Continuous Time and Communication in a Public-goods Experiment

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Abstract: We investigate the nature of continuous-time strategic interactions in public-goods games. In one set of treatments, four subjects make contribution decisions in continuous time while in another they make them only at discrete points of time. The effect of continuous time is muted in public-goods games compared to simpler social dilemmas; the data suggest that widespread coordination problems are to blame. With a rich communication protocol, these coordination problems disappear and the median subject contributes fully to the public good, with no time decay. At the median, the same communication protocol is less than half as effective in discrete time.

Keywords: public goods, voluntary contribution mechanism, continuous-time games

JEL codes: C72, C92, D70, H41

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1. Introduction

The provision of public goods is critical in every society, yet is always problematic. Since by definition nobody can be excluded from enjoying public goods once they have been provided, there is the incentive to free ride – to simply allow others to provide the good (whether it is a park, volunteerism, clean air, public health services, or national defense) and make use of it without contributing to it.

Many public-goods problems unfold in continuous time with flow payoffs accruing to the participants. Team sports are an excellent example: All team members benefit from a win, but individual players may expend differential costly effort over the course of the contest. Other examples of continuous-time team production – charity call centers manned by volunteers, construction projects with earnings bonuses for speedy completion – are quite common in economic settings. Yet the vast experimental and theoretical literatures on public goods, reviewed below, have, almost without exception, focused on the evolution of contributions in discrete time settings. This may miss important issues, as continuous time has the potential to alter the nature of strategic interaction in fundamental ways (Simon and Stinchombe, 1988).

Indeed, a recent experiment (Friedman and Oprea, 2012) shows that continuous time can generate extremely high rates of cooperation in very simple (2 action) and small (2 player) prisoner's dilemma games. The logic for the result is simple. In continuous time, attempts to initiate cooperation are virtually costless as unrequited attempts can be reversed nearly instantly. Likewise, once cooperation is achieved, the temptation to defect drops to nearly zero since the other player can match a defection almost instantly. Subjects thus establish and maintain cooperation quite consistently in continuous-time prisoner's dilemmas.

There is good reason to question whether such cooperative behavior will extend to more complex settings. The rapid-fire responses necessary to deter defection and establish cooperation in continuous time require no coordination in a two-player game. In a multi-player public-goods game, however, players face a difficult coordination problem. To initiate cooperation profitably in our game, a player must be confident that every other player will reciprocate fully and promptly; and to deter defection, the non-defectors must coordinate both the timing and severity of punishment. We conjectured that absent a coordination device, cooperative strategies would be difficult to implement in continuous-time public-goods games, and therefore continuous time alone would have less impact than in simpler settings.

A natural coordination device is to allow subjects to communicate. Non-binding free-form communication, after all, has a proven track record at encouraging Pareto-efficient outcomes in many games, as discussed in our literature review below. Of course communication may aid cooperation even in standard discrete-time public goods via moral suasion and promise-keeping. However, in continuous time communication has the added potential to coordinate the near-instant responses that support high rates of cooperation in simpler games. This led us to a second conjecture: in a tough public-goods game, communication may have a much larger impact in continuous time than in discrete time.

In this paper we report the results of an experiment designed to test these two conjectures. Our 2x2 design varies the timing protocol (discrete time vs. continuous time) and the communication protocol (no communication vs. rich unlimited communication). We find support for both of our motivating conjectures. Continuous time *per se* has little systematic effect on cooperation rates: we observe low initial contributions that decline over time in both discrete and continuous time. However, when we introduce a rich communication protocol, continuous time generates extremely high and sustained cooperation rates – the median subject quickly contributes 100% to the public good and this lasts to the end of the game.

The results also support our second conjecture: communication leads to less than half as much cooperation in discrete time. Moreover, communication works much more slowly and less reliably across groups than in the continuous treatment.

Several other points are worth mentioning. First, we use a very challenging set of parameters: our MPCR is only 0.3 with 4 players, so the payoff difference between zero contribution and full contributions is a mere 20% of earnings. This makes the high cooperation rates achieved in continuous time all the more striking. Second, we ran and report a robustness communication treatment in which subjects had access to a small set of pre-programmed messages rather than free form chat. We found that this had very little impact on cooperation (relative to no communication) in either continuous or discrete time. As in several previous experiments (discussed later), the richness of the message space seems to be an important consideration with respect to the effectiveness of cheap talk.

The remainder of the paper is structured as follows. We review related literature in Section 2, and describe our experimental procedures and implementation in Section 3. The results are presented in Section 4, and we offer a discussion in Section 5. Section 6 concludes. Appendices collect instructions to subjects and supplementary data analysis.

2. Related Literature

A well-known stylized fact is that there is an intermediate level of contributions in the beginning of standard linear public-goods games, but that this declines steadily to a very low contribution rate by the end of 10 periods. Many people are initially attracted to the efficiency of making public contributions, but this proves unsustainable. This is particularly true when the marginal per-capita return (MPCR) is low, as in our design. This pattern is often considered to result from the presence of conditional cooperators; these people make contributions until they see that others are not doing so, so the heterogeneity of the participants drives contribution rates down over time. See the surveys by Ledyard (1995) and Chaudhuri (2011) for considerably more background detail.[1]

Three decades of laboratory experiments have identified several different mechanisms for enhancing contribution rates in public-goods games. Each mechanism has some degree of effectiveness, yet none is completely satisfactory. One mechanism, first investigated by Yamagishi (1986, 1988) and Ostrom, Walker, and Gardner (1992), permits players to punish other members of the group at some personal cost. In an influential and frequently replicated study, Fehr and Gächter (2000) first match players for 10 rounds of a standard linear public goods game. Then, in a second set 10 rounds, the players see the contributions of others in their group and have the option of punishing each of them – by reducing own payoff by a point, the player can reduce another player's payoff by 10 percent, up to a maximum of 100 percent. The average contribution to the public good is 19% without punishment and 58% with punishment. The extent to which an individual contributes less than the group average is highly correlated with the amount of punishment received.

A second mechanism involves sorting players into groups of cooperators and conditional cooperators either endogenously or exogenously. The premise is that many people are conditional cooperators.[2] Excluding non-cooperators, then, can enable conditional cooperators

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[1] Chaudhuri (2011) provides an excellent survey on developments in laboratory public-goods experiments since the early Ledyard (1995) survey on this topic. The survey covers a number of articles relevant to our own study, including (but are not limited to) Gächter, Renner, and Sefton (2008), Walker and Halloran (2004), Sefton, Shupp, and Walker (2007), Egas and Riedl (2008), and Carpenter (2007).

[2] For a review and a discussion of policy implications regarding condition cooperation, see Gächter (2007).

to sustain a high contribution rate; see Sonnemans, Schram, and Offerman (1999), Keser and van Winden (2000), Fischbacher, Gächter, and Fehr (2001), Brandts and Schram (2001), and Fischbacher and Gächter (2009, 2010). In other studies the experimenter forms non-random subgroups from the population. For instance, Gunnthorsdöttir, Houser, McCabe, (2007) sort people into high, medium and low contributing groups of fixed-size of four, while Croson, Fatás, and Neugebauer (2006) exclude the lowest contributor from receiving the group's payoffs; both treatments considerably enhance contribution rates in the public-goods game. Ehrhart and Keser (1999) and especially Cinyabuguma, Page, and Putterman (2005) find that groups that can expel individuals achieve higher contribution levels. See also Charness and Yang (2008), Page, Putterman, and Unel (2005), and Ahn, Isaac, and Salmon (2009).[3]

We employ a third mechanism, communication. The impact of communication on behavior has been studied in economics for over 20 years (and over a longer period in psychology). Previous experimental work suggests that the impact varies with the game type and the message technology, but the specifics are far from settled. In many situations involving equilibrium selection, such as coordination games, simple forms of anonymous communication are highly effective. For example, Cooper, DeJong, Forsythe, and Ross (1992) shows that access to even simple and pre-fabricated pre-play messages (cheap talk) suffices to implement the payoff-dominant (and therefore the efficient) equilibrium. Charness (2000) finds that communication induces people to play the strategy consistent with payoff dominance 89 percent of the time, compared to 35 percent of the time without communication. In a minimum-effort

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[3] In terms of why a sense of group membership might affect behavior in our experimental environment (as in Charness, Rigotti, and Rustichini, 2007) it could be argued that this makes it more costly to violate promises or agreements. One may well feel worse when one has acted selfishly towards group members or close friends or kin than when the victim is a stranger (or even an adversary). For example, there may be some sense of guilt or shame. If one expects one's fellow group members to behave in a more pro-social (or more pro-group) manner, then there would be a correspondingly higher sense of guilt from disappointing these expectations. There may well be other emotions that come into play and the level of tolerance may vary across continuous and discrete time.

game, Brandts and Cooper (2007) show that a simple communication strategy of specifically requesting high effort and pointing out the mutual benefits of high effort is very effective, even more than increasing the marginal incentive for providing higher effort.

Matters are somewhat different when there is a unique (and inefficient) equilibrium. Some of the earlier studies featured face-to-face communication. Dawes, McTavish, and Shaklee (1977) find that such communication produces significantly more public-good provision in a dilemma situation, but only when this communication pertains to the task at hand. Isaac and Walker (1988) find considerable success with face-to-face communication in a public-goods game with the same MPCR and group size as in our experiment. Without communication, the rate of contribution starts at around 50% and dwindles to less than 10% by period 10; with face-to-face communication, the contribution rate is 90-100% throughout these 10 periods (see their Figure 1). Yet face-to-face communication is really an uncontrolled case (see the discussion in Roth, 1995 for example), given the lack of anonymity and potential consequences for selfish people after the termination of the experimental session.

Whether anonymous communication can allow players to escape a unique and inefficient equilibrium may well depend on the message technology. Minimalist (check-a-box) communication appears to be quite ineffective in such cases. For example, Charness (2000) finds that minimalist communication is completely ineffective in escaping a Prisoner's Dilemma, in great contrast to the results in a coordination game. Andreoni (2005) has a "non-binding" condition in which the trustee can elect to give the first-mover the option of restoring payoffs to the original endowment, but this implicit promise is only partially effective. While responders indeed make more favorable choices when they have made promises, first-movers don't trust the claim. Charness and Dufwenberg (2010) find that bare promises have only a slight effect in a trust game. In Ben-Ner, Putterman, and Ren (forthcoming), participants interact in trust games with two different forms of pre-play communication: numerical (tabular) only, or both verbal (in a chat box) and numerical. Numerical communication increases trusting and/or trustworthiness to only a modest extent.

On the other hand, free-form anonymous communication has been found to lead to a substantial and significant increase in Pareto-improving outcomes even when there is a unique and inefficient equilibrium. Charness and Dufwenberg (2006) show that free-form communication is quite effective in a hidden-action environment identical to that in Charness and Dufwenberg (2010), while Charness and Dufwenberg (2011) find similar effectiveness in a hidden-information environment. Verbal (chat) communication substantially enhances trusting and trustworthy behavior in Ben-Ner, Putterman, and Ren (forthcoming). Finally, Brandts, Charness, and Ellman (2011) find that free-form communication increases both price and quality in a sequential game, leading to higher earnings for both parties.

To the best of our knowledge, Dorsey (1992) is the first paper to employ continuous time with public goods: throughout the period subjects can make "cheap talk" decisions seen by the other players, but only final decisions count for payment. (As explained below, our continuous flow-payoff setting is strategically very different.) The results are mixed – while Dorsey's continuous time treatment increases contributions in provision point (i.e., threshold public goods) games, it is largely ineffective in the standard linear set-up. More recently, as noted earlier, Friedman and Oprea (2012) explore a two-person social dilemma in continuous time. They find remarkably high rates of mutual cooperation, ranging from 81 percent to 93 percent depending on the parameters. Control sessions with repeated matching over 8 discrete time sub-periods achieve less than half as much cooperation, and cooperation rates approach zero in one-shot control sessions. On the other hand, Oprea, Henwood and Friedman (2011) find no tendency for continuous time to encourage cooperation in 12-member groups playing a multilateral Hawk-

Dove game.

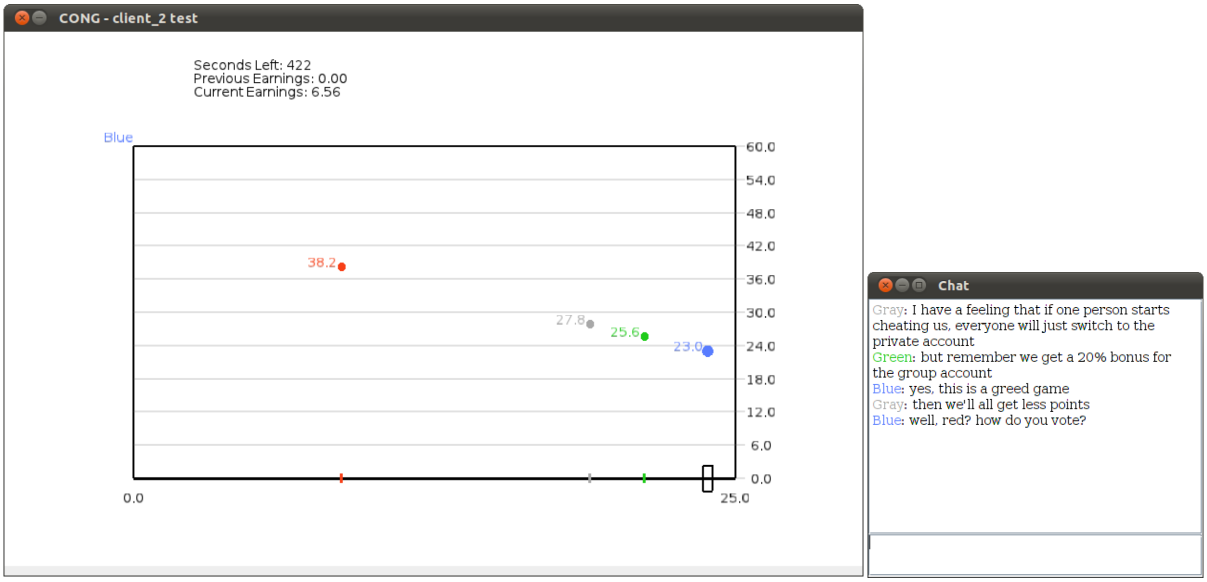
Evidently continuous time by itself does not automatically boost cooperation. It may well be that communication is also required for effective enforcement mechanisms in multi-player settings.

3. Experimental Design and Implementation

We conducted the experiment at the University of California, Santa Cruz. Participants in all sessions were randomly selected (using online recruiting software) from our pool of volunteers, which included undergraduates from all major disciplines. None of them had previously participated in a public-goods game experiment. On arrival, subjects received written instructions (Appendix I) that also were read aloud before beginning the experiment.

In all treatments, participants played the same public-goods game. Each person received an endowment of 25 tokens and could allocate these between their private account and the group account. Every token retained in the private account was worth one point to that player. Each token put into the group account became worth 1.2 points shared equally across all four people in the group, so the marginal per capita return (MPCR) is 0.3. Relative to no contribution, the societal gain from full contribution is only 20%, while the private risk is substantial – absent reciprocation, one loses 70% of any contribution. These parameters are the least conducive to cooperation of any we saw in the published experimental literature and were selected to create a challenging environment.

The experiment used a software package called ConG, for Continuous Games. Figure 1 shows the user interface. Each participant can adjust her strategy using the slider (seen as a small open rectangle) at the bottom. A position all the way to the left indicates zero contribution; a position all the way to the right indicates full (25) contribution. Contributions of other



Seconds Left: 422
Previous Earnings: 0.00
Current Earnings: 6.56

Gray: I have a feeling that if one person starts cheating us, everyone will just switch to the private account
Green: but remember we get a 20% bonus for the group account
Blue: yes, this is a greed game
Gray: then we'll all get less points
Blue: well, red? how do you vote?

Figure 1: Player screen and chat box. The player screen approximates that of player Blue midway through session CC4, and the chat box contents are excerpted from that session.

participants are shown according to an assigned color, and colored bubbles float above strategies to show current payoff rates.

In our continuous-time treatments, strategies can be moved at any time and as frequently as desired – the computer response time is less than 100 milliseconds, and gives users the experience of continuous action. In the chat treatments a second window showing the history of conversation (with participants shown by color) floats next to the screen.

This screen layout was designed to make the game easy to understand and to enable quick absorption of feedback. Post-experiment questionnaires and discussions with subjects indicate that subjects found the interface intuitive, and fully understood the rules of the game after the first few seconds.

Table 1: Treatments

	No Communication	Limited Communication	Full Communication
Discrete	**DN**: 5 groups	DL: 6 groups	**DC**: 6 groups
Continuous	**CN**: 5 groups	CL: 5 groups	**CC**: 5 groups

We study two time treatments, Continuous and Discrete. In each case, the total decision-making time (including communication where applicable) was 10 minutes. In Continuous time subjects could freely change allocations at any time and continuously earned flow payoffs according to the parameterized public-goods function described earlier. Allocations of the other three group members were color-coded, and could be seen with an imperceptible lag of less than 100 milliseconds. In Discrete time subjects made their allocation choices during 10 one-minute sub-periods during which they could not see others' choices. At the end of each minute, the computer took a snapshot of choices and these applied to the entire sub-period. (People were made aware that decisions at the end of the sub-period were the only ones with payoff relevance.) Participants were then shown the sub-period allocations and received 60 seconds of the corresponding flow payouts (i.e. 1/10 of the nominal amount).

Our other treatment variable was the communication protocol. The baseline was No Communication. In Full Communication treatments, the four group members shared a chat room in a separate window. Entries were color-coded (to allow subjects to correlate messages to actions) and unrestricted, although participants were asked to avoid using inappropriate language. We also ran a diagnostic treatment with Limited Communication in which group members again shared a chat window, but could only click buttons, not type out messages. The button menu included only the colors of all other players, "go left", "go right", "stay still" and "ok". There were no limits on the frequency of communication.

The treatments are summarized in Table 1. We had 20 participants in four of the treatments and 24 in the other two, for a total of 128 people. Average earnings for a 30–minute session were $14.00, including a $5 show-up fee.

4. Experimental results

The analysis focuses on our main four treatments, listed in bold in Table 1. Section 4.1 evaluates the two motivating conjectures; the evidence is largely favorable. Section 4.2 examines the underlying dynamics. To better understand the behavioral foundations, section 4.3 examines the variability of strategies within and across groups. Finally, section 4.4 examines the Limited Chat treatment to better understand the robustness of the results.

4.1 Main Results

Figure 2 plots empirical cumulative distribution functions (CDFs) of contribution rates for the four main treatments, presenting all data points in one glance. Table 2 summarizes the same data in numerical form. Several patterns are worth pointing out.

First, our replication of the standard protocol – the DN treatment – generates a familiar pattern seen repeatedly in previous work: contribution rates on average are quite small by any metric.

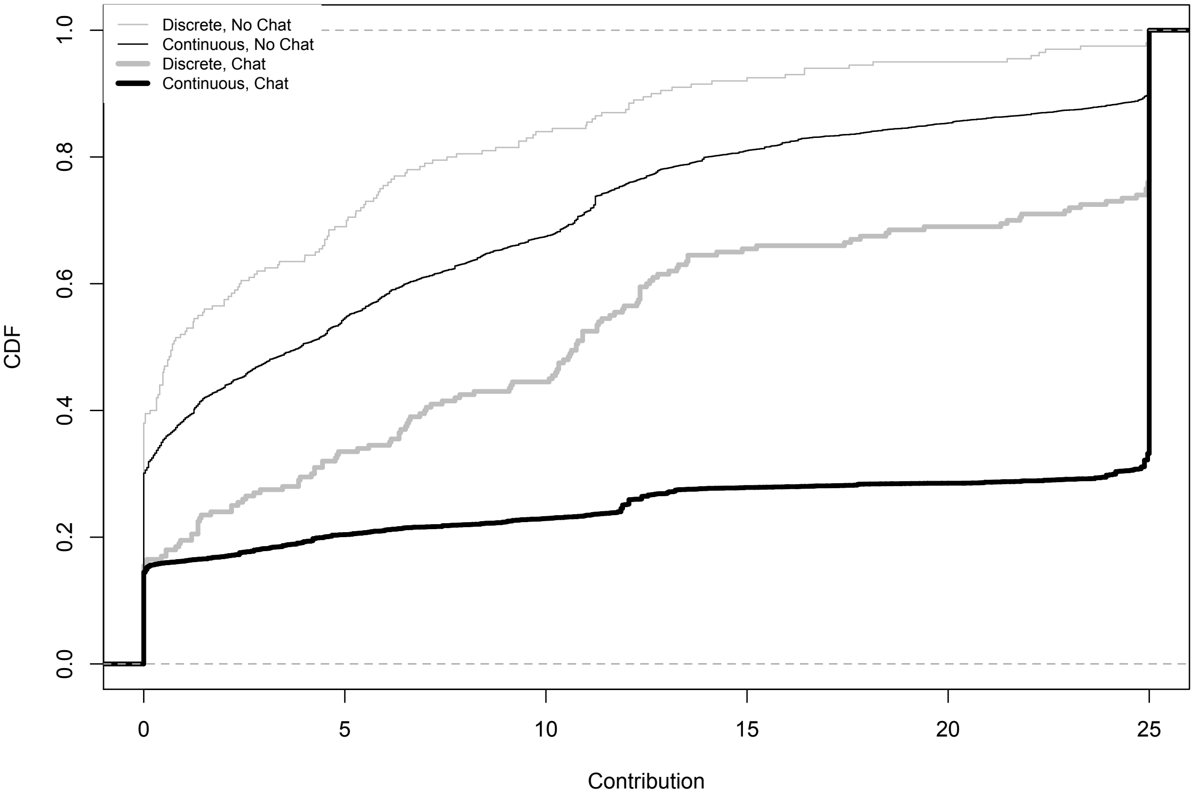


Figure 2: CDFs of contributions by treatment.

Second, while continuous time has a positive impact on contribution rates with or without communication, the size of the effect is quite modest in the latter case. Subjects only contribute fully about 1/4 of the time in the CN treatment and average contributions are less than 1/3 of socially-optimal levels. Thus the data seem to support our first conjecture: continuous time *per se* indeed has a much smaller impact on cooperation than in the simpler environment of Friedman and Oprea (2012).

Table 2: Summary statistics on contributions

Treatment	DN	DC	CN	CC
Median Contribution	0.71	10.75	3.89	25.00
Mean Contribution	4.21	11.94	7.35	18.87
Standard Deviation	6.28	9.70	8.61	10.01
Rate of Maximum Contribution	0.02	0.24	0.10	0.67

Third, when communication is added to continuous time, cooperation rates rise to impressive levels. A full 2/3 of the time, subjects contribute everything to the public good and the median rate across subjects is 100%. The DC treatment helps to put data from this treatment in perspective. Communication generates a median contribution rate half of that observed in continuous time and the rate of full contribution is less than half of the continuous rate. The data thus qualitatively supports our second conjecture: communication is far more effective in continuous than in discrete time.

The CDFs finally tell us that the data is quite bi-modal with modes concentrated at the boundaries, especially in the CC treatment. To accommodate serious boundary problems and failures of normality, we will focus the remainder of our analysis on behavior at the median and supplement it with non-parametric analysis. Thus we formally test our conjecture using a quantile regression that decomposes the data into treatment effects and evaluates their statistical significance at the median. This model uses individual subjects' median overall contributions as its independent variable and dummy variables for Continuous (Cont_i) time and Communication (Comm_i) and the interaction between the two as dependent variables. Results are as follows (with *p*-values listed in parentheses below):

$$\text{Contributions}_{\text{it}} = \underset{(0.250)}{0.615} + \underset{(0.009)}{5.30}*\text{Cont}_i + \underset{(0.000)}{9.88}*\text{Comm}_i + \underset{(0.000)}{9.21}*\text{Cont}_i*\text{Comm}_\text{i} \tag{1}$$

Notice first that the Continuous dummy in (1) is significantly greater than zero but is relatively small, implying a rise of only 5.3/25, or about 20 percentage points, in total contributions due to continuous time alone. A conservative Mann-Whitney test on group medians cannot distinguish contributions across the two treatments (p=0.11, one-tail test). The economically small effect of continuous time and the statistically mixed comparison with discrete time support our first conjecture and give us a first result:

Result 1: *Continuous time alone leads to only modest improvements in cooperation relative to discrete time.*

Model (1) also sheds light on our second conjecture. The Communication term in the quantile regression is highly significant and suggests a 9.88 point increase in median contributions in discrete time. However, the interaction term is equally significant and suggests an additional 9.21 point increase in continuous time. Thus the quantile regression suggests that communication is twice as effective in continuous as in discrete time at the median. A conservative Mann Whitney on group medians test further supports this conclusion: contributions are significantly higher in the CC than in the DC treatment at the five percent level (p=0.048, one-tailed test).[4] The data therefore supports our second conjecture and gives us a second result:

Result 2: *The median impact of communication is twice as large in continuous as in discrete time. Communication and continuous time together improve cooperation more than does communication alone.*

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[4] Arguably, it is more appropriate to omit very early data when comparing the effectiveness of communication: communication unfolds over time and is likely therefore to be ineffective in the early moments of the experiment. If we omit the first 60 seconds before calculating group-wise medians, the distinction between CC and DC becomes stronger -- the p-value drops to 0.032. If we omit the first 120 seconds the p-value drops to 0.021.

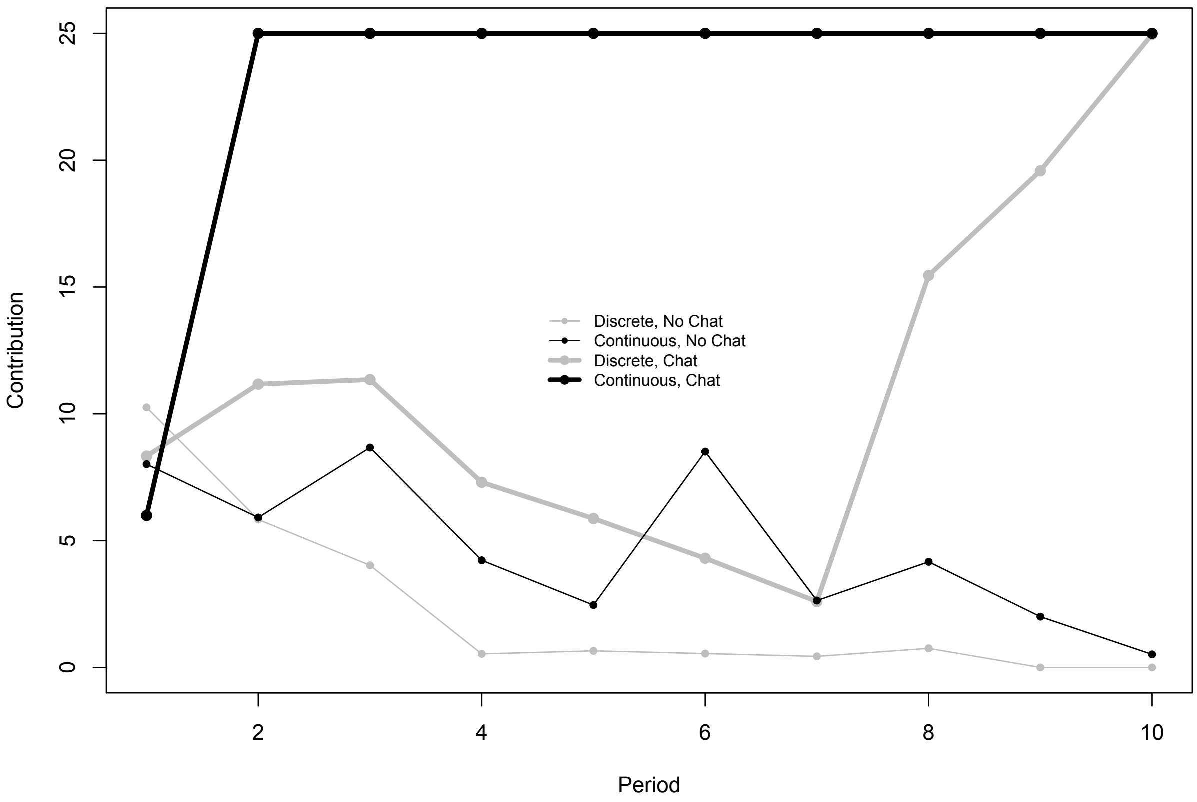

Figure 3: Median contributions by period and treatment.

4.2 Dynamics

We have reported aggregate results so far. Figure 3 disaggregates the data over time, plotting the evolution of median contributions over the course of the session.[5] In the DN treatment we replicate the standard finding in the literature: Cooperation begins at moderate levels and drops to nearly zero over time. Continuous time itself does nothing to break this pattern: in the CN treatment there is again clear evidence of cooperative decay.

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[5] To maintain comparability with the Discrete treatments, the Continuous data are aggregated over 60-second intervals in this Figure. The full detail for the Continuous treatments can be seen in Appendix II.

More interesting patterns emerge with communication. Median rates of cooperation in the CC treatment rise almost immediately to 100% and stay there for the remainder of the session. In the CD treatment, cooperation follows the classical pattern over the first 70 percent of the session, dropping towards nearly zero. However, in the final three periods median contributions rates reverse course, reaching 100% by the end in an interesting inversion of the end-game effects that are often observed in experiments.[6] [7]

Result 3: *Cooperative decay occurs in all treatments except CC.*

4.3 Variability and Coordination

Our working hypothesis in designing this experiment was that continuous-time strategies would be difficult to coordinate in settings as rich as public goods games and that communication might mitigate coordination failures. The analysis so far shows that communication indeed unleashes the cooperative potential of continuous time.

But do the data suggest that communication accomplishes this by easing coordination problems? A key indicator is the stability of contribution rates over time. Excessive variability from second to second (or from one discrete period to the next) suggests that players are failing to coordinate their behavior.

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[6] It is not clear whether the late movement to full contribution is a long-run trajectory for some groups or whether this is an entirely artifactual end-game effect. One group with this pattern mentioned the notion of "let's get there at least at the end" before achieving full contribution, while the other group with this pattern did not. Our suspicion is that some groups will indeed converge to full contribution in DC, while others will converge to little or no contribution. But of course this is an empirical question requiring more evidence.

[7] As Appendix II shows, these aggregates mask considerable heterogeneity in the communication treatments. In the CC treatment, five of the six groups have converged on full contribution while one other group seems to be drifting down to the zero-contribution level. The panel for the DC treatment indicates even more of a split, with three groups making full contributions at the end (with only two of these groups ramping up these contributions in the final three periods as in the aggregate plot) and two other groups having very low contributions at the end.

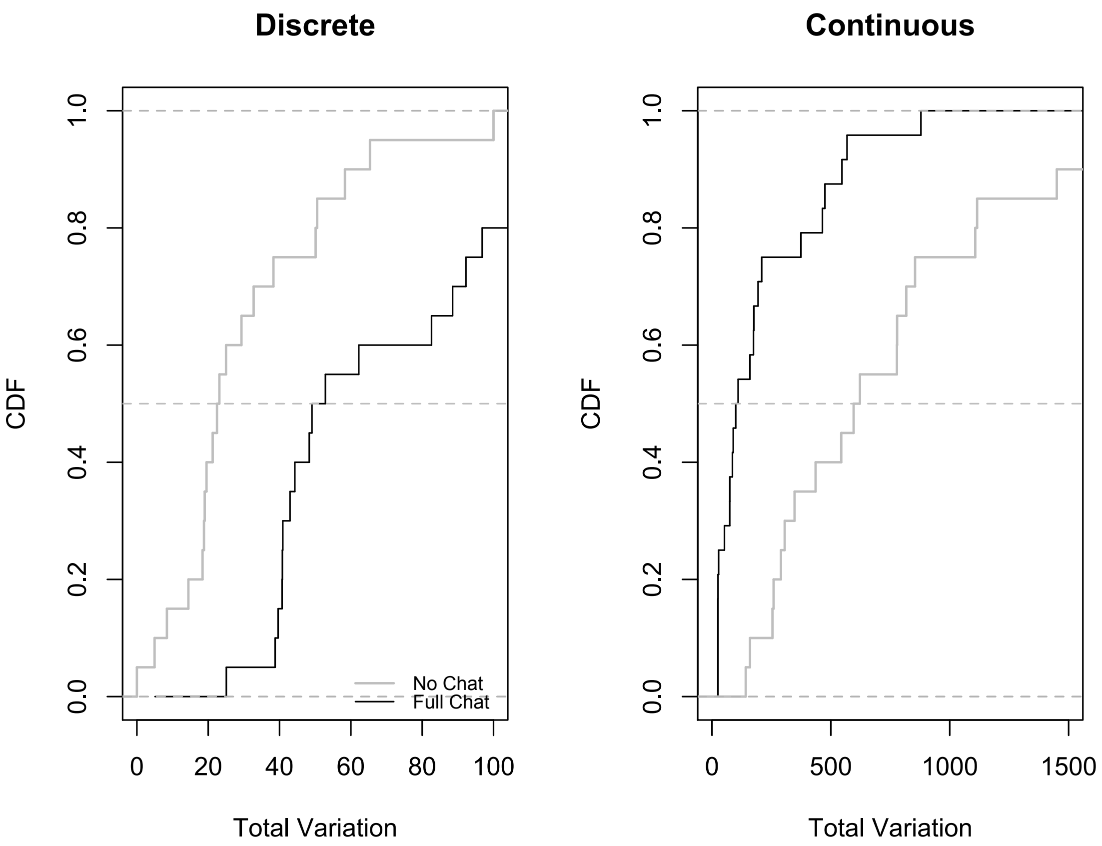

Figure 4: Total variation by treatment

In discrete time, an intuitive way to measure variability is the sum of absolute changes in a player's choices. That is, if player *i* chooses contribution level x_{it} in subperiod *t*, then the measure is

$$TV_i = \sum_{t=2}^{10} | x_{it} - x_{it-1} |,$$

which is known as Total Variation (TV). The first two columns of Table 3 show that communication more than doubles TV in our discrete-time treatments.

Table 3: Total Variation in Contribution Rates

	DN	DC	CN	CC
Mean	31.0	70.1	796	206.7
Median	22.8	51.0	609	105

Mathematicians extend the definition of TV to continuous time by taking the supremum (least upper bound) of expressions like the last one over all discrete time grids:

$$TV_i = \sup_G \sum_{t=2}^{n_G} | x_{it} - x_{it-1} |$$

where *G* is a finite grid of time points $0 < t_1 < t_2 < ... < t_{n_G} < 10$ of the 10-minute time interval. The last two columns of the Table 3 show that communication greatly *decreases* TV in continuous time, the opposite of its impact in discrete time.

Figure 4 confirms both effects by looking at the CDFs of individual players' total variation. Further confirmation comes from a conservative group-level Wilcoxon test, which rejects the null hypothesis of equal TV for the two communication treatments with two-tailed p-values of 0.018 in the Continuous data and 0.008 in the Discrete data.

Together, this analysis gives us a fourth result:

Result 4: *Communication tends to increase the stability of strategies in continuous time. By contrast, communication significantly decreases stability in discrete time.*

This result conforms with our intuition on the differential impact of communication in continuous and discrete time. While communication seems to enhance coordination and stabilize behavior in continuous time it has no such impact in discrete time. To the contrary, in discrete time, communication actually disrupts coordination on the inefficient Nash equilibrium! While communication is efficiency-enhancing in both cases, it appears to enhance efficiency by stabilizing "good" behavior in continuous time and by destabilizing "bad" behavior in discrete time (but without successfully achieving a higher contribution rate).

It is interesting to note, finally, that these measures likely understate the degree of coordination achieved in the CC treatment. As analysis of the chat data in Appendix III shows, even CC groups that fail to achieve full contribution and that exhibit considerable variability often do so because they (for some unknown reason) try to institute a complicated rotation scheme in which one member and then another reaps the maximum individual payoff while the others contribute.

4.4 Robustness: The LC Treatments

How important is the message space for coordinating strategies? Recall that several previous studies suggested that a richer message space (or the possibility of endogenous messages, which may include promises and bring guilt into the picture) is needed to effectively change behavior when there is a unique (but socially-inefficient) equilibrium. Our LC treatments allow us to examine whether this holds true for our data.

In fact, we do find support for this notion. Table 4 shows the median rate, the mean rate, and the rate of maximum contribution with limited communication.

Table 4: Summary statistics on contributions with limited communication

Treatment	CL	DL
Median Contribution	7.26	5.77
Mean Contribution	10.13	8.35
(Standard Deviation)	9.73	8.27
Rate of Maximum Contribution	0.15	0.04

Comparing to the figures in Table 2, we see that these are all intermediate between the rates with no communication and with free-form communication, and always closer to the no-communication results. In the case of discrete time, the median total contribution rate for limited communication is not significantly different than that for free-form communication or no communication (Wilcoxon-Mann-Whitney rank sum tests give $p = 0.421$ and $p = 0.075$, respectively, two-tailed tests). In the case of continuous time, there is no difference in the median contribution rates with no communication and limited communication ($p = 0.792$, two-tailed test), while there is a marginally significant difference in the rates with free-form and limited communication ($p = 0.0637$, two-tailed test). The mean total variation is also intermediate, but again much closer to the level observed with no communication, in both continuous and discrete time. Overall, there is no instance in which limited communication leads to significantly different results than with no communication, and this result also holds for patterns over time.[8]

This gives us a final result:

Result 5: *Limited communication has no significant effect on contributions, relative to no communication.*

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[8] These results do not change if we omit the first 60 seconds (to allow time for communication to get under way) before calculating by-session medians.

5. Discussion

We find that contribution rates are higher with free-form communication. This effect is considerably larger with continuous time, as there is an impressive overall contribution rate of over 75 percent and a median rate of 100 percent. The point is that, despite the very tough parameters, we see remarkably high degrees of cooperation in CC, and no evidence whatsoever of unraveling at the end of the period. In contrast, there is not much difference in behavior without communication across discrete and continuous time. In our control treatment (DN), we see consistent convergence towards the inefficient Nash equilibrium of zero contribution, minimal variation, and an overall contribution rate of 17 percent. The mixed treatment DC produces more heterogeneous results, with some groups achieving high contribution levels by the end and other groups decaying almost to zero contribution. The remaining mixed and intermediate treatments likewise produce considerable variability and heterogeneity, but seldom converge to full contribution.

Given these striking patterns in the data, it is natural to explore further *why* they occur. In the rest of this section, we offer some possible reasons, focusing on when players persistently try for high contribution levels, and when they succeed.

A useful point of departure is the continuous time Prisoner's Dilemma (CPD) experiments mentioned earlier (Friedman and Oprea, 2012). Despite very tough parameters, player pairs managed to achieve very high levels of cooperation via "pulsing" behavior. This non-verbal form of communication was very effective in conveying threats and promises in continuous time, and the vast majority of the players soon adopted it. By contrast, players in the continuous-time Hawk-Dove (CHD) experiment mentioned earlier (Oprea et al. 2011) had no effective way to communicate – the actions of any one player were hardly visible to the 11 others in the group. Despite facing much easier parameters, the Hawk-Dove players failed to achieve any degree of cooperation.

Our continuous public-goods (CPG) game falls somewhere in between these CPD and CHD games. Each player's action can be seen clearly by the three other CPG players, but its meaning is not nearly as obvious as to the one other player in CPD. If a player in our CPG game pulses from a very low contribution rate to a high rate and returns after a few seconds, the other players might interpret that as a request to increase their own contributions. Or they might all think that it is someone else's turn, or just wait to see whether someone else responds. Another complication is that the action space is not binary but rather is the entire interval [0, 25].[9] Absent full communication, the intent of pulses (and other allocation adjustments) remains quite ambiguous.

Free-form chat evidently resolves the ambiguity. Players know that there will be nowhere to hide if they misbehave and, in continuous time, that retribution can come quickly. Being able to respond immediately and in an unambiguous manner seems sufficient to achieve a high level of cooperation. In discrete time, free-form chat still reduces ambiguity and does enhance cooperation rates to some degree, but it seems that the inability to respond immediately reduces the effectiveness of promises and threats.

To delve more deeply into how free-form communication affected behavior we need to look at the micro-details of the chat in each session in relation to the observed behavior. This means that we have a quite limited set of data points (five in each treatment, except for six in

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[9] Hoggatt, Friedman and Gill (1976) document pulsing behavior in an early near-continuous time oligopoly game with a similar action space. As in the DC treatment of our public goods game, they observed considerable heterogeneity and variability. One of the co-authors participated in these experiments as a student in the Berkeley MBA program.

CC), so it is difficult to draw strong conclusions. Nevertheless, we present detailed summaries of the chat logs of each of the 11 full-communication groups listed in Appendix III.

We can make several observations from these micro-results. Three groups in continuous time made four-person agreements that were kept for the duration. One other group made no full agreements, while another group made an initial agreement to rotate that worked, but a new partial agreement to contribute half appeared to de-stabilize matters. Finally, one group was able to cooperate without any explicit agreement, but with references to being a team and having team players. The latter would appear to be a direct influence of a sense of group membership and a reluctance to disappoint others by not being sufficiently pro-social with respect to the group.

It seems clear that agreements amongst all of the group members are the most effective, as three-person agreements usually collapse. It may also be that a sense of solidarity can substitute for explicit agreements, leading to "the dog that didn't bark" in the case of group 2 (which achieved nearly 100 percent cooperation). The groups with four-person agreements had high contribution rates: 79.85 percent, 92.24 percent, and 93.47 percent; the group that had a four-person agreement in force until people tried to change it had a contribution rate of 74.57 percent during the three periods in question. With the exception of the group 2 case of strong group identity, the lack of a four-person agreement leads to lower contribution rates (48.05 percent for group 5 and about 25.9 percent for group 1 during the time there was no four-person agreement). Four-person agreements were essentially never broken, with only brief deviations that were remedied by quick and successful peer pressure.

Matters are rather different with discrete time. There were only three four-person agreements before the late stages, and two of these were violated. Group 5 was the only one to sustain an agreement throughout the session, and had the highest mean contribution rate of any group. There were also two agreements made in late stages (in groups 2 and 4), and these were kept, leading to high contributions at the end. Also, in contrast to the results with continuous time, there was relatively little discussion on this point, with an average of less than two mentions per session. It seems that in discrete time it is both more difficult to reach four-person agreements and more difficult to sustain them. Perhaps there is less of a sense of camaraderie, as we never observe any discussion of team play; instead we see outbursts of emotion and harsh language.

To some extent, the higher emotional content with discrete time may stem from the fact that there is a build-up to a specific moment and all attention is focused on it; in comparison, with continuous time there is no deadline effect and people seem to be more relaxed.[10] In this respect, there is a sense of immediacy in continuous time, as one can make instant responses to deviations in continuous time and can quickly see the response to a response; in contrast, one must wait to take action in discrete time and must wait another minute or more to see whether the offender makes a suitable response. The possibility of peer pressure is therefore more salient in continuous time. Nevertheless, we do see some trend towards effective agreements at the end of more than one session; perhaps communication in discrete time might also be effective, but just requires more time.

Finally, why is the impact of limited communication so similar to that of no communication? One possibility is that the simple choices from the restricted menu were just not sufficient. However, our sense is that it is impossible with limited communication to actually have the clear agreements that seem so helpful in achieving the high contribution rates that we observe with full communication. In the absence of clear and convincing evidence that others will cooperate, it seems quite difficult to reach efficient non-equilibrium play. On the other

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[10] Another possible factor involves the frequency of mention of how to make the most money, and the relative benefits of the private and group accounts. On average, this was mentioned more than twice as often in the continuous-time sessions as in the discrete-time sessions.

hand, we suspect that such simple messages would be quite effective in a coordination game like the Battle of the Sexes, and perhaps lead to explicit alternation between the two pure-strategy equilibria.

6. Conclusion

Recent research shows that subjects can generate high levels of cooperation in continuous time settings (Friedman and Oprea, 2012) by rapidly responding to one another's actions. So far these findings have only been established in very simple two-player, two-action social dilemmas. We advance this research agenda by examining behavior in much more complex social dilemmas – public goods games – in continuous time laboratory settings. Our findings are instructive.

Continuous time alone has only a modest positive impact on cooperation in our four-player, public goods game relative to the discrete time treatment. By itself, the continuous-time treatment generates highly volatile contributions, indicating widespread failure of subjects to coordinate their strategies and expectations. We suspect that these two patterns are related: coordination difficulties make it difficult to implement the rapid-fire responses necessary for achieving and sustaining cooperation in simpler continuous time games. As a result contributions start low in continuous time and fall steadily over time, mirroring the typical pattern in discrete time public goods games.

Free form cheap-talk communication solves this continuous-time coordination problem. When we allow subjects to communicate using a chat interface in continuous time, median cooperation rates quickly rise to 100% and stay there without diminishing. This massive communication effect seems to be special to continuous time: in discrete time, where rapid-fire responses are not possible, the effect of communication on median contributions is less than half as large.. The data also suggests that this large effect relies on a rich message space: A more limited form of pre-programmed communication, studied in a robustness treatment, has a much smaller effect.

Many directions seem promising for future work – research on continuous-time strategic behavior is, after all, still in its infancy. One open question is whether the difference in the effectiveness of communication between continuous and discrete time will remain in the very long run. The data give conflicting but tantalizing clues. On the one hand several groups in our DC treatment reverse cooperative decay late in the game and establish full cooperation by the end. On the other hand, some chat content suggests that such late rallies may be an endgame effect, and more discrete time periods would simply delay their appearance. The answer to this and other open questions awaits new experiments that include dozens of additional periods in discrete time.

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Appendix I

A separate instruction set was distributed in each treatment but great pains were taken to maintain the same language across treatments wherever possible. Below we provide instructions sets from two of our treatments as examples. Section I.A. reproduces the Discrete, Full Communication (DC) instructions while section I.B. reproduces the Continuous, Full Communication (CC) instructions.

I.A Discrete, Full Communication (DC) Instructions

Instructions (DC-PG)

Welcome! This is an economics experiment. If you pay close attention to these instructions, you can earn a significant sum of money, which will be paid to you in cash at the end.
Please remain silent and do not look at other participants' screens. If you have any questions, or need assistance of any kind, please raise your hand and we will come to you. If you disrupt the experiment by talking, laughing, etc., you may be asked to leave and may not be paid. We expect and appreciate your cooperation today.

The basic idea

At the beginning of the experiment, you will be anonymously matched with three other participants. You will each choose an allocation, which can be adjusted during the experiment. Your earnings accumulate at a rate that depends on the allocation you choose and the allocations chosen by the other participants you are matched with. The details are explained below.
The earnings you accumulate will be added up at the end of the experiment, and converted to US dollars at a rate written on the white board. Before you leave the lab, you will sign a receipt and will be paid in cash.

How earnings are computed

You have 25 tokens, and you choose how many tokens to allocate to the group account and how many to allocate to your private account. Each token in your private account always pays a rate of 1.0 point. Each token allocated to the group account yields a rate of 1.2 points, divided equally among all members of your group.
You can choose any allocation of the 25 tokens; fractional tokens are OK. You will be able to adjust your allocation choice during the experiment.

For example, suppose that you always allocate 20 tokens to your private account and 5 to the group account, and that the combined allocation of the other 3 members of your group is always 20 tokens. Then the group account would total 25 tokens and it would yield 1.2* 25 = 30 points distributed equally to the 4 members. You then would receive 7.5 points from the group account and 20 from your private account, a total of 27.5 points.

On the other hand, suppose that you always contributed 15 tokens to the group account and kept 10 for your private account, and that the combined group allocation of the other 3 members was 40 tokens. Then the group account has 55 tokens and you would receive 10 + 1.2 * 55/4 = 10 + 16.5 = 26.5 points. If you spent half the session in a situation that paid 27.5 points and the other half in a situation that paid 26.5 points, then you would earn (27.5+ 26.5)/2 = 53/2 = 27 points.

To give a few more examples, if everyone places all of their tokens in their private account, each player will earn at a rate of 25 points. In this situation, were you to instead unilaterally put all of your tokens in the group account you would earn 7.5 points while the other people in the group would earn 32.5.

If everyone places all of their tokens into the group account, each player will earn at a rate of 30 points. In this situation, were you to instead unilaterally put all of your tokens in the private account, you would earn 47.5 points while the other people in the group would earn 22.5.

Screen display

It's hard to keep track of your earnings when you and the other members of your group are adjusting allocations during the experiment. The computer does the calculations for you, as in Figure 1. You drag the rectangular slider at the bottom of the screen to adjust your allocation to the group account between 0 and 25; the rest stays in your private account.

The experiment will be divided into ten consecutive one-minute sub-periods. The rectangular bar at the top of the screen will show how much time has elapsed so far in the current sub-period; when the bar is completely filled, the sub-period is over. During each sub-period you will secretly choose an allocation and other participants will do the same. The computer will use only the allocations selected at the moment the sub-period ends and will pay no attention to slider positions earlier in the sub-period.

At the beginning of the experiment, a message on the screen will tell you the color assigned to you. Colored dots will appear each sub-period to show the allocation choices and earnings for you and the other members of your group from the previous sub-period. Dots further to the right indicate larger allocations to the group account, and higher dots indicate larger earnings that sub-period; exact earnings in the previous sub-period are shown in the numbers floating next to each dot.

Above the box you will see how many sub-periods are left in the experiment, and your "Current Earnings" shows your points accumulated so far.

Chat window

During the experiment you will have access to a Chat window, as in Figure 2. You simply type in any permissible message, hit Return, and your message will be shown to all of the people in your group. You can adjust your allocation choice and chat at the same time, if you wish. Messages will be color-coded using the same color assignments used for dots.

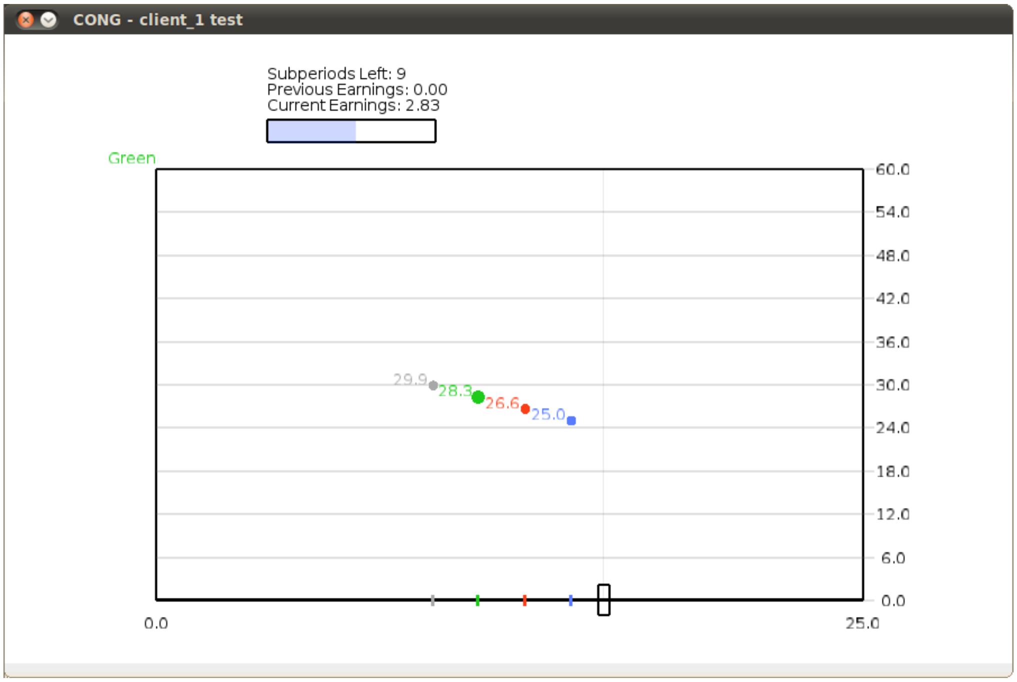
In chat, you are permitted to discuss anything related to today's experiment, but not to reveal your true identity or to discuss what might happen outside the lab. All chat messages will be recorded permanently, so please avoid trash talk.

Frequently asked questions

Q1. Is this some kind of psychological experiment with an agenda you haven't told us?
Answer. No. It is an economics experiment. If we do anything deceptive or don't pay you cash as described then you can complain to the campus Human Subjects Committee and we will be in serious trouble. These instructions are meant to clarify the game and to explain you how you earn money; our interest is simply in seeing how people make decisions.

Q2. Will the formula for calculating earnings ever change? Is there any random element?
Answer. The earnings calculation never changes, and there is no random element. Your earnings depend entirely on your allocation decisions and those of the other members of your group.

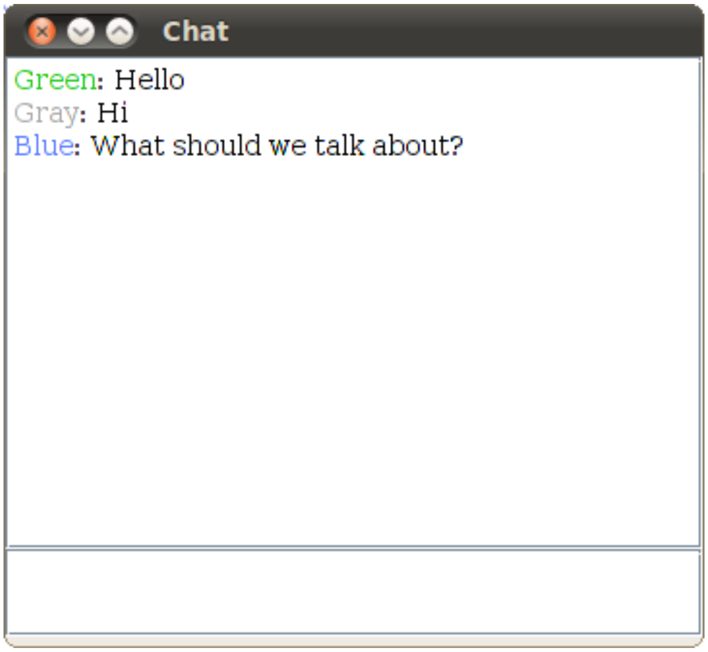
Figure 1



CONG - client_1 test

Subperiods Left: 9
Previous Earnings: 0.00
Current Earnings: 2.83

Figure 2



Green: Hello
Gray: Hi
Blue: What should we talk about?

I.B Continuous, Full Communication (CC) Instructions

Instructions (CC-PG)

Welcome! This is an economics experiment. If you pay close attention to these instructions, you can earn a significant sum of money, which will be paid to you in cash at the end.
Please remain silent and do not look at other participants' screens. If you have any questions, or need assistance of any kind, please raise your hand and we will come to you. If you disrupt the experiment by talking, laughing, etc., you may be asked to leave and may not be paid. We expect and appreciate your cooperation today.

The basic idea

At the beginning of the experiment, you will be anonymously matched with three other participants. You will each choose an allocation, which can be adjusted during the experiment. Your earnings accumulate at a rate that depends on the allocation you choose and the allocations chosen by the other participants you are matched with. The details are explained below.
The earnings you accumulate will be added up at the end of the experiment, and converted to US dollars at a rate written on the white board. Before you leave the lab, you will sign a receipt and will be paid in cash.

How earnings are computed

You have 25 tokens, and you choose how many tokens to allocate to the group account and how many to allocate to your private account. Each token in your private account always pays a rate of 1.0 point. Each token allocated to the group account yields a rate of 1.2 points, divided equally among all members of your group.

You can choose any allocation of the 25 tokens; fractional tokens are OK. You will be able to adjust your allocation choice during the experiment.

For example, suppose that you always allocate 20 tokens to your private account and 5 to the group account, and that the combined allocation of the other 3 members of your group is always 20 tokens. Then the group account would total 25 tokens and it would yield $1.2 * 25 = 30$ points distributed equally to the 4 members. You then would receive 7.5 points from the group account and 20 from your private account, a total of 27.5 points.
On the other hand, suppose that you always contributed 15 tokens to the group account and kept 10 for your private account, and that the combined group allocation of the other 3 members was 40 tokens. Then the group account has 55 tokens and you would receive $10 + 1.2 * 55/4 = 10 + 16.5 = 26.5$ points. If you spent half the session in a situation that paid 27.5 points and the other half in a situation that paid 26.5 points, then you would earn $(27.5+ 26.5)/2 = 53/2 = 27$ points.

To give a few more examples, if everyone places all of their tokens in their private account, each player will earn at a rate of 25 points. In this situation, were you to instead unilaterally put all of your tokens in the group account you would earn 7.5 points while the other people in the group would earn 32.5.

If everyone places all of their tokens into the group account, each player will earn at a rate of 30 points. In this situation, were you to instead unilaterally put all of your tokens in the private account, you would earn 47.5 points while the other people in the group would earn 22.5.

Screen display

It's hard to keep track of your earnings when you and the other members of your group are adjusting allocations during the experiment. The computer does the calculations for you, as in Figure 1. You drag the rectangular slider at the bottom of the screen to adjust your allocation to the group account between 0 and 25; the rest stays in your private account.

The experiment will last for ten minutes. Each participant will be randomly assigned an initial allocation and will be allowed to freely adjust his or her own allocation choice throughout the experiment. At each moment, your earnings will accumulate at a rate determined by your current choice and the current choices of other members of your group.

At the beginning of the experiment, a message on the screen will tell you the color assigned to you. Colored dots will always show the current allocation choices and earnings rate for you and the other members of your group. Dots further to the right indicate larger allocations to the group account, and higher dots indicate larger earnings that sub-period; exact earnings rate are shown in the numbers floating next to each dot.

Above the box you will see how many seconds are left in the experiment, and your "Current Earnings" shows your points accumulated so far.

Chat window

During the experiment you will have access to a Chat window, as in Figure 2. You simply type in any permissible message, hit Return, and your message will be shown to all of the people in your group. You can adjust your allocation choice and chat at the same time, if you wish. Messages will be color-coded using the same color assignments used for dots.

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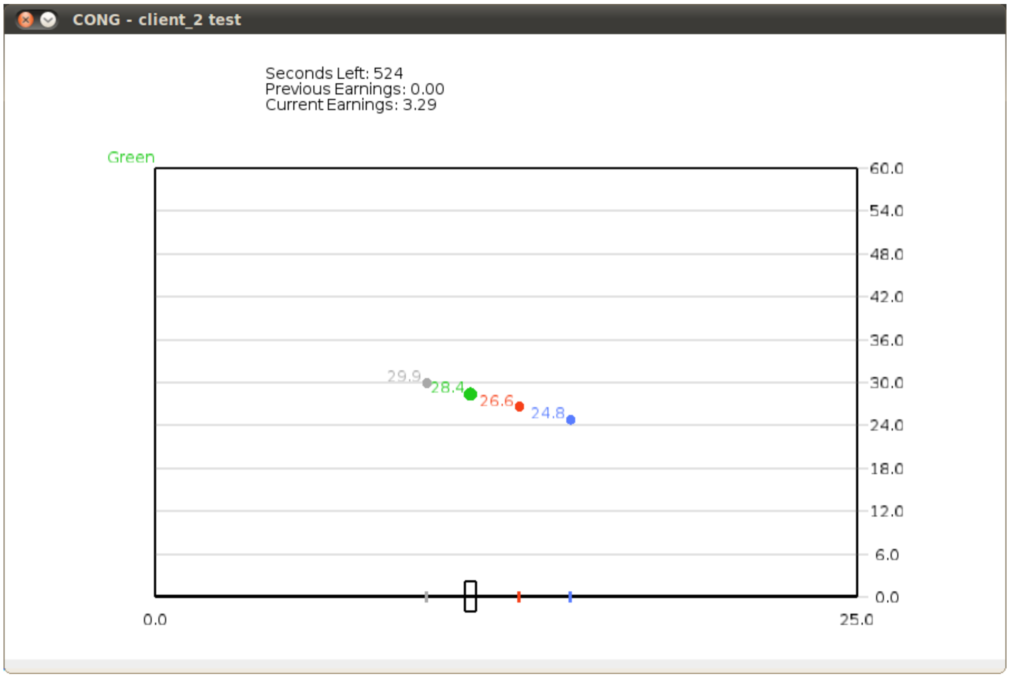
Q2. Will the formula for calculating earnings ever change? Is there any random element?

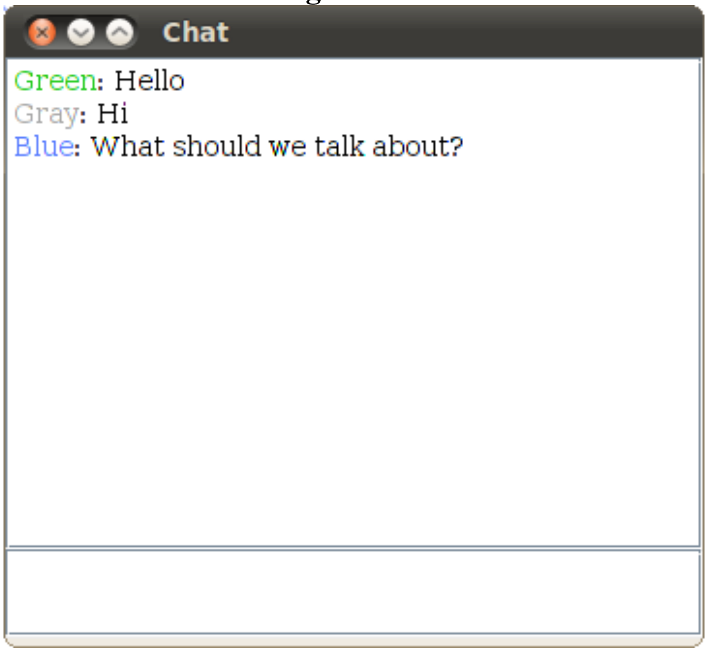
Answer. The earnings calculation never changes, and there is no random element. Your earnings depend entirely on your allocation decisions and those of the other members of your group.

Figure 1



Seconds Left: 524
Previous Earnings: 0.00
Current Earnings: 3.29

Figure 2



Green: Hello
Gray: Hi
Blue: What should we talk about?

Appendix II: Supplementary Analysis of Choice Data

Table II.1: Summary Statistics By Treatment and Group

Treatment	Group	Median	Mean	Rate of Max Play	Total Variation
CN	1	6.468	9.110	0.022	4451
CN	2	11.270	13.182	0.293	2088
CN	3	0.357	5.254	0.098	2341
CN	4	0.000	1.393	0.015	851
CN	5	5.536	7.830	0.085	6190
CL	1	1.587	6.954	0.059	1647
CL	2	0.000	2.648	0.001	622
CL	3	13.413	13.049	0.148	2700
CL	4	11.587	12.179	0.152	5477
CL	5	3.690	6.876	0.038	3826
CL	6	24.960	19.066	0.485	4118
CC	1	8.016	10.496	0.160	1953
CC	2	25.000	24.292	0.968	100
CC	3	25.000	19.963	0.728	498
CC	4	25.000	23.061	0.902	679
CC	5	11.865	12.013	0.323	1516
CC	6	25.000	23.368	0.927	214
DN	1	1.379	3.452	0.000	75
DN	2	6.111	7.023	0.000	152
DN	3	0.000	1.250	0.000	46
DN	4	0.397	4.198	0.050	162
DN	5	2.599	5.125	0.050	186
DL	1	0.000	2.796	0.025	102
DL	2	5.179	6.744	0.000	234
DL	3	0.000	4.048	0.100	215
DL	4	11.091	10.749	0.050	254
DL	5	18.294	17.401	0.000	109
DC	1	6.806	10.301	0.100	307
DC	2	6.587	10.092	0.225	223
DC	3	11.270	10.088	0.025	228
DC	4	10.218	11.704	0.150	219
DC	5	25.000	17.502	0.700	425

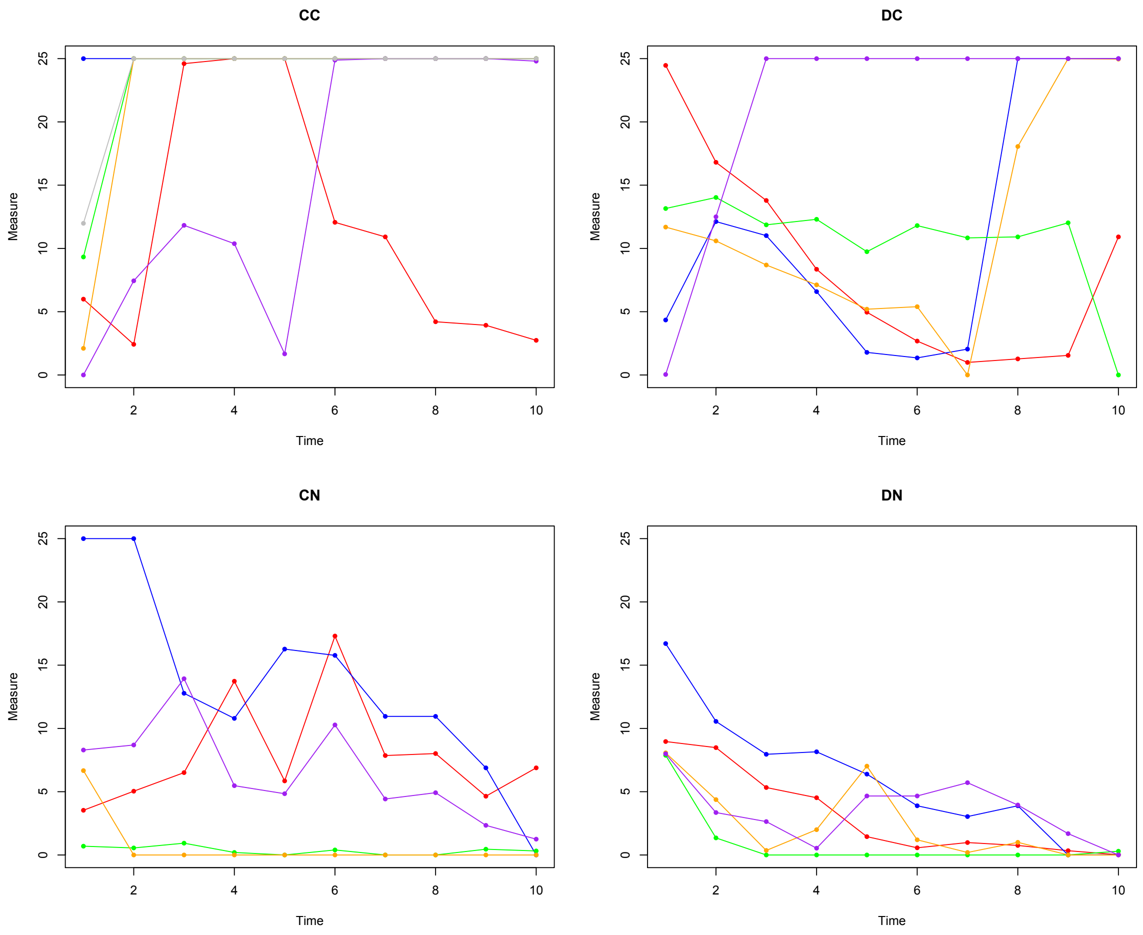


Figure II.1: Median contribution over time, by group

Note: In panel CC, one group -- plotted in gray -- nearly perfect overlaps other groups and is therefore difficult to distinguish. This group achieves 100% cooperation by the second bin and experiences a slight reduction in cooperation at the very end.

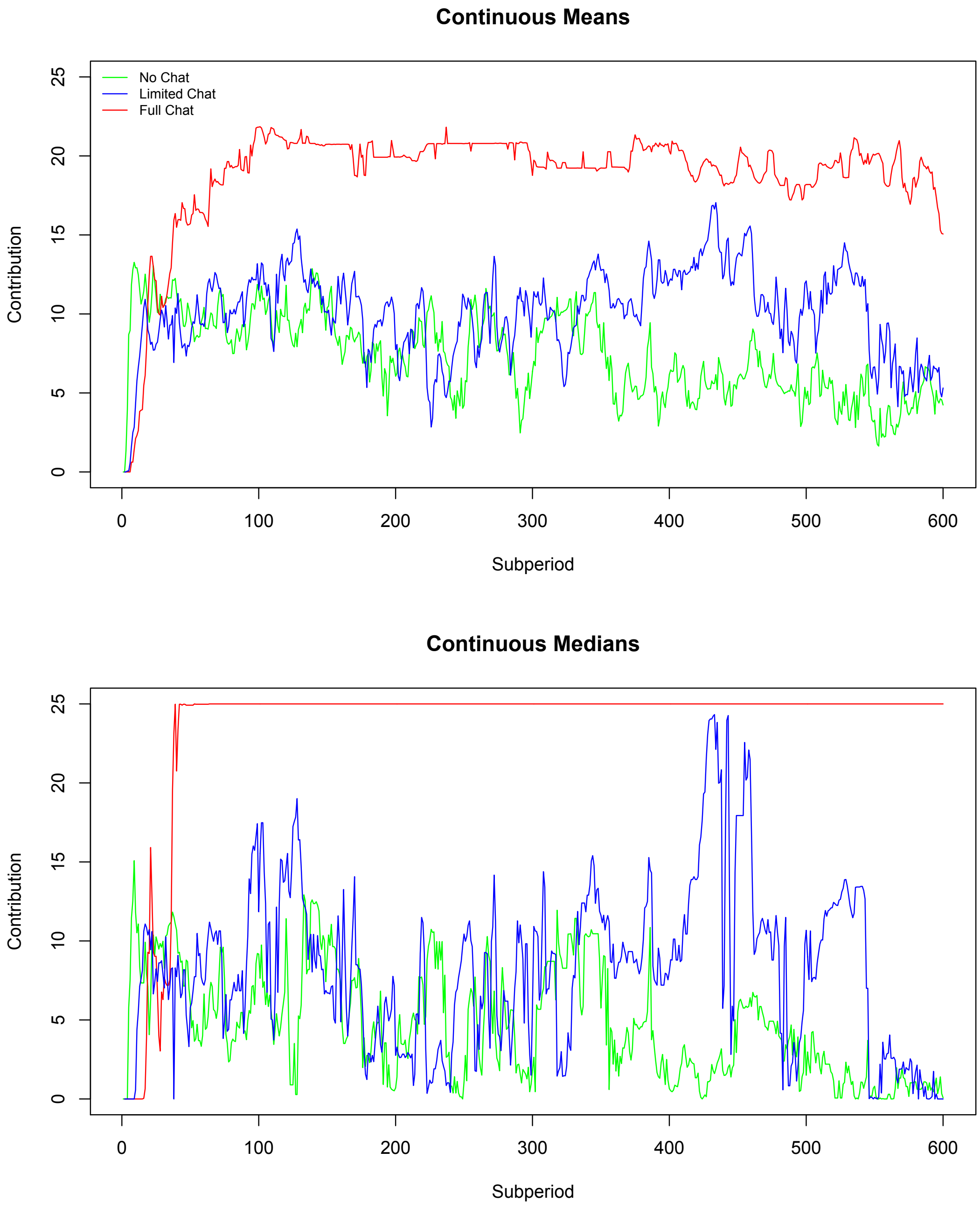


Figure II.2: Mean and Median contributions for continuous treatments aggregated by second.

Appendix III: Discussion and Analysis of Chat Data

Groups in the continuous-time treatment

We first examine the six CC groups, cross-referencing the summary statistics from Appendix I and the time series graphed in Figure 4 of Appendix I. Below, we provide summaries of the behavior and the communication for each group.

- CC Group 1 had the lowest mean contribution rate (10.496/25 = 41.98 percent) and the highest total variation. Here there was an agreement at about minute 2.5 to allow one person at a time to not contribute. This worked for a while and another agreement was proposed (to contribute half); however, this one was only kept by three people and lasted only briefly. This then broke down almost completely.
- Group 2 had an extremely high mean contribution rate (97.17 percent) and the lowest total variation. People started contributing 25 from the beginning, with no agreements made. Communication consisted of words of encouragement, such as "Good job team", "Team players", and "Haha yeah glad we got a good team tho".
- Group 3 had a moderately high contribution rate (79.85 percent) and an intermediate level of total variation. There was an initial agreement to all contribute 25, which was kept for about 2.5 minutes. A new agreement was then made to alternate one person out from contributing; this was kept until near the end, when everyone contributed 25.
- Group 4 had a high mean contribution rate (92.24 percent) and an intermediate level of total variation. There was an early agreement to contribute 25, which was kept by all except for a brief deviation at the end of the third minute (from which the deviating party was cajoled) and a small deviation at the very end. One of the players wanted to deviate in the middle of the session, but was talked out of it.
- Group 5 had a relatively low mean contribution rate (48.05 percent) and considerable total variation. There was a three-party agreement early on, but it was only kept briefly (probably because two people made no contributions). After minute 1.5, only two players communicated (and frequently), keeping an agreement to contribute 25 for the rest of the session, despite the others' behavior.
- Group 6 had a very high contribution rate (93.47 percent) and a low level of total variation. Here there was an early agreement to contribute 25, which was maintained until the end, except for some brief deviations by two of the players near the end. Severe admonishments by the others stopped these deviations.

Groups in the discrete-time treatment

- Group 1 had an intermediate mean contribution rate (41.20 percent) and a moderate level of total variation. There were no full agreements, but there was an early 3-person one.

When the other didn't cooperate at t= 60 seconds (and was excoriated), cooperation broke down. Two people tried to get it going later, but failed.

- Group 2 had an intermediate mean contribution rate (40.37 percent) and a moderate level of total variation. There was an early agreement on contributing half, but only three people did so; people complied with a new agreement for one period, then it decayed. New agreement for 25 at t=480; an excuse at 480, a defection (followed by chastisement) at 540, and full contribution at the end.
- Group 3 had an intermediate mean contribution rate (40.35 percent) and a low or moderate level of total variation. There were no real agreements, just some partial ones that weren't honored. Strong words and complaints in the middle, with some people displaying considerable annoyance until the end. Some people doing half and some people did very little; these were not always the same people.
- Group 4 had an intermediate mean contribution rate (46.82 percent) and a low level of total variation. An early agreement to contribute half was violated, with an apology. It was then kept once and slipped away, with more violations. They agreed to be selfish at 360, and then were able to agree to contribute 25 at the end.
- Group 5 had a relatively high mean contribution rate (70.01 percent), as well as a high level of total variation. There was an early agreement to rotate one person out, but there was some confusion. But the alternation was successful from period 3 to the end, except that everyone contributed 25 at the very end.

Continuous-time sessions (CC)

Group 1 *(Red line)*

- Agreement at 110 by all to alternate one person to the left every 30 seconds. Kept until around 255, when another agreement was proposed
- Agreement to go halfway at around 255. Worked for 3 people, but only briefly. It broke down into intermittent behavior after that. No later agreements made.

Group 2 *(Blue line)*

- No agreements at all. All started contributing from the beginning.

Group 3 *(Green line)*

- Agreement to all contribute 25 at 21.
- At around 167, then there was an agreement for one person at a time to contribute 0, with red going first. Red then went for about the agreed time. Then Green did so, and then Blue did so. And then Gray did so briefly, and then all contributed 25 for the last 100 seconds or so.

Group 4 *(Orange line)*

- Agreement at around 70 to all contribute 25. Three people deviated briefly at around 170 and then went back after some cajoling at around 180-190. Then people all contributed 25 until the end. Red wanted to deviate throughout the 200's, but was talked out of it.

Group 5 (*Purple line)*

- Agreement by 3 people at around 84, but not Gray. Then not much happened until around 300, when both Red and Green started contributing 25 (until the end), apparently not minding that Blue and Gray weren't helping. And only Red and Green communicated after 98.

Group 6 (*Gray)*

- Agreement at about 30 to contribute 25. Maintained until the end, except for some brief deviations by Gray and Blue near the end. These deviations were stopped by severe admonishments by Red and Green at 562-565.

Discrete-time sessions (DC)

Group 1 *(Red line)*

- Three people agreed at 45 to put all in, but Green didn't communicate. Green then contributed a little at 60, while everyone else contributed 25.
- Green was excoriated at 67-68 and 88. At 120, Red and Blue contributed little, while Green and Gray contributed a lot.
- Blue promised to go to the right at 143, and did so. But Green did not and Red lost faith. Green resisted public and got harangued throughout the 200's and did cooperate at 180 and 240. But by then none of the other 3 were contributing much.
- Intermittent attempts at cooperation later, but without much success. No agreements.

Group 2 (*Blue line*)

- Agreement on halfway before 60. Complaint about Green at 80.
- New agreement at 99. Everyone complied at half.
- Decay at 180. Stayed low until new agreement at 471.
- 3 people went right at 480, but not Red, who made an excuse.
- Agreed again, but this time Green defected. Complaint. New agreement, and this time they all did 25 at 600.

Group 3 *(Green line)*

- Discussion early on about efficiency vs. greed, but no agreement. An attempt at agreement to go half at 180, but not everyone agreed, and Green defected.
- Some partial agreement to go half at 240, but not honored by 2 people. Lots of complaints/strong words between 240 and 300. And they stayed aggravated until the end, with some people doing half and some doing very little.

Group 4 *(Orange line)*

- Agreed early on middle, but Blue defected at 60. Blue “apologized” and agreed on middle for 120. Kept agreement, some slippage at 180.
- Some discussion about doing 25 at 240, but no agreement. Green was selfish again.
- Agreed on left at 360 and followed through.
- Agreed to go more to the right for 480 and they did. Then agreed on all the way right at around 500 and did so at 540 and 600.

Group 5 *(Purple line)*

- No solid agreement before 60; Green was the only one to contribute. Agreement at 90 for 3 people to contribute at 120, with rotation to private for one. But it didn’t get implemented for 120 (Green defected), but there was some confusion. Worked at 180, rotated successfully at 240. Agreement for Blue to go left at 300, all followed it. Successful alternation all the way until the end, when everyone contributed 25. There was a lot of discussion about how to choreograph this.