Fairness in an Intergenerational Dictator Game With Social Interaction

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Abstract

Experimental evidence indicates that non-monetary considerations influence subjects' allocation behavior. We investigate the impact of a multi-generation setup on allocation behavior. We modify the traditional dictator game by introducing 3 generations A, B and C. A takes an arbitrary share of a pie and passes the rest to B. B divides the rest of the pie between her and C. We find that this intergenerational dictator game increases generosity with respect to comparable traditional dictator games. Introducing a social interaction between A and B on the one hand and B and C on the other hand leads to a higher share of passed money. If it is unclear to B if a subject passed the share or if the passed share was generated by a random process, B becomes more parsimonious. In all treatments B reciprocates for A's behavior in his decision upon the share which C receives.

JEL classification: A13; C71; C72; C78; C91; D62; D63; D64; J71; Q30

1 Introduction

In a series of experimental studies economic researchers have challenged the classical economic assumption that economic agents are solely motivated in their actions by monetary incentives. In different situations people frequently exhibit behavior suggesting that non-monetary considerations must have driven their decisions. The dictator game is one of the most famous experiments to show that non-pecuniary motivations are inherent in human nature. In that game, two anonymous subjects are matched, one being the allocator, the other being the recipient. The allocator is free to split a certain amount of money between him and the recipient while the recipient's role is completely passive. No matter how the allocator decides, the split is made according to her decision. The game theoretic prediction is that in a subgame perfect equilibrium, the rational allocator will take the whole pie for herself, leaving zero to the recipient.

In a seminal study on the dictator game, Forsythe *et al.* (1994, herafter, FHSS) found that contrasting to the game-theoretic predictions, the recipients obtained 21% of the pie on average. Since the dictator game does not give rise to any strategic considerations in the classical economic sense, there must have been something else at play in the allocator's decision process such as the desire to behave fairly towards the recipient. After FHSS many studies to be surveyed below tried to shed light on this puzzle. Hoffman *et al.* (1994, 1996) controlled their experiments for subject-experimenter-anonymity, Bohnet and Frey (1999), Charness and Gneezy (2003), and Burnham (2003) used different degrees of social distance. Goeree *et al.* (2007) and Leider et al. (2007) used a network structure, to elicit subjects allocation behavior. Andreoni and Bernheim (2007) introduced a random element in the procedure. Several authors used different models of preferences to explain the empirical data e.g. Fehr and Schmidt (1999), Bolton and Ockenfels (2000), Andreoni and Miller (2002), Charness and Rabin (2002). Bardsley (2005) and List (2007) interpret the other-regarding behavior found so far as an experimental artefact.

Our approach focusses on different aspects. The key issue of our study is an innovation with regard to the setup of the traditional dictator game. We enhance the dictator game by a third generation, giving the setup a sequential character. We call the game *intergenerational dictator game*.[1] In particular we investigate the influence of social interaction, the role of reciprocity in the intergenerational setup and the impact of introducing a random element. More precisely we set up treatments with three subjects each, A, B, and C. In each treatment subject A is asked to divide a certain amount of money Π into an amount $\alpha\Pi$ which she can keep for herself and an amount $(1-\alpha)\Pi$, to be passed to subject B. Subject B again is asked to divide $(1-\alpha)\Pi$ into an amount $\beta(1-\alpha)\Pi$ that she can keep for herself, and an amount $(1-\beta)(1-\alpha)\Pi$ to be passed to subject C. In all treatments we ensure (almost) anonymity between subject A and subject C. To the best of our knowledge, there had only be one setup so far which is remotely similar to our game but this setup lacks the sequential character: Karni *et al.* (2001) studied the behavior of a dictator who assigned probabilities of winning a lottery to two recipients. Our setup could also be seen in line with the literature on common-pool-resources. Here, other-regarding behavior in intergenerational setups is investigated. Studies most related are the ones by Chermak and Krause (2002), Sadrieh (2003) and Fischer *et al.* (2004). The problem with regard to these approaches is that subjects are confronted with complicated strategic considerations which make it difficult to interpret their decisions with regard to other-regarding behavior. However, our approach is so simple that subjects' intention to be altruistic is obvious.

Treatment 1 is just a sequence of the usual two person dictator game. In treatment 2 and 3 we introduce some social interaction both between A and B and between B and C, contributing to the studies where social distance is varied. The social interaction consists of a simple cooperative form of a scrabble game, where additional money can be earned. In treatment 2 the social interaction takes place after A, or B be respectively, have divided the pie, while in treatment 3 the order is reversed: Social interaction takes places first between A and B (B and C, respectively) and then A (B, respectively) makes his decision how to divide the pie. The final treatment 4, is similar to treatment 1 but we introduce a random element which has some parallels to the one by Andreoni and Bernheim (2007). In this treatment, there is no social interaction, but a coin decides whether subjects A or a random mechanism

[1]The name *sequential* dictator game was already used by Cason and Mui (1998), who had two dictator games played one after another but with new endowments each

splits the pie.[2]

Our main findings are, first, that even in the normal intergenerational dictator game (treatment 1), subject A passes an amount to B and C , which is significantly higher than in the two person dictator game. Second, the generosity of players A and B is enhanced in the social interaction treatments. Third, we do not find significant differences between behavior in treatments 2 and 3, although the motivation for being more generous than in treatment 1 may be different in both treatments. In treatment 2 the reason for being generous could be to avoid a situation where you have to spend some embarrassing time with B (or C, respectively). In treatment 3, by contrast, the driving force of enhanced generosity may be increased affection towards B (or C) induced by the previous social interaction. Comparing treatment 4 to treatment 1, we do not find significant differences concerning A's behavior, while there was, to our surprise, a highly significant difference between the allocation behavior of the Bs.

Moreover, we controlled for some socio-economic data of the participants. For the As, we find that income is positively related to generosity. Gender does matter, since females are less parsimonious than males. For the second generation (subjects of type B), the dominant factor determining their allocation choice is the first generations' choice and the introduction of the random element. The other factors influencing the first generations' decision have, at maximum, a very weak impact on the second generation's choice.

As mentioned above, several researchers have tried to explain the observed deviation from the game-theoretic prediction assuming perfectly rational economic subjects. While in FHSS the recipient's anonymity was not ensured with respect to the experimenter, Hoffman *et al.* (1994, hereafter HMSS) and Hoffman *et al.* (1996, herafter HMS) conducted experiments where they ensured not only in-between subject anonymity but also subject-experimenter anonymity finding that this treatment reduces generosity dramatically. They conclude that these results reject the hypothesis of other-regarding preferences and claimed that the desire to build up a reputation of being fair is the reason for the subjects being fair in dictator games. As soon as subjects are completely anonymous, they do not have to care about the regard of others and behave in accordance with the traditional model of the purely selfish agent.

[2] We are thankful for John List's inspiration on the setup of treatment 4.

Bolton *et al.* (1998) conducted a study where they use menu-based games where the allocator has to choose between two or more divisions of the pie. They were not able to sustain the result that subject-experimenter anonymity reduces subjects' generosity. Bohnet and Frey (1999) used the experimental study by HMS as point of departure. They compared treatments where the allocator saw the recipient (while the recipient did not see the allocator), where the allocator saw the recipient and received some personal information about the recipient and where the recipient and the allocator had bidirectional visual contact. They found that the narrowed social distance increases generosity significantly for the two last treatments. Since repeated game effects could not play a role in the second treatment, because the recipient did not get to know the allocator, repeated game theory could not be used as explanation. Bohnet and Frey conclude, that the social interaction 'transforms anonymous, faceless entities into visible, specified human beings' (Bohnet and Frey, 1999, p. 339) and thereby reject the traditional model of the selfish agent in contrast to HMS, claiming that empathy and hence altruism is important. Charness and Gneezy (2003) found that giving the allocator the name of the recipient increases the allocator's generosity significantly. The same happens in the study by Burnham (2003) when photos were showed. Goeree *et al.* (2007) examined the network structure among teenage-girls. Using this structure to explain allocation behavior they find that increasing social distance reduces generosity. Leider et al. (2007) confirmed this evidence for social-networks.

Other approaches abstracting from anonymity but introducing preferences, governed by certain features, have been made to explain the subjects' behavior in these experiments. Fehr and Schmidt (1999) and Bolton and Ockenfels (2000) explained the evidence by subjects' inequality aversion. Andreoni and Miller (2002) interpreted their experimental results by different types of players which have their own payoff and overall welfare as arguments in their utility function. Depending on the type, both arguments are incorporated in different utility functions. Charness and Rabin (2002) findings from an experimental study could also be explained best by preferences which depend on overall welfare rather than on inequality aversion, in addition they incorporated reciprocity in subjects' preferences. Rabin (1993) and Dufwenberg and Kirchsteiger (1998) explicitly used reciprocity as explanation. Here, in addition to the subject's own payoff it mattered to the subjects whether their 'partner' treated them nicely or not. If the 'partner' was nice, subjects felt the desire to reciprocate by being nice, too, the same hold true if the 'partner' was perceived as unfair. The results of the classic ultimatum games, where the recipient had the option to forfeit the allocators offer and leaving both with a zero payoff, confirmed this theory (see e.g. Güth *et al.*, 1986). Charness' and Rabin's (2002) results from a response games (where two subjects interacted sequentially) also suggested that reciprocity was an important element in subjects' decision making process. In their experimental study, subjects' willingness to sacrifice money in order to generate a fair outcome was reduced if previously the other subjects themselves were not willing to sacrifice in order to obtain a fair allocation.

Bardsley (2005) and List (2007) gave the allocators the opportunity to take money from the the recipient. They found, that the share of subjects taking money was larger than subjects, who give nothing in the traditional setup. Thus, they conclude 'heretically' that the evidence for other-regarding behavior so far could be an experimental artefact due to 'Hawthorne'-effects.

Another aspect which has been investigated is the role of gender. Bolton and Katok (1995) rejected the hypothesis of gender as explanatory variable in dictator type games while Eckel and Grossman (1997) reported that females are more generous than males. Andreoni and Vesterlund (2001) reconciled the different results by stating that males and females response differently to the 'price' of giving. Also other personal data like age and study subject could play a role: List (2004) found in an experimental study on contribution to the Red Cross that age mattered. Carter and Irons (1991) found that studying economics lead to a significantly different behavior in ultimatum games. Meier and Frey (2004) also found that business students are less likely to donate money than other students.

Our paper is organized as follows: In section 2 we explain the experimental setup. In section 3 and 4 we analyze the allocation choices of the first generation (A), using non-parametric methods in the former section and a regression analysis in the latter. In section 5 and 6 we use the same instruments as in section 2 and 3 to examine the second generation's (B's)behavior. We summarize our results in section 7.

2 Experimental Design

The experiments were conducted from October 2000 to January 2001 in Heidelberg with 117 participants in 9 sessions and from May 2002 to August 2007 in Kiel with 572 participants in 54 sessions. Participation was restricted to one session. The basic design has already been briefly sketched in the introduction. The average amount of money which could be earned varied from 13€ to 15.5€ depending on the treatment. This amount included a show-up fee of 3€. Duration of the experiments was between 35 to 90 minutes depending on the treatment. The instructions for the experiments are attached in the appendix (the instructions were in German, the attached version is a translation). We did not conduct any pre-test. All observations were used for the analysis. The subjects for the experiment were recruited by posting notices in student cafeterias. Hence, there was a wide range of study subjects among the subjects. Participants were gathered in one room where they had to read the instructions. After 5 to 10 minutes (depending on the treatment) questions with respect to the experimental procedure were answered. Then, subjects were randomly assigned to groups. Usually, there were 3 to 5 groups in one session. One group consisted of 3 members: A, B and C, except for the replication treatment of the traditional dictator game. The position of being A, B or C was again assigned randomly. For treatment 3, after random division in groups and assignments, we asked the subjects, whether within one randomly selected group, subjects knew each other. If this was the case, we rematched the subjects in order to preclude that some pre-relationship influenced the subjects behavior.

The basic game was what we call a *intergenerational dictator game*. The experimental setup is sketched in Figure 1. Subject A received an amount Π

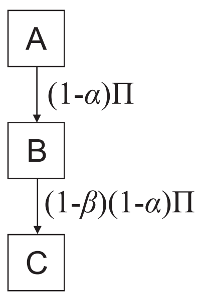

Figure 1: Basic Experimental Setup

of 30€ (we did not use experimental currency units). From this pie A had to choose a share α which he kept for himself while passing the rest $(1-\alpha)\Pi$ to B and C. Afterwards, B divided the rest between B and C, taking a share β from the rest for herself. Also B had complete discretion over the allocation of the rest of the pie.

Treatment 1 used the basic game while ensuring anonymity between A, B and C. Subjects of type A were lead into separate rooms each and were told to divide the pie between A and B. This division was done in writings on a form (form 1, see appendix), the money was payed, later. During their decision the subjects were left alone in the room. In addition, personal data (gender, study subject, income and age) were asked and to be filled into the form. Then, form 1 was collected and subjects of type A received a second form (form 2a) in which they were asked about their motivation concerning the division of the pie and about their knowledge about the concept of sustainability. Afterwards, subjects of type A were paid their money and left without further meeting subjects B or C, who waited in separate rooms, one for subjects of type B one for those of type C. Then, subjects of type B were lead into separate rooms each where they were informed by the experimenter how much money of the initial pie (Π) had been passed on to them by the subject A within their group. Subjects of type B were then asked to split the remaining amount $(1-\alpha)\Pi$ between her and subject C. B filled out the same form 1 as A. When making their decisions subjects of type B were left alone in their rooms. After form 1 had been collected, B had to fill out some form 2b where he had to answer the same questions as A in form 2a, but in addition, B was asked which amount of money he had expected to be passed on by subject A. Then subjects of type B were paid their money and left. Finally, subjects of type C were informed each about the amounts of money which had been passed, and then were asked to also fill out form 2b. Then, subjects of type C were paid and left. Anonymity was ensured in the sense that A was not confronted with B and C and vice versa as soon as the treatment started. Subjects only briefly saw each other during the instructions where between 9 and 15 subjects were to be randomly matched while being gathered in one room (for treatment 3 we controlled that subjects within one group did not know each other). The lower degree of anonymity with regard to FHSS and HMSS (who put allocators and recipients into different rooms from the begin on) might be appropriate since Frohlich *et* *al.* (2001) have shown that subjects might doubt the existence of a recipient if there are no clues about her actually participating in the experiment.

Treatment 2 resembled treatment 1 except that A and B interacted socially after A had divided the pie. Subject of type A made his allocation choice first, being alone in the room when filling out form 1. After A had completed his form, B was lead into the same room and was informed about the amount of money, A had passed to him. Then, A and B had to play a simple form of scrabble, where they cooperatively tried to gain as many points as possible by forming German words from the scrabble letters within 15 minutes, following the usual rules of the scrabble game with respect to assigning points to the letters they managed to use. In that game they could earn up to 3€ depending on their success. The resulting payoff from the scrabble game was split equally between A and B. The same procedure was repeated for B and C who also played the scrabble game after B had made her decision and filled out their forms.

Treatment 3 was similar to treatment 2 except that the choice on the allocation of the pie was made after (!!!) the scrabble game. A and later B, were left alone in the room while making their decision and filling out their forms.

Treatment 4 was the same as treatment 1 except that B and C did not know if the initial allocation decision was made by either A or by a random mechanism. By tossing a coin (in presence of A), we decided whether the allocation was made by A or by the random mechanism. If A was to make the choice, the experiment continued as in treatment 1. If the random mechanism was supposed to allocate the money, we used a 20-sided dice (again, in the presence of A) to determine the amount of money for A. Subject A could receive integer values between 10€ and 30€. We left out 11 as a possible amount to obtain 20 possible allocation which could be assigned to A - the idea was to keep the frequently (by A) chosen allocation (10,20) in the allocation set. We exclude the values 0 through 9 since it was unlikely that A would take less than one third of the pie. Since the random process yielded only integer allocations, A (if A was allocating) was only allowed to choose integer allocations, too, i.e. A was not allowed to pass on e.g. 12.5 Euros.

We also ran a replication of the traditional dictator game to ensure that our results are comparable to other studies. Here, the setup was similar to treatment 1 except that there were only two generation A and B.

3 Analysis of A's Allocation Choice

In this section, we analyze the allocation choice of subject A, and we compare the impact of the different treatments. The results from the four treatments are displayed in histograms in Figure 2. Here, we see that in the

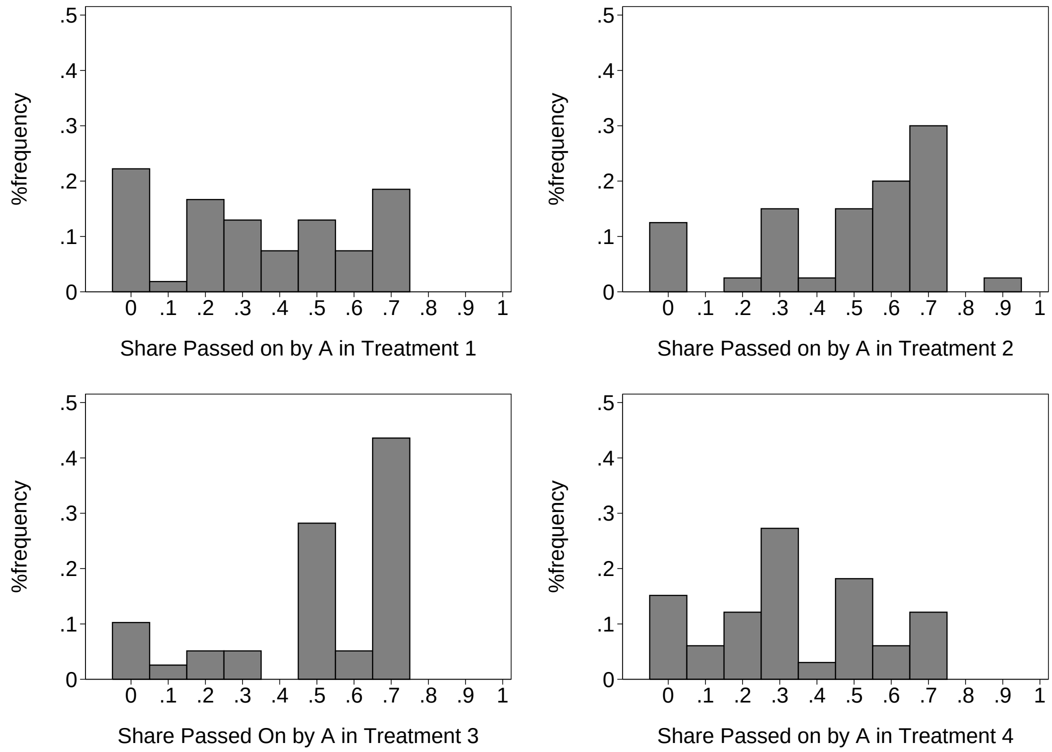

Figure 2: Comparison of Shares Passed on by A: Treatment 1-4

treatments *with* social interaction the frequency of shares passed on by subject A peak at 0.7, while *without* social interaction the frequency of those shares is rather uniformly distributed between passing nothing to 0.7. Note, that the 0.7 column represents the subjects passing two thirds, which is the fair share.

Interestingly, there is one observation in treatment 2 where A passed on 26 Euros and thus significantly more than two third of the pie. This is surprising since that choice increases social inequity in favor of B and C. Since subjects had to explain their allocation (in form 2), it turned out that this subject had understood the rules but deliberately chose this allocation.

She claimed that her monthly income (which was around 350 to 500€ after having paid rent for housing) was sufficient for living, and therefore she had no desire to earn additional money. She said, she just participated to have fun. Since from her perception the other subjects' participation was mainly motivated by monetary considerations, she argued that it would be welfare maximizing to pass on more money than the equal split. This case favors the social welfare preference model by both Andreoni and Miller (2002) and Charness and Rabin (2002) in contrast to the difference aversion models by both Fehr and Schmidt (1999) and Bolton and Ockenfels (2000). Admittedly, such cases obviously do not occur very often.

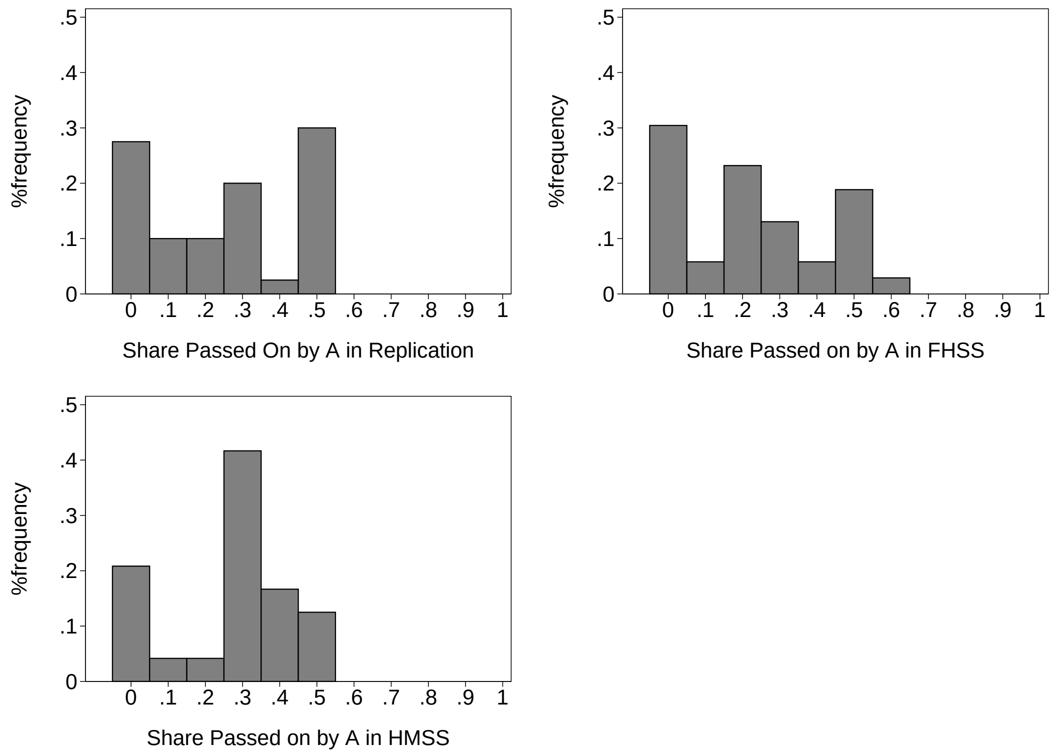

Figure 3: Comparison of Shares Passed on by A: Replication vs FHSS and HMSS

We also compare our results from the replication for our setup with the traditional dictator game by FHSS [3] and HMSS [4] which are shown in Figure 3. The results of our replication are roughly in line with the ones by FHSS

[3] The sample for FHSS consists of a pooled sample combining the 'dictator pay' treatments for both the pie sizes with 5$ and the one with 10$.

[4] The sample for HMSS consists of the sample from the 'Dictator Random Entitlement' treatment whose setup was pretty similar to the one by FHSS.

and HMSS, though the share of subjects allocating fairly is higher in our setup (but not significantly). In both FHSS and HMSS, subjects passed a maximum share of 0.5 (except for 2 subjects out of 69 in FHSS), while in our treatment 1 almost 30% of the subjects A chose to pass on a share between 0.5 and 0.66. The explication is of course the reference point of a fair allocation which leaves every generation with an equal share of one third. One has to note that some differences in distribution may stem from the different pie-sizes used in the different treatments.

Table 1 lists the means of the allocation choices of A and the number of observations for the different treatments. In the treatments 1 and 4, subject A

Table 1: Descriptive Results Allocation Choice A

Treatment	Number Obs	Mean Share Passed on by A
1	54	0.34
2	40	0.48
3	39	0.49
4	33	0.33
Replication	40	0.26
FHSS	69	0.23
HMSS	24	0.27

passes on an average of one third of the initial pie. Social interaction lowers the average of shares kept by A for himself to about one half. In our replication, allocators pass an average share of 0.26 of the pie, which is in line with the results by FHSS where this share is 0.23 and HMSS which have the same mean share passed. We observe that both traditional dictator games show a lower mean than the results obtained in our treatment 1 with a mean of 0.34.

In Table 2 we report the results of testing whether the outcomes of the different treatments are significantly different. The tables lists the corresponding test-statistics, p-values are in parenthesis beneath (the same holds for the rest of the tables with treatment comparisons using these tests).

We employed the Mann-Whitney (MW), the Anderson-Darling (AD), and the Epps-Singleton (ES) test. FHSS compared different non-parametric tests (Kolmogerov-Smirnov, Cramér-von-Mises, AD, MW and ES) using Monte Carlo simulations. They found that the AD and the ES test had the highest power. Hence, we also use these two tests. In addition we include the MW test since the concept of the sample comparison is different from AD and the

Table 2: Treatment Comparison of A's Allocation Choice

Treatments Compared	MW	AD	ES
1A vs 2A	-2.549	3.544	10.107
	(0.011)	(0.014)	(0.039)
1A vs 3A	-2.827	3.891	10.658
	(0.005)	(0.010)	(0.031)
1A vs 4A	0.295	0.343	1.599
	(0.768)	(0.913)	(0.809)
2A vs 3A	-0.384	0.785	1.559
	(0.701)	(0.494)	(0.816)
2A vs 4A	2.934	4.184	11.795
	(0.003)	(0.007)	(0.019)
3A vs 4A	3.188	4.716	14.420
	(0.001)	(0.004)	(0.006)
1A vs Repl	1.850	2.921	12.331
	(0.064)	(0.028)	(0.015)
1A vs FHSS	-2.686	5.150	15.883
	(0.007)	(0.002)	(0.003)
1A vs HMSS	1.455	2.710	17.093
	(0.146)	(0.038)	(0.002)
Repl vs FHSS	-0.751	0.985	5.220
	(0.453)	(0.368)	(0.266)
Repl vs HMSS	0.261	0.972	8.117
	(0.794)	(0.375)	(0.087)

ES test, while the Kolmogerov-Smirnov and the Cramér-von-Mises test both are rather similar to the AD test. Note, that the AD and the Mann-Whitney test require continuous data (in the sense that there are no ties). Since ties are a frequent result in dictator games, this must be taken into consideration (see FHSS). While the MW statistic can be corrected for ties, the AD test data have to be transformed by adding a random term to ensure continuity. This random term is uniformly distributed between [0,0.0001] and hence, due to the small value added, does not distort the results.

Our main findings from the comparisons are as follows: If we compare the outcome of a treatment without social interaction (i.e. 1 or 4) to the outcome of a treatment with social interaction (i.e. 2 or 3), i.e. 1A vs. 2A, 1A vs. 3A, 2A vs. 4A, and 3A vs. 4A, we find that the outcomes are significantly different at the 5%-level. This together with the results from Table 1 shows that social interaction (either before of after the allocation choice) increases generosity significantly.

On the other hand, we do not find significant differences concerning the choices of subject A in the treatments 1 vs 4 and 2 vs 3. This means that, first, if there is no social interaction, the introduction of the random element does not influence A's choice significantly. Second, and more interestingly, given that social interaction takes place, it does not seem to play a major role for A's choice whether the interaction takes place before or after A has to decide on the division of the pie between herself and subjects B and C, although the motivation to pass on a larger share compared to the treatments without social interaction might be different.

In treatment 2 where A decides before social interaction, there are two possible explanations why A passed on more compared to the anonymous dictator games in treatment 1 and 4. The first explanation is that A wants to give B an incentive to cooperate. If B feels to be treated unfairly by A, she might want to punish A by refusing to cooperate in the scrabble game and hence reciprocate according to the model suggested by Rabin (1993). Even if this behavior would be at B's own expense (since B earns half of the payoff from the scrabble game), this explanation would be consistent with results from previous experiments.[5] In our setup, the impact of this reasoning should not be too large since the possible loss for A (through punishment by B) is rather low. Playing scrabble alone would not dramatically reduce a subject's earnings. If we additionally compare the rather low maximum possible payoff of 3 Euros in the scrabble game to the pie of 30€, the influence of the willingness to provide incentives to cooperate should not be too strong. Still, 10% of the subjects in treatment 2 explained their choice by the incentive argument, however, 7.5% particularly hinted at the negligible impact of cooperation in their justification due to the small money at stake.

A second explanation for A's generosity in treatment 2 could be that A wants to avoid an awkward situation when being forced to be together and to carry out a joint task with another person in the same room for 15 minutes, after he had treated that person not nicely by being greedy. However, except for one subject no one explained her behavior in the line of that argument. Nonetheless, we believe that there is a considerable influence of that

[5]In ultimatum games, e.g. in Güth *et al.*, 1986, subjects revealed a strong desire to reciprocate such that they incurred costs in order to punish someone they perceived as behaving unfairly.

effect of anonymity. A share of subjects 20% of the subjects of type A in treatment 1 and 12% of those subjects in treatment 4 justified their behavior with anonymity. This allows us to reverse the argument: If subjects name anonymity to justify their greediness, being generous implies that social affection is important.

In treatment 3, there could also be two particular effects increasing generosity. The first effect may be that A feels the obligation to reward B for successful cooperation. However, only 5% of the subjects from treatment 3 named this argument (from an incentive point of view there would be no reason for this behavior).

The second effect could be that subjects got to know each other during the scrabble game which lead to an increased affection of A towards B. Actually, only one subject mentions this as a reason. Still, the argument must be seen in the same way as for treatment 2, by proving its existence indirectly through the high share mentioning anonymity in treatment 1 and 4.

There might be a third effect playing a role in both treatments 2 and 3 which, opposite to the effects described previously, reduces the amount that A passes to B: Since B plays the scrabble game twice, while A and C play only once, A might account for this and, by concerns of equity pass less than one third to B. This argument would be supported by the results of both Fehr and Schmidt (1999) and Bolton and Ockenfels (2000). Actually, 5% of the subjects of type A taking part in treatment 2 and 18% of those taking part in treatment 3 put forward this argument to explain their decision.

Comparing the results from our sequential dictator game to the results from the traditional standard two person dictator games we find that there is a significant difference between our results and the results by FHSS for all test statistics. Comparing our results to HMSS, we obtain significant differences for both the Anderson-Darling and the Epps-Singleton test statistic, while the Mann-Whitney test statistic is insignificant, which possibly has to do with the high number of ties in the sample by HMSS. When people decide how much to keep for themselves, it obviously does matter whether they share a pie with one or with several people. Thus, in our set-up subjects pass on considerably more compared to two person dictator games.

Comparing the results from our replication with the results from the traditional dictator game by FHSS and HMSS yields no significant difference.

Obviously, the slight differences in instructions and procedure do not matter. [6]

Since experimental studies, e.g. Eckel et al. (1998) and Andreoni and Vesterlund (2001), often find significant differences in behavior with respect to gender, we compared males' and females' decisions within the samples. The descriptive results are summarized in Table 3.

Table 3: Descriptive Results Allocation Choice *A* With Regard to Gender

Treatment	Mean Share Passed On by Males	Obs Males	Mean Share Passed On by Females	Obs Females
1A	0.31	37	0.40	17
2A	0.43	29	0.61	11
3A	0.46	28	0.56	11
4A	0.30	17	0.36	16
Replication	0.21	24	0.33	16

Males pass on less in every treatment, especially in treatment 2 this effect is very pronounced.

Table 4: Testing for the Impact of Gender on *A*'s Choice

Treatment	MW	AD	ES
1A	-1.145	1.039	2.492
	(0.252)	(0.340)	(0.646)
2A	-2.266	3.227	5.345
	(0.023)	(0.019)	(0.254)
3A	-1.119	1.034	2.444
	(0.263)	(0.342)	(0.655)
4A	-0.871	0.815	2.831
	(0.384)	(0.476)	(0.587)
Replic	-1.514	1.536	5.817
	(0.130)	(0.167)	(0.213)

In Table 4 one can see the results from comparing males and females *within* the treatments. We observe that only in treatment 2 there is a significant difference between male and female behavior (p-values around 0.02).

[6] Note, that the Epps-Singleton statistic for the comparison of our replication and HMSS is close to being significant with a p-value of 0.088. But comparing the pooled FHSS sample to HMSS would yield a p-value of 0.034 for Epps-Singleton, hence we neglect this effect and still assume that there is no substantial difference.

An explanation why females are more generous in treatment 2 could be that females could feel more uncomfortable being in a room and cooperating with someone whom they had treated unfairly. In fact only one subject, being female, justified her generosity by putting forward this argument.

We checked for treatment differences if we only considered males (see Table 5) and females, respectively (see Table 6). Since the sample size of

Table 5: Treatment Comparison of A's Allocation Choice Males

Treatments Compared	MW	AD	ES
1A vs 2A	-1.704	1.734	5.271
	(0.088)	(0.126)	(0.261)
1A vs 3A	-2.209	3.027	6.821
	(0.027)	(0.024)	(0.146)
1A vs 4A	0.216	0.218	0.417
	(0.829)	(0.991)	(0.981)
2A vs 3A	-0.686	0.654	0.839
	(0.493)	(0.606)	(0.933)
2A vs 4A	1.808	1.879	5.030
	(0.071)	(0.106)	(0.284)
3A vs 4A	2.175	2.627	7.416
	(0.030)	(0.041)	(0.116)
1A vs Replication	1.615	2.133	7.462
	(0.106)	(0.076)	(0.113)

the gender subsamples is smaller than the overall sample size, some differences being significant for the overall sample are no longer significant for some of the subsamples. Especially, the Epps-Singleton test seems to perform poorly with small samples. Hence, we do not further consider the results obtained by this test statistic in this section.[7] Employing the Mann-Whitney and the Anderson-Darling tests, we do find some differences in gender.

First, for both subsamples we again do not observe significant differences between the different treatments *with* social interaction (i.e. 2 vs. 3) and those *without* (i.e. treatments 1 vs. 4).

Secondly, for males, the results from treatment 1 vs. 2 and those from treatment 2 vs. 4 are no longer significantly different, whereas the differences between both treatment 1 vs. 3 and treatment 3 vs. 4 prevail. Obviously, males do care about social interaction if it takes places after the allocation

[7]While still reporting the results from the ES for the sake of completeness.

Table 6: Treatment Comparison of A's Allocation Choice Females

Treatments Compared	MW	AD	ES
1A vs 2A	-2.347	3.340	5.994
	(0.019)	(0.015)	(0.200)
1A vs 3A	-1.914	2.786	3.987
	(0.056)	(0.031)	(0.408)
1A vs 4A	0.617	0.798	1.334
	(0.537)	(0.491)	(0.856)
2A vs 3A	-0.493	0.388	1.851
	(0.622)	(0.892)	(0.763)
2A vs 4A	-3.118	5.810	10.830
	(0.002)	(0.001)	(0.029)
3A vs 4A	-2.693	4.031	7.849
	(0.007)	(0.006)	(0.097)
1A vs Replication	1.147	1.329	8.035
	(0.251)	(0.220)	(0.090)

decision has been made, while social interaction does not matter for them if it takes places before the allocation decision. There are two possibilities why males could be less generous in treatment 2 than in treatment 3: Either, they simply do not imagine that being together with someone you have not treated nicely, before, could feel uncomfortably or they simply do not care because of indolence. Females by contrast are sensitive to social interaction regardless when it takes place (though due to the small sample size the comparison of treatment 1 vs. 3 is slightly above the 0.05 p-value for the MW-test).

Besides sex, we controlled for age [8], income, and the subject studied by participants. Using Spearman's rank correlation coefficient to test for the impact of age, we do not find a significant correlation between the share taken by subject A and his age for treatment 1,3 and 4. In treatment 2, by contrast, we find a significant positive relationship between the two variables (p-vale of 0.04). Obviously younger subjects fear the prospect of being together with someone in one room for 15 minutes whom they have not treated fairly more than older subjects, and therefore pass on a higher share.

Controlling for income we find a significant negative relation between income and the share taken by A only in treatment 1. Interestingly, in treatment 4 which has the same incentive structure as treatment 1, we do not find this

[8] Carrying out an experimental study on contributions to the Red Cross, List (2004) found that age matters.

relation. We also did not find any impact of the subject being studied by the participants on the share taken by A.

4 Regression Analysis of A's Allocation Choice

We now link the collected personal data of the subjects A to the share of money passed on by them. We regress gender, income, age, study subject and knowledge about the concept of sustainability ('Sust') on the share passed on by A, '$PassedA$'. For gender we use a dummy taking the value 0 in case of a female subject and 1 in case of a male subject. Income is divided into eight classes starting from less than 350€ after having paid the rent for housing. The next income class ranges from 350€ to 500€, 500€ to 650€, and so on in steps of 150€. We code these classes as 0,1,2,...,7 for the regression. To capture the impact of the study subject, we split the participants into four groups of study subjects: Economics (including business), science (including natural science, engineering, mathematics), humanities (including e.g. languages, arts, history) and others. The last group contains study subjects which from our point of view do not really fit the three other categories like e.g. agricultural science, psychology or political science and the few participants which did not study. Sustainability is captured by a dummy variable taking the value of 1 if the subject gave a correct definition on sustainability and 0 otherwise. Treatments are also captured by a dummy variable for each treatment 2-4 leaving treatment 1 as the benchmark setting. Hence, we have the following regression equation.[9]

$$\begin{aligned} Passed\_A &= \beta_0 + \beta_1 \cdot Gender + \beta_2 \cdot Income + \beta_3 \cdot Age \\ &\quad + \beta_4 \cdot Subject + \beta_5 \cdot Sust + \beta_6 \cdot Treatment + \varepsilon \end{aligned}$$

Using a model selection procedure suggested by Herwartz (2007), we obtain the regression model whose results are displayed in Table 7, p-values are in parenthesis beneath point estimates. A Wald coefficient test shows that treatment dummy 2 and treatment dummy 3 are not significantly different, hence confirming the evidence from the former section. We therefore replace both variables by a dummy for social interaction (Soc_Int).

Including personal data in the analysis reveals that gender is significant at the 1%-level. Females pass on around 12 percentage points more of the

[9]Note, that treatment and study subject regressors have more than one column.

Table 7: Regression A, Dependent Variable: Share Passed On by A

Variable	OLS	Tobit
Constant	0.305	0.285
	(0.000)	(0.000)
Gender	-0.117	-0.141
	(0.002)	(0.001)
Income	0.057	0.064
	(0.006)	(0.006)
Soc_Int	0.169	0.183
	(0.000)	(0.000)

pie than males. Note further, that female candidates were underrepresented in the treatments 2 and 3: We had about 25% females in the pooled sample of treatments 2 and 3, compared to 31% and 48% females in treatments 1 and 4, respectively. These two facts explain the even stronger effect of social interaction in the regression analysis (significant at the 1%-level) increasing the share that A passed on by almost 17 percentage points while comparing simple treatment means yields only difference of 14 percentage points. The intuition behind this is the non-weighted sample: Since females tend to be more generous, the social interaction effect is underestimated by comparing pure sample means if females are underrepresented in these samples.

Income plays also a role although the impact is considerably smaller than both the gender and the social interaction effect. If income is increased by 150€ (on average), subjects of type A pass on 6 percentage points more of the pie for themselves. The income effect is significant at the 5%-level .

As typical for regressions using dictator game data, the adjusted R-squared of our regression is rather low with 0.17. Thus, personal data do matter but their predictive power is not overwhelming. Moreover, it is well known that in regressions with dictator game data the error terms are mostly non-normally distributed. This is also the case in our analysis since the Jarque-Bera statistic rejects the normality hypothesis with a p-value of 0.005. That means that the regression is still unbiased but that the t-statistic and hence the p-value in general cannot be interpreted in the same way as with normally distributed error terms. However, this effect vanishes when sample size increases. According to Ratcliffe (1968) a number of 80 observations is sufficient to ensure proper interpretation of the t-statistic which is given in our study with 166 observations. A White test rejects heteroscedasticity (p-value 0.394), hence in this respect OLS is a suitable tool.

Observe, however, that in dictator games the subjects' decision process could be divided into two steps. In a first step, the candidates have to make a discrete choice: Do I pass money on or not? Conditionally on having decided to pass on some money, in a second step, the candidates decide how *much* to pass on. It could therefore be the case that the factors determining the first decision are different from those determining the second one. OLS fails to differentiate the impacts on the two subdecisions and could therefore generate a biased estimate. We therefore also employ the Tobit regression method, which accounts for this problem. The results, however, summarized in Table 7 remain almost unchanged.[10]

5 Analysis of B's Allocation Choice

In this section we will analyze the allocation choices of B, and we again compare the impact of the different treatments. The results from the four treatments are displayed in Figure 4. Treatment 1-3 show peaks at shares of 0.5 of the remaining pie (having been passed on from A to B), while for treatment 4 the most frequent share passed on by subject B lies between 0 and 0.1. One subject from treatment 2 passed on the complete pie. Her explanation (saying she had confidence in C) did not make sense. Another subject from treatment 4 passed 8€ (=62%) of 13€ (which had been passed on by A) to C. She explained her decision by writing that "5" and "8" were her "lucky numbers". Interestingly, she chose to take the smaller amount herself, not giving any explanation for that.

Table 8 reports the results for descriptive statistics of the different treatments. Treatments '4self' and '4dice' refer to the subsamples of treatment 4 where candidates A or the random process via a 20-sided dice, respectively, determine the allocation. Again, we find that in the social interaction treatments subjects pass on more than in the other treatments. Interestingly, subjects in treatment 4 are substantially less generous than in treatment 4 although the average share passed by A was almost the same in both treatments and hence could not be the reason for this difference. Analyzing the allocation behav-

[10]Note, however, that in a Tobit regression the regression coefficients can no longer be interpreted as elasticities.

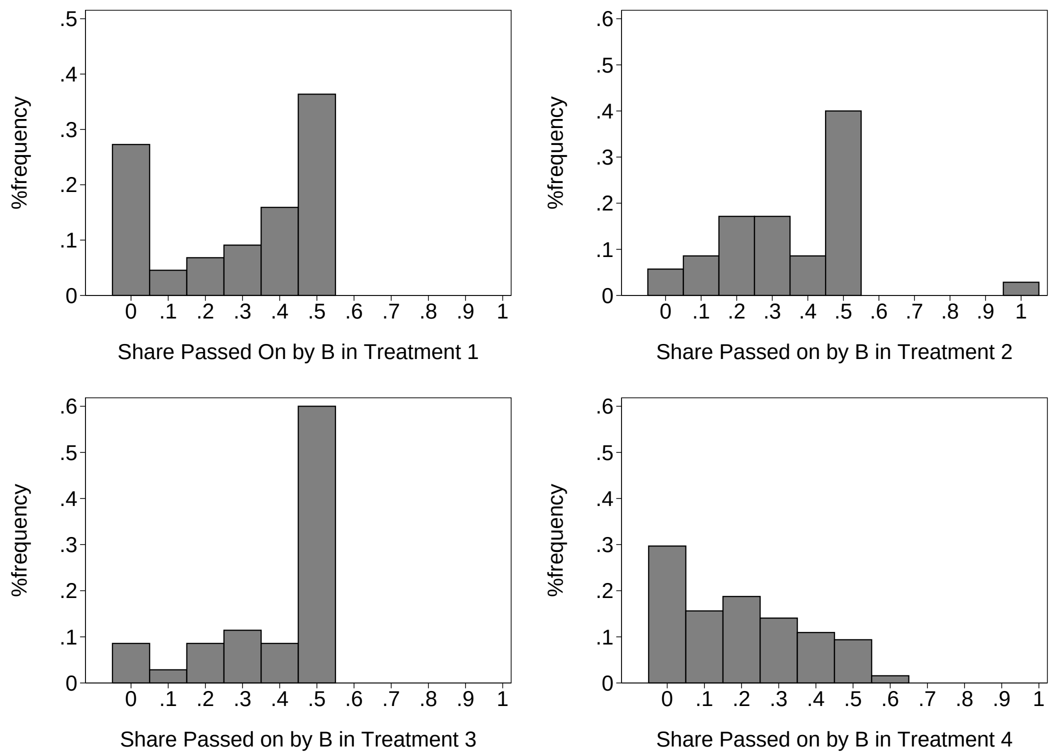

Figure 4: Comparison of Shares Passed on by B: Treatment 1-4

ior of candidates of type B is more complicated than analyzing A's allocation choice. Since the amount of money candidate A passed on to candidate B is not constant among subjects we have an additional factor which potentially influences B's choice. Previous experimental evidence from e.g. ultimatum games (see e.g. Güth et al., 1982), have shown that subjects tend to reciprocate on A's choice if they have the chance to do so. Normally, reciprocity takes place between the same subjects and follows a 'tit-for-tat' strategy. That means, once a person X has treated person Y nicely, Y reciprocates by treating person X nicely, too. In our case, however, candidate B cannot apply this strategy to candidate A since it has only influence on C's share. But even if C has not been involved in A's decision, B could feel the desire to reciprocate somehow and since C is the only one left, B could reciprocate with C. Thus, if A was e.g. generous towards B, B could tend to be generous towards C. We will coin this undirected reciprocity 'social history' effect. Now, this social history effect could (and it does as we will see below) have a strong impact on B's choice and, as a consequence, could render the conclusions from the sample comparison invalid. Spearman's rank correlation test strongly confirms

Table 8: Descriptive Results Allocation Choice B

Treatment	Mean Share passed by B	Obs
1	0.29	44
2	0.37	35
3	0.39	35
4	0.20	64
4self	0.19	28
4dice	0.20	36
Repl	0.26	40
FHSS	0.23	69
HMSS	0.27	24

the existence of the social history effect with a p-value of 0.000. Nonetheless, we present those results in Table 9, since they show some interesting features.

Similarly to the choice of A, we find that the outcomes from the treatments with social interaction, i.e. 2 and 3, differ from those without, i.e. treatments 1 and 4. This does not necessarily need to be triggered by the social interaction itself but could also be evoked by the social history effect. Comparing the choice from B as allocator who faces one recipient C with the choices of our replication of the traditional dictator game shows no significant differences in behavior. Using the results from actual traditional dictator games, we find a significant difference for the ES statistic, but ignore this result.[11] Different from A's allocation behavior, we find a significant difference concerning the outcomes of treatment 1 and 4 (p-value of 0.02). This is rather surprising since we would expect that the decision set-up for candidates B is not much different from that for candidates A. One might guess that candidates B could somehow infer from the amount they observe, whether the decision was caused by candidate A or by the random process and then behave differently if the random process was at play. This could make sense since subjects have a slight tendency to choose prominent numbers such as 25, 20, 15 or 10 Euros for the shares to be passed. Table 10 shows the results from comparing the share which the dice passed to B and which subjects A passed to B. There are no significant differences but the MW- and the AD-statistic are at least not far from being significant.

[11]Remember, that comparing pooled FHSS to HMMS yielded also a significant difference with the ES statistic

Table 9: Treatment Comparison of B's Allocation Choice

Treatments compared	MW	AD	ES
1B vs 2B	-1.291	1.545	11.755
	(0.197)	(0.167)	(0.019)
1B vs 3B	-2.159	3.596	5.613
	(0.031)	(0.013)	(0.230)
1B vs 4B	-2.386	3.670	17.548
	(0.017)	(0.012)	(0.002)
2B vs 3B	1.032	1.129	4.914
	(0.302)	(0.296)	(0.296)
2B vs 4B	-4.144	7.872	18.958
	(0.000)	(0.000)	(0.001)
3B vs 4B	-4.792	11.330	33.108
	(0.000)	(0.000)	(0.000)
4Bself vs 4Bdice	0.089	0.492	2.537
	(0.929)	(0.766)	(0.638)
1B vs Replication	0.700	1.100	4.036
	(0.484)	(0.308)	(0.401)
1B vs FHSS	-1.596	1.916	13.997
	(0.111)	(0.100)	(0.007)
1B vs HMSS	1.034	1.669	9.943
	(0.301)	(0.139)	(0.041)

Table 10: Comparison of the Subsamples: Subject vs. the Dice in Treatment 4a

MW	AD	ES
-1.668	1.742	4.849
(0.095)	(0.127)	(0.303)

Nonetheless, comparing the outcomes of the subsamples for B's behavior in treatment 4, where either a subject A ('4self') or the dice ('4self') was responsible for the allocation, we do not find any significant difference. Hence, subjects B in treatment 4 were not able to tell apart if a subject A or the random process was responsible for the allocation or it simply did not matter for subject A.

Thus, there must be something different at play. The random element somehow seems to trigger different behavior. According to Malle and Knobe (1997) people differentiate between behavior which they perceive as intentional or as unintentional, respectively. Applied to our situation, if subject B knows or presumes that subject A was responsible for the division of the initial pie, it would make sense to reciprocate on that action, whereas it would not make sense if B knows or presumes that the pie has been divided by a random process. Blount (1995) confirms this reasoning: In a series of ultimatum games, the average minimum offer acceptable significantly declined if there was a random process replacing the proposer and hence confirming the important role of attributing intentions. But this explanation, however, is not satisfactory to explain the difference between the two treatments 1 and 4 because it should be the case that the reciprocity effects cancel out on average: Some of the candidates A were generous, some were greedy, but the average share passed by A was the same in treatment 1 and 4. Hence, something else must be at play. Charness and Rabin (2002) found in a series of one-shot sequential experiments (where reciprocity is possible) that *friendly* behavior had almost no influence on the response of the second mover and thus almost no explanatory power for respondents behavior, whereas different levels of *unfriendly* did. Offerman (2002) confirmed the evidence so far. In his study, it mattered to subjects if an action was chosen intentionally by another subject or by nature. If the action was intentionally, subjects reciprocated strongly if it was negative while being rather neutral in face of a positive action. Conducting another psychological experiment Knobe (2006) confirmed this evidence, showing that some person X was much more inclined to perceive an action by some person Y as intentional, rather than as a random by-product, when this action had a bad rather than a good effect for X. Inferring this evidence to our case, we can interpret our results in the following way: If in treatment 4, candidate B receives a high share, he tends to ignore this as possible generosity by A because she cannot be sure, whether it was really caused by A or rather the result of a random process, in which case reciprocity would not make sense. In case of receiving a small share, candidate B is more likely to attribute this result to the greed of A and thus indirectly reciprocate negatively to C.

This reasoning is in line with our data from the questionnaires where subjects explained their allocation behavior: In treatment 1, 9.1% explained their behavior by some kind of positive reciprocity, while in treatment 4, no one used positive reciprocity as explanation. However, 4.7% of the subjects explained their behavior with negative reciprocity, being in line with the argument that in doubt, people assume intentions (which does not makes sense because they could not know if a candidate A or the random process was responsible). Note, that 15.9% of the subjects in treatment 1 used negative reciprocity as explanation. Hence, our results are in line with the claim that negative reciprocity seems to be more important than positive reciprocity.

The allocation choice of the female and male Bs can be seen in Table 11.

Table 11: Descriptive Results of B's Choice With Regard to Gender

Treatment	Mean share passed by Males	Obs Males	Mean share passed by Females	Obs Females
1B	0.25	28	0.36	16
2B	0.36	21	0.38	14
3B	0.37	23	0.43	12
4B	0.18	33	0.21	31
4BDice	0.17	20	0.23	16
4BSelf	0.20	13	0.19	15

Testing for gender effects, we did not find significant differences between females and males in any treatment (see table 12).[12]

Table 12: Testing for the Impact of Gender on B's Choice

Treatments Compared	MW	AD	ES
1B	-1.246	1.720	4.722
	(0.213)	(0.128)	(0.317)
2B	0.190	0.526	3.540
	(0.849)	(0.738)	(0.472)
3B	0	1.118	8.230
	(1)	(0.303)	(0.084)
4B	-0.422	0.728	2.248
	(0.673)	(0.539)	(0.690)
4BSelf	-0.305	0.556	3.251
	(0.760)	(0.712)	(0.517)
4BDice	-0.934	0.668	2.268
	(0.350)	(0.600)	(0.687)

Testing for other effects such as income and subject of study did also not yield any significant results.

[12]The p-values are sufficiently high such that we do not expect to obtain significant results when even doubling samples size. The gender effect in treatment 2 when A allocated vanishes, possibly being dominated by the social history effect.

Comparing the allocation behavior of A's and B's reveals that the Bs pass on a lower share of the remaining pie than A pass on from the whole pie. Depending on the treatment, they pass on an average of between 5 to 13 percentage points less. The difference is significant for all treatments [13].

Table 13: Comparison of A's and B's Allocation Choice Within Treatments

Treatments	MW	AD	ES
1	1.192	3.029	20.140
	(0.233)	(0.025)	(0.000)
2	3.220	6.650	27.190
	(0.002)	(0.000)	(0.000)
3	3.245	5.774	33.011
	(0.001)	(0.001)	(0.000)
4	-2.828	4.230	9.652
	(0.005)	(0.006)	(0.047)

6 Regression Analysis of B's Choice

As already mentioned, the choice of B may be influenced by the varying amounts received by B. We are not talking about a possible scale effect here, since FHSS have already shown that the size of the pie is of minor importance for the dictator's choice what percentage of the pie to keep for herself. However, given the knowledge about the initial size at A's disposal, the amount received by B should matter through the social history effect. Hence, again, a regression analysis may explain which factors determine B's behavior. To explain the allocation behavior of B, that means the share passed on by B is dependent variable, we use a similar regression equation as used for A, extended by some further regressors. In order to capture the social history effect we add the share that A passed on to B ('$Passed\_A$'). Additionally, we include two dummies, indicating whether B's expectation was disappointed ('$More\_expexted$', meaning that B expected more than she actually received), or exceeded ('$Less\_expexted$', hence, being positively surprised, meaning that he expected less than he actually received), respectively. Again, we merge the treatment dummies for the social interaction in this regression since a Wald coefficient test cannot rejects the equality of the regressors at the 5%-significance

[13]except for the MW-statistic in treatment 1

level. Thus, we have the following regression equation:

$$\begin{aligned}Passed\_B &= \beta_0 + \beta_1 \cdot PassedA + \beta_2 \cdot LessexpB + \beta_3 \cdot MoreexpB \\ &+\beta_4 \cdot GenderB + \beta_5 \cdot IncomeB + \beta_6 \cdot AgeB \\ &+\beta_7 \cdot SubjectB + \beta_8 \cdot SustB + \beta_9 \cdot Treatment + \varepsilon\end{aligned}$$

We apply again the model selection procedure by Herwartz (2007) and obtain the final model which can be seen in Table 14. We observe that the social

Table 14: Regression B, Dependent Variable: Share Passed On by B

Variable	OLS	Tobit
Constant	0.207	0.145
	(0.000)	(0.002)
Passed_A	0.386	0.502
	(0.000)	(0.000)
Less_expected	-0.163	-0.193
	(0.000)	(0.000)
Exp4	-0.107	-0.117
	(0.000)	(0.001)

history effect represented by the variable *PassedA*, indicating how much A passes on to B, is a highly significant regressor and its importance is very strong compared to all the other effects we have found so far. If A passes on 1 percentage point more, B reciprocates with regard to C, by passing on almost 0.4 percentage points more, too. This finding is confirmed by 18.6% of the candidates B explaining their behavior by reciprocity (Note that more than one explanation was possible). Interestingly, 10.3% of the candidates A obviously anticipated this social history effect. They explained their choice by expressing the hope to act as a role model for B.

Gender could matter since females are again slightly more generous than males and since the result is significant at the 10 % level (being even close to the 5% significance level), but the gender regressor was discarded in the model selection process. Income is not significant any longer. Age, study subject and knowledge about the concept of sustainability are not important in this regression, either. Neither is it important, if the candidate expected more money.

Interestingly, the expectations of candidates B about the generosity of A play an important role. We observe that subjects who had been positively surprised by A's choice ('*less_expected*') reciprocate negatively, passing on 16 percentage points less. At first glance, this seems to be counterintuitive keeping in mind that we diagnosed a strong propensity to reciprocate positively. There are two possible explanations for this paradox. The first is a self-selection effect: In an environment, where subjects have not yet made any experiences about how dictators divide a pie, the adequate heuristic would be, to infer from yourself. If you are a selfish type, you would expect others to be selfish, too, and vice versa. Thus, the expectation dummy could also be interpreted a person type dummy. Under this hypothesis, there is a positive correlation between the group of greedy types and those being surprised positively. Therefore of the dummy variable *Lessexpected* takes the value 1, we are likely to pick a selfish type which would pass on little, anyway. This finding of different preference types with respect to altruism is consistent with results from Andreoni and Miller (2002). [14] The second explanation is that candidate B could have an a priori idea about how much C should obtain, say 33,3%. If A passes on more than expected, say 70% instead of 66,6%, B may feel inclined to keep the extra of 3,3%.

A further interesting result is, that social interaction does not matter any longer since the dummies for treatment 2 and 3 are no longer significant. Hence, the results from the treatment comparison must be interpreted in the following way: There is a treatment effect. But this effect does not stem from the social interaction between B and C, but from the social interaction before between A and B. This effect is transferred via the social history effect. Finally, treatment 4 turns out to be significantly different from all other treatments (almost at the 1% significance level) confirming the results from the simple treatment comparison.

One problem in our analysis could be possible multicollinearity between the variables $ShareA$ and *Lessexpected* since both regressors are related. If $ShareA$ is low, B receives a large amount. As a consequence, it is likely that B expected less. However, the VIF of *Lessexp* is 1.15 and hence very far away from the values > 10 where multicollinearity problems begin.

Adjusted R-squared is 0.29 which is higher than in case of analyzing A's

[14] They distinguished between three types of preferences among subjects: The selfish type whose utility depends only on his own pay off, the Rawlsian type with a Leontief utility function with his and the recipients payoff as arguments and finally the Utilitarian type where the payoffs for the allocator and the recipient are perfect substitutes.

allocation behavior. We attribute this to the strong influence of reciprocity. Now, error terms are normally distributed (Jarque-Bera statistic cannot reject the normality, the p-value is 0.17). A White test rejects heteroscedasticity at the 0.98 level, hence OLS is suitable. Again, a Tobit regression might be more appropriate. The results are also reported in Table 14. Significance levels improve a little for the expectation term.

7 Conclusion

Our experimental evidence confirms the well-established fact that people are willing to sacrifice resources for the sake of others without a reward in the economic sense. Furthermore, people are aware of the intergenerational context of out experimental setup. They pass on more resources than in the one-generation-model, the traditional dictator game, indicating that they take the upcoming generation into consideration. They also deliberately sacrifice resources to trigger generosity in the next generation via the channel of reciprocity. If people see that a previous generation behaved generously, they, at least partly, take this generation as a role model and reciprocate by behaving generously, too. Thus, there is empirical evidence for the social history effect. According to our experimental evidence, there is a tendency of people to behave sustainably. Social interaction enhances (intergenerational) generosity. The reasons for this generosity are not entirely obvious but there are some clues. People seem to feel uncomfortable if they behave greedy without the veil of complete anonymity. Social interaction can lead to increased affection triggering altruism. For the first generation, gender does matter in this setup: Females seem to feel more uncomfortable with behaving unfairly if the rest gets to know about it and they have to be around with the rest for a while. Thus, for future research it is necessary to ensure that females and males are represented with the same shares in the samples. More income increases the willingness to sacrifice resources at the initial stage. For the second generation, the impact of gender is considerably softened, the impact of income vanishes completely. The dominant factor determining the second generation's choice is the choice of the first generation. For the second generation, expectations are important. But not in the sense of reciprocal behavior but in order to identify if you are more or less altruistic. The introduction of a random element showed that people's willingness to reciprocate positively is only triggered if they know that this was a deliberate choice. Once they have the perception that it was unintended, they cease to reciprocate positively. If you choose a positive action to induce imitation, ensuring to convey that this decision was intentional is imperative. If you fail to do so, people will not reciprocate. Inferring from this, one has to emphasize the importance of indicating that a choice was consciously made if you try to induce certain behavior of other people by acting as a role model.

Our model also contributes to the class CPR-experiments, where subjects proved to be altruistic. Still, in these setups the desire to be altruistic could be mixed with strategic considerations, while in our setup the wish to be altruistic is obvious.

Generalizing the results from our setup is maybe a little speculative, nonetheless, we dare doing so. Our setup could be perceived as an ideal type in the sense of Max Weber to capture problems with intertemporal externalities like environmental problems (e.g. greenhouse effect). The predicament with intergenerational externalities is the dynamic aspect of the externality: The benefits of the detrimental behavior accrue to the present generation while the future generations have to pay the costs. Since there is at the moment no one who has to bear the burden, there is no one with an interest in the classical economics sense who could vote for the introduction of instruments which would control the externality. Monetary concerns cannot not be used to induce someone's willingness to sacrifice resources for the sake of future generations, these instruments can only be used once people have agreed on helping the future generations. Hence, achieving the agreement is the crucial point. Our results allow for a rather optimistic conclusion: People, confronted with very simple forms of intergenerational problems, seem to be aware of the intergenerational dilemma and seem to be willing to sacrifice (at least to a certain extent) in order to guarantee intergenerational fairness.

8 Appendix

Instructions for Experiment I

There are three candidates called A, B and C who are in a group. Candidates are randomly assigned to a group. The candidates do not get to know each other. The task is to divide 30 Euros.

Step 1: Candidate A chooses an amount between 0 and 30 Euros which she keeps for herself. The rest R will be divided by candidate B between B and C.

Step 2: Candidate B gets to know the amount R, which A passed to B and C. He chooses an amount between 0 and R which she keeps for herself. Candidate C receives the rest of the money.

Step 3: The allocated amounts will be paid to A, B and C.

The experiment is finished.

Instructions for Experiment II

There are three candidates called A, B and C who are in a group. Candidates are randomly assigned to a group. The task is to divide 30 Euros. A and B and respectively B and C meet, while A and C do not meet.

Step 1:	Candidate A chooses an amount between 0 and 30 Euros which she keeps for herself. The rest R will be divided by candidate B between B and C.
Step 2:	Candidate A and B play a kind of Scrabble (game with letters, instructions see below), where they can earn additional money. The better A and B cooperate the higher the amount which they earn. Each of them receives half of the amount earned. Time limit for the letter game is 15 minutes.
Step 3:	Candidate B chooses an amount between 0 and R which she keeps for herself. C receives the rest of the money.
Step 4:	Candidate B gets to now the amount R, which A passed to B and C
Step 5:	Candidate B and C play the letter game. See Step 2.
Step 6:	The allocated amounts including the profit from playing the letter game will be paid to A, B and C.

The experiment is finished.

Instructions for the letter game:
There are 101 letters, which should be used to form words. The numbers on the letters indicate the value of each letter. Words may be formed crosswise. For each used letter you obtain points according to the value of the letter. Letters which appear in two words count twice.
One point is worth 2 cents.
The amount earned by player A and B (likewise B and C) is split equally between them.
Note, that you can earn a maximum amount of 6 Euros (that means 3 Euros for each of the players).

Instructions for Experiment III

There are three candidates called A, B and C who are in a group. Candidates are randomly assigned to a group. The task is to divide 30 Euros. A and B and respectively B and C meet, while A and C do not meet.

Step 1:	Candidate A and B play a kind of Scrabble (game with letters, instructions see below), where they can earn additional money. The better A and B cooperate the higher the amount which they earn. Each of them receives half of the amount earned. Time limit for the letter game is 15 minutes. B leaves the room.
Step 2:	Candidate A chooses an amount between 0 and 30 Euros which she keeps for herself. The rest R will be divided by candidate B between B and C.
Step 3:	Candidate B gets to know the amount R, which A passed to B and C
Step 4:	Candidate B and C play the letter game. See Step 1.
Step 5:	Candidate B chooses an amount between 0 and R which she keeps for herself. Candidate C receives the rest of the money.
Step 6:	The allocated amounts including the profit from playing the letter game will be paid to A, B and C.

The experiment is finished.

Instructions for the letter game:
There are 101 letters, which should be used to form words. The numbers on the letters indicate the value of each letter. Words may be formed crosswise. For each used letter you obtain points according to the value of the letter. Letters which appear in two words count twice.
One point is worth 2 cents.
The amount earned by player A and B (likewise B and C) is split equally between them.
Note, that you can earn a maximum amount of 6 Euros (that means 3 Euros for each of the players).

Instructions for Experiment IV

There are three candidates called A, B and C who are in a group. Candidates are randomly assigned to a group. The candidates do not get to know each other. The task is to divide 30 Euros.

Step 1: It is decided by tossing a coin whether i) a 20-sided dice divides for candidate A or ii) candidate A divides herself.

i) In case the 20-sided dice divides for candidate A:

Step 2: The 20-sided dice determines an integer amount (in Euros, no Cents) between 10 and 30 Euros which A receives. The rest R will be divided by candidate B between B and C.

Step 3: Candidate B gets to know the amount R, which A passed to B and C. She chooses an amount between 0 and R which she keeps for herself. Candidate C receives the rest of the money.

Step 4: The allocated amounts will be paid to A, B and C.

The experiment is finished.
i) In case candidate A divides herself:

Step 2: Candidate A chooses an amount between 0 and 30 Euros which he keeps for herself. The rest R will be divided by candidate B between B and C.

Step 3: Candidate B gets to know the amount R, which A passed to B and C. She chooses an amount between 0 and R which she keeps for herself. Candidate C receives the rest of the money.

Step 4: The allocated amounts will be paid to A, B and C.

The experiment is finished.

Instructions for the Experiment V

There are two candidates called A and B who are in a group. Candidates are randomly assigned to a group. The candidates do not get to know each other. The task is to divide 30 Euros.

Step 1: Candidate A chooses an amount between 0 and 30 Euros which she keeps for herself. The rest R will be given to candidate B.

Step 2: The allocated amounts will be paid to A, B and C.

The experiment is finished.

FORM 1

Date: ______________

Group: [ ]   Type: [ ]

The amount of 30 EUR is divided in

[EUR] for me

[EUR] for B and C. (B will then divide this amount between herself and C).

Statistical data

1.) Age: [years]

2.) Gender: [ ] female [ ] male

3.) Study subject: ______________________________

4.) After having paid my rent, I have an average monthly income of

- [ ] below 350 Euro
- [ ] 350 - 500 Euro
- [ ] 500 - 650 Euro
- [ ] 650 - 800 Euro
- [ ] 800 - 950 Euro
- [ ] 950 - 1100 Euro
- [ ] above 1100 Euro

FORM 2

Questionnaire Player B

Fill in the your subject number (e.g. A1) ☐

Fill in the date, please ☐

1.) Explain, why you divided the amount in the way you did.

2.) Did the amount of money you received from Player A correspond to your expectations? Cross accordingly:

a.) ☐ Yes, roughly.

b.) ☐ I expected more, roughly ☐ Euro.

c.) ☐ I expected less, roughly ☐ Euro.

3.) Have you ever heard the expression sustainability?

Yes ☐ No ☐

If yes, what do you mean by it?

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