

# **Social Interaction Effects in the Laboratory and Society: Conformism and Reciprocity in Public Good Provision**

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## Abstract

People contribute more to public goods, the more others give (“crowding-in”). We investigate two possible causes of crowding-in: reciprocity, the usual explanation, and conformism, a neglected alternative. The issue is important since conformism has more scope to bring about endogenous social interaction effects outside the laboratory. We use Bardsley’s (2000) Conditional Information Lottery design to control “others’ behaviour” without deception. We observe how contributions correlate within a group and between groups. Our main result is that conformism does cause crowding-in. The common presumption of reciprocity therefore deserves to be questioned, even in the laboratory.

Keywords: public goods, reciprocity, conformism, social interaction, deception

JEL-codes: C9, C92, H41

## 1 Introduction

Experiments on voluntary contributions to public goods have found, *inter alia*, that there is a positive correlation between the amount people contribute and expected contributions from others (see, for example, Croson 1998, Keser and van Winden 2000, and Fischbacher et al. 2001). This fact is referred to as the “crowding-in” of voluntary contributions.<sup>1</sup> Further, there is evidence about the direction of causation (Weimann 1994, Bardsley 2000): low contributions from others cause low contributions in response; it is not just that subjects believe their own contributions to be representative. Field studies of voluntary contributions also frequently find crowding-in, in contrast to the strict crowding-out prediction of economic theory. We believe that both methods are picking up the same phenomenon. It is an intriguing one because crowding-in is at odds with altruism, the economist’s stock departure from the self-interest assumption, in either its pure or impure (warm-glow) varieties. Altruism predicts crowding-out, that someone contributes less rather than more as others contribute more

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<sup>1</sup> We define “crowding-in” as the fact that  $\partial g_i / \partial G_{-i} > 0$ , where  $g_i$  is the contribution of individual  $i$ , and  $G_{-i}$  denotes the sum of others’ contributions.  $g_i$  is assumed to be a function of, inter-alia, others’ contributions and the productivity of the public good, defined here by equation 1 in the main text.

(Sugden 1984) while the motive of warm glow giving is self-centered and therefore independent of others' behaviour (Andreoni 1998).<sup>2</sup>

Economists have proposed theories of *reciprocity* that provide a straightforward explanation of crowding-in (see Sugden (1984) and Rabin (1993) and, for a recent survey, Falk and Fehr 2002). According to reciprocity theories, if enough contributions are made towards the public good by others, the individual will feel obliged to make contributions in return. Conversely, if the benefit from the public good turns out to be low because others do not contribute much, he or she will feel entitled to withhold contributions. So reciprocity predicts crowding-in. Note that the crowding-in prediction also implies one of *uniformity* in public good provision; if there is enough crowding-in then in equilibrium agents will be making similar contributions. For this reason, some economists (Falk and Fischbacher 2002, Gächter et al. 2002) have recently posited reciprocity as a plausible mechanism for bringing about a variety of endogenous "social interaction effects". For the purposes of this article, these are understood as processes by which agents who share membership of a particular group come to exhibit similar behaviour. "Endogenous" restricts these processes to those that come about through the behaviour of the agents, rather than through their prior characteristics. An often cited example is the correlation of criminal activity within a neighbourhood, after controlling for known social and economic determinants of crime (Glaeser et al. 1996, Case and Katz 1991).

We wish to concentrate on another possible explanation of crowding-in, however, which has thus far been largely ignored by both economic theorists and experimentalists. A positive correlation between individuals' contributions might show *conformism*, that is, imitative behaviour, not reciprocity.<sup>3</sup> It is central to the economist's notion of reciprocal motives that others' behaviour matters through its effect on the individual's welfare. We believe this is implausible for various well-known examples of social interaction effects. These might better be thought of as effects of conformism.

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<sup>2</sup> It is in interior equilibrium contexts such as modelled in the standard theory of public good provision (Bernheim et al. 1986) that standard selfishness, or altruism, predicts crowding-out (someone contributes less as others contribute more). Field studies should, therefore, find crowding-out but often find crowding-in. Lab games usually have corner solutions, so there is no crowding-out prediction.

<sup>3</sup> Conformism is sometimes referred to as "social influence". We use the first term to avoid confusion with influences which affect expected payoffs - as they do in Becker's well-known 1991 article, for example.

There appears to be crowding-in of voluntary contributions to *charities*, for example. Bryan and Test (1967) find crowding-in of contributions to charities in field experiments. Andreoni (1998) reports that charitable fund-raisers anticipate crowding-in, through their awareness of the importance of “seed money”. Contributors often have no, or very diffuse, prospects of being beneficiary of the charities’ activities,<sup>4</sup> though, which makes reciprocity seem out of place. Consider, for examples, overseas famine relief efforts such as Live Aid, or voluntarily funded children’s homes such as Dr. Bernado’s homes in the UK. Andreoni and Petrie (2001) and List and Lucking-Reiley (2001) both suggest social motives other than reciprocity as potential explanation for such observations.

Another example is provided by a study of Ichino and Maggi (2000). Investigating shirking behaviour in regional branches of a national bank, these authors find a clear positive relationship between a worker’s shirking level and the average shirking level of his coworkers. Since the bank is national, however, this seems to involve *local* crowding-in of contributions to a *national* public good. This study forms part of a growing body of empirical literature on social-interaction effects (for a survey see Mansky 2000). Whilst reciprocity might explain aspects of these effects, it is not clear why we should regard it as the only or even most plausible mechanism. If, for example, some youths engage in car crime following the example of their peers, one cannot interpret this as reciprocity since those who are influenced are not the owners of vehicles. Their peers’ actions impact on the welfare of a different set of agents.

In this paper we examine conformism as an alternative explanation for crowding-in and, by extension, social interaction effects in general. Conformism is a central concern of social psychology; the relevant literature there is vast (see Moscovici (1985) for a review). Conformism differs from reciprocity since appropriate reciprocal behaviour depends on the welfare effects of the stimulus behaviour, whilst conformist behaviour does not. For example, a conformist would contribute to a useless public “good”, which benefits no-one, if he observes enough others making contributions. A reciprocally motivated agent would not, since he does not benefit from their behaviour.

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<sup>4</sup> Except perhaps in a formal sense. Charitable contributions are in fact often modelled as public good problems (see, for example, Sugden (1982)). One problem with reciprocity as an account of charity provision is that it is hard to see, for example, how overseas famine relief is motivated by a desire to return benefits obtained from other donors. If A helps someone, B, that C cares about, it is (in natural language) B that benefits, not C. See Schwartz (1982) for relevant philosophical analysis of the concept of welfare.

We adopt a formulation of conformism by Cialdini (1993) called “social proof” in our discussion, because it ought to apply in a public good context and because it contrasts in interesting ways to standard economic rationality. However, we do not depend on a specific version of conformism to formulate our experimental prediction. Our main concern is more modest: to test a null hypothesis that reciprocity can account for all the crowding-in present in our data.

According to Cialdini’s “principle of social proof”, “We view a behaviour as correct in a given situation to the degree that we see others performing it” (1993, p95). Social proof is supposedly more powerful the more similar are the others performing the action to ourselves, and “...when we are unsure of ourselves, when the situation is unclear or ambiguous, when uncertainty reigns” (p106). In a one-shot public good context, though there is no uncertainty in the standard sense, there may be ambiguity since there is a conflict to be resolved between collective and individual interests. A person’s estimation of how reasonable it is to free-ride might be influenced by the observed behaviour of others, independently of its effect on her. Notice that the interpretation of “correct” behaviour in Cialdini’s formulation is left open, whilst in economics it is usually defined as utility maximisation.<sup>5</sup> We find it plausible that conformism might operate normatively in a public good context, that is, on the agent’s conception of social or moral norms. If the former is modified, this could explain local variation in provision of national public goods (as in the Maggi (2000) study), since social norms are by definition local, that is, specific to a community.<sup>6</sup>

To isolate conformism, we provide individuals with welfare-irrelevant information about others’ behaviour and see to what extent it changes their own behaviour. Our broad strategy therefore is the same as that of Cason and Mui (1998). In their study subjects make two consecutive choices in a dictator game. Before the second choice each subject receives information about another subject’s first choice. The authors find no significant difference between this treatment and a control providing irrelevant information only. A weak indirect effect is found only in that decisions are more self-regarding in the irrelevant information

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<sup>5</sup> This definition is preserved in the (very different) economic formulations of conformism in “information cascade” models (for which consult Bikhchandani et al. (1998)). Unlike social proof, these do not apply in a public good context since there is no uncertainty about the utility maximising action to be resolved.

<sup>6</sup> This is what distinguishes them conceptually from *moral* norms, which are in some sense universal.

treatment. Asking subjects to choose contingent on all possible first choices did not reveal any indication for conformism, either.

In a related experiment by Brandts and Fatás (2001) subjects in a first round participate in a two-person sequential game, of the following form. Each subject is paired with a second subject with whom they have to play a public good game. Next, average first stage donations are revealed. Then each subject is re-matched with a second subject. Finally, each person  $a$  has to submit a strategy conditioning play on their new partner's first stage decision. That is, each person's first stage decision is matched with another subject's second stage decision and vice versa. The authors find conformism (the effect of the average donations information on a subject's second stage decision) to be rather a weak force and conclude that models of social behaviour need not incorporate it.

We believe that such a conclusion is premature. In the Cason and Mui (1998) study, very little social information was imparted; the decision of just one other individual was revealed. Almost no information about *normal* behaviour was therefore imparted. In the Brandts and Fatás study, payoffs are such that first stage contributions might be entirely egoistic, driven by a desire to trigger return donations in the second stage. Therefore no clear information is given about the normality of the nonselfish behaviour required of second movers. The use of a summary statistic about others' behaviour might also suppress conformism since it conceals the variance it exhibits. Furthermore, both experiments make use of the strategy method, which requires subjects to submit a complete strategy at one point in time. This involves a loss of spontaneity in subjects' decisions, which could interfere with the operation of conformism.<sup>7</sup>

In contrast, our experiment exposes subjects to a large amount of social information, maintains a clear distinction between selfish and nonselfish action, and uses a more natural procedure than the strategy method. The remainder of the paper is organized as follows. Section 2 describes the experimental design in full detail. Section 3 reports our results. In section 4 we discuss possible economic implications. Section 5 concludes.

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<sup>7</sup> The Brandts and Fatás (2001) and Cason and Mui (1998) studies exhaust examinations of conformism by experimental economists. The study by Falk and Fischbacher (2002) elides reciprocity and conformism both in the text and experiment.

## 2. The Design

### 2.i Operational Distinction Between Conformism and Reciprocity

The basic design is as follows. Two groups of experimental subjects, groups A and B, participate in a sequential public good game. Groups A and B correspond to treatments; in treatment/group A subjects observe only their own group's behaviour. In treatment/group B they also observe another group's behaviour, that of group A.<sup>8</sup> In both groups, in stage one,  $n-1$  subjects decide on the use of their endowment, whilst in stage two the  $n$ th agent decides. Subjects in group B make their contributions after having seen the contributions of group A. What happens in A is irrelevant for the payoff of B subjects and vice versa. We are interested in comparing behaviour in the following types of situation:

1. An agent (in group B) sees members of his own group contributing on average 90% of their endowment, and members of group A 95% of theirs, before making his contribution.
2. An agent (in group B) sees 90% contributions in his group, and 5% in group A, before making his contribution.

If reciprocity is the only cause of crowding-in, contributions will be the same in these two situations. Alternatively, if there is conformism, contributions will be lower in the second situation.<sup>9</sup> To test a hypothesis such as this we need a method of controlling "others' behaviour", without using deception.<sup>10</sup> We use the Conditional Information Lottery (CIL) procedure developed in Bardsley (2000), which *dramatises* branches of the game tree of interest. This means that in each task, a situation unfolds which could arise during the game described above, and a subject must decide how to behave in that situation. A real play of the game is also included as one of these tasks, and only this determines payoffs for the whole experiment. In this task, since it depicts a real situation, actual choices are shown from all subjects. In the other, fictional, situations, subjects are shown dummy contributions entered into the software by the experimenters, but decisions made do not affect payoffs. This

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<sup>8</sup> From this point on, "A" refers to group A, six subjects in an implementation of our design, whilst "A" refers to treatment A, comprising 78 subjects (the 13 group As) – and similarly for group B / treatment B.

<sup>9</sup> This hypothesis is simpler than the test we in fact use (see next section), and is for illustrative purposes only. The test we report in section 3.i combines all the relevant data for a single test.

<sup>10</sup> Deception is prohibited by economic experimentalists because of the fear that it may pollute the subject pool. See Bonetti (1998) and Hertwig and Ortmann (2002) for recent discussions.

procedure is explained and demonstrated in advance. Subjects do not know ex-ante which situation is real, which gives them an incentive to treat each one as if it, alone, is real.<sup>11</sup>

The fictional situations differ systematically in the information shown about “others’ behaviour”. Our main interest is in these fictional situations, rather than in the real one, since they allow us to construct test situations, like that shown above, in which one would observe differences between reciprocal and conformist behaviour. These situations would be unlikely to occur with sufficient frequency for the use of a “choice-only” design, in which subjects just play a single straight game. One might run thousands of choice-only experiments, at great cost, before one had sufficient observations of a test situation. In comparison, we can place every subject at any node of any branch of the game tree of interest using the CIL method. We use it to place each subject in last position in test situations such as the one above, to probe the functional form of contributions - as a function, that is, of the behaviour of a subject’s own group and that of another group.

The design provides appropriate means to isolate conformism. First, our procedures leave unchanged the presentation of the choice situation, relative to a choice-only design. Unlike the strategy method, subjects in our experiment do not specify in advance a complete strategy contingent on all possible behaviour by others. Rather, subjects task by task are presented with specific situations which they have to react to. Second, rather than reacting to a sample of others’ behaviour or a summary statistic chosen by the experimenter, subjects react to contributions shown from all other subjects participating in a session. The one-shot character of the experiment preserves statistical independence across subjects across tasks, assuming that subjects understand they cannot learn about others’ behaviour from task to task.<sup>12</sup>

## **2.ii The Game**

Subjects are endowed 10 with tokens each. A token is worth € 0.70. The tokens can either be kept or contributed to a public good which generates a payoff for everyone in the group. Subjects are divided into two treatment groups A and B. Payoffs are determined independently in both groups according to:

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<sup>11</sup> The design issues are discussed in detail in Bardsley (2000). We see it as a promising alternative to the strategy method in affording control over “others’ behaviour” without using deception.

<sup>12</sup> The event that the next task is real is the event that all the previous ones were fictional. If not all subjects understood this, the effects of the interaction are presumably dilute, since they interact only once over 13 tasks.



$$C_i = 10 - g_i + 0.5 \sum_{a=1}^n g_a \quad (\text{equation 1})$$

where  $C_i$  is subject  $i$ 's payoff in tokens and  $g_i$  is an individual  $i$ 's contribution to the public good. The number of players is  $n = 6$  both in groups A and B. The following order of events is observed in each task:

Stage 1. In Group A, agents 1-5 decide.

Stage 2. Agent 6 in A sees their decisions then decides.

Stage 3. Group B see group A's decisions then subjects 1-5 decide.

Stage 4. Agent 6 in B sees their decisions then decides.

### 2.iii The Task Set

#### Tasks 1-4

Each subject faced 13 tasks, one of which was the real game. The dummy contributions can be described as follows. Let  $X_i$  stand for a vector of five contributions. Subjects in treatments A and B are both exposed to  $X_i$ .  $X_i$  represents others' contributions in a subject's own group. Let  $Y$  stand for a vector of six contributions. Only subjects in treatment B are exposed to the stimuli contained in  $Y$ .  $Y$  represents *another group's* contributions. Subjects contribute to the public good in one real situation and twelve fictional ones. The fictional situations are the same for all subjects and differ with respect to i) the information contained in  $X_i$  and (in treatment B)  $Y$ , and ii) whether the subject moves first or last in the sequence. The position in the sequence determines the extent of information given about a subject's own group when it is her turn to contribute. We are primarily interested in the following tasks:

Situation	$X_i$						$Y$					
1 <i>LowLow</i>	2	0	2	1	0	_	0	0	3	0	1	0
2 <i>LowHigh</i>	1	2	0	0	1	_	10	10	7	9	9	8
3 <i>HighLow</i>	9	10	8	9	9	_	0	0	2	1	0	0
4 <i>HighHigh</i>	10	9	8	8	10	_	9	10	8	7	10	9

(numbers show fictional contributions, “\_” indicates a subject's position in the sequence)

Task number  $j$  in treatment A consists of row  $j$  of  $X_i$ , whilst the corresponding task in treatment B also contains row  $j$  of  $Y$ . Thus in task 1, group A see others' contributions of 2, 0, 2, 1 and 0 on their screens before they are asked to make a decision, whilst in treatment B in

task 1, subjects first see contributions in the other group of 0, 0, 3, 0, 1 and 0, then they see 2, 0, 2, 1 and 0 from other members of their own group before being asked to allocate their endowment.

In tasks 1-4, our aim is to observe each subject's contribution contingent on the contributions of all others. In these tasks, all subjects in fact were placed at the end of the  $X_i$  sequence, though each subject sees contributions from others in her group before she decides.<sup>13</sup> The tasks are designed to separate reciprocity from conformism. Subjects in treatment  $B$  are shown high and low contributions from their own group, coupled with high and low contributions in the other group. This enables within-subject comparisons which hold constant the behaviour of a subject's own group and vary the contribution levels of the other group (task 1 versus 2, 3 versus 4).<sup>14</sup> The information contained in  $Y$  is irrelevant from a strategic point of view, regardless of whether subjects are reciprocal or egoistic. For, placed last in the sequence, subjects are fully informed about the decisions of others in their own group. Therefore, it is plausible to assume that effects of the information in  $Y$  can be effects of conformism only.

Our null hypothesis for these tasks, of reciprocity being the unique cause of crowding-in, is  $H_0: (\bar{g}_{B4} - \bar{g}_{B1}) = (\bar{g}_{B3} - \bar{g}_{B2})$  tested against the alternative  $H_1: (\bar{g}_{B4} - \bar{g}_{B1}) \neq (\bar{g}_{B3} - \bar{g}_{B2})$ .<sup>15</sup> This is the most exacting test we can construct of the null hypothesis. For this difference in differences should show compound effects of conformism, if it is present. In task 2 contributions should be higher than in task 1, since subjects see much higher contributions in the other group than in task 1. Whilst in task 3, they should be lower than they are in task 4, since they see much lower contributions from the other group than in task 4. Under conformism, therefore, the difference between contributions in tasks 2 and 3 in treatment  $B$ ,  $(\bar{g}_{B3} - \bar{g}_{B2})$ , should be dampened both by a discouragement of low contributions in task 2 and a discouragement of high contributions in task 3, compared to the difference in tasks 1 and 4,

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<sup>13</sup> This does not amount to deception, however, since subjects are told before the experiment starts that, in the fictional situations, the task information is just an artefact of the design.

<sup>14</sup> There is a limit to the extent to which this constancy is possible, since presenting exactly the same stimuli in  $X_i$  or  $Y$  might encourage some subjects to think that a certain task cannot possibly be the real game situation (that is, if they remember the stimuli from a previous task).

<sup>15</sup> A one-tailed test is inappropriate since inequality aversion predicts an effect in the opposite direction to conformism, if subjects are concerned about inequality relative to both groups A and B.

$(\bar{g}_{B4} - \bar{g}_{B1})$ . Note also that this test combines data from all the four tasks into a single test, so that we avoid running multiple tests of the same hypothesis.<sup>16</sup>

### Tasks 5-6

It is often assumed that subjects react to the mean contribution from others in public good games (for example, by experimenters seeking workable implementations of the strategy method in games with a large strategy space). We included two tasks to test this assumption. We are able to vary “others’ contributions” whilst keeping their mean and median constant, because we expose subjects to full vectors of others’ contributions rather than to a summary statistic. Tasks 5 and 6 below test this assumption.

One plausible way in which subjects might be sensitive to the distribution of contributions is if some of them do not wish to give more than the minimum given by others.<sup>17</sup> Tasks 5 and 6 share the same mean and median contribution, but task 6 contains a free-rider:

Situation	$X_i$						$Y$					
5	5	10	9	5	9	–	0	0	3	0	3	2
6	10	0	9	10	9	–	0	0	3	0	4	2

So, if subjects exhibit such sensitivity to the minimum one would expect lower contributions in task 6. Our null hypothesis, then, is  $H_0: \bar{g}_5 = \bar{g}_6$ . The alternative is  $H_1: \bar{g}_5 \neq \bar{g}_6$ , since sensitivity to the minimum is not the only conceivable effect. They might also exhibit more sensitivity to the upper quartile, for example.

### Tasks 7-12

In addition to these, in six tasks all subjects are placed first in the sequence (though the task is, of course, presented as having the same structure as all the others). In these, nobody can observe anything about their own group before making a decision. That is, each subject in

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<sup>16</sup> We might instead have expressed our hypothesis between treatments, as  $H_0: (\bar{g}_{A2} - \bar{g}_{A3}) = (\bar{g}_{B2} - \bar{g}_{B3})$  tested against the alternative  $H_1: (\bar{g}_{A2} - \bar{g}_{A3}) \neq (\bar{g}_{B2} - \bar{g}_{B3})$ . However, a potentially confounding factor here would be the greater quantity of information which subjects in treatment *B* have to digest compared to that in treatment *A*.

<sup>17</sup> A more sophisticated but demanding version of this kind of sensitivity is built into Sugden’s (1984) reciprocity theory. There, the presence of one free-rider does not negate an obligation towards others who contribute if there are enough of them to make the public good a worthwhile scheme collectively – as there are in task 6.

treatment group A contributes here without any information about others' behaviour. They are then shown their own contribution together with four elements of a row of  $X_{-i}$ , completing the first stage, followed by the 5<sup>th</sup> element of that row of  $X_{-i}$ , completing the second stage of the game. Subjects in group B observe a row of  $Y$  (the other group's behaviour) before being asked to make a contribution. After this they are also showed the  $X_{-i}$  information in the same manner as for group A. These tasks were to included primarily to camouflage the real task. They also allow us to see whether, in group B, subjects are influenced by contributions in group A. Here, though, either reciprocity or conformism could account for such an effect. For subjects in group B might use the information to update their beliefs about likely contributions in their own group, using it to work out appropriate reciprocal behaviour.

An issue arose as to whether or not to inform group A subjects that their contributions are, in the real task, to be shown to subjects in group B. We resolved this as follows. Since we did not wish to compromise the honesty of the design, in the real task subjects were fully informed about their decision environment. This entailed that we told them their decisions were going to be revealed in that task. However, we also told them this during some of the fictional tasks, whilst in the others they were told that their decisions were not to be revealed. This procedure kept the design deception-free, since subjects knew that in the fictional tasks any piece of ostensible information is in fact just an artefact of the experiment, and enabled us in addition to see if there was an effect of this information. One might expect an effect here, since it puts  $A$  subjects in the role of an example to the other group.<sup>18</sup>

The stimuli for the tasks in which subjects were placed in first position are shown below. The column headed " $A$ : Yes/No" indicates whether or not subjects in treatment  $A$  were told that their contributions would be shown to the other group. So in task 7, for example, subjects were told their contributions would be revealed (although in fact treatment  $B$  subjects were shown row 7 of  $Y$ ), whilst in task 8 they were told they would not be revealed (and  $B$  subjects were shown row 8 of  $Y$ ).

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<sup>18</sup> Our expectation was that being in this role should raise contributions of  $A$  subjects, since it reduces the "social distance" between themselves and  $B$  subjects (see below).

Situation	A: Yes/No	$X_i$						$Y$					
7	No	–	0	0	0	0	0	0	0	10	9	8	6
8	Yes	–	0	10	1	5	1	1	0	4	0	0	7
9	No	–	0	9	10	10	5	0	10	0	0	2	0
10	Yes	–	1	0	3	5	5	0	9	0	0	7	0
11	No	–	6	6	3	0	0	4	5	10	7	10	0
12	Yes	–	0	10	0	7	5	3	5	0	0	5	5

We are interested to test certain other hypotheses to assess the consistency of our results with those from comparable experiments. There should be crowding-in amongst subjects within each group. This can be tested by within-subject comparison of behaviour in treatment  $A$ , tasks 1 and 2 versus 3 and 4. Secondly, Bardsley (2000) reports that contributions diminish within one-shot sequential public good games. If that is correct we should observe lower contributions by last movers than by first movers in the real task.

There are other potentially interesting comparisons between our data and other results. First, it is possible that contributions decrease as the experiment progresses, as in Bardsley (2000) although this would not reflect interaction between egoists and reciprocators, which is the usual explanation. Further, Shafir and Tversky (1992) and Bardsley (2000) report that subjects are most cooperative when uninformed about others' actions. If that is correct,  $A$  subjects might give less in tasks 3 and 4, when their own group funds the public good almost maximally and they alone respond, than in 7-12, when they are in first position contributing at the same time as four others. However, to test these additional hypotheses would involve running multiple tests on the same data, undermining statistical significance. For example, we already use data from  $A$  tasks 3 and 4 to look for crowding-in within a group, as discussed in the preceding paragraph. We do not, therefore, test these hypotheses. Rather we simply examine descriptive statistics on these comparisons to see if they suggest the hypothesis that our results are different from those of comparable designs. This is an exercise in hypothesis forming, not testing. To this end we also include a regression reported in the appendix, which can be regarded as a summary of the entire dataset.

To summarise this section, the following schema maps our treatments and tasks with the effects we are able to investigate:

Treatment *A*:

Tasks: 1-4: last mover's reaction to contributions from other members of her own group

7-12: effect of revelation to the other group on first mover's contributions

Treatment *B*:

1-4: effect of the other group's behaviour on the last mover

7-12: effect of the other group's behaviour on the first mover

Treatments *A* and *B*:

5-6: effect of the distribution of contributions

13 (real): last mover effect.

These six hypotheses are tested in sections 3.i to 3.vi. The rest of section 3 assesses further the consistency of our results with other experimental findings from a descriptive point of view, in line with the discussion above.

## **2.iv Implementation**

To make the supposed contributions in these situations appear as realistic as possible, real decision data from a previous public goods experiment has been used to generate them.<sup>19</sup> Secondly, participants are seated distantly from each other in the computer laboratory on terminals separated by partitions such that it becomes impossible to see anyone else's screen. In addition, whenever it is someone's turn to make a contribution via the keyboard of the computer everybody else had to enter a letter randomly generated by the program, further disguising who was doing what when. Finally, within a session the stage of the real decision round was randomly determined; subjects and experimenters learn about it only after all rounds were terminated. The order of tasks was randomised across subjects, excepting the real task of course, to eliminate any confounding timing effects.

Notice again that the procedures enable us to face subjects with situations being exactly the same for all. The full set of situations used in the experiment is reported in appendix A-I. Within-subjects comparisons can be applied to test for conformism. In addition, the

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<sup>19</sup> Data was obtained from Bardsley, from his (2000) simultaneous play, one-shot public good treatment. This might look unrealistic if the last placed subject gives more than the others, but this is only the case in one task (task 8). In Bardsley (2000) last-placed subjects (in the sequential condition) *did* occasionally contribute the most.

assignment of groups A and B is equivalent to an assignment to treatments that differ only with respect to whether or not the contributions of the second group are shown.

156 subjects participated in 13 sessions (12 subjects per session) conducted in the experimental laboratory of CREED at the University of Amsterdam, in January 2002. Average earnings were €13.70 per subject in approximately one hour. Subjects went through detailed instructions on their computer screens (see appendix). They experienced three practice rounds before the actual experiment started. In addition, a summary containing the vital elements of the procedures were read aloud to subjects by the experimenter. We checked subjects' understanding of how many situations they would be shown (a total of 13) and how many of these would be real and so relevant for payoffs (only one). We explicitly pointed out that nothing could be learnt from one situation to another, because of the CIL set-up.

### 3 Results

#### **Treatment A**

Situation	Real	1	2	3	4	5	6	7	8	9	10	11	12
Mean	4.83	2.14	1.67	4.49	4.29	4.03	3.87	5.51	4.74	5.09	5.03	5.00	4.88
Std	3.50	3.38	3.12	4.20	4.28	3.80	4.14	3.43	3.61	3.60	3.72	3.56	3.77

#### **Treatment B**

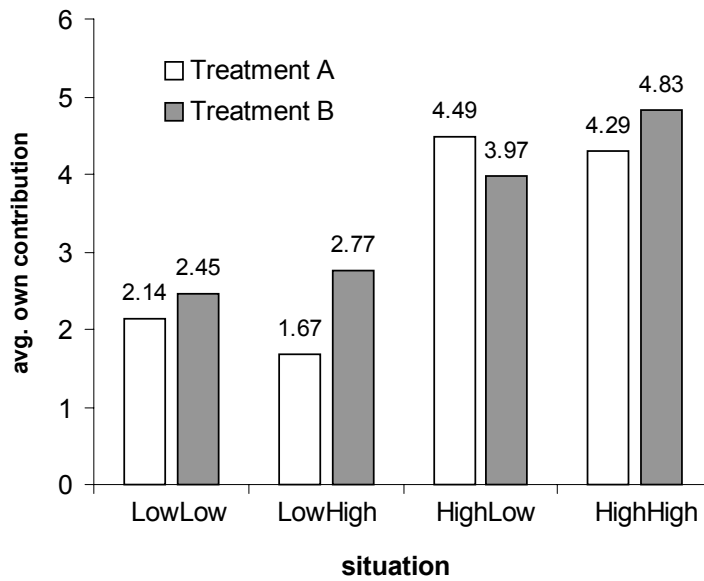
Situation	Real	1	2	3	4	5	6	7	8	9	10	11	12
Mean	4.71	2.45	2.77	3.97	4.83	4.14	4.69	4.64	4.37	4.62	4.71	5.26	4.62
Std	4.02	3.73	3.86	4.35	4.47	4.25	4.30	4.14	3.81	4.00	4.00	3.95	3.93

Notes: N=156 (78 subjects in each treatment).

**Table 1: Summary Statistics by Task**

#### **3.i. There is crowding-in independently of reciprocity**

Inspection of table 1 shows that comparisons both across and within treatments are generally in the direction predicted by the alternative, conformism hypothesis. This can also be seen in figure 1 below, representing tasks 1-4.



**Figure 1: Mean contributions in test conditions**

Key: LowLow = Task1, LowHigh = Task 2, HighLow = Task 3, HighHigh = Task 4

Contributions are higher in treatment *B* for task 2 than in task 1 and lower in task 3 than they are in task 4. This suggests that observing flourishing cooperation in another group discourages low contributions, whilst observing low contributions there discourages high ones. Inspection of the results across treatments suggests the same interpretation.

Now consider our hypothesis test for reciprocity versus conformism. The difference between contributions in situations *HighHigh* (task 4) and *LowLow* (task 1) is 2.38 tokens. Between situations *HighLow* (task 3) and *LowHigh* (task 2) it is only 1.2 tokens. Under the null-hypothesis of zero conformism these differences would be the same. A two-tailed Wilcoxon signed-rank test indicates that a difference of this magnitude would be observed with probability  $p = 0.049$  under the null hypothesis. We ought, therefore, to reject our null hypothesis that reciprocity alone can account for the crowding-in in our data.<sup>20</sup>

We should note that an apparent exception to the general picture outlined above, however, is provided by task 6. Although intended to test a different hypothesis, this is similar in structure to task 3. The results, though, appear to be in the other direction than suggested by

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<sup>20</sup> If we had instead used the between treatment comparison, the corresponding rank sum test would return  $p = 0.026$ . See note 15 above; this does *not* constitute additional evidence.



conformism if we compare across treatments, though not if we compare within treatments. One possible reason, other than sampling error, is that seeing low contributions in the other group, and high ones in their own, caused some subjects to overlook the 0 in their own group in task 6 or read it as a 10. Subjects have more information to digest in treatment *B* than in *A*, which we already noted might cause confounding effects. This is why we used homogeneous behaviours within groups to test our conformism hypothesis and do not conduct across treatment comparisons. Our regression analysis cannot clarify matters further since only tasks 1-6, plus 13 observations from the real task, are used to calculate the coefficient for the other group's impact on *B* second movers.<sup>21</sup>

### **3.ii Subjects in first position are also influenced by the other group's decisions**

In our design, all subjects move first in situations 7 to 12. First movers in treatment *B* contribute more the higher contributions are in the other group. Total dummy contributions from the other group were 33, 12, 12, 16, 36 and 18 tokens in tasks 7-12 respectively. Inspection of table 1 shows contributions from *B* subjects are weakly monotonic in the order of total dummy contributions in these tasks. The difference in mean contributions between situations 8 and 11, with lowest and highest dummy contributions respectively, is statistically significant (two-tailed Wilcoxon signed rank test,  $p = 0.00$ ).

### **3.iii There is substantial crowding-in within a subject's own group**

In both treatments subjects appear to respond with low contributions when contributions in their own group are low; and they respond with high contributions when contributions in their own group are high. Recall that treatment *A* data is to be used to test this hypothesis. We compare the mean of a subject's contribution in tasks 1 and 2 with that in tasks 3 and 4. The Wilcoxon signed rank test on the difference between these means returns  $p = 0.00$  for a two tailed test, so we reject the null hypothesis of zero crowding-in within a group.

From table 1, our data suggest the hypothesis that crowding-in is stronger vis-à-vis a subject's own group than with respect to the other group. For if both sets of contributions exerted equally powerful influence on *B* second movers, the latter would give at least as much in task 2 as in task 3. The results suggest the opposite, though. Greater within-group crowding-in is

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<sup>21</sup> See appendix. The coefficients are of the sign and magnitude suggested by our hypothesis test, but not significant (in the regression  $p = 0.102$  for a two-tailed test on the coefficient on the other group's contributions).

also suggested by the regression analysis for treatment *B*, which estimates a higher coefficient for the mean of other *B* group contributions than for that of *A* group contributions, as determinants of *B* subjects' contributions in last position (see appendix). However, we cannot test this conjecture since we have already assigned data from each task to test one hypothesis.

### **3.iv Subjects contribute *less* when their contributions are to be shown to another group**

In tasks 7-12, subjects are placed in first position. Recall that in tasks 8, 10, and 12, subjects in group *A* are told that their contributions are going to be shown to subjects in group *B*, but that in 7, 9 and 11, they are told that these will not be shown.

Our prior expectation was that subjects would contribute *more* in tasks 8, 10 and 12, since there is less social distance. For subjects are placed in the position of examples to the other group. There was no evidence to this effect however. Our results suggest, on the contrary, that subjects contribute *less*. Inspection of table 1 shows that contributions from *A* subjects are generally lower for tasks 8, 10, 12 and the real task, than in tasks 7, 9 and 11. A two sided rank-sum test based on individual averages of *A* first movers indicates that contributions are not equal across these two sets of tasks ( $p = 0.021$ ).

### **3.v Crowding-in tracks the average of others' contributions, independently of their pattern**

The comparison between tasks five and six, within subjects, shows no significant difference. There is no evidence in our dataset, therefore, that subjects react to other features of the distribution of others' contributions than their mean or median.

### **3.vi Last Movers Contribute Less than First Movers**

Consider the real task. Here, in treatment *A*, first movers contributed a mean of 5.08 tokens, and second movers 5.05. The corresponding medians are 5 and 3 tokens respectively. In treatment *B*, first movers gave a mean of 3.61 tokens, and second movers 3. Here the medians were 5 and zero tokens respectively. Combining data across treatments, this difference is statistically significant. The rank sum test returns  $p = 0.033$ , for two-tailed test (a two sample test is appropriate, since a subject moves either first or second). The difference between first and last movers' contributions is reflected in the regression analysis (see appendix), where a median contribution of 10 tokens is estimated to produce 6 tokens in response from the (average) last mover.

### **3.vii Treatment *A* Subjects appear to contribute at least as much in first position as when others were extremely cooperative.**

Consider tasks 3 and 4. Here, subjects are shown the highest contributions from others in their group in the task set. In treatment *A* the mean contribution is always higher, however, in tasks 7-12, where subjects are uninformed about the others' contributions (since they are in first position).<sup>22</sup>

### **3.viii Contributions appear to decline as the experiment progresses**

This emerges from the regression analysis, in which the order in which a task appeared is an independent variable. See the appendix; the coefficient on the order of tasks is negative and statistically "significant",<sup>23</sup> indicating that the later tasks are associated with lower contributions.

## **4 Discussion**

### **4. i Reciprocity *and* conformism, or just conformism?**

The null hypothesis that reciprocity alone could account for the crowding-in present in our data is rejected (result 3.i. above). It would probably be unwise to read too much significance into the results of one experiment. Assume for the purposes of our discussion though, that the result is robust to further testing, the aim being to examine the consequences to see what is at stake here. If crowding-in is not all reciprocity-driven, we are left with two alternatives. Either there is some mixture of reciprocity and conformism responsible or it is *all* conformism. Our data do not actually allow us to distinguish these alternatives. We have noted already that our data suggest the hypothesis that crowding-in is more pronounced within groups than between them, which one might think is indicative of reciprocity being the stronger force. However, since we find some conformism with respect to the other group, it is likely that at least part of the crowding-in *within* a group is also conformist in nature.

Recall also that *social proof* is hypothesised to be an effect on behaviour which is more powerful the more similarity there is between agents who observe each other's behaviour. Since agents differ in our experiment principally in their group membership, other

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<sup>22</sup> The relevant data is for treatment *A* only, since in treatment *B* there are confounding influences from the other group's contributions.

<sup>23</sup> See the main text. The regression is included for descriptive purposes only.

characteristics being withheld, it is possible, so far as our experiment is concerned, that crowding-in is *entirely* conformist! That is, more crowding-in might occur within groups because subjects see their group allocation as a respect in which they are similar or different to the others.<sup>24</sup>

On the other hand, there is evidence from other experiments that reciprocity is a force to be reckoned with. In particular, *punishment* behaviour seems to be well explained as stemming from negative reciprocity – the desire to inflict harm on those who harm oneself. The experiment by Fehr and Gächter (2000) shows that subjects are willing to punish non-contributors at a cost to themselves in public good games. A real life example is social sanctions inflicted (usually with great invective) on strike breakers. However, punishment, if effective, is itself a public good, voluntary contributions to which may themselves exhibit crowding-in. Disentangling the relative responsibility of conformism and reciprocity for crowding-in is therefore a complex matter which we must pass over here as an issue for future research.

Parallel questions arise for the interpretation of result 3.iv. That first movers are influenced by the actions of the other group might just show rational belief formation. The information (in treatment *B*) about another group will be relevant, if one has no prior information about the behaviour of one's own group, if subjects are concerned to anticipate their own group's contributions. For they can use this information to update their beliefs about them. We cannot quantify the extent of such updating however, since the effect we observe is probably an admixture of updating and conformism, given our first result.

#### **4.ii Better a free-rider than a sucker**

We found no evidence that being in the position of an example to the other group raises contributions. Instead, it appears to *lower* them (result 3.iv). This was surprising to the authors. A possible explanation, which we hope to investigate in future research, is that subjects feel some embarrassment if they are seen to have made high contributions when others in their group make low ones. In other words, it is better to be seen as a free-rider than as a sucker! Embarrassment might be expected to be lower in the anonymous environment of this experiment, though, than in many real world public good contexts, so this is a potentially

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<sup>24</sup> We owe this point to Robert Sugden (personal correspondence).

important finding. Here, a subject's contribution was to be shown to the other group, but their identities withheld from it. If a subject was to be "suckered", making high contributions whilst the others took a free-ride, subjects in the other group would only know that *someone* in the other group was in this embarrassing position.

Our results here contrast with the well-known experiments by Hoffman et al. (1996) in which reduced social distance (more knowledge about who did what) either amongst subjects or between subjects and the experimenter *reduced* selfish behaviour, in dictator games. In the dictator game though, there is no prospect of being suckered by others since only unilateral action is involved. Future research could discriminate between situations in which reduced social distance causes either pro-social or selfish behaviour, and what causes these effects. In Hoffman et al. (1996), the effects of social distance are interpreted as rational economic effects, because with diminished anonymity the prospect arises of post-experimental interaction with either the experimenter, his collaborator(s), or other subjects. In our experiment it would have to be the prospect of interaction with the experimenter, since only the experimenter could work out which subject did what.<sup>25</sup> However, it is in our opinion relatively improbable that the experimenter would remember an individual subject's contribution *and* have future interaction with them outside the lab. Therefore a pure embarrassment or shame effect, operating independently of anticipated future payoffs, might be more plausible. We can relate as anecdotal evidence that suckered subjects seemed to suffer embarrassment on collecting their payoff.<sup>26</sup>

#### **4.iii Comparison with other experiments**

Results 3.i, 3.ii, 3.iv and 3.v concern hypotheses about public good provision tested for the first time in this experiment. The other results mirror findings from other experiments. Regarding 3.iii, we have already discussed evidence of crowding-in within groups from other experiments (see section 1).

Result vii is in the direction indicated by Shafir and Tversky (1992) or Bardsley (2000), who report that subjects are more cooperative in simultaneous play than when they already know that the other agents involved have cooperated. That is, although we cannot conduct a formal test, our data do not suggest the hypothesis that subjects give less in first position than when

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<sup>25</sup> We did not implement a double-blind procedure, but we maintained anonymity across subjects.

they already know that others have made very high contributions. This is puzzling in the same way as the aforementioned results; one would expect prudence to dictate lower contributions when subjects are uninformed than when they know that others have been extremely cooperative. For when they are uninformed and make a high contribution, they risk being suckered by the others.

Regarding 3.viii, there are many experiments which report decreasing contributions as a repeated game progresses. Isaac, Walker and Thomas (1984), Andreoni (1988) and Burlando and Hey (1997) are some examples. However, in that setting there are rival explanations: subjects may be learning to be selfishly rational, there may be interaction across tasks between egoists and reciprocators, or there may be strategic decay. Since our subjects only interact once, from these candidates only learning to be rational is possible in our experiment. Our data are consistent with the result of Bardsley (2000), which found a decay effect in a one-shot context (also using the CIL design), suggesting the hypothesis that subjects become more egoistically rational as they gain experience.

We conclude that our data appear to be entirely consistent with the results of comparable studies. This is encouraging for the CIL design.

#### **4.iv Economic Implications**

Knowledge about the existence of conformism is of considerable importance to economics. First, conformism can give rise to the complementarity of decisions regardless of any strategic considerations. We have already noted that this is of relevance to the question of why people contribute to charity (Andreoni 1998, Lucking-Reily and List 2001). Economists have wondered why voluntary contributions to charities do not exhibit strict crowding out, and the associated phenomena of neutrality with respect to the number of contributors and negligible provision. We have argued that reciprocity is out of place as an explanation here. Conformism is a better candidate explanation.<sup>27</sup>

Amongst the practical implications of a conformism effect are various policy spillovers. We adapt a hypothetical example from Manski (2000, p128): Consider an educational intervention

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<sup>26</sup> They were rather red in the face at this point.

<sup>27</sup> One should note, however, that conformism makes reference to non-conformist behaviour too; logically, not everyone can be a follower.

providing to improve the behaviour of the worse behaved pupils in a school but not the others. If individual disruptiveness increases with the average disruptiveness of the students in the school, then an effective intervention program not only improves the most difficult children's conduct but, as they become better behaved, indirectly improves other pupils' conduct, with a feedback to further progress by the selected students. Contextual interactions and correlated effects imply no such feedback.<sup>28</sup> Such an effect, if it existed, would presumably have wider scope than if it were driven by reciprocity only, in which case the secondary impact would be restricted to those children whose welfare is affected by the conduct of the worst behaved – those who are bullied by them or in the same classes, for example.

Real examples are readily available in the field of crime prevention. Consider, for example, currently fashionable “CPTED” schemes to reduce crime (CPTED stands for Crime Prevention Through Environmental Design). These programmes aim to shape public spaces to make crime less likely.<sup>29</sup> A key ingredient is the provision of better opportunities for surveillance by passers by and residents. Economists would naturally think of this as operating through a constraint modification, that is, by reducing opportunities for crime in the redesigned area. If that is the only mechanism at work, the schemes risk the displacement of crime into less well designed areas. Under conformism however, there may be knock-on effects operating by changing people's perceptions of normal behaviour. The schemes will be less prone to the displacement problem, and so more likely to reduce total, not just local, criminal activity.<sup>30</sup> If local concentration of crime is attributable to reciprocity, on the other hand, then the displacement problem for such schemes is presumably unabated, since all that changes for reciprocators is a local constraint factor. Any *motivation* to retaliate is unaffected.

In a similar vein, Kahan (1997) argues that crime prevention can be achieved through belief management. Police would be well advised to concentrate resources on hitting highly visible crime such as vandalism and public disorder offences, to create the impression of a low-crime environment. Kahan frames his analysis entirely in terms of conformism, as an effect on agents' conceptions of social norms, rather than reciprocity.

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<sup>28</sup> Contextual interactions occur when individual behaviour varies with exogenous characteristics of assembled group members, not their behaviour. Correlated effects are entirely attributable to individuals' characteristics. See Manski (2000) p127.

<sup>29</sup> For an empirical evaluation of CPTED schemes to reduce robberies see Casteel and Peek-Asa (2000).

<sup>30</sup> See Braga (2001) for related evidence that increased policing of crime “hot spots” may not result in crime displacement.

Another practical implication is one for optimal public housing composition. Simply put, under conformism people with “bad” socio-economic characteristics (including those associated with crime) can be expected to behave worse if one puts them all together than they would (severally) in isolation. For a culture of misbehaviour is likely to develop, with agents learning undesirable behaviour from each other. Even if conformism turns out to be a “weak” effect on the individual in single interactions such as that of our experiment, this might be all that is needed to bring about a substantial ghetto effect with extended interaction over time. The policy implication is that social housing ought to be dispersed, not concentrated. The effectiveness of this depends on the functional form of conformism being something other than a simple tendency to the *mean* of prior behaviours, but this is not difficult to imagine. If it is representative, normal, behaviour that exerts an effect, one would expect a tendency towards *median* behaviour to be more likely, for example. For, by definition, this is in general more representative of the distribution. Relocation of ghetto inhabitants would be effective in reducing total crime, in a comparative static sense, if they are dispersed enough not to affect perception of typical behaviour in the alternative, non-ghetto locations. Some empirical evidence that such relocation is actually effective in reducing crime is provided by a housing mobility experiment reported by Ludwig et al. (2001). This study found a correlation between relocation and juvenile arrest rates when households were given the opportunity to relocate.

It should be emphasised that conformism is a more general mechanism than reciprocity for bringing about endogenous social interaction effects, since it is not restricted to contexts in which the agents who observe each other affect each other’s welfare. In the social housing dispersion example, for instance, people can learn nicer behaviours without having to infer that these benefit them personally.

## **Conclusions**

We find evidence that reciprocity cannot explain all the crowding-in shown in our data. The influence of the other group’s contributions is in the direction suggested by conformism. We cannot really quantify the extent of conformism versus reciprocity in our data though, since it may be group-relative. The respective extent of these two factors as causes of crowding-in therefore remains an open question, and one that deserves to receive more attention in the future. Our experiment also uncovered another interesting effect, a suppression of contributions on the part of subjects placed in the position of examples to another group. We



interpret this as an aversion to being seen to be suckered by other subjects, that is, a desire to avoid embarrassment. Our data display no evidence of sensitivity to the minimum of others' contributions apart from in so far as this is correlated to their mean or median. The results we present cohere well with those from other comparable experiments, and are therefore promising for the CIL design.

We hope to encourage more debate both amongst experimentalists and economists generally about the interpretation of endogenous social interaction effects. There is much at stake here, since experimental economics is becoming increasingly influential as a check on the way the economics profession understands individual behaviour. It is of considerable importance for our understanding of these interaction effects whether they stem from reciprocity or conformism, since the former is a much less general mechanism. These effects in turn have implications for public policy, effects which are likely to be more limited in scope if they stem from reciprocity. This paper has taken on the reciprocity hypothesis on its home ground, the public good experiment, and found that it cannot account for all the complementarity between people's decisions. For all we know at present, conformism may be the more important effect even in laboratory games.

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## Appendix

### i. Task Set.

Situation	A: Yes/No	$X_i$						$Y$					
1*(LowLow)	No	2	0	2	1	0	_	0	0	3	0	1	0
2*(LowHigh)	No	1	2	0	0	1	_	10	10	7	9	9	8
3*(HighLow)	No	9	10	8	9	9	_	0	0	2	1	0	0
4*(HighHigh)	No	10	9	8	8	10	_	9	10	8	7	10	9
5	No	5	10	9	5	9	_	0	0	3	0	3	2
6	No	10	0	9	10	9	_	0	0	3	0	4	2
7	No	_	0	0	0	0	0	0	0	10	9	8	6
8	Yes	_	0	10	1	5	1	1	0	4	0	0	7
9	No	_	0	9	10	10	5	0	10	0	0	2	0
10	Yes	_	1	0	3	5	5	0	9	0	0	7	0
11	No	_	6	6	3	0	0	4	5	10	7	10	0
12	Yes	_	0	10	0	7	5	3	5	0	0	5	5
13	Yes	Real Task (no dummies; actual contributions shown)											

\* set up as test conditions for reciprocity versus conformism (see section 2.iii)

Notes: Numbers show fictional contributions. “\_” indicates a contribution to be made by the subject – hence the position of “\_” shows a subject’s position in the sequential game. The number of each situation is an arbitrary index and does *not* correspond to the order in which a subject faced the tasks (see main text). The “A: Yes/No column” indicates whether or not *A* subjects were told whether their contributions would be shown to *B* subjects.

## Appendix.

### ii. Regression Analysis of the Results.

Since the data are doubly censored (a subject cannot contribute either less than zero or more than their endowment), a two-limit tobit model is appropriate, with a lower limit of zero and an upper limit of ten tokens. The censoring details for each model show that censoring is a very significant feature of the data, which implies that models with unlimited dependent variables would be seriously biased. There are 900 left-censored observations and 391 right censored ones from a total of 2028 (=156 subjects x 13 tasks); more than half of the observations are censored. The Hausman test (conducted on uncensored models) shows the random effects model to be appropriate. We estimate one model for each position in the sequence, across both treatments (hence four estimations). Details are shown below.

	I	II	III	IV
Median own group	--	0.619 (0.086)	--	0.678 (0.111)
Median other group	--	--	0.093 (0.059)	0.161 (0.099)
Information Dummy	-0.612 (0.268)	--	--	--
Round	-0.160 (0.037)	-0.160 (0.085)	-0.082 (0.047)	-0.227 (0.103)
Observations (N*13 tasks)	533	481	533	481
Un-/left-/right- censored	321/118/94	179/232/70	248/164/121	152/223/106

*Notes:* Standard errors in parentheses. Constant suppressed. Real task included. N = 78 per treatment (total 156). "Med" denotes median.

I First Movers in A  
II Second Movers in A  
III First Movers in B  
IV Second Movers in B

Table 2: Estimation results: Two-Limit Tobit Regression, Random Effects Model.

## **Appendix.**

### **iii. Instructions**

Welcome. In this experiment, which investigates decision making, you will be placed in a number of situations. Only one of these will be real, the others will be fictional. An example of this procedure follows below.

Example: The experimenter, through this program, is about to make you an offer. The offer is one of three possibilities. One but only one of these is REAL. You have to decide whether to accept or reject each of the three possible offers, before you learn what the offer was. What happens then depends on how you responded to the real offer.

1. You will be paid 5 euros if you do a maths test and score above 60% AND take part in this experiment. make your choice. Make your choice: accept/reject.
2. You will be paid 4 euros if you fill out a questionnaire about your diet AND take part in this experiment. Make your choice: accept/reject.
3. You have been offered 7 euros. You do NOT have to do anything in return [other than participate in this experiment. Make your choice: accept/reject.

The offer was in fact number 3.

The 7 euros you have just accepted are your resources to be used in the experiment. They have been given as 10 tokens worth 0.7 euros each. You can walk away with more or less than this depending on how you and other subjects behave in the real situation, but you cannot leave with less than 3.5 euros. Below you are given more detailed instructions. Scroll through these and press the button at the end to indicate that you are ready.

The 13 situations you will confront are all set in the following context. There are two sets of six people, A, B, C, D, E, F (persons A-F) and G, H, I, J, K, L (persons G-L). Persons A-F are linked to each other but not to G-L. Similarly, G-L are linked to each other but not to A-F. You are in set G-L. Each person has 10 tokens [worth 0.7 euros each]. In each situation, each person must decide how to use their ten tokens. Each person will leave the experiment with a monetary reward. The size of this depends on what everyone with whom they are linked, themselves included, does with their tokens in the real situation.

There are two possible uses for each token: it can be taken by you personally or put into an account: account #2. To begin with there are no tokens in this account. Only people G-L can put money into it; persons A-F have an entirely separate account, account #1. The money in account #2 will be multiplied by 3 and split equally between agents G-L. The money in account #1 will be multiplied by 3 and split equally between agents A-F.

This means that each token taken by you leads to a reward to yourself of 0.7 euros but does not benefit anyone else, whilst each token put into account #2 results in a payment of approximately 0.35 euros to everyone in G-L including you, there being 6 people in this set:  $(0.7 \times 3)/6 = 0.35$  euros.

You decide how much of the 7 euros goes into account #2 by entering a number of 0.7 euro tokens [0-10] to go in. Any token you do not put into account #2 goes to you; not putting it into account #2 is the same as taking it directly. A table showing the amount of cash you will be paid based on how many tokens there are in account #2 has been placed by your computer for ease of reference.

### *Instructions for group A*

In each situation, you will be given the following details:

(i) numbers representing others' decisions

(ii) whether or not the decisions of A-F are shown to G-L before they make their decisions.

As in the example of the offer, only in one case does any of this correspond to anything real. In this real situation, both (i) and (ii) are genuine information, so, for example, the numbers will show the actual choices made by persons A-F. In the others, (i) and (ii) describe purely fictional situations, so nothing really happens, nothing you decide will actually be shown to anyone else and the numbers are made up by the experimenter. Note that because there is only one real situation it is not possible to learn about other people's behaviour as the experiment progresses.

Regarding (i), each situation shows five of the six people deciding at the same time what to do with their tokens. Then the sixth person makes their decision. So, if you are one of the five people in first position, you will decide without seeing others' choices, but you will be shown others' decisions after you have made yours. If you are placed in last position, you will decide after seeing the five other decisions from persons A-F. The letters A-F do NOT correspond to the order; any person may be placed in either position.

Regarding (ii), it is possible, in the real situation only, that the decisions made by A-F including yours, will be shown to agents G-L before they make their decisions.

After each situation has been played out, you will be shown how much money you will be given if that is the real one.

We would like you to treat each situation as if it is real and the only situation. Note that, for all you know, each one could be the real one, in which case ALL information you are given about it is accurate, and only the real one has any effect on the outcome. (Remember that for one of the offers in the example, all the information turned out to be accurate but the other offers were purely fictional. The experiment works in the same way.)



The section below shows you the display you will see during the experiment.

MainForm

situation **practice 1**

you are **F**

earnings **9.45**  
(only if this is the real situation)

It is not your turn. Enter the letter  
e to proceed :

e

Confirm

Wait for the next situation.

Tokens in account #1:  
8 3 5 3 4 0

Total in account #1 so far: 23

Account #1:  $23 * 0.5 = 11.5$

Tokens kept: 2

Earnings: 13.5 tokens = 9.45 euro  
(if this is the real situation)

Decisions of persons A-F are shown to persons G-L

This shows that 23 tokens were put into account #1 by persons A-F. When it is not your turn to make a decision you will be asked to enter a letter.

You will now face three practice situations to get used to the program. These are fictional situations and therefore none of them are paid.

*Instructions for group B:*

In each situation, you will be given the following details:

- (i) numbers representing decisions of persons A-F.
- (ii) numbers representing decisions of other persons in G-L.

As in the example of the offer, only in one case does any of this correspond to anything real. In this real situation, both (i) and (ii) are genuine information, so the numbers will show the actual choices made by persons A-F and the actual choices made by persons G-L. In the others, i and ii describe purely fictional situations, so nothing really happens, nothing you decide will actually be shown to anyone else and the numbers are made up by the experimenter. Note that because there is only one real situation it is not possible to learn about other people's behaviour as the experiment progresses.

Each situation shows five of the six people in A-F deciding at the same time what to do with their tokens. Then the sixth person makes their decision. Then, decisions from A-F are shown to G-L. Then the same process happens with persons G-L; 5 of the 6 decide first, followed by the last person. So, if you are one of the five people in first position, you will decide after seeing decisions from A-F but without seeing others' choices from G-L. In this case you will be shown others' decisions from G-L after you have made yours. If you are placed in last position, you will decide after seeing the five other decisions from persons G-L. Decisions by persons G-L are NOT shown to persons A-F. The letters G-L do NOT correspond to the order; any person may be placed in either position.

After each situation has been played out, you will be shown how much money you will be given if that is the real one.

We would like you to treat each situation as if it is real and the only situation. Note that, for all you know, each one could be the real one, in which case ALL information you are given about it is accurate, and only the real one has any effect on the outcome. (Remember that for one of the offers in the example, all the information turned out to be accurate but the other offers were purely fictional. The experiment works in the same way.)

The section below shows you the display you will see during the experiment.

MainForm

situation <b>practice 1</b> you are <b>G</b> earnings <b>9.10</b> (only if this is the real situation)	It is not your turn. Enter the letter v to proceed : <input type="text" value="v"/> <input type="button" value="Confirm"/>	Wait for the next situation.
---	---	------------------------------

Tokens in account #1: 0 5 7 3 0 2 Total in account #1 so far: 17	Tokens in account #2: 9 3 5 3 4 0 Total in account #2 so far: 24 Account #2: $24 * 0.5 = 12.0$ Tokens kept: 1 Earnings: 13.0 tokens = 9.10 euro (if this is the real situation)
--	---

This shows that 17 tokens were put into account #1 by persons A-F. 24 Tokens were put into account #2 by persons G-L. When it is not your turn to make a decision you will be asked to enter a letter. You will now face three practice situations to get used to the program. These are fictional situations and therefore none of them are paid.