On the Validity of Cost-Saving Methods in Dictator-Game Experiments: A Systematic Test

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Abstract

Motivated by methodological concerns, theoretical considerations, and evidence from previous studies, this paper makes a contribution to conducting dictator-game experiments under resource constraints. Using a holistic and strictly controlled approach, we systematically assess the validity of common cost-saving dictator-game variants. We include five common approaches and compare them to a standard dictator game: involving fewer receivers than dictators; paying only some subjects or decisions; role uncertainty at the time of the transfer decision; a combination of random decision payment and role uncertainty. To test the validity of subjects' dictator-game decisions, we relate them to complementary individual difference measures of generosity: social value orientation, personal values, and a donation to charity. In line with previous evidence, our data show that dictator behavior is quite sensitive to the applied methods. The standard version of the dictator game has the highest validity. Involving fewer receivers than dictators and not paying for all decisions yields comparably valid results. These methods may, therefore, represent feasible alternatives for the conduct of dictator games under contraints. By contrast, in the dictator-game variants where only some subjects are paid or where subjects face uncertainty about their final player role, the expected associations with other measures of generosity are distorted. Under role uncertainty, generosity is also biased upwards. We conclude that these methods are inappropriate when the researchers are interested in valid individual measures of generosity.

Keywords: Dictator Game, Costs, Incentives, Unbalanced Matching, Random Payment, Role Uncertainty, Social Value Orientation, Personal Values, Donation, Methodology, Experiment

JEL-Classification: C72, C91, D03
PsycINFO-Classification: 2260, 2360, 3020

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1 Introduction

The dictator game is a powerful and extensively used paradigm in the social sciences to study social preferences. It is very popular because of its simplicity.[1] The standard version of the dictator game entails dyads of randomly "one-to-one" matched dictators and receivers. Player roles are previously allocated by chance. The dictator takes one decision and determines her own payoff – and the payoff of the matched receiver, who is inactive – by splitting a given prize. Despite the simple structure of the dictator game, experimenters often face constraints in their research projects which complicate the implementation of the standard variant of the dictator game: available monetary funds or laboratory resources are limited; the number of available subjects is restricted (e.g., school children, managers); the required subjects live in remote areas (e.g., foreign countries, villages) or are hardly accessible due to legal regulations (e.g., prison inmates, kindergarten children); researchers need to make within-subject comparisons from multiple decisions (e.g., by contrasting a subject's behavior across different games or in different player roles). Besides these constraints, the dictator game often solely serves as a "companion game" which adds an individual measure of generosity to the main experimental data.

To handle the above constraints and obtain the aspired data from the experimental subjects, researchers often modify the standard protocol of the dictator game. There are different approaches: First, as the receiver is an inactive player, the number of receivers is reduced. An unbalanced matching is applied such that only some dictators are actually matched with receivers (e.g., Houser and Schunk, 2009)[2]. Second, only some players are paid after the dictators have taken a decision (e.g., Dufwenberg and Muren, 2006; Holm and Engseld, 2005; Kahneman et al., 1986). Third, the dictator game is repeated or the subjects make several similarly structured decisions, including a dictator-game transfer, either in separate games or by applying the strategy method (Selten, 1967). For the subjects' payoff determination, one decision is randomly chosen and paid out (e.g., Ashraf et al., 2006; Cappelen et al., 2007; Eckel and Grossman, 2000; Rankin, 2006). Fourth, introducing role uncertainty, all available subjects first take a decision in the role of the dictator. Afterwards, actual player roles are randomly assigned, the determined dictators are randomly matched with the receivers, and the players are paid accordingly (e.g., Charness and Rabin, 2002; Engelmann and Strobel, 2004; see also Iriberri and Rey-Biel, 2011). Many studies also combine random payment of decisions and role uncertainty to obtain a maximum amount of data and to enable within-subject comparisons. In this case, the subjects take several decisions as dictators. Afterwards, the payoff-determining decision is randomly chosen, actual player roles are randomly assigned, and players are randomly matched and paid out (e.g., Andreoni and Vesterlund, 2001; Ashraf et al., 2006; Blanco et al., 2011; Castillo and Cross, 2008; Charness and Rabin, 2002; Engelmann and Strobel, 2004). Fifth, some economic studies (but usually psychological ones) do not offer the participants monetary incentives and let the subjects take hypothetical decisions (Ben-Ner et al., 2008; Bühren and Kundt, 2015; Dalbert and Umlauft, 2009; Lönnqvist et al., 2011).

The above approaches all implement different variants of the dictator game. A randomization procedure is applied – either with regard to the final matching of the players, how many players

[1] For an excellent metastudy on the empirical evidence from dictator-game experiments, see Engel (2011).

[2] These authors do it for practical reasons, but do not reveal this to their subjects. This can be considered a borderline case of deception. This method is also often used in social psychology experiments. There, the receivers do often not exist at all (e.g., Batson et al., 1997, admitted on p. 1339). Subjects are typically not aware of this fact.

are actually paid, the determination of the payoff-relevant decision, or the assignment of actual player roles. The different randomization procedures yield different probabilities for how strongly a dictator's decision eventually determines her own payoff and that of a matched receiver. At the same time, fewer resources (receivers, sessions, money, etc.) are required and the experiment can be implemented without inadequate deception (e.g., Ortmann and Hertwig, 2002).

In this paper, we systematically investigate the impact of probabilistic incentives in dictator games – induced by the different forms of randomization found in the experimental literature. Using a holistic and strictly controlled approach, we study the important question whether the *source* of randomization, i.e., unbalanced matching of dictators and receivers, random payment of players and decisions, and role uncertainty, alters generosity and underlying motivations. Furthermore, we examine how the *degree* of randomization, i.e., single (e.g., in case of unbalanced matching) and multiple randomizations (e.g., in a situation with random payment of decisions *and* role uncertainty), interfere with dictator behavior.

Our study is inspired by methodological and practical considerations, in conjunction with mixed results from previous studies. For example, in his metastudy on individual dictator-game behavior, Engel (2011) finds that the less a dictator is sure that her intended generosity affects the receiver, the less she transfers to the receiver. Moreover, when only some decisions of a dictator, or some dictators, are paid at random, no significant effect on generosity is detected (see also Bolle, 1990, and Laury, 2005). Engel (2011) generally uses metaregressions. However, when applying individual data or when making different specifications in his models, the results differ (p. 591). Referring to the random payment of decisions in their experiment (including a dictator game), Ashraf et al. (2006) acknowledge: "*While we control for design effects to the best of our ability, we cannot exclude the possibility that our design affects behavior differently than other, more standard, designs*" (p. 197). Moreover, while some studies (e.g., Engelmann and Strobel, 2004) suggest that results are robust towards role uncertainty, Iriberri and Rey-Biel (2011), who directly address the impact of role uncertainty in a modified dictator game, find it to overestimate the prevalence of altruistic preferences.[3] They conclude that "[...] *our results definitely warn against the use of role uncertainty in future experimental designs aiming to identify different motives behind non-selfish behavior*" (p. 171). In a similar vein, Charness and Rabin (2002) admit that "*Our use of role reversal and multiple games in sessions may have generated different behavior than had each participant played just one role in one game*" (p. 827). All in all, in the context of specific forms of randomization, it remains an open question how changing the incentive structure influences generosity and its foundations.

To the best of our knowledge, our study is the first to investigate systematically the different randomization procedures and their potential influence on dictator-game giving within a cohesive and controlled experimental approach. Our study adds the following contributions to the literature: i) We study and compare the impact of unbalanced matching, random payment of players and decisions, and role uncertainty using exactly the same dictator game in all the treatments. ii) As far as we are aware, the role of unbalanced matching has not been studied so far. Depending on which field expert is asked, one gets divergent answers about the appropriateness of (secretly) involving fewer receivers than dictators with regard to deception issues or the validity of the results. iii) We include a combination of randomly paying decisions and role uncertainty, which is often done when social

[3] In a related study, Burks et al. (2003) find that playing both roles in a trust game reduces both trust and reciprocity.

preferences are elicited. iv) To assess potential differences in the validity of dictator behavior across treatments, we relate the subjects' decisions to alternative individual difference measures typically associated with generosity: social value orientation, personal values, and a donation. From this perspective, our study also provides potential new insights into previous results on the existing (or non-existing) associations of generosity with other individual differences measures such as personality. v) The experimental sessions were run under tight control concerning the time schedule, the recruited subjects' characteristics, and the subjects' perception of the experimental tasks.

In line with previous evidence, our data indicate that dictator behavior is quite sensitive to the specific game situation. The standard version of the dictator game has the highest validity. Unbalanced matching and random payment of decisions do not significantly change dictator behavior. The results of dictator games applying these procedures are comparable to the reference (standard) treatment without randomization. These procedures may therefore represent feasible resource-saving alternatives for conducting dictator-game studies under constraints. At first sight, random payment of only some players does not influence the average amount transferred to the receiver. Yet, we find that the behavior does not sufficiently correlate with complementary measures of generosity. Furthermore, supporting previously expressed concerns by other experimentalists, role uncertainty biases transfers upwards and distorts the associations typically found with alternative measures of generosity.

The paper proceeds as follows: In the next section, we describe our experimental setup and the specific procedures that ensure strict control and comparability of the treatments. We also make some theoretical considerations about the impact of probabilistic incentives on generosity across treatments. In Section 3, we present our results by showing the dictators' transfers and the validity of the distinct dictator-game decisions with regard to their correlation with alternative generosity measures. We will also analyze the characteristics of the recruited subjects and their understanding and perception of the decision situation. In Section 4, we discuss our results. Section 5 concludes.

2 The Experiment

2.1 *Experimental treatments*

In all treatments, we study the same dictator game. The dictator determines the allocation of €10 among herself and a receiver by choosing an integer amount x from €0 to €10 to be given to the receiver. The basic structure of the dictator game is described with the same standard text in the instructions of all treatments. The treatment variations, i.e., the different randomization procedures, were made explicit in a separate paragraph of the instructions.[4]

We implement the different dictator-game randomization procedures in six treatments. We deliberately exclude purely hypothetical decisions. Paying (at least some) subjects in the laboratory has become standard in experimental economics. It is argued that monetary incentives ensure that participants perceive their behavior as relevant, experience real emotions, and take decisions with real economic consequences (Falk and Heckman, 2009; Smith, 1976).[5] In our treatments, we vary

[4]Instructions were provided in German. Please refer to Appendix A for the original instructions of all treatments and an English translation.

[5]For the effects of the presence or absence of monetary incentives on other-regarding behavior in a dictator game, see Bühren and Kundt (2015).

Table 1: Experimental Treatments

Treatment	Feature	Subjects	Dictators	Receivers	Sessions	P_D	P_R
N–N	Reference treatment for all comparisons.	64	32	32	2	1.00	1.00
N-N/2	There are fewer receivers than dictators.	48	32	16	2	1.00	0.50
Pay50	Only 1/2 of the dictator-receiver dyads are paid.	64	32	32	2	0.50	0.50
Dec50	Only one out of two decisions from one subject is paid.	64	32	32	2	0.50	0.50
Rol50	Uncertainty about actual player role during decision.	32	32	0	1	0.50	0.50
DeRo25	Combination of the features from *Dec50* and *Rol50*.	32	32	0	1	0.25	0.25
Total:		304	192	112	10		

Notes. P_D (P_R) denotes the probability for a dictator's decision to be payoff-relevant for the respective dictator (a randomly regarded receiver) in a specific treatment.

how 1) the dictators and the receivers are matched, 2) the dictator decisions are payoff-relevant for either player, 3) the player roles are assigned. Table 1 provides an overview for all the treatments. It depicts their characteristics, the involved players, and the probability for a dictator's decision to be payoff-relevant for the respective dictator and for a randomly regarded receiver, respectively, when a dictator takes her decision in a specific dictator game. The probability depends on the applied randomization procedure.

i) Treatment *N-N* (reference treatment): Subjects are first randomly assigned a player role (dictator or receiver) with a probability of 1/2. After role assignment, there is the same number N of dictators and receivers. Then, dictators and receivers are randomly matched, resulting in N dictator-receiver dyads. This implies that the dictators' decisions determine for certain their own and the matched receiver's payoff. Finally, the dictator determines the allocation. All dictator decisions are paid. *N-N* represents the most common, but also most demanding, form of dictator-game implementation. It requires most resources (budget, time, and lab space) and serves as the "gold standard" for our comparisons.[6] Behavior in the standard version of the dictator game has been shown to relate to behavior in the field (e.g., Barr et al., 2010; Franzen and Pointner, 2013).

ii) Treatment *N-N/2* (unbalanced matching): In this treatment, we introduce an unbalanced matching of dictators and receivers. First, subjects are randomly assigned a player role with a probability of 2/3 for taking the role of a dictator and with a probability of 1/3 for taking the role of a receiver. Consequently, there are twice as many dictators than receivers. Next, dictators

[6]For a general debate on the interpretation of generosity in give-variants of the dictator game, refer to List (2007). In our experiment, we use the same frame across treatments.

and receivers are randomly matched, resulting in $N/2$ actual dictator-receiver dyads. $N/2$ of the dictators remain actually unmatched. Dictator decisions determine their own payoffs with certainty. A receiver's payoff is determined only with a probability of 1/2 from the perspective of a dictator. Finally, the dictator determines the allocation. All dictator decisions are paid (at least to the dictators). The advantage of this treatment is, that compared to *N-N*, only 1/2 of the receivers are needed.

iii) Treatment *Pay50* (random payment): First, subjects are randomly assigned a player role (dictator or receiver) with a probability of 1/2. After role assignment, there is the same number N of dictators and receivers. Then, dictators and receivers are randomly matched, resulting in N dictator-receiver dyads. Finally, the dictator determines the allocation. For payoff determination, 1/2 of the dictator-receiver groups are randomly determined and paid out. This means that the dictators' decisions determine their own and the matched receiver's payoff with a probability of 1/2. *Pay50* costs less than *N-N*, because only 1/2 of the subjects (dictator-receiver dyads) are actually paid.

iv) Treatment *Dec50* (random decision): This treatment entails a second dictator-game decision. First, subjects are randomly assigned a player role with a probability of 1/2. After role assignment, there is the same amount N of dictators and receivers. Then, dictators and receivers are randomly matched, resulting in N dictator-receiver dyads. Dictators take the same decision as in *N-N* twice. For payoff determination, each decision is chosen with a probability of 1/2 and yields the payoffs for either player. This means that a dictator's decision determines her own payoff and the matched receiver's payoff with a probability of 1/2. There is no intermediate feedback provided on the outcome of the first decision.[7] Relative to *N-N*, twice as many observations are collected. Moreover, the data allow for within-subject comparisons using repeated decisions.

v) Treatment *Rol50* (role uncertainty): Here, we implement role uncertainty. First, all subjects determine the allocation in the role of a dictator. Then, they are randomly assigned their actual player role with a probability of 1/2. Consequently, there are as many dictators as there are receivers (N). Consecutively, dictators and receivers are randomly matched, resulting in $N/2$ dictator-receiver dyads. This implies that a subject's decision determines her own payoff and another subject's payoff with a probability of 1/2. Requiring only 1/2 of the subjects relative to *N-N*, *Rol50* generates the same number of independent dictator observations.

vi) Treatment *DeRo25* (random decision and role uncertainty): This treatment combines the treatments *Dec50* (random decision) and *Rol50* (role uncertainty). First, all subjects determine the allocation in two consecutive dictator games as in *Dec50*. To determine the players' payoffs, one of the two decisions is chosen with a probability of 1/2. Then, subjects are randomly assigned their actual player role with a probability of 1/2. Consequently, there is the same number N of dictators and receivers. Dictators and receivers are randomly matched, resulting in N dictator-receiver dyads. The procedure implies that a subject's decision in one of the two dictator games determines her own payoff and another subject's payoff with a probability of 1/4. In contrast to *N-N*, this treatment generates twice as many observations and requires only 1/2 of the subjects. It also permits within-subject comparisons.

[7] We chose exactly the same dictator-game decision twice in order to study a pure repetition effect. Choosing a different task would have been arbitrary. As shown later, a good proportion of the subjects actually takes two different decisions.

2.2 *Experimental procedures*

General procedures. The experiment was conducted at the Cologne Laboratory for Economic Research (CLER), located at the University of Cologne. The lab has 32 cubicles, allowing us to include 16 dictator-receiver dyads in the standard version of the dictator game, i.e., N=16. For each treatment, the number of sessions and involved subjects depended on the treatment (see Table 1). We collected decisions from 32 dictators in each treatment. In total, 304 subjects (192 dictators) participated.[8] Subjects were recruited via the online recruiting system ORSEE (Greiner, 2015) and participated only in one session. After subjects had arrived at the laboratory, they drew a random code and were seated in separate and opaque cubicles. Subjects were not allowed to communicate. In case of any question, they had to raise their hand so that the experimenter could come and help. The experiment started after all subjects had read the instructions on paper, the experimenter had publicly read through the instructions, and questions regarding the structure of the experiment had been answered in private. The experiments were conducted on a computer, using the experimental platform z-Tree (Fischbacher, 2007), and lasted about one hour. After the main experimental task, subjects were asked to fill in a list of questionnaires with items on their understanding of the experiment (comprehension questions), empirical and normative beliefs, social value orientation, personal values, personality, willingness to make a donation, and sociodemographic background. Concluding the session, subjects were compensated individually with a fixed amount of €4 for showing up, along with the amount that they had earned in the experiment.[9]

Specific Controls. Due to the nature of the experiment, random treatment assignment was not possible within experimental sessions. Therefore, we took specific care of the following factors in order to keep the experimental conditions as constant as possible across treatments and sessions. *i) Schedule*: The experimental sessions for the treatments were scheduled on a Tuesday or Wednesday, between 9:00 a.m. and 12:15 p.m., during the students' summer term. In the treatments with two sessions, we placed one session on a Tuesday and the other one on a Wednesday in two different weeks.[10] Further, we scheduled one session at 9 a.m. and one session at 11 a.m.[11] Taken togehter, this ensured no bridging days, and balanced week days and daytime, and a short time span for conducting the entire experiment. *ii) Experimenter*: To avoid experimenter effects (e.g., Roth et al., 1991; Zizzo, 2010), the same experimenter always conducted the experiment applying exactly the same experimental protocol. Instructions were neutrally written, avoiding terms like "dictator", "giving", or "fair", because loaded instructions may alter the dictators' behavior (e.g., Burnham et al., 2000). *iii) Sex*: We strictly balanced the sex of the participants, because women typically transfer more than men in dictator games (e.g., Eckel and Grossman, 1998) and tend to participate more often than men in experiments (e.g., Cleave et al., 2013). For this purpose, males and females were separately invited to the same session. We ensured that 50% of the dictators were female by preparing different sets of cubicle codes for each sex. *iv) Age*: Our aim was to restrict the student sample to participants who were born between 1990 and 1999, yielding a typical age span for the current student cohort, because age might have a positive effect on transfers (Engel, 2011). *v) Culture*: To avoid variance in the subjects' decisions due to the subjects' cultural background (e.g.,

[8]For details on the recruited subjects' characteristics, refer to Table 2 in subsection 3.1.

[9]In Appendix B, the different stages of the experiment are depicted. Personality data are not reported in this paper.

[10]*Dec50* took place only on a Tuesday. *Pay50* (which was added later) was played within one week.

[11]Please refer to Appendix C for the session plan of the experiment.

Henrich et al., 2005; Chuah et al., 2007), we only invited subjects who indicated German as their mother tongue, which was used as an indicator for the subjects' origin. *vi) Experience*: We restricted the subject pool to participants who had no previous experience with dictator-game experiments. Moreover, we aimed to invite only subjects who had not participated more than seven times in other experiments before.[12] This procedure was chosen to enroll subjects who had *some* experience with the situation and procedures typically applied in laboratory experiments. On the other hand, extensive personal experiences may cause the subjects to play interactive games in the lab as if they have some repetition, and the experimenter may have little to moderate this phenomenon (for a similar argument, see Levitt and List, 2007; Matthey and Regner, 2013, find that participation in previous experiments tends to increase the amount subjects allocate to themselves in dictator games). We also limited the share of business and economic students – the biggest group in our university's laboratory subject pool and potentially familiar with the dictator game and less cooperative (Frank et al., 1993). *vii) Laboratory*: Finally, we would like to emphasize that our general laboratory setup (32 opaque cubicles, €4 show-up fee) is comparable to the majority of laboratories in the German speaking countries (Germany, Austria, Switzerland). An inquiry conducted prior to the experiment (we received 23 responses to 26 inquiries) yielded that, on average, laboratories are equipped with 26 cubicles and pay an average show up fee of €4.20 to their subjects.

2.3 *The impact of probabilistic incentives*

Assuming self-regarding, purely money-maximizing preferences, dictator transfers would not differ among the six dictator-game treatments. Dictators would keep the entire endowment for themselves. Further, according to the *invariance hypothesis* (Güth et al., 1982; Bolle, 1990), dictators would make the same decision in each treatment, independently of how their decision converts into actual payoffs. However, following Camerer (2003), Smith (2010), and Cox (2010), who emphasize that dictator-game results are quite sensitive to procedural changes, and based on previous results, we consider two arguments suggesting behavioral differences across treatments.

2.3.1 The probability of being pivotal

First, in his metastudy on giving behavior in dictator games, Engel (2011) finds that the less a dictator is sure that her intended behavior becomes effective, i.e., determines the payoff of the receiver, the less she transfers to the receiver. In this case, generosity is less likely to enhance the receiver's welfare. Likewise, low transfers are less likely to hurt the receiver (see also Andreoni and Bernheim, 2009). One possible explanation for this pattern can be derived from the dictator's social image concerns: the less effective the dictator's behavior becomes, the less she needs to care about her appearance in the eyes of the receiver. An inequity aversion model (e.g., Fehr and Schmidt, 1999) would make the same prediction, because a disutility from advantageous inequality becomes less likely when the receiver's payoff is affected only with some probability smaller than one. Following these results, we expect dictator transfers to decrease, when it becomes less likely that the receiver is affected by the dictator's decision (see P_R in Table 1).

[12] At some stage, we also had to invite more experienced subjects in order to fill the experimental sessions.

2.3.2 Expected payoffs

Second, in their review articles, Camerer et al. (1999) and Engel (2011) show that people are less generous when playing with higher stakes. Higher stakes are also found to reduce transfers induced by presentation effects or image concerns. In our experiment, absolute stakes (the endowment) are held constant. Yet, the likelihood that a dictator can actually affect her own payoff decreases in most treatments relative to the reference treatment. According to the above results, if the dictator's expected payoff becomes smaller, her transfers will increase and she can expose (more) socially desirable behavior at a lower cost (see P_D in Table 1). An inequity aversion model would predict, ceteris paribus, lower transfers, when the dictator can affect her own payoff only with some probability, but for sure determines the receiver's wealth.[13]

2.3.3 Predictions

To predict behavior for the different variants of the dictator game, we consider a subject deciding in the role of a dictator at the moment of her decision.[14] In the treatment *N-N* (our reference treatment), the subject knows that her decision is for sure payoff-relevant for herself and the matched receiver. In the treatment *N-N/2*, the dictator's allocation decision becomes less effective and affects a randomly regarded receiver only with a probability of 1/2. The impact on her own payoff remains constant. Therefore, according to the first argument above, we expect the dictators to transfer less in *N-N/2* as compared to *N-N*. The Fehr-Schmidt model on inequity aversion would predict the same tendency as the disutility from advantageous inequality becomes less likely.[15]

In the treatments *Pay50*, *Dec50*, and *Rol50*, the dictator's decision affects the payoff of a randomly chosen receiver, but also her own payoff, only with a probability of 1/2 due to payment, decision, or role uncertainty. The dictator's decision becomes less effective, but her own stake also becomes relatively smaller compared to *N-N*.[16] Here, the above arguments conflict: on the one hand, transfers may increase, because presentation effects or image concerns may become more prevalent, as they are less costly. On the other hand, transfers may decrease, because the dictators need to care less about their appearance in the eyes of the receiver. When we assume that dictators prefer to save money over spending money, i.e., that the first argument dominates the second one – as long as a dictator has *some* stake (i.e., her decision influences her own payoff to some extent)[17], we expect lower transfers in *Pay50*, *Dec50*, and *Rol50*, respectively, relative to *N-N*.[18] One argument which supports this assumption is that people tend to be self-servingly biased (e.g., Loewenstein

[13]The case becomes more complicated when both the dictator's and the receiver's payoffs are probabilistic and when the probablities differ. This is not the case in our experiment.

[14]For the treatments *Rol50* and *DeRo25*, this implies that the subject will not necessarily be assigned the role of the dictator for payoff determination afterwards.

[15]Generally, the model predicts a maximum transfer of x=5. The detailed calculations are displayed in Appendix D.

[16]Here, we make the assumption that the *source* of randomization (either through payment, decision, or role uncertainty) does not matter for the dictator. We will come back to the plausibility of this assumption in the discussion section.

[17]This would not hold for a purely hypothetical decision. There, the probability of a dictator's decision to become payoff-relevant for either player drops to zero. The dictator has no stake at all. Hence, based only on the second argument, hypothetical transfers would be driven by image concerns (towards the receiver and potentially also the experimenter) and are therefore higher than in *N-N*.

[18]Engel (2011) also finds a significant negative effect of random pay if no study dummies are added to his meta-regression (p. 591). The expectation of lower transfers in *Rol50* is corroborated by the result that people tend to act less pro-socially, when they are forced to take the role of the opposite player (e.g., Burks et al., 2003). By contrast, Iriberri and Rey-Biel (2011), find that pro-social behavior in a dictator game increases with role uncertainty.

Table 2: Transfers across Treatments

Treatment	No. of Decisions	AV	SD	Med	Mode	Min	Max	0	≥5
N-N	1	1.69	1.97	1	0	0	5	0.47	0.16
N-N/2	1	2.06	2.11	1.5	0	0	7	0.41	0.16
Pay50	1	1.47	1.83	0.5	0	0	5	0.50	0.13
Dec50	1	1.63	1.84	1	0	0	5	0.41	0.16
	2	1.81	1.82	1	0	0	5	0.31	0.16
Rol50	1	2.81	2.21	4	0	0	7	0.31	0.28
DeRo25	1	2.63	2.34	3	0	0	10	0.28	0.22
	2	2.75	2.46	3	0	0	10	0.28	0.22
All		2.19	2.13	2	0	0	10	0.34	0.18

Notes. AV (SD, Med, Min, Max) denotes Average, Standard deviation, Minimum, Maximum of the transfers. 0 (≥5) denotes the relative frequency of transfers x=0 ($x \geq 5$).

et al., 1993), such that they care more for their own than for the payoff of other players.[19] For the treatment *DeRo25*, the same relation holds regarding the comparison with *N-N* and relative to *Pay50*, *Dec50*, and *Rol50*, respectively, because in *DeRo25* a dictator affects her own payoff and the payoff of a randomly chosen receiver only with a probability of 1/4. The Fehr-Schmidt model would also partly predict lower transfers in *Pay50*, *Dec50*, and *Rol50*, as well as in *DeRo25*, respectively, relative to *N-N*. For β - parameters 0 and 0.25, the model predicts treatment-invariant transfers of x=0. For β=0.6, dictator transfers are equal to x=5 in the reference treatment *N-N*, and x=0 in the other treatments.[20]

3 Results

We first analyze the transfers across the different treatments and study the specific impact of unbalanced matching, random payment, decision uncertainty, and role uncertainty relative to the reference treatment.

3.1 *Transfers*

We show a characterization of the dictator transfers in each treatment in Table 2.[21] Table 3 depicts the results from different statistical tests for each treatment's comparison with the reference treatment in Table 4.

Comparing *N-N* (1.69) with *N-N/2* (2.06), *Pay50* (1.47), and *Dec50* (1.63, 1.81),[22] respectively, we find no evidence that dictator transfers significantly differ. Effect sizes are rather low. Conversely,

[19] Fehr and Schmidt (1999) make a similar assumption in their model on inequity aversion: A player's disutility from disadvantageous inequality is larger than her disutility if the player is better off than another player.

[20] β=0, 0.25, and 0.6 are parameter values which are typically found in populations (Fehr and Schmidt, 1999), but see also Blanco et al. (2011) for lower values. Lower values would predict no treatment differences.

[21] Figure 1 in Appendix E shows the distribution of transfers for each treatment.

[22] 34.37% of the dictators made a different transfer in the second game. First and second transfers are very similar in *D50* (p=0.271, Fisher-Pitman permutation test for paired replicates, henceforth: *FPP*).

Table 3: Statistical Tests

Statistical Test	FPT	t-test	EST	Cohen's d	95% CI
Unbalanced matching					
N-N vs. *N-N/2*	0.498	0.466	0.662	0.184	-1.396, 0.646
Random payment					
N-N vs. *Pay50*	0.695	0.648	0.890	0.115	-.732, 1.170
Random decision					
N-N vs. *Dec50*: Decision 1	0.952	0.896	0.701	0.033	-.892, 1.017
N-N vs. *Dec50*: Decision 2	0.844	0.793	0.499	0.066	-1.074, 0.824
N-N vs. *Dec50*: AV (1,2)	0.973	0.947	0.483	0.017	-0.973, 0.911
Role uncertainty					
N-N vs. *Rol50*	0.042	0.036	0.166	0.537	-2.171, -0.079
Random payment & role uncertainty					
N-N vs. *DeRo25*: Decision 1	0.098	0.088	0.388	0.433	-2.019, 0.144
N-N vs. *DeRo25*: Decision 2	0.071	0.062	0.409	0.476	-2.177, 0.053
N-N vs. *DeRo25*: AV (1,2)	0.076	0.071	0.389	0.459	-2.089, 0.089

Notes. The table shows the results from statistical tests for the impact of unbalanced matching, random payment and decision selection, and role uncertainty. FPT (t-test) depicts the p-values from a Fisher-Pitman permutation tests for two independent samples (independent two-sample t-test). EST shows the p-values from an Epps-Singleton two-sample test. Cohen's d displays the effect size for the difference in means between two treatments. 95% CI provides the 95% confidence interval for the difference in means from the t-test.

if we contrast dictator transfers from the reference treatment with *Rol50* (2.81) and *DeRo25* (2.63, 2.75),[23] we find them to be significantly higher, but not differently distributed. Effect sizes range from moderate to strong.

Next, we pool our data for regression analyses. In Table 4, we first assess whether the probability of a dictator's decision to be payoff-relevant for herself (P_D) and for the receiver (P_R) systematically influences transfers. Model 1 shows that the degree of how much a dictator's decision can actually convert into both players' payoffs has no significant predictive power – as predicted.[24] Based on these results, we estimate a line of new models, where we include dummy variables for unbalanced matching (coded 1 if *N-N/2*), random payment (coded 1 if *Pay50*), random decision (coded 1 if *Dec50* and *DeRo50*), and role uncertainty (coded 1 if *Rol50* and *DeRo50*). Models (2) to (6) in Table 4 convey that only role uncertainty has a robust and significantly positive impact on dictator generosity.

[23] 40.62% of the dictators made a different transfer in the second game. First and second transfers are very similar in *DR25* (p=0.531, FPP).

[24] Our design allows us to compare all treatments with each other. To further show that P_D and P_R do not systematically influence transfers across treatments, we run a pooled regression where treatment dummies predict transfers (see Table 12 in Appendix F). Model 1 replicates our non-parametric findings. A series of adjunct Wald-tests exemplarily shows that a comparison of the coefficients from *Pay50*, *Dec50*, and *Rol50* – the treatments where $P_D = P_R = 0.5$ – yields divergent results (*Pay50* vs. *Dec50*: p=0.582; *Pay50* vs. *Rol50*: p=0.009; *Dec50* vs. *Rol50*: p=0.031).

Table 4: Pooled Regression Analysis on the Effects of Randomization

Transfer	(1)	(2)	(3)	(4)	(5)	(6)
P_D	-0.001					
	(0.008)					
P_R	-0.010					
	(0.010)					
Unbalanced matching		-0.013				0.336
		(0.405)				(0.483)
Random payment			-0.725**			-0.258
			(0.362)			(0.446)
Random decision				0.195		-0.047
				(0.324)		(0.370)
Role uncertainty					1.016***	1.047***
					(0.330)	(0.370)
Constant	2.711***	2.075***	2.194***	2.008***	1.734***	1.727***
	(0.422)	(0.166)	(0.168)	(0.184)	(0.170)	(0.308)
Observations	192	192	192	192	192	192
Prob > F	0.233	0.975	0.047	0.547	0.002	0.026
R^2	0.015	0.000	0.017	0.002	0.053	0.060

Notes. Coefficients from OLS-regression analyses. Robust standard errors are shown in parentheses. Significance levels: *** $p<.01$, ** $p<.05$, * $p<.1$.

3.2 *Validity*

To asses the validity of the different dictator-game variants further, we now relate dictator transfers to two individual difference measures: social value orientation (SVO) and personal values. Both measures are widely used in experimental economics and social psychology and have been shown to explain individual differences in dictator-game giving (e.g., Cornelissen et al., 2011; Lönnqvist et al., 2013). The measures were elicited after the main experimental task. The application of both measures was balanced in each experimental session, i.e., half of the subjects first completed the SVO measure and half of the subjects started with the values questionnaire.

In the incentivized SVO slider measure (Murphy and Ackermann, 2014), subjects repeatedly choose between different own/other payoff allocations. The decisions resemble repeated dictator-game decisions. From the respondents' aggregated decisions, a SVO angle and a "type" (altruistic, pro-social, individualistic, and competitive) can be calculated. A higher angle indicates a greater concern for the welfare of others. We expect the angle to be positively correlated with dictator transfers.[25]

In the non-incentivized Personal Values Questionnaire (Schwartz et al., 2012), subjects respond to 57 questions on how similar they consider themselves to a described person. From their responses, the importance of ten basic values is derived for each individual (Schwartz, 1992). It was important for us to include a non-incentivized measure of pro-sociality, because incentivized measures are potentially

[25]It has to be noted here that this well-established measure utilizes random payment and role uncertainty. First, all subjects take several distribution decisions. For payoff determination, subjects are randomly assigned the role of a "dictator" or a "receiver". Dictators and receivers are randomly matched in pairs. In each pair, one dictator decision is randomly chosen and determines the pair's payoff.

Table 5: Correlation of Dictator Transfers with Alternative Measures of Generosity

Measure/Treatment	*N-N*	*N-N/2*	*Pay50*	*Dec50*	*Rol50*	*DeRo25*
Social Value Orientation	0.669***	0.670***	0.608***	0.723***	0.272	0.573***
Self-Direction	0.078	-0.028	-0.015	0.068	-0.111	-0.166
Stimulation	-0.081	-0.197	-0.046	-0.350**	-0.162	-0.232
Hedonism	0.048	-0.144	-0.014	-0.188	-0.468***	-0.174
Achievement	-0.360**	-0.039	-0.136	-0.173	0.449**	0.220
Power	-0.289$^{(*)}$	-0.158	-0.101	-0.394**	-0.220	0.028
Security	-0.516***	-0.513***	-0.062	-0.244	-0.275	0.304*
Tradition	0.058	0.030	-0.086	0.074	-0.033	0.195
Conformism	0.108	0.197	0.378**	0.308*	0.167	-0.051
Benevolence	0.163	0.317*	0.275	0.005	0.043	0.034
Universalism	0.407**	0.279	-0.095	0.333*	0.340*	-0.083
Self-Transcendence	0.424**	0.421**	0.066	0.256	0.239	-0.104
Self-Enhancement	-0.333*	-0.056	-0.143	-0.343**	0.137	0.130
Donation	0.454***	0.429**	0.093	0.319*	0.291	0.116

Notes. Spearman correlation coefficients. In *D50* and *DeRo25*, correlations are calculated with dictators' average transfers. Significance levels: *** $p<.01$, ** $p<.05$, * $p<.1$, $^{(*)}$ $p=.1$.

susceptible to spillover effects, i.e., subjects might try to balance behaviors across decisions (hedging or moral balancing effects, e.g., Merritt et al., 2010). Among the basic values, we expect the values along the dimensions of Self-Transcendence (consisting of Benevolence and Universalism and mapping the ability to transcend one's own interests for the sake of others) and the Self-Enhancement (consisting of Power and Achievement and mapping the importance of one's will to pursue one's own interests) to be positively (negatively) correlated with the dictators' generosity.[26]

Correlations are displayed in Table 5.[27] For the SVO angle, we find significant moderate to high positive correlations with dictator transfer in *N-N*, *N-N/2*, *Pay50*, and *Dec50*. In *DoRe25*, the correlation is positive and moderate. In *Rol50*, we do not find a positive correlation between the SVO angle and dictator transfers.

Regarding values, in *N-N*, *N-N/2*, and *Dec50*, we observe the expected positive (negative) correlations between dictator transfers and Self-Transcendence (Self-Enhancement) values. In *Pay50*, *Rol50* and *DeRo25*, Self-Transcendence and Self-Enhancement values show no significant correlations with the dictators' generosity (except for a single correlation with Universalism in *Rol50*). Most of the relevant correlation coefficients are very small. Interestingly, and contrary to the other treatments, Achievement and Security values display a moderate and significantly positive correlation in *Rol50* (*DeRo25*) with the transfer in the dictator game.

Finally, we relate the subjects' behavior in the dictator game to a naturally occurring decision

[26]Please refer to Appendix G for the complete list and a short description of the ten basic values.

[27]For the treatments *Dec50* and *DoRe25*, we use the dictators' average transfer – taken from their first and second dictator-game decision – for the correlations. Separate correlations yield very similar results.

situation on charitable giving, which creates a – presumably commonly accepted – positive externality outside the laboratory (this approach is similar to Benz and Meier, 2008). In doing this, we can assess how dictator-game behavior, as measured in the laboratory in the different treatments, relates to an external donation to charity. For this purpose, we provided the subjects with the opportunity to make a donation from their show-up fee in order to support a medical drug program against Malaria in Kenia run by Unicef.[28] Subjects could donate any amount between 0 and 400 Cents (€4; complete show-up fee). In the description of the donation call, we explicitly conveyed that the donated money would actually be transferred to Unicef and that the subjects could check the transfer receipt afterward.[29]

Results can again be inferred from Table 5. We find significant moderate positive correlations in the treatments *N-N*, *N-N/2*, and *Dec50*. In *Pay50*, *Rol50*, and *DeRo25*, dictator transfers are not significantly correlated with the amount donated to Unicef.

When we correlate pairwise – separately for dictators and receivers – Self-Transcendence Values, Self-Enhancement Values, SVO angle, and the donation, respectively, we find them to be significantly correlated (see Table 12 in Appendix I). Correlation coefficients range from moderate to high. These results suggest that those variables measure similar constructs and can be applied as additional controls.

3.3 *Controls*

As a last step, we run some controls. We look at potential time effects, the characteristics of the recruited subjects, and how they perceived the decision situation. We also check if the different treatments evoke different social norms, dictator beliefs on what other dictators do, and what the receivers expect them to do.

Time effects. The day (Tuesday is coded 1) and the daytime (9 a.m. is coded 1) of the experimental sessions were not significantly correlated with dictator transfers.[30]

Subjects' characteristics. Table 2 provides an overview on the characteristics of the recruited subjects deciding as dictators. The table conveys that we managed to select rather similar subjects into the different treatments. Sex (50% females) and age (average = 23.30 years) of the dictators is well-balanced. Further, a high percentage (89.06%) of the subjects was born in Germany (as described above, all invited subjects had previously indicated German as their mother tongue; this we took as a proxy for their nationality when we recruited them). Subjects had moderate experience (on average, 5.64 participations) with previous experiments. Past dictator-game experience was ruled out by the recruiting software. A share of 69.27% of the subjects majored in a discipline different from business or economics. From our selection criteria, sex (r=0.177, p=0.014, PBC), experience with previous experiments (ρ=-0.175, p=0.017, Spearman rank correlation, henceforth: *SRC*), and studying business or economics (r=-0.144, p=0.045, PBC) show a significant single correlation with the (average) dictator transfer.[31]

In Table 10 (see Appendix F), we display the results from a multiple regression analysis, where we

[28] Detailed information on the program can be found here: https://www.unicef.org/health/index_malaria.html.

[29] The original donation call and the transfer receipts are displayed in Appendix H. The receipt also includes the donations from the receivers (see Figure 2).

[30] Day: r=-0.045, p=0.539; daytime: r=0.040, p=0.583, Point biserial correlations (henceforth: *PBC*).

[31] The other correlations are: Age: ρ=-0.094, p=0.193, SRC; Born in Germany: r=0.032, p=0.658, PBC.

Table 6: Dictators' Characteristics, Comprehension Questions, and Expectations

Treatment	Fem	Age (SD)	Ger	Exp	Maj	P_D	P_R	B_E	B_N
N-N	0.50	23.00 (2.87)	0.94	5.03	0.84	1.00 (0.90)	1.00 (0.76)	2.13	4.44
N-N/2	0.50	23.00 (3.32)	0.75	5.06	0.84	1.00 (0.90)	0.75 (0.74)	2.06	3.75
Pay50	0.50	23.44 (3.71)	0.96	7.00	0.44	0.50 (0.56)	0.50 (0.56)	2.09	3.91
Dec50	0.50	23.03 (3.29)	0.94	5.12	0.81	0.50 (0.64)	0.50 (0.63)	1.59	4.28
Rol50	0.50	24.28 (7.00)	0.84	5.63	0.50	0.50 (0.61)	0.50 (0.64)	3.00	4.16
DeRo25	0.50	23.06 (2.49)	0.97	11.65	0.72	0.25 (0.30)	0.25 (0.32)	3.00	4.34
Total:	0.50	23.30 (4.04)	0.89	5.64	0.74				

Notes. Fem (Age, SD, Ger, Exp, Maj) denotes the share of females (average age, the standard deviation of dictators' age, the share of dictators born in Germany, the number of previous participations in experiments, and the share of dictators who have a different major subject than Business Administration or Economics). P_D (P_R) denotes the dictators' median (average) estimation of the probability for a dictator's decision to be payoff-relevant for the respective dictator in a specific dictator game (the dictators' median (average) estimation of the probability for a dictator's decision to be payoff-relevant for a randomly regarded receiver in a specific dictator game). B_E (B_N) denotes the dictators' empirical expectation regarding the behavior of the other dictators (the dictators' expectation regarding what they think the receivers expect them to transfer). In the treatment *Rol50*, one subject was 58 years old. When we exclude this subject from the average age calculation, the average age is 23.19 (SD=3.40). In the treatment *DeRo25*, two subjects had participated in – curiously enough – 93 previous experiments. When we exclude these subjects from the average participation calculation, the average experience in previous experiments is 6.03.

predict dictator transfers depending on a treatments dummy and subjects' characteristics. The first model confirms our results from the above non-parametric analyses on dictator generosity. In Model 2, we add the subjects' characteristics. The results convey that when we add multiple controls, only age and being a business/economics student is significantly negatively associated with transfers.[32]

Comprehension questions. To assess whether the dictators had correctly understood our treatment interventions, we asked them after the main experimental task to indicate how likely it is that their decisions determine their own payoff (see P_D in Table 1) and the payoff from a randomly regarded receiver (see P_R in Table 1).[33] Overall, the median and average values from the dictators' answers suggest that they had understood how their decisions affect their own and the receiver's payoff. In the latter case, the results are less strong, though.[34]

Empirical and normative expectations. To examine whether the dictators perceive the decision situation differently across the treatments with regard to a normative evaluation of their transfers (see, e.g., Bicchieri, 2006), we asked them to state two beliefs after the main experimental task. First, we asked them to estimate the average transfer of the other subjects deciding in the dictator role. Second, we asked them to state their second-order belief on what they should transfer from the perspective of the receivers.[35] Regarding the empirical expectations, we find no evidence that

[32]The coefficient for Age contrasts with previous results (e.g., Engel, 2011). Yet, the coefficient is very small and the students are still very young in our sample due to the limitied age span. Our finding for studying business or economics corroborates the findings from Frank et al. (1993).

[33]These probabilities are determined by the likelihood (depending on the treatment) that a subject – taking the decision as a dictator – is actually matched with a receiver, and is actually assigned the role of the dictator (receiver), and that a specific decision is actually chosen for payment.

[34]Our conjecture is – since the structure of the games is simple and was carefully explained by the experimenter – that subjects, instead of not understanding the game, made mistakes in their explicit calculations or did not fully understand the questions about the game. Support for this conjecture comes from the treatment *N-N*, which is the simplest of all. Even there, not all subjects gave the correct answers (i.e., 100%).

[35]To limit the amount of incentivized tasks, we chose not to reward dictators for accuracy of expectations. The evidence about whether payment increases accuracy or not is not conclusive. In an extensive study on the relationship

the dictators have different beliefs in *N-N* as compared to *N-N/2*, *Pay50*, and *Dec50*.[36] In the treatments *R50* and *DR25*, the dictators expect the other dictators to transfer significantly more than in *N-N* (p=0.024; p=0.014, FPT) (see B_E in Table 2).[37] The normative expectations are very similar across the treatments. Only in *N-N/2*, do the dictators believe that they should transfer somewhat less as compared to *N-N* (p=0.079, FPT) (see B_N in Table 6).[38]

4 Discussion

We now discuss our findings from the different dictator-game variants with regard to average transfers, their relation to the complementary measures of generosity, and previous findings from the literature. We have shown that the *degree* of how much a dictator's decision can actually convert into both players' payoffs has no significant impact on transfers (see, e.g., Model 1 in Table 4). We will therefore now discuss how the *source* of randomization affects behavior in order to assess the suitability of each dictator-game variant as a cost-saving complement to the standard version of the dictator game.

Reference treatment: In *N-N*, subjects transfer slightly less to the receivers as compared to what Engel (2011) reports on average generosity in student samples (24.7%). This might be an artefact of the methodological heterogeneity in the dictator games this author analyzes, or of the University of Cologne's subject pool (for a comparable result, see Camerer, 2003). Most importantly, generosity in *N-N* shows the expected correlations with dictators' social value orientation, personal values, and the external donation (cf. Cornelissen et al., 2011; Lönnqvist et al., 2013; Benz and Meier, 2008). In this respect, *N-N* provides the most convincing results such that the experimenter can indeed measure what she wants to measure.

Unbalanced Matching: In *N-N/2*, we find no evidence that the dictators behave differently as compared to our reference treatment, although the results are less pronounced with regard to their validity. The dictators seem not to react towards a decrease in the effectivity of their choice concerning the payoff of the receiver. Based on this finding, we do not necessarily want to encourage experimenters to apply unbalanced matching in order to save resources. But if there are plausible arguments for using unbalanced dictator-receiver groups in special cases, unbalanced matching may be a feasible alternative in order to gather the required data (e.g., Houser and Schunk, 2009; see also Falk and Walkowitz, 2017, for a recent study on prison inmates). With regard to deception, Ortmann and Hertwig (2002) write that there must be a direct and salient connection between the decisions taken and the desired monetary outcome in order to secure the interpretability and so the internal validity of the experiment. In this respect, our results suggest that unbalanced matching can be made transparent in order to avoid notions of deception.[39] We also want to emphasize that *N-N/2* is only *one* possible variant for testing the effects of involving fewer receivers. In principle, only

between beliefs and decisions, Costa-Gomes and Weizsäcker (2008) conclude that the effects of the belief elicitation procedure on decisions is mostly insignificant. Gächter and Renner (2010) show, in a public-good-game setting, that incentives on beliefs strengthen the relationship between beliefs and contributions, but the effect is quite small.

[36] p=0.927; p=0.996, p=0.129, Fisher-Pitman permutation test for two independent samples, henceforth: *FPT*.

[37] p=1.000, FPT.

[38] The other comparisons yield: *N-N* vs. *Pay50* (*Dec50, Rol50, DeRo25*): p=0.210 (p=0.796, p=0.515, p=0.873), FPT.

[39] A discussion of the question whether not making it transparent *is* actually deceptive is beyond the scope of this paper.

two receivers are needed to meet the requirement of a "random" matching of dictators and receivers without deception. This variant would be the most conservative (and efficient) test for studying the impact of involving fewer receivers than dictators. We decided to involve twice as many dictators than receivers to maintain comparable incentives across our treatments. An interesting question for future research is whether our finding also holds when the number of receivers is further reduced.

Random payment: The results from *Pay50* provide an illustrative case for the benefits of not only considering dictators' average transfers, but also their underlying motivations. On the one hand, we find no evidence that the transfers are significantly different from the reference treatment. On the other hand, we find the transfers in *Pay50* to expose insufficient correlations with alternative measures of generosity. Therefore, if researchers are interested in absolute average levels of generosity in the subject pool, paying only some subjects might be feasible. Yet, when generosity is tested and quantified against other competing motives (e.g., efficiency), random payment might be misleading. Moreover, if the researchers are interested in underlying motivations, or when the dictator game adds an individual control for altruistic preferences to other experimental data, associations between the experimental tasks might be biased when subjects are paid randomly.

Our results from *Pay50* differ from Sefton (1992) – not with regard to the overall conclusion about the appropriatenes of random payment, but with regard to average behavior. In his experiment, dictators are significantly more generous in relation to the control treatment without random payment, when only 25% of the subjects are paid. The differences among his and our results might be due to the fact that, in our study, we pay more subjects (50% instead of 25%; this is closer to the reference treatment) and we have higher stakes (€10 to be split instead of $5; people are less generous when playing with higher stakes; see, e.g., Camerer et al., 1999, and Engel, 2011). Further, as Table 6 shows, selected subjects in *Pay50* have participated in more experiments (7 vs. 5.03 average participations) and the proportion of non-business/economics students is lower (44% vs. 84%). The multiple regression analysis in Table 12 (Appendix F) shows that being a business/economics student indeed negatively correlates with the dictators' generosity in the whole study sample. Yet, we find no such significant correlation for the *Pay50* sub-sample (ρ=-0.117, p=0.523, SRC). The same holds for previous experience, which is not significantly correlated with generosity in *Pay50* (ρ=-0.286, p=0.119, SRC). Generally, if major subject and experience actually affected the subjects' preferences, the strength of the correlations with complementary measures of generosity should not be affected, because the subjects' preferences are only "shifted". This is not the case in *Pay50*, though.

Random decision: Using the data of *Dec50* and a regression analysis where we jointly evaluate the data of *Dec50* and *DeRo25* (see Table 4), we can infer that the mere fact that a dictator takes a second, similarly structured, decision does not significantly influence her behavior. While a good proportion of the dictators takes a different decision in the second dictator game, we do not find broad differences with regard to the averages and validity of the exposed behaviors in *Dec50* relative to our reference treatment. Like in *N-N/2*, the results are less strong, though. The results from *Dec50* suggest that payoff uncertainty, caused by a randomization of the payoff-relevant tasks, does not sufficiently change dictator behavior. This corroborates previous results on how the elicitation and payment of multiple choices (e.g., via the strategy method) – without role uncertainty – influence behavior (Brandts and Charness, 2000; Engel, 2011; Laury, 2005).

Role uncertainty: In the treatment *Rol50*, the dictators transfer significantly higher amounts to the receivers as compared to the reference treatment. The same tendency can be observed in the treatment *DeRo25*, which combines decision with role uncertainty (see Iriberri and Rey-Biel, 2011, for a similar finding). Hence, in *DeRo25*, role uncertainty seems to dominate the effect of random decision selection. A regression analysis, where we pool the data from both treatments, confirms that role uncertainty drives up the dictators' transfers (see Table 4). Inflated generosity may become an issue when it is tested against other motives (see, however, Engelmann and Strobel, 2004, who state that, according to their data, role uncertainty does not change the relative importance of inequality aversion, efficiency, and maximin preferences, p. 867). Most importantly, the subjects' transfers in *Rol50* and *DeRo25* do not expose the expected correlations with the implemented individual difference measures for generosity. By contrast, personal values typically negatively associated with dictator transfers (Achievement, Security) show significant positive correlations. Moreover, transfers do not significantly correlate with the outcome of the donation task either (Table 5). Picking up the above quote from Iriberri and Rey-Biel (2011), dictator games with role uncertainty indeed seem to identify different motives behind observed non-selfish behavior. Therefore, when the dictator game adds a control for altruistic preferences to other experimental data, associations between the experimental tasks might be biased. This is of specific interest, because in this case experimenters typically want to elicit the social preferences from *all* participants – and a dictator game with role uncertainty offers a very convenient way for doing so, as the receivers are initially not needed. Taking our results, one possibility to solve such a challenge is either to let most subjects decide as dictators and assign only a few subjects the role a receiver. Alternatively, subjects can make a donation to an external receiver, e.g., a charity (see our results from the reference treatment, or Benz and Meier, 2008, and Falk et al., 2016).

In *Rol50*, the recruited subjects are somewhat older relative to the reference treatment (due to an outlier) and the amount of students majoring in business or economics is higher (see Table 2). Both facts have not driven the transfers upwards, because the corresponding correlation coefficents are not significant, very small, and partly even negative in *R50*.[40] The same holds for the potential impact of more experienced subjects in *DeRo25* (see Table 2).[41] As we have also shown in a multiple regression (Table 12 in Appendix F), experience has no impact, and age and major subject have a negative impact on dicator transfers, generally speaking. Therefore, we do not believe that these variables have pushed the transfers in *Rol50* or *DeRo25* upwards.

Table 2 and our analyses convey the idea that dictators in *Rol50* and *DeRo25* have higher empirical beliefs concerning the transfers of the other dictators relative to *N-N* and *Dec50*.[42] We also find that empirical expectations and transfers are highly correlated in all treatments (all ρ>0.354, p<0.047, SRC). Hence, dictator transfers in *Rol50* and *DeRo25* relate to what the dictators believe about the behavior of the other dictators. Contrary to the other treatments, in *Rol50* and *DeRo25* the behavior of other subjects is potentially payoff-relevant for a subject. Although not strategically relevant, there is an inter-player dependence and the players may be forced to put themselves into the shoes of the other player and into either player role. Therefore, transfers might reflect a notion of empathy or reciprocity toward other players who might affect a subject's payoff (Batson et al.,

[40] Age: ρ=0.009, p=0.962, SRC; Major: r=-0.192 , p=0.292, PBC.
[41] Experience: ρ=-0.062, p=0.751, SRC.
[42] As shown, normative expectations do not significantly differ across these treatments.

1988). The results from our correlational analysis above, however, speak against this interpretation. Alternatively, transfers might entail an element of “wishful” or “fearful” thinking: because subjects face the threat of ending up as receivers, they evaluate the game outcomes from the receiver’s perspective. They make a generous transfer and hope that they will be matched with an equally-minded dictator in case they are actually assigned the receiver role. Testing and disentagling these alternative explanations is an interesting path for future work. Another important aspect for future methodological work relates to the fact that our (standard) measure for social value orientation also comprises uncertainty about the decision-maker’s actual role. Nevertheless, individual SVO angles are highly positively correlated with dictator generosity in almost all treatments (except *Rol50*) and with the other social preference measures. Based on our data, but also suporting a line of other studies which use the technique of random decision payment and role uncertainty (e.g., Charness and Rabin, 2002), our conjecture, as yet to be verified, is that, if experimental subjects take sufficient decisions under role uncertainty, the distorting effect of role uncertainty will become smaller.

As a final remark, we initially intended to link our results to the studies cited in the introduction, which use different forms of randomization. After another careful reading, we realized that this is an impossible endeavor for at least three reasons: First, dictator transfers in these studies depend on many treatment interventions that render comparisons very difficult. Second, different randomization techniques are combined. Third, average generosity in some cases cannot be calculated based on the information provided in the papers. However, our attempt underlines the need for a cohesive study for a better understanding of the impact of the different randomiziation procedures, but also that further evidence which warrants our findings is needed.

5 Conclusion

Using a holistic approach, the aim of this paper is systematically to assess the validity of common cost-saving dictator-game variants. Cost-saving is typically achieved through different randomization techniques. We use five distinct approaches and compare them to a standard dictator game: involving fewer receivers than dictators; paying only some subjects or decisions; role uncertainty at the time of the transfer decision; a combination of random decision payment and role uncertainty. The experiment is conducted under strict control regarding the time and the subjects’ characteristics. To test the validity of subjects’ dictator-game decisions, we relate them to complementary individual difference measures of generosity: social value orientation, personal values, and an external donation to charity. In line with previous evidence (e.g., Camerer, 2003; Iriberri and Rey-Biel, 2011; Sefton, 1992), our data show that dictator behavior is quite sensitive to the applied methods. The standard version of the dictator game has the highest validity. Involving fewer receivers than dictators and not paying for all decisions yields comparably valid results. These methods may, therefore, represent feasible alternatives for the conduct of dictator games under contraints. By contrast, in the dictator-game variants where only some subjects are paid or where subjects face uncertainty about their final player role, the expected associations with other measures of generosity are distorted. Under role uncertainty, generosity is also biased upwards. We conclude that these methods are inappropriate when the researchers are interested in valid individual measures of generosity.

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Appendix

Appendix A: Experimental Instructions (in German [English])

Preamble (in all treatments)

Instruktionen für das Experiment

Sie nehmen nun an einem wirtschaftswissenschaftlichen Entscheidungsexperiment teil. Bitte lesen Sie sich die Instruktionen für das Experiment sorgfältig durch. Für die gesamte Dauer des Experiments ist es sehr wichtig, dass Sie nicht mit anderen Experimentsteilnehmern sprechen. Wenn Sie etwas nicht verstehen sollten, schauen Sie bitte noch einmal in diese Instruktionen. Falls Sie dann noch Fragen haben, geben Sie uns bitte ein Handzeichen. Wir werden dann zu Ihnen an die Kabinen kommen und Ihre Fragen persönlich beantworten.

Für Ihr Erscheinen erhalten Sie eine Aufwandsentschädigung in Höhe von 4 €. Im Verlauf des Experiments können Sie Geld hinzuverdienen. Die Höhe Ihres Verdienstes hängt von Ihren Entscheidungen oder von den Entscheidungen anderer Teilnehmer ab. Sie erfahren zu keinem Zeitpunkt den Namen der anderen Teilnehmer. Genauso erfahren die anderen Entscheider zu keinem Zeitpunkt Ihre Identität.

Alle Daten und Antworten werden anonym ausgewertet. Um Anonymität zu gewährleisten, haben Sie eine persönliche Kabinennummer gezogen.

Am Ende des Experiments bitten wir Sie, noch ein paar Fragen zu beantworten

Instructions for the experiment.

You are now taking part in an economic decision experiment. Please read the following instructions for the experiment carefully. Throughout the entire experiment it is very important that you do not talk to any of the other participants. In case you do not understand something, please read the corresponding instructions again. If you then still have questions, please raise your hand. We will come to your cubicle and answer your questions personally. For showing up, you will receive a fixed amount of 4 €. In this experiment, you can earn additional money. The amount of money you can earn during the experiment depends on your decisions or on the decisions of other participants. You are at no point in time informed about the names of the other participants. Likewise, the other participants do not get to know your identity at any point in time. All data and answers will be analyzed anonymously. In order to assure anonymity, you have drawn a personal cubicle number. After the experiment, we will ask you to answer some questions.

Description of the dictator game (in all treatments)

Experimentsbeschreibung

In diesem Experiment gibt es zwei mögliche Rollen, die Sie als Teilnehmer potentiell haben können: Person A und Person B. Person A trifft eine für Person A und Person B auszahlungsrelevante Entscheidung. Person A erhält eine Anfangsausstattung in Höhe von 10 €.

Person A hat nun die Möglichkeit, einen beliebigen Teil dieser Anfangsausstattung an Person B zu transferieren. Dabei kann Person A nur ganzzahlige Beträge transferieren, d.h. Person A kann nur eine Zahl aus der Menge $\{0, 1, 2, 3, \ldots, 10\}$ wählen.

Person B erhält keine Anfangsausstattung und trifft keine Entscheidung.

Die Auszahlung von Person A bestimmt sich wie folgt:

+ Anfangsausstattung von Person A
- dem von Person A an Person B transferierten Betrag
= Auszahlung von Person A

Die Auszahlung von Person B bestimmt sich wie folgt:

+ der von Person A an Person B transferierte Betrag
= Auszahlung von Person B

Description of the experiment

In this experiment, there are two roles that you can potentially have as a participant: Person A and Person B. Person A takes a decision which is payoff-relevant for Person A and Person B. Person A receives an endowment of €10. Person A can transfer any part of the endowment to Person B. Person A can only transfer integer amounts, i.e., Person A can only choose numbers from {0, 1, 2, 3, . . . , 10}. Person B receives no endowment and makes no decision. The payoffs are calculated as follows: The payoff of Person A is determined by: + Endowment of Person A - Transfer from Person A to Person B = Payoff of Person A. The payoff of Person B is determined by: + Transfer from Person A to Person B = Payoff of Person B.

Treatment *N-N* (Reference Treatment)

Bitte beachten Sie:

Jedem Teilnehmer wird zunächst zufällig die Rolle von Person A oder die Rolle von Person B zugeordnet.

Nach der Zuordnung hat genau die Hälfte der Teilnehmer die Rolle von Person A. Die andere Hälfte der Teilnehmer hat die Rolle von Person B.

Jeder Person A wird zufällig eine Person B zugeordnet.

Person A trifft die beschriebene Entscheidung einmal.

Die Auszahlung einer Person A hängt von ihrer eigenen Entscheidung ab.

Die Auszahlung einer Person B hängt jeweils von der Entscheidung von Person A innerhalb einer Zuordnung ab.

Please note: Every participant is first randomly assigned the role of Person A or the role of Person B. After the assignment, exactly half of the participants have the role of Person A. The other half of the participants have the role of Person B. Every Person A is randomly matched with a Person B. Person A makes the described decision once. The payoff of a Person A depends on her own decision. The payoff of a Person B depends on the decision of Person A within a matching group, respectively.

Treatment *N-N/2* (Unbalanced Matching)

Bitte beachten Sie:

Jedem Teilnehmer wird zunächst zufällig die Rolle von Person A oder die Rolle von Person B zugeordnet.

Nach der Zuordnung haben genau 2/3 der Teilnehmer die Rolle von Person A. 1/3 der Teilnehmer hat die Rolle von Person B.

Jeder Person A wird zufällig mit einer Wahrscheinlichkeit von 50% eine Person B zugeordnet.

Person A trifft die beschriebene Entscheidung einmal.

Die Auszahlung einer Person A hängt von ihrer eigenen Entscheidung ab. Dies gilt auch für den Fall, wenn Person A keine Person B tatsächlich zugeordnet wurde.

Die Auszahlung einer Person B hängt jeweils von der Entscheidung von Person A innerhalb einer Zuordnung ab.

Please note: Every participant is first randomly assigned the role of Person A or the role of Person B. After the assignment, exactly 2/3 of the participants have the role of Person A. 1/3 of the participants have the role of Person B. Every Person A is randomly matched, with a probability of 50%, with a Person B. Person A makes the described decision once. The payoff of a Person A depends on her own decision. This also holds if Person A is not actually matched with a Person B. The payoff of a Person B depends on the decision of Person A within a matching group, respectively.

Treatment *Pay50* (Random Payment)

Bitte beachten Sie:

Jedem Teilnehmer wird zunächst zufällig die Rolle von Person A oder die Rolle von Person B zugeordnet.

Nach der Zuordnung hat genau die Hälfte der Teilnehmer die Rolle von Person A. Die andere Hälfte der Teilnehmer hat die Rolle von Person B.

Jeder Person A wird zufällig eine Person B zugeordnet.

Person A trifft die beschriebene Entscheidung einmal.

Nach der Entscheidung von Person A werden 50% der Zuordnungen von Person A und Person B zufällig ausgewählt. Diese werden dann tatsächlich, jeweils in Abhängigkeit von der Entscheidung von Person A innerhalb einer Zuordnung, ausgezahlt. Die anderen 50% der Zuordnungen werden nicht tatsächlich ausgezahlt.

Die tatsächliche Auszahlung einer Person A hängt somit von ihrer eigenen Entscheidung ab und davon, ob ihre Zuordnung für die Auszahlung zufällig ausgewählt wurde.

Die tatsächliche Auszahlung einer Person B hängt jeweils von der Entscheidung von Person A innerhalb einer Zuordnung ab, und davon, ob ihre Zuordnung für die Auszahlung zufällig ausgewählt wurde.

Please note: Every participant is first randomly assigned the role of Person A or the role of Person B. After the assignment, exactly half of the participants have the role of Person A. The other half of the participants have the role of Person B. Every Person A is randomly matched with a Person B. Person A makes the described decision once. The payoff of a Person A depends on her own decision. The payoff of a Person B depends on the decision of Person A within a matching group, respectively. After the decision of Person A, 50% of the Person A-Person B matching groups are randomly chosen. They are actually paid out, depending on the decision of Person A within a matching group, respectively. The other 50% of the matching groups are not actually paid out. Therefore, the actual payoff of a Person A depends on her own decision and on whether her matching group was randomly chosen, respectively. The actual payoff of a Person B depends on the decision of Person A and on whether her matching group was randomly chosen, respectively.

Treatment *Dec50* (Random Decision)

Bitte beachten Sie:

Jedem Teilnehmer wird zunächst zufällig die Rolle von Person A oder die Rolle von Person B zugeordnet

Nach der Zuordnung hat genau die Hälfte der Teilnehmer die Rolle von Person A. Die andere Hälfte der Teilnehmer hat die Rolle von Person B.

Jeder Person A wird zufällig eine Person B zugeordnet.

Person A trifft die beschriebene Entscheidung zweimal.

Bei der zweiten Entscheidung bleiben die Rollen und die Zuordnung der Teilnehmer dieselben.

Es wird eine Entscheidung von Person A mit einer Wahrscheinlichkeit von 50 % gewählt, welche die Auszahlungen von Person A und Person B bestimmt.

Die Auszahlung einer Person A hängt jeweils mit einer Wahrscheinlichkeit von 50% von ihrer ersten oder von ihrer zweiten Entscheidung ab.

Die Auszahlung einer Person B hängt jeweils mit einer Wahrscheinlichkeit von 50% von der ersten oder von der zweiten Entscheidung von Person A innerhalb einer Zuordnung ab.

Please note: Every participant is first randomly assigned the role of Person A or the role of Person B. After the assignment, exactly half of the participants have the role of Person A. The other half of the participants have the role of Person B. Every Person A is randomly matched with a Person B. Person A makes the described decision twice. For the second decision, the participants' roles and the matching of Person A and Person B remain the same. One decision of Person A is randomly chosen with a probability of 50% and determines the payoffs. The payoff of a Person A depends, with a probability of 50%, on her first or on her second decision, respectively. The payoff of a Person B depends, with a probability of 50%, on the first or on the second decision of Person A within a matching group, respectively.

Treatment *Rol50* (Role Uncertainty)

Bitte beachten Sie:

Jeder Teilnehmer entscheidet zunächst in der Rolle von Person A.

Jeder Teilnehmer trifft die beschriebene Entscheidung einmal.

Danach wird jedem Teilnehmer zufällig die Rolle von Person A oder die Rolle von Person B zugeordnet.

Nach der Zuordnung hat genau die Hälfte der Teilnehmer tatsächlich die Rolle von Person A. Die andere Hälfte der Teilnehmer hat die Rolle von Person B.

Jeder Person A wird zufällig eine Person B zugeordnet.

Die Auszahlung einer Person A hängt von ihrer Entscheidung, die sie als Person A getroffen hat, ab.

Die Auszahlung einer Person B hängt jeweils von der Entscheidung von Person A innerhalb einer Zuordnung ab.

Please note: Every participant first takes a decision in the role of Person A. Every participant makes the described decision once. Then, every participant is randomly assigned the role of Person A or the role of Person B. After the assignment, exactly half of the participants have the role of Person A. The other half of the participants have the role of Person B. Every Person A is randomly matched with a Person B. The payoff of a Person A depends on the decision she has taken in the role of Person A. The payoff of a Person B depends on the decision of Person A within a matching group, respectively.

Treatment *DeRo25* (Random Decision and Role Uncertainty)

Bitte beachten Sie: Jeder Teilnehmer entscheidet zunächst in der Rolle von Person A.

Person A trifft die beschriebene Entscheidung zweimal. Danach wird jedem Teilnehmer zufällig die Rolle von Person A oder die Rolle von Person B zugeordnet.

Nach der Zuordnung hat genau die Hälfte der Teilnehmer tatsächlich die Rolle von Person A. Die andere Hälfte der Teilnehmer hat tatsächlich die Rolle von Person B.

Jeder Person A wird zufällig eine Person B zugeordnet.

Es wird eine Entscheidung von Person A mit einer Wahrscheinlichkeit von 50 % gewählt, welche die Auszahlungen von Person A und Person B bestimmt.

Die Auszahlung einer Person A hängt jeweils mit einer Wahrscheinlichkeit von 50% von ihrer ersten oder von ihrer zweiten Entscheidung ab.

Die Auszahlung einer Person B hängt jeweils mit einer Wahrscheinlichkeit von 50% von der ersten oder von der zweiten Entscheidung von Person A innerhalb einer Zuordnung ab.

Please note: Every participant first takes a decision in the role of Person A. Every participant makes the described decision twice. Then, every participant is randomly assigned the role of Person A or the role of Person B. After the assignment, exactly half of the participants have the role of Person A. The other half of the participants have the role of Person B. Every Person A is randomly matched with a Person B. One decision of Person A is randomly chosen with a probability of 50% and determines the payoffs. The payoff of a Person A depends, with a probability of 50%, on her first or on her second decision, respectively. The payoff of a Person B depends, with a probability of 50%, on the first or on the second decision of Person A within a matching group, respectively.

Appendix B: Stages of the Experiment

Table 7: Stages of the Experiment

1. Introduction	
2. Dictator Game(s): *N-N*, *N-N/2*, *Pay50*, *Dec50*, *Rol50*, *DeRo25*	
3. Comprehension questions	
4. Empirical and normative Beliefs	
5a. Social Value Orientation	5b. Personal Values Questionnaire
6a. Personal Values Questionnaire	6b. Social Values Orientation
7. Big 5 Personality Questionnaire (data not reported here)	
8. Donation Task	
9. Sociodemographics Questionnaire	
10. Payout	

Notes. In the stages 5 and 6, half of the subjects first completed the SVO measure (a) and half of the subjects (b) started with the values questionnaire.

Appendix C: Session Plan

Table 8: Session Plan

Session No.	Week	Date	Day	Time	Treatment
1	1	May 23, 2017	Tuesday	9:00 - 10:15	*N-N*
2	1	May 23, 2017	Tuesday	11:00 - 12:15	*Dec50*
3	1	May 24, 2017	Wednesday	9:00 - 10:15	*N-N/2*
4	1	May 24, 2017	Wednesday	11:00 - 12:15	*DeRo25*
5	2	May 30, 2017	Tuesday	9:00 - 10:15	*Dec50*
6	2	May 30, 2017	Tuesday	11:00 - 12:15	*N-N/2*
7	2	May 31, 2017	Wednesday	9:00 - 10:15	*Rol50*
8	2	May 31, 2017	Wednesday	11:00 - 12:15	*N-N*
9	3	July 25, 2017	Tuesday	9:00 - 10:15	*Pay50*
10	3	July 26, 2017	Wednesday	11:00 - 12:15	*Pay50*

Appendix D: Fehr-Schmidt Predictions

Table 9: Fehr-Schmidt Predictions

x	P_D	$P_D \cdot$ (10 - x)	P_R	β	x_i - x_j	$P_R \cdot \beta \cdot (x_i$ - $x_j)$	$U_D(x)$
Treatment: *N-N*							
0	1	10	1	0.6	10	6	4
1	1	9	1	0.6	8	4.8	4.2
2	1	8	1	0.6	6	3.6.	4.4
3	1	7	1	0.6	4	2.4	4.6
4	1	6	1	0.6	2	1.2	4.8
5	1	5	1	0.6	0	0	5*
Treatment: *N-N/2*							
0	1	10	0.5	0.6	10	3	7*
1	1	9	0.5	0.6	8	2.4	6.6
2	1	8	0.5	0.6	6	1.8	6.2
3	1	7	0.5	0.6	4	1.2	5.8
4	1	6	0.5	0.6	2	0.6	5.4
5	1	5	0.5	0.6	0	0	5
Treatment: *Pay50*, *Dec50*, *Rol50*							
0	0.5	5	0.5	0.6	5	1	3.5*
1	0.5	4.5	0.5	0.6	3.5	1.05	3.45
2	0.5	4	0.5	0.6	2	0.6	3.4
3	0.5	3.5	0.5	0.6	0.5	0.15	3.35
Treatment: *DeRo25*							
0	0.25	2.5	0.25	0.6	2.5	0.375	2.125*
1	0.25	2.25	0.25	0.6	1.25	0.1875	2.0625
2	0.25	2	0.25	0.6	0	0	2

Notes. x=dictator transfer, P_D (P_R) is the probability for a dictator's decision to affect the payoff of the respective dictator in a specific dictator game (the probability for a dictator's decision to affect the payoff of a randomly regarded receiver in a specific dictator game), β is the inequity aversion parameter of the dictator, x_i (x_j) is the payoff of the dictator (receiver). * marks the optimal choice for the dictator, i.e., the transfer that generates the highest utility $U_D(x)$.

Appendix E: Distribution of Transfers

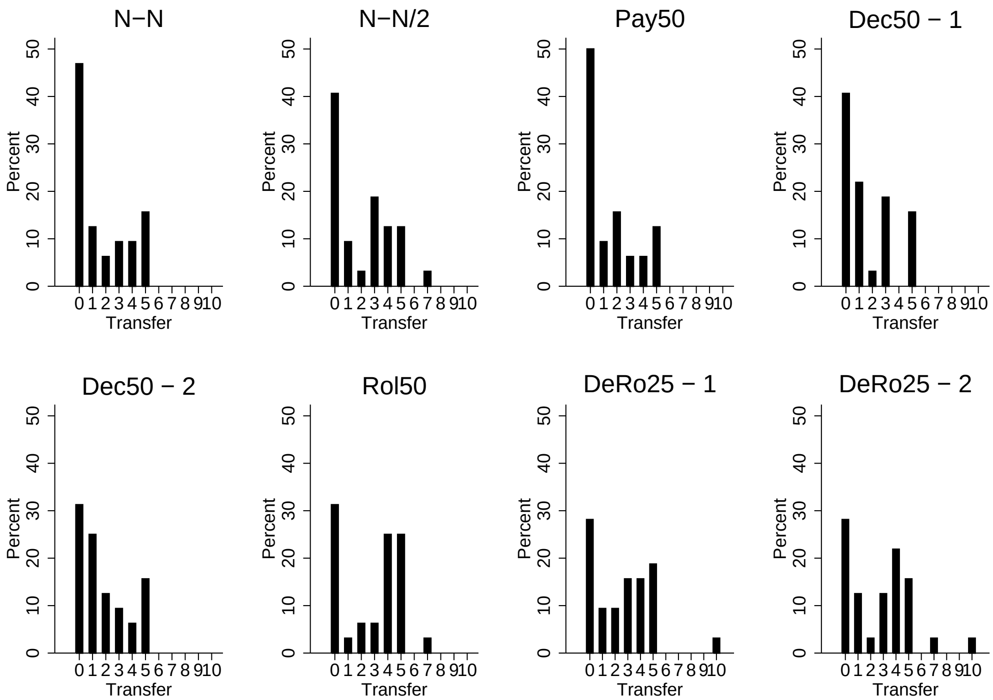


Figure 1: Transfers across Treatments

Appendix F: Multiple Regressions on Transfers

Table 10: Metaregressions on Transfers

Transfer	(1)	(2)
N-N/2	0.375	0.416
	(0.511)	(0.494)
Pay50	-0.219	0.194
	(0.476)	(0.487)
Dec50	0.031	0.059
	(0.471)	(0.444)
Rol50	1.125**	1.679***
	(0.523)	(0.515)
DeRo25	1.000*	0.936$^{(*)}$
	(0.544)	(0.571)
Female		0.486
		(0.331)
Age		-0.070**
		(0.028)
German		0.215
		(0.447)
Experience		-0.022
		(0.027)
Business/Economics		-0.718**
		(0.338)
Constant	1.688***	3.082***
	(0.349)	(0.931)
Observations	192	192
Prob > F	0.049	0.000
R^2	0.060	0.137

Notes. Coefficients from OLS-regression analyses. The reference category is *N-N*. Robust standard errors are shown in parentheses. In the lower panel of the table, the p-values of Wald tests are depicted. Significance levels: *** $p<.01$, ** $p<.05$, * $p<.1.$, $^{(*)}$ $p=.1$.

Appendix G: Schwartz Values

Table 11: The Ten Basic Personal Values

Value	Description
Self-direction	Independent thought and action – choosing, creating, exploring
Stimulation	Excitement, novelty, and challenge in life
Hedonism	Pleasure and sensuous gratification for oneself
Achievement	Personal success through demonstrating competence according to social standards
Power	Social status and prestige, control or dominance over people and resources
Security	Safety, harmony, and stability of society, of relationships, and of self
Tradition	Respect, commitment, and acceptance of the customs and ideas that traditional culture or religion provides
Conformity	Restraint of actions, inclinations, and impulses likely to upset or harm others and violate social expectations or norms
Benevolence	Preservation and enhancement of the welfare of people with whom one is in frequent personal contact
Universalism	Understanding, appreciation, tolerance, and protection for the welfare of all people and for nature
Self-Transcendence	Transcending one's own interests for the sake of others (calculated by the mean of Benevolence and Universalism Values)
Self-Enhancement	Pursuing one's own interests (calculated by the mean of Achievement and Power Values)

Appendix H: Donation Task

Description of the task

Vielen Dank für Ihre Teilnahme an diesem Experiment.

Für Ihre Teilnahme erhalten Sie wie immer einen Festbetrag in Höhe von 4 Euro (=400 Cent).

Trotz vieler Fortschritte im Kampf gegen Malaria sterben weltweit jährlich noch immer 800.000 Kinder an den Folgen. Typische Symptome für Malaria sind Kopfschmerzen, Erschöpfung und Muskelschmerzen. Wiederkehrende heftige Fieberanfälle können tödlich sein. Malaria ist jedoch heilbar. Doch nur jedes dritte erkrankte Kind erhält rechtzeitig Medikamente, die dafür notwendig wären. Moderne ACT-Kombinationspräparate helfen gegen die gefürchtete Fieberkrankheit. Ein an Malaria erkranktes Kind erhält sie zweimal täglich, etwa drei Tage lang. Im Kampf gegen Malaria können auch kleine Spenden hilfreich sein und viel bewirken. (Auskunft laut Webseite von UNICEF).

Sie können nun bestimmen, ob und wie viel Sie zur Unterstützung von UNICEF spenden wollen. Sie können diese Entscheidung vollkommen frei und anonym treffen. Der Betrag, den Sie spenden, wird dafür verwendet werden, Malaria-Medikamente für Kinder zu kaufen, damit diese damit behandelt und geheilt werden können. Mit Ihrer Spende können Sie daher helfen, Kinder vor Malaria zu schützen. Ihr Spendenbetrag wird nach Beendigung der Studie durch die Studienleiter für den Kauf von Malariamedikamenten für Kinder gespendet. Die Belege dafür können auf Nachfrage bei den Studienleitern eingesehen werden.

Ich möchte gern folgenden Betrag an UNICEF spenden (bitte tragen Sie hier einen Wert zwischen 0 und 400 Cent ein):

Thank you for your participation in this experiment.

As always, you will receive a fixed amount of 4 Euro (= 400 cents) for your participation. In spite of the many advances in the fight against malaria, 800,000 children worldwide still die from its consequences every year. Typical symptoms of malaria are headache, fatigue, and muscle pain. Recurring violent fever attacks can be fatal. Malaria is, however, curable. But only one out of every three sick children receives necessary medication in time. Modern ACT combination drugs help against the fevered febrile disease. A child suffering from malaria receives it twice daily, for about three days. In the fight against malaria, small donations can also be helpful and have a significant impact (according to the UNICEF website). You can now determine if, and how much, you want to donate to support UNICEF. You can make this decision completely freely and anonymously. The amount you donate will be used to buy malaria drugs for children so that they can be treated and cured. With your donation, you can help protect children from malaria. Your donation amount will be donated by the experimenters after completion of the study for the purchase of malaria drugs for children. The receipts can be viewed on request by the study directors. I would like to donate the following amount to UNICEF (please enter an amount between 0 and 400 cents):

Payment receipt (first wave)

unicef

für jedes Kind

2165674

Universität zu Köln
Seminar für Allgemeine BWL
Herrn Dr. Walkowitz
Albertus-Magnus-Platz
50923 Köln

22.06.2017

Vielen Dank für Ihre Hilfe!

Sehr geehrter Herr Dr. Walkowitz,

vielen Dank für die Spende aus Ihrer Sammlung in Höhe von 256,68 €. Wir werden sie in Ihrem Sinne für die weltweite UNICEF-Arbeit verwenden.

Gemeinsam ändern wir die Welt für Kinder

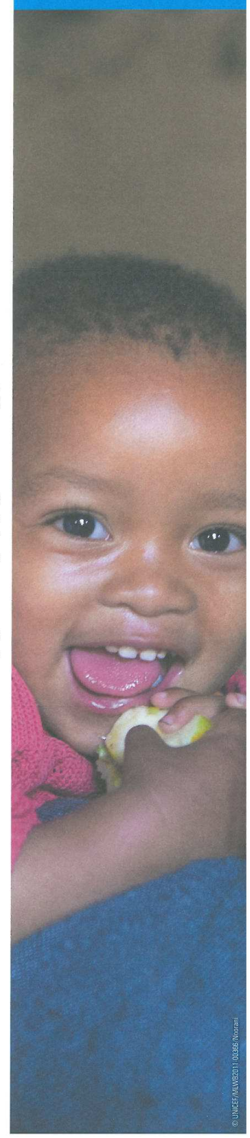
Mit Ihrer Spende helfen Sie UNICEF ganz konkret, Kindern ein besseres Leben zu ermöglichen. Denn jedes Kind hat das Recht auf eine Kindheit — unabhängig davon, wo es lebt, welche Hautfarbe es hat oder welcher Religion es angehört. Gemeinsam tragen wir dazu bei, dass aus diesem Recht Wirklichkeit wird.

Ihre Spende hilft nachhaltig - damit Kinder sich gut entwickeln und ihre Zukunft mitgestalten können. Im Namen der Kinder danke ich Ihnen herzlich für Ihre Unterstützung!

Mit freundlichen Grüßen

Katja Dickel

i. A. Katja Dickel
Spenderservice
Bereich Finanzen und Verwaltung



DZI Spenden-Siegel Geprüft + Empfohlen!

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Amtsgericht Köln VR 5068, Spendenkonto: Bank für Sozialwirtschaft Köln
IBAN: DE57 3702 0500 0000 3000 00, BIC: BFSWDE33XXX

Figure 2: Donation Receipt for Dictators and Receivers (first Wave)

Payment receipt (second wave)

unicef

für jedes Kind

2165674

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Telefon: 0221-93650-208

20.10.2017
FV/bur

Vielen Dank für Eure Spende!

Liebe Spenderinnen und Spendern der Universität zu Köln,

Sie haben **57,- €** für die UNICEF-Projekte in Kenia gespendet. Dafür danken wir Ihnen sehr!

Kenia gehört zu den Ländern, in denen Mangelernährung gerade Kleinkinder besonders bedroht. Es kommt häufig zu anhaltenden Dürreperioden mit der Gefahr von Missernten. Viele Menschen leben in Armenvierteln. Kenia ist außerdem Heimat für Hunderttausende Flüchtlinge zum Beispiel aus dem krisengeschüttelten Somalia oder aus Äthiopien – die Hälfte von ihnen Kinder. Oftmals sind diese Familien schon bei ihrer Ankunft stark geschwächt.

Doch die Lage ist nicht hoffnungslos. Wir können viel tun. Mit Ihrer Spende versorgen wir mangelernährte Kinder mit nahrhafter Erdnusspaste. Diese vitamin- und mineralstoffhaltige Zusatznahrung stärkt die Abwehrkräfte der Kinder und hilft ihnen, wieder Gewicht zuzunehmen. So konnten bereits 250.000 gefährdete Kinder wieder zu Kräften kommen.

Außerdem können wir mit Ihrer Unterstützung verhindern, dass es überhaupt zu Mangelernährung bei Kindern kommt. Von geschulten Gesundheitshelfern lernen Mütter zum Beispiel von Anfang an, ihre Kinder zu stillen, die richtige Beikost zu geben und nahrhaftes Gemüse anzubauen. Wir stellen auch Maßbänder und Waagen bereit, um die Säuglinge regelmäßig zu messen und zu wiegen. So lässt sich drohende Mangelernährung rechtzeitig erkennen.

UNICEF versteht sich als Anwalt der Kinder und setzt sich dafür ein, dass ihre Rechte in allen Ländern der Erde verwirklicht werden. Wir danken Ihnen, dass Sie uns bei dieser großen Aufgabe helfen.

Viele Grüße

D. Burmester

i. A. Dorina Burmester
Spenderservice
Bereich Finanzen und Verwaltung

TransparenzPREIS 2012
2. Platz
1. Platz 2010 für transparente Berichterstattung und hervorragende Informationsvermittlung.

DZI Spenden-Siegel Geprüft + Empfohlen!

Figure 3: Donation Receipt for Dictators and Receivers (second Wave)

Appendix I: Correlations of Complementary Generosity Measures

Table 12: Correlations of Complementary Generosity Measures

Correlation	Dictators (n=192)	Receivers (n=112)
Self-Transcendence Values and SVO angle	0.289***	0.344***
Self-Enhancement Values and SVO angle	-0.224***	-0.218**
Self-Transcendence Values and donation	0.216***	0.223**
Self-Enhancement Values and donation	-0.116	-0.254***
Self-Transcendence Values and Self-Enhancement Values	-0.554***	-0.562***
SVO angle and donation	0.321***	0.504***

Notes. Spearman correlation coefficients. SVO=Social Value Orientation. Significance levels: *** p<.01, ** p<.05, * p<.1