Studying deception without deceiving participants: An experiment of deception experiments

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Abstract

Like avoiding labor protection law via foreign subcontractors, banning deception in economic experiments does not exclude experiments with participants in the role of experimenters who, similar to properly incentivized subcontractors, can gain by deceiving those in the role of proper participants. We compare treatments with and without possible deception by ‘experimenter-participants’ in a dictator experiment and test whether participants in the role of experimenters engage in deception and whether deception affects behavior of ‘participant-participants’. We find that most participants in the role of experimenters engage in deception and no difference in behavior of participant-participants between the treatments, even when - after debriefing - repeating the experiment without deception. Our results can be viewed as a contribution to study the effects of unethical behavior via outsourcing it by letting subcontractors do the harm.

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.¹ Putting a ban on the use of certain practices such as the payment of unfair wages to workers does not prevent employers to engage in such exploitative practices by subcontracting. Indeed, in many

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¹For a discussion of the use of deception in economic experiments, see Hey [1991], Davis and Holt [1993], and especially Andersson [2002].

Western economies the effects of labor regulations can be easily circumvented by outsourcing the hiring of workers to foreign subcontractors. In this sense, deception in experiments is analogous to exploitative practices on the labor market. One can avoid being guilty of engaging in deception by allowing a properly incentivized participant to deceive the other participants.²

In this sense we, the authors of this study, engage in subcontracting and therefore can study deception, a taboo in experimental economics, without getting 'our hands dirty'. We do not engage in deception. As experimenters, we allow and properly incentivize a participant in the experimenter role, called 'experimenter-participant', to deceive participants in the usual role of experimental participants, labelled as 'participant-participants'.

The experimental scenario is a variant of the dictator game (Forsythe et al. 1994) in which two participant-participants - an allocator and a recipient - must decide without knowing that the experimenter-participant can increase her payoff substantially by excluding one of them - the recipient - from interaction. To motivate our choice of experimental scenario, imagine an allocator who sacrifices own payoff to help the recipient. Doing so strongly depends on trusting that the recipient actually gains from own sacrifice. On the other hand, an allocator who cares a lot about the recipient might be very frustrated when learning - after debriefing - that her solidarity might have been wasted. This, in our view, suggests that we might observe the often feared effects of deception in experiments, i.e. that after once being deceived one will doubt the instructions forever (see e.g. Hey 1991, Davis and Holt 1993, and Ledyard 1994).

To capture deception effects, we repeat without forewarning the experiment after debriefing participant-participants on the actual role of the experimenter-participant. In the repetition of the experiment, the two participant-participants play in the same role and the experimenter-participant is unable to exclude any participant-participants from interaction. Based on this experimental scenario, we test for differences in behavior of participant-participants in the same (dictator) game context before and after debriefing of possible deception. In spite of the apparent reasons why deception might impact on behavior of participant-participants, we find no evidence for its effects. This is in line with the results of previous studies, finding no clear deception effects.³ We also support the

²Uri Gneezy has pointed out and encouraged us to discuss the analogy of studying deception in experiments without deception and such subcontracting. The latter has been investigated experimentally also in the context of principal-agent relationships and delegation, see e.g. Hamman et al. [2010] and Bartling and Fischbacher [2012].

³Ortmann and Hertwig [2002] review a wide range of psychology studies involving deception. They find mixed results on the impact of deception on negative emotions such as embarrassment, sadness, or discomfort. Some studies show that deceived participants do not experience such emotions. In contrast, other studies reveal that such negative emotions have an impact on the behavior of participants. They also find that suspicion impacts on behavior of participants in experiments. However, such effect appears to be significant only when experiments are similar to previous ones. Jamison et al. [2008] test the effect of deception regarding the identity of other players on future behavior in experiments different to the initial one. They find evidence that deception has some (minor) impact on both the selection of participants and the behavior of participants who return to the lab. However, they cannot differentiate between the effect of selection of participants and the effect of deception. For a

hypothesis that strong incentives to deceive others induce people to deceive. This is consistent with the evidence from previous experiments (e.g. Gneezy 2005), that people are more likely to deceive the higher the amount they gain from this. More generally, our results show that ruling out 'bad practices' by banning them without guaranteeing that they cannot be outsourced via sub-contracting is ineffective.

To justify our choice of experimental scenario further, let us compare it with another one, more in the spirit of deception game experiments (e.g. Gneezy 2005, Charness and Dufwenberg 2005, Hurkens and Kartik 2009, and Sutter 2009). In the so-called 'yes/no game' (Güth et al. 2005), unlike in the ultimatum game (Güth et al. 1982), a responder must accept or reject the offer by a proposer without knowing the actual offer. This could be implemented by first allowing the proposer to send some stated offer but not necessarily the true one to the responder who then, depending on the stated offer, can accept or reject the offer.

In our view, such a scenario more often than not would trigger suspicion concerning the truth of the message. So the disappointment when actually learning that one has been deceived by the proposer will not come completely unexpected. In our view, deception effects are more likely and stronger when one has been unaware of possible deception and when actually done something very costly, based on one's trust that everything is done as perceived. We do not claim that deception games like the one just described cannot capture the crucial aspects of our deception design, but (still) consider being deceived by the social scenario - whether or not there exists a recipient - as a very unsettling experience from which one could expect more detrimental effects.

The remainder of the paper is organized as follows. The experimental scenario is described in details below. This is followed by a presentation of the experimental procedures and 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 is an experimenter-participant who can 'employ' either one or two participant-participants to play a variant of 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 refer to E as experimenter-participant, and to A and R as allocator and recipient, respectively.

more general discussion of the use of deception in experiments, see e.g. Bonetti [1998], Hey [1998], McDaniel and Starmer [1998], and Hertwig and Ortmann [2008].

In the dictator game, the allocator is given a positive monetary amount p to share with the recipient. This monetary amount p can be either small, i.e. $p = \underline{p}$, or large, i.e. $p = \bar{p}$, thus $0 < \underline{p} < \bar{p}$. The allocator must decide how much of p is passed on to R. The recipient decides the minimum allocation by A that she is willing to accept. 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 unacceptable gift, and possibly voice her anger (see Xiao and Houser 2005). Neither A nor R know whether $\underline{p}$ or $\bar{p}$ can be shared when deciding. A and R condition their choices on both $\underline{p}$ and $\bar{p}$ knowing only the probability distribution, i.e. how likely $p = \underline{p}$ and $p = \bar{p}$ are. To run the dictator experiment, E receives a positive monetary endowment e . This also includes the show-up fee s to each the allocator and recipient, thus $e - 2s > 0$.

In the experiment, there are two treatments. In the main treatment, E can save the show-up fee to R and additionally collect what A gives to R by not hiring R. Since A and R are initially not aware that E can avoid hiring R and this is known to E, by not hiring R the E-participant deceives A and R. In the control treatment, E must hire both A and R, that rules out deception.

In both treatments, there are two tasks. In each task, A and R play the dictator game which E has implemented. The first task is known to all participants from the beginning of the experiment while the second task is only announced after the first task.⁴

In the main treatment, E can avoid hiring R, and thus engage in deception, only in the first task. In the second task, E must hire both A and R. At the end of the first task and before the second task is announced, A and R are debriefed of the possibility of deception by E in only the first task. In the instructions for the second task, A and R are informed that in the second task E must hire both A and R. We shall come back to this in Section 3.

Let us now describe the rules of the main treatment in more detail. First, E decides whether to hire only A or both A and R, without knowing whether $\underline{p}$ or $\bar{p}$ can be shared between A and R (she only knows how likely $p = \underline{p}$ and $p = \bar{p}$ are). Second, A chooses her allocations, i.e. $\underline{a}$ for $p = \underline{p}$ and $\bar{a}$ for $p = \bar{p}$, where $0 \leq \underline{a} \leq \underline{p}$ and $0 \leq \bar{a} \leq \bar{p}$, without knowing that E can avoid hiring R. Third, R chooses her minimum acceptable gifts, i.e. $\underline{m}$ for $p = \underline{p}$ and $\bar{m}$ for $p = \bar{p}$, where $0 \leq \underline{m} \leq \underline{p}$ and $0 \leq \bar{m} \leq \bar{p}$, without knowing the amounts $\underline{a}$ for $p = \underline{p}$ and $\bar{a}$ for $p = \bar{p}$, and without knowing that E can avoid hiring her. Finally, A and R are debriefed that E had the possibility to choose only A (see specifically the so-called 'A and R-Message' in the Online Appendix B, Section B.2.2, alerting participant-participants of the possibility of deception in the first task).⁵ After that, E, A and R are informed that A and R play the same dictator game in the

⁴The use of unannounced tasks in experiments is relatively common amongst psychologists and has also been employed by economists, see e.g. Andreoni [1988], Fehr and Gächter [2000], Masclet et al. [2003]. The main purpose of using unannounced tasks is to prevent choices in one task to be influenced by anticipating later ones, for example in the sense of repeated interaction effects.

⁵This 'A and R-Message' appeared as a separate screenshot which participants could not overlook.

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

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

same roles once again and that this time E must hire both A and R. Only after having made their decisions for the second task, A and R learn their respective choices for both tasks, whether $\underline{p}$ or $\bar{p}$ can be shared for both tasks, and also whether E hired only A or both A and R in the first task.

Payoffs for both tasks and all roles in the main treatment are summarized in Table 1. In the first task, if E hires both A and R, A receives $\underline{p} - \underline{a} + s$ for $p = \underline{p}$ and $\bar{p} - \bar{a} + s$ for $p = \bar{p}$; E earns $e - 2s$ for both $p = \underline{p}$ and $p = \bar{p}$; R receives $\underline{a} + s$ if $\underline{a} \geq \underline{m}$ and s if $\underline{a} < \underline{m}$ for $p = \underline{p}$ and $\bar{a} + s$ if $\bar{a} \geq \bar{m}$ and s if $\bar{a} < \bar{m}$ for $p = \bar{p}$. However, if E chooses only A, nothing changes for A, E receives $e - s + \underline{a}$ for $p = \underline{p}$ and $e - s + \bar{a}$ for $p = \bar{p}$ and R receives nothing from A and E. In the second task, E is forced to hire both A and R. Thus, payoffs to E, A, and R are the same as when R is hired voluntarily.

In the control treatment, all what differs is that E is forced to hire both A and R in the first task. Thus, in both tasks, payoffs are the same as when E recruits both, A and R, in the main treatment.

Given the above scenario, do we expect new insights for dictatorial reward allocation (see Shapiro 1975, Mikula 1973, and Forsythe et al. 1994)? What could be new for the control treatment, without the possibility of deception, would be a change of gift-giving and acceptance behavior in the repetition. To the best of our knowledge, there is no learning study of repeated dictator game play. This is surprising since generosity might change with experience and since often dictator generosity is mentioned, for example, to justify social preferences (see Bolton and Ockenfels 2000 and Fehr and Schmidt 1999). For the main treatment, deception and debriefing may crowd out more or less completely generosity of allocations. At the same time, allocations may also increase as a result of participant-participants feeling more solidarity with each other (they both are 'victims'). Either way, even if there are no significant deception effects, we might observe rather similar 'experience effects' for both treatments.

3 Experimental procedures and hypotheses

One 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 the other 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 of the two tasks was randomly drawn with equal probabilities for payment at the end of the session.

We were naturally interested in 'seducing' the experimenter-participant by letting her gain a considerable amount by excluding the recipient from interaction. Thus, we set $e = 15$ and $s = 5$, with 1 point=1 EURO, also to compensate participants in the role R in case of a small gift. The other parameters were $\underline{p} = 8$ and $\bar{p} = 24$. The latter was 80% likely, because we predicted allocators with $p = \bar{p}$ to hide greed by giving at most $\bar{a} = \underline{p}/2$.

We invited 51 groups of participants for the roles E, A, and R for the 8 sessions of the main treatment and 50 groups of participants for the same roles for the 8 sessions of the control treatment. A session included only complete groups of three participants to rule out any doubt that there exists a participant for each role.

In the first task, the E-participants in the 101 E-A-R groups were shown the instructions to A and R-participants, so it was known to all participants that:

1. $\underline{p} = 8$, $\bar{p} = 24$, $s = 5$ and $\bar{p} = 24$ has 80% probability,
2. A must choose $\underline{a}$ for $\underline{p} = 8$ and $\bar{a}$ for $\bar{p} = 24$ while R must choose $\underline{m}$ for $\underline{p} = 8$ and $\bar{m}$ for $\bar{p} = 24$, and $\underline{a}$, $\bar{a}$, $\underline{m}$, and $\bar{m}$ are restricted to integers in the range $0 \leq \underline{a} \leq 8$, $0 \leq \bar{a} \leq 24$, $0 \leq \underline{m} \leq 8$ and $0 \leq \bar{m} \leq 24$, respectively,
3. if $p = \underline{p}$, A receives $\underline{p} - \underline{a} + s$ while R receives $\underline{a} + s$ if $\underline{a} \geq \underline{m}$ and s if $\underline{a} < \underline{m}$, and if $p = \bar{p}$, A receives $\bar{p} - \bar{a} + s$ while R receives $\bar{a} + s$ if $\bar{a} \geq \bar{m}$ and s if $\bar{a} < \bar{m}$,
4. E is an experimenter-participant who can, for instance, observe the interaction between A and R.⁶

The instructions to A and R-participants for the first task in both treatments are given in Section B.2.1 of the Online Appendix B.

In addition to instructions to A and R-participants, E-participants were given specific instructions. In the main treatment, E-participants were informed of the following:

1. $e = 15$,

⁶This avoids 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 of these two types of deception, see Hey 1998). Only the use of erroneous information is usually not tolerated by economists (see Hertwig and Ortmann 2008).

2. E can choose between hire only A and both A and R,
3. by not hiring R, E earns $e - s + \underline{a}$ if $p = \underline{p}$ and $e - s + \bar{a}$ if $p = \bar{p}$ while R gets nothing from A and E and A earns the amount as in the instructions to A and R; otherwise, E earns $e - 2s$, while both A and R earn the amount as in the instructions to A and R,
4. A and R are not aware that E can choose between 'only A' and 'both A and R',
5. A and R will eventually be informed that E in fact can exclude R, and that by not hiring R the E-participant can save s and collect $\underline{a}$ or $\bar{a}$.⁷

The instructions to E-participants in the main treatment, including the debriefing message that is sent to A and R at the end of the first task ('A and R Message'), are shown in Section B.2.2 of the Online Appendix B.

In the control treatment, E knows that:

1. $e = 15$,
2. she must monitor A and R,⁸
3. she receives $e - 2s$.

Also the instructions to E-participants in the control treatment in the first task are given in the Online Appendix B, Section B.2.3.⁹

In the second task of the main treatment all participants know that A and R face the same decision task again while E cannot avoid hiring R. In the control treatment, the second task is just a repetition. In the main treatment, A and R know E's payoff and their own payoff. The instructions to A, R, and E-participants for the second task in both treatments are shown in Section B.3 of the Online Appendix B.

3.1 Hypotheses

The experiment is designed to test for deception effects: do allocations in the main treatment become less generous after deception and debriefing? Or do they rather tend to increase to compensate for what happened in the first task? Similarly, do acceptance thresholds increase or decrease? Since we cannot tell what to expect, our alternative hypothesis is two-sided. We test whether allocations *change* after debriefing of possible deception, i.e. whether in the main treatment gifts and acceptance thresholds in the second task differ from those in

⁷Ortmann and Hertwig [2002] report on evidence from nine psychology studies showing that direct experience of deception affects behavior in experiments while it is dubious whether the mere possibility of deception can alter behavior in experiments.

⁸This is exactly what A and R also read in the instructions to A and R-participants.

⁹After the instructions, all participants were asked to answer eight questions designed to check their understanding of the game and the payoff rules. Participants were asked to correct their mistakes (if any) until they could find the correct answer.

the first task. We also compare choices across treatments, i.e. we test whether in the second task choices of allocators and recipients in the main treatment differ from those in the control treatment. In addition, we test for differences in behavior between the first and second task in the control treatment and also differences in behavior for $p = \underline{p}$ and $p = \bar{p}$. The former is to capture possible experience effects. The null hypothesis is always that choices do not differ.

Since E would gain a considerable amount by hiring only A, we expect experimenter-participants to engage in deception. In particular, we compare the actual proportion of E-participants choosing only A with the proportion of E-participants choosing only A that would be observed if E-participants made their choices randomly, i.e. if E-participants chose 'only A' and 'both A and R' with equal probabilities.

4 Results

The experiment was conducted in the laboratory of the Max Planck Institute of Economics in Jena (Germany). Participants were students from various disciplines at the University of Jena. They were randomly allocated to sessions of the two treatments and also to the roles E, A, and R within each session. Participants 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 treatments are summarized in Table 6 and Table 7 in the Online Appendix A.

4.1 Choices of experimenter-participants

Of 51 E-participants in the main treatment, 46 hired only A. This obviously rejects the null hypothesis that experimenter-participants made their choice randomly (binomial test, p-value is 0.000).¹⁰

4.2 Choices of allocators and recipients

The choices of allocators and recipients for both tasks in each treatment are summarized in Table 2 and Table 3. These are complemented by Figure 1 and Figure 2 in the Online Appendix A, showing the distributions of choices by allocators and recipients for $p = \underline{p}$ and $p = \bar{p}$, respectively. In both treatments and tasks, there are hardly any 'overgenerous allocations' in the sense of $\underline{a} > \underline{p}/2$ and $\bar{a} > \bar{p}/2$. The average $\bar{a}$ consistently exceeds the average $\underline{a}$. Recipients are willing to accept less than what allocators give on average. For both allocators and recipients in both treatments, choices made in the second task do not differ so much from those made in the first task.

¹⁰All reported p-values are two-sided.

Table 2: A summary of allocations in the two treatments.

	$p = \underline{p}$				$p = \bar{p}$			
	Treatment		Treatment		Treatment		Treatment	
	Main	Control	Main	Control	Main	Control	Main	Control
	Task	Task	Task	Task	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 3: A summary of minimum acceptable allocations in the two treatments.

	$p = \underline{p}$				$p = \bar{p}$			
	Treatment		Treatment		Treatment		Treatment	
	Main	Control	Main	Control	Main	Control	Main	Control
	Task	Task	Task	Task	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

To see whether choices of allocators change after debriefing of possible deception, we compare the distributions of allocations in the two successive tasks in the main treatment. We find no large or significant difference (one-sample permutation test, p-value is 0.192 for $p = \underline{p}$ and 0.395 for $p = \bar{p}$). We also test for differences in allocations made in the second task between the treatments. We try several non-parametric tests (e.g. Siegel and Castellan 1988), comparing various aspects of the distributions of allocations in the treatments. However, again we find no significant difference (Mann-Whitney test, p-value is 0.384 for $p = \underline{p}$ and 0.29 for $p = \bar{p}$; Kolmogorov-Smirnov test, p-value is 0.87 for $p = \underline{p}$ and 0.246 for $p = \bar{p}$; two-samples permutation test, p-value is 0.499 for $p = \underline{p}$ and 0.371 for $p = \bar{p}$). This suggests that either deception and experience effects are compensatory or that deception has no effect at all on choices of allocators in the main treatment. In view of no-feedback information regarding A and R-behavior between tasks we consider experience effects rather unlikely. This is confirmed by the data in the control treatment, showing no difference between the first and second task (one-sample permutation test, p-value is 1.000 for $p = \underline{p}$ and 0.716 for $p = \bar{p}$). Thus, we conclude that there are no serious deception effects.¹¹

¹¹That average allocation was consistently though not significantly higher for both tasks

Concerning recipients, we perform analogous tests. We find no large or significant difference in choices between the first and second task in the main treatment (one-sample permutation test, p-value is 0.909 for $p = \underline{p}$ and 0.646 for $p = \bar{p}$), and no significant difference in choices made in the second task between the treatments (Mann-Whitney test, p-value is 0.714 for $p = \underline{p}$ and 0.852 for $p = \bar{p}$; Kolmogorov-Smirnov test, p-value is 0.991 for $p = \underline{p}$ and 0.602 for $p = \bar{p}$; two-samples permutation test, p-value is 0.436 for $p = \underline{p}$ and 0.459 for $p = \bar{p}$). There is also no significant difference between the two tasks in the control treatment (one-sample permutation test, p-value is 1.000 for $p = \underline{p}$ and 0.892 for $p = \bar{p}$). Thus, as for allocators, deception has no effects on choices of recipients. The lack of significant effects for both allocators and recipients supports the null hypothesis that choices do not differ, neither between the two tasks of the main treatment, nor across the two treatments' second tasks, nor between the first and second task in the control treatment.¹²

4.3 Further results

Our results suggest that deception has no effect on average behavior of allocators and recipients. However, do individual choices of allocators and recipients vary in the second task in the main treatment?

When we consider the distributions of choices of allocators in the two treatments, who choose a smaller, identical, or higher gift in the second task relative to the first task, we see that in both treatments most allocators choose the same gift in both tasks, consistently for $p = \underline{p}$ and $p = \bar{p}$. There is no significant difference between the treatments (Fisher's exact test, p-value is 0.205 for $p = \underline{p}$ and 0.136 for $p = \bar{p}$). Similarly, most recipients choose the same minimum acceptable gift in both tasks in both treatments (see Table 8 and Table 9 in the Online Appendix A for details of the distributions). Here again, we find no significant difference between the treatments (Fisher's exact test, p-value is 0.916 for $p = \underline{p}$ and 0.945 for $p = \bar{p}$). These results rule out any doubt against 'no effect' of deception. In addition, they support that experience plays no role.

in the main treatment relative to the control treatment is not surprising: individual choices quite naturally are positively correlated between two successive and similar tasks, and also between $p = \underline{p}$ and $p = \bar{p}$ within each task. Since participants were randomly allocated to sessions, the slight difference in the first task must be due to random selection effects.

¹²For control, the distributions of allocations in the first task reveal no large or significant difference between the treatments (Mann-Whitney test, p-value is 0.787 for $p = \underline{p}$ and 0.577 for $p = \bar{p}$; Kolmogorov-Smirnov test, p-value is 0.999 for $p = \underline{p}$ and 0.858 for $p = \bar{p}$; two-samples permutation test, p-value is 0.866 for $p = \underline{p}$ and 0.656 for $p = \bar{p}$). Also for recipients, there is no difference in the first task between the treatments (Mann-Whitney test, p-value is 0.629 for $p = \underline{p}$ and 0.717 for $p = \bar{p}$; Kolmogorov-Smirnov test, p-value is 0.605 for $p = \underline{p}$ and 0.924 for $p = \bar{p}$; two-samples permutation test, p-value is 0.334 for $p = \underline{p}$ and 0.35 for $p = \bar{p}$).

Table 4: Results of a random-effects tobit regression of the relative share of p allocated.

Covariate	Model			
	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 5: Results of a random-effects tobit regression of the minimum relative share of p accepted.

Covariate	Model			
	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

Note: Covariates are size of p (_24), task number (_2), treatment (_MAIN), and interactions of task and treatment (_2*MAIN), size of p and task (_24*2), and size of p , task, and treatment (_24*2*MAIN). Standard errors are given in parentheses: ** indicates significance at the 5% level, and *** at the 1% level.

4.4 Regression analysis

We support the above results by two random-effects tobit regressions, with the choices of allocators and the choices of recipients as dependent variables. Table 4 reports estimates of a model with the relative share of p allocated to R as the dependent variable. Table 5 gives the estimates of a model with the minimum relative share of p accepted by R as the dependent variable. In both models, we use three non-interactive covariates: _24 (a dummy for the amount p , equal to 1 if $p = \bar{p}$, 0 otherwise), _2 (a dummy for the task number, equal to 1 if the task is 'second', 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 is in the

main treatment, and 0 otherwise), $_24*2$ (equal to 1 if $p = \bar{p}$ and the task is 'second', and 0 otherwise), and $_24*2*MAIN$ (equal to 1 if $p = \bar{p}$, the task is 'second', and is in the main treatment, and 0 otherwise).

The results in Table 4 and Table 5 show that only $_24$ has a marginally significant impact, both on the amount allocated to R and on the minimum acceptable gift for R. All other variables have no significant effect. Thus, allocators give relatively less (even though they allocate more in absolute terms) when $p = \bar{p}$, and there is no difference in allocations between tasks and treatments. Similarly, recipients expect relatively less when $p = \bar{p}$, and again there is no difference between tasks and treatments.

5 Conclusions

Deception does not significantly affect behavior, at least it does not significantly change behavior in our experiment. More specifically, we cannot reject the null hypotheses that deception has no effect on behavior. Choices are not significantly different before and after deception and debriefing, and also do not significantly vary across treatments. This is also reflected in the individual choices of allocators and recipients, who allocate the same amount respectively expect the same amount to be allocated before and after debriefing. In addition, allocations do not change across tasks in the treatment without deception. In general, gifts are relatively lower when the amount that allocators and recipients can share is large. This is anticipated by recipients who expect relatively less.

Are these results surprising? In spite of the obvious reasons why deception might change behavior in our experiment, we find no significant effects. This might be due to our design being limited to only one repetition after debriefing, that rules out long-run deception effects. Thus, one could view our experimental scenario as a worst case for observing deception effects. Another reason for the lack of effect might be participant-participants blaming us, the authors of this paper, as responsible for the deception, for example because we, the authors, are responsible for 'seducing' the experimenter-participants. This is possible but it is hard to imagine that it matters for participant-participants whether the experimenter-participants or we, the authors, are responsible. After all, experimenter-participants could have resisted more often to seduction.¹³ One of our anonymous referees speculated that it might matter where the experiment was run, namely in Germany with most participants being German. There might be different behavioral responses to negative emotions such as disappointment in different countries or cultures, and the level of trust and more generally what people expect from each other may vary with culture and the historical period, although with the 'global economy' this becomes less likely. Nevertheless, one could run the experiment with different cultural groups to explore this issue more thoroughly. The other hypothesis that we support is that people exploit the opportunity to gain by deceiving others. Apparently, experimentalists in

¹³In their recent experiment, Bartling and Fischbacher [2012] find that responsibility attribution can be effectively shifted from a principal to the agent.

economics can easily avoid engaging in deception by 'hiring' or 'subcontracting' participants in the role of experimenters and incentivizing them properly.

From a methodological point of view, observing that deception does not change behavior of participants in experiments may be seen as comforting, since bad experiences may not question trusting instructions in future experiments. Furthermore, banning deception alone seems to be as insufficient as regulations to protect participants against deception in experiments or workers against illegal employment contracts which can be circumvented by outsourcing the hiring of labor via 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.

For a broader picture of deception in general, the conclusion of no effect of deception after debriefing may be seen as encouraging: after having experienced deception we do not question our behavior fundamentally, as one could expect, since human life provides plenty of such experiences. On the other hand, this could also mean that we are already so pessimistic that a further bad experience does not matter much. It is from this perspective that observing no deception effects might be troublesome. We might be living in a world where one more deception does not matter much since we anyhow expect the worst from each other.

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Online Appendix A: Tables and figures

Table 6: Experimental earnings in the main treatment.

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

Note: The first, second, and third component are average, minimum, and maximum payoff (EUROS).

Table 7: Experimental earnings in the control treatment. The first, second, and third component are average, minimum, and maximum payoff (EUROS).

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

Note: The first, second, and third component are average, minimum, and maximum payoff (EUROS).

Table 8: Frequency distributions of the direction of changes of allocations in the second task relative to the first task in the two treatments.

Treatment	$p = \underline{p}$				$p = \bar{p}$			
	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 9: Frequency distributions of the direction of changes of minimum acceptable allocations in the second task relative to the first task in the two treatments.

Treatment	$p = \underline{p}$				$p = \bar{p}$			
	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

The histograms of allocations in the treatments.

The histograms of minimum acceptable allocations in the treatments.

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

The histograms of allocations in the treatments.

The histograms of minimum acceptable allocations in the treatments.

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

Online Appendix B: Experimental instructions¹⁴

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 Detailed instructions for the first task

B.2.1 Detailed instructions to A and R-participants for the first task in 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 where the probability of sharing €24 is 80%, i.e. on average, one in five A, E, R groups can share only €8. Only E learns which of the amounts has randomly been selected.¹⁵

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 offer x , passed on to R, must be non-negative and no more than €8 if A and R can share €8, and non-negative and no more than €24 if A and R can share €24.

THIRD: before R learns about the offer choice x by A, and without knowing whether he, R, and A can actually share €8 or €24, R must choose 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:

¹⁴The reported instructions are a translation from the original instructions in German.

¹⁵In the mean treatment, E was informed of the actual amount that A and R could share just after having made her decision.

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.

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 Detailed instructions to E-participants for the first task in the main treatment

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 the E-participant in your group. As such will have to be aware of the common instructions for the A and R-participant. What you will now read are the instructions for both, the A and R-participant. The following instructions are the only ones read by the A and R-participant.

Insert [B.2.1] detailed instructions to A and R-participants for the first task in 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 [Detailed instructions to A and R-participants for the first task in both treatments] above, if you decide to engage both A and R. However, only A earns the same amounts as in the [Detailed instructions to A and R-participants for the first task in 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 [Detailed instructions to A and R-participants for the first task in 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 Detailed instructions to E-participants for the first task in the control treatment

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 the E-participant in your group. As such will have to be aware of the common instructions for the A and R-participant. What you will now read are the instructions for both, the A and R-participant. The following instructions are the only ones read by the A and R-participant.

Insert [B.2.1] detailed instructions to A and R-participants for the first task in 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 Detailed instructions for the second task

B.3.1 Detailed instructions to A-participants for the second task in the 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 that 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 Detailed instructions to R-participants for the second task in the 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 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. 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 $\text{€}(15 - 2 \times 5) = \text{€}5$ for sure and you, as R, earn the show-up fee of €5 plus the offer x 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 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 Detailed instructions to E-participants for the second task in the 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., $\text{€}(15 - 2 \times 5) = \text{€}5$.

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 Detailed instructions to A-participants for the second task in the control treatment

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 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, regardless of what R decides.

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, 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 Detailed instructions to R-participants for the second task in the 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 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 Detailed instructions to E-participants for the second task in the 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.