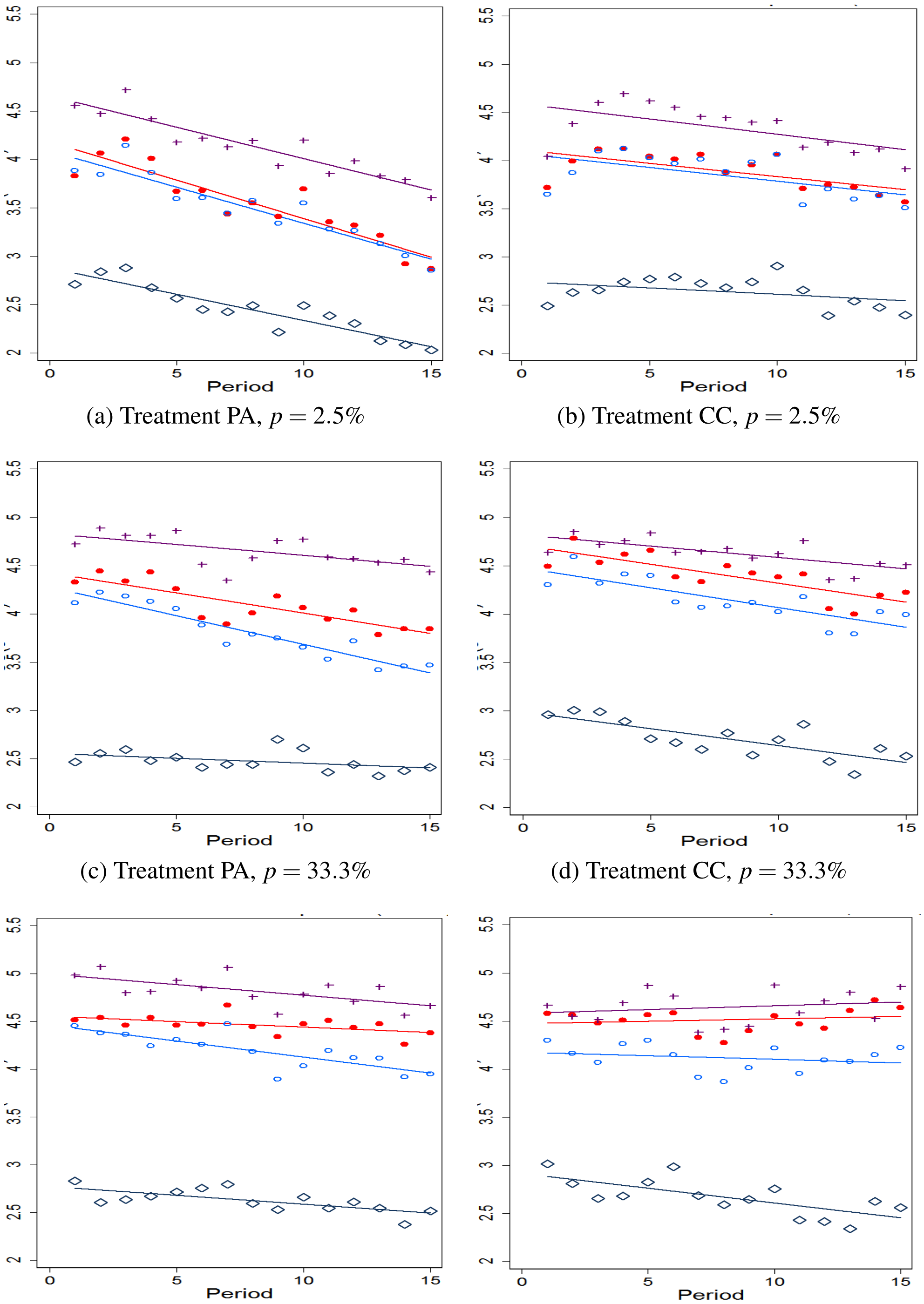
(b) Adjustment Probability $p = 33.3\%$



(c) Adjustment Probability $p = 49\%$

Notes: Average values in each round. $\bullet / c_i^0 \quad + / c_i^+ \quad \circ / c_i^= \quad \diamond / c_i^-$

Figure 4.1: Independent and adjusted contributions



(a) Treatment PA, $p = 2.5\%$

(b) Treatment CC, $p = 2.5\%$

(c) Treatment PA, $p = 33.3\%$

(d) Treatment CC, $p = 33.3\%$

(e) Treatment PA, $p = 49\%$

(f) Treatment CC, $p = 49\%$

Notes: Average values in each round. $\bullet / c_i^0 \quad + / c_i^+ \quad \circ / c_i^= \quad \diamond / c_i^-$

Figure 4.2: Independent and adjusted contributions by treatment

where the dependent variable is the independent contribution c^0_{it}. The explanatory variables are lagged contribution c^0_{it-1}, squared lagged contribution $(c^0_{it-1})^2$ to control for possible non-linear correlations, lagged payoffs, and lagged adjustments $(\Delta^+_{i,t-1}, \Delta^=_{i,t-1}, \Delta^-_{i,t-1})$. The estimation method is OLS with robust errors clustered on individuals.[13]

Results in Table 4.7 confirm that the lagged independent contribution has a significant positive effect for any frame and any probability level, while the squared lagged contribution does not significantly affect the independent contribution. The effect of lagged adjustment is mostly significant for $\Delta^+_{i,t-1}$ and $\Delta^-_{i,t-1}$. This observation suggests inertia in stating independent contributions.

Result 5: *Independent contributions are positively and significantly affected by the previous contributions.*

In closing this section, let us consider the trust game character in the experimental setup of Fischbacher and Gächter (2010). Differently from our experiment, they exclude that independent contributions alone determine the final payoff what occurs in our setup with probability $1-2p$ varying from only 2% (when $p = 49\%$) to 95% (when $p = 2.5\%$). Therefore, the independent choices in their experiment cannot be interpreted as contributions without conditioning as in the usual public good game. Each independent contribution corresponds to the trustor's choice in one of four equally likely trust games (in normal form) where each trustor confronts the three other players as trustees.

If applied to our design with two players, the protocol of Fischbacher and Gächter (2010) would have implied a probability of adjustment $p = 50\%$, a borderline case due to $1-2p = 0\%$ which we approximated via $p = 49\%$. Hence, when comparing our findings with those of Fischbacher and Gächter (2010), e.g. on the dynamics of voluntary cooperation for repeated play by random strangers, one should concentrate on $p = 49\%$. For this case, we observe a rather high and persistent level of cooperation which, in the setup of Fischbacher and Gächter (2010) might be endangered by free-riding attempts of the three trustees −a phenomenon which we excluded by selecting a two-player game. This, in turn, may trigger a decline of voluntary cooperation for large p due to the strategic uncertainty when "following" (Fischbacher and Gächter (2010) did not explore experience effects) .

[13]Additionally, we estimated all specifications of Table 4.7 with random and fixed effects (the latter is preferred over random effects checking at the Hausman test), using Tobit with robust standard errors and finally the Arellano-Bond linear dynamic panel-data estimation method using Generalized Method of Moments (GMM) with robust standard errors. However, some specifications still have a significant second order correlation ($p(ar1) < 0.05$ and $p(ar2) < 0.05$), implying that their estimates are inconsistent. Generally all specifications are consistent with the results of this section.

Table 4.6: Contributions and adjusted contributions through rounds[†]

Treatment	PA and CC			PA			CC		
Probability	2.5%	33.3%	49%	2.5%	33.3%	49%	2.5%	33.3%	49%
					c_i^0				
	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)
Round	-0.05***	-0.04***	-0.00	-0.08***	-0.04***	-0.01	-0.03*	-0.04***	0.01
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
Constant	4.15***	4.57***	4.51***	4.18***	4.43***	4.55***	4.11***	4.71***	4.47***
	(0.11)	(0.10)	(0.10)	(0.25)	(0.24)	(0.22)	(0.27)	(0.28)	(0.26)
Observations	3780	3780	3780	1860	1860	1860	1920	1920	1920
R^2	0.01	0.00	0.00	0.01	0.00	0.00	0.00	0.00	0.00
F	20.28	12.82	0.07	27.47	9.97	0.55	3.25	11.97	0.29
					c_i^+				
	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)
Round	-0.05***	-0.02*	-0.01	-0.06***	-0.02	-0.02	-0.03*	-0.02	0.01
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Constant	4.62***	4.83***	4.78***	4.66***	4.83***	5.00***	4.59***	4.82***	4.58***
	(0.11)	(0.11)	(0.11)	(0.16)	(0.16)	(0.16)	(0.16)	(0.16)	(0.16)
Observations	3780	3780	3780	1860	1860	1860	1920	1920	1920
R^2	0.00	0.00	0.00	0.01	0.00	0.00	0.00	0.00	0.00
F	14.87	3.44	0.30	14.19	1.69	1.58	3.13	1.75	0.19
					$c_i^=$				
	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)
Round	-0.05***	-0.05***	-0.02*	-0.07***	-0.06***	-0.03**	-0.03	-0.04**	-0.01
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Constant	4.08***	4.38***	4.32***	4.09***	4.28***	4.46***	4.07***	4.48***	4.17***
	(0.11)	(0.11)	(0.11)	(0.15)	(0.15)	(0.15)	(0.16)	(0.16)	(0.16)
Observations	3780	3780	3780	1860	1860	1860	1920	1920	1920
R^2	0.00	0.00	0.00	0.01	0.01	0.00	0.00	0.00	0.00
F	18.03	17.75	2.81	20.81	13.41	3.97	2.61	5.64	0.18
					c_i^-				
	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)
Round	-0.03***	-0.02**	-0.02**	-0.05***	-0.01	-0.02	-0.01	-0.04**	-0.03**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Constant	2.81***	2.78***	2.85***	2.88***	2.56***	2.78***	2.75***	2.99***	2.92***
	(0.10)	(0.10)	(0.10)	(0.13)	(0.13)	(0.13)	(0.14)	(0.14)	(0.14)
Observations	3780	3780	3780	1860	1860	1860	1920	1920	1920
R^2	0.00	0.00	0.00	0.01	0.00	0.00	0.00	0.00	0.00
F	9.64	4.65	5.53	13.69	0.51	1.72	0.71	4.97	3.90

[†] OLS regressions. Coefficients and standard errors (in parenthesis) are reported

$c_i^?$, where $? \in \{-,=,+\}$, is chosen directly in CC or via $c_i^? = c_i^0 + \Delta_i^?$ in PA.

Table 4.7: Independent contributions depending on lagged contributions, and lagged adjustments

Treatment	PA+CC			PA			CC		
Probability	2.5%	33.3%	49%	2.5%	33.3%	49%	2.5%	33.3%	49%
	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)
$c^0_{i,t-1}$	0.95***	0.84***	0.82***	0.99***	0.77***	0.81***	0.89***	0.90***	0.81***
	(0.08)	(0.05)	(0.06)	(0.09)	(0.07)	(0.08)	(0.15)	(0.07)	(0.08)
$(c^0_{i,t-1})^2$	0.00	0.00	0.01	0.00	0.01	0.01	-0.00	-0.00	0.01
	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
$c_{-i,t-1}$	-0.36	0.03	-0.06	-0.72***	-0.02	-0.15	0.12	0.03	0.01
	(0.29)	(0.08)	(0.07)	(0.22)	(0.11)	(0.12)	(0.58)	(0.12)	(0.08)
$\text{Payoff}_{i,t-1}$	0.53	-0.00	0.13	0.98***	0.07	0.25	-0.07	-0.02	0.04
	(0.36)	(0.11)	(0.09)	(0.28)	(0.15)	(0.16)	(0.72)	(0.15)	(0.10)
$\Delta^+_{i,t-1}$	0.12***	0.05**	0.11***	0.11**	0.10***	0.17***	0.13***	0.03	0.08**
	(0.03)	(0.02)	(0.03)	(0.05)	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)
$\Delta^=_{i,t-1}$	-0.02	0.04	-0.07**	0.01	0.00	-0.12***	-0.06	0.07	-0.03
	(0.05)	(0.04)	(0.04)	(0.07)	(0.04)	(0.04)	(0.07)	(0.05)	(0.05)
$\Delta^-_{i,t-1}$	0.12***	0.04*	0.04*	0.11**	0.07**	0.06	0.14***	0.02	0.03
	(0.03)	(0.02)	(0.02)	(0.04)	(0.03)	(0.04)	(0.05)	(0.02)	(0.02)
Constant	-4.38	0.53	-0.61	-8.44***	-0.01	-1.58	0.94	0.62	0.18
	(3.27)	(0.96)	(0.85)	(2.51)	(1.34)	(1.42)	(6.49)	(1.39)	(0.97)
Observations	3656	3780	3652	1800	1860	1796	1856	1920	1856
R^2	0.66	0.74	0.71	0.63	0.70	0.69	0.69	0.77	0.73
F	333.87	494.14	505.92	135.05	253.96	246.15	273.03	264.30	298.70

OLS estimation. Robust standard errors in parenthesis clustered by individuals.

4.3.3. On modes of behavior

We finally analyze modes of behavior, mainly two types of contributors: conditional cooperators and exploiters, or free-riders. With the help of our notation, we can define these two types of behavior as follows:

Conditional cooperation:

- for $c_i^0 = 0$: $\Delta_i^+ > 0, \Delta_i^= = 0$;
- for $0 < c_i^0 < e$: $\Delta_i^= = 0, \Delta_i^- < 0, \Delta_i^+ > 0$;
- for $c_i^0 = e$: $\Delta_i^= = 0, \Delta_i^- < 0$.

Exploitation (or Freeriding): $c_i^0 = 0$, all $\Delta_i^?$ minimal.

As confirmed by data in Table 4.8, the share of free-riding choices is lower than that of conditional cooperating choices. Furthermore, the share of free-riding choices is higher in treatment PA, possibly due to the stronger cognitive demand of this treatment,[14] and decreases with probability p for both frames.[15] On the other hand the frame is relevant only for $p = 2.5\%$.

Result 6: *Conditional cooperation is significantly more frequent than freeriding in both treatments.*

Our findings are consistent with those obtained by Fischbacher et al. (2001), who classify subjects on the basis of "follower" contributions reacting to quantitative "leader" contributions. They categorize roughly a third (30%) of their subjects as free riders and roughly half (50%) as conditional cooperators. This experiment has been replicated, with some variations, at different locations, and the results are generally confirmed. [16] However, results from single locations in

[14]In treatment PA, subjects are forced to adjust their independent contribution in order to determine a given final contribution.

[15]The definition of the conditional cooperator does not exclude $c_i^0 = 0$ what, however, is rarely observed in our data. Indeed, conditional cooperators with $c_i^0 = 0$ account for less than 10% of the observations, with nearly half of these rare results (45%) concentrated on $p = 2.5\%$.

[16]Kocher et al. (2008) ran the experiment at single locations in the United States, Austria, and Japan (the original experiment by Fischbacher et al. (2001) was conducted in Switzerland) finding similar shares of types in Austria and Japan, but significantly different ones in the United States with a higher proportion of conditional cooperators (80.6%) and a lower one of free riders (8.3%). Herrmann and Thöni (2009) replicated the experiment at four locations in Russia: the distribution of types is very similar across locations. Moreover, while the proportion of conditional cooperators in this study is comparable to the one of Fischbacher et al. (2001), free riders account only for 6.3% of the total. Thöni et al. (2009) ran the experiment with a large pool of subjects in Denmark: a vast

different (countries and) cultures are questionable since local differences may matter more than cultural ones.

Closer to our setting are the experiments by Fischbacher and Gächter (2010) and Fischbacher et al. (2012), who try to assess the effects of using the strategy (vector) method when eliciting cooperative preferences. With their methodology they classify roughly 55% of subjects as conditional cooperators and 23% as free riders. They also confirm, consistently with other repeatedly played public goods experiments, that contributions decline over time.

Table 4.9 replicates the dynamic analysis presented in Table 4.7 by adding as explanatory variables the two behavioral types. The independent contribution is – as expected – negatively correlated with free-riding behavior in the previous round while positively correlated with previous conditional cooperation.

Table 4.10 shows how the probability of being either type is affected by past choices and payoffs. Lagged independent contribution significantly affects only the probability of being free-rider. Lagged adjustments Δ_i^+ affect positively the probability of being conditionally cooperative, independently of the probability of adjustment, and negatively the probability of free-riding, in both cases with similar magnitude. Lagged adjustments Δ_i^- affect negatively both tendencies, with higher magnitude for conditionally cooperating than free-riding.

Table 4.8: Type of contributions by treatment and sequence

	Conditional Cooperator by sequences					
	Sequence A and B		Sequence A		Sequence B	
CC and PA	0.295		0.318		0.273	
CC	0.284		0.305		0.263	
PA	0.307		0.333		0.283	
T-test†	***(0.01)***		***(0.02)***		(0.09)	
	Conditional Cooperator by probabilities					
	$p = 2.5\%$	T-test $(2.5\% - 49\%)$	$p = 33.3\%$	T-test $(2.5\% - 33.3\%)$	$p = 49\%$	T-test $(33.3\% - 49\%)$
CC and PA	0.253	***(0.00)***	0.317	***(0.00)***	0.315	(0.84)
CC	0.235	***(0.00)***	0.009	***(0.00)***	0.306	(0.75)
PA	0.272	***(0.00)***	0.324	***(0.00)***	0.325	(0.97)
T-test†	***(0.01)***		(0.36)		(0.21)	
	Exploiter by sequences					
	Sequence A and B		Sequence A		Sequence B	
CC and PA	0.147		0.143		0.151	
CC	0.167		0.154		0.181	
PA	0.127		0.132		0.122	
T-test†	***(0.00)***		***(0.02)***		***(0.00)***	
	Exploiter by probabilities					
	$p = 2.5\%$	T-test $(2.5\% - 49\%)$	$p = 33.3\%$	T-test $(2.5\% - 33.3\%)$	$p = 49\%$	T-test $(33.3\% - 49\%)$
CC and PA	0.183	***(0.00)***	0.135	***(0.00)***	0.125	(0.18)
CC	0.195	***(0.00)***	0.157	***(0.00)***	0.149	(0.47)
PA	0.170	***(0.00)***	0.112	***(0.00)***	0.099	(0.22)
T-test†	***(0.04)***		***(0.00)***		***(0.00)***	

† P-values in brackets for T-test on frame effect.

majority (70.2%) of subjects are conditional cooperators, while 13.9% are free riders. Martinsson et al. (2013) replicated the experiment in Vietnam and Colombia finding similar distributions of types except for the proportion of conditional cooperators (50% in Vietnam and 62.5% in Colombia). Moreover, when compared with the other studies, the proportion of free riders, 4.2%, is lower than in Switzerland, Austria, Denmark and Japan.

4.4. Conclusions

Evidence from public good experiments have shown that, in general, average contributions to the public good are higher than theoretically predicted.[17] To explain this finding, "warm glow" or altruistic preferences have frequently been invoked. As observed by Gächter (2007), such intrinsic motives leave one's behavior independent of others' contributions. However, individual choices often depend on how others behave; in particular, most people are conditionally cooperative: they mainly want to contribute if others do the same.[18]

We have contributed to the analysis of this behavior in public good games with a novel experimental design, eliciting multidimensional choice data based on qualitative information. In addition to multidimensional choices and use of the strategy vector method we have enlarged our data set by simulating pairs of subjects in order to asses the robustness of our findings.

Our results reveal that conditional cooperation is affected by the salience of conditioning. In particular, higher probability of adaptation implies higher and more persistent cooperation, while the opposite holds for lower probability of adaptation. Moreover, reacting to coinciding independent contributions implies impressive conformity in contributing, whereas when reacting to higher (lower) independent contributions there are average upward and, more strongly, downwards effects in contributing. From the analysis of modes of behavior and their dynamics we can confirm that being a "conditional cooperator" versus "free-rider" does not appear to be an intrinsic characteristic, even though the proportions we find are consistent with those found in the literature: such attitudes respond to framing and own past choices: in spite of some inertia our data do not suggest persistent classification of subject types.

Our overall results (actual and simulated) could be used to suggest nudging via imposing the more welfare enhancing frame since choice sets for independent and final contributions are always the same. Such nudging would be in line with libertarian paternalism (Thaler and Sunstein, 2003). From an institutional point of view, one would suggest to design public good contribution schemes enhancing the role of trust (like our 49% setting). From a sequencing point of view, one might want to schedule high probability adaptation early, strengthening, respectively suggesting, the trust game character initially, similar to positioning healthy food products at the entrance of supermarkets.

[17] See e.g. Ledyard (1995).

[18] Several recent studies looked for a classification of social preferences according to such ideas, see Cooper and Kagel (2013) for a recent review.

Acknowledgments

We would like to thank the Max Planck Institute of Jena for funding and supporting this research.

Table 4.9: Independent contribution and lagged behavior

Treatment	PA+CC			PA			CC		
Probability	2.5%	33.3%	49%	2.5%	33.3%	49%	2.5%	33.3%	49%
	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)
$c^0_{i,t-1}$	0.86***	0.76***	0.71***	0.88***	0.64***	0.76***	0.82***	0.88***	0.67***
	(0.09)	(0.07)	(0.09)	(0.10)	(0.09)	(0.12)	(0.16)	(0.09)	(0.12)
$(c^0_{i,t-1})^2$	0.01	0.01*	0.02**	0.01	0.02***	0.01	0.00	-0.00	0.02*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
$c_{-i,t-1}$	-0.37	0.01	-0.08	-0.74***	-0.04	-0.16	0.12	0.03	-0.02
	(0.29)	(0.08)	(0.07)	(0.22)	(0.11)	(0.12)	(0.57)	(0.12)	(0.08)
$\text{Payoff}_{i,t-1}$	0.54	0.02	0.16*	1.00***	0.11	0.26	-0.08	-0.02	0.08
	(0.36)	(0.10)	(0.09)	(0.28)	(0.15)	(0.16)	(0.72)	(0.15)	(0.11)
$\Delta^+_{i,t-1}$	0.09**	0.03	0.10***	0.10*	0.09**	0.17***	0.07	0.00	0.05
	(0.04)	(0.03)	(0.03)	(0.05)	(0.04)	(0.04)	(0.06)	(0.03)	(0.04)
$\Delta^=_{i,t-1}$	-0.01	0.04	-0.06*	0.02	0.00	-0.12***	-0.05	0.05	-0.02
	(0.05)	(0.04)	(0.03)	(0.07)	(0.04)	(0.04)	(0.07)	(0.05)	(0.05)
$\Delta^-_{i,t-1}$	0.13***	0.05**	0.05**	0.11**	0.07**	0.07	0.16***	0.04	0.04
	(0.04)	(0.02)	(0.02)	(0.05)	(0.03)	(0.04)	(0.05)	(0.02)	(0.03)
Freerider_{t-1}	-0.34*	-0.33*	-0.46**	-0.42*	-0.61**	-0.25	-0.28	-0.10	-0.61**
	(0.19)	(0.18)	(0.22)	(0.23)	(0.26)	(0.32)	(0.30)	(0.24)	(0.31)
Cond. Coop.$_{t-1}$	0.09	0.10	0.05	-0.08	0.04	0.04	0.32**	0.19*	0.09
	(0.11)	(0.08)	(0.09)	(0.14)	(0.11)	(0.11)	(0.16)	(0.11)	(0.16)
Constant	-4.25	0.53	-0.56	-8.30***	0.01	-1.52	1.19	0.65	0.16
	(3.25)	(0.96)	(0.87)	(2.52)	(1.34)	(1.47)	(6.41)	(1.40)	(1.00)
Observations	3656	3780	3652	1800	1860	1796	1856	1920	1856
r2	0.66	0.74	0.71	0.63	0.71	0.69	0.69	0.77	0.73
F	268.96	402.04	408.39	114.13	211.65	194.53	226.52	217.76	269.99

OLS estimation. Robust standard errors in parenthesis clustered by individuals.

Table 4.10: Probit estimation for behavioral types†

	Conditional Cooperator			Freerider		
Probability	2.5%	33.3%	49%	2.5%	33.3%	49%
	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)
Round	-0.00	-0.00	-0.01*	0.01*	0.00	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
$c^0_{i,t-1}$	-0.09	0.00	-0.01	-1.36***	-1.26***	-1.19***
	(0.09)	(0.08)	(0.07)	(0.18)	(0.12)	(0.10)
$(c^0_{i,t-1})^2$	0.02**	0.00	0.01	0.09***	0.11***	0.10***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
$c_{-i,t-1}$	-0.12	0.03	0.09	0.82	-0.04	-0.03
	(0.25)	(0.10)	(0.07)	(0.59)	(0.15)	(0.12)
$\text{Payoff}_{i,t-1}$	0.16	-0.04	-0.11	-1.06	0.04	0.01
	(0.31)	(0.12)	(0.09)	(0.74)	(0.19)	(0.15)
$\Delta^+_{i,t-1}$	0.36***	0.27***	0.27***	-0.31***	-0.24***	-0.24***
	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)
$\Delta^=_{i,t-1}$	-0.10	0.12**	0.07	0.18***	-0.01	0.16***
	(0.07)	(0.06)	(0.07)	(0.07)	(0.06)	(0.05)
$\Delta^-_{i,t-1}$	-0.23***	-0.27***	-0.22***	-0.11***	-0.02	-0.07**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.05)	(0.03)
Constant	-2.65	-0.94	-0.25	10.12	0.26	0.48
	(2.83)	(1.13)	(0.83)	(6.64)	(1.73)	(1.35)
Observations	3656	3780	3652	3656	3780	3652
Pseudo-R^2	0.22	0.23	0.23	0.49	0.56	0.54

† Probit estimation for conditional cooperative and freeriding choices.
Dependent variable: binary decision to behave as conditional cooperator (or freerider).
Robust standard errors clustered by individuals in parenthesis.

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Appendix C

Translated Instructions

INSTRUCTIONS TO PARTICIPANTS

Introduction

Welcome to our experiment!
During this experiment you will be asked to make several decisions, and so will the other participants.
Please read the instructions carefully. Your decisions, as well as the decisions of the other participants will determine your payoff according to rules, which will be explained shortly. The tokens that you earn during the experiment will be converted to euros at the rate of 1 token = €0.50. In addition to the earnings from your decisions over the course of the experiment, you will receive a show-up fee of €2.50.
Please note that hereafter any form of communication between the participants is strictly prohibited. If you violate this rule, you will be excluded from the experiment with no payment. If you have any questions, please raise your hand. The experimenter will come to you and answer your questions individually.

Once you are ready to begin the experiment, please click on the 'OK' button on the screen. When everyone is ready, the experimenter will read the instructions aloud, and then the experiment will start.

Description of the Experiment

This experiment is fully computerized. You will be making your decisions by clicking on appropriate buttons on the screen. All the participants are reading the same instructions and taking part in this experiment for the first time, as you are.

The experiment is composed by three phases (Phase 1, Phase 2, and Phase 3). Each phase consists of 15 identical rounds, in which you will be required to perform a Task as explained below. The whole experiment hence will consist of 45 rounds.

During the experiment, groups of 2 participants will be randomly formed, and in every round of the same Phase you will be interacting with a different participant (how to interact with the other will be explained shortly). In other words, you will never be interacting more than once with the same participant through the same Phase of the experiment.

Description of the Task

In each round, you and the other will be endowed with nine (9) tokens.
In each round, both you and the other participant will have to take decisions.

(1) Firstly, you, as well as the other, have to decide, individually and independently, how many of the nine tokens you are endowed with you want to contribute to a project.
During the experiment, with some probability you will have the possibility to modify your initial contribution (how to modify the contribution will be explained shortly).
The probability with which you will be able to modify your contribution will be communicated *before* taking your first decision.
The same level of probability applies to you and the other participant you interact with in every single round.
There are three possible levels of probability: you could modify your contribution by 2,5%, 33,3% and 49%.
The level of probability will remain fixed for all the rounds in a given Phase.
In every Phase, the probability will be different, but as explained you will be informed about it before starting the tasks.

(2) Secondly, after every participant has completed the first task, you, as well as the other, have to decide, individually and independently, how much you would like to modify your initial contribution in case you will you will be given the possibility to do so.
More specifically, you will be asked to modify your contribution in the three following situations:

- if the other has contributed to the project more than you;
- if the other has contributed to the project less than you;
- if the other has contributed to the project as much as you.

For each of the three cases, you will be asked by how many tokens you want to modify your initial contribution. You can add at most as many tokens as nine minus your initial contribution. You can subtract at most as many tokens as your initial contribution.
You will be asked by how many tokens you want to modify your initial contribution *before* knowing if the other has contributed more than you, less than you or as much as you.
After all participants have taken the decision regarding the modified contribution, the computer will randomly select if any participant will adapt and

which participant is allowed to modify the initial contribution, and will communicate the outcome to both of you.
Please note that there is always the possibility that neither you nor the other will be allowed to modify.

The computer will work out and communicate to you if and how much your initial contribution will be adapted depending on both yours and the other participant' decisions.

In particular, if you have been selected to adapt, the computer will automatically adapt your contribution on the basis of the decision you have taken before (2) and, given the decision on contribution stated in (1) by the other participant that in the current round is interacting with you, and will show on your screen your payoff for this round.

If instead the other has been selected to adapt, the computer will automatically adapt his/her contribution on the basis of the decision he/she has taken before (2) and, given the decision on contribution stated in (1) by you, and will show on your screen your payoff for this round.

NOTE: The computer ***will not*** *inform you about the size of the initial contribution of the other participant with whom you interact in each round, but it will tell you if his/her contribution has been greater, lower or equal to your initial contribution.*

Summing up, your payoff for each round will depend on:

- your initial decision to contribute (1);
- the probability that you (or the other) have to modify the former decision;
- your decision on how much, if at all, to modify your initial contribution depending on the other's initial decision to contribute;
- the decision of the other on how much, if any, to modify his/her initial contribution to your initial decision to contribute.

Your payoff is determined in each round according to the following formula:

9 – your final contribution + 0.8(your final contribution + the other's final contribution)

The final contribution is the one that you decide in (1), which is adapted according to what you decided in (2) if you are given the possibility to adapt.

Information Feedback

Before proceeding to the next round, the computer will inform you of: (i) your decisions (your initial contribution (1) and your decisions to adapt (2)); (ii) the probability to adapt of the current Phase and the result of the related lottery; (iii) if you or the other participant with whom you interact in this round or none of you, are allowed to adapt your initial contribution; (iv) your payoff for the current round.

End of the Experiment

After completing the experiment, that is when the 45 rounds will be over, a lottery administrated by the computer will randomly select one round for each Phases to be considered for payment and will display it on your screen numbers with the corresponding payoff you made in those rounds.

Your total payoff from the experiment will be equal to the sum of:

- the payoff that you realised in the selected round in Phase 1;
- the payoff that you realised in the selected round in Phase 2;
- the payoff that you realised in the selected round in Phase 3;
- the participation fee.

A summary screen will display the total points you have accumulated and the corresponding earnings in euros. Please remain at your cubicle until asked to come forward and receive payment for the experiment.

After having finished the experiment, but before receiving your payoff, you will be asked also to fill up a short questionnaire about your demographics and other few questions.

Chapter 5

Gamblers or Investors? An Experiment on the Almost-Winning Outcome

with C. Choirat and D. Di Cagno

Abstract

Near-miss outcomes are real-life situations which increase the perceived probability of the occurrence of future successes. The Almost-Winning (AW) bias is the well-known cognitive bias that makes individuals unable to distinguish between situations in which near misses signal ability and situations in which near misses are completely meaningless, in the sense of being unrelated to future (likelihood of) winning. The empirical and neurological evidence shows that a near-miss increases gamblers' willingness to play: AW triggers a dopamine response similar to winning, in spite of no actual reward. Therefore, in a chance game, a sequence of AW outcomes easily generates an "irrational" willingness to continue playing, and might become a key factor in the development and maintenance of certain betting habits. We implement an experimental setting aimed at checking the relevance of the AW bias among ordinary students on order to evaluate its potential strength in absence of gambling pathologies. Two treatments are implemented in two different frames, an investment game (IG) and a slot machine game (SM), which try to avoid persistence at gaming.

Keywords: experiment, gambling, decision making under risk and uncertainty
JEL: C91, L83, D81

5.1. Introduction

Almost-Winning Outcomes (AW) are real-life situations which increase the perceived probability of the occurrence of future successes; however, when chance is binding, near-miss outcomes are completely meaningless, since they indicate nothing about the future likelihood of winning. The stemming cognitive bias makes individuals unable to distinguish between situations in which near misses signal ability and those in which no ability is involved.

This bias is part of the natural instinct when we face uncertain settings: Getting close to the goal increases the probability to achieve it in following trials. When gambling, the majority of people wrongly rely on previous events to predict future outcomes. Gamblers tend to create illusory links between independent events, forgetting or denying the exclusive randomness of the outcome.

Chase and Clark (2010) showed that near-miss outcomes (or almost winning) may elicit a dopamine response similar to winning, despite the fact that no actual reward is delivered. They found that in different games, near miss outcomes still activate parts of the brain associated with monetary wins and therefore increase individual willingness to play. This occurrence is highly relevant when we consider the fact that gambling is increasing in most developed countries, especially among young generations.

Previous empirical and theoretical research applies to compulsive gamblers, i.e., Benhsain et al. (2004), Camerer et al (2004), Chase and Clark (2010), Cote et al. (2003), Coventry and Hudson (2001), Coulombe et al. (1992), Dixon et al. (2010), Griffiths (1994), Ladouceur et al. (1991), van Holst et al. (2010): For those subjects strong evidence shows that near-misses are responsible in driving players to bet even though they keep losing. To the best of our knowledge little research has been devoted to near-miss effects on non-compulsive gamblers (Myrseth et al, 2010). Moreover, not many studies focus on the effect of AW outcomes on general decision making in other situations (such as management research, medicine and investment).[1]

Our focus is testing near-miss outcomes unrelated to pathological gambling: Individuals should recognize near-misses as meaningless signals in those situations that do not involve any degree of skill.

This study is based on an experimental study with multiple periods aimed at analyzing the almost winning bias through different information sets and decision-making contexts. We propose a simple chance game where subjects can decide which share of their initial endowment to allocate in a lottery: Thanks to our simple task, we are able to account for the effect of near-miss bias on their willingness to play (measured by the number of tokens allocated in the risky lottery).

We represent this chance game in two different framings: an investment game (IG) and a slot machine game (SM).[2] This is aimed to distinguish between the near-miss effect on players

[1] Near misses are seen as a possible distortion in decision making under risk and a miss opportunity of learning, Dillon and Tinsley (2008)

[2] We define a chance game as a game where no skill or ability can help individuals to correctly forecast the future outcome, think of a slot machine or picking a lottery number. Thus, our work focuses on the individual

when they are facing a chance game framed as a gamble and a chance game framed as a financial market. Since almost winning triggers a different cognitive process and the statistical risk perception is distorted (Dillon and Tinsley, 2010), we want to check if changing the frame distorts the individual's perception of the game, and the underlying probabilities.[3] Moreover, individuals could have different arousal and sensation seeking depending on the framing in which the game is proposed (Anderson and Brown 1984, Ladouceur et al. 1991).

Two different information settings are implemented in order to understand how to decrease the number of erroneous perceptions by warning subjects on AW bias and independence of events or disclosing the actual winning probabilities (nudging versus awareness).[4]

Through this experimental setting, we add new insights in near-miss effects, through three levels of analysis. First, we identify overall a persistent effect of AW outcomes on next trials, showing that also non-compulsive gamblers might be affected by the same cognitive bias. Surprisingly, individuals who generally prefer to avoid risk are more responsive to AW outcomes. Second, we want to see if framing matters in perceiving AW bias: We find that near-misses are binding not only in a traditional gambling game, like the slot machine, but also in the investment game (there is no significant difference in the AW bias across frames). Third, we discuss the role of information in correcting the misinterpretation of AW outcomes. Both nudging and informing are effective in reducing the willingness to choose the risky choice. Probability information even helps people to correctly interpret the game and the AW bias tends to disappear.

The paper is organized as follows: Section 5.2 describes the experimental protocol; descriptive and statistical data analysis is presented in Section 5.3, including a last Section of robustness check; final remarks and guidelines for future research are proposed in Section 5.4.

5.2. Experimental Protocol

We ran seven sessions with a total of 144 students [5], recruited among the undergraduate population of Luiss University of Rome using Orsee (Greiner, 2004), at the laboratory Cesare. All experimental sessions, based on three stages and a questionnaire, are fully computerized using z-Tree (Fischbacher 2007).[6] In each experimental session there are two stages with 20

representation of the near-miss outcomes in chance situations, where there is no possibility for the individual to affect the game result.

[3] We think that also noise traders face the financial market as a lottery.

[4] Occasional gamblers show that reminders about independence of events decrease the number of erroneous perceptions (Benhsain et al. 2004) and the motivation to pursue the game is weaker among participants who were reminded.

[5] We ran five sessions with 24 participants each and two sessions with 12 subjects only.

[6] The experiment is based on different treatments and framings in order to disentangle the almost winning effect from other effects. In this sense, a different set of instructions was provided to participants. English translation is reported in Appendix D, dataset and analysis are available upon request.

rounds each, in which the subjects face the basic task (Stage 1 and Stage 2 differ on the basis of the information provided to subjects, as it will be explained in section 5.2.3). Stage 3 proposes the Holt and Laury's (2002) protocol to elicit individual risk attitude.

We run this game proposing two different frames in order to detect if the occurrence of AW situations was more likely to affect experimental subjects when the same probabilistic decisions are framed as a chance game or as a skill game: Investment Game framing and the Slot Machine framing (IG and SM hereafter). Even if both framings reproduced the same chance environment, they differ in the way in which the decision task was presented.

For each round of the basic task, proposed in Stage 1 and Stage 2, participants decide how to allocate their endowment in a risky choice. In particular, in each round, participants decide how much to allocate of the initial endowment of 10 ECU (Experimental Currency Unit, with a conversion rate 1 ECU=€0.5) between a safe choice (keeping the money) and a risky choice. The risky choice is represented by a lottery with two possible outcomes, depending on the state of the world (respectively the good state, S_G and the bad state, S_B). The probability of the good state (S_G) is p_h: when the good state occurs, the outcome consists of the amount of experimental units bet in the risky option times a positive marginal return (the marginal rate of return in the good state is h, where $h > 1$) which gives more than the initial investment. In the bad state (S_B) (which occurs with probability p_l) individuals receive back their bet in the risky option times a low marginal rate of return (we name it in the bad state l where $l < 1$). The entire endowment (e) has to be allocated between the risky option (x_1) and the amount kept (x_0), which is characterized by a marginal rate of return equal to 1. In this sense the final payoff of each round is:

$$v(x_1) = \begin{cases} lx_1 + x_0 & \text{if } S_G \text{ occurs} \\ hx_1 + x_0 & \text{if } S_B \text{ occurs} \end{cases} \tag{5.1}$$

Where $x_0 = e - x_1$ and h (l) is the marginal return of investing from the good (bad) state. We impose p_h, p_l, h, l such that it is always optimal not to invest in the risky option for a risk-neutral individual, in particular every time this condition holds $p_h < \frac{1-l}{h-l}$ (the maximization problem is solved in Appendix D.1).

The result of each round is represented on the computer by a sequential set of images reproducing the occurrence of bad or good state. Images differ depending on the frame of the experimental session (see Instructions in Appendix D). After each round, payoff is calculated and privately communicated to the participants.

To ensure the financial salience of each decision, only one period per stage was randomly selected at the end of the experiment for payment (with no additional show-up fee). The final result from the experiment was communicated at the end of Stage 3: The computer randomly selected one round from each stage and computed the final payment. After answering the final questionnaire, subjects were individually paid.

5.2.1. Investment Game Framing (IG)

The IG frame proposes a game in which individuals choose how much to invest a given endowment e in a risky set of three assets. The risky option is an exchange-traded fund (ETF) that tracks a basket of assets related to three different (independent) markets: Microlift, Chip Corporation and Doltech. The good state (S_G), that is the winning state, is verified when all three markets present a bullish trend with probability $p_h = \frac{1}{8}$. The bad state (S_B) occurs when the outcome has at least one bear market with probability $p_l = \frac{7}{8}$. We set the marginal return in the good state $h = 3$ and the marginal return in the bad state $l = 0.1$.

In each round a scroll bar allows subjects to examine the 11 possible integer allocations (investing from 0 to 10 ECU in the risky asset) before taking their decision; for each potential investment the computer shows on the screen the possible result in terms of expected payoff, i.e. the potential earnings in the good and bad state. Once subjects made their investing decision, the computer selects the outcome of the three markets: the outcome of each of them will appear sequentially on screen (either a green arrow pointing up or a red arrow pointing down first for market Microlift, then Chip Corporation market and last for Doltech market).

We define the Almost Winning (AW hereafter) outcome when the first two markets are bullish, but not the third one. Other sequences of AW drawn, which include just one market bearing, might be included in the analysis; however, given the result is shown sequentially, it is reasonable to think that the dopamine rises while the first two markets are bullish and only the last one has a different trend, so we focus only on that sequence.

5.2.2. The Slot Machine Game Framing (SM)

The SM game proposes a game where individuals choose how much to bet of a given endowment e on a Slot Machine line with three cells. The risky option is represented by random draws of different icons representing fruit and "BAR" symbol. The good state (S_G), that is the winning outcome, occurs when 3 "BAR" symbols appear on the subject's screen with probability $p_h = \frac{1}{8}$. The bad state (S_B) occurs when at least one symbol drawn from the slot machine is different from "BAR" with probability $p_l = \frac{7}{8}$. We set the marginal return in the good state $h = 3$ and the marginal return in the bad state $l = 0.1$.

In each round, participants can decide how many ECU to bet on the slot machine, given their initial endowment of 10 ECU, and how much they prefer to keep. A scroll bar allows the individual to examine the 11 possible bets and for each potential bet the possible earnings in the good and the bad state. Then the slot machine appears, rolling three different images, and sequentially the cells stop to one symbol. We define the AW when "BAR" occurs in the first two cells, but not in the last one. Each round works as described in Section 5.2.1 and it is characterized by the same steps and layout.

5.2.3. Information on Almost-Winning

Each treatment (IG and SM) is divided by stages. In particular, Stage 1 is common to all sessions: After explaining the game structure, individuals played 20 rounds without any disclosure on probabilities or additional game rules.[7]

At the end of Stage 1 we elicit the probability beliefs of participants by asking them to guess the probability of winning among several options. Stage 2 differs from the first one in the following way:

1. **Stage 2A** in which individuals were informed on the probability of the good state, which is $p_h = \frac{1}{8}$. We call this stage the "Probability Stage."

2. **Stage 2B** in which individuals were informed about the possible effect of almost-winning outcomes, underlying the independence of each round played. We call this stage the "Warning Stage."[8]

In Stage 3, equal for all sessions, subjects play the well known Holt and Laury (2002) lottery protocol to control for individual risk aversion.

A questionnaire collects additional information about our subjects at the end of the experiment.

Since the actual random occurrence of AW could be poorly informative from our research point of view, we adopted different frequencies of almost winning: One generated randomly by the software (in this case probability of a random almost winning is $p_{AW} = \frac{1}{8}$ given three cells with probability of good state one half each) or forced such that the probability of almost winning is, at least, $p_{AW} = \frac{3}{8}$ while the probability of winning is kept constant $p_h = \frac{1}{8}$. Regardless the almost winning frequency, instructions did not change since the other salient information we provided was the winning probability in Stage 2A. To summarize the experimental protocol,

[7] Even if participants could ask questions individually to the experimenter after reading the instructions, very few of them asked about the probability of winning and the experimenter was trained not to provide this kind of information at that stage.

[8] At the beginning of the both stages a message appeared on the screen. In the probability stage, the message simply states "Note that the probability of three bullish markets (three bars) is $\frac{1}{8}$ ". In the warning stage the message at the beginning of the session states "Note that, if you nearly win in the previous round, i.e. two bullish markets (two bars), it does not have any effect on the winning probability in future rounds "

Table 5.1: Experimental Structure

	Session						
	1[1]	2	3[1]	4	5	6	7
Average Payment	15.10	9.05	16.93	12.00	10.46	13.95	12.55
Number of subjects	24	24	24	12	24	12	24
	Framing						
IG framing	x	x	x			x	x
SM framing				x	x		
	Stage 1 (20 rounds)						
No Information	x	x	x	x	x	x	x
	Stage 2 (20 rounds)						
Probability (2A)	x		x		x		x
Warning (2B)		x		x		x	
	Stage 3 (30 rounds)						
Holt and Laury's protcol	x	x	x	x	x	x	x
	AW						
Forced AW				x	x	x	x

Notes: Experiment structure for each session.
[1] These two sessions included an extra task at the end of the experiment, which is not analyzed in this work and did not affect the results of the first stages.

see Table 5.1.

5.3. Results

In this section we present the main results focusing on different levels of analysis. In Section 5.3.1 we start describing the variables collected through the experiment. Section 5.3.2 is based on the analysis of the AW and framing biases, underlying the role of risk preferences. The second step investigates the role of information, in particular whether informing on probabilities or nudging is more relevant to weaken the AW effect (Section 5.3.3). We conclude with robustness checks without relying on distributional assumptions, based on bootstrap procedure, Section 5.3.4.

5.3.1. Descriptive Analysis

Table 5.2 summarizes the findings related to tokens allocation differences by game rules and individual characteristics across stages. When we consider the first stage, the allocation of tokens is not significantly different when we compare rounds right after AW outcomes and those in which it didn't occur, although AW frequency is significantly relevant: When we force the frequency of AW, participants played significantly more. AW outcomes increase the average allocation of tokens to the risky choice and when this is more frequent it is associated to higher willingness to play, consistent with Cote et al. (2003) results.

Table 5.2: Tokens allocated in the risky choice

Investment differences by Stages			
	Average investment		
	Stage 1	**Stage 2A, Probability**	**Stage 2B, Warning**
Tokens allocated after an AW round	2.47	1.53	1.85
	(1831)	(1257)	(616)
Tokens allocated in other rounds	2.40	1.50	1.26
	(905)	(567)	(296)
WRT	*0.11*	*0.01*	*0.00*
Random AW[1]	2.50	1.66	1.22
	(1440)	(960)	(480)
Forced AW[1]	2.93	1.59	2.05
	(720)	(480)	(240)
WRT	*0.00*	*0.18*	*0.01*
SM[2]	1.99	1.08	1.66
	(720)	(480)	(240)
IG[2]	2.93	1.59	2.05
	(720)	(480)	(240)
WRT	*0.00*	*0.90*	*0.81*
	Individual characteristics		
Man	2.42	1.46	1.53
	(1820)	(1220)	(600)
Women	2.58	1.58	1.55
	(1060)	(700)	(360)
WRT:	*0.00*	*0.02*	*0.69*
Self evaluation on math capabilities: lower than average	3.23	1.41	3.46
	(300)	(200)	(100)
Self evaluation on math capabilities: average	2.66	1.75	1.48
	(1920)	(1220)	(700)
Self evaluation on math capabilities: higher than average	1.61	0.92	0.59
	(660)	(500)	(160)
WRT: Low vs. Average	*0.00*	*0.20*	*0.00*
WRT: Average vs. High	*0.00*	*0.00*	*0.00*
Do you like betting: no	2.31	1.35	1.51
	(1680)	(1160)	(520)
Do you like betting: yes	2.72	1.74	1.57
	(1200)	(760)	(440)
WRT	*0.05*	*0.50*	*0.49*

Notes: All tests reported are two-sided Wilcoxon Rank Sum Test (WRT). In parenthesis the number of observations.
[1] comparison between investment game with and without forced almost winning.
[2] We compare the Slot Machine Game and the Investment Game with forced AW outcomes.

We observe a significant difference between individuals playing the slot machine and the investment game framing. On average, subjects invest more in IG framing rather than betting on the classic slot machine.[9] When we control for elicited beliefs on probability (the winning probability they guessed at the end of Stage 1 when AW is more frequent) we find that there is no difference across framings and treatments, as shown in Table D.1 in Appendix D.2.

When we look at individual characteristics we find that female subjects tend to invest on average significantly more, as well as participants with lower math skills and those who enjoy more to gamble.

When we control for the following stages, we find very similar results, although we want to underline some interesting exceptions which can lead our regression analysis. In Stage 2A, probability knowledge provokes a severe drop in tokens allocated to the risky option, in particular when AW frequency was forced. Stage 2B is consistent to observations made for Stage 1. Awareness on winning probability and larger experience reduce the amount gambled

[9] We test the framing differences just with the sessions with forced almost winning, however, replicating the test and including also the IG with no forced AW, the difference of tokens allocation holds: Tokens allocated in the risky choice are greater in the investment game than the slot machine, regardless the AW frequency.

in both framings,showing that subjects seem to be able to evaluate the actual probabilities of winning.

The tokens allocated in the risky option increase when individuals are more inclined to risk. The sample is quite heterogeneous, risk-seeking individuals (46.53% of the total sample) tend to allocate an average of 2.67 tokens to the risky choice, which is higher compared to the average tokens allocated by risk-neutral individuals (18.06% of the total sample allocates just an average of 1.68 tokens to the risky option), and those showing risk averse preferences (35.42%) allocate only 1.25 tokens on average.

The individual decision on tokens allocation in Stage 1 is analyzed in Table 5.3 looking at the risk attitude. Risk averse individuals invest significantly more in the risky choice after an AW outcome and in those sessions where AW was more frequent, while instead risk-seeking individuals are consistent on their investment choice regardless the occurrence and frequency of AW outcomes.

On the other hand, risk seeking individuals invest significantly more in the IG framing than betting in the SM one: Individuals showing risk aversion are more consistent across framings.

Table 5.3: Tokens allocated in Stage 1 by risk averse/neutral/seeking individuals

	Average tokens allocation in Stage 1				
	Risk Averse	Risk Neutral	Risk Seeking	Wilcoxon rank-sum (R. Averse vs.Neutral)	Wilcoxon rank-sum (R. Neutral vs. Seeking)
Tokens allocated after an AW round	1.76	2.32	3.12	*0.00*	*0.08*
	(319)	(186)	(400)		
Tokens allocated in other rounds	1.56	1.90	3.20	*0.00*	*0.00*
	(650)	(308)	(873)		
WRT	*0.01*	*0.02*	*0.77*		
Random AW[1]	1.66	1.82	3.44	*0.01*	*0.00*
	(580)	(200)	(660)		
Forced AW[1]	2.17	2.20	3.53	*0.96*	*0.00*
	(200)	(120)	(400)		
WRT	*0.00*	*0.63*	*0.33*		
SM[2]	1.42	2.22	2.31	*0.00*	*0.07*
	(240)	(200)	(280)		
IG[2]	2.17	2.20	3.53	*0.96*	*0.00*
	(200)	(120)	(400)		
WRT	*0.30*	*0.06*	*0.00*		
Man	1.72	2.09	3.18	*0.00*	*0.00*
	(700)	(340)	(780)		
Women	1.67	1.99	3.30	*0.02*	*0.00*
	(320)	(180)	(560)		
WRT	*0.40*	*0.96*	*0.02*		
Self evaluation on math capabilities: lower than average	1.08	2.38	4.23	*0.00*	*0.00*
	(60)	(60)	(180)		
Self evaluation on math capabilities: average	2.01	2.47	3.19	*0.00*	*0.01*
	(660)	(340)	(920)		
Self evaluation on math capabilities: higher than average	1.17	0.73	2.61	*0.14*	*0.00*
	(300)	(120)	(240)		
WRT: Low vs. Average	*0.04*	*0.64*	*0.00*		
WRT: Average vs. High	*0.00*	*0.00*	*0.00*		
Do you like betting: no	1.50	2.15	2.99	*0.00*	*0.00*
	(600)	(300)	(780)		
Do you like betting: yes	2.00	1.93	3.57	*0.21*	*0.00*
	(420)	(220)	(560)		
WRT	*0.04*	*0.22*	*0.03*		

Notes: Risk preferences were inferred by the Holt and Laury's lotteries chosen in the third session of the experiment. All tests reported are two-sided Wilcoxon Rank Sum Test (WRT). In parenthesis the number of observations.
[1] comparison between investment game with and without forced almost winning.
[2] We compare the Slot Machine Game and the Investment Game whit forced AW outcomes.

Table 5.3 considers also the individual characteristics which we include as controls in our analysis: Females and males showing risk averse attitude allocate a similar amount of tokens to the risky option, while females showing risk-loving preferences are more willing to play riskier than male participants. Risk-loving individuals who poorly self evaluate their math skills tend to play significantly more. Enjoyment of gambling rises significantly with the average allocation of tokens to the risky option regardless of risk preferences.

These results are stressing the importance of risk preferences in this experiment, in particular risk attitude weakens or strengthens different cognitive bias provided by our experimental setting.

When we look at the individual allocation across stages and periods (Appendix D.2, Figure D.8 - Figure D.13) risk-averse subjects are characterized by a smoother-path, tending to zero token allocation to the risky choice, while positive investment to the risky option is more frequent when individuals are risk seeking. This result does confirm that risk lovers play more than the risk averse although it does not clarify who is more likely to be affected by AW outcomes.

5.3.2. AW Effect and Framing Effect

The descriptive analysis pointed out unexpected differences in tokens allocation when we consider risk preferences and that subjects tend to gamble more when they face the IG framing. The analysis focuses on both effects in particular the relationship between the allocation of tokens in the risky option in each round and the almost-winning outcome in previous rounds, controlling by possible framing and individual characteristics. We focus on the analysis of the first stage, where individuals have the same information. In particular, we implement the basic model:

$$y_{it} = \alpha + \beta_1 aw_{it-1} + \beta_2 ig_i + \beta_3 t_{it} + \gamma' X_{it-1} + \delta' Z_i + u_i + \varepsilon_{it} \tag{5.2}$$

$i = 1, ..., 144,\ t = 2, ..., 20$

Where y_{it} is the amount of tokens allocated to the risky option, aw_{it-1} is a dummy variable equal to 1 when AW outcome occurred in the previous period, while ig is the dummy referring to the investment game. We control for the period t and X_{it-1}, the outcome in the previous round associated to winning trials. Finally, we include some individual controls, such as gender, risk preferences, betting pleasure and self evaluation on math capabilities.
Table 5.4 collects the results of five specifications with an increasing number of control variables. In specification (1) and (2) we consider only the AW, framing effect and time: Tokens allocation

Table 5.4: Almost Winning effect on token's allocation in Stage 1

	Token allocation in risky choice, Stage 1				
	(1)	(2)	(3)	(4)	(5)
	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)
L.AW	0.50***	0.52***	0.31*	0.31	
	(0.19)	(0.19)	(0.19)	(0.19)	
Tr IG		0.92	0.45	0.50	
		(0.78)	(0.64)	(0.63)	
TrSM*L.AW=0					-0.73
					(0.68)
TrIG*L.AW=0					-0.49**
					(0.23)
TrSM*L.AW=1					-0.80
					(0.67)
Round	-0.15***	-0.15***	-0.12***	-0.12***	-0.12***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
L.Winning * L.Payoff			0.40***	0.40***	0.40***
			(0.06)	(0.06)	(0.06)
L.Winning			-4.24***	-4.23***	-4.27***
			(0.82)	(0.82)	(0.82)
L.Payoff			-0.34***	-0.34***	-0.34***
			(0.04)	(0.04)	(0.04)
Risk[1]			0.42***	0.35***	0.35***
			(0.12)	(0.12)	(0.12)
Female				-0.00	0.00
				(0.57)	(0.57)
Self evaluation on math capabilities[2]				-1.61***	-1.61***
				(0.49)	(0.49)
Do you like betting?[3]				0.78	0.78
				(0.56)	(0.56)
Constant	2.36***	1.66**	1.97**	4.65***	5.59***
	(0.38)	(0.70)	(0.99)	(1.57)	(1.63)
Constant(σ_u)	3.93***	3.91***	3.14***	3.01***	3.01***
	(0.27)	(0.27)	(0.24)	(0.23)	(0.23)
Constant (σ_e)	3.60***	3.60***	3.54***	3.54***	3.54***
	(0.08)	(0.08)	(0.07)	(0.07)	(0.07)
Observations	2736	2736	2736	2736	2736
ρ	0.54	0.54	0.44	0.42	0.42
N_{lc}	1126	1126	1126	1126	1126
N_{rc}	183	183	183	183	183
σ_e	3.60	3.60	3.54	3.54	3.54
σ_u	3.93	3.91	3.14	3.01	3.01

Notes: Panel tobit regressions with random effects, censored at 0 and 10. Stage 1 from all sessions are included in this analysis.
[1] The risk is measured from 0 (max. risk averse) to 10 (max. risk lover).
[2] In the questionnaire we asked "How do you consider your math capabilities?" The answers were Lower than Average (10.42 %), On Average (66.67%), Higher than the Average (22.92%)
[3] Dummy variable for Yes and No, in the questionnaire we asked "Do you like to bet?"
(* 0.1, ** 0.05, *** 0.01)

is significantly decreasing through time and positively related to almost winning outcomes: in particular seeing an AW increase of 0.5 for the average allocation to the risky option. IG framing coefficient is positive although not significant. Controlling for past outcomes and individual characteristics (model (3) and (4)) weaken the effect of AW but it remains positive on investment of the next round: Rather than simple multicollinearity between exogenous variables, we might focus on possible heterogeneity among individuals. Specification (5) of Table 5.4 checks the interaction between AW outcomes and framing effect with respect to the benchmark, the lagged AW outcome in IG framing: We conclude that AW outcomes are equally perceived in both framings, and significantly higher compared to lagged period without AW in the IG framing.

If the AW outcomes affect the pool of individuals recruited for this experiment which is not strictly related to compulsive behavior in gambling, we might claim that individuals are willing to play more when they see AW outcomes regardless of compulsiveness. Experience decreases the tokens allocated in the risky option while it is positively related to interaction between winning periods and payoff which means that winning tokens through risky option increases the willingness to play. Risk increases betting and is consistent to the analysis carried before, but the framing effect underlined in previous analysis is not significant. Table 5.4 includes some individual characteristics: Gender is not a relevant variable, instead the self statement on math capabilities is negatively correlated with playing more on the risky option.

Table 5.5: Almost-Winning effect on token's allocation in Stage 1, by risk preferences

	Token allocation in risky choice, Stage 1					
	Risk Averse			Risk Neutral+Risk Seeking		
	(1)	(2)	(3)	(4)	(5)	(6)
	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)
L.AW	0.75**	0.55*		0.38*	0.17	
	(0.31)	(0.31)		(0.23)	(0.24)	
Tr IG		-1.10			1.25	
		(1.11)			(0.77)	
TrSM*L.AW=0			0.62			-1.34
			(1.18)			(0.83)
TrIG*L.AW=0			-0.73*			-0.37
			(0.38)			(0.28)
TrSM*L.AW=1			0.84			-1.60**
			(1.15)			(0.81)
Round	-0.18***	-0.12***	-0.12***	-0.14***	-0.12***	-0.12***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
L.Winning * L.Payoff		0.79***	0.79***		0.27***	0.27***
		(0.12)	(0.12)		(0.07)	(0.07)
L.Winning		-8.52***	-8.57***		-2.81***	-2.84***
		(1.51)	(1.51)		(0.99)	(0.99)
L.Payoff		-0.52***	-0.51***		-0.28***	-0.28***
		(0.07)	(0.07)		(0.05)	(0.05)
Female		0.32	0.32		0.04	0.04
		(0.92)	(0.92)		(0.69)	(0.69)
Self evaluation on math capabilitie[1]s		-0.85	-0.85		-1.89***	-1.89***
		(0.76)	(0.76)		(0.61)	(0.61)
Do you like betting?[2]		1.71*	1.72*		0.65	0.65
		(0.98)	(0.98)		(0.68)	(0.69)
Constant	1.11*	5.31**	4.87*	3.10***	7.18***	8.74***
	(0.61)	(2.16)	(2.52)	(0.45)	(1.66)	(1.62)
Constant (σ_u)	3.71***	2.67***	2.67***	3.74***	3.03***	3.03***
	(0.45)	(0.36)	(0.36)	(0.32)	(0.28)	(0.28)
Constant (σ_e)	3.31***	3.21***	3.21***	3.71***	3.66***	3.65***
	(0.13)	(0.12)	(0.12)	(0.09)	(0.09)	(0.09)
Observations	969	969	969	1767	1767	1767
ρ	0.56	0.41	0.41	0.50	0.41	0.41
N_{lc}	502.00	502.00	502.00	624.00	624.00	624.00
N_{rc}	42.00	42.00	42.00	141.00	141.00	141.00
σ_e	3.31	3.21	3.21	3.71	3.66	3.65
σ_u	3.71	2.67	2.67	3.74	3.03	3.03

Notes: Panel tobit regressions with random effects, censored at 0 and 10. Stage 1 from all sessions are included in this analysis.
[1] In the questionnaire we asked "How do you consider your math capabilities?" The answers were Lower than Average (10.42 %), On Average (66.67%), Higher than the Average (22.92%)
[2] Dummy variable for Yes and No, in the questionnaire we asked "Do you like to bet?"
(* 0.1, ** 0.05, *** 0.01)

It is reasonable to think that subjects have different perceptions toward AW outcomes: Some of them could be more affected. We have individuals who prefer to adopt a safe strategy and to allocate zero tokens to the risky option, and others enjoying the possibility to bet constantly some positive amount. Our previous results already cast some light on differences among these two groups, and a big role is played by risk attitude. On the other hand, descriptive analysis pointed out that risk averse individuals are more likely to increase their bet after seeing the AW outcome (see Table 5.3)

When we carry the analysis dividing the sample in groups with different risk attitude, the AW effect is significantly relevant only for risk-averse individuals (see Table 5.5). Subjects showing risk aversion are consistent to the analysis made: Their investment in the risky option significantly decreases over time, while lagged payoffs have a positive effect when interacted with winning rounds. Those who enjoy betting are indeed playing more, and framing effect does not play any role. AW bias is significant regardless of the specification adopted, and when we look at the interaction between AW and treatment, it becomes clear that the AW effect is particularly relevant when we consider the investment game.[10]

Risk-seeking and neutral agents prefer to bet no matter what: Even though AW bias is positive and significant in some specifications, the magnitude of the effect is significantly smaller compared to the effect shown for risk-averse individuals. Framing is not significant but when we check on the interaction between framing and almost winning, we noticed that AW in investment game has a much stronger effect than the AW bias in a slot machine game. Individuals do not invest significantly more by framing, but AW bias could be more often misinterpreted in such context. Although self evaluation on math capabilities is irrelevant for risk-averse individuals, when we consider risk-neutral and risk-seeking ones, it becomes negatively (and significantly) related to tokens bet.

5.3.3. Warning or Nudging: This Is The Question

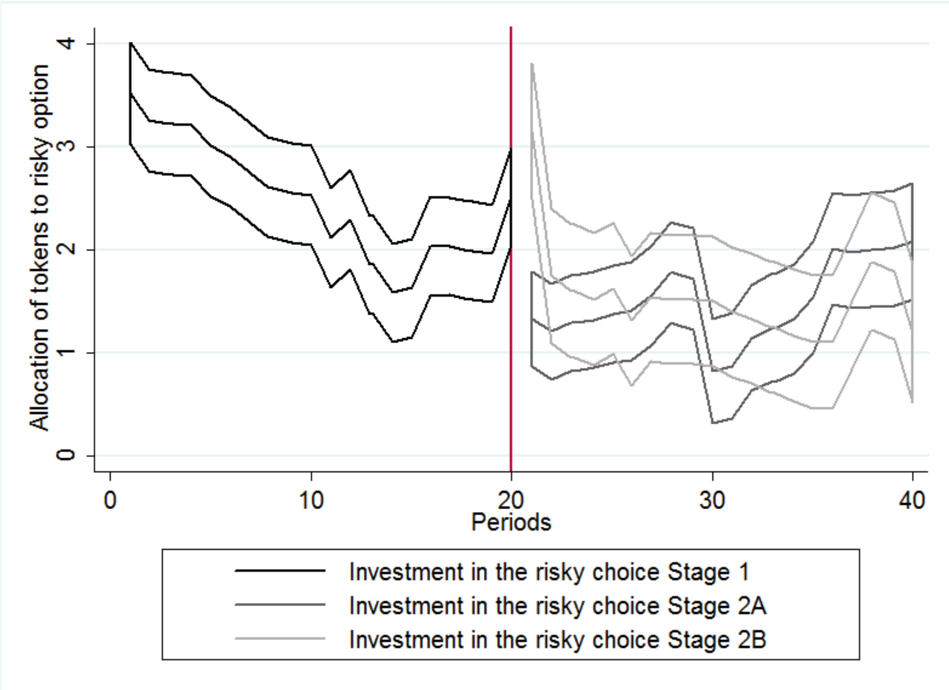
The second stage of both treatments is aimed to check how people are changing their gambling choices. Results from the first stage points out the role of risk attitude in reacting to AW cognitive bias; the second stage should help subjects to correctly interpret the game and the meaning of AW outcomes.

From the policy-maker perspective, results discussed in Section5.3.2 stress the role of risk attitude on the kind of "player"wrongly perceiving the AW bias. AW outcomes are tricks which promote a riskier behavior for those individuals which naturally prefer not to gamble.

The second stage is aimed to find the more effective way to help agents to correctly interpret the role of AW biases: We discuss the persistence in playing, but we are mainly interested if the AW bias is weakened by different information. Individuals were either informed about probabilities of winning or about the cognitive bias lying behind the AW outcome. We are basically investigating how individuals change their approach when either informed or nudged. Figure 5.1 can help to understand the average tokens allocation after information: Individuals knowing winning probabilities allocates significantly less tokens than the others. In Stage 2A, knowing the winning probability reduces suddenly the betting level; they review their winning expectations and decide to minimize their tokens allocation to the risky option (also the optimal solution of this game, discussed in Appendix D.1). After a first moment of disappointment, last periods of the Stage 2A are characterized by an increase in tokens allocated to the risky option.

[10]Tokens allocated in the slot machine after an AW outcome is not significantly different from the other rounds.

Figure 5.1: Allocation of tokens in Stage 2A and 2B



Notes: Kernel-weighted local polynomial smoothing with CI.

Players from Stage 2B, instead, are decreasing their bets smoothly. Last periods of both stages are characterized by end game effect where individuals increase significantly the token allocation to the risky option.

Table 5.6 represents the average investment through Stage 2A and Stage 2B. The results are underlying that informing individuals on independence between rounds is not sufficient to minimize the AW bias, while probabilities seem to have a stronger effect and individuals are less sensitive to AW bias. In Stage 2B, even though subjects adapt through experience lowering their investment in the risky choice, we show that warning per se is not very effective.

In Stage 2A, tokens allocation to the risky choice significant increases through time (the effect is not linear, as we saw in Figure 5.1, but the overall effect is driven by the increasing tokens allocation in the risky choices of last rounds); risk preferences are still playing a (positive) role as well as past outcomes. In Stage 2B risk preferences and time are not relevant, while higher math capabilities lower the willingness to play, even more sharply than what observed in Stage 1.

Finally, Table 5.7 distinguishes between risk averse and the others (we jointly consider risk-neutral and risk-seeking) in order to check which information was more relevant among stages and risk preferences.

Table 5.6: Almost-Winning effect on token's allocation in Stage 2A and 2B

	Token allocation in risky choice, Stage 2 and Stage 3					
	(1)	(2)	(3)	(4)	(5)	(6)
	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)
L.AW	0.53*	0.05		0.50**	0.54**	
	(0.29)	(0.28)		(0.24)	(0.26)	
Tr IG		-0.54			0.38	
		(0.87)			(1.31)	
TrSM*L.AW=0			0.50			-0.93
			(0.94)			(1.35)
TrIG*L.AW=0			-0.20			-0.51*
			(0.34)			(0.31)
TrSM*L.AW=1			0.30			-0.33
			(0.93)			(1.34)
Round	0.09***	0.06***	0.06***	-0.04**	-0.03	-0.03
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
L.Winning * L.Payoff		0.64***	0.65***		0.27**	0.27**
		(0.09)	(0.09)		(0.11)	(0.11)
L.Winning		-6.98***	-7.01***		-2.34*	-2.33*
		(1.13)	(1.13)		(1.30)	(1.30)
L.Payoff		-0.67***	-0.67***		-0.24***	-0.24***
		(0.06)	(0.06)		(0.07)	(0.07)
Risk[1]		0.60***	0.60***		0.11	0.11
		(0.19)	(0.19)		(0.21)	(0.21)
Gender		-0.32	-0.32		-0.23	-0.23
		(0.80)	(0.80)		(1.13)	(1.13)
Self evaluation on math capabilities[2]		-1.17*	-1.17*		-3.41***	-3.41***
		(0.67)	(0.67)		(1.05)	(1.05)
Do you like betting?[3]		0.20	0.21		0.17	0.17
		(0.79)	(0.79)		(1.14)	(1.14)
Constant	-3.23***	2.60	2.23	-0.77	7.17**	8.07**
	(0.62)	(2.36)	(2.38)	(0.68)	(2.93)	(3.32)
Constant(σ_u)	4.96***	3.41***	3.40***	4.16***	3.27***	3.27***
	(0.47)	(0.35)	(0.35)	(0.56)	(0.46)	(0.46)
Constant(σ_e)	3.92***	3.62***	3.62***	2.51***	2.49***	2.49***
	(0.12)	(0.11)	(0.11)	(0.10)	(0.10)	(0.10)
Observations	1824	1824	1824	912	912	912
ρ	0.61	0.47	0.47	0.73	0.63	0.63
N_{lc}	1101	1101	1101	488	488	488
N_{rc}	71	71	71	18	18	18
σ_e	3.92	3.62	3.62	2.51	2.49	2.49
σ_u	4.96	3.41	3.40	4.16	3.27	3.27

Notes: Panel tobit regressions with random effects, censored at 0 and 10. Stage 1 from all sessions are included in this analysis.
[1] The risk is measured from 0 (max. risk averse) to 10 (max. risk lover).
[2] In the questionnaire we asked "How do you consider your math capabilities?" The answers were Lower than Average (10.42 %), On Average (66.67%), Higher than the Average (22.92%)
[3] Dummy variable for Yes and No, in the questionnaire we asked "Do you like to bet?"
(* 0.1, ** 0.05, *** 0.01)

AW outcomes are weakly affecting individual's choices. Participants informed on winning probabilities in Stage 2A, regardless their risk attitude, are immune to AW bias in particular they tend to play even more safely in the investment game framing. Informing them on the market probabilities shows them the real nature of the investment game, just related with luck rather than knowledge on financial market. In Stage 2B, informing participants about the independence of rounds helps the risk-averse subsample to change their behavior toward AW outcomes; instead risk-seekers and risk-lovers positively and significantly allocate more tokens to the risky option after seeing the AW bias in the IG framing. This result is very surprising, as individuals showing risk averse preferences were more likely to be affected by AW outcomes, but nudging helps them to correctly consider the past round as an independent event. At the same time, risk-neutral and risk-seeking individuals decrease significantly the tokens allocated compared to the first stage, but it is more likely to incur in the misrepresentation of AW outcomes. When individuals play safer, the AW becomes more relevant.

Table 5.7: Almost-Winning effect on token's allocation in Stage 2A and 2B, by risk preferences

	Token allocation in risky choice, Stage 2 and Stage 3											
	Stage 2A						Stage 2B					
	Risk Averse			Others			Risk Averse			Others		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)
L.AW	0.34	-0.25		0.54	0.18		0.07	0.56		0.76**	0.55*	
	(0.52)	(0.46)		(0.34)	(0.33)		(0.38)	(0.40)		(0.31)	(0.33)	
Tr IG		-2.79*			0.61			0.49			0.98	
		(1.46)			(1.04)			(2.27)			(1.47)	
TrSM*L.AW=0			2.93*			-0.74			-1.19			-1.46
			(1.59)			(1.12)			(2.29)			(1.52)
TrIG*L.AW=0			-0.04			-0.31			-0.23			-0.71*
			(0.71)			(0.39)			(0.47)			(0.39)
TrSM*L.AW=1			2.49			-0.83			0.10			-1.23
			(1.57)			(1.10)			(2.28)			(1.51)
Round	0.16***	0.11***	0.11***	0.06**	0.04	0.04	-0.03	-0.01	-0.01	-0.04	-0.03	-0.03
	(0.04)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
L.Winning * L.Payoff		0.62***	0.63***		0.63***	0.63***		0.70***	0.70***		0.17	0.18
		(0.20)	(0.20)		(0.10)	(0.10)		(0.22)	(0.22)		(0.13)	(0.13)
L.Winning		-7.66***	-7.69***		-6.34***	-6.38***		-6.25**	-6.19**		-2.48	-2.54
		(2.28)	(2.29)		(1.31)	(1.32)		(2.62)	(2.60)		(1.60)	(1.61)
L.Payoff		-0.74***	-0.74***		-0.66***	-0.66***		-0.39***	-0.40***		-0.14*	-0.15*
		(0.13)	(0.13)		(0.07)	(0.07)		(0.11)	(0.11)		(0.08)	(0.08)
Male		0.30	0.34		-0.07	-0.08		-0.30	-0.29		-0.76	-0.73
		(1.32)	(1.32)		(0.94)	(0.93)		(1.63)	(1.61)		(1.30)	(1.30)
Self evaluation on math capabilities[1]		-1.42	-1.41		-0.96	-0.97		-1.62	-1.60		-5.32***	-5.30***
		(1.04)	(1.04)		(0.80)	(0.80)		(1.71)	(1.68)		(1.26)	(1.26)
Do you like betting?[2]		2.00	2.02		-0.26	-0.25		-0.96	-0.95		2.30*	2.28*
		(1.43)	(1.43)		(0.93)	(0.92)		(2.05)	(2.02)		(1.29)	(1.29)
Constant	-5.59***	5.80**	2.90	-1.94***	6.41***	7.29***	-1.73*	6.11	6.96	0.08	8.48***	10.08***
	(1.05)	(2.88)	(3.27)	(0.71)	(2.46)	(2.29)	(0.97)	(5.07)	(6.13)	(0.89)	(2.69)	(3.04)
Constant(σ_u)	4.16***	2.73***	2.74***	4.76***	3.45***	3.44***	3.69***	3.06***	3.01***	4.12***	2.79***	2.78***
	(0.79)	(0.57)	(0.57)	(0.52)	(0.42)	(0.42)	(0.83)	(0.72)	(0.71)	(0.70)	(0.50)	(0.50)
Constant(σ_e)	3.18***	2.77***	2.77***	4.10***	3.82***	3.82***	2.41***	2.33***	2.32***	2.55***	2.53***	2.53***
	(0.21)	(0.19)	(0.19)	(0.15)	(0.14)	(0.14)	(0.16)	(0.15)	(0.15)	(0.12)	(0.12)	(0.12)
Observations	570	570	570	1254	1254	1254	399	399	399	513	513	513
ρ	0.63	0.49	0.49	0.57	0.45	0.45	0.70	0.63	0.63	0.72	0.55	0.55
N_{lc}	420.00	420.00	420.00	681.00	681.00	681.00	251.00	251.00	251.00	237.00	237.00	237.00
N_{rc}	8.00	8.00	8.00	63.00	63.00	63.00	4.00	4.00	4.00	14.00	14.00	14.00
σ_e	3.18	2.77	2.77	4.10	3.82	3.82	2.41	2.33	2.32	2.55	2.53	2.53
σ_u	4.16	2.73	2.74	4.76	3.45	3.44	3.69	3.06	3.01	4.12	2.79	2.78

Notes: Panel tobit regressions with random effects, censored at 0 and 10. Stage 1 from all sessions are included in this analysis.

[1] In the questionnaire we asked "How do you consider your math capabilities?" The answers were Lower than Average (10.42 %), On Average (66.67%), Higher than the Average (22.92%)

[2] Dummy variable for Yes and No, in the questionnaire we asked "Do you like to bet?"

(* 0.1, ** 0.05, *** 0.01)

5.3.4. Robustness Check

Table 5.8: Robustness check by framing, information and AW

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Token allocation in risky choice								
	Stage 1			Stage 2A			Stage 2B		
	All	RA	RN+RS	All	RA	RN+RS	All	RA	RN+RS
	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)	β/(se)
L.AW	0.31	0.55*	0.17	0.05	-0.25	0.18	0.54**	0.56*	0.55
	(0.19)	(0.30)	(0.25)	(0.26)	(0.37)	(0.30)	(0.24)	(0.34)	(0.34)
Tr IG	0.50**	-1.10***	1.25***	-0.54**	-2.79***	0.61*	0.38	0.49	0.98***
	(0.20)	(0.42)	(0.24)	(0.27)	(0.70)	(0.35)	(0.26)	(0.54)	(0.37)
Round	-0.12***	-0.12***	-0.12***	0.06***	0.11***	0.04	-0.03	-0.01	-0.03
	(0.01)	(0.03)	(0.02)	(0.02)	(0.04)	(0.03)	(0.02)	(0.03)	(0.03)
L.Winning * L.Payoff	0.40***	0.79***	0.27***	0.64***	0.62**	0.63***	0.27	0.70*	0.17
	(0.08)	(0.16)	(0.10)	(0.13)	(0.27)	(0.15)	(0.17)	(0.38)	(0.19)
L.Winning	-4.23***	-8.52***	-2.81**	-6.98***	-7.66***	-6.34***	-2.34	-6.25	-2.48
	(1.03)	(1.80)	(1.26)	(1.43)	(2.84)	(1.66)	(1.93)	(4.66)	(2.15)
L.Payoff	-0.34***	-0.52***	-0.28***	-0.67***	-0.74***	-0.66***	-0.24**	-0.39*	-0.14
	(0.05)	(0.10)	(0.06)	(0.09)	(0.21)	(0.10)	(0.11)	(0.24)	(0.13)
Risk[1]	0.35***			0.60***			0.11**		
	(0.04)			(0.07)			(0.05)		
Male	-0.00	0.32	0.04	-0.32	0.30	-0.07	-0.23	-0.30	-0.76**
	(0.17)	(0.30)	(0.21)	(0.25)	(0.51)	(0.30)	(0.26)	(0.41)	(0.37)
Self evaluation on math capabilities[2]	-1.61***	-0.85***	-1.89***	-1.17***	-1.42***	-0.96***	-3.41***	-1.62***	-5.32***
	(0.18)	(0.29)	(0.21)	(0.24)	(0.48)	(0.31)	(0.35)	(0.37)	(0.65)
Do you like betting?[3]	0.78***	1.71***	0.65***	0.20	2.00**	-0.26	0.17	-0.96*	2.30***
	(0.18)	(0.41)	(0.20)	(0.26)	(0.93)	(0.30)	(0.30)	(0.55)	(0.51)
Constant	4.65***	5.31***	7.18***	2.60**	5.80**	6.41***	7.17***	6.11***	8.48***
	(0.69)	(1.24)	(0.65)	(1.27)	(2.38)	(1.28)	(0.90)	(2.31)	(0.77)
Constant(σ_u)	3.01***	2.67***	3.03***	3.41***	2.73***	3.45***	3.27***	3.06***	2.79***
	(0.14)	(0.25)	(0.17)	(0.24)	(0.51)	(0.29)	(0.27)	(0.48)	(0.31)
Constant(σ_e)	3.54***	3.21***	3.66***	3.62***	2.77***	3.82***	2.49***	2.33***	2.53***
	(0.11)	(0.18)	(0.12)	(0.17)	(0.25)	(0.20)	(0.17)	(0.30)	(0.19)
Observations	2736	969	1767	1824	570	1254	912	399	513
ρ	0.42	0.41	0.41	0.47	0.49	0.45	0.63	0.63	0.55
N_{lc}	1126.00	502.00	624.00	1101.00	420.00	681.00	488.00	251.00	237.00
N_{rc}	183.00	42.00	141.00	71.00	8.00	63.00	18.00	4.00	14.00
σ_e	3.54	3.21	3.66	3.62	2.77	3.82	2.49	2.33	2.53
σ_u	3.01	2.67	3.03	3.41	2.73	3.45	3.27	3.06	2.79

Notes: Estimation bootstrapping with 999 repetition, standard errors in parenthesis.
Panel tobit regressions with random effects, censored at 0 and 10. Stage 1 from all sessions are included in this analysis.
[1] The risk is measured from 0 (max. risk averse) to 10 (max. risk lover).
[2] In the questionnaire we asked "How do you consider your math capabilities?" The answers were Lower than Average (10.42 %), On Average (66.67%), Higher than the Average (22.92%)
[3] Dummy variable for Yes and No, in the questionnaire we asked "Do you like to bet?"
(* 0.1, ** 0.05, *** 0.01) (* 0.1, ** 0.05, *** 0.01)

In order to provide a robust test of our results on framing, AW outcomes and information, without relying on distributional assumptions, we adopt the Bootstrap procedure. Table 5.8 proposes a summary of the analysis carried out.

Results from Table 5.8 confirm our previous analysis and some additional considerations might be done. AW outcomes are affecting particularly risk-averse subjects in Stage 1, while the coefficients from Stage 2A confirm that the effect completely disappear. In Stage 2B, AW effect is statistically significant for all individuals, confirming that nudging is not the best option to teach individuals how to face AW bias.

Thank to the Bootstrap method we find a significant trend toward framing: Risk-averse individuals tend to invest more in the SM game, while risk-loving and risk-neutral individuals invest 1.25 tokens more (on average) on the risky choice when playing the investment game. This last trend is weakened by information stages. Finally, self evaluation on math capabilities are always negatively related to higher allocations of tokens to the risky choice, while people who like betting tend to play more in Stage 1.

5.4. Final Remarks

This work started as an attempt to study AW biases considering different frames and information, and the analysis got even more interesting when results pointed out that the AW bias was strengthened by these additional elements. We show through this experiment that the effect of near-misses is not only related to compulsive gamblers and with pure gambling environments, it is also related to in general agents who do not apply their knowledge of randomness in a game of chance and therefore develop irrational thinking. Our result underlines that individuals are more confident in investing larger shares of their endowment in the investment game, either driven by a possible bias which increased their overconfidence or they could have different sensation-seeking and arousal depending on the game proposed (Anderson and Brown 1984, Ladouceur et al. 1991, Odean 1998). As described by Langer (1975), in skill situations people try to behave as if they are maximizing the probability of success; choosing the strategy which should lead to the best outcome is a primary component of the skill game, and those related game skills may be responsible for the illusion of control.

In this sense, individuals playing in the IG framing are overestimating their probability of success, given that the financial market is mostly associated to investment abilities, but their bias toward AW is similar both in slot machine and investment game framings. When we test the role of information on AW effect we conclude that providing different information promotes agents' rational behavior, in particular winning probabilities help them to correctly perceive the cognitive bias related to almost winning outcomes and it induces a rational and safer behavior in participants right after informing them. Warning them on the cognitive bias due to AW outcomes induces a decreasing level of betting throughout the rounds, but this passage is smoother compared to individuals aware of the winning probability.

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Appendix D

Translated Instructions

Welcome to our experiment!

You are participating in an experiment in which you will make several economic decisions.

We are interested only to your decisions that will remain completely anonymous: this means that the experimenters will not be able to associate any decisions to your name.

These decisions will directly affect your payment for the experiment. At the end og the experiment you will be paid cash privately. In particular, in each stage of the experiment you will gain several ECU (Experimental Currency Units) that will be exchanged at the following rate:

1 ECU = 0.5 €

In this experiment you will take your decisions in different situations that we call "Stages".

The experiment consists of three Stages and a final Questionnaire.

Each Stage includes different rounds. Each decision that you will make and the result obtained in each Stage is independent from the others; this means that decisions taken in a Stage do not affect your results in any other Stage of the experiment.

At the end of the experiment, the computer randomly selects one round for each Stage and you will be paid the sum of the payoffs you realized in each round randomly selected.

In the following Instructions we will explain in details your task in each Stage.

After reading aloud the Instructions you will have some time to read them on your own. If you have any doubt please raise your hand and wait: one experimenter will come and help you individually as soon as she can. During the experiment work in silence and do not disturb other participants.

Enjoy

STAGE 1, Investment Game

In this Stage you have to decide how to invest your initial endowment for 20 rounds.

At the beginning of each round you will be endowed with 10 ECU and you have to allocate them between a risk free investment and a risky investment, based on a portfolio of three shares of three firms operating in independent markets, namely Microlift, Chip Corporation and Dolltech that DO NOT exist in the reality.

The risk free investment gives a zero net gain return, i.e. what you will invest in it will be entirely repaid to you no matter the trend of the three markets will be.

Your gain from the risky investment depends on the trend of the three markets. In particular, when all the markets are characterized by a bullish trend your initial investment is tripled. Whenever occurs one bear market you will experience a loss and your investment gain will be equal to 1/10 of your initial investment.

Assume, as an example, that in a given round you decide to invest 5 ECU in the risk free investment and 5 ECU in the risky investment. Your actual gain from this round will be determined by the trend of the three markets and by the amount of your endowment allocated to each investment. Since the repayment factor of the risk free investment is 1, you will gain 5 ECU for sure plus 15 ECU from the risky investment if all the 3 markets will have a positive trend. On the contrary if one or more of the markets will have a doom your gain will be: 5 ECU from the risk free investment plus 0.5 ECU from the risky investment.

To illustrate your choice task, look at Figure D.1 where 5 ECU are invested in the risky portfolio represented by the scroll bar cursor. The potential gains with three bullish markets is represented in the green box; whether one market does not have a bullish trend, potential gains are represented in the red box.

Figure D.1: Screenshot IG

Remember, you can move the cursor of the scroll bar to know all possible outcomes before confirming your final decision: you can check the potential results of your investment decision by scrolling the cursor on the bar. When you decide your preferred allocation, click OK.

When you confirm the investment allocation, the computer will show the three markets result. Figure D.2 and Figure D.3 show two possible examples of the result shown on the screen. The final result of your investment is computed below the market outcomes, and it computes the ECU gained in that round.

Figure D.2: Screenshot IG

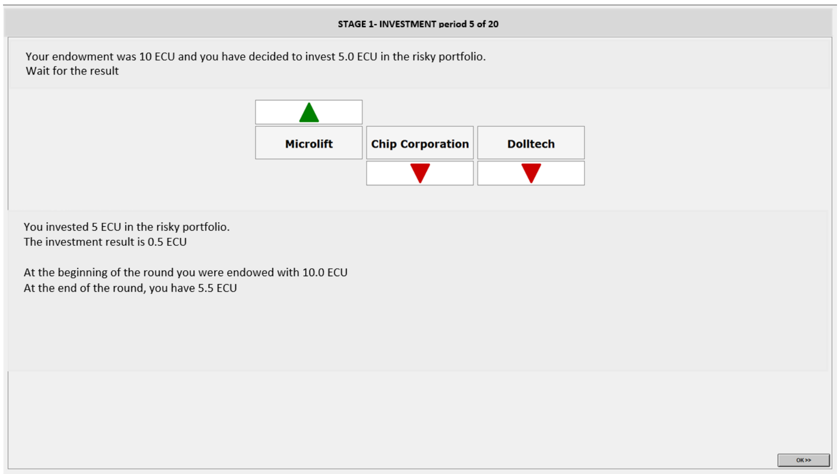
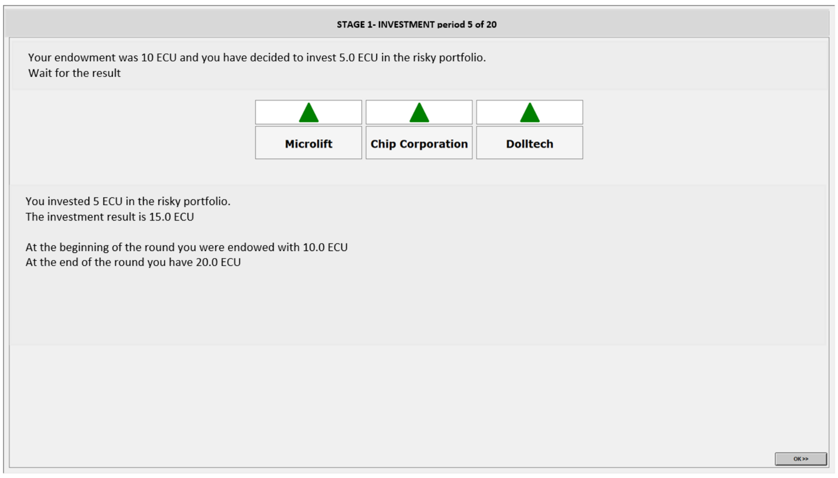


Figure D.3: Screenshot IG



In each round of Stage 1 you will receive the same initial endowment, i. e. in each of them you will have always 10 ECU to invest.

At the end of Stage one you will have also to answer to a question based on the 20 round played that will give you the possibility to gain 4 extra ECU.

At the end of the experiment the computer will randomly select just one round of Stage 1 and your actual gain from Stage 1 will be given by the amount of ECU you realized in that round plus 4 Ecu depending on how you answered to the final question on Stage 1.

STAGE 1, Slot Machine

In this Stage you have to decide how to bet your initial endowment for 20 rounds.

At the beginning of each round you will be endowed of 10 ECU and you will have to decide how much of them to bet on a slot machine or to keep in your pockets. This is a one row slot machine with three cells. The possible outcomes from the slot machine are the following: “BAR”, “Cherry”, “Lemon”, “Pear”, “Strawberry”.

Your gain from bidding depends on the occurrence of the “BAR”. In particular, when “BAR” occurs in all the cells your bet is tripled. Whenever occurs one or more cells different from “BAR”, you will experience a loss and your gain will be equal to 1/10 of your initial bet.

At the beginning of each round you will be asked how much you want to bet; for any possible choice (moving the cursor of the scrolling bar in Figure D.4) the computer will show you in the green box your potential gain in case of winning (the occurrence of three “BAR”) and your potential gain in RED box oherwise.

Figure D.4: Screenshot SM

When you confirm the investment allocation, the computer will show you the slot machine.

Assume, as an example, that in a given round you decide to bet 5 ECU in the slot machine and to keep the remaining 5 ECU of your endowment in your pocket. Your actual gain from this round will be determined by the 5 ECU you are not betting and by the slot machine result.

If 3 “BAR” will occur, as illustrate in Figure D.5, your payoff for the round will include the 5ECU you did not bet and 15 ECU from your bet that has been tripled. Otherwise, if one or more than one icons will be different from “BAR” your payoff for the round will be equal to the sum of the 5 ECU you did not bet plus 0.5 ECU (that is 1/10 of your initial bet), as shown in Figure D.6.

Figure D.5: Screenshot SM

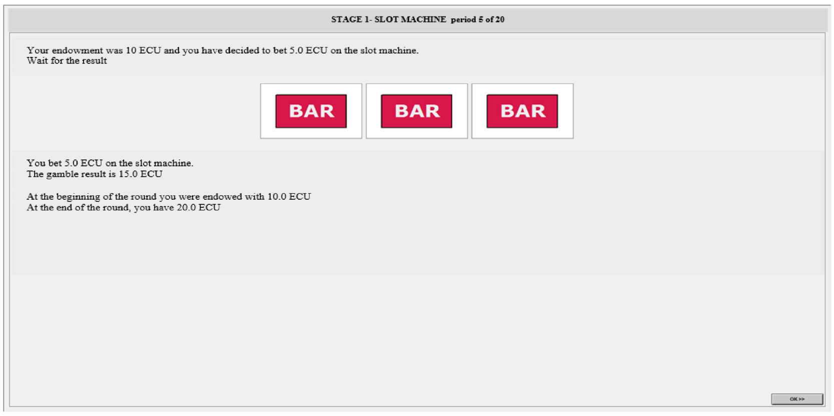
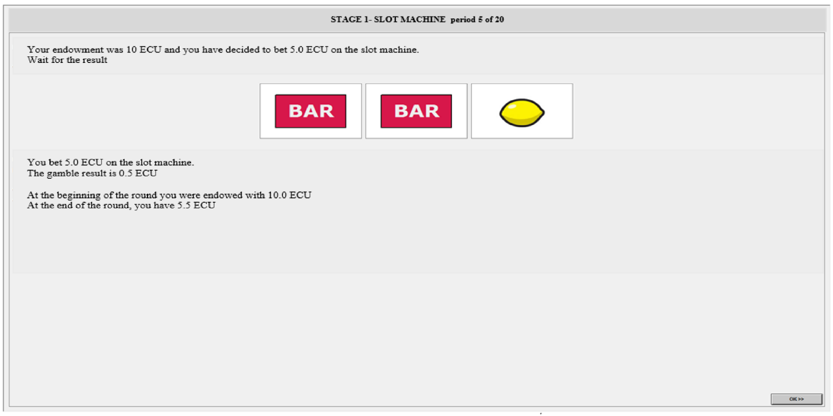


Figure D.6: Screenshot SM



Remember, you can move the cursor of the scroll bar to know all possible outcomes before confirming your final decision: you can check the potential results of your betting decision by scrolling the cursor on the bar. When you decide your preferred allocation, click OK.

In each round of Stage 1 you will receive the same initial endowment, i. e. in each of them you will have always 10 ECU to bet.

At the end of Stage one you will have also to answer to a question based on the 20 round played that will give you the possibility to gain 4 extra ECU.

At the end of the experiment the computer will randomly select just one round of Stage 1 and your actual gain from Stage 1 will be given by the amount of ECU you realized in that round plus 4 Ecu depending on how you answered to the final question on Stage 1.

STAGE II, Investment Game (Stage 2A, Probability)

Stage 2 presents the same structure of Stage 1. During this Stage you will take the same investment decisions between a risk free and a risky investment with an initial endowment of 10 ECU for 20 rounds.

Additionally to the information of Stage 1, in this Stage 2 the computer communicates you the probability that the three markets have a bullish trend for all rounds.

At the end of the experiment the computer will randomly select just one round of Stage 2 for payment and your actual gain for this Stage will be determined by the amount of ECU you realized in the selected round.

STAGE II, Investment Game (Stage 2B, Warning)

Stage 2 presents the same structure of Stage 1. During this Stage you will take the same investment decisions between a risk free and a risky investment with an initial endowment of 10 ECU for 20 rounds.

Additionally to the information of Stage 1, in this Stage 2 the computer communicates you a warning message related to the probability that the three markets have a bullish trend in the following round.

At the end of the experiment the computer will randomly select just one round of Stage 2 for payment and your actual gain for this Stage will be determined by the amount of ECU you realized in the selected round.

STAGE II, Slot Machine (Stage 2A, Probability)

Stage 2 presents the same structure of Stage 1. During this Stage you will asked to bid in a slot machine with an initial endowment of 10 ECU for 20 rounds.

Additionally to the information of Stage 1, in this Stage 2 the computer communicates you the actual probability of the occurrence of 3 “BAR” for all rounds.

At the end of the experiment the computer will randomly select just one round of Stage 2 for payment and your actual gain for this Stage will be determined by the amount of ECU you realized in the selected round.

STAGE II, Slot Machine (Stage 2B, Warning)

Stage 2 presents the same structure of Stage 1. During this Stage you will asked to bid in a slot machine with an initial endowment of 10 ECU for 20 rounds.

Additionally to the information of Stage 1, in this Stage 2 the computer communicates you a warining message related to the probability that three “BAR” occur in the following round.

At the end of the experiment the computer will randomly select just one round of Stage 2 for payment and your actual gain for this Stage will be determined by the amount of ECU you realized in the selected round.

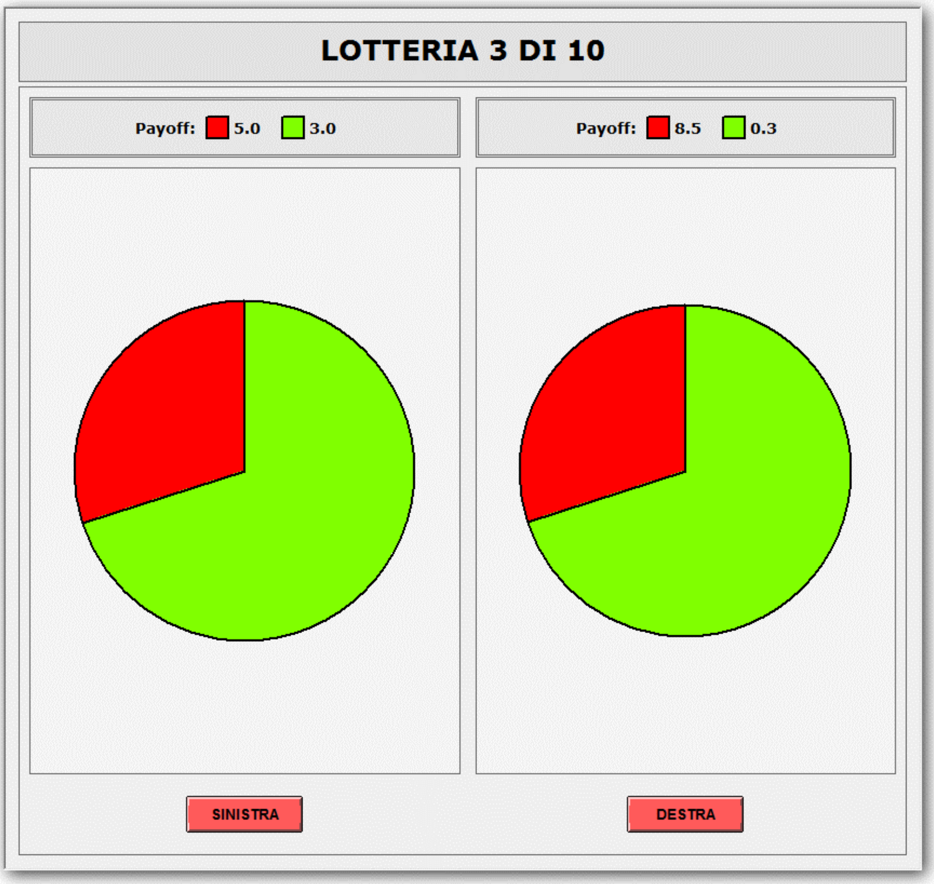
STAGE III

In this Stage you wil be asked to choose between lotteries with different prizes and chances of winning. You will be presented with a series of 10 lotteries where you will make choice between pairs of them.

For each pair of lotteries, you should indicate which of the two lotteries you prefer to play. You will actually get the chance to play one of the lotteries you choose, and will be paid according to the outcome of that lottery, so you should think carefully about which lotteries you prefer.

Figure 1ìD.7is an example of what the computer display of such a pair of lotteries will look like. The display on your screen will be bigger and easier to read.

Figure D.7: Lottery



Each lottery assigns a given probability (indicated by the corresponding slice) to win 4 different prizes, respectively: 0.3 ECU, 3 ECU, 5 Ecu and 8.5 ECU represented by the area of the corresponding colour that will remain the same during the 10 rounds.

In the above example the probability that the LEFT lottery pays 3 ECU is associated to the green colour area; the same probability is associated in the RIGHT lottery to a prize of 0.3 ECU. The probability that the lottery on the LEFT (RIGHT) pays respectively 5 ECU (8.5 ECU) is associated to the RED area.

Each pair of lotteries is shown on a separate screen on the computer. On each screen, you should indicate which of the lotteries you prefer to play by clicking on one of the two boxes beneath the lotteries. You should click the LEFT box if you prefer the lottery on the left, the RIGHT box if you prefer the lottery on the right.

Be careful: You should approach each pair of lotteries as if it is the one out of the 10 that you will play out, since at the end of the experiment the computer will randomly select one of the 10 rounds and you will play for real the lottery you selected in that round.

After you have worked through all of the pairs of lotteries, wait in silence that all participants end that Stage too. There is no reason to rush into, we will wait for everyone taking his/her choices.

At the end of the experiment the computer will select one of the 10 rounds and it will play for real the lottery that you have chosen in that run: a spinning device will appear on your screen on a “wheel of fortune”. Note that each round has the same probability to be selected. Once the computer will select the round to be implemented on your screen will appear the corresponding lottery pair with in evidence the one you choose in that round.

Assume, for the sake of an example, that you preferred, as shown above, the lottery on your LEFT. On it will appear a random device:

- if it will stops in the GREEN AREA you will gain 3 ECU for this Stage;
- if it will stops in the RED AREA you will gain 5 ECU for this Stage.

Summing up your payoff for this Stage is determined by 3 elements:

- which of the 10 rounds will be selected for payment;
- which lottery you preferred in that round (LEFT or RIGHT);
- the result of the random draws in the selected lottery.

YOUR PAYOFF FROM THE EXPERIMENT

Your final payoff from the experiment is given by the sum of the payoffs you gained in each Stage of the experiment More in details:

- your gain for the randomly selected round in Stage 1;
- 4 Ecu in case you answered correctly to the question proposed at the end of Stage 1 and 0 ECU otherwise;
- your gain for the randomly selected round in Stage 2;
- your gain for the randomly selected lottery in Stage 3.

D.1. Maximization Problem

We represent the utility function as:

$$U(x_1,h,l,p_h,p_l,e) = p_h \cdot u(h\cdot x_1) + p_l \cdot u(l\cdot x_1) + u(e-x_1) \tag{D.1}$$

We keep general risk preference for the maximization, and the following maximization problem is:

$$\begin{aligned} &\max_{x_1} p_h u(h\cdot x_1) + p_l u(l\cdot x_1) + u(e-x_1) \\ &\text{s.t. } x_1 \geq 0 \\ &e - x_1 \geq 0 \end{aligned} \tag{D.2}$$

and the lagrange form with $\lambda_1 \geq 0$ and $\lambda_2 \geq 0$:

$$L(x_1,\lambda_1,\lambda_2) = p_h u(h\cdot x_1) + p_l u(l\cdot x_1) + u(e-x_1) + \lambda_1(x_1) + \lambda_2(e-x_1) \tag{D.3}$$

where FOC:

$$\frac{\partial L}{\partial x_1} = 0 \tag{D.4}$$

$$p_h h\cdot u'(hx_1) + p_l l\cdot u'(lx_1) - u'(e-x_1) + \lambda_1 - \lambda_2 = 0$$

Then, we optimize for the different values assumed by λ_1 and λ_2

1. When $\lambda_1 = \lambda_2 = 0$, $x_1 > 0$ and $e - x_1 > 0$

$$p_h h\cdot u'(hx_1) + p_l l\cdot u'(lx_1) = u'(e-x_1) \tag{D.5}$$

$$p_h h\cdot u'(hx_1) + (1-p_h) l\cdot u'(lx_1) = u'(e-x_1) \tag{D.6}$$

$$p_h(h\cdot u'(hx_1) - l\cdot u'(lx_1)) = u'(e-x_1) - l\cdot u'(lx_1) \tag{D.7}$$

$$p_h = \frac{u'(e-x_1) - l\cdot u'(lx_1)}{(h\cdot u'(hx_1) - l\cdot u'(lx_1)} \tag{D.8}$$

2. When $\lambda_1 > 0$ and $\lambda_2 = 0$, $x_1 = 0$

$$p_h h \cdot u'(hx_1) + p_l l \cdot u'(lx_1) + \lambda_1 = u'(e - x_1) \tag{D.9}$$

$$p_h = \frac{u'(e - x_1) - l \cdot u'(lx_1) - \lambda_1}{h \cdot u'(hx_1) - l \cdot u'(lx_1)} \tag{D.10}$$

3. When $\lambda_1 = 0$ and $\lambda_2 > 0$ implies $x_1 = e$

$$p_h h \cdot u'(hx_1) + p_l l \cdot u'(lx_1) - \lambda_2 = u'(e - x_1) \tag{D.11}$$

$$p_h = \frac{u'(e - x_1) - l \cdot u'(lx_1) + \lambda_2}{h \cdot u'(hx_1) - l \cdot u'(lx_1)} \tag{D.12}$$

When we consider the linear case for risk neutral individuals, we obtain:

1. When $\lambda_1 = \lambda_2 = 0$

$$p_h = \frac{1 - l}{h - l} \tag{D.13}$$

Then we know that $p_h < 1$, then:

$$\frac{1 - l}{h - l} < 1 \tag{D.14}$$

$$h > 1 \tag{D.15}$$

In this case each allocation of x_1 would be the optimal choice.

2. When $\lambda_1 > 0$ and $\lambda_2 = 0$

$$p_h = \frac{1 - l - \lambda_1}{h - l} \tag{D.16}$$

where $\lambda_1 > 1 - h$

3. When $\lambda_1 = 0$ and $\lambda_2 > 0$

$$p_h = \frac{1 - l + \lambda_2}{h - l} \tag{D.17}$$

where $\lambda_2 < 1 - h$.

D.2. Figures and Tables

Table D.1: Elicited beliefs on probabilities by Stage and framing

Guessed probability of winning				
	Slot Machine	Investment Game	SM+IG	WRT
Stage 2A	14.029	16.392	15.210	0.817
	(24)	(24)	(48)	
Stage 2B	17.158	12.983	15.071	0.157
	(12)	(12)	(24)	
Stage 2 (A+B)	15.072	15.256	15.164	0.305
	(36)	(36)	(72)	
WRT	0.176	0.758	0.488	

Notes: All tests reported are two-sided Wilcoxon Rank Sum Test (WRT). In parenthesis the number of observations. We consider only sessions were AW were forced and so more frequent.

Figure D.8: Allocation of tokens, risk seeking individuals, Random AW

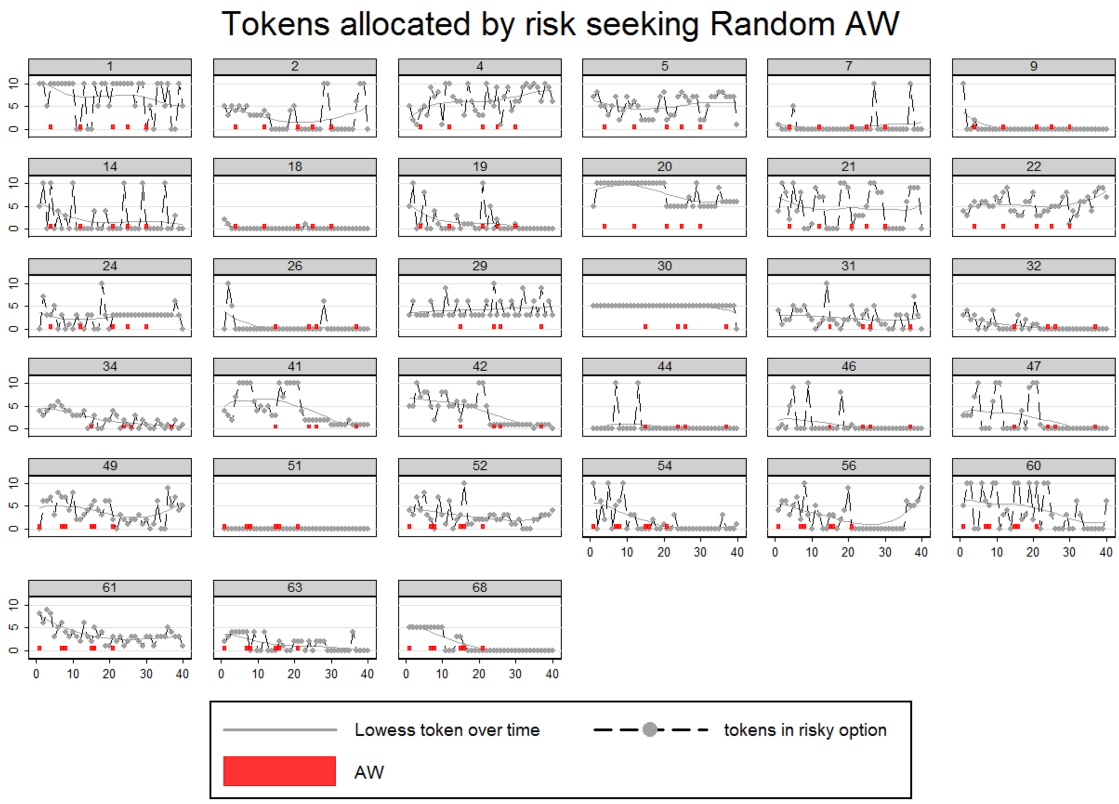


Figure D.9: Allocation of tokens, risk seeking individuals, Forced AW

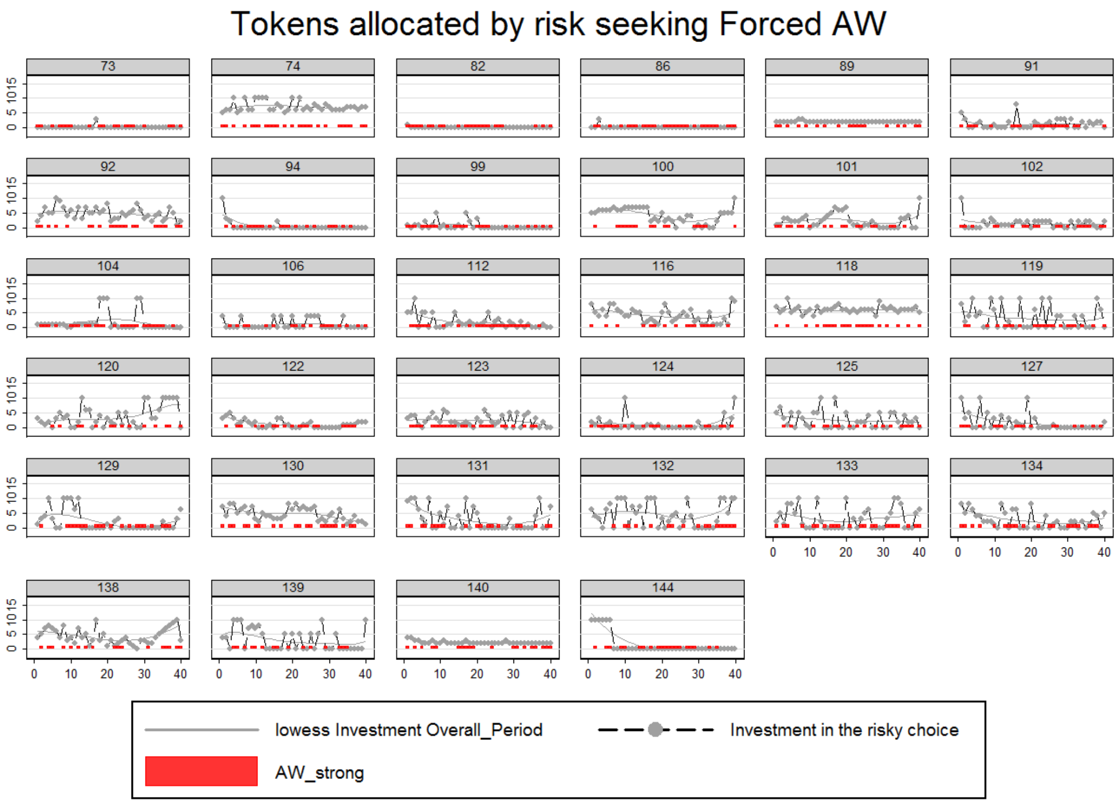


Figure D.10: Allocation of tokens, risk neutral individuals, Random AW

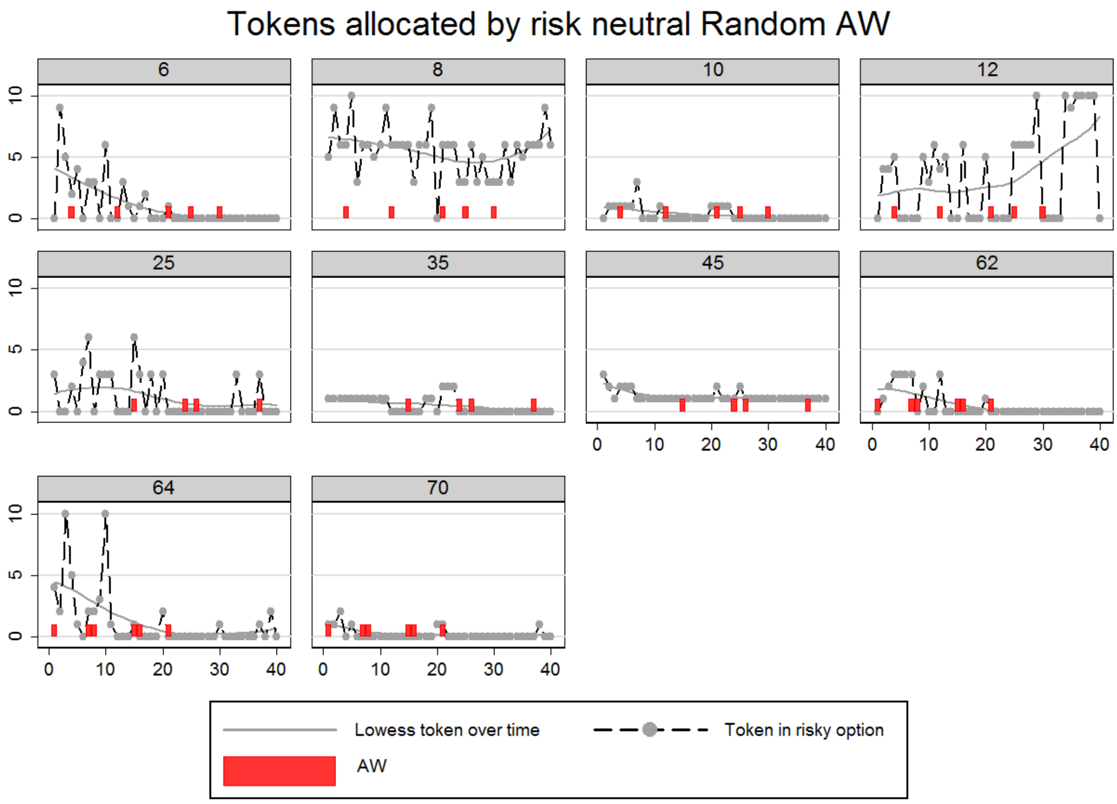


Figure D.11: Allocation of tokens, risk neutral individuals, Forced AW

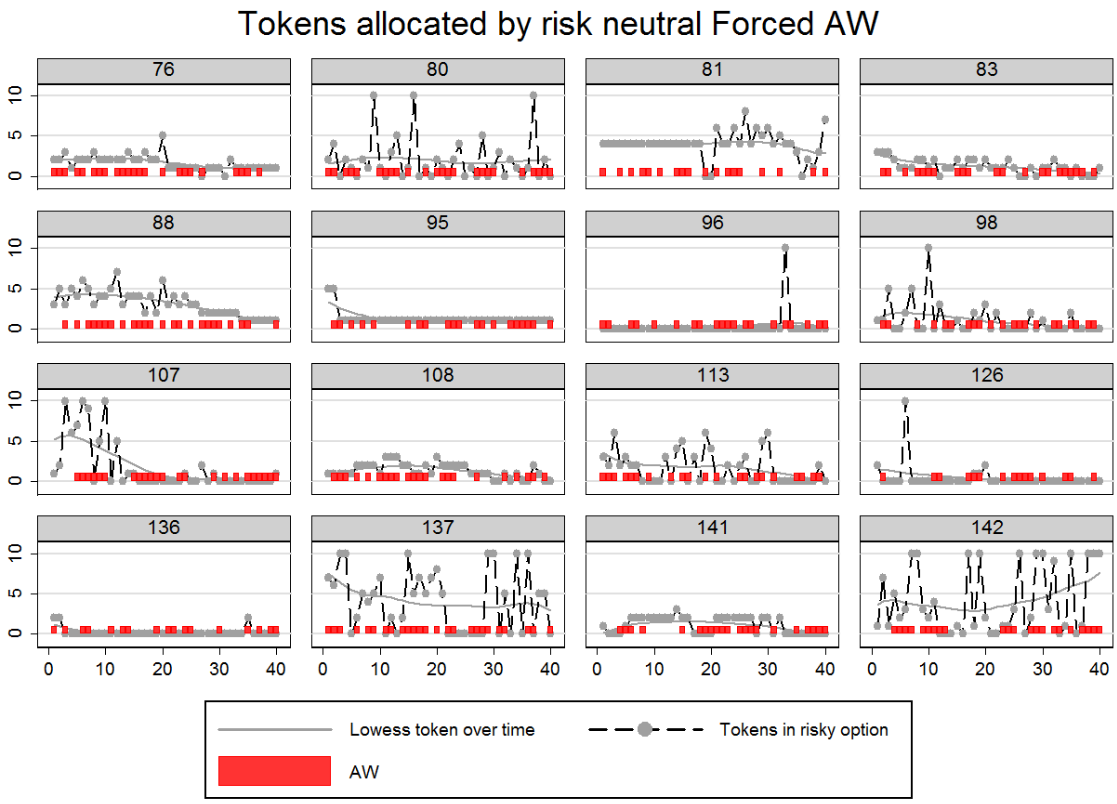


Figure D.12: Allocation of tokens, risk averse individuals, Random AW

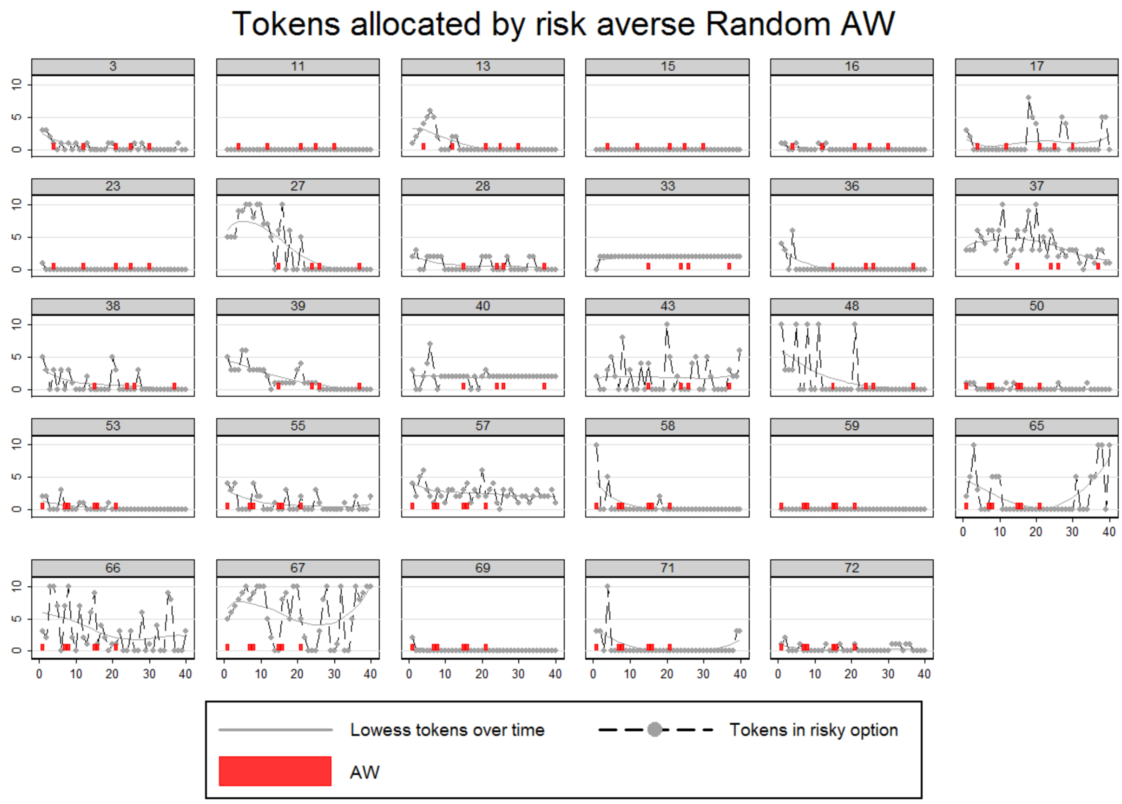


Figure D.13: Allocation of tokens, risk averse individuals, Forced AW