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Breaking the Norm

An Empirical Investigation into the Unraveling of Good Behavior

Ruth Vargas Hill

Eduardo Maruyama

Angelino Viceisza

Markets, Trade and Institutions Division

INTERNATIONAL FOOD POLICY RESEARCH INSTITUTE

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AUTHORS

Ruth Vargas Hill, International Food Policy Research Institute
Research Fellow, Markets, Trade and Institutions Division
r.v.hill@cgiar.org

Eduardo Maruyama, International Food Policy Research Institute
Research Fellow, Markets, Trade and Institutions Division
e.maruyama@cgiar.org

Angelino Viceisza, International Food Policy Research Institute
Postdoctoral Fellow, Markets, Trade and Institutions Division
a.viceisza@cgiar.org

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Contents

List of Tables

List of Figures

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ABSTRACT

We present results from an artefactual field experiment conducted in rural Peru that considers how observing nonreciprocal behavior influences an individual's decision to reciprocate. Specifically, we consider the behavior of second movers in a trust game, assessing how their decision to reciprocate is influenced by the observed behavior of others and the extent to which their actions can be observed. In documenting how an external shock to the number observed not to reciprocate influences reciprocation, the paper endeavors to provide some insight into how reciprocity can unravel when individuals are learning behavior in a new market institution.

Keywords: trust, norms, artefactual field experiment, Peru

1. INTRODUCTION

Mutual cooperation is essential to the development of many market institutions. Although simple spot market exchanges can take place in the presence of limited trust (Fafchamps 2004), market exchanges that entail trade in unobserved characteristics (such as trade in organic produce), trade with delayed delivery (such as in credit or insurance markets), or trade in which delivery is conditional on acts of nature (such as is often the case in contract farming) require a certain degree of trust or mutual cooperation. When the extent to which individuals are willing to trust and behave trustworthily is low, it is much more difficult for such markets to develop.

Recent experimental analyses have shown that there is considerable variation across societies in the probability that an individual will choose to cooperate when there is a short-term gain to defection (Bowles et al. 2004). A number of empirical studies have examined the determinants of this heterogeneity. The starting point for this paper is the observation that the similarity in behavior of individuals from the same society is consistent with a model of human behavior in which an individual's decision to reciprocate is influenced by the social environment that surrounds him or her (Manski 2000). This is in keeping with a large body of empirical and theoretical literature that suggests that the utility received from undertaking a particular action is in part determined by the extent to which it mimics the observed behavior of others.[1]

Bernheim (1994) and subsequently Becker and Murphy (2000) delineate three channels by which the actions of others influence utility. The most basic is that of externalities: actions taken by others may increase (or decrease) the returns an individual receives from undertaking the same action (such as in the case of contributing to a public good). The second mechanism is informational: to the extent that an individual is unsure about appropriate or optimal behavior, and to the extent he or she believes others in the group are better informed, the behavior of others may provide a source of information on the course of action he or she should take. Third, social interactions can influence an individual's preferences (Jones 1984; Bernheim 1994; Becker and Murphy 2000). In particular, the models of conformism set out in Jones (1984) and Bernheim (1994) allow for an individual to derive utility from minimizing the extent to which his or her actions deviate from the average behavior of others.[2] In the context considered here, one can think of social interactions determining the esteem, acceptance, guilt, or shame an individual feels as a result of choosing to adhere to or deviate from a norm of reciprocity.[3]

In this paper we present results from an artefactual field experiment (Harrison and List, 2004) conducted in rural Peru that considers how observing non-reciprocal behavior influences an individual's decision to reciprocate. Specifically, we consider the behavior of second movers in a trust game, assessing how their decision to reciprocate is influenced by the observed behavior of others in the same room and the extent to which their actions can be observed. In documenting how an external shock to the number of those observed not to reciprocate encourages others to deviate, the paper endeavors to provide some insight into how a norm of reciprocity can unravel when individuals are learning behavior in a new market institution. At a broader level of interpretation, it is an empirical investigation into the mechanisms by which social interactions affect behavior.

Empirically identifying the processes that give rise to a positive relationship between the propensity of an individual to behave in a certain way and the prevalence of that behavior in the group is

[1] Many empirical studies have documented how choices may be taken to mimic the observed behavior of others, but ones closely related to the case considered here are Asch (1951, 1955), Rosenbaum and Blake (1955), Rosenbaum (1956), Cason and Mui (1998), Falk and Knell (2004), Bardsley and Sausgruber (2005), and Croson and Shang (2008).

[2] Manski (2000) indicates that an action chosen by one agent may affect the actions of other agents through three channels: constraints, expectations, and preferences. These are essentially the same three channels. Externalities are a form of constraint interaction, information conveyed through observational learning is a form of expectation interaction (see also Chamley [2004]), and the third channel is a form of preference interaction.

[3] Elster (1989) discusses the role of social norms in economic theory and (1998) how different types of emotions affect social norms.

difficult. As Manski (2000) argues, a positive relationship between individual and group behavior could arise as a result of the influence of observed behavior (endogenous interactions), the influence of observed characteristics (contextual interactions), or individual and environmental characteristics shared by group members (correlated effects). This paper uses an experiment conducted in a controlled environment to identify the presence of endogenous interactions. Variation was exogenously introduced into the average observed behavior of the group, and this exogenous variation is used to identify the influence of observed behavior on individuals' decisionmaking. We posit a model that suggests that endogenous interactions arise as a result of preference interactions between group members: as more group members are observed not to reciprocate, the cost of deviation for any individual falls, resulting in a higher propensity to deviate.

Recently, two experimental studies have considered the degree to which shame and/or guilt explain an individual's decision to adhere to a norm of equality (Andreoni and Bernheim 2009) and reciprocal cooperation (Tadelis 2008). The approach of this paper is similar, with one major distinction. Unlike the authors of these studies, we assume that the disutility felt in departing from the norm is not only dependent on the degree to which an individual's actions are observed, that is, "audience effects" (Andreoni and Bernheim) or "extrinsic preferences for shame" (Tadelis), but is also to some extent determined by the share of peers that are observed to comply with the norm (as in Lindbeck, Nyberg, and Weibull [1999]).

We study a twice-repeated trust game, as described in Bower, Garber, and Watson (1997), but allow for a population of players that can observe each other's actions. We do so because our main interest is to study the influence of peer effects on behavior.[4] Although we present results for both the first mover (the player who decides whether or not to trust the other player by choosing to send money, denoted throughout as player A) and the second mover (the player who decides, having been trusted, whether or not to reciprocate that trust by sending money back to the first mover), in our analysis we are primarily interested in the second mover (player B). In particular, the focus of the paper is in studying player B's propensity to conform to the norm, and in assessing the behavioral impact of externally induced variation in what she observes.[5] This focus on the second mover is similar to the approach of Tadelis (2008).

In the following sections we develop a model of choices in a twice-repeated trust game that incorporates the social environment (Section 2), describe the experiments (Section 3), and present empirical results (Section 4). Section 5 concludes.

[4] Another important source of audience that has been discussed in the literature (e.g., Andreoni and Bernheim 2009; Sobel 2005) is the experimenter effect. This audience is fixed throughout our treatments.

[5] Throughout the paper, for ease of discussion, we use the convention that player B is female and player A is male; in the actual experiments, the gender balance was roughly equal.

2. MODEL

In this section we set out a model of a twice-repeated trust game between two players: player A, who is the first mover, and player B, who is the second mover. We allow player B's utility to depend both on her own material payoff and on that of her partner.[6] Furthermore, we seek to characterize how the social environment affects player B's decisions. In particular, we are interested in considering how variation in the observed behavior of peers affects player B's decision to reciprocate or not. We develop a model that allows player B's utility to depend both on her own material payoff and on that of her partner. Specifically, player B's willingness to pay for her partner's monetary payoff is constructed to depend on (a) an inherent preference for reciprocity, (b) observation of her peers' actions, and (c) the extent to which peers observe her actions.

As will be discussed below, the Cox, Friedman, and Gjerstad (2007) setup lends itself nicely to developing such a model.

The Tice-repeated Trust Game

Consider a population of M player As and M player Bs. The populations of player As and player Bs are in separate locations and although they can see other players that have the same "role," player As cannot see player Bs, and likewise player Bs cannot see player As. We could think of this as corresponding to a situation in which farmers in one village may be trading with a set of traders from a nearby market that they do not know. These players are randomly matched such that some player A and some player B are assigned to a pair j (for $j = 1, \ldots, k, \ldots, M$) for the duration of the game. These M pairs play the twice-repeated trust game. Our discussion pertains to some fixed pair k that is representative of all other pairs $-k$.

In each period t (for $t = 1, 2$), both player A and player B are given an initial endowment of x_t. Player A has a choice of two actions: he can exit the game, in which case both he and player B keep x_t (denote this action as E_t), or he can send x_t to player B (denote this action as T_t). If he chooses T_t, x_t is tripled and player B receives $3x_t$ in addition to the x_t she received at the beginning of the game. Player B now has a choice of two actions: she can equalize payoffs with player A by sending $2x_t$ back to player A (an action denoted as R_t), or she can keep all $4x_t$ for herself (an action denoted as D_t).[7] The choice of terminology for the players' actions, that is, E_t, T_t, D_t, and R_t, is intended to represent the following, respectively: exit, trust, defect, and reciprocate.

The one-shot trust game for period *t* is presented in extensive form in Figure 1. The payoffs displayed below the end nodes of the game tree are terminal payoffs. In our game these payoffs represent period (or stage) payoffs, as the one-shot trust game is repeated twice. Similarly to Bower, Garber, and Watson (1997), we assume that the terminal payoffs for the twice-repeated trust game are the sum of the stage payoffs and that there is no discounting. Given these assumptions, we construct the tree for the twice-repeated trust game in Figure 2.

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[6] Sobel (2005) reviews models of interdependent preferences and reciprocity. He categorizes (a) distributional preference models (e.g., Fehr and Schmidt 1999; Bolton and Ockenfels 2000; Charness and Rabin 2002); (b) models with preferences over generalized consumption goods; (c) models of intrinsic reciprocity, also known as models with belief-dependent preferences (e.g., Rabin 1993; Dufwenberg and Kirchsteiger 2004; Battigalli and Dufwenberg 2009); and (d) models of extrinsic reciprocity.

[7] We assume that $x_t = x$ for all t and that x_t is bounded above by a maximum, x.

Figure 1. One-shot extensive-form trust game, period t

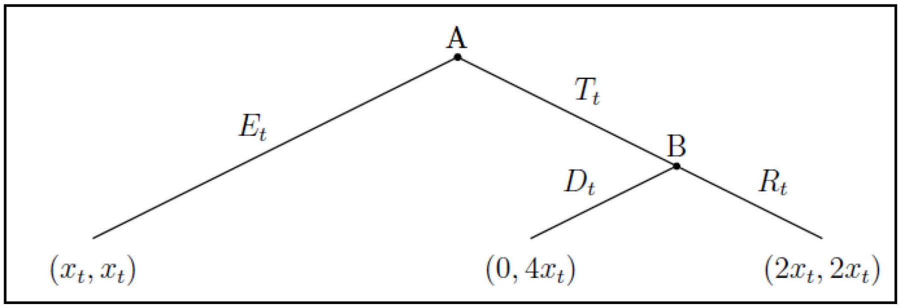
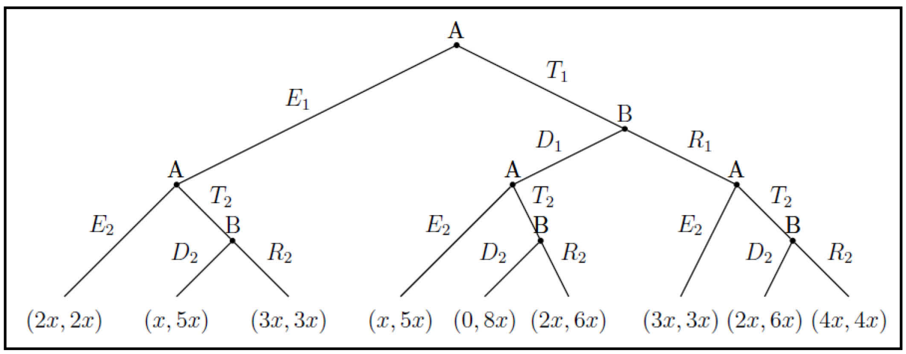


Figure 2. Twice-repeated extensive-form trust game



Preferences, Observability, and Beliefs

When we test the game in a naturally occurring environment, players face monetary payoffs that are the result of actions taken throughout the game protocol. It is thus important to specify players' preferences over these payoffs.

Since our main interest is player B's behavior, we complicate the exposition only with regard to the second mover's preferences. We let player A have self-regarding preferences that are strictly for increasing his monetary payoffs and unaffected by player B's. In other words, player A's per-period utility can be represented as an increasing function of only his monetary payoff, as follows:

$$u_t^A(x_t^A, x_t^B) = f(x_t^A), \tag{1}$$

with $f' > 0$. We assume u_t^i is additive in monetary payoffs across time periods such that all players rank terminal payoffs in the same manner as they rank stage payoffs and vice versa.

We adopt the linear version of the Cox, Friedman, and Gjerstad (2007) model of reciprocity and fairness to characterize player B's preferences as depending on the monetary payoff she receives x^B and the monetary payoff player A receives x^A, as follows:

$$u_t^B(x_t^A, x_t^B) = x_t^B + \theta_t x_t^A. \tag{2}$$

The most attractive feature of the Cox, Friedman, and Gjerstad model for our setup is that θ represents an emotional state (as opposed to a type parameter) that is a function of reciprocity and status variables. This formulation lends itself nicely to our question because we can let θ_t be a function that depends on an individual, time-invariant effect α that represents an inherent preference for reciprocity (i.e., type) and two social, possibly time-variant effects M_{1t} and M_{2t} that represent, respectively, the proportion of other player Bs that a given player B observes not reciprocating and the proportion of other player Bs that observe a given player B. Notice that α is essentially the reciprocity variable r in the Cox, Friedman, and Gjerstad formulation, whereas M_{1t} and M_{2t} jointly represent the (relative) status variable s.

This formulation accommodates changes in player B's emotional state θ over time, thus allowing us to index θ by t. For example, player B may observe distinct numbers of second movers defecting at different stages of the game. This will in turn affect player B's emotional state θ_t. This is different from a setup that treats θ as a type parameter, as type parameters are typically assumed to be fixed across time.

We formulate the emotional state function as

$$\theta_t = \theta(\alpha,\ M_{1t},\ M_{2t}) \tag{3}$$

Under the following assumptions:

- There are no spiteful states, that is, $\theta_t \geq 0$, and there are no spiteful types, that is, $\alpha \geq 0$. The terminology "spiteful" is used here in the sense of Levine (1998).
- At the individual level, for any given level of relative social status M_{1t} and M_{2t}, more reciprocal types are (weakly) more willing to pay their own for others' monetary payoffs, that is, $\frac{\delta\theta_t}{\delta\alpha} \geq 0$.
- At the social level, for any given level of reciprocity α, player B is (weakly) less willing to pay her own for the other's monetary payoff as more player Bs are observed not reciprocating, that is, $\frac{\delta\theta_t}{\delta M_{1t}} \geq 0$. Similarly, player B is (weakly) more willing to pay her own for the other's monetary payoff as more player Bs observe her, that is, $\frac{\delta\theta_t}{\delta M_{2t}} \geq 0$.
- A selfish (or self-regarding) individual with $\theta = 0$ is unaffected by social effects and therefore is not affected by player A's monetary payoff. In other words, a selfish type is not willing to pay her own for the other's payoff even in the presence of social effects, that is, $\theta(\alpha,\ M_{1t},\ M_{2t})|_{\alpha=0} = 0$ for any M_{1t} and M_{2t}. This assumption also helps to ensure that we avoid a waiting game. The fact that every player B's decision can depend on the decisions of other player Bs can give rise to a waiting game in which player B of pair k waits on player B of pair $k-1$, $k-1$ waits on $k-2, \ldots$, and $k-n$ waits on k. In such a case every player B waits and the game stalls. We assume that in the presence of social effects, an action taken by at least one other player B is sufficient to influence player B's decision. This assumption solves the waiting-game issue as long as there is a strictly positive proportion of self-regarding types with $\alpha = 0$ in the population. For this type, individual effects will trump social effects, inducing her to take actions independently of her peers.
- Individual effects persist when there are no social effects, that is, $\theta(\alpha, M_{1t}, M_{2t})|_{M_{1t}=M_{2t}=0} = \theta(\alpha)$.

This final assumption is not necessary for the analysis but ensures that linear models without social effects can arise as special cases of our setup. Additionally, the assumption regarding "no spiteful types" guarantees consistency with the first assumption of "no spiteful states" when social effects are not present, as states reduce to types (or monotone transformations thereof) in such cases.

It is the assumptions made about the social influences on preferences that are the focus of this analysis. Consider an increase in M_{1t}, the proportion of player Bs observed not reciprocating. As player B observes more player Bs not reciprocating, her willingness to pay her own for the other's monetary payoff decreases. The social effects make her less likely to reciprocate trust. We can think of this as deviation from a norm of reciprocity becoming less embarrassing the more others do likewise. This is how the relationship between social norms and economic incentives is characterized in Lindbeck, Nyberg, and Weibull (1999).[8] Similarly, consider an increase in M_{2t}, the proportion of player Bs that observe any given player B. As more player Bs observe her decision, she becomes more willing to pay her own for the other's monetary payoff. We could think of this as "shame," but other emotions could be at play as well (Elster 1998).

In deciding whether or not to trust, player A recognizes that player B's decision to reciprocate or not when trusted will depend on the utility she receives. Player B's per-period utility (recall expression 2) will take one of the following values if player A decides to trust. If player B reciprocates, then she achieves utility of $u_t^B(R_t|T_t) = (1+\theta_t)\,2x$. If she defects then her utility is $u_t^B(D_t|T_t) = 4x$. Given $x = 1$, player B prefers to reciprocate if and only if $\theta_t \geq 1$ and defect otherwise. Player A's decision will thus hinge on whether he believes player B finds herself in a high (reciprocal) emotional state $\theta_t \geq 1$ or in a low (nonreciprocal) emotional state $\theta_t < 1$ in period t.

Player A starts the game with the prior belief p that the player B with whom he is partnered is in a high (reciprocal) emotional state in the first period, that is, $P(\theta_1 \geq 1) = p$. Similarly, player A has a prior belief r that player B is in a high emotional state in the second period, that is, $P(\theta_2 \geq 1) = r$. Player A updates the prior belief r at the end of the first period once he observes player B's response. Player A's updated probability q represents the belief that player B is in a high emotional state in the second period, conditional on information from the first period, that is, $P(\theta_2 \geq 1|\theta_1)$. Although we have established that states may vary across time, player B's type and the population of types are unchanged across periods, which induces some dependence between player B's emotional state in the first and the second period. Player B's response in the first period thus reveals information about r to player A such that $P(\theta_2 \geq 1|R_1) > P(\theta_2 \geq 1|D_1)$.

This is information that player A receives only if he chooses the action T_1. If player A chooses the action E_1, player B is not given the opportunity to reveal information about herself. We can thus think of player A as gaining an informational advantage by choosing T_1, which allows him to hold more accurate beliefs about player B's state in period 2. This brings with it some advantage. When player A holds correct beliefs, his expected payoff is $x + P(\theta_2 \geq 1)x$. When player A holds incorrect beliefs, his expected payoff is $P(\theta_2 \geq 1)x$. The gain that results from being correct is thus equal to x. The informational gain of participating in the first round is ax, where a is the increase in accuracy of beliefs that results from participation in the first round.

In period 1 player A chooses T_1 if $u_1^A(T_1) + ax \geq u_2^A(E_2)$, which is $p_k 2x + ax \geq x$. We see from this that player A chooses T_1 if $p_k \geq \frac{1-a}{2}$. In round 2 there is no informational incentive to trust, and player A chooses T_2 when $q_k \geq \frac{1}{2}$.

Thus far we have ignored strategic motives for player B's actions. In the final round, player B will indeed behave according to her true emotional state. Actions in the final period thus reveal states such that $P(R_2|T_2|R_1|T_1) = P(\theta_2 \geq 1|\theta_1 \geq 1)$. However, in the first round, player B may behave strategically, behaving as if she had a high emotional state, knowing that doing so would increase player A's belief that $P(\theta_2 \geq 1)$, inducing trust and allowing player B to choose $D_2|T_2$ and receive $4x$. Indeed, if player B believes that her first-round behavior has a large impact on player A's beliefs, such that the difference between $P(\theta_2 \geq 1|R_1)$ and $P(\theta_2 \geq 1|D_1)$ is large, the optimal strategy for Player B would be $D_2|T_2|R_1|T_1$. As signaling models teach us, cognizant of this, player A will be cautious in updating his beliefs based on first-round behavior such that a may be quite small. Although we refrain from modeling this formally (given that this is not our main focus), this discussion does make us aware that player B may chose $R_1|T_1$ even if $\theta_1 < 1$ and this will also be reflected in player A's beliefs about the probability of $R_1|T_1$. As a result of the strategic incentive present in the first round, we may expect (a) a higher

[8] "The intensity of the norm, as perceived by the individual, is endogenous in our model: it depends on the number of people adhering to it. More exactly, we assume that living on transfers becomes relatively less embarrassing when more individuals do likewise" (Lindbeck, Nyberg, and Weibull 1999, p.3).

occurrence of $R_1|T_1$ than predicted by p and θ_1, and (b) the occurrence of $R_1|T_1$to be higher than the occurrence of $R_2|T_2$.

In summary, the model predicts the following for a given pair k. First, player A will trust player B if his prior beliefs about player B's emotional states in the two periods are sufficiently optimistic. Second, player B's response to trust in any given period depends on her emotional state, which is a function of her type θ; an individual, time-invariant effect; and (for player Bs of type $\alpha > 0$) her social environment, as captured by two social, possibly time-variant effects, M_{1t} (the proportion of player Bs observed not reciprocating) and M_{2t} (the proportion of player Bs observing player B). Next we discuss the implications and predictions of the model in the presence of information shocks that reduce p and r.

The Impact of Information that Reduces p and r

As will be discussed further in Section 3, we conducted a number of sessions of twice-repeated trust games in which in some sessions, some player As were randomly provided with information that would lower their prior beliefs that $\theta_1 \geq 1$ and $\theta_2 \geq 1$. We can conceptualize this as an information shock that reduces p and r. The presence of the information shock was not made known to any player Bs, so its effect on player Bs results from changes in the behavior of player As. In this section we explore the impact of this shock on the behavior of the player A that received the shock, the partnering player B, and the other player Bs. Given the presence of social effects in the formulation of player B's preferences, we expect that the information will have an effect on the player Bs that were not directly affected by the information shock. If social effects are not important in the formulation of player B's preferences, such information shocks will affect only the behavior of the pair that experienced them. This is the basis of the empirical tests that we conduct.

To better discuss the implications of such an information shock in our model, suppose there are two pairs k and h, and all other pairs are indicated by the set l. Consider pair k. The information we provide pushes p_k downward. Suppose this push makes player Ak distrust (i.e., $E_1 \succ^{Ak} T_1$). Consider pair$h$. Suppose the information pushes p_h downward, but not enough to induce lack of trust (i.e., $T_1 \succ^{Ah} E_1$). Player Bh is trusted in the first period and must decide whether or not to reciprocate. Finally, suppose that all player As in the set l trust in the first period and that a subset of the corresponding player Bs reciprocate.

Although the information shock has no direct impact on player Bh, the social environment in which player Bh makes her decision is different than the environment in which p is not shocked downward. Player Bh makes her decision in an environment in which less reciprocation is likely to be observed because (a) player Ak did not trust, causing player Bk to be observed not to reciprocate, and (b) conditional on having been trusted, the player Bs in set l may defect more frequently as they observe the apparent nonreciprocation of player Bk.

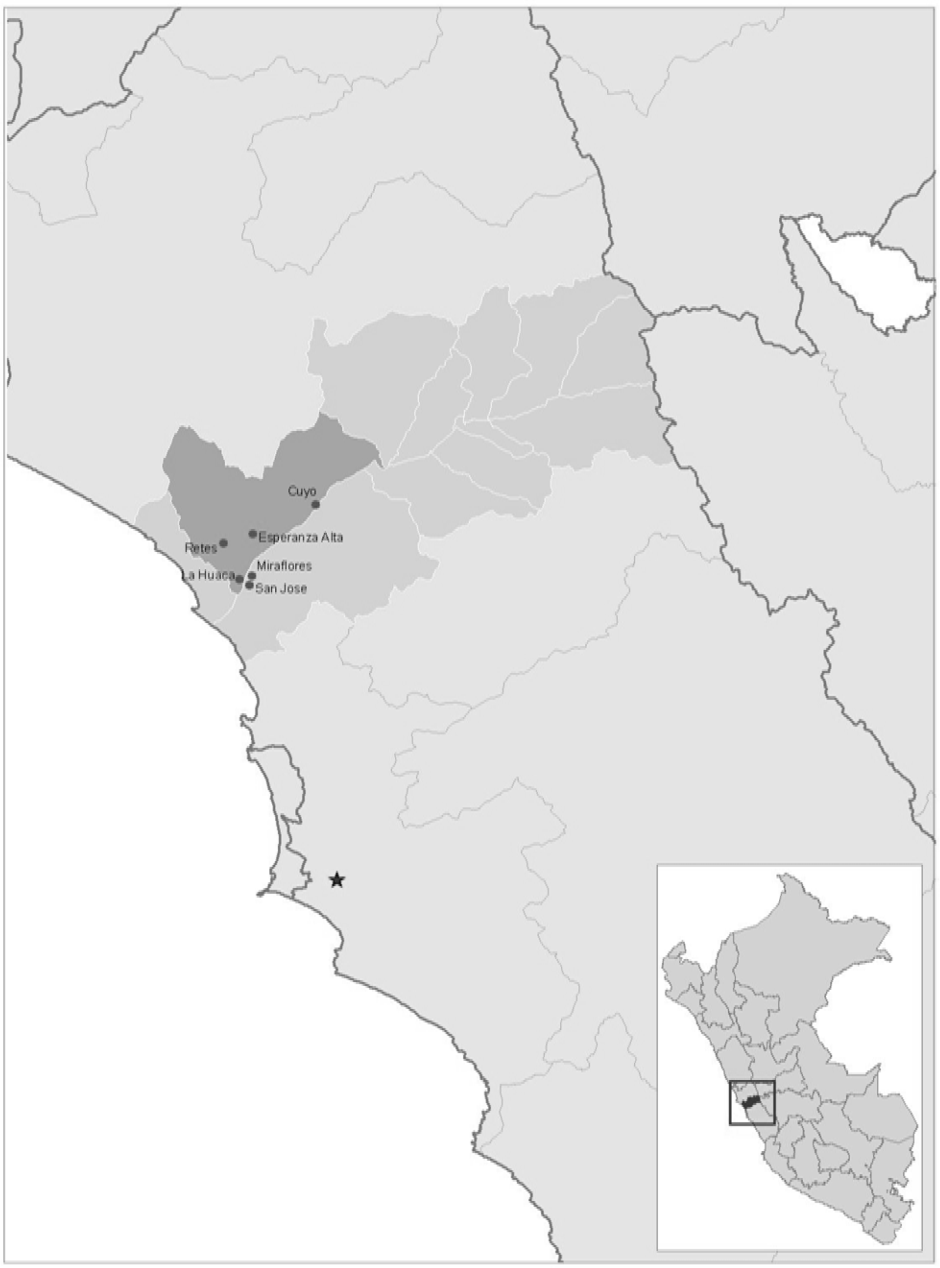
The fact that the information shock induces player As to trust less frequently causes player Bs to observe less reciprocation (as in pair k). This pushes M_{1t} upward and reduces player Bh's willingness to sacrifice her own for the other's monetary payoff, even though the shock did not affect the behavior of player Ah. Essentially, player Bh makes her decision in an environment in which reciprocation is less prevalent, which makes it less costly for her to defect. Note that M_{2t}, the proportion of player Bs observing her actions, is unchanged by the downward shock to p and r. Given the discussion of strategic behavior above, we expect round 2 behavior to be more closely driven by θ_t. Thus, we would expect the behavioral impact of changes in M_{1t} to be stronger in the second round.

In addition, we might expect alternative channels through which effects may occur. For example, player Bk may observe less reciprocation in the set l if a subset of player Bls react to less trust by either updating their beliefs about the types in the player A population or punishing their own player As to send a signal to the population of player As. For example, some player Bls may fail to reciprocate when trusted because they observe other player Bs (such as player Bk) not being trusted and, accordingly, believe that their corresponding player As are less likely to trust next period after having observed distrust on the part of other player As this period. Alternatively, some player B*l*s may fail to reciprocate in an attempt to punish player As for the distrust exhibited toward their peers this period. Although our model is silent in these respects, we note that both of these lines of reasoning predict effects in the first period of the game.

3. EXPERIMENTAL DESIGN

To test these hypotheses we conducted artefactual field experiments with 308 randomly selected individuals from seven rural communities surrounding the city of Huaral, 75 kilometers north of Lima (see Figure 3). In total, eight sessions were conducted.[9] Each session involved a group of around 18 player As and a group of around 18 player Bs located in separate communities.

Figure 3. Huaral communities in the intervention



[9] Prior to these sessions, one pilot session was conducted in the same communities for smaller stakes to test the experimental protocol. Given the much smaller stakes used, these data are not used in the analysis.

In five sessions a twice-repeated trust game (TG) was conducted. The TG was described in Section 2. In three sessions a slightly modified version of the same game—the modified trust game (MTG)—was conducted. The only difference between the TG and the MTG was that in the latter, player As received information about the likelihood that player Bs would reciprocate trust if trusted. We essentially shocked a main experimental parameter (player A's prior belief that player B would reciprocate) in order to create an instrument for identifying an endogenous social interaction effect. This approach can be compared to that of Casari, Ham, and Kagel (2007).

In the MTG all player As were told that in previous sessions almost half of player Bs chose not to reciprocate (this was based on the final-round behavior of the first experimental sessions of the TG), and those player As who were paired with player Bs who had chosen "keep all / send nothing" in the dictator game (discussed below) were informed of their partner's choice in that game. This information was given to reduce the likelihood that player As would choose to send. No information was conveyed to player Bs. The TG and MTG protocols were thus identical from player B's point of view.

The protocol for the TG and MTG is described in detail in the Appendix. Here we highlight two important features of the protocol followed.

Our primary aim is to assess how exogenously induced changes in the behavior player B observes alters her decision to reciprocate. Thus, it was important that we allow player Bs to observe their peers. To achieve this we stepped away from the standard (single- or double-blind) protocol of isolating experimental subjects. While subjects were instructed not to interact with each other, the experimental protocol allowed for visual observation of one's peers. This was facilitated by the use of white envelopes for keeping vouchers and yellow envelopes for sending vouchers. As the experimental protocol details further, however, player Bs were isolated from player As. Player Bs were located in one village, while player As were located in another.

We compare player B decisions across sessions to study the extent to which observing fewer people reciprocating trust (as a result of fewer people being trusted in the MTG sessions) makes an individual more or less likely to deviate from the norm of reciprocation. The key to our identification strategy is the introduction of an exogenous and credible information shock on player A's side, of which player B is unaware, which induces a change in the peer behavior that player B observes. This allows us to circumvent the problem of identification in the presence of what Manski (2000) terms the "reflection problem."

Although the TG and MTG protocols were identical for player Bs, it is possible for unobserved session-specific heterogeneity to cause variation in behavior across sessions. This can be controlled for in the empirical analysis by including session dummies. However, we can also use within-session heterogeneity in observability to construct individual-specific (rather than session-specific) measures of what was observed. To allow for this, seating was randomly determined at the beginning of the experiment session and held fixed throughout the session. The results section discusses the robustness of our results with regard to various spatial specifications.

In addition, in the week prior to participation in the trust games, all participants took part in a dictator game (DG). The DG is a two-player, one-shot, dichotomous-choice game in which the first mover has a choice between "keep all / send nothing" and "keep half / send half." The choice determines both the individual's own terminal payoff and the payoff of the individual's partner. All subjects played this game in the role of dictator. So subjects who played the role of player B in the main treatments also played the role of first mover in the DG. These data are used to control for players' raw other-regarding preferences in the analysis and to provide information in the MTG.

The DG was conducted during the listing of households, which occurred a week before the TG and MTG sessions were conducted. We chose to conduct the DG during listing for three reasons. First, conducting the DG days in advance mitigates potential confounding effects arising from subjects participating in several experimental treatments close together. Second, this ensures that the experimenter during the DG (i.e., the enumerator) is different from the person who actually pays the subject for his or her DG decision (i.e., the experiment assistant for the main treatment). Finally, this allows the MTG sessions to move faster, as the DG decisions are used as information during the MTG.

The DG involved the following steps. (1) The enumerator explained the game and payment procedures to the subject. The enumerator informed the subject that if he or she were selected to participate in the study, he or she would be paid for this choice. The enumerator encouraged the subject to, therefore, make his or her preferred choice. The enumerator also informed the subject that the choice would not affect the likelihood of being selected for participation in the study. (2) The subject was then handed a sheet of paper with the two options ("keep all / send nothing" and "keep half / send half") and an envelope. The instructions were to (a) circle the preferred choice in private, (b) fold the sheet of paper, (c) place the folded sheet of paper in the envelope, (d) seal the envelope, and (e) hand the envelope back to the enumerator. (3) The enumerator was explicitly informed not to interfere with the subject's decisionmaking process. Furthermore, the enumerator coded the subject's envelope with his or her unique household ID only after leaving the listed subject's household. (4) Listed subjects who participated in the experiment received payment for their DG decisions at the end of the TG or MTG, depending on which treatment they participated in. These earnings were received in addition to any earnings from participation in the main treatment.

4. DATA AND RESULTS

Data

Situated in the valley of the Chancay river, the Huaral area is one of Lima's main providers of fresh produce, poultry, and pork, which is why it is known as "Lima's pantry." Not surprisingly, the main income-generating activity for most of the households in Huaral is market-oriented agriculture. In spite of this, the majority of land parcels are small and poverty is still highly prevalent in the area.

The seven communities selected for the intervention were chosen based on (a) classification as rural by Peru's National Statistics Bureau (INEI), and (b) size. Selected communities had at least 100 households in the community.[10] Player A and player B sessions were conducted in separate communities simultaneously in order to guarantee that participants knew as little as possible about the individuals they had been paired with.

The experiments on average lasted two and a half hours, and average earnings were 34.08 soles (standard deviation: 16.88), which represents more than 6 percent of the local monthly minimum wage. Every participant responded to a short household survey near the end of his or her session, from which the basic characteristics of our sample were obtained. These are summarized in Table 1, compared by player role and type of game. T-tests of the differences in means between the types of game (TG and MTG) for each group of players (A and B) were provided in the table in order to determine whether the participants in the MTG sessions were different in observable characteristics from those in the TG sessions (even though participation in MTG and TG sessions was determined randomly).

Table 1. Sample means of basic characteristics by player and type of game

	Player A				Player B			
	Trust Game	Modified Trust Game	Difference		Trust Game	Modified Trust Game	Difference	
Female	0.687	0.702	-0.015		0.554	0.649	-0.095	
	(0.051)	(0.061)	(0.080)		(0.055)	(0.064)	(0.085)	
Age	44.517	36.381	8.136		44.428	39.825	4.603	
	(1.749)	(1.673)	(2.602)	***	(1.678)	(1.782)	(2.513)	*
Schooling	5.215	4.992	0.224		5.003	5.113	-0.110	
	(0.108)	(0.135)	(0.175)		(0.099)	(0.118)	(0.154)	
Any children	0.892	1.000	-0.108		0.940	0.893	0.047	
	(0.034)	(0.000)	(0.044)	**	(0.026)	(0.042)	(0.047)	
Household size	4.458	4.720	-0.262		4.554	4.875	-0.321	
	(0.202)	(0.262)	(0.331)		(0.174)	(0.292)	(0.320)	
Quecha-speaking mother	0.313	0.491	-0.202		0.325	0.228	0.097	
	(0.051)	(0.067)	(0.083)	**	(0.052)	(0.056)	(0.078)	
Father's schooling	5.048	4.200	0.848		4.819	4.571	0.248	
	(0.428)	(0.434)	(0.646)		(0.381)	(0.451)	(0.594)	
Catholic	0.819	0.807	0.012		0.892	0.842	0.049	
	(0.042)	(0.053)	(0.067)		(0.034)	(0.049)	(0.058)	
Rooms in house	3.892	3.140	0.752		4.169	3.375	0.794	
	(0.181)	(0.225)	(0.291)	**	(0.192)	(0.203)	(0.287)	***

[10] The communities were San Jose, Cuyo, Esperanza, La Huaca, La Caporala, Retes, and Miraflores.

Table 1. Continued

	Player A			Player B		
	Trust Game	Modified Trust Game	Difference	Trust Game	Modified Trust Game	Difference
Land (ha.)	1.495	0.332	1.164	1.010	0.482	0.528
	(0.369)	(0.172)	(0.494) **	(0.283)	(0.168)	(0.372)
Household annual income[b]	8.667	5.906	2.762	8.667	5.280	3.387
	(1.138)	(0.779)	(1.519) *	(1.146)	(0.549)	(1.457) **
Household wealth status[c]	4.494	4.460	0.034	4.590	4.911	-0.320
	(0.102)	(0.141)	(0.171)	(0.097)	(0.115)	(0.151) **
Ever paid in advance	0.157	0.080	0.077	0.145	0.071	0.073
	(0.040)	(0.039)	(0.060)	(0.039)	(0.035)	(0.055)
Ever received payment in	0.205	0.080	0.125	0.146	0.036	0.109
advance	(0.045)	(0.039)	(0.065) *	(0.039)	(0.025)	(0.052) **
Lent money often	1.518	1.360	0.158	1.518	1.250	0.268
	(0.065)	(0.080)	(0.104)	(0.067)	(0.058)	(0.095) ***
Sent in dictator game[d]	0.727	0.750	-0.023	0.803	0.750	0.053
	(0.051)	(0.063)	(0.082)	(0.049)	(0.083)	(0.093)
Observations	83	57		83	57	

Notes: Standard errors in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.
[b] In thousands of soles.
[c] Self-reported, 1 being "richest" and 6 being "poorest."
[d] Dictator game results were not available for 6 pairings in the Trust Game and 9 pairings in the Modified Trust Game.

Table 1 show that there are some significant differences among participants in TG and MTG sessions. Compared to the average player in an MTG session, the average player A in a TG session is older, is less likely to have children, is less likely to have a mother who is a native Quechua speaker, lives in a larger house, has more land, has a higher annual income, and is more likely to have received payments in advance. The average player B in a TG session is older, lives in a larger house, has a higher annual income, considers herself better off, has received payments in advance more often, and has lent money more often.

For our purposes, however, what matters is whether any of these differences have an effect on decisions to keep or send back money in the game, in the absence of the information treatment. We argue that the DG response from the listing stage of the experiment (see Appendix) is a good proxy for reciprocal tendencies, and we run a regression of this against the basic characteristics. The results are shown in Table 2. In the basic DG, we found that 24 percent of individuals responded that they would choose to keep all the money given to them (25 percent of player As and 23 percent of player Bs). We observe that only income and having lent money in the past are significantly related to the decision to send money in the DG. Given that there are some significant differences in these variables between players in TG and MTG sessions (particularly for player B participants), we use them as controls in all our estimations to control for possible selection bias on these observable characteristics.

Table 2. Determinants of sending behavior in dictator game

Dependent variable: 1 if sent, 0 if kept		
Female	0.022	
	(0.063)	
Age[b]	0.001	
	(0.002)	
Schooling[b]	0.035	
	(0.032)	
Any children	-0.022	
	(0.115)	
Household size	-0.006	
	(0.016)	
Mother's 1st language: Quechua	-0.052	
	(0.061)	
Father's schooling	0.001	
	(0.009)	
Catholic	-0.029	
	(0.082)	
Rooms in house	-0.001	
	(0.018)	
Land (ha.)	0.017	
	(0.013)	
Household annual income	0.007	
	(0.004)	**
Household self-reported wealth status	-0.001	
	(0.032)	
Ever paid in advance	0.007	
	(0.088)	
Ever received payment in advance	-0.008	
	(0.086)	
Lent money often	0.089	
	(0.052)	*
Constant	0.396	
	(0.326)	
Observations[c]	246	
R^2	0.08	

Notes: Standard errors in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.
[b] Imputed missing values for 29 observations.
[c] For this regression we pooled together the responses of all participants (player As and Bs).

Figures 4, 5, and 6 show the game trees (full, and separating sessions with and without information), indicating how the 140 pairs of participants are distributed along the decision process. Only slightly more (26 percent) player Bs chose to keep the money in round 1 than in the DG; however, a much higher proportion, 36 percent, chose to keep the money in round 2. The game tree suggests that in the MTG sessions there was a difference in both player A and B behavior: fewer player As sent and, conditional on being sent money, fewer player Bs reciprocated in the second and final round. It is these differences, particularly the latter, that we seek to explain in the following analysis.

Figure 4. Distribution of moves along twice-repeated trust game (all sessions)

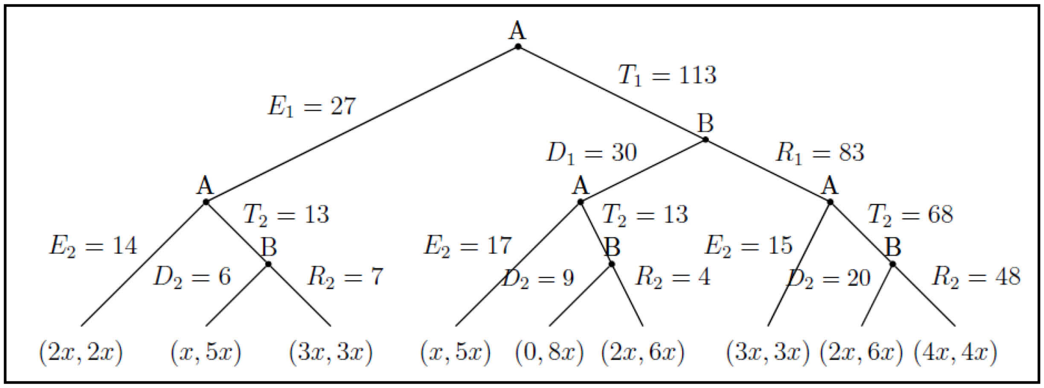


Figure 5. Distribution of moves along twice-repeated trust game ("no info" sessions)

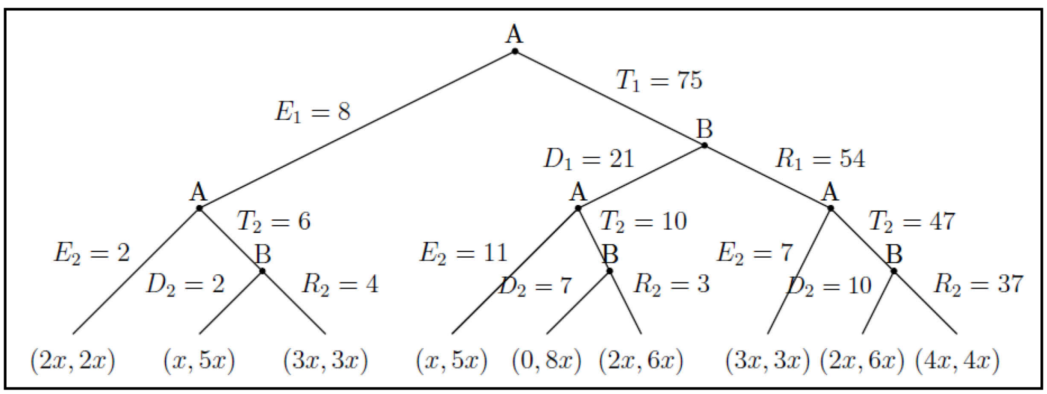
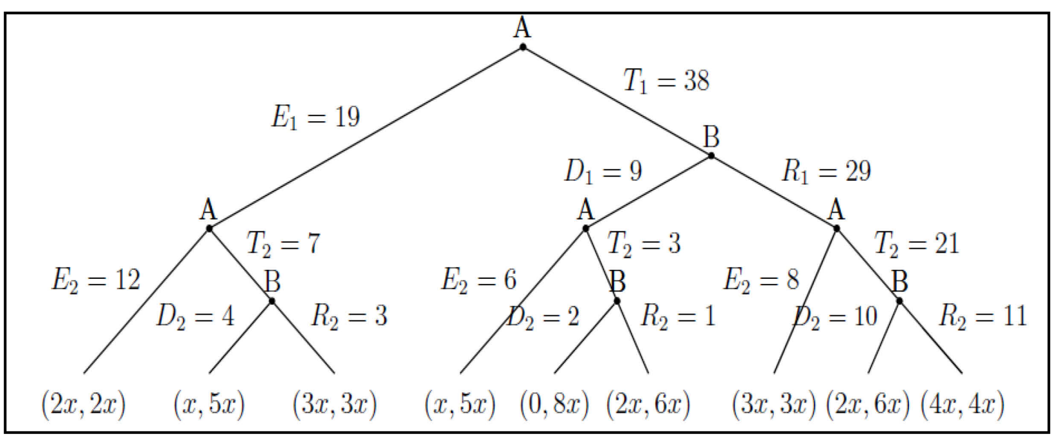


Figure 6. Distribution of moves along twice-repeated trust game ("info" sessions)



Basic Relationships

We begin by assessing the impact of information on player A behavior. We would expect that being provided with the information that one has been partnered with a self-regarding type would discourage player A from sending (the downward shock in p described in the final subsection of Section 2), and the game tree also seems to suggest that this is the case (Figures 5 and 6). This is tested in Tables 3 and 4, which regress trusting behavior on whether information was provided, the level of stakes, and individual controls. It is important to control for the history of round 1 in assessing round 2 results. To do this, we construct the history variable as 1 if player B defected in round 1, that is, behaved untrustworthily, and 0 if she reciprocated or had not been trusted by player A for starters. The key finding from these tables is that introducing information greatly reduces the probability that player A will trust. This exogenously affects the environment in which player Bs make their decisions (given that player B is unaware player A was provided with information), which is crucial for our analysis.

Although information has strong effects in both rounds, the role of information changes between rounds 1 and 2. In round 1 it is personal information that plays the largest role in determining behavior, with no impact of information on trusting behavior for those who were not provided with information on the person they were playing with. In round 2, the provision of information now has an impact at the group level, with those who were provided personal information being no more likely to exit than other player As in the room who were not provided with information. The results suggest that when player A observes more nonreciprocal behavior among the partners of his fellow player As (as is the case in the information treatments as shown below), he will choose not to send.

Table 3. Player A in round 1

Dependent variable: 1 if trust, 0 if exit			
Stake = 10	-0.015	-0.010	-0.026
	(0.065)	(0.065)	(0.067)
Personal information	-0.377	-0.460	
	(0.113) ***	(0.102) ***	
Information	-0.119		-0.231
	(0.074)		(0.068) ***
Household annual income	0.005	0.005	0.003
	(0.004)	(0.004)	(0.004)
Lent money often	0.013	0.016	0.037
	(0.057)	(0.057)	(0.059)
Constant	0.854	0.805	0.833
	(0.102) ***	(0.098) ***	(0.105) ***
Observations	133	133	133
R^2	0.18	0.16	0.10

Notes: Standard errors in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 4. Player A in round 2

Dependent variable: 1 if trust, 0 if exit						
Stake = 10	0.241		0.248		-0.241	
	(0.073)	***	(0.073)	***	(0.072)	***
Personal information	0.296		-0.075			
	(0.129)		(0.117)			
Information	-0.154				-0.146	
	(0.083)	*			(0.074)	*
B did not reciprocate	-0.426		-0.433		-0.423	
in round 1	(0.074)	***	(0.075)	***	(0.073)	***
Household annual income	-0.002		-0.001		-0.002	
	(0.004)		(0.004)		(0.004)	
Lent money often	0.048		0.052		0.045	
	(0.064)		(0.064)		(0.063)	
Constant	0.703		0.642		0.704	
	(0.115)	***	(0.111)	***	(0.114)	***
Observations	133		133		133	
R^2	0.29		0.27		0.29	

Notes: Standard errors in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

To analyze player B's decision to defect or reciprocate, we regress the choice to defect on the same controls, and additionally on M_{1t}, the proportion of other player Bs who do not reciprocate (whether they were trusted or not) in each round. We now know M_{1t} is driven in part by the information provided in player A's sessions. However, given that our model is one of interdependence between the choices of player Bs, we cannot assume that, for a given player B, the proportion of other player Bs in the room who chose to defect was not in turn caused by the behavior of that player (what Manski [2000] refers to as the reflection problem). It is thus necessary to instrument for the proportion of player Bs observed to defect. Given that this proportion increases exogenously with the provision of information (as fewer player As trusted), we use a dummy of the provision of information as an instrument. In particular, we use a dummy that takes the value of 1 if player B was in a session in which, unbeknownst to her, information was provided to player As, thereby discouraging trusting. Ordinary least squares (OLS) and instrumental variable (IV) results for first-round behavior are presented in Table 5, and OLS and IV results for second-round behavior are presented in Table 6.

Table 5 indicates that in round 1 M_{1t}, the proportion of other participants not reciprocating, has no significant effect on player B's behavior. This is true for both the OLS and instrumented regressions. However, in round 2 M_{1t} has a strong effect on player B's behavior. The more players observed not reciprocating, the more probable it is that player B will decide to defect when having the choice.[11] The insignificance of social influences in the first round, despite their significance in the second, is consistent with the model predictions, which suggest that strategic motives play a role in whether or not a self-regarding individual reciprocates or defects at this stage.

[11] We run regressions (1) and (3) using the proportion of other players not reciprocating in the previous round instead of the concurrent round. The effect seems to be immediate, as what happens in the room in the present round seems to explain more of the variation in behavior than does the behavior of others in previous rounds.

Table 5. Player B in round 1

Dependent variable: 1 if defect, 0 if reciprocate[b]	(1) Ordinary Least Squares	(2) Ordinary Least Squares	(3) Instrumental Variables	(4) Instrumental Variables
Proportion of Bs not	0.069	0.067	-0.777	-0.787
reciprocating, M_1[c]	(0.321)	(0.324)	(0.676)	(0.685)
Proportion of Bs		0.010		0.059
not seen, M_2		(0.162)		(0.170)
Stake = 10	0.063	0.064	0.164	0.166
	(0.096)	(0.097)	(0.121)	(0.123)
Household annual income	-0.003	-0.003	-0.003	-0.003
	(0.005)	(0.005)	(0.005)	(0.005)
Lent money often	-0.112	-0.112	-0.136	-0.137
	(0.077)	(0.077)	(0.081) *	(0.081) *
Constant	0.369	0.364	0.713	0.681
	(0.183) **	(0.203) *	(0.305) **	(0.305) **
Observations	112	112	112	112
R^2	0.04	0.04		

Notes: Standard errors in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

[b] Conditional on being trusted by player A.

[c] Defecting (by own choice) or because they were not trusted by player A.

As discussed in the final subsection of Section 2, player B's behavior may also be driven by whether or not other player Bs are trusted. In other words, player B may observe other player Bs not reciprocating purely because they were never trusted. This is different than a situation in which player B is trusted and decides to defect. If player B is indeed reacting to this information, the main story would be one of updating prior beliefs about emotional states in the player A population. We believe this story to be unlikely for a few reasons. First, player B already knows the action that her paired player A took when she takes her decision. So, updating is less relevant in this context, particularly since players knew that they were playing with the same person in both rounds. Second, even if player B were reacting to this information, we would expect her to be more likely to defect in the first round in anticipation of her paired player A exiting in the next round. Given that our main effects are for the second round, we think this is an implausible story. Finally, we ran an auxiliary regression including the number of player Bs not reciprocating, the number being trusted, and an interaction term between these two terms, which showed that the number of player Bs being trusted has no direct effect on defecting behavior, but only an indirect one through its impact on reciprocation.

In columns (2) and (4) of Tables 5 and 6 we also include the proportion of people sitting behind a given player as a measure for M_{2t} in the model. Seating was randomly assigned to individuals so variation in M_{2t} was randomly determined. Seating remained constant between rounds, that is, $M_{2t} = M_2$. As the proportion of people sitting behind the player increases, we would expect the probability that the player adheres to the norm of reciprocity rises (as the disutility that arises from being observed to deviate from the social norm increases). Again, in round 1 this measure of the social environment is insignificant in explaining player behavior. However, in round 2 we observe that the players' location in the room does have an influence on reciprocating behavior, with those located at the front of the room being more inclined to reciprocate than those located at the back of the room, where their actions are less observed.

Table 6. Player B in round 2

Dependent variable: 1 if defect, 0 if reciprocate[b]	(1) Ordinary Least Squares	(2) Ordinary Least Squares	(3) Instrumental Variables	(4) Instrumental Variables
Proportion of Bs not	0.606	0.622	0.692	0.770
reciprocating, M_1[c]	(0.195) ***	(0.192) ***	(0.329) **	(0.325) **
Proportion of Bs		-0.332		-0.338
not seen, M_2		(0.179) *		(0.180) *
Stake = 10	-0.011	0.001	-0.015	-0.006
	(0.108)	(0.107)	(0.109)	(0.108)
A trusted in round 1	0.259	0.211	0.263	0.217
	(0.183)	(0.182)	(0.184)	(0.183)
B did not reciprocate	0.385	0.364	0.385	0.364
in round 1[c]	(0.141) ***	(0.140) ***	(0.141) ***	(0.140) **
Household annual income	-0.002	-0.002	-0.002	-0.001
	(0.005)	(0.005)	(0.005)	(0.005)
Lent money often	-0.007	-0.009	-0.003	-0.003
	(0.077)	(0.077)	(0.081) *	(0.081) *
Constant	-0.254	-0.020	-0.308	-0.108
	(0.281)	(0.305)	(0.326)	(0.343)
Observations	94	94	94	94
R^2	0.18	0.21	0.18	0.21

Notes: Standard errors in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

[b] Conditional on being trusted by player A.

[c] Defecting (by own choice) or because they were not trusted by player A.

Does Observed Behavior Increase or Decrease Reciprocity?

Before assessing the robustness of our results, we first explore whether observed behavior is encouraging individuals to conform to or deviate from their previously disclosed preference for reciprocating. One way to test this idea is to find a proxy for what the individual would do in the absence of the group. We can think of this as a measure of the participant's true choice. As per the model, this reflects the optimal choice of x^B for utility given by $u^B(x^A, x^B) = x^B + \theta|_{a,\,M_{1t}=0,\,M_{2t}=0}\, x^A$ (i.e., utility with no peer effects). However, when the participant has to choose in a group setting, he or she might prefer to conform or deviate from this preference, depending on what he or she observes others doing. As a proxy for what the individual might do in the absence of the group, we use a preference for equality that was expressed in the choices made in the DG prior to participating in the TG or MTG.

In Table 7 we test the effect of the DG results and its interaction with the proportion not reciprocating in the room. We can appreciate in the second column that the proportion not reciprocating appears to matter for those who sent in the DG, while nonsenders (in the DG) are unaffected. This is true only for round 2 behavior.[12] In Table 8 we constructed a new dependent variable: 1 if the player's response diverges from what she did in the DG and 0 otherwise. This measure of divergence is regressed on the proportion of other player Bs not reciprocating and the usual set of controls. The first two columns present results for all player Bs, while the second two columns include only those who chose to send in the DG.[13] The results in column 3 show that for those participants who claim they would send in the DG, the probability of diverging increases as more people around them do not reciprocate. This confirms that, in the second round, player B is more likely to diverge from her DG response when she observes others not reciprocating.[14]

Table 7. Comparing player B in round 2 with dictator game (DG) results

B defects, round 2	(1) OLS	(2) OLS	(3) IV	(4) IV
Sent in DG	0.111	-0.045	0.080	0.206
	(0.151)	(0.346)	(0.160)	(0.609)
Proportion of Bs not	0.549		0.835	
reciprocating, M_1	(0.246)**		(0.501)	
M_1 × Sent in DG		0.604		0.645
		(0.271)**		(0.518)
M_1 × Kept in DG		0.255		0.890
		(0.639)		(1.211)
Proportion of Bs not	-0.403	-0.400	-0.377	-0.393
seen, M_2	(0.233)*	(0.234)*	(0.239)	(0.240)
Stake = 10	0.087	0.111	0.053	0.054
	(0.138)	(0.147)	(0.149)	(0.174)
Household annual income	-0.002	-0.002	-0.002	-0.002
	(0.011)	(0.011)	(0.011)	(0.011)
Lent money often	-0.005	-0.011	-0.013	-0.004
	(0.099)	(0.100)	(0.101)	(0.104)
A trusted in round 1	0.005	-0.021	0.015	0.025
	(0.253)	(0.260)	(0.256)	(0.276)
B did not reciprocate	0.163	0.156	0.185	0.178
in round 1	(0.220)	(0.222)	(0.226)	(0.228)
Constant	0.105	0.243	-0.013	-0.047
	(0.384)	(0.475)	(0.429)	(0.673)
Observations	65	65	65	65
R^2	0.20	0.20	0.18	0.18

Notes: Standard errors in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

[12] In round 1, the DG results did not have any explanatory power, either when entered directly or interacted with the proportion of people sending.

[13] We cannot run a regression for those who chose to keep in the DG due to insufficient observations.

[14] Again, a similar exercise for round 1 behavior yielded no significant results, and these results are omitted to save space.

Table 8. Explaining divergence from dictator game results

B diverges, round 2	(1) All Ordinary Least Squares	(2) All Instrumental Variables	(3) Sending types Ordinary Least Squares	(4) Sending types Instrumental Variables
Proportion of Bs not	0.177	0.124	0.586	0.714
reciprocating, M_1	(0.279)	(0.552)	(0.282) **	(0.551)
Proportion of Bs not	-0.310	-0.315	-0.484	-0.470
seen, M_2	(0.267)	(0.272)	(0.281) *	(0.286)
Stake = 10	0.042	0.049	0.139	0.135
	(0.158)	(0.170)	(0.156)	(0.157)
Household annual income	-0.010	-0.010	0.000	0.000
	(0.012)	(0.012)	(0.012)	(0.012)
Lent money often	-0.040	-0.038	-0.037	-0.043
	(0.113)	(0.114)	(0.111)	(0.113)
A trusted in round 1	-0.042	-0.045	-0.120	-0.129
	(0.290)	(0.291)	(0.327)	(0.329)
B did not reciprocate	-0.055	-0.061	0.010	0.016
in round 1	(0.251)	(0.256)	(0.286)	(0.288)
Constant	0.686	0.712	0.380	0.319
	(0.426)	(0.489)	(0.459)	(0.512)
Observations	65	65	65	65
R^2	0.06	0.06	0.18	0.18

Notes: Standard errors in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

These results suggest that people who would normally reciprocate are encouraged not to when they observe others deviating from the norm of reciprocation.[15] However, we note that in both cases, once the proportion of other players not reciprocating is instrumented, the variables of interest become insignificant (although the sign remains consistent). In the following, we explore alternative measures of the proportion of people observed not to reciprocate. However, first, we consider whether there is any evidence of imitation as a result of limited understanding of how to play the game.

Testing for Imitation in the Face of an Unknown Situation

A participant might be influenced by what other players in the session are doing if he or she does not understand the game or lacks the ability to decide on his or her own. In this case, a participant may choose to imitate the behavior of others assuming that this is indeed optimal behavior for the novel situation with which he or she is presented. We attempt to proxy the lack of ability to decide with education, age, and mother's native language. If the imitation hypothesis is right, we would expect that the influence of other participants (the proportion not reciprocating) should be less for players with higher ability (more educated, younger, or with Spanish-speaking mothers). The interaction between the proportion not reciprocating in the room and the proxy for ability would be significant and negatively correlated with reciprocating.

[15] Given the small numbers, it is hard to tell what the impact of group behavior is on those who would normally keep.

Table 9 shows the results for these tests and offers little support for the imitation hypothesis. Although education does appear to play a role in round 2 in that the R-squared increases to 0.29, the level of schooling and the interaction between the proportion of keepers and schooling is not significant. This suggests that imitation does not have a stronger effect for those who we might expect to be less likely to understand the game.[16]

Table 9. Testing for imitation in round 2

B defects, round 2	(1)		(2)		(3)	
Proportion of Bs not reciprocating, M_1	1.629		0.515		0.647	
	(0.977)		(0.647)		(0.234)	***
Schooling	-0.054					
	(0.116)					
M_1 × Schooling	-0.193					
	(0.190)					
Age			0.002			
			(0.008)			
M_1 × Age			0.002			
			(0.014)			
Quechua-speaking mother					0.049	
					(0.251)	
M_1 × Quechua-speaking mother					-0.116	
					(0.492)	
Proportion of Bs not seen, M_2	-0.436		-0.350		-0.335	
	(0.177)	**	(0.184)	*	(0.183)	*
Stake = 10	-0.026		-0.001		0.012	
	(0.102)		(0.107)		(0.118)	
Household annual income	0.001		-0.003		-0.002	
	(0.005)		(0.005)		(0.005)	
Lent money often	-0.018		-0.011		-0.010	
	(0.081)		(0.085)		(0.086)	
A trusted in round 1	0.042		0.223		0.213	
	(0.182)		(0.184)		(0.185)	
B did not reciprocate in round 1	0.304		0.386		0.362	
	(0.135)	**	(0.143)	***	(0.143)	**
Constant	0.462		-0.114		-0.037	
	(0.663)		(0.465)		(0.332)	
Observations	94		94		94	
R^2	0.18		0.21		0.18	

Notes: Standard errors in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

[16] It is possible that education could be capturing an income effect. However, interestingly (and rather surprisingly), the effect of education on household income is not significant when we estimate a basic earnings equation. A possible explanation for this might be the high number of women not participating in the labor market in our sample of participants, as well as the low returns to education in the types of jobs the household members in our sample are engaging in.

Determining who is Observed

Thus far we have considered only the impact of the average behavior of other player Bs in influencing player B's choice to reciprocate. However, given that the location of the players varies in the room, we would expect players' ability to observe the actions of other players to vary. As a result, a measure of the average behavior of other player Bs may not be the appropriate measure for what a given player observes. In this subsection we test the robustness of our results for alternate measures. First, we define the following:

- Players seen: The number of participants in the same row and in the rows in front of the player
- Proportion of players seen who do not reciprocate: The number of players seen not reciprocating divided by the number of players seen

In columns 1 and 2 of Tables 10 and 11 we use the proportion of players seen who do not reciprocate in place of the total proportion of other player Bs who do not reciprocate. When including the proportion of players seen who do not reciprocate, we observe very similar results to those presented earlier for the total proportion of other players in the room who do not reciprocate. However, in these regressions the proportion of players not seen no longer has a significant effect on behavior. This suggests that the significance of the proportion of players not seen in previous regressions arose as a result of the fact that an individual's position in the room determines the degree to which average behavior was observed. The insignificance of the proportion not seen in the Table 10 regressions indicates that shame is not such an important determinant of player behavior.

We further explore this issue by constructing a more flexible (and less dichotomous) variable to capture the impact of the players' ability to see other players' actions (given their location in the room). As before, the players not seen by any given player are those sitting in the rows behind him or her. However, we no longer assume that the player is able to observe the actions of everybody else in the same row and in the rows in front of him or her, at least not to the same degree. Building concentric semicircles around each player, we can assume that each of them can observe the actions of the other players in the different semicircles around him or her with varying degrees of difficulty. Evidently, the further away the other player is, the harder it will be to observe his or her actions.

The logic behind the concentric semicircles measure is better understood by looking at Figure 7. From player 15's point of view, the players in the back row (17 to 20) are in his or her "blind spot." Players 10, 11, 12, 14, and 16 are immediately next to him or her and are the most observable. Players in the second row (5 to 8) and players 9 and 13 have one player in between player 15 and themselves, so we can assume their actions are slightly more hidden. Players in the front row (1 to 4) are two players away from player 15, and hence even harder to observe. Thus, we build this measure by giving different weights to the players seen not reciprocating depending on the concentric semicircle they belong to.[17]

Columns 3 and 4 of Tables 10 and 11 include the concentric semicircles measure assuming only the first semicircle around the player is visible to him or her, while columns 5 and 6 assume the first semicircle is four times as visible as the other semicircles. We find that this last specification improves the R-squared considerably, giving us more robust results.

[17] Notice that our initial measure, the proportion of players seen who do not reciprocate, used in columns 1 and 2 of Tables 10 and 11, is just a special case of the concentric semicircles measure in which all the circles have the same weight.

Table 10. Robustness checks: Improving the measure of what is observed (dependent variable is defection in round 2)

B defects, round 2	(1) Ordinary Least Squares	(2) Instrumental Variables	(3) Ordinary Least Squares	(4) Instrumental Variables	(5) Ordinary Least Squares	(6) Instrumental Variables
Proportion of Bs seen	0.570	0.668				
not reciprocating	(0.181) ***	'(0.282) **				
Semicircle			0.117	0.194	0.083	0.094
measure			'(0.038) ***	'(0.084) **	'(0.023) ***	'(0.039) **
Stake = 10	0.032	0.032	0.023	0.018	0.003	0.000
	(0.106)	'(0.107)	'(0.107)	'(0.109)	'(0.105)	'(0.106)
Household annual	-0.002	-0.001	-0.003	-0.003	-0.002	-0.001
income	(0.005)	'(0.005)	'(0.005)	'(0.005)	'(0.005)	'(0.005)
Lent money	0.006	0.014	0.013	0.045	0.013	0.020
often	(0.084)	'(0.086)	'(0.085)	'(0.092)	'(0.083)	'(0.085)
A trusted	0.193	0.194	0.144	0.117	0.185	0.184
in round 1	(0.183)	'(0.183)	'(0.184)	'(0.190)	'(0.180)	'(0.180)
B did not reciprocate	0.360	0.359	0.330	0.307	0.347	0.345
in round 1	(0.140) **	'(0.140) **	'(0.141) **	'(0.146) **	'(0.138) **	'(0.139) **
Proportion of Bs	-0.201	-0.183	-0.026	0.159	-0.174	-0.157
not seen	(0.183)	'(0.187)	'(0.201)	'(0.273)	'(0.181)	'(0.188)
Constant	-0.047	-0.115	-0.031	-0.285	-0.071	-0.129
	(0.310)	'(0.345)	'(0.309)	'(0.400)	'(0.303)	'(0.344)
Observations	94	94	94	94	94	94
R^2	0.21	0.21	0.21	0.17	0.23	0.23

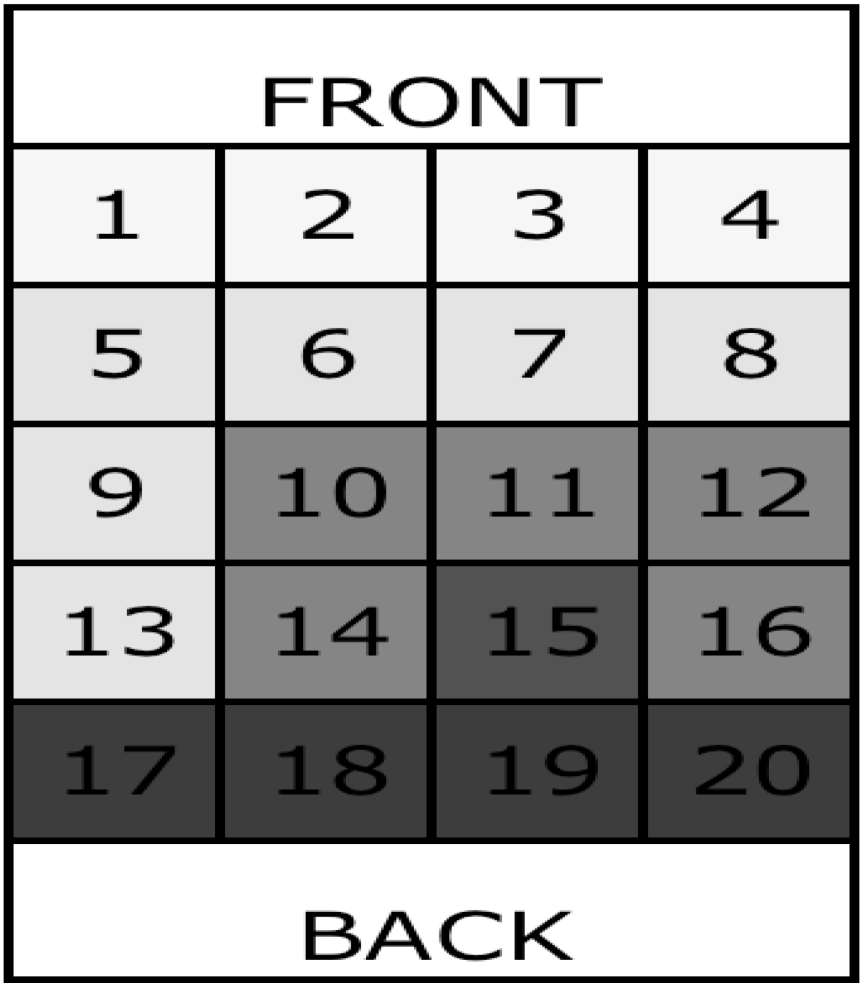
Notes: Standard errors in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 11. Robustness checks: Improving the measure of what is observed (dependent variable is divergence from dictator game results in round 2)

B diverges, round 2	(1) Ordinary Least Squares	(2) Instrumental Variables	(3) Ordinary Least Squares	(4) Instrumental Variables	(5) Ordinary Least Squares	(6) Instrumental Variables
Proportion of Bs seen	0.529	0.558				
not reciprocating	'(0.258) **	'(0.430)				
Semicircle			0.124	0.204	0.090	0.091
measure			'(0.062) *	'(0.160)	'(0.038) **	'(0.069)
Stake = 10	0.168	0.168	0.131	0.112	0.107	0.106
	'(0.156)	'(0.156)	'(0.157)	'(0.163)	'(0.155)	'(0.159)
Household annual	0.001	0.002	-0.001	-0.002	0.000	0.000
income	'(0.012)	'(0.012)	'(0.012)	'(0.012)	'(0.012)	'(0.012)
Lent money	-0.013	-0.013	-0.019	-0.024	-0.025	-0.025
often	'(0.110)	'(0.110)	'(0.111)	'(0.113)	'(0.109)	'(0.109)
A trusted	-0.138	-0.141	-0.122	-0.149	-0.107	-0.107
in round 1	'(0.328)	'(0.330)	'(0.328)	'(0.338)	'(0.322)	'(0.323)
B did not reciprocate	-0.001	0.000	0.015	0.036	0.039	0.040
in round 1	'(0.286)	'(0.287)	'(0.287)	'(0.295)	'(0.283)	'(0.286)
Proportion of Bs	-0.369	-0.359	-0.159	0.089	-0.301	-0.297
not seen	'(0.292)	'(0.314)	'(0.341)	'(0.574)	'(0.294)	'(0.333)
Constant	0.336	0.318	0.286	0.046	0.265	0.258
	'(0.467)	'(0.512)	'(0.478)	'(0.659)	'(0.464)	'(0.530)
Observations	53	53	53	53	53	53
R^2	0.18	0.18	0.17	0.14	0.20	0.20

Notes: Standard errors in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

Figure 7. Concentric semicircles from player 15's point of view



5. CONCLUSION

In this paper we present results from an artefactual field experiment conducted in rural Peru that considers how observing deviation from a norm of reciprocity influences an individual's decision to reciprocate. Empirically identifying the processes that give rise to a positive relationship between the propensity of an individual to behave in a certain way and the prevalence of that behavior in the group is difficult (Manski 2000). Possible explanations include influence of observed behavior, observed characteristics of group members, and common characteristics across individuals. We use exogenous variation in the average observed behavior of the group to identify the influence of observed behavior on individual decisionmaking. We find that the probability that an individual will deviate from a norm of reciprocity increases with the number of others observed to deviate. We posit a model that suggests that this endogenous interaction arises as a result of preference interactions between group members: as more group members are observed to deviate, the cost of deviation for any individual falls, resulting in a higher propensity to deviate.

We also used random variation in the position of group members to assess how observability affects behavior. We did not find that individuals who were more likely to be observed were less likely to reciprocate once we had controlled for what the individual observed.

In documenting how an external shock to the number of those observed to deviate from the norm of reciprocity encourages others to deviate, the paper endeavors to provide some insight into how a norm of reciprocity can develop or unravel when individuals are learning behavior in a new market institution. Further analysis on how behavior is influenced by the relationship between those who are observing and those who are observed to deviate would be a nice extension to this analysis.

APPENDIX: EXPERIMENTAL PROCEDURES

The trust game (TG) and modified trust game (MTG) were conducted according to the following procedures:

- Location and arrival: Player As were in one geographical location (i.e., village A) and player Bs were in a different geographical location (i.e., village B). We chose this procedure in order to prevent player As and player Bs from learning each other's identities at the time of the experiment and thus behaving differently. On average two hours before the experiment session started, enumerators would locate selected participants within their respective villages, informing them of the exact time and location of the study. Subjects were instructed to bring photo identification. The experiment session consisted of five stages: (1) sign-in, (2) explanation, (3) decisionmaking, (4) survey, and (5) payment. Each stage is described further below, with approximate durations in parentheses.
- Sign-in (15 minutes): Upon arrival, each subject would present his or her photo ID and be signed in. The participant would then draw a number out of a bag. This number randomly determined his or her seat and pair number, that is, the individual's location and pair throughout the experiment session. Subjects were not separated by dividers. Thus, random seating determined their degree of "observability" and "observableness" during the experiment. The layout of the sessions was typically the same. The experimenter was located at the front of the room, with three to five rows of four subjects spread across the room and the assistant experimenter in an adjacent room or hallway. Once all the subjects were seated, the explanation would begin.
- Explanation (45 minutes): Each experimenter explained the procedures for playing the TG in the same manner. Subjects were informed that they were players A or B. They were then informed that they would be playing a game twice with another person in another village of Huaral. They were informed that they would not learn the identity of this person and vice versa. Subjects were informed that the game would entail the following. Each player (i.e., player A and player B) would receive one voucher that was worth an amount x.[18] Upon receipt of the voucher, player A would move first. Player A had one of two choices: "send all / keep nothing" (T_t) or "keep all / send nothing" (E_t). If player A chose E_t, then that stage of the game would end. In other words, each player would keep his or her voucher of x and wait for the next play of the game. If player A chose T_t, then player B would receive three additional vouchers (each worth x) for a total of four vouchers in the amount of $4x$. In turn, player A would be left with 0. Player B now had two choices: "send half / keep half" (R_t) or "keep all / send nothing" (D_t). If player B chose R_t, then each player would end this stage with two vouchers in the amount of $2x$. If player B chose D_t, then player B would end the stage with $4x$ and player A with 0. Since a substantial proportion of the subjects were expected to have difficulty reading, subjects were instructed orally. In order to maintain consistency throughout the sessions, both experimenters maintained the same script throughout their respective sessions. These scripts were identical across player A and B sessions, with the exception of the MTG sessions, where additional information was given to the player As. The information on the proportion of player Bs that had reciprocated in a prior session was publicly announced, while the individual-specific information was relayed to each selected individual privately. Once the game had been explained, the subjects were quizzed on their understanding by presenting them with several hypothetical scenarios. In particular, the experimenter would propose hypothetical strategies and request players'

[18] The experimenter explained in detail that these vouchers would be exchanged for real money at the end of the session. The treatments calibrated x at 5 or 10 Peruvian soles.

feedback on their set of available moves or monetary payoffs. This served as an indication of issues that needed clarification prior to playing the game. Finally, subjects were instructed how to reveal their preference during the game. Subjects would receive two envelopes: one white and one yellow. The white envelope was to be used to "keep" vouchers and the yellow envelope was to be used to "send" vouchers. This enabled pairs to observe each others' actions.

- Decisionmaking (45 minutes): Player As were instructed to reveal their preference by either placing the voucher in the yellow envelope or not. While players were instructed to not communicate with each other, they were not visually isolated from their peers. Experimenter A collected all the yellow envelopes, placed them in an accordion folder (organized/coded by seat number) and delivered the folder to assistant experimenter A. Assistant experimenter A registered the decisions and called assistant experimenter B to transfer the decisions between villages. Assistant experimenter B registered and confirmed the decisions by repeating them, placed the corresponding number of vouchers (either three or zero) in the yellow envelopes, and placed the envelopes in the corresponding slots of another identical accordion folder. Experimenter B handed out the yellow envelopes and instructed the player Bs to check the contents of the yellow envelopes. Experimenter B asked the player Bs to reveal their preferences according to the contents of the yellow envelope. In particular, those player Bs who were sent vouchers had a choice to make. Like the player As previously, they would reveal their preferences by putting two vouchers in the yellow envelopes or not. Those player Bs who were not sent vouchers had no decision to make and placed their one voucher in the white envelope. Experimenter B collected the yellow envelopes, placed them in the accordion folder, and handed the folder to assistant experimenter B. Assistant experimenter B registered the decisions and called assistant experimenter A, who in turn registered and confirmed the decisions, and then placed the number of vouchers in the yellow envelopes. Experimenter A then handed out the yellow envelopes and instructed the player As to review the contents of their yellow envelopes. Any remaining vouchers at the end of the stage went into the white envelopes. This process was repeated twice.
- Survey (30 minutes): All subjects participated in a short household survey. This survey was conducted by the enumerators.
- Payment (15 minutes): Sessions lasted on average two and one half hours. Upon completion of the session, subjects were paid in private by the assistant experimenter for the following: (a) session earnings, (b) show-up earnings, (c) survey earnings, and (d) dictator game (DG) earnings.

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IFPRI HEADQUARTERS

2033 K Street, NW
Washington, DC 20006-1002 USA
Tel.: +1-202-862-5600
Fax: +1-202-467-4439
Email: ifpri@cgiar.org

IFPRI ADDIS ABABA

P. O. Box 5689
Addis Ababa, Ethiopia
Tel.: +251 11 6463215
Fax: +251 11 6462927
Email: ifpri-addisababa@cgiar.org

IFPRI NEW DELHI

CG Block, NASC Complex, PUSA
New Delhi 110-012 India
Tel.: 91 11 2584-6565
Fax: 91 11 2584-8008 / 2584-6572
Email: ifpri-newdelhi@cgiar.org