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Sustaining Group Reputation

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Abstract

When individuals trade with strangers, there is a temptation to renege on agreements. If repeated interaction or exogenous enforcement are unavailable, societies often solve this problem via institutions that rely on *group*, rather than individual, reputation. Groups can employ two mechanisms to uphold reputation that are unavailable to individuals: information sharing and in-group punishment. We design a laboratory experiment to distinguish the roles of these mechanisms when individual reputations are unobservable. Subjects are split into groups and play a trust game with random re-matching, where only the group identity of one's partner is known. Treatments differ by whether information about group members' transactions is shared and whether in-group punishment is possible. We find that information sharing encourages path dependence via group reputation: good (bad) behavior results in greater (fewer) gains from exchange in the future. However, the mere threat of in-group punishment is enough to discourage bad behavior.

JEL Classifications: C9, D02, D7

Keywords: Experimental Economics, Group Reputation, Information, Group Punishment, Gains from Trade, Trust Game, Juries

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1. Introduction

Many exchanges are inherently sequential. One party receives goods or money and then gives the other party something in return. Transactions on Ebay, financial agreements like bonds and loans, and informal deals (e.g., "I'll buy this round, you buy the next") all have a similar sequential flavor. This asynchrony means that, absent an enforcement mechanism, the second party has incentive to renege. Knowing this, the first party may never enter the exchange, and potential welfare gains are lost. For both parties to be willing to exchange, they must commit *ex ante* to fulfill their ends of the bargain *ex post* [Greif, 2000, 2006].

In much of the world, *formal* legal, judicial, and penal institutions that impartially convict and punish cheaters mitigate commitment problems. Such institutions are costly to build, however, and they are not the only solution [Dixit, 2004]; after all, long-distance trade predates the emergence of formal institutions by millennia [see e.g. Cunliffe, 2008]. Absent formal institutions, humans tend to form *spontaneous*, *private-order* institutions to facilitate exchange. Famously, Ostrom [1990, 2009] shows how self-governed groups develop institutions to solve many collective action problems (through norms and sanctions incentivizing commitment, monitoring, and conflict resolution). Williamson [1996] shows how such institutions function by substituting mechanisms such as market prices, short-term contracting, and reputation for formal institutions to encourage exchange.

We address one nearly ubiquitous private-order mechanism: *reputation*. A large literature shows how reputation, via repeat interaction, can sustain cooperation without third-party enforcement [e.g. Kreps and Wilson, 1982, Kreps et al., 1982].[1] Yet reputation is not always effective. When groups are large and histories are unknown, incentives to cheat can outweigh the returns from building relationships, since a cheater cannot gain a reputation that constrains her in the future [Ghosh and Ray, 1996, Kranton, 1996].

Such circumstances impede exchange unless private-order, reputation-building institu-

[1]Repeat interaction may promote cooperation even without reputation, e.g. when the discounted gains from future encounters exceed the gains from reneging in the present [Fudenberg and Maskin, 1986].

tions align all parties' incentives. Today, reputation-building companies like Yelp and TripAdvisor provide an independent source linking one's past actions to future rewards. Absent such technology - as in the developing world and the entirety of economic history - alternative private-order institutions often arise to facilitate commerce. The Maghribi Traders' Coalition [see Greif, 1993, 2006] was one such institution based on individual reputation. Yet, institutions based on *individual* reputation can be expensive to build and maintain without technologies that permit the rapid flow of trustworthy information (e.g., the Internet), because each individual's past actions must be collated and transmitted, while traders must be monitored to ensure they abide by the institution's rules.

Given the costs of monitoring individual reputation, people have instead built institutions based on *group* reputation. To trade, one need only know a counterpart's group identity and that their group members have not cheated in the past [Ghatak and Guinnane, 1999]. This has an advantage over mechanisms reliant on individual reputation or on formal legal/penal institutions: it is less expensive to collect and disseminate information on group reputation. Yet, institutions based on group reputation can be costly in other ways. In particular, there is a tension between individual and group incentives: individuals impose a negative externality when they cheat, since they damage the reputation of the entire group [Tirole, 1996, Winfree and McCluskey, 2005, Levin, 2009]. In the absence of strong institutional or social sanctions, group members have little incentive to internalize this externality.[2]

Two mechanisms have historically been used to align individual and group incentives and maintain group reputation: *in-group punishment*[3] and *information sharing.* In-group punishment, i.e. punishment of one's *own* group members, enables groups to sanction cheaters

[2]Two other issues may arise: group identity may be difficult to ascertain, and sanctions may disproportionately harm some members. Both may cause unravelling, the former since unidentifiable insiders would have incentive to cheat, knowing that they will not be punished, and the latter if those disproportionately harmed are powerful enough to opt out. Thus, group reputation institutions typically rely on specific cultural, linguistic, or ethnic traits to identify members, and members are equal enough that no individual has the power to opt out [Greif, 2002, 2004, Besley and Coate, 1995]. For parsimony, we eschew such concerns and ask *how* such institutions work when members can be identified and no individual can opt out.

[3]For a broad historical overview of in-group punishment, see Levinson [2003]. In-group punishment has many other applications; e.g. in microfinance, where social sanctions encourage compliance, as most loans are based on joint liability [Besley and Coate, 1995, Ghatak and Guinnane, 1999, de Aghion, 1999].

and thereby maintain the group's reputation in others' eyes. Information sharing, e.g. in-group gossip about past interactions with *other* groups, works more subtly. If Group A members know how their fellows were treated by Group B, Group B gains a reputation with all of Group A, not just those with whom they interacted. This increases the costs of cheating for Group B since it reduces potential future gains from trade with all of Group A.[4]

While it is true that many mechanisms could sustain group reputation - for instance, instead of the "stick" of in-group punishment, the "carrot" of in-group rewards[5] - we choose to focus on a particular type of in-group punishment and information sharing because the two are frequently used together, yet we know little about how they work independently. One historical example of an institution that used both mechanisms was the community responsibility system (CRS) employed by medieval European trading cities. The CRS punished *all* traders from a community for a transgression by any *one* member [Greif, 2002, 2004, Boerner and Ritschl, 2009], and this was enforced by community courts that punished in-group cheaters to uphold the group's reputation. Information sharing was also essential to the CRS, as individual merchants benefitted from knowing which groups had cheated group members in the past. Similar in-group punishment mechanisms were used by medieval merchant and craft guilds [Milgrom et al., 1990, Greif et al., 1994, Richardson, 2005, Richardson and McBride, 2009]. These guilds fined or expelled members who broke rules and threatened the group's reputation (e.g., trading with a city on which the guild placed an embargo).

A related mechanism exists today in cultures that sanction kin for "shaming" the family. When family reputations impact marital and employment opportunities, families have incen-

[4]While increased cheating costs will not always induce a selfish, money-maximizer to uphold group reputation (at the expense of individual gain), Healy [2007] shows that with a sufficient percentage of *pro-social, reciprocal* types and sufficiently low discount rates, incentives to preserve group reputation can encourage even selfish types to cooperate through the penultimate period. Healy's result hinges on the assumption that some degree of "stereotyping" occurs in anonymous settings; individuals don't know the "types" they deal with, so they update their beliefs by assuming that all individuals from a cheater's group are cheaters. Note that when groups share information, stereotyped beliefs will spread more quickly through the population.

[5]Indeed, Fehr and Fischbacher [2003] suggest that some level of in-group altruism may have evolved in humans. For another alternative, see Kandori [1992], where *social* sanctions uphold group reputation in an environment where information is not perfect; as we will discuss below this differs from our experiments, where punishment is more formal and information (when present) is perfect within a group.

tive to punish those who commit crimes or transgress social norms. Such punishments can be drastic: in tribal societies, females are occasionally murdered in "honor killings" by male relatives as punishment for sexual transgressions [Levinson, 2003].[6] Kin group reputations are also upheld by information sharing. If a member of kin group A disrespects a member of kin group B, all members of kin group B soon find out. This damages the reputation of *all* members of kin group A in the eyes of *all* members of kin group B - not just the offending party's reputation is tainted. Knowing this, members of kin groups have less incentive to disrespect members of other groups even when in-group punishment is limited.

The key empirical fact is that wherever in-group punishment upholds group reputation, groups *also* share information to some degree. Since both mechanisms can sustain group reputation, it is unclear *ex ante* whether both are necessary or how they interact. It is an inherently empirical question 1) whether the mechanisms are substitutes, complements, or neither and 2) whether one mechanism dominates when both are present. It is also not clear *ex ante* whether these mechanisms sustain group reputation through reinforcement and/or deterrence. Individuals may condition their behavior on past information or punishments, or the mere presence of the mechanisms may encourage good behavior. The latter is akin to how the "shadow of the law" affects behavior, as the mere threat of punishment creates a default outcome which individuals contract around [Mnookin and Kornhauser, 1979, Ayres and Gertner, 1989, 1991, Stevenson and Wolfers, 2006].

We report an experiment parsing the roles of in-group punishment and information sharing in sustaining group reputation. Subjects play repeated investment trust games [Berg et al., 1995] with pre-play, cheap-talk 'contracting' and random rematching, where group, but not individual, identity is known. Parties have incentive to 'cheat' and individual repu-

[6]Fearon and Laitin [1996] argue that one reason that inter-ethnic conflict is relatively rare is that ethnic groups have incentive to punish transgressors from their own group, since doing so maintains group reputation and helps avoid more costly conflict. Legislative politics provides a less violent example: since individual politicians' reputations are tied to party reputation, and vice versa, party leaders often reward and punish with committee memberships and other resources to encourage "party discipline" [Cain et al., 1987, Katz and Sala, 1996]. Many business practices and features of political institutions also facilitate the establishment and maintenance of group reputation [Seabright, 1993, Bar-Isaac, 2007, Evans and Guinnane, 2007]. Winfree and McCluskey [2005] note, however, that such mechanisms are not always available, especially in larger groups.

tation tracking is infeasible. A 2x2 design varies whether, 1) senders can appeal to a jury, in which receivers vote to punish cheaters in *their own* group (*in-group punishment*), and 2) subjects get information on how group members fared (*information sharing*).

Because individuals trade with two different groups, with whom they typically have different histories, we can identify the effects of both mechanisms by decomposing history-dependence into session-wide and group-specific components. We demonstrate the importance of group reputation by examining whether individuals treat members of a group differently based on both own and group members' history of interaction with that group. This differs from Buchan and Croson [2004] and Falk and Zehnder [2007] who ask whether people treat in- and out-group members differently in trust games.

We find that differential treatment of two groups arises endogenously from the history with each group – and not just that people treat in- and out- groups differently (we do not induce feelings of group membership beyond assigning a "color", and we find no evidence that being of the same color encourages cooperation). Moreover, in our information-sharing treatments, subjects' choices depend on information about *others'* past interactions with a particular group – and not just that trade is discouraged (encouraged) indiscriminately by all negative (positive) information about past trades. Information sharing therefore accentuates path-dependence and aligns individual behavior with the incentives faced by the group. Unlike information sharing, in-group punishment reduces incentives to cheat merely by being possible. Under threat of punishment, individuals cheat much less in the jury treatments, regardless of juries' past actions. Receivers do cheat less after being punished, but we see a treatment effect in the first period. The "shadow of the law" encourages trade, even though the law is rarely used.

Previous experiments on cheating in trade showed that the opportunity to cheat sharply reduces gains from exchange [Cassar et al., 2009] while information-sharing on *individual* reputation reduces the likelihood and cost of cheating [Cassar et al., 2010]. Similarly, individual reputation-building encourages trust and reciprocity in repeated trust games, both in

repeat dyadic interaction and among strangers with known reputations [Bohnet and Huck, 2004, Bohnet et al., 2005, Bracht and Feltovich, 2009, Charness et al., 2011]. Group reputation has received recent attention in the lab. In addition to Healy [2007], who shows that group reputation encourages cooperation in a labor market setting, Huck and Lünser [2010] compare repeated trust games where subjects receive information about their partner's play to others where subjects only receive information about the matching group as a whole. They find that group and individual information are substitutes in small groups and can encourage exchange. Similarly, in a public goods game, McIntosh et al. [forthcoming] assortatively match subjects on either individual past behavior or group past behavior. They too find that individual and group information are substitutes.

Also related are experiments showing that in-group punishment can encourage cooperation [Fehr and Gächter, 2000, Andreoni et al., 2003] and that in-group members punish one another more harshly [Bernhard et al., 2006, Oxoby and McLeish, 2007]. The most closely related papers explore voting mechanisms by which peers can sanction non-contributors, [e.g Kroll et al., 2007, Ertan et al., 2009, Putterman et al., 2011] which show that democratically established formal sanctions encourage public goods contributions. There is a notable public goods aspect to the maintenance of a group's reputation. However, these experiments differ from our setting in that the interest in punishment in a public goods setting is *direct*: subjects punish to ensure that counterparts contribute in the future. In our setting, the recipient of punishment is not the only audience being addressed. With group reputation, punishment of intransigent *receivers* is intended to convince *senders* to trade.

2. Experimental Design

We use a 2x2 experimental design to explore how group information (Info) and in-group punishment (Jury) affect the gains from exchange in repeated investment trust games with pre-play communication and random rematching. Subjects know the group identity of the subject they are matched with, but not the individual identity.

In the baseline treatment (*No Jury - No Info*), subjects have no jury to enforce contracts and can only acquire group reputation information through their own experience. Sixteen subjects are each randomly assigned a color (red or blue) and a role (sender or receiver), which they maintain throughout an entire 10-period session. Subjects were not told the number of periods, and the game ended more than 30 minutes before the allotted time slot to control for endgame effects. There are eight subjects of each color, four senders and four receivers. At the beginning of each period, a randomly paired sender and receiver learn the color of their counterpart and are endowed with 10 experimental currency units (ECU). The sender chooses to send any integer amount between 0 and 10 ECU, knowing that this will be multiplied by 3 and given to the receiver. The receiver then chooses to send back to the first mover any amount between 0 and the amount received. At the end of the period, the sender earns (10 − ECUs sent to the receiver + ECUs returned from the receiver) and the receiver earns (10 + ECUs sent from the sender − ECUs returned to the receiver).

Prior to each period, the receiver can send a *non-binding* message reading "I will return X% of the total amount that I receive." The receiver may either choose an integer X between 0 and 100 and send the message, or click to indicate that she does not want to send a message. The sender sees the message (or that no message was sent) and makes her decision. The receiver then sees the amount sent and the implied return, given the message she sent, and chooses how much to return. Last, parties see their payoffs and are reminded of the implied return given the message. Crucially, the message allows both subjects and experimenters to clearly define instances of "cheating". The message can be viewed as an informal contract which is violated by a receiver returning less than offered in the message.

Our baseline measures trade under cheap talk contracting and private information on group reputation. The money-maximizing subgame perfect equilibrium (SPE) of the stage game yields no trade because receivers have dominant strategies to return nothing. Nevertheless, individuals send sizable amounts in the one-shot game, and receivers typically return

positive amounts.[7] Because of potential gains from exchange, first-mover trust is often reciprocated by the second mover. Our treatments vary 1) the amount of information revealed about group members' history and 2) access to the jury mechanism to ask whether and how they encourage trade.

In the *No Jury - Info* treatment, procedures are identical to the *No Jury - No Info* treatment except that, at the end of each period, subjects also see what happened to each other player of the same color and type.[8] E.g., all blue senders learn the following about other blue senders: 1) their counterpart's color; 2) the message sent; 3) the number of ECUs sent; 4) the implied return; 5) the number of ECUs returned; and 6) their earnings. This models an information sharing agreement among group members who, after each transaction, report the details to group members. Alternatively, it models a small-group setting, where information about the actions of all other group members is easily attained. Note that the *No Jury - Info* treatment does not change the money-maximizing SPE of the stage game, but the richer information set may allow agents to more accurately assign a reputation to each group.

The *Jury - No Info* treatment is also identical to the *No Jury - No Info* treatment with one exception. After each transaction, if the amount returned is less than offered in the receiver's message, the sender may incur a cost of 2 ECU to seek restitution. Disputes are taken before the *three other receivers of the same color as the defendant.* These receivers learn the facts of the transaction and then vote anonymously on whether to require the "cheater" to fulfill the contract.[9] If a majority vote for the plaintiff, the defendant pays the plaintiff the outstanding balance plus the dispute cost (2 ECU). If a majority vote for the defendant, the transaction stands and the dispute cost incurred by the sender is sunk.

In the stage game, payoff maximizing receivers are indifferent between voting for the

[7] Cheap talk promotes trust in one-shot games [e.g. Charness and Dufwenberg, 2006, Bohnet and Baytelman, 2007, Ben-Ner and Putterman, 2009], but in repeated games Bracht and Feltovich [2009] find no effect.

[8] This is similar to Cassar et al. [2010], where information is shared within but not between trade networks.

[9] If multiple senders file against receivers of the same color, cases are tried sequentially, and details of later cases are revealed only after earlier cases are complete. Note that the juries will not perfectly overlap since a cheater is not on the jury for his own case.

plaintiff and the defendant, as neither outcome affects their payoffs. However, if voters care about group reputation, then they may be inclined to vote for the plaintiff. On the other hand, if they also intend to cheat, then they may vote the defendant in hopes that other receivers will reciprocate when they are a defendant. Note that the jury also induces part of the information effect of the Info treatments, since parties to any trial will receive information about trades that they would not observe in the baseline treatment.

The *Jury - Info* treatment identifies the joint effect of information sharing and in-group punishment by combining the within-group information sharing of the Info treatments with the Jury mechanism. Here the group information table also includes details of any trials.

Finally, as a robustness check on our *Jury - Info* treatment, we also report a treatment in which receivers choose whether or not to participate in trials. Before learning transaction details, receivers must choose whether to *opt in* at a cost of 2 ECU per trial. This increases the cost of managing group reputation. We call this the *Jury - Info - Pay* treatment, and the results, which support our other findings, are reported in the appendix. Detailed instructions for each treatment and some sample screenshots are in Appendix B.

We recruited subjects randomly from the student body of a mid-sized university in the United States. A total of 272 subjects participated, 16 per session. We ran 17 total sessions: 3 sessions of each treatment (including the *Jury - Info - Pay* robustness check treatment) except for the *Jury - Info* treatment, for which we ran 5. Subjects entered the lab and were randomly assigned to visually isolated computer terminals at which they privately read the instructions. At the end of each 60 minute session subjects were paid their earnings in cash (mean = $16.25) plus a $7 show-up payment for arriving to the session on time.[10]

[10]The $7 show-up payment is standard in the lab at which the experiments were run; it induces a high show-up rate and allows researchers to increase the variance of salient payments while ensuring participants are compensated for their time. Receivers' overall earnings (including show-up payments) ranged from $21.25 to $41.50 (rounded to the nearest quarter), while senders' earnings ranged from $13.50 to $23.50.

3. Hypotheses

The theory overviewed in the introduction indicates that both information sharing and juries can promote trade by encouraging group members to sustain group reputation. But are they effective in a laboratory setting, where group identity is weakly induced? Does one mechanism dominate? Are they complements? Substitutes? The goal of this paper is to answer these questions. In this section, we outline the logic of these different hypotheses and the behavioral patterns each would imply.

Hypothesis 1 (no effect): Neither Info nor Jury promote trade.

Under this hypothesis our experiment reduces to a simple trust game as in Berg et al. [1995] but with pre-play communication. If players are money-maximizing, senders should send 0 regardless of communication, since they expect receivers to return 0. However, a meta-study of experimental trust games by Johnson and Mislin [2011] indicates that senders send on average 50 percent of their endowment, and receivers return 37 percent of what they receive. Moreover, in repeated trust games with pre-play communication, Bracht and Feltovich [2009] report that cheap talk has little effect on senders or receivers. Since neither juries nor information work under this hypothesis, there should be no treatment differences.

Prediction 1: If neither Info nor Jury promote trade, senders should send on average half of their endowment and receivers return 37% of what they receive in all treatments.

Hypothesis 2 (only Info matters): Info encourages trade when receivers have been honest in the past but discourages trade when receivers have cheated in the past.

We focus here on Info, ignoring the role of juries. Under information sharing, senders have much more information about how receivers in each group act, which they may use to assign a reputation to each group. If senders base their decisions on perceived group reputation, path dependence will be accentuated. Receivers' past actions impose an externality on their

group members, encouraging trade when they have (mostly) been honest and discouraging trade when they have cheated.[11] The expected net effect on senders is unclear because it depends on the endogenous, empirical distribution of receivers' actions.

The effect on *receiver* behavior is also *ex ante* unclear. Cheating may damage the group's reputation and thus lower future gains from exchange, and this disincentive may deter cheating. However, if receivers expect other group members to act honestly, they have incentive to free ride on the group's otherwise strong reputation (i.e., cheat). The relative strength of these incentives depends on the empirical distribution of sender and receiver behavior, and we do not wish to speculate on it here.

Prediction 2: If Info allows senders to assign receiver group reputations, they will send more (less) when matched with receivers from a group who have not cheated (cheated) any of their own group members in the past. The effect on receivers is indeterminate.

Hypothesis 3 (only Jury matters): Jury encourages trade via deterrence and further encourages trade when cheating receivers are convicted.

We focus here on Jury, ignoring the role of Info. Juries can encourage trade via two, non-mutually exclusive avenues. First, their mere presence may increase the amount sent if the threat of costly punishment deters cheating. If receivers believe they will be convicted for cheating with sufficient probability – which may be reasonable, since there is no voting cost – they have incentive to pay back at least as much as they promised in pre-play communication (or, at least pay back enough to avoid a lawsuit). This in turn encourages senders to send more in the first place, knowing that that they will likely receive more in return.

Second, the effect of juries may depend on whether the juries actually convict cheaters or not. Juries have full information about each case, including the fact that the accused cheated and by how much. Hence, if juries uphold group reputation by adjudicating impartially, as in Greif [2002, 2004] or Levinson [2003], then we expect to see path dependence in the Jury

[11] This is consistent with Greif [2002, 2004], in which individuals sometimes seek to escape the burden of their group's reputation.

treatments: groups that convicted in the past will receive more from senders in the future, while groups that acquitted in the past will receive less.

Prediction 3: In Jury treatments, senders will send more and receivers will return more relative to No Jury treatments regardless of how juries were used in the past. If cheated senders tend to win (lose) their trials, in the future they will send more (less) to receivers of the cheater's group and receivers in that group will return more (less).

Hypothesis 4 (both matter): Info and Jury are both effective.
Hypothesis 4a (complements): When combined, they amplify the effects of each other.
Hypothesis 4b (substitutes): Their combined effect equals either's effect alone.
Hypothesis 4c (neither): When combined, their effect is additive.

We cannot determine which of these alternatives is correct theoretically without imposing strong additional assumptions, but our experiment will allow us to disentangle these hypotheses empirically. First, if both H2 and H3 are correct, then information sharing and juries may be either complements or substitutes. If they are complements, juries will encourage trade and limit cheating initially, and this will be accentuated via information sharing, creating a superadditive effect. If they are substitutes, because e.g. the presence of *any* sanction – explicit via jury punishment or implicit via group reputation – induces senders to send more, then a second mechanism may be redundant.

Finally, juries and information sharing might be neither complements nor substitutes. This can occur if H2 is correct and H3 is correct for some – but not all – subjects. In this case, juries will sometimes encourage trust early on and sometimes not. Information sharing will then facilitate either positive or negative path dependence. Since these two effects partially cancel each other out, the two effects will not amplify each other *on average*. Instead, if enough senders are encouraged by the presence of a jury to send more early on, the effect of combining the jury with information sharing is additive: combined they encourage more trade than when either is employed alone, but the effect is not superadditive.

Prediction 4a: If Info and Jury are complements, their combined effect will be superadditive.

Prediction 4b: If Info and Jury are substitutes, then data from treatments employing either or both mechanisms will be indistinguishable.

Prediction 4c: If Info and Jury are neither complements nor substitutes, their combined effect will be additive.

4. Results

For our analysis, we focus on the 4 main treatments: *No Jury - No Info*, *No Jury - Info*, *Jury - No Info*, and *Jury - Info*. As a robustness check, we compare these treatments to one in which receivers paid to vote in the jury (*Jury - Info - Pay*) in Appendix A.3.

4.1. Summary Statistics and Basic Results

Table 1 reports summary statistics by treatment. Numerous treatment effects stand out in a simple analysis of means, suggesting that we can reject Hypothesis 1 that neither mechanism has an effect. First, the amount sent is lower in the *No Jury - No Info* treatment than in the other treatments. This is also apparent in figure 1, showing time series of amount sent in each session by treatment.[12] Differences in amount sent are important, because total welfare is almost solely dependent on the amount sent.[13] Total welfare increases threefold for every ECU sent, while every ECU returned is a transfer. Importantly, the mean amount sent in our baseline treatment is consistent with Johnson and Mislin [2011], who find that subjects send 50% of their endowment in trust games without cheap talk, and also consistent with Bracht and Feltovich [2009] who find that cheap talk alone has no effect on trust in repeated games.

[12]See figures A1 and A2 in appendix A for additional figures showing histograms of behavior by treatment.
[13]In the Jury treatments, total welfare also depends on whether a trial occurs, since a trial cost 2 ECU.

Treatment	*No Jury - No Info*			*No Jury - Info*			*Jury - No Info*			*Jury - Info*		
Variables	Mean	SE	N	Mean	SE	N	Mean	SE	N	Mean	SE	N
Amount Sent	5.233	0.246	240	5.992	0.263	240	6.175	0.239	240	6.798	0.190	400
Amount Returned	5.033	0.399	240	5.579	0.375	240	8.325	0.394	240	8.825	0.336	400
Message % (if sent)	0.518	0.136	224	0.459	0.123	229	0.466	0.107	213	0.485	0.121	368
% returned (if sent)	0.297	0.018	186	0.297	0.015	177	0.430	0.011	202	0.418	0.010	332
Receiver Earnings/pd	20.667	0.564	240	22.396	0.598	240	19.667	0.384	240	20.720	0.333	400
Sender Earnings/pd	9.800	0.302	240	9.588	0.273	240	12.417	0.200	240	12.605	0.203	400
Sent 10	0.225	0.027	240	0.358	0.031	240	0.333	0.030	240	0.455	0.025	400
Returned 0 $\mid$ sent > 0	0.471	0.032	240	0.425	0.032	240	0.204	0.026	240	0.250	0.022	400
Returned $<$ Message	0.483	0.032	240	0.488	0.032	240	0.325	0.030	240	0.293	0.023	400
Dispute Filed							0.627	0.068	51	0.635	0.053	85
Dispute Won		NA			NA		0.500	0.090	32	0.519	0.069	54

Table 1: Summary Statistics

To identify main effects of Jury and Info, we compare session-level means of the amount sent over the final 5 periods. With few sessions per treatment, the following analysis reports conservative tests for treatment effects. Comparing all Jury sessions to all No Jury sessions, the amount sent is significantly higher with the Jury (one-sided Wilcoxon test, $W_{6,8} = 6$, p-value < 0.01). We find no significant difference in the amount sent between Info and No Info (one-sided Wilcoxon test, $W_{6,8} = 21$, p-value $= 0.38$). Table 2 reports one-sided Wilcoxon test statistics and p-values for each pairwise treatment comparison. We find marginally significant differences in amount sent between Jury - Info and both No Jury treatments and between Jury - No Info and No Jury - Info treatments. All other comparisons are statistically insignificant.

From table 1, there are also treatment differences in the percent returned. These are apparent in figure 2, which show time series for each session by treatment. To identify the main effects of Jury and Info, we compare session-level means of the percent returned over the final 5 periods. The average percent returned is significantly higher in the Jury sessions than in the No Jury sessions (one-sided Wilcoxon test, $W_{6,8} = 3$, p-value < 0.01), but we find no significant difference between Info and No Info sessions (one-sided Wilcoxon test, $W_{6,8} = 21$, p-value $= 0.38$). Table 3 reports one-sided Wilcoxon test statistics and p-

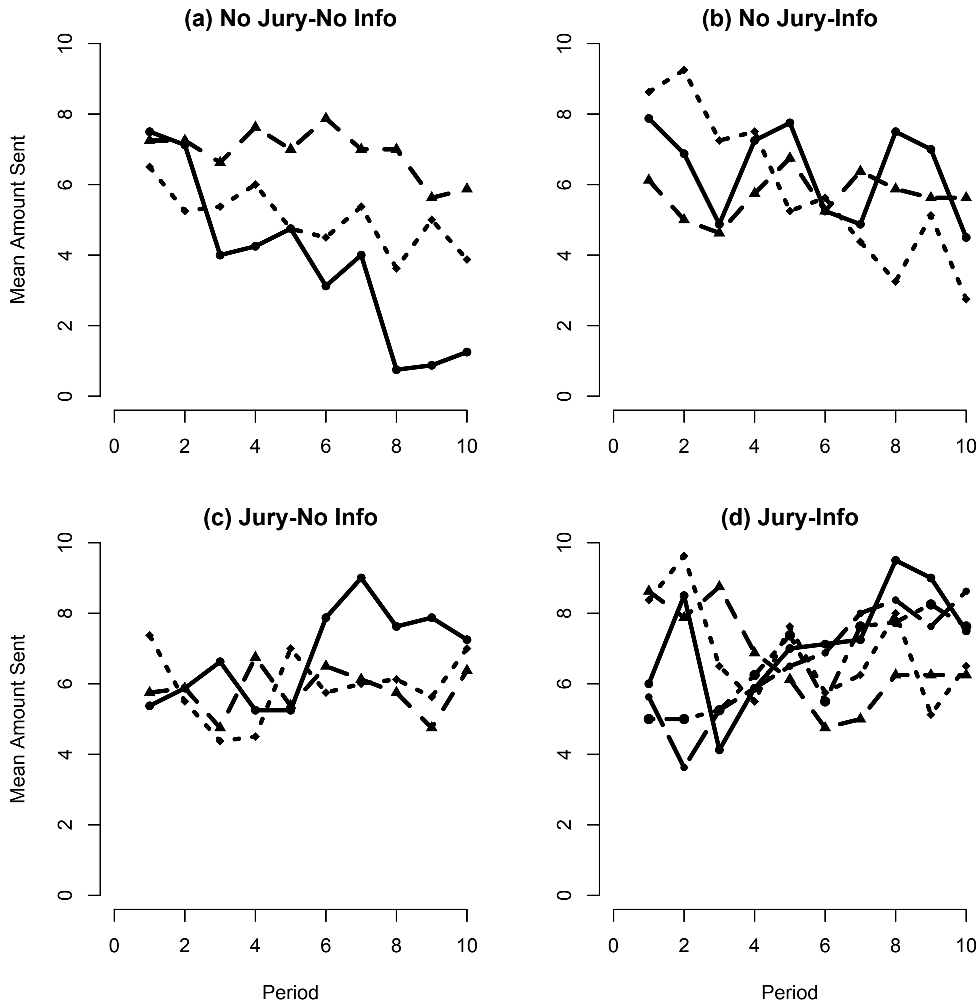

Figure 1: **Time Series of Amount Sent by Session and Treatment.**

	No Jury - Info	Jury - No Info	Jury-Info
No Jury - No Info	$W_{3,3} = 4$ (0.50)	$W_{3,3} = 2$ (0.20)	$W_{3,5} = 2$ (0.07)
No Jury - Info		$W_{3,3} = 0$ (0.05)	$W_{3,5} = 2$ (0.07)
Jury - No Info			$W_{3,5} = 6$ (0.39)

Wilcoxon tests of the hypothesis that the row treatment means are lower than the column treatment means. One-sided W-statistics with p-values in parentheses.

Table 2: Treatment Comparisons of Mean Amount Sent (period 6-10)

values for each pairwise treatment comparison. We find (marginally) significant differences in percent returned in all pairwise comparisons between Jury and No Jury treatments. All

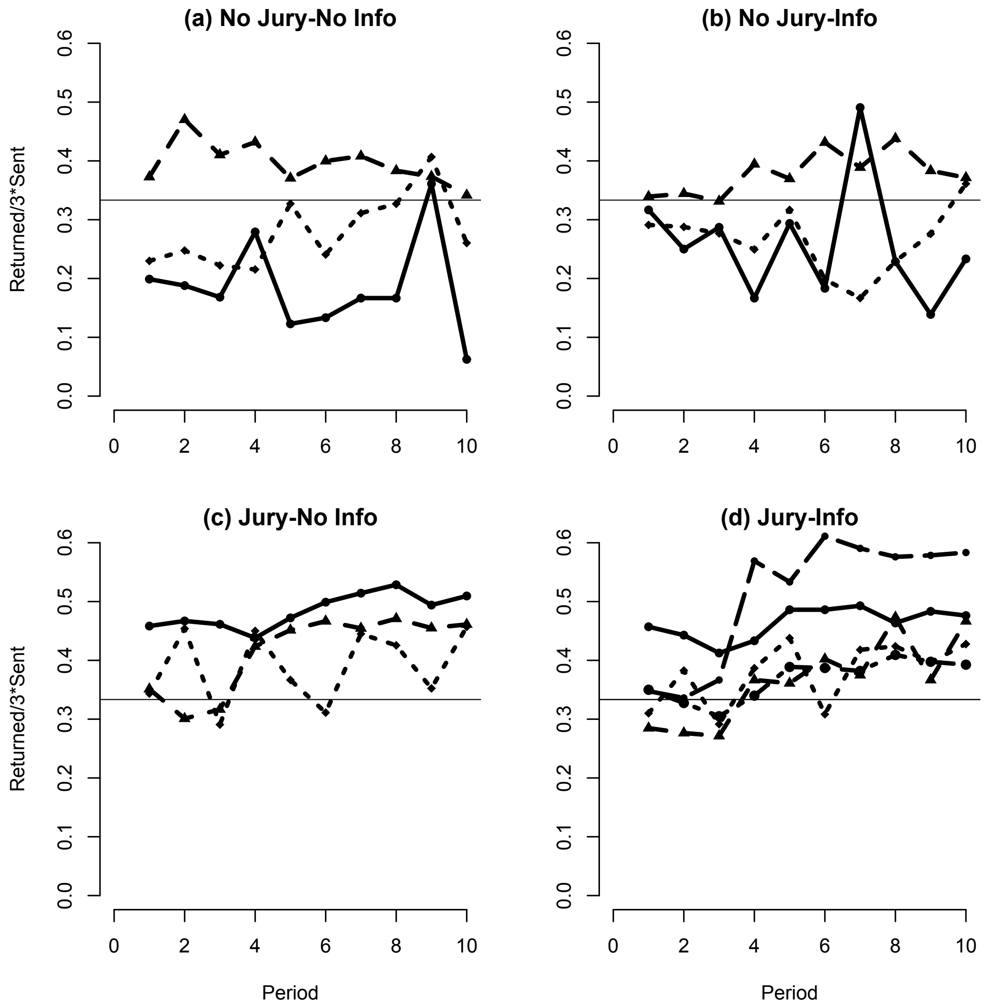

Figure 2: **Time Series of Percent Returned by Session and Treatment.** The horizontal line marks the percent return that leaves the sender at least as well off as he began.

other comparisons are statistically insignificant.

	No Jury - Info	Jury - No Info	Jury-Info
No Jury - No Info	$W_{3,3} = 4$ (0.50)	$W_{3,3} = 0$ (0.05)	$W_{3,5} = 0$ (0.02)
No Jury - Info		$W_{3,3} = 1$ (0.10)	$W_{3,5} = 2$ (0.07)
Jury - No Info			$W_{3,5} = 9$ (0.71)

Wilcoxon tests of the hypothesis that the row treatment means are lower than the column treatment means. One-sided W-statistics with p-values in parentheses.

Table 3: Treatment Comparisons of Mean Percent Returned (period 6-10)

Thus, preliminary analysis based on session-level data suggests that the Jury impacts the behavior of both senders and receivers, while Info has no effect, providing support for Hypothesis 3. However, collapsing our rich data to the session level and comparing means can only tell us so much since we do not control for endogenous variables which may affect behavior. For example, the amount sent likely depends on the message received, and the percent returned may depend on the amount sent.[14]

Even more importantly, this analysis ignores information available to players from previous periods of play. From the logic spelled out in Hypothesis 2, we expect this information to play an important role in decision-making, and the role of such information may vary across treatments. For instance, senders that are "ripped off" in early periods may be reluctant to send to a receiver of that color in future periods. This possibility is magnified in the Info treatments - a sender may also be reluctant to send to a receiver of a color that ripped off *any* of the other senders from his group. Likewise, in the Jury treatments, Hypothesis 3 indicates that a sender may avoid sending to receivers of a color that did not convict a fellow receiver for a previous transgression. In short, session-level analysis ignores the role of path dependence, and as we note in section 3 path dependence may play an important role under both Info and Jury. We address this in the next section, and while we confirm that the mere presence of the Jury mechanism is sufficient to encourage trade, we also identify an important role of group-level information in *accentuating* path dependence, supporting Hypothesis 4 that both mechanisms influence behavior.

Finally, we find no evidence of in-group favoritism, even in the first period. Elsewhere, inducing a "minimal group" has been shown to encourage in- and out-group tendencies [e.g. Tajfel, 1970]. Our senders actually send more to receivers of the other color in the first period (7.23 vs 5.71), and if we run the analyses reported below including controls for within-color matches, we find no evidence of significant increases in cooperation (tables available upon request). We thus exclude this comparison from our analysis.

[14]Regression analysis reported in appendix A.1 provides support for these claims.

4.2. Controlling for Path Dependence

4.2.1. Amount Sent

To highlight the role of path dependence, figure 3 displays time series of the probability of sending the entire endowment and of sending nothing, by treatment. In early periods, both Info treatments exhibit higher probabilities of sending the whole endowment than No Info treatments. However, in both Jury treatments, the probability increases over time, while it is flat or declining in the No Jury treatments.[15] In both Jury treatments, the probability of sending nothing remains flat, while in both No Jury treatments, it increases over time.

In Table 4, we analyze mixed-effects linear regressions where the dependent variable is the amount sent by S. We include treatment dummy variables, a period trend, the message sent, a dummy if no message was sent, and the average amount previously sent by S in all periods prior to t.[16] We also include nested random effects for each subject-in-session to control for repeated measures. Crucially, these specifications also contain regressors controlling for past actions that may affect senders in the present. In Column 1 of Table 4, we include two variables to control for the sender's personal history: the fraction of previous periods he has been ripped off by a receiver in the group with which he is currently matched, and the fraction of previous periods he has been ripped off by a receiver in the group with which he is *not* currently matched. If group reputation is salient in the decision to send, then the coefficient on the former but not the latter should be significant. That is, we expect a group reputation-sensitive player that has been ripped off by a blue receiver to be wary of sending to blues in the future but not to reds. We include the latter variable because being ripped off in the past by anyone, regardless of group, could make a sender wary of trusting in the future, regardless of group identity. But this is not what we find.[17] Instead, prior ripoffs by

[15]The No Jury evidence is consistent with evidence that group reputation can encourage early-period cooperation in experimental labor markets [Healy, 2007].

[16]This last variable attempts to control for individual idiosyncrasies and reflects the fact that we cannot use fixed effects since each individual was observed in only one treatment.

[17]The coefficient on the "previous ripoff, other" variable enters as statistically significant if it is included in the regression reported in column 3, where we break down information-specific and jury-specific past

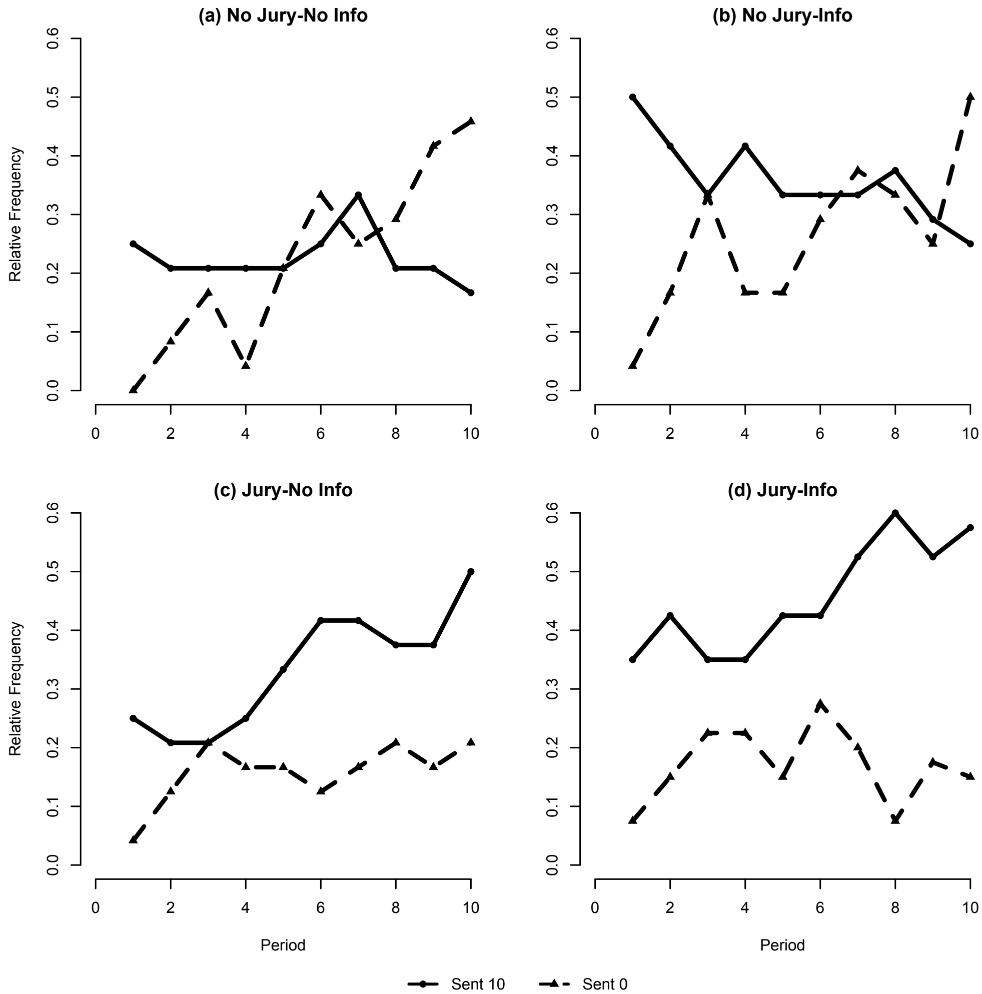

Figure 3: **Time Series of the Probability of Sending both 10 and 0 Units by Treatment.** Each panel displays time series of average observed probabilities for one treatment.

receivers of the same color as the sender's counterpart significantly decrease the amount sent (both economically and statistically), whereas prior ripoffs by receivers in the other group have no significant effect (we thus drop the latter variable in future regressions).[18]

After controlling for the sender's own history in column 1, it is apparent that the Jury and Info treatments encourage more trade than the *No Jury - No Info* treatment, consistent with Hypotheses 2 and 3 . Yet, this regression tells us little about the causal mechanisms

actions in various components. The primary results do not change however; the *Jury - Info* coefficient is still significantly greater than the *Jury - No Info* coefficient (two-sided p-value = 0.05) and is marginally greater than the *No Jury - Info* coefficient (two-sided p-value = 0.15). These results are available upon request.

[18]Without controlling for past actions (as in our regressions in appendix A1), we observe a significant time trend, but these results suggest that this trend is actually path dependence inherent in a multi-period game.

	(1)	(2)	(3)
Action taken by:		**Sender**	
Dependent Variable:		Amount Sent	
No Jury-Info	1.030*	1.043*	1.641**
	(0.563)	(0.580)	(0.650)
Jury-No Info	1.436**	1.627***	1.561**
	(0.575)	(0.600)	(0.608)
Jury-Info	1.439***	1.634***	2.590***
	(0.517)	(0.539)	(0.572)
Inverse Period	1.321	1.578*	0.822
$(\frac{1}{t})$	(0.864)	(0.841)	(0.839)
Message * Message Sent	10.118***	10.219***	10.738***
(message % * message dummy)	(0.914)	(0.919)	(0.905)
No Message	0.750	0.777	0.959
(no message dummy)	(0.594)	(0.597)	(0.585)
Avg. Previously Sent	0.391***	0.390***	0.390***
$(\frac{\sum_{t=1}^{t-1} sent}{t-1})$	(0.057)	(0.057)	(0.056)
Previous Ripoff, Same	-2.101***	-1.882***	-1.884***
(fraction of pvs. pds. S ripped off by R's group)	(0.333)	(0.352)	(0.346)
Previous Ripoff, Other	-0.490		
(fraction of pvs. pds. S ripped off by other (not R's) group)	(0.365)		
Previous Win * Jury		-0.018	0.403
(# of pvs. trials won by S vs. R's group * jury dummy)		(0.375)	(0.373)
Previous Lose * Jury		-0.233	-0.142
(# of pvs. trials lost by S vs. R's group * jury dummy)		(0.310)	(0.308)
Previous Ripoff of Group * Info			-1.312**
(fraction of pvs. times others in S's group ripped of by R's group * info)			(0.625)
Previous Wins of Group * Jury * Info			-0.207
(# of pvs. trials won by others in S's group vs. R's group * jury * info)			(0.257)
Previous Losses of Group * Jury * Info			-1.142***
(# of pvs. trials lost by others in S's group vs. R's group * jury * info)			(0.234)
Constant	-1.607**	-1.997***	-2.104***
	(0.791)	(0.775)	(0.769)
Wald Test p-values			
No Jury-Info = Jury-No Info	0.477	0.327	0.904
No Jury-Info = Jury-Info	0.425	0.269	0.107
Jury-No Info = Jury-Info	0.995	0.990	0.063*
Observations	1008	1008	1008
Log Likelihood	-2626.9	-2627.5	-2605.3
χ^2-Statistic	313.0	313.8	374.2

Mixed effects results reported, with random effects on session and sender; standard errors in parentheses.
All regressions include a constant term; period 1 observations dropped from analysis.
*** p<0.01, ** p<0.05, * p<0.1, two-sided tests

Table 4: Mixed Effects Regressions Explaining the Amount Sent by Treatment

underlying either treatment effect. Senders know their own history in all four treatments. Thus, columns 2 and 3 also include controls for past actions that may influence the amount

sent in one or both of the Jury and Info treatments but not the *No Jury - No Info* baseline.

Column 2 controls for previous jury votes (thus these variables are interacted with a jury dummy). If juries sustain group reputation, sender behavior should depend on past jury outcomes. If a sender lost previous trials against members of the receiver's group, then he may expect to lose a new trial. The converse is true if the jury convicted in the past. Yet, while the presence of a jury encourages trade (sizable coefficients on the jury treatment dummies are highly significant), past jury votes have no significant effect on behavior - in part because ripoffs are relatively rare in the Jury treatments (occurring in 30% of cases vs. nearly 50% in No Jury treatments). As we highlight in section 4.2.2, the *threat* of trial is sufficient to induce high returns from receivers, which spills over into the amount sent.

Column 3 controls for previous actions observed only in the Info treatments. First, we control for the fraction of times others in the sender's group have been ripped off by a receiver in the group with which the sender is currently matched.[19] The other two variables are only relevant in the *Jury - Info* treatment: the number of trials won and lost by other senders in one's group against receivers from the group with which the sender is currently matched.

The coefficients on "previous ripoffs of others" and "previous trials lost by others" both have strong, significantly negative effects on amount sent. Perhaps more importantly, after controlling for information-specific variables, we find a (marginally) statistically significant difference in the amount sent in the *Jury - Info* treatment versus *every other treatment.* Wald tests reject the null that the *Jury - Info* coefficient equals both the *No Jury - Info* and *Jury - No Info* coefficients at p = 0.107 and 0.063. Note that these are conservative p-value estimates, as they come from two-sided tests. Thus, consistent with Hypothesis 4c, the Jury and Info have an *additive* effect; if receivers uphold group reputation by convicting transgressors, the combination of Jury and Info encourages more trade than either Jury or

[19]In an alternative specification, we include an interaction between the fraction of times others in the sender's group have been ripped off by a receiver in the group with which the sender is currently matched with and a *No Info* dummy. Since senders have no information on group members, we expect this coefficient to be highly insignificant. The p-value is 0.964 and none of the coefficients of interest change meaningfully.

Info individually.[20]

> **Finding 1**: Controlling for past actions, subjects send less in *No Jury - No Info* than all other treatments.
>
> **Finding 2**: Controlling for past actions, the average amount sent is greater with both jury and information-sharing than with either alone.

These findings shed light on the mechanisms generating welfare gains in the Info and Jury treatments. First, past actions are decisive in the Info treatments. Consistent with Hypothesis 2, if group members are always ripped off by a receiver's group (i.e., previous ripoffs of group = 1), practically all of the increased welfare from information-sharing disappears (a Wald test cannot reject the null hypothesis that the estimated coefficients on the *No Info - Jury* dummy variable and the Previous Ripoffs of Group*Info sum to 0, p-value = 0.63, two-sided test).

In Jury treatments (as shown below), individuals and groups are less likely to have been ripped off in the past, so the Previous Ripoff variables take different values across treatments. Here, group information-sharing accentuates path dependence. Info treatment groups with a "good" experience maintain higher amounts sent, and groups with a "bad" experience exhibit sharper declines. Figure 4 displays time series of mean amount sent by subjects with good and bad experiences with their counterpart's group.[21] This accounts for the treatment differences in time trends noted in figure 3 and highlights the additivity of the mechanisms.

> **Finding 3**: Info treatments produce more welfare than the *No Jury - No Info* baseline because subjects *actually use* the information; welfare is reduced if receivers treated a sender's group members poorly in the past.

[20] Jury and Info are neither substitutes or complements, however. We find find no evidence of crowding out or of super-additive effects.

[21] The proportion of senders who have a "good" experience - shown at the bottom of each panel in figures 4 and A2 - can increase slightly from one period to the next due to random re-matching (i.e., if senders happen to be randomly matched with receivers of a color with whom they have had better experiences in the past). We include a second figure with a more lenient definition of "good" in the appendix. It is similar.

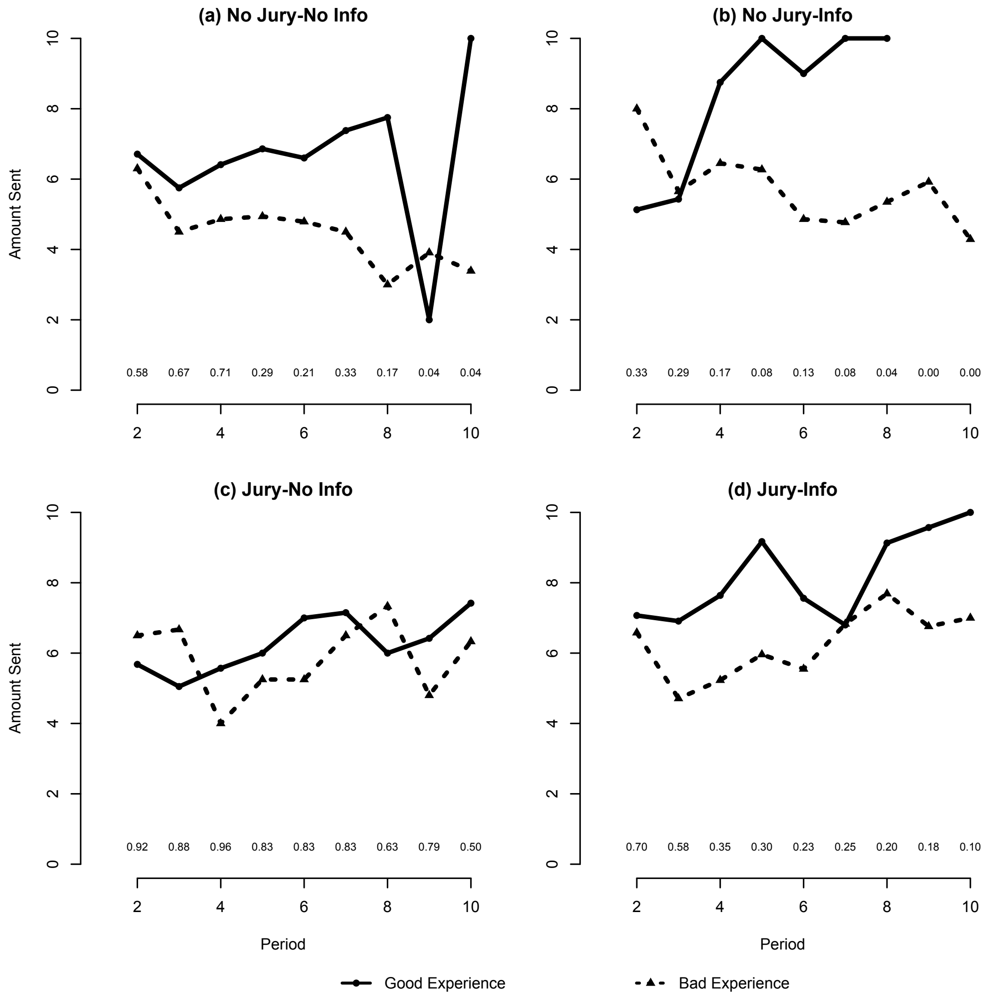

Figure 4: **Time Series Amount Sent for "Good" and "Bad" Experiences by Treatment.** Each panel displays data for one treatment. The solid line depicts amount sent by individuals who had a "good" experience with members of their counterpart's group, where 1) in the *No Info* treatments, a good experience is defined as never having been cheated by a member of the group with whom the sender is currently matched, 2) in the *Info* treatments, a good experience is defined as a situation in which neither the sender nor any of the senders of the same color have been cheated by a member of the group with which the sender is currently matched, and 3) in the *Jury* treatments, a good experience is defined as a situation similar to the *No Info* and *Info* treatments, but a good experience is also possible if a jury punished the guilty party in every previous instance of cheating. The numbers printed below the lines indicate the proportion of senders who have a "good" experience with their counterpart's group in each period.

Finding 4: Jury treatments produce more welfare than the *No Jury - No Info* baseline because of the mere existence of the jury. Past jury decisions only significantly impact the amount sent when combined with information-sharing.

4.2.2. Amount Returned

The findings in section 4.1 indicated that Jury but not Info increases amount returned. In this section, we confirm this controlling for potential path dependence.

Table 5 reports linear mixed effects estimates explaining the difference between the percent returned and the percent offered in the message. We include treatment dummies, a period trend, the amount sent, a constant, and variables controlling for past actions. All columns of Table 5 include a control for an individual's average difference in percent returned and percent offered pre-play in all previous periods. The coefficient on this variable is highly significant. The simple fraction of ripoffs in previous periods is not statistically significant. Moreover, both Jury treatment dummies are significant.

In Column 2, we include two variables controlling for personal experience in the jury treatments: the number of times the receiver has been convicted and acquitted in the past. Actions taken by previous juries have an important effect on returns. If a receiver has been convicted in the past, she returns more in the future. Although this is not surprising, note that this effect only accounts for a small fraction of the effect of the Jury treatment on receivers' behavior; the coefficients on the *Jury - No Info* and *Jury - Info* dummies decrease only slightly and remain highly significant. This suggests that some feature of the jury mechanism affects the behavior of receivers beyond the impact of how it has been employed in the past.[22] Receivers adjust their behavior in anticipation of punishment for cheating.

Finally, in Column 3, we include three variables incorporating information-sharing: the fraction of previous periods that in-group receivers ripped off a sender in the current sender's group, and the number of previous trials in which other group members were convicted and acquitted. If group reputation is salient to receivers, these coefficients should be significant. However, this is not what we find. Not only are none of these coefficients statistically significant, but coefficients on all other variables are nearly identical to those in Column 2.

[22] A Wilcoxon test of the null hypothesis that there is no difference in percent returned by receivers in the Jury and No Jury treatments provides support. We reject in favor of the alternative hypothesis that Jury sessions provide higher returns than No Jury sessions in the first period ($W_{8,6} = 37$, p-value = 0.05).

	(1)	(2)	(3)
Action taken by:		**Receiver**	
Dependent Variable:		Returned % - Message %	
No Jury-Info	-0.000	-0.000	0.013
	(0.034)	(0.033)	(0.037)
Jury-No Info	0.108***	0.093***	0.093***
	(0.035)	(0.034)	(0.034)
Jury-Info	0.085***	0.071**	0.081**
	(0.031)	(0.031)	(0.032)
Inverse Period	-0.036	-0.018	-0.025
$(\frac{1}{t})$	(0.036)	(0.036)	(0.038)
Sent	0.001	0.001	0.000
(amt. sent by S)	(0.002)	(0.002)	(0.002)
Avg. Previously Returned – Message	0.541***	0.531***	0.527***
$(\frac{\sum_{t=1}^{t-1} \%returned-\%message}{t-1})$	(0.048)	(0.048)	(0.049)
Previous Ripoff	-0.007	-0.024	-0.027
(fraction of pvs. pds. R ripped off one in S's group)	(0.017)	(0.018)	(0.018)
Previous Conviction * Jury		0.050***	0.049***
(# of pvs. trials convicted * jury)		(0.016)	(0.016)
Previous Acquittal * Jury		0.013	0.015
(# of pvs. trials acquitted * jury)		(0.019)	(0.019)
Previous Ripoff of Group, Same * Info			-0.028
(fraction of prior times others in R's group ripped off S's group * info)			(0.036)
Previous Conviction, Group * Jury * Info			-0.002
(# of trials other members of R's group convicted * jury * info)			(0.014)
Previous Acquittal, Group * Jury * Info			-0.006
(# of trials other members of R's group acquitted * jury * info)			(0.014)
Constant	-0.092***	-0.090***	-0.087***
	(0.031)	(0.030)	(0.030)
Wald Test p-values			
No Jury-Info = Jury-No Info	0.002***	0.006***	0.033**
No Jury-Info = Jury-Info	0.005***	0.019**	0.044**
Jury-No Info = Jury-Info	0.449	0.470	0.707
Observations	748	748	748
Log Likelihood	471.0	476.2	476.7
χ^2-Statistic	192.2	203.5	206.5

Mixed effects results reported, with random effects on session and receiver.
Standard errors in parentheses. All regressions include a constant term.
Period 1 and observations where no message or 0 sent dropped from analysis.
*** p<0.01, ** p<0.05, * p<0.1, two-sided tests

Table 5: Mixed Effects Regressions Explaining the Percent Returned - Percent Offered by Treatment

The interpretation is straight-forward. Information-sharing has no direct effect on receivers' decisions, while the jury increases the amount returned relative to the message.[23]

[23]The fact that we observe no impact of information sharing on receiver behavior presents a puzzle. Is this because receivers are unaware that senders discriminate based on group behavior? Do they attempt to

The jury mechanism works in two ways. First, receivers in groups that have convicted in the past return more in the future. Second, the mere presence of a jury increases returns, regardless of past votes, and thus the threat of punishment is sufficient to increase cooperation. The fact that information sharing has no average effect on receivers may help explain the merely additive effect on senders' decisions noted above.

> **Finding 5**: Prior convictions induce higher returns in Jury treatments, but Info treatments do not affect their behavior relative to the *No Jury - No Info* baseline.
>
> **Finding 6**: Receivers return more in Jury treatments, but the Info treatment does not affect their behavior relative to the *No Jury - No Info* baseline.

4.2.3. Discussion

In sum, juries and information-sharing work in different ways to increase welfare. The jury institution operates through its effect on receivers - they return more relative to the amount promised in jury treatments than they do in non-jury treatments. This encourages senders to send more, since they receive greater returns the more they send. Information-sharing works through its effect on group reputation. Senders in groups that have been cheated in the past send much less than those in groups who have not been cheated, and information-sharing reinforces this since individuals can also condition behavior on group members' experiences. To reiterate, information accentuates path dependence among sender - expanding trade when the group has a history of successful exchange and restraining it when the group has been exposed to cheating. The jury mechanism simply deters cheating, and when combined with information-sharing, creates positive feedback facilitating further gains from exchange.

free-ride on the reputation of their peers? Or is their behavior driven by the fact that they know that senders are unaware of their individual history when sending? Unfortunately, our design and data do not allow us to directly address these questions. We cannot reject that receivers are unaware of the potential gains from group reputation among receivers. To a sender, the value of group information is clear - it allows a sender to make inferences about the distribution of types with which she might be matched. The value of Info to receivers is subtly more complex; they must place themselves into the role of a sender and then backwards induct to the conclusion that maintaining a positive group reputation would encourage generosity. We are indebted to a referee for a discussion of this important issue.

4.3. When do subjects sue? When are cheaters convicted?

Since the Jury increases welfare, we briefly review how subjects employ it. Table 6 reports mixed effects linear probability estimates of the sender's decision to "sue". In a variety of specifications, we analyze the effects of information-sharing on the probability of suing, and we find no evidence of significant effects. Our findings suggest that the likelihood of suing varies significantly only with the magnitude of the ripoff.

Action taken by:	**Sender**			
Dependent Variable	Dispute (0/1)			
Jury-Info	-0.117	-0.128	-0.132	-0.093
	(0.089)	(0.089)	(0.090)	(0.105)
Inverse Period	-0.006	-0.081	-0.087	-0.151
($\frac{1}{t}$)	(0.134)	(0.151)	(0.153)	(0.156)
Sent	0.002	-0.000	-0.000	0.020
(amt. sent by S)	(0.016)	(0.016)	(0.016)	(0.020)
Ripoff Magnitude	0.043***	0.044***	0.045***	0.033***
((message % * amt. received from S) - amt. returned by R)	(0.008)	(0.008)	(0.011)	(0.008)
Previous Ripoff		-0.131	-0.106	-0.052
(fraction of previous pds. S ripped off by R's group)		(0.124)	(0.165)	(0.166)
Previous Win * Jury			0.005	-0.006
(# of previous trials won by S vs R's group * jury)			(0.109)	(0.108)
Previous Lose * Jury			-0.024	-0.040
(# of previous. trials lost by S vs R's group * jury)			(0.066)	(0.065)
Previous Ripoff of Group * Info				0.029
(fraction of pvs. times others in S's group ripped off by R's group * info)				(0.225)
Previous Wins of Group * Jury * Info				0.009
(# of pvs. trials won by others in S's group vs. R's group * jury * info)				(0.077)
Previous Losses of Group * Jury * Info				-0.104
(# of pvs. trials lost by others in S's group vs. R's group * jury * info)				(0.071)
Better than Autarky * Ripoff				-0.200*
(equals **1** if 10 - amt. sent + amt. returned > 10, equals **0** otherwise)				(0.106)
Constant	0.457***	0.532***	0.537***	0.569***
	(0.144)	(0.160)	(0.163)	(0.164)
Observations	136	136	136	136
Log Likelihood	-75.82	-75.26	-75.18	-74.11
χ^2-Statistic	37.75	39.18	39.39	47.09

Mixed effects results reported, with random effects on session and sender.
Standard errors in parentheses; all regressions include a constant term.
Period 1 observations dropped from analysis; only observations in jury treatments and where ripoff occurred included; *** p<0.01, ** p<0.05, * p<0.1, two-sided tests

Table 6: Mixed Effects Linear Probability Estimates Explaining the Decision to Dispute, Jury Treatments

That senders do not always file disputes when ripped off could suggest path dependence -

failure to convict in the past might make present filings seem futile. Our data do not support this. Instead, some of the effect is driven by the fact that not all ripoffs leave a sender worse off than he would have been without trade. When we include a dummy variable that takes a value of 1 when the sender was ripped off but received a payoff larger than his endowment, it is negative and significant; a sender is 20 percentage points less likely to sue, all else equal.

> **Finding 7**: Cheated senders are more likely to file a dispute the more they are ripped off. Info does not affect the likelihood of filing.

When disputes are filed, it is also important to know what factors influence juries' decisions. Table 7 reports mixed effects linear probability estimates of the decision to convict the receiver.[24] Here we report two specifications, one of which controls for treatment differences and one of which includes additional variables accounting for path dependence in the *Jury - Info* treatment. The coefficient on ripoff magnitude is positive and significant. We cannot distinguish between motivations behind jury votes. Perhaps voters aim to protect their group's reputation, but altruistic (or fairness) motivations are also plausible. Our Jury - Info - Pay treatment in Appendix A.3 indicates that individuals are still willing to punish, even when it is costly, providing some evidence for the group reputation interpretation.

> **Finding 8**: Conviction rates increase in the magnitude of the ripoff. Information-sharing does not impact voting.

5. Conclusions

Our experiment highlights the crucial role of both information-sharing and in-group punishment in sustaining group reputation and facilitating exchange. We find that the mere presence of an in-group punishment mechanism reduces the likelihood of cheating: that is, the "shadow of the law" is enough to encourage good behavior amongst subjects. Meanwhile,

[24] These differ from all previous regressions in that we include random effects only at the session level, since jury voting is a collective decision.

Action Taken By:	**Receiver's Group**	
Dependent Variable	Sender Won Dispute (0/1)	
Jury-Info	-0.190	-0.278
	(0.187)	(0.216)
Inverse Period	-0.092	-0.004
($\frac{1}{t}$)	(0.205)	(0.225)
Sent	-0.032	-0.035
(amt. sent by S)	(0.027)	(0.030)
Ripoff Magnitude	0.034***	0.036**
((message % * amt. received from S) - amt. returned by R)	(0.012)	(0.015)
Previous Ripoff of Group, Same * Info		0.361
(fraction of pvs. times R's group members ripped off S's group * info)		(0.289)
Previous Conviction, Group * Jury * Info		-0.108
(# of trials members of R's group convicted * jury * info)		(0.098)
Previous Acquittal, Group * Jury * Info		0.105
(# of trials members of R's group acquitted * jury * info)		(0.078)
Better than Autarky * Ripoff		0.108
(equals **1** if 10 - amt. sent + amt. returned > 10, equals **0** otherwise)		(0.150)
Constant	0.715***	0.654**
	(0.251)	(0.259)
Observations	86	86
Log Likelihood	-57.64	-55.96
χ^2-Statistic	9.376	14.14

Mixed effects results reported, with random effects on session.
Standard errors in parentheses; all regressions include a constant term.
Period 1 dropped from analysis; only observations in jury treatments and
where dispute occurred included; *** p<0.01, ** p<0.05, * p<0.1, two-sided tests

Table 7: Mixed Effects Linear Probability Estimates Explaining Jury's Decisions to Convict, Jury Treatments

information sharing accentuates path-dependence since individuals condition their behavior not only on their own prior experiences but also on the experiences of others in their group. The two mechanisms are thus additive, as the punishment mechanism puts groups on a more favorable path and information sharing causes positive feedback from a mutually beneficial history. While we explore these mechanisms in the context of a single experimental environment, the underlying issues are important to a variety of contexts, many of which were discussed in detail in the introduction. Indeed, our findings suggest a primary reason why many modern and historical institutions combine these elements.

Our work is also related to research on the effects of identity on economic behavior [see e.g. Akerlof and Kranton, 2000]. In recent experiments, Chen and Chen [2011] demonstrated

that a salient group identity can facilitate cooperation among members of a group. Another important function of group identity is found in the fact that it increases incentives to maintain the group's reputation, since one's own identity is tied to others' perceptions of the group. In our setting, by inducing group identity more strongly, we might be able to further increase the gains from exchange. However, it is also possible that stronger group identities will lead to differential treatment of in- and out-group members, as in Chen and Li [2009], perhaps leading to a trade off of increased trade within the group against increased cheating outside the group. We do not explore these possibilities here, since aside from randomly assigning individuals to groups labeled by different colors, we make no effort to induce identity. However, we suggest this as a fruitful avenue for future research.

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Appendix (for online publication)

A. Additional Tables and Figures

A.1. Regression Analysis of Basic Treatment Effects

Table A1 reports the results of linear regressions that provide additional support for the evidence presented in section 4.1 explaining messages sent by Receivers, amounts sent by Senders and percent returned by Receivers. In each regression, we include nested random effects at the session and subject-in-session levels to control for repeated measures.

	(1)	(2)	(3)	(4)	(5)	(6)
Action taken by:	**Receiver**		**Sender**		**Receiver**	
Dependent Variable:	Message %		Amount Sent		Returned % - Message %	
No Jury-Info	-0.058*	-0.043*	1.307*	1.126*	0.023	-0.000
	(0.031)	(0.024)	(0.724)	(0.674)	(0.065)	(0.034)
Jury-No Info	-0.062**	-0.053**	1.952***	2.042***	0.190***	0.109***
	(0.031)	(0.025)	(0.724)	(0.674)	(0.064)	(0.035)
Jury-Info	-0.041	-0.035	2.192***	2.039***	0.130**	0.086***
	(0.028)	(0.022)	(0.646)	(0.602)	(0.058)	(0.031)
Inverse Period	-0.119***	-0.062**	3.056***	2.437***	-0.002	-0.034
$(\frac{1}{t})$	(0.025)	(0.026)	(0.815)	(0.832)	(0.033)	(0.035)
Avg. of Previous Messages		0.276***				
$(\frac{\sum_{t=1}^{t-1} \%message}{t-1})$		(0.035)				
Message * Message Sent			10.205***	10.398***		
(message % * message dummy)			(0.928)	(0.933)		
No Message			0.803	0.887		
(no message dummy)			(0.602)	(0.605)		
Avg. Previously Sent				0.355***		
$(\frac{\sum_{t=1}^{t-1} sent}{t-1})$				(0.058)		
Sent					0.001	0.001
(amt. sent by S)					(0.002)	(0.002)
Avg. Prevoiusly Returned – Message						0.546***
$(\frac{\sum_{t=1}^{t-1} \%returned - \%message}{t-1})$						(0.045)
Constant	0.549***	0.405***	-0.732	-2.882***	-0.222***	-0.095***
	(0.023)	(0.026)	(0.738)	(0.804)	(0.048)	(0.030)
Wald Test p-values						
No Jury-Info = Jury-No Info	0.893	0.688	0.372	0.174	0.010**	0.002***
No Jury-Info = Jury-Info	0.532	0.705	0.171	0.128	0.065*	0.005***
Jury-No Info = Jury-Info	0.440	0.410	0.710	0.997	0.296	0.450
Observations	938	938	1008	1008	760	748
Log Likelihood	814.3	834.9	-2658.4	-2646.2	438.8	470.9
χ^2-Statistic	28.05	91.08	223.7	260.7	12.46	189.5

Mixed effects results reported, with random effects on session (1-6), receiver (1-2, 5-6), and sender (3-4). Standard errors in parentheses; all regressions include a constant term; period 1 observations dropped from analysis. Observations where no message was sent dropped in (1), (2), (5), and (6), and observations where 0 sent dropped from analysis in (5) and (6); *** p<0.01, ** p<0.05, * p<0.1, two-sided tests

Table A1: Mixed Effects Regressions Explaining Sender and Receiver Decisions

We first seek the determinants of the message sent. Since this is the first action taken in each period, it affects all subsequent actions taken (i.e., amount sent and received, the jury outcomes). Column 1 reports the results of regressions where the dependent variable is the message sent (in percentage terms) and the independent variables are treatment dummies (*No Jury - Info*, *Jury - No Info*, and *Jury - Info*)[1], a period trend, and a constant. Column 2 includes a variable which averages the message that the receiver sent in all previous periods in order to control for individual idiosyncrasies.[2] We find that relative to the Baseline (*No Jury - No Info*), both the *No Jury - Info* and the *Jury - No Info* treatments significantly *reduce* the percent offered in the pre-play message, but there is no difference in the *Jury - Info* treatment. It is possible that these results arise because, in the absence of a mechanism such as information or a jury to encourage compliance, receivers in the *No Jury - No Info* treatment have to promise more in order to encourage senders to send tokens. Nevertheless, as we show next, the amount sent is significantly smaller in the *No Jury - No Info* treatment than in all other treatments.

We analyze the determinants of the amount sent in columns 3 and 4, and we again include treatment dummies and a period trend. In these regressions, we control for the message amount (if there was a message) and include a separate dummy equaling one if no message was sent. Column 4 also includes a variable controlling for the average amount sent in previous periods. The results suggest that both information and access to the jury significantly *increase* the amount sent. Figure A1 indicates that much of the reason for observed treatment differences is that subjects in the *No Jury - No Info* treatment were far less likely to send their entire endowment than they were in the other 3 treatments (observed probabilities are 0.225 in the *No Jury - No Info* treatment vs. 0.358, 0.333, and 0.455 in the *No Jury - Info*, *Jury - No Info*, and *Jury - Info* treatments, respectively). We cannot reject hypotheses that these 3 treatment coefficients are equal (Wald test p-values are at the bottom of Table A1).

Finally, we analyze the receiver's decision of how much to return to the sender. It is clear from the summary statistics that the receiver returns significantly more than the Nash equilibrium prediction of 0 in all four treatments. Yet, just looking at the percent returned is slightly misleading - since we know that the message sent was not merely cheap talk, it is more informative to analyze the amount that the receiver returned relative to the amount he offered to return in the message. Figure A2 displays histograms of the relative returns on trust for each treatment, where the relative return is defined as $(\frac{AmountReceived}{AmountOffered} - 1) * 100$. In all treatments the modal return is exactly the amount offered (0%), but the probability of returning *nothing* is substantially higher in the two No Jury treatments. This result is confirmed in columns 5 and 6 of Table A1. In these columns, we regress the difference between percentage returned and percentage offered in the pre-play message on the treatment dummies, a period trend, the amount sent, and a constant. Column 6 also includes a control for the receiver's previous behavior. Positive and significant estimated coefficients on the two jury treatment dummy variables indicate that the presence of the jury institution increases

[1]We use treatment dummies instead of jury and info dummies because while the No Jury-Info and Jury-Info treatments both include the group information table, the information conditions are not technically identical, since the jury provides additional information.

[2]In order to incorporate this variable, we dropped period one observations. We cannot add individual fixed effects, since each individual was assigned to only one treatment.

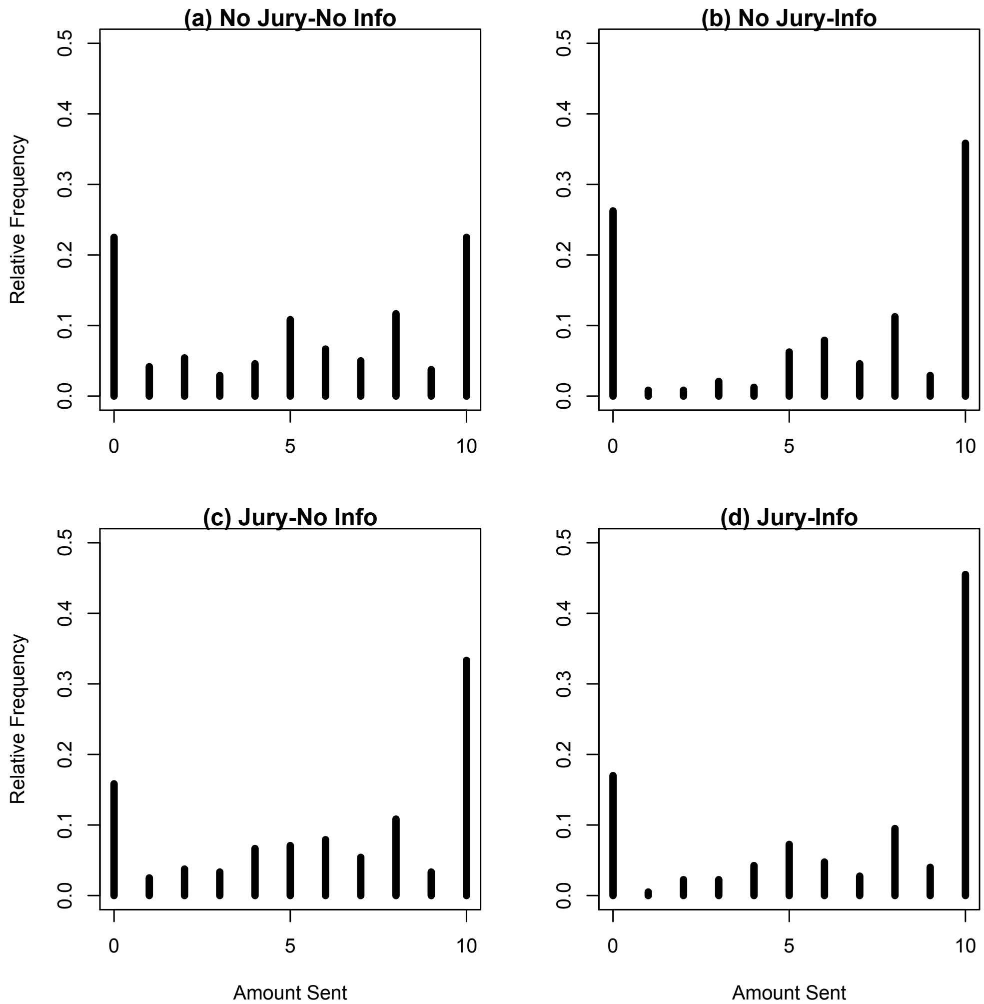


Figure A1: **Histograms of Amount Sent by Treatment.** Each panel displays a histogram of amounts sent by S for one treatment.

the percent returned (relative to the message), but the *No Jury - Info* treatment is not statistically different from the *No Jury - No Info* baseline. Moreover, the coefficients on the two jury treatments are significantly greater than the coefficient on the *No Jury - Info* treatment.

Many important phenomena are apparent in these regression results. First, they confirm the finding from our non-parametric analysis that the Jury treatment has a powerful impact on amount sent. Moreover, these regressions suggest that the Info treatment also appears to encourage trust (measured in amount sent). On the other hand, columns (5) and (6) confirms that the presence of in-group punishment from a jury is enough to encourage many receivers to fulfill their promises, but the indirect punishment that operates through group reputation was not. On the surface, these results are difficult to reconcile - if receivers were more likely to fulfill their promises in only the jury treatments, why did senders send more in *both* the jury *and* the group information treatments? To answer this question, we performed the analyses reported in section 4.2.

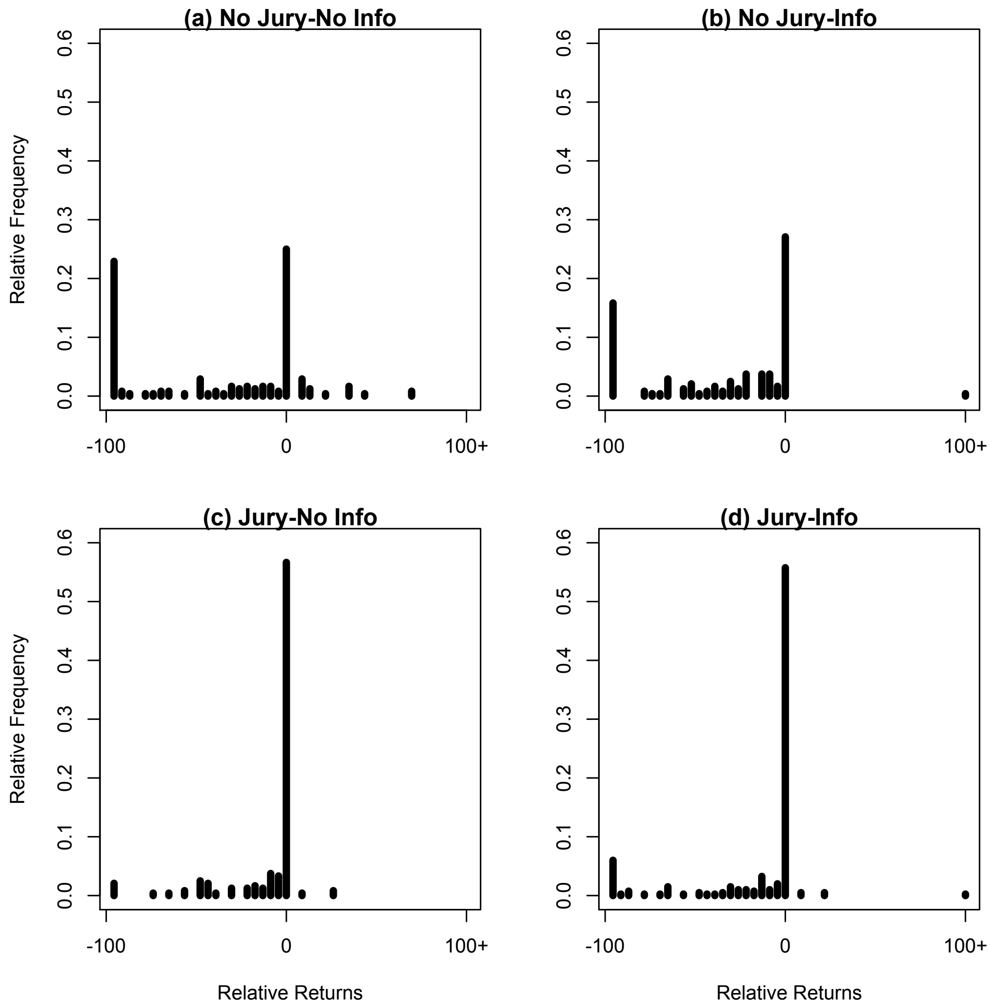
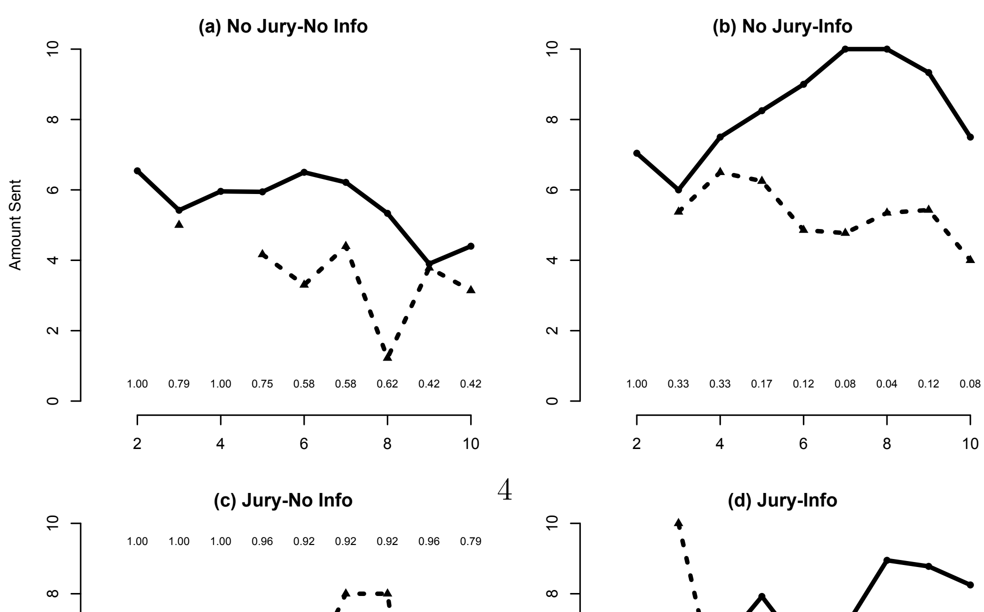


Figure A2: **Histograms of Relative Returns by Treatment.** Each panel displays a histogram of S's relative return on trust for one treatment. Relative returns are computed as $\frac{AmountReturned}{AmountOffered} - 1$ and reported in percentage terms, so that 0% implies that S got exactly what was offered in pre-play communication, and -100% indicates that R returned 0 ECU.

A.2. Alternative Definition of "Good" and "Bad" Experience



A.3. Robustness check: Paying to Vote

As a stress test on the effectiveness of the jury mechanism, we conducted a treatment denoted *Jury - Info - Pay* in which prospective members of the jury could opt-in to jury service at a cost of 2 ECU/vote. This treatment provides a setting in which upholding group reputation through the jury mechanism is costly. In the other treatments, there is no direct penalty for voting.

Returning to Table 1, we see that in terms of amount sent, amount returned, percent returned, and probability of ripping off the sender, the *Jury - Info - Pay* treatment is nearly indistinguishable from the *Jury - Info* treatment. In Appendix A (Tables A5 and A6), we report the results of regressions similar to those in Tables 4 and 5, but including the Pay treatment. The results are essentially the same as those in the *Jury - Info* treatment, with the exception that the negative impact of acquittals on amount sent is slightly mitigated in the Pay treatment. There is no difference in the amount returned, although receivers who have previously been acquitted return slightly less in the Pay treatment. These results suggest that despite the increased cost of appeals to the jury, there is no increase in the rate or magnitude of broken promises.

One likely difference between the *Jury - Info* and *Jury - Info - Pay* treatments is in the likelihood of filing and of winning disputes in case of ripoffs.[3] To determine whether these differences are statistically significant and to understand the origins of any such differences, Table A2 reports mixed effects linear probability estimates where the dependent variable takes a value of 1 when a sender who was ripped off chose to employ the Jury mechanism and 0 otherwise. In column (1), the independent variables are treatment dummy variables (*Jury - No Info* is the baseline), a period trend, the amount sent, the magnitude of the ripoff, and a constant term. The coefficient on the *Jury - Info - Pay* treatment dummy is negative and significant, but this coefficient is not significantly different than the one on the Jury-Info treatment (two-sided p-value = 0.384). This suggests that introducing a cost to upholding group reputation has no significant effect on the sender's decision to sue, all else equal. In columns (2) and (3), we include additional variables meant to account for path dependence, and we interact them with the Pay treatment. As in the Jury treatments without Pay, we find no evidence of significant path dependence, though the estimated coefficient on the "better than autarky" dummy is negative and significant. In neither case are the coefficients on the *Jury - Info* and *Jury - Info - Pay* treatments significantly different.

Finding A1: The sender's decision to sue does not change significantly when receivers have to pay to opt-in to the jury mechanism, all else equal.

Table A3 reports mixed effects linear probability estimates of the decision to convict the receiver in the *Jury - No Info*, *Jury - Info,* and *Jury - Info - Pay* treatments. Here we again investigate the impact of the Pay treatment as well as its interaction with variables designed to account for path dependence. As before, a positive and significant coefficient on the ripoff magnitude indicates that the likelihood of winning the dispute is sensitive to the extent of the violation. In column (2), when we include variables for path dependence,

[3] In one *Jury - Info - Pay* session, no receiver ever returned less than offered, so this analysis is based on additional data from two sessions.

	(1)	(2)	(3)
Action taken by:		**Sender**	
Dependent Variable		Dispute (0/1)	
Jury-Info	-0.120	-0.133	-0.143*
	(0.090)	(0.090)	(0.086)
Jury-Info-Pay	-0.207**	-0.209**	-0.234**
	(0.097)	(0.096)	(0.099)
Inverse Period	-0.051	-0.125	-0.126
$(\frac{1}{t})$	(0.116)	(0.133)	(0.132)
Sent	0.003	-0.001	0.014
(amt. sent by S)	(0.014)	(0.014)	(0.017)
Ripoff Amount	0.043***	0.046***	0.037***
((message % * amt. received from S) - amt. returned by R)	(0.007)	(0.007)	(0.009)
Previous Ripoff		-0.121	-0.105
(fraction of previous pds. S ripped off by R's group)		(0.105)	(0.107)
Previous Win * Pay			0.274
(# of previous trials won by S vs R's group * pay)			(0.171)
Previous Lose * Pay			-0.076
(# of previous trials won by S vs R's group * pay)			(0.125)
Better Than Autarky * Ripoff			-0.151*
(equals **1** if 10 - amt. sent + amt. returned > 10, equals **0** otherwise)			(0.088)
Constant	0.468***	0.547***	0.541***
	(0.129)	(0.145)	(0.143)
Wald Test *p*-values			
Jury-Info = Jury-Info-Pay	0.384	0.530	0.560
Observations	197	197	197
Log Likelihood	-115.4	-114.7	-112.1
χ^2-Statistic	53.58	55.17	61.90

Table A2: Mixed Effects Linear Probability Estimates Explaining the Decision to Dispute, Jury and Pay Treatments

the estimated coefficient on the Pay treatment is negative and significant. Yet a Wald test cannot reject the null hypothesis that the coefficients on the *Jury - Info* and *Jury - Info - Pay* treatments are equal. Column (2) of Table A4 reports the output of similar regressions explaining individuals' decisions to convict. Here we also find that conviction decisions are sensitive to the magnitude of the ripoff. However we observe no treatment difference. As before, voters who have convicted in the past are more likely to do so in the future, but the rate of conviction declines slightly with the number of times an individual votes.

Finding A2: There are no statistically significant differences in the conviction rate in the *Jury - Info* and *Jury - Info - Pay* treatments, all else equal.

Column (1) of Table A4 reports a regression for the *Jury - No Info*, *Jury - Info*, and *Jury - Info - Pay* treatments where the dependent variable takes a value of 1 if the voter chose to convict and 0 otherwise. Here too, we find that the decision to convict is influenced by the magnitude of the ripoff, and we find that individual differences are an important driver of behavior. Those who voted to convict in the past are more likely to do so again, although

	(1)	(2)
Action Taken By:	**Receiver's Group**	
Dependent Variable	Sender Won Dispute (0/1)	
Jury-Info	-0.163	-0.207
	(0.199)	(0.210)
Jury-Info-Pay	-0.347	-0.495*
	(0.236)	(0.263)
Inverse period	-0.070	0.084
($\frac{1}{t}$)	(0.181)	(0.205)
Sent	-0.023	-0.013
(amt. sent by S)	(0.021)	(0.021)
Ripoff Magnitude	0.024**	0.019*
((message % * amt. received from S) - amt. returned by R)	(0.010)	(0.010)
Previous Ripoff of Group, Same * Info		0.162
(fraction of pvs. times R's group members ripped off S's group * info)		(0.219)
Previous Conviction, Group * Jury * Info		-0.089
(# of trials members of R's group convicted * jury * info)		(0.091)
Previous Acquittal, Group * Jury * Info		0.111
(# of trials members of R's group acquitted * jury * info)		(0.075)
Previous Conviction, Group * Jury * Pay		0.329**
(# of trials members of R's group convicted * jury * pay)		(0.142)
Previous Acquittal, Group * Jury * Pay		-0.089
(# of trials members of R's group acquitted * jury * pay)		(0.091)
Constant	0.699***	0.603***
	(0.226)	(0.229)
Wald Test p-values		
Jury-Info = Jury-Info-Pay	0.378	0.233
Observations	115	115
Log Likelihood	-75.33	-71.83
χ^2-Statistic	7.936	15.78

Table A3: Mixed Effects Linear Probability Estimates Explaining Jury's Decisions to Convict, Jury and Pay Treatments

convictions become less likely the more times individuals are asked to vote.

Finding A3: There are no treatment differences in individual voting behavior.

	(1)	(2)
	Convict (1/0)	
Jury-Info	-0.188	-0.203
	(0.131)	(0.138)
Pay		-0.123
		(0.247)
Period	0.007	0.004
	(0.017)	(0.017)
Amt Sent - Amount Returned	0.017***	0.017***
	(0.006)	(0.006)
Previous Conviction Count	0.139***	0.127***
	(0.022)	(0.023)
Previous Vote Count	-0.075***	-0.068***
	(0.016)	(0.016)
Previous Convictions by Group Members * Jury * Info	-0.052	-0.041
	(0.049)	(0.048)
Previous Acquittals by Group Members * Jury * Info	0.042	0.045
	(0.043)	(0.043)
Previous Convictions by Group Members * Pay	.	0.045
	.	(0.117)
Previous Acquittals by Group Members * Pay	.	0.019
	.	(0.124)
Allocation Better than Autarky	0.048	0.036
	(0.070)	(0.067)
Constant	0.605***	0.624***
	(0.118)	(0.122)
Observations	258	274
Log Likelihood	-155.4	-165.0

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, two-sided tests

Table A4: Mixed Effects Regressions Explaining Individual Votes, Jury and Pay treatments

	(1)	(2)	(3)
Action taken by:		**Sender**	
Dependent Variable:		Amount Sent	
Jury-Info	-0.044	-0.059	1.218**
	(0.498)	(0.522)	(0.504)
Pay	0.389	0.281	0.914
	(0.557)	(0.602)	(0.572)
Inverse Period	0.040	-0.532	-1.953**
$(\frac{1}{t})$	(0.888)	(0.896)	(0.907)
Message * Message Sent	14.893***	15.678***	16.879***
(message % * message dummy)	(1.059)	(1.066)	(1.027)
No Message	2.480***	2.846***	3.371***
(no message dummy)	(0.656)	(0.657)	(0.630)
Avg. Sent Pvs.	0.283***	0.282***	0.243***
$(\frac{\sum_{t=1}^{t-1} sent}{t-1})$	(0.060)	(0.060)	(0.058)
Pvs Ripoff, Same	-1.653***	-0.400	-0.263
(fraction of pvs. pds. S ripped off by R's group)	(0.385)	(0.484)	(0.466)
Pvs Ripoff, Other	-0.563		
(fraction of pvs. pds. S ripped off by other (not R's) group)	(0.419)		
Pvs. Win * Jury		-0.786**	-0.398
(# of pvs. trials won by S vs. R's group * jury dummy)		(0.376)	(0.366)
Pvs. Win * Pay		1.453	1.038
(# of pvs. trials won by S vs. R's group * pay dummy)		(0.904)	(0.883)
Pvs. Lose * Jury		-0.853***	-0.792***
(# of pvs. trials lost by S vs. R's group * jury dummy)		(0.307)	(0.297)
Pvs. Lose * Pay		-0.357	0.169
(# of pvs. trials lost by S vs. R's group * pay dummy)		(0.487)	(0.555)
Pvs. Ripoff of Group * Info			-1.481**
(fraction of pvs. times others in S's group ripped of by R's group * info)			(0.651)
Pvs. Wins of Group * Jury * Info			-0.448*
(# of pvs. trials won by others in S's group vs. R's group * jury * info)			(0.238)
Pvs. Wins of Group * Pay			0.439
(# of pvs. trials won by others in S's group vs. R's group * pay)			(0.380)
Pvs. Losses of Group * Jury * Info			-1.253***
(# of pvs. trials lost by others in S's group vs. R's group * jury * info)			(0.214)
Pvs. Losses of Group *Pay			0.893***
(# of pvs. trials lost by others in S's group vs. R's group * pay)			(0.284)
Constant	-1.495*	-1.801**	-1.937***
	(0.781)	(0.774)	(0.734)
Observations	792	792	792
Log Likelihood	-2011.8	-2004.9	-1966.6
χ^2-Statistic	343.9	363.6	479.3

Table A5: Mixed Effects Regressions Explaining the Amount Sent, Jury and Pay Treatments

	(1)	(2)	(3)
Action taken by:		**Receiver**	
Dependent Variable:		Returned % - Message %	
Jury-Info	-0.007	-0.007	0.011
	(0.020)	(0.018)	(0.020)
Pay	-0.028	-0.015	-0.000
	(0.022)	(0.021)	(0.023)
Inverse Period	-0.143***	-0.144***	-0.141***
$(\frac{1}{t})$	(0.035)	(0.035)	(0.036)
Sent	-0.003	-0.003*	-0.003*
(amt. Sent by S)	(0.002)	(0.002)	(0.002)
Avg. Returned – Message Pvs.	0.604***	0.590***	0.577***
$(\frac{\sum_{t=1}^{t-1} \%returned - \%message}{t-1})$	(0.030)	(0.035)	(0.035)
Pvs. Ripoff	-0.059***	-0.048***	-0.022
(fraction of pvs. pds. R ripped off one in S's group)	(0.015)	(0.015)	(0.016)
Pvs. Conviction * Jury		0.024**	0.019*
(# of pvs. trials convicted * jury)		(0.011)	(0.011)
Pvs. Acquittal * Jury		-0.014	-0.019
(# of pvs. trials acquitted * jury)		(0.013)	(0.013)
Pvs. Conviction * Pay		-0.011	-0.026
(# of pvs. trials convicted * pay)		(0.024)	(0.033)
Pvs. Acquittal * Pay		-0.052***	-0.058**
(# of pvs. trials acquitted * pay)		(0.019)	(0.025)
Pvs. Ripoff of Group, Same * Info			-0.103***
(fraction of prior times others in R's group ripped off S's group * info)			(0.032)
Pvs. Conviction, Group * Jury * Info			0.008
(# of trials other members of R's group convicted * jury * info)			(0.009)
Pvs. Acquittal, Group * Jury * Info			-0.004
(# of trials other members of R's group acquitted * jury * info)			(0.010)
Pvs. Conviction, Group * Pay			0.017
(# of trials other members of R's group convicted * pay)			(0.017)
Pvs. Acquittal, Group * Pay			0.009
(# of trials other members of R's group acquitted * pay)			(0.012)
Constant	0.076***	0.071***	0.066***
	(0.023)	(0.022)	(0.023)
Observations	619	619	619
Log Likelihood	537.1	549.2	556.0
χ^2-Statistic	498.1	545.4	567.7

Table A6: Mixed Effects Regressions Explaining the Percent Returned - Percent Promised, Jury and Pay Treatments

B. Experiment Instructions

Below we include instructions for each treatment. Each page of the instructions is labeled in the order subjects saw it in the experimentAny words surrounded by "@" symbols indicate parameters that were automatically filled by the experiment software and may have been specific to the subject.

B.1. No Jury - No Info

Page 1

You are now participating in a decision making experiment. If you follow the instructions carefully, you can earn a considerable amount of money depending on your decisions and the decisions of the other participants. Your earnings will be paid to you in CASH at the end of the experiment

This set of instructions is for your private use only. During the experiment you are not allowed to communicate with anybody. If you have any questions, please raise your hand, and an experimenter will come to your seat and answer them privately. Any violation of this rule excludes you immediately from the experiment and all payments.

This experiment will consist of several periods.

In this experiment, there will be two groups of people, Red and Blue. Each group is composed of 8 people, divided into two types, Senders and Receivers. In total there are 4 Red Senders, 4 Red Receivers, 4 Blue Senders, and 4 Blue Receivers.

You are a @myColor@ @myPlayerType@.

At the beginning of each period you will be randomly paired with either a Red or Blue @partnerPlayerType@. You will never be paired with another @myPlayerType@.

Page 2

Each Sender begins each period with 10 Experimental Currency Units (ECUs). A Sender may choose to send none, any, or all of these ECUs to the Receiver he/she is paired with by typing the amount into a box in the center of the screen and then clicking "Send".

Any ECUs that a Sender sends to a Receiver will be subtracted from the Sender's account, multiplied by 3 and transferred to the Receiver. Any ECUs that a Sender chooses not to send to the Receiver remain the Sender's earnings. (Only Senders will be able to send ECUs and have them multiplied.)

Page 3

Each Receiver enters a period with 10 ECUs.

After the Sender makes a decision, the Receiver will see how many ECUs were sent by the Sender.

The amount sent by the Sender will be multiplied by 3 and added to the Receiver's account. Then the Receiver decides to send none, any or all of these ECUs to the Sender by typing the amount into a box in the center of the screen and then clicking "Send". (Only Receivers will make this decision.)

Page 4

A Message

However, before the Sender decides how many ECUs to send to the Receiver, each Receiver will have the opportunity to send a message to the Sender.

All messages read:

"I will return X% of the total amount that I receive."

Note that the total ECUs received is equal to 3 x ECUs sent. When the percentage is computed, the number is rounded up (i.e. 14.4 becomes 15).

And the Receiver can either: 1) choose X to be an integer between 0 and 100 and click the button labeled Send Message, or 2) click the button labeled No Message.

If the Receiver chooses to send a message, the Sender will read the message prior to making his decision, and if the Receiver chooses No Message, then the Sender will see a message that reads:

"The Receiver has chosen not to send a message."

Page 5

In each period, each Receiver is paired with one Sender for the entire period. (One "period" consists of one Message, one Sender deciding how many ECUs to send to one Receiver and that Receiver deciding how many of the multiplied ECUs to send to the paired Sender.)

Example: If the Receiver offers to return 60% of the ECUs received, and the Sender sends 8 ECUs, then to fulfill the offer, the Receiver would send at least 15 ECUs back to receiver (0.6 * 8 ECUs * 3 = 14.4, rounded up to 15).

Page 6

Earnings

After each player has finished making decisions, you will receive detailed feedback on the outcome of the period.

A Sender's earnings for a period are:

Earnings = Starting ECUs
minus Amount Sent to Receiver
plus Amount Sent from Receiver

A Receiver's earnings for a period are:

Earnings = Starting ECUs
plus Amount Sent by Sender x 3
minus Amount Sent to Sender

Page 7

After each period, you will review the results and click "Continue". The period will begin when all players have clicked "Continue".

At the end of the experiment the sum of your ECUs from all periods will be converted to dollars at a rate of @exchangeRate@ ECUs = 1 Dollar and paid to you privately in cash, plus $7 for arriving to the experiment on time.

This is the end of the instructions. If you have any questions please raise your hand and an experimenter will come by to answer them.

B.2. No Jury - Info

Page 1

You are now participating in a decision making experiment. If you follow the instructions carefully, you can earn a considerable amount of money depending on your decisions and the decisions of the other participants. Your earnings will be paid to you in CASH at the end of the experiment

This set of instructions is for your private use only. During the experiment you are not allowed to communicate with anybody. If you have any questions, please raise your hand, and an experimenter will come to your seat and answer them privately. Any violation of this rule excludes you immediately from the experiment and all payments.

This experiment will consist of several periods.

In this experiment, there will be two groups of people, Red and Blue. Each group is composed of 8 people, divided into two types, Senders and Receivers. In total there are 4 Red Senders, 4 Red Receivers, 4 Blue

Senders, and 4 Blue Receivers.

You are a @myColor@ @myPlayerType@.

At the beginning of each period you will be randomly paired with either a Red or Blue @partnerPlayer-Type@. You will never be paired with another @myPlayerType@.

Page 2

Each Sender begins each period with 10 Experimental Currency Units (ECUs). A Sender may choose to send none, any, or all of these ECUs to the Receiver he/she is paired with by typing the amount into a box in the center of the screen and then clicking "Send".

Any ECUs that a Sender sends to a Receiver will be subtracted from the Sender's account, multiplied by 3 and transferred to the Receiver. Any ECUs that a Sender chooses not to send to the Receiver remain the Sender's earnings. (Only Senders will be able to send ECUs and have them multiplied.)

Page 3

Each Receiver enters a period with 10 ECUs.

After the Sender makes a decision, the Receiver will see how many ECUs were sent by the Sender.

The amount sent by the Sender will be multiplied by 3 and added to the Receiver's account. Then the Receiver decides to send none, any or all of these ECUs to the Sender by typing the amount into a box in the center of the screen and then clicking "Send". (Only Receivers will make this decision.)

Page 4

A Message

However, before the Sender decides how many ECUs to send to the Receiver, each Receiver will have the opportunity to send a message to the Sender.

All messages read:

"I will return X% of the total amount that I receive."

Note that the total ECUs received is equal to 3 x ECUs sent. When the percentage is computed, the number is rounded up (i.e. 14.4 becomes 15).

And the Receiver can either: 1) choose X to be an integer between 0 and 100 and click the button labeled Send Message, or 2) click the button labeled No Message.

If the Receiver chooses to send a message, the Sender will read the message prior to making his decision, and if the Receiver chooses No Message, then the Sender will see a message that reads:

“The Receiver has chosen not to send a message.”

Page 5

In each period, each Receiver is paired with one Sender for the entire period. (One “period” consists of one Message, one Sender deciding how many ECUs to send to one Receiver and that Receiver deciding how many of the multiplied ECUs to send to the paired Sender.)

Example: If the Receiver offers to return 60% of the ECUs received, and the Sender sends 8 ECUs, then to fulfill the offer, the Receiver would send at least 15 ECUs back to receiver (0.6 * 8 ECUs * 3 = 14.4, rounded up to 15).

Page 6

Earnings

After each player has finished making decisions, you will receive detailed feedback on the outcome of the period.

A Sender’s earnings for a period are:

Earnings = Starting ECUs
minus Amount Sent to Receiver
plus Amount Sent from Receiver

A Receiver’s earnings for a period are:

Earnings = Starting ECUs
plus Amount Sent by Sender x 3
minus Amount Sent to Sender

Page 7

After all decisions have been made, you will see a table displaying the results for other @myPlayerType@s in your group. Specifically, for each other @YourColor@ @myPlayerType@ you will see:

1. The color of the @partnerPlayerType@ with whom they interacted
2. The message they sent/received (or No Message if a message wasn’t sent)
3. How many ECUs were sent to the Receiver

4. The amount the Receiver would need to send back to fulfill the message they sent (if they sent a message)

5. How many ECUs were sent to the Sender

6. Their earnings for the period.

Page 8

After each period, you will review the results and click “Continue”. The period will begin when all players have clicked “Continue”.

At the end of the experiment the sum of your ECUs from all periods will be converted to dollars at a rate of @exchangeRate@ ECUs = 1 Dollar and paid to you privately in cash, plus $7 for arriving to the experiment on time.

This is the end of the instructions. If you have any questions please raise your hand and an experimenter will come by to answer them.

B.3. Jury - No Info

Page 1

You are now participating in a decision making experiment. If you follow the instructions carefully, you can earn a considerable amount of money depending on your decisions and the decisions of the other participants. Your earnings will be paid to you in CASH at the end of the experiment

This set of instructions is for your private use only. During the experiment you are not allowed to communicate with anybody. If you have any questions, please raise your hand, and an experimenter will come to your seat and answer them privately. Any violation of this rule excludes you immediately from the experiment and all payments.

This experiment will consist of several periods.

In this experiment, there will be two groups of people, Red and Blue. Each group is composed of 8 people, divided into two types, Senders and Receivers. In total there are 4 Red Senders, 4 Red Receivers, 4 Blue Senders, and 4 Blue Receivers.

You are a @myColor@ @myPlayerType@.

At the beginning of each period you will be randomly paired with either a Red or Blue @partnerPlayerType@. You will never be paired with another @myPlayerType@.

Page 2

Each Sender begins each period with 10 Experimental Currency Units (ECUs). A Sender may choose to send none, any, or all of these ECUs to the Receiver he/she is paired with by typing the amount into a box in the center of the screen and then clicking “Send”.

Any ECUs that a Sender sends to a Receiver will be subtracted from the Sender’s account, multiplied by 3 and transferred to the Receiver. Any ECUs that a Sender chooses not to send to the Receiver remain the Sender’s earnings. (Only Senders will be able to send ECUs and have them multiplied.)

Page 3

Each Receiver enters a period with 10 ECUs.

After the Sender makes a decision, the Receiver will see how many ECUs were sent by the Sender.

The amount sent by the Sender will be multiplied by 3 and added to the Receiver’s account. Then the Receiver decides to send none, any or all of these ECUs to the Sender by typing the amount into a box in the center of the screen and then clicking “Send”. (Only Receivers will make this decision.)

Page 4

A Message

However, before the Sender decides how many ECUs to send to the Receiver, each Receiver will have the opportunity to send a message to the Sender.

All messages read:

“I will return X% of the total amount that I receive.”

Note that the total ECUs received is equal to 3 x ECUs sent. When the percentage is computed, the number is rounded up (i.e. 14.4 becomes 15).

And the Receiver can either: 1) choose X to be an integer between 0 and 100 and click the button labeled Send Message, or 2) click the button labeled No Message.

If the Receiver chooses to send a message, the Sender will read the message prior to making his decision, and if the Receiver chooses No Message, then the Sender will see a message that reads:

“The Receiver has chosen not to send a message.”

Page 5

In each period, each Receiver is paired with one Sender for the entire period. (One “period” consists of one Message, one Sender deciding how many ECUs to send to one Receiver and that Receiver deciding how

many of the multiplied ECUs to send to the paired Sender.)

Example: If the Receiver offers to return 60% of the ECUs received, and the Sender sends 8 ECUs, then to fulfill the offer, the Receiver would send at least 15 ECUs back to receiver (0.6 * 8 ECUs * 3 = 14.4, rounded up to 15).

<u>Page 6</u>

Earnings

After each player has finished making decisions, you will receive detailed feedback on the outcome of the period.

A Sender's earnings for a period are:

Earnings = Starting ECUs
minus Amount Sent to Receiver
plus Amount Sent from Receiver

A Receiver's earnings for a period are:

Earnings = Starting ECUs
plus Amount Sent by Sender x 3
minus Amount Sent to Sender

<u>Page 7</u>

If the Receiver does not fulfill the offer, then the Sender will have the opportunity to pay a cost of @disputeCost@ ECUs to Dispute the outcome. If no message was sent or the Receiver fulfilled the offer, then no Dispute is possible.

If a Sender chooses to Dispute the outcome, then all other Receivers of the same color as the Receiver he/she is paired with will decide on the outcome of the Dispute. Specifically, each other Receiver of the same color will review the details of the dispute and then Vote whether to require the Receiver to Fulfill the offer.

If a majority votes Fulfill:

1. The Receiver will pay to the Sender the difference between the amount he/she originally sent to the Sender and the amount indicated in the message.

2. The Receiver will pay @disputeCost@ ECUs to the Sender.

If a majority votes Do Not Fulfill:

1. The Sender receives no additional ECUs.

If more than one Sender chooses to Dispute the outcome in a single period, the order of Disputes will be random.

Page 8

Example: Continuing the example from earlier (in which the Receiver offers to return 60% of the amount received), suppose the Sender sends 8 ECUs (which multiply into 24 ECUs), and the Receiver returns 10 ECUs. In this case, the amount returned is less than 60% of the amount received (15 ECUs), so the Sender can choose to dispute the outcome for @disputeCost@ ECUs. Then, the other Receivers of the same color as the disputed Receiver will vote whether to require the offer to be fulfilled. On the other hand, if the Receiver returns 15 or more ECUs, the Sender cannot dispute the outcome. If the other Receivers vote to require that the offer is fulfilled, then the Receiver must pay the Sender a 5 ECU payment of the difference between the promised amount (15 ECUs) and the returned amount (10 ECUs), as well as an additional @disputeCost@ ECUs.

Page 9

After each period, you will review the results and click "Continue". The period will begin when all players have clicked "Continue".

At the end of the experiment the sum of your ECUs from all periods will be converted to dollars at a rate of @exchangeRate@ ECUs = 1 Dollar and paid to you privately in cash, plus $7 for arriving to the experiment on time.

This is the end of the instructions. If you have any questions please raise your hand and an experimenter will come by to answer them.

B.4. Jury - Info

Page 1

You are now participating in a decision making experiment. If you follow the instructions carefully, you can earn a considerable amount of money depending on your decisions and the decisions of the other participants. Your earnings will be paid to you in CASH at the end of the experiment

This set of instructions is for your private use only. During the experiment you are not allowed to communicate with anybody. If you have any questions, please raise your hand, and an experimenter will come to your seat and answer them privately. Any violation of this rule excludes you immediately from the experiment and all payments.

This experiment will consist of several periods.

In this experiment, there will be two groups of people, Red and Blue. Each group is composed of 8 people, divided into two types, Senders and Receivers. In total there are 4 Red Senders, 4 Red Receivers, 4 Blue

Senders, and 4 Blue Receivers.

You are a @myColor@ @myPlayerType@.

At the beginning of each period you will be randomly paired with either a Red or Blue @partnerPlayerType@. You will never be paired with another @myPlayerType@.

Page 2

Each Sender begins each period with 10 Experimental Currency Units (ECUs). A Sender may choose to send none, any, or all of these ECUs to the Receiver he/she is paired with by typing the amount into a box in the center of the screen and then clicking "Send".

Any ECUs that a Sender sends to a Receiver will be subtracted from the Sender's account, multiplied by 3 and transferred to the Receiver. Any ECUs that a Sender chooses not to send to the Receiver remain the Sender's earnings. (Only Senders will be able to send ECUs and have them multiplied.)

Page 3

Each Receiver enters a period with 10 ECUs.

After the Sender makes a decision, the Receiver will see how many ECUs were sent by the Sender.

The amount sent by the Sender will be multiplied by 3 and added to the Receiver's account. Then the Receiver decides to send none, any or all of these ECUs to the Sender by typing the amount into a box in the center of the screen and then clicking "Send". (Only Receivers will make this decision.)

Page 4

A Message

However, before the Sender decides how many ECUs to send to the Receiver, each Receiver will have the opportunity to send a message to the Sender.

All messages read:

"I will return X% of the total amount that I receive."

Note that the total ECUs received is equal to 3 x ECUs sent. When the percentage is computed, the number is rounded up (i.e. 14.4 becomes 15).

And the Receiver can either: 1) choose X to be an integer between 0 and 100 and click the button labeled Send Message, or 2) click the button labeled No Message.

If the Receiver chooses to send a message, the Sender will read the message prior to making his decision, and if the Receiver chooses No Message, then the Sender will see a message that reads:

"The Receiver has chosen not to send a message."

Page 5

In each period, each Receiver is paired with one Sender for the entire period. (One "period" consists of one Message, one Sender deciding how many ECUs to send to one Receiver and that Receiver deciding how many of the multiplied ECUs to send to the paired Sender.)

Example: If the Receiver offers to return 60% of the ECUs received, and the Sender sends 8 ECUs, then to fulfill the offer, the Receiver would send at least 15 ECUs back to receiver (0.6 * 8 ECUs * 3 = 14.4, rounded up to 15).

Page 6

Earnings

After each player has finished making decisions, you will receive detailed feedback on the outcome of the period.

A Sender's earnings for a period are:

Earnings = Starting ECUs
minus Amount Sent to Receiver
plus Amount Sent from Receiver

A Receiver's earnings for a period are:

Earnings = Starting ECUs
plus Amount Sent by Sender x 3
minus Amount Sent to Sender

Page 7

After all decisions have been made, you will see a table displaying the results for other @myPlayerType@s in your group. Specifically, for each other @YourColor@ @myPlayerType@ you will see:

1. The color of the @partnerPlayerType@ with whom they interacted
2. The message they sent/received (or No Message if a message wasn't sent)
3. How many ECUs were sent to the Receiver

4. The amount the Receiver would need to send back to fulfill the message they sent (if they sent a message)

5. How many ECUs were sent to the Sender

6. Their earnings for the period.

Page 8

If the Receiver does not fulfill the offer, then the Sender will have the opportunity to pay a cost of @disputeCost@ ECUs to Dispute the outcome. If no message was sent or the Receiver fulfilled the offer, then no Dispute is possible.

If a Sender chooses to Dispute the outcome, then all other Receivers of the same color as the Receiver he/she is paired with will decide on the outcome of the Dispute. Specifically, each other Receiver of the same color will review the details of the dispute and then Vote whether to require the Receiver to Fulfill the offer.

If a majority votes Fulfill:

1. The Receiver will pay to the Sender the difference between the amount he/she originally sent to the Sender and the amount indicated in the message.

2. The Receiver will pay @disputeCost@ ECUs to the Sender.

If a majority votes Do Not Fulfill:

1. The Sender receives no additional ECUs.

If more than one Sender chooses to Dispute the outcome in a single period, the order of Disputes will be random.

Page 9

Example: Continuing the example from earlier (in which the Receiver offers to return 60% of the amount received), suppose the Sender sends 8 ECUs (which multiply into 24 ECUs), and the Receiver returns 10 ECUs. In this case, the amount returned is less than 60% of the amount received (15 ECUs), so the Sender can choose to dispute the outcome for @disputeCost@ ECUs. Then, the other Receivers of the same color as the disputed Receiver will vote whether to require the offer to be fulfilled. On the other hand, if the Receiver returns 15 or more ECUs, the Sender cannot dispute the outcome. If the other Receivers vote to require that the offer is fulfilled, then the Receiver must pay the Sender a 5 ECU payment of the difference between the promised amount (15 ECUs) and the returned amount (10 ECUs), as well as an additional @disputeCost@ ECUs.

Page 10

After each period, you will review the results and click "Continue". The period will begin when all players have clicked "Continue".

At the end of the experiment the sum of your ECUs from all periods will be converted to dollars at a rate of @exchangeRate@ ECUs = 1 Dollar and paid to you privately in cash, plus $7 for arriving to the experiment on time.

This is the end of the instructions. If you have any questions please raise your hand and an experimenter will come by to answer them.

B.5. Jury - Info - Pay

Page 1

You are now participating in a decision making experiment. If you follow the instructions carefully, you can earn a considerable amount of money depending on your decisions and the decisions of the other participants. Your earnings will be paid to you in CASH at the end of the experiment

This set of instructions is for your private use only. During the experiment you are not allowed to communicate with anybody. If you have any questions, please raise your hand, and an experimenter will come to your seat and answer them privately. Any violation of this rule excludes you immediately from the experiment and all payments.

This experiment will consist of several periods.

In this experiment, there will be two groups of people, Red and Blue. Each group is composed of 8 people, divided into two types, Senders and Receivers. In total there are 4 Red Senders, 4 Red Receivers, 4 Blue Senders, and 4 Blue Receivers.

You are a @myColor@ @myPlayerType@.

At the beginning of each period you will be randomly paired with either a Red or Blue @partnerPlayerType@. You will never be paired with another @myPlayerType@.

Page 2

Each Sender begins each period with 10 Experimental Currency Units (ECUs). A Sender may choose to send none, any, or all of these ECUs to the Receiver he/she is paired with by typing the amount into a box in the center of the screen and then clicking "Send".

Any ECUs that a Sender sends to a Receiver will be subtracted from the Sender's account, multiplied by 3 and transferred to the Receiver. Any ECUs that a Sender chooses not to send to the Receiver remain the Sender's earnings. (Only Senders will be able to send ECUs and have them multiplied.)

Page 3

Each Receiver enters a period with 10 ECUs.

After the Sender makes a decision, the Receiver will see how many ECUs were sent by the Sender.

The amount sent by the Sender will be multiplied by 3 and added to the Receiver's account. Then the Receiver decides to send none, any or all of these ECUs to the Sender by typing the amount into a box in the center of the screen and then clicking "Send". (Only Receivers will make this decision.)

Page 4

A Message

However, before the Sender decides how many ECUs to send to the Receiver, each Receiver will have the opportunity to send a message to the Sender.

All messages read:

"I will return X% of the total amount that I receive."

Note that the total ECUs received is equal to 3 x ECUs sent. When the percentage is computed, the number is rounded up (i.e. 14.4 becomes 15).

And the Receiver can either: 1) choose X to be an integer between 0 and 100 and click the button labeled Send Message, or 2) click the button labeled No Message.

If the Receiver chooses to send a message, the Sender will read the message prior to making his decision, and if the Receiver chooses No Message, then the Sender will see a message that reads:

"The Receiver has chosen not to send a message."

Page 5

In each period, each Receiver is paired with one Sender for the entire period. (One "period" consists of one Message, one Sender deciding how many ECUs to send to one Receiver and that Receiver deciding how many of the multiplied ECUs to send to the paired Sender.)

Example: If the Receiver offers to return 60% of the ECUs received, and the Sender sends 8 ECUs, then to fulfill the offer, the Receiver would send at least 15 ECUs back to receiver (0.6 * 8 ECUs * 3 = 14.4, rounded up to 15).

Page 6

Earnings

After each player has finished making decisions, you will receive detailed feedback on the outcome of the period.

A Sender's earnings for a period are:

Earnings = Starting ECUs
minus Amount Sent to Receiver
plus Amount Sent from Receiver

A Receiver's earnings for a period are:

Earnings = Starting ECUs
plus Amount Sent by Sender x 3
minus Amount Sent to Sender

Page 7

After all decisions have been made, you will see a table displaying the results for other @myPlayerType@s in your group. Specifically, for each other @YourColor@ @myPlayerType@ you will see:

1. The color of the @partnerPlayerType@ with whom they interacted
2. The message they sent/received (or No Message if a message wasn't sent)
3. How many ECUs were sent to the Receiver
4. The amount the Receiver would need to send back to fulfill the message they sent (if they sent a message)
5. How many ECUs were sent to the Sender
6. Their earnings for the period.

Page 8

Under the History Table, each Sender will receive information indicating whether the Receiver fulfilled the offer as indicated in the message (by returning at least as much as was indicated in the message). If the Receiver does not fulfill the offer, then the Sender will have the opportunity to pay a cost of @disputeCost@ ECUs to Dispute the outcome. If no message was sent or the Receiver fulfilled the offer, then no Dispute is possible.

If a Sender chooses to Dispute the outcome, then all other Receivers of the same color as the Receiver he/she is paired with will decide whether to Vote or Abstain. Receivers who choose to Vote will pay a cost of @voteCost@ and then will vote on the outcome of the Dispute. Specifically, each other Receiver of the same color who chooses to Vote will review the details of the dispute and then Vote whether to require the Receiver to Fulfill the offer. If multiple disputes occur in a single period, each Receiver will choose whether

to Vote or Abstain only once.

If a majority of those voting votes Fulfill:

1. The Receiver will pay to the Sender the difference between the amount he/she originally sent to the Sender and the amount indicated in the message.
2. The Receiver will pay @disputeCost@ ECUs to the Sender.

If a majority of those voting votes Do Not Fulfill:

1. The Sender receives no additional ECUs.

If more than one Sender chooses to Dispute the outcome in a single period, the order of Disputes will be random.

If all vote to Abstain, then the Receiver will not be required to Fulfill the offer.

If an equal amount vote FULFILL and NOT FULFILL, then the outcome will be decided with a computerized coin flip.

Page 9

Example: Continuing the example from earlier (in which the Receiver offers to return 60% of the amount received), suppose the Sender sends 8 ECUs (which multiply into 24 ECUs), and the Receiver returns 10 ECUs. In this case, the amount returned is less than 60% of the amount received (15 ECUs), so the Sender can choose to dispute the outcome for @disputeCost@ ECUs. Then, the other Receivers of the same color as the disputed Receiver will decide whether to Vote or Abstain. Those that choose to Vote will pay a cost of @voteCost@ and then vote whether to require the offer to be fulfilled. On the other hand, if the Receiver returns 15 or more ECUs, the Sender cannot dispute the outcome. If the other Receivers vote to require that the offer is fulfilled, then the Receiver must pay the Sender a 5 ECU payment of the difference between the promised amount (15 ECUs) and the returned amount (10 ECUs), as well as an additional @disputeCost@ ECUs.

Page 10

After each period, you will review the results and click "Continue". The period will begin when all players have clicked "Continue".

At the end of the experiment the sum of your ECUs from all periods will be converted to dollars at a rate of @exchangeRate@ ECUs = 1 Dollar and paid to you privately in cash, plus $7 for arriving to the experiment on time.

This is the end of the instructions. If you have any questions please raise your hand and an experimenter will come by to answer them.

Vote Entry Decision (If necessary)

You are @myColor@ @myPlayerType@ @myNumberInGroup@.

One or more Red/Blue Sender(s) has chosen to Dispute the outcome of their interaction with a @myColor@ Receiver.

You can choose to VOTE or ABSTAIN during the dispute(s) to follow.

If you choose to VOTE, you must pay @voteCost@ ECU, and you will be able to vote in all of the Disputes involving a @myColor@ Receiver. If you are not eligible to vote (because you are part of the dispute), then you will not pay the cost.

If you choose to ABSTAIN, you will neither be charged nor be able to vote in any Dispute.

If a majority of those voting vote to FULFILL, then the @myColor@ Receiver will have to fulfill the offer and pay the an additional @disputePenalty@ ECU.

If a majority of those voting vote to NOT FULFILL, then nothing will happen.

If an equal amount vote FULFILL and NOT FULFILL the outcome will be decided with a computerized coin flip.

If all @myColor@ Receivers ABSTAIN, the order will not be fulfilled. Please choose indicate your choice by clicking the VOTE or ABSTAIN button.

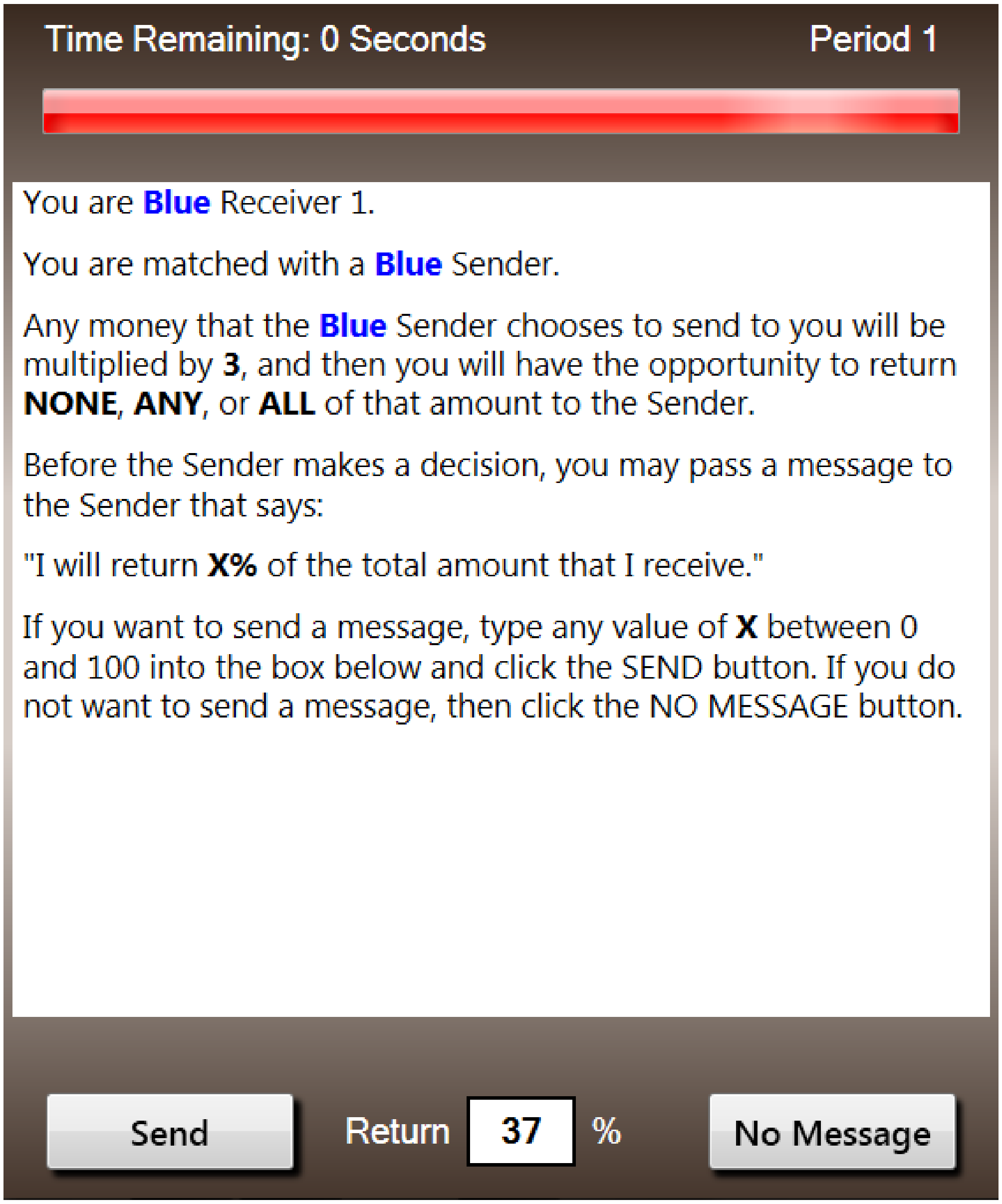


Figure B1: **Screen Shot of Receiver's Message Decision.**

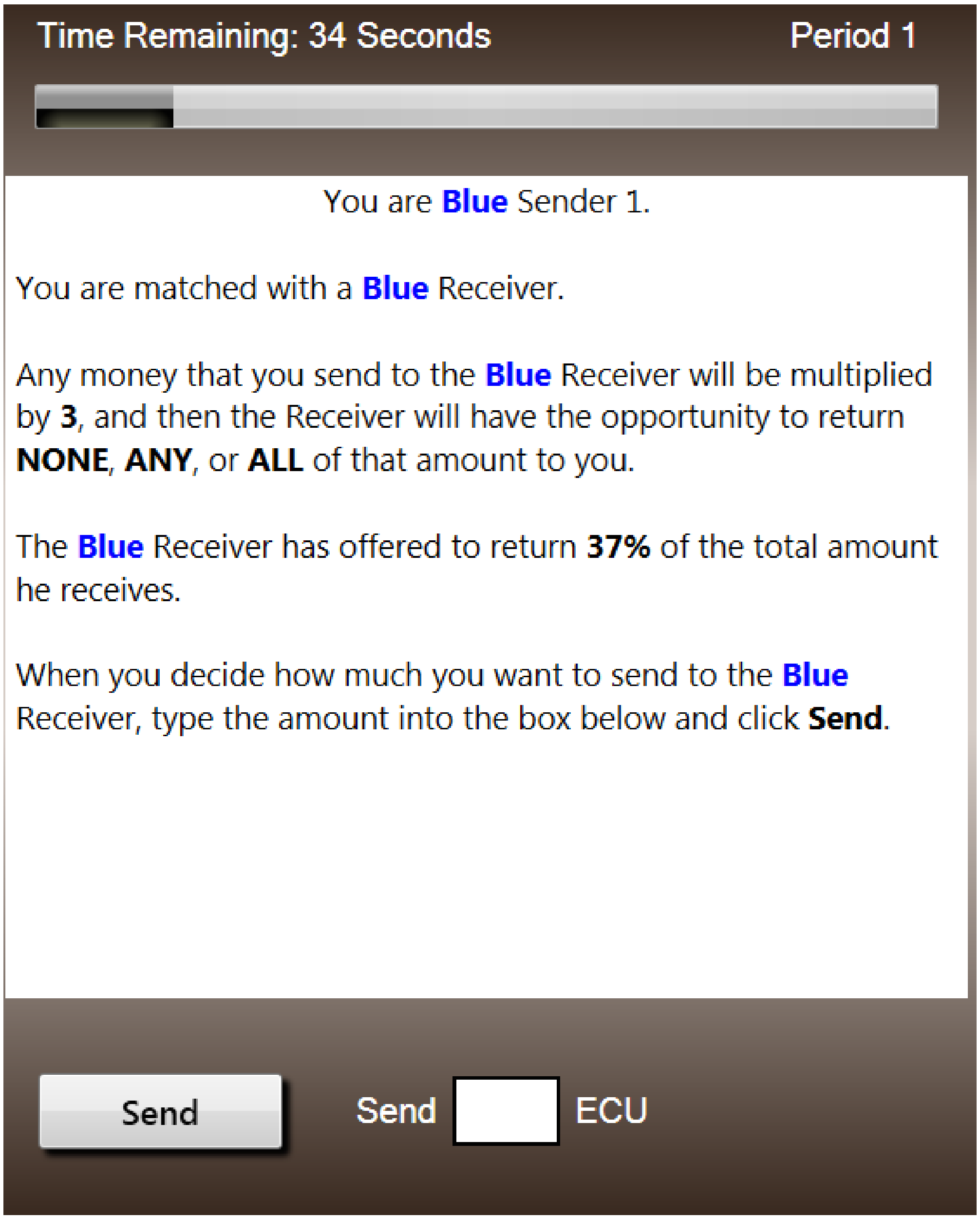


Figure B2: **Screen Shot of Sender's Decision.**

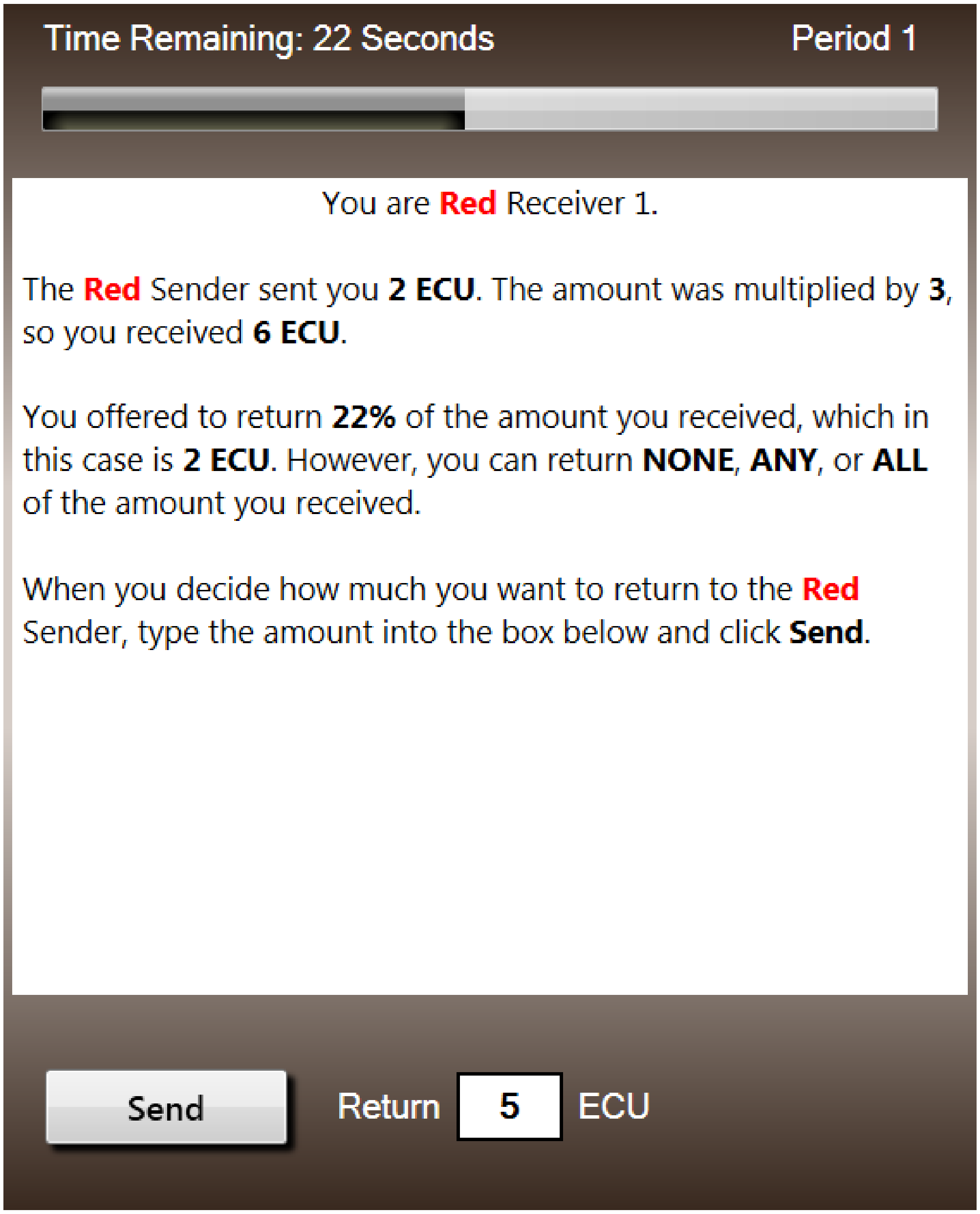


Figure B3: **Screen Shot of A Receiver's Return Decision.**

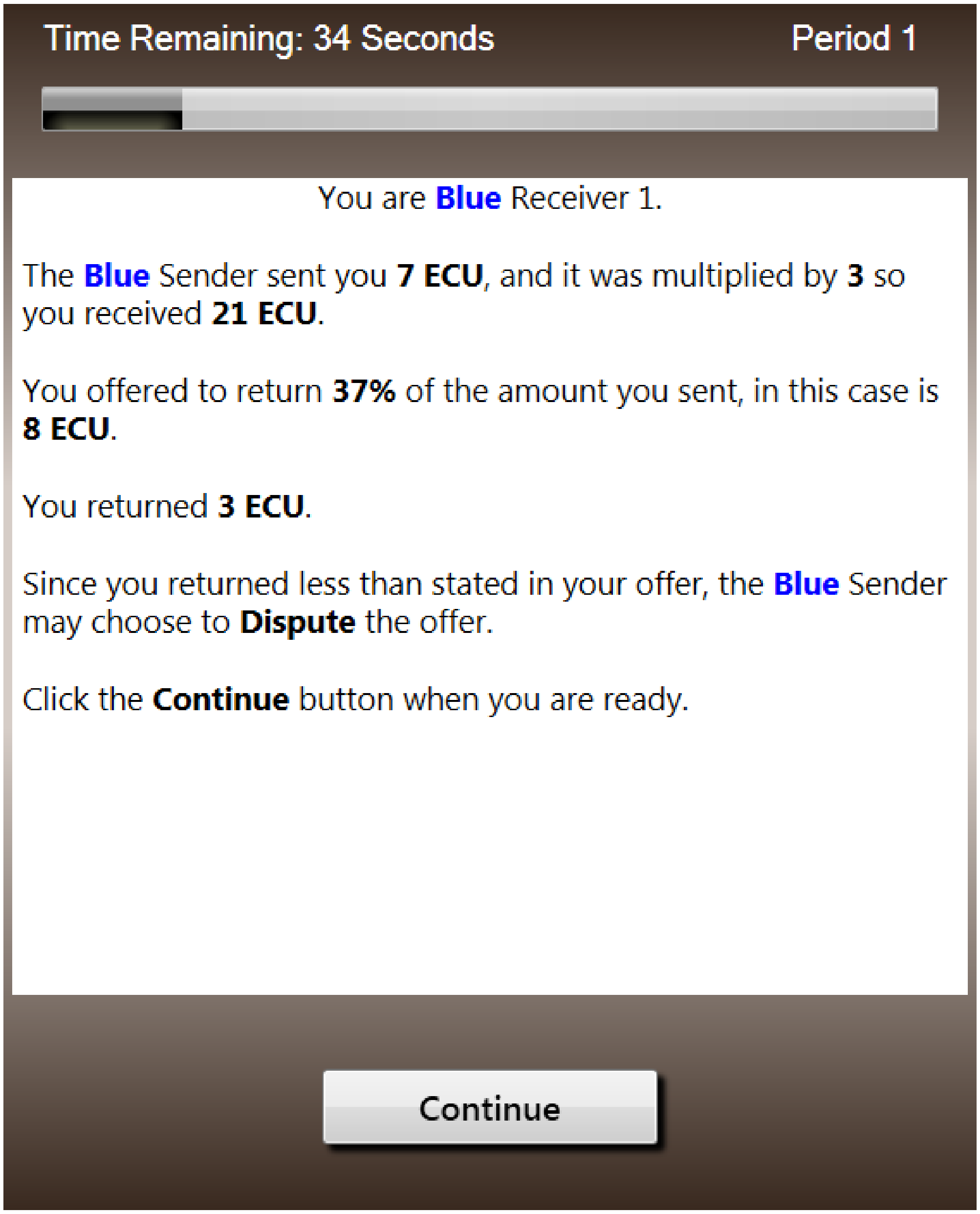


Figure B4: **Screen Shot Showing the Results of a Receiver's Return Decision.**

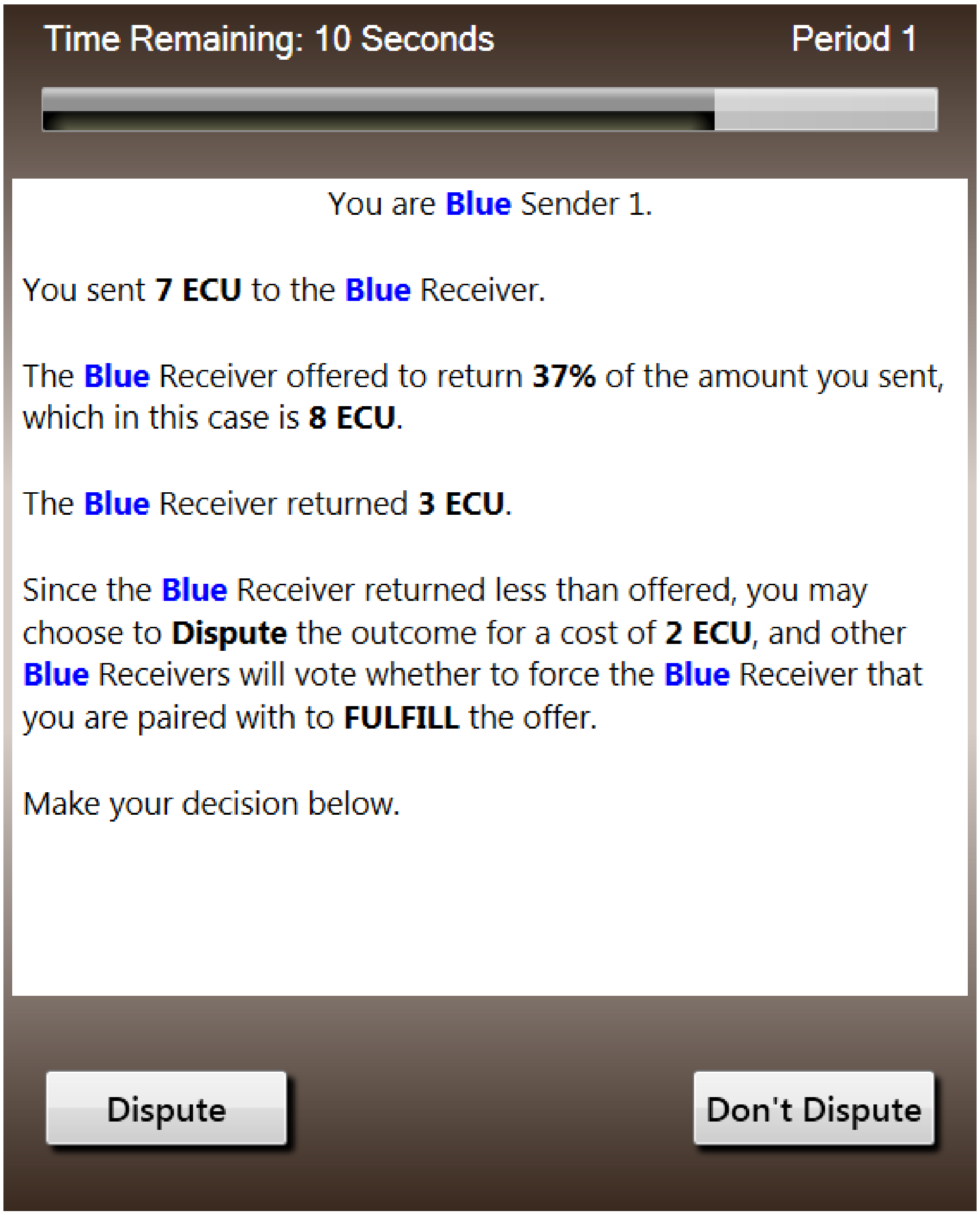


Figure B5: **Screen Shot of Sender's Dispute Decision.**

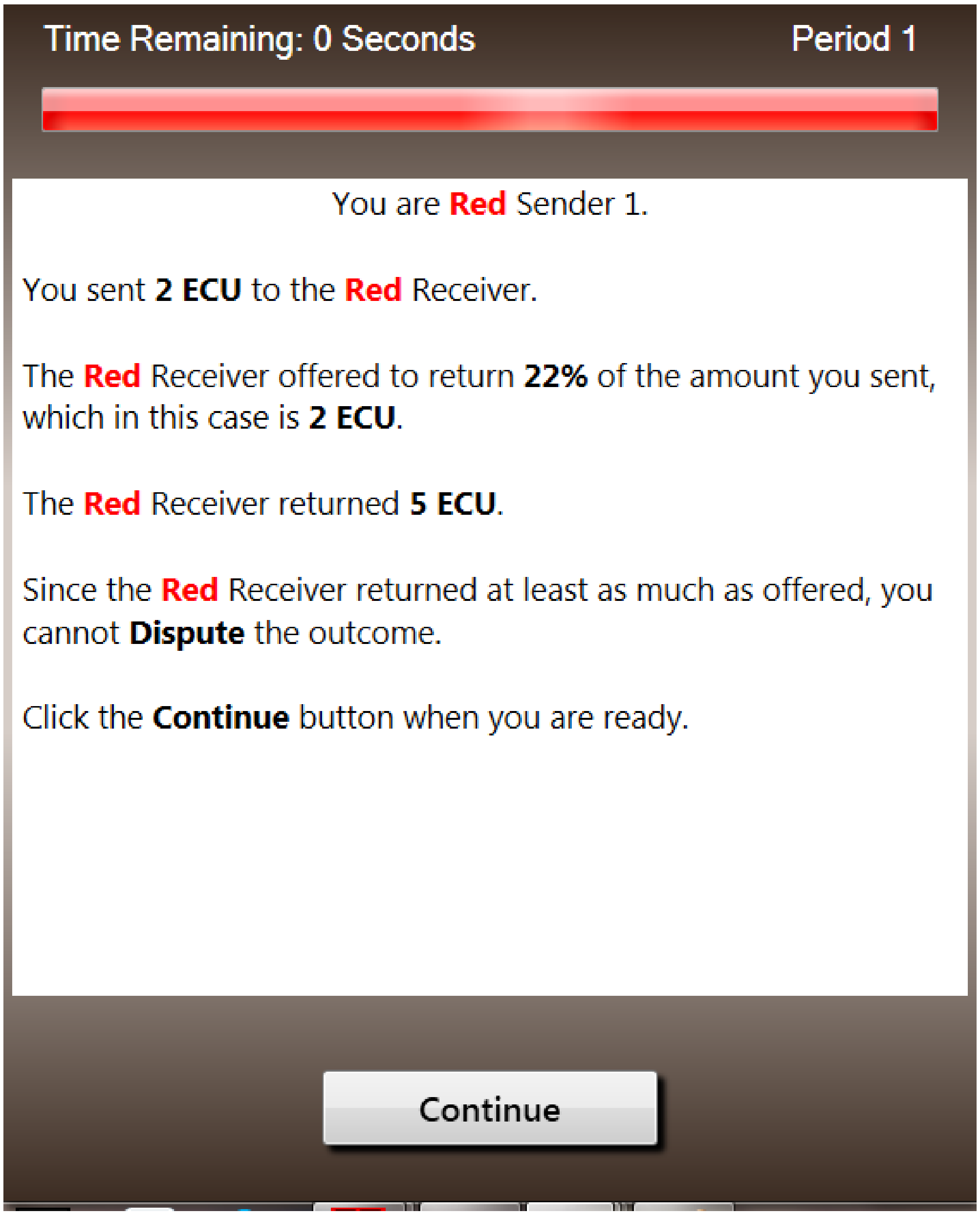


Figure B6: **Screen Shot of a Sender Who Cannot Dispute.**

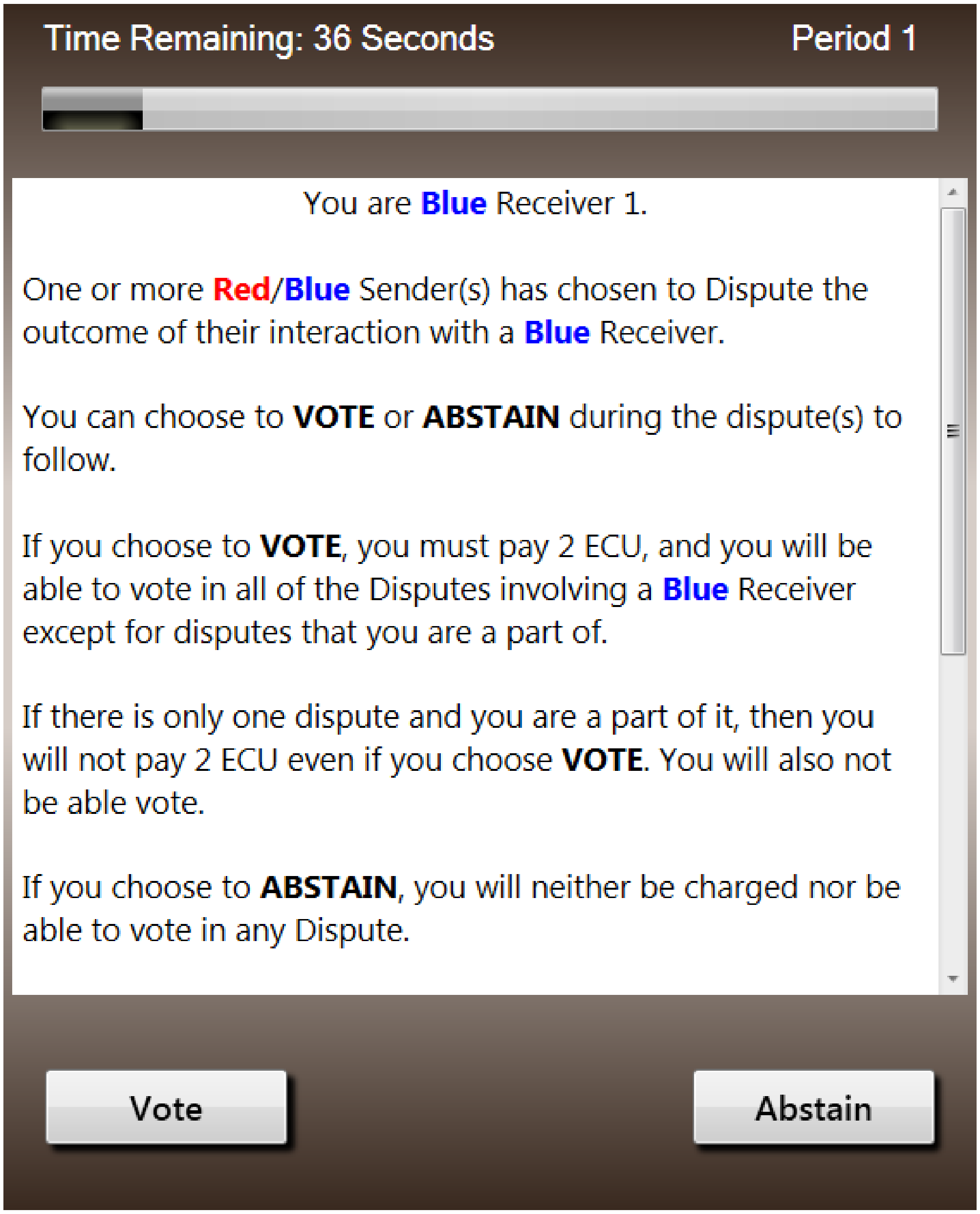


Figure B7: **Screen Shot of Vote Entry Decision - Pay Treatment.**

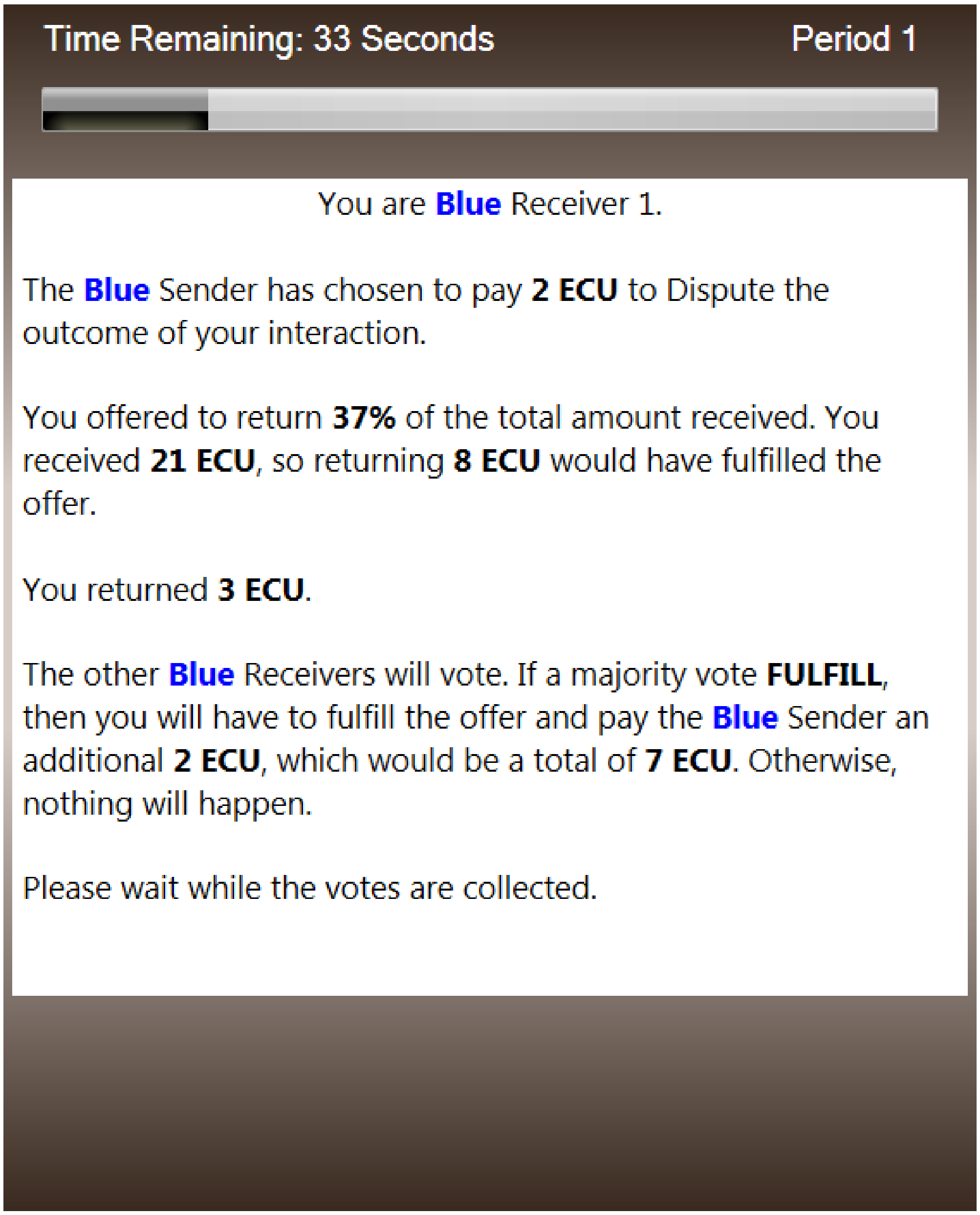


Figure B8: **Screen Shot of Receiver Dispute.**