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Institutional Quality, Culture, and Norms of Cooperation: Evidence from a Behavioral Field Experiment

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Abstract

We design an experiment to examine the causal effect of legal institutional quality on informal norms of cooperation, and study the interaction of institutions and culture in sustaining economic exchange. 346 subjects in Italy and Kosovo play a market game under different and randomly allocated institutional treatments, which generate different incentives to behave honestly, preceded and followed by a non-contractible and non-enforceable trust game. Significant increases in individual trust and trustworthiness follow exposure to 'better' institutions. A reduction by one percentage point in the probability of facing a dishonest partner in the market game, which is induced by the quality of legal institutions, increases trust by 7 to 11%, and trustworthiness by 13 to 19%. This suggests that moral norms of cooperative behavior can follow improvements in formal institutional quality. Cultural origin, initial trust and trustworthiness influence opportunistic behavior in markets, but only in the absence of strong formal institutions.

Keywords: legal institutions, culture, trust, trustworthiness, markets, experimental methods

JEL classification: K40, O17, Z10

1. Introduction

How does the quality of institutions affect norms of good conduct, such as trust and trustworthiness? How do values and institutions interact to sustain economic exchange? While there is a consensus that both good formal institutions and high societal trust are beneficial for trade and development, how they interact and co-evolve is much less clear. On the one hand, the literature suggests a positive relationship. Theoretical models argue that well functioning and impartial enforcement of contracts enhances societal trust (Guiso, Sapienza and Zingales 2008a; Tabellini 2008). On the other hand, formal institutions, by reducing the marginal returns to being trustworthy, may crowd out trust and trustworthiness (Aghion et al. 2010). A scatter-plot of societal levels of trust against the quality of institutions in a cross-section of countries displayed in Figure 1 illustrates the complex nature of this relationship. While the correlation between trust and rule of law is positive, the correlation between trust and regulatory quality is nil or even slightly negative. Showing a causal link from institutions to trust with happenstance data is difficult because institutions and beliefs are co-determined (Piketty 1995)[i] and co-evolve under the influence of common historical events.[ii,iii]

In this paper, we use the experimental method to introduce an exogenous variation in the quality of formal enforcement institutions and measure their effect on moral norms of cooperation. We address two main questions. First, we study the causal effect of formal enforcement on informal norms of trust and trustworthiness, through their influence on cooperative behavior in markets. Second, we shed light on how institutions and pre-existing social norms interact to sustain market efficiency and cooperation.

Our experiment consists of four parts: first, a trust game to measure pre-existing social norms of trust and trustworthiness. Second, ten rounds of a market game, in which subjects chose

whether to trade honestly, cheat or stay out, in the absence of any institution. Third, ten rounds of the market game under one of two formal enforcement institutional treatments: either a Partial Enforcement System (PES) treatment, which reproduces basic features of a closed network justice system, such as the Mafia, or an Impartial Enforcement System (IES), which captures key traits of an impartial justice system for which all agents are equal in front of the law. Finally, another trust game identical to the first one. The different institutional treatments in the market game generate sharply different incentives to deviate from non-cooperation (that is, cheating in the market game). More precisely, the Nash equilibrium is to: cooperate with probability 0 under no institutions, with probability 1 in the IES treatment, and with probability between 0 and 1 in the PES treatment (mixed strategy equilibrium). Allocation to the institutional treatment to experimental session is random. We rely on the initial and final one-shot non-contractible and non-enforceable investment games to measure trust and trustworthiness as moral norms, separate from the cooperative norms occurring in the market game. We do so in order to avoid the confounding effect of reputational concerns in repeated interactions or that of institutional incentives, which can influence the cost of cooperation. Playing a trust game before and after our exchange game is a key feature of our design and is motivated by our desire to study how pre-existing norms, or "culture", affect behavior under different institutions, and how different institutions foster different dynamics in the evolution of trust and trustworthiness. Relying on within subject variation in trust also reinforces the validity of our causal estimates and overcomes any possible deviation from randomization in the allocation to treatment.

The experiments were conducted in the field with 169 subjects in Italy (both in the North and in the South) and 178 in Kosovo during the summer of 2011. Our results indicate that better formal enforcement (impartial adjudication of tort), has a positive effect on informal norms

of cooperation: trust and trustworthiness are, respectively, between 12% and 18% and between 20% and 31% higher under the impartial institutions treatment compared with the partial institutions treatment. Consistent with our design, the impartial treatment reduces the frequency of non-cooperative behavior, namely cheating in markets. In turn, Wald estimates indicate that a reduction by 1 percentage point in the frequency of facing a non-cooperative partner (a cheater) in the trading game leads to a 7-11% increase in trust and a 13-19% increase in trustworthiness. Within Italy, the effect of impartial vs. partial institutions on trustworthiness is equivalent to three-fourths of the difference between Milan and Palermo. In Kosovo, it is about three-fourths of the difference between Pristina, the capital city, and Mitrovica, the scene of major tensions during the 1999 civil war. The effect is particularly robust in Kosovo and holds even in a first difference specification, which measures the variation in trust and trustworthiness within subjects, across treatments.

Pre-existing trust and trustworthiness are associated with less cheating and, more generally, cultural differences captured by participants' regional origins, explain opportunistic behavior; but only for those that did not experience impartial institution in the market game. This suggests that trust may act as a substitute for formal institutions in supporting exchange, but only in the absence of strong formal institutions. Impartial formal institutions produce more cooperative behavior independently on pre-existing moral norms and culture.

This paper makes two contributions. First, it adds to the literature on the origin of trust. Theoretical models see legal enforcement as having either a positive or negative effect on trust (Guiso et al. 2008a; Tabellini 2008; Aghion et al. 2010). Empirically, recent papers have pointed to a positive relationship between institutional quality and trust, based on evidence that societal trust is higher today in regions that experienced good quality institutions in the past (Guiso et al. 2008b; Tabellini 2010; Grosjean 2011a). A possible limitation to causal

identification in these studies is that good historical institutions were themselves the outcome of high societal trust, and both have persisted until today. By randomly allocating our subjects to different institutional environments, we are able to identify a positive causal effect of institutions. Our findings suggest that trust and trustworthiness can result as a by-product of higher formal institutional quality.

Second, we contribute to the literature on the role of culture and its interaction with formal institutions in determining opportunistic behavior in markets and market efficiency. By running our experiment in regions with different levels of trust, we can observe how behavior under each exogenously imposed institution varies across cultures. Running experiments in the field and the selection of our experimental sites are both driven by a desire to capture substantial cultural differences and thereby enhance external validity of our findings. We ran sessions in the North of Italy, characterized by good formal institutions and high trust; Sicily, characterized in theory by the same formal institutions but in practice all too familiar with partial, closed network contract enforcement institutions and low trust; and Kosovo, characterized by weak formal institutions and relatively high trust.

The paper is organized as follows. Section 2 presents our main hypothesis in light of the empirical and theoretical literature on the co-evolution of social norms and institutions and their influence on economic exchange. Section 3 describes the experiment. Section 4 presents the data and descriptive statistics. Section 5 analyzes the effect of legal institutions on trust and trustworthiness. Section 6 addresses the role of pre-existing norms on market behavior and efficiency and their interaction with institutional quality. Section 7 concludes. Appendix A presents the theoretical solution to the trading game. Appendix B includes additional results and descriptive statistics. Appendix C contains the experimental instructions.

2. Background and Hypotheses

A fundamental proposition in economics is that markets achieve 100% efficiency, that is the maximization of possible benefits from trade for buyers and sellers. This, however, is based on the hypothesis of frictionless markets. On the contrary, actual markets face many trading frictions since contracts are not always perfectly or costlessly enforceable. In this case, the fear of dealing with a cheating partner might drive market opportunities and surplus down. Such "cheating frictions" present formidable obstacles not just in places where formal contract enforcement institutions are weak, as in markets of the ancient and medieval world (Greif 1993) and in many developing economies (Fafchamps 2004, 2006) but also in economically advanced countries with good enforcement institutions (Williamson and Kerekes 2011), since it is rarely possible to specify by contract all dimensions of an economic transaction.

Interpersonal networks based on kinship and reputation have been recognized as playing an important role in enforcing trade and promoting cooperation (Fafchamps and Minten 2001; Greif 2006).[iv] However, the very interpersonal nature of these institutions limits the scope of exchange and may reduce efficiency by diverting trade to better connected but less efficient traders (Fafchamps 2002, 2004, Kuran and Lustig 2012). Impartial institutions and the rule of law are deemed to become necessary to sustain large-scale impersonal trade (North 1991; Dixit 2004). Nevertheless, the observation that trade can flourish when contracts are not enforceable, either due to their incomplete nature or to the absence of institutions, has revived interest in the positive role of social norms and of trust and trustworthiness, in particular (Fafchamps 2006). Although there is a clear consensus in the literature that both good quality institutions and high trust promote trade, cooperation, and development,[v] the question of how institutions and social norms interact and co-evolve is much less clear.

The literature offers many definitions for trust, depending on the specific context and content of the study. Here we follow Gambetta (2000) and define trust as "the subjective probability with which an agent assesses that another agent or group of agents will perform a particular action". In our experimental context, trust is the expectation that another subject will return at least as much as he was given or more, sharing some of the gains. So when discussing trust and trustworthiness as moral norms, we refer to these non-contractible expectations and behaviors, distinguished from the ones that occur in contractible market environments. The literature has tried to distinguish among the two. In the theoretical model of Guiso et al. (2008a), trust is based on culturally transmitted beliefs about others' trustworthiness and on real experiences of cooperation. Societies can be trapped in an "equilibrium of mistrust" if the net benefits from cooperation are not sufficiently high to induce people to experience cooperation and update the low priors they may hold on others' trustworthiness. Institutions play a role by determining the net benefits from cooperation. Shocks to the quality of institutions, if capable of inducing significant increases in cooperation, may shift societies to a cooperative equilibrium, even when the shock is temporary.

Tabellini (2008) considers a model in which culturally transmitted values enhance the probability of cooperation. This model distinguishes between localized trust, which is based on interpersonal relationships, and generalized trust, which can sustain exchange with anonymous others. Only improvements in impartial enforcement are capable of crowding in generalized trust, while improvements in local enforcement have an opposite effect by reducing the relative return from trading with anonymous versus local partners. This suggests a complementarity between impartial contracting institutions and societal norms of generalized trust and trustworthiness.

The special role of impartial enforcement institutions has also been highlighted in the political science and sociology literature. Among the first, Rothstein and Stolle (2008) finds that the specific institutions that explain variation in societal trust across countries are the supposedly impartial enforcement institutions, such as the legal system and the police, rather than the more partisan political and representational institutions. Among sociological works, Hruschka (2010) shows that adherence to impartial norms of conduct is correlated with the quality and impartiality of legal enforcement institutions.[vi]

By contrast, the negative relationship in Tabellini (2008) between local, as opposed to impartial, enforcement and generalized trust is reminiscent of a possible negative effect of legal enforcement on trust, which has been discussed elsewhere in the literature. Crowding out may occur because better external enforcement weakens reputational incentives (McMillan and Woodruff 2000) and decreases the returns to being trustworthy (Bohnet, Frey and Huck 2001; Jackson 2011). Under perfect (or close enough) contract enforcement, behavior is entirely dictated by the perspective of monetary punishment, so that there is no return to honesty and trust may be crowded out.

To sum up, the theoretical literature discusses two countervailing effects of enforcement institutions on social norms, in which trust is either crowded in or out by better legal enforcement. Empirically, a number of papers find evidence that good quality historical institutions have a long lasting positive effect on trust (Guiso et al. 2008b; Tabellini 2010; Grosjean 2011a). However, a possible limitation to causal identification in these studies is that good historical institutions were themselves the outcome of high societal trust, which has persisted until today. Another limitation emanates from recent evidence that good historical institutions can persist at a very local level. Identification in Guiso et al. (2008b), Tabellini (2010) and Grosjean (2011a) is based on the assumption that formal institutions are constant

in a given country, so that variations in trust can be attributed to culturally transmitted social norms and not to contemporaneous institutional quality. However, recent evidence by Becker, Hainz and Woessman (2011) shows that there is less corruption in local courts and police in regions of a given country that were part of the Habsburg Empire. If both historical and contemporaneous local institutions are different, observed trust may not necessarily be reflective of cultural norms inherited from historical institutions but, rather, justified by higher institutional quality today.

Beyond exploring the effect of institutions on trust, we are also interested in how pre-existing trust, or more generally culture, affects the functioning of institutions. Fisman and Miguel (2007), in a study on parking violations committed by diplomats stationed in New York, finds that cultural origins matter in determining behavior in the absence of formal enforcement, but such an effect disappears very rapidly once enforcement is imposed. With immunity, diplomats from countries with high corruption committed more parking infractions than those from less corrupt countries, but infractions were reduced dramatically once immunity was removed.[vii]

In short, theoretical works and empirical evidence suggest two testable hypotheses that will be addressed in this paper. First, impartial institutions in a market environment have a positive effect on non-market moral norms such as trust and trustworthiness. Second, pre-existing culture may be important at intermediate levels of institutional development, but it ceases to play any role in cooperative behavior in markets once good impartial institutions are in place.

A challenge to laboratory studies on the evolution of trust consists in the velocity with which social norms change. The theoretical models reviewed above conceptualize trust as an inherited cultural variable that exhibits remarkable persistence over time. The implication that

trust is slow to change has been supported by several empirical studies (Durante 2011; Grosjean 2011a; Nunn and Wantchekon 2011). However, in certain contexts, rapid changes in trust and norms of good conduct are shown to occur. For example, variations in trust are observed as migrants adapt to their new environment (Algan and Cahuc 2010) or after experiencing violence during a civil war (Cassar, Grosjean and Whitt 2012). Cialdini, Reno and Kallgren (1990) shows that an exogenous manipulation in perceived social norms about littering has an immediate effect on littering behavior. The "broken window theory" in sociology is based precisely on the idea, supported by empirical evidence (Holden 2008; Keizer, Lindenberg and Steg 2008), that pro (or anti)-social behavior can easily be triggered by small, local changes in disorder.

3. Experimental Design

Each experimental session was comprised of four parts followed by a survey: an initial trust game (a.k.a. investment game) in which subjects played both the part of the trustor and the trustee (Part 1); 10 rounds ("days") of trading in the market game under no institutions (Part 2); 10 rounds of trading under either partial enforcement system (PES) or impartial enforcement system (IES) (Part 4); a final trust game (Part 3), for a total of 24 decisions per subject.

3.1. Trust Game

To measure initial and final levels of trust and trustworthiness as moral social norms we use a modification of the standard protocol of the investment game of Berg, McCabe, and Dickhaut (1995). In this game subjects have the ability to "invest" by sending money to an anonymous experimental partner. The amount of money sent is then multiplied by three

before reaching the partner. It is then the partner's turn to decide how much of the received amount to return to the original investor. By considering the amounts that subjects invest and then return, we can determine to what extent subjects trust others and how trustworthy they are. In our version, subjects played both the role of Sender and that of Receiver. We used the strategy method (for which Receivers have to decide how much to send back to the Sender under all possible amount that they could have received) to prevent players from knowing anything about trust and reciprocity of the fellow subjects, so as to limit the dependency between the specific trust experienced in the first game and the following exchange games. Senders could choose to invest any amount between 0 and 10 Euro while Receivers had to decide how much they would send back for each possible amount that they could receive, ranging from a minimum of 0 to a maximum of 30 Euro. The amount sent by Sender (X) is considered a signal of trust because larger amounts X sent translate into larger pies that Receiver has to divide. By sending higher amounts, Sender's best possible payoff from the game increases, but at the same time her worst-case payoff from the game decreases, relative to the scenario where she sends nothing at all. The amount sent back by Receiver is considered as a measure of trustworthiness or reciprocity. If one of these four decisions (as Sender of Receiver in either Part 1 or Part 4) was randomly drawn to be the one to be paid, the experimenter randomly matched subjects into pairs and computed their profits depending on the actual partners' choices.

3.2. Market Game

The central part of the experiment consists of playing in a market game under different institutional treatments: first 10 rounds of trading under no institutions (NoES), then 10

rounds of either partial (PES) or impartial institutions (IES) depending on the randomly selected treatment for that session.

No Institutions (NoES). The basic framework consists of a trading game in which 8-10 players decide whether or not to cheat an anonymous counterparty, or not to trade at all, for each one of 10 days for which trading partners change each day. In practice, cheating in markets happens when, for example, a buyer doesn't pay, a check bounces, or a seller deliver a lower quality or defective good. In the experiment players trade an abstract good, so we do not go into details and we simply ask them to either cheat, not cheat or stay out of trade. Matrix 1 displays the parameters chosen for the baseline game:

	No cheat	Cheat	Out
No Cheat	20, 20	0, 30	1, 1
Cheat	30, 0	10, 10	1, 1
Out	1, 1	1,1	1, 1

Matrix 1. NoES Payoff matrix, Cheating - No Institutions.

This treatment of the market game reproduces the features of a prisoner-dilemma game. Each individual has a private incentive to cheat. However, if everyone follows the same rationale, the exchange generates lower social welfare. Maximum social welfare and efficiency (40 total surplus, equally split between traders) are reachable only when both parties do not cheat. Given our payoffs, we find 2 equilibria: (Cheat, Cheat) and (Out, Out) which is payoff dominated by the first one. As long as the payoff from trading and cheating is higher than the payoff from opting out, we expect everyone to participate in equilibrium and Cheat. In this case the equilibrium quantity would be 1 per couple of players and the total surplus

would be 10 per player. Such an outcome is in stark contrast with the equilibrium that would be obtained under perfect and costless enforcement: as long as the payoff from trading is higher than the payoff from opting out, under perfect enforcement everyone would trade in equilibrium, with an equilibrium quantity of 1 per couple of players, and a total surplus of 20 per player per day.[viii]

Between the two benchmarks of perfectly running institutions or a complete lack of an enforcing system, we can investigate the effects of different institutions. An experiment is not expected to reflect all aspects of the real world, but just what one thinks are the most important features for understanding the issue of interest. In our case, we cannot model all the dimensions of a contract enforcement institution, but we want to generate substantial variation in deviations from cooperation while focusing on one aspect in particular that has been the focus of an important literature (see Section 2): partial vs. impartial administration of justice.

Partial Enforcement System (PES). In this treatment, subjects can ensure themselves against being cheated on by buying "protection" against a cheating counterpart. Purchasing protection costs 5, which has to be paid regardless of whether such protection is used or not later on. If a player buys protection and is cheated, the cheater not only loses all she has gained by cheating, but also gets punished. This payoff scheme is designed to mimic what happens when a partial, closed network institution, such as the mafia, is in charge of enforcing contracts. Typically in these settings, individuals who are determined to participate in economic activities may be induced to pay for protection regardless of whether they will require the services of the local boss or not, and in return are ensured against the claims, rightful or not, of competitors and commercial partners. There is always the incentive,

though, not to pay the "protection fee", or to cheat hoping that the partner is not protected. This is reflected in the payoff matrix of the game (see Appendix A).

Every trading day, subjects have to decide whether or not they want to buy protection and whether they want to trade honestly, cheat or stay out, before knowing the choice of their trading partner for that day. During instructions, we explained to the subjects each possible decision, presenting all the following four possible scenarios (in addition to the staying out option): neither subject has protection, only the subject has protection, only the partner has protection, both have protection. When neither side purchases protection, the payoff structure is the same as in NoES. When both parties buy protection, the final result depends on whether none, one or both cheated. Traders who don't cheat earn 15 (20 from honest exchange, minus the 5 payment to purchase protection). If both traders cheat, then the "protection agency" makes sure that exchange does follow through and imposes an additional cost of 3 as punishment for cheating; therefore, both traders end up with a payoff of 12 (20 of a honest exchange, minus the 5 payment to purchase protection, minus 3 punishment for cheating). When both parties have protection and one cheats while the other doesn't, the one that doesn't can get the contract enforced anyway, so she still earns 15, while the cheating party, as before, gets 12. Last, the case in which only one trader buys protection. The trader that buys protection and doesn't cheat gets 15 no matter what the partner does (20 of a honest exchange, minus the 5 payment to purchase protection). The partner receives 20 if she doesn't cheat or -3 if caught cheating. If the trader cheats, she earns 25 (30 from cheating, minus 5 to purchase protection) no matter what the partner does, since the "protection agency" will protect her no matter what. The non-protected trader will instead earn 0 if he doesn't cheat or -3 if cheats. As in the previous cases, staying out of the market yields a profit

of 1. A payoff matrix and a full description of the solution to this game are provided in Appendix A.

The only pure strategies equilibrium of this game is for both players to stay out. However, the game has many equilibria in mixed strategies in which players can randomize between the different strategies, with the exception of [buying protection, stay out], which is a purely dominated strategy. This outcome is consistent with our desire to generate an equilibrium in which the probability of cheating is between 0 and 1. Also, in the presence of multiple equilibria, individual beliefs on the probability of being cheated will determine the specific strategy played by each subject. Therefore, we expect pre-existing levels of trust to influence the outcomes of the game in the field.

Impartial Enforcement System (IES). For this treatment, we model an impartial judicial system as an institution in which each subject has the option of taking a cheating partner to court. The court then enforces order: whoever cheats has to pay full price plus a fine and whoever is cheated receives full amount minus a court fee. This treatment aims at reproducing the trade-offs faced by citizens when deciding to use an impartial justice system: going to court is an option open to everyone, but still voluntary; it is moderately costly but, when used, it restores the outcomes of honest market exchanges.

Similarly to the PES treatment, subjects have to decide at the beginning of each trading day whether they want to have the option of taking a cheating partner to court or not and whether they want to trade honestly, cheat or stay out, before knowing the choice of their trading partner. Selecting this option is free. A small fee is required only when someone actually takes a cheater to court. We elicit this decision before the behavior of the counterparty is revealed for simplicity, much like in the strategy method. Pairs where neither side wants to take the counterpart to court face the same payoffs as in the no institution case

(NoES). On the contrary, when a subject decides to take a cheating trading partner to court, the court forces the cheating party to trade honestly and pay a fine of 5. Going to court costs 2, which are deduced from subjects' profit for the day only when courts are involved in solving the dispute. When neither party cheats, each trader still receives a payoff of 20. In case a trader that has been cheated has selected to go to court, she earns 18 if she didn't cheat (the honest exchange payoff of 20, minus 2 for taking the counterpart to court) or 13 if she also cheated (the honest exchange payoff of 20, minus 2 for taking the counterpart to court, minus a fine of 5 for having cheated as well). Lastly, when a subject decides not to go to court while her partner does, her payoff is still 20 if nobody cheated, 13 if she cheated or 0 if her partner cheated. Staying out of the market, either by opting out or by being matched with a subject that opted out, still yields 1. A full payoff matrix for this game is provided in Appendix A.

In this treatment, going to court and trading honestly for both partners is a Nash-Equilibrium in pure strategies. It is, however, not unique. The case where both players stay out is also a Nash equilibrium, payoff dominated by the first one. As in the NoES case, we expect individuals to play the payoff-dominant Nash equilibrium.

There are two crucial differences between the IES and the PES treatments. The first is impartiality of enforcement. The court system in the IES treatment is impartial: all the cheaters are punished in the court system, even if they themselves initiated the process by taking a cheater to court. Under PES, a cheater that has private protection can cheat and not only get away with it but also get her partner to pay without herself having to pay, if the partner does not have protection. The second difference between our institutional treatments lies in the level of cooperation that they induce. Subjects are expected to cheat with probability 1 in equilibrium under the NoES treatment, with probability 0 under the IES

treatment, and with probability between 0 and 1 under the PES treatment. The exogenous variation in the probability of cheating introduced within the experiment is crucial for our analysis of treatment effects on trust and trustworthiness.

3.3. Experimental Protocol

The games were played with paper and pencil to be able to reach our targeted subject pool in the field. Each point was equivalent to 1 Euro in both sites[ix]. After the experimenters read each part of the instructions aloud and explained the various possible scenarios, the subjects had to go through a set of comprehension questions before playing the actual games. Subjects were randomly and anonymously re-matched for each of the 24 decisions they had to take.[x] It was stressed during instructions that each choice subjects had to make had the same probability of being selected for payment. On average, each session lasted about 2 hours.

Subjects were not given any information on the nature nor on the sequence of the tasks beforehand. They knew the total number of tasks, but no details were given until the instructions for the corresponding stage of the experiment were handed out. Trust game results in Part 1 were not revealed to the players, unless that first activity was the one actually selected for payment at the very end of the session. The fact that participants knew the total number of activities in the experiment implies that they were aware, at the time of playing the second trust game, that that was the final task of the session. This end-game feature stacks the deck against us finding a significant difference in the change of trust or trustworthiness following the different treatments. The fact that we still find significant differences means that our results are lower bounds.

Each session was randomly assigned only one of the two treatments (either IES or PES) in addition to the NoES treatment administered to everyone. In the market games, at the

beginning of each round, participants were given a sheet of paper, featuring one line for each of the ten trading days. Each line was divided in two parts: on the left side, subjects had to mark their choices (by checking the corresponding boxes) concerning eventual use of the court system or of the protection and their trading strategy; on the right side, similar boxes were used by the experimenter to report, at the end of each day, the decisions of the trading partner and the resulting profit. Partners were randomly and anonymously re-matched each day by the experimenter who also computed the profits on the basis of the relevant payoff matrix. Subjects were constantly reminded that, were one of these trading days be the one selected for payment, they would gain the profits they made for that day.

When all decision sheets were collected, the experimenter asked one of the subjects to draw a number from a hat. The numbers ranged from 1 to 24, equal to the total number of decisions made during the experiment. The number determined the decision to be implemented. While the assistants computed the payments, participants filled out a survey. The survey featured basic demographic and socio-economic questions, as well as questions on beliefs and behaviors related to the social preferences and behaviors elicited through the experiment. The survey included questions on trust in institutions and people, on experiences of economic exchanges, on borrowing, help-seeking in different situations, and on exposure to illicit activities.

4. Data and Descriptive Statistics

4.1. Sample Size and Selection of Subjects

We ran 37 experimental sessions: 19 in Italy (169 subjects) and 18 in Kosovo (178 subjects). The average number of participants in each session is 9.56 (min. 7, max 12).[xi] The majority of sessions has either 10 (58% of sessions) or 8 participants (25% of sessions).

In Italy, subjects were recruited through the help of producers' and workers' associations in 3 different regions, Lombardy, Liguria and Sicily. Each association sent to its members the invitation to participate in an economic study, specifying its duration and the range of possible gains. When enough people had volunteered, the time and place for the session was agreed upon. Sessions usually took place in the offices of the association. This choice of recruitment system answered two basic needs. The first was a need to overcome the logistical challenges of recruiting people for 2 hours sessions in the middle of the summer: associations had the network and capacity to bring together enough members to allow us to conduct our sessions. Second, one of the objectives of this study is to assess how the preferences and behavior we observe within the experiment generalize to economically relevant choices in the real world: workers and producers associations gave us access to a sample of business owners and employees from different sectors, who regularly have to make decisions in their jobs similar to the ones they faced in experiment.

In Kosovo, participants were recruited at random through paper invitations. Invitations were dropped off at every 5th doorway of both rural and urban areas of 10 different locations.

Both the survey instrument and experimental instructions were translated into local languages using the double translation procedure to ensure consistency across sites. Nevertheless, to account for the fact that recruitment procedures differed between the two countries, all specifications will include country fixed effects in the regression analysis.

4.2. Descriptive Statistics

Summary statistics on the socio-demographic and economic background of our subject pool are presented in Table 1. The objective of collecting such information is to investigate potential heterogeneous effects of experimental treatments but also to check the validity of

the randomized allocation procedure to the different experimental treatments. Apart from a higher proportion of students in the IES treatment (27% versus 14% in the PES treatment, t-stat of 3.1) and a larger average household size in the PES treatments (4.7 versus 4.2 in the IES treatment, t-stat of 1.9), covariates are well balanced across the experimental treatments.[xii] Despite randomizing the assignment of treatments to sessions, initial trust and trustworthiness turned out to be significantly higher in the IES than in the PES treatment. Trust is measured as the amount sent and trustworthiness as the average amount returned as percentage of the amount sent, averaged over all the possible amounts sent (elicited with the strategy method). Subjects assigned to the PES and IES treatments sent on average 5.2 and 5.9 Euro (t-stat of 2.76), and returned on average 48% and 58% (t-stat of 4.56) respectively. Such differences are entirely driven by Kosovo (t-stats of 3.07 and 0.82 in Kosovo and Italy respectively). They certainly represent a concern for our identification strategy, which we address by controlling for initial trust and trustworthiness in all regressions shown below, and by presenting results in first differences.

Initial trust and trustworthiness differ across countries. Subjects in Kosovo and Italy sent on average 5.6 and 5.4 Euro and returned on average 55% and 50%, respectively. These differences are statistically significant.[xiii] Our small sample size makes it impossible to detect significant within-country differences. In particular, subjects from Southern Italy appear less trusting and more trustworthy than those from the North, but these differences are not statistically significant. For this reason, in what follows we show results disaggregated by country only.

Descriptive statistics of the outcome variables are presented in Figures 2 to 6. Panel (a) in Figure 2 displays the average amount sent, that is trust in the final trust game after each of the two different institutional treatments for the whole sample and for each country separately.

Figure 3 shows similar results for trustworthiness: on average, participants sent 6.5 and 5.4 Euro, and returned 60% and 45% of the amount received respectively in the games following the IES and PES treatments. The amounts sent and percentages returned in the second trust game are higher following the impartial enforcement system treatment compared with the partial enforcement one (t-stat of 3.87 and 5.31, respectively). The differences are particularly large in Kosovo. Panel (b) in Figure 2 presents the average individual increase in amounts sent between the two trust games, before and after the experimental institutional treatment. Similarly, Panel (b) in Figure 3 presents the average individual difference in the percentage returned between the two trust games. Taking first differences within individuals gets rid of any individual heterogeneity and of any departure from perfect randomization across treatments. Trust increases after both treatments but much more so in the IES treatment: the average difference in amount sent is 0.6 and 0.2 Euro after the IES and PES treatments respectively. According to a simple t-test, the difference is statistically significant (t-stat of 1.49). For trustworthiness, partial enforcement institutions actually lead to a decrease in average percentage returned of 3.8 percentage points, whereas impartial institutions lead to an increase of 2.2 percentage points, a difference that is statistically significant according to a simple t-test (t-stat of 2.39). As a result, trustworthiness is higher under the impartial enforcement treatment compared with the partial enforcement treatment, and the difference is statistically significant overall, in Italy and in Kosovo (t-stats of 5.31, 2.75 and 5.14, respectively).

The remaining figures display the measures of individual behavior and market efficiency in the market game: cheating behavior (Figure 4), market participation decisions (Figure 5), and traders' total individual profits (Figure 6) under the three different situations: no contract enforcement institutions (NoES), partial contract enforcement institutions (PES) and

impartial contract enforcement institutions (IES). Figures B1 to B3 in Appendix B display the evolution of cheating, opting out and trading profits throughout the game under the three institutional set-ups. Market participation and profits are highest and cheating is lowest under the IES treatment. On average, participants opt out of trade 1 trading day per round in the NoES treatment, 0.6 trading days in the PES treatment, and 0.4 trading days in the IES treatment. They cheat 3.61 times out of the 10 rounds in the absence of institutions, 3.65 times under partial institutions but only 2.20 under the impartial ones. Total profits over the 10 rounds are on average 131, 121 and 167 Euro in the NoES, PES and IES treatments respectively. A direct comparison of profit across treatments warrants caution: in the PES subjects have to pre-pay the 5 fee for ensuring private protection and this further reduces profits. The quality of contract enforcement institutions seems to have a non-monotonic effect on cheating behavior and on market efficiency. In Kosovo, cheating is actually higher under PES than under NoES (4.3 and 3.6 times over the 10 rounds respectively). As a result, total surplus is not higher under PES than under NoES: total profits are on average 123 and 138 Euro in PES and NoES, respectively. On the contrary, in Italy cheating is lower under PES than under NoES (2.97 and 3.62 over the 10 rounds respectively), yet profits remain lower. The next section turns to regression analysis to test the statistical significance and robustness of these results.

5. The Causal Effect of Institutions on Trust

We turn now to testing through regression analysis our first hypothesis: impartial contract enforcement institutions in markets lead to higher trust and trustworthiness as moral norms (in non contractible environments), compared with partial institutions, through their effect on cooperative behavior in markets. Descriptive evidence in the previous section indicates that

our institutional treatments were successful in generating the predicted changes. In this section, we first show the effect of our institutional treatments on trust and trustworthiness in a reduced form regression framework. Second, we quantify the effect of a reduction in the frequency of non-cooperation, that is to say cheating in markets, on trust and trustworthiness with a Wald estimate.

5.1. Empirical Specification

Since allocation to treatment is random, the causal effect of the institutional treatment on trust and trustworthiness is obtained by comparing, across treatment groups, the average amounts sent and returned, respectively, in the second trust game. We control for country fixed-effects in order to take into account any difference in the implementation of the experiment in the different countries. All regressions also control for behavior in the first trust game, in order to control for differences in initial trust and trustworthiness. For robustness, we present additional specifications to show that our results are robust to the inclusion of additional individual controls. We estimate the following regression:

$$T_{2i} = \alpha + \beta D + \gamma T_{1i} + \vartheta C + \delta X_i + \varepsilon_i \quad (1)$$

where T_{2i} and T_{1i} denote the behavior (either trust or trustworthiness) of individual *i* in the second and first trust game, respectively. *D* is a dummy variable capturing the institutional treatment and taking value 1 for the impartial contract enforcement institutional treatment (IES) and 0 for the partial contract enforcement institutional treatment (PES). C is a country dummy. X_i is a vector of individual controls, such as age, gender, marital status, education, individual income, employment status and an individual estimate of risk aversion measured by a survey question about a lottery choice between a safe and a risky option. We present results with and without this set of individual controls.

For robustness, we also estimate the model in first differences. The first difference model estimates the variation of trust and trustworthiness *within* individuals as a function of the experimental treatment. For this specification, we estimate the following model:

$$T_{2i} - T_{1i} = \alpha + \beta D + \varepsilon_i \quad (2)$$

β is interpreted as the causal effect of the treatment: it estimates the differential variation within subjects, across treatments, in trust and trustworthiness levels between period 1, before the treatment is administered, and period 2, after the treatment is administered.[xiv]

Throughout our tables of results, in the regressions using the full sample we report robust standard errors as well as robust standard errors clustered at the session level to take into account any potential correlation among individual errors of participants in the same session (37 clusters). Regressions ran on individual country or on treatment sub-samples use robust standard errors, given the lower number of clusters.

5.2. Reduced Form Results

Regressions' results are presented in Table 2. Columns 1 and 2 display results for the pooled sample when the dependent variable is the amount sent by the first player in the final trust game, that is, our measure of trust. Columns 6 and 7 display regression results for the percentage returned by the second player (averaged over all the possible amounts received elicited via the strategy method) in the final trust game, that is, our measure of trustworthiness. Columns 4, 5 and 9, 10 report the results of similar specifications using the country subsamples data for the same dependent variables. For each measure and in each sample, results are presented without and with individual controls.

As anticipated by the uncontrolled tests on means, impartial enforcement institutions (IES) have a positive, statistically significant and robust effect on both the amount sent and the

percentage returned in the final trust game, compared to partial enforcement institutions (PES). These effects are robust to the inclusion of additional controls for individual characteristics[xv] and are also robust within Italy and within Kosovo. The effects both on trust and trustworthiness are robust and significant at the 1% level in Kosovo and at the 10% level in Italy. For each behavior in the second trust game: trust or trustworthiness specifications displayed here control only for the corresponding behavior in the first trust game. However, all results are robust to controlling both for trust and trustworthiness behavior in the first trust game. Results are not only robust but also statistically and economically more significant when country fixed effects are not included.[xvi]

The effect of contract enforcement institutions on trust and trustworthiness is economically meaningful. Having traded under impartial contract enforcement institutions as opposed to partial enforcement institutions leads to 12% to 18% higher amounts sent and 20% to 31% higher percentages returned, depending on whether we control for individual characteristics and for behavior in the initial trust game. The effect of institutions far outweighs that of any individual characteristics, including the regional origin of our subject pool, as captured by our country dummy. Controlling for individual characteristics, the coefficient on the institutional treatment is 1.6 times higher than the coefficient on the country dummy for Kosovo for trust and 2.1 for trustworthiness. Within Italy, the effect of impartial vs. partial institutions on trustworthiness is equivalent to three fourths of the initial trust difference between Milan and Palermo, in Sicily. In Kosovo, it is about three fourths of the difference between Pristina, the capital city, and Mitrovica, the scene of major tensions during the 1999 civil war.

Columns 3 and 8 present the results of first-difference specifications (2) for trust and trustworthiness, respectively. The coefficient associated with the impartial enforcement treatment is still positive. It is only marginally significant for within-subject differences in

amount sent, but its significance reaches the 5% level for within-subject differences in percentage returned. Results for individual countries subsamples are similar to the ones discussed so far. Impartial institutions lead to positive and statistically significant individual increases in trust and trustworthiness in the Kosovo sub-sample and in trustworthiness only in the Italy subsample.[xvii]

Such a rapid change in trust observed after exposure to different institutions in an experimental setting is intriguing in light of the literature on the slow changing nature of culture which we reviewed in Section 2, and is more in line with the results of Cialdini et al. (1990). Here, we obtain our results in the specific context of a very small economy comprised of only 8-10 players, each one expected to meet all the other players at least once throughout the market game. Such opportunities for trade enable individuals to update priors about others in a rapid way. Other studies have demonstrated how trade (Maystre et al. 2009) and exchange of information, measured by access to phones or television (Fisman and Khanna, 1999; Head and Mayer 2008) accelerate cultural change.

5.3. Quantifying the Effect of Non-cooperation

Beyond the gross effect of our treatment, we are interested in the effect of experiences of non-cooperation (that is, cheating) in markets on trust and trustworthiness. This is indeed the main channel through which institutions are expected to affect moral norms of cooperation in theoretical models à la Guiso et al. (2008a) and Tabellini (2008). We first compute the subjective probability that any other trader in the game is a non-cooperator based on the individual frequency of having met a cheating partner in the trading game relative to the number of participants in the session.[xviii] We then present a Wald estimate. In the first stage, the frequency of cheating in the trading game is regressed on the institutional treatment.

Results are displayed in Table 3. Consistent with our experimental design and with the descriptive evidence provided above, the IES treatment reduces the frequency of cheating by 45% on average (significant at the 1% level). Wald estimates of the effect of a reduction in the frequency of non-cooperation in the trading game on trust and trustworthiness are presented in Table 4. On average, a reduction by 1 percentage point in the probability of non-cooperation in the trading game increases amounts sent in the trust game by between 7% and 11%, depending on the specification. The corresponding increases in trustworthiness are between 13% and 19%. Again, all effects reported here are robust to controlling for trust and trustworthiness behavior in the first trust game at the same time; and are robust and statistically more significant when omitting all controls including country fixed effects.

These estimates can only be interpreted as instrumental variable estimates if the reduction in non-cooperation is the only mechanism through which the treatment affects trust and trustworthiness. Alternative mechanisms may work through the effect of treatment on the frequency of exchange, on profits, and on subjects' own behavior. Variations in the volume of trade are not consistently associated with variations in trust or trustworthiness. Similarly, the higher level of profits accruing to subjects under impartial enforcement does not significantly correlate with final levels of trust and trustworthiness. [xix] Another possibility is that players' own behavior in the market game explains behavior in the final trust game. Players' own cooperative behavior is significantly affected by the treatment and is positively associated with trust in the second trust game, but not with trustworthiness. This is entirely consistent with our proposed explanation that institutions influence trust through their effect of non-cooperation in the market game, but is subject to the caveat that player's own behavior in the market game is endogenous to her behavior in the trust game. Since we estimate within subject variation in trust and trustworthiness, we can rule out that the

observed effect is due to any individual idiosyncrasy, or due to individual behavior in the first trust game that would influence behavior in the second trust game. However, we cannot entirely rule out that other channels, for example framing of the experiment, are at play.

6. The Interaction Between Culture and Enforcement Institutions

6.1. Empirical Specification

The second hypothesis we wish to empirically test deals with the relationship between pre-existing culture, particularly initial trust and trustworthiness, and behaviors in market under the different enforcement institutions. We therefore estimate the following relationship:

$$MBehav_{it} = \alpha + \beta D + \delta T_{i1} + \vartheta C + \varphi X_i + \gamma_i + \gamma_t + \varepsilon_{it} \quad (3)$$

where $MBehav_{it}$ captures individual market behavioral outcomes for individual *i* (cheating, participation, as well as her trading profit) on day *t=[1,10]* of trading. *D={NoES, PES, IES}* is the experimental treatment. *C* denotes country fixed effects. T_{i1} captures the behavior of agent *i* in the first trust game. γ_i is an individual effect and γ_t is a vector of dummy variables for each trading day (time fixed effect). ε_{it} is the error term. We estimate this model in the pooled sample as well as in the different treatments sub-samples in order to test whether trust or trustworthiness has a differential effect under different contract enforcement institutions. Because the first trust game is played before the trading game, even before the trading game instructions are administered, we can use behavioral estimates of trust from the first trust game as measures of pre-existing culture without worrying about the reverse causal effect of trading behavior on trust. Nevertheless, we suspect the presence of an omitted variable bias due to unobservable individual characteristics that could influence both behavior in the trust game and behavior in the market game. In an attempt to control for such bias, we control for individual characteristics such as age, gender, education, income, employment status, and risk

aversion in X_i. δ should still only be interpreted as indicative of a correlation between trust and market behavior in the market game.

6.2. Results

Results of the regressions investigating the role of pre-existing culture, namely initial trust and trustworthiness, on market behavior and market efficiency are displayed in Panels a and b of Table 5. All models are estimated with random effects.[xx] Columns 1, 5 and 9 in each Panel display the results for the pooled treatments when the dependent variables are, respectively, the number of times the individual cheats, stays out, and his or her total profit. Cheating behavior is strongly curtailed in the impartial enforcement institution treatment, while participation is increased. Cheating is lower and participation higher in the PES treatment as well, compared to the baseline of no institutions. In the pooled regressions, initial trust and trustworthiness are negatively associated with cheating (significant at the 1 % level), but not with participation decisions or profit. However, investigating the interplay between initial trust and trustworthiness and the different institutional treatments leads to a more contrasted picture.

Regressions displayed in columns 2 to 4, 6 to 8 and 10 to 12 estimate the effect of initial trust (Panel a) or initial trustworthiness (Panel b) on each dependent variable within each of the three institutional treatments. Trust and trustworthiness deter cheating, but only in the absence of impartial institutions. Under impartial institutions, neither trust nor trustworthiness has any influence on cheating behavior (Column 4 in both panels). The interpretation is that when contract enforcement institutions are present, economic incentives have a salient effect on cheating behavior, akin to Fisman and Miguel (2007), Bohnet et al. (2001), and McMillan and Woodruff (2000). Trust is not necessary and it does not affect

market behavior. Similarly, neither trust nor trustworthiness is associated with market participation decision in the presence of impartial institutions (Column 8 in both panels). By contrast, initial trust and initial trustworthiness are both significantly and negatively associated with opportunistic behavior when either no institution or partial enforcement institutions only are present (Columns 2 and 3 in both panels). Initial trust is associated with more participation in the presence of partial institutions but with lower participation in the absence of institutions, although the latter effect is only marginally significant (Columns 6 and 7 in Panel b). There is no robust effect of individual trust on individual profits.

Elements of culture other than trust and trustworthiness may play a role. In Table 6, we provide results of specifications in which culture is proxied by participants' region of birth instead of initial behavior in the trust game. We reach similar conclusions. Culture is an important determinant of opportunistic behavior and of market participation decisions, but only in the absence of impartial institutions. Cheating and opting out of trading are more prevalent in absence of institutions (NoES) in the South of Italy. As a result, profits are much lower in Southern Italy under NoES and PES. Cheating and opting out of trading are more prevalent under partial institutions (PES) than under NoES or IES in Kosovo. However, subjects display no significant differences in cheating behavior, opting out, or profits under IES regardless of the region they originate from.

7. Conclusion and Policy Implications

We designed a framed field experiment to identify the causal effects of legal institutions on trust and trustworthiness and shed light on how formal institutional quality and cultural traits interact to sustain market exchange. We obtained several results. The quality of legal institutions has a positive causal effect on trust and trustworthiness, in a way that suggests

that moral norms of cooperative behavior can result as a positive by-product of improvement in formal institutional quality. Better legal institutions enhance trust and trustworthiness namely by reducing the frequency with which subjects face opportunistic agents when trading. This reduction enables individuals to revise upwards their beliefs about other people generalized trustworthiness and it results in higher trust. Even more strikingly, agents not only trust more but also reciprocate by being more trustworthy. This is important because such generalized norms of trust and trustworthiness play a crucial role in supporting exchange and cooperation when contracts are incomplete or nor easily enforceable. This finding contributes to the literature that roots trust in the well functioning of impartial institutions, provides empirical support to models such as Guiso et al. (2008a) and Tabellini (2008), and complements existing non-experimental empirical evidence. Our controlled experiment not only establishes a causal link from formal institutions to culture, ruling out the feedback effect of culture on the design of institutions, but also opens the black box of institutions by focusing on one dimension of enforcement institutions: partiality vs. impartiality. Our empirical analysis quantifies the effect of impartial enforcement institutions on trust through their influence on cooperation in a contractible environment.

Another noteworthy finding is that pre-existing trust and trustworthiness, or more generally cultural origins, influence market participation and opportunistic behavior in the absence of formal enforcement, or when formal enforcement is based on personalized networks; but they cease to matter once strong and impartial formal institutions are in place. This echoes previous findings in non-experimental settings (Fisman and Miguel 2007, Grosjean 2011b) and suggests that cultural norms substitute formal enforcement when the latter is weak.

Our study offers practical contributions for the reform of governance institutions. It provides evidence of variations in the evolution of trust and trustworthiness and in opportunistic

behavior under different institutions in different cultural contexts. This information is valuable to policy makers and members of the judicial system in the debate over the contribution of informal institutions to public order and efficiency. Our study concludes, on an optimistic note, that formal institutions can work not only to sustain economic exchange but also to build trust, even in low trust environments such as the South of Italy or even if current formal institutions are poorly developed, as in Kosovo. However, in a real world environment, the problem is how to generate such positive institutional change. Some studies have shown how different modes of institutional transplants -whether such institutions are imposed or adopted in a democratic fashion- affect the likelihood of their success (Dal Bo, Foster and Putterman 2010). We aim to explore these issues in further work. This additional study would cast light on the issue of the endogenous evolution of institutions, as well as how culture and pre-existing norms may affect it.

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[i] For example, Alesina and Angeletos (2005) describes how beliefs about redistribution influence, and are influenced, by actual redistribution policies. In Aghion et al. (2010), low trust individuals demand more regulation as they cannot rely on trust to enforce contracts. Williamson and Kerekes (2011) discusses the strong empirical correlation between culture and formal institutions in a cross-section of countries.

[ii] On the persistence of historical events on formal institutions, see, among others: Engerman and Sokoloff (1997), Acemoglu, Johnson and Robinson (2001), Dell (2011). On the persistence of historical events on norms, attitudes and trust: Guiso et al. (2008a), Durante (2011), Grosjean (2011a and 2011b), Nunn and Wantchekon (2011), Voigtländer and Voth (forth).

[iii] Most exogenous factors that influence formal institutions might also influence trust, and vice versa. For example, the exclusion restriction for one of the most popular instrument for institutions, legal origins (Djankov et al. 2002, La Porta, Lopez-de-Silanes, and Shleifer 2008), is violated if Europeans who transplanted legal traditions also transplanted aspects of beliefs or even regulatory traditions that may influence trust. For more details and examples on how

institutions first established by European migrants were endogenous to their cultural beliefs, see Nunn (2012).

[iv] In support of this hypothesis, Cassar, Friedman and Schneider (2009) provide evidence based on laboratory markets experiments showing that reputation-based networks significantly reduce cheating and increase efficiency with respect to a baseline of completely anonymous interactions in the absence of legal enforcement institutions, but, even if in theory they could achieve 100% efficiency, in practice they always fail to do so.

[v] The literature is too large to be adequately reflected here. For the role of formal and informal institutions in supporting trade: Fafchamps (2006), Greif (2006), North (1991), Dixit (2004); for the role of formal institutions in promoting growth and development, see namely Rodrik, Subramanian and Trebbi (2002), Acemoglu, Johnson, Robinson (2001), Dell (2011); for the role of trust in promoting cooperation, development and growth: Guiso, Sapienza and Zingales (2006, 2008a, 2008b, 2009), Tabellini (2008, 2010); Algan and Cahuc (2010).

[vi] This is measured through answers to a survey question called 'Passenger's dilemma', which asks for respondents' willingness to lie to the police in order to save a friend from jail in an hypothetical situation.

[vii] Similarly, Grosjean (2011b) finds that a culture of violence brought by Scots-Irish and Scottish Highlander settlers to the 18th century US only persisted where formal enforcement institutions were absent.

[viii] For example, the continuous double auction (CDA), which is usually run with the underlying assumption that contracts are perfectly and costlessly enforceable, always delivers 100% efficiency (Cassar, Friedman and Schneider 2009).

[ix] Despite differences in GDP per capita, in order to recruit and incentivize subjects we needed to use the same payoffs in Kosovo as in Italy, given the Kosovo high cost of living.

[x] A computer program displayed couples of random numbers reproducing participants' IDs, which were used to determine the random matching into pairs in this as well as in the other tasks of the experiment. The program was set so that repetition of the same pairs was kept to a minimum.

[xi] One session had 7 participants because 1 person left after the first trust game. The experimenter filled out the subject's decision sheet using random choices generated by the computer. Other participants in the session were aware of this.

[xii] Separate data for Kosovo, Northern and Southern Italy are presented in Table B1 in Appendix B.

[xiii] The t-stats are1.67 and 2.29, respectively.

[xiv] This model gets rid of any potential unobserved heterogeneity at the individual level or any departure from perfect randomization across treatments.

[xv] Table B2 in Appendix B shows results from regression analysis of the individual characteristics correlated with trust and trustworthiness in the initial trust game, for the whole and individual country samples. Our survey measure of risk-aversion, based on a non-incentivized, hypothetical choice between a safe and a risky lottery, is not significant except for trustworthiness in Kosovo.

[xvi] The results of these robustness specifications are not displayed here for economy of space, but are available upon request.

[xvii] Results for individual country subsamples in Appendix B Table B3.

[xviii] This probability is computed as: (Number of cheating partners/(Number of participants*Trading days in treatment round))*100 for each individual *i*.

[xix] The results of specifications supporting the discussion in this paragraph are not displayed for economy of space but are available upon request.

[xx] A series of Hausman specification tests cannot reject the hypothesis that individual effects are adequately modeled by random effects. The value of the Hausman statistics for the basic specification in the pooled sample (Columns 1, 5 and 9) is 7.38, 1.12 and 0.05 when the dependent variable is, respectively, cheat, stay out of trading and profit.

Tables

Table 1: Summary Statistics

Variable		Obs	All Mean	All Std. Dev.	PES Mean	PES Std. Dev.	IES Mean	IES Std. Dev.	(i)
Amount Sent TG1	Amount sent in first trust game	346	5.55	2.39	5.24	2.29	5.94	2.47	2.76
Amount Sent TG2	Amount sent in second trust game	346	5.92	2.62	5.43	2.44	6.51	2.72	3.87
Amount Sent TG2-TG1	Difference in Amount sent between first and second trust game	346	0.36	2.3	0.2	2.09	0.57	2.53	1.49
% Returned TG1	Percentage returned in first trust game	346	52.70	20.23	48.37	17.99	58.06	21.58	4.56
% Returned TG2	Percentage returned in second trust game	346	51.63	28.24	44.64	20.92	60.25	33.34	5.31
% Returned TG2-TG1	Difference in Percentage returned between first and second trust game	346	-1.11	23.39	-3.80	14.70	2.19	30.64	2.39
Prob(Partner cheat)	(Number cheating partners/(Number of participants*Trading days in treatment round))*100	346	3.54	2.41	4.27	2.43	2.63	2.06	-6.7
Gender (1 if male)		346	0.66	0.47	0.66	0.48	0.66	0.47	0.09
Age		342	36.14	14.94	35.81	14.45	36.56	15.56	0.46
Number of children		342	1.01	1.89	1.11	2.14	0.88	1.51	-1.11
Household size		339	4.47	2.34	4.68	2.45	4.20	2.18	-1.89
Married		342	0.45	0.50	0.48	0.50	0.41	0.49	-1.29
Separated		342	0.04	0.19	0.05	0.21	0.03	0.16	-1.03
Widow		342	0.01	0.11	0.01	0.10	0.01	0.11	0.21
Single		342	0.50	0.50	0.46	0.50	0.55	0.50	1.63
Employee (or self-employed)		344	0.50	0.50	0.54	0.50	0.46	0.50	-1.51
Student		344	0.20	0.40	0.14	0.35	0.27	0.45	-3.1
Unemployed		344	0.17	0.38	0.20	0.40	0.14	0.35	-1.34
Inactive or other		344	0.12	0.33	0.12	0.33	0.12	0.33	-0.11
Primary or secondary edu.		344	0.08	0.26	0.09	0.29	0.06	0.23	-1.11
High school		344	0.51	0.50	0.49	0.50	0.52	0.50	0.56
Post high school		344	0.23	0.42	0.25	0.44	0.19	0.40	-1.33
Graduate education		344	0.19	0.39	0.16	0.37	0.23	0.42	1.45
Household income (Euro,		333	54	92	59	107	48	70	-1.05
Socio-economic status (1 poorest-10 richest)		341	4.61	1.85	4.66	1.73	4.55	2.01	-0.53
Risky lottery Choice		344	0.15	0.36	0.16	0.36	0.15	0.36	-0.17
Business owner		345	0.34	0.48	0.34	0.48	0.34	0.48	0.00

Notes: (i) t statistics of t-test of the difference between PES and IES.

Table 2: Trust and Trustworthiness Results

OLS estimation	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Amount Sent TG2			**Amount Sent TG2-TG1**	**Amount Sent TG2**	**Amount Sent TG2**	**% Returned TG2**			**% Returned TG2-TG1**	**% Returned TG2**	**% Returned TG2**
Sample	Pooled				Italy	Kosovo	Pooled				Italy	Kosovo
Mean Dep. Var.	5.92			0.364	4.76	5.76	49.95			-1.114	46.43	53.3
IES	1.08***	0.64***	0.71***	0.37+	0.67*	0.98***	15.61***	8.48***	9.80***	5.99**	10.29*	9.05***
	[0.28]	[0.24]	[0.26]	[0.25]	[0.39]	[0.34]	[3.08]	[2.60]	[3.03]	[2.68]	[6.04]	[3.02]
	{0.34}	{0.25}	{0.25}	{0.26}			{3.99}	{3.41}	{3.47}	{3.32}		
Kosovo		0.23	0.44					3.30	4.58			
		[0.23]	[0.35]					[2.50]	[2.90]			
		{0.24}	{0.39}					{3.38}	{3.64}			
Amount Sent TG1		0.62***	0.60***		0.50***	0.68***						
		[0.05]	[0.06]		[0.11]	[0.06]						
		{0.06}	{0.06}									
% Returned TG1								0.75***	0.73***		0.71***	0.75***
								[0.07]	[0.07]		[0.15]	[0.08]
								{0.07}	{0.07}			
Individual controls	no	no	yes	no	yes	yes	no	no	yes	no	yes	yes
Observations	346	346	334	346	165	169	346	346	334	346	165	169
R-squared	0.04	0.36	0.39	0.01	0.30	0.58	0.08	0.36	0.39	0.02	0.28	0.62

Robust standard errors reported in brackets. Robust standard errors clustered at the session level (37 clusters) in curly brackets. Individual controls are gender, marital status, education level, employment status, socio-economic status (10 step income ladder) and risky lottery choice. All regressions with a constant. *** p<0.01, ** p<0.05, * p<0.1, + p<0.15.

Table 3: The Effect of Institutions on Cheating

First Stage Regressions	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	**Prob (Partner cheat)**							
Sample	Pooled				Italy		Kosovo	
Mean Dep. Var	3.54				3.27		3.80	
IES	-1.64***	-1.62***	-1.63***	-1.58***	-0.90**	-0.92**	-2.34***	-2.21***
	[0.24]	[0.27]	[0.24]	[0.27]	[0.38]	[0.38]	[0.41]	[0.42]
	{0.52}	{0.52}	{0.51}	{0.51}				
Kosovo	0.53**	0.52	0.50**	0.54				
	[0.24]	[0.36]	[0.24]	[0.37]				
	{0.53}	{0.57}	{0.54}	{0.57}				
Amount sent TG1	-0.03	-0.02			0.04		0.02	
	[0.05]	[0.05]			[0.08]		[0.08]	
	{0.06}	{0.06}						
% returned TG1			0.00	-0.01		0.00		-0.01
			[0.01]	[0.01]		[0.01]		[0.01]
			{0.01}	{0.01}				
Individual controls	no	yes	no	yes	yes	yes	yes	yes
Observations	346	334	347	334	165	165	169	169
R-squared	0.13	0.17	0.13	0.17	0.16	0.16	0.28	0.28
F stat IES	46.18	37.18	47.47	35.23	5.65	5.86	32.88	28.19

Robust standard errors reported in brackets. Robust standard errors clustered at the session level (37 clusters) in curly brackets. Individual controls are the same as in Table 2. All regressions with a constant. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Trust as a Function of the Probability of Facing a Cheater

Second Stage (Wald estimates)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	**Amount Sent**				**% Returned**			
Sample	Pooled		Italy	Kosovo	Pooled		Italy	Kosovo
Mean Dep. Var. var	5.92		4.76	5.76	49.95		46.43	53.3
Prob (Partner cheat)	-0.39**	-0.44**	-0.74	-0.42***	-5.14***	-6.19***	-11.24	-4.09***
	[0.16]	[0.17]	[0.50]	[0.15]	[1.74]	[2.14]	[7.71]	[1.52]
	{0.19}	{0.21}			{2.72}	{3.05}		
Kosovo	0.43*	0.67*			6.03**	7.91**		
	[0.26]	[0.38]			[2.76]	[3.77]		
	{0.33}	{0.41}			{5.17}	{5.46}		
Amount sent TG1	0.61***	0.59***	0.52***	0.69***				
	[0.06]	[0.06]	[0.11]	[0.07]				
	{0.06}	{0.06}						
% returned in TG1					0.74***	0.70***	0.73***	0.72***
					[0.08]	[0.08]	[0.19]	[0.09]
					{0.08}	{0.08}		
Individual controls	no	yes	yes	yes	no	yes	yes	yes
Observations	346	334	165	169	346	334	165	169
R-squared	0.25	0.27	0.01	0.46	0.18	0.16	-0.26	0.48
F	46.88	123.95	2.47	231.58	40.98	88.94	113.43	120.37

Robust standard errors reported in brackets. Robust standard errors clustered at the session level (37 clusters) in curly brackets. Individual controls are the same as in Table 2. All regressions with a constant. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Market Game Results - Initial Trust and Trustworthiness

GLS Random Effect Panel Estimation

Panel a. Trust	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
		Cheat			**Out (wish not to trade)**					**Profit**		
Sample	Pooled	NoES	PES	IES	Pooled	NoES	PES	IES	Pooled	NoES	PES	IES
PES	-0.02				-0.03***				-1.04***			
	[0.02]				[0.01]				[0.27]			
	{0.02}				{0.01}				{0.28}			
IES	-0.11***				-0.05***				3.34***			
	[0.02]				[0.01]				[0.36]			
	{0.02}				{0.01}				{0.33}			
TG1 Amount Sent	-0.02***	-0.02***	-0.02***	-0.01	-0.00	-0.01***	0.01*	0.00	-0.07	-0.04	-0.04	-0.15
	[0.01]	[0.01]	[0.01]	[0.01]	[0.00]	[0.00]	[0.00]	[0.00]	[0.07]	[0.08]	[0.11]	[0.11]
	{0.00}				{0.00}				{0.05}			
Country dummy	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Trading day dummies	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	6,679	3,339	1,860	1,480	6,680	3,340	1,860	1,480	6,679	3,339	1,860	1,480
Number of subjects	334	334	186	148	334	334	186	148	334	334	186	148
Wald chi2	1622	1764	1029	162	361.1	3062	105.6	62.29	380.5	1493	115.2	52.15

Panel b. Trustworthiness	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
		Cheat			**Out (wish not to trade)**					**Profit**		
Sample	Pooled	NoES	PES	IES	Pooled	NoES	PES	IES	Pooled	NoES	PES	IES
PES	-0.02				-0.03***				-1.03***			
	[0.02]				[0.01]				[0.27]			
	{0.02}				{0.01}				{0.28}			
IES	-0.11***				-0.05***				3.33***			
	[0.02]				[0.01]				[0.36]			
	{0.02}				{0.01}				{0.33}			
TG1 Amount Returned	-0.20***	-0.20***	-0.30***	-0.11	-0.02	-0.04	-0.05	0.02	-0.17	-0.62	0.84	0.01
	[0.07]	[0.07]	[0.10]	[0.09]	[0.02]	[0.03]	[0.04]	[0.03]	[0.73]	[0.83]	[1.35]	[1.08]
	{0.06}	{0.07}	[0.12]	[0.09]	{0.03}	{0.03}	[0.04]	[0.03]	{0.70}	{0.85}	[1.34]	[1.19]
Country dummy	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Individual Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Trading day dummies	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	6,679	3,339	1,860	1,480	6,680	3,340	1,860	1,480	6,679	3,339	1,860	1,480
Number of subjects	334	334	186	148	334	334	186	148	334	334	186	148
Wald chi2	1713	3471	841.2	160.18	360.7	3102	103.2	60.17	379.5	1654	116.3	51.82

GLS individual panel regression with random effect. Robust standard errors reported in brackets. Robust standard errors clustered at the session level (37 clusters) in curly brackets. Individual controls are gender, marital status, education level, employment status, socio-economic status (10 step income ladder). Trustworthiness: amount returned as a percentage, divided by 100 (i.e. between 0 and 1). All regressions with a constant. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Market Game Results - Regional Origins

GLS Random Effect Panel Estimation

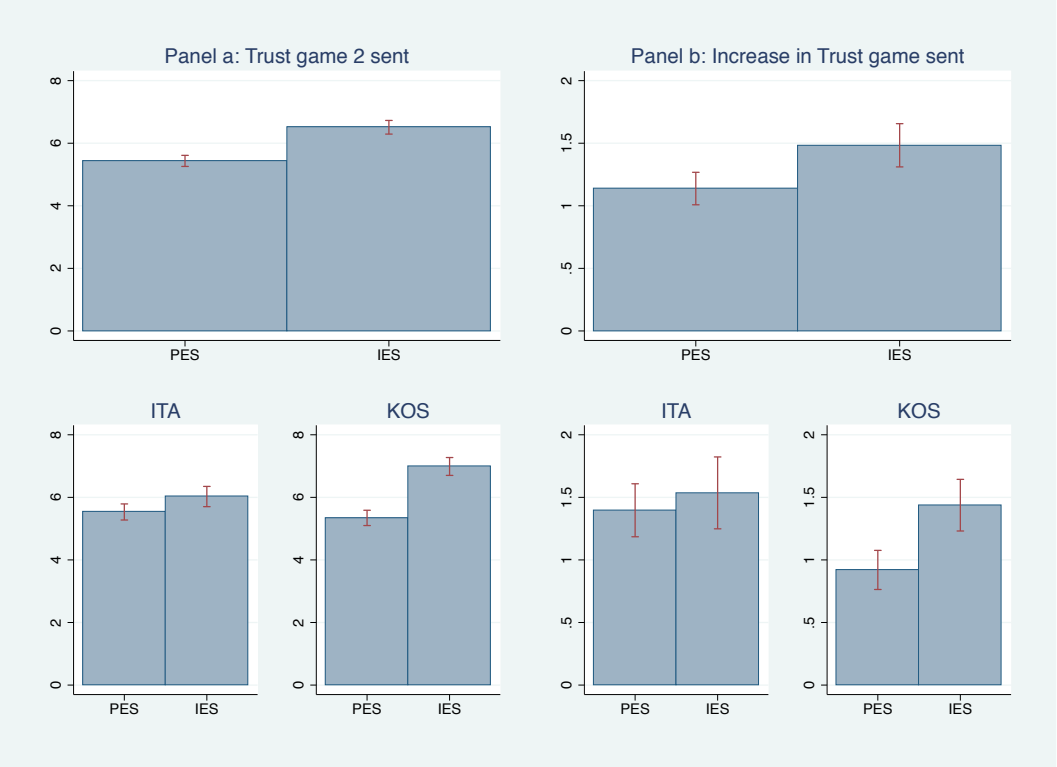
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
		Cheat				**Out (wish not to trade)**				**Profit**		
	Whole	NoES	PES	IES	Whole	NoES	PES	IES	Whole	NoES	PES	IES
PES	-0.02				-0.03***				-1.02***			
	[0.02]				[0.01]				[0.27]			
	{0.02}				{0.01}				{0.28}			
IES	-0.11***				-0.05***				3.32***			
	[0.02]				[0.01]				[0.36]			
	{0.02}				{0.01}				{0.34}			
Sicily	0.10***	0.13***	0.08	0.03	0.02*	0.03**	0.01	0.01	-1.56***	-2.11***	-1.27*	-0.25
	[0.04]	[0.04]	[0.05]	[0.06]	[0.02]	[0.01]	[0.03]	[0.02]	[0.46]	[0.59]	[0.69]	[0.71]
	{0.04}				{0.01}				{0.48}			
Kosovo	0.04	-0.01	0.16***	-0.05	-0.02	-0.02	-0.06**	0.01	-0.36	-0.64	0.34	-0.14
	[0.04]	[0.05]	[0.06]	[0.07]	[0.02]	[0.02]	[0.02]	[0.02]	[0.48]	[0.76]	[0.66]	[0.74]
	{0.04}								{0.59}			
Individual Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Trading day dummies	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	6,679	3,339	1,860	1,480	6,680	3,340	1,860	1,480	6,679	3,339	1,860	1,480
Number of subjects	334	334	186	148	334	334	186	148	334	334	186	148
Wald chi2	1695	2677	921.1	159.37	360.8	6816	104.5	60.86	401.8	2733	121.6	52.12

GLS individual panel regression with random effect. Robust standard errors reported in brackets. Robust standard errors clustered at the session level (37 clusters) in curly brackets. Excluded regional category is North Italy. Individual controls: age, gender, marital status, education, income, employment status. All regressions with a constant. *** p<0.01, ** p<0.05, * p<0.1.

Figures

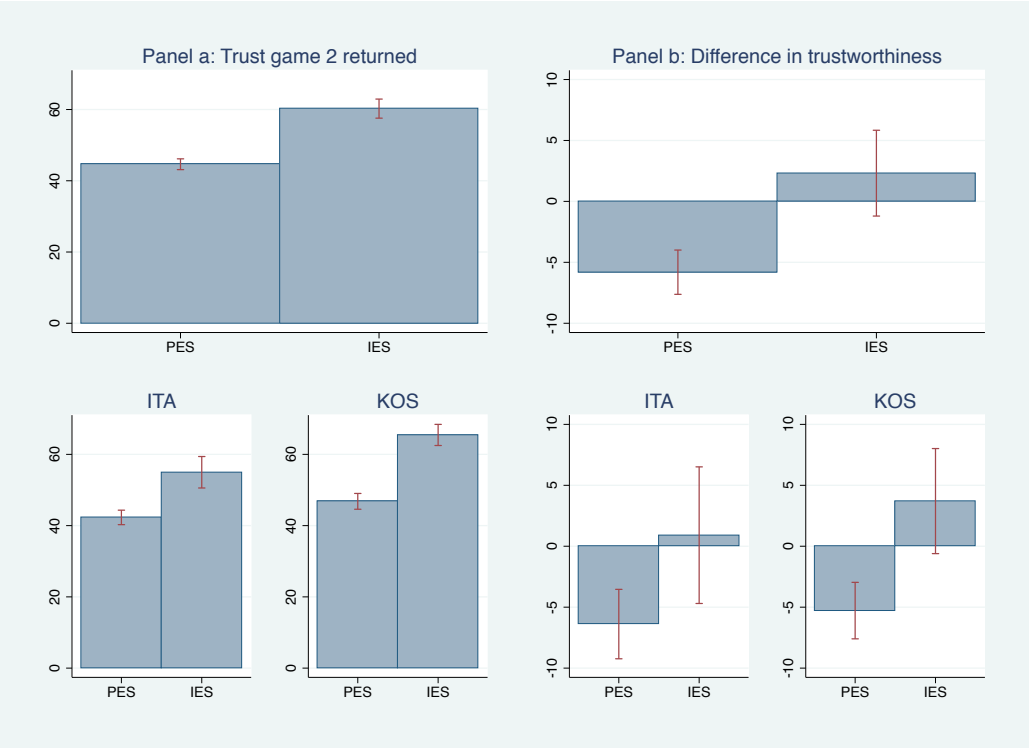
Figure 1: Trust and Quality of Institutions in a Cross-Section of Countries

Figure 2: Trust across Treatments: behavior in game 2 (a) and first difference (b)



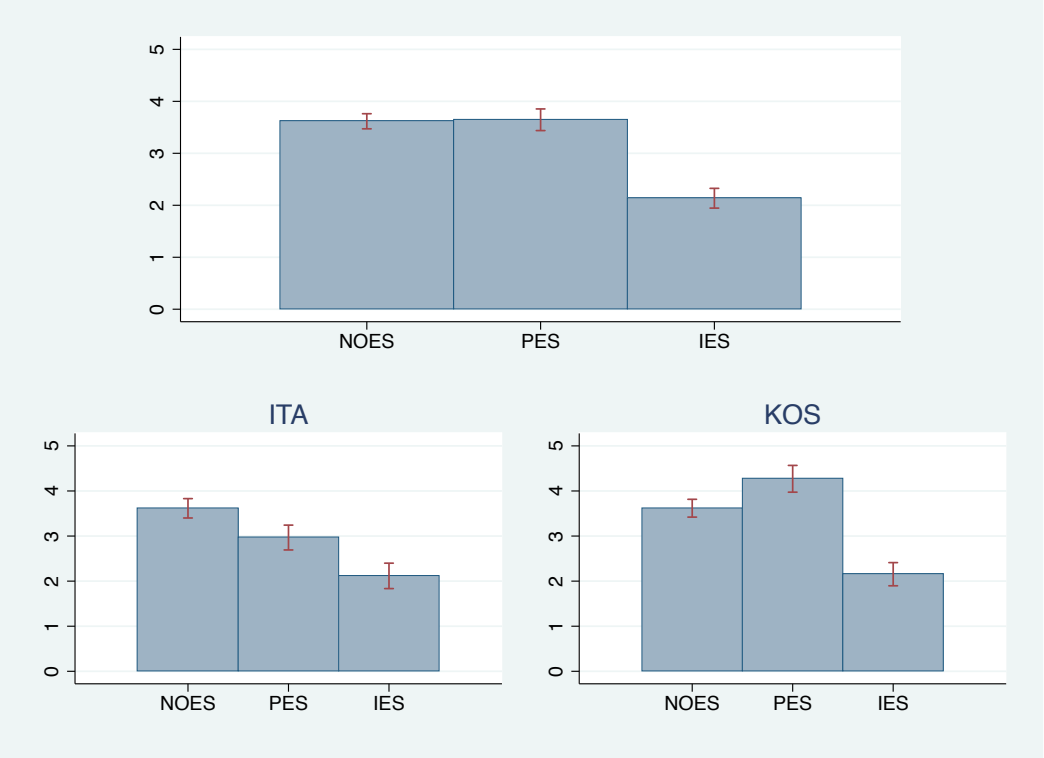
Notes: bar graphs of averages. Spikes represent the standard error around the mean.

Figure 3: Trustworthiness across Treatments: behavior in game 2 (a) and first difference (b)



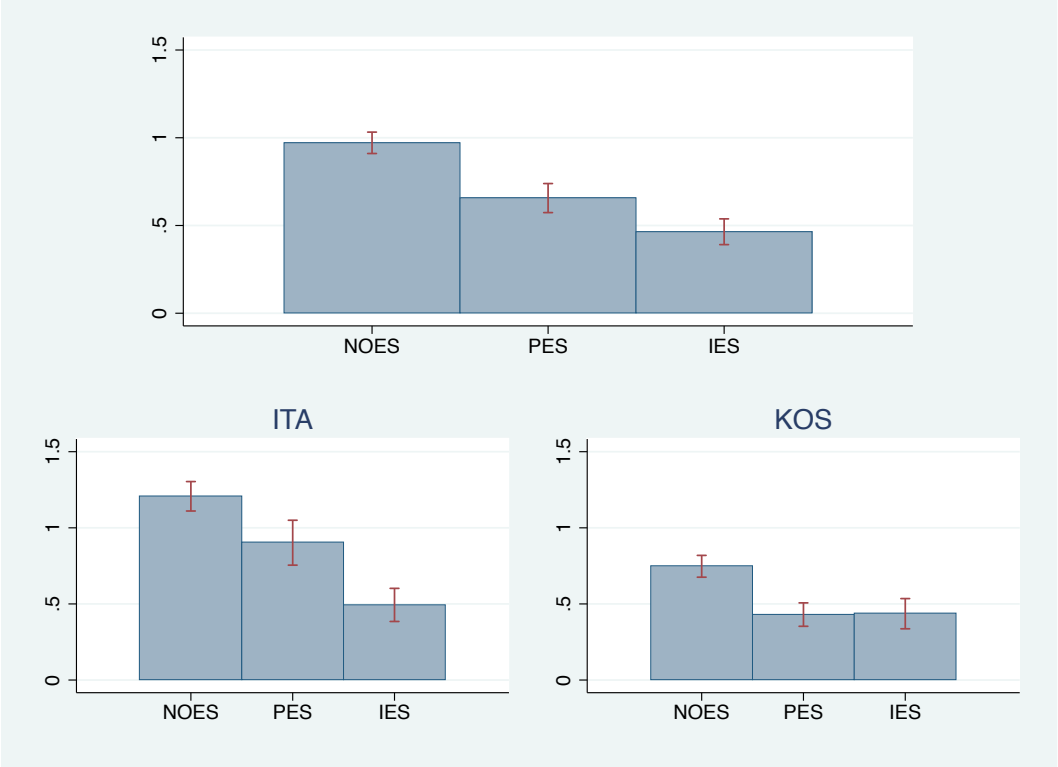
Notes: bar graphs of averages. Spikes represent the standard error around the mean.

Figure 4: Cheating in the Trade Game



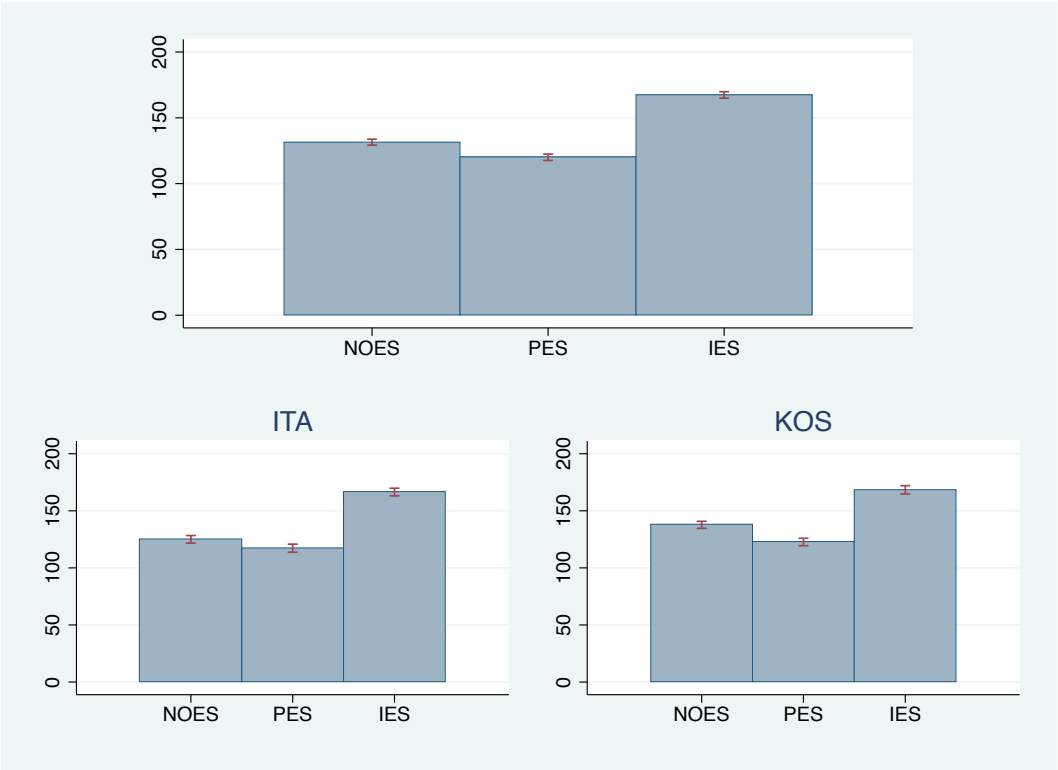
Notes: bar graphs of averages. Spikes represent the standard error around the mean.

Figure 5: Opting Out of Trade in the Trade Game



Notes: bar graphs of averages. Spikes represent the standard error around the mean.

Figure 6: Trading Profits in the Trade Game



Notes: bar graphs of averages. Spikes represent the standard error around the mean.

APPENDIX A: EXCHANGE GAME SOLUTIONS

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No Institutions (NoES). The solution to the market game in absence of institutional contract enforcement is discussed in the paper (section 3.2). Given the payoffs displayed in Matrix 1, cheat is a weakly dominant strategy in this game. By elimination of weakly dominated strategies, we obtain one Nash equilibrium (Cheat, Cheat) which gives a payoff of 10 to each one of the two trading partner. There is another equilibrium in pure strategy: (Out, Out), but it is payoff dominated for both players by the (Cheat, Cheat) Nash equilibrium.

Partial Enforcement System (PES). In the partial enforcement treatments agents have to choose a strategy comprised of whether to get protection {P, NP} and whether to cheat, trade honestly or stay out, denoted respectively by {C, H, O}. The payoffs to this game can be represented in normal form as follows:

	P C	P H	P O	NP C	NP H	NP O
P C	12,12	12,15	-4,-4	25,-3	25,0	-4,1
P H	15,12	15,15	-4,-4	15,-3	15,20	-4,1
P O	-4,-4	-4,-4	-4,-4	-4,1	-4,1	-4,1
NP C	-3,25	-3,15	1,-4	10,10	30,0	1,1
NP H	0,25	20,15	1,-4	0,30	20,20	1,1
NP O	1,-4	1,-4	1,-4	1,1	1,1	1,1

(P, O) is the only strictly dominated strategy for this game. The only equilibrium in pure strategy in this game is {(NP,O);(NP,O)}. Indeed, if player 2 plays O, player 1 can do no better than not to buy protection and play O (or anything else). While there is no other equilibrium in pure strategies, this game has several equilibria in mixed strategies (see discussion in section 3.2).

Impartial Enforcement System (IES). Under the impartial enforcement treatment agents have to choose a strategy comprised of whether to take a cheating partner to court {C, NC} and whether to

cheat, trade honestly or stay out, denoted respectively by {C, H, O}. The payoffs to this game can be represented in normal form as follows:

	C C	C H	C O	NC C	NC H	NC O
C C	13,13	15,18	1,1	13,13	30,0	1,1
C H	18,15	**20,20**	1,1	18,15	20,20	1,1
C O	1,1	1,1	1,1	1,1	1,1	1,1
NC C	13,13	15,18	1,1	10,10	30,0	1,1
NC H	0,30	20,20	1,1	0,30	20,20	1,1
NC O	1,1	1,1	1,1	1,1	1,1	1,1

{C H, C H} is obtained as a Nash-equilibrium in pure strategies by iterated deletion of weakly dominated strategies. It is not unique, though, as equilibria in which both players play O (regardless of whether they play C or not) are also Nash equilibria of this game, but it is payoff-dominant.

APPENDIX B: ADDITIONAL TABLES AND FIGURES

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Table B1: Summary Statistics by Country

	Italy North			Italy South			Kosovo		
Variable	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
Gender (1 if male)	99	0.53	0.50	70	0.63	0.49	177	0.75	0.43
Age	99	45.40	13.37	67	43.45	13.15	176	28.15	11.67
Number of children	97	1.29	2.48	68	1.38	1.65	177	0.71	1.53
Household size	98	2.79	1.35	67	3.31	1.20	174	5.86	2.25
Married	99	0.62	0.49	70	0.63	0.49	173	0.28	0.45
Separated	99	0.06	0.24	70	0.06	0.23	173	0.02	0.13
Widow	99	0.00	0.00	70	0.01	0.12	173	0.02	0.13
Single	99	0.32	0.47	70	0.30	0.46	173	0.68	0.47
Employee (or self-employed)	99	0.77	0.42	69	0.67	0.47	176	0.29	0.45
Student	99	0.04	0.20	69	0.04	0.21	176	0.35	0.48
Unemployed	99	0.00	0.00	69	0.19	0.39	176	0.27	0.44
Inactive or other	99	0.19	0.40	69	0.10	0.30	176	0.09	0.29
Primary or secondary edu.	99	0.14	0.35	68	0.09	0.29	177	0.03	0.18
High school	99	0.49	0.50	68	0.44	0.50	177	0.54	0.50
Post high school	99	0.09	0.29	68	0.06	0.24	177	0.37	0.48
Graduate edu.	99	0.27	0.45	68	0.41	0.50	177	0.06	0.24
Household income (Euro)	96	2.29	1.44	67	2.02	1.93	170	103.32	108.34
Socio-economic in. (1 poorest-10 richest)	99	4.79	1.45	69	4.46	2.06	173	4.57	1.97
Business owner	99	0.19	0.40	70	0.36	0.48	175	0.05	0.22
Risky lottery choice	99	0.38	0.49	70	0.41	0.50	176	0.29	0.45

Table B2: Individual characteristics correlated with initial trust

OLS Estimation	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Amounts Sent				**% Returned**			
Sample	Pooled	Italy North	Italy South	Kosovo	Pooled	Italy North	Italy South	Kosovo
Gender (1 if female)	-0.53	-1.07*	-1.08	0.01	-6.60	-10.03	-3.98	-6.07
	[0.29]	[0.53]	[0.83]	[0.46]	[4.28]	[7.21]	[11.48]	[8.32]
Age	-0.00	0.00	-0.04	0.00	-0.12	0.02	-0.09	-0.45
	[0.02]	[0.03]	[0.04]	[0.03]	[0.22]	[0.36]	[0.54]	[0.43]
Household size	0.01	0.00	-0.37	0.00	0.05	-0.54	0.17	0.00
	[0.07]	[0.20]	[0.31]	[0.09]	[1.48]	[2.78]	[4.67]	[1.54]
Married	0.05	2.03*	0.03	-2.34*	-6.57	-4.46	-16.03	-15.42
	[0.68]	[0.87]	[0.94]	[1.12]	[7.55]	[10.45]	[15.82]	[20.51]
Separated	0.00		-1.17	-0.25	0.52		-0.19	0.85
	[0.88]		[1.22]	[0.75]	[14.22]		[15.39]	[17.63]
Widow	-0.09	-0.48	-1.39	0.04	0.15	-8.54	0.49	0.23
	[0.41]	[0.81]	[0.73]	[0.55]	[6.65]	[9.01]	[12.90]	[9.84]
Student	0.01	0.12	0.02	-0.11	0.21	0.56	1.00	0.12
	[0.52]	[1.52]	[1.00]	[0.55]	[7.73]	[9.93]	[30.48]	[8.98]
Unemployed	-0.10		-0.58	0.00	-2.33		-18.89	0.08
	[0.45]		[1.20]	[0.53]	[7.71]		[19.46]	[9.76]
Inactive or other	0.02	-0.57	0.03	0.06	-5.22	-13.58	-9.53	-3.35
	[0.50]	[0.60]	[1.00]	[0.83]	[6.30]	[9.16]	[13.88]	[12.95]
High school	-0.81*	-0.40	-2.24	-1.71*	-1.13	-2.36	0.52	-13.32
	[0.37]	[0.59]	[1.52]	[0.73]	[10.81]	[11.62]	[38.95]	[18.88]
Post high school	-0.89	-0.45	-3.22	-1.77*	-2.47	-6.28	-23.65	-11.11
	[0.54]	[0.96]	[1.82]	[0.74]	[11.16]	[13.87]	[39.91]	[19.01]
Graduate edu.	-0.20	0.05	-1.03	-1.72	0.15	0.18	24.62	-10.92
	[0.52]	[0.80]	[1.62]	[1.01]	[11.71]	[12.93]	[39.89]	[21.57]
Individual monthly income (Euro)	-0.00	-0.09	0.01	-0.00	-0.01	-0.54	-0.72	-0.01
	[0.00]	[0.10]	[0.13]	[0.00]	[0.01]	[1.51]	[2.37]	[0.01]
Subjective economic status	0.01	0.01	0.00	0.00	-0.01	-1.56	-1.94	0.00
	[0.09]	[0.21]	[0.16]	[0.12]	[1.44]	[2.34]	[3.62]	[1.99]
Business owner	-0.29	-0.44	0.04	-1.85**	-8.93	-9.47	0.40	-20.34
	[0.39]	[0.76]	[0.81]	[0.54]	[5.06]	[10.96]	[14.89]	[11.17]
Observations	316	96	60	160	316	96	60	160
R-squared	0.00	0.01	0.02	0.01	0.00	0.01	0.01	0.01

Robust standard errors reported in brackets. All regressions with a constant. *** p<0.01, ** p<0.05, * p<0.1.

Table B3: Trust and Trustworthiness Results, First Differences Estimation (Country Sub-Samples)

	(1)	(2)	(3)	(4)	(5)	(6)
	Amount sent			**% Returned**		
Sample	Italy North	Italy South	Kosovo	Italy North	Italy South	Kosovo
Mean Dep. Var.	0.24	0.34	0.44	-7.39	5.43	-0.19
IES	0.02	0.42	0.54*	6.73**	2.89	6.27**
	[0.54]	[0.63]	[0.31]	[3.38]	[9.26]	[2.90]
Observations	99	70	177	99	70	177
R-squared	0.00	0.01	0.02	0.04	0.00	0.03

Robust standard errors in brackets All regressions with a constant. *** p<0.01, ** p<0.05, * p<0.1

Figure B1: Evolution of Cheating over trading days

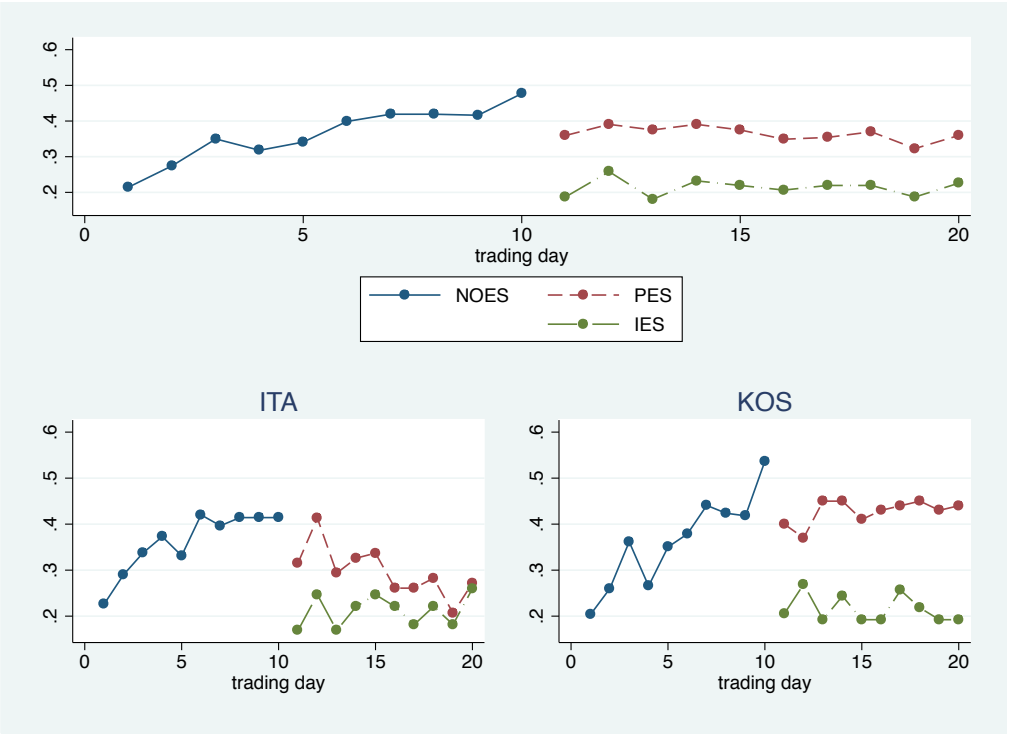


Figure B2: Evolution of participation over trading days

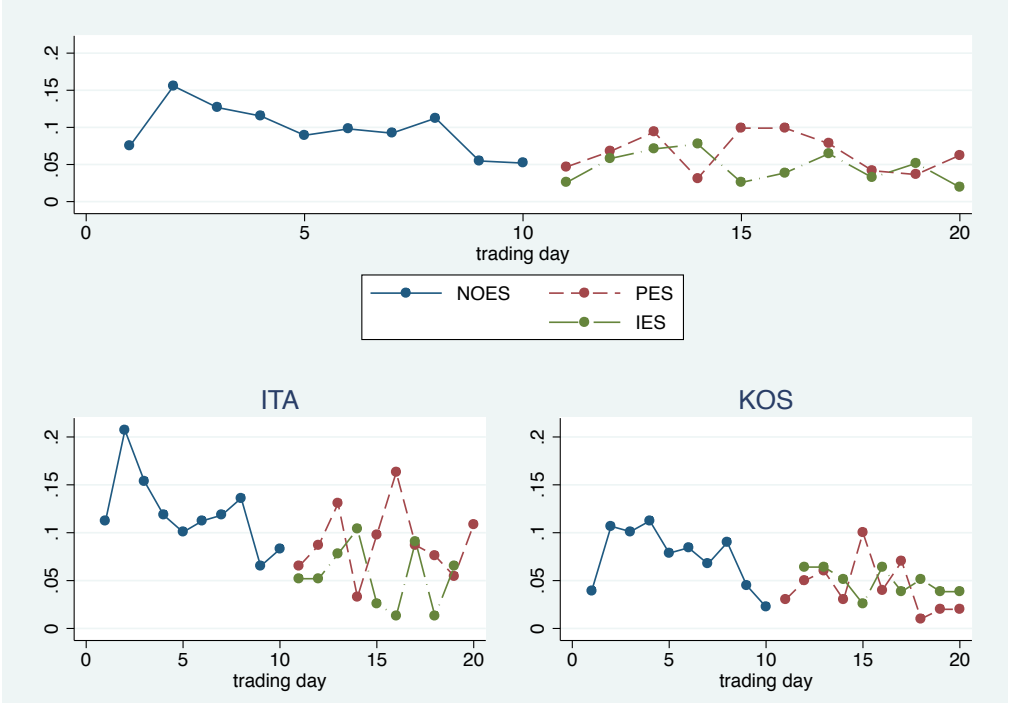
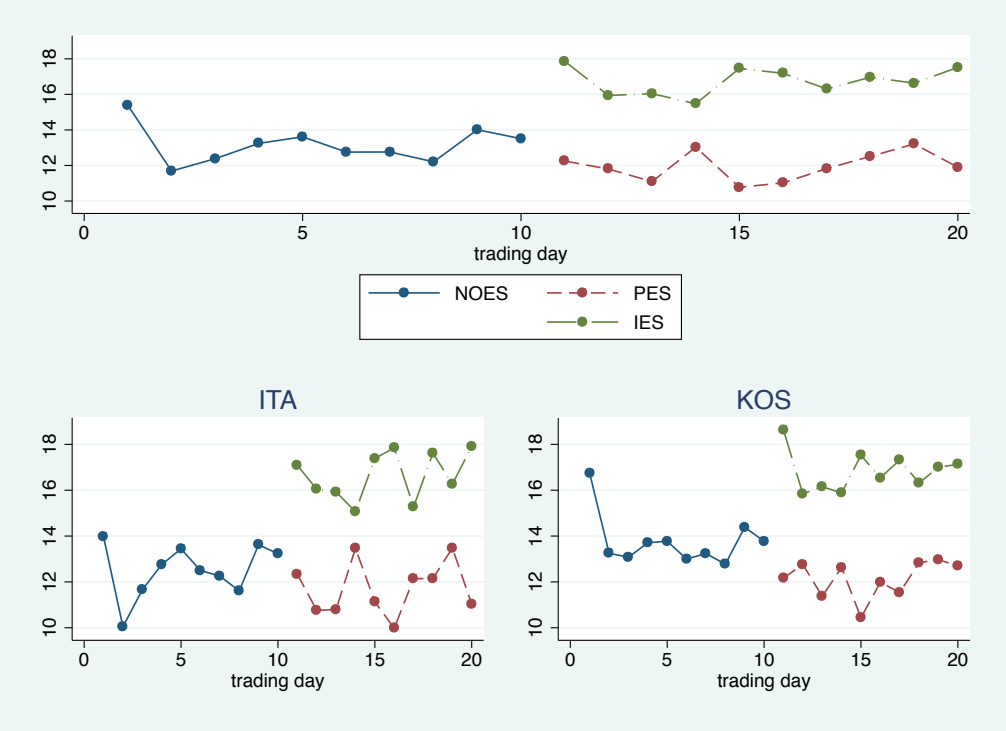


Figure B3: Evolution of traders' profits over trading days



APPENDIX C: EXPERIMENTAL INSTRUCTIONS

For Online Publication Only

Welcome!

Hi! Thank you for being here today to participate in this study about the economics of decision making. My name is …, and today with us are also …., who will help me conduct the activities. This study will be conducted in different places with different people. Since it is important that all participants to the study receive the same information, I will read from these instructions for the whole duration of the activities. Funds to conduct this study have been provided by the University of Delaware and the University of San Francisco.

Your participation in this study is voluntary. You have read and signed the consent form, so you know your rights and the potential risks from participating to this study. However, we think you will find this study interesting. In addition to a show-up fee of 10 Euros, you could earn a considerable amount of money, which will be paid to you in cash at the end of the experiment. The amount of money that you will go home with today depends partly on your choices in the activities that follow and partly on luck, so please do follow these instructions carefully.

This study may take up to 2 hours, so if you think you will not be able to stay that long without leaving please let us know now. If at any time you find that this is something in which you do not wish to participate for any reason, you are of course free to leave whether we have started the task or not.

This study will consist of several activities and a final survey. We will assign a number 1 to 4 for each activity. The numbering of the activity is for experimental purposes only. For each activity you will be asked to make some decisions. Each activity will involve a different number of decisions, divided in the following way:

Activity 1: 2 decisions
Activity 2: 10 decisions
Activity 3: 10 decisions
Activity 4: 2 decisions

At the end, we will randomly select one of these decisions to be the one that is actually paid. We will draw a number, 1-24 representing one of these decisions. For example, if 1 is drawn, you will be paid for the first decision in Activity 1. If 16 is drawn you will be paid according to the results in the 16^{th} decision (Decision 4 in Activity 3).

Your decisions in the activities and your answers to the survey will be PRIVATE and ANONYMOUS. You have been given a number. This number is your ID. You will have to ensure that your ID number is on each of the decision sheets and on the survey. The only people who will know your ID number are the experimenter and the experiment assistants. Your name will not appear on any of the decision sheets or survey, and it will not be possible to link your name to your ID. Please do not show your ID number to anyone.

You will be given instructions for the tasks today. We will read through the instructions together.

The instructions are simple and you will benefit from following them carefully. Please do not talk during any of the tasks. Thank you for participating!

Please make sure your mobile phones are turned off and in your bag to avoid interruptions during the meeting!

ACTIVITY 1

This task is for randomly assigned pairs of individuals. Each pair is made up of Player 1 and Player 2. Remember, none of you will know whom you are matched with, not even at the very end of our study.

For this activity, each player begins with 10 Euros.

PLAYER 1:
Player 1 will decide, how many, if any, of his 10 Euros he wishes to send to Player 2. Player 1 can choose to give any amount between 0 and 10 to Player 2. The chosen amount will then be tripled by the experimenter before it is passed on to Player 2.

PLAYER 2:
Player 2 will decide, for each possible amount that he/she can receive, how much, if any, he/she wishes to send back to Player 1. Note that Player 2 will decide how much to send back BEFORE knowing how much, if any, the Player 1 he/she will be paired with has sent.

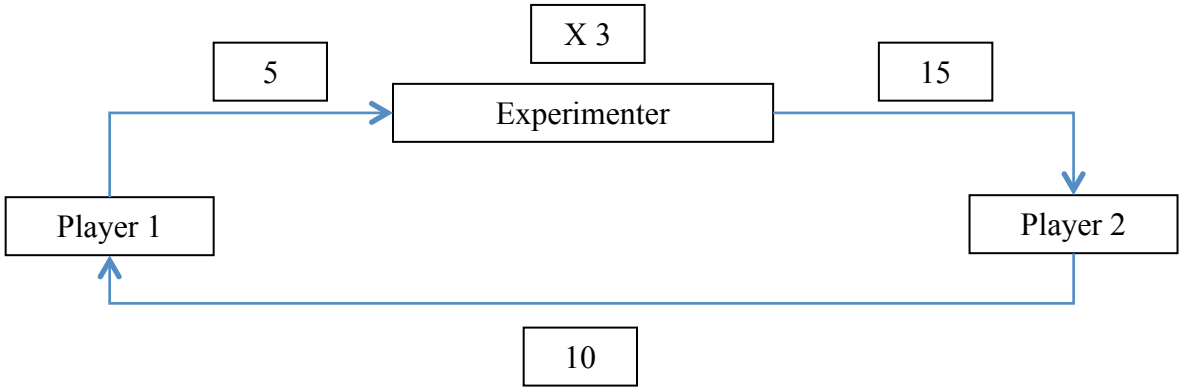
Each of you will play both roles, first the role of Player 1 and then the role of Player 2. We will randomly assign each Player 1 to a Player 2 (different from him/herself). For each pair, we will see how much Player 1 sends, what Player 2's decision is for that particular amount, and compute the payment. If at the end of the session, we draw decision 1 as the decision to be paid, each of you will be paid according to her decision as Player 1. If we draw decision 2 instead, each of you will be paid according to her decision as Player 2.

The payments for this activity are calculated in the following way: Player 1 earns the portion he/she kept from his/her original 10 Euros, plus whatever was returned by Player 2. Player 2 goes home with his/her original 10 Euros, plus whatever was given to him/her by Player 1 and tripled by the experimenter, minus whatever was given back to Player 1.

Here are some examples:

Initial Balance:
Player 1: 10
Player 2: 10

Step 1:
Player 1: 10-5 = 5
Player 2: 10+15=25



Ending Balance:
Player 1: 15
Player 2: 15

Step 2:
Player 1: 5+10=15
Player 2: 25-10=15

Please refer to the chart above for an example:

- The 1st Player gives 5 to the 2nd Player.
- This amount is tripled, so the 2nd Player gets 15 (3*5=15) *over and above* the initial 10 (which cannot be used to return money to the 1st Player). At this point, the 1st Player has 5 and the 2nd Player has 25 (10+15=25).
- Then the 2nd Player has to decide whether he wishes to send anything back to the 1st Player, and if so, how much.
- Imagine the 2nd Player decides to send 10 to the 1st Player. At the end, the 1st Player will go home with 15 (5+10=15) and the 2nd Player will go home with 15 (25-10=15).

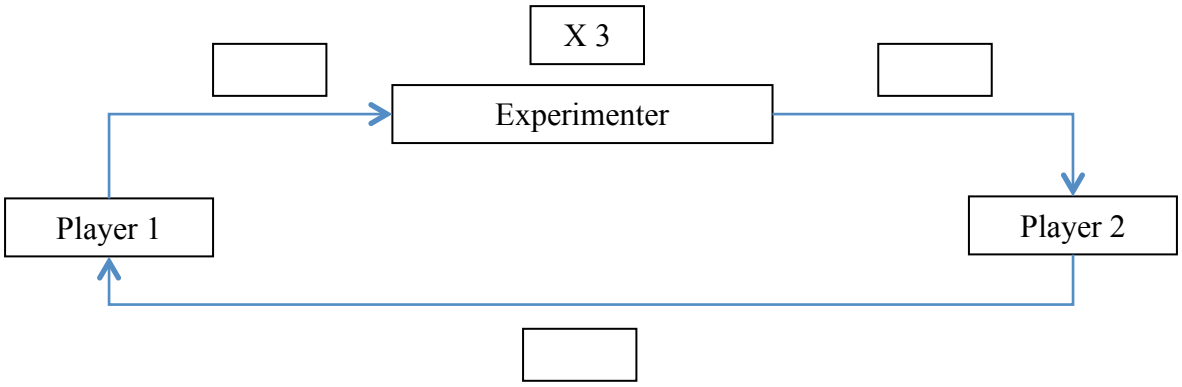
Another example:

- Imagine that the 1st Player gives all 10 to the 2nd Player.
- This amount gets tripled so the 2nd Player gets 30 (3*10=30) *over and above* the initial 10 (which cannot be used to return money to the first Player). At this point, the 1st Player has 0 and the 2nd Player has 40 (10+30=40).
- The 2nd Player has to decide whether they wish to send anything back to the 1st Player, and if so, how much.
- Suppose the 2nd Player decides to send 15 to the 1st Player. At the end, the 1st Player will go home with 15 (0+15=15) and the 2nd Player will go home with 25 (40-15=25).

Quiz:

Initial Balance:
Player 1: 10
Player 2: 10

Step 1:
Player 1:
Player 2:



Ending Balance:
Player 1:
Player 2:

Step 2:
Player 1:
Player 2:

Imagine that Player 1 gives 4 to Player 2.

1. At this point, Player 1 has ___ Euro.
2. The researcher triples the amount allocated by Player 1, so Player 2 receives ___ *over and above* the initial 10. At this point, Player 2 has ___ Euro.
3. Then Player 2 has to determine the amount to return to Player 1 from the ___.Suppose Player 2 decides to return 2 to Player 1. Player 1 goes home with ___ and Player 2 goes home with ___ Euro.
4. Suppose Player 2 decides to return 5 to Player 1. Player 1 goes home with ___ and Player 2 goes home with ___Euro.

Is everything clear? Are there any questions? If not, let's proceed with the actual activity.

PLAYER 1
We will now distribute the first decision sheet and we will go over it together.

Please write your ID number in the space at the top left of the decision sheet. The decision sheet tells you that you are Player 1 and looks like this:

ID…… **ACTIVITY 1**

You are Player 1.
Please select any amount between zero and 10 (in increments of 1, no cents) to send to Player 2:

_____

Remember that this amount will be tripled before reaching Player 2. Player 2 will then decide how much to send you back (Remember, Player 2 also started with 10, but she/he cannot send any of this back to you).

You must decide how much you want to send to Player 2 and write the amount in the space provided. When you have made your decision, place the sheet face down on the table and we will collect it.

PLAYER 2
We will now distribute the second decision sheet and we will go over it together.

Please write your ID number in the space at the top left of the decision sheet. The decision sheet tells you that you are a Player 2 and looks like this:

ID…… **ACTIVITY 1**

You are Player 2.

Please select the amount you would return to Player 1 for each of the scenarios below:

Scenario	Your Initial Endowment	If Player 1 Gives You	You Receive	You Give back to Player 1: (Please write down **one amount per row, it has to be between 0 and the amount you receive**)
1	10	0	0	
2	10	1	3	
3	10	2	6	
4	10	3	9	
5	10	4	12	
6	10	5	15	
7	10	6	18	
8	10	7	21	
9	10	8	24	
10	10	9	27	
11	10	10	30	

Since you will be matched randomly with a Player 1 (other than yourself), you will have to decide how much you want to send back for each possible amount that Player 1 can send. Since Player 1 can send from 0 to 10 Euros, there are 11 possible scenarios, represented by the rows of your decision sheet. Your endowment is always 10 Euros, as shown in the second column of the decision sheet. The third column shows the possible amounts that Player 1 can send. The fourth column shows the amount sent by Player 1 multiplied by 3, which is what you will receive. In the last column on the right you have to write your decision for each possible scenario.

Write how much you want to send back to Player 1 for each possible scenario in the space provided. When you have made all your decisions, place the sheet face down on the table and we will collect it.

Activity 2 and 3

In activity 2 and 3 we are going to create a market for a fictitious good. You will be traders in this market. For instance, you could be buyers or sellers of the good on this market. Remember that the information that you will receive (about your decision and your profits) is PRIVATE. To ensure the best results for yourself and complete data for the experimenters, please do not talk with other market participants while trading is in progress and do not discuss your information with others at any point during the experiment or afterwards.

Each time period for trading is called a TRADING DAY and will last for a maximum of 2 minutes. During a trading day you can decide whether or not to trade a unit of the fictitious good. For instance, you can see the decisions that you will take as those of a seller deciding whether to sell a unit of the good or not, or as those of a buyer deciding whether to buy a unit of the good or not. At the end of the trading day we will randomly match you with another trader (whom we will refer to as trading partner or counterpart), carry out the trade and calculate your profits for the Day. Then, a new Trading Day will begin. You will receive a new unit to trade for each new Day. Each trading day you will be paired with a different trading partner. There will be 10 Trading Days in activity 2 and 10 trading days in activity 3. The rules regulating trade on the market will be different in activity 2 and activity 3, so please pay special attention to that. We will explain and conduct activity 2 first, and then explain and conduct activity 3.

If at the end of the experiment we randomly draw a number between 3 and 22, then one of the trading days of activity 2 or 3 will get paid. In that case, your profits will be what you made on that randomly chosen trading day.

ACTIVITY 2 - NoES

Each trading day (for the next 10 trading days) you will have the chance to trade one unit of the good. You can choose among the following 3 actions:

1. Do not trade
2. Trade and not cheat
3. Trade and cheat

Your profit will depend on what your trading partner does. Your trading partner has the same 3 options: 1. doesn't trade; 2. trades and doesn't cheat; 3. trades and cheats. Your profits can then be summarized in the following table:

You/ Partner doesn't trade: **Your profit: 1** Partner's profit: 1	Partner cheats	Partner doesn't cheat
You cheat	**Your profit: 10** Partner's profit: 10	**Your profit: 30** Partner's profit: 0
You don't cheat	**Your profit: 0** Partner's profit: 30	**Your profit: 20** Partner's profit: 20

1. If you do not trade, your earning for the Day will be **1 token**.

2. If you trade and do not cheat, your profit will depend on whether your trading partner cheats or doesn't cheat:

- If the trading partner doesn't cheat, you earn **20 Euros** (and the trading partner too earns 20 Euros)
- If the trading partner cheats, you earn **0 Euros** (and the trading partner earns 30 Euros).

3. If you trade and cheat, again your profit will depend on whether your trading partner cheats or doesn't cheat:

- If the trading partner cheats, you earn **10 Euros** (and the trading partner too earns 10 Euros)
  If the trading partner doesn't cheat you earn **30 Euros** (and the trading partner earns 0 Euros).

One more note. In case you decide to trade but get matched with a partner that decided not to trade, your profit will be 1 Euro.

Is everything clear on how your profits are calculated? Are there any questions? If not, let's proceed with the actual activity.

We will now distribute your decision sheet and we will go over it together.

Please write your ID number in the space at the top left of the decision sheet. Your decision sheet has the following features..

For each of the trading days in this round, the first three columns show the three possible decisions that you can take: not trade, trade and not cheat, or trade and cheat. The last two columns on the right will be filled by us and will indicate the decision of the trading partner you are matched with and your profit for the Trading day.

If one of the trading days of this activity gets drawn for payment you will be paid according to your profits for that trading day.

You will have to mark with an X your desired choice for the current trading day. Please do not fill the decision sheet for the trading days still to come. Also, remember that you cannot change the choice you took in previous trading days.

Now please write your decision for the trading day number 1. When you are done, place the decision sheet face down and we will collect it.

ACTIVITY 3 - IES

The basic trading rules are the same as before, with the exception that now, if you want, you can take someone that cheats you to court. Taking someone to court will cost you **2 Euros** and it will cost the guilty party **5 Euros**. The cost of **2 Euros** will only be incurred if your trading partner cheats you and you want to take her/him to court. For simplicity, we will ask for you to decide whether you wish to take a cheating partner to court or not before the trade actually happens. We will only charge you the 2 Euros if you are cheated. We will compute your earnings for each trading day, depending on your decisions and those of the partner you are paired with. Each trading day you'll be paired with a different trading partner. There will be 10 Trading Days in this activity.

If none of you take each other to court, your profits will be calculated in exactly the same way as before. If you or the other player instead goes to court the profits will be calculated in the following way:

Each trading day (for the next 10 trading days) you will have the chance to trade one unit of the good. You can choose among several actions:

1. Do not trade
2. Trade and not cheat, and
    a. Opt TO take a cheating trading partner to court
    b. Opt to NOT take a cheating trading partner to court
3. Trade and cheat, and
    a. Opt TO take a cheating trading partner to court
    b. Opt to NOT take a cheating trading partner to court

Your profit will depend on what the trading partner does and whether you or the trading partner goes to court. The trading partner has the same options: 1. doesn't trade; 2. trades and doesn't cheat; 3. trades and cheats. If you cheat the trading partner will also have the option to take your to court or not. Your profits can then be summarized in the following table (note that if nobody takes her counterpart to court, the profits are computed as in Activity 2):

You/ Partner doesn't buy: **Your profit: 1** Partner's profit: 1	Partner cheats	Partner doesn't cheat
You cheat	No court **Your profit: 10** Partner's profit: 10 *Court* ***Your profit: 13*** *Partner's profit: 13*	No court **Your profit: 30** Partner's profit: 0 *Court* ***Your profit: 15*** *Partner's profit: 18*
You don't cheat	No court **Your profit: 0** Partner's profit: 30 *Court* ***Your profit: 18*** *Partner's profit: 15*	**Your profit: 20** Partner's profit: 20

1. If you do not trade, you will earn **1 token**.

2. If you trade and do not cheat, your profit will depend on whether your trading partner cheats or doesn't cheat, and whether you take the trading partner to court or not.

- If the trading partner doesn't cheat, you earn **20 Euros** (and the Seller too earns 20 Euros).
- If the trading partner cheats and you don't take him/her to court you earn **0 token** (and the trading partner earns 30 Euros)
- If the trading partner cheats and you take him/her to court you earn **18 Euros** (and the trading partner earns 15 Euros)

3. If you trade and cheat, again your profit will depend on whether your trading partner cheats or doesn't cheat, and whether the trading partner takes you to court or not.

- If the trading partner doesn't cheat and
  - doesn't take you to court, you earn **30 Euros** (and the trading partner earns 0 token);
  - takes you to court, you earn **15 Euros** (and the trading partner earns 18 Euros)
- If the trading partner cheats and
  - neither of you send each other to court, you earn **10 Euros** (and trading partner too earns 10 Euros)
  - both or one of you take each other to court, you earn **13 Euros** (and the trading partner too earns 13 Euros)

One more note. In case you decide to trade but get matched with a partner that decided not to trade, your profit will be 1 Euro.

Is everything clear? Are there any questions? If not, let's proceed with the actual activity. We will now distribute your decision sheet and we will go over it together.

Please write your ID number in the space at the top left of the decision sheet.

Your decision sheet has the following features.

It looks exactly as the decision sheet in the previous round, except for one thing: for each trading day, there is a row which asks you if you want to take the trading partner to court, in case the trading partner cheats. After marking your choice (court/no court), you have to decide whether you want to not trade, trade and not cheat, trade and cheat, as you did in the previous round. As in the previous activity, the columns on the right will be filled by us and will show the decisions (relative to going to court and trading) of the partner you'll be matched with and your profits for that trading day.

If one of the trading days of this activity gets drawn for payment, you will be paid according to your profits for that trading day.

You will have to mark with an X your desired choices for the current trading day. Please do not fill the decision sheet for the trading days still to come. Also, remember that you cannot change the choices you took in previous trading days.

Now please write your decision for the trading day number 1. When you are done, place the decision sheet face down and we will collect it.

ACTIVITY 3 - PES

The basic trading rules are the same as during the first 10 trading periods, with the exception that now, if you want, at the beginning of each trading day you can purchase "Personal Protection". Personal Protection will help you to make sure your trading partner doesn't cheat. If you desire to purchase Personal Protection, you need to pay 5 Euros at the beginning of each period and are incurred as a cost no matter what the partner does, whether she/he cheats, doesn't cheat or decides not to trade. The benefit of paying for personal Protection is that if a trading partner cheats you, the trading partner will be forced to carry out the contract, and pay 3 Euros as penalty. We will compute your earnings for each trading day, depending on your decisions and those of the partner you are paired with. There will be 10 Trading Days in this activity.

Your profit depends on whether you and your trading partner buy Personal Protection or not and on what you and your partner decide in terms of trading, cheating or not cheating. We have now 4 possible cases with respect to paying for Personal Protection:

1) Neither you nor the trading partner purchases Personal Protection
2) Both you and the trading partner purchase Personal Protection
3) Only you purchase Personal Protection
4) Only the trading partner purchases Personal Protection

1. Neither you nor the trading partner purchases Personal Protection

If none of you purchase Personal Protection, your profits will be calculated in exactly the same way as in the initial 10 periods:

You/ Partner doesn't trade: **Your profit: 1** Partner's profit: 1	Partner cheats	Partner doesn't cheat
You cheat	**Your profit: 10** Partner's profit: 10	**Your profit: 30** Partner's profit: 0
You don't cheat	Your profit: 0 Partner's profit: 30	Your profit: 20 Partner's profit: 20

2. Both you and the trading partner purchase Personal Protection

If both you and the trading partner purchase Personal Protection, the profits are calculated according to table below:

You and/or trading partner doesn't trade: **Your profit: -4** Partner's profit: -4	Partner cheats	Partner doesn't cheat
You cheat	**Your profit: 12** Partner's profit: 12	**Your profit: 12** Partner's profit: 15
You don't cheat	**Your profit: 15** Partner's profit: 12	**Your profit: 15** Partner's profit: 15

If you purchase Personal Protection and do not cheat, your profit will always be 15 Euros (20 – 5, with 5 being the payment for the Personal Protection). If you cheat, since the trading partner has Personal Protection too, the Personal Protector will take 3 Euros from you as cheating penalty, in addition to the 5 Euros you have to pay anyway because you purchased Personal Protection. So, in the case you cheat you earn 12 Euros. If either you or the trading partner matched with you decide not to trade with each other, your profit will be -4 Euros (1 token for not trading – 5 Euros for getting Personal Protection).

3. Only you purchase Personal Protection
If only you purchase Personal Protection, the profits are calculated according to the table below:

You and/or trading partner doesn't trade: **Your profit: -4** Partner's profit: 1	Partner cheats	Partner doesn't cheat
You cheat	**Your profit: 25** Partner's profit: -3	**Your profit: 25** Partner's profit: 0
You don't cheat	**Your profit: 15** Partner's profit: -3	**Your profit: 15** Partner's profit: 20

If you purchase Personal Protection and do not cheat, your profit will always be 15 Euros (20 – 5, with five being the payment to the Personal Protector). If you cheat, your profit will always be 25 Euros since either the trading partner doesn't cheat you (30 – 5, with five being the payment for the Personal Protector) or cheats you, but the Personal Protector makes sure he/she pays the full amount. If you decide not to trade, your profit will be -4 Euros, since you have already committed to buy protection. Your profit will be -4 Euros also in case you wanted to trade but the trading partner matched with you decided not to trade.

4. Only the trading partner purchases Personal Protection
If only the trading partner purchases Personal Protection, the profits are calculated in a similar way according to the table below:

You and/or trading partner doesn't trade: **Your profit: 1** Partner's profit: -4	Partner cheats	Partner doesn't cheat
You cheat	**Your profit: -3** Partner's profit: 25	**Your profit: -3** Partner's profit: 15
You don't cheat	**Your profit: 0** Partner's profit: 25	**Your profit: 20** Partner's profit: 15

If you don't purchase Personal Protection and do not cheat, your profit will be 20 Euros in case the trading partner doesn't cheat or 0 Euros if the trading partner cheats you. If you cheat, since the trading partner has Personal Protection, you will have to pay in full and, in addition, the Personal Protector will take all your profits and an extra 3 Euros from you as penalty. So, in this case you cheat you earn -3 Euros. If you or the trading partner decide not to trade, your profit will be 1 Euro since you did not buy protection.

Is everything clear? Are there any questions? If not, let's proceed with the actual activity. We will now distribute your decision sheet and we will go over it together.

Please write your ID number in the space at the top left of the decision sheet. The decision sheet has the following features.

It looks exactly as the decision sheet in the previous round, except for one thing: for each trading day, there is a row, which asks you if you want to buy personal protection for that day. . After marking your choice (protection/no protection), you have to decide whether you want to not trade, trade and not cheat, trade and cheat, as you did in the previous round. As in the previous activity, the columns on the right will be filled by us and will show the decisions (relative to purchasing protection and trading) of the partner you'll be matched with and your profits for that trading day.

If one of the trading days of this activity is drawn for payment, we will pay you according to your profits for that trading day.

You will have to mark with an X your desired choices for the current trading day. Please do not fill the decision sheet for the trading days still to come. Also, remember that you cannot change the choices you took in previous trading days.

Now please write your decision for the trading day number 1. When you are done, place the decision sheet face down and we will collect it.