Experimental Markets with Frictions

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Abstract

Decentralized and impersonal exchange is fundamental to contemporary economies, where many interactions take place among individuals with low levels of information about their counterpart. We review the experimental literature about markets with frictions, where strangers interact in pairs formed at random in economies of indefinite duration. We focus on the impact of communication on the efficiency of the outcome and report results of a new experiment.

Keywords: coordination, cheap-talk, deception, indefinitely repeated game, social norms.

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1 Introduction

A large portion of economic discussions in academic and policy circles revolve around dissecting the pros and cons of different market outcomes without considering in great detail how those outcomes can be achieved, if they can be achieved at all. “The market” is simply treated as a black box: if any gains from trade exist, then such gains are instantaneously and fully exploited through effortless interactions guided by Adam Smith’s invisible hand.

But economic interactions rarely fit this idealized view: exploiting gains from trade is far from being a smooth process. Laboratory experiments have provided a valuable tool to study economic interactions and the process of exchange in the presence of “frictions.” This paper surveys recent experimental work on markets with frictions.

There is a variety of reasons why exploiting gains from trade may not be easy. In traditional economies based on community-type interactions, economic exchanges occur within a stable social circle, which allows community members to realize all potential gains without great difficulties. Common resources such as pastures, forests, fisheries, irrigation systems have been traditionally managed based on a repeated interaction among a defined group of well-known community members (Ostrom et al., 2000, Casari, 2007). In contrast, in developed economies many transactions take the form of *impersonal* exchanges. By this, we mean exchanges that are decentralized and—unlike personal exchange—do not involve high levels of information about others’ past behavior (Granovetter, 1985, North,1990, Seabright, 2004).

Impersonal exchange is central to economic development because it offers an important advantage over personal exchange: it expands the set of potential economic interactions, which in turn allows a society to benefit from specialization and trade. But shifting exchange from a personal to an impersonal sphere of interaction also carries a disadvantage: it does away with the reciprocity mechanisms that are available when individuals engage in recurrent relationships within a small group of well-known counterparties—such as the circle of usual trading partners or the members of a clan, for instance. This inability to trigger reciprocal mechanisms magnifies the incentives for opportunistic behavior, especially when (i) it is possible to break promises, (ii) there is lack of information about past behaviors of others, (iii) there are difficulties in communicating or in coordinating trade, or when (iv) enforcement and punishment institutions are poorly functioning. Any of these features contributes to create barriers to trade, for example by exacerbating lack of trust in others; see Ostrom (2010). We will use the word "frictions" to denote any collection of impediments to exchange—such as the aforementioned ones. If frictions cannot be overcome, then the additional opportunities for mutual gain offered by impersonal exchange may remain out of reach.

Over the course of history, societies have developed a whole host of institutions both formal—regulatory, enforcement, legal—and informal—such as network formations, reputation building, and social customs—specifically designed to support decentralized economic exchanges that would otherwise be unattainable. For example, the steady economic expansion during the commercial revolution in Medieval Europe from the XIII century on witnessed the development of an array of formal and informal institutions aimed at capturing new exchange opportunities. This fundamental shift in the quantity and scope of trade relied on the creation of the Merchant Law, the design of banking and financial instruments in Venice and Genoa, the expansion of monetary exchange, the creation of trading organizations such as the Hanseatic League. This process took centuries (Spufford 2003, Greif 2006).

The economic literature has adopted a variety of models to characterize market frictions, and understand their impact on economic efficiency. Frictions can be found in many economic models, going from models of informational asymmetries (Akerlof, 1970) to models of sticky prices (Calvo, 1983), from models of limited communication (Townsend, 1987) to life-cycle models (Samuelson, 1958). In the following section we review a basic framework, which is based on random, pairwise trade meetings. Such focus is motivated by the fact that models built around random and pairwise trade meetings characterize a class of theoretical environments that encompass a large segment of research both in microeconomics and in macroeconomics. The paper then proceeds as follows: Section 3 presents a literature review of the recent experimental work in this area. Section 4 presents a new experiment on the role of communication in markets with frictions. Section 5 concludes.

2 Modeling decentralized frictional markets

In this paper we focus on a class of environments that make frictions explicit by adopting three basic features:

(i) The economy has a fixed population and an infinite horizon, with trade interactions taking place over a sequence of periods;

(ii) Trade is decentralized in the sense that every interaction is anonymous, whereby in each period individuals are randomly paired to play a game;

(iii) There are severe communication, informational, enforcement and punishment limitations.

Models with such features have been adopted in microeconomics, to study economic governance (Dixit, 2003), the organization of commerce (Milgrom et al., 1990), and social norms (Kandori, 1992; Ellison, 1994). Random-matching models are also used in the macroeconomics and finance literatures to study unemployment (Diamond, 1982; Mortensen and Pissarides, 1994), monetary exchange (Kiyotaki and Wright, 1989), and over-the-counter markets (Duffie et al., 2005).

One of the central features of these theoretical models of frictional economies is that reciprocal mechanisms of exchange are unavailable; consequently, agents must find other ways to ensure that the opportunities for mutual gain offered by impersonal exchange do not remain out of reach. Another important feature of these economies is that they generally admit multiple equilibria, possibly including one implementing the efficient outcome. The basic idea is formalized in Kandori (1992) and Ellison (1994), who study cooperation in prisoners' dilemma games that are embedded in a random matching model; they demonstrate how groups of self-regarding, patient individuals can overcome short-run temptations to deviate from the efficient outcome even in the presence of severe communication, informational, enforcement and punishment limitations. Full cooperation can be maintained if *all* members of the economy participate in punishing a deviation by defecting forever. This community punishment scheme is decentralized.

To be sure, such a social norm of behavior requires a great deal of coordination. Yet, the theoretical literature does not discuss how agents may end up coordinating on a strategy, and in particular on a common punishment scheme. In fact, the literature often assumes, implicitly or explicitly, that a population coordinates on the best equilibrium, including the efficient outcome if available (e.g. Milgrom et al. 1990, Aliprantis et al. 2007). This assumption has far from trivial policy implications, hence it is meaningful to look deeper into the empirical validity of such theoretical view. In fact, one would imagine that economy-wide coordination on a strategy may be very challenging when the number of agents is large and communication is constrained.

Several questions arise: how, in practice, do groups of individuals reach efficient outcomes when theoretically feasible in markets with frictions? What strategies do they adopt to sustain it? What institutions promote efficient outcomes and can these institutions emerge endogenously? And how does the size of a group impact outcomes and strategy adoption?

A flurry of experimental studies has been recently conducted to provide an answer to some of these questions. In this paper we will focus on *indefinitely* repeated games with random matching, a design that can be seen as the closest empirical counterpart of the above-described theoretical economies. In such experimental markets, trading is modeled as a social dilemma so that maximum welfare corresponds to maximum cooperation. A group of subjects face an indefinite sequence of opportunities to cooperate (= trade). In each period, subjects randomly encounter an anonymous trade counterpart. Participants cannot rely on relational contracting because in such experimental economies they cannot identify others and do not know their histories of actions. For all these reasons, following the terminology proposed in Camera and Casari (2009), we will say that interaction takes place among *strangers*. Such a design captures the gist of interactions taking place in societies where, due to a variety of reasons, people may not know each other and may not trust each other (e.g., consider the role played by globalization and technology).

The section that follows presents a methodological overview of recent experiments with indefinite repetition and random matching. Then, we survey experimental studies that look at some of the institutions that can reduce the negative impact of frictions on cooperation: monitoring, personal punishment, monetary exchange, and communication. Finally, we will report results of an experiment specifically designed to assess whether and how different types of communication can help to sustain efficient, cooperative outcomes in frictional markets populated by strangers.

3 A review of recent experimental work on markets with frictions

One of the most important traits of models of markets with frictions is that the economy has an infinite horizon. Clearly, experiments cannot have an infinite duration, and generally, sessions do not last more than a few hours and subjects receive their payment at the end, so it does not matter to which specific period of the experiment the payment refers to.

Experimentalists have followed different approaches to implement models of infinite duration in the lab. A first approach is to have a pre-set–and long "enough"–number of repetitions that is publicly announced. Generally, an end-game effect emerges in the last periods, hence those are dropped from the analysis (Selten and Stoeker, 1986; Normann and Wallace, 2012). This approach, however, does not allow a direct application of Folk Theorem-type results because there is a deterministic, publicly known end-period.

A second approach consists in having a pre-determined duration, hidden from participants (e.g., Wilson and Sell, 1997; Stahl, 2009). Making the ending unknown allows avoiding the insurgence of an end-game effect. In this case, participants do not know for sure whether or not the current period is the last one. The experimenter, however, loses control over subjects' beliefs on the continuation probability, because beliefs are unobserved, and may vary across periods and subjects.

A third approach is a continuation rule following an explicit random process (Roth and Murnighan, 1978). At the end of each period, a random draw determines whether the supergame ends or continues for one additional period. The continuation probability is usually constant: a supergame that has reached period t continues into period t+1 with a fixed probability δ. If subjects are risk neutral, one can interpret δ as the discount factor. The expected duration of a supergame is $1/(1-\delta)$ periods, and in each period the supergame is expected to go on for $\delta/(1-\delta)$ additional periods (e.g. Engle-Warnick and Slonim, 2006, Davis et al., 2011). One way to give subjects a stronger feeling of an indefinite horizon is to let them interact sporadically over several weeks through an internet experiment (Wright, 2010).

A fourth approach is to have a two-period game, where period one is the stage game, and the second and last period represents a reduced-form continuation game (Angelova et al, forth.; Cooper and Kuhn, 2011). In period two subjects play a coordination game where payoffs correspond to the net present value of the stream of payoffs of the indefinitely repeated continuation game. Despite the finite horizon of interaction, this set-up admits multiple equilibria (Benoit and Krishna, 1985). In this manner, subjects can deter defections in the first period by threatening to choose an action corresponding to the inefficient equilibrium in the second period. This approach drastically reduces the set of available strategies, excluding for instance tit-for-tat and t-period punishment strategies.

A fifth approach is to monotonically shrink payoffs across periods, in order to simulate discounting (Fréchette et al. 2003; Cabral et al., 2012). To ensure that sessions have a finite duration, this last approach has to be combined with one of the previous four.

A second important characteristic of models of frictional markets is that interactions are decentralized. In this regard, a crucial variable is thus the size of the economy, which can impact on the frequency with which the same subjects interact together. Most experiments on repeated games study two-person economies (e.g., Roth and Murnighan, 1978; Murnighan and Roth, 1983; Palfrey and Rosenthal, 1994; Dal Bo, 2005; Aoyagi and Fréchette, 2009). The two subjects who populate the economy form a fixed pair and interact together in every period. Hence, the interaction is not anonymous, and individual histories are known.

By contrast, larger economies (N>2) exhibit a variety of anonymity and monitoring levels. If in all periods a subject interacts with everyone in his economy, then we say that interaction takes place among "partners" here, even if there are more than two persons there is no anonymity, because a subject knows for sure his counterpart in each period (e.g., Cason and Mui, 2010). The focus of the present study, instead, is on economies in which interaction is among strangers. Here, agents do not regularly meet the same counterpart and do not know if this happens; in each period, agents are randomly matched with each other (Schwartz et al., 2000, Duffy and Ochs, 2009, Duffy et al., forthcoming, Camera and Casari 2009, 2010; Camera et al. 2012). Strangers can be anonymous or not, depending on whether one can recognize someone previously encountered.

The literature has considered three basic architectures for strangers' economies: circle economies, turnpike economies and random matching economies. In a circle economy, agents are located on a circle, and the probability of an encounter between two agents declines with their distance (Dixit, 2003). In a turnpike environment, agents are arrayed along two parallel lines moving in opposite direction; hence agents who currently are in a match cannot meet at any future date (Townsend, 1980). With random matching, agents in an economy have the same probability of encountering each other, regardless of their distance (Diamond, 1982).

Strangers may observe different amounts of information about the choice of others in the economy. With *public monitoring*, subjects share the same information about the outcomes realized in the economy; for instance, they all observe a summary of all outcomes in the economy (Camera and Casari, 2009). With *private monitoring*, instead, subjects do not share the same information about the outcomes realized in the economy, because they cannot see outcomes in meetings other than their own (Schwartz et al., 2000; Duffy and Ochs, 2009; Camera and Casari, 2009, 2010, Camera et al., 2012). Information may also be *imperfect*, as when the counterpart's actions of are not directly observable (Aoyagi and Fréchette, 2009). Degrees of anonymity and of monitoring are two independent dimensions; hence we can have four types of situations.

There are Folk Theorem-type results proving that—even under these weaker informational conditions—agents can support equilibria with high levels of cooperation, provided that agents are sufficiently patient. The key to do so is to prevent defections by means of sanctioning schemes based on *community enforcement*; below, we explain how this can be done.

The folk theorems for two-person economies (Friedman, 1971) are easily extended to economies of strangers with *perfect* public monitoring; in such a case an agent can react to others' behavior as if facing a single counterpart. With *public monitoring*, a given outcome can be supported as an equilibrium with the same discount factor as in the standard Folk-theorem. Moreover, if public monitoring is not anonymous, then the strategy set is richer, because it includes strategies conditional on individual histories.

Folk theorem-type results exist also for environments with *private monitoring*, and where agents are anonymous. The results have been derived in the path-breaking works of Kandori (1992) and Ellison (1994), who consider economies with $N>2$ agents who play a prisoner's dilemma in pairs, know only the outcomes of their interactions and ignore the opponent's identity. If agents are sufficiently patient, then the efficient outcome can be supported as a sequential equilibrium. The equilibrium strategy considered is for an agent to start cooperating and to keep cooperating until an opponent defects. At that point, the agent should switch to "defect forever." This implies adoption of a form of "collective punishment:" everyone starts punishing everyone else after just one deviation. This *grim trigger strategy* can be similarly used in economies with public monitoring, but it works at a different speed. When agents cannot identify the defector and target him for punishment, they must resort to punish everyone, as a community. With public monitoring, information about a defection is instantaneously provided to the whole economy; hence punishment can involve everyone starting from the period immediately following a defection. With private monitoring, instead, punishment gradually spreads in the economy: the information about the initial defection spreads by contagion, meeting by meeting, until it eventually reaches everyone. To deter opportunistic behavior, agents must adopt a strategy that threatens a punishment sufficiently severe to lower the continuation payoff following a defection. If everyone adopts such a norm of community punishment, then this substantially drives down continuation payoffs, but it requires a higher discount factor than in an economy with public monitoring.

The theoretical predictions of these folk theorems for matching models have been recently tested in the lab. In an experiment Duffy and Ochs (2009) compared a fixed pairing (partner) versus a random pairing treatment (strangers). In the random pairing there was private monitoring and the conditions were set in a way that full cooperation was an equilibrium outcome. The study finds a remarkably higher aggregate cooperation in fixed than in random pairing, hence raising the point that, despite the theoretical viability of supporting cooperative equilibria with random pairing and private monitoring, this is something that empirically is difficult to attain. Duffy and Ochs (2009) employed an identical stopping probability in all treatments, which favored cooperation in fixed pairing because the expected number of encounters with the same counterpart is lower under random pairing treatment; i.e., the critical discount factor above which full cooperation can be sustained if subjects are risk neutral was lower under fixed pairings.

Camera and Casari (2009) studied in the lab the impact of private vs. public monitoring while keeping constant the expected number of future encounters in all treatments. They report that public monitoring promotes cooperation only when it is non-anonymous: high cooperation levels emerge in situations where subjects know identities and histories of opponents, but they are similar to private monitoring when subjects see aggregate outcomes without observing identities. These findings suggest that it is easier to coordinate on strategies conditioning on individual histories rather than on the grim trigger strategy. Additional support for this finding and the importance of reputational-based strategies comes also from the trust game experiment in Duffy et al. (forthcoming), and the experiment on color-coded reputation in Stahl (2009).

It is common practice in experimental economics to repeatedly expose subjects to the same decisional situation, i.e., to have several supergames. This allows the experimenter to study learning and other meta-supergames effects. In this respect, a variety of ways to partition subjects have been adopted, with different levels of potential spill-over across supergames. One protocol rules out that anyone may share a common past partner (Dal Bó, 2005). This procedure ensures that the decisions one subject made in one supergame could not affect, in any way, the decisions of subjects he or she would meet in the future. Aliprantis et al. (2006) call it "anonymous" matching. Another protocol is "absolute strangers" (Camera and Casari, 2009, 2010, Camera et al., 2012). This procedure partitions the economy's participants in such a way that no two subjects are in the same economy for more than one supergame. Subjects may have shared a common past partner in supergames three or later. A third possible protocol is to populate all economies with the same subjects, across supergames (Schwartz et al., 2000, Duffy and Ochs, 2009). There are other protocols in between absolute strangers and identical economies. In Aoyagi and Fréchette (2009) each agent plays K>10 supergames: in the first 10 they partition agents in fixed pairings, and in the remaining K-10 supergames they randomly rematch participants.

3.1 Peer punishment

Limitations in punishment are a key friction that prevents market exchange from achieving efficiency. In the previous section we have focused on decentralized punishment mechanisms that involve and extend to the entire group (community enforcement, or group punishment). Here, instead, we focus on mechanisms that offer the option to punish specific individuals, to which we refer as *peer* (or personal) *punishment* as a way to differentiate such mechanisms from punishment schemes that indiscriminately target the entire community. Note also that, since peer punishment can be centralized or decentralized (e.g., Kosfeld et al, 2009), here we focus on decentralized punishment mechanisms which is the version favored by almost all experimentalists.

There is a burgeoning literature on peer punishment, which is overwhelmingly focused on finitely repeated social dilemmas among partners (fixed matching) following the pioneering study in Ostrom, et al. (1992). In such a setting, subjects have shown a surprising tendency to engage in costly personal punishment of others, especially defectors. Though this behavior is inconsistent with personal income maximization, it has been shown to be remarkably robust. Under finite time horizons, efficiency with peer punishment depends on two factors: the fine-to-fee ratio (Nikiforakis and Normann, 2008) and time horizon's length (Gaechter et al., 2010).

Very few studies consider peer punishment in an indefinitely repeated setting; Dreber et al. (2008) study interactions among partners, and show that that the option of costly punishment increases the amount of cooperation but not the average payoff of the group. Camera and Casari (2009) study punishment in strangers' economies of indefinite duration. Every participant played a supergame of indefinite duration in a group of four subjects. In every period, the group was randomly partitioned into two pairs of subjects and every pair played a prisoner's dilemma. The interaction was anonymous and subjects could only observe actions and outcomes in their own pair. Hence, though each group interacted repeatedly, this design made it impossible for a single participant to build a reputation. In a treatment, they introduced the possibility to adopt peer punishment. This amounted to adding a costly opportunity to immediately respond to a counterpart's action by lowering her payoff. Cooperators and defectors alike could be punished. This design is useful to isolate possible elements or economic institutions that can facilitate selecting the cooperative equilibrium through community enforcement. In particular, it allows studying if and how subjects use peer punishment to complement or to substitute for *informal* sanctioning schemes that rely on future defections. A main finding is that costly peer punishment significantly promotes cooperation and efficiency.

Personal punishment seems to boost cooperation only in small part by deterring defections and in large part by avoiding that cooperators switch to defection after punishing. This finding is interesting as the literature emphasizes the former aspect, though recent studies on peer punishment find support for the latter aspect, even in finitely repeated interaction (Casari and Luini, 2009).

Camera et al. (2012) identify the individual strategies employed by subjects and Bigoni et al. (forthcoming) extend such an analysis to a non-standard subject pool comprising clerical workers at a large US university. These workers were mostly long-time local residents, and exhibited a wide variation in age and educational backgrounds. This paper reports substantial differences between subject pools both in aggregate and individual behavior and both in the design with and without the personal punishment opportunity. Students exhibit higher levels of aggregate cooperation than workers, and are more inclined than workers to sanction defections through decentralized punishment and personal punishment, when available. Students and workers, however, adopt "always defect" less frequently, when personal punishment is available.[1]

3.2 Monetary exchange

Money is an institution that can help participants in markets with frictions to achieve better outcomes. There exist a handful of experimental studies involving the possibility to carry out monetary exchange in decentralized markets. In some experimental economies, money by design crowds-out consumption and *must* be used to avoid inefficient outcomes (e.g., Duffy and Ochs, 1999, 2002), while in others this is not so (Camera and Casari, 2010). Another key design difference is between commodity money, which is when money has redemption or utility value (Brown, 1996, Duffy and Ochs, 1999), and fiat money, which is an intrinsically worthless object (Duffy and Ochs, 2002, Camera and Casari, 2010, Duffy and Puzzello, 2011).

[1] It must be noted that there can be also an "antisocial" use of peer punishment, i.e., a use which goes in a direction opposite to increasing the incentives to adopt strategies that are socially optimal (e.g., see Faillo et al., 2013).

Some experiments have compared the relative efficiency of money versus barter in models with "double coincidence of wants" where subjects earn payoffs only if they barter or trade. By design, in these experiments subjects face complex storage problems involving multiple goods and money must be used to expand the efficiency frontier. In particular, Brown (1996) and Duffy and Ochs (1999) present a test of the commodity money, random matching model of Kiyotaki and Wright (1989). In a subsequent experiment, Duffy and Ochs (2002) study also fiat money with and without storage cost. In these economies, money could not be ignored because it could not be discarded; money was frequently held by agents when they also had the option of holding a good with a higher storage cost. Money tended to be held when it had a lower storage cost, even if this was not optimal. Camera and Casari (2010) propose a novel experimental framework to study fiat money, based on a gift-giving or helping game nested in a random matching model. In every encounter one subject has a good that can be either consumed or given to her opponent who values it more. Social efficiency requires that everyone with a good transfers it to others; this outcome can be sustained through a social norm based on community enforcement of defections, which completely ignores money. Indeed, by design, monetary trade *cannot* sustain full cooperation. This set-up eliminates the need to barter or trade to earn high payoffs; in this manner, subjects can focus on the inter-temporal dimension of cooperation, which is central to the theory of money. By lifting the bias toward employing money to achieve efficiency, this design allows studying why a monetary system emerges, as opposed to simply describing what money does in the experiment. Intrinsically worthless tokens acquired value in the experiment. Their presence increased predictability of play and promoted trust that cooperation would be reciprocated when strangers could trade help for a token. In a subsequent study, Duffy and Puzzello (2011) add a centralized market to a gift giving game in order to test the theory in Aliprantis et al. (2007), where monetary trade can sustain social efficiency. The study shows that, under a certain pricing rule, monetary fosters allocative efficiency.

3.3 Communication

There exists a considerable experimental literature that has explored the role of cheap-talk as a tool to signal intentions. Here, we review some recent experiments about communication that involve either coordination games or social dilemmas, focusing on how communication can promote the emergence of social norms of cooperation *when subjects do not know the reputation of others*.[2] In particular, we will focus on two distinct issues, which are the ones dominating the recent literature: how communication can help to solve *coordination* problems, and how it can behaviorally discourage *opportunism* to improve outcomes in social dilemmas (e.g., Ostrom, 2010, Strassmair and Sutter, 2009). Most studies with communication adopted a partner setting and found that communication generally increased cooperation in a voluntary contributions game (Dawes et al, 1977; Marwell and Ames, 1981; Isaac and Walker, 1988, Ostrom et al, 1992).

Cason and Mui (2010) study a collective resistance game among three players, with finite and indefinite repetition. A leader may "transgress" against one or both of two responders, who then may coordinate their individual responses through structured communication. They consider both three- and nine-subject economies with random matching. Adding communication facilitates coordination and always increases the chance of achieving the efficient outcome.[3]

Cooper and Kuhn (2011) look at collusion in two-period Bertrand duopoly games, with structured or free-form messages. In the first period the two subjects can send structured messages to each other before playing a social dilemma. In the second period, they play a coordination game. This second period is interpreted as the reduced-form of a continuation game with an infinite horizon. Structured communication does not raise cooperation but free-form communication does. They put forward three reasons for this effect of free-form communication: subjects (i) formulate explicit threats to punish cheating, (ii) exchange promises to cooperate, and (iii) invoke the mutual benefit

[2] For an overview of the effects of communication in experiments see Sally (1995), Crawford (1998) and Ostrom (2010). Most of the literature on reputation scores and internet auctions is outside the area of interest.

[3] Other recent studies on communication are Bochet et al. (2006), which studies different forms of communication in a public good game, and Bigoni, et al. (2012), which studies communication with indefinitely repeated interactions.

of cooperation. Additionally, Cooper and Kuhn (2011) study the impact of renegotiation, by comparing economies with communication in both periods as opposed only to the first. Theory suggests that the possibility to renegotiate lowers the initial incentives to collude. Contrary to this prediction, cooperation is higher when communication occurs in both periods.[4]

Finally, Camera, et al. (forhtcoming) randomly divided a group of strangers into pairs to play a prisoners' dilemma, as in Camera and Casari (2009). Cooperation did not increase when subjects could send public messages amounting to binding promises of future play.

4 Communication in markets with frictions: a new experimental approach

Here we propose a new experiment on communication among strangers where subjects can send a *public* message to the entire economy (not a private message to their opponent). Three elements characterize this study. First, cooperation is part of equilibrium, unlike other communication experiments, such as the finitely repeated public goods game in Wilson and Sell (1997) where only defection is consistent with Nash equilibrium. Due to indefinite repetition, instead, our design admits multiple equilibria, ranging from full defection to full cooperation. This raises the issue of coordination *along with* the issue of opportunism. In coordination games there is no incentive to misrepresent intentions, even when there is conflict of interest (e.g., battle of the sexes). Here, by contrast, communication can be used in opposing ways: as a coordination device, for those who are motivated by long-run efficiency, and as a tool for deception, for those tempted by short-run gains.

Second, we compare the effects of free-form and structured communication, which does not allow conveying individual histories, strategies, approval or disapproval. Structured messages are not necessarily credible (see the discussion in Farrell and Rabin, 1996), self-signalling or self-committing; on the contrary, they can be outright deceptive. A contribution of this study is to

[4] In that study, cooperative messages should not be credible because only defection is consistent with Nash equilibrium.

quantify the incidence of deception by an appropriate design of structured communication. Third, we consider a market with frictions as interactions are anonymous and decentralized.

4.1 Experimental Design

We ran experiments under four anonymous communication formats: *No-communication, Messages, Multiple Messages,* and *Chat* (Table 1). The stage game is a prisoner's dilemma where a subject choose between *Y* (*cooperate*) and *Z* (*defect*). Individual payoffs are 25 points under mutual cooperation, 10 under mutual defection, 5 or 30 under discordant choices. An experimental session involved twenty subjects who interacted in economies of four subjects.[5] Each subject participated in five supergames,[6] where a supergame could comprise many periods. In each period subjects were randomly matched in pairs within their economy. Subjects could observe all past actions taken in their economy but could not see individual histories (= public monitoring) , i.e. monitoring was anonymous. Subjects could identify neither their current nor past opponents. Hence, subjects could not use strategies based on reputation.

A supergame consists of an indefinite sequence of periods where the continuation probability is $\delta = 0.95$, which enables to sustain the efficient outcome as a sequential equilibrium (for all $\delta > 0.25$). This rule induces an expected duration of a supergame of 20 periods.

	No-communication		*Messages*		*Multiple Messages*		*Chat*
Communication frequency	n/a		Every period		Every 4 periods		Every 4 periods
Message space	n/a		Y, Z, not sure		Y, Z, not sure (4 iterations)		Free-form text
Session dates	27.4.05	1.9.05	15.2.07	28.4.10	9.2.07	23.4.10	23.2.07
No. of periods	129	125	109	42	35	39	159

Table 1: Treatments and Sessions

[5] The sessions were run at Purdue University's VSEEL lab and involved 140 subjects. Average earnings were $24.08 and average session duration was slightly below 3 hours, including instruction reading and a quiz.

[6] Subjects were informed that no two participants ever interacted together for more than one supergame. Subjects may have indirectly shared a common past opponent only after the second supergame.

Notes: Exchange rate: $.13 for every 10 points. The sessions run on 27.4.05 and 1.9.05 are also analyzed in Camera and Casari (2009).

In three treatments, subjects could communicate at the beginning and then also during the supergame. Communication was free-form in the *Chat* treatment, structured in all other treatments.

Messages treatment. In every period subjects had the opportunity at no cost to suggest a play (Y, Z, not sure), by making a message public in their economy. A message included three parts: a suggestion (Y, Z, not sure) for the subject herself, for her anonymous match, and for everyone else.[7] The default message "not sure" appeared as a blank space, and could also be sent by clicking a "No Suggestion" button. We refer to it as a *neutral* message, to which different subjects could attach a different meaning, and refer to Y and Z as *explicit* messages. When choosing their action, subjects could see everyone's messages, but were unable to identify the sender of the message.

Multiple Messages treatment. Subjects entered a pre-play communication stage at the start of period one, and then every four periods. The communication stage comprised four steps. In step one, subjects sent a message as described in the *Messages* treatment. Then, subjects saw all messages sent in the economy, and had the opportunity to revise their step-one message. Steps three and four followed the same procedure. This protocol gave subjects the same (expected) number of opportunities for communication as in the *Messages* treatment and could help them to achieve a consensus and so facilitate coordination on play.

Chat treatment. Subjects entered the pre-play communication stage at the start of period one, and then every four periods. Communication was free-form and took place through a chat box that remained open for two minutes.[8] The procedure aimed at avoiding direct identification of subjects during the communication stage.

Introducing communication does not remove the equilibrium multiplicity of the baseline game.

[7] We refer to the first part of the message (message sent "for oneself") as signaling the sender's intention of play; the other parts of the message suggest play to others. The analysis focuses on the first part of the message.

[8] We thank John Kagel for having kindly provided the chat program.

Subjects can simply choose to ignore messages. Due to public monitoring, the possibility to send messages does not increase the speed with which a defection can be communicated to others. However, public messages can facilitate a reversion to cooperation after a defection.

4.2 Four Results

Result 1: *Structured communication did not significantly raise cooperation relative to No-communication. In contrast, free-form communication supports almost full cooperation.*

The level of cooperation in an economy is the fraction of Y actions in that economy. Instead, the level of coordination on cooperation in an economy is the fraction of periods in which all subjects in that economy cooperated. A Kruskal-Wallis test does not reject the null hypothesis that observations from the *No-communication*, *Messages*, and *Multiple Messages* are drawn from the same population with respect to average cooperation rates in all periods (p-value>0.1, n=150). This result is confirmed by pairwise comparisons with the *No Communication* treatment (Mann-Whitney tests, n1=n2=50, p-value>0.10).[9] Similarly, we fail to find evidence that structured communication significantly increases the level of *coordination on cooperation* with respect to the *No Communication* treatment (35.7% vs. 28.6%, Kruskal-Wallis test on three treatments, n=150, p-value>0.1; Mann-Whitney pairwise tests, n1=n2=50, p-value>0.10).

More evidence comes from measuring cooperation through linear regressions, which fail to find evidence that structured communication increases cooperation relative to *No-communication*. Cooperation rates at the economy level are regressed against treatment dummies and other variables for supergames, and duration of previous supergames (see Supplementary Materials).[10]

[9] Unless otherwise stated, in the Result section the unit of observation is an economy in a supergame. The results of the statistical tests in the paper rely on the assumption that all observations are independent. All tests are two-sided.

[10] Structured communication brings about an *increase* in the cooperation rate, which is not significant in the *Messages* treatments and only weakly significant in the *Multiple Messages* treatment. The difference in cooperation rates between *No-communication* and *Multiple Messages* is significant according to a probit regression but not according to the above non-parametric test. While in the regression one observation is a single choice by a subject in a period, in the test it is the average choice in an economy. So, the regression gives more weights to longer supergames.

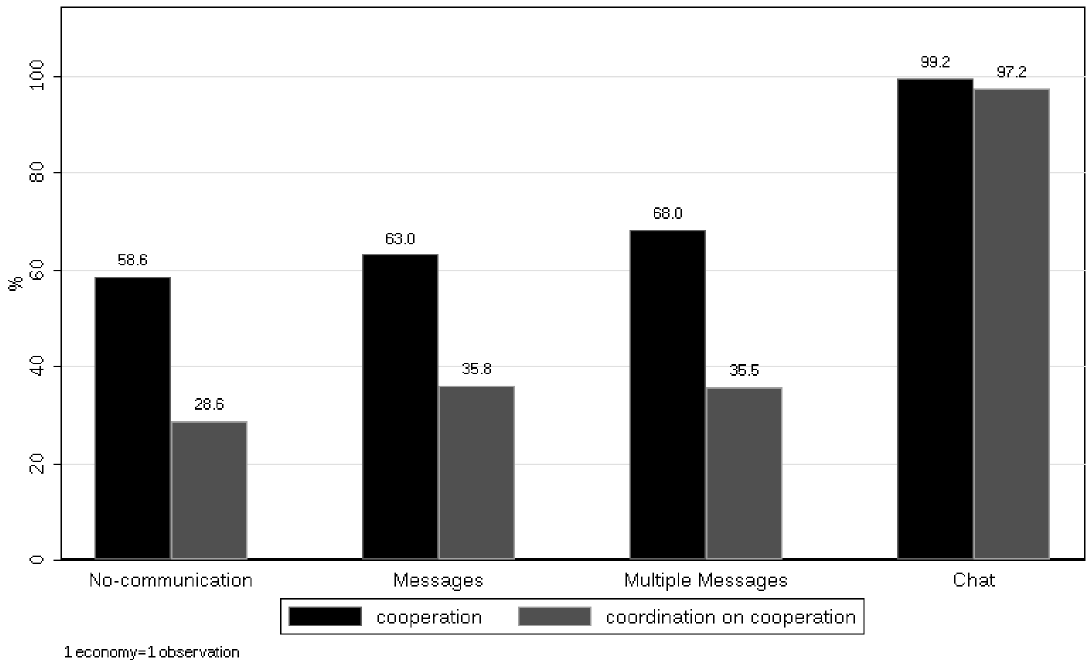

Figure 1: Cooperation rates by treatment

By contrast, the *Chat* treatment exhibits average cooperation rates more than 30 percentage points higher than all other treatments; this difference is significant (Kruskal-Wallis test on the four treatments, n=175, p-value<0.01; pairwise Mann-Whitney, n1=25, n2=50, p-value<0.01). An independent coder analyzed the chat messages to assess whether subjects communicated contingent strategies. In every economy subjects discussed a plan of action directed toward achieving cooperation. However, only in 40% of the economies subjects discussed a possible punishment strategy to be implemented in the event of a deviation from the plan of action. This suggests that communicating contingent strategies is not essential to achieve 100% cooperation.

Result 2: *The possibility of renegotiation did not significantly reduce initial cooperation.*

Periodic opportunities to communicate can facilitate re-coordination on cooperation after defections. Such possibility to "renegotiate" weakens the credibility of punishment threats. Hence, theory predicts lower initial cooperation rates in treatments with communication than without.

Cooperation in period 1 was 70.5% in *No-communication*, 78.0% in *Messages*, 76.5% in *Multiple Messages*, 100.0% in *Chat*. A Kruskal-Wallis test does not reject the hypothesis that observations from the first three treatments are drawn from the same distribution (n=150, p-value> 0.10). Results from a linear regression explaining cooperation rates at the economy level in period 1 highlights a non-significant increase in both treatments with structured communication (see Supplementary Materials). This does not support the conjecture that the possibility to renegotiate lowers initial cooperation rates. Result 2 answers a methodological question raised in Cooper and Kuhn (2011) about the possibility of studying collusion in finitely repeated games to draw inference on infinitely repeated games. Some of their results on the effects of communication still hold in an indefinitely repeated setting. In particular, we confirm that the possibility of renegotiation is not detrimental to cooperation, as opposed to a no-communication setting.[11]

Result 3: *Structured messages signaled intentions: subjects tended to act in accordance with their own public messages. Moreover, subjects' choices were affected by others' messages.*

Subjects seized on communication opportunities when available, widely sending messages that explicitly stated their intended play (66%-88% of instances with a message Y or Z, Table 3). In particular, with structured communication, subjects rarely made statements of defection (5.1%, 154/3020 for *Messages*, 20/440 for *Multiple Messages*, Table 2).

In all treatments, there is coherence at the individual level between the statements made public and choices subsequently taken. In the *Messages* treatment, while a subject who publicly signaled her intention to cooperate (message "Y") did cooperate in 64.1% of periods immediately following communication, a subject who signaled defection (message "Z") cooperated only in 7.1% of cases (Table 2). In the *Multiple Messages* treatment there is an even stronger coherence between messages sent and subsequent choices. As a consequence, subjects could rely on public statements

[11] Andersson and Wengstrom (2012) also investigate the effects of renegotiation and report that, in a two-stage game, pre-play communication only has a significant impact on subjects' cooperation when no renegotiation is possible.

made by others about their intended play to forecast behavior in the economy. Our data show that actions are a function of the type of messages seen.

A subject cooperated more frequently the greater was the number of cooperative messages observed. For example, in the *Messages* treatment, those who sent a neutral message ("not sure") cooperated in 62.0% of cases when everyone else sent a cooperation message and 40.1% otherwise (Table 2). Subjects who sent a neutral message cooperated less than the economy's average. We say that a message is *informative* if it is positively or negatively correlated with a specific action.

Subject's message about her intended play	Cooperation frequency	Cooperation frequency conditional on the messages sent by others: zero, one, or two Y messages	Three Y messages
Messages			
Not sure	0.449	0.401 (800)	0.620 (221)
Y (cooperation)	0.641	0.577(1273)	0.783 (572)
Z (defection)	0.071	0.058(120)	0.118 (34)
Total	0.547	0.484(2193)	0.712 (827)
Multiple Messages			
Not sure	0.547	0.524 (21)	0.563 (32)
Y (cooperation)	0.752	0.730 (159)	0.769 (208)
Z (defection)	0.150	0.125 (8)	0.167 (12)
Total	0.700	0.681 (188)	0.714 (252)

Table 2: Communication as a signal of intentions

Notes: *Table 2 only considers (i) messages sent by the subject about her intended play and (ii) the concordance between the message and the action immediately following the message. The number of observations is reported in parenthesis.*

Linear regressions confirm that, in the *Message* and *Multiple Messages* treatments, the greater is the number of cooperative messages observed, the higher is the cooperation rate for the representative subject (see Supplementary Materials). This finding supports the interpretation that sending a message of cooperation was perceived as signaling truthfully the intention of the sender.

Result 4: *Subjects used structured communication for two opposite goals: either to coordinate on cooperation or to capture short-run rents through deception.*

Because in the experiment messages were informative, they shaped beliefs. From Result 3, one can conjecture that the more cooperative messages were made public, the stronger was the belief that a social norm of cooperation could be supported. This means that subjects could make several uses of communication. The socially desirable, or benevolent, use is to help coordination on cooperation by reinforcing the belief that the sender of a cooperative message will cooperate. However, there is also a socially undesirable use of communication. Subjects could behave deceptively by sending a cooperative message to reinforce the belief in a cooperative outcome while intending to defect.

The data provide evidence on these conflicting uses of public messages. Based on the messages sent and observed, we can quantify a lower bound for the incidence of deceptive and benevolent use of communication. For this purpose, we define two types of subjects. A *deceptive subject* is someone who, at least once during the supergame, signaled her intention to cooperate, observed that everyone else also shared a similar intention, and defected the period immediately following such communication. A *benevolent subject* either cooperated in all those periods of a supergame when all other subjects signaled the intention to cooperate; or cooperated in all periods of a supergame when she signaled a cooperative intention. Clearly, not all subjects fit either type because either behaved differently or never faced a situation in which they could behave deceptively or benevolently.[12]

The prevalent use of communication in all treatments is benevolent. In the *Messages* treatment 41.9% of subjects were deceptive and 49.5% were benevolent when given the opportunity (no.obs. 52/124, 95/192, respectively); in the *Multiple Message* treatment 27.7% were deceptive and 68.0% were benevolent (31/112, 134/197). This finding suggests that what prevented structured communication from facilitating the implementation of a social norm of cooperation is not entirely explained by limitations in the message space. As we have seen messages are empirically

[12] This definition pins down a lower bound for deceptive behavior. For example, someone who sends a cooperative message, then observes two others who sent a cooperative message and one who sent a defection message is not classified as being deceptive even if she defects in the following period. Similarly, we cannot say whether a subject is benevolent if she never sent a cooperative message and she never observed three cooperative messages. Hence, we have fewer than 200 observations per treatment that we can use to classify subjects as deceptive or benevolent type.

informative and not necessarily theoretically credible. The crux of the matter is that there were subjects who made a deceptive use of communication. Deception diluted the meaningfulness of the public messages of cooperation, and reduced the value of making public the intention to cooperate.

Finally, signaling intentions through structured communication did not have positive impact on profits in the economy. We regress the average profit in each economy on the average number of public statements that were explicit about the sender's intended play (Y or Z messages "for oneself"), controlling for supergame order and length. None of these coefficients is significant in the *Messages* and *Multiple Messages* treatments (see Supplementary Materials).

5 Discussion and conclusions

This paper has surveyed recent experiments on a segment of the economics literature about decentralized markets with frictions; specifically, models characterized by random, pairwise interactions taking place over an indefinite time horizon. Our review of the literature indicates how experiments can fruitfully investigate the effects of frictions and the impact of institutions—such as money, peer punishment, and communication—on the efficiency of the outcomes. We have then presented results from a new experiment on the effects of free-form and structured communication in the random matching model of social dilemmas in Camera and Casari (2009).

The focus on pre-play communication in this paper is motivated by the observation that the experimental literature has identified cheap talk as an institution that promotes social efficiency even when the socially efficient outcome is not a Nash equilibrium. One would thus expect cheap-talk to have a similar or even more beneficial impact in settings where the socially efficient outcome *is* a theoretical equilibrium, albeit one that is empirically infrequently observed.

The new experiment discussed here introduced free-form and structured communication consisting of anonymous public messages. We find that some types of communication did not help cooperation *even if the efficient outcome was an equilibrium*. This result is novel and can be ascribed to the conflicting short and long-run incentives confronting subjects. The design allows us to assess the impact of different roles of public statements: for coordination on efficient play and for deception (= falsely signaling intentions). Deceptive and benevolent uses of communication coexisted, with the prevalent use of communication being benevolent. The presence of deception destroyed trust among those participants who used communication to coordinate on cooperation and maximize long-run gains. In a way, deceptive communication diluted the effectiveness of making public cooperative statements as a way to coordinate on efficient Nash play.

Our findings for structured communication mirror some of the results obtained in finitely repeated social dilemmas, where cheap-talk can have no impact or even a negative impact on social welfare. This suggests that the same behavioral mechanism tied to communication in finitely repeated games—where defection is the unique equilibrium—is also present when the game is indefinitely repeated, so that cooperation *is* also an equilibrium. In a way, it is surprising that the behavioral impact of structured communication is unaffected by the change in equilibrium set, especially because of the associated introduction of a non-trivial coordination problem. Only in the chat treatment communication played a positive role. Perhaps the possibility to convey strategies and verbally punish defectors helped the implementation of a norm of cooperation.

To the best of our knowledge, this is the first study that investigates communication in an economy of strangers that lasts indefinitely. It leaves open several questions, such as which elements of communication can promote efficiency with pairwise, random interaction.

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