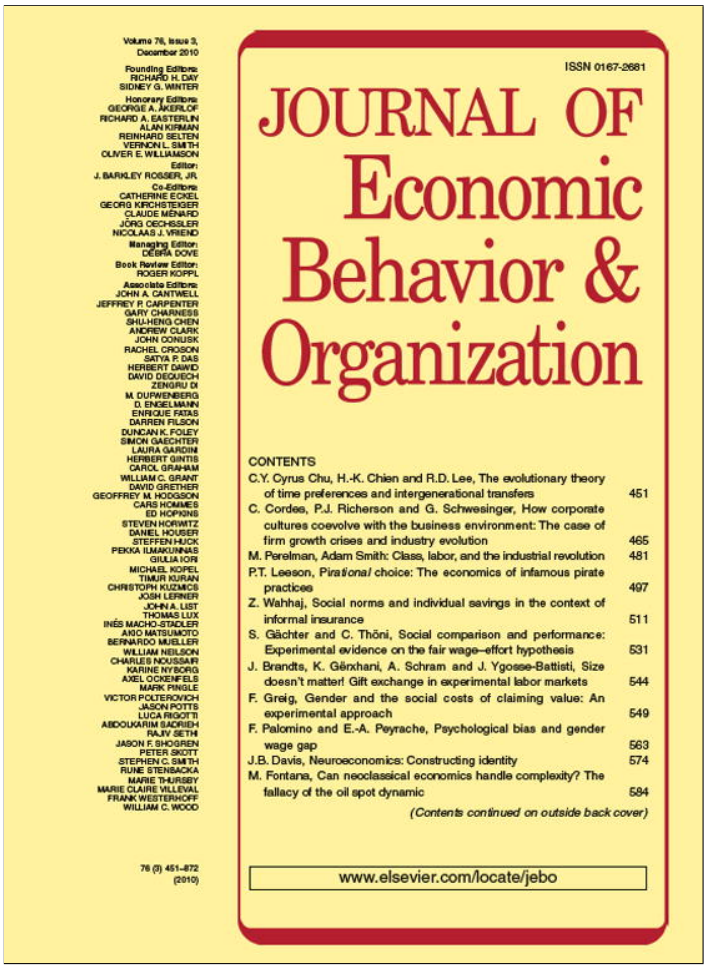

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Anonymity in the dictator game revisited

Axel Franzen*, Sonja Pointner

Department of Economics and Social Sciences, Institute of Sociology, University of Bern, Lerchenweg 36, 3012 Bern, Switzerland

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ABSTRACT

Giving in the dictator game has often been interpreted as evidence of other-regarding preferences. We suspect that giving is determined by subjects' attempts to appear fair in the eyes of recipients and the experimenter. Therefore, we investigate behavior in the dictator game by using the randomized response technique to increase anonymity. Overall, 290 subjects participated in two experiments. The results demonstrate that the randomized response technique reduces giving to negligible amounts compared to the standard double blind condition. Thus, our results suggest that individuals closely follow egoistic motives in the dictator game when anonymity is convincingly implemented.

1. Introduction

One of the most puzzling results in experimental game theory is that subjects share substantial amounts of resources with their recipients in the dictator game. The dictator game is a very simple one-shot decision situation in which an endowment is assigned to two players. One of them – the dictator – can distribute the amount in any way he/she wants and the recipient has to accept the allocation. Assuming individuals are payoff maximizing and given that recipients remain anonymous, dictators should keep the whole endowment and assign nothing to the recipient.

Many experiments show that observed behavior is at odds with this expectation. In the very first experiment using the dictator game, Kahneman et al. (1986) asked students to choose between an even split of $20 or assigning $2 to the recipient and keeping $18 for themselves. Three quarters of the subjects chose the even split and thus deviated substantially from maximizing monetary payoffs. Camerer (2003) reviewed eleven studies and concluded that on average about 20 percent of endowments are given to recipients. More recent experiments demonstrate that the amount allocated depends on various conditions. For instance, less money is transferred when subjects have to "earn" resources as compared to when they simply receive "windfall" gains (Arkes et al., 1994; Hoffman et al., 1994; Cappelen et al., 2007; Cherry, 2001; Cherry et al., 2002; List and Cherry, 2008; Oxoby and Spraggon, 2008), when there is a large social distance between proposers and recipients (Hoffman et al., 1996; Bohnet and Frey, 1999; Rankin, 2006; Charness and Gneezy, 2008), when recipients are poor (Branas-Garza, 2006; Aguiar et al., 2008; Cappelen et al., 2008) or when recipients are high status (Ball and Eckel, 1998; Harbaugh, 1998). In sum, most experimental results suggest that individual behavior is driven not only by selfishness but also by a

* Corresponding author.
E-mail addresses: franzen@soz.unibe.ch (A. Franzen), pointner@soz.unibe.ch (S. Pointner).

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concern for others well-being. Based on this evidence some authors suggest utility models that incorporate other-regarding preferences (e.g. Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000).

However, there are also studies which cast doubt on the idea that individuals really display other-regarding preferences. Bardsley (2008) and List (2007) report that dictators even withdraw resources from recipients when they have the choice in the "taking game". The result is difficult to reconcile with the assumption of other-regarding preferences. Rather the choice set seems to influence behavior (Neilson, 2009). Other studies find that dictators prefer not to enter the game or to exit it, if receivers have no knowledge about participating in the experiment (Dana et al., 2006; Lazear et al., 2006; Broberg et al., 2007). Sharing is also vastly reduced when the responsibility of the dictator is diminished, e.g. if receivers' payoff is not only determined by the dictator but also by chance (Dana et al., 2007). These results suggest that dictators might share only because they want to be seen as fair actors in the eyes of recipients.

Additionally, subjects might try not only to impress participants but also the experimenter. This idea is not new. It is well known that experiments are reactive settings. Subjects know that they are observed and hence their behavior is influenced by the expected expectations of the experimenter. Usually, experimenters try to counterbalance the "Pygmalion Effect" by implementing double blind procedures so that decisions are not disclosed to the experimenter or the recipient (Hoffman et al., 1996, hereafter HMS). But even in their most strict double blind condition (DB1) of HMS (1996) experiment, 36 percent of the dictators allocated at least some endowment to the recipient and some even gave half of the endowment. We suspect that conditions of anonymity were imperfectly conveyed to subjects or simply not believed by them. In the experiments that follow we use the "randomized response technique" and demonstrate that using it in dictator experiments reduces giving to almost zero.

The remainder of the paper is organized into four sections. The next section explains the randomized response technique. The third section describes our first experiment in which one group played the dictator game using the randomized response technique (treatment group) and the other group played it under standard double blind conditions (control group). In the fourth section we present results showing that other-regarding preferences almost disappear under the randomized response condition. The section also describes a second experiment in which we tested whether our results obtained with the randomized response are a methodological artifact. Finally, in the last section we summarize and discuss the results, and propose an alternative interpretation of giving under conditions of imperfect anonymity.

2. The randomized response technique

The randomized response technique (RRT) was originally designed by Warner (1965) to eliminate the response bias for sensitive questions in surveys and has since been further refined by several authors (e.g. Greenberg et al., 1969). RRT guarantees respondents almost perfect anonymity by using two random devices to conceal respondents' answer from the interviewer. A respondent is first asked to use a random device (for instance flipping a coin) and then to answer one out of two questions depending on the outcome of the coin toss. One of the two questions is a further random device (like whether the respondent's mother was born in an even month) and the other question is the one of interest (for instance whether the respondent ever engaged in a certain non appropriate behavior). The result of the first random device (coin toss) is concealed from the interviewer, who then cannot know whether a respondent's answer was provided in response to the target question or in response to the random question.[1] While individual behavior is obscured, the researcher can still estimate the proportion of the relevant behavior in question because he/she knows the proportion of positive answers provided to the non-target question. Let $P(A)$ be the probability that the outcome of the first random device leads respondents to the target question and $P(\bar{A})$ the probability that it leads to the random question. Let $P(B)$ be the probability of a positive answer to the questions and $P(\bar{B})$ the probability of a negative answer. Then the total proportion of positive answers is given by

$$P(B) = P(B|A)P(A) + P(B|\bar{A})P(\bar{A}) \tag{1}$$

The proportion of positive answers by respondents who answered the target question can be found by rearranging (1)

$$P(B|A) = \frac{P(B) - P(B|\bar{A}) \cdot P(\bar{A})}{P(A)} \tag{2}$$

The probabilities $P(B|\bar{A})$, $P(A)$ and $P(\bar{A})$ are determined by the RRT design and are therefore known whereas $P(B)$ is empirically determined through the survey. Thus, the proportion of positive answers to the behavior in question $P(B|A)$ can be calculated. Notice that RRT does not guarantee perfect anonymity to respondents since a positive answer implies the possibility that this answer refers to the target question.

[1] The efficiency of RRT has been evaluated in several studies (Lamb and Stem, 1978; Lensvelt-Mulders et al., 2005; Locander et al., 1976; Van der Heijden et al., 2000). Such evaluations consist in applying RRT to a sample of individuals for whom the true proportion of the occurrence of a specific behavior (e.g. sexual misconduct, fiscal fraud) is known. Naturally, the interviewees do not know this. The literature demonstrates that RRT often outperforms other questioning techniques in identifying the true proportion in the population.

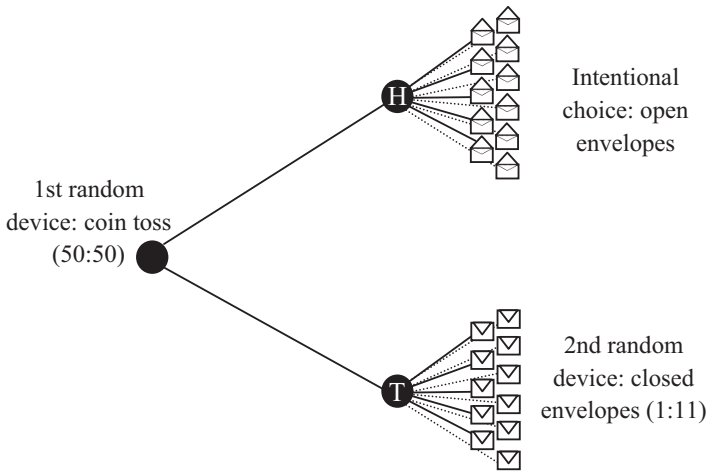

Fig. 1. RRT and the choice options in the dictator game.

3. Design of the experiment

RRT can be applied to continuous choices, such as in the dictator game, as well as to dichotomous choices. In our experimental design we used a coin toss as the first random device. Depending on the outcome of the coin flip, subjects either choose from a pack of eleven closed or eleven open envelopes (see Fig. 1). The eleven envelopes contained two euro vouchers, a white and a green one. The white voucher denoted the amount of money dictators could keep and the green voucher displayed the amount dictators could assign to the recipient. The value of the two coupons varied from zero to ten in one euro increments summing to the total endowment of 10 euro. Thus, every envelope contains one element of the binary value set {(0, 10) (1, 9) (2, 8) (3, 7) (4, 6) (5, 5) (6, 4) (7, 3) (8, 2) (9, 1) (10, 0)}.

If a subject's coin toss was heads, subjects were instructed to look in the open envelopes and pick the distribution he/she like most.[2] If the coin toss was tails, subjects were instructed to randomly pick one of the closed envelopes. Since there are eleven combinations, the probability of picking any one is $P(x) = (1/11)$. After making a choice, subjects had to conceal all open envelopes, keep the one they selected, and throw all other now closed envelopes into a box located by the exit. Subjects then left with their chosen envelope and exchanged the white voucher for euro. This procedure made it impossible for the experimenter to know whether the envelopes that were kept by the subjects were picked by chance (closed envelopes) or were intentionally chosen (open envelopes).

To conduct the experiment we recruited 177 students from a large university in Germany. Ninety-six were randomly assigned to the RRT condition and 81 to the control group. In each condition 8–10 students were invited to the lab at a time. Students could choose a seat upon arrival. They were divided by blinds so that they could not observe each other.[3] In the RRT condition every booth was equipped with the instructions, a coin, and the two piles of closed and open envelopes. The instructions, which were also read aloud by a student assistant, explained the dictator game and the purpose of the coin toss. Subjects were instructed that they were matched with another student, randomly chosen from the university, whose identity will not be disclosed. After having read the instructions, subjects were asked to toss the coin. If the coin was heads subjects were asked to choose from the open envelopes; if the coin was tails participants were asked to choose from the closed envelopes. The instructions explicitly mentioned the possibility of keeping or giving everything. After the decision, participants sealed all open envelopes, kept their selected one, and threw the remaining 21 envelopes into a box in the lab. No envelopes remained in the booths in the laboratory and subjects knew that the experimenter could not know from which pack they chose. Afterwards, subjects left the laboratory to exchange the white voucher for real money. Subjects slipped their envelopes under a closed door, and behind the door two voluntary student assistants were waiting to convert the white vouchers into cash. The cash amount was then slipped back under the door. As such, subjects had no visual contact with members of the research team and payout was strictly anonymous and not face-to-face. The instructions explicitly pointed out these details (see Appendix A for the instructions).

The experimental conditions of the control group were designed to be as similar as possible to the conditions of the treatment group (besides using RRT), and as close as possible to HMS's double blind procedure. In the control group the experimental booths contained the instructions, a large envelope containing ten one euro vouchers, and two small envelopes, one red and one blue. Subjects were instructed that they were matched with another student, randomly chosen from the

[2] Open envelopes were sorted, upmost the combination with the highest value for the white voucher (and the lowest value for the green voucher) and at the bottom the lowest values (zero) for the white voucher (and the highest value for the green voucher). Therefore, dictators must not search for their preferred distribution.

[3] When the participants arrived, they could immediately turn to a place in the laboratory. We did not control the names of the subjects on the participant list, as is standard in other experiments.

university, whose identity would not be disclosed, and that they could divide the ten euros between themselves and this other person, again, mentioning explicitly the possibility of keeping or giving everything. Subjects were directed to put the euro vouchers they wanted to keep into the red envelope and the ones they wanted to give away into the blue envelope. Both envelopes were then placed in a larger envelope which was then dropped in a box. A student assistant took the box to another room and placed the envelopes containing the real money requested on a desk. Subjects passed this room on their way out and could pick up their envelopes, containing the payoff, without facing anybody. During the whole procedure there was only an assisting student in the room who also read the instructions and answered questions if any were raised. It was also explained to subjects that the procedure of placing the vouchers into envelopes guaranteed the anonymity of their decision.

Although we intended to design our experimental control group as close as possible to the double blind condition of HMS, it did differ in some aspects. First, HMS assigned one person to monitor the subjects (to read the instructions aloud in order to verify that all got the same written directives and to assure that the subjects understood the rules). The monitor in HMS was paid $10, which was observed by the other participants and maybe motivated the other subjects to keep a greater part for themselves (HMS suggested this effect themselves). Furthermore, the difference between the results of HMS's DB1 condition (with monitor) and DB2 condition (without monitor) is negligible, so we proceeded without a monitor. Instead, we took a student assistant who read the instructions aloud but apart from this was not involved in the experiment. Hence, we tried to avoid the monitor effect and to reduce the impact of the experimenter.

Second, in DB1, HMS used two envelopes solely with blank "banknotes" in order to emphasize anonymity. No money was enclosed in these two envelopes. All other envelopes were filled with 10 dollar bills and 10 blank slips. So, the two subjects who got only blank slips were doomed to give nothing to the recipient and this heightens anonymity for the other subjects; if every test person in one session decides to keep the whole amount, the experimenter would not know if the decision was due to the blanks or due to egoistic preferences. Without the two envelopes containing only blank slips, anonymity would be restricted if all subjects would keep the whole endowment. However, a totally egoistic scenario is – as results of HMS show – very improbable and as long as somebody allocates a small amount, anonymity is secured even without the two blank envelopes. Therefore, we did not use the blank slip method in our control group.

Third, HMS used one dollar banknotes, while we used vouchers, since there is only a one euro coin, and retaining anonymity would be more difficult using coins.

Fourth, we did not invite the recipients into the lab whereas HMS gathered both types, dictators and recipients, and divided them afterwards in two separate rooms. Inviting recipients demonstrates authenticity of the procedure. This way, dictators should become aware that they cause real consequences for real recipients. However, seeing recipients has also the disadvantage that dictators might doubt anonymity and assign a positive probability to meeting recipients. Since anonymity is very crucial in our experiment we decided not to invite recipients to the lab but did highlight the existence of receivers by the instruction wording and by the envelope procedure with different colors for dictators and recipients. In case subjects doubt the existence of recipients then this should have a negative effect on the willingness to make a donation. However, our results show, that we did not have unusually low donations in the control group, suggesting that the lack of contact with recipients did not impact behavior.

Our fifth and last difference concerns the wording of the instructions. HMS (in DB1 and DB2) avoid wording that would infer a "sense of coupling between dictator and his or her counterpart" (HMS, 1996: 654). Therefore, their instructions avoid formulations like "divide" and "has been provisionally allocated". According to HMS, such wording would decrease the social distance between dictators and recipients and should be avoided. However, we think that the enhancement of social distance is undesirable.[4] Rather we believe that the decision should correspond to a real social interaction and should not be too artificial. Also, we did not want to confound anonymity with social distance. Consequently, we choose a similar standard wording as is used in Forsythe et al. (1994).

4. Results

Fig. 2 displays the distribution of the amounts allocated by the 81 subjects who participated in the control treatment. On average, subjects donated 3.09 euro to their recipients. Subjects most often chose an egalitarian distribution. About 22 percent (18 subjects) of all subjects decided to allocate half of their endowment, and 18.5 percent (15 subjects) gave nothing. In the experiments by HMS (DB1 and DB2) 58–64 percent of participants allocated nothing. We believe that this difference is mainly due to the specific instruction wording of HMS who explicitly tried to increase social distance between dictators and recipients, while we tried to manipulate anonymity without increasing social distance. We constructed the wording in such a way that the dictators could have the impression that they were part of a real social exchange situation (see Appendix A for wording).

In the RRT treatment group 96 subjects participated. Twelve sessions were played and 8.4 subjects attended each session on average. Fig. 3 displays the distribution of the allocated amounts. Since heads and tails should have occurred with equal frequency (first random device), about 48 subjects should have chosen the pack with the open envelopes and 48 subjects should have selected one of the closed envelopes. Selection from closed envelopes was done randomly (second random

[4] This was already pointed out by Frohlich et al. (2001) and Eckel and Grossman (1996).

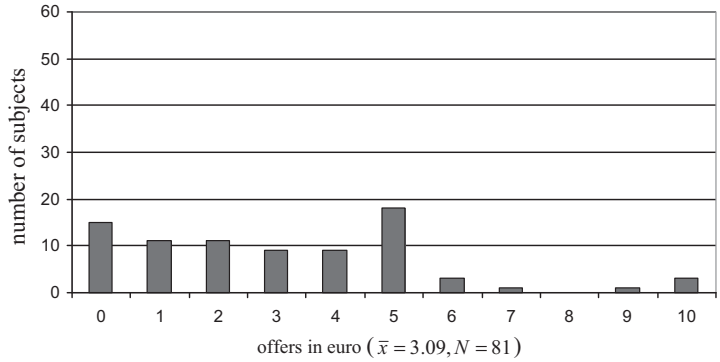

Fig. 2. Simple dictator game (control group), absolute frequency of amount given to recipient.

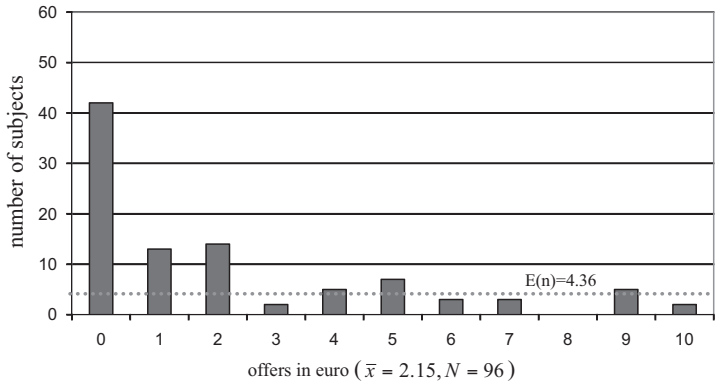

Fig. 3. RRT dictator game, absolute frequency of amount given to recipient.

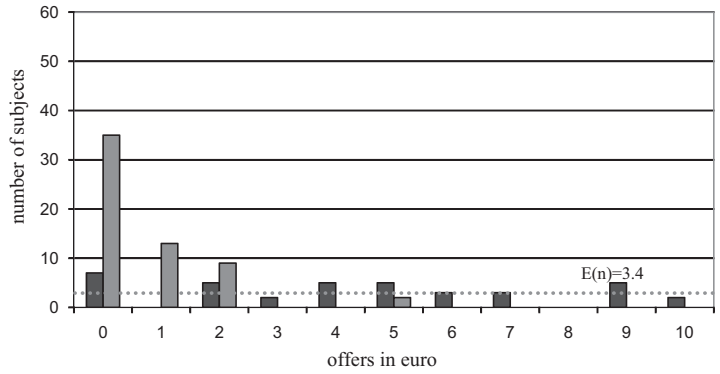

Fig. 4. RRT dictator game with known distribution of open and closed envelopes (open envelopes: light grey bars, $\bar{x} = 0.69$, $n = 59$; closed envelopes: dark grey bars, $n = 37$).

device) so that an expected number of 4.36 subjects should be in each allocated money category due to both random devices. Thus, in order to find the number of subjects who intentionally choose a certain allocation the expected number of 4.36 has to be subtracted from the numbers displayed in Fig. 3. Obviously, most subjects decided to keep everything or to give only one or two euro to the recipient. All other frequencies are very close to zero if the expected number of 4–5 subjects is subtracted.

Since the RRT has never been used before in a dictator game, we tested it by marking the closed envelopes from the inside. Thus, after the experiment we were able to identify the envelopes that were picked from the pack of closed envelopes, and to distinguish the intentional from the randomly determined choices. The result of the separate distribution is displayed in Fig. 4. Overall, 59 subjects (61 percent) chose the open envelopes, and 37 (39 percent) used the closed envelopes. This distribution is significantly different from the expected value of 48 (t-value = −2.295, $p = 0.024$). Thus, either the randomization process of tossing the coin resulted in a skewed distribution or some subjects chose intentionally from the open pack despite the result of the coin throw.

Almost 60 percent (35 out of 59) of the participants who turned to the open envelopes kept the ten euro and gave zero to the recipient. 22 percent (13 subjects) only gave one euro and 15 percent (9 subjects) gave two euro away. Fig. 4 also shows that only two subjects split the endowment in half and assigned five euro to the recipient. Overall, the 59 subjects who chose

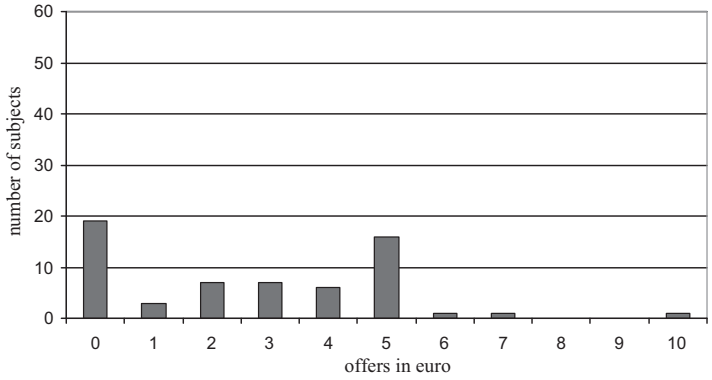

Fig. 5. Simple dictator game with anteceding coin toss ($\bar{x} = 2.7$, $n = 61$).

from the pack of open envelopes allocated only 41 euro to recipients from the total amount of 590 euro or 7 percent of their endowment. This is considerable less than ever observed under the standard double blind conditions. In comparison with our control group ($\bar{x} = 3.09$), the donated sum decreased to an average of 69 cents. This difference is statistically significant (t-value = 7.67, p = 0.000).

Thirty-seven subjects picked one of the closed envelopes. Thus, the expected number of participants in each category is 3.4. Fig. 4 shows that the observed frequencies are rather close to the expected ones. There are only two allocations (giving one euro and eight euro) that were never realized. However, a chi-square test confirms that the observed frequencies differ randomly from expected ones (χ^2 = 15.04, with df = 10). Thus, the second random device performed very well.

The difference between our first double blind procedure and the dictator game with RRT is tremendous, suggesting that RRT reduces other-regarding preferences considerably. We believe that the RRT method is a very successful way of convincing subjects of the anonymity of the experiment. However, there is an alternative explanation. Flipping a coin could produce a „gambling effect", i.e. participants might feel like they won a lottery. Thus, subjects could have formed the belief that they deserve the property rights of the ten euro.

To exclude such a gambling effect as an explanation for the observed behavior, we designed a second experiment combining a coin toss with the standard double blind procedure. In our second experiment, subjects had to toss a coin; if the coin was heads subjects could stay in the experiment and were told that they are in charge of distributing the endowment; if the coin was tails subjects were told that they have to leave the laboratory (all of them received a show up fee).[5] In sum, we use the coin toss from the RRT at the beginning of the simple dictator game but left everything else unchanged from the design of the control group of our first experiment. If a gambling effect exists, our subjects should reduce their donations in comparison with our simple dictator game without coin toss. If results remain as in experiment one, then a gambling effect can be ruled out. Fig. 5 shows results from the second experiment.

We invited 113 persons to this second experiment. Fifty-two threw tails and were subsequently sent to the cash point to pick up their show-up fee and left the laboratory. The other 61 subjects stayed in the lab and were given the instructions with the red and blue envelopes in a big cover as in the first experiment. On average, the subjects donated 2.70 euro. There is no significant difference in comparison with our first experiment without a preceding coin toss (t-value = 0.927, p = 0.36). Also, a Kolmogorov–Smirnov test shows that the distributions of the two experiments are not different (K–S-test for two samples: z-value = 0.745, p = 0.636), although in the second dictator game with coin toss, the proportion of fair offers (26.2 percent) is slightly higher than in the first experiment (22.2 percent). In sum, the results demonstrate that the coin toss per se generates no difference in behavior. Thus, the results in the RRT dictator game are not generated by a lottery effect. We conclude therefore, that the degree of anonymity in the RRT dictator game is decisive for the reduction of giving. The relicts of positive offers (donations of one or two euro) are maybe due to a residual distrust in the anonymity of the situation.

5. Conclusion and discussion

Anonymity is a very crucial issue in order to test the extent of other-regarding preferences of subjects. If subjects feel observed for any reason by recipients or by the experimenter or by any other party or fear that their identity will be disclosed, then subjects might share some resources for reasons of social desirability, reputation, or reciprocity. Thus, when subjects assign a positive probability of meeting the recipients in or after the experiment, they might share resources because they might fear that the recipients will reciprocate one way or the other. Experiments using the double blind condition as the one in Hoffman et al. (1996) are objectively anonymous. However, the crucial question is whether subjects were convinced that they remain anonymous. The RRT seems to ensure anonymity much better than the standard double blind condition. As a result, subjects give considerable less of their endowments away than in other "normal" anonymous conditions. We still had two out of 59 subjects that split the endowment in half and we did observe 22 participants who gave one or two

[5] The persons who left the laboratory were ruled out from being recipient, this was mentioned in the instructions.

euro away. Nonetheless, these results still cast doubt on the interpretation that individuals share resources in anonymous decision situations. First of all, the amount allocated to the recipient was considerably low (7 percent). Furthermore, it might also be the case that some subjects were still not convinced that their behavior would not be disclosed.

However, there is an alternative interpretation. The coin toss should have assigned half of the subjects to the open envelope condition and the other half to the closed envelopes. Taking a 90 percent confidence interval around the expected value of 48 into consideration, we should expect between 43 and 53 subjects in each condition.[6] However, the open envelope condition contained 59 individuals or 6 more than the confidence interval would allow. Thus, it is likely that some subjects did not follow the instructions and switched from the closed envelope condition to the open envelope condition. There could be different motives for doing so, such as maximizing the payoff (see also Fischbacher and Heusi, 2008). Accordingly, the low proportion of giving in the RRT treatment might be due to the selection of selfish individuals into the open envelope condition.

The exact number of cheaters is unknown but we can consider a few possible scenarios. One scenario is that 11 subjects (the difference between the expected 48 and the observed 59) moved from the closed envelopes to the open ones. In the worst case all 11 cheaters kept the whole endowment for themselves and donated nothing to recipients. Subtracting 11 zero givers from the 59 in the open envelope condition still results in a mean of giving of 0.85 euro (0.69 euro before). This is still a very low proportion of sharing which is a statistically significant difference from giving in the standard double blind condition (t-value = 6.89, $p = 0.000$).[7] Even if only 43 subjects (lower bound of the confidence interval) were randomly assigned to the open envelope condition and 16 cheated, mean giving would increase to 0.94 euro which is still very low and a statistically significant difference from giving in the double blind condition (t-value = 6.44, $p = 0.000$).

Alternatively, one could also consider the expected mean of the weighted means of having from 0 up to 59 cheaters. Assuming again that cheaters gave nothing, the expected means ($\bar{x}_0$ to $\bar{x}_{59}$) are calculated by reducing the number of zero givers according to the assumed number of cheaters and by weighting them with the probabilities (P) derived from the binominal probability density function.[8]

$$E(\bar{X}) = E(\bar{x}_0) \cdot P_0(c = 0) + E(\bar{x}_1) \cdot P_1(c = 1) + E(\bar{x}_2) \cdot P_2(c = 2) + \ldots + E(\bar{x}_{59}) \cdot P_{59}(c = 59), \tag{3}$$

with c denoting the number of cheaters. The sum of the expected means results in a value of $E(\bar{X}) = 0.865$. Thus, the expected amount of cheating would change our results only slightly.

Moreover, one might wonder how the interpretation of the results is affected under the assumption that *all* subjects cheated and decided for themselves to either pick from the pack of open or closed envelopes. In this case, subjects going voluntarily to the closed envelopes must be motivated by trying to avoid the decision altogether or choosing an exit option. Experiments with the dictator game providing an exit option suggest that the distribution of offers of exit option choosers is very similar to the distribution of offers by those who do not exit (see Dana et al., 2006). If there is no selection of subjects into the group of closed envelopes, there can also be no selection of subjects into the group of open envelopes. Thus, the assumption of *general dishonesty* suggests finding similar distributions in the open envelope condition as in the standard double blind experiment. However, our results are not compatible with this consequence and do therefore not support the assumption of general dishonesty.

Another concern could be that only altruistically motivated subjects comply with the instructions and that *all* selfish individuals are dishonest. However, we do observe 37 subjects in the closed envelope condition. According to the assumption they must all be altruists, otherwise they would have left this condition. Since we observe 37 randomly assigned altruists in the closed envelope condition we would also expect around 37 altruists in the open envelope condition. This consequence is hardly compatible with the distribution of giving in the open envelope condition. Thus, our experimental data probably contains some cheaters. But it is very unlikely that there were more than 16 selfish individuals moving from the closed to the open envelope condition.[9]

Since there is some evidence of cheating future applications of the randomized response technique that are not able to control for it should take this into account. One way of doing this is to incorporate a deviation factor ($0 \leq \gamma \leq 1$) into Eq. (2). In our case we encounter some deviation concerning the first random mechanism $P(A)$:

$$P(B|A) = \frac{P(B) - P(B|\bar{A}) \cdot P(\bar{A})(1 - \gamma)}{P(A)(1 + \gamma)} \tag{4}$$

Of course the size of γ is undetermined and depends on different circumstances as incentives and population. It is presumably small in our case: $0.06 \leq \gamma \leq 0.17$.

Given our results we believe that subjects behave reasonably close to maximizing private payoffs under convincing conditions of anonymity. Of course, this does not mean that fairness or altruism never exists. Much to the contrary, sharing, volunteering or donating is observed as soon as subjects suspect a chance of being observed. We know from other experiments that subjects assign prestige to volunteers and altruists. It has also been shown that individuals with higher prestige

[6] The values are obtained by calculating the binominal probability distribution for $p = 0.5$ and $N = 96$.

[7] All reported t-test values refer to two tailed tests for comparing groups with unequal variances.

[8] These probabilities are adjusted for the fact that they sum up to only 0.99 instead of 1 since the maximal number in the open envelope condition is 59 and not 96.

[9] According to the binominal probability density function, the probability that the group of closed envelopes contained even more than 53 subjects is only 5%.

have more influence over others (e.g. Willer, 2009). Thus, there are advantages to volunteering, donating or behaving altruistically when behavior is disclosed to others. However, when subjects cannot be observed their behavior becomes much more selfish. Therefore, we believe that the results with the dictator game were misinterpreted in the past. The amount of resources shared in other dictator experiments is most likely due to an imperfect management of anonymity and not due to an intrinsic inclination of subjects to show other-regarding preferences. Thus, results of the randomized response dictator game further questions theories of fairness which assume that decision makers care for the outcome of others (e.g. Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000).

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Appendix A. Supplementary data

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