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# Institutional Quality, Culture, and Norms of Cooperation: Evidence from Behavioral Field Experiments

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## Abstract

We 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. A total of 346 subjects in Italy and Kosovo played a market game under different and randomly allocated institutional treatments, which generated different incentives to behave honestly, preceded and followed by a noncontractible and nonenforceable trust game. Significant increases in individual trust and trustworthiness followed exposure to better institutions. A 1-percentage-point reduction in the probability of facing a dishonest partner in the market game, which is induced by the quality of legal institutions, increases trust by 7–11 percent and trustworthiness by 13–19 percent. 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.

## 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

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exchange? While there is a consensus that both good formal institutions and high levels of societal trust are beneficial for trade and development, how they interact and coevolve 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 2008b; Tabellini 2008). On the other hand, formal institutions, by reducing the marginal returns from being trustworthy, may crowd out trust and trustworthiness (Aghion et al. 2010). Figure 1, a scatterplot of societal levels of trust against the quality of institutions in a cross section of countries, illustrates the complex nature of this relationship.<sup>1</sup> While the correlation between trust and rule of law is positive, the correlation between trust and regulatory quality is nonexistent or even slightly negative. Showing a causal link from institutions to trust with happenstance data is difficult because institutions and beliefs are codetermined (Piketty 1995)<sup>2</sup> and coevolve under the influence of common historical events.<sup>3,4</sup>

In this study, 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 preexisting social norms interact to sustain market efficiency and cooperation.

Our experiment consists of four parts. The first is a trust game to measure preexisting social norms of trust and trustworthiness. The second consists of 10 rounds of a market game in which subjects chose whether to trade honestly,

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<sup>1</sup> Figure 1 is based on data from World Bank, Worldwide Governance Indicators (<http://info.worldbank.org/governance/wgi/index.aspx#home>), for 2000, and World Values Survey, WVS Wave 4 (1999–2004) (<http://www.worldvaluessurvey.org/WVSDocumentationWV4.jsp>).

<sup>2</sup> For example, Alesina and Angeletos (2005) describe how beliefs about redistribution influence, and are influenced by, actual redistribution policies. Aghion et al. (2010) state that low-trust individuals demand more regulation because they cannot rely on trust to enforce contracts. Williamson and Kerekes (2011) discuss the strong empirical correlation between culture and formal institutions in a cross section of countries.

<sup>3</sup> On the persistence of historical events on formal institutions, see, among others, Engerman and Sokoloff (1997), Acemoglu, Johnson, and Robinson (2001), and Dell (2011). On the persistence of historical events on norms, attitudes, and trust, see Guiso, Sapienza, and Zingales (2008b), Durante (2011), Grosjean (2011, 2014), Nunn and Wantchekon (2011), and Voigtländer and Voth (2012).

<sup>4</sup> 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 instruments 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 about and examples of how institutions first established by European migrants were endogenous to their cultural beliefs, see Nunn (2012).

cheat, or stay out, in the absence of any institution. The third part involves 10 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) treatment, which captures key traits of an impartial justice system in which all agents are equal in front of the law. The fourth part is another trust game that is identical to the first one. The different institutional treatments in the market game generate sharply different incentives to deviate from noncooperation (that is, cheating in the market game). More precisely, the Nash equilibrium is to cooperate with a probability of 0 under an enforcement system with no institutions (NoES), with a probability of 1 under the IES treatment, and with a probability between 0 and 1 under the PES treatment (mixed-strategy equilibrium). Allocation to the institutional treatment in the experimental session is random. We rely on the initial and final one-shot noncontractible and nonenforceable investment games to measure trust and trustworthiness as moral norms, separate from the cooperative norms occurring in the market game. We do so 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 behavior is affected by preexisting norms, or culture, 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 subjects 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, 12–18 percent and 20–31 percent higher under the IES treatment than under the PES treatment. Consistent with our design, the impartial treatment reduces the frequency of noncooperative behavior in the market game. In turn, Wald estimates indicate that a 1-percentage-point reduction in the frequency of facing a noncooperative partner (a cheater) in the trading game leads to a 7–11 percent increase in trust and a 13–19 percent increase in trustworthiness. In Italy, the effect of impartial versus partial institutions on trustworthiness is equivalent to three-fourths of the difference between Milan and Palermo. In Kosovo, it is equivalent to about three-fourths of the difference between Pristina, the capital city, and Mitrovica, the scene of major tensions during the civil war in 1999. 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.

Preexisting trust and trustworthiness are associated with less cheating, and more generally, cultural differences captured by participants' regional origins



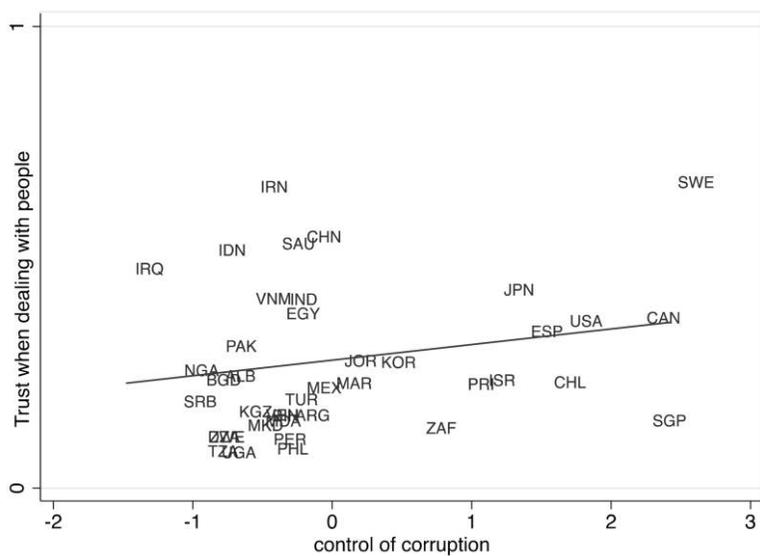
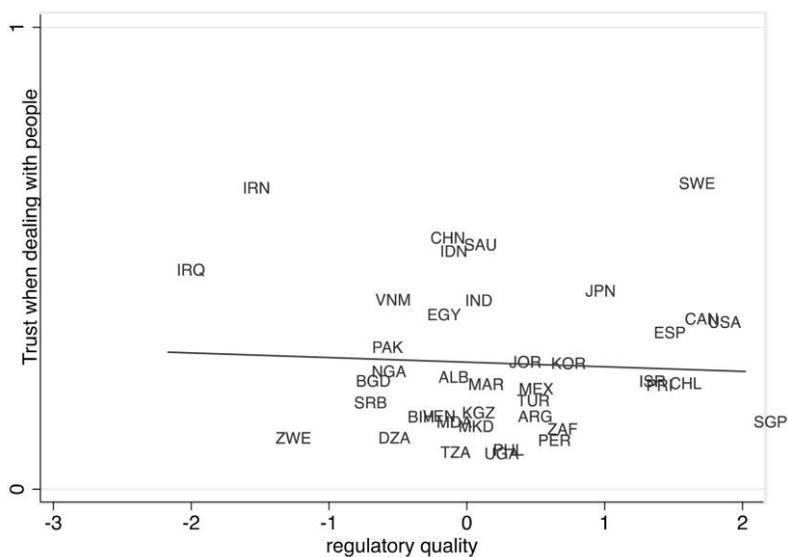


Figure 1. Continued

explain opportunistic behavior, but only for those who did not experience an 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 independent of preexisting 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, Sapienza, and Zingales 2008b; Tabellini 2008; Aghion et al. 2010). Empirically, recent papers have pointed to a positive relationship between institutional quality and trust on the basis of evidence that societal trust is higher today in regions that experienced good-quality institutions in the past (Guiso, Sapienza, and Zingales 2008a; Tabellini 2010; Grosjean 2011). A possible limitation to causal identification in these studies is that historical institutions were themselves the outcome of 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 develop as a by-product of better 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 northern Italy, which is characterized by good formal institutions and high levels of trust; in Sicily, which is characterized in theory by the same formal institutions but in practice by all-too-familiar partial, closed-network contract enforcement institutions and low levels of trust; and in Kosovo, which is characterized by weak formal institutions and relatively high levels of trust.

The paper is organized as follows. Section 2 presents our main hypothesis in light of the empirical and theoretical literature on the coevolution 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 preexisting norms on market behavior and efficiency and their interaction with institutional quality. Section 7 concludes. The online appendixes present the theoretical solution to the trading game, additional results and descriptive statistics, and the experimental instructions.

## 2. Background and Hypotheses

A fundamental proposition in economics is that markets achieve 100 percent 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 down market opportunities and surplus. 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, 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).<sup>5</sup> 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, because of either their incomplete nature or 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 levels of trust promote trade, cooperation, and development,<sup>6</sup> the question of how institutions and social norms interact and coevolve is much less clear.

The literature offers many definitions of 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 noncontractible expectations and behaviors, distinguished from the ones that occur in contractible market environments. The literature has tried to distinguish between the two. In the theoretical model of Guiso, Sapienza, and Zingales (2008b), trust is based on

<sup>5</sup> In support of this hypothesis, Cassar, Friedman, and Schneider (2009) provide evidence based on laboratory market 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 percent efficiency, in practice they always fail to do so.

<sup>6</sup> The literature is too large to be adequately reflected here. For the role of formal and informal institutions in supporting trade, see Fafchamps (2006), Greif (2006), North (1991), and Dixit (2004). For the role of formal institutions in promoting growth and development, see Rodrik, Subramanian, and Trebbi (2004), Acemoglu, Johnson, and Robinson (2001), and Dell (2011). For the role of trust in promoting cooperation, development, and growth, see Guiso, Sapienza, and Zingales (2006, 2008a, 2008b, 2009), Tabellini (2008, 2010), and Algan and Cahuc (2010).

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 prior assumptions they may hold on others' trustworthiness. Institutions play a role in determining the net benefits resulting 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) find 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.<sup>7</sup>

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 there is no return to honesty and trust may be crowded out.

In short, the theoretical literature discusses two countervailing effects of enforcement institutions on social norms, in which trust is crowded either 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, Sapienza, and Zingales 2008a; Tabellini 2010; Grosjean 2011). However, a possible limitation to causal identification in these studies is that historical

<sup>7</sup> This is measured through answers to a survey question known as Passengers' Dilemma, which asks about respondents' willingness to lie to the police to save a friend from jail in a hypothetical situation.

institutions were themselves the outcome of 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, Sapienza, and Zingales (2008a), Tabellini (2010), and Grosjean (2011) is based on the assumption that formal institutions are constant in a given country, so variations in trust can be attributed to culturally transmitted social norms and not to contemporaneous institutional quality. However, recent evidence from Becker, Hainz, and Woessman (2011) shows that there is less corruption in local courts and among 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 may be justified by higher institutional quality today.

Beyond exploring the effect of institutions on trust, we are also interested in how preexisting trust, or more generally culture, influences the effectiveness of institutions. Fisman and Miguel (2007), in a study of parking violations committed by diplomats stationed in New York, find 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 levels of corruption committed more parking infractions than those from less corrupt countries, but infractions were reduced dramatically once immunity was removed.<sup>8</sup>

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

One challenge to laboratory studies of the evolution of trust is 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 inherited component of trust and the implication that trust is slow to change have been supported by several empirical studies (Butler, Giuliano, and Guiso 2012; Durante 2011; Grosjean 2011; 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 residents experience violence during a civil war (Becchetti, Conzo, and Romeo 2013; Cassar, Grosjean, and Whitt 2013). Cialdini, Reno, and Kallgren (1990) show that an exogenous manipulation of perceived social norms about littering

<sup>8</sup> Similarly, Grosjean (2011) finds that a culture of violence brought to the United States by Scots-Irish and Scottish Highlander settlers in the 18th century persisted only where formal enforcement institutions were absent.

has an immediate effect on littering behavior. The broken-windows 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. Rand et al.'s (2013) social heuristics hypothesis suggests precisely that intuition can trigger internalized norms of cooperation.

### 3. Experimental Design

Each experimental session comprised four parts that were followed by a survey. These parts included an initial trust game (or investment game) in which subjects played both the part of the trustor and that of the trustee (part 1), 10 rounds (days) of trading in the market game under NoES (part 2), 10 rounds of trading under either PES or IES (part 3), and a final trust game (part 4), 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 used by 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 3 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 (in which receivers have to decide how much to send back to the sender under all possible amounts that they could have received) to prevent players from knowing anything about fellow subjects' trust and reciprocity, 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, 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. The amount sent by the sender ( $X$ ) is considered a signal of trust because larger amounts sent translate into larger amounts that the receiver has to divide. By sending larger amounts, the 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 in which she sends nothing at all. The amount sent back by the receiver is considered a measure of trustworthiness or reciprocity. If one of these four decisions (as sender or receiver in either part 1 or part 4) was randomly drawn to be the one to be

Table 1  
Payoff Matrix for Cheating in an Enforcement  
System with No Institutions

	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

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 a market game under different institutional treatments. The first 10 rounds of trading are conducted under a regime with no institutions. The second 10 rounds are conducted under regimes with either partial or impartial institutions, depending on the randomly selected treatment for that session.

#### 3.2.1. Enforcement System with No Institutions

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 during which trading partners change each day. In practice, cheating in markets happens when, for example, a buyer does not 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. Table 1 displays the parameters chosen for the baseline game.

This treatment of the market game reproduces the features of a Prisoners' Dilemma. Each individual has a private incentive to cheat. However, if everyone follows the same rationale, the exchange generates lower levels of social welfare. Maximum social welfare and efficiency (a total surplus of 40, equally split between traders) are attainable only when both parties do not cheat. Given our payoffs, we find two equilibria, [cheat, cheat] and [out, out], with payoff dominated by the former. 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 to 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.<sup>9</sup>

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 rather 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 versus impartial administration of justice.

### 3.2.2. Partial Enforcement System

In this treatment, subjects can insure themselves against being cheated on by others by buying protection against a cheating counterparty. Purchasing protection costs €5, which has to be paid regardless of whether such protection is used later. If a player buys protection and is cheated, the cheater not only loses all she has gained by cheating but also is 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 or not they will require the services of the local boss. In return, they are insured against the claims, rightful or not, of competitors and commercial partners. There is always the incentive, though, either not to pay the protection fee or to cheat in hopes that the partner is not protected. This is reflected in the payoff matrix of the game (see the online appendix).

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. In our instructions, we explain to the subjects each possible decision, presenting all four of the following possible scenarios (in addition to the option of staying out): neither subject has protection, only the subject has protection, only the partner has protection, or 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 do not 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 from an honest exchange,

<sup>9</sup> For example, the continuous double auction, which is usually run with the underlying assumption that contracts are perfectly and costlessly enforceable, always delivers 100 percent efficiency (Cassar, Friedman, and Schneider 2009).

minus the €5 payment to purchase protection, minus €3 as punishment for cheating). When both parties have protection and one cheats while the other does not, the one who does not cheat can get the contract enforced anyway, so she still earns €15, while the cheating party, as before, gets €12. Last is the case in which only one trader buys protection. The trader who buys protection and does not cheat gets €15 no matter what the partner does (€20 from an honest exchange minus the €5 payment to purchase protection). The partner receives €20 if she does not cheat or loses €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 nonprotected trader will instead earn €0 if he does not cheat or lose €3 if he 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 the online appendix.

The only pure-strategies equilibrium in 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 about the probability of being cheated will determine the specific strategy played by each subject. Therefore, we expect preexisting levels of trust to influence the outcomes of the game in the field.

### 3.2.3. Impartial Enforcement System

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 the full price plus a fine, and whoever is cheated receives the full amount minus a court fee. This treatment aims to reproduce the trade-offs faced by citizens when an impartial justice system is used: going to court is an option open to everyone but is still voluntary; it is moderately costly, but when used, it restores the outcomes of honest market exchanges.

As with 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 and whether they want to trade honestly, cheat, or stay out, before they know 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. For simplicity, we elicit this decision before the behavior of the counterparty is revealed, much like in the strategy method. Pairs in which neither side wants to take the counterparty to court face the same payoffs as in the NoES case. 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 is deducted 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 who has been cheated has selected to go to court, she earns €18 if she did not cheat (the honest-exchange payoff of €20 minus €2 for taking the counterparty to court) or €13 if she also cheated (the honest-exchange payoff of €20, minus €2 for taking the counterparty to court, minus a fine of €5 for having cheated as well). Last, when a subject decides not to go to court but 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 opts out, still yields €1. A full-payoff matrix for this game is provided in the online appendix.

In this treatment, for both partners, going to court and trading honestly is a Nash equilibrium in pure strategies. It is, however, not unique. The case in which both players stay out is also a Nash equilibrium, with 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 who 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 a probability of 1 in equilibrium under the NoES treatment, with a probability of 0 under the IES treatment, and with a probability between 0 and 1 under the PES treatment. The exogenous variation in the probability of cheating introduced in the experiment is crucial for our analysis of treatment effects on trust and trustworthiness.

It is important to notice that our choice of treatment terminology (partial or impartial) alludes only to a specific form of partiality: one based on receiving justice only after having paid an upfront payment. Outside our experimental environment, the term "partiality" in relation to a judicial system may suggest a much bigger picture of discrimination based on race, gender, and status, in addition to the one observed in Mafia-type systems of private protection. Here, we narrow the definitions of partial and impartial to the specific treatment institutions defined above. For this work, the most important discriminating feature is that in one system nobody can be excluded from receiving a just adjudication of tort, while in the other system only those who paid up front are entitled. This distinction implies that, in our experimental design, partial and impartial enforcement mechanisms rely on the difference between up-front sunk costs (conditional enforcement system) and costs that can be made contingent on being cheated (unconditional enforcement system). In our choice of study design, these are necessary features of the two enforcement systems.

A PES treatment, as the Mafia type, runs on the premise that smooth transactions can be delivered after the collection of an up-front payment (known as *il pizzo*). Protection is exclusionary, only for the people who pay. We do not think that a Mafia-style system that would require payment *ex post* is conceivable (or realistic): it is precisely the objective of this system to make sure that trades run smoothly, without cheating incidents. If they were to be paid *ex post*, members of the organization would have an interest in increasing the amount of incidents, so their business would be limited to revenge or vendetta instead (Gambetta 1996).

By contrast, an impartial court system is characterized by the impossibility of excluding anybody from accessing the enforcement system and by the absence of discretionary sunk costs. Additional individual costs are conditional on using the system. Of course, a court system also comes with sunk costs, such as paying for judges, but given the fungibility of public funds, we do not think such costs would directly enter individual decision making in the market game that we consider.<sup>10</sup>

One could conceive two other treatments—an IES with up-front cost and a PES with costs contingent on being cheated. These treatments, however, would be interesting only from a theoretical perspective.

### 3.3. *Experimental Protocol*

The games were played with paper and pencil so that we could reach our targeted subject pool in the field. Each point was equivalent to €1 at both study sites.<sup>11</sup> 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 rematched for each of the 24 decisions that they had to make.<sup>12</sup> It was stressed during the reading of the instructions that each choice that the 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 ahead of time about the nature or the sequence of the tasks. 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 end of the session. The fact that participants knew the total number of activities in the

<sup>10</sup> Another way to look at the differential effect of up-front versus conditional payments is to see it not just in connection with the partiality or impartiality of a system but also in its relation to individual risk preferences. Yet we never find risk preferences to affect our results in a significant way. This provides comforting evidence that this distinction in risk may play only a minor role.

<sup>11</sup> Despite differences in gross domestic product per capita, to recruit and incentivize subjects we needed to use the same payoffs in Kosovo as in Italy, given the high cost of living in Kosovo.

<sup>12</sup> A computer program displayed pairs of random numbers reproducing participants' identity cards, which were used to determine random matching into pairs in this as well as in the other tasks in the experiment. The program was set so that repetition of the same pairs was kept to a minimum.

experiment implies that they were aware, at the time of playing the second trust game, that they were completing 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 after the different treatments. The fact that we still find significant differences means that our results are lower bounds.

Each session was randomly assigned 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 with one line for each of the 10 trading days. Each line was divided into 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, should one of these trading days be the one selected for payment, they would incur the profits and losses that occurred 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, equaling the total number of decisions made during the experiment. The number determined the decision to be implemented for payout. This payout would then be added or subtracted to the attendance fee (€10). Final earnings were always positive. While the assistants calculated the payments, participants filled out a survey. The survey featured basic demographic and socioeconomic questions, as well as questions about beliefs and behaviors related to the social preferences and behaviors elicited during the experiment. The survey included questions about trust in institutions and people, experiences of economic exchanges, borrowing, seeking help in different situations, and 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 was 9.56 (minimum, seven; maximum, 12).<sup>13</sup> The majority of sessions had either 10 (58 percent of sessions) or eight participants (25 percent of sessions).

In Italy, subjects were recruited with the help of producers' and workers' associations in three different regions: Lombardy, Liguria, and Sicily. Each association sent to its members the invitation to participate in an economic study,

<sup>13</sup> One session had seven participants because one 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.

specifying its duration and the range of possible gains. When enough people had volunteered, the time and place for the session was agreed on. 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-hour 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, because one of the objectives of this study was to assess how the preferences and behavior we observe in 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 the experiment.

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

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

#### 4.2. Descriptive Statistics

Summary statistics reflecting the sociodemographic and economic backgrounds of our subject pool are presented in Table 2. The objective of collecting such information was to investigate potential heterogeneous effects of experimental treatments but also to check the validity of the randomized allocation procedure of the different experimental treatments. Apart from a higher proportion of students in the IES treatment (27 versus 14 percent in the PES treatment;  $t$ -statistic of 3.1) and a larger average household size in the PES treatments (4.7 versus 4.2 in the IES treatment;  $t$ -statistic of 1.9), covariates were well balanced across the experimental treatments.<sup>14</sup> Despite randomizing the assignment of treatments to sessions, initial trust and trustworthiness turned out to be significantly higher in the IES treatment than in the PES treatment. Trust was measured as the amount sent, and trustworthiness was measured as the average amount returned as a percentage of the amount sent, averaged over all the possible amounts sent (elicited using the strategy method). Subjects assigned to the PES and IES treatments sent, on average, €5.2 and €5.9 ( $t$ -statistic of 2.76) and returned, on average, 48 and 58 percent ( $t$ -statistic of 4.56), respectively. Such differences are entirely driven by Kosovo ( $t$ -statistics of 3.07 and .82 in Kosovo and Italy, respectively). They certainly represent a concern for our identification strategy, which we address by controlling for initial trust and trust-

<sup>14</sup> Separate data for Kosovo and northern and southern Italy are presented in the online appendix.

Table 2  
 Summary Statistics for the Sociodemographic and Economic Backgrounds of the Participants

	All		PES		IES		PES – IES ( <i>t</i> -Statistic)	
	N	Mean	SD	Mean	SD	Mean		SD
Amount sent in TG1 (€)	346	5.55	2.39	5.24	2.29	5.94	2.47	2.76
Amount sent in TG2 (€)	346	5.92	2.62	5.43	2.44	6.51	2.72	3.87
Amount Sent TG2 – TG1 (€)	346	.36	2.3	.2	2.09	.57	2.53	1.49
Percentage returned in TG1	346	52.70	20.23	48.37	17.99	58.06	21.58	4.56
Percentage returned in TG2	346	51.63	28.24	44.64	20.92	60.25	33.34	5.31
Percentage Returned TG2 – TG1	346	-1.11	23.39	-3.80	14.70	2.19	30.64	2.39
Probability of a partner cheating	346	3.54	2.41	4.27	2.43	2.63	2.06	-6.7
Gender (equals one if male)	346	.66	.47	.66	.48	.66	.47	.09
Age (years)	342	36.14	14.94	35.81	14.45	36.56	15.56	.46
Number of children	342	1.01	1.89	1.11	2.14	.88	1.51	-1.11
Household size	339	4.47	2.34	4.68	2.45	4.20	2.18	-1.89
Marital status:								
Married	342	.45	.50	.48	.50	.41	.49	-1.29
Separated	342	.04	.19	.05	.21	.03	.16	-1.03
Widowed	342	.01	.11	.01	.10	.01	.11	.21
Single	342	.50	.50	.46	.50	.55	.50	1.63

Employment status:													
Employee or self-employed	344	.50	.50	.54	.50	.46	.50	.46	.50	.23	.50	.46	-1.51
Student	344	.20	.40	.14	.35	.27	.35	.27	.45	.56	.45	.27	-3.1
Business owner	345	.34	.48	.34	.48	.34	.48	.34	.48	.40	.48	.34	.00
Unemployed	344	.17	.38	.20	.40	.14	.40	.14	.35	.42	.35	.14	-1.34
Inactive or other	344	.12	.33	.12	.33	.12	.33	.12	.33	.42	.33	.12	-1.11
Education level:													
Primary or secondary	344	.08	.26	.09	.29	.06	.29	.06	.23	.42	.23	.06	-1.11
High school	344	.51	.50	.49	.50	.52	.50	.52	.50	.40	.50	.52	.56
Post high school	344	.23	.42	.25	.44	.19	.44	.19	.40	.42	.40	.19	-1.33
Graduate education	344	.19	.39	.16	.37	.23	.37	.23	.42	.42	.42	.23	1.45
Household income (€ per capita)	333	54	92	59	107	48	107	48	70	70	70	48	-1.05
Socioeconomic status	341	4.61	1.85	4.66	1.73	4.55	1.73	4.55	2.01	2.01	2.01	4.55	-0.53
Risky lottery choice	344	.15	.36	.16	.36	.15	.36	.15	.36	.36	.36	.15	-1.17

**Note.** The probability of partners cheating is calculated as  $[\text{number of cheating partners}/(\text{number of partners} \times \text{trading days in treatment round})] \times 100$ . For socioeconomic status, 1 = poorest and 10 = richest. PES = partial enforcement system; IES = impartial enforcement system; TG1 = first trust game; TG2 = second trust game.

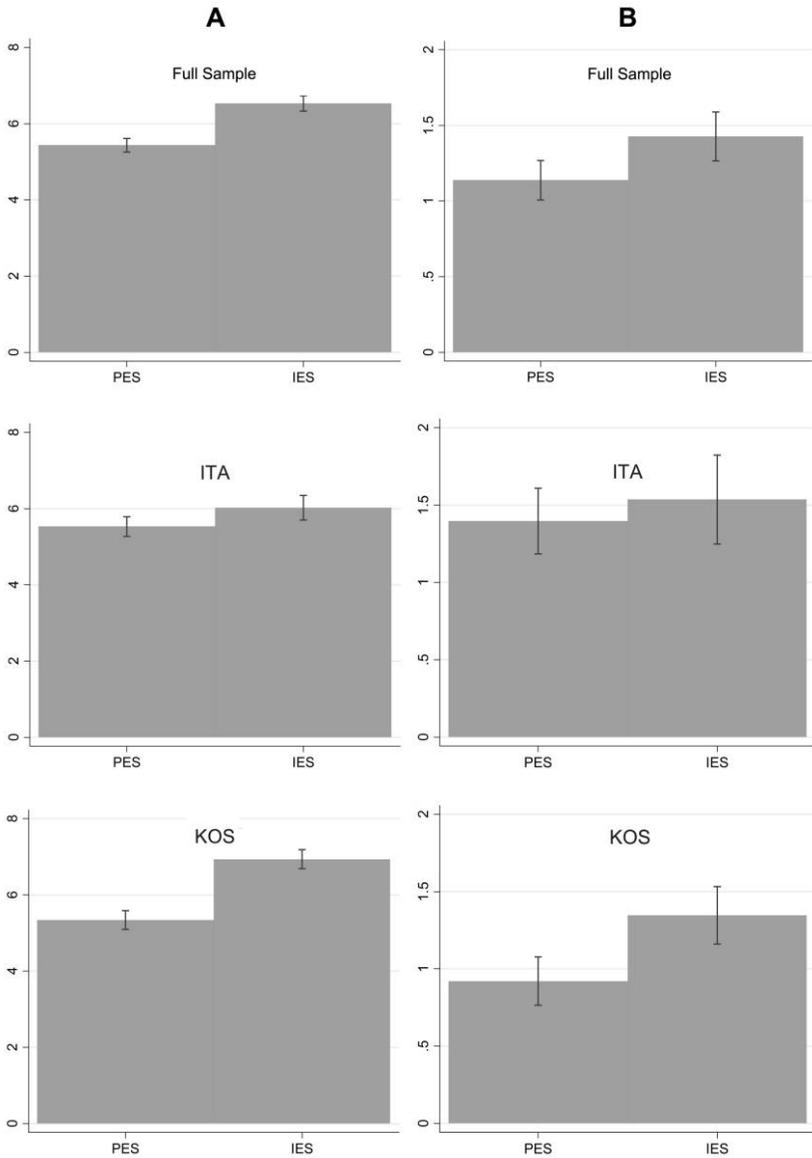
worthiness in all regressions described below and by presenting results in first differences.

Initial trust and trustworthiness differ across countries. Subjects in Kosovo and Italy sent, on average, €5.6 and €5.4 and returned, on average, 55 and 50 percent, respectively. These differences are statistically significant.<sup>15</sup> Our small sample size makes it impossible to detect significant within-country differences. In particular, subjects from southern Italy appear to be 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.

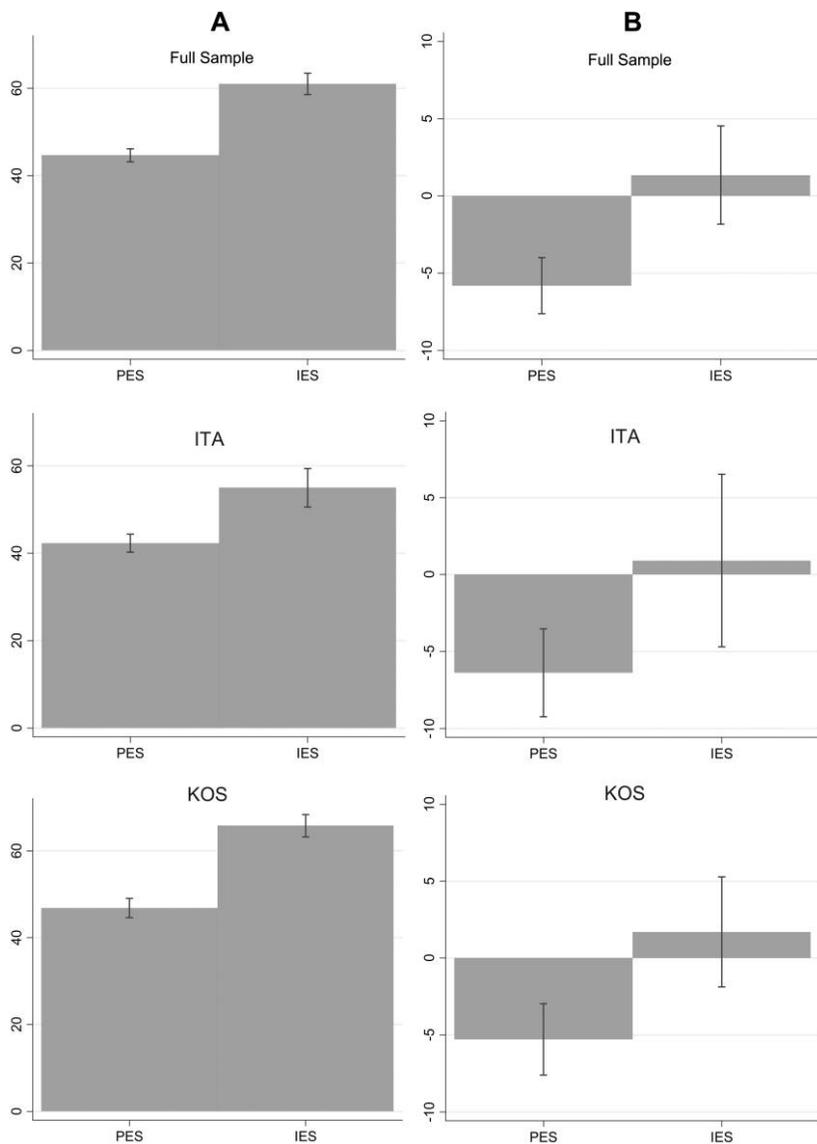
Descriptive statistics for the outcome variables are presented in Figures 2–6. Figure 2A displays the average amount sent—that is, trust in the final trust game after each of the two different institutional treatments, both for the whole sample and for each country separately; spikes represent the standard error of the mean. Figure 3 shows similar results for trustworthiness: on average, participants sent €6.5 and €5.4 and returned 60 and 45 percent of the amount received, respectively, in the games using the IES and PES treatments. The amounts sent and the percentages returned in the second trust game were higher after the IES treatment than after the PES treatment ( $t$ -statistics of 3.87 and 5.31, respectively). The differences are particularly large in Kosovo. Figure 2B presents the average individual increase in amounts sent between the two trust games, before and after the experimental institutional treatment. Similarly, Figure 3B presents the average individual difference in the percentage returned between the two trust games. Taking first differences within individuals eliminates individual heterogeneity and any departure from perfect randomization across treatments. Trust increases after both treatments, but it increases much more so in the IES treatment: the average difference in the amount sent is €6 and €2 after the IES and PES treatments, respectively. According to a simple  $t$ -test, the difference is statistically significant ( $t = 1.49$ ). For trustworthiness, the PES treatment actually led to a 3.8-percentage-point decrease in the average percentage returned, whereas the IES treatment led to a 2.2-percentage-point increase, a difference that is statistically significant according to a simple  $t$ -test ( $t = 2.39$ ). As a result, trustworthiness is greater under the IES treatment than under the PES treatment, and the difference is statistically significant overall in Italy and in Kosovo ( $t$ -statistics of 5.31, 2.75, and 5.14, respectively).

The remaining figures display the average measures (and standard errors of the mean, represented by spikes) 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 NoES, PES, and IES situations. Figures in the online appendix show the evolution of cheating, opting out, and trading profits throughout the game under the three treatments. Levels of market participation and profits are highest and cheating is least com-

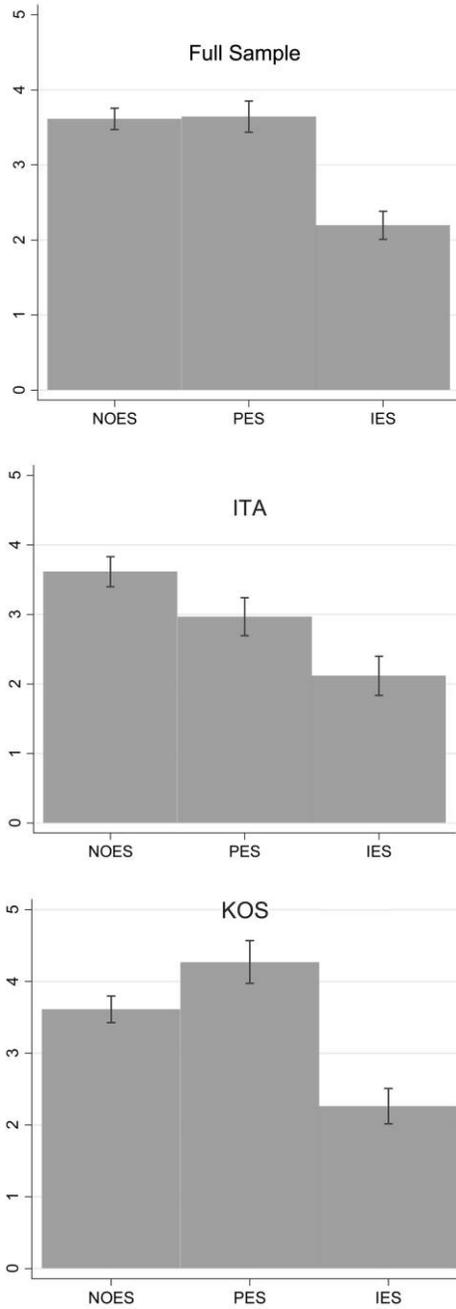
<sup>15</sup> The  $t$ -statistics are 1.67 and 2.29, respectively.



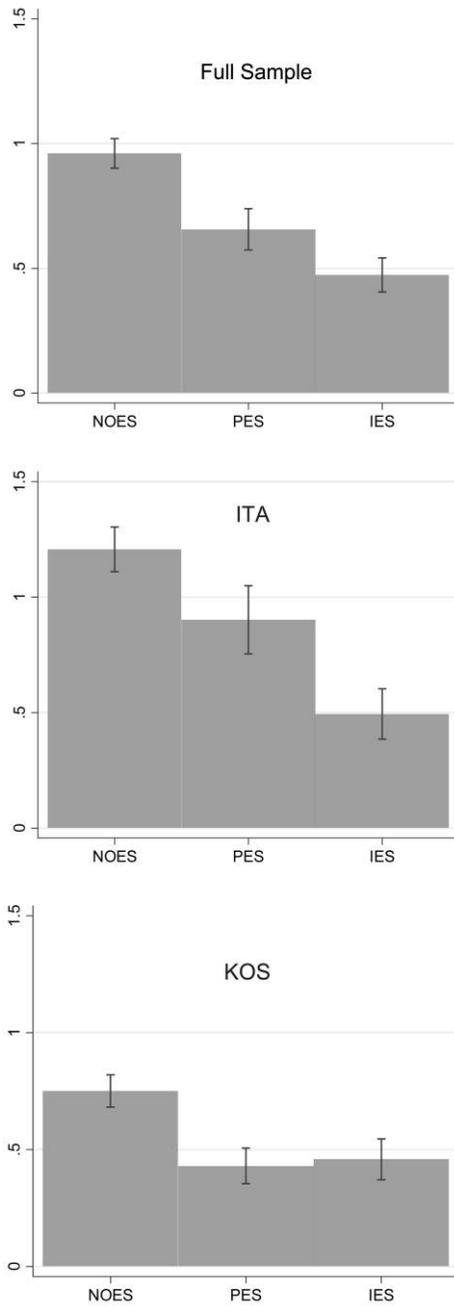
**Figure 2.** Trust across treatments: (A) behavior in trust game 2 and (B) first-difference averages.



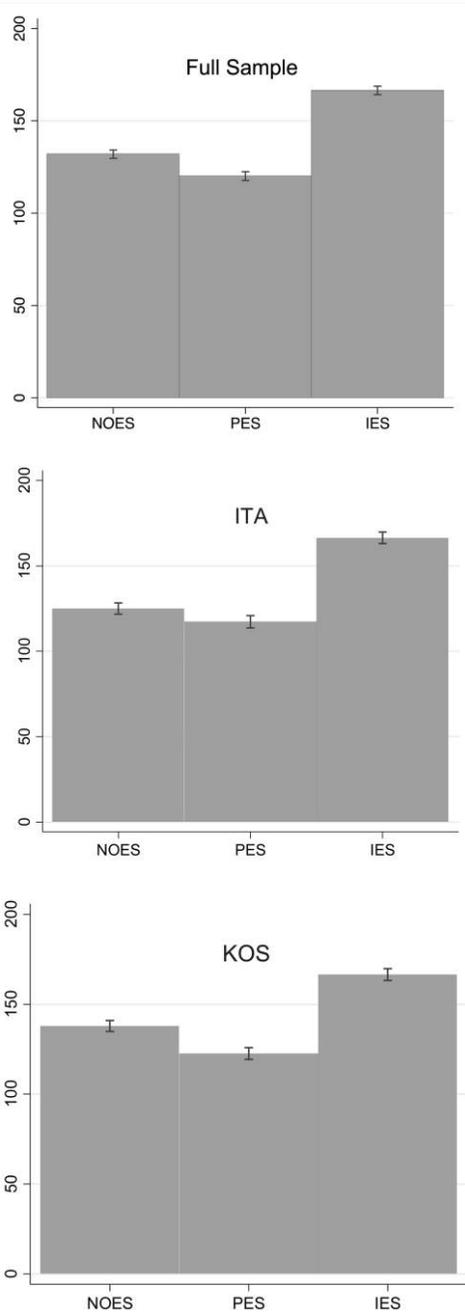
**Figure 3.** Trustworthiness across treatments: (A) behavior in trust game 2 and (B) first-difference averages.



**Figure 4.** Cheating in the trade game: averages



**Figure 5.** Opting out of trade in the trade game: averages



**Figure 6.** Trading profits in the trade game: averages

mon under the IES treatment. On average, participants opt out of trade for 1 trading day per round in the NoES treatment, .6 of a trading day per round in the PES treatment, and .4 of a trading day per round in the IES treatment. They cheat 3.61 times in 10 rounds under the NoES, 3.65 times under PES, and only 2.20 under IES. Total profits over the 10 rounds are, on average, €131, €121, and €167 in the NoES, PES, and IES treatments, respectively. A direct comparison of profit across treatments warrants caution: in PES, subjects have to prepay the fee of €5 for ensuring private protection, and this further reduces profits. The quality of contract enforcement institutions seems to have a nonmonotonic effect on cheating behavior and market efficiency. In Kosovo, cheating is actually more frequent 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 in the PES and NoES treatments, respectively. On the contrary, in Italy cheating is less frequent under PES than under NoES (2.97 and 3.62 times over the 10 rounds, respectively), yet profits remain lower. We next turn to regression analysis to test the statistical significance and robustness of these results.

## 5. The Causal Effect of Institutions on Trust

Here we test through regression analysis our first hypothesis: impartial contract enforcement institutions in markets lead to higher levels of trust and trustworthiness as moral norms (in noncontractible environments) than do partial institutions, through their effect on cooperative behavior in markets. Descriptive evidence in Section 4 indicates that our institutional treatments were successful in generating the predicted changes. Here 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 noncooperation on trust and trustworthiness with a Wald estimate.

### 5.1. Empirical Specification

Since allocation to treatment is random, the causal effect of 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 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 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 games, respectively. Dummy variable  $D$

captures the institutional treatment and takes a value of one for the IES treatment and zero for the PES treatment. The term  $C$  is a country dummy. The term  $X_i$  is a vector of individual controls, such as age, gender, marital status, education level, 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 model:

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

The term  $\beta$  is the causal effect of the treatment: it estimates the differential variation within subjects, across treatments, and in trust and trustworthiness levels between period 1, before the treatment is administered, and period 2, after the treatment is administered.<sup>16</sup>

Throughout our tables, 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 run on individual country or treatment subsamples use robust standard errors, given the lower number of clusters.

### 5.2. *Reduced-Form Results*

Regression results are presented in Table 3. Columns 1–3 display results for the pooled sample when the dependent variable is the amount sent by the first player in the final trust game (our measure of trust). Columns 7–9 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 (our measure of trustworthiness). Columns 5, 6, 11, and 12 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, the IES treatment had a positive, statistically significant, and robust effect on both the amount sent and the percentage returned in the final trust game, compared with the PES treatment. The effect of treatment on trustworthiness is further decomposed to reveal that impartial enforcement positively affects both unconditional altruism and reci-

<sup>16</sup> This model eliminates any potential unobserved heterogeneity at the individual level and any departure from perfect randomization across treatments.

Table 3  
Trust and Trustworthiness Results from Ordinary Least Squares Regressions

	Amount Sent				Percentage Returned							
	Pooled Sample		Pooled Sample		Pooled Sample		Pooled Sample					
	TG2	TG2 - TG1	TG2	TG2 - TG1	TG2	TG2 - TG1	TG2	TG2 - TG1				
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	
IES	1.08** (.28) [.34]	.64** (.24) [.25]	.71** (.26) [.25]	.37 <sup>†</sup> (.25) [.26]	.67 <sup>†</sup> (.39)	.98** (.34)	15.61** (3.08) [3.99]	8.48** (2.60) [3.41]	9.80** (3.03) [3.47]	5.99* (2.68) [3.32]	10.29 <sup>+</sup> (6.04)	9.05** (3.02)
Kosovo		.23 (.23)	.44 (.35)				3.30 (2.50)	4.58 (2.90)				
Amount sent in TG1		.62** (.05)	.60** (.06)		.50** (.11)	.68** (.06)						
Percentage returned in TG1							.75** (.07)	.73** (.07)			.71** (.15)	.75** (.08)
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 <sup>2</sup>	.04	.36	.39	.01	.30	.58	.08	.36	.39	.02	.28	.62

**Note.** Robust standard errors are in parentheses, and robust standard errors clustered at the session level (37 clusters) are in brackets. Individual controls are gender, marital status, education level, employment status, socioeconomic status (1 = poorest, 10 = richest), and risky lottery choice. All regressions include a constant term. The means for the dependent variables are as follows: for the pooled sample, 5.92 for amount sent in TG2, .364 for amount sent TG2 - TG1, 49.95 for percentage returned in TG2, and -1.114 for percentage returned TG2 - TG1; for Italy, 4.76 for amount sent in TG2 and 46.43 for percentage returned in TG2; and for Kosovo, 5.76 for amount sent in TG2 and 53.3 for percentage returned in TG2. TG1 = first trust game; TG2 = second trust game; IES = impartial enforcement system.

<sup>†</sup>  $p < .15$ .

<sup>+</sup>  $p < .1$ .

\*  $p < .05$ .

\*\*  $p < .01$ .

procity.<sup>17</sup> We also explore whether individuals' initial endowments of trust and trustworthiness are sources of heterogeneity in treatment effects, but it appears that the treatment equally influences individuals with high and low levels of initial trust and trustworthiness.<sup>18</sup> These effects are robust to the inclusion of additional controls for individual characteristics<sup>19</sup> and are also robust in Italy and in Kosovo. The effects on both trust and trustworthiness are robust and significant at the 1 percent level in Kosovo and at the 10 percent level in Italy. For each behavior in the second trust game, the trust or trustworthiness specifications displayed here control only for the corresponding behavior in the first trust game. However, all results are robust to controlling for both trust and trustworthiness behavior in the first game. Results are not only robust but also statistically and economically more significant when country fixed effects are not included.<sup>20</sup>

The effect of contract enforcement institutions on trust and trustworthiness is economically meaningful. Having traded under the IES treatment as opposed to the PES treatment leads to amounts sent that are 12–18 percent higher and percentages returned that are 20–31 percent higher, 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. When we control for individual characteristics, the coefficients on the institutional treatment are 1.6 and 2.1 times higher than the coefficient on the

<sup>17</sup> Following a suggestion by an anonymous referee, we explored the effect of treatment on trustworthiness by decomposing trustworthiness into unconditional altruism and reciprocity, to get a sense of whether the treatment enhances concern for others generally (for example, altruism) or instead makes cooperation more conditional. Exploiting the fact that participants made their choices as the receiver in the trust game using the strategy method, we first performed, for each individual, a regression of the amount returned in the first (second) trust game on the amount received. We then stored the parameters of these individual-level regressions: the constant term is a proxy of individual unconditional altruism, while the estimated coefficient on the amount received variable is a proxy of individual reciprocity. Finally, we ran regression 1 in Table B2 in the online appendix, replacing the average share returned in the first (second) trust game with our measures of altruism and reciprocity, to explore how our treatment affected these two components of the receivers' behavior in the trust game. Regression coefficients on our IES treatment indicator are positive in both regressions, which suggests that impartial enforcement has a positive effect on both unconditional altruism and reciprocity. This effect is statistically significant only for reciprocity.

<sup>18</sup> To explore heterogeneous treatment effects based on subjects' initial endowment of trust and trustworthiness, we generate a variable equal to one if a subject's level of trust (trustworthiness) in the first trust game is greater than the median value of trust (trustworthiness) displayed by participants in the experiment. We then run regression (1), adding an interaction term between our treatment variable and the high-trust (high-trustworthiness) indicator. We find that the treatment effect on trust and trustworthiness is not affected by individuals' endowment of trust and trustworthiness, respectively. The coefficient on the interaction term  $IES \times \text{high trust (high trustworthiness)}$  is generally positive (although not for Kosovo) and insignificant. The results of these regressions are available on request.

<sup>19</sup> Table B3 in the online appendix shows results from regression analysis of the individual characteristics correlated with trust and trustworthiness in the initial trust game, for the full sample and individual country samples. Our survey measure of risk aversion, based on a nonincentivized, hypothetical choice between a safe lottery and a risky lottery, is not significant except for trustworthiness in Kosovo.

<sup>20</sup> The results of these robustness specifications are available on request.

country dummy for Kosovo for trust and trustworthiness, respectively. In Italy, the effect of impartial versus partial institutions on trustworthiness is equivalent to three-fourths of the initial difference in trust 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 4 and 10 in Table 3 present the results of first-difference specifications (eq. [2]) for trust and trustworthiness, respectively. The coefficient associated with the IES treatment is still positive. It is only marginally significant for within-subject differences in the amount sent, but its significance reaches the 5 percent level for within-subject differences in the percentage returned. Results for individual country subsamples are similar to those discussed above. Assignment to the IES treatment leads to positive and statistically significant individual increases in trust and trustworthiness in the Kosovo subsample and increases in only trustworthiness in the Italy subsample.<sup>21</sup>

We also explore how long lasting the treatment effect might be by testing its sensitivity to cheating in the last round. Our analysis shows that the treatment effect on trustworthiness is insensitive to what happens in the last rounds of trading. Results of the empirical analysis are displayed in Table B5 in the online appendix. The table shows the number of times each respondent has been cheated by his or her partner in the last five rounds of treatment. On average, respondents have been cheated 2.24 times during the last five rounds. As expected, respondents in the PES treatment are cheated more often (2.68 times) than respondents in the IES treatment (1.69 times). Columns 1–3 of Table B5 reproduce columns 7–9 of Table 3, in which trustworthiness is regressed on the treatment and on a number of control variables to which the variable indicating having been cheated in the last five rounds is added. Results are unchanged. In other words, better-quality institutions lead to higher levels of trust, and the effect is robust to the most recent experiences of noncooperation. Columns 4–6 of Table B5 take this test further by adding an interaction term between the treatment and the number of times participants were cheated in the last five rounds. This enables us to test whether the treatment has a differential effect as a function of recent experiences of cheating. Again, the treatment effect is insensitive to the most recent experiences of cheating. This insensitivity suggests that the treatment effect of being exposed to good institutions may be long-lived.

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 reviewed in Section 2 and is more in line with the

<sup>21</sup> Conducting the analysis for macroregions (northern Italy, southern Italy, and Kosovo), although extremely interesting, poses the challenge of statistical power. While our results are qualitatively robust to the geographical split, regression coefficients for the two Italian macroregions generally lose significance because of the reduced number of observations. The results suggest a stronger effect of treatment in northern Italy than in southern Italy, but it is worth noting that this may be also because of the larger number of participants from the north than from the south. The results of these regressions are displayed in the online appendix.

results of Cialdini, Reno, and Kallgren (1990). Kandori (1992) and Rand et al. (2013) suggest a compelling interpretation of our results. Kandori (1992) shows that when an agent can observe only her own history of trade, cooperation can be sustained only by an equilibrium in which an experience of dishonesty incites an individual to cheat all of her future partners, which unravels community trust. In this context, better enforcement institutions can protect community trust by preventing defection. Rand et al.'s (2013) social heuristics hypothesis illustrates how experiences in one context of interactions can spill over to another. Experiences of cooperation in the market game can shape subjects' intuition, predisposing them to cooperation in the trust game. In this interpretation, good-quality institutions, by favoring cooperation over defection, foster cooperative social norms, which are internalized and predispose agents to cooperate in other environments, even when the institutional framework no longer has any power.

It should also be noted that we obtain our results in the specific context of a very small economy composed of only 8–10 players, each of whom is expected to meet all the other players at least once during the market game. Such opportunities for trade enable individuals to update prior assumptions about others rapidly. Other studies have demonstrated how trade (Maystre et al. 2014) and 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 Noncooperation

Beyond the overall effect of our treatment, we are interested in the effect of experiences of noncooperation (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, Sapienza, and Zingales (2008b) and Tabellini (2008). We use a Wald estimate to quantify the effect of noncooperation on trust and trustworthiness. We first compute the subjective probability that any other trader in the game is a non-cooperator because of the individual frequency of having met a cheating partner in the trading game relative to the number of participants in the session.<sup>22</sup> We then regress the frequency of cheating in the trading game on the institutional treatment. Results are displayed in Table 4. Consistent with our experimental design and with the descriptive evidence provided above, the IES treatment reduces the frequency of cheating by 45 percent on average (significant at the 1 percent level).<sup>23</sup> Wald estimates of the effect of a reduction in the frequency of noncooperation in the trading game on trust and trustworthiness are presented in Table 5.<sup>24</sup> On average, a 1-percentage-point reduction in the probability of noncooperation in the trading game increases amounts sent in the trust game by 7–11 percent, depending on the specification. The corresponding increases

<sup>22</sup> This probability is computed as (number of cheating partners)/(number of participants × trading days in treatment round) × 100 for each individual  $i$ .

<sup>23</sup> Results for individual country subsamples are in Table B6 in the online appendix.

<sup>24</sup> The Wald estimators are  $\hat{\beta}_{\text{Wald}} = [\hat{E}(T|\text{IES} = 1) - \hat{E}(T|\text{PES} = 1)] / [\hat{E}(\text{Cheated}|\text{IES} = 1) - \hat{E}(\text{Cheated}|\text{PES} = 1)]$ , where  $T$  is trust and trustworthiness.

Table 4  
 Results of First-Stage Regressions of the Effect of Institutions on the Probability of a Partner Cheating

	Pooled Sample			Italy		Kosovo		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IES	-1.64** (.24) [.52]	-1.62** (.27) [.52]	-1.63** (.24) [.51]	-1.58** (.27) [.51]	-.90* (.38)	-.92* (.38)	-2.34** (.41)	-2.21** (.42)
Kosovo	.53* (.24) [.53]	.52 (.36) [.57]	.50* (.24) [.54]	.54 (.37) [.57]				
Amount sent in TGI	-.03 (.05) [.06]	-.02 (.05) [.06]			.04 (.08)		.02 (.08)	
Percentage returned in TGI			.00 (.01) [.01]	-.01 (.01) [.01]		.00 (.01)		-.01 (.01)
Individual controls	No	Yes	No	Yes	Yes	Yes	Yes	Yes
Observations	346	334	347	334	165	165	169	169
R <sup>2</sup>	.13	.17	.13	.17	.16	.16	.28	.28
F-statistic for an IES	46.18	37.18	47.47	35.23	5.65	5.86	32.88	28.19

Note. Robust standard errors are in parentheses, and robust standard errors clustered at the session level (37 clusters) are in brackets. Individual controls are gender, marital status, education level, employment status, socioeconomic status (1 = poorest, 10 = richest), and risky lottery choice. All regressions include a constant term. The means for the dependent variables are as follows: 3.54 for the pooled sample, 3.27 for Italy, and 3.80 for Kosovo. IES = impartial enforcement system; TGI = first trust game.

\*  $P < .05$ .

\*\*  $P < .01$ .

Table 5  
Second-Stage Regressions (Wald Estimates) of Trust as a Function of the Probability of a Partner Cheating

	Amount Sent				Percentage Returned							
	Pooled Sample		Italy		Kosovo		Pooled Sample		Italy		Kosovo	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)				
Probability of partner cheating (instrumented by IES)	-.39* (.16) [.19]	-.44* (.17) [.21]	-.74 (.50)	-.42** (.15)	-5.14** (1.74) [2.72]	-6.19** (2.14) [3.05]	-11.24 (7.71)	-4.09** (1.52)				
Kosovo	.43* (.26) [.33]	.67* (.38) [.41]			6.03* (2.76) [5.17]	7.91* (3.77) [5.46]						
Amount sent in TG1	.61** (.06) [.06]	.59** (.06) [.06]	.52** (.11)	.69** (.07)								
Percentage returned in TG1					.74** (.08) [.08]	.70** (.08) [.08]	.73** (.19)	.72** (.09)				
Individual controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes				
Observations	346	334	165	169	346	334	165	169				
R <sup>2</sup>	.25	.27	.01	.46	.18	.16	-.26	.48				
F-statistic	46.88	123.95	2.47	231.58	40.98	88.94	113.43	120.37				

Note. Robust standard errors are in parentheses, and robust standard errors clustered at the session level (37 clusters) are in brackets. Individual controls are gender, marital status, education level, employment status, socioeconomic status (1 = poorest, 10 = richest), and risky lottery choice. All regressions include a constant term. The means for the dependent variables are as follows: for the amount sent, 5.92 for the pooled sample, 4.76 for Italy, and 5.76 for Kosovo; for the percentage returned, 49.94 for the pooled sample, 46.43 for Italy, and 53.3 for Kosovo. IES = impartial enforcement system; TG1 = first trust game.

+  $p < .1$ .  
\*  $p < .05$ .  
\*\*  $p < .01$ .

in trustworthiness are 13–19 percent. Again, all effects reported here are robust to controlling for trust and trustworthiness behavior in the first trust game and are robust and statistically more significant when omitting all controls, including country fixed effects.<sup>25</sup>

These estimates can be interpreted as instrumental variables estimates only if the reduction in noncooperation 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, on subjects' own behavior, or through the combination of other-regarding preferences and other people's earnings in the session. Variations in the volume of trade are not consistently associated with variations in trust or trustworthiness. The higher level of profits accruing to subjects under the IES treatment does not significantly correlate with final levels of trust and trustworthiness. We find no evidence that other-regarding preferences or aversion to inequality play any significant role in the variation in trust and trustworthiness.<sup>26</sup> Since we estimate within-subject variation in trust and trustworthiness, we can also rule out that the observed effect is due to any individual idiosyncrasy or to individual's own behavior either in the first trust game or from the market game dictating behavior in the final trust game. Overall, we do not find any evidence that channels other than noncooperation are at play, but we cannot entirely rule out the possibility that other channels—for example, unintended and undetected framing in the experiment—could be at play.

## 6. The Interaction between Culture and Enforcement Institutions

### 6.1. Empirical Specification

The second hypothesis that we empirically test deals with the relationship between preexisting culture, particularly initial trust and trustworthiness, and behaviors in the market under the different enforcement institutions. We estimate the relationship

$$\text{MBehav}_{it} = \alpha + \beta D + \delta T_{it} + \vartheta C + \phi X_i + \gamma_t + \gamma_i + \varepsilon_{it}, \quad (3)$$

where  $\text{MBehav}_{it}$  captures individual market behavioral outcomes for individual  $i$  (cheating, participation, and trading profit) on day  $t = [1, 10]$  of trading. The term  $D = \{\text{NoES, PES, IES}\}$  is the experimental treatment, and  $C$  denotes country fixed effects. The term  $T_{it}$  denotes the behavior of agent  $i$  in the first trust game. The term  $\gamma_i$  is an individual effect, and  $\gamma_t$  is a vector of variables for each trading day (time fixed effect). The error term is  $\varepsilon_{it}$ . We estimate this model in the pooled sample as well as in the different treatment subsamples 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,

<sup>25</sup> Results for individual country subsamples are in Table B7 in the online appendix.

<sup>26</sup> The results of specifications supporting the discussion here are available on request.

even before the trading game instructions are administered, we can use behavioral estimates of trust from the first trust game as measures of preexisting 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 level, income, employment status, and risk aversion in  $X_i$ . The term  $\delta$  should still be interpreted only 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 preexisting culture, namely, initial trust and trustworthiness, in market behavior and market efficiency are displayed in Tables 6 and 7. All models are estimated with random effects.<sup>27</sup> Cheating behavior is strongly curtailed in the IES treatment, while participation is increased. Rates of cheating are lower and participation higher in the PES treatment as well, compared with the baseline of the NoES treatment. In the pooled regressions, initial trust and trustworthiness are negatively associated with cheating (significant at the 1 percent 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 complicated picture.

Regressions for the NoES, PES, and IES treatments in Tables 6 and 7 estimate the effect of initial trust or initial trustworthiness on each dependent variable for each of the three treatments. Trust and trustworthiness deter cheating, but only in the absence of impartial institutions. Under the IES treatment, neither trust nor trustworthiness has any influence on cheating behavior. The interpretation is that when contract enforcement institutions are present, economic incentives have a salient effect on cheating behavior, akin to what has been described by Fisman and Miguel (2007), Bohnet, Frey, and Huck (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 a market participation decision in the IES treatment. 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. Initial trust is associated with more participation in the PES treatment but with lower participation in the NoES treatment, although the

<sup>27</sup> A series of Hausman specification tests cannot reject the hypothesis that individual effects are adequately modeled by random effects. The values of the Hausman statistics for the basic specification in the pooled sample (in Tables 6 and 7) are 7.38, 1.12, and .05, respectively, when the dependent variable is, respectively, cheat, stay out of trading, and profit.

Table 6  
Generalized Least Squares Random Effect Panel Estimation of Market Game Results: Initial Trust

	Cheat				Out				Profit			
	Pooled Sample	NoES	PES	IES	Pooled Sample	NoES	PES	IES	Pooled Sample	NoES	PES	IES
PES	-.02 (.02) [.02]				-.03** (.01) [.01]				-1.04** (.27) [.28]			
IES	-.11** (.02) [.02]				-.05** (.01) [.01]				3.34** (.36) [.33]			
Amount in sent TG1	-.02** (.01) [.00]	-.02** (.01)	-.02** (.01)	-.01 (.01)	-.00 (.00) [.00]	-.01** (.00)	.01+ (.00)	.00 (.00)	-.07 (.07) [.05]	-.04 (.08)	-.04 (.11)	-.15 (.11)
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
Subjects	334	334	186	148	334	334	186	148	334	334	186	148
Wald $\chi^2$ value	1,622	1,764	1,029	162	361.1	3,062	105.6	62.29	380.5	1,493	115.2	52.15

**Note.** Robust standard errors are in parentheses, and robust standard errors clustered at the session level (37 clusters) are in brackets. Individual controls are gender, marital status, education level, employment status, and socioeconomic status (1 = poorest, 10 = richest). Trustworthiness is calculated as the amount returned as a percentage, divided by 100 (that is, between 0 and 1). All regressions include a constant term. All regressions include country dummies, individual controls, and trading day dummies. NoES = enforcement system with no institutions; PES = partial enforcement system; IES = impartial enforcement system; TG1 = first trust game.

\*  $p < .1$ .

\*\*  $p < .01$ .

Table 7  
Generalized Least Squares Random Effect Panel Estimation of Market Game Results: Initial Trustworthiness

	Cheat			Out			Profit			
	Pooled Sample	NoES	PES	Pooled Sample	NoES	PES	Pooled Sample	NoES	PES	IES
PES	-.02 (.02) [.02]			-.03** (.01) [.01]			-1.03** (.27) [.28]			
IES	-.11** (.02) [.02]			-.05** (.01) [.01]			3.33** (.36) [.33]			
Percentage returned from TG1	-.20** (.07) [.06]	-.20** (.07) [.07]	-.30** (.10) [.12]	-.02 (.02) [.03]	-.04 (.03) [.03]	-.05 (.04) [.04]	-.17 (.73) [.70]	-.62 (.83) [.85]	.84 (1.35) [1.34]	.01 (1.08) [1.19]
Observations	6,679	3,339	1,860	6,680	3,340	1,860	6,679	3,339	1,860	1,480
Subjects	334	334	186	334	334	186	334	334	186	148
Wald $\chi^2$ value	1,713	3,471	841.2	360.7	3,102	103.2	379.5	1,654	116.3	51.82

**Note.** Robust standard errors are in parentheses, and robust standard errors clustered at the session level (37 clusters) are in brackets. Individual controls are gender, marital status, education level, employment status, and socioeconomic status (1 = poorest, 10 = richest). Trustworthiness is calculated as the amount returned as a percentage, divided by 100 (that is, between 0 and 1). All regressions include a constant term. All regressions include country dummies, individual controls, and trading day dummies. NoES = enforcement system with no institutions; PES = partial enforcement system; IES = impartial enforcement system; TG1 = first trust game.

\*\*  $p < .01$ .

latter effect is only marginally significant (Table 7). 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 8, we provide results of specifications in which culture is proxied by participants' region of birth instead of by initial behavior in the trust game. We reach similar conclusions. Culture is an important determinant of both opportunistic behavior and market participation decisions, but only in the absence of impartial institutions. Cheating and opting out of trading are more prevalent under the NoES treatment in southern Italy. As a result, profits are much lower in southern Italy under the NoES and PES treatments. Cheating and opting out of trading are more prevalent under the PES than under the NoES or IES treatments in Kosovo. However, subjects display no significant differences in cheating behavior, opting out, or profits under the IES treatment, regardless of their region.

## 7. Conclusion and Policy Implications

We designed a framed field experiment both to identify the causal effects of legal institutions on trust and trustworthiness and to shed light on how formal institutional quality and cultural traits interact to sustain market exchange.<sup>28</sup> 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 by reducing the frequency with which subjects face opportunistic agents when trading. This reduction enables individuals to revise upward their beliefs about other people's generalized trustworthiness, and it results in higher levels of trust. Even more striking, agents not only trust more but also reciprocate by being more trustworthy. This is important because generalized norms of trust and trustworthiness play a crucial role in supporting exchange and cooperation when contracts are incomplete or not easily enforceable. This finding contributes to the literature that roots trust in the proper functioning of impartial institutions, provides empirical support to models such as those of Guiso, Sapienza, and Zingales (2008b) and Tabellini (2008), and complements existing nonexperimental empirical evidence. Our controlled experiment not only establishes a causal link between formal institutions and culture by 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 versus impartiality. Our empirical analysis quantifies the effect of impartial en-

<sup>28</sup> Ethical reasons would prevent a study of this kind to be implemented as a randomized control trial in the field. In economics, a long tradition of analyzing the effects of different market institutions via controlled laboratory experiments provides support to the idea that subjects who are given real incentives and face real choices, as they did in this setting, would exhibit behavior indicative of the real behavior they would exhibit outside the laboratory (Smith 1994; Roth 2002).

Table 8  
Generalized Least Squares Random Effect Panel Estimation of Market Game Results: Regional Origins

	Cheat			Out			Profit			
	Pooled Sample	NoES	PES	Pooled Sample	NoES	PES	Pooled Sample	NoES	PES	IES
PES	-.02 (.02) [.02]			-.03** (.01) [.01]			-1.02** (.27) [.28]			
IES	-.11** (.02) [.02]			-.05** (.01) [.01]			3.32** (.36) [.34]			
Sicily	.10** (.04) [.04]	.13** (.04)	.08 (.05)	.02+ (.02) [.01]	.03* (.01)	.01 (.03)	-1.56** (.46) [.48]	-2.11** (.59)	-1.27+ (.69)	-.25 (.71)
Kosovo	.04 (.04) [.04]	-.01 (.05)	.16** (.06)	-.02 (.02)	-.02 (.02)	-.06* (.02)	-.36 (.48) [.59]	-.64 (.76)	.34 (.66)	-.14 (.74)
Observations	6,679	3,339	1,860	6,680	3,340	1,860	6,679	3,339	1,860	1,480
Subjects	334	334	186	334	334	186	334	334	186	148
Wald $\chi^2$ value	1,695	2,677	921.1	360.8	6,816	104.5	60.86	401.8	121.6	52.12

**Note.** Robust standard errors are in parentheses, and robust standard errors clustered at the session level (37 clusters) are in brackets. The excluded regional category is northern Italy. Individual controls are age, gender, marital status, education level, income, and employment status. All regressions include a constant term. All regressions include individual controls and trading day dummies. NoES = enforcement system with no institutions; PES = partial enforcement system; IES = impartial enforcement system.

+  $p < .1$ .  
\*  $p < .05$ .  
\*\*  $p < .01$ .

forcement institutions on trust through their influence on cooperation in a contractible environment.

Another noteworthy finding is that preexisting 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 2014) and suggests that cultural norms substitute for formal enforcement when the latter is weak.

Our study offers practical contributions for the reform of governance institutions and, in particular, contributes to the debate over the contribution of informal institutions to public order and efficiency. Our study indicates, 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 southern 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 future research that will cast light on the issue of the endogenous evolution of institutions and how culture and preexisting norms may affect it.

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