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Sustaining Group Reputation

Erik O. Kimbrough* and Jared Rubin^{†,‡}

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Abstract

When individuals trade with strangers, there is a temptation to renege on contracts. In the absence of repeated interaction or exogenous enforcement mechanisms, this problem can impede valuable exchange. Historically, individuals have solved this problem by forming institutions that sustain trade using *group*, rather than individual, reputation. Groups can employ two mechanisms to uphold reputation that are generally unavailable to isolated individuals: information sharing and in-group punishment. In this paper, we design a laboratory experiment to distinguish the roles of these two mechanisms in sustaining group reputation and increasing gains from trade. We find that information sharing encourages path dependence via group reputation; good (bad) behavior by individuals results in greater (fewer) gains from exchange for the group in the future. However, the mere threat of in-group punishment is enough to discourage bad behavior, even if punishment is rarely employed. When combined, information sharing and in-group punishment work as complements; the presence of in-group punishment encourages cooperation early on, and information sharing reinforces this behavior over time.

JEL Classifications: C9, D02, D7, Q2

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

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1 Introduction

Most trade is sequential. One party receives goods or money and then gives the other party something in return. This means that, absent any enforcement mechanism, the second party has incentive to renege on the original agreement. Knowing this, the first party will not enter into the trade arrangement in the first place, and potential welfare gains are lost.

In order for both parties to be willing to engage in trade, they must be able to commit *ex ante* to fulfilling their end of the bargain *ex post* (Greif, 2000, 2006). This problem is solved today in many parts of the world with well-functioning legal, judicial, and penal institutions that impartially convict and punish cheaters. Such institutions are costly to build, however, and they are clearly not the only solution (Dixit, 2004); after all, trade predates the emergence of these types of institutions by millennia (see e.g. Cunliffe, 2008).

Repeated interaction is one solution for sustaining trade in the absence of third-party enforcement (Fudenberg and Maskin, 1986). When the discounted gains from future encounters exceed the gains from reneging in the present, both parties will willingly trade. Yet, there are many circumstances under which repeated interaction cannot sustain trade. If individuals have many options for trade partners and information about their past actions is not available, the incentive to cheat can outweigh the returns from repeated relationships, since the cheating party cannot be punished in the future (Ghosh and Ray, 1996; Kranton, 1996).

Under these circumstances, trade is unlikely to occur unless some type of reputation-based or punishment institution is present. Reputation building can encourage trade by linking past actions to future rewards. The Maghribi Traders' Coalition studied by Greif (1993, 2006) provides an excellent example of how such an institution can facilitate trade through a mechanism allowing agents to build individual reputations. Yet, a problem with institutions based on individual reputation is that they are expensive to build and maintain.

Information about each individual's past actions must be collected and traders must be monitored to ensure that they abide by the rules governing the institution.

Since information on individual reputation is costly to collect, a common solution for facilitating exchange is the establishment of institutions meant to create and sustain *group* reputation. Institutions that rely on group reputation have a clear advantage over those that rely on individual reputation or those that rely on formal legal and penal institutions: it is much less expensive to collect and disseminate information on group reputation. Instead of having to obtain information about past actions of each individual trade partner, all one has to know about a counterpart is their group identity and whether any of their group members have cheated in the past. This makes group reputation an attractive mechanism to facilitate trade (Ghatak and Guinnane, 1999).

Yet, institutions based on group reputation can be costly in other ways. In particular, there is a cost that arises from the tension between individual and group incentives: individuals impose a negative externality on their group members 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, there is little incentive for group members to internalize the externality that their actions impose on fellow group members.

How do groups overcome this problem and align the incentives of the individual with those that are best for the group? Historically, groups formed institutions that promoted enforcement through *in-group punishment*.¹ One example of such an institution was the community responsibility system (CRS) used by medieval European trading cities. In this institution, *all* traders from an entire merchant community were punished for a transgression by any *one* of their group members (Greif, 2002, 2004; Boerner and Ritschl, 2009). The key

¹For a broad historical overview, see Levinson (2003). For a theoretical analysis of enforcement of group reputation enhancing norms via in-group sanction, see Kandori (1992).

institutional feature of the CRS was a system of courts that encouraged groups to punish cheaters within their own group in order to uphold the group’s reputation. Similar in-group punishment mechanisms were used by medieval merchant guilds (Milgrom et al., 1990; Greif et al., 1994) and craft guilds (Richardson, 2005; Richardson and McBride, 2009). Group reputation was important to both types of guilds since their access to markets depended on it.

Institutions that sustain exchange and cooperation through in-group punishment are also widespread in the modern world. An important example comes from microfinance, where most loans are based on joint liability, and co-signing peers encourage compliance through social sanctions (Besley and Coate, 1995; Ghatak and Guinnane, 1999; de Aghion, 1999). Fearon and Laitin (1996) provide an example from political science, arguing that one reason that inter-ethnic conflict is the exception rather than the rule is that ethnic groups have significant incentive to punish transgressors from their own group, since doing so maintains the group’s reputation and helps avoid conflict. Legislative politics provides yet another example of widespread in-group punishment. Since individual politicians’ reputations are also tied to the reputation of their party, party leaders often reward and punish with committee memberships and other resources in order to encourage individual actions consistent with the objectives of the group (Cain et al., 1987; Katz and Sala, 1996).²

Recent theory and experimental evidence suggests that another mechanism besides in-group punishment is available to groups to encourage individuals to uphold group reputation: *information sharing*. Specifically, we mean in-group sharing of information about individuals’ past interactions with other groups. If members of Group A know how their fellow members were treated by those in Group B, Group B quickly gains a reputation with all

²More generally, there are a host of business practices as well as features of political institutions that 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.

members of Group A, not just those with whom they interacted. This increases the costs of cheating for Group B members since it reduces potential future gains from trade with all of Group A. While these increased costs of cheating are not always enough to induce a purely-selfish, money-maximizing individual to act in a manner that upholds group reputation (at the expense of individual gain), Healy (2007) shows that if there is a sufficient percentage of *pro-social, reciprocal* types³ and individuals have a sufficiently low discount rate, the incentives to preserve group reputation can encourage even selfish types to sustain cooperation through the penultimate interaction. Healy’s result hinges on the assumption that some degree of “stereotyping” occurs in anonymous settings; individuals don’t actually know the “types” of the people they are dealing with, so they update their beliefs by assuming that all individuals from a cheater’s group are likely cheaters. This theoretical construct should be exacerbated when groups are able to share information, since stereotyped beliefs spread quicker throughout the population.

In most real-world settings where in-group punishment is used to uphold group reputation, there is some degree of information sharing within groups; this is certainly true of the historical institutions noted above (the CRS and guilds) as well as contemporary institutions such as microfinance and political parties. Since both information sharing and in-group punishment can work to sustain group reputation, it is not clear *ex ante* whether both are necessary or how they interact when both are present. Our paper addresses this problem with an experiment aimed at parsing out the roles that in-group punishment and information sharing play in sustaining group reputation. In our experiment, subjects play a repeated trust game (as in Berg et al., 1995) with pre-play, cheap-talk communication and random rematching where group identity, but not individual identity, is known. This creates a situation similar to those described above in which parties have incentives to “cheat” and

³In a variety of well-known experiments, individuals exhibit extensive pro-social behavior (see Cooper and Kagel, 2013, for a summary).

individual reputation mechanisms are infeasible. We employ a 2x2 experimental design varying, 1) whether receivers have access to a jury system, through which group members can vote to punish cheaters in *their own* group (*in-group punishment*), and 2) whether subjects are given information on how others in their group fared (*information sharing*).

Our design allows us to distinguish whether and how the two mechanisms of sustaining group reputation influence behavior in an environment where individual reputation concerns are absent. Specifically, because individuals trade with members of two different groups, with whom they typically have different histories of interaction, we can identify the effects of both mechanisms by decomposing history-dependence into session-wide and group-specific components. In other words, we demonstrate the importance of group reputation by examining whether individuals treat members of a group differently according to both their own *and* their group members' history of interaction with that group.

Previous experimental work on cheating in trade has demonstrated that the opportunity to cheat substantially reduces the gains from exchange (Cassar et al., 2009), and additional work emphasized the power of information-sharing to facilitate *individual* reputation-building to reduce the likelihood and cost of cheating (Cassar et al., 2010). Similarly, individual reputation-building has been shown to encourage trust and reciprocity in repeated trust games, both in repeat dyadic interaction and in interactions with strangers whose reputations are known (Bohnet and Huck, 2004). Group reputation has also received some recent attention in the lab. In addition to Healy (2007), who establishes the effectiveness of group reputation in encouraging cooperation in a labor market experiment, Huck and Lünser (2010) report an experiment with repeated extensive form trust games comparing a situation where subjects receive information about about the play of the individual with whom they are matched to one where they only receive information about the behavior of the matching group as a whole. They find some evidence that group information is a substitute for individual information in small groups and can encourage the gains from exchange. Similarly,

McIntosh et al. (forthcoming) report a public goods game where individuals are assortatively matched over time according to either individual past behavior or the past behavior of their group. They too find that individual and group information are substitutes.

Our experiment differs from previous work in that we explore the separate and joint impact of two mechanisms that use group reputation to facilitate cooperation. We not only show *that* these mechanisms work, but *why* they work. We find that both information-sharing and in-group punishment can increase welfare relative to a baseline where neither is available. Moreover, when *both* are present, they act as complements, further increasing welfare gains. With information, individuals are able to tailor their behavior not only to their own history of interaction but also to that of their group members – expanding trade when it has been successful and restraining it when previous efforts led to cheating. Information sharing therefore accentuates path-dependence and aligns individual behavior with the incentives faced by the group. Unlike information sharing, in-group punishment works by reducing the incentives to cheat in the first place. Knowing that they may be punished by their group members, individuals cheat much less in the jury treatments, regardless of the past actions of the juries.⁴ Thus, the complementarities are obvious: the presence of in-group punishment encourages cooperation early on, and information sharing reinforces this behavior over time. This is perhaps a primary reason why groups that rely on in-group punishment to sustain group reputation also frequently share information within their groups (both historically and contemporarily). Information sharing *reinforces* the gains made via in-group punishment.

2 Experimental Design

We employ a 2x2 experimental design to explore the impact of group information (Info) and in-group punishment (Jury) on the gains from exchange in repeated investment trust games

⁴We do find that individuals cheat even less when they have been convicted in the past. Regardless, there is a clear treatment effect independent of previous actions.

with pre-play communication and random rematching (see Berg et al. (1995) for the classic reference on investment trust games). 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 access to the jury to enforce contracts and can only acquire group reputation information through their own experience. Sixteen subjects are each initially 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 which period the game would end, and the game ended more than 30 minutes before the allotted time slot, so we expect there to be no endgame effects. In total there are eight subjects of each color, and of those eight, four are senders and four are 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 can choose to send any integer amount between 0 and 10 ECU to the receiver, knowing that the amount sent 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 $(10 - \text{ECUs sent to the receiver} + \text{ECUs returned from the receiver})$ and the receiver earns $(10 + \text{ECUs sent from the sender} - \text{ECUs returned to the receiver})$.

Prior to the sender's decision, the receiver has the opportunity to send a *non-binding* message stating "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 a button indicating that she does not want to send a message. The sender then reads the message (or reads that the receiver has chosen not to send a message) and makes her decision. The receiver then sees a message indicating the promised return, given the message she sent, and chooses how much to return. Finally, both parties are informed about their final payoffs as well as whether the amount that the receiver would return if she fulfilled the message

she sent. Crucially, the message allows both subjects and experimenters to clearly define instances of “cheating”. The message can be viewed as an informal contract, and the contract is violated by any receiver returning less than promised in the message.

It should be clear that the subgame perfect Nash equilibrium of the game is for the sender to send nothing because the receiver has a dominant strategy to keep the whole amount sent. Nevertheless, it is well known that individuals regularly send substantial amounts in one-shot trust games, and receivers typically return at least some portion of what they receive. There are potential gains from exchange, and trusting behavior by the first mover is often rewarded with reciprocity by the second mover. Moreover, there is extensive evidence that “cheap talk” can encourage such behavior (e.g Charness and Dufwenberg, 2006; Bohnet and Baytelman, 2007; Ben-Ner and Putterman, 2009; Balliet, 2010).

Thus, our baseline treatment provides us with estimates of the gains from exchange with cheap talk contracting when individuals have only private information about previous interactions with blue and red counterparts. Our other treatments vary 1) the amount of information subjects receive about the outcomes of others in their group and 2) access to the jury contract enforcement mechanism.

In the *No Jury - Info* treatment, the procedures are identical to the *No Jury - No Info* treatment except that, at the conclusion of each period, subjects also receive information about what happened to each other player of the same color and type.⁵ For example, all blue senders learn the following information about each other blue sender at the end of the period: 1) the color of their matched receiver counterpart; 2) the message sent; 3) the number of ECUs sent; 4) the amount the receiver would need to return to satisfy the message sent; 5) how many ECUs were actually returned; and 6) the sender’s earnings. This models an information sharing agreement among members of group who, at the conclusion

⁵This technique is similar to that applied in Cassar et al. (2010), where information is shared within trading networks but not between them.

of each transaction, report the details to others in the group. Alternatively, it also models a small-group setting, where information about the actions of all other group members is easily attainable. Note that this information does not change the subgame perfect Nash equilibrium of the stage game, but by providing richer information about the actions taken by others of the same type and their counterparts, it may help shape agents' beliefs about the actions that will be taken by their future counterparts. In other words, in this relatively small world (with 8 potential trading partners) the richer information set produced by the *No Jury - Info* treatment allows 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. At the conclusion of each transaction, senders review the outcome and compare the amount returned by the receiver to the amount "promised" in the message. If the amount returned is less than promised, the sender has the option of appealing to a jury of the receiver's peers to attempt to force the receiver to pay the remaining amount. Specifically, if a sender seeks restitution, he incurs a cost of 2 ECU and the dispute is 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 "cheating" receiver to fulfill the contract. If a majority vote in favor of the plaintiff, the defendant must pay the plaintiff the outstanding balance plus the cost of dispute (2 ECU) incurred by the plaintiff. If a majority vote in favor of the defendant, the original transaction stands and the dispute cost incurred by the sender is sunk.

In theory, voting receivers are indifferent between voting in favor of the plaintiff and the defendant, as neither outcome affects their payoffs. However, if voters are concerned about their group's reputation and its impact on their future gains from exchange (which depend wholly on how much senders are willing to send to them), then they may be inclined to vote in favor of the plaintiff. On the other hand, if they also intend to not repay future promises in full, then they may vote in favor of other receivers of their color in hopes that those receivers

will do the same for them when they are a defendant. Note that the jury institution also induces part of the information effect of the Info treatments, since the parties to any case that goes to a vote will receive information about the decisions of others that they would not receive in the baseline treatment. Our fourth treatment, the *Jury - Info* treatment, identifies the joint effects of information sharing and in-group punishment directly 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 appeals to the jury.

Finally, as a robustness check on our *Jury - Info* treatment, we also run a treatment in which receivers choose whether or not to participate in each vote. Specifically, before receiving any information about the details of the transaction, receivers must choose whether to *opt in* at a cost of 2 ECU per case. This increases the cost of managing group reputation. We call this the *Jury - Info - Pay* treatment. 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 in our experiments, 16 per session. We ran 3 sessions of each 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 conclusion of the session subjects were paid their earnings in cash plus a \$7 show-up payment for arriving to the session on time. Average earnings were \$16.25 for a 60 minute session (not including the show-up payment).

3 Results

3.1 Summary Statistics and Basic Results

For most of the 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 will compare these

treatments to the one where receivers paid to vote in the jury (*Jury - Info - Pay*) in Section 3.4.

Table 1 reports summary statistics for all treatments. There are numerous treatment effects that stand out through a simple analysis of the means. First, note that the mean amount sent is lower in the baseline treatment (*No Jury - No Info*) than in the other three treatments. This can also be seen in figure 1, which plots histograms of the amount sent by senders in each treatment. The differences in amount sent are important, because total

<i>Treatment</i>	<u><i>No Jury - No Info</i></u>			<u><i>No Jury - Info</i></u>			<u><i>Jury - No Info</i></u>			<u><i>Jury - Info</i></u>			<u><i>Jury - Info - Pay</i></u>		
Variables	Mean	SE	N	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	6.833	0.233	240
Amount Returned	5.033	0.399	240	5.579	0.375	240	8.325	0.394	240	8.825	0.336	400	8.675	0.392	240
Message % (if sent)	51.799	0.907	224	45.891	0.811	229	46.601	0.736	213	48.530	0.630	368	45.698	0.634	232
% returned (if sent)	0.297	0.018	186	0.297	0.015	177	0.430	0.011	202	0.418	0.010	332	0.405	0.012	203
Receiver Earnings (per pd)	20.667	0.564	240	22.396	0.598	240	19.667	0.384	240	20.720	0.333	400	21.446	0.446	240
Sender Earnings (per pd)	9.800	0.302	240	9.588	0.273	240	12.417	0.200	240	12.605	0.203	400	11.871	0.262	240
Sent 10	0.225	0.027	240	0.358	0.031	240	0.333	0.030	240	0.455	0.025	400	0.358	0.031	240
Returned 0 (if sent > 0)	0.471	0.032	240	0.425	0.032	240	0.204	0.026	240	0.250	0.022	400	0.242	0.028	240
Returned < Message	0.483	0.032	240	0.488	0.032	240	0.325	0.030	240	0.293	0.023	400	0.288	0.029	240
Dispute Filed							0.627	0.068	51	0.635	0.053	85	0.475	0.064	61
Dispute Won		NA			NA		0.500	0.090	32	0.519	0.069	54	0.345	0.090	29
Dispute Won, one voter							N/A			N/A			0.769	0.122	13

Table 1: Summary Statistics

welfare is almost solely dependent on the amount sent.⁶ Total welfare increases threefold for every ECU sent, whereas every ECU returned is merely a transfer from one player to another.

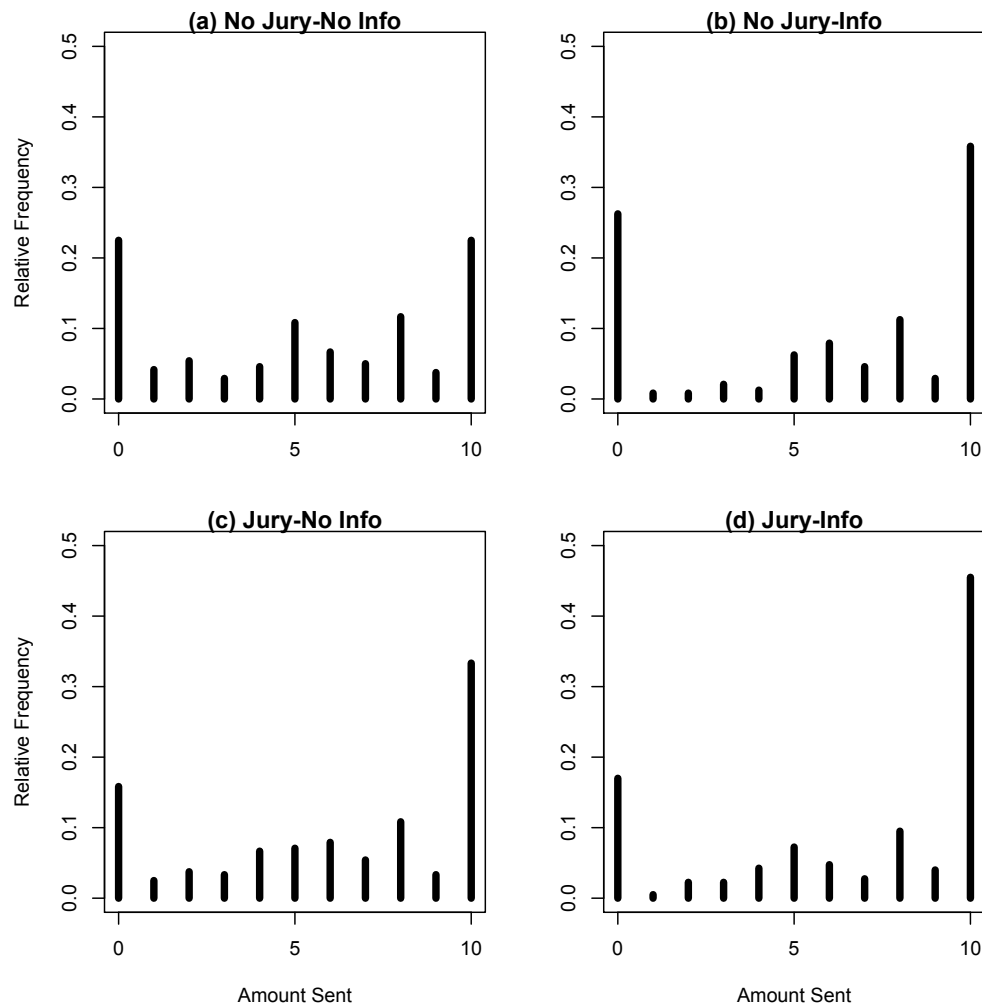


Figure 1: **Histograms of Amount Sent by Treatment.** Each panel displays a histogram of amounts sent by S for one treatment.

Returning to table 1, there also appear to be treatment differences in the amount returned by receivers. The relevant difference here, however, is between the *Jury* and *No Jury* treatments. This suggests that while both the availability of information about others in

⁶In the Jury treatments, total welfare is also a function of whether a trial occurs, since a trial reduces total welfare by 2 ECU.

one's group and the jury mechanism are working to increase the amount sent, they work in different ways.

Of course, comparing means can only tell us so much. In order to understand the determinants of the amount sent and returned, we must control for other variables which may affect the decision in question. For example, the amount sent is likely a function of the message received (as well as whether a message was sent), and the amount returned is almost certainly a function of the amount sent. To this end, Table 2 reports the results of linear regressions 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.

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 a regression 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*)⁷, 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.⁸ 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

⁷We 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.

⁸In 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.

Action taken by: Dependent Variable:	(1) Receiver Message %	(2) Receiver Message %	(3) Sender Amount Sent	(4) Sender Amount Sent	(5) Receiver Returned % - Message %	(6) Receiver Returned % - Message %
No Jury-Info	-0.058* (0.031)	-0.043* (0.024)	1.307* (0.724)	1.126* (0.674)	0.023 (0.065)	-0.000 (0.034)
Jury-No Info	-0.062** (0.031)	-0.053** (0.025)	1.952*** (0.724)	2.042*** (0.674)	0.190*** (0.064)	0.109*** (0.035)
Jury-Info	-0.041 (0.028)	-0.035 (0.022)	2.192*** (0.646)	2.039*** (0.602)	0.130** (0.058)	0.086*** (0.031)
Inverse Period ($\frac{1}{t}$)	-0.119*** (0.025)	-0.062** (0.026)	3.056*** (0.815)	2.437*** (0.832)	-0.002 (0.033)	-0.034 (0.035)
Avg. Message Pvs. ($\frac{\sum_{t=1}^{t-1} \%message}{t-1}$)		0.276*** (0.035)				
Message * Message Sent (message % * message dummy)			10.205*** (0.928)	10.398*** (0.933)		
No Message (no message dummy)			0.803 (0.602)	0.887 (0.605)		
Avg. Sent Pvs. ($\frac{\sum_{t=1}^{t-1} sent}{t-1}$)				0.355*** (0.058)		
Sent (amt. sent by S)					0.001 (0.002)	0.001 (0.002)
Avg. Returned - Message Pvs. ($\frac{\sum_{t=1}^{t-1} \%returned - \%message}{t-1}$)						0.546*** (0.045)
Constant	0.549*** (0.023)	0.405*** (0.026)	-0.732 (0.738)	-2.882*** (0.804)	-0.222*** (0.048)	-0.095*** (0.030)
Wald Test <i>p</i> -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

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); *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2: Mixed Effects Regressions Explaining Sender and Receiver Decisions

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 1 indicated 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 2).

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 2 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 2. 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 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, both in-

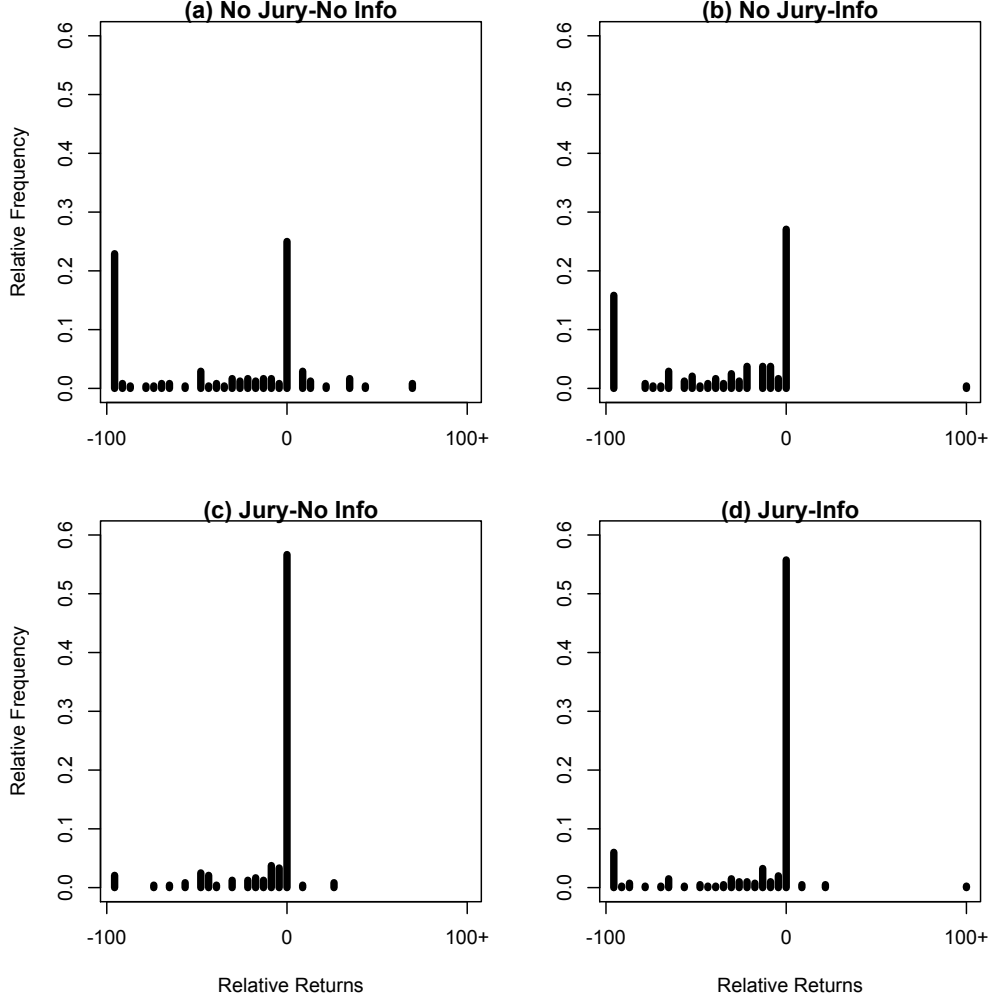


Figure 2: **Histograms of Relative Returns by Treatment.** Each panel displays a histogram of S's relative return on trust for one treatment. Relative returns are computed as $\frac{\text{AmountReturned}}{\text{AmountOffered}} - 1$ and reported in percentage terms, so that 0% implies that S got exactly what was offered in pre-play communication, and -100% indicates that R returned 0 ECU.

formation and the jury institution appear to encourage trust (measured in amount sent in columns). On the other hand, columns (5) and (6) indicate 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 informa-

tion treatments? To answer this question, we must dig deeper and identify the mechanisms through which information and juries work to facilitate trust. In particular, the regressions in Table 2 ignore the information available to players from previous periods of play. We should expect this information to play an important role in decision-making, and it may be the case that the role of past information varies in the different treatments. Senders that are “ripped off” in early periods may be reluctant to send anything to a receiver of that color in future periods. This possibility is magnified in the Information treatments - a sender may be reluctant to send anything in the future to a receiver of a color that ripped off *any* of the other three members of his group as well. Likewise, in the Jury treatments, a sender may avoid sending anything to receivers of a color that did not convict a fellow receiver for a previous transgression. In other words, the simple regressions presented in Table 2 do not account for the role that path dependence plays in the sending and returning decision. We address this concern in the next section.

3.2 Controlling for Path Dependence

In this section, we control for the effect of past actions on the send and return decisions. We start by analyzing how past actions affect the sender’s decision.

3.2.1 Amount Sent

To highlight the importance of path dependence, recall Figure 1, which showed that there is a substantially higher probability of sending the whole endowment in all three Jury and Info treatments relative to the *No Jury - No Info* baseline. Figure 3 displays time series of the probability of sending the entire endowment and of sending nothing, by treatment. In early periods, both treatments that provide group Info exhibit higher probabilities of sending the whole endowment than their No Info counterparts. However, in both Jury treatments, the probability of sending the whole endowment increases over time, while it is roughly flat or

declining in the No Jury treatments.⁹ Similarly in both Jury treatments, the probability of sending nothing remains flat, while in both No Jury treatments, it is increasing over time.

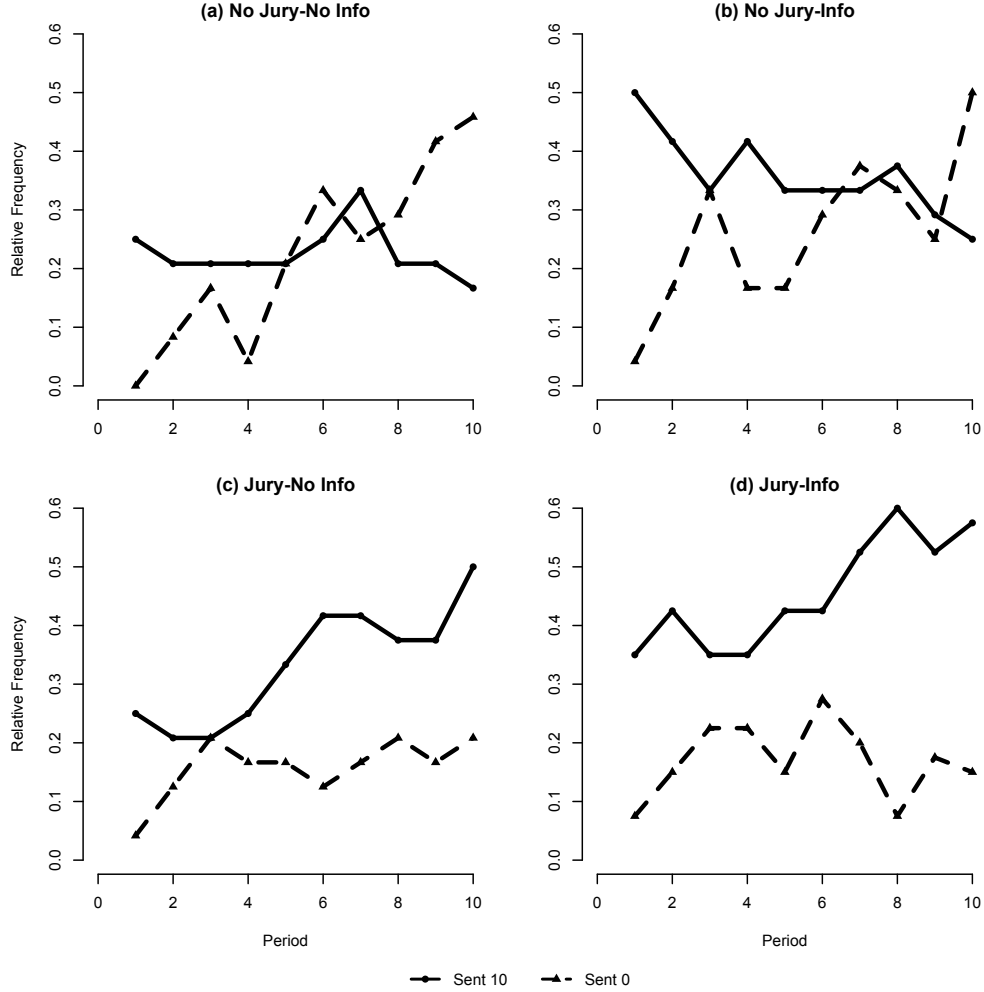


Figure 3: **Time Series of the Probability of Sending both 10 and 0 Units by Treatment.** Each panel displays time series of average observed probabilities for one treatment.

In Table 3, we analyze mixed-effects linear regressions where the dependent variable is the amount sent by S. As before we include nested random effects for each subject-in-session to control for repeated measures. These regressions are structured similarly to those in Column 4 of Table 2, but with the inclusion of variables encoding various past actions that may affect

⁹The evidence from the No Jury treatments is consistent with evidence that group reputation can encourage early-period cooperation in experimental labor markets (Healy, 2007).

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

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.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 3: Mixed Effects Regressions Explaining the Amount Sent by Treatment

the sender’s action in the present. In Column 1 of Table 3, we include two variables that help control for the sender’s history (as opposed to the history of those in his group): 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 plays a salient role 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 it is possible that being ripped off in the past by anyone, regardless of group, will make a sender wary of trusting a receiver in the future, regardless of group identity. But this is not what we find.¹⁰ Instead, we find that being ripped off by a receiver of the same color as the sender is currently matched with significantly decreases the amount sent (in both an economic and statistical sense), whereas being ripped off by a receiver in the other group has no statistically significant effect (we thus drop the latter variable in future regressions). The rest of the coefficients are similar to those found in Table 2, although the period trend is no longer statistically significant. Thus, without controlling for past actions (as in Table 2), it may appear that there is a trend over time that is unrelated to the history of interactions, but these results suggest that this trend is actually a result of path dependence inherent in a multi-period game.

After controlling for the sender’s previous interactions with receivers in column 1, it is still apparent that both the presence of a jury institution and group information encourage senders to be more trusting than in the *No Jury - No Info* treatment. Yet, this regression tells us little about the mechanisms that are specific to the inclusion of information or a jury.

¹⁰The 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 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 ($p = 0.05$) and is marginally greater than the *No Jury - Info* coefficient ($p = 0.15$). These results are available upon request.

After all, one has information on his own past interactions in all four treatments. To address the treatment-specific information, columns 2 and 3 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 past actions of the jury that are only relevant for the Jury treatments (and these variables are thus interacted with a dummy equaling one if the treatment contains a jury option). In theory, if group reputation is sustained by the jury, one would suspect that it matters how past juries actually voted. If in previous periods, the sender has lost trials against other members of the receiver’s group, then the sender may reasonably expect that he will lose a trial if he takes a transgressing receiver to court. The converse is true if the jury frequently convicted in the past. Yet, we find that the past voting decisions of the jury had no statistically significant effect on sending behavior. The mere presence of a jury is still statistically and economically significant (the coefficients on the two jury treatment dummies are highly significant), but the actual past actions of the jury appear to have no effect on behavior. In part, this is because ripoffs are relatively rare in the Jury treatments - occurring in roughly 30% of cases vs. nearly 50% in the other two treatments. As we will highlight in the next section, the *threat* of going to trial is sufficient to induce good behavior on the part of receivers, which spills over into the amount sent.

In column 3, we control for previous actions which may affect decisions in the information treatments. The first variable we control for is 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. Senders only have this information in the information treatments, and it is therefore interacted with an Info dummy. The other two variables are ones that are only relevant in the presence of both the jury institution and information (i.e., the *Jury - Info* treatment). These are, namely, the total 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 the previous ripoffs of others in the sender’s group as well as the previous trials lost by

others in the group both have a strong and statistically significant negative effect on the amount sent. Perhaps more importantly, after controlling for information-specific variables, it appears that there is a (marginally) statistically significant difference in the amount sent when both the jury and information are present (i.e., the *Jury - Info* treatment) and *every other treatment*. We can reject the null that the *Jury - Info* coefficient equals the *No Jury - Info* and the *Jury - No Info* at $p = 0.107$ and 0.063 . This means that the jury institution and information are *complements*; as long as receivers act to uphold group reputation by convicting their fellow transgressors, the presence of the jury and information encourages greater trade than either the jury or information do individually.

Result 1: After controlling for previous actions, the amount sent is lower in the *No Jury - No Info* treatment than in all other treatments.

Result 2: After controlling for previous actions, the amount sent is greater when both the jury and information are present than when one but not both are present.

These results shed significant light on the mechanisms generating welfare gains in the information and jury treatments. First, it suggests that past actions are extremely important in the information treatments. If members of one's group are always ripped off by the receiver's group (i.e., the variable *previous ripoff of group* equals 1), practically all of the increased welfare made available by the presence of information 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, $p\text{-value} = 0.63$).

This allows us to explain the treatment differences in time trends noted in Figure 3. Figure 4 displays time series of the probability that a receiver rips off a sender by treatment and reveals a clear difference between the Jury and No-Jury treatments. When the jury is available, individuals and groups are much less likely to have been ripped off in the past, and thus, the *Pvs Ripoff* variables take different values across treatments. This explains a large

part of the temporal decline in amount sent in the No Jury treatments, and it highlights the *complementarity* of the two mechanisms, for which we find further support in the interaction of the Jury-Info and Previous Convictions variables.

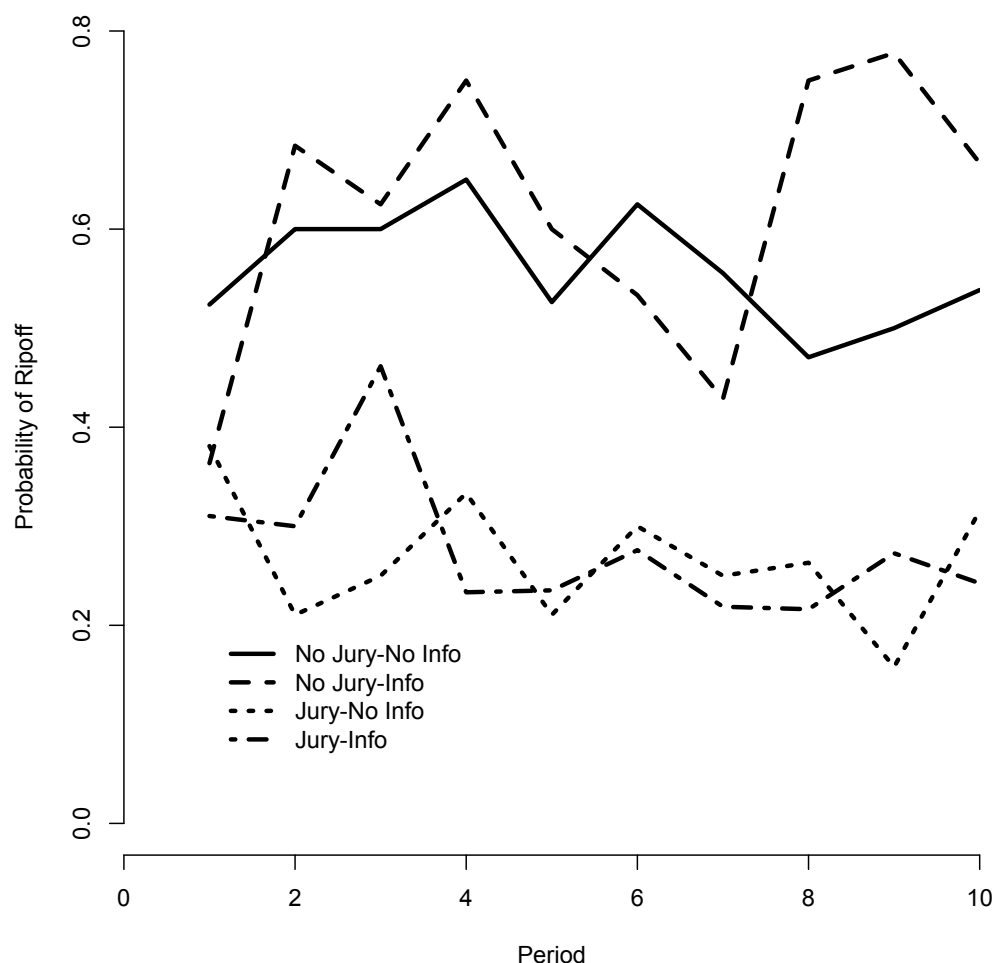


Figure 4: **Time Series of the Probability that a Receiver Returned Less than Promised by Treatment.** Each line displays a time series of average observed probability for one treatment.

Combined, these results help resolve the contradiction pointed out at the end of the last subsection. There, we noted that although senders sent more in all of the Jury and Info treatments, receivers only returned more in the presence of a jury. Given this, why would senders be willing to send *more* in the *No Jury - Info* treatment? The answer lies in large

part in the fact that the results in Table 2 did not account for past actions. In the information treatments, senders have four times as much information as in the *No Jury - No Info* treatment. When receivers have previously resisted the temptation to rip off senders in the group in question, senders are more trusting; the opposite is true when receivers have repeatedly ripped senders off. Hence, while receivers may not return more relative to the message *on average* in the information treatments, when they do fulfill their promises, it encourages senders to send more (relative to no-information treatments). In this sense, making group information available accentuates path dependence. Groups in the Info treatments with a “good” history maintain higher levels of trust, and groups with a “bad” history, exhibit sharper declines.

Result 3: There are greater welfare gains in the two information treatments 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.

Result 4: There are greater welfare gains in the jury treatments than the *No Jury - No Info* baseline because of the mere existence of the jury. Past decisions by juries do not significantly impact the amount sent in the absence of information-sharing, but senders do take into account how juries treated their group members when the jury is combined with information-sharing.

Once more, the history of actions taken in the jury treatments only impacts the amount sent when combined with information sharing (as evidenced by the negative and significant coefficient on our variable measuring previous trials lost by the group). This suggests that the jury institution encourages trust through a different mechanism than the one that works via information sharing. When the jury is available, the fact that a cheating receiver can be directly punished is sufficient to encourage greater trust. The most likely explanation for this result is that the presence of the jury reduces the incentive of the receiver to take

actions that could send them to trial. We test whether this is the case in the next section, which analyzes the actions taken by receivers.

3.2.2 Amount Returned

The results reported in Table 2 indicated that the addition of the jury but not of information-sharing positively affects the amount returned relative to the amount promised in the message. In this section, we break this result down further to control for the potential presence of path dependence.

Table 4 reports the results of mixed effects linear regressions explaining the difference between the percent returned and the percent offered in the pre-play message. These regressions are structured similarly to the one reported in Column 6 of Table 2, but with the addition of variables which control for past actions. Column 1 of Table 4 includes a variable that measures the the average difference in percent returned and percent offered pre-play in all previous periods. This variable is highly significant. The simple fraction of ripoffs is not statistically significant. The other coefficients are very similar to those found in Column 6 of Table 2.

In Column 2 of Table 4, we include two variables that control for personal experience in the jury treatments: the number of times the receiver has been convicted and acquitted in the past. Here, we find that the actions taken by previous juries have an important effect on the amount returned. If the receiver has been convicted in the past, she is likely to return more (relative to the amount promised) in the future. Although this is not surprising, we do note that this effect only accounts for a small fraction of the effect that the presence of a jury has on the receiver's 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. Receivers adjust their behavior in anticipation of punishment

for cheating.

Finally, in Column 3 of Table 4, we include three variables that may be affected by information: the fraction of previous periods that others in the receiver’s group ripped off a sender in the group with which the receiver is currently matched, and the number of previous trials in which other group members were either convicted or acquitted. If group reputation is a salient motivation in receiver’s actions, we would expect these coefficients to be statistically significant. However, this is not what we find. Not only are none of these coefficients statistically significant, but the coefficients of all other variables are nearly identical to those reported in Column 2.

The interpretation of these results is straight-forward. Information-sharing has no direct effect on receiver’s decisions, while the jury institution increases the amount sent relative to the message. It does so in two ways. First, receivers who are in groups that have convicted in the past return more in the future. Second, the mere presence of the jury institution increases the amount returned, regardless of past votes; the threat of punishment is sufficient to induce additional cooperation.

Result 5: Receivers return significantly more relative to the message when juries are present than when they are not, but the presence of information-sharing does not affect their behavior relative to the *No Jury - No Info* baseline.

Result 6: Receivers who were previously convicted return significantly more relative to the message in the jury treatments, but information about other receivers in their group does not affect their behavior relative to the *No Jury - No Info* baseline.

3.2.3 Discussion

Combining these results with those reported in the previous section, we see that juries and information work in different ways to facilitate welfare gains. The jury institution operates

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

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.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 4: Mixed Effects Regressions Explaining the Percent Returned - Percent Offered by Treatment

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 that they send. Information-sharing works through a

different channel, namely through its effect on group reputation. Senders who are in groups that have been cheated in the past send much less than those who are in groups who have not been cheated in the past, and this is reinforced with information-sharing since individuals can also condition behavior on the experiences of others in their group. In other words, information accentuates path dependence - 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, on the other hand, simply reduces the likelihood of cheating, and when it is combined with information-sharing, this creates positive feedback facilitating further gains from exchange.

3.3 When do subjects sue? When are cheaters convicted?

Since the Jury clearly has a positive welfare impact, increasing both the amount sent and the amount returned, it is important to understand how subjects choose to employ it. To this end, Table 5 reports mixed effects linear probability estimates of the decision by the sender to employ the Jury mechanism. In a variety of specifications, we analyze the effects of information-sharing on the probability of a sender filing a dispute, and we find no evidence of statistically significant effects. Our results suggest that the likelihood of suing varies significantly only with the magnitude of the ripoff: the larger the difference between the amount promised and the amount received, the greater the probability of filing a dispute.

The fact that senders do not always file disputes in case of ripoffs could imply that some of the effect is due to path dependence - failure to convict in the past might make future filings seem futile. However, insignificant estimates of the effect of previous actions do not favor this interpretation. Instead it is likely that some of this effect is driven by the fact that not all ripoffs leave a sender worse off than he would have been in the absence of trade - when we include a dummy variable that takes a value of 1 when the sender was ripped off but still received a payoff larger than his initial endowment, it is negative and significant. In

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

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

Table 5: Mixed Effects Linear Probability Estimates Explaining the Decision to Dispute, Jury Treatments

such cases the sender is 20 percentage points less likely to file a dispute, all else equal.

Result 7: The likelihood that a cheated sender will file a dispute is strongly increasing in the degree to which she was ripped off. The presence of information-sharing does not affect the likelihood of filing.

Given that a dispute is filed, it is also important to know what factors influence juries' decisions. Table 6 reports mixed effects linear probability estimates of the decision to convict

the receiver.¹¹ Here we report two specifications, one of which controls for treatment differences and one of which includes additional independent variables accounting for the direct effects of past actions that would be public knowledge only in the *Jury - Info* treatment. Again, the coefficient on variable measuring the magnitude of the violation is positive and significant. Here we cannot distinguish between motivations behind jury votes. It is possible that voters are motivated by the desire to protect their group’s reputation and to encourage future exchange, but purely altruistic motivations are equally plausible.

Action Taken By: Dependent Variable	Receiver’s Group	
	Sender Won	Dispute (0/1)
Jury-Info	-0.190 (0.187)	-0.285 (0.215)
Inverse Period ($\frac{1}{t}$)	-0.092 (0.205)	-0.025 (0.224)
Sent (amt. sent by S)	-0.032 (0.027)	-0.024 (0.027)
Ripoff Magnitude ((message % * amt. received from S) - amt. returned by R)	0.034*** (0.012)	0.030** (0.012)
Pvs. Ripoff of Group, Same * Info (fraction of pvs. times R’s group members ripped off S’s group * info)		0.372 (0.289)
Pvs. Conviction, Group * Jury * Info (# of trials members of R’s group convicted * jury * info)		-0.114 (0.098)
Previous Acquittal, Group * Jury * Info (# of trials members of R’s group acquitted * jury * info)		0.098 (0.078)
Constant	0.715*** (0.251)	0.663** (0.259)
Observations	86	86
Log Likelihood	-57.64	-56.22

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

Table 6: Mixed Effects Linear Probability Estimates Explaining Jury’s Decisions to Convict, Jury Treatments

Similarly, we report in the Appendix (Table A1) results of regressions exploring individuals’ voting patterns. Column (1) reports a regression for the *Jury - No Info* and *Jury - Info* treatments where the dependent variable takes a value of 1 if the voter chose to convict and

¹¹These regressions differ from the previous ones in that we include random effects only at the session level, since jury voting is a collective decision.

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 convictions become less likely the more times individuals are asked to vote. We find no evidence that information-sharing influences voting decisions.

Result 8: At both the group and individual level, the rate of conviction is increasing in the difference between the amount offered by the receiver and the amount actually returned. Information-sharing does not impact voting.

3.4 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 A2 and A3), we report the results of regressions similar to those in Tables 3 and 4, 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.

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

Table 7: Mixed Effects Linear Probability Estimates Explaining the Decision to Dispute, Jury and Pay Treatments

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.¹² To determine whether these differences are statistically significant and to understand the origins of any such differences, Table 7 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

¹²In one *Jury - Info - Pay* session, no receiver ever returned less than offered, so this analysis is based on additional data from two sessions.

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 ($p = 0.384$). 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.

Result 9: 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 8 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, 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.¹³

Result 10: There are no statistically significant differences in the conviction rate in the *Jury - Info* and *Jury - Info - Pay* treatments, all else equal.

¹³Column (2) of Table A1 in Appendix A 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.

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

Table 8: Mixed Effects Linear Probability Estimates Explaining Jury's Decisions to Convict, Jury and Pay Treatments

4 Conclusions

Our experiment highlights the crucial role of both information-sharing and in-group punishment in sustaining group reputation and facilitating exchange. While in-group punishment reduces the likelihood of cheating, information sharing accentuates path-dependence since individuals can condition their behavior not only on their own prior experiences but also on the experiences of others in their group. This generates strong complementarities between the two mechanisms, since 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 results 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 this case, by inducing group identity, 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

A Additional Regression Tables

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

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A1: Mixed Effects Regressions Explaining Individual Votes, Jury and Pay treatments

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

Table A2: Mixed Effects Regressions Explaining the Amount Sent, Jury and Pay Treatments

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

Table A3: 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 experiment, and next to the page number we indicate which instructions were shown in which treatments. Any words surrounded by “@” symbols indicate parameters that were automatically filled by the experiment software and may have been specific to the subject.

Page 1 - All treatments

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 - All treatments

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 - All treatments

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 - All treatments

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 - All treatments

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 ($0.6 * 8 \text{ ECUs} * 3 = 14.4$, rounded up to 15).

Page 6 - All treatments

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 - NoJury-Info, Jury-NoInfo, Jury-Info, and Pay treatments

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 - Jury-NoInfo and Jury-Info treatments

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 Alternate - Pay treatment

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 - Jury-NoInfo and Jury-Info treatments

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 Alternate - COSTLY treatment

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 - All treatments

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.

Time Remaining: 0 Seconds

Period 1

You are **Blue** Receiver 1.

You are matched with a **Blue** Sender.

Any money that the **Blue** Sender chooses to send to you will be multiplied by **3**, and then you will have the opportunity to return **NONE, ANY, or ALL** of that amount to the Sender.

Before the Sender makes a decision, you may pass a message to the Sender that says:

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

If you want to send a message, type any value of **X** between 0 and 100 into the box below and click the SEND button. If you do not want to send a message, then click the NO MESSAGE button.

Send

Return %

No Message

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

Time Remaining: 22 Seconds

Period 1

You are **Red** Receiver 1.

The **Red** Sender sent you **2 ECU**. The amount was multiplied by **3**, so you received **6 ECU**.

You offered to return **22%** of the amount you received, which in this case is **2 ECU**. However, you can return **NONE**, **ANY**, or **ALL** of the amount you received.

When you decide how much you want to return to the **Red** Sender, type the amount into the box below and click **Send**.

Send

Return

5

ECU

Figure B2: Screen Shot of Receiver's Return Decision.

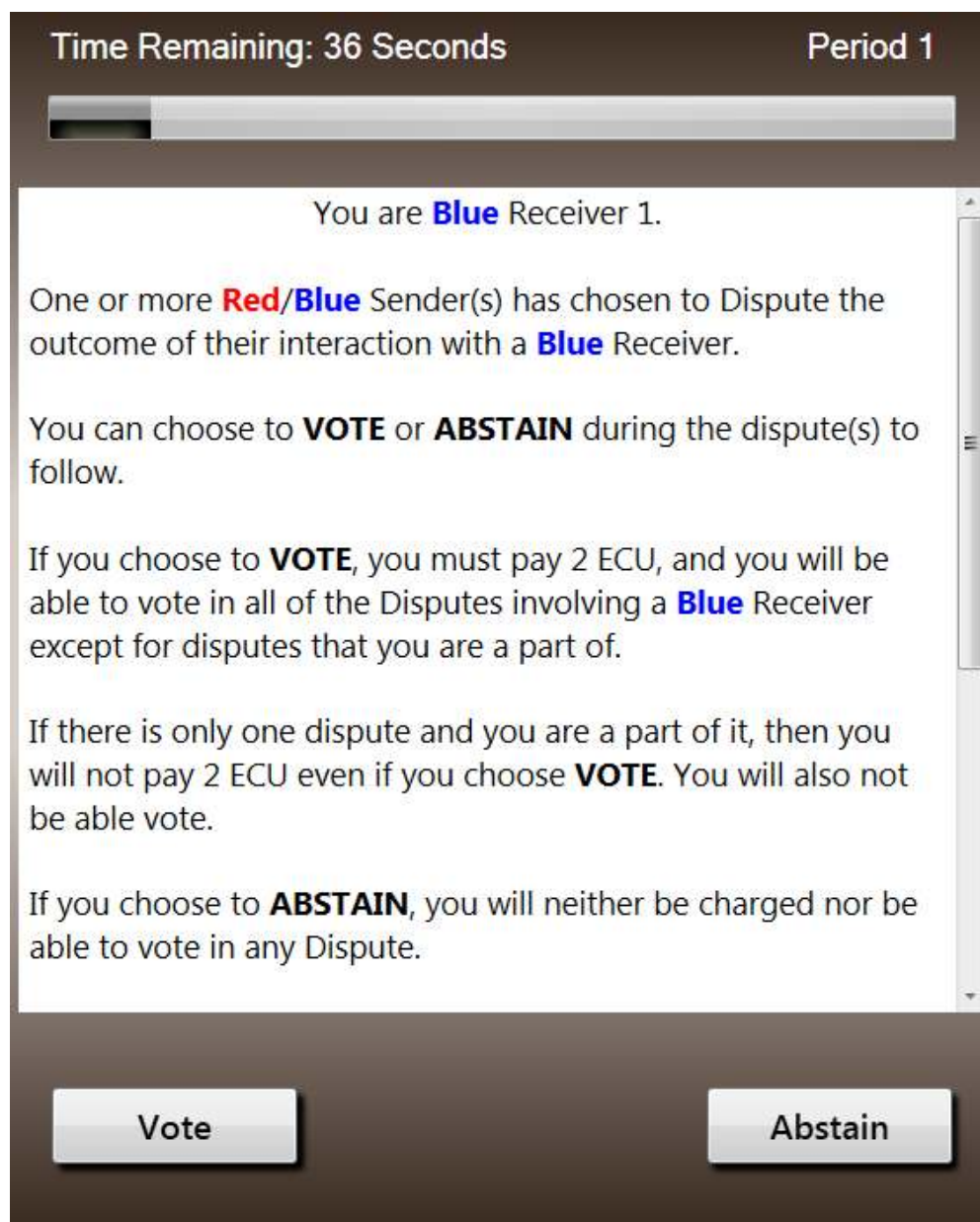


Figure B3: Screen Shot of Vote Decision.

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