“Networks, Social Information and Compliance”

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Thesis submitted for the degree of Doctorate of Philosophy

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Abstract

The work developed in my PhD on "Networks, Social Information and Compliance" focuses on compliance (voluntary and non), diffusion of information and spillovers in diverse network structures. More specifically, we test experimentally how minimal social information is diffused through different network structures and its role on increasing the level of efficiency along with its positive effect on voluntary compliance of emergent social norms and tax compliance. In the first two chapters, we implement a networked version of the trust game with two senders and one receiver. We manipulate in a minimal way the social information available in the network and, the novelty of our work consists in the introduction of non-binding suggestion about the level of trust and trustworthiness, which is totally fair and (partially) efficient. It is also manipulated in the two studies the selection mechanism of the roles in the game by introducing social status. Our findings suggest that social information has a positive and significant effect on increasing the level of trust in the network. The non-binding suggestion has also a positive and significant effect on individual decisions. In the last chapter, we study in a laboratory experiment how tax compliance information is diffused in a fixed-six-nodes circle network. The game has four information conditions: No Info, Full Info, Positive Info and Negative Info. In the No info treatment, subjects get individual information about whether they were audited, the outcome of it and her final payoff. In the Positive Info (Negative Info) treatment, participants get information whether adjacent connected nodes were audited and found compliant (noncompliant). In the Full Info, participants get both positive and negative signals. We control for the effect of signals on participants' beliefs on the ex-ante fixed and unknown audit probability by an incentive compatible mechanism. The tax rate and fine rate are fixed and known by the subjects. Our findings suggest that positive and negative signals have a significant effect in the levels of reporting and compliance at individual level. Indeed, diffusion of non-strong negative signal (one bad example) has a negative effect on individuals' tax compliance. The diffusion of strong positive signals (two good examples) is required to generate any increase in compliance decisions within networks.

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Introduction

The following thesis developed during my PhD studies on "Networks, Social Information and Compliance" focuses on compliance (voluntary and non), diffusion of information and spillovers in diverse network structures. More specifically, we test experimentally how minimal information travels through different network structures and its role on increasing the level of efficiency along with its positive effect on voluntary compliance of emergent social norms and tax compliance. This is particularly important because firms or individuals do not make decisions in isolation, but instead they take into account the behavior and experiences of their rivals or partners in the same network.

In the first chapter of this dissertation, we conduct a controlled laboratory experiment to investigate trust and trustworthiness in a networked investment game in which two senders interact with a receiver. We investigate to what extent senders and receivers comply with an exogenous and non-binding recommendation. We also manipulate the level of information available to senders regarding receiver's behavior in the network. We compare a baseline treatment in which senders are only informed about the actions and outcomes of their own investment games to two information treatments. In the reputation treatment, senders receive *ex ante* information regarding the average amount returned by the receiver in the previous period. In the transparency treatment, each sender receives *ex post* additional information regarding the returning decision of the receiver to the other sender in the network. Across all treatments and for both senders and receivers, the non-binding rule has a significant and positive impact on individual decisions. Providing senders with additional information regarding receiver's behavior affects trust at the individual level, but leads to mixed results at the aggregate level. Our findings suggest that reputation building, as well as allowing for social comparison could be efficient ways for receivers to improve trust within networks.

In the second chapter, we investigate trust and trustworthiness in a three-node network in which two senders (peripheral players) interact with a receiver (central player). We manipulate endogenously the social status of the players by allocating the central roles by merit (deservedness) as proxy of high status legitimacy. Indeed, and following Eckel et al. (2010), best performers in a trial phase get the role of high status receivers, while bottom performers get the role of low status senders. The aim is to study how differences in status as well as minimal manipulation of social information have an impact on investment and returning decisions. As in the first chapter of this thesis, we also investigate the compliance to an exogenous and totally fair recommendation about the proportion to invest and return. We compare a baseline merit treatment (no social info) with two social information treatments: Reputation merit and Transparency merit.

We observe that the exogenous recommendation has a positive and significant impact in the investment decisions, but it does not have an impact on the receivers' returning decisions. Indeed, trustworthy behavior is not built over time across treatments, which implies a lack of reciprocity from the high status receivers towards the low status senders. Low status senders tend to penalize the high status receivers' misbehavior by increasing the cases in which no trust is displayed. In the baseline merit, the proportion of observations in which nothing was sent reaches almost to half of interactions (49,38%), while in the reputation merit and in the transparency merit treatment to 29.17% and 26.61% respectively. Therefore, our findings suggest that social information seems to be critical in increasing significantly the level of trust at individual levels, however not the levels of trustworthiness. In fact, social comparison and reputation building increase the level of trust in networks characterized by differences in status.

In the last chapter, we study in a laboratory experiment how tax compliance information travels through a fixed-six-nodes circle network structure. This game introduces four information conditions: No Info, Full Info, Positive Info and Negative Info. In the No info treatment, subjects get individual information about whether they were audited, the outcome of it and her final payoff. In the Positive Info (Negative Info) treatment, participants get information about whether adjacent connected nodes were audited and found compliant (or noncompliant). In the Full Info, participants get both positive and negative signals. We specifically control for the effect of signals on participants' beliefs on the ex-ante fixed and unknown audit probability by asking them using an incentive compatible mechanism. We keep the tax rate and fine rate fixed and known by the subjects. Receiving positive and negative signals have a significant effect in the levels of reporting and compliance at individual level. However, results are mixed at the aggregate level. Our findings suggest that the diffusion of one negative signal (a bad example) has a negative effect on individuals' tax compliance. The diffusion of strong positive signals (two good examples) is required to generate any increase in compliance decisions within network.

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Chapter 1

In *Gov* We Trust: Voluntary compliance in networked investment games *

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1. Introduction

Trust is an essential component of most social and economic interactions. Because of trust, agents are willing to exchange, even in situations where standard theory would predict no exchange at all. For that reason, trusting behaviors have been extensively documented in the economic literature. The most popular experimental game used to address this issue is without doubt the two players investment game (Berg et al., 1995). This experimental setting allows the investigation of trust – and trustworthiness – in a bilateral relationship between a single sender and a single receiver. However, there are many examples of economic interactions in which several senders interact with the same receiver and where the information available to the actors involved is not complete, but partial. This is the case of private agents interacting with a government. One example in which trust is involved in the relationship between agents and authorities is the payment of taxes. Private agents decide to give part of their earnings in the forms of voluntary or involuntary taxes, expecting to receive efficient and fair benefits from the system in return (Andreoni et *al.*, 1998; Feld and Frey, 2002; Hofmann *et al.*, 2008; Li et al., 2011). The trusting decision of private agents is directly related to economic and non-economic factors, which determine the behavioural relationship between them and the government (Feld and Frey, 2007; Torgler, 2007). One of the most evident economic factor would be the presence of tax legislation that sets exogenous rules to both tax payers and tax authority. Another factor would be that agents could access information regarding the outcome of previous interactions between the government and other agents.

Recent networked extensions of the trust game have indeed demonstrated the comparative nature of trust: one's decision to trust may be affected by the experience of others (Barrera and Buskens, 2009; Buskens et al., 2010; Cassar and Rigdon, 2011). In this current study, we address two important features that may affect trusting behaviors in a three player networked investment game with two senders and one receiver. First, we manipulate the amount and the nature of information that the senders have about the performance of the receiver. Second, we investigate to what extent the implementation of an exogenous, non-binding and weakly framed rule influences both trust and trustworthiness.

Some recent experimental studies have addressed the impact of information flows on trust and trustworthiness in network embedded settings. Buskens et al. (2010) analyse an investment game where two senders repeatedly face the same receiver. Cassar and Rigdon (2011) also implement repeated three player investment games with a stranger matching protocol. Both experimental settings compare two information conditions. In the first condition each sender only observes the actions and outcomes of her own investment game, resulting in a situation comparable to the standard two player investment game. In the full information condition, additional information is provided to senders: the amount invested by the other sender, as well as the amount returned to the other sender. The availability of complete information on third-party interaction has a dramatic impact on cooperation. Both trust and trustworthiness tend to increase with full information. Real-life examples involving complete information over interactions in a network are however not so common. Most of the time, one could expect to face situations where the information is only incomplete. In our study, we investigate the impact on minimal additional information on trust and trustworthiness. More precisely, in the different treatments, we provide to the senders the minimal information about the decisions of the receiver – the amount returned or the average amount returned to both senders – in previous interactions within the network, without revealing the actions of fellow senders.

In this study, we also address voluntary compliance to exogenous norms. This research question is directly related to the recent experimental literature investigating cheating and lying behaviors (Serra-Garcia et al., 2011; Erat and Gneezy, 2012; Rosaz and Villeval, 2012; Fischbacher and Utikal, 2013; López-Pérez and Spiegelman, 2013; Gneezy et al., 2013; Cappelen et al., 2013; Reuben and Stephenson, 2013; Xiao, 2013). Experimental studies have shown that individuals may be willing to bear monetary cost to display honest behaviors, suggesting the presence of aversion to dishonesty (Gneezy, 2005; Sanchez-Pages and Vorsatz, 2007; Hurkens and Kartik, 2009; Lundquist et al., 2009; Fischbacher and Heusi, 2013). There exists a direct relationship between trust and rule compliance. To follow-up with our previous example, empirical studies have shown that trust in authority is positively and significantly related to – voluntary – tax compliance (Torgler, 2002 and 2003; Torgler and Schneider, 2004 ; Halla, 2010; Li et al, 2011; Doerrenberg and Peichl, 2013). This finding suggests that by increasing the level of trust, authorities could find an efficient and effective way to ensure tax compliance and limit the use of costly enforcement actions like audits (Listokin and Schizer, 2012; Luttmer and Singhal, 2014). By analyzing trust, we explore one of the major components of tax morale, which is defined as the intrinsic motivation of paying taxes, and helps to deter tax evasion.

In this work, we investigate the interaction between trust, trustworthiness, and the presence of an exogenous, non-binding rule, which is fair and efficient for both senders and receiver. We implement a repeated three-player investment game where two senders face the same receiver. Participant in the role of sender chooses a proportion of her endowment to forgo. This amount is multiplied and received by the receiver. The receiver decides then how much to return to the sender. In our experimental setting, both senders and receivers face a random suggested rule regarding the proportion to send, or to send back to the partner. The rule is a non-binding signal, which is also private information and weakly framed in the instructions. We are interested in how participants in our experiment follow the norm suggested by the signal, making compliance explicit.

Besides, by implementing three different treatments (information conditions), we want to examine the effect in the level of senders' trust and receiver trustworthiness when the available information changes, and how different policies alter performance, using as proxies the level of effort and the level of trust.

In the baseline treatment, at the end of each period, participants are informed of the outcome of their own interaction only. The participants play twenty periods, in the same role following a partner matching protocol at a cohort level. To the baseline, we add two additional treatments that provide senders with new information. In the reputation treatment, senders get ex ante information regarding receiver's trustworthiness before choosing an amount to send. This information consists in the average amount send back by the receiver to both of the senders associated to her in the previous period; in a sense, this is the minimal informational that a sender may get about the past performance of receiver. In the transparency treatment, we aim to analyze how perception of equity and fairness affects trust after knowing ex-post the amount of resources sent by the receiver to each sender in the group in that period; again, this is the minimal ex-post information senders may get about their receiver's current performance.

Our paper is closely related to Buskens et al. (2010) and Cassar and Rigdon (2011) that also investigate trust and trustworthiness in a three player networked investment game. Our experimental framework differs however from these studies in several aspects. First, both senders and receivers face a rule or signal, which is fair and efficient relative to the equilibrium of the game, suggesting the amount to send or to return. This allows us to investigate voluntary compliance to exogenous norms. Second, while previous experiments have examined the impact of full information flows across network, we only provide senders with partial information regarding the outcome of previous interactions within the network.

To anticipate our results, we observe that both *ex ante* and *ex post* information on receiver's behavior significantly impact sender's investment decision at the individual level. Results are however mixed at the aggregate level. The average level of trust is higher in the transparency treatment compared to the baseline and the reputation treatment. This finding suggests that in our experimental framework, allowing for *ex post* social comparison is more effective than providing *ex ante* information on receiver's level of trustworthiness. Contrasting with the previous literature, we do not observe any impact of the provision of additional information on trustworthiness. Finally, individual decisions from both senders and receivers are significantly affected by the presence of a non-binding exogenous recommendation.

The rest of our paper is organized as follows. Section 2 describes our experimental design and procedures. Section 3 reports the experimental results. Finally, section 4 concludes this paper.

Experimental design and procedures

2.1. The three players trust game

The standard trust game first introduced by Berg et al. (1995) concentrates on interactions in a two-node network. One node is occupied by a first mover or sender, and the other node by a second mover or receiver. In the first stage, the first mover decides how much of her endowment to send to the second mover. The second mover receives that amount multiplied by a factor k, with $k>1$. In the second stage, the second mover decides how much to send back to the first mover. We implement a three-node networked investment game that has already been addressed in the literature (e.g. Buskens *et al.*, 2010; Cassar and Rigdon, 2011). Two senders interact with the same receiver (see figure 1). The game consists of two simultaneous trust games. The two senders decide of a proportion of their endowment to invest. The receiver receives separately these amounts multiplied by four and decides for each receiver of a proportion to send back. Both trust games are separable in the sense that the decision of the receiver for one sender is conditioned only by the amount received from this sender. The presence of a networked structure therefore does not directly affect the outcomes of the trust games. However, in some treatments senders can receive information regarding actions across the network.

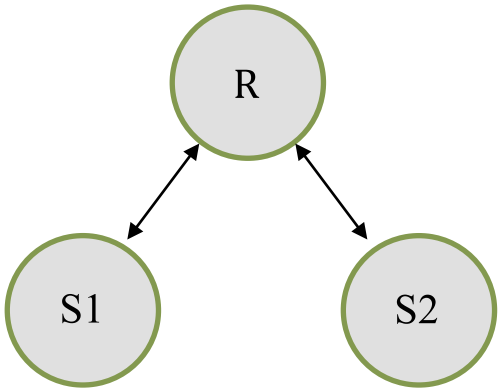

Figure A: The three-node network of two senders (S1 and S2) and one receiver (R).

At the beginning of the experiment, all participants are randomly assigned to a role (sender or receiver) and a cohort of 6 participants. This cohort includes four senders and two receivers. Along the twenty rounds of the experiment, participants keep their role. At the beginning of each round, two three-node networks are formed in the cohort. The four participants in the role of sender are randomly matched with one of the two participants in the role of receiver. Because the matching process is applied within and not between cohorts, each cohort corresponds to an independent observation. The game is repeated over twenty rounds. Each round consists of five different stages. Table A displays the basic sequence of the game.

Table A: Proceeding of an experimental round

1st stage: Senders	2nd stage: Senders	3rd stage: Senders	4th stage: Receiver	5th stage: Receiver
Real-Effort Task	Signal 1: die	Trust (X)	Signal 2: die	Trustworthiness (Y)

1st stage – Real effort stage: At the beginning of each round, senders perform in a real-effort task for one minute. This task is implemented so that participants earn their own endowment before investing it in the trusting decision[1]. It consists in adding two-digit numbers for one minute. For each addition correctly solved, sender's endowment increases by 2 ECU. An upper limit is defined, so that endowment cannot exceed 10 ECU. Receivers do not participate to this task and receive a fixed endowment of 5 ECU.

2nd stage – Signal for sending decision: Each *S*ender throws a virtual die and get a non-binding signal (see section 2.2) about the proportion of endowment she should send to the receiver.

3rd stage – Sending decision: Each sender j decides of a proportion X_j of her endowment to send to the receiver. She can choose a proportion to send in {0%; 20%; 40%; 60%; 80%; 100%}. The amount sent is multiplied by four and received by the second mover.

4th stage – Signal for returning decision: The receiver observes both amounts received from the senders she is matched with. She then throws for each sender a virtual die and get non-binding signals (see section 2.2) about the proportion of endowment she should return to each sender.

5th stage – Returning decision: The receiver decides for each sender j of a proportion Y_j from the amount received from j to return. She can choose a proportion to return in {0%; 20%; 40%; 60%; 80%; 100%}.

At the end of the period, participants are informed of the outcome of the game. The receiver observes information on both trust games, whereas senders are only displayed the actions and outcomes from the trust game they have played.

The game described above corresponds to our baseline treatment. In two other treatments, additional information is provided to senders. We further describe both information treatments in section 2.3.

2.2. The Signal

In our experimental framework, both senders and receivers face a non-binding recommendation before taking their decisions. The level of recommendation S is determined by the outcome D of a virtual six-sided die (see *table 2*). The value of the signal is then defined as:

$$S = 0.2 \cdot (D - 1)$$

[1] See Houser and Xiao (2014) for a discussion of "house money effects" in trust games.

The signal therefore follows a discrete uniform distribution: S ~ DU(6, 0, 0.2). Since the beginning of the experiment, all participants know the distribution of the recommendation S.

Table B: Outcome of the die and corresponding recommendation

Outcome of the die:	1	2	3	4	5	6
% to keep	100%	80%	60%	40%	20%	0%
% to send to the receiver or % to send back to the sender	0%	20%	40%	60%	80%	100%

Assuming that players are rational and only aim at maximizing own profit, the inclusion of a non-binding recommendation does not alter theoretical predictions. The subgame perfect Nash equilibrium can be solved by backward induction. Receiver maximizes her profit by keeping for herself the full amount received from the sender. Considering this, the sender does not invest any amount. The equilibrium is therefore characterized by an absence of interaction, and payoffs equal initial endowments for both players.

Let's consider the situation where both sender and receiver fully comply with their respective signal. The realized recommendation S takes value $S_k \in \{0\ ;\ 0.2\ ;\ 0.4\ ; 0.6\ ; 0.8\ ; 1\}$ with fixed probability $p_k = \frac{1}{6}$. The expected value of the exogenous recommendation is then 50%:

$$E[S] = \sum_{k=1}^{6} p_k \cdot S_k = \frac{1}{6} \cdot \sum_{k=1}^{6} S_k = 0.5$$

Let *e* be the initial endowment of the sender. In a situation of full compliance, i.e. the decisions from the sender (X) and from the receiver (Y) are fully conditioned on the outcome of the die, E[X]=E[Y]=0.5. The expected profits of the complying sender (π_S^c) and the complying receiver (π_S^c) can therefore be expressed as:

$$E[\pi_S^c] = E[e - Xe + 4Xe \cdot Y] = 1.5e$$

$$E[\pi_R^c] = E[(4Xe) \cdot (1 - Y) + 5\,] = e + 5$$

Endowment *e* is earned by senders at the beginning of each period through a real-effort stage. The implementation of this stage only aims at avoiding windfall money effect, i.e. individual decision being influenced by the fact that senders do not consider the money they invest as theirs. We set an upper limit to the endowment corresponding to a performance of five additions or more in the real-effort task. This threshold has been implemented to limit heterogeneity in sender's endowment. Indeed, a large majority of the participants to the addition task manage to reach this threshold within a minute[2]. Interestingly, in the presence of a maximal endowment, the expected profits of a complying sender and a complying receiver are equal:

$$E[\pi_S^c|e=10]=E[\pi_R^c|e=10]=15$$

The exogenous rule could therefore be considered as fair, in the sense that the expected profit of both parties are equal, provided that they perfectly comply to the rule over all periods. Furthermore, the resulting outcomes would be significantly higher for both parties than those implied by the Nash equilibrium where for which is displayed.

2.3. Information treatments

In this experiment, we manipulate the information available to the senders across three different treatments. In the *baseline treatment*, senders do not receive information on receiver's past behavior in previous rounds before taking their investment decision, and only receive feedback regarding their own trust game at the end of the period. This experimental framework is close from the "partial information" condition investigated in Cassar and Rigdon (2011). In two other treatments, we provide to the senders additional information regarding senders' behavior within the network. Unlike previous experimental studies that concentrate on situations of full information, we investigate the role of partial information on trust and trustworthiness.

In the reputation treatment, senders receive additional information before reaching their investment decision. This *ex ante* information regarding the receiver's reputation corresponds to the average proportion returned in the previous round by the receiver they are matched with in that round[3]. Senders do not get separate information regarding both amounts received and returning decisions taken by the receiver in the previous round. We therefore consider this information as minimal.

[2] In our data, across all treatments and periods, senders earn the maximal endowment of 10 ECU in 96.94% of the cases.
[3] Assume that a receiver returned 20% to one sender and 40% to the other sender in the first round. At the beginning of the second round, the two senders that will be associated to that receiver will observe a reputation of 30% before making their investment choice.

In the transparency treatment, senders receive additional information at the end of each round. This *ex post* information corresponds to the outcome of the other trust game in the network. More precisely, each sender observes the proportion that the receiver returned to herself, but also the proportion returned to the other sender. This information is minimal in the sense that they do not observe how much the other sender invested.

2.4. Procedures

All the experimental sessions were conducted at ESSEXLab in the University of Essex. We electronically recruited 108 participants, mainly business and economics undergraduate students, all inexperienced in trust games. For each treatment, we collected data from six independent observations of six participants. On average, a session lasted 100 minutes, including initial instructions, quiz, trial phase, final questionnaire and payment of the subjects. The average payment was around £12.50, including a show up fee of £5. The instructions were read aloud. The only difference in the instructions was the information available to them according to each treatment. The experiment was computerized using Z-TREE (Fischbacher, 2007).

3. Results

3.1. Trust and trustworthiness across treatment

3.1.1 Descriptive statistics

Table 1 reports summary statistics regarding average decisions of both senders and receivers. Throughout this paper we define "trust" as the proportion of endowment sent by the first mover and "trustworthiness" as the proportion of the received amount returned by the second mover. Wilcoxon Mann-Whitney tests are run on independent observations (cohorts) to test for significant differences between treatments.

The upper panel of table 1 displays outcomes for senders in total and for each treatment. The first column displays the average proportion sent. Trust is in average higher in the transparency treatment than in the baseline treatment (p=0.0372) and the reputation treatment (p=0.0483). There is no significant difference between the proportion sent by first movers in the baseline and the reputation treatments (p=0.2461). The second column considers only the positive proportions, excluding all interactions where the first mover decided to send nothing. Senders' decisions do not significantly differ between treatments in this case. This finding suggests that the higher level of trust observed in the transparency treatment compared to both other treatments is mainly lead by a lower share of senders showing no trust at all.

The third column of table 1 confirms this assertion as the proportion of interactions, in which no trust is displayed – no amount sent – is lower in the transparency treatment (17.92%) than in the baseline treatment (24.84%) and the reputation treatment (28.75%). Furthermore, we observe large differences between treatments in the occurrence of situations where receivers face no choice at all, i.e. both senders do not send anything in the same period. This situation occurs for 7 out of 240 observations (2.92%) in the transparency treatment. This figure is larger in the baseline treatment with 15 out of 240 observations (6.25%) and much larger in the reputation treatment with 32 out of 240 observations (13.33%).

Table 1: Descriptive Statistics

Trust – (Proportion X sent by senders)				
	Proportion sent (Including X=0)	Proportion sent (X>0 only)	Proportion of X=0	Average profit of sender (in £ / $)
Total	0.3879 (0.3213)	0.5093 (0.2715)	23.84%	£4.50 / $7.36 (2.26) (3.69)
Baseline	0.3795 (0.3206)	0.5050 (0.2707)	24.84%	£4.66 / $7.63 (2.52) (4.12)
Reputation	0.3454 (0.3165)	0.4848 (0.2700)	28.75%	£4.46 / $7.31 (1.98) (3.23)
Transparency	0.4388 ** (0.3206)	0.5345 (0.2720)	17.92% *	£4.35 / $7.14 (2.24) (3.66)

Trustworthiness– (Proportion Y returned by receivers)				
	Proportion returned (Including X=0)	Proportion returned (X>0 only)	Proportion of Y=0 (X>0 only)	Average profit of receiver (in £ / $)
Total	0.2518 (0.2839)	0.3305 (0.2825)	24.98%	£6.07 / $9.94 (3.99) (6.54)
Baseline	0.2833 (0.3156)	0.3767 (0.3118)	24.10%	£5.80 / $9.50 (4.15) (6.79)
Reputation	0.2288 (0.2697)	0.3211 (0.2692)	25.44%	£5.57 / $9.12 (3.61) (5.91)
Transparency	0.2433 (0.2613)	0.2964 (0.2596)	25.38%	£6.84 / $11.20 * (4.10) (6.54)

Notes : Standard deviations are displayed in parentheses ; Stars report significance level from Wilcoxon Mann-Whitney tests run on independent observations (cohorts of 6 participants) to confirm differences with the baseline treatment.
* 90% significance ** 95% significance *** 99% significance

The lower panel of table 1 reports receivers' returning decisions. Considering only the interactions for which receivers made a decision (X>0), the proportion returned in the transparency and the reputation treatments tends to be lower than in the baseline treatment. These differences are however not statistically significant (p=0.3367 and p=0.1495 respectively).

Result 1: *Senders make positive offers more often in the transparency treatment than in the baseline and reputation treatments. There is no significant difference across treatments regarding the returning decision of receivers.*

Senders make offers more often in the transparency treatment, even though receivers do not return more (and return even less, although not significantly). For that reason, the average payoff of senders do not differ across treatments, whereas the payoff of receivers is about 18% higher in the transparency treatment than in the baseline treatment (p=0.0782).

Result 2: *In average, receivers earn higher profits in the transparency treatment. The average profit of senders does not significantly differ across treatments.*

3.1.2 Trust

Figure 1 displays for each treatment the average proportion of endowment sent by first movers across periods. It suggests that previously observed treatment differences in trusting behavior are mainly lead by differences in dynamics. Trust tends to decrease over time in the baseline treatment (Spearman's Rho=-0.3860, p=0.0928) and in the reputation treatment (Spearman's Rho=-0.5017, p=0.0242). This is not the case for the transparency treatment in which trust does not significantly vary over time (Spearman' Rho=0.2012, p=0.3950).

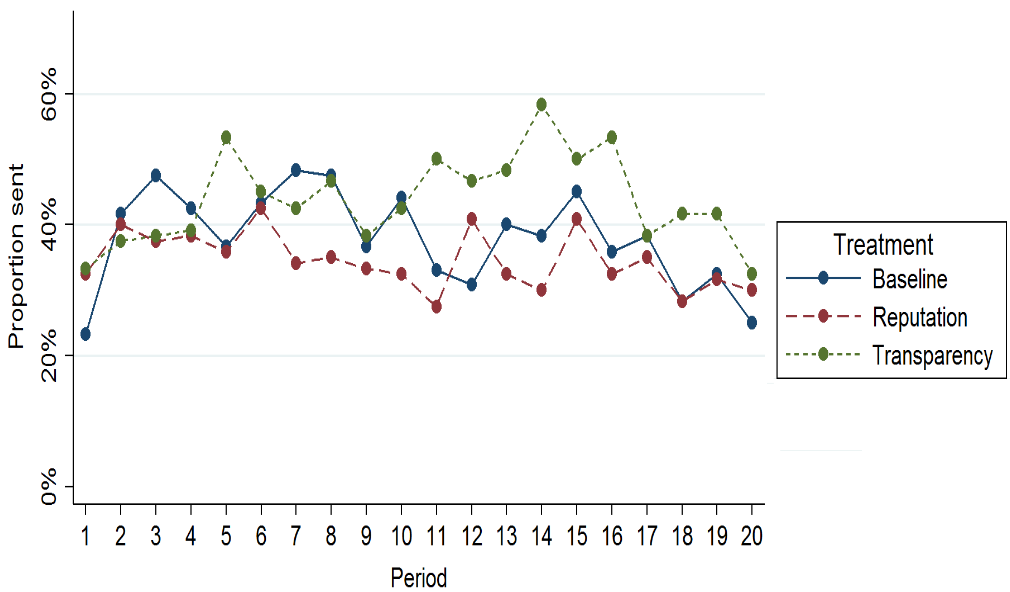

Figure 1 – Proportion of endowment sent (Trust) across periods

Consistent with Result 1, treatment differences in the level of trust merely reflect differences in the proportion of senders keeping their whole endowment. Figure 2 displays these proportions for the first 10 periods and the last 10 periods of the game.

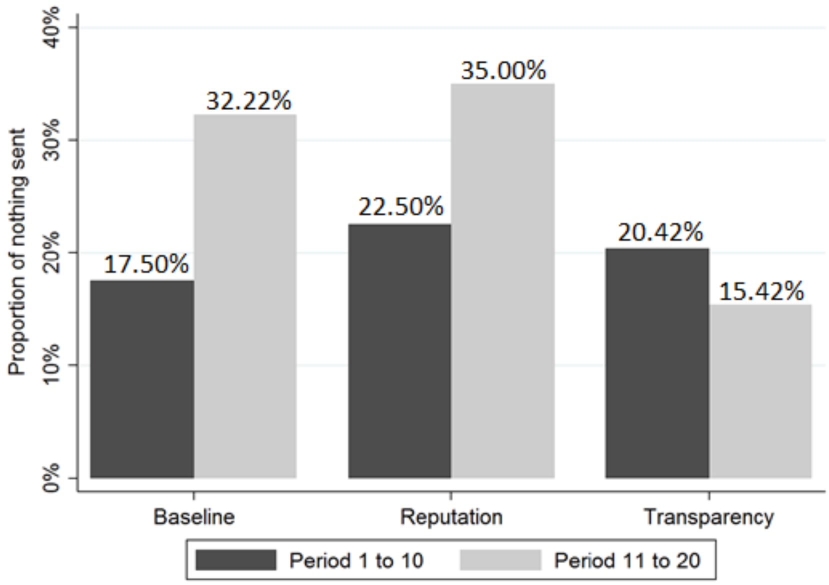

Figure 2 – Proportion of situations with no trust at all

It appears that the share of senders showing no trust tends to increase over time in the baseline (Spearman's Rho=0.6739, p=0.0011) and in the reputation (Spearman's Rho=0.7855, p=0.0000) treatments. This share tends to decrease over time in the transparency treatment, although not significantly (Spearman's Rho=-0.2843, p=0.2244).

Result 3: *Trust does not vary over time in the transparency treatment, whereas it decreases in the baseline and reputation treatments.*

Table 2 reports estimates of the determinants of trust[4] at the individual level. Controlling for amounts sent and received in previous period, outcome of the die, time trend and demographic variables, we observe in column (1) that trust is larger in the transparency treatment than in both the baseline and the reputation treatment. Sender's trust tends to increase with the proportion received back in t-1 and to decrease over time. We also observe that the outcome of the die is a significant determinant of the amount sent by first movers. Compliance to the rule is discussed in further details in section 3.3.

[4] Senders could send between 0% and 100% of their initial endowment with increments of 20%. Given the discrete nature of the corresponding variable, we decide to run random-effect ordered probit rather than standard random-effect regression.

Table 2
Random-effects ordered probit regressions for trust - pooled sample

	Proportion X_{it} sent by sender	
	(1)	(2)
Lag Proportion Sent (X_{it-1})	0.811*** (0.206)	0.836*** (0.198)
Lag Proportion Received (Y_{it-1})	0.638*** (0.088)	0.566*** (0.099)
Die Outcome (Rule)	0.138*** (0.032)	0.143*** (0.033)
Baseline Treatment	*Ref.*	*Ref.*
Reput. Treatment	-0.063 (0.208)	-0.388* (0.243)
Reput. Treatment × Reputation of the receiver	-	1.298** (0.559)
Transp. Treatment	0.303** (0.125)	-
Transp. Treatment × Received as much as other sender	-	0.154 (0.115)
Transp. Treatment × Received less than other sender	-	0.286** (0.122)
Transp. Treatment × Received more than other sender	-	0.140 (0.131)
Period Number	-0.016** (0.007)	-0.015** (0.006)
Female	-0.237 (0.178)	-0.272 (0.167)
Age	0.041 (0.033)	0.039 (0.030)
British	-0.066 (0.204)	-0.081 (0.197)
# observations	1366	1366
# individuals	72	72
Log pseudo-likelihood	-2089.7483	-2065.1954

Standard errors (in parentheses) are clustered on the independent observation level (cohorts of six individuals)
* 90% significance ** 95% significance *** 99% significance

To reach a better understanding of treatment differences in the level of trust, we include additional control in column (2) of table 2. Although trust in the reputation treatment is in aggregate not different than in the baseline treatment, regression results suggest that the reputation of the receiver matters. The *reputation treatment* dummy variable is associated to a negative and significant coefficient, whereas the coefficient associated to the interaction variable *reputation treatment* × *reputation of the receiver* is significantly positive. This finding suggests that when facing a receiver with low reputation, participants send in average less than senders in the baseline treatment. Sender's trust increases however dramatically with the reputation of the receiver. Because of heterogeneity in receivers' reputation, we do not observe in aggregate any significant difference in trust between baseline and reputation treatments. These findings are discussed in subsection 3.2.1. Column (2) of table 2 also emphasizes the role of social comparison in the transparency treatment. Only participants who received back in previous period a lower proportion than the other sender tend to send more than in the baseline treatment. Further investigation of the role of information in the transparency treatment is provided in subsection 3.2.2.

3.1.2 Trustworthiness

Figure 3 displays for each treatment the proportion of amount received that senders return in average over time. Consistent with result 1, we do not observe any treatment difference in average return. Trustworthiness decreases significantly over time in the baseline treatment (Spearman's Rho=-0.5260, p=0.0172) and does not significantly vary overtime in both reputation (Spearman's Rho=-0.3190, p=0.1704) and transparency (Spearman's Rho=-0.3054, p=0.1904) treatments.

Column (1) of table 3 reports estimates of the determinants of trustworthiness at the individual level across treatments. We do not observe any difference in receiver's individual decision between treatments. The variables *Lagged total proportion sent* and *Lagged total proportion returned* correspond to the sum of the outcomes of both interactions in previous period. Considering all treatments, we observe reciprocity from the receiver, in the sense that the proportion returned increases with the amount received. The coefficient associated to the lagged total amount returned is positive and significant, suggesting that trustworthiness builds over time. As it was the case for senders, the recommended rule associated to the die plays a positive and largely significant role in receiver's returning decision.

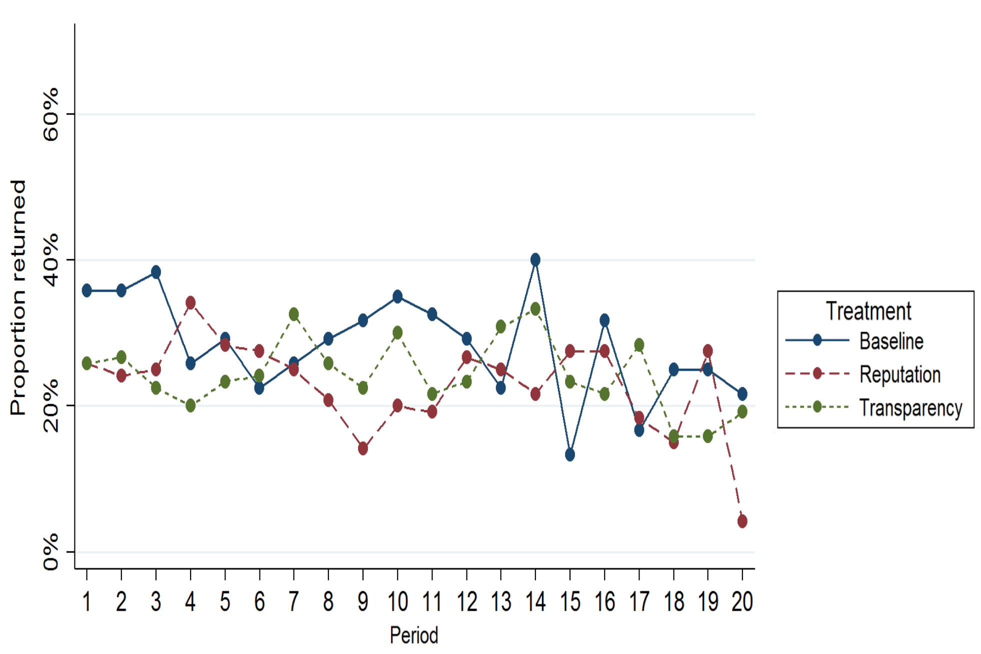

Figure 3 – Proportion returned (Trustworthiness) across periods

Columns (2) (3) and (4) display results of within-treatment regressions. We do observe that determinants of trustworthiness vary between treatments. Reciprocity, i.e. a positive effect of the amount returned on the proportion returned, appears only in the reputation and transparency treatments. Consistent with the *comparative trust hypothesis* formulated in Cassar and Rigdon (2011)[5], we observe that in the reputation treatment that returning decision to one sender is affected by the other sender's investment decision. A lower amount invested by a receiver would make the offer of the other receiver look more generous. Returning decision would then be based on the relative, rather than the absolute investment decision of senders. However, we do not find support for this hypothesis in the baseline and the transparency treatment. In all treatments, the outcome of the die plays a significant role in returning decisions.

Result 4: *Although we do not observe aggregate difference in trustworthiness between treatments, we observe reciprocity in the information treatments only. In the reputation treatment, reciprocity is based on comparative trust*

[5] In a networked trust games with two senders and one receiver, Cassar and Rigdon find evidence that the return decisions by receivers depend in part on the investment behavior along the other link in the network.

Table 3: Random-effects ordered probit regressions for trustworthiness – Treatment-specific samples

	Proportion Y_i returned by receiver			
	(1) Pooled	(2) Baseline	(3) Reputation	(4) Transparency
Proportion sent by sender	0.411* (0.219)	-0.037 (0.393)	0.755*** (0.170)	0.671* (0.376)
Proportion sent by other sender	0.016 (0.122)	0.137 (0.193)	-0.477** (0.230)	0.176 (0.147)
Lag. Total Proportion Sent	0.044 (0.100)	-0.124 (0.192)	0.303*** (0.101)	-0.019 (0.196)
Lag. Total Proportion Returned	0.156** (0.066)	0.160* (0.084)	0.213 (0.170)	0.176 (0.147)
Die Outcome (Rule)	0.220*** (0.029)	0.164*** (0.043)	0.225*** (0.043)	0.283*** (0.064)
Baseline Treatment	*Ref.*	-	-	-
Reput. Treatment	-0.205 (0.326)	-	-	-
Transp. Treatment	-0.256 (0.238)	-	-	-
Period Number	-0.016** (0.007)	-0.006 (0.005)	-0.031*** (0.006)	-0.019 (0.021)
Female	0.390 (0.324)	0.645 (0.689)	0.937 (0.735)	-0.288 (0.064)
Age	0.025 (0.022)	-0,019 (0.032)	0.159 (0.273)	0.101 (0.108)
British	-0.272 (0.317)	0.427 (0.513)	-0.701 (0.750)	0.067 (0.504)
# observations	1041	342	323	376
# groups	36	12	12	12
Log pseudo-likelihood	-1540.9140	-548.3909	-443.7712	-518.6968

Standard errors (in parentheses) are clustered on the independent observation level (cohorts of six individuals)
* 90% significance ** 95% significance *** 99% significance

3.2. The role of partial information in investment decisions

3.2.1 Reputation of receivers

In the reputation treatment, senders observe the reputation, i.e. the average proportion returned in previous period, of their receiver before proceeding to their investment decisions. We observe that the average proportion of endowment invested in the reputation treatment is not significantly different than in the baseline treatment. Preliminary regression analysis (see column (2) of table 2) however suggests that additional information provided in the reputation treatment has a significant impact on individual decision.

Table 4: Random-effects ordered probit regressions for trust - Treatment-specific samples

	Amount sent by sender S_i		
	(1) Baseline	(2) Reputation	(3) Transparency
Lag Amount Sent	1.279*** (0.234)	0.239 (0.363)	0.870** (0.395)
Lag Amount Received	0.651*** (0.083)	0.399* (0.205)	0.736*** (0.170)
Die Outcome (Rule)	0.101* (0.056)	0.167*** (0.046)	0.163** (0.066)
Reputation of the receiver	-	1.196** (0.542)	-
Receiver with no reputation [a]		-0.663 (0.538)	
Received less than other sender	-	-	0.298** (0.138)
Received more than other sender	-	-	0.090 (0.142)
Period Number	-0.032*** (0.009)	-0.012 (0.012)	0.000 (0.003)
Female	-0.163 (0.256)	-0.477 (0.350)	-0.290 (0.240)
Age	0.013 (0.036)	0.055 (0.057)	0.021 (0.034)
British	-0.196 (0.315)	-0.180 (0.311)	0.193 (0.434)
# Observations	454	456	456
# Groups	24	24	24
Log pseudo-likelihood	-667.1874	-663.9358	-720.8006

Standard errors (in parentheses) are clustered on the independent observation level (cohorts of six individuals)* 90% significance ** 95% significance *** 99% significance

[a] Receivers that did not receive any endowment in previous period could not make any choice and therefore did not build any reputation. As such, senders were only informed that the receiver could not make any choice.

Column (2) of table 4 reports estimates of the determinants of trust in the reputation treatment. Unlike for the baseline and the transparency treatment, amount sent in the previous period does not play a significant role, suggesting low persistence over time. Both the amount received back in the previous period and the outcome of the die affect positively and significantly the investment decision of senders. We take into account the situations for which no reputation was displayed, i.e. the receiver did not receive any amount from senders in previous period. Displaying no reputation does not appear to have any significant impact on trust in the reputation treatment.

The reputation of the receiver appears to be a critical determinant of trust. The probability to observe no trust (X=0) decreases significantly with the reputation of the receiver. Table 5 reports for each potential reputation the frequencies and the predicted probabilities to observe respectively no trust and a total trust. Our model predicts that the probability for a sender to send nothing is reduced by more than half when the receiver has a reputation of 50% rather than 0%. In the same way, the probability for the sender to invest her whole endowment increase dramatically with the reputation, from 3.56% (Reputation of 0) to 23.57% (Reputation of 1).

One could argue that the impact of reputation could reflect the interaction dynamics within the cohort and not the effect of the display of reputation to senders at the beginning of the period. We compare the impact of the reputation displayed to the senders in the reputation senders, and the reputation not displayed to senders in the baseline and transparency treatment. We observe that trust is positively correlated to the reputation of the receiver in the reputation treatment (rho=0.2453, p=0.0000), whereas it does not affect trust in the baseline treatment (rho=0.0683, p=0.1568) or the transparency treatment (rho=0.0527, p=0.2692). Even more striking, the average profit of a receiver is positively correlated to her average reputation over the 20 periods (rho=0.2863, p=0.0000). In the opposite, in the baseline and the transparency treatment, the average proportion returned in previous period affects negatively profits (rho=-0.7712, p=0.0000 for baseline; rho=-0.6245, p=0.0000 for transparency). Because reputation is not displayed to senders in these treatments, returning more does not pay off.

Table 5: Distribution of receivers' reputation and predicted trust

Reputation	Frequency	Predicted probability for no trust (X=0)	Predicted probability for full trust (X=1)
0%	33	0.3178***	0.0356*
10%	30	0.2798***	0.0450**
20%	36	0.2440***	0.0562**
30%	27	0.2109***	0.0696***
40%	28	0.1805***	0.0852***
50%	28	0.1530***	0.1034***
60%	9	0.1284***	0.1243***
70%	3	0.1067**	0.1479***
80%	2	0.0877*	0.1744***
90%	2	0.0713	0.2037***
100%	0	0.0574	0.2357***

* 90% significance ** 95% significance *** 99% significance

Note: The reputation of the receiver is displayed to two senders, but reported only once in this table. Predicted probabilities are computed for average values of every variable except *reputation of the receiver* and *no reputation*

Result 5: *In the reputation treatment, sender's investment decision is directly related to receiver's reputation. Although building reputation could be an efficient way to increase profit, only few receivers do so. As such, trust and profits in the reputation treatment are not different than in the baseline treatment.*

3.2.2 Social comparison

In the transparency treatment, the sender observes both the proportion returned to her and the proportion returned to the other sender by the receiver at the end of each round. We have observed so far that the level of trust in the transparency treatment is higher than in the baseline and the reputation treatments (see result 1 and 2). Furthermore, estimates reported in column (2) of table 2 suggest that this difference is mainly lead by senders that received less than the fellow sender.

In average in the transparency treatment, the investment decision of a sender that received back a lower proportion that the other send increases by about 6 percentage points. In the opposite, senders who observed to be above in terms of received proportion decrease in average their investment by about 5 percentage points. One should however be careful when interpreting these figures, as three main effects may be at stake. First, provided that sender's investment decision and receiver's returning decision are strongly correlated, these variations could merely reflect a "regression to the mean" phenomenon[6]. Second, we have observed that trust is positively related to the proportion received in previous period. As such, one could expect senders that receive more to increase their investment in further decisions. Third, senders have the opportunity to compare proportions received in the transparency treatment. A sender that received less than the other sender could see in this position an encouragement to invest more.

Column (3) of table 4 reports estimates of the determinants of trust in the transparency treatment and allows isolating the effect of the displayed information on both proportions returned by the receiver in previous period. As for all other treatments, the proportion received in the previous interaction positively affects sender's trust. Controlling for this effect and for the previous investment decision, we observe that those who receive less than the other sender in the previous round invest more than senders who receive an equal or larger proportion. This effect might explain the higher trust level in the transparency treatment compared to the baseline and the reputation treatments. Inequality between senders in the returning decision of receivers could then be an efficient way to increase the total trust in the network[7].

The average absolute differential between proportions returned to both senders by a sender in the transparency treatment is about 22.5 percentage points. This figure is not significantly different than the average absolute differential observed in the reputation treatment (21.75 percentage points, p=0.8169). It is however significantly lower than in the baseline treatment (29 percentage points, p=0.0487). It appears then that receivers in the transparency treatment do benefit from the fact that return inequalities increase trust and profits, without increasing the level of relative inequality in the network.

Result 6: *In the transparency treatment, providing information on both returning decisions from the receiver affects positively the trust of senders that are disadvantaged. As a result, trust is larger in the transparency treatment than in the baseline and reputation treatments.*

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[6] Considering the investment decision as partly stochastic, one could expect participants who send more (resp. less) than the mean to decrease (resp. increase) their investment decision in the subsequent period.

[7] Although we have implemented a stranger matching mechanism, i.e. participants are re-matched every round in networks, the relatively small size of our cohorts enhances the effect of information on trust dynamics.

3.3. Compliance to the rule

3.3.1 Compliance from the senders

Previous regression analysis has shown that the outcome of the die, corresponding to a suggested proportion to send, has a systematic impact on investment decisions. This effect is positive and significant, suggesting that the higher the exogenous recommendation, the higher the amount invested by the sender. In table 6, it is reported the descriptive statistics for the compliance by the senders. Overall, we do not find evidence for treatment difference in compliance to the rule. The proportion of investment decision for which the sender has followed the rule is 30.00% in the baseline treatment, 33.13% in the reputation treatment and 30.00% in the transparency treatment[8]. We do not observe any significant treatment difference when we evaluate the average proportion of times that senders send more and less than less, also reported in table 6.

Table 6: Compliance to the rule- Descriptive statistics for the senders

Proportion of senders FOLLOWING the rule			
Total	Baseline	Reputation	Transparency
31.05%	30.00%	33.12%	30.00%
(0.4628)	(0.4573)	(0.4711)	(0.4587)
Proportion of senders SENDING MORE than the rule			
Total	Baseline	Reputation	Transparency
24.09%	24.30%	21.25%	26.60%
(0.42780	(0.4297)	(0.4095)	(0.4426)
Proportion of senders SENDING LESS than the rule			
Total	Baseline	Reputation	Transparency
44.86%	45.62%	45.62%	43.33%
(0.4975)	(0.4986)	(0.4986)	(0.496)

Notes: Standard deviations are displayed in parentheses; Stars report significance level from Wilcoxon Mann-Whitney tests run on independent observations (cohorts of 6 participants) to confirm differences with the baseline treatment.

*90% significance ** 95% significance *** 99% significance

Figure 4 reports the distribution of investment decisions for each outcome of the die. We observe that participants tend to follow the rule as long as the rule is not too high. The proportion suggested by the die is the modal outcome when this suggestion lies between 0% and 60%. However, when the outcome of the die suggests sending 80% or 100% of sender's endowment, the modal investment decision is to send 0%. This observation echoes previous experimental findings

[8] We also do not find significant difference between treatments when conditioning the decision to follow the rule on the outcome of the die.

(e.g. Dale and Morgan, 2010) that show in the context of public good games that non-binding recommendations could affect individual decisions, provided that they are seen as “reasonable”.

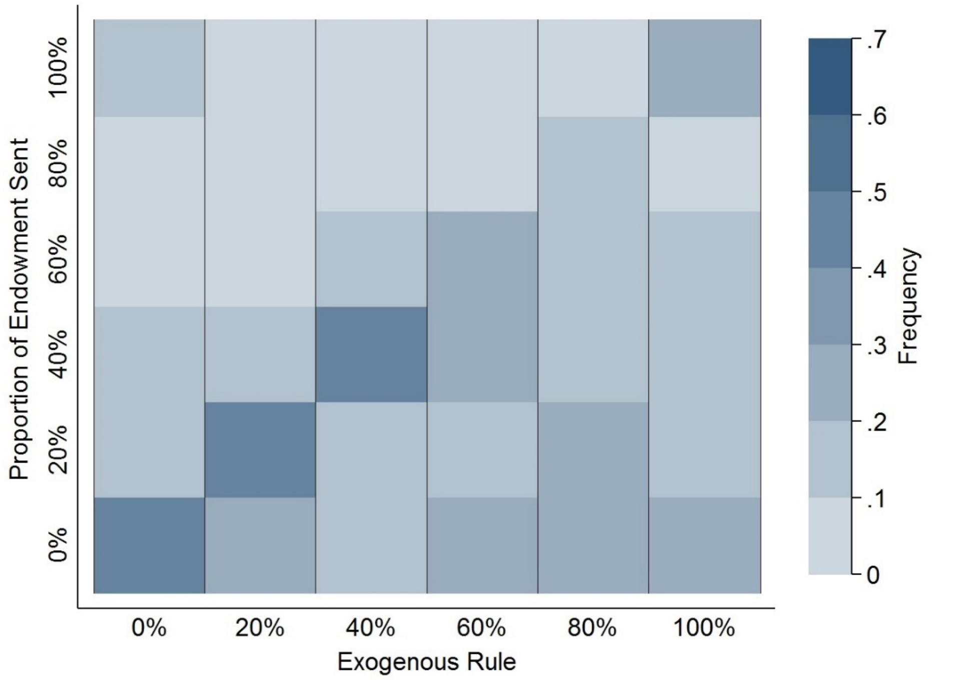

Figure 4 – Frequencies of investment decisions regarding exogenous rule

Column (1) of table 7 reports estimates of the determinants of compliance decision from the senders. Consistent with previous observation the higher the proportion suggested by the rule, the lower the probability that sender complies with it. Regression results also suggest that compliance slightly decreases over time. We observe a negative and significant correlation between period and compliance with the rule (Spearman’s rho=-0.1017, p=0.0538). Here again, we do not find evidence of difference in compliance among treatments.

Table 7
Random-effect probit regression for compliance (following the suggested rule)

	Individual decision corresponds to the rule suggested by the die	
	(1) Senders	(2) Receivers
Baseline Treatment	*Ref.*	*Ref.*
Reputation Treatment	0.066 (0.102)	-0.004 (0.208)
Transparency Treatment	-0.017 (0.133)	0.062 (0.199)
Outcome of the die	-0.801*** (0.159)	-1.384*** (0.103)
Proportion Received	-	0.006 (0.241)
Period	-0.015** (0.007)	-0.012 (0.009)
Female	0.091 (0.124)	0.265 (0.200)
Age	-0.053** (0.025)	-0.030 (0.022)
British	-0.064 (0.121)	-0.068 (0.306)
Constant	1.066* (0.561)	0.791 (0.555)
# Observations	1440	1096
# Individuals	72	36
Log pseudo-likelihood	-841.3950	-616.5934

Standard errors (in parentheses) are clustered on the independent observation level (cohorts of six individuals)
* 90% significance ** 95% significance *** 99% significance

3.3.2 Compliance from the receivers

Such as sender's investment decision, previous regression analysis has shown that receiver's returning decision is positively and significantly affected by the outcome of the die (see table 3). Table 8 displays the descriptive statistics for the compliance of the receivers. The proportion of returning decision for which the rule have been followed by the receiver is 29.58% in the baseline treatment, 29.37% in the reputation treatment and 32.29% in the transparency treatment. No significant difference between treatments is observed in the decision to comply with the suggested rule.

Table 8
Compliance to the rule- Descriptive statistics for the receivers

Proportion of senders FOLLOWING the rule			
Total	Baseline	Reputation	Transparency
30.41%	29.58%	29.37%	32.29%
(0.4602)	(0.4568)	(0.4559)	(0.468)
Proportion of senders SENDING MORE than the rule			
Total	Baseline	Reputation	Transparency
13.95%	17.50%	13.13%	11.04%
(0.3466)	(0.3803)	(0.3402)	(0.3137)
Proportion of senders SENDING LESS than the rule			
Total	Baseline	Reputation	Transparency
55.62%	52.91%	57.29%	56.66%
(0.4969)	(0.4996)	(0.4951)	(0.496)

Notes : Standard deviations are displayed in parentheses ; Stars report significance level from Wilcoxon Mann-Whitney tests run on independent observations (cohorts of 6 participants) to confirm differences with the baseline treatment.
*90% significance ** 95% significance *** 99% significance

We also report in table 8, the percentage of cases in which receivers returned less than the rule and more than the rule. We do not find any significant difference between the treatments and the baseline.

Figure 5 reports for each outcome of the die the distribution of receivers' returning decisions. Whereas suggestions of relatively small return are often followed, only few receivers comply with rule suggesting a return above 50%. It should however be noted that the rule significantly improves returning decision, even if it suggests a high return. When the outcome of the die suggests to return the whole amount received, receivers do so in about 14.87% of cases. Considering other rules, receivers decide to return the whole amount received in only 2.55% of cases. Column (2) of table 7 reports estimates of the determinants of compliance decision from the receivers. It appears that only the outcome of the die has a significant and negative impact on the probability to follow the rule.

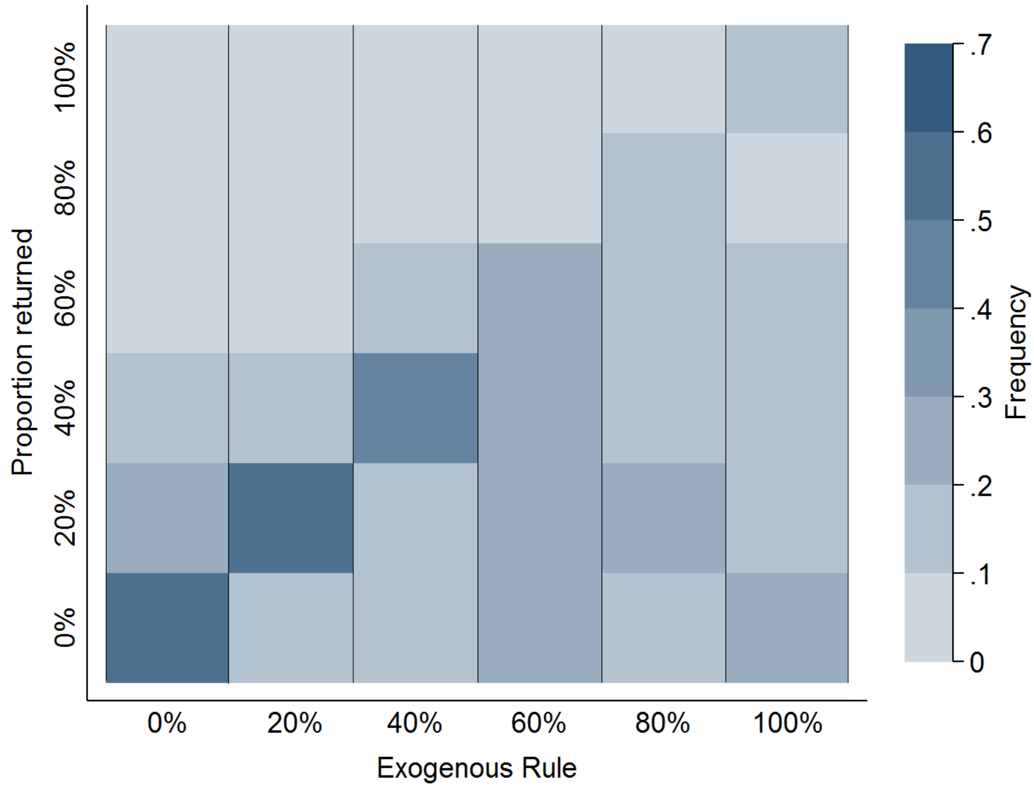

Figure 5 – Frequencies of returning decisions regarding exogenous rule

Result 7: *For both senders and receivers and across all treatments, the non-binding rule suggested by the die has a significant and positive impact on individual decisions. Compliance to the rule tends to decrease as the amount suggested by the rule increases.*

4. Conclusion

We implement a controlled laboratory experiment to investigate trust and trustworthiness in a networked investment game in which two senders interact with a receiver. Previous studies using a comparable framework (Buskens et al., 2010; Cassar and Rigdon, 2011) have shown that providing full information regarding actions and outcomes across the network could increase both trust and trustworthiness. We compare a baseline treatment in which senders are only informed about the actions and outcomes of their own investment games to two information treatments.

In the reputation treatment, senders receive *ex ante* information regarding the average amount returned by the receiver in the previous period. In average, neither the level of trust nor the level of trustworthiness is affected by the introduction of additional information in this treatment. Receiver's reputation however significantly affects sender's investment decision at the individual level. Introducing reputation allows trustworthy receivers to benefit from higher investment. In the opposite, it disadvantages receivers with low reputation. Although receivers face strong incentives to build reputation, we do not observe any increase in trustworthiness compared to the baseline treatment. For that reason, despite a significant effect of reputation at the individual level, the level of trust does not increases at the aggregate level.

In the transparency treatment, each sender receives *ex post* additional information regarding the returning decision of the receiver to the other sender in the network. Sender's investment decision significantly increases in the transparency treatment, despite the fact that receivers returning decision does not vary in average. As a result, receiver's profit is in average higher in the transparency treatment than in the baseline and reputation treatments. Receivers, in the transparency treatment, benefit from social comparison between senders. A sender who has been disadvantaged tends to trust more in the following round, whereas no change in trust is observed for those who received an equal or larger proportion than their counterpart. In contrast to Cassar and Rigdon (2011), we do not observe evidence of comparative trust, i.e. returning decisions depending on the investment decision in the other link in the network. In their experimental setting, senders face full disclosure regarding the amount sent and the amount returned along the other ling of the network. Receiver's returning decision could therefore be used as a hint to promote trust. Senders in our experimental design only face partial information, and do not observe the other sender's actions. The incentive for receivers to reward trustful senders might then appear less attractive than in a full information setting. Although inequalities in return could be used as a lever to increase trust within the network, we observe than these inequalities are significantly lower than in the baseline treatment.

Another novelty in our experimental design lies in the provision of an exogenous, non-binding and private recommendation to participants before they reach their sending or returning decision. For both senders and receivers, this non-binding rule has a significant and positive impact on individual decisions. This finding is observed across all treatments, suggesting that additional information does not substitute for the rule as a determinant of individual decision. The fact that participants voluntarily comply with the rule echoes previous experimental results on aversion to dishonesty (e.g. Gneezy, 2005; Sanchez-Pages and Vorsatz, 2007; Hurkens and Kartik, 2009; Lundquist et al., 2009; Fischbacher and Heusi, 2013). Data also suggest that compliance to the rule significantly decreases as the amount suggested by this rule increases. This finding is consistent with Dale and Morgan (2010) that have shown in the context of a public good game that non-binding recommendations could affect individual decisions, as long as they are seen as reasonable.

Overall, our experimental study offers precious insight on the role of information in network embedded trust games. So far, trust has been mainly addressed in the context of bilateral interactions in the experimental literature. There are however many examples of economic interactions for which trusting decision could be affected by network effects. For instance, empirical studies have shown that trust in the governing institutions is a significant determinant of (voluntary) tax compliance (e.g. Scholtz and Lubell, 1998; Torgler, 2003). In this context, it appears particularly relevant to consider how the diffusion of information regarding governments' actions across social networks could impact individuals' trust. Our findings suggest for instance that reputation building, as well as allowing for social comparison could be efficient ways to improve trust within networks.

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Chapter 2

"Trust, Social Information and Compliance in a Three-Node Network: the role of social status"*

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1. Introduction

1.1. Motivation

Trust and trustworthiness play an important role in economic interactions. Trusting behaviors have been extensively studied in the economic literature, and particularly in the experimental one, since Berg et al. introduced the first trust game in 1995. In Berg et al. (1995), two players (one sender and one receiver) interact in a complete information framework. However, bilateral exchanges are often embedded in networks: an agent (receiver) may represent the interests of multiple principals (senders). There are potential gains from trust: if an agent displays trustworthiness, then both parties are better off than in the situation in which no trade takes place. When people interact repeatedly, social information of current and past interactions are essential when individuals make their decisions in a larger network, even though the social information available is often limited and so, not complete.

People do not always exchange with others who belong to the same social status in the group. Therefore, interactions between agents with different social status within networks are common in real life situations. According to Ball et al. (2001), a person's status is a ranking in a hierarchy that is socially recognized and typically carries with it the expectation of entitlement to certain resources. Individuals can be ranked in different ways, for example based on specific skills and accomplishments. In a particular social context, an individual's status entitles to certain privileges and may affect how she interacts with others. In our experiment, and following Eckel et al. (2010) [9], high status is awarded to the best performers on a task in a preliminary stage, and therefore granted by merit or deservedness, while low status is given to bottom performers. One straight application of this setting could be in the labor market and in the relationship of trust between employees with different ranks or positions within organizations (e.g. employees exchanging with a team leader, or stakeholders interacting with a

[9] Eckel et al (2010) use performance on trivia quiz as proxy of status in order to assigned roles in a network. Best performers become central players and bottom performers became peripheral players.

CEO). Recent research findings suggest that companies might benefit by raising the level of trust in organizations (e.g. Austin, 2013; Brown et al, 2015). Lower bureaucracy, simpler procedures and higher level of productivity characterize high-trust settings.

In this study, and following Borzino et al. (2015), we implement a networked version of the standard trust game with two senders and one receiver. In Borzino et al (2015), the roles are assigned arbitrarily at the beginning of the game. However, in this study, we manipulate social status by introducing a selection mechanism in a preliminary stage of the game, as in Eckel et al (2010). Therefore, we use merit (deservedness) as proxy of high status legitimacy. This means that best performers in a preliminary unrelated task get the role of high status receivers while bottom performers become low status senders. We evaluate whether the introduction of social status has an effect in the level of trust and trustworthiness.

We manipulate in a minimal way the social information available to low status senders about the high status receiver's behavior compared with a control treatment (baseline merit) in which no social information is given to senders. In the Transparency merit treatment, senders receive information about the proportion returned by the receiver at the end of each round. In the Reputation merit treatment, both senders receive information about the average proportion returned by the receiver in the previous round. We assess how these policies alter performance when differences in social status are involved among the network members.

Another feature in our study is the introduction of a private and non-binding suggestion about the amount to send and return. This suggestion is fair because, if both sender and receiver follow the suggestion, they guarantee to each other equal expected payoffs. This suggestion mimics a social norm related to fairness and equality. Therefore, we want to evaluate whether the high status receivers and low status senders follow the suggestion making voluntary compliance of the social norm explicit.

Our results suggest that, both *ex ante* and *ex post* information given to low status senders about the high status receiver's performance, have a significant effect in investment decisions. Trust is substantially higher in the transparency merit compared with the baseline merit in the whole experiment. In the transparency merit treatment, high status receivers take advantage of the social comparison than he triggers by returning unequal proportions to the low status senders in the group. Low status senders tend to increase their investment in the following round when receiving less than other senders in their group. Besides, we find support for the "competition for cooperation hypothesis" formulated by Cassar and Rigdon (2011): low status senders receiving more, tend to send even more in the following round in order to keep the *"grace*" of the high status receiver.

In the reputation merit condition, the aggregate level of trust is not higher than in the baseline merit treatment. However, investment decisions are directly and positively related with the reputation of the high status receiver. This finding suggests that receiving information about the reputation of the high status receiver, increase the level of trust at individual level.

Interestingly, trustworthiness is not significantly different across the three treatments. Our results suggest that trustworthy behavior does not change over time implying a lack of reciprocity from the high status receivers towards the low status senders. Low status senders tend to penalize high status receivers' misbehavior by increasing the frequency of no trust in which nothing is sent. In the baseline merit condition, nothing is sent in the 49,38% of the cases, while in the reputation merit and in the transparency merit treatments goes down to 29.17% and 26.61% respectively. Therefore, the presence of social information seems to be critical in a significant increase of trust.

The exogenous non-binding suggestion has a positive and significant impact on the level of trust but high status receivers' decisions are still not affected by the non-binding recommendation.

The rest of study is organized as follows: section 1.2. provides the literature background. Section 2 expands on the experimental design and procedures. The results are described in section 3, while section 4 concludes. We also compare the results obtained in Borzino et al. (2005) with the ones from the current study. Appendix 1 of this chapter shows the results from the comparison.

2.2 .Background

Recent experimental studies focus on interaction in three–node networked trust games in which two senders interact with one receiver. Their findings suggest that the level of trust and trustworthiness are affected when the senders receive full information about the outcomes of the interactions in the group (Buskens et al., 2010; Cassar and Rigdon, 2011). In both studies, full information (Full-Info treatment) significantly increase the level of trust and trustworthiness compared with a No-Info treatment in which subjects get information about individual outcomes only at the end of each round.

It is not easy to find real life examples where the information is complete. In this study, and following closely Borzino et al (2015), we manipulate in a minimal way the information senders receive about the receiver's performance. Borzino et al (2015) analyze a three-node networked trust game in which two senders interact with one receiver. The assignment of the fixed roles (sender or receiver) is random. Their results show that social information has a significant impact on trust but not a significant effect on trustworthiness.

The difference between the current study and Borzino et al (2015) is the endogenous manipulation of the social status of the players. We are interested in the relationship of trust between senders and receiver who belong to different status. In our study, high status is awarded by merit and then used as a proxy for legitimacy. Recently, lab experiments have been conducted in order to analyze the interactions between high status individuals with low status ones. On the one hand, Bosco and Marcheselli (2005) found that performance-based allocation of statuses results in a discrimination expressed by higher status participants and; Nikiforakis et al. (2014) found that higher status subject do not hesitate to exploit those of lower status. On the other hand, Morozova (2015) found that the high status players behave fairly and altruistically towards their low status group members, as their actions can be resulting from either being oblivious to groups as such, or driven by a 'noblesse oblige' effect. The author conducted an extension of that by Chen and Li (2009), incorporating status in the group identities in a dictator and trust games. The status is given to the players according to their performance in a trial phase and so, awarded by deservedness as a proxy for status legitimacy, as in Ball et al. (2001).

Legitimacy plays an important role in the interaction between high and low status members. Turner and Brown (1978) show that the lack of status legitimacy in higher status groups results in hostility towards lower status individuals, while the lack of it biases lower status groups against higher status ones. Nadler (2002) suggests that in the latter case the low status groups would be reluctant to accept help from higher status individuals, yet the reverse effect is not be present. Liebe and Tunic (2010) test the "noblesse oblige" hypothesis in a dictator game, in which high status individuals donate more, and the higher the status of the recipient, the less she receives in donations. Furthermore, high-status individuals show a greater in-group bias if they perceive the status hierarchy as more legitimate, while low-status individuals favour high status members.

An important feature of our experiment is the introduction of a non-binding recommendation about the amount to send and return. This social norm fair in terms of expected pay-offs. Recent studies have focused on the role of non-binding suggested donations in the context of public good games. Karlan and List (2007)'s field experiment shows that suggestions do not increase the level of donations. In a lab experiment, Dale and Morgan (2010) find socially optimal suggestions to be ineffective. However, when the suggestion is more moderate, it has a significant effect compared with the control treatment. Warwick (2005) suggests that reasonable suggested donations produce an effective increase in giving compared with situation in which the suggestions are socially optimums. Weyant and Smith (2006) find similar results in their field experiment. Marks, Schanberg and Croson (1999) find in a lab experiment that suggested donations have a positive effect when the contributors obtain different payoffs from giving. From these results, one might hypothesize that non-binding suggestions may have a positive effect on trust and trustworthiness in a trust game. In this line, Borzino et al (2015) introduce a private suggestion about the level of investments and returns in a networked trust game, and find that the suggestions have a significant and positive impact on the players' decisions, when the suggestion is "reasonable".

Our study contributes to the literature by exploring the effect of manipulation of the social status in a three-node networked investment game by assigning the roles according to the performance of the players in a trial phase. Therefore, we manipulate the social status of the players using merit (or deservedness) as proxy for high status legitimacy. We also manipulate the social information available to the low status senders: they receive info about the performance of the high status receiver in the current (Transparency merit treatment) or previous round (Reputation merit treatment) related to a benchmark in which no information is provided. We want to study how social information interacts with differences in status in a networked trust game. Besides, we want to evaluate whether status differences have an effect in the compliance of the social norm.

2. Experimental design and procedures

2.1. The three players investment game

Berg et al. (1995) introduced the first trust game (or investment game), which consists in two players (one sender and one receiver) interacting sequentially. First, the sender decide how much to send to the receiver and that amount in multiply by a factor k where k>1. In a second stage, the receiver decided how much to send back to the sender.

In this work, we implement a three-node investment game with two senders and one receiver following the experimental design in Borzino et al. (2015), as presented in Figure 1.

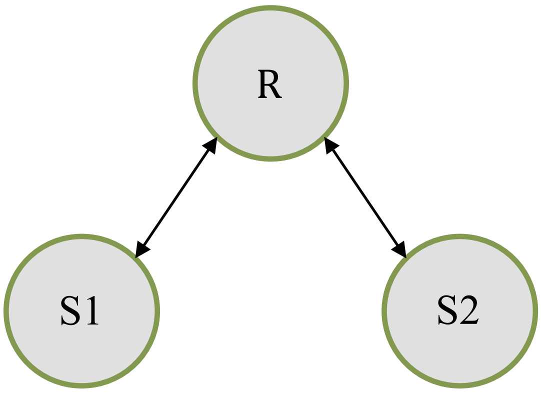

Figure 1: three-node investment game with two senders (S1 and S2) and one receiver (R)

In Borzino et al. (2015) a random assignment of roles (sender or receiver) was implemented at the beginning of the experiment. In this study, we implement a selection mechanism in which the roles are assigned according to players' performance of the players in a preliminary task. The task consists in adding two digits number for three minutes. The best performers become high status receivers and the worse performers become low status senders, following the mechanism used in Eckel et al (2010) [10]. The players are randomly assigned to a cohort of 6 players formed by four low status senders and two high status receivers. The roles are fixed for the entire duration of the game (20 periods). Each cohort represents our independent observation given that re-matching process is applied only within

[10] Eckel et al (2010) use a trivia quiz of 15 questions. In this study instead, the task consists in adding to digits numbers for three minutes. The average score was 23.59 correct answers (with a minimum of 2 and a maximum of 49).

each cohort at the end of each round. Each round consists on 5 stages display in table 1, which illustrates the sequence of the game.

Table 1: sequence of each experimental round

1st stage: Low status Senders	2nd stage: Low status Senders	3rd stage: Low status Senders	4th stage: High status Receiver	5th stage: High status Receiver
Real-Effort Task	Signal 1: die	Trust (X)	Signal 2: die	Trustworthiness (Y)

1st stage – real effort task: In this stage, low status senders perform a real-effort task for one minute. The task consists in adding two digits numbers (the same task that they perform in the trial phase) for one minute. This task gives senders the opportunity to earn their endowment[11]. Each correct answer is multiplied by a factor of 2 ECU. The number of correct answers cannot exceeds 5 meaning that the can get a maximum of 10 ECU as endowment. High status receivers do not participate in this task but receive a fixed endowment of 5 ECU at the end of each round.

2nd stage- Signal for sending decision: Each low status sender get a non-binding signal (see section 2.2) about the proportion to send by throwing a virtual die of six faces.

3rd stage- Sending decision: Each low status sender decides the proportion of her endowment to send to the high status receiver. It can be any proportion among the following: 0%, 20%, 40%, 60%, 80% or 100%. The amount sent is multiplied by four and received by the high status receiver.

4th stage- Signal for returning decision: Before deciding the proportion to return back to each of the two low status senders matched with her, the high status receiver gets two non-binding signals about the proportion to return to each low status sender. She throws a virtual die twice, one for each sender.

5th stage- Returning decision: The high status receiver decides the proportion to return back to each low status sender (S1 and S2) matched with him in the group in that particular round.

At the end of the each period, participants are informed about their individual outcome. The high status receiver gets information about the two interactions with the two low status senders while senders received only personal information about the game they have just played.

[11] See Houser and Xiao (2014) for a discussion of "house of money effects" in trust games.

The game described above corresponds to the baseline merit treatment. In the two other treatments, additional information about the performance of the high status receiver is given to the low status senders at the end (*ex post*) or at the beginning (*ex ante*) of each round accordingly. Further description of the treatments will be given in section 2.3.

2.2. The Signal

In our experimental framework, we introduced a non-binding recommendation that low status senders and high status receivers get before deciding the proportion to send or return respectively. The level of recommendation is represented by the outcome of the die (see table 2). The value of the signal is defined as:

$$S = 0.2 \cdot (D - 1)$$

The signal follows a discrete uniform distribution S ~ DU(6, 0, 0.2), which is known by the subjects since the beginning of the experiment.

Table 2: Recommendation associated to each outcome of the die

Outcome of the die:	1	2	3	4	5	6
% to keep	100%	80%	60%	40%	20%	0%
% to send to the receiver or % to send back to the sender	0%	20%	40%	60%	80%	100%

A sub-game perfect Nash equilibrium is the only equilibrium of this game if all the players are totally rational and profit-maximizers. This equilibrium can be solved by backward induction: the sender sends nothing given that she anticipates that the receiver maximizes his profit by not returning anything back. Therefore, this equilibrium is characterized by absence of interaction and their final payoffs are equal to their initial endowments. Under this scenario, the introduction of the non-binding signal does not change the theoretical prediction.

Now, let us consider the case in which both type of players, low status sender and high status receiver, totally comply with their respective signals. The recommendation S may take value $S_k \in \{0 ;\ 0.2 ;\ 0.4 ; 0.6 ; 0.8 ; 1\}$ with a fixed probability of $p_k = \frac{1}{6}$. The expected value of the exogenous signal is then 50 % as observed below:

$$E[S] = \sum_{k=1}^{6} p_k \cdot S_k = \frac{1}{6} \cdot \sum_{k=1}^{6} S_k = 0.5$$

If we assumed that the decisions of both low status sender (X) and high status receiver (Y) fully follow the outcome of the die, and then E[X]=E[Y]=0.5. The expected payoff of the full compliant low status sender (π_S^c) and the full compliant high merit receiver (π_R^c) expressions are:

$$E[\pi_S^c] = E[e - Xe + 4Xe \cdot Y] = 1.5e$$

$$E[\pi_R^c] = E[(4Xe) \cdot (1 - Y) + 5\,] = e + 5$$

where e is low status sender's endowment earned at the beginning of each round in the real-effort task. The implementation of the real effort task is aimed to avoid "windfall money effect" in the context of trust games described by Houser and Xiao (2015). The authors sustain that decisions using "own money" significantly and substantively differ from those using found money. We set an upper limit to the amount of endowment that low status senders can get in each round (10 ECU) in order to limit heterogeneity in low status senders' endowment. In fact, low status senders reached in a 93.55% of the observations the maximum endowment of 10 ECU[12]. In the presence of the maximum endowment, the expected profit of a compliant sender and a compliant high status receiver are equal:

$$[\pi_S^c | e = 10] = E[\pi_R^c | e = 10] = 15$$

The rule may be considered fair as both the low status sender and the high status receiver follow the rule guarantee equal expected payoffs by following the rule. Besides, the resulting outcomes would be significantly higher for both senders and receiver than the ones implied by the perfect sub-game Nash equilibrium, even when the social welfare is maximized when trust is complete, and senders invest 100% of their earned endowment.

[12] We find that 71 senders (over a total of 76) get the maximum endowment 10 ECU in each of the 20 rounds.

2.3. Information

In this experiment, we manipulate the information available to low status senders about the performance of the high status receiver. In the baseline merit treatment, the only information the participants get is individual (their personal interaction with the high status receiver).

In the reputation merit treatment, we give information to the low status senders about the performance of the high status receiver in the previous round. This *ex ante* information is a proxy to the reputation of the high status receiver, measured as the average proportion that that high status receiver sent to low status senders in the previous round[13]. We consider this to be minimal information about the performance of high status receivers.

In the transparency merit treatment, low status senders receive information about the performance of high status receiver in that round. This *ex post* information provides the proportion received back from the high status receiver and, besides, the proportion received back by the other low status sender in the same network. We consider this to be minimal information about the performance of high status receiver.

2.4. Procedures

All the experimental sessions were conducted at ESSEXLab in the University of Essex. We electronically recruited 114 participants, mainly business and economics undergraduate students, all inexperienced in trust games. We collected data from six independent observations of six participants for the baseline merit and the reputation merit and seven for the transparency merit. On average, a session lasted 100 minutes, including initial instructions, quiz, trial phase, final questionnaire and payment of the subjects. The average payment was around £12.50, including a show up fee of £5. The instructions were read aloud. The only difference in the instructions was the information available, according to each treatment. The experiment was computerized using Z-TREE (Fischbacher, 2007).

[13] As an example, suppose that that receiver returned to one sender 20% and to the other sender 40% in t-1. This means that, at the beginning of the following round, the senders will observe that the receiver's reputation will be 30%. Note, that there is a re-matching inside each cohort of 6 players each round.

2. Results

3.1. Trust and trustworthiness across treatment

3.1.1 Descriptive statistics

Table 3 reports average trust and average trustworthiness for the whole experiment, and for each treatment. We define "trust" as the proportion of endowment sent by senders to the receivers and, "trustworthiness" as the proportion of the amount received that the receiver decided to send back to the sender. In this experiment, roles are assigned according to the performance of the players in a preliminary stage of the game. The best performers have the role of high status receivers and the bottom performers the role of low status sender.

In table 3, Wilcoxon-Mann Whitney tests are used to test any significant differences between treatments. The first part of table 3 shows the results for senders (for the three treatments and for each treatment). The first column illustrates the average proportion sent, including the cases in which no amount was sent. Trust is slightly higher in the transparency merit treatment relative to the baseline and for the reputation merit treatment, however differences are not statistically significant (p=0.1531 and p=0.3914, respectively). In the second column, we consider only the situations in which the trust is positive and, again we find no significant treatment differences.

In the third column, we display the proportion of no trust at all. The proportion of interactions in which nothing was sent (no trust), is significantly lower in the transparency merit (26.17%; p=0.0302) and in the reputation merit (29.17%; p=0.0149) relative to the baseline merit condition (49,38%). Additionally, we observe large differences among treatments when we consider only the cases in which high status receivers face no choice i.e. both low status senders send nothing in the same round. This occurs in 65 out of 240 observations (27,08%) in the baseline merit; 19 out of 240 observations (7,91%) in the reputation merit and; 22 out of 280 observations (7,85%) in the transparency merit treatment.

Table 3: Descriptive statistics for trust and trustworthiness

	Trust – (Proportion X sent by senders)			
	Amount sent (Including X=0)	Amount sent (X>0 only)	Proportion of X=0	Average profit of sender (in £ / $)
Total	0.3684 (0.3564)	0.5633 (0.2904)	34.61%	£6.38 / $10 (3.42) (5.60)
Baseline Merit	0.2887 (0.3468)	0.5703 (0.0177	49.38%	£7.10 / $11.62 (3.33) (5.46)
Reputation Merit	0.3758 (0.3432)	0.5305 (0.0157)	29.17%**	£6.30 / $10.32 (3.31) (5.43)
Transparency Merit	0.4303 (0.363)	0.5863 (0.0146)	26.61%**	£5.84 / $9.57 (3.48) (5.71)
	Trustworthiness– (Proportion Y returned by receivers)			
	Amount returned (Including X=0)	Amount returned (X>0 only)	Proportion of Y=0 (X>0 only)	Average profit of receiver (in £ / $)
Total	0.2077 (0.2692)	0.2399 (0.2864)	49.65%	£5.24 / $8.59 (0.27) (0.44)
Baseline Merit	0.1675 (0.2899)	0.23 (0.3424)	57.92%	£5.19 / $8.51 (0.27) (0.44)
Reputation Merit	0.207 (0.2452)	0.2123 (0.2543)	47.29%	£5.26 / $8.61 (0.26) (0.42)
Transparency Merit	0.2435 (0.2666)	0.269 (0.2703)	43.75%	£5.28 / $8.64 (4.108) (0.44)

Notes: Standard deviations are displayed in parentheses; Stars report significance level from Wilcoxon Mann-Whitney tests run on independent observations (cohorts of 6 participants) to confirm differences with the baseline treatment.
*90% significance ** 95% significance *** 99% significance

The bottom part of table 3 reports trustworthiness, i.e. the decisions of the high status receivers. In the first column, we consider all the decisions including the cases in which no-trust is displayed by the low status senders (X=0). The proportion returned by the receivers is slightly higher in the transparency and in the reputation merit treatments compared with the baseline, but not significant (p=0.1495 and p=0.1331).

When we consider only the interactions where the trust displayed was positive (X>0) in the second column, average trustworthiness is lower in the reputation merit but higher in the transparency merit related to the baseline merit. These differences are again not statistically different (p=0.7761 and p=0.5348 respectively).

In the third column, we show the proportion of observations in which nothing is returned when the amount received was positive (X>0). High status receivers tend to return more positive amounts in the reputation and in the transparency merit treatment but these differences are not statistically different from the baseline (p=0.3017 and p=0.4795, respectively). High status receivers return nothing to the low status sender (who sent a positive amount in first place) 139 times out of 240 observations (57.92%) for the baseline merit; 227 times out of 480 observations (47.29%) in the reputation merit treatment and; 140 times out of 320 observations (43.75%) for the transparency treatment.

Result 1: The proportion of No-Trust significantly decreases with social information (Reputation and Transparency), when receivers deserve their central position.

Albeit low status senders give slightly more in the transparency and in the reputation conditions, they do not receive more in return compared with the baseline. This is the reason why senders' average payoff is lower in the transparency merit and in the reputation merit treatments in comparison with the baseline merit but the differences are insignificant in both cases.

3.1.1 Trust

Figure 2 shows the average proportion of the endowment sent by the low status senders by treatment and period. It suggests that trust is mainly lead by differences in dynamics across treatments. Trust does not vary over time in the baseline merit (Spearman's rho=-0.0619; p=0.1758) and in the transparency merit (Spearman's rho=-0.0571; p=0.1770), but tends to decrease in the reputation merit treatment (Spearman's rho=-0.0967; p=0.0341).

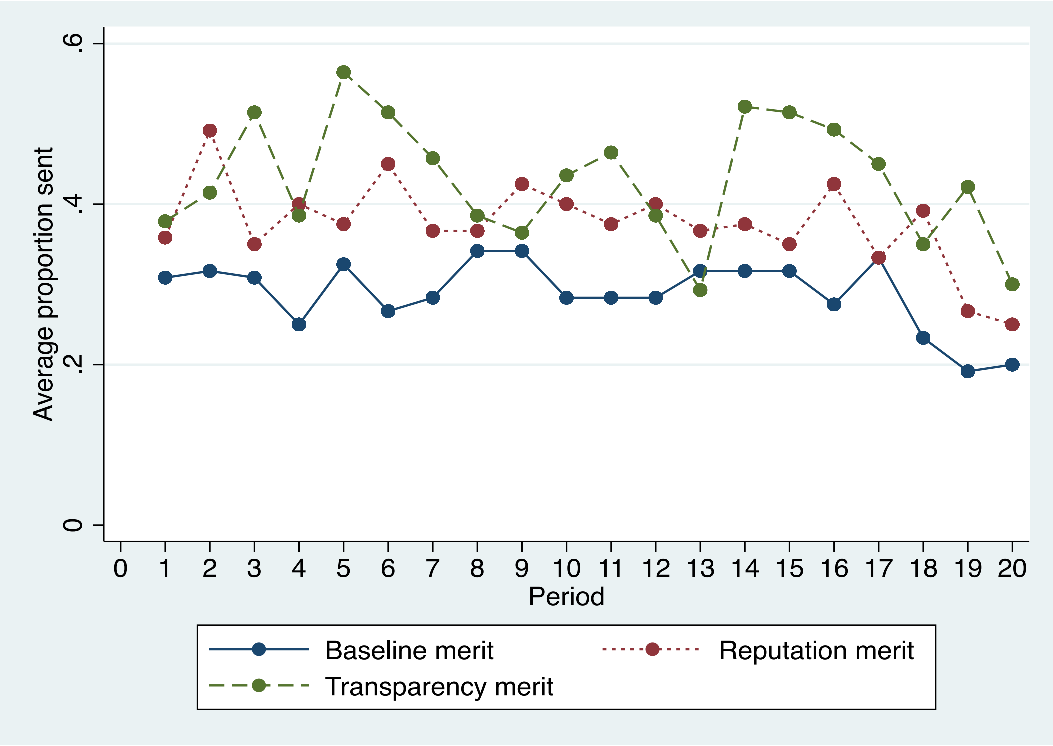

Figure 2: Average proportion of endowment sent (trust) by treatment over the 20 periods

The levels of trust in the reputation merit and in the transparency merit treatments tend to be above the one observed in the baseline.

Figure 3 shows the percentage of cases of no trust divided in blocks of 10 periods, i.e. the first 10 periods and the last 10 periods of the game. It seems that the number of decisions in which nothing is sent does not vary over time in the baseline merit (Spearman's rho=0.0661; p=0.1480), but increases in the reputation merit (Spearman's rho=0.0906; p=0.0472) and in the transparency merit (Spearman's rho=0.1244; p=0.0032). Consistent with result 1, we observe that the proportion of nothing sent in the first and in the second block of 10 periods are significantly lower in the transparency merit (p=0.0173 and p=0.0376, respectively) and in the reputation merit (p=0.0387 and p=0.0273, respectively) than in the baseline merit.

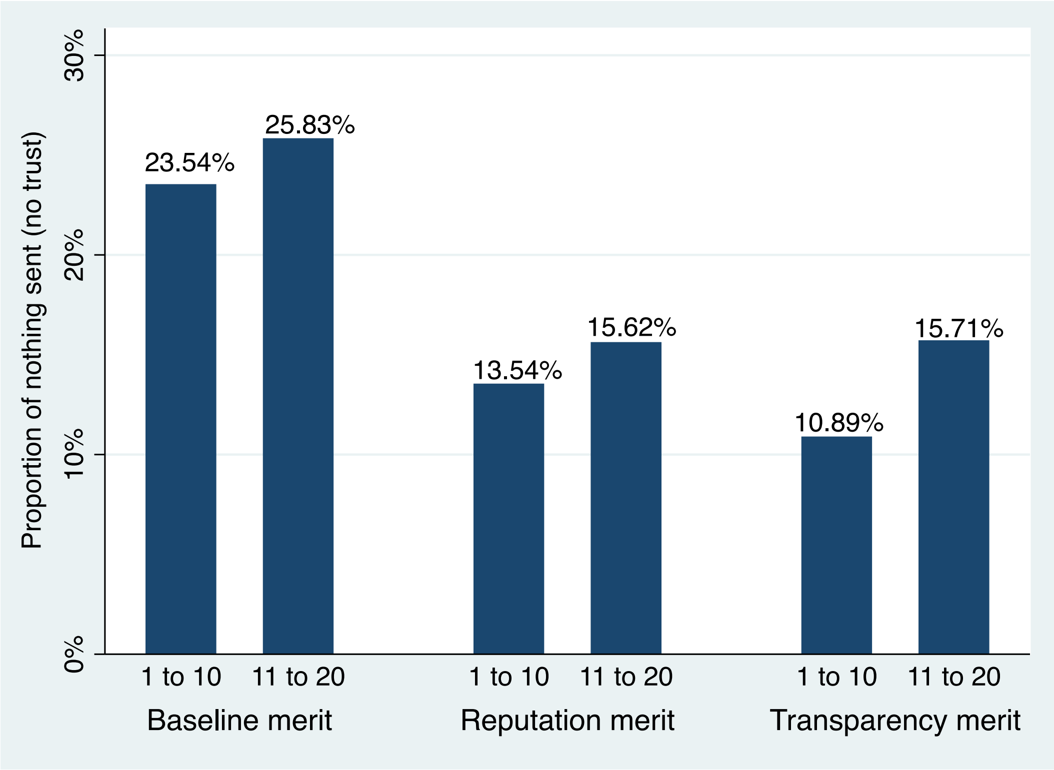

Figure 3: Average proportion of nothing sent (no trust) by treatment in block of 10 rounds

Result 2: No-Trust does not vary over time in the baseline merit, however it increases in the reputation and transparency merit treatments.

Table 4 reports the econometric regressions for the proportion sent[14] by the low status senders. We control for past decisions for the outcome of the die, period and demographic variables.

From column (1), trust is significantly higher in the transparency merit treatment than in the baseline. Trust increases over time and we also observe that the outcome of the die has a positive impact in the decision of the low status senders. Compliance to the exogenous rule will be discussed in section 3.3.

In column (2), we add additional controls in order to have a better understanding of the determinants of the trust. We introduce an interactive variable for the reputation of the high status receiver (*Reputation merit treatment x reputation of the high status receiver*). This is a continue variable which capture the effect of the reputation of the high status receiver in the low status senders' decisions. The

[14] Sender can send 0%, 20%, 40%, 60%, 80% or 100% of their endowment. Given the nature of the dependent variable, we decided to run a random effect ordered probit clustering by independent observation, rather than standard random effect regression.

coefficient is positive and significant suggesting that investment decisions are directly related to the reputation of the high status receiver. However, we do not observe that there is an aggregate effect in the level of trust in the reputation related to baseline (column 1) given that the coefficient of *Reputation treatment* is positive but insignificant. We will discuss extensively the role of the high status receivers' reputation in section 3.2.1.

Table 4: Random-effects ordered probit for the trust- pooled sample

	Proportion sent by Senders (Xit)	
	(1) Pooled 1 Merit	(2) Pooled 2 Merit
Outcome of the die (Rule)	0.172*** (0.0251)	0.177*** (0.0278)
Baseline Merit Treatment	*Ref*	*Ref*
Reput. Merit Treatment	0.6070 (0.4310)	0.4729 (0.4509)
Reput. Merit Treat x reputation of high status receiver	-	0.6613*** (0.217)
Transp. Merit Treatment	0.7837* (0.0085)	-
Transp. Merit Treatment x received as much as other low status sender	-	0.4661 (0.4655)
Transp. Treatment x received less	-	0.3522*** (0.0824)
Transp. Treatment x received more	-	0.6146*** (0.145)
Period	0.0179** (0.0085)	-0.0197*** (0.0084)
Female	-0.2866 (0.2423)	-0.2464 (0.239)
British	-0.0574 (0.2568)	-0.0506 (0.2593)
Log pseudo likelihood	-2159.5779	-2032.5032
Observations	1,520	1,520
Number of indiv	76	76

We also introduced in column (2), three interactive variables for the transparency merit treatments: *Transparency Merit Treatment x received as much as the other low status sender, Transparency Merit Treatment x received less, Transparency Merit Treatment x received more.* We capture the role of social comparison between low status senders after knowing the amount received by the other sender from the same high status receiver relative to the baseline case in which both senders receive back the same. Participants who received less than the other low status sender in the previous round tend to significantly increase the amount sent in t. Yet, low status senders who received more than the other sender also tend to significantly increase the level of trust in order to keep the "*grace*" of the high status receiver triggering a competition between senders in order to keep receiving the rewarding returns of the high status receiver. This last finding is consistent with the *competition for cooperation hypothesis* formulated by Cassar and Rigdon (2011). We will discuss more extensively the role of social comparison in section 3.2.2.

3.1.2. Trustworthiness

The average proportion returned by the high status receivers in the different treatments over the 20 rounds is displayed in Figure 4.

Consistent with Result 1, when we consider the 20 rounds, the level of trustworthiness is not significantly different in the reputation merit and in the transparency merit treatments in comparison with the baseline merit. From Figure 4, it tends to decrease over time in the baseline merit (Spearman's Rho=-0.0950; p=0.0375), in the reputation merit (Spearman's Rho=-0.1355; p=0.0029) and, in the transparency merit treatment (Spearman's Rho=-0.1448; p=0.006).

Column (1) of table 5 reports estimates of the determinants of trustworthiness at individual level across treatments considering only the cases of positive trust. The variable *Lagged total proportion sent* corresponds to the sum of decisions of the low status senders in the previous round and, *Lagged total proportion returned* relates to the sum of the proportion returned by the receiver in t-1. As coefficients are not significant, we do not observe reciprocity from the high status receivers. The rule does not seem to have a significant effect in the decisions of the high status receivers either.

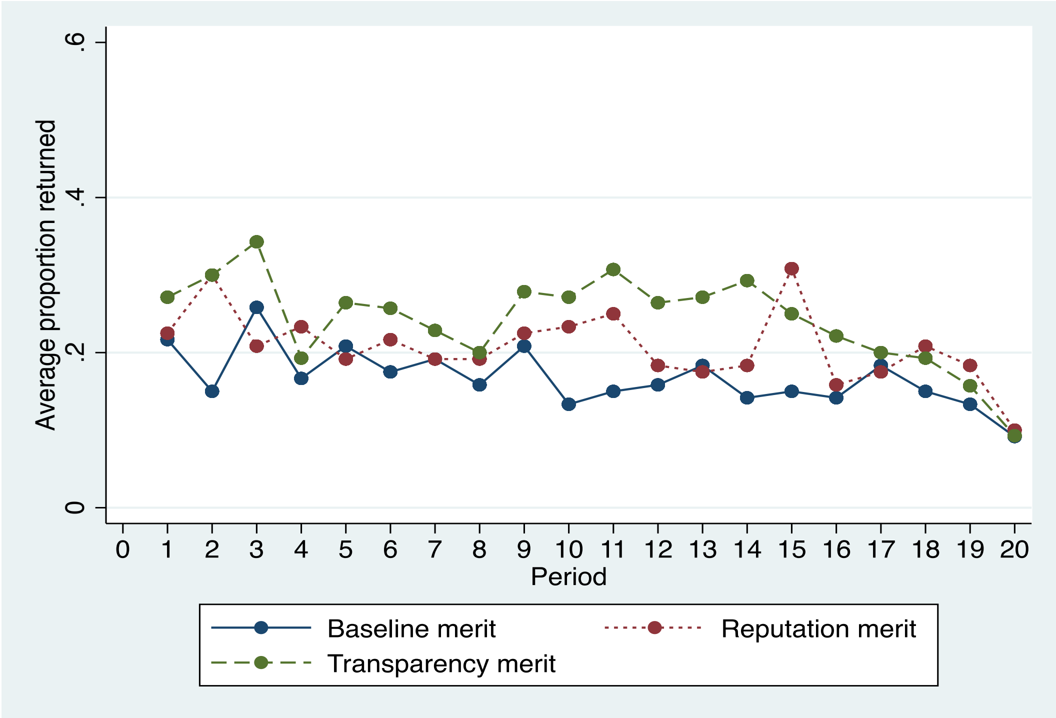

Figure 4: Average proportion returned (trustworthiness) by treatments over the 20 periods

Columns (2) to (4) display results of the within-treatments estimations. We do not observe any significant determinant of the trustworthiness, as in column (1). We do observe that high status female receivers tend to return less than males. This gender effect is significant when we consider all treatments (column 1) and in the within-treatment regression for transparency (column 3). The percentage of high status female receivers was 34% in the baseline, 50% in the reputation and, 43% in the transparency treatment.

Result 3: Trustworthiness is not driven by reciprocity from the high status receivers. No sign of "noblesse oblige" from the high status receivers with respect to the low status senders.

Table 5: Random-effects ordered probit regressions for trustworthiness- Pooled and Specific Samples

	Proportion returned by Receivers (Yit)			
	(1) Pooled Merit	(2) Baseline Merit	(3) Reputation Merit	(4) Transparency Merit
Lag Total Proportion Sent	-0.103	0.0203	-0.216	-0.106
	(0.117)	(0.140)	(0.270)	(0.186)
Lag Total Returned	0.119	0.156	0.182	0.0634
	(0.128)	(0.379)	(0.189)	(0.211)
Proportion sent by low status sender	0.365	0.516	0.544	0.174
	(0.267)	(0.777)	(0.503)	(0.247)
Proportion sent by other low status sender	0.160	0.381	0.204	0.0429
	(0.170)	(0.312)	(0.224)	(0.288)
Outcome of Die (Rule)	0.0543	-0.0459	0.0894	0.0817
	(0.0376)	(0.0288)	(0.0645)	(0.0599)
Baseline Merit Treatment	*Ref*	-	-	-
Reputation Merit Treatment	0.167	-	-	-
	(0.346)	-	-	-
Transparency Merit Treatment	0.179	-	-	-
	(0.387)	-	-	-
Period	-0.0232***	-0.0327**	-0.0208	-0.0210**
	(0.00779)	(0.0129)	(0.0150)	(0.0107)
Female	-0.540**	-0.749	-0.365*	-0.656
	(0.210)	(0.824)	(0.210)	(0.405)
British	-0.102	-0.532	0.264	-0.0177
	(0.358)	(1.140)	(0.185)	(0.590)
Log pseudo likelihood	-1318.2005	-298.9212	-440.8670	-553.4704
Observations	958	248	321	389
Number of individuals	38	12	12	14
Robust standard errors in parentheses*** p<0.01, ** p<0.05, * p<0.1				

3.2. The impact of social information in investment decisions

3.2.1. Reputation of the high status receiver

In the reputation merit treatment, low status senders get information about the reputation of the high status receiver (i.e. the average proportion returned to both low status senders in the previous period) before making their own decisions. In table 4 (column 1) the proportion sent in the reputation merit treatment is not significantly affected by the *ex ante* information compared with the baseline merit. Consistent with result 1 and as showed in Figure 3, the lack of reciprocity leads to situations in which no trust is displayed, particularly in the second part of the experiment. As suggested on table 4, we observe that the reputation of the high status receiver has a positive and significant impact on the level of trust. Figure 5 represents a frequency matrix comparing the proportion sent according to the reputation of the receiver.

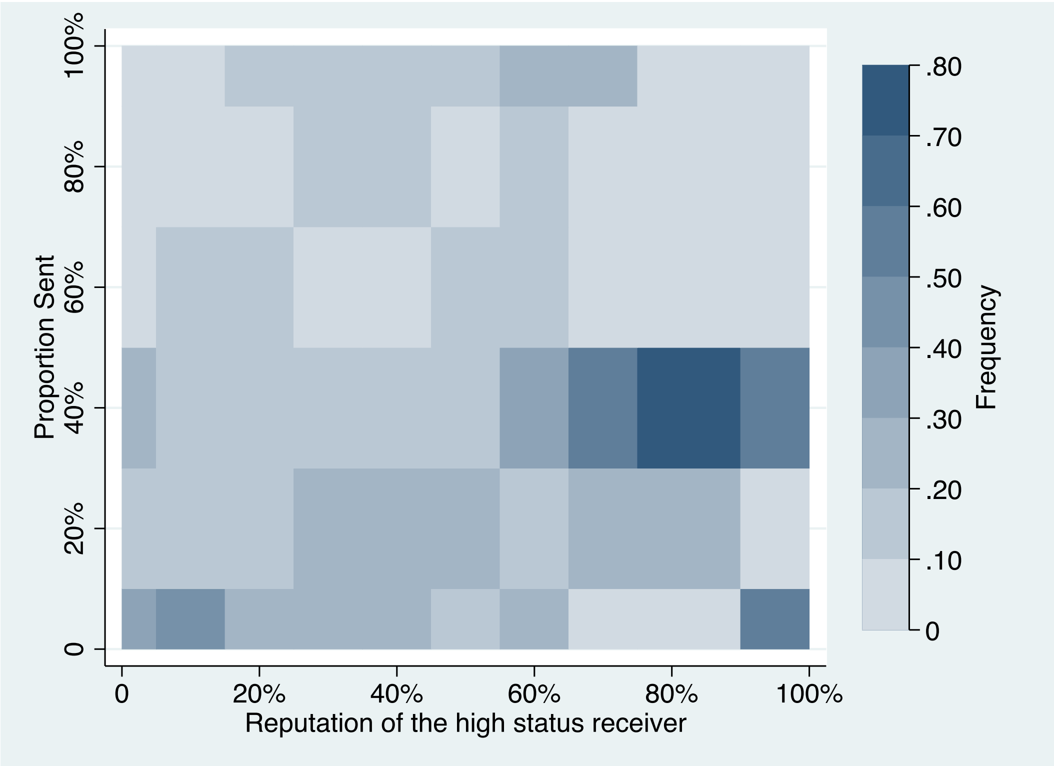

Figure 5: matrix of frequencies of the proportion sent related to the reputation of the receiver

Figure 5 strongly suggests that the proportion sent increases with high status receiver's reputation merit, albeit less than proportionally. When the reputation of the receiver with status is high (more than 60% to 100%), the modal is to send 40% of the endowment.

Table 6 shows the determinants of trust within-treatment for the baseline merit, reputation merit and transparency merit. The outcome of the die has a significant and positive effect in the decisions of low status senders.

Besides, we control for demographic variables. We observe that low status female senders tend to give less than males in the baseline merit and in the transparency merit treatment, however statistically significant only in the baseline. This is not the case in the reputation merit condition: female participants tend to send significantly more than males[15].

Table 6: Random effects ordered probit regressions for trust- treatment-specific samples

	Proportion Sent by Senders (Xit)		
	(1) Baseline Merit	(2) Reputation Merit	(3) Transparency Merit
Outcome of the die (Rule)	0.119**	0.140***	0.259***
	(0.0472)	(0.0390)	(0.0360)
Receiver with no reputation[16]	-	-0.5751	-
		(0.196)	
Reputation of the high status receiver	-	0.4779*	-
		(0.2566)	
Senders receive equal proportions			*Ref*
Sender receives more than the other sender	-		0.6985***
			(0.170)
Sender receives less than the other sender	-		0.4284***
			(0.0805)
Period	-0.0201	-0.0185	-0.0177
	(0.0153)	(0.0130)	(0.0153)
Female	-0.667**	0.5753	-0.3985
	(0.304)	(0.3625)	(0.3224)
British	-0.0175	-0.1810	0.0942
	(0.501)	(0.5672)	(0.1884)
Log pseudo likelihood	-548.10422	-652.9555	-839.30545
Observations	480	480	560
Number of indiv	24	24	28
Robust standard errors in parentheses*** p<0.01, ** p<0.05, * p<0.1			

Reputation of the receiver, consistently with Table 4 and Figure 5, has a positive and significant impact on investment decisions. Table 7 reports for each potential reputation of the high status receiver, the frequencies and the predicted probabilities to observe respectively no trust (X=0) and total trust (X=1), using the model of Table 6 column (2). Our model predicts that the probability for a low

[15] The percentages of low status female senders are 71% in the baseline merit, 62% in the reputation merit and, 54% in the transparency merit treatment.

[16] Receivers that did not receive any endowment in previous period could not make any choice and therefore did not build any reputation. As such, senders were only informed that the receiver could not make any choice.

status sender to send nothing is reduced in 10.88 percentage points when the reputation is 0% rather than 100%. In the same way, the predicted probability of send everything increases from 6.13%, when the reputation is 0%, to 13.69% when the reputation is 100%.

Result 4: In the reputation merit treatment, investment decisions are directly related with the reputation of the high status receiver.

Table 7: Predicted probabilities for the trust

Reputation	Frequency	Predicted probability for no trust (X=0)	Predicted probability for full trust (X=1)
0%	37	.2218***	.0613***
10%	23	.2088***	.0669***
20%	33	.1963***	.0730***
30%	22	.1842***	.0794***
40%	25	.1726***	.0863***
50%	15	.1615***	.0936***
60%	4	.1508***	.1013***
70%	2	.1406***	.1095***
80%	2	.1309 ***	.1182***
90%	0	.1217 ***	.1237 ***
100%	1	.1130 ***	.1369 ***

Notes: the reputation of the high status receiver is displayed to the two low status senders, but reported only once in this table. Predicted probabilities are computed for average values of every variable except *reputation of the high status receiver* and *no reputation*
*90% significance ** 95% significance *** 99% significance

3.2.2 Social comparison

In the transparency merit treatment, the low status sender receives information about the proportion returned to her and to the other sender. Results 1 and 2 show that trust is higher in the transparency merit treatment than in the baseline. We also reported the determinants of trust in column (2) of Table 4. They suggest that low status senders react when they receive less than the other sender in the group, but also when they receive more than the other counterpart.
Column (3) of Table 6 reports the regression for the transparency merit treatment and allows us to understand the determinants of trust within the treatment. When

receiving less than the other sender from the same high status receiver, individuals tend to increase the proportion sent in the following round. This result suggests an upward social comparison (i.e. comparison with others who are better off or superior) from the senders who received less. This finding is in line with previous experimental results in Borzino et al (2015). Senders also trust more when they received more than the fellow sender in the previous round compared to the baseline, relative to the case in which both receive the same. In a sense, competition between the low status senders makes senders who received the higher return to increase the amount invest in order to keep receiving generous return from receivers in the following round. This finding is consistent with the *"competition for cooperation hypothesis"* as in Cassar and Rigdon (2011).

Social comparison increases trust. Inequality in the reciprocity of the high status receivers might boost trust levels of senders in the network without an increase in the level of trustworthiness[17].

Result 5: In the transparency merit condition, senders receiving less than the other sender, tend to invest more in the following round. Furthermore, senders who received more than the other sender also tend to increase the level of trust relative to the case in which both senders receive the same amount in return. Receivers exploit the social comparison and do not increase the returns.

3.3. Compliance

3.3.1. Compliance from the low status senders

In the previous estimates, we controlled for the outcome of the die and, our results suggest that it has a positive and significant effect in the decision of the senders (see Tables 4 and 6).

In table 8, we report the percentage of cases in which low status senders follow the rule; send more than the rule and less than the rule per treatment. Our results show no significant difference among treatments in following the rule. However, senders in the transparency merit treatment, low status senders tend to over-

[17] We have implemented a stranger matching protocol, i.e. participants are re-matched every round in network. The small size of our cohort augments the effect of the social information on the level of trust.

comply (under-comply) with the rule more (less) frequently than in the baseline (p=0.0498 and p=0.00297, respectively).

Table 8: Descriptive statistics- Compliance to the rule- Low status Senders

Proportion of senders FOLLOWING the rule			
Total	Baseline merit	Reputation merit	Transparency merit
30.65%	30.20% (0.3512)	28,96% (0.3447)	32.50% (0.3601)
Proportion of senders SENDING MORE than the rule			
Total	Baseline merit	Reputation merit	Transparency merit
22.30%	16.87% (0.2664)	23.33% (0.2877)	26.07%** (0.2748)
Proportion of senders SENDING LESS than the rule			
Total	Baseline merit	Reputation merit	Transparency merit
47.03%	52.91% (0.220)	47.70% (0.2205)	41.42%** (0.2439)

Notes: Standard deviations are displayed in parentheses; Stars report significance level from Wilcoxon Mann-Whitney tests run on independent observations (cohorts of 6 participants) to confirm differences with the baseline merit treatment.
*90% significance ** 95% significance *** 99% significance

We do not find any difference in the compliance to the rule in the reputation merit treatment compared with the baseline.

Table 9 reports the results of a regression for the compliance of the rule by low status senders. As in Table 8, we do not find any significant difference between treatments. However, consistent with Table 8, low status senders send more than the rule more often in the transparency merit compared with the baseline. In Table 9, the variable *Outcome of the die (Rule)* suggests that, as the suggested rule increases, the low status senders tend to follow less the rule. In column 3, we also see that female low status senders send significantly more than the rule less often than male counterparts. *Age* is positively related to the compliance of the rule.

Figure 6 relates the investment decisions with the exogenous rule. Senders tend to follow the rule, particularly when the proportion suggested to send is from 0% to 60%. When the outcome of the die suggests sending more than 60%, the modal is to send 0%. This suggests that low status senders follow the rule as long as it seems “reasonable” in line with previous experimental findings in Borzino et al (2015).

Table 9: Random effect probit- Compliance to the rule-Low status Senders

	Individual decision corresponds the rule suggested by the die		
VARIABLES	(1) Follow the rule	(2) Send less than rule	(3) Send more than rule
Baseline merit	*Ref*	*Ref*	*Ref*
Reputation merit	0.0334	-0.181	0.295
	(0.110)	(0.202)	(0.345)
Transparency merit	0.183	-0.306**	0.274**
	(0.118)	(0.226)	(0.369)
Outcome of the die (Rule)	-0.153***	0.559***	-0.559***
	(0.0493)	(0.0429)	(0.0554)
Period	-0.00774	0.0132	-0.00411
	(0.00769)	(0.00914)	(0.00827)
Lagged payoff	0.0148	0.114***	-0.185***
	(0.0122)	(0.0255)	(0.0278)
Female	0.126	0.190	-0.368**
	(0.138)	(0.174)	(0.181)
Age	-0.0110	-0.0430**	0.0425***
	(0.00946)	(0.0167)	(0.00683)
British	-0.0160	-0.0266	-0.0190
	(0.160)	(0.197)	(0.185)
Constant	0.0325	-2.067***	0.967**
	(0.303)	(0.424)	(0.444)
Observations	1,439	1,439	1,439
Number of ind.	76	76	76

Robust standard errors in parentheses*** p<0.01, ** p<0.05, * p<0.1

Compliance to the rule has a negative and significant correlation with period, suggesting that the low status senders tend to comply less over time in the transparency merit (Spearman's rho=-0.0912; p=0.0309). However, in the baseline merit and in the reputation merit treatments, senders comply to the rule more over time but not significantly (Spearman's rho=0.0051;p=0.9110 and Spearman's rho=0.0203; p=0.6571, respectively).

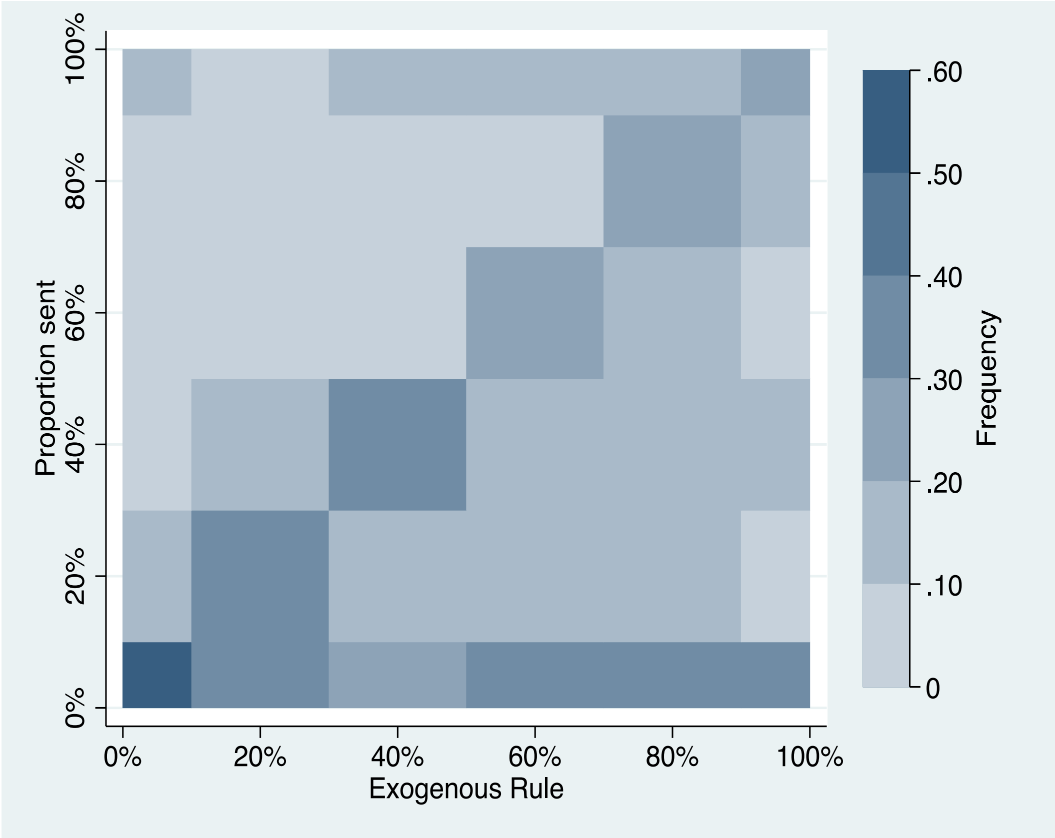

Figure 6: matrix of frequencies of the proportion sent related to the exogenous rule

3.3.2. Compliance from the high status receivers

Our previous regressions suggest that the outcome of the die has no significant impact in the decision of the high status receivers in the reputation, transparency and baseline treatments (see table 3).

Table 10 presents the percentage of times in which receivers follow the rule, send less and send more than the rule in each treatment. Receivers follow the rule slightly more in the reputation merit and in the transparency merit treatment than in the baseline merit but these differences are not significant. Receivers also return less than the rule in significantly fewer cases in the reputation merit than in the baseline merit treatment. No difference is found in the transparency merit compared with the baseline merit treatment.

Table 10: Descriptive statistics- Compliance to the rule - High status Receivers

Proportion of receivers FOLLOWING the rule			
Total	Baseline merit	Reputation merit	Transparency merit
21.71%	18.33% (0.2819)	25.62% (0.2697)	21.25% (0.2735)
Proportion of senders RETURNING MORE than the rule			
Total	Baseline merit	Reputation merit	Transparency merit
14.80%	14.16% (0.3200)	13.45% (0.2107)	16.42% (0.2045)
Proportion of senders RETURNING LESS than the rule			
Total	Baseline merit	Reputation merit	Transparency merit
63.48%	67.50% (0.1340)	60.83%** (0.1973)	62.32% (0.2005)

Notes: Standard deviations are displayed in parentheses; Stars report significance level from Wilcoxon Mann-Whitney tests run on independent observations (cohorts of 6 participants) to confirm differences with the baseline treatment.

*90% significance ** 95% significance *** 99% significance

Table 11 displays the regressions for the compliance to the rule by high status receivers. Consistent with table 10, high status receivers return more than the rule more frequently than in the baseline. However, no significant treatment differences are observed when we analyse the decision of *follow the rule* and *send more than the rule* among treatments. The variable *Outcome of the die (Rule)* is negative and significant in the column (1) suggesting that the higher the suggested proportion to send, the lower the tendency to follow this suggestion. High status female receivers send less than the suggested rule more than males.

Table 11: Random effect probit- Compliance to the rule- High status Receivers

	Individual decision corresponds the rule suggested by the die		
	(1)	(2)	(3)
VARIABLES	Follow the rule	Send less than rule	Send more than rule
Baseline merit	*Ref*	*Ref*	*Ref*
Reputation merit	0.279	-0.614**	0.435
	(0.204)	(0.239)	(0.359)
Transparency merit	0.0923	-0.493	0.536
	(0.187)	(0.309)	(0.364)
Outcome of the die (Rule)	-0.427***	0.864***	-0.622***
	(0.0666)	(0.103)	(0.0857)
Period	-0.00425	0.00883	-0.00647
	(0.00816)	(0.0109)	(0.0129)
Lagged payoff	-0.0247	0.152	-0.271
	(0.155)	(0.202)	(0.242)
Female	0.122	0.213	-0.524*
	(0.323)	(0.264)	(0.272)
Age	0.00409	-0.0233	0.0200
	(0.00816)	(0.0202)	(0.0259)
British	-0.114	0.205	-0.0189
	(0.134)	(0.269)	(0.274)
Constant	0.359	-2.402**	1.367
	(1.017)	(1.097)	(1.757)
Observations	1,444	1,444	1,444
Number of indiv	38	38	38

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure 7 displays the matrix of frequencies linking the proportion returned and the outcome of the die. The rule has not a significant effect in the level of trustworthiness. Only few high status receivers comply with the rule when it is below 50% and then, suggestions related to small return are often followed. However, the modal is to return nothing whatever the level of the suggested rule. Consistent with Table 5, it appears that the outcome of the die has not a significant impact in the decisions of the receivers.

Result 6: The exogenous rule has a positive and significant impact on senders' decisions. The rule has not a significant impact in the decisions of receivers

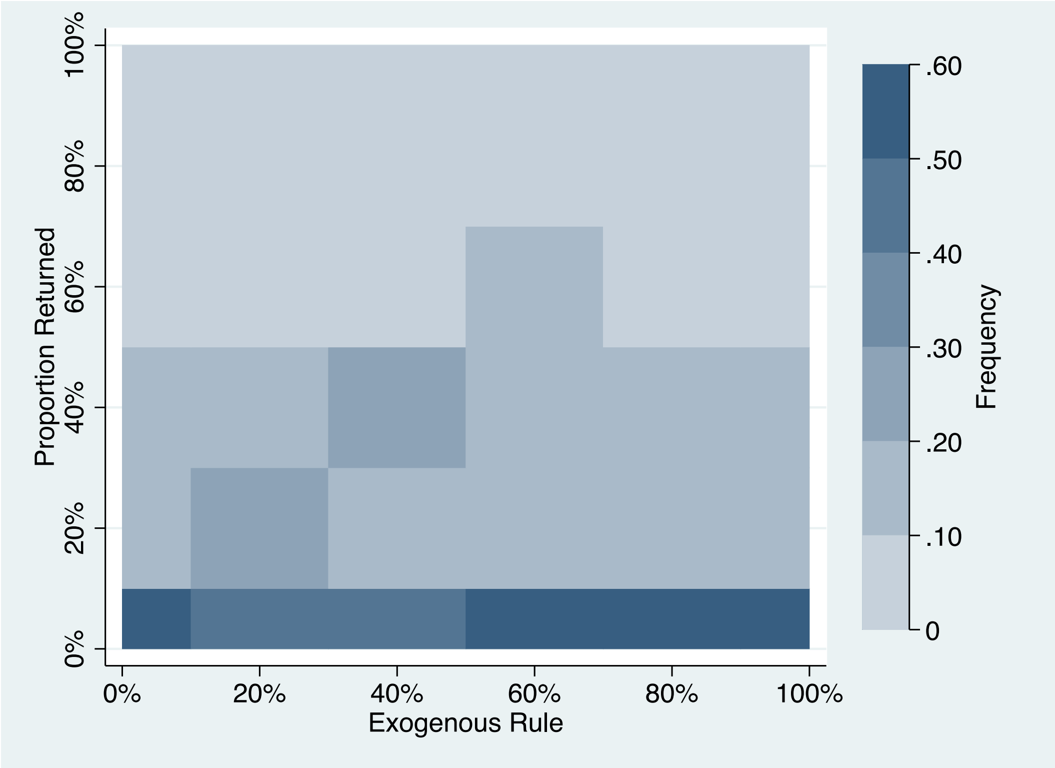

Figure 7: matrix of frequencies of the proportion returned related to the exogenous rule

4. Conclusion

Trust is an essential component of economic and social interactions. For this reason trust has been extensively studied in the economic literature, particularly in the experimental one since Berg et al (1995) introduced the first trust game. In this game, two players interact in a full information setting. However, there are plenty of examples in real life where more than one sender exchange with one receiver. Previous experimental works studied trust and trustworthiness between two senders and one receiver in a network with complete information (Buskens et al., 2010; Cassar and Rigdon, 2011). Their findings suggest that full information regarding actions and outcomes in the network have a positive and significant impact in the level of trust and trustworthiness. However, it is quite rare to find situations in real life in which the information available to the players is complete. In this line, Borzino et al (2015) implemented a controlled laboratory study in which two senders interact repeatedly with a receiver in a three-node network with minimal manipulation of the social information available to the senders about the performance of the receiver in that particular round or in the previous round, according to the treatment.

Borzino et al (2015) introduced a non-binding recommendation to participants about the proportion to invest or return. Their findings suggest that the recommendation has a positive and significant impact on the decisions. The suggested rule has two main characteristics: fair and [partially] efficient. Fair because, by following the suggestion, the players guarantee equal expected payoffs and, [partially] efficient because even when the suggestion generates moderate efficiency gains, they could decide not to follow the rule and send (return) even more than the proportion suggested expanding the collective welfare. The fact that participants voluntarily comply with the rule reiterates previous experimental studies related to aversion of dishonesty (e.g. Gnezzy, 2005; Sanchez-Pages and Vorzatz, 2007; Fischbacher and Föllmi-Heusi, 2013).

We closely replicate the experimental design of Borzino et al (2015), but with the implementation of a selection mechanism in order to manipulate the social status of the players in the network. Participants complete a task in a trial phase and we reward the best performers with prominent role in the experiment of high status receivers. Bottom performers are assigned the role of low status senders. We evaluate whether social status has an effect on the level of trust and trustworthiness in the different information treatments.

Our findings suggested that reputational information of the high status receiver has a significant impact on the level of trust at individual level. In the transparency merit treatment, each sender receives information about proportion returned to the other low status sender by the same high status receiver in the network. We found that the level of trust is significantly higher than in the baseline merit treatment. We found that the decision of senders is lead by those who received less than the other sender in the network and also by those who receive more than the other sender. This finding is consistent with the "competition for cooperation hypothesis" formulated by Cassar and Rigdon (2011): the low status sender who observes that the other low status sender received less than him, tends to increase his level of trust in the following round in order to keep the *"grace*" of the high status receiver. High status receivers take advantage of social comparisons generated and keep trustworthiness low.

We studied the impact of the suggestion in the decision of players. We observed that while the non-binding recommendation has a positive and significant impact on the level of trust, it does not have a significant effect in the receivers' decisions.

Overall, our experimental offers an important insight about the effect of social information in networked investment games when social status is manipulated. Across treatments, we do not find signs of reciprocity, and so of “noblesse oblige” between receivers and senders. Senders penalize receivers by increasing frequency of cases in which no trust is displayed. In fact, in the baseline merit condition, the 49,38% of participants’ decisions are in line with the Nash equilibrium. The introduction of social information decreases that percentage to 29.17% in the reputation merit and to 26.61% in the transparency merit. Trust could be efficiently increased by social comparison, as well as reputation building in a network characterized by differences in status.

Furthermore, we compared the results from Borzino et al. (2015) (Chapter 1 of this thesis) with the results from this current study. The comparison of the two studies can be found in the Appendix 1 of this Chapter.

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Chapter 3

“Network, Spillovers and Compliance”*

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1. Introduction

Tax evasion is an illegal concealment of a taxable activity and a worrisome phenomenon. Measuring the economic cost of evasion is difficult since agents who engage in evasion have incentives to hide their behavior. Tax evasion is an economically significant activity and it seems important to understand the decision process of taxpayers when they chose between engaging in evasion or declaring their real income. Hence, studying the determinants of tax compliance is essential to design effective audit policy that deters evasion.

The initial standard analysis of compliance proposed by Allingham and Sandmo (1972) and Yitzhaki (1974) model an isolated taxpayer who maximizes her expected utility by facing a decision under risk. This theoretical approach assumes that the taxpayers are totally individualist and amoral. However, it is conceivable that individual decision of paying taxes depends on the behavior of others in community and affected by social norms (Myles and Naylor, 1996; Andreoni et al., 1998; Alm and Torgler, 2006; Fortin et al, 2007, Cummings et al., 2009; Lefebvre et al, 2014). Social information may have an important effect on tax compliance, particularly when the agents are interconnected nodes in a network. It is possible that social information has a stronger impact when the individuals know who the reference group members are, because they value more the behavior of individuals they know better (Lefebvre et al, 2014). However, according to Fortin et al. (2007), research on tax evasion usually ignores "peers effects" or "social interaction effects". This omission is due to the fact that testing for such effects is notoriously difficult for two reasons. First, outcomes data rarely reveal the reference group composition, whether it is the family, the neighborhood, or the work colleagues. Second, even when the group composition is known, estimating interaction-based models raises severe identification problems (Manski, 1993) and even when identifiable, they may prove hard to be estimated (Moffitt, 2001; Durlauf and Cohen-Cole, 2004; Blume and Durlauf, 2005).

Experimental studies can be useful in solving these problems (Charness et al., 2014). Reference groups are naturally defined as participants in a particular lab session who share similar characteristics. In fact, the reference group are exogenously imposed and consists of all those who happen to show up in a particular session. Every subject interacts with a single well-defined reference group of the same size. This new approach allows analysing the social interactions in a much easier way than that using survey data or interaction-based models. Indeed, because these hardly ever provide any information about their reference groups, the analyst often assumes individuals interact with those who share similar attributes: age, education, income, vicinity, etc. (eg Van Praag and Frijters, 1999).

Recent field and lab experiments (Innes and Mitra, 2013; Lefebvre et al, 2014) find evidence that supports the "broken window theory" formulated by Wilson and Kelling (1982). According to this theory, signs of disorder generate more disorder when people can observe others to violate a social norm (see Keizer et al., 2008). A critical mass model also suggests that individuals take part in an activity only if a sufficient proportion of the population is engaged in the activity (Schelling, 1978). These models suggest that in the domain of taxes high levels of compliance in the group (good examples) might have a positive effect on individuals, while low levels of compliance (bad examples) might discourage them to behave honestly.

To the best of our knowledge, only a few attempts have been made to document the impact of social interaction on tax compliance using experimental data. Fortin et al. (2007) tested in a controlled lab experiment the impact of information on the other network members' mean reporting decisions on individual tax compliance. They do not find evidence of an endogenous social information effect. Lefebvre et al. (2014) conducted lab experiments in Belgium, France and the Netherlands in order to study the influence of social information when information about others' average reporting decisions in past sessions is given to subjects. They observe an asymmetric effect of the information on tax compliance: high levels of compliance do not have a disciplinary effect whereas low levels of compliance significantly increase tax evasion for certain audit probabilities which are known by the subjects.

These contrasting results show the need for further investigation of the influence of social information on individual behavior. Indeed, we know little about how compliance information is diffused through different network structures, particularly when the information is positive (i.e. I know that a node, which is connected to me in the network, has been audited and found totally honest). Therefore, it is not clear if the disciplinary impact of good examples is comparable with the deteriorating effect of bad examples. This is particularly interesting because diffusion of information is free (in terms of auditing costs), and it could be effective in order to deter tax evasion (Fellner et al., 2013).

If information travels faster than sanctions, tax agencies may implement successful deterrence strategies at a reduced cost (this is particularly true for large and complex societies where auditing is expensive).

In this paper, we test whether social information has an effect on the level of compliance in a fixed-six-nodes circle network, particularly when the information diffused is positive (compliant behavior), and negative (un-compliant behavior). To analyze whether individuals have the same attitude towards tax evasion, we run an experiment with four information conditions: No Info, Full Info, Positive Info and Negative Info. In the No info treatment, individuals get individual information about whether they have been audited, the outcome of it and their final payoff. In the Positive Info (Negative Info) treatment, participants receive information whether the nodes connected to them have been audited and found compliant (noncompliant). In the Full Info, participants receive both positive and negative signals.

Our design has four interesting features. First, following Fortin et al (2007), individual monetary payoffs do not depend on the other participants' behavior, which allows us to isolate better the effect of social information in the network. In all our treatments, taxes and fines do not provide any public good, so there is no group externality linked with compliance and taxes do not change the distribution of income. Second, we will specifically control for the effect of signals on participants' beliefs on the ex-ante fixed and unknown probability of being audited by asking them using an incentive compatible mechanism. This feature increases the external validity of our study given that the audit probability is also unknown by the taxpayers in real life. Third, we keep a fixed tax rate and fine rates fixed and known by the subjects. And fourth, we use a real effort task. As suggested by recent experimental literature (Bruggen and Strobel, 2007), providing subjects with endowments like "manna from heaven" seems to affect their behavior compared with the case in which subjects are asked to perform some easy tasks to gather their endowments. Looking at experiments on taxation, a common result is that the adoption of a real effort procedure usually leads to higher levels of tax compliance. Also, such characteristic increases its external validity making the experiment less artificial.

Our results suggest that receiving positive signals (good examples) and negative signals (bad examples) have a significant effect on the levels of reporting and full compliance at individual level. However, results are mixed at aggregate level. Average compliance is lower in the Negative Info compared with the No Info, Full Info and Positive Info. The average subjective audit probability stated by the subjects is also lower in the Negative Info in comparison with the three other information treatments.

These results suggest that the low compliance in the Negative Info treatment could be explained by the low subjective audit probability. Non-strong (by being only one) negative signals received in the previous rounds by the subjects have negative and significant change on full compliance and reporting decisions in the Negative Info compared with the case of having received no signals in the prior round. This result suggests that bad examples may have a contagious effect in the network. Instead, strong (by being two) positive signals have a positive and significant effect on the level of reporting and full compliance decisions compared with no signals received in the previous period in the Positive Info. This result suggests that positive signals and so, good examples may have a disciplinary effect on the level of compliance and reporting decisions. The impact of positive signals in the Positive Info treatment as well as the effect of negative signals in the Negative info cannot be explained by a significant change in the subjective probability of being audit compared with the case of no signals received in the previous round.

The rest of our paper is organized as follows. Section 2 described our experimental design and procedures. Section 3 reports the experimental results while Section 4 concludes this paper.

2. The experimental design and procedures

2.1. The experiment

The purpose of our experiment is to specifically test whether different informational signals have an effect on the level of compliance. We run an experiment with four information conditions: No Info, Full Info, Positive Info and Negative Info. We implement these four treatments in a fixed circle network of 6 participants where each individual is connected to the adjacent neighbors. The network determines the observational structure, that is, after each round, the subjects are only able to receive information from their connected neighbors[18] (see Figure 1).

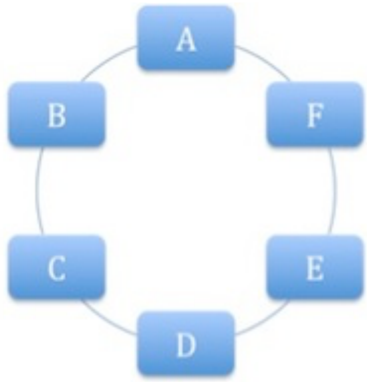

Figure 1: Circle network with 6 nodes

Without a positive group externality, individuals' payoffs do not depends on other subjects' behavior. This feature of our design allows us to isolate better the effect of social information in the network (Fortin et al, 2007). We also use an incentive compatible mechanism in order to control for the effect of signals on participants' beliefs on the ex-ante fixed and unknown probability of being audited. Besides, we implement a real effort task. Experiments on taxation shows that the adoption of a real effort procedure usually leads to higher levels of tax compliance making the experiment less artificial (Bruggen and Strobel, 2007).

2.2. The tax game

The sequence of each experimental round of the Tax game proceeds as follow:

- *Stage1- Real effort task*: Participants complete a real effort task at the beginning of each round (adding two digits numbers for one minute). After completing the task, their score is multiplied by a random factor, which can be 50, 100 or 150 (Lefevbre et al. 2014).

[18] The network structure is known by the subjects. Figure 1 was introduced in the instructions and showed in stage 2 of each round (see section 2.2).

- *Stage 2 – Reporting decision*: Participants know their income and report any fraction of it. Reported income pays a flat tax rate of 25%.
- *Stage 3- Belief elicitation*: Participants are asked to guess the probability of being audited in the next stage by choosing a number from 0 to 100. If the guess is within the interval ±10 of the real probability (i.e. p=0.25), participants are rewarded at the end of the experiment with no feedback.
- *Stage 4- Audit*: Participants are audited with a probability of 0.25. This probability is unknown by subjects and it is kept constant across the treatments. If an untruthful declaration of income is detected, a fine has to be paid. The fine is determined by the difference between the reported income and their endowment, which represents the amount evaded in that particular round.

In the No info treatment, subjects are only informed whether they were audit of not and their payoff at the end of each round. In the other three treatments, additional informational signals are given to the subjects from the two nodes connected to them in the circle network. Further description of the treatments will be given in section 2.3.

2.2.1. Expected utility and payoffs

We consider a subject *i,* who is part of a network of six members. Given that each subjects in each round may earn 0, 2, 4, 6, 8 or 10 points in the real effort task,[19] which is multiplied by one of the random factors: 50, 100 or 150,[20] the expected gross income is 1000 ECU.

Subjects are given the chance to evade taxes, where the individual evasion is given by:

$$E_{i,t} = \max\ [t_{i,t}; 0] \tag{1}$$

with t being the flat tax rate of 0,25 paid by subject *i* in round t, and $E_{i,t} = Y_{i,t} - I_{i,t}$, where $Y_{i,t}$ is the initial endowment and $I_{i,t}$ the reported income.

[19] In the 94.82% of the observations, subjects earn 10 points in the real effort task at the beginning of each round.
[20] This aims at capturing different possible gross incomes.

Audit probability is exogenous and defined by p=0.25 which is unknown and equal for all subjects. Therefore, individuals must determine how much income to report by estimating the probability of being audited (p^e). If an audit occurs and tax evasion is detected (i.e. $E_{i,t}$ >0), the subject has to pay immediately a fine λ, which is 100% of $E_{i,t}$. Hence:

$$\lambda = \begin{cases} 1 & \text{if an audit occurs,} \\ 0 & \text{otherwise} \end{cases} \tag{2}$$

The expected utility, $E[U_i]$, is defined to be:

$$E[U_i] = \{p^e\ U(Y_i - tI_{i,} - \lambda E_i) + (1 - p^e)\ U(Y_i - tI_i).\} \tag{3}$$

This equation represents the private expected value associated with tax compliance when the subject makes a choice in the amount to declare (I_i). Assuming that the subject is risk averse, the private utility $U(.)$ is increasing and concave in the consumption. By taking the first order derivate with respect to I_i and rearranging, a maximizing agent declare their full income Y=I, if

$$p^e > \frac{t}{\lambda} = p = 0.25 \tag{4}$$

As long as the expected probability of being audited exceeds the threshold value of 0.25, agents have no incentive to evade. If the expected below is strictly below the threshold, agents evade in full. Interestingly, if agents fully anticipate the true probability of being audited (we adjusted the probability of being audited to 0.25 in all treatments) agents are indifferent between evading and complying.[21]

In each period, subjects' experimental payoff is equal to $Y_{i,t}$ net of taxes that they pay after the declaration and sanction (if audited). We randomly select one of the 20 periods to compute earnings and the individual payoff from the game is given by:

$$\pi_{i,t} = Y_{i,t} - I \times t_{i,t} - \lambda(Y_{i,t} - I_{i,t}) \tag{5}$$

[21] An alternative model could consider Bayesian updating of beliefs. As we concentrate on the comparative statics analysis of our experimental results, we leave this model to a different paper we are currently working in.

2.3. Treatments

We manipulate the informational signals in the following way:

- *In the No Info treatment*, at the end of each round, subjects are just informed on whether they have been audited and on their net payoffs. This means that the subjects know the final individual benefits post-taxes.

- *In the Full Info treatment*, at the end of each round, they have the same information than in the No Info treatment but, they have also information whether their neighbors connected to them were audited or not, and the outcome of the audition (i.e., if she was fully honest or not). Therefore, positive and negative signals are diffused.

- *In the Positive Info treatment*, at the end of each round, the subjects have the same information than in the No Info treatment and besides they are informed whether their connected neighbors were audited *and* found fully honest. This means that only positive signals are diffused.

- *In the Negative Info treatment*, at the end of each round, the subjects have the same information than in the No Info treatment and besides they are informed whether their connected neighbors were audited *and* found dishonest. This means that only negative signals are diffused.

2.4. Procedures

All our treatments follow a partners matching protocol, that is, the group composition is kept constant. Moreover, the subjects' positions within the network are randomly determined at the beginning of the experiment and kept fixed throughout all the rounds (fixed position). The group size is always of 6, following a partner matching protocol and the game is repeated for 20 rounds. We have 9 independent observations per treatment and 54 subjects per cell (4 sessions of 18 subjects for each treatment). In total, 216 subjects participate in the experiment. The average payment of each subject was £13. The experiment was computerized in Z-tree (Firshbacher, 2007).

3. Results

The 216 subjects were equally distributed across treatment in 9 networks of 6 participants, making a total of 54 subjects per treatment. Table 1 display that samples are well balanced across all treatments with respect to age, gender, Europeans (including British participants) and students from the faculty of Social Sciences. No significant differences were found across treatments.

Table 1: Demographics characteristics- Full sample- Means and standard deviations

	No Info		Full Info		Positive Info		Negative Info	
	Mean	Sd	Mean	Sd	Mean	Sd	Mean	Sd
Gender: female	0.63	0.48	0.59	0.49	0.59	0.49	0.57	0.49
Social Sciences	0.37	0.48	0.46	0.49	0.41	0.49	0.48	0.49
Europeans	0.63	0.48	0.62	0.48	0.61	0.48	0.61	0.48
Age [years]	21.24	3.33	20.79	2.52	22.27	3.89	22.09	5.52

Table 2 reports the results for the average proportion of reported endowment, average full compliance and subjective probability of being audited in percentage for each treatment. In table 2, Wilcoxon Mann Whitney test were computed on independent observations (network) in order to test significant differences between treatments. The first part of Table 2 illustrates the average proportion of endowment reported in the each treatment. The first column displays the results for the whole experiment and we observe that proportion of endowment reported is lower in the Negative Info compared with the No Info, Full Info and Positive Info treatments. However, this is only significant when we compare the Negative Info with the Full Info treatment (p=0.0476).

From column 2 to column 5, we show the average reported endowment in each block of 5 rounds. We observe that the average proportion reported in the Negative Info is significantly lower compared with the Full Info treatment in the 1-5 rounds and in the 11-15 and 16-20 rounds of the experiment (p=0.099; p=0.0243 and, p=0.0152, respectively). We found also a significant difference between the Full Info and the No info treatment in the first 5 rounds (p=0.099) and between the Positive Info and the Negative Info treatment in the last 5 rounds (p=0.0851).

Table 2: Average proportion of endowment reported; full compliance and stated subjective audit probability by treatment

Average proportion of endowment reported							
	Rounds						
Treatments	All 20	1-5	6-10	11-15	16-20	0% reported (All 20)	
No info	0.628	0.636	0.627	0.616	0.634	13.50%	
	(0.419)	(0.392)	(0.418)	(0.435)	(0.430)		
Full info	0.692	0.689	0.671	0.696	0.709	11.70%	
	(0.394)	(0.4050	(0.401)	(0.402)	(0.024)		
Positive Info	0.636	0.659	0.651	0.596	0.636	18.20%	
	(0.424)	(0.425)	(0.402)	(0.440	(0.428)		
Negative Info	0.55	0.582	0.575	0.498	0.545	18.00%	
	(0.43)	(0.412)	(0.421)	(0.443)	(0.439)		
Average proportion of Full Compliance							
	Rounds						
	All 20	1-5	6-10	11-15	16-20		
No info	0.459	0.392	0.463	0.467	0.515		
	(0.498)	(0.489)	(0.499)	(0.499)	(0.5)		
Full info	0.499	0.452	0.47	0.522	0.552		
	(0.5)	(0.498)	(0.5)	(0.498)	(0.5)		
Positive Info	0.472	0.455	0.488	0.456	0.489		
	(0.499)	(0.498)	(0.5)	(0.498)	(0.5)		
Negative Info	0.35	0.356	0.352	0.326	0.366		
	(0.477)	(0.479)	(0.4780	(0.469)	(0.482)		
Average proportion of Subjective audit Probability							
	Rounds						
	All 20	1-5	6-10	11-15	16-20	First Only	Last Only
No info	0.4057	0.4015	0.3936	0.4029	0.4247	0.3737	0.458
	(0.264)	(0.249)	(0.263)	(0.282)	(0.259)	(0.202)	(0.271)
Full info	0.3964	0.4068	0.3783	0.4036	0.397	0.3844	0.398
	(0.247)	(0.237)	(0.272)	(0.2626)	(0.258)	(0.198)	(0.275)
Positive Info	0.3951	0.3936	0.413	0.4038	0.3701	0.3496	0.398
	(0.256)	(0.228)	(0.272)	(0.262)	(0.258)	(0.1829)	(0.257)
Negative Info	0.3461	0.3669	0.358	0.3355	0.3243	35.96	0.314
	(24.04)	(0.229)	(0.241)	(0.2513)	(0.237)	(0.197)	(0.242)

In column 6, we observe that the percentage of cases in which nothing is reported. Subjects in the Full Info report significantly more positive proportions of their endowment compared with the Negative Info treatment.

As signals are sent only when subjects fully report all their income (when positive), or when they fail to do so (negative ones), in the second part of Table 2, we display the average proportion only for the 100% of endowment reported (full compliance). In the column, we observe that proportion of cases in which the whole endowment is reported is lower in the Negative Info compared with the No Info, Full Info and Positive Info, however not significantly. The proportion of full compliance is significantly higher in the Positive Info in the rounds from 6-10 compared with the Negative Info (p=0.0922) and, in the Full Info compared with the Negative info in the rounds from 11-15 and 16-20 (p=0.0693 and p=0.0615, respectively).

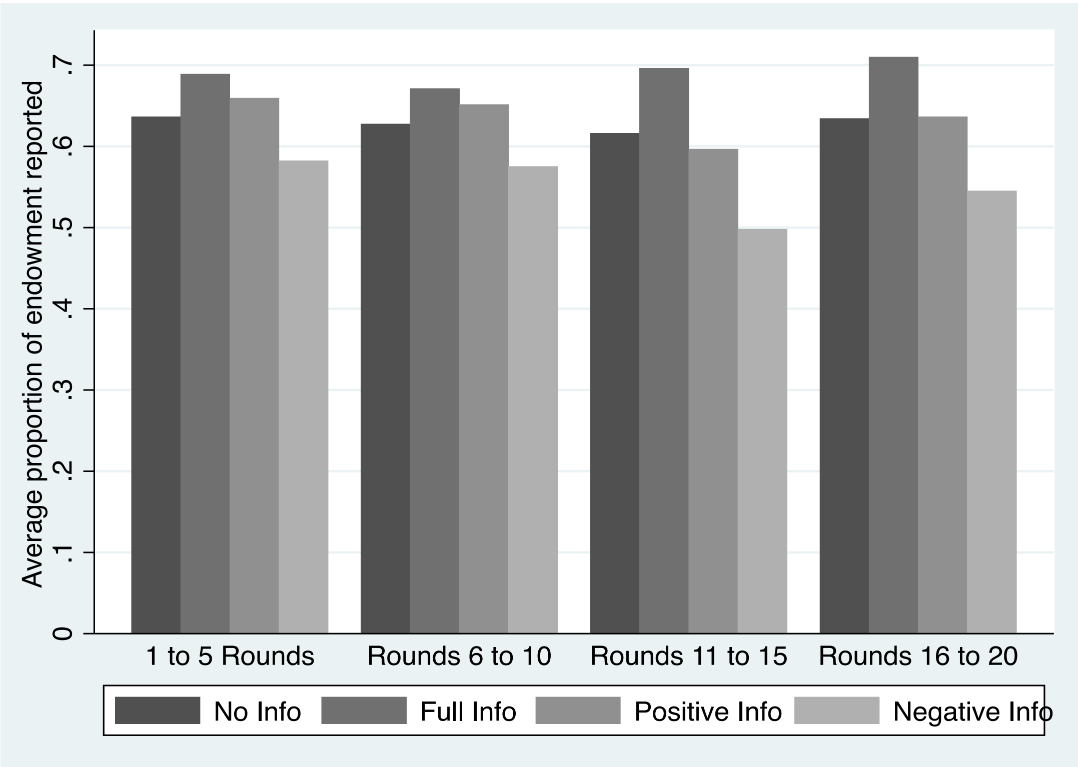

Figure 2: Average proportion of endowment reported in block of five rounds by treatment

Figure 1 shows the average proportion of endowment reported by subjects in each block of five rounds by treatment. As we display in Table 2, we observe that subjects in the Negative Info report less than in the Full Info, Positive Info and No Info. The proportion of endowment reported does not vary significantly over time in the No Info (Spearman's rho=0.0227; p=0.4557), Full Info (Spearman's rho=0.0447; p=0.1424), Positive Info (Spearman's rho= -0.0121; p=0.6908) and in the Negative Info (Spearman's rho=-0.0485; p=0.1115).

Figure 2 shows that the average proportion of full compliance is lower in the negative Info compared to the Full Info, Positive Info and No Info treatment in each block of five periods of the game. We observe that the average full compliance increases over time in the Full Info treatment (Spearman's rho=0.0811; p=0.0077) and in the No Info (Spearman's rho=0.0870; p=0.0042). Conversely, full compliance does not vary over time in the Positive Info (Spearman's rho=0.0232; p=0.4471) and in the Negative Info (Spearman's rho=-0.0020; p=0.9471).

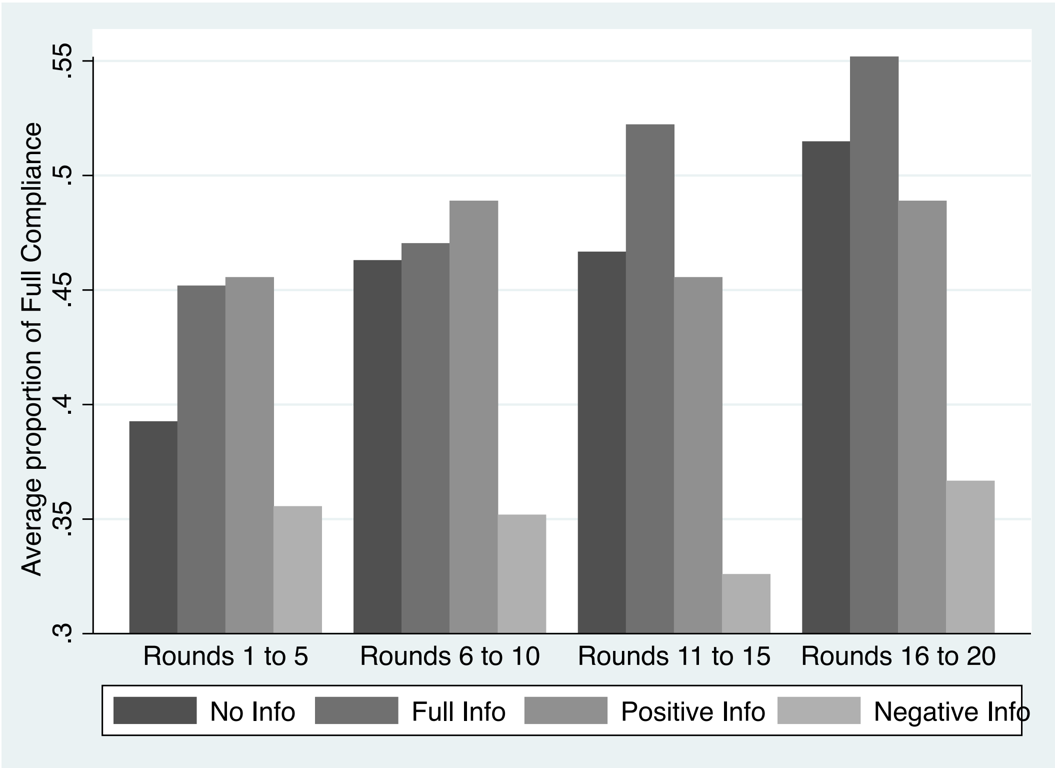

Figure 3: Average proportion of full compliance in block of five rounds by treatment

As the expected probability of being audited play a crucial role in the decisions of our participants, in the last part of Table 2, we report the results for the belief elicitation of the subjective probability of being audited in each period before the participants make their reporting decisions. In the first column, we observe that the stated subjective probability in the Negative Info is lower than the No Info, Positive Info and Negative Info treatment, but only significant compared with the Full info treatment (p=0.098). This means that the lower average reported endowment and average proportion of full compliance is principally driven by a lower subjective audit probability in the Negative Info. Column 4 and 5 display the percentage of the subjective probability in the last part of the experiment (round 11-15 and 16-20). We observe that, from round 11 to 15, average stated subjective audit probability in the Positive Info is significantly higher than in the Negative Info (p=0.097), while in the rounds from 16 to 20, the average beliefs is significantly higher in the No Info treatment than in the Full Info (p=0.0280), Positive Info (p=0.0182) and in the Negative Info (p=0.006).

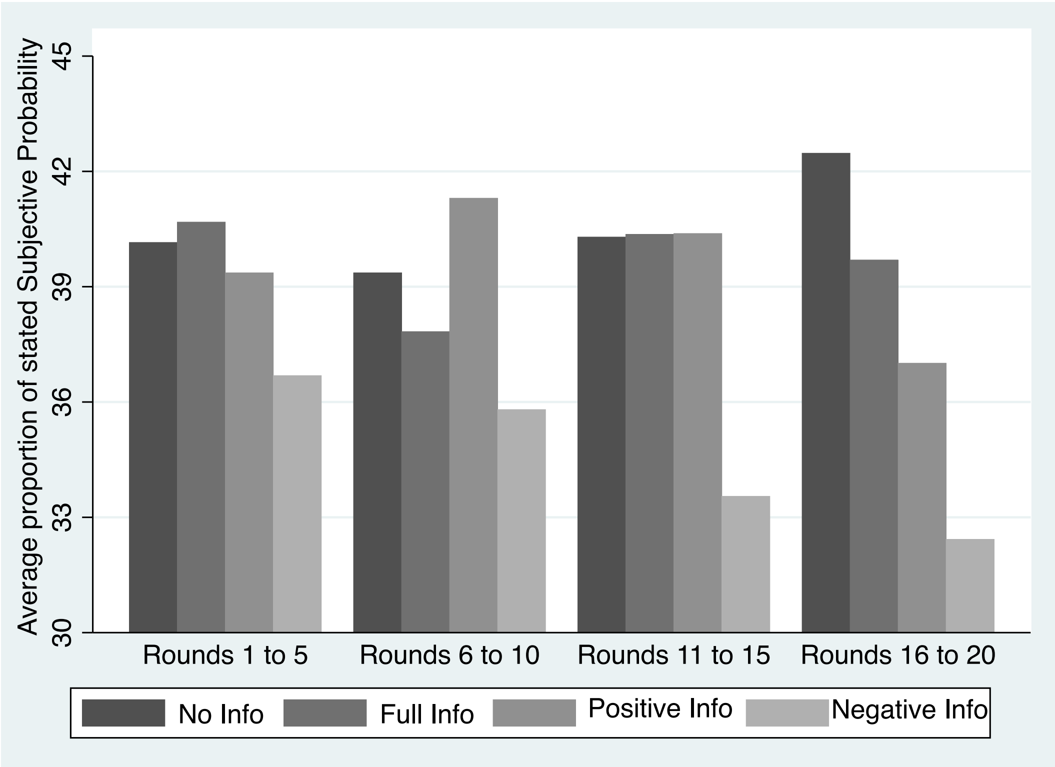

Figure 4: Average proportion of stated subjective probability in block of five rounds by treatment

Figure 3 shows the beliefs about the subjective probability are lower in the Negative Info compared with the Full Info, Positive Info and No Info in each block of five periods. The stated subjective audit probability significantly decreases over time in the Negative Info (Spearman's rho=-0.0928; p=0.0023), while it does not significantly vary in the No Info (Spearman's rho=0.0326; p=0.2848), in the Positive Info (Spearman's rho=-0.0479; p=0.1160) and in the Full Info (Spearman's rho=-0.0025; p=0.9353).

In the last part of Table 2, we also compare the stated beliefs in the first round and in the last round across treatments (see column 6 and 7). We show that there are not significant differences across treatments when we compare the average stated beliefs in the first round. This means that the individuals' beliefs before starting the game are not significantly different across treatments. However, we observe that there are significant differences when we compare the stated subjective probability only in round 20 (last round) in the information treatments with respect to the baseline (No Info). The mean of stated audit probability is marginally smaller in the Positive Info (39.80%; p=0.095) and Negative Info (31.40%; p=0.0071) than in the No Info treatment (45.80%). The mean stated probability is also higher in the Positive Info compared with the Negative Info (p=0.0380).

Result 1: In line with the existence of network spillovers, the expected audit probability decreases over time in the Negative Info. Receiving only negative signals from the connected links in the network makes the beliefs about the subjective audit probability decrease in the Negative Info compared with an environment, in which both positive and negative information (good and bad examples) are disseminated.

3.1. The Signals

In our experiment, subjects update their beliefs about the subjective audit probability after receiving individual information whether they have been audited or not at the end of each round and, after getting two signals from the adjacent players connected with them in the network (one from each of them). The signals can be Positive, Negative and No signal. In the No Info treatment, individuals receive only individual outcome of the audit and get no signals from their neighbors. In the Full Info, players receive signals from the each connected links, which can be (one or two) Positive, (one or two) Negative, Mixed (one positive and one negative) and No signal. Note that participants receive a Positive Signal if the neighbor has been audited *and* found fully honest, otherwise No signal is received from that node; a Negative signal whether the adjacent node has been audited *and* found dishonest, otherwise No signal is received from that neighbor. Mixed signal if the player receives one positive and one negative signal from the two players linked to her.
In table 3, we report the total frequency and the percentage of each signal and the audits received by the subjects in each treatment.

Table 3: Total amount of audits and signals by treatment- frequency and percentage

	No Info		Full Info		Positive Info		Negative Info	
	Freq.	%	Freq.	%	Freq.	%	Freq.	%
Audits	266	24.62	271	25.09	271	25.09	263	24.35
Type of Signal								
One Positive	-	-	214	19.81	222	20.55	-	-
Two Positive	-	-	19	1.76	11	1.02	-	-
One Negative	-	-	200	18.52	-	-	296	27.40
Two Negative	-	-	13	1.20	-	-	32	2.96
None	1080	100	602	55.74	847	78.43	752	69.63
Mixed	-	-	32	2.96	-	-	-	-

We find that there is not a significant difference between the proportion of one and two positive signals in the Full Info compared with the Positive Info (p=0.8474 and p=0.8719, respectively). The proportion of one negative signals received by the subjects in the negative Info is significantly higher than in the Full info treatment (p=0.0228), however there is not a significant difference between the proportion of two negative signals received in the Negative Info compared with the ones received in the Full info (p=0.7293). We observe that in the Positive Info the overall proportion of positive signals is significantly lower than negative signals in the Negative Info (p=0.0171). In the Positive Info, subjects receive significantly more "no signals" than in the Negative Info (p=0.0001). The frequency of negative signals is significantly higher in the No Info treatment than in the Full Info (p=0.0049). The total amount of audits does not significantly differ across treatment.

Result 2: Bad examples (negative signals) are significantly more frequent than good examples (positive signals) when we compare the Positive Info and Negative Info treatments. This means that there is more evasion in the Negative Info than in the Positive Info treatment. However, the total amount of audits is not significantly different across treatments.

3.2. Beliefs and reporting decisions

Figure 1 displays the linear predictions for the average reporting decisions and average estimated probability of audit by individual for the whole game in each treatment. We observe that there is a direct relationship between the average individual reporting decision and the average stated probability in the No Info, Full info and Negative Info, but not in the Positive Info. Spearman correlations between the average reporting decisions and the average beliefs about the audit probability by individual is positive and significant in the Full Info (rho=0.1873; p=0.000) and in the Negative info (rho=0.0692; p=0.0230), but insignificant in the No info treatment (rho=0.0129; p=0.1873). However, the correlation is negative and significant in the positive info (rho=-0.0861; p=0.0046).

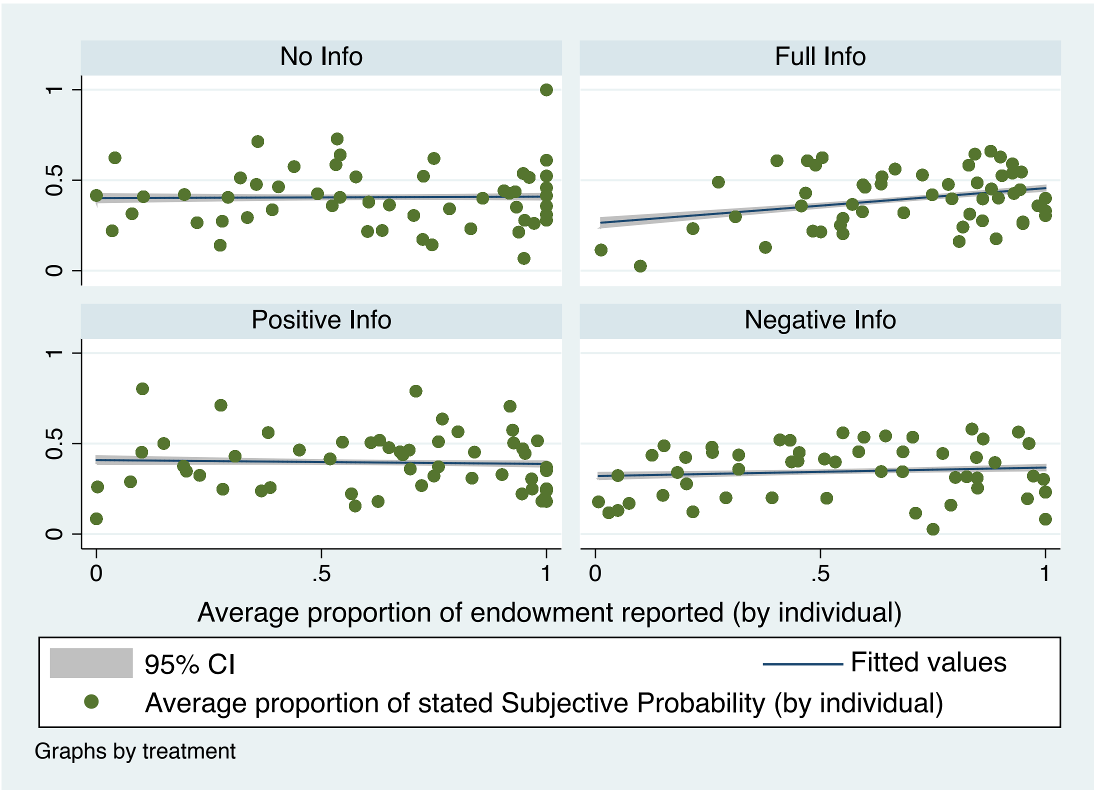

Figure 5: Linear predictions of the average beliefs and average reporting decisions by individual across treatments

3.3. Beliefs and full compliance decisions

Figure 2 shows the linear prediction of average estimated probability of being audited and number of full compliance decisions by individual in the 20 periods in each treatment. We observe the number of decisions of full compliance by individual has a positive and significant relationship with their belief about the subjective probability of being audited in the Full Info (Spearman rho=0.0597; p=0.0499). Conversely, we observe that this relationship is negative and significant in the Positive Info (Spearman's rho=-0.1291; p=0.000) and in the Negative Info (Spearman rho=-0.0917; p=0.0025). In the No info, average individual decisions of full compliance are also negatively related to the average stated subjective audit probability, but not significantly (Spearman's rho=-0.0471; p=0.1221).
Figure 2 also presents separately the average beliefs of subjects who complied in all the 20 rounds of game with those who did not. The average beliefs of subjective the audit probability of totally honest players is 49.35% in the Negative Info; 34.46% in the Full Info; 27.75% in the Positive Info and; 18.15% in the Negative Info treatment. Conversely, the average beliefs of the totally dishonest players is 45% in the No Info; 11.40% in the Full Info; 37.22% in the Positive Info and; 21.80% in the Negative Info treatment. This means that, in the Full Info treatment, totally honest subjects tend to overestimate significantly the probability of being audited compared with non-compliant subjects (p=0.0372).

This result is consistent with preferences to conform to others so that higher beliefs about the probability of audit are related to higher expectation about others compliance after receiving positive or negative signals. At the same time higher compliance expectations about others compliance may lead to more compliance. Another explanation is that the pattern could reflect a false consensus effect whereby people tend to overestimate the extent to which their own behavior is also exhibited by others (Ross et al., 1977). Conversely, we do not observe the same pattern in the Positive Info: totally honest subjects decide to comply in the whole game even though their beliefs about the audit probability are significantly lower than totally dishonest subjects (p=0.0793). Instead, we do not observe a significant difference between the average subjective probability of the totally honest and totally dishonest players in the No Info and Negative Info treatments.

The percentage of subjects who reported 100% of their endowment (totally honest) in the 20 periods is 14.8% in the No Info, 5% in the Full Info, 9.2% in the Positive Info and 5% in the Negative Info. Conversely, the percentage of subject who never did full compliance was 18.5% in the No Info, 1.6% in the Full Info, 11.1% in the Positive Info and 16.6% in the Negative Info.

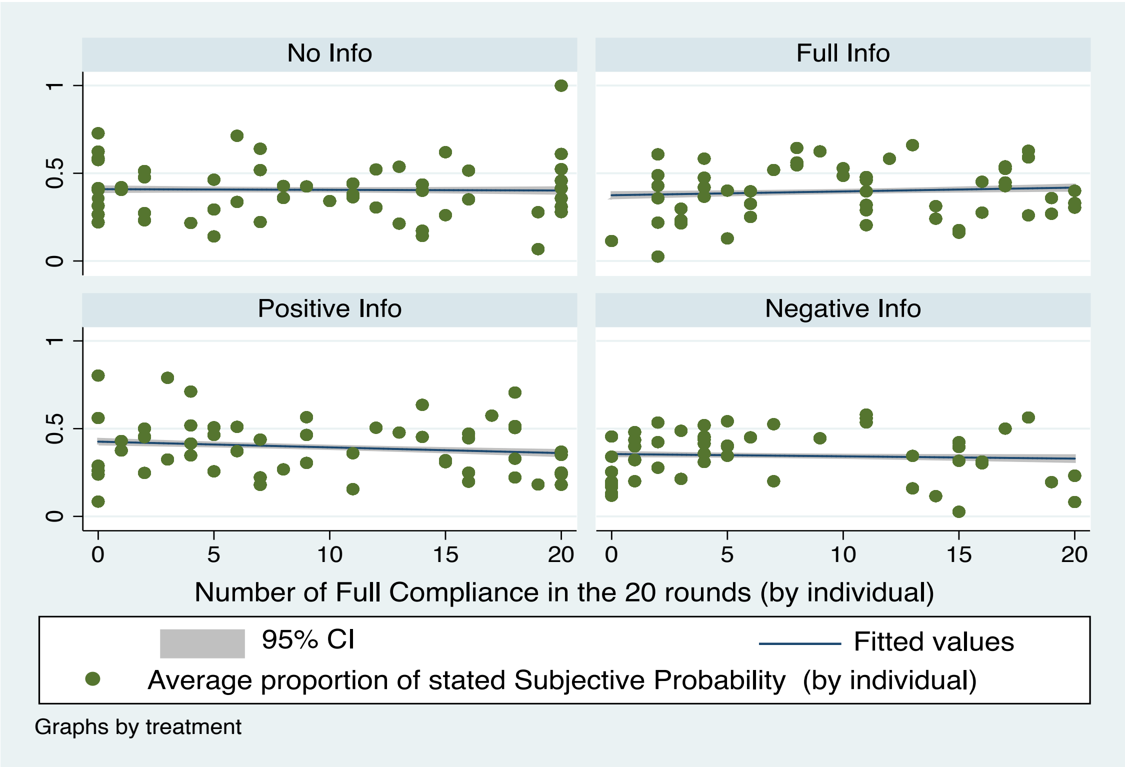

Figure 6: Linear predictions of average beliefs and number of decisions of full compliance by individual across treatments

Result 3: Totally honest subjects, who fully comply the 20 rounds, overestimate in average the subjective audit probability compared than totally dishonest counterparts in the Full Info. Conversely, we observe the reverse pattern in the Positive Info. Totally compliant subjects believe that the subjective audit probability is significantly lower than totally non-compliant subjects. Instead, there is not a significant difference on the average beliefs between the totally honest players and the totally dishonest ones in the No Info and in the Negative Info treatments.

3.4. Econometric analysis

Table 4 reports detailed econometric regressions for the proportion of endowment reported, full compliance and beliefs of the subjective probability of being audited across treatments. We compare three information treatment (Full Info, Positive Info and Negative Info) with the No Info treatment in which only individual information is available to the participants and so a natural baseline. The dependent variable *"Proportion Reported"* is a continuous variable from 0 to 1 indicating the proportion of endowment reported by the subjects; *"Full compliance"* is a binary dependent variable indicating whether the 100% of the endowment was declared or not; and *"Subjective probability*" is a continuous variable from 0 to 1 that indicate the proportion of the stated subjective audit probability[22]. We also control for period to account for learning effects.

Column (1) presents the results for the reported endowment for the 20 periods. We do not observe a treatment effect when we compare the Full Info, the Positive Info and the Negative Info treatment with the No Info treatment, which is our reference. The proportion of the endowment reported tends to decrease but not significantly over time. In Column (2), we observe that subjects make full compliance significantly less in the Negative Info than in the No Info treatment. Reporting decisions are higher in the Positive and Full Info treatments compared with the No Info, but not significantly. We also see that the individuals tend to significantly increase their decisions of declaring 100% of their endowment over time.

Column (3) reports the results for the estimated subjective probability and it is observed that beliefs about the audit probability are significantly lower in the Negative Info compared with the No Info treatment. This result suggests that subjects make full compliance significantly less in the Negative Info than in the No Info treatment because their beliefs about the subjective probability are

[22] Subjects can report any amount from 0 up to their endowment and can state the subjective probability as any probability from 0% to 100%. Given the nature of these two dependent variables, we decide to run in Table 3 random effect linear regressions clustering by independent observation (network of 6 players). Instead, the dependent variable "full compliance" is a binary variable, which can take value of 1 (100% of endowment reported) or 0 otherwise. We decide to run for this variable a random effect probit model clustering by independent observation.

significantly lower than in the No Info. Beliefs in the Positive and Full Info are lower than in the No Info treatment, however not significantly.

Result 4: Individuals subjective audit probability is significantly lower in the Negative Info than in the No info treatment. This result drives subjects to comply significantly less in the Negative Info than in the No Info treatment at aggregate level.

In columns from (4) to (6), we add additional control variables in order to better understand determinants of the reporting decisions, full compliance and belief formation at individual level across treatments. We introduce lagged interactive variables for each type of signal received by the subjects in the previous round in each treatment. These variables are dummies, which capture the effect of each type of signal received in the previous round on individual decisions.

More specifically, in Column (4), we observe that subjects tend to increase significantly the proportion reported in t after receiving two negative signals in t-1 in the Full info treatment *("Lagged Two Negative Signal x Full Info*"). Subjects also tend to increase significantly the proportion reported and the full compliance after receiving two positive signals in the previous round *("Lagged Two Positive Signals x Full Info*") in the Full Info compared with the No Info treatment. This result could be driven by a significant increment of their beliefs about the subjective audit probability after receiving two positive signals in t-1 (see Column 6). Subjects also tend to increase their stated subjective probability after receiving one negative signal and two mixed signals in the Full Info in comparison with the No Info treatment.

We also introduce interactive variables for the lagged signals received in t-1 in the Positive treatment. It is displayed that receiving two positive signals in the previous round *("Lagged Two Positive signals x Positive Info")* has a negative and significant effect in the full compliance decision in the Positive Info compared with the No Info. Receiving no signals *("Lagged No signal x Positive Info")* or one positive signal in t-1 *("Lagged one Positive signal x Positive Info")* affect positively to the decision of fully comply in the Positive Info compared with the No Info, but no significantly. In column 6, we show that receiving one or two positive signals in the previous round affect positively, but insignificantly, the beliefs about the subjective audit probability in the Positive Info compared with the No info treatment.

Table 4: Pooled sample – Random effect linear regression model (proportion reported and subjective probability) and random effect probit model (full compliance) both clustering at group level

	(1)	(2)	(3)	(4)	(5)	(6)
	POOLED 1			POOLED 2		
VARIABLES	Proportion Reported	Full Compliance	Subjective Probability	Proportion Reported	Full Compliance	Subjective Probability
No Info	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*
Full Info	0.0637	0.210	-0.00926			
	(0.0473)	(0.162)	(0.0330)			
Lagged No Signal x Full Info				0.0589	0.152	-0.02231
				(0.0470)	(0.174)	(0.03361)
Lagged One Negative signals x Full Info				-0.00445	0.0425	0.03857**
				(0.0379)	(0.131)	(0.01761)
Lagged Two Negative Signals x Full Info				0.114**	0.0402	-0.01645
				(0.0474)	(0.229)	(0.04173)
Lagged One Positive signal x Full Info				-0.00950	0.0165	0.00367
				(0.0204)	(0.122)	(0.01713)
Lagged Two Positive Signals x Full Info				0.106*	0.906*	0.07301*
				(0.0581)	(0.504)	(0.04208)
Lagged Two Mixed Signals x Full Info				0.00631	0.293	0.08039***
				(0.0754)	(0.221)	(0.02856)
Positive Info	0.00810	0.0731	-0.0105			
	(0.0576)	(0.183)	(0.0321)			
Lagged No Signal x Positive Info				0.00512	0.0222	-0.01632
				(0.0606)	(0.191)	(0.03099)
Lagged One Positive signal x Positive Info				-0.00259	0.0382	0.02950
				(0.0397)	(0.122)	(0.02557)
Lagged Two Positive Signals x Positive Info				0.118	0.843***	0.02202
				(0.0771)	(0.255)	(0.05238)
Negative Info	-0.0777	-0.429*	-0.0595*			
	(0.0681)	(0.254)	(0.0342)			
Lagged No Signal x Negative Info				-0.0820	-0.455*	-0.05543
				(0.0722)	(0.259)	(0.03511)
Lagged One Negative signals x Negative Info				-0.000988	-0.0991	-0.02269
				(0.0234)	(0.0751)	(0.01428)
Lagged Two Negative Signals x Negative Info				0.0399	0.445**	0.01691
				(0.0705)	(0.182)	(0.02773)
Period	-0.00113	0.0164***	-0.000580	-9.89e-05	0.0202***	-0.000988
	(0.00106)	(0.00458)	(0.000720)	(0.00114)	(0.00499)	(0.000771)
Constant	0.640***	-0.345***	0.412***	0.628***	-0.384***	0.4182***
	(0.0372)	(0.130)	(0.0265)	(0.0375)	(0.127)	(0.02759)
Log Likelihood		-2109.364			-1986.2848	
Observations	4,317	4,320	4,320	4,102	4,104	4,104
Number of ind	216	216	216	216	216	216

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

We now analyze the role of the different signals on the decisions in the Negative Info. We observe that receiving no signal in the previous round *(Lagged No Signal x Negative Info)* has a negative effect in the proportion reported, full compliance and in the belief of the subjective audit probability. However, these effects are only significant on the full compliance decisions when we compare the Negative Info with the No Info treatment. One negative signal received in the Negative Info in t-1 *(Lagged One Negative Signal x Negative Info)* has a negative effect in the reporting decisions, in the full compliance and in the beliefs of the subjective audit probability in the Negative Info compared with the no Info treatment, however insignificantly.

Subjects tend to significantly comply more after receiving two negative signals in the Negative Info compared with the No Info. They also tend to increase their reported endowment and the stated subjective probability but insignificantly. In other words, Participants increase full compliance after receiving two signals (positive or negatives) in Positive and Negative Info compared with the No Info treatment. In the Full Info, two signals increase both full compliance and proportion of endowment reported.

Result 5: Receiving two signals about others compliance behavior do encourage a higher compliance in the Positive Info, Full Info and Negative Info compared with the No info treatment. This suggests that strong (by being two) signals received from the connected links in the network have a disciplinary effect on individuals' behavior.

We now explore more about the behavioral determinants of compliance at treatment level. Table 5 display results of the within-treatments estimations for the No Info and Full Info treatment. We compute regressions for each dependent variable: Proportion of endowment reported, full compliance decision and stated subjective probability. We also control for the stated probability of being audit (*Subjective probability*) in order to evaluate its effect in the reporting and in the full compliance decisions.

However, the parameter estimates of *"Subjective Probability"* is biased because a simple random effects linear model or probit model omits the potential simultaneity between individual reporting decisions with their beliefs about the subjective audit probability. Recall that this bias may arise from the fact that individual reporting decisions and, their beliefs feed on one another. In order to tackle the simultaneity problem, we implement a simultaneous equation model (2SLS) clustering by independent observations (network of 6 players).

Table 5: specific samples for the No Info and Full Info treatment. Simultaneous equation model (2SLS) clustering at group level

	(1)	(2)	(3)	(4)	(5)	(6)
	No Info			Full Info		
VARIABLES	Proportion Reported	Full Compliance	Subjective probability	Proportion Reported	Full Compliance	Subjective probability
Lagged No Signal				*Ref*	*Ref*	*Ref*
Lagged Positive Signal	-	-	-	0.0193	0.0338	-0.00885
				(0.0333)	(0.0404)	(0.0133)
Lagged Two Positive signal	-	-	-	0.0313	0.160	0.0504
				(0.113)	(0.134)	(0.0472)
Lagged Negative Signal	-	-	-	-0.00904	0.0114	0.0636***
				(0.0393)	(0.0345)	(0.0191)
Lagged Two Negative signal	-	-	-	0.0129	-0.0437	-0.0227
				(0.0437)	(0.0628)	(0.0584)
Lagged Two Mixed signal	-	-	-	0.0666	0.120	0.0653*
				(0.0751)	(0.0841)	(0.0368)
Subjective Probability	0.139	-0.117	-	0.555*	0.318	-
	(0.192)	(0.332)		(0.285)	(0.282)	
Lagged audited	-0.0149	-0.0269	0.000941	0.0208	0.0303	-0.0112
	(0.0298)	(0.0228)	(0.0296)	(0.0318)	(0.0405)	(0.0130)
Period	2.21e-05	0.00647*	0.00157	0.00339	0.00961***	0.000177
	(0.00177)	(0.00366)	(0.00149)	(0.00268)	(0.00346)	(0.00188)
Age	0.00526	0.0225***	0.00732	-0.0277***	-0.0297**	-0.00444
	(0.00713)	(0.00577)	(0.00659)	(0.00715)	(0.0118)	(0.00991)
Female	0.0893	0.0444	-0.0149	-0.0822	-0.117	-0.000589
	(0.0721)	(0.0777)	(0.0378)	(0.0708)	(0.0841)	(0.0447)
Europeans	0.0620	0.0501	-0.0730	0.0234	0.0935*	-0.0212
	(0.0558)	(0.0492)	(0.0533)	(0.0577)	(0.0530)	(0.0450)
Constant	0.367*	-0.0882	0.290	1.028***	0.869***	0.490**
	(0.204)	(0.169)	(0.188)	(0.173)	(0.327)	(0.213)
Log Likelihood						
Observations	1,025	1,026	1,026	1,025	1,026	1,026
R-squared	0.014	0.025	0.034	0.074	0.084	0.015

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

We observe that *" Subjective Probability"* (= "Lagged Subjective Probability") has a positive effect on the reporting decisions in No Info and in the Full Info, but only significantly in the latter. We also evaluate the effect on the different signals received by the subjects in the previous period in the Full Info compared with the case of having received no signal (reference). We do not observe a significant effect of the signals in the participants' reporting and compliance decisions. However, we do observe that receiving a negative signal or mixed signals in t-1 has a positive effect on the beliefs about the audit probability compared with the case of no signals received in t-1.

Besides, we control by demographic variables and time trend. We observe that the proportion of endowment reported, full compliance decisions and the beliefs about the subjective audit probability tend to increase over time, but only significantly for the full compliance decisions in the No Info and Full Info.

Result 6: Receiving negative and mixed signals (one positive and one negative) increase the beliefs about the subjective audit probability in the Full Info, but it does not affect significantly the reporting and compliance decisions.

In Table 6, we report the results for the within-estimations for the Positive and Negative Info. We introduce lagged variables that capture the effect of the different signals received by the subjects in t-1. We observe that receiving two positive signals (a strong signal) in the previous round have a positive and significant effect on the reporting and full compliance decisions compared with the case of receiving No signals in t-1. There is not a significant effect in the decisions when one positive signal is received comparing with the case of having received no signals in t-1. In column (4) and (5), we notice that one negative signal received in the previous round decrease significantly reporting and full compliance decisions compared with No signals received in t-1 in the Negative Info treatment (in which strong signals are not needed; one is enough). However, in column (6) it is shown that the beliefs decrease but not significantly after receiving one negative signal compared with no signals received in t-1. Receiving two negative signals has a positive effect but insignificant in the decisions compared with no signals received in the previous round.

Table 6: specific samples for the Positive and Negative Info - Simultaneous linear regression model (2SLS) clustering at group level

	(1)	(2)	(3)	(4)	(5)	(6)
	Positive Info			Negative Info		
VARIABLES	Proportion Reported	Full Compliance	Subjective probability	Proportion Reported	Full Compliance	Subjective probability
Lagged No Signal	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*
Lagged One Positive Signal	-0.0206	-0.0253	0.0200			
	(0.0429)	(0.0383)	(0.0186)			
Lagged Two Positive Signals	0.176*	0.255***	0.0333			
	(0.103)	(0.0801)	(0.0482)			
Lagged One Negative Signal				-0.0698**	-0.0558*	-0.00751
				(0.0286)	(0.0301)	(0.0150)
Lagged Two Negative Signals				0.00628	0.0543	0.0142
				(0.0656)	(0.0547)	(0.0432)
Subjective Probability	0.161	-0.00409		0.290	0.0112	
	(0.214)	(0.242)		(0.305)	(0.321)	
Lagged audited	-0.0851**	-0.106***	-0.0387	-0.0429	-0.0293	0.00830
	(0.0395)	(0.0352)	(0.0282)	(0.0364)	(0.0427)	(0.0154)
Period	-0.000578	0.00292	-0.00217**	-0.00189	0.00110	-0.00345***
	(0.00122)	(0.00254)	(0.00105)	(0.00277)	(0.00318)	(0.000807)
Age	-0.00796	-0.00549	-0.00456	0.00739*	0.00898	-0.00489
	(0.00525)	(0.00708)	(0.00556)	(0.00423)	(0.00623)	(0.00323)
Female	-0.000806	-0.0347	0.0245	-0.0498	-0.0357	0.00447
	(0.0818)	(0.103)	(0.0512)	(0.0626)	(0.0503)	(0.0316)
Europeans	0.176***	0.0749	-0.0319	0.155**	0.161***	0.00187
	(0.0562)	(0.0644)	(0.0566)	(0.0628)	(0.0456)	(0.0319)
Constant	0.669***	0.567**	0.533***	0.269	0.0773	0.488***
	(0.181)	(0.231)	(0.133)	(0.197)	(0.250)	(0.0898)
Observations	1,026	1,026	1,026	1,026	1,026	1,026
R-squared	0.060	0.021	0.019	0.074	0.037	0.019

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

We introduce the independent variable "*Lagged Audited*" to capture the effect of past audits in the previous round on the decisions. We find that reporting and full compliance decisions decreases when subjects were audited in t-1 in the Positive Info treatment. This is consistent with "the bomb crater effect", which refers to the idea that individuals might perceive the risk of being audited to fall immediately after an audit (see Guala and Mittone, 2005; Mittone, 2006).

We also control for demographic variables and time trend. We observe that reporting and compliance decisions do not vary significantly over time, but the beliefs about the subjective probability of being audit tend to decrease significantly in the Positive Info and Negative info. Europeans participants tend to report more in both Positive and Negative Info. We also control for the beliefs about the audit probability and notice that beliefs have not a significant effect in reporting and compliance decisions in the Positive and Negative Info treatments. This result suggests that subjects' beliefs do not significantly affect compliance and reporting decisions in the Positive and Negative Info treatment.

Result 7: Receiving strong (by being two) positive signals affect positively reporting and full compliance decisions compared with receiving No signals in the Positive Info. Past audits affects negatively the reporting and compliance decisions only in the Positive Info. In the Negative Info, subjects also tend to report and comply less after receiving non-strong (only one) negative signal than no signals in the previous round. Negative and positive signals received in the Negative and Positive Info respectively do not have a significant effect on beliefs compared with no signals received in the previous round.

As a robustness check, table 7 displays the determinants of the proportion of endowment reported within-treatment for the No Info, Full Info, Positive Info and Negative Info. The models are estimated by implementing simultaneous Tobit models in order to address the simultaneity between the subjective probability stated by the subjects and their reporting decisions. We estimate simultaneous two-limit Tobit models in column 1 to 4 and simultaneous upper-limit Tobit models in column 5 to 8. We observe that the results in Table 7 are consistent with Result 7. In the Positive Info, receiving strong (by being two) positive signals have a positive and significant effect in the reporting decisions compared with no signals received in t-1 (in both Column 3 and 7). Instead, receiving non-strong (by being only one) negative signals in the Negative Info have a negative and significant effect compared with no signals received in the previous round (in both Column 4 and 8). Instead, we do not observe a significant effect of any signal compared with no signal received in the Full Info.

Table 7: Simultaneous two limits and upper limit Tobit cluster at group level for the proportion of endowment reported by treatment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	TWO LIMITS				UPPER LIMIT			
VARIABLES	Proportion Reported				Proportion Reported			
	No Info	Full Info	Positive Info	Negative Info	No Info	Full Info	Positive Info	Negative Info
Lagged No signal	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*	*Ref*
Lagged Positive Signal		0.0619	-0.0626			0.0495	-0.0418	
		(0.0807)	(0.121)			(0.0642)	(0.0763)	
Lagged Two Positive Signal		0.223	0.634*			0.140	0.441*	
		(0.321)	(0.379)			(0.271)	(0.230)	
Lagged Negative Signal		-0.0298		-0.146**		-0.00912		-0.0973*
		(0.102)		(0.0668)		(0.0718)		(0.0501)
Lagged Two Negative Signal		0.00383		0.0169		-0.00442		0.0227
		(0.121)		(0.139)		(0.0864)		(0.101)
Lagged Two Mixed Signal		0.235				0.175		
		(0.242)				(0.195)		
Subjective Probability	0.226	1.151	0.447	0.310	0.0920	0.804	0.171	0.337
	(0.575)	(0.812)	(0.640)	(0.665)	(0.457)	(0.544)	(0.409)	(0.485)
Lagged Audited	-0.0445	0.0641	-0.262**	-0.0853	-0.0358	0.0390	-0.164**	-0.0606
	(0.0816)	(0.0853)	(0.120)	(0.0800)	(0.0500)	(0.0699)	(0.0710)	(0.0569)
Period	0.00348	0.0104	0.000600	-0.00282	0.00406	0.0103*	0.00127	-0.00133
	(0.00650)	(0.00777)	(0.00495)	(0.00490)	(0.00467)	(0.00591)	(0.00294)	(0.00416)
Age	0.0314	-0.0697***	-0.0188	0.0198	0.0230*	-0.0512***	-0.0115	0.0154
	(0.0201)	(0.0242)	(0.0166)	(0.0160)	(0.0133)	(0.0159)	(0.00970)	(0.0122)
Female	0.231	-0.198	-0.0223	-0.0550	0.134	-0.186	-0.0232	-0.0710
	(0.183)	(0.190)	(0.250)	(0.119)	(0.140)	(0.148)	(0.172)	(0.0825)
Europeans	0.177	0.111	0.412***	0.296***	0.108	0.0724	0.251***	0.249***
	(0.141)	(0.123)	(0.151)	(0.0888)	(0.0932)	(0.0926)	(0.0937)	(0.0609)
Constant	-0.155	1.868***	0.968*	0.0527	0.158	1.619***	0.960**	0.172
	(0.525)	(0.593)	(0.521)	(0.455)	(0.346)	(0.433)	(0.385)	(0.337)
Observations	1,026	1,026	1,026	1,026	1,026	1,026	1,026	1,026

Robust standard errors in parentheses*** p<0.01, ** p<0.05, * p<0.1

We control for past audit ("Lagged audited") and, consistent with Result 7, it has only a significant effect in the Positive Info. In fact, subjects tend to report less after being audited

We also control for the stated subjective probability, demographic characteristics of the subjects and time trend. We do not observe a significant increase of the reporting decisions over time. The stated subjective probability also does not have an effect in the reporting decisions. Europeans participants tend to report significantly more than non-Europeans in the Positive and Negative Info.

4. Conclusion

We implement a controlled laboratory experiment to investigate the effect of social information has an effect on the level of individual compliance in a fixed-six-nodes circle network, particularly when the information is positive (compliant behaviour) and negative (un-compliant behaviour). Few attempts have been made to document an impact of social interactions on tax compliance using experimental data. What little evidence exists is rather inconclusive (see Fortin et al., 2007; Lefebvre et al., 2014). We analyse whether individuals have the same attitude towards tax evasion by introducing four information conditions: No Info, Full Info, Positive Info and Negative Info.

In the No info treatment, individuals get individual information about whether they were audited, the outcome of it and her final payoff. In the Positive Info (Negative Info) treatment, participants get information whether the nodes they are connected with were audited and found compliant (noncompliant). We run a second control treatment (Full Info) in which participants get both positive and negative signals.

This tax game has four interesting features: First, and following Fortin et al. (2007), individual monetary payoffs do not depend on the other participants' behavior. This allows us to isolate better the effect of social information in the network. Second, we specifically control for the effect of signals on participants' beliefs on the ex-ante fixed and unknown probability of being audited by asking them to use an incentive compatible mechanism. Third, we keep the tax rate and fine rate fixed and known by the subjects. Four, subjects earn their endowment in each round in a real effort task.

Our results suggest that positive signals (good examples) and negative signals (bad examples) have a significant impact on the reporting and full compliance decisions at individual level. However, we find mixed results at aggregate level.

In the Negative Info, average compliance and the average stated subjective audit probability are significantly lower in comparison with the No Info treatment. This suggests that subjects comply significantly less because their beliefs about the audit probability are significantly lower in the Negative Info than in the No Info. In fact, beliefs of the subjective audit probability decreases significantly over time in the Negative Info, while it does not vary in the No Info, Full Info and Positive Info treatments. Conversely, strong (for being two) positive signals about neighbors' compliance behavior do encourage a higher compliance in the Positive Info and in the Full Info compared with the No Info treatment.

In the Negative Info, subjects tend to comply significantly less after receiving non-strong (only one) negative signals from the linked neighbors in comparison with no signals received in the previous round. This result suggests that non-strong negative signals cause a contagious effect on individuals' behavior. The fact that participants tend to comply less after receiving bad examples is in line with previous experimental literature, which find evidence of the "broken window effect" (Fortin et al., 2007; Innes and Mitra, 2013; Lefevbre et al., 2014).

Positive signals in the Positive Info treatment, even though significantly less frequent than negative signals in the Negative Info, have a positive and significant effect on full compliance and reporting decisions in comparison with no signals received in the previous round. This result suggests that good examples have a disciplinary effect on subjects' decisions.

Overall, our experimental study offers a precious insight on the role of positive and negative signals (good and bad examples, respectively) on individuals' tax compliance decisions. In fact, social information has an important effect on tax compliance when people are interconnected nodes in a network because they tend to value more the behavior of others they know better (Lefebvre et al., 2014). In this context seems relevant to consider how diffusion of social information regarding others' tax compliance behavior across social networks could impact individuals' decisions on tax compliance. Our results suggest that allowing for diffusion of good examples may have a disciplinary effect on tax compliance decisions and so, it could be a good way to improve tax compliance while, diffusion of bad examples seems to have a deteriorating effect on individuals' tax compliance decisions within networks.

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Appendix

Chapter 1

In *Gov* We Trust:
Voluntary compliance in networked investment games

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Instructions

This is an experiment to study decision-making. The instructions are simple and if you follow them carefully you will get an amount of money in cash at the end of the experiment in a confidential manner. All through the experiment you will be treated anonymously. Neither the experimenters nor the people in this room will ever know your particular choices or the amount of money that you get. Talking is forbidden during the experiment. You cannot use your mobile phones while in the laboratory. If you have any questions, raise your hand and remain silent. You will be attended to as soon as possible.

This experiment consists of two different blocks. We will explain now the instructions of Block 1 and at the end of this block you will receive the instructions of Block 2.

First Block

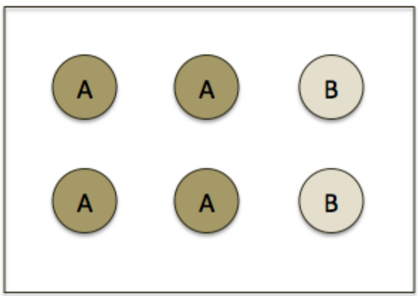
All participants face a simple task. The task consists of adding two-digit numbers for 3 minutes. We will call this task the *adding task*. The more correct answers you get, the more earnings you make in this block, as each correct answer is paid £0.05. Errors do not count.
Once the task is completed, you will get information about your performance (the number of correct answers and your earnings). You will not get information about the performance of the other participants.

Second block

The second block has a total of 20 rounds. At the beginning of the experiment, you will be randomly assigned to a group of six participants. The composition of each group will not change, but you will never know the identity of the other participants in your group.

Figure 1

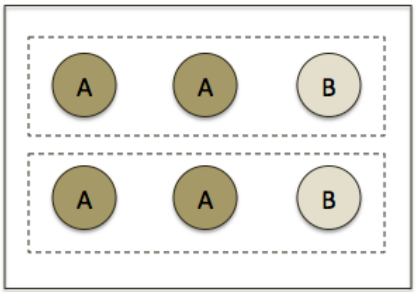
Group of six participants



At the beginning of the experiment, you will be randomly assigned to one of the two following roles: A and B. Your roles will not change along the experiment, so you will be A or B during the 20 rounds. There will be twice as many A than B participants, so the probability of becoming A (B) is 2/3 (1/3). You will know your role as soon as the second block starts.

Figure 2

Teams of three participants



Each round you will make decisions in independent teams of three people: two A and one B. At the end of each round teams will be reshuffled within groups. That is, you may or may not be playing with the same participants in the next round. As the number of rounds (20) exceeds the number of group members (6), you know for sure you will be repeatedly interacting with the other participants in your group, even when the probability of making decisions with the same participants in two consecutive rounds is very low.

Participants A and B will make different decisions in this block, so we will present them one after the other.

Participants A

As a participant A, you would make three different types of decisions per round, in three stages:

Stage 1: Adding task
You would face again the *adding task* at the beginning of each round. You would add two-digit numbers for 1 minute. Your individual round endowment will be determined by your individual performance, as your endowment will be the number of correct answers multiplied by 2 with a maximum of 10 ECU (Experimental Currency Units). Getting more than 5 correct answers does not give you additional endowment. At the end of the minute, you will get information about your performance and your endowment; you will get no information about the performance of others.

Stage 2: Rolling a die
Once you get your endowment, you will roll a virtual die of six faces. All numbers from 1 to 6 share the same probability of being selected by the computer. The number you get gives you a rule to follow in your next decision.

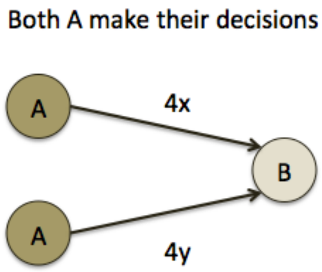
Stage 3: Sending
Each number is associated with one specific rule about the proportion of your endowment to send to the participant B you are matched with in that particular round. The table below shows the amount to send associated to each number:

Outcome of the die:	**1**	**2**	**3**	**4**	**5**	**6**
% of endowment you keep	100%	80%	60%	40%	20%	0%
% of endowment you send	0%	20%	40%	60%	80%	100%

Note that the rule is not binding. In other words, you may or may not follow the rule (and send a different proportion of your endowment to B). You will be the only participant knowing the outcome of the die.

Your outcome will be determined by your decision and B's decision. We will multiply by 4 any amount you send to B. For instance, if you send 10 ECU, he will get 40. If you send 8, he will get 32, and so on. The following figure represents the decisions made by both participants A sending two amounts (x, y) to B, being both amounts multiplied by 4.

Figure 3



B's decisions are explained below.

Participants B

As a participant B, you would make two different types of decisions per round, in two stages:

Stage 1: Rolling two dice
After you get information about the amount sent by the participants A you are matched with in that particular round, you roll a virtual die of six faces twice. All numbers from 1 to 6 share the same probability of being selected by the computer. The numbers give you again a rule to follow in your next decision.

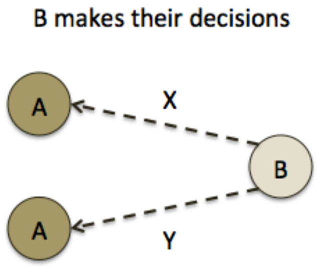
Stage 2: Sending
Each number is associated with a specific rule about how much to send back to each A in that particular round. The table below links each outcome of the die with a proportion to keep and to send back to each A:

Outcome of the die:	**1**	**2**	**3**	**4**	**5**	**6**
% you keep	100%	80%	60%	40%	20%	0%
% you send back to A	0%	20%	40%	60%	80%	100%

Note that the rule is not binding. In other words, you may or may not follow the rule (and send a different proportion to each A). You will be the only participant knowing the outcome of each die.

You will get information about the amounts sent by each A in one screen. For simplicity, you will make a decision about how much to send back to each A in separated screens. So, you will first be reminded about the amount sent by the first participant A you are matched with (let's call him A1), multiplied by four. You will roll the first virtual die and make a decision about how much to send to A1. Then you will be reminded about the amount sent by A2, multiplied by 4, and you will throw a second die and make a second decision. Both decisions may or may not be the same. The following figure shows the decisions of B.

Figure 4



Participants A will never know the outcome of each die. They will only be informed about the amount sent back to each of them.

Earnings and some examples

At the end of the experiment, we will randomly select one of the rounds to compute your earnings. As all round shave the same probability of being selected, you should pay attention to every decision you make.
Participants A will be paid depending on their interactions with the participant B in that round, following the logic explained above. Participants B will be paid by ONE of the interactions with ONE participant A, plus a fixed amount of 5 ECU.
Imagine that round 17 is randomly selected to compute payoffs. Then, both A1 and A2 will be paid the amount of ECU they get back from B, using the exchange rate of 1 ECU= £0.40.
To compute the earnings of participant B, one of the two interactions is also randomly selected. Imagine that the interaction with participant A2 in that round 17 is randomly chosen. Then, participant B earns the amount he keeps in that interaction (that is, what he gets from A2 multiplied by 4 minus what he sends back), plus the fixed amount of 5 ECU. The total amount of ECU is then exchanged to £ using the same rate (1 ECU= £0.40).

Let us show you some examples of how earnings are computed in some polar cases. For simplicity, we will assume that both players will make similar decisions in all rounds, and they will be paid by one of this rounds at random.

Case 1

Participants A always get an endowment of 10 ECU.

Participants A never follow the rule suggested by the die, and decide to send nothing to B.

Participant B in this case does nothing, as there is nothing to decide about.

Participants A will be paid at the end of the experiment 10 ECU (the amount they keep) and participant B gets 5 ECU, or £2.50 and £1.25, respectively (plus their earnings in the first block of the experiment).

Case 2

Participants A always get an endowment of 10 ECU.

Participants A always follow the rule suggested by the die. They will be sending 0, 2, 4, 6, 8 or 10 ECU depending on the number they get back from the die. As the outcome of the die is absolutely random, both A will be sending each amount one sixth of the times, as all the amounts are equally likely. On average each A will be sending 5 ECU (the average of 0, 2, 4, 6, 8 and 10), and B will be getting on average 20 ECU.

Participant B never follows the rule suggested by the die, and decides to send nothing back to participants A.

Participants A will be paid at the end of the experiment 5 ECU on average, as they sent half of their initial endowment. Participant B will get on average 20 ECU, or £5.

Case 3

Participants A always get an endowment of 10 ECU.

Participants A always follow the rule suggested by the die. They will be sending 0, 2, 4, 6, 8 or 10 ECU depending on the number they get back from the die. As the outcome of the die is absolutely random, both A will be sending each amount one sixth of the times, as all the amounts are equally likely. On average each A will be sending 5 ECU (the average of 0, 2, 4, 6, 8 and 10), and B will be getting on average 20 ECU.

Participant B always follows the rule suggested by the die, and decides to send back to participants A 0%, or 20%, or 40%, and so on. Again, the outcome of the die is random in the sense that all amounts are equally likely. So, in average, he will send back the average of 0-20-40-60-80-100%, which is 50%. As he gets (on average) 20 ECU, he will send back (on average) 10 ECU.

Participants A will be paid at the end of the experiment 15 ECU, as they sent 5 ECU and get 10 ECU back from B. Participant B will also get on average 15 ECU, because he has 5 ECU as a fixed payoff, and keeps half of the amount he gets (another 10 ECU).

Case 4

Participants A always get and endowment of 10 ECU.

Participants A never follow the rule suggested by the die, but decide to send more than the outcome of it. They will send 2, 4, 6, 8 or 10 ECU. On average each A will be sending 6 ECU (the average of 2, 4, 6, 8 and 10), and B will be getting on average 24 ECU.

Participant B never follows the rule suggested by the die, and decides to send back to participants A more than the die says, 20%, or 40%, and so on. So, in average, he will send back the average of 20-40-60-80-100%, which is 60%. As he gets (on average) 24 ECU, he will send back (on average) 14.4 ECU.

Appendix

Chapter 2

Trust, Social Information and Compliance in a Three-Node Network:
the role of social status

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Appendix 1

1. Comparison between studies (Chapter 1 and 2): random assignment to roles vs. allocation of roles by merit

In this appendix, we want to compare the results obtained in the Chapter 1 (Borzino et al., 2015) and Chapter 2. As shown in table 1 below, we implement different selection mechanism of the fixed roles in the two studies. In Chapter 1, a random allocation of the roles (sender and receiver) is implemented. In Chapter 2, we manipulate the social status by assigning the roles according to the performance of the players in a preliminary stage: best performers become high status receivers and, bottom performers become low status senders. By comparing the two experiments, we study the effect of social status on trust, trustworthiness and compliance in each information conditions.

Table 1: Experimental dimensions of Chapter 1 (Borzino et al., 2015) and Chapter 2

		Information Treatments		
Selection Mechanism of Roles	Random (Borzino et al., 2015)	Baseline (6 indep. observations)	Reputation (6 indep. observations)	Transparency (6 indep. observations)
	Merit (Chapter 2)	Baseline Merit (6 indep. observations)	Reputation Merit (6 indep, observations)	Transparency Merit (7 indep. observations)

In table 2, we present the descriptive statistics of both studies with and without social status. The first column illustrates the average proportion sent, including the cases in which senders exhibited no trust. We do not observe significant differences when we compare the average proportion sent by the senders between the bundles of the two baselines, the two reputation and the two transparency treatments. In the second column, we consider only the cases in which positive amounts were sent to the receivers. Again, we do not find any significant difference.

Table 2: descriptive statistics across experiments- TRUST and TRUSTWORTHINESS

	Trust (Proportion X sent by senders)			**Trustworthiness** (Proportion Y return by receivers)		
	Amount Sent (Including X=0)	Amount Sent (X>0 only)	Proportion of X=0	Amount Return (Including X=0)	Amount Return (X>0 only)	Proportion of Y=0 (X>0 only)
Total	0.334	0.5313	37.12%	0.225	0.303	29.74%
	(0.1008)	(0.1119)		(0.0099)	(0.0131)	
Baseline	0.3795	0.505	24.84%	0.2833	0.3767	24.10%
	(0.3213)	(0.2707)		(0.3156)	(0.3118)	
Baseline Merit	0.2887	0.5633	49.38%*	0.1675*	0.23*	35.38%*
	(0.34680	(0.2904)		(0.2899)	(0.3424)	
Total	0.3606	0.5076	28.96%	0.2179	0.267	25.77%
	(0.0106)	(0.0107)		(0.0083)	(0.0098)	
Reputation	0.3454	0.4848	28.75%	0.2288	0.3211	25.44%
	(0.3165)	(0.272)		(0.2697)	(0.2692)	
Reputation Merit	0.3758	0.5703	29.17%	0.207	0.2123	26.10%
	(0.3432)	(0.2771)		(0.2452)	(0.2543)	
Total	0.4342	0.5609	22.60%	0.243	0.283	25.09%
	(0.0106)	(0.1007)		(0.0081)	(0.0091)	
Transparency	0.4388	0.5345	17.92%	0.2433	0.2964	25.38%
	(0.3206)	(0.272)		(0.2697)	(0.2596)	
Transparency Merit	0.4303	0.5863	26.61%	0.2435	0.269	24.82%
	(0.363)	(0.2966)		(0.2666)	(0.2703)	

Notes: Standard deviations are displayed in parentheses; Stars report significance level from Wilcoxon Mann-Whitney tests run on independent observations (cohorts of 6 participants) to confirm differences with the baseline treatment.

*90% significance ** 95% significance *** 99% significance

In the third column, we present the proportion of no trust displayed by senders. In this case, we found a marginally significant difference when we compare the baseline with the baseline merit treatment (p=0.0782). The proportion of nothing sent in the baseline merit treatment by the low status senders almost doubled the one in the baseline treatment. In fact, in the baseline merit, low status senders play the Nash equilibrium 49,38% of the times compared with the 24.84% in the baseline treatment.

The right part of table 2 shows the results for trustworthiness. Trustworthiness is marginally higher in the baseline compared with the baseline merit treatment (p=0.0782). However, when we compare the reputation (transparency) with the reputation merit (transparency merit) treatments, no significant difference are found.

In the second column, we only consider the return decisions when the amount sent was positive (X>0). Receivers send very marginally less in the baseline merit condition compared with receivers in the baseline treatment (p=0.1020). No differences were found when we compare reputation with reputation merit and transparency with transparency merit.

In the third column, we display the average proportion of no return (no trustworthiness) when the amount received was positive (X>0). Receivers return positive amounts fewer times in the baseline merit than the receivers in the baseline (p=0.0921). The difference between the reputation and reputation merit treatment as well as the difference between the transparency and transparency merit treatments are not significant.

Result 1: Trustworthiness is lower in the baseline merit compared with the baseline, in which no manipulation of social status is implemented. Senders play more often the Nash equilibrium in the baseline merit compared with senders in the baseline treatment.

1.*1*. Econometric Analysis

1.1.1- Trust

Table 3 reports the econometric regressions for trust. We control for past decisions (proportion sent) as well as for the outcome of the die, period and demographic variables. The variable *Lag Proportion Sent* corresponds to the decision of the senders in the previous round. We find no significant affect of merit in any of the conditions.

However, we find a gender effect when we compare the baseline merit with the baseline condition and, when we compare the transparency and the transparency merit treatments. Female senders tend to send significantly less in the baseline merit compared with the female senders in the baseline. Furthermore, low status female senders tend to send significantly less in the transparency merit compared with the female senders in the transparency.

Result 2: Low status female senders tend to send less in the baseline merit and in the transparency merit compared with female senders in the baseline merit and in the transparency merit treatments, respectively.

Table 3: Random effects ordered probit for Trust- pooled samples

	Proportion sent by the sender (X_{it})		
	(1) Baseline vs Baseline Merit	(2) Reputation vs Reputation Merit	(3) Transparency vs Transparency Merit
Outcome of die (Rule)	0.1121***	0.1362***	0.2006***
	(0.0356)	(0.0256)	(0.0431)
Period	-0.0217**	-0.01944	-0.0120**
	(0.011)	(0.0142)	(0.0036)
Baseline treatment	*Ref*	-	-
Baseline merit treatment	-0.6299	-	-
	(0.4378)		
Reputation treatment	-	*Ref*	-
Reputation merit treatment	-	0.0986	-
		(0.3198)	
Transparency treatment	-	-	*Ref*
Transparency merit treatment	-	-	-0.225
			(0.2495)
Female	-0.4983**	0.143	-0.5261**
	(0.2515)	(0.333)	(0.2166)
Age	0.0462	0.0221	0.0386***
	(0.0810)	(0.0849)	(0.0123)
British	-0.2847	-0.128	0.0777
	(0.3852)	(0.4104)	(0.2587)
Log-Pseudo likelihood	-1340.3845	-1440.8406	-1647.2247
Observations	960	960	1040
Number of indiv	48	48	52

Robust standard errors in parentheses*** p<0.01, ** p<0.05, * p<0.1

1.1.2. Trustworthiness

In table 4, we report the regressions on trustworthiness considering only the cases of positive trust. We control for past decisions, outcome of the die and demographics. We find that the receivers tend to return significantly less in the baseline merit compared with the receivers in the baseline (p=0.039). This finding is consistent with table 2 and Result 1.

In the second column, we display the regression comparing the trustworthiness of receivers in the reputation merit with the one of receivers in the reputation treatment, which is our reference. We do not find any significant difference.

In the third column, we present the econometric results and again no significant difference when we compare the level of trustworthiness in the transparency merit treatment with the transparency treatment.

Table 4: Random effects ordered probit for Trustworthiness- pooled samples

	Proportion returned by Receivers (Y_{it})		
	(1) Baseline vs Baseline Merit	(2) Reputation vs Reputation Merit	(3) Transparency vs Transparency Merit
Outcome of the Die (Rule)	-0.109	0.0144	-0.0303***
	(0.0224)	(0.0145)	(0.0227)
Baseline treatment	*Ref*	-	-
Baseline merit treatment	-0.6948**	-	-
	(0.489)		
Reputation treatment	-	*Ref*	-
Reputation merit treatment	-	0.126	-
		(0.4022)	
Transparency treatment	-	-	*Ref*
Transparency merit treatment	-	-	-0.209
			(0.3590)
Period	-0.0261***	-0.0280**	-0.0214
	(0.00594)	(0.025)	(0.0089)
Age	0.0257	0.0800	0.135**
	(0.0247)	(0.148)	(0.0571)
British	-0.0939	-0.0865	0.194
	(0.6014)	(0.4645)	(0.413)
Female	0.0962	0.3192	-0.5081**
	(0.3086)	(0.4009)	(0.2466)
Log Pseudo Likelihood	-1171.9163	-1222.4644	-1440.467
Observations	960	960	1040
Number of indiv	24	24	26

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

2. Conclusion

We compare the results obtained in Chapter 1 (Borzino et al, 2015) with the results from the current study. This means that we compare three treatments (baseline, reputation and transparency), in which the roles are assigned randomly, with the three treatments in the current study (baseline merit, reputation merit and transparency merit), in which we give to the subjects the same information and we manipulate the social status of the players. Our findings suggest that the introduction of social information has a positive impact on the level of trust in networks with no differences in social status and in networks characterized by differences in social status.

Appendix 2

Instructions

This is an experiment to study decision-making. The instructions are simple and if you follow them carefully you will get an amount of money in cash at the end of the experiment in a confidential manner. All through the experiment you will be treated anonymously. Neither the experimenters nor the people in this room will ever know your particular choices or the amount of money that you get. Talking is forbidden during the experiment. You cannot use your mobile phones while in the laboratory. If you have any questions, raise your hand and remain silent. You will be attended to as soon as possible.

This experiment consists of two different blocks. We will explain now the instructions of Block 1 and at the end of this block you will receive the instructions of Block 2.

First Block

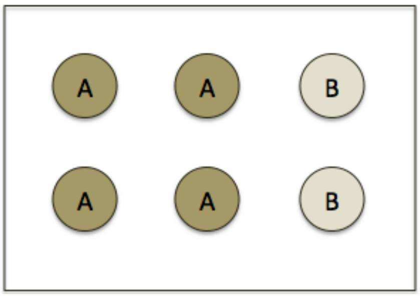
All participants face a simple task. The task consists of adding two-digit numbers for 3 minutes. We will call this task the *adding task*. The more correct answers you get, the more earnings you make in this block, as each correct answer is paid £0.05. Errors do not count.
Once the task is completed, you will get information about your performance (the number of correct answers and your earnings). The number of correct answers that you get at this stage, will determine your role in the game. The players with more correct answers will get the role of B and the players with less correct answers will get the role of A. You will keep the same role for the entire experiment.

Second block

The second block has a total of 20 rounds. At the beginning of the experiment, you will be assigned one of the two roles (A or B) depending on your performance in the trial phase. The players with more correct answers will be get the role of B (top performers) and the players with less correct answers will get the role of A (bottom performers). You will be part of a group of six participants. The composition of each group will not change, but you will never know the identity of the other participants in your group.

Figure 1

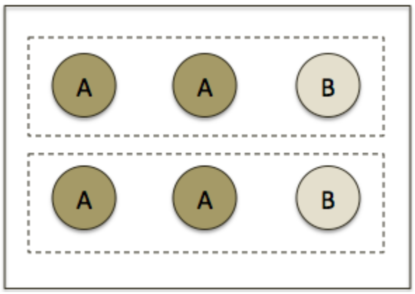
Group of six participants



Your roles will not change along the experiment, so you will be A or B during the 20 rounds. There will be twice as many A than B participants. You will know your role as soon as the second block starts.

Figure 2

Teams of three participants



Each round you will make decisions in independent teams of three people: two A and one B. At the end of each round teams will be reshuffled within groups. That is, you may or may not be playing with the same participants in the next round. As the number of rounds (20) exceeds the number of group members (6), you know for sure you will be repeatedly interacting with the other participants in your group, even when the probability of making decisions with the same participants in two consecutive rounds is very low.

Participants A and B will make different decisions in this block, so we will present them one after the other.

Participants A

As a participant A, you would make three different types of decisions per round, in three stages:

Stage 1: Adding task

You would face again the *adding task* at the beginning of each round. You would add two-digit numbers for 1 minute. Your individual round endowment will be determined by your individual performance, as your endowment will be the number of correct answers multiplied by 2 with a maximum of 10 ECU (Experimental Currency Units). Getting more than 5 correct answers does not give you additional endowment. At the end of the minute, you will get information about your performance and your endowment; you will get no information about the performance of others.

Stage 2: Rolling a die
Once you get your endowment, you will roll a virtual die of six faces. All numbers from 1 to 6 share the same probability of being selected by the computer. The number you get gives you a rule to follow in your next decision.

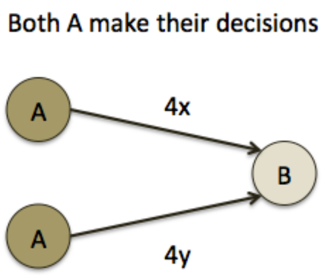
Stage 3: Sending
Each number is associated with one specific rule about the proportion of your endowment to send to the participant B you are matched with in that particular round. The table below shows the amount to send associated to each number:

Outcome of the die:	**1**	**2**	**3**	**4**	**5**	**6**
% of endowment you keep	100%	80%	60%	40%	20%	0%
% of endowment you send	0%	20%	40%	60%	80%	100%

Note that the rule is not binding. In other words, you may or may not follow the rule (and send a different proportion of your endowment to B). You will be the only participant knowing the outcome of the die.

Your outcome will be determined by your decision and B's decision. We will multiply by 4 any amount you send to B. For instance, if you send 10 ECU, he will get 40. If you send 8, he will get 32, and so on. The following figure represents the decisions made by both participants A sending two amounts (x, y) to B, being both amounts multiplied by 4.

Figure 3



B's decisions are explained below.

Participants B

As a participant B, you would make two different types of decisions per round, in two stages:

Stage 1: Rolling two dice
After you get information about the amount sent by the participants A you are matched with in that particular round, you roll a virtual die of six faces twice. All numbers from 1 to 6 share the same probability of being selected by the computer. The numbers give you again a rule to follow in your next decision.

Stage 2: Sending

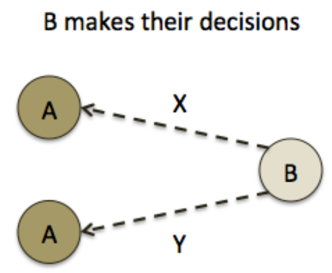
Each number is associated with a specific rule about how much to send back to each A in that particular round. The table below links each outcome of the die with a proportion to keep and to send back to each A:

Outcome of the die:	**1**	**2**	**3**	**4**	**5**	**6**
% you keep	100%	80%	60%	40%	20%	0%
% you send back to A	0%	20%	40%	60%	80%	100%

Note that the rule is not binding. In other words, you may or may not follow the rule (and send a different proportion to each A). You will be the only participant knowing the outcome of each die.

You will get information about the amounts sent by each A in one screen. For simplicity, you will make a decision about how much to send back to each A in separated screens. So, you will first be reminded about the amount sent by the first participant A you are matched with (let's call him A1), multiplied by four. You will roll the first virtual die and make a decision about how much to send to A1. Then you will be reminded about the amount sent by A2, multiplied by 4, and you will throw a second die and make a second decision. Both decisions may or may not be the same. The following figure shows the decisions of B.

Figure 4



Participants A will never know the outcome of each die. They will only be informed about the amount sent back to each of them.

Earnings and some examples

At the end of the experiment, we will randomly select one of the rounds to compute your earnings. As all round shave the same probability of being selected, you should pay attention to every decision you make.
Participants A will be paid depending on their interactions with the participant B in that round, following the logic explained above. Participants B will be paid by ONE of the interactions with ONE participant A, plus a fixed amount of 5 ECU.
Imagine that round 17 is randomly selected to compute payoffs. Then, both A1 and A2 will be paid the amount of ECU they get back from B, using the exchange rate of 1 ECU= £0.40.
To compute the earnings of participant B, one of the two interactions is also randomly selected. Imagine that the interaction with participant A2 in that round 17 is randomly

chosen. Then, participant B earns the amount he keeps in that interaction (that is, what he gets from A2 multiplied by 4 minus what he sends back), plus the fixed amount of 5 ECU. The total amount of ECU is then exchanged to £ using the same rate (1 ECU= £0.40).

Let us show you some examples of how earnings are computed in some polar cases. For simplicity, we will assume that both players will make similar decisions in all rounds, and they will be paid by one of this rounds at random.

Case 1

Participants A always get an endowment of 10 ECU.

Participants A never follow the rule suggested by the die, and decide to send nothing to B.

Participant B in this case does nothing, as there is nothing to decide about.

Participants A will be paid at the end of the experiment 10 ECU (the amount they keep) and participant B gets 5 ECU, or £2.50 and £1.25, respectively (plus their earnings in the first block of the experiment).

Case 2

Participants A always get an endowment of 10 ECU.

Participants A always follow the rule suggested by the die. They will be sending 0, 2, 4, 6, 8 or 10 ECU depending on the number they get back from the die. As the outcome of the die is absolutely random, both A will be sending each amount one sixth of the times, as all the amounts are equally likely. On average each A will be sending 5 ECU (the average of 0, 2, 4, 6, 8 and 10), and B will be getting on average 20 ECU.

Participant B never follows the rule suggested by the die, and decides to send nothing back to participants A.

Participants A will be paid at the end of the experiment 5 ECU on average, as they sent half of their initial endowment. Participant B will get on average 20 ECU, or £5.

Case 3

Participants A always get an endowment of 10 ECU.

Participants A always follow the rule suggested by the die. They will be sending 0, 2, 4, 6, 8 or 10 ECU depending on the number they get back from the die. As the outcome of the die is absolutely random, both A will be sending each amount one sixth of the times, as all the amounts are equally likely. On average each A will be sending 5 ECU (the average of 0, 2, 4, 6, 8 and 10), and B will be getting on average 20 ECU.

Participant B always follows the rule suggested by the die, and decides to send back to participants A 0%, or 20%, or 40%, and so on. Again, the outcome of the die is random in the sense that all amounts are equally likely. So, in average, he will send back the average of 0-20-40-60-80-100%, which is 50%. As he gets (on average) 20 ECU, he will send back (on average) 10 ECU.

Participants A will be paid at the end of the experiment 15 ECU, as they sent 5 ECU and get 10 ECU back from B. Participant B will also get on average 15 ECU, because he has 5 ECU as a fixed payoff, and keeps half of the amount he gets (another 10 ECU).

Case 4

Participants A always get and endowment of 10 ECU.

Participants A never follow the rule suggested by the die, but decide to send more than the outcome of it. They will send 2, 4, 6, 8 or 10 ECU. On average each A will be sending 6 ECU (the average of 2, 4, 6, 8 and 10), and B will be getting on average 24 ECU.

Participant B never follows the rule suggested by the die, and decides to send back to participants A more than the die says, 20%, or 40%, and so on. So, in average, he will send back the average of 20-40-60-80-100%, which is 60%. As he gets (on average) 24 ECU, he will send back (on average) 14.4 ECU.

Appendix

Chapter 3

Networks, Spillovers, and Compliance

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Instructions

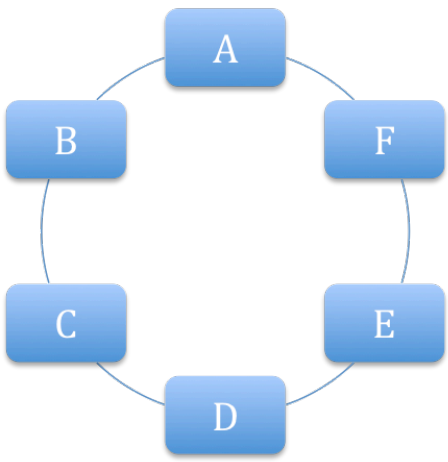
Welcome to our experiment! This is an experiment about decision-making. The instructions are simple. If you follow them carefully, you can earn more money depending on your own decisions. These instructions and your decisions in this experiment are solely your private information. During the experiment you are not allowed to communicate with any of the other participants or with anyone outside the laboratory. Please switch off your mobile phone now. If you have any questions at any time during the course of this experiment, please raise your hand. An experimenter will assist you privately.

This experiment consists of twenty (20) consecutive decision rounds. Each decision round consists of two stages described below. Your payoff in this experiment will be in Experimental Currency Units (ECU). Once the experiment is over, the computer will randomly select one round, which will be used for payments. That means you will be paid for one (randomly chosen) round out of the 20 rounds. The payoff from the selected round will be converted to pounds at the following rate:

75 ECUS = £1

Your decisions will be recorded privately at your computer terminal. You will be paid individually and privately in cash at the end of the experiment.

At the beginning of the experiment, participants will be randomly divided into groups of six (6) individuals. The composition of the groups will remain the same in each round. This means that you will interact with the same people in your group throughout the experiment. Each player will be given an identifying letter: A,B,C,D, E and D. You will see your ID on your screen. As you can see from figure below, each group member will be connected to two group members. For example, if your identifying letter is A, you will be connected to players B and F. Individuals in your group will NOT be identified in any way. Thus, information about individual results will be completely anonymous.



<u>**STAGE 1 OF EACH ROUND**</u>

In Stage 1 of each round, you will have the opportunity to make money (ECUs) by performing a task, which will last **60 seconds**.

The task consists of adding two-digit numbers. For each correct answer, you will receive 2 ECU. The maximum amount of ECU you could earn each round is 10 ECU. Once you have finished your task, you will be informed about how many ECU you have earned. At the same time, the computer will randomly select one factor out of the following three: 50, 100 or 150. Your earnings from Stage 1 consist of total ECUS from the adding task multiplied by the randomly selected factor.

Your earnings in Stage 1 = (ECU from adding task)*(selected factor)

At the end of Stage 1, you will be informed about how many ECU you earned from the adding task; the selected multiplying factor and your total earnings from Stage 1.

STAGE 2 OF EACH ROUND

Your task in Stage 2 of each round will be to decide how many ECU from your earnings in Stage 1 you want to state (from 0 up to your total earnings in Stage 1). Once you have made your decision a deduction fee will be applied. This deduction fee will consist of 25% of your stated earnings.

After all participants have made their decisions in Stage 2, you will be informed of your stated earnings in Stage 2, the deduction fee applied to you and your final earnings from Stage 2.

STAGE 3 OF EACH ROUND

After all individuals have made their decisions in Stage 2, in Stage 3 the computer will randomly select, with a probability *p* (from 0 to 100), some group members. This probability *p* is unknown to all individuals. If the computer selects you, it will check whether or not your stated earnings in Stage 2 are equal to your earnings in Stage 1. If your stated earnings in Stage 2 were not equal to your earnings in Stage 1, the difference between your earnings from Stage 1 and your stated earnings in Stage 2 will be subtracted from your earnings. If your stated earnings in Stage 2 are equal to your earning in Stage1, no amount will be subtracted from your earnings. Same if the computer does not select you.

Before you are informed about the result of the computer random check, you will be asked to guess the value of the probability *p*. If your guess is within the interval ±10 of the real probability, you will receive 100 ECU extra at the end of the experiment.

YOUR EARNINGS IN STAGE 3

(i) If you WERE NOT selected by the computer:

Your earnings in Stage 3= Earnings in Stage 1
– (Stated Earnings in Stage 2)*0.25

(ii) If you WERE SELECTED by the computer & your stated earning in Stage 2 WERE NOT equal to your earnings in Stage 1:

Your earnings in Stage 3= Earnings in Stage 1
– (Stated Earnings in Stage 2)*0.25
– (Earnings in Stage 1 – Stated Earnings in Stage 2)

(ii) If you WERE SELECTED by the computer & your stated earning in Stage 2 WERE equal to your earnings in Stage 1:

Your earnings in Stage 3= Earnings in Stage 1
– (Stated Earnings in Stage 2)*0.25

After the computer finishes checking stated earnings from the selected individuals in the second stage, you will receive the following information about yourself :

- Whether or not you were selected by the computer;
- Whether or not your stated earnings in Stage 2 were equal to your earnings in Stage 1; and
- Your total earnings from Stage 3.

The same process will be repeated for a total of 20 rounds.
At the end of the experiment you will be informed about which round was selected for payment; your total earnings in the selected round; whether or not you got the extra 100 ECU from guessing the probability correctly in the selected round; and your final earnings from the experiment.

The following examples are for illustrative purposes only

Example 1.

Stage 1:

- Suppose that you have got 5 correct answers from the adding task.
- That means your have earned 2 ECU * 5 = 10 ECU from the adding task.
- Suppose that the randomly selected factor has been 100.
- Thus your total earnings from Stage 1 are 10 ECU*100 =1000 ECU.

Stage 2:

- Suppose that you state that your earnings are 1000 ECU.
- Thus your deduction fee will be 250 ECU (1000 ECU*0.25 = 250 ECU).
- Your earnings from Stage 2 will be 1000 ECU from Stage 1 – 250 ECU deduction fee = 750 ECU.

Stage 3:

- Suppose the computer selects you.
- Your stated earnings in Stage 2 were 1000 ECU = your earnings in Stage 1.
- Your earnings in Stage 3 will be your earnings from Stage 2 = 750 ECU.

Example 2.

Stage 1:

- Suppose that you have got 5 correct answers from the adding task.
- That means your have earned 2 ECU * 5 = 10 ECU from the adding task.
- Suppose that the randomly selected factor has been 100.
- Thus your total earnings from Stage 1 are 10 ECU*100 =1000 ECU.

Stage 2:

- Suppose that you state that your earnings are 500 ECU.
- Thus your deduction fee will be 125 ECU (500 ECU*0.25 = 125 ECU).
- Your earnings from Stage 2 will be 1000 ECU from Stage 1 – 125 ECU deduction fee = 875 ECU.

Stage 3:

- Suppose the computer selects you.
- Your stated earnings in Stage 2 were 500 ECU which are not equal to your earnings in Stage 1.
- Your earnings in Stage 3 will be earnings from Stage 2 – (earnings from Stage 1 – stated earnings in Stage 2) = 875 ECU – (1000 ECU – 500 ECU) = 375 ECU.

Example 3.

Stage 1:

- Suppose that you have got 5 correct answers from the adding task.
- That means your have earned 2 ECU * 5 = 10 ECU from the adding task.
- - Suppose that the randomly selected factor has been 100.
- Thus your total earnings from Stage 1 are 10 ECU*100 =1000 ECU.

Stage 2:

- Suppose that you state that your earnings are 500 ECU.
- Thus your deduction fee will be 125 ECU (500 ECU*0.25 = 125 ECU).
- Your earnings from Stage 2 will be 1000 ECU from Stage 1 – 125 ECU deduction fee = 875 ECU.

Stage 3:

- Suppose the computer does not select you.
- Your earnings in Stage 3 will be equal to your earnings in Stage 2 = 875 ECU.

QUESTIONS TO HELP YOU BETTER UNDERSTAND THE DECISION TASKS

When everyone has finished reading the instructions, and before the experiment begins, we will ask you a few questions regarding the decisions you will make in the experiment. The questions will help you understand the calculation of your earnings and ensure that you have understood the instructions.

Please answer these questions on your computer terminal. Please type your answer in the box next to the corresponding question. Once everyone has answered all questions correctly we will begin the experiment.