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**Abstract**

Group compensation and public announcement of performance are two common aspects of working with other people. We randomly assign these aspects to real-effort tasks. Following task completion and payment, subjects are given an unexpected opportunity to donate to a local charity. Group compensation and public announcement of performance have little effect on work performance but striking spillover effects on subsequent donations. Public announcement of performance doubles the amount donated to charity, and group compensation significantly increases the share donating. The results suggest that interpersonal interactions in the workplace environment may have important spillover effects on prosocial behavior outside of work.
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# 1 Introduction

Working life can involve a variety of interpersonal interactions. Field evidence demonstrates multiple ways in which social interactions in the workplace affect worker productivity (Mas and Moretti, 2009; Bandiera et al., 2010; Chiaburu and Harrison, 2008). In this study we explore whether interactions during work have spillover effects on an individual's behavior toward those outside of the workplace. We examine two common aspects of working with others: group compensation (i.e. interdependent payoffs, as occur with team projects, profit sharing, or bonuses) and public announcement of performance.

Long literatures in social psychology and experimental economics have advanced our understanding of the effects of groups. The "Social Identity Theory" of Tajfel (1970) and Tajfel et al. (1971) has endured over years of tests that have repeatedly shown that the mere perception of belonging to a group can alter behavior. Subjects can be categorized in groups described as "minimal" in that they are based on trivial similarities or even randomly assigned, yet this mere categorization has reliably induced in-group favoritism and out-group discrimination (Tajfel and Turner, 1986).

We hypothesize that working with others can affect prosociality toward those who are neither in the group nor even in the workplace. Research on behavior in social/group settings suggests such spillovers. If prosociality is motivated by adherence to social norms or attempts to promote one's social image, then priming greater attention to others in a group could influence beliefs about the socially-appropriate amount to give. In support of such behavior, Bicchieri and Xiao (2009) found that manipulating expectations about labmates' generosity influences subjects' own generosity in the Dictator Game. Priming identification with a group may also provide psychological benefits (Akerlof and Kranton, 2000). Such benefits could affect attitudes and thereby have spillover effects. Indeed, Hargreaves Heap and Zizzo (2009) provided support for psychological benefits of artificial groups while also indicating that these negatively affected trust toward labmates who were not in the group. Such spillovers need not be restricted to others in the laboratory, and broader spillovers should be expected if working with others affects brain chemistry. Social interactions can naturally enhance levels of chemicals such as oxytocin, which has been found to increase generosity toward other participants in the laboratory and toward real charitable organizations (Zak et al., 2007; Barraza and Zak, 2009; Barraza et al., 2011; Van IJzendoorn et al., 2011).

This study employed a laboratory experiment designed to test for prosociality spillovers of working with others. All subjects performed the same set of real-effort tasks for compensation. Randomization followed a two-by-two design that allowed us to estimate isolated effects of two common aspects of working with others as well as any potential interaction between these aspects. First, some subjects were paid a piece rate based on their own performance ("Individual"), while others were randomly assigned to groups and paid based on the group's performance ("Group"). Second, some subjects' performance was kept as private knowledge ("Private"), while others were informed that at the end of the tasks they would be asked to announce their performance to everyone in the laboratory ("Public"). The two-by-two design examined the "Group" and "Public" treatments together and in isolation, reflecting the imperfect correlation across workplaces between these aspects of working with others. Regardless of treatment group, subjects were told they would be paid for correct answers provided in each of four tasks. Subjects were not informed before the tasks that they would later have an opportunity to donate from their earnings. Upon completion of the tasks, subjects were shown a screen stating that they had completed all four tasks, then shown the amount they had earned, then given an opportunity to donate a portion of their earnings to a local charity, then asked to complete a survey, and then paid. Reflecting real-life working environments, our experimental design is tailored to explore the spillover effects on pro-social behaviour allowing for interactions of both aspects of working with others because these are not perfectly correlated across workplaces.

We found that both treatments significantly affected donations despite having no significant effects on performance or earnings. Specifically, each treatment increased donations, with the result obtaining statistical significance on the share donating for the "Group" treatment, on the amount donated for the "Public" treatment, and in Tobit regressions for both treatments. The pattern of results is consistent across levels of individuals' earnings, group earnings, subject scores on the Cognitive-Reflective Test of Frederick (2005), and performance on each of the tasks. We posit that these effects are likely driven by the social treatments' manipulation of the extent to which subjects think about other individuals other than themselves. Consistent with this other-regarding mechanism, we find that subjects who report knowing other subjects in the laboratory show significantly stronger donation responses to the social treatments despite being no more generous than other subjects in the baseline "Individual, Private" treatment.

Our study adds to existing literature looking at the role of behavioral spillovers (see, e.g., Dolan and Galizzi, 2015; McCannon and Rodriguez, 2016). Bruhin et al. (2016) find evidence of *inter*personal spillovers in blood donation, whereas we examine an *intra*personal spillover from the earnings environment to the donation decision. Pan and Houser (2013) found that subjects assigned to groups that were given a more cooperative work task were subsequently more generous toward out-group members in trust games, a result similar to the spillovers we find. The direction of causality of our spillover is opposite that found by Anik et al. (2013), who provide evidence that prosocial bonuses in the form of donations to charity lead to happier and more satisfied employees at an Australian bank. Our experiment is more similar in design to those of Erkal et al. (2011) in that subjects earn money based on performance and can then give some away, particularly their variant in which subjects are not told about donation opportunities until after they earn. Erkal et al. (2011) induce sizable earnings differences between each group member and study gifts to members of the group, whereas we pay all subjects a piece rate and study donations to a real charity.[1]

Our findings offer a novel consideration for interpreting studies of audience effects on prosociality. These studies have shown that identification of subjects and their decisions through announcements or the presence of an audience may have a positive impact on charitable donations (Ariely et al., 2009), establishing a 50-50 sharing norm (Andreoni and Bernheim, 2009) and increasing contributions to a public good (Andreoni and Petrie, 2004; Rege and Telle, 2004; Soetevent, 2005; Alpizar et al., 2008; Denant-Boemont et al., 2011; Samek and Sheremeta, 2014). Whereas these studies expose the prosocial behavior to an audience, our study exposes work performance to an audience. The fact that we still find significant spillover effects on private prosocial behavior isolates direct effects of exposure to an audience that may be operating in addition to the social image concerns that are generally thought to mediate audience effects on prosociality.

The effects of the aspects of group work that we study may be viewed as an additional example of studies that have found effects of priming with social factors and emotional state on prosociality. Inducing anger among subjects lowers contributions in a public goods game (Drouvelis and Grosskopf, 2016), while priming subjects on cooperation increases contributions (Drouvelis et al., 2015). Priming subjects with religious words has increased donations in the Dictator Game (Shariff and Noranzayan, 2007). Priming subjects with a picture of eyes has been found in the laboratory to increase donations in the Dictator Game (Haley and Fessler, 2005) and in the field to increase payments for purchases (Bateson et al., 2006), decrease littering (Ernest-Jones et al., 2011), and increase recycling (Ekström, 2012). Allowing subjects to identify with their experimental partners by viewing them (Bohnet and Frey, 1999), seeing photographs of them (Scharlemann et al., 2001), or communicating with them (Bochet et al., 2006) has been shown to generate positive effects on prosocial behavior. Putting givers "in the receiver's shoes" by having subjects make decisions for both roles before randomly assigning them to either the giver or receiver role had a significant positive effect on the amount given (Andreoni and Rao, 2011). More generally, this study contributes to

[1]Our paper takes up the challenge in theirs that "Future work can consider whether similar results hold in other environments, such as those where earnings are determined using a piece-rate scheme and where subjects give to an actual charity instead of each other."

a growing body of experimental literature exploring the behavioral determinants of charitable donations. Many of these studies have examined the importance of economic mechanisms such as using matching to alter the price of giving (Karlan and List, 2007) and providing gifts for potential donors (Falk, 2007; Garbarino et al., 2013). Increasingly, however, this research highlights the importance of social interactions in determining charitable giving.

The paper proceeds as follows. Section 2 describes the design of the experiment. Sections 3 and 4 present the results for earnings performance and for donations, respectively. We discuss the findings in Section 5 and conclude in Section 6.

## 2 Experiment Design

To address our research questions, we employed a 2x2 factorial (between-subjects) design analyzing the effects of two different incentive schemes (individual piece-rate vs. revenue-sharing forms of compensation) and information structures (public announcement vs. no public announcement). We measure productivity by observing subjects' performance in four different timed tasks. Similar to Wozniak et al. (2014), our design consists of two types of tasks: math and word tasks. Having each subject complete both types of tasks allows all subjects to respond to treatment even if, say, certain cultural groups or genders have difficulty with one type of task (Niederle and Vesterlund, 2010). In addition, each of these two tasks consists of an easy and hard version. Subjects were given a 5-minute time limit for each task and were shown the timer.

For both the math and the language task, items were presented to subjects on a computer screen. Subjects would type in an answer and click the "Submit" button. After each submission, a new item was immediately shown. For the math task, subjects were asked to multiply two numbers. The easy (hard) version of this task involved the multiplication of two one-digit (two-digit) numbers. For the word task, each subject had to arrange pairs of letters to form a word. The easy (hard) version of this task involved re-arranging two (four) pairs of letters. Subjects were told that they have to use all pairs of letters to form the correct word and can re-arrange the order of the pairs but not the order of the letter within each pair. For both the math and the word task, two sheets of scratch paper and a pen were provided, but no other form of assistance was available.

Each subject performed all four tasks. At the beginning of each task, instructions were provided on screen and read aloud. Subjects within a session performed tasks of equivalent difficulty at the same time, and we alternated the order of the tasks across sessions.[2] Prior to any of the four tasks,

[2]Specifically, we ordered the more familiar math task first but varied the order in which the easy or the hard version

subjects were provided with written and read instructions that described the general form of the treatment. The general instructions varied according to the two dimensions of treatment: whether subjects were compensated in groups and whether performance was announced.

We study two incentive schemes: a piece-rate and a revenue-sharing compensation scheme. In treatments with the piece-rate compensation ("Individual" treatments), subjects were paid according to their own performance. This means that the payoff a subject receives is equal to the piece rate multiplied by the number of correct responses of the subject for that particular task. The piece-rate incentives differ depending on whether subjects perform the easy or the hard version of a task. In both the math and the word tasks, a subject received £0.03 for each correct answer in the easy task and £0.21 for each correct answer in the hard task. As the hard version of a task requires subjects to put in more effort to provide a correct answer relative to the corresponding easy version, we chose a higher compensation rate for the former compared to the latter version of a given task.

In treatments with the revenue-sharing form of compensation ("Group" treatments), subjects were randomly assigned to 3-person groups and were paid according to the group's performance. We maintained the same piece rate of £0.03 (£0.21) for each easy (hard) answered correctly, but subjects were paid the piece rate multiplied by the average number of correct responses in the group for that particular task. In other words, the payments for a group's total productivity were split evenly among all 3 members of a group. Assignment to this compensation scheme should decrease payments to high performers and increase payments to low performers. The design therefore allows us to test whether there is a homogeneous effect of group assignment or a heterogeneous effect by compensation by comparing both the average donations across treatment arms and differences in donations by own and group performance. Subjects were not told which subjects were in their group but were given the number of their group as a label.

The second dimension of treatment varied the information structure. In half of the sessions ("Private" treatments), subjects learned about only their own performance. In the other half of the sessions (the "Public Announcement" or "Public" treatments), subjects were informed that at the end of final task they would be asked to stand up and tell the others in the room how many correct answers they had provided in each of the four tasks. The "public" treatment was therefore similar to the group announcement and audience treatments in past work, though the announcement related to performance and not to the donations outcome of primary interest.[3] After the final task,

of each task was experienced first. Subjects therefore performed the tasks in one of two orders: [easy math, hard math, easy word, hard word] or [hard math, easy math, hard word, easy word]. We find no effect of the ordering on earnings or donations.

[3]This and all other treatments received ethics approval from the Institutional Review Board. Subjects were allowed to withdraw from the experiment at any point.

subjects in "Public" treatments had to announce their performance on each of Tasks 1, 2, 3, and 4, in that order. These announcements were made in order of subject ID, which was determined by randomly-assigned seating.

After completion of the final task, subjects were informed of their earnings from the experiment. They were then shown a screen asking whether they would like to donate part of their earnings to a well-known local charity.[4] Subjects could obtain information about the charity by clicking on its onscreen logo. An important feature of our design is that subjects were not informed about the option to donate to the charitable cause until after they had completed all tasks and, in relevant treatments, learned about the performance of the other subjects in their group or their session. This aspect of our design enables us to analyze potential spillover effects on charitable giving which may come from the form of compensation scheme, information sharing, or the interaction of both. After subjects' donation decision had been made, we also collected data on their demographic characteristics and administered the Cognitive Reflection Test (CRT) of Frederick (2005). The CRT is a short list of questions, three of which have an answer that is seemingly intuitive but incorrect, and correct answers to these questions provide a measure of one's proclivity for reflection that is correlated with cognitive outcomes.

We conducted 16 sessions: two sessions for each of the two sequences and each of the four treatment combinations. A total of 282 subjects participated in the experiment. A breakdown of the number of participants per sequence and treatment is given in Table 1.

Table 1: Overview of experimental treatments

| | Private | Public |
|---|---|---|
| Individual | Sequence 1: 34 subjects<br>Sequence 2: 36 subjects | Sequence 1: 36 subjects<br>Sequence 2: 35 subjects |
| Group | Sequence 1: 36 subjects<br>Sequence 2: 36 subjects | Sequence 1: 33 subjects<br>Sequence 2: 36 subjects |

All the subjects were recruited at the University of Birmingham, using the ORSEE software (Greiner, 2015) and were randomly selected from a large database of volunteers who have previously signed up to participate in economics experiments. The vast majority of participants were undergraduate students from various academic fields. Subjects were allocated to treatments at random. The experiment was conducted in the Birmingham Experimental Economics Laboratory (BEEL) and all treatments were computerized and programmed with the Multistage software from Caltech.

[4]The charity is called Acorns Children's Hospice of Birmingham, which provides specialist care for babies, children and young people who are life-limited or life-threatened. More information about the charity can be found here: https://www.acorns.org.uk/.

The full set of instructions used in the experiment is provided in Appendix A. At the end of a session, subjects were paid in private according to their total earnings from all relevant tasks. Average earnings per treatment (including a show-up fee of £2.50) were as follows: £15.11 for the "Individual, Private", £16.11 for the "Group, Private", £16.17 for the "Individual, Public", and £15.34 for the "Group, Public."[5] Sessions lasted, on average, 65 minutes. Appendix Table B.1 shows that treatment groups are balanced in terms of pre-determined subject characteristics captured by survey. Appendix Table B.2 shows a descriptive analysis of these pre-determined subject characteristics; making a donation is positively correlated with age, being married, and having a non-UK nationality.

# 3 Analysis of Performance

We first examine whether the treatments affected subjects' earnings performance. Subjects were paid for correct answers to each of four tasks. We provide summary statistics showing that subjects were able to perform all four tasks and that there was variation in the number of each task completed. We then focus on earnings from a subject's own performance in these tasks as a summary measure of performance in the experiment.

Table 2 presents subjects' average performance as measured by the number of correct responses provided for each of the tasks and treatments. As anticipated, we observe that subjects were more productive in the easy tasks than in the difficult ones. In all four treatments, the best performance is recorded in the easy math task and the worst performance is observed in the hard math task. The coefficient of variation is greater than 0.15 in all cases and nearly 0.7 in some, indicating considerable variation in performance within all tasks. Appendix Table B.3 displays regressions testing for significant differences in the number of responses attempted, left blank, and answered correctly. Estimated effects of the "Group" and "Public" treatments on each of these three outcomes are generally positive and not statistically significant.

[5] At the time of the experiment £1 was equivalent to US$1.52.

Table 2: Average Correct Responses by Task and Treatment

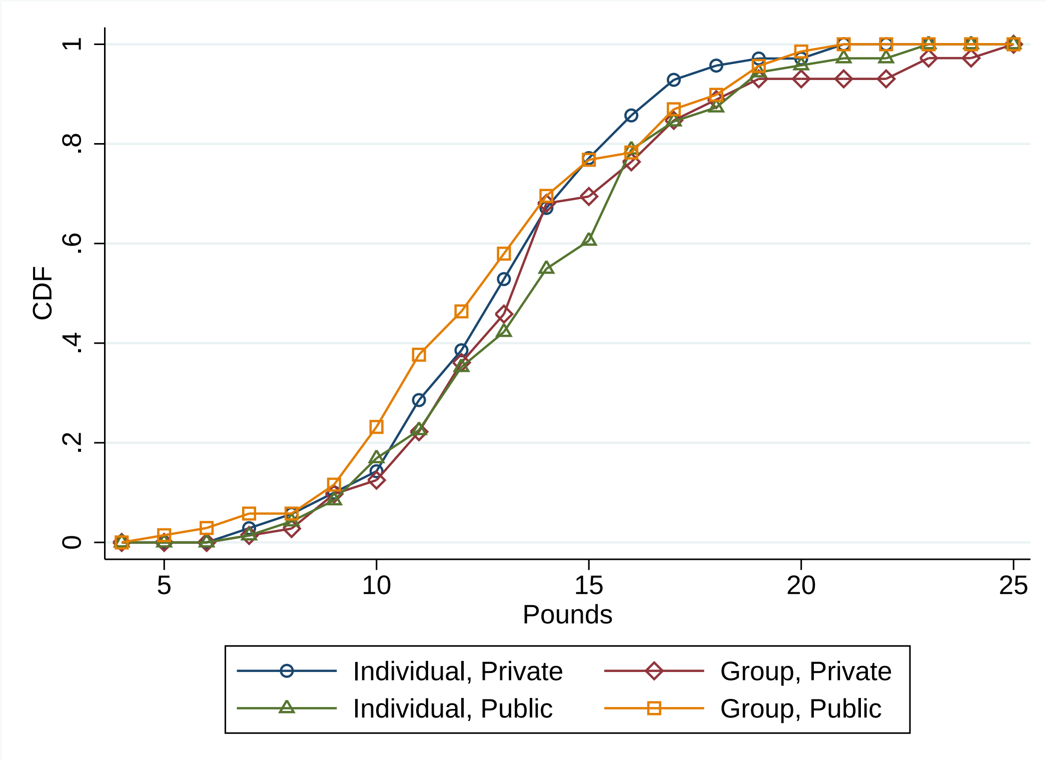
| | Private | | | | Public | | | |
|---|---|---|---|---|---|---|---|---|
| | Math | | Word | | Math | | Word | |
| | Easy | Hard | Easy | Hard | Easy | Hard | Easy | Hard |
| Individual | 150.24 | 8.64 | 99.09 | 16.89 | 154.89 | 9.24 | 104.04 | 18.86 |
| | (40.14) | (6.00) | (18.69) | (7.37) | (44.22) | (5.64) | (16.17) | (8.34) |
| Group | 150.92 | 8.65 | 103.97 | 19.75 | 144.78 | 7.22 | 98.48 | 18.07 |
| | (45.33) | (5.47) | (17.07) | (9.18) | (48.81) | (4.89) | (19.00) | (7.48) |

Numbers in the cells indicate average performance (number of correct answers) in each task. Numbers in parentheses indicate standard deviations.

We use earnings from a subject's own correct answers as a summary measure of performance. Earnings from own performance, or own earnings, is defined as the total earnings generated by the subject, whether or not these were split with a group, so that that the measure is consistent across the "Individual" and "Group" treatments.[6] Figure 1 displays the cumulative distribution function (CDF) of this measure of earnings from one's own performance. Subjects in the "Group, Public" treatment were slightly more likely than others to earn amounts close to £10, while those in the "Individual, Private" treatment were slightly less likely to earn amounts greater than about £17, but overall the distributions appear similar.

[6]Recall that the piece rates paid to the group in "Group" treatments were the same as the rates paid to the subject in "Individual" treatments.

Figure 1: Distribution of Amounts Earned by Own Performance




*Notes:* N=282. Kruskal-Wallis equality of populations rank test p-value=0.261.

To test for differences in performance across treatments we estimate OLS regressions of the form

$$Y_i = \beta_0 + \beta_1 \cdot 1\,[Group,\, Private_i] + \beta_2 \cdot 1\,[Individual,\, Public_i] + \beta_3 \cdot 1\,[Group,\, Public_i] + \epsilon_i \quad (1)$$

In equation 1 the independent variables are indicators for each of the treatments other than the baseline "Individual, Private" treatment. Thus, $\beta_0$ gives the average value of the outcome in the baseline treatment, and the other coefficients each show the difference between this and the average in one of the other treatment arms. For most regressions we report heteroskedasticity-robust standard errors, which are equivalent to standard errors clustered by subject because each outcome is only measured once per subject. When we explore the effect of group earnings we cluster by group to account for the correlation in earnings among group members.

We report the estimates from our own-earnings regressions in Table 3. The dependent variable is earnings from one's own performance, as previously described and shown in Figure 1. The main results appear in model (1), in which we include the entire experimental sample. The first two coefficients indicate that both the "Group" and "Public" treatments in isolation increase earnings by about £0.80 over the mean of £12.84 in the control group, but neither effect is statistically significant. The point estimate for the effect of combining the treatments is even smaller.

Table 3: Effects on Own Earnings, in Aggregate and by Cognitive Level

| | All Subjects | CRT=0 | CRT=1 | CRT=2 | CRT=3 |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Group, Private | 0.770 | -0.032 | 3.147*** | -0.700 | -1.266 |
| | (0.573) | (1.077) | (1.041) | (0.891) | (1.471) |
| Individual, Public | 0.828 | -0.151 | 2.118** | -0.124 | 0.703 |
| | (0.538) | (0.809) | (1.034) | (1.000) | (1.209) |
| Group, Public | -0.232 | -1.002 | 0.966 | -3.180*** | 1.777 |
| | (0.553) | (0.876) | (1.089) | (0.726) | (1.108) |
| Constant | 12.841*** | 12.194*** | 11.340*** | 14.850*** | 14.445*** |
| | (0.349) | (0.589) | (0.556) | (0.561) | (0.780) |
| N | 282 | 93 | 75 | 66 | 48 |
| Adj. R-Squared | 0.007 | -0.015 | 0.077 | 0.128 | 0.036 |

Notes: Heteroskedasticity-robust standard errors are in parentheses. In all models the dependent variable is an individual's own earnings in a given task.

To explore the performance findings further we also report the estimates when the sample is restricted to individuals receiving different CRT scores. Recall that we assessed one type of cognitive ability using subjects' responses to the Cognitive Reflection Test introduced by Frederick (2005). The research suggests that subjects with a higher number of correct responses are more likely to reflect on their answers and less likely to choose intuitive responses. In Models (2)-(5) of Table 3 we report results from four separate regressions corresponding to each possible number of correct responses to the CRT task. When we look separately at each sub-sample of subjects, we find that the pattern of the main result in Model (1) is driven almost entirely by subjects who provided only one correct answer to the three CRT questions (Model (3)). The small value of the constant in this column shows that these subjects performed worse than all others in the control group, including those with CRT equal to zero (though the difference is not statistically significant). These individuals performed significantly better when either compensation depended on group performance or performance was announced publicly, but not when both of these held. By contrast, we find nearly opposite patterns when we look at subjects with higher cognitive abilities. Individuals with a CRT score of either 2 or 3 perform significantly better than others in the baseline treatment, but the insignificant point estimates for the "Group" treatment show that they earned roughly £1 less when sharing these earnings with a group. We interpret these results as suggesting that while high-ability subjects may free-ride, low-ability subjects may exert more effort when they know their performance will be shared with others, perhaps to save face or avoid letting the group down.

Table 3 showed that average effects on earnings were not significant. Appendix Table B.4 shows that effects on the distribution of income were also limited. The "Public" treatment only increased the earnings of relatively high performers, and "Group" treatment effects were all statistically insignificant.

While our main focus is donations and not performance, for context we offer some potential explanations for the lack of significant effects on performance. First, subjects' effort in the tasks may have been determined by intrinsic motivation, such as a desire to see how many correct responses they could provide. Such intrinsic motivation could weaken the effects of extrinsic motives (see Gneezy et al. (2011) for a review on motivation crowding-out effects). Second, Corgnet et al. (2015) document free-riding by Internet browsing, but subjects in this experiment had no access to the Internet or other alternative use for their time and hence could not fully benefit from free-riding. Third, the treatments may have had offsetting effects on motivation, such as if the group treatment reduced financial incentives but induced altruism or competitiveness toward group members. Assessing the importance of such factors may be useful for research focused on work incentives and performance measurement.

# 4 Results for Charitable Donations

In this section we examine our primary outcomes: the spillover effects from group compensation and announcement of performance onto subsequent prosocial behavior. We reiterate that these are, in fact, spillover effects, because donations were made privately, after performance and compensation, in a manner that was not directly affected by the treatments. We first report summary statistics on donations in each treatment and show the CDF for each. We then estimate the effects of treatment on the extensive and intensive margins of donations, and we perform heterogeneity analysis to describe the most likely mechanism for these effects.

Average donations and standard deviations are reported in Table 4. In all four treatments, the majority of subjects are giving a positive amount to the charity. In particular, 51% of the subjects do so in the "Individual, Private", 62% of the subjects in the "Individual, Public", 72% of the subjects in the "Group, Private" and 71% of the subjects in the "Group, Public". This provides evidence that, on average, subjects are willing to sacrifice part of their earned income from the experiment. Across all subjects, average donations are highest in the "Individual, Public" treatment (£1.42) and lowest in the "Individual, Private" treatment (£0.67).

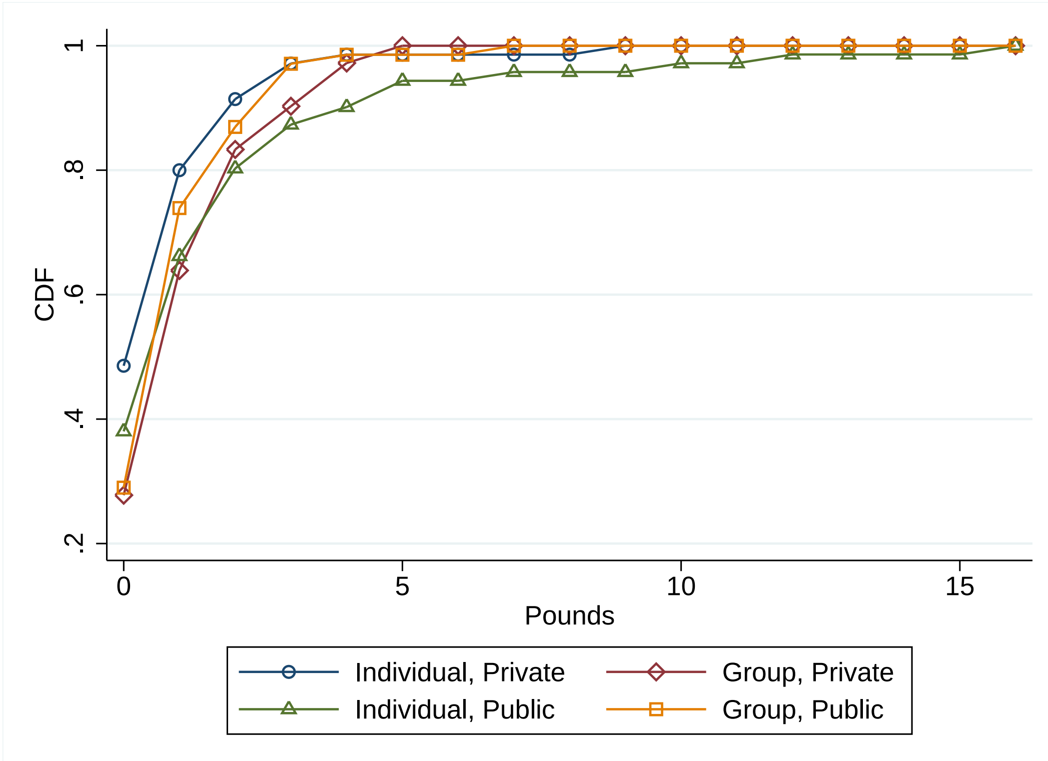
Table 4: Average Donations

| | Private | Public | Total |
|---|---|---|---|
| Individual | 0.665<br>(1.316) | 1.423<br>(2.724)<br>[0.033] | 1.047<br>(2.170) |
| Group | 0.983<br>(1.201)<br>[0.081] | 0.859<br>(1.092)<br>[0.020] | 0.922<br>(1.146) |
| Total | 0.827<br>(1.265) | 1.145<br>(2.097) | 0.985<br>(1.733) |

Notes: Numbers in cells correspond to average donations (in pounds). Numbers in parentheses correspond to standard deviations. Numbers in brackets provide the p-value of a Mann-Whitney test of equality of distribution with that in the "Individual, Private" group.

Figure 2 displays the CDF for each treatment. Mostly clearly we see the differences on the extensive margin of choosing to make a nonzero donation, with subjects that were compensated in groups showing a greater likelihood of doing so. While it is the group treatments that appear to have the largest effect on the extensive margin of giving or not, the "Individual, Public" treatment appears to have had the greatest effect on the intensive margin of amount donated. We can also see that the distribution of gifts in the baseline "Individual, Private" treatment is nearly dominated by the distributions of the other three treatments, and that "Group, Public" is nearly dominated by the other two. Hence, as with performance, the "Group" and "Public" treatments appear to have the greatest effect when used in isolation.

Figure 2: Distribution of Amounts Donated




*Notes:* N=282. Kruskal-Wallis equality of populations rank test p-value=0.0676.

The main results of the paper are displayed in Table 5. We formally test for treatment effects on donations by again estimating equation 1. Here we examine as outcomes a dummy variable indicating that an individual gives to the charity (in Model (1)), the unconditional amount donated (in Model (2)), and the amount donated conditional on making a nonzero donation (in Model (3)). Model (2) captures the effect on dollars donated, which would be the most relevant outcome for a charity, while the other two together form a two-part model in which one part (1) isolates the extensive margin and the other (3) isolates the intensive margin. We also estimate a Tobit model to simultaneously account for responses on both the extensive and intensive margins (in Model (4)). Focusing first on the more comprehensive Tobit model, we find that both the "Group" and "Public" treatments significantly increase prosociality when used in isolation, and that the effect of "Group, Public" is also positive and marginally significant.[7] Turning to the linear models, we see the same pattern of coefficients. Specifically, Model (1) indicates that subjects are significantly more likely to

[7]The estimated effects of the three treatments are significantly different from each other. We can reject (with p-value 0.0314) the hypothesis that the "Group" and "Public" treatment dimensions have additive effects. We surmise that payoff commonality and public announcement of performance do not reinforce each others' effects because the combination invites comparison of oneself with others in the group. In evidence of the negative feelings generated by such comparisons, Card et al. (2012) find that information about peers' salaries causes those receiving below-median pay to report lower satisfaction and a higher prevalence of looking for a new job.

donate if they were compensated in groups rather than individually. The point estimate of the effect of public announcement alone was positive but half as large and not statistically significant. Model (2) indicates that announcing performance in public did significantly increase the amount donated under individual compensation, by approximately £0.76, on average, more than doubling the baseline mean contribution of £0.67. This treatment also shows a positive and marginally significant effect on the amount conditional on a nonzero donation (Model 3).[8] Overall, these results provide evidence that all treatments had significant positive spillover effects on prosocial behavior.

Table 5: Spillover effects on donations – Regression results

| | (1) Any Donation | (2) Amount | (3) Cond. Amount | (4) Tobit |
|---|---|---|---|---|
| Group, Private | 0.208** | 0.318 | 0.068 | 0.810** |
| | (0.080) | (0.212) | (0.316) | (0.369) |
| Individual, Public | 0.105 | 0.758** | 1.003* | 1.110** |
| | (0.084) | (0.359) | (0.547) | (0.535) |
| Group, Public | 0.196** | 0.194 | -0.084 | 0.649* |
| | (0.082) | (0.205) | (0.311) | (0.367) |
| Constant | 0.514*** | 0.665*** | 1.293*** | -0.296 |
| | (0.060) | (0.157) | (0.267) | (0.321) |
| N | 282 | 282 | 181 | 282 |
| Adj. R-Squared | 0.020 | 0.015 | 0.034 | |

Heteroskedasticity-robust standard errors are in parentheses. Columns (1)-(3) report results of OLS regressions. Dependent variables, by model, are an indicator for a nonzero donation (1), the amount donated (2), and the amount donated conditional on a nonzero donation (3). Model (4) is a Tobit model for donations, and reported coefficients are marginal effects on observed donations.

We provide several additional tests to explore two potential mechanisms for prosociality spillovers: income effects and awareness of others. For one, to the extent that the treatments increased earnings, earnings could have increased charitable donations. Though the earnings effects reported in Table 2 were not statistically significant, the point estimates for the two main treatments were positively signed. We offer six pieces of evidence that the prosociality spillovers were not driven by income effects. First, the point estimate in Table 3 for the effect of the "Group, Public" treatment on earnings was negative, yet this treatment significantly increased the probability of a donation. Second, the point estimates in Table 3 for the "Individual, Public" treatment indicate that it raised earnings by £0.828 (6.44 percent) and donations by £0.758 (113.99 percent), implying an income elasticity of 17.7 (=113.99/6.44), which is an order of magnitude larger than income elasticities estimated in the literature.[9] Third, as we show in Appendix Table B.5, the pattern of spillover effects is the

[8] Difference in conditional mean suggest responsiveness on the intensive margin but should be interpreted with caution given responses on the extensive margin, which may involve new gifts of positive-but-small amounts that would lower the conditional mean.

[9] Observational studies have examined income effects on donations using individual income tax data. Auten et al.

same across all cognitive levels. When we drop subjects with only one correct answer to the CRT questions, those whose earnings increased most under the treatments, we obtain noisier but larger estimated effects in the Tobit model estimated in Table 5. Fourth, we use random variation in earnings to show that the causal effect of income on donations was small. Specifically, we use the "Group, Private" and "Group, Public" treatment samples and regress donations on the amount of earnings that were generated by group members. Since groups were randomly assigned this provides random variation in income, and the regression results in Table 6 show that additional income had small and statistically insignificant effects on donations. Fifth, Appendix Table B.9 repeats the regressions above but includes a control for the subjects' total earnings (not included in our preferred specifications in Table 5 because income is endogenous), and results are unchanged. These results are consistent with the findings of both Drouvelis and Marx (mimeo) and Tonin and Vlassopoulos (2017) that earned income in experiments did not affect donations.

Table 6: Irrelevance of Earnings to Donation Decision

| | All Group Sessions | | Group, Private Sessions | | Group, Public Sessions | |
|---|---|---|---|---|---|---|
| | (1) Donation | (2) Any Donation | (3) Donation | (4) Any Donation | (5) Donation | (6) Any Donation |
| Groupmembers' Earnings | 0.035 | 0.006 | -0.044 | 0.023 | 0.125 | -0.016 |
| | (0.045) | (0.021) | (0.057) | (0.030) | (0.074) | (0.030) |
| Constant | 0.614 | $0.660^{***}$ | $1.384^{**}$ | $0.512^{*}$ | -0.193 | $0.849^{***}$ |
| | (0.387) | (0.191) | (0.542) | (0.282) | (0.589) | (0.265) |
| N | 141 | 141 | 72 | 72 | 69 | 69 |
| Adj. R-Squared | -0.004 | -0.007 | -0.010 | -0.006 | 0.020 | -0.012 |

Heteroskedasticity-robust standard errors are in parentheses. In odd-numbered models the dependent variable is the amount donated to the charity. In even-numbered models the dependent variable is an indicator for a nonzero donation.

A final piece of evidence relates to income effects as well as the nature of the Group treatments. This evidence appears in Appendix Table B.6, which presents results of regressing donations in Group treatments on the subject's own earnings, the earnings of the group, and the interaction of the two. Individuals' own earnings are potentially endogenous to the donation decision, but random assignment of groups insures exogeneity of group earnings, and the interaction of the two captures effects of relative performance on donations. For example, if being saddled with a low-performing group reduced donations, then the coefficient on the Groupmembers' Earnings term would be negative, and if this effect were stronger for high performers, then the coefficient on

(2002) obtain estimates ranging from 0.29 to 0.45 for the elasticity of charitable donations with respect to transitory annual income. Similarly, the corresponding estimates of Bakija and Heim (2011) are close to 0.33. In a laboratory study, Erkal et al. (2011) find an earnings elasticity of giving to lab-mates of 1.2 when payoffs are randomly assigned.

the interaction term would be significantly different from zero. However, we find no effects that are significantly different from zero at even the 10 percent level. This result, that group performance has no effect on donations (whether a subject is high- or low-performing), suggests that the donation spillovers are not driven by the revenue-sharing of the Group treatments, which would favor low performers and reduce the income of high performers. Rather, it would appear that what mattered was membership in the group, which was relevant for all subjects in these treatments.

We also consider whether spillovers are particular prevalent for subjects of a certain ability type revealed through the tasks. We employed both math and language tasks with the expectation that some subjects would perform better in one of these than the other. Appendix Table B.7 tests whether subjects who were above or below median for each task were the drivers of the prosociality spillovers that we find. For the "Group, Private" treatment, point estimates are somewhat larger for subjects who did well in either of the easy tasks but smaller for subjects who did well in either of the hard tasks. For the two "Public" treatments, estimates are larger for above-median subjects, regardless of the task. None of these differences are statistically significant. Spillovers appear to occur among all types of subjects, whether high or low ability and whether linguistically or mathematically inclined.[10]

Finally, we explore heterogeneity along a dimension related to our hypothesis that the treatments increase thoughts about others. One of the questions subjects were asked in the survey was "How many participants in the experiment do you know by name?" We used this question to split the sample into subjects who knew at least one other person in the laboratory and subjects who knew no other participants. Subjects who know other subjects by name are possibly most representative of members of real work groups and are probably most likely to think about others when treated with a connection to others in the laboratory, especially when the connection is a public announcement to everyone in the laboratory. We ran the same set of donation regressions on each of the subsamples, and the results appear in Table 7. The values of the constants in these regressions show that individuals who know others in the laboratory are no more generous than their peers in the baseline "Individual, Private" treatment. However, the estimated effects of each of the "Group" and "Public" treatments are greater among the sample of subjects who know others. In this subsample the effects are statistically significant, even at the 0.01 level for the "Individual, Public" treatment, despite the fact that it makes up only about one quarter of the full sample. Again, these results are not driven by income effects, as we show in Appendix Table B.10 that earnings in each treatment are quite

[10] We also test, in Appendix Table B.8, whether the spillover effect of public announcement of performance differed by the order in which subjects made their announcements. Because this order was randomly assigned and was irrelevant for "Private" treatments, it is not surprising that "Order" has no significant effect in general. More importantly, interactions of this variable with the "Public" treatments also produce insignificant effects, indicating that the order in which a subject made announcement in these treatments did not matter.

similar between subjects who know others and subjects who do not.

Table 7: Importance of Social Connections to Donation Decision

| | Subject Doesn't Know Others | | | Subject Knows Others | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Any Donation | Amount | Tobit | Any Donation | Amount | Tobit |
| Group, Private | 0.171* | 0.113 | 0.526 | 0.306** | 0.827** | 1.394** |
| | (0.095) | (0.259) | (0.452) | (0.152) | (0.338) | (0.556) |
| Individual, Public | -0.009 | 0.597 | 0.690 | 0.388*** | 1.169*** | 1.836*** |
| | (0.100) | (0.484) | (0.697) | (0.146) | (0.376) | (0.565) |
| Group, Public | 0.206** | 0.194 | 0.686 | 0.141 | 0.131 | 0.405 |
| | (0.092) | (0.262) | (0.447) | (0.172) | (0.238) | (0.533) |
| Constant | 0.549*** | 0.764*** | -0.186 | 0.421*** | 0.399** | -0.434 |
| | (0.070) | (0.205) | (0.394) | (0.116) | (0.171) | (0.413) |
| N | 204 | 204 | 204 | 78 | 78 | 78 |
| Adj. R-Squared | 0.027 | -0.001 | | 0.063 | 0.117 | |

Heteroskedasticity-robust standard errors are in parentheses. The sample is split into subjects who report knowing zero other subjects in the lab (models 1-3) and subjects who know at least one other subject in the lab (models 1-3). Dependent variables are either an indicator for a nonzero donation (models 1 and 4), or the amount donated. Models 3 and 6 are Tobit models for donations, and reported coefficients are marginal effects on observed donations.

Two potential explanations for the observed treatment effects on donation behavior are social image/comparisons and empathy. Regarding the first explanation, psychological research (surveyed in Corcoran et al., 2011) demonstrates that social comparisons - comparisons between the self and others - are a fundamental mechanism influencing individuals' behavior. Knowing more subjects in the lab may have prompted subjects to increase their donations in expectation of talking about their decisions with the people they know after the end of the experiment. An alternative (or complementary) explanation is suggested by research that finds that subjects who have been primed in a high empathy condition are more likely to exhibit altruistic behavior (e.g., Batson et al., 1981; Toi and Bateson, 1982; Andreoni and Rao, 2011). Both the Group and Public treatments involve others, and these others are more salient when some are known, which may have shifted subjects' attention from their private gain to the public good. We feel that both the social-comparison and identity-priming explanations may be relevant, and in future research it would be valuable to devise separate tests for each of these mechanisms. Our evidence suggests that "thinking about others," as defined by the presence of at least one of these mechanisms, is the most likely driver of the estimated prosociality spillovers of working with others.

## 5 Discussion

In this section we address three goals. The first is to consider the evidence for which mechanism drives the spillover effects we find. The second is to place these findings in the larger literature on charitable giving. The third is to note limitations of the study.

We conducted this research because a variety of noted studies implied that working with others might have spillover effects on prosociality in other domains. Existing work suggests a variety of mechanisms by which such spillovers could occur. One possibility is the idea of "conscience accounting," as proposed by Gneezy et al. (2014), who found that behaving immorally increased subjects' guilt and subsequent donations. In our setting, subjects in the social treatments might feel they could have done more in terms of performance, which might lead them to donate more. However, this explanation does not appear consistent with the full set of results. For one, the social treatments do not have a significant average effect on performance, and hence most subjects were likely working as hard as they would have in the control group.[11] For another, high-CRT subjects do appear to free-ride in terms of performance when in groups, yet the effect on their donations is similar to the effect for the average subject.

Another possibility is that the Public treatments increased donations because the announcement of performance in the tasks caused subjects to incorrectly infer that the amount of their donation would also become public. While this was the objective in studies in which donations were announced, there are a few reasons this possibility seems unlikely to explain our results. One reason is the design, in which subjects were informed in advance that performance in the tasks would be announced publicly, and there was no such information about announcement of donations. On the contrary, subjects in all treatments received the instructions "You will now be given an opportunity to donate some of your income from the experiment to a charity, and last, you will be asked to complete a survey." Our findings across multiple treatments also argue against the misinterpretation mechanism. The "Group, Private" treatment did not involve announcement of performance, and hence, subjects would have no reason to believe their donations would be announced. Nonetheless, this treatment (a) significantly increased the number of donors, (b) produced larger point estimates in all spillover regressions than the "Group, Public" treatment (Table 5), and (c) disproportionately affected subjects who knew another subject in the laboratory (Table 7), for whom we find larger, and statistically-significant, effects on both number of donors and amount donated.

[11] Appendix Table B.3 shows that for individual tasks there were some significant differences in the number of items left blank, but the sign of these differences is not consistent across Group treatments, and there are almost no significant differences in the number of correct responses.

In our view, our work treatments most likely generate prosociality spillovers because they encourage subjects to think about individuals other than themselves. Past research has found that individuals become more generous when they are asked to put themselves in others' shoes (Andreoni and Rao, 2011) or when they learn that others have been generous (Bicchieri and Xiao, 2009). Greater awareness of or attention to other individuals could therefore increase donations by making subjects think more about either the recipients of their gifts or about the likely donations of the peers whose presence has been made more salient. The fact that our effects are strongest among subjects who know other subjects in the laboratory suggests the latter channel, i.e. that subjects consider the likelihood of generosity by others and then attempt to adhere to this norm.

Our finding of prosociality spillovers fits into a growing body of economic research on social motivations for giving. For example, Meer (2011) found that college graduates were more generous toward their alma mater when socially connected to their solicitor. One result that has proven robust to numerous settings is that informing individuals about the donations of others tend to increase these individuals' donations (List and Lucking-Reiley, 2002; Shang and Croson (2009); Huck and Rasul, 2011; Huck et al., 2015). Donations have also been shown to respond to expressions of support for the charity (Kessler, 2017), solicitor attractiveness (Landry et al., 2006; Landry et al., 2010), repetition of requests (Meer and Rosen, 2011), and social pressure (DellaVigna et al., 2012). The results of our experiment suggest that merely interacting with a random group of people can affect the degree to which we look outwards and donate.

Finally, we note some important limitations of the study. The spillover effects we estimate in the controlled environment of the laboratory may differ from those occurring in real-world workplaces. In particular, we estimate the effects of two specific aspects common to working with others, but we have abstracted from other important aspects, such as social interplay, strategy, and hierarchy. These aspects of groups may have either positive or negative spillovers, and they may overwhelm the effects of the aspects we examine. We hope that our findings of prosociality spillovers from some aspects of working with others will motivate research into other aspects and working environments. The spillovers that we find are small, and we would not expect these to be considered by the firm, but future quasi-experimental research could estimate the broader spillover effects of working with others and test whether these are large enough to justify policy that encourages (or discourages) interconnected work.

## 6 Conclusion

We have examined the impact of two aspects of group work on individuals' work performance and prosocial behavior: compensation based on individual or group performance and performance that was either private or public information. We find that the studied aspects of working with others have little effect on performance but significant spillover effects. Specifically, the public announcement of performance more than doubles the amount donated to the charity. Group-based compensation significantly increases the probability of subjects' donating to the charity. Each has significant effects in a Tobit regression. Our treatment effects follow similar patterns across subjects with different cognitive abilities and incomes but are strongest among subjects who know other subjects in the laboratory.

The spillover effects we estimate have relevance for manager incentives, workplace charity campaigns, retail-transaction solicitations, and the design of experiments on prosocial behavior. Managers can manipulate the extent to which employees are aware of and affected by each other in hopes of increasing productivity, and our results suggest externalities that should be factored into such manipulations. Workplace charity campaigns, such as those run by the United Way, could convene meetings of groups of workers before requesting donations. Charities that solicit donations through retail transactions may want to focus their efforts in busy shopping centers. Researchers should consider the possibility that audiences have effects not only through social sanctioning but may also simply increase the degree to which subjects think about others. For all of these audiences it would be worth exploring whether other aspects of working with others have similar spillover effects on prosocial behavior.

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## Appendix A: Experiment Instructions

*[Note: These are the written instructions as presented to subjects facing the "Individual, Private" treatment in Sequence 1. Amendments to the remaining treatments are given in square brackets. For Sequence 2, the order of the tasks was as follows: hard math, easy math, hard word, easy word]*.

**INSTRUCTIONS**

Welcome! You are about to take part in an experiment. This experiment is run by the "Birmingham Experimental Economics Laboratory" and has been financed by various research foundations. Just for showing up you have already earned £2.50. You can earn additional money depending on the decisions made by you and other participants. It is therefore very important that you read these instructions with care.

*It is important that you remain silent and do not look at other people's work. If you have any questions, or need assistance of any kind, please raise your hand and an experimenter will come to you. You may use the provided scrap paper but no phones, calculators, or other devices. If you use a device, talk, laugh, exclaim out loud, etc., you will be asked to leave and you will not be paid. We expect and appreciate your following of these rules.*

We will first jointly go over the instructions. After we have read the instructions, you will have time to ask clarifying questions. Please do not touch the computer or its mouse until you are instructed to do so. Thank you.

This experiment consists of four different timed tasks. You will be paid a fixed amount of money for each correct answer you provide in each task. [**Group**: At the beginning of the experiment, you will be put in a group with two other people selected at random. You will be paid a fixed amount of money for each correct answer your group of 3 people provides in each task. Your group earnings will be divided equally among the 3 group members.] The total amount of money you will earn from this experiment will be £2.50 for showing up plus the sum of your earnings from each task of the experiment.

After Task 4 you will be told how many correct responses you gave in each of the tasks. [**Public:** You will then be asked by the experimenter to stand up and tell the others in the room the number of correct answers you provided in each of the four tasks.] After this you will collect your earnings.

Following these instructions you will find the instructions for Task 1 of the experiment. You will receive new instructions for the other tasks once everyone in the room has completed Task 1.

**Task 1 (These instructions also appeared onscreen)**

Task 1 consists of solving 2-number multiplication problems like the following example:

5 x 7 = 35.                    9 x 8 = 72.

You will have 5 minutes to provide answers.

You will be paid 3 pence for each correct answer provided during the 5 minute time limit.

[**Group:** Your group will be paid 3 pence for each correct answer provided during the 5 minute time limit. These earnings will be divided equally among the 3 members of your group. In other words, you will receive 1 penny for each correct answer provided by you or either of your group members.]

To answer a problem, you will simply type the numbers on the keyboard, then press OK and another problem will appear. You can choose not to answer a question by pressing the OK button. The answer will then be recorded as being incorrect and you will be moved to the next problem. To help with time management, there will be a clock counting down the seconds for the 5 minute duration.

**Task 2 (These instructions also appeared onscreen)**

Task 2 consists of solving 2-number multiplication problems like the following example:

10 x 97 = 970.              20 x 30 = 600.

You will have 5 minutes to provide answers.

You will be paid 21 pence for each correct answer provided during the 5 minute time limit.

[**Group:** Your group will be paid 21 pence for each correct answer provided during the 5 minute time limit. These earnings will be divided equally among the 3 members of your group. In other words, you will receive 7 pence for each correct answer provided by you or either of your group members.]

To answer a problem, you will simply type the numbers on the keyboard, then press OK and another problem will appear. You can choose not to answer a question by pressing the OK button. The answer will then be recorded as being incorrect and you will be moved to the next problem. To help with time management, there will be a clock counting down the seconds for the 5 minute duration.

**Task 3 (These instructions also appeared onscreen)**

Task 3 consists of arranging pairs of letters to form words like the following examples:

PO, TS = POTS.     GE, MA = MAGE.

You must use all the letters. You can change the order of the pairs but you cannot change the order of the two letters within each pair. You will have 5 minutes to provide answers.

You will be paid 3 pence for each correct answer provided during the 5 minute time limit.

[**Group:** Your group will be paid 3 pence for each correct answer provided during the 5 minute time limit. These earnings will be divided equally among the 3 members of your group. In other words, you will receive 1 penny for each correct answer provided by you or either of your group members.]

To answer a problem, you will simply type the word on the keyboard, then press OK and another problem will appear. You can choose not to answer a question by pressing the OK button. The answer will then be recorded as being incorrect and you will be moved to the next problem. To help with time management, there will be a clock counting down the seconds for the 5 minute duration.

**Task 4 (These instructions also appeared onscreen)**

Task 4 consists of arranging pairs of letters to form words like the following examples:

TR, EA, TS, RE = RETREATS.     CU, FF, LI, NK = CUFFLINK.

You must use all the letters. You can change the order of the pairs but you cannot change the order of the two letters within each pair. You will have 5 minutes to provide answers.

You will be paid 21 pence for each correct answer provided during the 5 minute time limit.

[**Group:** Your group will be paid 21 pence for each correct answer provided during the 5 minute time limit. These earnings will be divided equally among the 3 members of your group. In other words, you will receive 7 pence for each correct answer provided by you or either of your group members.]

To answer a problem, you will simply type the word on the keyboard, then press OK and another problem will appear. You can choose not to answer a question by pressing the OK button. The answer will then be recorded as being incorrect and you will be moved to the next problem. To help with time management, there will be a clock counting down the seconds for the 5 minute duration.

*At the end of Task 4, the experimenter makes the following announcement:* You can now see the number of correct answers you gave in each of the four tasks. Please give me a moment to print the results.

**[Public:** I will now call you one by one to stand up and tell the others in the room the number of correct answers you provided in each of the four tasks.]

OK, we are almost done! You will now be given an opportunity to donate some of your income from the experiment to a charity, and last, you will be asked to complete a survey.

*After this announcement, subjects received the following instructions onscreen:*

Thank you, you have completed all 4 Tasks. Your total earnings from today's experiment (including your £2.50 show-up fee) sum to £[***Autofill***].

Would you like to donate some of your earnings to Acorns Children's Hospice of Birmingham [**Written with hyperlink to separate linked screen**]? If so, please enter the amount (between £0 and £[***Autofill***]) in the box provided.

[***Input field***]

[***OK button***]

Thank you. The experimenter will now ask you to complete a short questionnaire and will give you the payment for your earnings.

---

*Separate linked screen*

Acorns provides specialist care for babies, children and young people who are life-limited or life-threatened. As well as this, Acorns offers support for the whole family; including mums, dads, brothers and sisters, helping them to cope with the huge challenges they face living with and caring for a child with complex disabilities. In order to continue its work, the charity must raise more than £9 million every year, and it relies on the public for the vast majority of that amount.

[***OK button***]

# Appendix B: Additional Figures and Tables

Table B.1: Covariates Balanced Across Treatments

| | (1) CRTscore | (2) Male | (3) Age | (4) UK | (5) Married | (6) Knows Labmate |
|---|---|---|---|---|---|---|
| Group, Private | 0.079 | 0.085 | -0.304 | 0.110 | 0.040 | 0.034 |
| | (0.171) | (0.084) | (0.816) | (0.083) | (0.045) | (0.077) |
| Individual, Public | 0.209 | 0.233*** | 0.800 | -0.007 | 0.071 | 0.024 |
| | (0.187) | (0.082) | (0.799) | (0.085) | (0.049) | (0.076) |
| Group, Public | 0.118 | 0.093 | -0.487 | 0.124 | 0.001 | -0.040 |
| | (0.191) | (0.085) | (0.752) | (0.083) | (0.040) | (0.074) |
| Constant | 1.143*** | 0.443*** | 21.957*** | 0.529*** | 0.057** | 0.271*** |
| | (0.128) | (0.060) | (0.582) | (0.060) | (0.028) | (0.054) |
| N | 282 | 282 | 280 | 282 | 281 | 282 |
| Adj. R-Squared | -0.006 | 0.018 | 0.001 | 0.004 | 0.001 | -0.007 |

Heteroskedasticity-robust standard errors are in parentheses. The "outcomes" are, respectively, the subject's Cognitive-Reflexive Test score, a dummy for male, age, a dummy for United Kingdom nationality, a dummy for being married, and a dummy for knowing another subject in the laboratory.

Table B.2: Characteristics by Choice of Whether to Donate

| | (1) CRTscore | (2) Male | (3) Age | (4) UK | (5) Married | (6) Knows Labmate |
|---|---|---|---|---|---|---|
| Any donation | -0.082 | 0.095 | 1.038* | -0.107* | 0.072** | -0.001 |
| | (0.135) | (0.062) | (0.528) | (0.060) | (0.031) | (0.056) |
| Constant | 1.297*** | 0.485*** | 21.297*** | 0.653*** | 0.040** | 0.277*** |
| | (0.108) | (0.050) | (0.376) | (0.048) | (0.019) | (0.045) |
| N | 282 | 282 | 280 | 282 | 281 | 282 |
| Adj. R-Squared | -0.002 | 0.005 | 0.008 | 0.007 | 0.012 | -0.004 |

Heteroskedasticity-robust standard errors are in parentheses. The "outcomes" are, respectively, the subject's Cognitive-Reflexive Test score, a dummy for male, age, a dummy for United Kingdom nationality, a dummy for being married, and a dummy for knowing another subject in the laboratory. "Any donation" is a dummy variable, and its coefficient captures the difference from the average value of the characteristic among subjects choosing to not donate.

Table B.3: Performance by Type of Task

Items Answered Correctly

| | (1) Own Earnings | (2) Easy Math | (3) Easy Words | (4) Easy Tasks | (5) Hard Math | (6) Hard Words | (7) Hard Tasks |
|---|---|---|---|---|---|---|---|
| Group, Private | 0.770 | 0.674 | 4.887 | 5.560 | 0.010 | 2.864* | 2.874 |
| | (0.751) | (8.097) | (3.357) | (9.800) | (1.359) | (1.583) | (2.394) |
| Individual, Public | 0.828 | 4.644 | 4.957** | 9.601 | 0.597 | 1.973 | 2.570 |
| | (0.544) | (7.416) | (2.256) | (8.494) | (0.906) | (1.235) | (1.817) |
| Group, Public | -0.232 | -5.460 | -0.607 | -6.068 | -1.425 | 1.187 | -0.239 |
| | (0.539) | (7.792) | (3.591) | (9.406) | (1.115) | (1.616) | (1.673) |
| Constant | 12.841*** | 150.243*** | 99.086*** | 249.329*** | 8.643*** | 16.886*** | 25.529*** |
| | (0.414) | (6.164) | (2.433) | (7.260) | (0.919) | (0.788) | (1.130) |
| N | 282 | 282 | 282 | 282 | 282 | 282 | 282 |
| Adj. R-Squared | 0.007 | -0.004 | 0.011 | 0.002 | 0.007 | 0.006 | 0.007 |

Items Attempted

| | (1) Easy Math | (2) Easy Words | (3) Easy Tasks | (4) Hard Math | (5) Hard Words | (6) Hard Tasks |
|---|---|---|---|---|---|---|
| Group, Private | 1.551 | 4.705 | 6.256 | 11.520*** | 5.896* | 17.416*** |
| | (7.841) | (3.199) | (9.550) | (3.745) | (2.991) | (5.516) |
| Individual, Public | 6.237 | 5.716** | 11.954 | 3.461* | 4.968** | 8.429** |
| | (7.292) | (2.394) | (8.560) | (1.699) | (1.817) | (3.002) |
| Group, Public | -4.867 | -1.158 | -6.025 | -0.112 | 0.685 | 0.573 |
| | (7.815) | (3.433) | (9.561) | (1.337) | (2.519) | (3.388) |
| Constant | 153.157*** | 102.129*** | 255.286*** | 12.271*** | 26.243*** | 38.514*** |
| | (6.129) | (2.287) | (7.306) | (0.662) | (1.333) | (1.323) |
| N | 282 | 282 | 282 | 282 | 282 | 282 |
| Adj. R-Squared | -0.003 | 0.017 | 0.005 | 0.042 | 0.023 | 0.053 |

Items Left Blank

| | (1) Easy Math | (2) Easy Words | (3) Easy Tasks | (4) Hard Math | (5) Hard Words | (6) Hard Tasks |
|---|---|---|---|---|---|---|
| Group, Private | 0.262 | -0.100 | 0.162 | 11.863*** | 2.790 | 14.654*** |
| | (0.334) | (0.171) | (0.366) | (3.602) | (2.172) | (4.633) |
| Individual, Public | 0.569 | 0.090 | 0.659 | 2.909** | 2.781* | 5.691*** |
| | (0.417) | (0.193) | (0.418) | (1.034) | (1.324) | (1.907) |
| Group, Public | -0.151 | -0.353** | -0.505 | 1.586 | 0.095 | 1.681 |
| | (0.273) | (0.154) | (0.294) | (1.153) | (1.614) | (2.399) |
| Constant | 0.571** | 0.614*** | 1.186*** | 0.429** | 6.543*** | 6.971*** |
| | (0.246) | (0.180) | (0.286) | (0.176) | (1.134) | (1.175) |
| N | 282 | 282 | 282 | 282 | 282 | 282 |
| Adj. R-Squared | 0.011 | 0.006 | 0.022 | 0.047 | 0.007 | 0.046 |

Standard errors clustered by subject are in parentheses. In Models (1) and (2), the dependent variables are the number of correct answers in the easy and hard tasks, respectively. In Models (3)-(6), we include as dependent variables the number of correct responses in each of the four tasks. This allows us to disentangle which task can explain better any potential differences recorded in the pooled data reported in the first two models.

Table B.4: Effects on Quintiles of Total Earnings

| Percentile: | 20th | 40th | 60th | 80th |
|---|---|---|---|---|
| Group, Private | 0.580 | 0.300 | 0.570 | 0.700 |
| | (0.594) | (0.623) | (0.456) | (0.815) |
| Individual, Public | -0.240 | 0.630 | 1.470*** | 1.080 |
| | (0.596) | (0.625) | (0.458) | (0.818) |
| Group, Public | 0.430 | 0.350 | -0.420 | -0.680 |
| | (0.600) | (0.630) | (0.461) | (0.823) |
| Constant | 10.620*** | 12.150*** | 13.470*** | 15.090*** |
| | (0.423) | (0.444) | (0.325) | (0.580) |
| N | 282 | 282 | 282 | 282 |
| Adj. R-Squared | | | | |

Notes: In all models the dependent variable is an individual's total earnings. Each column displays results of a quantile regression for the quantile listed at the top of the column.

Table B.5: Donation Effects by Cognitive Level

| | All Subjects | CRT=0 | CRT=1 | CRT=2 | CRT=3 |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Group, Private | 0.810** | 1.347* | 0.417 | 0.342 | 1.581 |
| | (0.369) | (0.801) | (0.537) | (0.602) | (0.961) |
| Individual, Public | 1.110** | 2.231* | 0.653 | 0.070 | 1.601 |
| | (0.535) | (1.304) | (0.657) | (0.708) | (1.347) |
| Group, Public | 0.649* | 1.279* | 0.205 | -0.171 | 1.489 |
| | (0.367) | (0.751) | (0.522) | (0.764) | (0.961) |
| Constant | -0.296 | -0.823 | 0.372 | 0.034 | -1.025 |
| | (0.321) | (0.722) | (0.404) | (0.450) | (0.937) |
| N | 282 | 93 | 75 | 66 | 48 |

Heteroskedasticity-robust standard errors are in parentheses. All models are Tobit models for donations, and reported estimates are marginal effects on observed donations. Each model reports estimates for a group with a different CRT score.

Table B.6: Donation Invariance to Own and Others' Earnings

| | All Group | | Group, Private | | Group, Public | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Donation | Any Donation | Donation | Any Donation | Donation | Any Donation |
| Groupmembers' Earnings | -0.005 | -0.109 | -0.239 | -0.259 | -0.088 | 0.017 |
| | (0.210) | (0.109) | (0.337) | (0.182) | (0.331) | (0.159) |
| Own Earnings | -0.040 | -0.076 | -0.157 | -0.188 | -0.145 | 0.030 |
| | (0.131) | (0.071) | (0.226) | (0.115) | (0.191) | (0.101) |
| Own Earnings * Groupmembers' | 0.003 | 0.009 | 0.014 | 0.021 | 0.018 | -0.003 |
| | (0.016) | (0.008) | (0.025) | (0.013) | (0.027) | (0.013) |
| Constant | 1.138 | 1.644* | 3.543 | 3.092* | 1.510 | 0.469 |
| | (1.747) | (0.962) | (3.124) | (1.663) | (2.394) | (1.276) |
| N | 141 | 141 | 72 | 72 | 69 | 69 |
| Adj. R-Squared | -0.017 | -0.011 | -0.027 | 0.012 | -0.004 | -0.038 |

Heteroskedasticity-robust standard errors are in parentheses.

Table B.7: Donation Spillovers Uncorrelated with Task Performance

| | Easy Math | | Hard Math | | Easy Word | | Hard Word | |
|---|---|---|---|---|---|---|---|---|
| | Low | High | Low | High | Low | High | Low | High |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Group, Private | 0.640 | 0.933** | 0.885 | 0.776 | 0.408 | 1.217** | 0.883* | 0.680 |
| | (0.590) | (0.448) | (0.570) | (0.475) | (0.564) | (0.469) | (0.497) | (0.514) |
| Individual, Public | 0.880 | 1.299** | 0.601 | 1.440** | 1.004 | 1.326** | 0.518 | 1.618** |
| | (0.879) | (0.619) | (0.756) | (0.722) | (0.890) | (0.638) | (0.659) | (0.800) |
| Group, Public | 0.573 | 0.645 | 0.550 | 0.681 | 0.554 | 0.797* | 0.286 | 0.976* |
| | (0.571) | (0.447) | (0.522) | (0.513) | (0.554) | (0.455) | (0.521) | (0.507) |
| Constant | -0.091 | -0.437 | -0.077 | -0.478 | -0.129 | -0.519 | -0.220 | -0.311 |
| | (0.502) | (0.385) | (0.431) | (0.456) | (0.478) | (0.407) | (0.404) | (0.446) |
| N | 139 | 143 | 130 | 152 | 137 | 145 | 140 | 142 |

Heteroskedasticity-robust standard errors are in parentheses.

Table B.8: Donation Spillovers Uncorrelated with Order of Announcement

| | (1) Any Donation | (2) Amount | (3) Tobit |
|---|---|---|---|
| Order * Individual, Public | 0.000 | 0.013 | 0.016 |
| | (0.013) | (0.070) | (0.090) |
| Order * Group, Public | -0.018 | -0.028 | -0.064 |
| | (0.014) | (0.033) | (0.057) |
| Group, Private | 0.207** | 0.315 | 0.795** |
| | (0.081) | (0.210) | (0.366) |
| Individual, Public | 0.100 | 0.639 | 0.962 |
| | (0.143) | (0.680) | (0.935) |
| Group, Public | 0.341** | 0.423 | 1.169* |
| | (0.135) | (0.385) | (0.614) |
| Order | 0.005 | 0.013 | 0.024 |
| | (0.008) | (0.020) | (0.034) |
| Constant | 0.470*** | 0.555** | -0.492 |
| | (0.088) | (0.272) | (0.463) |
| N | 282 | 282 | 282 |
| Adj. R-Squared | 0.016 | 0.008 | |

Heteroskedasticity-robust standard errors are in parentheses.

Table B.9: Spillover effects on donations – Controlling for earnings

| | (1) Any Donation | (2) Amount | (3) Cond. Amount | (4) Tobit |
|---|---|---|---|---|
| Group, Private | 0.207** | 0.324 | 0.080 | 0.815** |
| | (0.081) | (0.203) | (0.296) | (0.363) |
| Individual, Public | 0.105 | 0.764** | 1.014* | 1.116** |
| | (0.084) | (0.352) | (0.534) | (0.526) |
| Group, Public | 0.196** | 0.192 | -0.087 | 0.648* |
| | (0.082) | (0.209) | (0.318) | (0.371) |
| Constant | 0.504*** | 0.760 | 1.473 | -0.205 |
| | (0.153) | (0.606) | (0.980) | (0.910) |
| N | 282 | 282 | 181 | 282 |
| Adj. R-Squared | 0.016 | 0.012 | 0.028 | |

Heteroskedasticity-robust standard errors are in parentheses. Columns (1)-(3) report results of OLS regressions. Dependent variables, by model, are an indicator for a nonzero donation (1), the amount donated (2), and the amount donated conditional on a nonzero donation (3). Model (4) is a Tobit model for donations, and reported coefficients are marginal effects on observed donations. All regressions the same as in Table 5 except for the inclusion of a control variable for the subjects' total payoff.

Table B.10: Income Patterns by Whether Subject Knows Labmates

| | All Subjects | Subject Doesn't Know Others | Subject Knows Others |
|---|---|---|---|
| | (1) | (2) | (3) |
| Group, Private | 0.770 | 0.717 | 0.801 |
| | (0.573) | (0.674) | (1.073) |
| Individual, Public | 0.828 | 0.722 | 1.012 |
| | (0.538) | (0.647) | (0.946) |
| Group, Public | -0.232 | -0.034 | -0.750 |
| | (0.553) | (0.648) | (1.095) |
| Constant | 12.841*** | 12.622*** | 13.429*** |
| | (0.349) | (0.430) | (0.563) |
| N | 282 | 204 | 78 |
| Adj. R-Squared | 0.007 | -0.003 | -0.003 |

Heteroskedasticity-robust standard errors are in parentheses.