

A SHARED SENSE OF RESPONSIBILITY: MONEY VERSUS EFFORT CONTRIBUTIONS IN
THE VOLUNTARY PROVISION OF PUBLIC GOODS*

JARED C. CARBONE[†]
ROBERT S. GAZZALE[‡]

APRIL 2017

Abstract

A frequently cited argument against the use of market-based instruments to provide public goods is that they diminish our sense of responsibility to be good citizens. We report on a laboratory experiment exploring whether the choice of some to contribute money in lieu effort affects the voluntary contributions of those who continue to provide effort. Subjects complete lab tasks as a contribution to a public good — carbon emission reductions. These effort contributions decrease as peers accept an offer to contribute money instead of effort. However, the aggregate result masks significant heterogeneity. Those who choose not to buy out despite its expected profitability have no response to the treatment, while those for whom it would not be profitable to buy out register large reductions in effort contributions. The magnitude of these responses increases in the share of the group accepting the buyout, suggesting that is the act of peers buying out—rather than the introduction of monetary incentives— that drives the effect.

JEL Classification: C90, C91, H41, Q54

Key words: experimental economics, public goods, effort contribution, environment, climate change

*We gratefully acknowledge support from the Norwegian Research Council as part of the NORKLIMA project. The views expressed here and any errors are our own.

[†]Division of Economics and Business, Colorado School of Mines, Golden, CO, 80401; The Ragnar Frisch Centre for Economic Research, Oslo, Norway; jcarbone@mines.edu. Carbone is associated with CREE — Oslo Centre for Research on Environmentally friendly Energy. CREE is supported by the Research Council of Norway.

[‡]Department of Economics, University of Toronto, 150 St. George Street, Toronto, ON M6G 1W2; and The Ragnar Frisch Centre for Economic Research, Oslo, Norway; robert.gazzale@utoronto.ca.

A SHARED SENSE OF RESPONSIBILITY: MONEY VERSUS EFFORT CONTRIBUTIONS IN THE VOLUNTARY PROVISION OF PUBLIC GOODS*

APRIL 2017

Abstract

A frequently cited argument against the use of market-based instruments to provide public goods is that they diminish our sense of responsibility to be good citizens. We report on a laboratory experiment exploring whether the choice of some to contribute money in lieu effort affects the voluntary contributions of those who continue to provide effort. Subjects complete lab tasks as a contribution to a public good — carbon emission reductions. These effort contributions decrease as peers accept an offer to contribute money instead of effort. However, the aggregate result masks significant heterogeneity. Those who choose not to buy out despite its expected profitability have no response to the treatment, while those for whom it would not be profitable to buy out register large reductions in effort contributions. The magnitude of these responses increases in the share of the group accepting the buyout, suggesting that is the act of peers buying out—rather than the introduction of monetary incentives— that drives the effect.

JEL Classification: C90, C91, H41, Q54

Key words: experimental economics, public goods, effort contribution, environment, climate change

1 INTRODUCTION

A frequently cited argument against the use of market-based instruments to provide public goods is that by diminishing our sense of responsibility to be good citizens, these mechanisms may ultimately reduce voluntary contributions. For example, many environmental interest groups oppose the use of emission permit trading systems (cap-and-trade) to control air pollution despite their cost effectiveness (Heal 2007). Two related but distinct objections to market-based instruments have been noted. First, it has been argued that by changing pollution from a sin against nature, remedied only by personal atonement, into a commodity that can be bought and sold with no special social consequences, market-based instruments may weaken our resolve in dealing with environmental challenges. A second aspect which may reduce the shared sense of responsibility and sacrifice is the fact that these market-based mechanisms allow some to contribute money in lieu of effort. If some choosing to contribute money instead of effort causes others to reduce their voluntary effort contributions, it presents a challenge for policymakers as the market's efficiency gains arise from those with a high opportunity cost of direct effort choosing monetary contributions instead.

In this paper, we focus on this second objection to market-based instruments. We report on results of a laboratory experiment designed to explore the hypothesis that an individual's voluntary effort contributions to the public good is affected by the contribution vehicle — personal effort versus money — used by her peers.

*We gratefully acknowledge support from the Norwegian Research Council as part of the NORKLIMA project. The views expressed here and any errors are our own.

This hypothesis has a long history in political philosophy (Rousseau 1762), and has more recently been articulated in the context of environmental policy by Sandel (2012). Circumstantial evidence does suggest that individuals differentiate between money and effort contributions to the public good, even when they ultimately result in the same level of overall provision. For example, environmental activist Al Gore was widely criticized when a think tank revealed that his personal estate required twenty times as much electricity as a typical household. Gore declared his lifestyle carbon neutral because he purchased offsets to his polluting activities, but many viewed this gesture as insufficient. According to the president of the think tank, “If he [Gore] is going to be a spokesman for global warming, he has to be willing to make the same sacrifices [as those unable to afford offsets]” (Humphrey 2007). The clear implication was that Gore would be a more sympathetic advocate if he reduced the size of his carbon footprint by lifestyle changes rather than by financial transaction.¹

Whether some contributing money in lieu of effort translates into reduced contributions from those who continue to supply effort is an open question. While we are aware of no previous study that tests for a causal effect, previous results lend support to the conjecture. For example, previous experiments have documented conditional cooperation in the provision of public goods, whereby a subject’s voluntary contribution to the public good is increasing in the contribution of others (Fischbacher, Gächter and Fehr 2001). If individuals effectively discount monetary contributions relative to equivalent effort contributions, then observing reduced effort contributions when peers contribute money could be interpreted as a form of conditional cooperation. The previous studies do not, however, focus on this distinction between money and effort contributions. In other contexts, experiments have examined the general issue of whether people differentiate between money and equivalent time or effort, finding mixed results (Brüggen and Strobel 2007, Ellingsen and Johannesson 2009, Vilares, Dam and Kording 2011). None of these studies focuses on public goods provision. Finally, there is an extensive literature in economics and psychology focused on measuring the crowding out of intrinsic motivation when explicit rewards (markets or monetary incentives) for performance are introduced (e.g., Gneezy and Rustichini 2000, Frey and Jegen 2001). None of the studies examines how the behavior of peers — specifically their decision to buy their way out of making direct contributions to public goods — affects a person’s motivation.

To test whether voluntary effort contributions are affected by the contribution vehicle chosen by peers, the institution we study is a buyout option. In an environment where participation, but not effort, is enforceable, we allow individuals to contribute money in lieu of effort. In our experiment, subjects complete simple lab tasks. In some rounds, task performance is rewarded with carbon-emission reductions (a public good), where more tasks completed leads to larger reductions. That is, we ask subjects to make effort contributions to the public good. In other rounds, task performance is rewarded with a cash payment, where subjects receive a piece rate per

¹Similarly, the Mid-Atlantic Regional Council of Carpenters came under fire for outsourcing shifts on their picket lines (Alexander 2007). Food cooperatives are another example. The oldest and largest in the U.S. requires each member to contribute 2.75 hours every four weeks, resulting in “a feeling of being a member-owner that one cannot get from merely investing one’s money.”(<http://foodcoop.com/go.php?id=40>)

task completed. In our main treatment, we introduce the possibility of buyouts in later emission-reduction rounds: subjects can pay a fee (used to purchase emission reductions) that relieves them of the responsibility to complete tasks for emission reductions and allows them to continue to work for cash payments. We test for the effect of peer buyouts by comparing the number of tasks completed by treatment-session subjects who do not buy out to the number of tasks completed by subjects in control sessions in which no buyout option was available.

Our hypothesis is that peers *choosing* to contribute money in lieu of effort leads to a reduction in the effort contributions of those who continue to contribute effort. To test this hypothesis, it is necessary to have a laboratory environment where: i) subjects have the choice whether to buy out; ii) within a group, there are both subjects who do and do not buy out; and iii) the number accepting the buyout varies across groups. To capture the spirit of the efficiency objectives of market-based mechanisms, we seek a setting where those with the highest opportunity cost of effort find the buyout most attractive. We achieve these goals in our fair-buyout treatment: we offered the buyout to all at a price that both replaces the average effort and is likely to be acceptable only for those with the highest outside-option earnings.

When choice determines who buys out, however, those who do not buy out (and thus the group to which we compare control subjects) is not a random sample. If those accepting the buyout would have been those with the highest effort contributions, then a reduction in average effort contributions, relative to the control group, could be explained by composition effects. While we present evidence that the effect identified in this treatment is not due to selection, we also conduct a second treatment where a profitable buyout option is only offered to a random subset of subjects. While perhaps less realistic, the comparison between treatment subjects not offered the buyout and control subjects is unaffected by selection. However, subjects reducing effort contributions in this case might be reacting to a perceived unfairness in not being offered a profitable buyout opportunity.

In both treatments, peers electing to buy out reduces the number of emission-reducing tasks completed by subjects who do not buy out (relative to the number of tasks completed by control subjects). However, the aggregate result masks significant heterogeneity across individual responses. Those who do not accept the fair buyout despite its expected profitability have no response to the treatment while those for whom it would not be profitable to buy out register large reductions in task performance. Further, the effect depends on the act of peers buying out as opposed to just the introduction of the buyout option. We find a 5-10% reduction in emission-reducing tasks completed for every 10 percentage-point increase in subjects who buy out (if the response is assumed to be linear). Our two treatments yield very similar responses, suggesting that neither selection nor the perceived unfairness of random selection are likely to explain our results.

Climate change policy motivates the main ideas in the paper and serves as the context of our experiment. It is easy to imagine their relevance in other contexts however. If our hypothesis is correct, then people may experience important effects on their intrinsic motivation in any setting where voluntary service is a mainstay of public goods provision. For example, many of the basic

functions of local communities rely intensively on donations of time and effort from their members to community centers, schools and town councils. Our results suggest that market-based initiatives designed to bolster contributions to these activities (or allow them to be made as efficiently as possible) may have hidden costs in the form of reduced voluntary effort contributions. Our results may also help explain why many groups and organizations expressly prohibit their members from replacing effort obligations with cash contributions.

Section 2 describes the design of the laboratory experiment and Section 3 describes the results as well as robustness checks. Section 4 discusses some of the broader implications of our findings and offers suggestions for future research.

2 EXPERIMENT DESIGN

In this section we present our experiment design. We first detail the control sessions where no buyout option is available. We then describe the treatment in which all subjects are offered the option to buy out (**fair buyout**), followed by the treatment where the buyout option was randomly assigned (**unfair buyout**). Finally, we discuss possible confounding effects and specific aspects of the experiment designed to address these issues.

Subjects were undergraduates at Williams College.² We conducted four sessions per condition, totalling 61 control subjects, 64 fair-buyout and 61 unfair-buyout subjects. Each session had 16 subjects, except one unfair buyout (13 subjects) and two control sessions (14 and 15 subjects). Subjects earned \$16.50 on average, which includes a \$5 show-up fee. Sessions lasted approximately 60–75 minutes. The experiment was programmed and conducted with z-Tree (Fischbacher 2007).

2.1 Control Session Procedures

At the start of a session, each subject was randomly assigned a computer, implicitly assigning her to one of two equally sized groups (plus or minus one). Each subject received a written copy of the instructions (section A in the online supplement) which were subsequently read aloud. At the end of the session, subjects completed a brief demographic questionnaire.

A session proceeded in a series of five-minute rounds in which subjects had the opportunity to complete computer-based tasks. In **earning rounds**, a subject earned a piece rate for each task completed. In **environment rounds**, we presented subjects with a different task. As a reward for performance in these rounds, we purchased and retired carbon emission permits from the EU Emission Trading Scheme (EU ETS) in proportion to the number of tasks completed.³

At the beginning of each round, subjects learned whether the round was an environment or

²Subjects were recruited through the online recruitment system ORSEE (Greiner 2015).

³See http://ec.europa.eu/clima/policies/ets/index_en.htm for information on the EU ETS. That we implement public good contributions by making purchases on a market contains some obvious irony. Importantly, any objection to making pollution control a commodity that can be bought and sold should register in both the control and treatments. The difference between the control and treatments is whether a peer's contribution stems from her effort or her money, and the interaction between the treatment and the background permit market is not obvious. While the presence of the market-based instrument may make buyout behavior more salient, the fact that all contributions eventually involve money could hinder finding a significant treatment effect.

an earning round. Subjects knew neither the total number of rounds nor how many of each type of round. In fact, all sessions lasted five rounds: rounds 1, 3 and 5 were environment rounds and rounds 2 and 4 were earning rounds. At the end of each earning round, the subject's screen displayed the number of tasks she completed as well as her earnings in dollars for the round. At the end of each environment round, her screen displayed the number of tasks both she and her *group* completed as well as the number of tons of carbon emissions both she and her group prevented. At all points in the session, a subject had access to all previously revealed information.

We used two tasks: an **encryption task** and a **counting task**. Subjects completed one task in earning rounds and the other in environment rounds, with the assignment of tasks varied between sessions. For the encryption task, the subject's computer screen displayed a table translating each letter of the alphabet into a unique two-digit number.⁴ For each task, the subject's screen displayed two letters. The subject successfully completed the task by translating the letters into their numeric equivalent and entering the correct four numbers. For each counting task, a subject's screen displayed a table with four rows and four columns, with each cell containing a randomly generated "0" or "1". The subject successfully completed the task by correctly entering the total number of zeros in the table. For both task types, the subject received the next task only after completing the current task.

The subject received ten cents per completed encryption task in earning rounds and we retired one permit per 150 completed tasks when we presented encryption tasks in environment rounds. The subject received 7.5 cents per completed counting task in earning rounds and we retired one permit per 200 completed tasks when we presented counting tasks in environment rounds.⁵ Prior to the first round, subjects participated in two one-minute practice rounds: one for each of the tasks.

We now highlight some additional aspects of the experiment environment. First, in addition to the experiment interface, a subject's computer screen displayed an internet browser. We included the browser to establish an opportunity cost of completing the tasks. Subject instructions noted the existence of the internet browser and made it clear — in neutral language — that they were free to use it during the experiment.

The instructions also included basic information about the link between carbon emissions and global warming, and described how we would purchase permits. They also explained how subjects could verify the permit purchases after the session.

While we informed each subject about the number of environment tasks completed by members of her group, we provided no information about the other group. Likewise, in the treatment sessions we describe next, a subject learns how many group members accepted the buyout, but learns nothing about the other group. We split each session into two fixed groups for two reasons. First, even weak group identity may make the actions of others more salient (e.g., Chen and Li 2009). Establishing group membership may make the information we provided about peers' performance and choices more important. Second, a subject did not know which individuals in

⁴We randomly generated one encryption key each round for all subjects.

⁵Pilot-session subjects completed approximately 25% fewer encryption tasks per round than counting tasks.

the room were in her group. It was our hope that this uncertainty would make a neighbor's audible keyboard effort less relevant, reducing the likelihood that a subject conditioned her effort on her neighbor's current actions, as opposed to the buyout option and the number of group members accepting the buyout.

2.2 Treatment Sessions — Fair Buyout

Treatment sessions proceeded in the same manner as control sessions with the difference that we surprised subjects with a buyout option at the start of the second and third environment rounds (rounds 3 and 5). The buyout option enabled a subject to pay a buyout fee and then complete earning tasks for money instead of environment tasks to retire emission permits. Importantly, the buyout fees were used to purchase as many carbon permits as possible. The round proceeded as a normal environment round for those subjects not accepting the buyout.⁶

In the fair-buyout treatment, we informed subjects that the buyout option was available to all in the group on the same terms. In specifying the terms of the buyout, we had two goals. First, we wanted emission permits purchased with the buyout fees to approximately replace the contributions those accepting the buyout would have made had they instead performed environment tasks. We thus set the buyout fee to exactly purchase the average number of permits produced by the group's subjects in the first environment round. Second, we wanted to make sure that some, but not all, subjects accepted the buyout. We accomplished this goal by strategically setting the piece rate on earning tasks for those accepting the buyout. For each group, we calculated the median number of tasks completed in the previous earning round, and set the piece rate so that earnings from this median number of tasks exactly equaled the buyout fee. While we informed subjects that the buyout fee exactly replaced the group's average round-1 permit production, we only told them the piece rate they would receive upon acceptance of the buyout without indicating how we calculated the rate.

Subjects simultaneously chose whether to accept the buyout option at the beginning of the round. When making this decision, each subject's monitor displayed information about the expected profitability of accepting the buyout. That is, based on the piece rate established for the round, we calculated the amount of money the subject would earn if she replicated her performance from the previous earning round. We then subtracted the buyout fee from this number to estimate the net monetary impact of accepting the buyout. In addition, the subject received information about the expected effect of her buyout on the number of carbon permits retired. Her screen displayed the exact number of permits that would be purchased with her buyout fee, as well as the number of permits that were retired as a direct result of her performance on the environment tasks in round 1. The difference of these two numbers gave an estimate of the net effect of accepting the buyout on emissions reductions.

After all subjects decided whether to accept the buyout option, each learned the number of

⁶We used the term "buyout" in the experiment as it is the most straightforward way to explain the option—a monetary payment relieving one of a previous obligation. Despite its many neutral uses (e.g., finance, employment and real estate), some may find it to have negative connotations. We do note that when the underlying context is clear, more neutral wording does may not impact behavior (e.g., Abbink and Hennig-Schmidt (2006) in the context of bribery).

group members accepting the option.⁷ Subjects also learned the expected direct impact of the buyouts on group contributions. We first calculated the number of carbon permits purchased with buyout fees collected in the round. Next, for each buyout subject, we took the number of permits retired as a direct result of his performance on environment tasks in round 1, and summed this across all who accepted the buyout. We then reported to all group members the difference between this sum and permits purchased with the buyout fees.

2.3 Treatment Sessions — Unfair Buyout

At the start of round 3 in the unfair-buyout treatment, we informed all subjects that one half of each group — selected at random — would be offered a buyout option for that round. In round 5, we informed all subjects that the same subjects would receive the buyout option. They learned that if a subject accepted the buyout, she completed earning tasks and paid a fee equal to one-half her earnings in that round. It was thus common knowledge that all subjects receiving the buyout option would find it profitable to accept. Before choosing, each subject receiving the buyout option was informed of the expected profitability of accepting the offer using the same calculation as in the fair-buyout treatment, and received an estimate of the impact of her buyout on the number of permits retired. As a consequence of the design for the buyout fee in this treatment, these fees were not sufficient to replace effort contributions (unlike the fair buyout), and subjects had sufficient information to infer this. Subjects knew the number of group members offered the buyout and, prior to completing tasks, learned how many group members accepted the buyout.

2.4 Discussion

We have taken a number of steps to rule out alternative explanations for our findings. First, we have ensured that neither the marginal cost nor benefit of an individual's effort depend directly on the behavior of others in the experiments. There are no effort or contribution complementarities in producing the group's emission reduction, and we have chosen a public good (abatement of carbon emissions) whose value, given the scale of the problem, is independent of the amount contributed by other subjects. Moreover, even if a subject cares about emissions abated by other subjects (as opposed to worldwide emissions), the fee in the fair-buyout treatment ensured a level of group abatement largely neutral to the number of buyouts accepted.

Second, a subject for whom buying out is not profitable may correctly infer that she is less adept at the earning task than some of her peers in our fair-buyout treatment. This is information she would not have received absent the buyout opportunity. If it is the receipt of this information that causes a reduction in effort, this would be a different causal mechanism than the one we have put forward. Therefore, we control for this type of "bad" news in our regression analysis.

Third, the fair-buyout treatment is vulnerable to selection effects as discussed. We take two approaches to address this concern. First, the unfair-buyout treatment is not subject to the same criticism because subjects are chosen randomly to receive the buyout offer. Second, in the fair-

⁷Subjects were given no information about the decisions or performance of session participants outside of their own group.

buyout treatment, we identify subject characteristics which *ex post* predict buyout acceptance and test whether these characteristics are correlated with improvements in environment-task performance in the control sessions.

3 RESULTS

We start by establishing successful randomization across treatments. In Table 7 in section B of the online supplement, we report by treatment the means and standard deviations for a number of standard demographic variables. We also report p -values for tests of differences across control and treatments. We find no significant differences. To summarize, approximately half of subjects were male and half were Caucasian. They were, on average, in the second-year of university studies with approximately two economics courses, and reported attending religious services less than once per month. On average, a subject had fewer than one friend in the same session.

We analyze the results as follows. We begin with the fair-buyout treatment. In Section 3.1, we show that the treatment successfully induced buyouts, particularly amongst those who would profit from accepting the offered buyout. We then establish (Section 3.2) our main treatment effect: the introduction of a buyout opportunity reduces effort contributions, particularly among those who would have likely lost money by accepting the buyout. In section 3.3, we show that it is peers *choosing* to buy out (as opposed to the simple introduction of the buyout option) that drives the effect, and show that we do not find support in the data for alternative explanations. Finally, we address the issue of selection. We do not find evidence that high-performing control subjects would have been the most likely to accept the buyout if offered (Section 3.4). We also find a large response to the unfair-buyout treatment, where selection effects are ruled out by random assignment to the buyout option (Section 3.5).

Our key outcome measure is δ_i : for subject i , tasks completed for the environment in round 5 minus the number completed in round 1. By focusing on the change in tasks completed, we control for any idiosyncratic differences across treatments in initial task performance. We define this measure for all control-session subjects, and for all treatment-session subjects who do not buyout in round 5. To compare different subject populations, we calculate $\bar{\delta}$, the average δ_i across subjects in the population. Because we cannot reject the hypothesis that the distribution of δ_i (the *change* in environment tasks) is independent of which task was completed for the environment ($p = 0.126$), we group observations from both tasks together.

Finally, while each treatment had two buyout rounds — rounds 3 and 5 — we focus primarily on round 5. When offered for the second time, subjects have had a chance to process the lessons of the first buyout round, and we thus expect a stronger treatment effect in round 5. In fact, the data bear this out. We find no statistically significant effect to either treatment in round 3, but a strong effect in round 5 of both treatments. We discuss in greater detail why round-3 and round-5 behavior might have differed in Section 4.

3.1 Buyouts in the Fair-Buyout Treatment

Table 1 describes the pattern of buyouts in rounds 3 and 5 of the fair-buyout treatment. For each 8-subject group, the second column identifies the task subjects performed to earn permits. The next two columns indicate the number accepting the buyout in each buyout round. In the final column, we calculate **Expected Permit Change** for the last buyout round. This is the estimate, provided to subjects, of the direct effect of buyouts for each group: the number of permits purchased with buyout fees minus the number of permits produced in the first environment round by subjects accepting the buyout.

Group	Environment Task	Buyouts		Expected Permit Change from
		Round 3	Round 5	Round-5 Buyouts
1	Encryption	4	3	0.13
2	Encryption	2	1	-0.04
3	Encryption	2	1	0.02
4	Encryption	4	3	-0.05
5	Counting	2	0	0.00
6	Counting	2	2	-0.13
7	Counting	3	3	0.10
8	Counting	2	1	-0.08

Table 1: Fair-Buyout Treatment: Number of subjects (out of eight) accepting buyout offer and expected change in permit provision by those accepting buyout, per group.

The typical group registered more than one buyout per round with buyouts ranging between zero and four subjects and slightly fewer subjects buying out in round 5 than in round 3. There is no discernible relationship between the number of buyouts and the environment-round task (encryption or counting). Finally, as the average subject retired approximately one-third of a permit in round 1, the difference between what buyout subjects provided by completing round-1 environment tasks and what their fees purchased is relatively small. In summary, we successfully induced variation in buyouts across groups while largely replacing the permits those accepting the buyout might have been expected to provide.

In Figure 1, we use a scatter plot to depict the relationship between round-5 buyout acceptance and task performance.⁸ On the horizontal axis, we have a subject’s **Earning Task Index**: the ratio round-4 tasks completed by a subject (the last earning round in the experiment) to the median number of tasks completed in that round by members of her group. A subject whose index number is greater than one will profit from accepting the round-5 buyout as long as she completes as many earnings tasks in round 5 as she did in round 4.⁹ In what follows, we refer to those subjects for whom this condition is satisfied (not satisfied) as “profitable” (“unprofitable”) subjects. On the vertical axis, we have a subject’s **Environment Task Index**: the ratio of the number of tasks a

⁸Performing the same analysis for round-3 buyout status yields very similar results.

⁹With an average Round-5 buyout price of \$4.93, the subject completing 10% fewer Round-4 tasks than the median (Earning Task Index=0.9) would lose about \$0.50 if she accept the buyout and replicated her Round-4 performance.

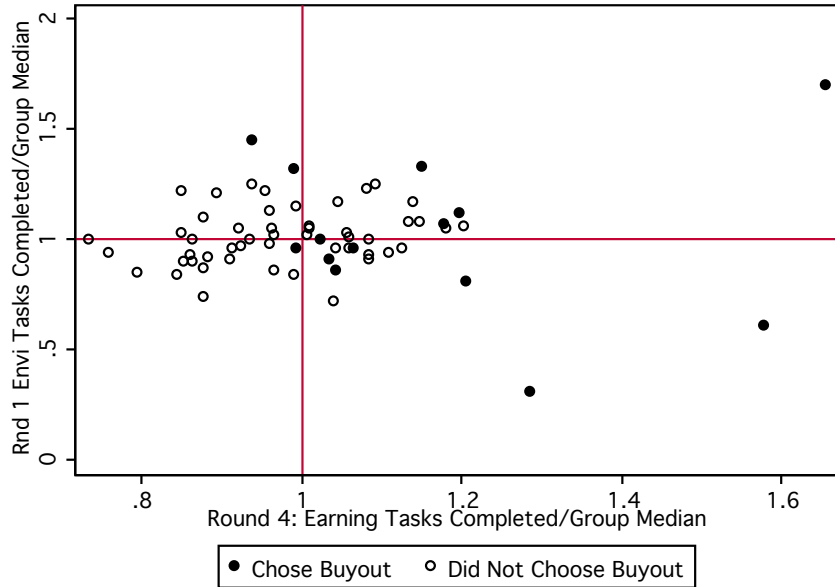


Figure 1: Fair-Buyout Treatment: Relationship between round-5 buyout acceptance in fair-buyout treatment and relative performance on round-1 environment task and round-4 earnings task.

subject completed in round 1 to the median number of tasks completed in that round by members of her group.¹⁰

We see that buyout acceptance is strongly correlated with its expected profitability. Subjects with an earning-task index above one are much more likely to accept the buyout option, although many rejected a buyout expected to be profitable. In contrast, it is difficult to discern any relationship between environment-task performance and buyout acceptance. In particular, while some may have rejected a profitable buyout because its acceptance would have reduced their public goods contribution, this does not appear to be a general result. Finally, it is worth noting that there is not a strong association between environment and earning task performance in the data.

3.2 The Effect of the Fair-Buyout Treatment on Effort Contributions

For our first treatment-effect measure, we calculate for each subject in round 5 who does not buyout (control subjects and treatment subjects not accepting the buyout) the percent change in tasks completed from round 1 to round 5 ($\delta_i/\text{Tasks}_{1i}$). In Figure 2, we depict, for the control and each treatment, cumulative distribution functions of this calculation.

We see nearly uniform continued effort contributions in control sessions. In both control and fair-buyout sessions, a significant proportion of subjects complete more tasks in round 5 than in round 1, a pattern consistent with the combination of continued effort and learning.¹¹ 33% of control subjects and 26% of non-buyout subjects in the fair-buyout treatment increased tasks

¹⁰The average subject retired .33 permits in the first environment round, meaning that the subject who completed 10% more Round-1 tasks than the median (Earning-Task Index=1.10) would have reduced her contribution by approximately .03 tons if she accepted the Round-5 buyout instead of replicating her Round-1 environment-task performance.

¹¹For example, we observed subjects experimenting with keyboard and mouse placement.

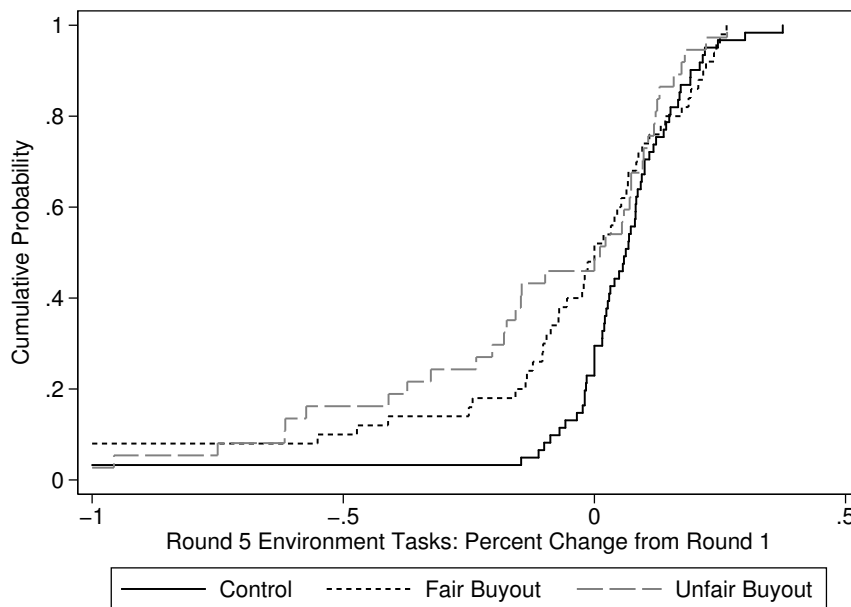


Figure 2: CDFs of percent change in environment tasks completed from round 1 to round 5 for control, fair-buyout and unfair-buyout treatment subjects. (Treatment series exclude subjects accepting round-5 buyout.)

completed by more than 10%. However, the fraction of subjects completing fewer tasks in round 5 is markedly larger in the fair-buyout treatment than in the control (48% versus 23%).

Our next measure is the average change from round 1 to round 5 in environment tasks completed. In Table 2, the rows identify the population: **All Subjects**; subjects for whom accepting the buyout would have been profitable (**Profitable Subjects**); and subjects for whom it would not have been (**Unprofitable Subjects**). The first two columns identify the treatment and control groups, with each cell identifying the average performance change for the subpopulation ($\bar{\delta}$) and number of observations (n). The third column reports the p -value of the Wilcoxon rank-sum test of equal distributions. In the final column, we provide a back-of-the-envelope calculation of the economic size of the treatment by estimating the change in round-5 tasks completed—relative to round-1 tasks completed—if the control group had the treatment’s $\bar{\delta}$ instead of its own.¹²

Formal analysis of treatment-wide effects echoes the graphical analysis in Figure 2. First, control subjects completed slightly more tasks in round 5 than in round 1 (Wilcoxon signed-rank test, $p = 0.002$). Second, subjects in the fair-buyout treatment who did not accept the buyout completed fewer tasks relative to round 1. The difference between control and fair-buyout subjects not accepting the buyout is statistically significant ($p = 0.042$). We estimate that had we offered the fair buyout to control subjects, subjects not accepting the buyout would have completed 8.8% fewer tasks (assuming acceptance uncorrelated with δ_i). In terms of total permits, including those from buyout fees, whereas the average control group increased permits retired by 2.4%, the average

¹²We take the difference in $\bar{\delta}$ s and divide by the average number of tasks completed in round-1 of the control sessions.

	Control	Fair Buyout	p -value	Difference as % of Round-1 Control
All Subjects	$\bar{\delta} = 1.8$ $n = 61$	$\bar{\delta} = -3.5$ $n = 50$	0.042	-8.8
Unprofitable Subjects	$\bar{\delta} = 0.2$ $n = 29$	$\bar{\delta} = -7.9$ $n = 29$	0.002	-15.1
Profitable Subjects	$\bar{\delta} = 3.2$ $n = 32$	$\bar{\delta} = 2.6$ $n = 21$	0.387	-0.9

Table 2: Mean Changes In Environment Tasks Completed by Treatment and Expected Profit: All control subjects plus fair-buyout subjects who do not buy out. A profitable subject completed more tasks in round-4 than her group’s median, and thus would have been expected a positive payoff from accepting a buyout. p -values for the Wilcoxon rank-sum test, testing the hypothesis that samples are drawn from the same distribution.

$$\delta_i = \text{Tasks}_{5i} - \text{Tasks}_{1i}, i \in \text{subjects who do not accept buyout. } \bar{\delta} = \sum_i \delta_i/n.$$

buyout group saw a decrease in total permits of 5.2%.¹³

The treatment averages mask the significant heterogeneity shown in Figure 2. For example, in terms of percent change from round 1, the top third of the control and fair-buyout distributions are nearly indistinguishable. In the bottom two comparisons of Table 2, we distinguish between those who would and would not have expected the buyout to be profitable based on round-4 earning tasks completed. We construct these groups *ex post* for control subjects using the same rule: a subject completing more round-4 task than her group’s median is designated profitable.

On average, unprofitable subjects in the fair-buyout sessions completed fewer tasks in round 5 relative to round 1, whereas the corresponding subjects in control sessions completed approximately the same number of tasks in the two rounds. This difference is both statistically significant and large in an economic sense. We estimate that had we offered the buyout in control sessions, unprofitable subjects would have completed approximately 15.1% fewer round-5 environment tasks. While the increase in tasks completed is slightly smaller for profitable subjects in fair-buyout sessions than for the corresponding control subjects, we cannot reject the hypothesis that this difference is due to chance. Thus, the aggregate results presented in the first row mask substantial heterogeneity across individuals, as the treatment effect appears to be isolated amongst those for whom the buyout did not make economic sense to accept.

3.3 Decomposing the Causal Mechanism for the Fair-Buyout Treatment Effect

We now present the results of a regression analysis designed to investigate the cause of our treatment effect. For treatment subjects who likely would not have profited from accepting the buyout, we find a strong negative correlation between the number of peers who buy out and improvements in environment task performance. This is consistent with our preferred explanation that

¹³We have 8 treatment and 8 control groups. A Monte Carlo permutation test shows that if we randomly select 8 of these 16 observations, only 3.1% of the time do we get an average reduction of at least 5.2%.

peers choosing to contribute money instead of effort reduces the contributions of those who continue to contribute effort. We do not find support for the competing hypotheses described in 2.4.

In all models, the dependent variable is δ_i , the change from round 1 to 5 in tasks completed for the environment. We have one observation from each of the 61 control subjects and the 50 subjects who do not accept the buyout in the fair-buyout treatment. We estimate all models using generalized least squares (GLS) with random effects at the group level.

One factor that will influence the change in environment tasks completed across rounds is task learning. In fact, a number of subjects completed more tasks in round 5 than in round 1. All else equal (including effort, ability and tasks completed in round 5) the subject who learned task performance quickly (e.g., in the practice round) will complete more round-1 tasks than the subject who learned more slowly (e.g., in round 3) and will thus have a smaller increase in tasks completed. Therefore, to control for learning independent of any treatment effect, in all specifications we include **Round 1 Tasks Completed**.

We present our results in Table 3. Our base model (specification (1)) includes controls for the pattern of buyouts and whether the subject would have found the buyout profitable. In the former category, we have **Buyout Session**, an indicator variable equal to 1 for subjects participating in a buyout session, and **Buyout Rate**, the fraction of group members accepting the buyout. To control for expected buyout profitability, we include **Expected Profitability** >0 , an indicator variable equal to 1 if the number of tasks completed in the preceding earning round was greater than the group's median and 0 otherwise.¹⁴ Because this variable is defined for both control and treatment subjects, it captures the effect of high earnings independent of any buyout-option effect. We therefore interact **Expected Profitability** >0 with both **Buyout Session** and **Buyout Rate** to isolate the effect of the buyout on profitable subjects. With this specification, **Buyout Session** and **Buyout Rate** capture the treatment effect on unprofitable subjects.

The coefficients we estimate for our base model suggest that the treatment effect depends on the number of peers accepting the offer. Because we control for the buyout rate, the **Buyout Session** indicator variable captures the effect of the buyout offer on subjects if no subject in the group accepts the buyout. We find no effect: the coefficient on **Buyout Session** is very imprecisely estimated and we cannot, therefore, reject the null hypothesis of no association.¹⁵ We do find a strong effect from the buyout rate. Looking at the subjects who expected the offered buyout to be unprofitable, we estimate an approximately 5.5% reduction in tasks completed for every 10 percentage-point increase in the percentage of subjects who buy out (assuming a linear response). The equal-in-magnitude but positive coefficient on **Buyout Rate X Expected Profitability** >0 is consistent with finding no treatment effect for profitable subjects.

In models (2) – (4), we control for alternative explanations. In model (2), we investigate whether subjects who did not accept an offered buyout reacted to buyout-induced changes in aggregate contributions to the public good. We include the difference between the number of per-

¹⁴Estimation results do not qualitatively change when we use continuous measures of expected buyout profitability.

¹⁵In the one buyout group where nobody accepted the round-5 buyout, only 1 out of 8 subjects completed fewer tasks in round 5 than in round 1, and this subject's decrease was only 1% of round-1 tasks.

	(1)	(2)	(3)	(4)
Round 1 Tasks Completed	0.0681 (0.124)	0.0687 (0.125)	0.0439 (0.140)	0.0747 (0.125)
Buyout Session	3.378 (5.356)	4.275 (5.512)	3.389 (5.378)	1.336 (6.177)
Buyout Rate	-0.551*** (0.189)	-0.582*** (0.195)	-0.550*** (0.190)	-0.593*** (0.200)
Expected Profitability>0	2.364 (3.840)	2.356 (3.849)	2.669 (3.939)	2.286 (3.852)
Buyout Session X Expected Profitability>0	-5.161 (7.845)	-5.094 (7.863)	-5.605 (7.962)	-3.074 (8.462)
Buyout Rate X Expected Profitability>0	0.636** (0.299)	0.624** (0.300)	0.643** (0.301)	0.679** (0.307)
Expected Permit Change		19.21 (26.83)		
Change Group Permits 1 to 3			4.894 (12.85)	
Buyout Loss				5.558 (8.306)
Constant	-3.441 (7.178)	-3.476 (7.195)	-2.333 (7.773)	-3.801 (7.217)
Observations	111	111	111	111

Table 3: Fair-Buyout Regression Analysis: Random-effects GLS models of $\delta_i = \text{Tasks}_{5i} - \text{Tasks}_{1i}$, $i \in$ subjects who do not buyout. Standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Independent Variables: **Buyout Session** = 1 if buyout offered this session, else 0; **Buyout Rate** = percentage of group accepting buyout this round; **Expected Profitability>0** = 1 if subject completed more round-4 Earning Tasks than group median, else 0; **Expected Permit Change** = expected change in the aggregate permits retired this round based on round 1 performance and buyout pattern; **Change Group Permits 1 to 3** = change from round 1 to 3 in permits provided by others in same group; **Buyout Loss**: If buyout offered and $E[\text{Profit}] \leq 0$, then equals $-E[\text{Prof}]$, else 0; Interactions indicated with X.

mits purchased with buyout fees and the number of permits produced in round 1 by those who bought out in round 5 (**Expected Permit Change**).¹⁶ In model (3) we explore whether subjects conditioned their round-5 effort contribution on actual round-3 contributions of her peers — a form of conditional cooperation. We include a variable capturing the difference between round-1 and round-3 permits provided by a subject’s peers (**Change Group Permits 1 to 3**), where round-3 permits include those provided by buyout fees. In both cases, we cannot reject the hypothesis

¹⁶Expected Permit Change is set equal to 0 for control subjects.

that the coefficient on the added variable is zero, and including the variables do not qualitatively change the coefficients we estimate in model (1).

Finally, a fair-buyout subject who would lose money accepting a buyout attractive to others receives negative information about her relative abilities in the earning task, potentially reducing motivation in the buyout round. However, we do not estimate a significant coefficient for the variable capturing the fact that a fair-buyout subject's earnings are worse than average (**Buyout Session X Expected Profitability** >0) in specifications (1)–(3). We also control for the information conveyed by the buyout's unaffordability in specification (4) by including **Buyout Loss** which for unprofitable buyout-session subjects equals the loss (in dollars) that she would realize if she had accepted the buyout and completed the same number of earning tasks as she did in the previous round. We set **Buyout Loss** equal to zero for all other subjects. The inclusion of this variable does not affect our results.

To summarize, we find that the strength of the treatment effect is increasing in the number of peers who accept the buyout offer, consistent with our hypothesis that some reduce their effort contribution to the public good when peers choose to contribute money as opposed to effort. We rule out the possibility that subjects are responding to a perceived change in the level of the public good provided when peers buy out. As well, we do not find support for the idea that unprofitable subjects reduce their effort contributions because they are discouraged when they learn that they are not productive enough take advantage of the buyout option.

3.4 *Are Subjects Who Do Not Buyout Selected on Environment-Task Performance?*

Interpreting our fair-buyout results as a treatment effect relies on comparing those who do not accept the fair buyout with control-session subjects. The latter, however, includes both those who would and would not have accepted the buyout if offered. If those control subjects who would have accepted the buyout are those with larger performance increases (larger δ s), then some of what we attribute to treatment could be the result of composition. In Table 2, the increase in environment tasks completed was in fact larger for control subjects who would have found a buyout profitable than for control subjects who would not have found the buyout profitable. Although this difference is not statistically significant ($p=0.948$) and cannot explain the large decrease in tasks completed by treatment subjects not finding the buyout profitable, we investigate the role of selection. Before turning to our analysis of the unfair-buyout treatment which eliminates the influence of selection, in this section we provide evidence that composition is not driving our fair-buyout results.

Our strategy is to identify observable factors predicting acceptance of the buyout in the fair-buyout treatment, and show that these factors do not predict performance improvement in control-session subjects. In addition to buyout profitability, we have identified two relevant demographic characteristics. First, subjects who did not accept an offered round-5 buyout report a higher level of our ordinal measure of **Religiosity** than subjects in our control group ($p = 0.039$, Table 7 in the online supplement).¹⁷ Second, gender (captured with the indicator variable **Male**) is correlated

¹⁷While the correlation of self-reported religiosity and rejecting the buyout is not surprising, the results of our exit

with accepting the round-5 buyout ($\rho = 0.3024$, $p = 0.015$). We can find no other characteristic associated with membership in, or selection into, the population rejecting the buyout.

In Table 4, we present regression analysis results. In all models, we include the variables capturing characteristics associated with the buyout group, **Male** and **Religiosity**. We include **Environment Task Index** for two reasons. First, the number of permits an individual might produce if she rejects the buyout may influence her decision. Second, those a with higher round-1 performance (i.e., a higher index) might have learned task performance more quickly and thus might be expected to have a lower δ_i . We include either the continuous measure of earning-task performance (**Earning Task Index**) or the binary measure of expected buyout profitability (**Expected Profitability**>0).

	(1)	(2)	(3)	(4)
	isBuyout	isBuyout	δ_i	δ_i
Environment Task Index	0.206 (0.239)	0.194 (0.230)	-20.505 (15.032)	-12.279 (11.214)
Male(d)	0.196* (0.113)	0.239** (0.097)	-3.220 (2.452)	-2.409 (1.718)
Religiosity	-0.028** (0.013)	-0.031*** (0.011)	0.899 (0.944)	0.912 (0.961)
Earning Task Index	1.433*** (0.438)		20.137 (16.941)	
Expected Profitability >0 (d)		0.239** (0.095)		5.472 (4.777)
Observations	64	64	61	61

Table 4: Probit models of buyout decision in fair-buyout treatment sessions (1–2); OLS models of δ_i in control sessions (3–4). Standard errors, clustered by group, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Independent Variables: **Environment Task Index** = round-1 tasks completed relative to group median; **Male**= 1 if male, else 0; **Religiosity** = “How frequently do you attend religious services?” Ordinal with 0=Less than once a year, 5=More than once a week; **Earning Task Index** = round-4 tasks completed relative to group median; **Expected Profitability**>0 = 1 if round-4 tasks completed > group median, else 0;

In models (1) and (2), we estimate probit models of the round-5 buyout decision using all treatment subjects. The dependent variable, **isBuyout**, equals one if the subject accepted the round-5 buyout and zero otherwise. Whether we use the continuous measure of earning-task performance (**Earning Task Index** in model (1)) or the indicator for expected buyout profitability (**Expected Profitability**>0 in model (2)), strong past performance on earning tasks increases the likelihood of buying out. Gender and religiosity are both correlated with buyout acceptance.

In models (3) and (4), we use the same explanatory variables in OLS models of the change in questionnaire do not give any deeper insight.

environment-task performance from round 1 to round 5 (δ_i) for our 61 control-session subjects. All coefficients are imprecisely estimated. In particular, whereas tasks completed for earning strongly predicts buyout acceptance, neither our continuous measure of earning-task performance (model (3)) nor our binary measure (model (4)) predicts improvement in tasks completed for the environment. In summary, these results do not support the hypothesis that high δ s are more likely to be found in the cohort that would have accepted the buyout of control sessions.

Finally, we estimated alternative specifications for (1)–(4), including all other demographics variables. As noted above, no other observable characteristic is associated with buyout acceptance. Furthermore, *no* observable characteristic is correlated with δ_i in control-session subjects.

3.5 *The Effect of the Unfair-Buyout Treatment on Effort Contributions*

This section uses the results of the unfair-buyout treatment to address the issue of selection. The key difference between the fair-buyout and unfair-buyout treatments is that, in the latter, we only offered the option to one half of each treatment group chosen at random, with the same subjects offered the option in the two buyout rounds. Thus, an analysis of subjects not offered the buyout will not be affected by selection. We show that while subjects who do not buyout in this treatment register larger environment-task decreases than those in the fair buyout treatment (perhaps due to differences in perceived unfairness), consistent with our main hypothesis we see effort contributions from those not offered the buyout are decreasing in the number of peers who accept.

Table 5 details subject characteristics by buyout status for the unfair-buyout subjects. First, the experiment achieved the desired randomization. Comparing the offered and not-offered groups, there is no significant difference in either environment-task performance ($p = 0.574$) or earning-task performance ($p = 0.160$). Further, amongst those offered the buyout, those accepting the buyout do not significantly differ from those who do not accept it in terms of either environment-task ($p = 0.795$) or earning-task ($p = 0.150$) performance. However, compared to those who did not buy out in round 5, those who bought out in round 5 were more likely to have bought out in round 3 ($p = 0.013$). We also note that, as in the fair buyout, there is heterogeneity between groups in the number who buyout, with round-5 buyout rates ranging from 12.5% to 57.1%.¹⁸

In measuring the treatment effect, we first look at those who do not buy out—whether because the buyout was not offered or it was rejected. We first refer back to Figure 2 where we depict, by treatment, cumulative distribution functions of the percent change in tasks completed (round 5 relative to round 1) by subjects who do not buyout (either by choice or unavailability). As in the control and fair-buyout sessions, we see a significant portion of subjects in the unfair buyout completing more tasks in round 5 than in round 1, with 27% increasing by at least 10%. However, we see an even more pronounced treatment effect, with over 43% of non-buyout subjects registering decreases in tasks completed of at least 10%.

Formal (Wilcoxon rank-sum) tests show that the difference between the control and the unfair buyout is similar to the difference between the control and the fair buyout. On average, a non-buyout subject in the unfair-buyout treatment completed 7.1 fewer tasks in round 5 than round 1.

¹⁸Four subjects bought out in a group of 7 which explains why the range of buyout rates exceeds 50%.

	Buyout Accepted:		No	Yes
	Buyout Offered:		Yes	Yes
	No	Yes	Yes	Yes
Environment Task Index	1.045 (0.236)	1.042 (0.164)	1.036 (0.162)	1.043 (0.168)
Earning Task Index	1.034 (0.170)	0.962 (0.168)	1.050 (0.154)	0.937 (0.167)
Buyout in Round 3	0.000 (0.000)	0.677 (0.475)	0.286 (0.488)	0.792 (0.415)
	<i>n</i>	30	31	7
			7	24

Table 5: Unfair Buyout Treatment: Subject characteristics (means and standard deviations) by round-5 buyout status.

This is statistically different than the 1.8 task average increase in the control ($p=0.028$). Our main comparison—between control subjects and those not offered the unfair buyout—rules out selection. Whereas the subjects not accepting the unfair buyout completed 6.1 *more* tasks on average, subjects not offered the unfair buyout completed 10.2 fewer tasks on average, again significantly less than the control ($p=0.008$).

In Table 6, we present estimates of OLS models of δ_i with errors clustered at the group level. In both specifications, we have one observation from each of the 61 control subjects and the 37 subjects who either were not offered or did not accept the unfair buyout. In model (1), in addition to **Round 1 Tasks Completed**, we include only indicator variables capturing whether an unfair-buyout subject was not offered the buyout or chose not to accept the buyout. We estimate that — compared to control subjects — those not offered the buyout significantly decrease tasks completed. In the second model, we interact these indicator variables with the buyout rate. The estimates support the hypothesis that the decrease from those not receiving the offer comes not from the introduction of the buyout, but rather from the fraction accepting the offer. This is consistent with our findings from the fair-buyout treatment.

Overall, we find that the reduction in tasks completed by non-buyout subjects in this treatment is at least as large as the reduction by non-buyout subjects in the fair-buyout treatment, despite the fact that those who reject the unfair buyout register larger increases than the average control subject. We speculate that perceived differences in fairness of the buyout rules may play a role in the particularly large decrease from those not offered the buyout. However, restricting the comparison to only those subjects not offered the buyout gives results consistent with the fair-buyout results in that the output reductions arise from the number accepting the buyout. Therefore, in a comparison immune to selection effects, we find further support for the hypothesis that peers choosing to contribute money in lieu of effort results in reduced contributions from those who must continue to make effort contributions.

	(1)	(2)
Round 1 Tasks Completed	0.0599 (0.113)	0.120 (0.113)
Buyout Not Offered	-11.95*** (3.299)	8.063 (8.121)
Buyout Not Accepted	4.548 (5.910)	7.136 (13.43)
Not Offered X Buyout Rate		-0.511*** (0.191)
Not Accepted X Buyout Rate		-0.105 (0.523)
Constant	-1.829 (7.046)	-5.396 (6.985)
Observations	98	98

Table 6: Unfair-Buyout Regression Analysis: Random-effects GLS models of $\delta_i = \text{Tasks}_{5i} - \text{Tasks}_{1i}$, $i \in$ subjects who do not buyout in control and unfair buyout. Standard errors, clustered by group, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Independent Variables: **Buyout Not Offered**= 1 if unfair-buyout subject not offered buyout, else 0; **Buyout Not Accepted**= 1 if subject did not accept offered buyout, else 0; Interactions indicated with X.

4 CONCLUSION AND DISCUSSION

If the fact that Mr. Gore chooses to avail himself of market-based solutions to environmental problems results only in hurt feelings, then perhaps there is little to worry about. If similar choices result in measurable changes in aggregate contributions to public goods, then they have the potential to alter the effectiveness of policy interventions.

We designed an experiment to test the hypothesis that some choosing to contribute money instead of effort to the public good results in reduced effort contributions by those who continue to contribute effort. We find substantially decreased effort contributions, although the effects are heterogeneous. Those who decline the buyout despite its profitability do not decrease effort contributions. Those for whom it the buyout is unprofitable respond by completing fewer tasks on average. Importantly, the decreased effort contribution from this latter group depends on the number of group members accepting the offer.

Broadly, we see two implications. First, our setting elicited high levels of effort contributions throughout the rounds of control sessions. Thus, the introduction of the buyout appears to have disrupted a setting which in volunteerism was sufficient to ensure that public goods were provided. The provision of many local public goods rely on similar institutions and, therefore, could be similarly susceptible to our treatment. Our experiment may help explain why many groups and organizations (including the running club to which one of the authors belongs) do not allow

members to replace effort obligations with cash contributions.

Second, our study is, to the best of our knowledge, the first empirical support for a link between willingness to exert effort for the public good and the contribution vehicle of others. While our results do not argue against market-based solutions in general, the effects we find are large. Both the pattern of our results and the environment in which they occurred point to aspects for policy makers and future researchers to consider.

Notably, in both treatments, the large negative effects only occurred the second time we offered the buyout. Importantly, subjects who decreased output in round 5 mostly did *not* buyout in round 3,¹⁹ and thus could have, but did not, decrease output when the buyout was first introduced. Based on this, two hypotheses as to why the treatment effect might be stronger in round 5 present themselves. First, consistent with other studies (e.g., Cooper and Stockman 2002), it might simply be that it takes time for subjects to realize how they feel about the institution. Alternatively, it might be that in round 3, subjects are unsure about what the future holds. For example, a subject unable to buyout in round 3 might have believed it a one-time occurrence or that future buyouts would be profitable. When, in round 5, she sees that the buyout option is a durable and unprofitable feature of the experiment, she may have plausibly reacted more strongly.

Our experiment environment captures many aspects of common buyout options. Of course, there will be significant variability in buyout environments. We consider how some of these differences between our environment and particular buyout opportunities might affect outcomes.

First, subjects likely felt little group identity and were not selected into the group based on intrinsic concern for the public good. It is unclear whether changing these aspects would strengthen or weaken the effects found in the laboratory.

Second, characteristics of the public-good effort task may well affect willingness to reduce effort contributions when others replace effort with money. Our laboratory tasks were solitary and separable. Furthermore, subjects reported little intrinsic motivation to complete the tasks, and performance — both in terms of number of tasks and ultimate emission reduction — was measurable. Differences in any of these dimensions will plausibly affect one's willingness to contribute effort to the public good when others contribute monetarily.

Third, an individual's effort contribution to the public good will be public in some environments. People may use public participation and effort choice to signal attributes such as caring for the environment (Bénabou and Tirole 2006). This signal may be more clear from a person who has publicly passed up an opportunity to buy out of the prosocial effort, which may mitigate the strong effect we find in the laboratory.

Fourth, the fact that *total* contributions decreased from round 1 to 5 in the fair-buyout treatment (compared to increasing in the control treatment) is likely due, in part, to limited potential efficiency gains in our experiment. In other environments, potential efficiency gains may be much more substantial. When opportunity costs of effort contributions vary widely across individuals, requiring those who buyout to replace more than their effort might both offset and reduce the

¹⁹Consider subjects who completed fewer tasks in round 5 than in round 1. In the fair-buyout treatment, 19 of the 24 such subjects did not buyout in round 3. In the unfair-treatment, all 17 such subjects were not able to buyout.

behavioral response of those who do not buyout.²⁰

A final aspect of our experiment that deserves further consideration is the fact that those accepting the buyout were able to earn money as the alternative activity. One possibility is that — to the extent that market-based mechanisms highlight or exacerbate pre-existing inequality — an aversion to income inequality might underlie objections to their use. The notion that people care about their relative consumption in addition to their absolute level of consumption has a long history in economics (Duesenberry 1949), and the public policy implications of this hypothesis have been well studied (e.g., Boskin and Sheshinski 1978, Layard 1980, Persson 1995, Carroll, Overland and Weil 1997). Thus, due to the increased earning inequality introduced by the buyout in our experiment, we cannot rule out the possibility that inequality aversion contributed to our treatment effect.²¹ A second possibility is that earning money in lieu of contributing effort to the public good might have a more direct effect: it may simply be aggravating to non-buyout subjects that some are earning money while others contribute their labors to the public good. In other contexts, someone accepting a buyout may be doing something “better” with his time, such as caring for a family member, which may mitigate the perceived injustice.

²⁰With no clear correlation in performance across tasks, selection did not play a large role in our main treatment. When the opportunity cost of effort and public-good productivity are positively correlated, allowing those with the highest opportunity cost to buy out will compound, and may exacerbate, the identified behavioral response.

²¹We note, however, the while concern for inequality is plausible as a reason to oppose the use of markets, why it should be linked to the crowding effect we observe is far less clear, as reduced effort does nothing to reduce inequality.

REFERENCES

- Abbink, Klaus and Heike Hennig-Schmidt**, "Neutral versus loaded instructions in a bribery experiment," *Experimental Economics*, June 2006, 9 (2), 103–121.
- Alexander, Keith L.**, "Outsourcing the Picket Line," *The Washington Post*, July 24, 2007.
- Bénabou, Roland and Jean Tirole**, "Incentives and Prosocial Behavior," *American Economic Review*, December 2006, 96 (5), 1652–1678.
- Boskin, Michael J. and Eytan Sheshinski**, "Optimal Redistributive Taxation When Individual Welfare Depends upon Relative Income," *Quarterly Journal of Economics*, November 1978, 92 (4), 589–601.
- Brüggen, Alexander and Martin Strobel**, "Real effort versus chosen effort in experiments," *Economics Letters*, 2007, 96 (2), 232–236.
- Carroll, Christopher D., Jody Overland, and David N. Weil**, "Comparison Utility in a Growth Model," *Journal of Economic Growth*, December 1997, 2 (4), 339–367. 10.1023/A:1009740920294.
- Chen, Yan and Sherry Xin Li**, "Group Identity and Social Preferences," *American Economic Review*, March 2009, 99 (1), 431–457.
- Cooper, David J. and Carol Kraker Stockman**, "Fairness and Learning: An Experimental Examination," *Games and Economic Behavior*, October 2002, 41 (1), 26–45.
- Duesenberry, James Stemble**, *Income, Saving and the Theory of Consumer Behaviour*, Cambridge, MA: Harvard University Press, 1949.
- Ellingsen, Tore and Magnus Johannesson**, "Time is not money," *Journal of Economic Behavior & Organization*, October 2009, 72 (1), 96–102.
- Fischbacher, Urs**, "z-Tree: Zurich Toolbox for Ready-made Economic Experiments," *Experimental Economics*, June 2007, 10 (2), 171–178.
- , **Simon Gächter, and Ernst Fehr**, "Are people conditionally cooperative? Evidence from a public goods experiment," *Economics Letters*, 2001, 71 (3), 397–404.
- Frey, Bruno S. and Reto Jegen**, "Motivation Crowding Theory: A Survey of Empirical Evidence," *Journal of Economic Surveys*, December 2001, 15 (5), 589–611.
- Gneezy, Uri and Aldo Rustichini**, "A Fine Is a Price," *Journal of Legal Studies*, January 2000, 29 (1), 1–17.
- Greiner, Ben**, "Subject Pool Recruitment Procedures: Organizing Experiments with ORSEE," *Journal of the Economic Science Association*, July 2015, 1 (1), 114–125.

Heal, Geoffrey, "A Celebration of Environmental and Resource Economics," *Review of Environmental Economics and Policy*, Winter 2007, 1 (1), 7–25.

Humphrey, Mark, "Group says Gore's home overuses electricity," *USA Today*, February 27, 2007.

Layard, R., "Human Satisfaction and Public Policy," *The Economic Journal*, 1980, 90 (360), 737–750.

Persson, Mats, "Why Are Taxes So High in Egalitarian Societies?," *The Scandinavian Journal of Economics*, 1995, 97 (4), 569–580.

Rousseau, Jean-Jacques, *The Social Contract* 1762.

Sandel, Michael J., *What Money Can't Buy*, Farrar, Straus and Giroux, 2012.

Vilares, Iris, Gregory Dam, and Konrad Kording, "Trust and Reciprocity: Are Effort and Money Equivalent?," *PLoS ONE*, 02 2011, 6 (2), e17113.

ONLINE SUPPLEMENT TO
A SHARED SENSE OF RESPONSIBILITY: MONEY VERSUS EFFORT CONTRIBUTIONS IN
THE VOLUNTARY PROVISION OF PUBLIC GOODS
BY JARED C. CARBONE AND ROBERT S. GAZZALE

A SESSION INSTRUCTIONS

Introduction

- You are about to participate in a session in which you will complete tasks at a computer. This is part of a study intended to provide insight into certain features of decision processes. I encourage you to follow the instructions carefully. You will be paid in cash at the end of the session.
- *During the session, I ask that you please do not talk to or otherwise communicate with (e.g., text messaging) each other.* If you have a question, please raise your hand and a session moderator will assist you.
- In consideration for others in the session, we do ask that you take a moment to make sure that the ringer on your cell phone is off.
- Your computer screen should have two open windows: an internet browser and a “Welcome to z-Leaf” window. If these two windows are not open on your screen, please raise your hand.
- During the session, you are free to use the internet browser on your computer. The session monitor will inform you when you are required to respond to the z-Leaf window.
- You should have a pen and two blank sheets of paper at your workspace. Please check to make sure the pen works. If you do not have these items, or if your pen does not work, please raise your hand.
- During this session, you are going to be assigned to a group. In some parts of this experiment, you and your group members will have the opportunity to complete computer-based tasks for the environment, where more tasks completed will lead to larger reductions in greenhouse-gas emissions, believed by most scientists to be a leading contributor to global warming. Before continuing with the more detailed session instructions, we would like to give you an idea of how this will work.
- **According to the Intergovernmental Panel on Climate Change:** Global atmospheric concentrations of greenhouse gas emissions have increased markedly as a result of human activities. Scientists have very high confidence that this process has caused increases in global

average air and ocean temperatures, widespread melting of snow and ice, and rising sea levels. The likely range for warming by the end of the 21st century is between 5 and 14 degrees F. The effects will last for centuries due to the long-lived nature of emissions. The longer-term magnitude of these effects and the impacts on different regions of the planet are less certain but will likely include widespread changes in precipitation amounts, ocean salinity, wind patterns and aspects of extreme weather including droughts, heavy precipitation, heat waves and the intensity of tropical cyclones. While some changes might be beneficial (like longer growing seasons for agriculture), on balance scientists believe these changes will be damaging to humans. There is also some small risk that there could be catastrophic impacts, such as major reductions in polar ice that could result in approximately 15-20 feet of sea level rise.

- The tasks you and your group complete in the **Environment Rounds** directly reduce greenhouse gas emissions. The European Union Emission Trading System, which came into effect in 2005, caps the number of tons of greenhouse gas emissions EU member countries may produce in any year. In order to emit greenhouse gases, a polluter needs a permit, with the total number of permits equal to the total number of tons allowed. Based on the number of tasks you and your group complete in Environment rounds, session sponsors will purchase the appropriate number permits. We will retire these permits. That is, we will not sell these permits to emit greenhouse gases, and thus the number of tons of greenhouse gases emitted will decrease by this amount. Therefore, your effort in Environment rounds directly reduces greenhouse gas emissions.
- Driving a mid-size car from Palm Beach, FL to Williamstown, MA produces the equivalent of approximately .7 tons of carbon dioxide, a greenhouse gas.

Procedures

- At the start of the session, the computer randomly assigns one-half of participants to the Blue group and one-half to the Purple group. You remain in the same group for the entire session.
- This session will proceed in a series of 5-minute rounds.
- In **Environment Rounds**, you are presented with counting tasks in your z-Leaf window. At the end of the round, the computer adds up the number of tasks completed by members of your group. For each 200 counting tasks, the session sponsors will purchase and retire a permit to emit one ton of carbon dioxide, directly reducing global greenhouse-gas emissions by that amount. (If the total is not divisible by 200, the appropriate fraction of a permit will be purchased.)
- In **Earning Rounds**, you are presented with encryption tasks in your z-Leaf window. At the end of the session, you receive a cash payment of \$0.10 (10 cents) for each encryption task you complete.

- **The Encryption Task:** In your z-Leaf window, there will be a table translating each letter of the alphabet into a unique 2-digit number. This table remains the same for the round. For each task, your monitor presents you with 2 letters. You complete a task by translating the 2 letters into their numeric equivalent, entering the correct 4 numbers (without spaces) into a box on your monitor, and clicking the **OK** button. Once you have entered the correct 4 numbers, your monitor presents you with the next 2 letters.
- **The Counting Task:** For each task, your z-Leaf window presents you with a table with 4 rows and 4 columns with either a 0 or 1 in each position. You complete a task by counting the number of zeros, entering the correct number into a box on your monitor, and clicking the **OK** button. Once you have entered the correct number of zeros, your z-Leaf window presents you with the next table.
- At the start of each round, your z-Leaf window will inform you whether the round is an Environment Round or an Earning Round.
- At the end of each Earning Round, your z-Leaf window will display the number of encryption tasks you completed and the amount of money you earned.
- At the end of each Environment Round, your z-Leaf window will display the number of counting tasks completed by you and your group as a whole. It will also display the number of tons greenhouse gas emissions your group has prevented.
- The experiment will start with two one-minute practice rounds (one encryption and one counting). The purpose of the practice rounds is to give you an opportunity to familiarize yourself with the computer interface. You will not earn money in the practice rounds, nor will greenhouse gas permits be purchased.
- At the end of the experiment, I will pay you, in cash, your participation fee plus your earnings from all Earning Rounds. I will also provide you with a web address where you may verify the purchase of greenhouse gas permits that your group earned. At the end of the calendar year, you may verify that all of the permits have been retired (that is, none have been resold).
- Are there any questions?

A.1 Buyout Instructions

You and everyone in your Group will be offered a Buyout Option this round.

Please read carefully.

- If you accept the **Buyout Option**, you pay a **Buyout Fee**: a flat fee which session sponsors will use to purchase and retire as many greenhouse gas permits as possible.

- If you accept the Buyout Option, your computer will present you with earning (encryption) tasks. The amount you would receive per completed encryption task may be different than in regular Earning Rounds. If you accept the Buyout Option, your earnings, which may be negative, would be the number of encryption tasks you complete times the amount per completed encryption task, minus the Buyout Fee.
- All members of your group are offered the same Buyout Fee, which you would pay only if you accept the Buyout Option. Also, all members of your group will be offered the same number of cents per completed encryption task.
- The Buyout Fee has been calculated to replace the average number of permits produced in the first Environment Round. Thus, if your 8 group members retired a total of 8 pollution permits in the first Environment Round, the calculated Buyout Fee will be exactly equal to the price of 1 pollution permit.
- If you do not accept the Buyout option, your Environment Round proceeds as usual. Your computer presents you with counting tasks, with the session sponsor purchasing and retiring 1 ton of greenhouse gas emissions for each 200 tasks.
- At the end of the round, the computer adds up:
 1. all Environments tasks completed by group members not accepting the Buyout option, with the session sponsor purchasing and retiring a permit for 1 ton of greenhouse gas emissions for each 200 successfully completed Environment tasks; and
 2. the Buyout Fees collected from group members accepting the Buyout Option, with the session sponsor using these fees to retire as many greenhouse gas permits as possible.
- On the next screen, your monitor will display information which may be helpful in deciding whether to accept the Buyout Option:
 - the Buyout Fee;
 - the amount you will receive per encryption task if you accept the Buyout option;
 - your earnings if you complete as many encryption tasks as last round;
 - the number of greenhouse gas permits retired if you pay the Buyout Fee; and
 - the number of greenhouse gas permits retired as a result of the number of counting tasks you personally completed in the first Environment Round.
- Are there any questions?

B ADDITIONAL RESULTS

	Control	Fair Buyout	Unfair Buyout	Fair Buyout Buyout=No	<i>p</i> -value	
	(1)	(2)	(3)	(4)	(1)=(2)=(3)?	(1)=(4)?
Male	0.541 (0.502)	0.541 (0.502)	0.500 (0.504)	0.420 (0.499)	0.868	0.205
Caucasian	0.508 (0.504)	0.574 (0.499)	0.562 (0.500)	0.560 (0.501)	0.738	0.586
School Year	2.131 (1.176)	2.279 (1.082)	2.516 (1.221)	2.480 (1.233)	0.176	0.119
Number Econ Courses	2.083 (2.854)	2.155 (2.581)	2.172 (3.293)	1.900 (2.957)	0.467	0.194
Religiosity	0.885 (1.439)	1.086 (1.430)	1.203 (1.371)	1.280 (1.485)	0.132	0.039
Number Friends in Session	0.881 (1.161)	0.741 (1.069)	0.609 (1.149)	0.600 (1.245)	0.345	0.129
Observations	61	61	64	50		

Table 7: Summary Statistics for Subject Demographic Characteristics

Male = binary indicator is 1 if subject is male, else 0; **Caucasian** = binary indicator is 1 if subject is Caucasian, else 0; **School Year** = ordinal years of college completed; **Number Econ Courses** = ordinal number of college economics courses completed; **Religiosity** = “How frequently do you attend religious services?” Ordinal with 0=Less than once a year, 5=More than once a week. **Number Friends in Session** = ordinal number of friends in session. Statistical tests for Male and Caucasian (binary variables) use Chi-Square. Other comparisons (i.e., ordinal) use Kruskal Wallis.