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Studying deception without deceiving participants: An experiment of deception experiments

Federica Alberti and Werner Güth*

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Abstract

Banning deception in economic experiments does not exclude experiments with participants in the role of experimenters who can gain by deceiving those in the role of participants. We compare treatments with and without possible deception by experimenter-participants to test whether deception affects behaviour of participant-participants in a dictator experiment and whether participants in the role of experimenters engage in deception. We find no difference in behaviour of participant-participants between the treatments whereas most participants in the role of experimenters engage in deception.

Keywords: Experimental economic methods; Deception; Experiments.

JEL classification: A12; C90.

1 Introduction

Deception in experiments is not just an academic discussion in the ivory tower of experimental researchers (for a discussion of the use of deception in experiments, see e.g. Hey 1991 and Davis and Holt 1993). If one puts a ban on the use of certain practices such as deception in experiments one can always avoid being guilty by letting others engage in such practices. In this perspective, the use of deception in experiments is analogous to the payment of unfair wages on the labour market. One can always avoid being guilty of engaging in the payment of unfair wages to workers by letting an independent but properly incentivized subcontractor to engage in that practice.[1]

This is exactly how we have implemented our study of deception experiments. We did not engage ourselves in deception but, as experimenters, we allowed 'experimenter-participants' to engage in deception and provided them

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[1]Uri Gneezy has pointed out to us the analogy of studying deception experiments without deception and such subcontracting.

with a strong incentive to deceive 'participant-participants'. The experimental scenario was a dictator game in which two participant-participants had to make their decisions without knowing that the experimenter-participant had the possibility to increse her payoff substantially by excluding one of them from interaction. The experiment was repeated after debriefing without forewarning, with the two participant-participants playing in the same role and with the experimenter-participant being unable to exclude any participant-participants from interaction. We used this experimental scenario to test for any difference in behaviour of participant-participants before and after deception. Surprisingly, we found no significant difference. We also tested whether giving people a relatively strong incentive to deceive others can induce them to engage in deception. Our results indicate that this is the case.

Our study of deception without deceiving participants makes a contribution to the recent debate on the use of deception in experiments (see e.g. Bonetti 1998, Hey 1998, McDaniel and Starmer 1998, Ortmann and Hertwig 2002, and Hertwig and Ortmann 2008).[2] It also adds important evidence to the growing body of literature on deception, emotions, and incentives (see e.g. Gneezy 2005, Charness and Dufwenberg 2005, and Sutter 2009). The experimental scenario is described below, followed by a presentation of the experimental protocol and research hypotheses, and by an illustration of the experimental results. Some discussion and final remarks conclude the paper.

2 The experimental scenario

In the experimental scenario, there are an experimenter-participant, who is supposed to run a dictator game, and two participant-participants, playing the dictator game. More precisely, there are three roles:

- Role E of an experimenter-participant,
- Role A of an allocator, and
- Role R of a recipient.

We will sometimes refer to E, A, and R, respectively, as experimenter-participant, allocator, and recipient.

In the dictator game, the allocator is given an endowment p to share with the recipient. p can be either small, i.e. $p = \underline{p}$, or large, i.e. $p = \overline{p}$, thus $0 < \underline{p} < \overline{p}$. Notice that neither A nor R know whether $\underline{p}$ can be shared or $\overline{p}$ can be shared when they make their decision. They only know the probability distribution, i.e. how likely is $p = \overline{p}$ to occur.

To run the dictator game, E receives an endowment e. This also includes the show-up fee s to each and both the allocator and recipient, thus $e - 2s > 0$.

[2] One major concern with the use of deception in experiments is that it may affect behavior of subjects in future experiments. Ortmann and Hertwig [2002] review a wide range of psychology studies involving deception. Their main conclusion is that deception generates suspicion and this is likely to alter behavior in experiments. Jamison et al. [2008] test the effect of deception regarding the identity of other players on future behavior in other experiments. They find evidence that deception has an impact on both the selection of subjects and the behavior of subjects who return to the lab. However, they cannot differentiate between the effect of selection of subjects and the effect of deception.

E too does not know whether $\underline{p}$ can be shared or $\overline{p}$ can be shared between A and R; she only knows the probability distribution.

In the experiment there are two treatments. In the main treatment, E can save the show-up fee to R by not hiring R. In the control treatment, E is exogenously imposed to hire both A and R.

Let us first describe the rules of the main treatment. First, E chooses between 'only A' and 'both A and R'. Second, A chooses his allocation $\underline{a}$ for $p = \underline{p}$ as well as $\overline{a}$ for $p = \overline{p}$, where $0 \leq \underline{a} \leq \underline{p}$ and $0 \leq \overline{a} \leq \overline{p}$. Third, without knowing the amounts $\underline{a}$ for $p = \underline{p}$ and $\overline{a}$ for $p = \overline{p}$, R chooses his minimum acceptable allocation $\underline{m}$ for $p = \underline{p}$ and also $\overline{m}$ for $p = \overline{p}$, where $0 \leq \underline{m} \leq \underline{p}$ and $0 \leq \overline{m} \leq \overline{p}$.[3] Fourth, A and R learn that E actually had the possibility to choose 'only A'. Fifth, E, A and R are informed that A and R must play the same dictator game once again, and this time E is exogenously imposed to choose both 'A and R'. Finally, after having made their decision for the second task, A and R learn their respective choices for both tasks, whether $\underline{p}$ can be shared or $\overline{p}$ can be shared for both tasks, and also whether E chose 'only A' or 'both A and R' in the first task.

Payoffs for each and both tasks and each and all roles in the main treatment are summarized in Table 1. In the first task, if E chooses 'both A and R', A receives $\underline{p} - \underline{a} + s$ for $p = \underline{p}$ and $\overline{p} - \overline{a} + s$ for $p = \overline{p}$; E receives $e - 2s$ for both $p = \underline{p}$ and $p = \overline{p}$; R receives $\underline{a} + s$ if $\underline{a} \geq \underline{m}$ and s if $\underline{a} < \underline{m}$ for $p = \underline{p}$ and $\overline{a} + s$ if $\overline{a} \geq \overline{m}$ and s if $\overline{a} < \overline{m}$ for $p = \overline{p}$. In contrast, if E chooses 'only A', A again receives $\underline{p} - \underline{a} + s$, for $p = \underline{p}$, and $\overline{p} - \overline{a} + s$, for $p = \overline{p}$; E receives $e - s + \underline{a}$ for $p = \underline{p}$ and $e - s + \overline{a}$ for $p = \overline{p}$; R receives nothing. In the second task, E cannot choose between 'only A' and 'both A and R', thus payoffs to E, A, and R are the same as when E chooses 'both A and R'.

In the control treatment all what differs is that the decision of the experimenter-participant is always exogenously imposed to be 'both A and R'. Thus, payoffs are the same as when E chooses 'both A and R' in the main treatment.

3 The experimental protocol

The obvious difficulty with implementing the main treatment was to guarantee the availability of participants in the role R who, depending on the choice of the experimenter-participant, might be excluded from interaction. Hence, without informing any participants about this (if not at the end of the experimental session), the non-hired recipient received the show-up fee$s$ from the experimenters, i.e. the authors of this paper. Otherwise payments were due as detailed in Section 2 [where one task was randomly drawn for payment at the end of the session and the probability was equal for the two tasks].

We were naturally interested in 'seducing' the experimenter-participant while persuading the allocator and recipient that the experimenter-participant would gain a considerable amount by excluding the recipient from interaction.[4] There-

[3] Thus, like in reward allocation (see Shapiro 1975 and Mikula 1973) and dictator experiments (see Forsythe et al. 1994), R cannot punish A but can reject an allocation that is unacceptable for him, and thus possibly voice his anger (see Xiao and Houser 2005).

[4] Ortmann and Hertwig [2002] report on evidence from psychology studies that direct experience of deception is likely to affect behavior in experiments, whereas it is dubious whether the mere possibility of deception can alter behavior in experiments.

fore, we set $e = 15$ and $s = 5$, with 1 point=1 EURO, also to compensate participants in the role R in case of a low allocation. The other parameters were $\underline{p} = 8$ and $\overline{p} = 24$. The latter was 80% likely, because we predicted allocators for $p = \overline{p}$ to hide greed by allocating $\overline{a} = \frac{\underline{p}}{2}$ only.

We invited 51 groups of participants for the roles E, A, and R for the 8 sessions that we ran for the main treatment and 50 groups of participants for the roles E, A, and R for the 8 sessions that we ran for the control treatment. [In the sessions for the control treatment, we decided to rule out any doubt that there exists an experimenter-participant for each allocator and recipient. Thus, as for the main treatment, a session included groups of three participants for the roles E, A, and R.] In the first task, in the 101 groups it was commonly known that E is an experimenter-participant who observes the interaction between A and R (for details of the main experimental instructions, see Section B.2 in Appendix B).[5] Thus, no A or R participant in the main treatment had reasons to believe that E may actually choose to hire only A. Moreover, no participant was initially aware that there will be another task after the initial one (for details of further experimental instructions, see Section B.2 in Appendix B).[6]

Allocations and minimum acceptable allocations were restricted to integers in the range $0 \leq \underline{a} \leq 8$, $0 \leq \overline{a} \leq 24$, $0 \leq \underline{m} \leq 8$ and $0 \leq \overline{m} \leq 24$, respectively. The parameters $\underline{p} = 8$, $\overline{p} = 24$, $s = 5$ as well as the probability 80% for $p = \overline{p}$ were commonly known whereas $e = 15$ was initially known only to E. Since we were interested in choices of allocators and recipients before and after deception, their roles were kept fixed throughout the experiment.

4 Research hypotheses

The experiment was principally designed to test for a difference in behaviour of allocators and recipients before and after deception. More specifically, we tested whether there is a difference in choices of allocators and recipients between the second task and the first task in the main treatment (within subjects comparison). Furthermore, we tested whether there is a difference in choices of allocators and recipients in the second task between the main treatment and the control treatment (between subjects comparison). We also tested for a difference in behaviour for $p = \underline{p}$ and $p = \overline{p}$.

We tested these hypotheses against the null hyptheses that choices are the same for both tasks in the main treatment, choices are the same in the new task for the main treatment and control treatment, and choices are the same for $p = \underline{p}$ and $p = \overline{p}$, respectively.

Since E would gain a considerable amount by hiring only A, we also tested whether experimenter-participants engage in deception by excluding recipients from interaction. In particular, we compared the actual proportion of experimenter-participants choosing 'only A' to the proportion of experimenter-participants

[5]Note this does not involve deception by *commission*, i.e. participant-participants were not told lies about the role of E. Instead, this involves deception by *ommission*, i.e. participant-participants were not told everything about E at the beginning of the experiment (for a definition and discussion of these two types of deception, see Hey 1998).

[6]Such practice is relatively common amongst psychologists, and has recently been employed by leading economists (see e.g. Andreoni 1988, Fehr and Gächter 2000, Masclet et al. 2003). The main purpose of using unannounced treatments in experiments is to prevent choices in one treatment to be influenced by the existence of a second treatment.

choosing 'only A' that would be observed whether experimenter-participants made their decisions randomly.

5 Results

The experiment was conducted in the laboratory of the Max Planck Institute of Economics in Jena (Germany). Subject participants were students from various disciplines at the University of Jena. Participants were randomly allocated to sessions that we ran for the two treatments and also were randomly allocated to the roles E, A, and R within each session.[7] They were recruited using ORSEE (Greiner 2004). The experiment was computerized, using z-tree (Fischbacher 2007). The average duration of a session was about 45 minutes. The average, minimum, and maximum earnings for each and both tasks and each and all roles in the two treatments are summarized in Table 2 and Table 3.

5.1 Choices of experimenter-participants

Of the 51 E-participants in the main treatment, 46 chose to hire only A. This obviously rejects the null hypothesis that experimenter-participants made their choice randomly (binomial, $p = 0.000$).[8]

5.2 Choices of allocators and recipients

The choices of allocators and recipients for both tasks in each tratment are summarized in Table 4 and Table 5 in Appendix A. These are complemented by Figure 1 and Figure 2, showing the distribution of choices by allocators and recipients for $p = \underline{p}$ and $p = \overline{p}$, respectively.

With regard to allocators, we first tested whether there is any difference in choices between the two tasks in the main treatment, for both $p = \underline{p}$ and $p = \overline{p}$. We found no large or statistically significant difference (one-sample permutation, $p = 0.192$ for $p = \underline{p}$ and $p = 0.395$ for $p = \overline{p}$). In addition, we tested for differences in choices between the two tasks in the control treatment. Here again, and not surprisingly, we found no statistcially significant difference (one-sample permutation, $p = 1$ for $p = \underline{p}$ and $p = 0.716$ for $p = \overline{p}$).

After that, we tested for differences in allocations made in the second task between the two treatments. We tried several non-parametric tests (see e.g. Siegel and Castellan 1988), comparing various aspects of the distributions of allocations in the treatments. However, we found no statistically significant difference (Mann-Whitney, $p = 0.384$ for $p = \underline{p}$ and $p = 0.29$ for $p = \overline{p}$; Kolmogorov-Smirnov, $p = 0.87$ for $p = \underline{p}$ and $p = 0.246$ for $p = \overline{p}$; two-samples permutation, $p = 0.499$ for $p = \underline{p}$ and $p = 0.371$ for $p = \overline{p}$;). For control, we also looked at the distributions of allocations in the first task for both treatments. We found no large or statistically significant difference (Mann-Whitney, $p = 0.787$ for $p = \underline{p}$ and $p = 0.577$ for $p = \overline{p}$; Kolmogorov-Smirnov, $p = 0.999$ for $p = \underline{p}$

[7] Dreber and Johannesson [2008] show that male subjects are more likely to deceive others if they can gain from doing so. In addition, Jamison et al. [2008]find that female subjects who have experienced deception in experiments are less likely to return to the lab. Unlike Dreber and Johannesson, we did not control for gender differences. In our experiment, the proportion of female subjects was similar across sessions.

[8] All reported p-values are two-sided.

and $p = 0.858$ for $p = \overline{p}$; two-samples permutation, $p = 0.866$ for $p = \underline{p}$ and $p = 0.656$ for $p = \overline{p}$).[9]

Concerning recipients, we performed analogous tests. We found no large or significant difference in choices between the two tasks in the main treatment (one-sample permutation, $p = 0.909$ for $p = \underline{p}$ and $p = 0.646$ for $p = \overline{p}$), no difference in choices between the two tasks in the control treatment (one-sample permutation, $p = 1.000$ for $p = \underline{p}$ and $p = 0.892$ for $p = \overline{p}$), no difference in choices made in the second task between the two treatments (Mann-Whitney, $p = 0.714$ for $p = \underline{p}$ and $p = 0.852$ for $p = \overline{p}$; Kolmogorov-Smirnov, $p = 0.991$ for $p = \underline{p}$ and $p = 0.602$ for $p = \overline{p}$; two-samples permutation, $p = 0.436$ for $p = \underline{p}$ and $p = 0.459$ for $p = \overline{p}$), and again no differences in choices made in the first task between the two treatments (Mann-Whitney, $p = 0.629$ for $p = \underline{p}$ and $p = 0.717$ for $p = \overline{p}$; Kolmogorov-Smirnov, $p = 0.605$ for $p = \underline{p}$ and $p = 0.924$ for $p = \overline{p}$; two-samples permutation, $p = 0.334$ for $p = \underline{p}$ and $p = 0.35$ for $p = \overline{p}$).

The lack of significant effects for both allocators and recipients is in accordance with the null hypothesis that choices were the same for both tasks in the main treatment, and also with the null hypothesis that choices were the same in the second task for both treatments. This suggests no effect of deception in the main treatment.

5.3 Further results

From what is reported above it emerges that deception had no effect on behavior of allocators and recipients, on average. However, did *individual choices* of allocators and recipients change in the second task in the main treatment?

When we consider the distributions of choices of allocators in the two treatments, who chose a smaller, identical, and higher amount in the second task relative to the first task, we see that in both treatments most allocators chose the same allocation in both tasks, consistently for $p = \underline{p}$ and $p = \overline{p}$. We find no statistically significant difference between treatments ($\chi^2 = 3.36 < \chi^2(2, 0.025) \cong 7.82$ and $\chi^2 = 4.02 < \chi^2(2, 0.025) \cong 7.82$ for $p = \underline{p}$ and for $p = \overline{p}$). Similarly, most recipients chose the same minimum acceptable allocation in both tasks in both treatments (see Table6 and Table 7 in Appendix A for details). Here again, we find no statistically significant difference between the treatments ($\chi^2 = 0.15 < \chi^2(2, 0.025) \cong 7.82$ and $\chi^2 = 0.13 < \chi^2(2, 0.025) \cong 7.82$ for $p = \underline{p}$ and $p = \overline{p}$, respectively). These results rule out any doubt that there was nothing but 'no effect' of deception behind the average choices of allocators and recipients in the main treatment.

5.4 Regression analysis

We confirm our results by two random-effects tobit regressions, respectively with the choices of allocators and the choices of recipients as dependent variables. In Table 8 we report estimates of a model with the proportion of endowment allocated as the dependent variable. In Table 9 we report estimates of a model with

[9] That average allocation was consistently higher, though not significantly higher, for both tasks in the main treatment relative to the control treatment is not surprising, since individual choices were positively correlated between tasks, and also correlated between $p = \underline{p}$ and $p = \overline{p}$ within each task. Since subjects were randomly allocated to sessions, the slight difference in the first task must be due to chance.

the proportion of minimum acceptable allocations as the dependent variable. In both models we use three non-interactive covariates: _24 (a dummy for the amount p that A and R can share, equal to 1 if $p = \overline{p}$, 0 otherwise), _2 (a dummy for the task number, equal to 1 if the task is 'first', 0 otherwise), and _MAIN (a dummy variable for the treatment, equal to 1 if the treatment is 'main', and 0 otherwise). We also consider three interaction variables: _2*MAIN (equal to 1 if the task is 'second' and in the main treatment, and 0 otherwise), _24*2 (equal to 1 if $p = \overline{p}$, and the task is 'new', and 0 otherwise), and _24*2*MAIN (equal to 1 if $p = \overline{p}$, the task is 'new', and in the main treatment).

The results in Table 8 and Table 9 show that only _24 has a marginally significant impact, both on the amount allocated by A and on the minimum acceptable allocation by R. All other variables have no significant effect. In particular, allocators tend to give relatively less when $p = \overline{p}$, and there is no difference between tasks and treatments. Similarly, recipients tend to expect relatively less when $p = \overline{p}$,, and again there is no difference between tasks and treatments.

6 Conclusions

Deception does not significantly affect behavior, at least not in our experiment. More specifically, we cannot reject the null hypotheses that deception has no effect, against the hypotheses that deception has an effect. Choices were not significantly different before and after deception, and also did not significantly vary between treatments with and without deception. This is also reflected in the individual choices of allocators and recipients, who in general allocated the same amount respectively expected the same amount to be allocated before and after deception. In addition, allocations were relatively lower in proportion when the amount that allocators and recipients could share was large. Recipients seemed to correctly anticipate this by expecting less.

Overall, these results are surprising. Inspite of some obvious reason why deception might impact on behavior of participant-participants there is no clear evidence for its effect. The other hypothesis that we tested, regarding choices by experimenter-participants, was confirmed, showing how people exploit the opportunity to gain by deceiving others when they can gain from doing so. This result is in line with the results of a recent study by Gneezy [2005].

From a methodological point of view, observing that deception does not change the behaviour of participants in experiments may be seen as conforting, since it allows for the hope that bad experiences do not question trusting others again in the future. Furthermore, banning deception alone seems to be as insufficient as imposing regulations to protect labourers which can be easily circumvented by outsourcing the hiring of labour by the use of subcontractors. If one wants to ban deception or impose other ethical requirements, the addressees should not only be the experimenter, respectively the employer, but all parties involved.

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Appendix A: Tables and figures

Table 1: A summary of payoffs in the main treatment.

			Role		
Task	E chooses	p	E	A	R
1	'both A and R'	$\underline{p}$	$e-2s$	$p-\underline{a}+s$	$\underline{a}+s$ if $\underline{a}\geqslant\underline{m}$ s if $\underline{a}<\underline{m}$
		$\overline{p}$	$e-2s$	$p-\overline{a}+s$	$\overline{a}+s$ if $\overline{a}\geqslant\overline{m}$ s if $\overline{a}<\overline{m}$
	'only A'	$\underline{p}$	$e-s+\underline{a}$	$p-\underline{a}+s$	0
		$\overline{p}$	$e-s+\underline{a}$	$p-\overline{a}+s$	0
2	'both A and R'	$\underline{p}$	$e-2s$	$p-\underline{a}+s$	$\underline{a}+s$ if $\underline{a}\geqslant\underline{m}$ s if $\underline{a}<\underline{m}$
		$\overline{p}$	$e-2s$	$p-\overline{a}+s$	$\overline{a}+s$ if $\overline{a}\geqslant\overline{m}$ s if $\overline{a}<\overline{m}$

Table 2: Experimental earnings in the main treatment. The first, second, and third component are average, minimum, and maximum payoff (EUROS).

	Role			
Task	E	A	R	All
1	(14.27, 5, 22)	(18.86, 8, 29)	(5.98, 5, 19)	(13.04, 6, 23.33)
2	(5, 5, 5)	(18.65, 8, 29)	(10.25, 5, 18)	(11.3, 5, 17.33)
Both	(9.64, 5, 13.5)	(18.75, 8, 29)	(8.12, 5, 18.5)	(12.17, 6, 20.33)

Table 3: Experimental earnings in the control treatment. The first, second, and third component are average, minimum, and maximum payoff (EUROS).

	Role			
Task	E	A	R	All
1	(5, 5, 5)	(20.22, 9, 29)	(9.66, 5, 17)	(11.63, 6.33, 17)
2	(5, 5, 5)	(20.3, 9, 29)	(10, 5, 18)	(11.77, 6.33, 17.33)
Both	(5, 5, 5)	(20.26, 9, 29)	(9.83, 5, 17.5)	(11.7, 6.33, 17.17)

Table 4: A summary of allocations in the two treatments.

	$p = \underline{p}$				$p = \overline{p}$			
	Treatment				Treatment			
	Main		Control		Main		Control	
	Task		Task		Task		Task	
	1	2	1	2	1	2	1	2
obs.	51	51	50	50	51	51	50	50
mean	2.451	2.627	2.38	2.4	6.902	7.176	6.5	6.38
st. dev.	1.591	1.523	1.51	1.565	4.553	4.39	4.082	4.333
min.	0	0	0	0	0	0	0	0
max.	6	5	6	6	14	13	12	13

Table 5: A summary of minimum acceptable allocations in the two treatments.

	$p = \underline{p}$				$p = \overline{p}$			
	Treatment				Treatment			
	Main		Control		Main		Control	
	Task		Task		Task		Task	
	1	2	1	2	1	2	1	2
obs.	51	51	50	50	51	51	50	50
mean	1.373	1.412	1.66	1.64	2.843	3.039	3.6	3.66
st. dev.	1.823	1.152	1.56	1.495	3.319	3.594	4.454	4.47
min.	0	0	0	0	0	0	0	0
max.	5	4	6	6	12	13	17	17

Table 6: Frequency distributions of the direction of changes of allocations in the second task relative to the first task in the two treatments.

	$p = \underline{p}$				$p = \overline{p}$			
Treatment	Less	Same	More	All	Less	Same	More	All
Main	5	34	12	51	10	26	15	51
Control	5	40	5	50	9	34	7	50
Both	10	74	17	101	19	60	22	101

Table 7: Frequency distributions of the direction of changes of minimum acceptable allocations in the second task relative to the first task in the two treatments.

	$p = \underline{p}$				$p = \overline{p}$			
Treatment	Less	Same	More	All	Less	Same	More	All
Main	4	44	3	51	5	39	7	51
Control	5	42	3	50	6	37	7	50
Both	9	86	6	101	11	76	14	101

Table 8: Results of a random-effects tobit regression of the proportion of pie allocated and pie size (_24), task number (_2), treatment (_MAIN), and interactions of task and treatment (_2*MAIN), pie size and task (_24*2), and task, treatment, and pie size (_24*2*MAIN). Standard errors are given in parentheses: ** indicates significant at the 5% level, and *** at the 1% level.

	Model			
Covariate	1	2	3	4
_24	-0.029*** (0.008)	-0.029*** (0.008)	0.024** (0.011)	-0.024** (0.011)
_2	0.009 (0.008)	-0.001 (0.011)	0.003 (0.013)	0.005 (0.015)
_MAIN	0.03 (0.044)	0.020 (0.044)	0.020 (0.044)	0.020 (0.044)
_2*MAIN	-	0.020 (0.015)	0.020 (0.015)	0.017 (0.019)
_24*2	-	-	-0.009 (0.015)	-0.012 (0.019)
_24*2*MAIN	-	-	-	0.004 (0.022)
const	0.269*** (0.032)	0.274*** (0.032)	0.272*** (0.032)	0.272*** (0.032)
obs.	404	404	404	404
groups	101	101	101	101

Table 9: Results of a random-effects tobit regression of the proportion of minimum acceptable allocation and pie size (_24), task number (_2), treatment (_MAIN), and interactions of task and treatment (_2*MAIN), pie size and task (_24*2), and task, treatment, and pie size (_24*2*MAIN). Standard errors are given in parentheses: ** indicates significant at the 5% level, and *** at the 1% level.

	Model			
Covariate	1	2	3	4
_24	-0.063*** (0.007)	-0.063*** (0.007)	-0.066*** (0.010)	-0.066*** (0.010)
_2	0.004 (0.007)	0.001 (0.010)	-0.001 (0.012)	-0.001 (0.013)
_MAIN	-0.038 (0.043)	-0.041 (0.043)	-0.041 (0.043)	-0.041 (0.043)
_2*MAIN	-	0.006 (0.014)	0.006 (0.014)	0.006 (0.017)
_24*2	-	-	0.005 (0.014)	0.005 (0.017)
_24*2*MAIN	-	-	-	0.001 (0.020)
const	0.179*** (0.031)	0.180*** (0.031)	0.181*** (0.031)	0.181*** (0.031)
obs.	404	404	404	404
groups	101	101	101	101

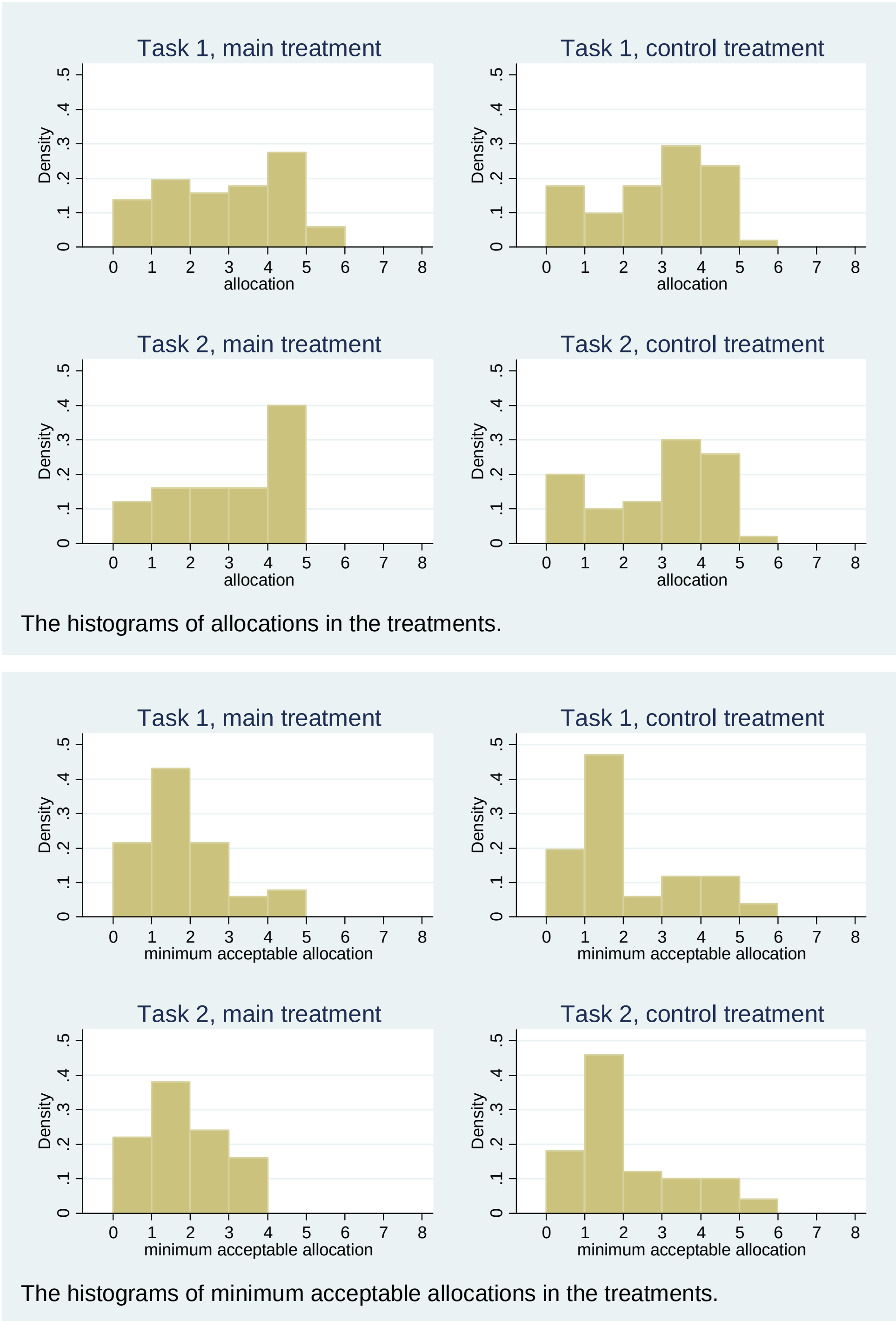

Figure 1: The histograms of allocations and minimum acceptable allocations for $p = \underline{p}$.

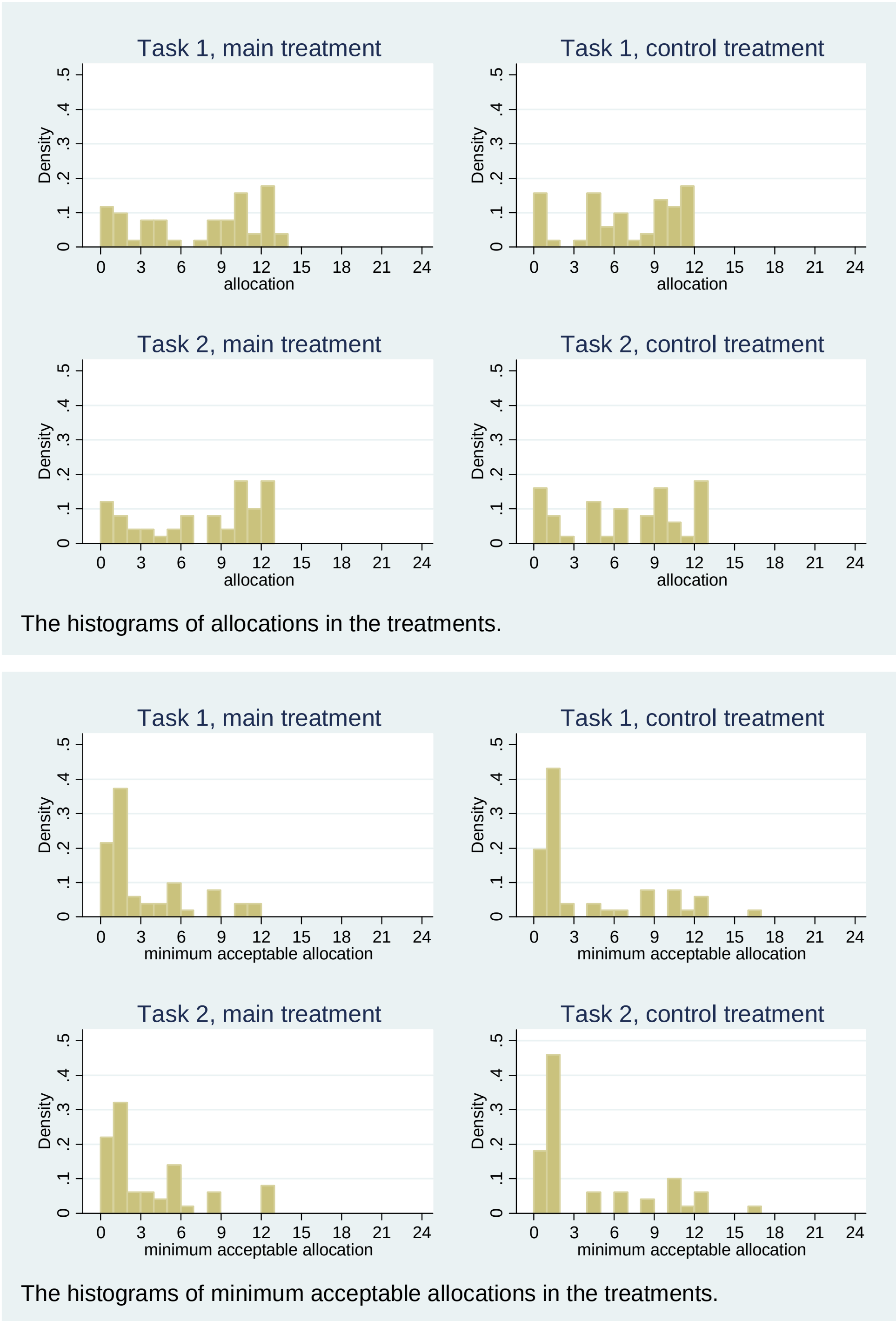

Figure 2: The histograms of allocations and minimum acceptable allocations for $p = \overline{p}$.

Appendix B: Experimental instructions[10]

B.1 General instructions

Thank you for participating in this experiment. You can find more detailed instructions below. Please take your time to read those instructions carefully. If you have a mobile phone, please switch it off. During the experiment you are not allowed to talk to other participants. If you have any question, please raise your hand and one of us will come to help. If you violate these rules, we have to exclude you from the experiment and any payoffs.

In the experiment we use € (EUROS) payments. Any amount of money that you may earn in this experiment will be paid to you in cash at the end of the experiment. Payments are carried out privately, i.e. the other participants will not see your earnings.

Please note that the instructions are written in male gender, but refer to both genders equally.

B.2 Main experimental instructions (first task)

B.2.1 Main instructions for A and R, both treatments

In the experiment you will interact with two other participants. The three participants will be randomly assigned to one of the three roles A, E, and R, i.e. each group of three participants has one A-, one E-, and one R-participant.

You are an A- or R-participant. We will inform you later in which of the two roles (A or R) you will participate. We now describe the process of decision making where we speak of A, E, and R instead of A-, E-, and R-participants.

First: a random draw decides whether A and R can share €8 or €24. Only E learns which of the amounts has randomly been selected.

Second: without knowing whether he, A, and R can share €8 or €24, A must decide how much of €8 should be passed on to R in case A and R can only share €8 and how much of €24 should be passed on to R in case A and R can share €24. Note the amount of offer x, passed on to R, must be no less than nothing and no more than €8 if A and R can share €8, and no less than nothing and no more than €24 if A and R can share €24.

Third: before R learns the offer choice x by A, and without knowing whether he, R, and A can actually share €8 or €24, R must decide the minimum offer x by A that he, R, is willing to accept in case A and R can share €8 and the minimum offer x by A that he, R, is willing to accept in case A and R can share €24.

Payoffs: Payoffs are determined as follows:

If A and R can share €8, A earns €8 minus the offer x plus the show-up fee of €5 regardless of what R decides; R earns the same show-up fee plus the offer x if x is no less than the minimum offer that R is willing to accept and only €5 if x is less than the minimum offer that R is willing to accept.

If A and R can share €24, A earns €24 minus the offer x plus the show-up fee of €5 regardless of what R decides; R earns the same show-up fee plus the offer x if x is no less than the minimum offer that R is willing to accept and only €5 if x is less than the minimum offer that R is willing to accept.

[10]The reported instructions are a translation from the original instructions in German.

E essentially acts as experimenter who, for instance, can observe your choices. So, for instance, if A and R can only share €8 your E-participant can see that this is respected in the sense that A gets €8 minus x plus the show-up fee of €5 and that R gets the same show-up fee of €5 plus the offer x if x is no less than the minimum offer that R is willing to accept and only the show up fee of €5 if x is less than the minimum offer that R is willing to accept.

B.2.2 Main instructions for E, main treatment

Insert main instructions for A and R, both treatments here.

Now that you are aware of the situation which A and R confront we can tell you what you, as the E- participant, have to decide. As an experimenter E you receive a budget of €15 for monitoring the choice x by the A-participant. There is no additional budget when you additionally monitor the acceptance choice of x by R. Thus, you must engage the A-participant but you can avoid engaging the R-participant.

Note that the show-up fee of €5 for A and R must be paid from your budget. Thus you, as E, earn €15 minus twice the show-up fee, i.e., €$(15-2 \times 5) =$ €5, if you decide to engage both A and R, whereas you earn €15 plus x minus one show-up fee, i.e. €$(10 + x)$, if you decide to engage only A. A and R earn as the amounts which you have seen when reading the [*Main instructions for A and R, both treatments*] above, if you decide to engage both A and R. However, only A earns the same amounts as in the [*Main instructions for A and R, both treatments*] whereas R earns nothing, if you decide to engage only A.

NOTE: When deciding about x the A-participant is not at all aware that you can avoid engaging R as you have seen when reading [*Main instructions for A and R, both treatments*] above. Thus A expects that there is actually an R-participant who may or may not be willing to collect x. However, finally A and R learn that you had the choice of (not) engaging R. Actually, the corresponding message to A and R will be:

A AND R-MESSAGE: The E-participant was not just monitoring your interaction: E had the choice between engaging you both, A and R, or only A. Actually by not engaging R the E-participant could gain by not having to pay the show-up fee of €5 to R and by additionally collecting the amount x which A has passed on to R. The E-participant, however, could not avoid engaging A and paying the show-up fee of €5 out of the budget of €15.

B.2.3 Main instructions for E, control treatment

Insert main instructions for A and R, both treatments here.

Now that you are aware of the situation which A and R confront we can tell you that, as an experimenter E, you receive a budget of €15 for monitoring the choice x by the A- participant. There is no additional budget when you additionally monitor the acceptance choice of x by R. Nonetheless, you must engage the A-participant and you cannot avoid engaging the R-participant. Note that the show-up fee of €5 for A and R must be paid from your budget. Thus you, as E-participant, will earn €15 minus twice the show-up fee, i.e., €$(15-2 \times 5) =$ €5.

B.3 Further experimental instructions (second task)

B.3.1 Further instructions for A, main treatment

Before you and the R-participant learn what E-decided and before you and the R-participant are informed on what the other participant decided, you and the R-participant must repeat the same decision task once again. The amount that you and R can share will be the same as before. As before, you must decide how much of €8 and how much of €24 you should pass on to R. R must decide the minimum offer x that he, R, is willing to accept in case you and R can share €8 and the minimum offer x that he, R, is willing to accept in case you and R can share €24. E will also participate in the same role as he did before. However, this time E must engage you, A, and he cannot avoid engaging R. The show-up fee of €5 must be paid from his budget. Thus E earns €$(15-2 \times 5)$ = €5 for sure and you, as A, earn €8 minus the offer$x$ plus €5 in case you and R can share €8 and €24 minus the offer x plus €5 in the event A and R can share €24, regardless of what R decides.

NOTE: Once you and the R-participant have completed your decision task you will learn what the E-participant decided in the previous task. You will also learn what the R-participant decided in both tasks and whether you and the R-participant can share only €8 or €24. This will determine your actual payoffs for each task. After that, a random draw will decide whether you, E, and R are paid the earnings from this task or the earnings from the previous task. Note the amount from this task is as probable as the amount from the previous task.

B.3.2 Further instructions for R, main treatment

Before you and the A-participant learn what E-decided and before you and the A-participant are informed on what the other participant decided, you and the A-participant must repeat the same decision task once again. The amount that you and A can share will be the same as before. As before, A must decide how much of €8 and how much of €24 he should pass on to R. You must decide the minimum offer x by A that you are willing to accept in case you and A can share €8 and the minimum offer x that you are willing to accept in case you and A can share €24. E will also participate in the same role as he did before. However, this time E must engage A and he cannot avoid engaging you, R. The show-up fee of €5 must be paid from his budget. Thus E earns €$(15-2 \times 5)$ = €5 for sure and you, as R, earn the same show-up fee of €5 plus the amount of offer x by A if the amount of offer x is no less than the minimum amount that you are willing to accept and only €5 if the amount of offer x is less than the minimum amount that you are willing to accept.

NOTE: Once you and the A-participant have completed your decision task you will learn what the E-participant decided in the previous task. You will also learn what the A-participant decided in both tasks and whether you and the A-participant can share only €8 or €24. This will determine your actual payoffs for each task. After that, a random draw will decide whether you, E, and A are paid the earnings from this task or the earnings from the previous task. Note the amount from this task is as probable as the amount from the previous task.

B.3.3 Further instructions for E, main treatment

Before the A- and R-participants learn how you decided and, and before A learns what R decided and R learns what A decided, A and R must repeat the same decision task once again. The amount that A and R can share will be the same as before. You will also participate in the same role as you did before, i.e. E. As before, you receive a budget of €15 for monitoring the choice x by the A- participant. There is no additional budget when you additionally monitor the acceptance choice of x by R. However, this time you must engage the A-participant and you cannot avoid engaging the R-participant. The show-up fee of €5 for A and R must be paid from your budget. Thus you, as E-participant, will earn €15 minus twice the show-up fee, i.e., €$(15-2 \times 5) =$ €5.

NOTE: Once the A and R-participants have completed their decision task they will learn whether you decided to engage them both, A and R, or only A. A will also learn what R decided in both tasks and R will learn what A decided in both tasks. Both A and R will learn whether they can share only €8 or €24. This will determine their actual payoffs for each task. After that, a random draw will decide whether you, A, and R are paid the earnings from this task or the earnings from the previous task. Note the amount from this task is as probable as the amount from the previous task.

B.3.4 Further instructions for A, control treatment

Before you and the R-participant are informed on what the other participant decided, you and R must repeat the same decision task once again. The E-participant will also participate in the same role as he did before. The amount that you and R can share will be the same as before. As before, you must decide how much of €8 and how much of €24 you should pass on to R. R must decide the minimum offer x that he, R, is willing to accept in case you and R can share €8 and the minimum offer x that he, R, is willing to accept in case you and R can share €24. As before, you earn €8 minus the offer x plus the show-up fee of €5 in case you and R can share €8 and €24 minus the offer x plus the show-up fee of €5 in case you and R can share €24.

NOTE: Once you and the R-participant have completed your decision task you will learn what the R-participant decided in both tasks and whether you and the R-participant can share only €8 or €24. This will determine your actual payoffs for each task. After that, a random draw will decide whether you, A, E and R are paid the earnings from this task or the earnings from the previous task. Note the amount from this task is as probable as the amount from the previous task.

B.3.5 Further instructions for R, control treatment

Before you and the A-participant are informed on what the other participant decided, you and A must repeat the same decision task once again. The E-participant will also participate in the same role as he did before The amount that you and A can share will be the same as before. As before, A must decide how much of €8 and how much of €24 he should pass on to you. You must decide the minimum offer x that you are willing to accept in case you and A can share €8 and the minimum offer x that you are willing to accept in case you and A can share €24. As before, you earn the show-up fee of €5 plus the offer

x by A if x is no less than the minimum offer x that you are willing to accept and only the show-up fee of €5 if x is less than the minimum offer x that you are willing to accept.

NOTE: Once you and the A-participant have completed your decision task you will learn what the A-participant decided in both tasks and whether you and the A-participant can share only €8 or €24. This will determine your actual payoffs for each task. After that, a random draw will decide whether you, E and A are paid the earnings from this task or the earnings from the previous task. Note the amount from this task is as probable as the amount from the previous task.

B.3.6 Further instructions for E, control treatment

Before the A-participant learns what R decided and the R-participant learns what A decided, A and R must repeat the same decision task once again. The amount that A and R can share will be the same as before. You will also participate in the same role as you did before, i.e. E. As before, you receive a budget of €15 for monitoring the choice x by the A-participant. There is no additional budget when you additionally monitor the acceptance choice of x by R. Nonetheless, you must engage the A-participant and you cannot avoid engaging the R-participant. The show-up fee of €5 for A and R must be paid from your budget. Thus you, as E-participant, will earn €15 minus twice the show-up fee, i.e. €$(15-2 \times 5)$ = €5.

NOTE: Once the A and R-participants have completed their decision task A will learn what R decided in both tasks and R will learn what A decided in both tasks. Both A and R will learn whether they can share only €8 or €24. This will determine their actual payoffs for each task. After that, a random draw will decide whether A and R are paid the earnings from this task or the earnings from the previous task. Note for you the amount from this task is the same as the amount from the previous task.

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