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How voice shapes reactions to impartial decision makers: An experiment

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How Voice Shapes Reactions to Impartial Decision Makers: An Experiment*

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Abstract

We study how participation in decision processes shapes people's behavior towards impartial authorities. In an incentivized laboratory experiment, an impartial decision maker at first decides about the allocation of money between two subordinates. Treatments differ in the opportunity for one subordinate to state her opinion about the allocation before the decision is made ("voice"). We elicit the subordinate's attitude towards the impartial decision maker in a subsequent dictator game. Voice substantially increases transfers in the dictator game towards the former decision maker. Average transfers are 90% higher in voice treatments than in baseline. These positive voice effects occur irrespectively of the previous allocation decision by the impartial decision maker. Our results therefore stress the positive effects of participation in decision procedures when people interact with impartial authorities.

Key words: voice, procedure, fairness, impartial decision maker, participative decision making, communication, laboratory experiment

JEL: C91, D03, D23, K40

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I. Introduction

Imagine you are sued in court or you get a ticket from the police. Alternatively, put yourself in the shoes of a worker whose boss decides how to allocate bonuses between team members. In these cases, an authority that should act impartially makes a decision that affects you. Would you appreciate stating your opinion in the process? Even, if you were not able to influence the outcome? And would the participation affect your subsequent behavior towards the authority?

When dealing with authorities, people not only care about their outcomes, but also about the fairness of the way in which these outcomes are reached (e.g., Thibaut and Walker 1975, Lind and Tyler 1988). Procedural justice research has gathered considerable evidence that when people perceive procedures as fair, this leads to positive evaluations of decision makers and an enhanced willingness to cooperate and accept decisions (cf. Lind and Tyler 1988, Tyler and Lind 1992, Tyler and Blader 2000, Tyler 2006). More recently, economists have also investigated how people react to the fairness of the decision making process, both theoretically and empirically (e.g., Brandts and Charness 2003, Bolton et al. 2005, Dal Bó et al. 2010, Frey et al. 2004, Frey and Stutzer 2001, 2005, Trautmann 2009). We study a key aspect affecting people's fairness evaluation of a procedure: "voice" (Hirschman 1970), defined as "some form of participation in decision making by expressing one's own opinion" (Folger 1977, p.109). The extent to which one sided communication affects the decision of the addressee of voice has been studied in dictator games (e.g., Charness and Rabin 2005, Rankin 2006, Mohlin and Johannesson 2008, Yamamori et al. 2008, Xiao and Houser 2009, Andreoni and Rao 2011). In this paper, our focus differs from this line of research in that we are interested in the effect of voice on the behavior of those who could state their opinion.[1] We examine whether subordinates' participation in the decision process via voice positively affects their subsequent behavior towards an impartial authority. In particular, we investigate whether such a "voice effect" is unanimously positive irrespective of the decision by the authority or whether allowing participation may be detrimental when decisions are unfavorable for a subordinate. Shedding light on the effect of voice on subordinates' reactions to impartial decision making is important, as authorities are most effective when subordinates voluntarily comply with their decisions and group members are willing to cooperate (e.g., Tyler 2006, Tyler and Blader 2000). Consider the illustrative examples above: local authorities and courts lack the means to enforce every single rule and decision; they have to rely on the citizens' cooperation to some degree; team members and managers have to work together in the future and they gain from mutual cooperative behavior.

Voice is generally attributed a positive effect on procedural fairness evaluations and hence on people's attitudes towards deciding authorities (cf. Lind and Tyler 1988, Cohen-Charash and Spector

[1] We refer to a companion paper, Kleine et al. 2014, for a detailed analysis of the effect of requests on the impartial decision maker.

2001, Colquitt et al. 2001).[2] However, the influence of decision outcomes on the voice effect remains ambiguous. A vast amount of research in procedural justice research stresses a "value-expressive" or "noninstrumental" function of voice (e.g., Katz 1960, Tyler et al. 1985, Tyler 1987, Lind et al. 1990). According to this function, people seem to derive utility from the mere fact that they can state their opinions even without influencing outcomes (e.g., Lind and Tyler 1988, Tyler and Lind 1992 for "relational" explanations of this effect). Behavior in line with a non-instrumental function has been found in the ultimatum game when responders could express themselves without the opportunity to influence the decision. Responders accept lower offers more frequently when they are able to express their opinions on the allocation to the proposer after the decision had been made (Xiao and Houser 2005), or else to the experimenter (Ong et al. 2012, see also Ong et al. 2013).

On the contrary, studies in principal-agent settings on voice towards self-interested decision makers find that voice fosters positive or negative reciprocal reactions – depending on whether decision makers followed the voiced suggestions (Corgnet and Hernán-González 2013, Mertins 2010). By the same token, in the ultimatum game, the opportunity for voicing requests towards the proposer before she makes her decision increases the minimum amount responders are willing to accept (Ong et al. 2012, see also Rankin 2003). This "expectation effect" of voice is explained as follows: people want their voice to matter and react negatively when expectations are not met (Ong et al. 2012, Folger 1977). These findings relate to an instrumental function of voice according to which people prefer voice procedures in order to influence outcomes (e.g., Thibaut and Walker 1975, 1978, Shapiro and Brett 1993).

In the interaction between subordinates and impartially deciding authorities both functions – instrumental and non-instrumental – could determine subordinates' behavior. We conduct a laboratory experiment in order to provide controlled evidence for the general voice effect in an incentivized decision environment and to discriminate between the two different functions of voice. In the experiment an impartial decision maker, who has no monetary stakes in the outcome of the decision, has to settle the conflicting interests of two subordinates. She allocates money between them. In two *voice* treatments, one subordinate expresses her opinion about a fair allocation of the money towards the decision maker. The *voice* treatments differ in the extent of the participation opportunity. In the *baseline*, no communication is possible. In a subsequent part of the experiment, the subordinate player who has been given voice is the sender in a dictator game with the former decision maker being the receiver. At this point, the subordinate player is not yet informed about the actual decision in the first part. Therefore she can condition her transfers in the dictator game on any possible allocation from the first part. The transfer in the dictator game is our main variable of interest. We interpret this transfer as

[2] This research mainly relies on surveys and experiments on self-reported fairness perception (but see, e.g., Lind et al. 1990 and van den Bos 1996, who also use the outcome of a real effort task as dependent variable).

a general measure for the subordinate's attitude towards the decision maker and hence the treatment differences in transfers as a measure for the effect of voice.

In our experiment, voice leads to unanimously positive effects on subordinates' subsequent behavior. We find strong treatment differences in subordinates' transfers. Subjects in both *voice* treatments send significantly more money to the decider than in the *baseline*. On average, transfers increase by 90%. Most interestingly, this positive effect on transfers is largely independent of allocation decisions in the first part of the experiment. The positive effect even holds for unfavorable outcomes. Furthermore, we find no differences across the two *voice* treatments. This indicates that the extent of voice is not decisive for the positive voice effect to persist.

Apparently, in our setting, the "value-expressive" function of voice seems to dominate the "instrumental" function. Our results highlight the importance of voice for the design of decision procedures when impartial authorities and subordinates interact. Impartial decision makers such as referees in sports, judges and juries in courts, ombudsmen in public administration and business organizations, or editors of journals may rely on voice procedures to get support for their decisions even when unfavorable decisions have to be taken.

For the remainder of the paper, we proceed as follows: We explain our experimental design in detail in section II, derive our behavioral hypotheses in section III, and report the results in section IV. In two additional treatments in section V, we further elaborate on the mechanisms behind the voice effect. Section VI briefly summarizes and discusses the results.

II. Experimental Design and Procedure

Table 1 gives an overview of the experimental design. The game consists of three parts. Subjects know that there will be several parts, but receive specific information about the content of each part only immediately before playing the relevant part of the experiment. Information about the other participants' decisions and about any earnings is given to the subjects at the end of the experiment, only. Subjects are explicitly told that they cannot lose money they have earned in a previous part in any of the subsequent parts. In the experiment, we use an experimental currency unit (ECU). All instructions[3] are read out aloud by the experimenter immediately before the relevant part to achieve common knowledge about the procedure.

At the beginning of the experiment, each subject is randomly assigned one of the three roles A, X, or Y. Players keep their roles across the three parts of the experiment. Subjects are then matched in groups of three, with one player from each role.

[3] See section VII.1 in the Appendix for an English translation of the instructions.

Table 1

Experimental Design

Part 1:	Real-effort task by players X and Y with asymmetric workload and piece rate ***Treatment variation:*** *player X sends/ does not send a message to player A* - *baseline:* no message - *narrow voice*: statement about a fair allocation - *broad voice:* statement about a fair allocation plus written message (limited to 800 characters) Allocation decision by impartial player A
Part 2:	(Unannounced) dictator game with player X as dictator and player A as receiver (strategy method for all 21 possible allocations from part 1)
Part 3:	Belief elicitation of players X and Y about chosen allocation by player A in part 1

Part 1:

In part 1, players X and Y complete a real-effort task of counting zeros on a screen of zeros and ones (see Abeler et al. 2011). Size and difficulty of the screens are identical, but the number of screens to be solved and the piece rate differs between players X and Y. Player X has to solve 12 screens, while player Y has to solve only 4 screens. Player X realizes 150 ECU per screen, while player Y realizes only 50 ECU per screen. Players cannot move to the next part unless they have completed their task.

Thus, player X contributes 1800 ECU and player Y contributes 200 ECU to an amount of 2000 ECU generated in total. We chose an asymmetric workload and piece rate to induce a normative conflict (Konow 2000, Reuben and Riedl 2013, Nikiforakis et al. 2012) among players. Thus, we provide arguments for differing opinions about a fair allocation of the 2000 ECU between players X and Y. Focal normative rules which could be considered by the players as fair are equity output (players X and Y deserve an allocation according to the ECU they produce, i.e., 1800 ECU for player X and 200 for player Y), equity input (players X and Y deserve an allocation according to the number of tasks they solve, i.e., 1500 ECU for player X and 500 ECU for player Y), and equality (equal split of the amount of money, i.e., 1000 ECU for player X and 1000 ECU for player Y). Player A, who will later decide about the actual allocation, is not involved in the real-effort task. After completing the real-effort task, all three players indicate in private which allocation of the total amount between player X and player Y they would consider as fair. It is made explicit that this information will not be revealed to the other players and has no influence on the earnings of the players.

The next step is subject to our treatment variation. In the treatments *broad voice* and *narrow voice*, player X sends a message to player A. In the *narrow voice* treatment, player X states to player A which allocation she would consider as fair. In addition to the stated number, in the *broad voice* treatment, player X can send a written message (limited to 800 characters) to player A. In the *baseline*, no messages are sent. In no treatment can players Y or A send a message.[4]

Finally, player A is asked to allocate the amount of 2000 ECU between players X and Y "in a fair way". Her decision is confined to 21 possible allocations in steps of 100 ECU (from player X receiving 2000 ECU and player Y receiving 0 ECU to player X receiving 0 ECU and player Y receiving 2000 ECU). Player A is impartial in her decision making: she receives a lump-sum payment of 5 Euro (equivalent to 1000 ECU). Therefore, her decision does not influence her payoffs in the first part.

Part 2:

In part two, player X receives an additional endowment of 1000 ECU and plays a dictator game (Forsythe et al. 1994) with player A as the receiver.[5] Player X can transfer any integer amount up to 1000 ECU to player A. At this point, player X does not know the actual allocation decision by player A in the first part. Using the strategy method (Selten 1967),[6] player X indicates her transfer for each of the 21 possible allocations in the first part. At the end of the experiment, only the transfer corresponding to the actual decision of player A is realized.[7]

Part 3:

In the third part, we elicit incentivized beliefs (expectations) of players X and Y about the allocation chosen by player A in the first part. For a correct guess, a player receives 250 ECU; for a guess that deviates from the actual allocation by one step only, she receives 50 ECU. If the guess deviates further from the actual allocation, the player does not receive any additional earnings in this part.

The game is played only once. After part 3, subjects learn the payoff-relevant decisions of the other players and their earnings. The players' earnings are calculated as follows:

[4] We restrict the voice opportunity to player X to elicit the mere effect of voice, irrespective of other strategic considerations. If player Y also had this possibility, this would have induced beliefs about the other player's message for player X. It would have introduced a strategic component of voice and uncertainty about the effectiveness of player X's message in comparison to player Y's message.

[5] We conducted two additional treatments (*baseline-uninvolved* and *narrow-voice-uninvolved*), in which the receiver of the second-part dictator game is not the impartial decision maker, but an uninvolved third party – in our setting, a charity. We refer to section V for more details.

[6] Brandts and Charness (2011) argue that for the comparison between treatments the main limitation of the strategy method is that it "provides a lower bound for testing for treatment effects" (p. 392).

[7] At the end of this part, players X and Y also indicate for every possible allocation how satisfied they would be with the allocation.

- Earnings of player A = lump-sum payment from part 1 + transfer from player X in part 2
- Earnings of player X = amount allocated by player A in part 1 + 1000 ECU - transfer to player A in part 2 + earnings from the belief elicitation
- Earnings of player Y = amount allocated by player A in part 1 + earnings from the belief elicitation

At the end of the experiment, we elicit participants' social value orientations by applying a standard test by McClintock and Liebrand (1988), in order to be able to control for differences in dictator game transfers due to differences in social preferences. Furthermore, all subjects answer non-incentivized questionnaires including inter alia questions about the perceived fairness of the allocation and the procedure, as well as demographics.[8]

The experiment was conducted at the Cologne Laboratory for Economic Research in May and June 2012 using z-tree (Fischbacher 2007). 264 participants were recruited via ORSEE (Greiner 2004) from the subject pool of the laboratory. 87 subjects participated in the *narrow voice* treatment as well as in the *baseline* (29 independent observations each) and 90 subjects participated in the *broad voice* treatment (30 independent observations). Participants were mainly students from various disciplines (39% majoring in economics) with a mean age of 24.80 years (sd=5.18). 56% were female. Sessions lasted approximately 90 minutes on average. The experimental currency was converted into Euro (2 ECU = 0.01 EUR) at the end of the experiment and paid out in cash. Participants earned 14.74 EUR[9] on average (sd=3.00), including a show-up fee of 4 EUR.

III. Behavioral Predictions

Our main variable of interest is the transfer by player X in the dictator game as a measure for the player's attitude towards the impartial decision maker. In particular, we are interested in testing if subjects only value voice as an instrument to influence outcomes or if voice has a value in itself irrespective of outcomes. Applying the standard assumptions of rational and self-interested players, predictions for transfers in the dictator game are straightforward: independently of the treatment and the possible allocation in the first part, player X will not transfer any money to the impartial decision maker. However, a vast body of experimental literature on dictator games has shown that transfers are common in such a non-strategic setting (for a meta-study, see Engel 2011). We are particularly interested in finding out how our treatment variations of voice towards the impartial decision maker affect the giving behavior and if the effect depends on the possible allocation chosen in part 1.

[8] Although we elicit variables for all players, according to our research questions we limit our analysis to players X. We discuss some of the questionnaire data in section IV.5. Additional variables not reported do not provide further insights for our research question. A list of the questions asked to player X can be found in the Appendix VIII. 5.
[9] 14.74 EUR corresponded to 18.39 USD at the time of the experiment.

On the one hand, it is claimed that voice has a "value-expressive" function (e.g., Tyler 1987). According to this function, positive voice effects on subordinates' attitudes and on their subsequent behavior should occur irrespectively of the outcomes they receive from the authorities' decisions. We assume that these effects concerning the attitude towards the decision maker translate into actual behavior in the dictator game. If a "value-expressive" function of voice prevails in our setting, we should observe the following hypothesis to hold:

Hypothesis 1a: Transfers in part two are generally higher in both *voice* treatments than in the *baseline*. Transfers in the *voice* treatments are higher than in the *baseline* for every possible allocation chosen in part one.

On the other hand, some authors stress an "instrumental" function of voice in decision-making processes. People want to have voice in order to control the outcomes of the decision (e.g., Thibaut and Walker 1975, 1978). Therefore, voice can raise expectations about outcomes (Folger 1977). In interaction with self-interested decision makers, it has been shown that voice increases positive reactions only for favorable outcomes, but increases negative reactions if outcomes fall short of expectations (Corgnet and Hernán-Gonzáles 2013, Ong et al. 2012, Mertins 2010). If this "expectation effect" determines subordinates' behavior in our setting as well, voice effects will depend on the favorability of the allocation in the first part of the experiment. Hence, the competing hypothesis is as follows:

Hypothesis 1b: Transfers in part two are higher in the *voice* treatments than in the *baseline* only if the allocations for players X are favorable. If rather unfavorable outcomes are reached in part one, transfers in part two of the *voice* treatment are lower than in the *baseline*.

With the two different *voice* treatments, we test the effect of the extent of voice. In the *narrow voice* treatment, communication between player X and player A is restricted to the indication of a fair allocation. The *broad voice* treatment allows for a greater opportunity to express one's opinion, in that player X additionally sends a written message to player A. Based on a monotonicity argument, we expect the voice effects that are hypothesized in 1a and 1b to be more pronounced the more voice is granted:

Hypothesis 2: Voice effects are stronger in the *broad voice* treatment than in the *narrow voice* treatment.

Apart from the behavior of players X towards the impartial decision makers, we are interested in the question to what extent expectations about the actual outcome in the allocation part are influenced by the voice opportunity. As mentioned before, Folger (1977) argues that participation in the decision-

making process might raise people's beliefs about what they deserve. This has been shown in the strategic environment of the ultimatum game where voice towards the proposer seems to increase expectations for outcomes (Ong et al. 2012). If we assume that statements towards the impartial decision maker are biased by self-interest (e.g., Babcock and Loewenstein 1997) and that people expect their voice to influence outcomes, we should observe similar patterns in our setting:

Hypothesis 3: Players X expect higher outcomes from part onw in the *voice* treatments than in the *baseline*.

IV. Results

IV.1 Main Effect of Voice

We directly turn to our main Hypotheses 1a and 1b. Therefore, we analyze the general effect of voice on transfers from players X to players A in the (unannounced) dictator game, as well as the dependence of the voice effect on the allocation chosen by player A in part one. Figure 1 illustrates a substantial positive effect of voice on transfers: average transfers are higher for every possible allocation in part one in both *voice* treatments than in the *baseline*. This effect is especially pronounced for all allocations which guarantee players X more than 50% of the total amount produced in the real-effort task. A sizeable jump in transfers at the equal split is present in all three treatments. It may be explained by the fact that allocations to players X below 50% of the total amount are not supported by any of the focal normative fairness rules. However, even for these "unfair" allocations to players X, differences between the *voice* treatments and the *baseline* remain present and have the same (positive) sign as the differences in response to rather favorable allocations. Accordingly, mean transfers over all possible allocations differ largely between the *voice* treatments and the *baseline*. On average, players X transfer 210 ECU (sd=186) in the *narrow voice* treatment and 181 ECU (sd=137) in the *broad voice* treatment to the impartial decision makers (averages over all possible allocations). Transfers are the lowest in the *baseline* with an average of 103 ECU (sd=120). Mann-Whitney tests show that mean transfers per player X are significantly higher in the *voice* treatments than in the *baseline* (*narrow* vs. *baseline*: $|z|=2.444$, $p=0.0145$; *broad* vs. *baseline*: $|z|=2.390$, $p=0.0169$).[10] In order to provide further evidence for the main effect of higher transfers in both *voice* treatments, we conduct random effects Tobit regressions, considering transfers in the dictator game as the dependent variable.[11] Results are presented in Table 2.

[10] Throughout this paper, reported p-values are always two-sided.

[11] We use Tobit regressions because, in dictator games, giving possibilities are exogenously restricted with an upper and a lower bound; the lower bound is usually zero-giving. Bardsley (2008) shows that subjects also take money if they have the opportunity. In our setting, this seems plausible, since transfers show a general downward trend from favorable to unfavorable allocations to players X and often stop at the zero transfer level for the most unfavorable allocations. The

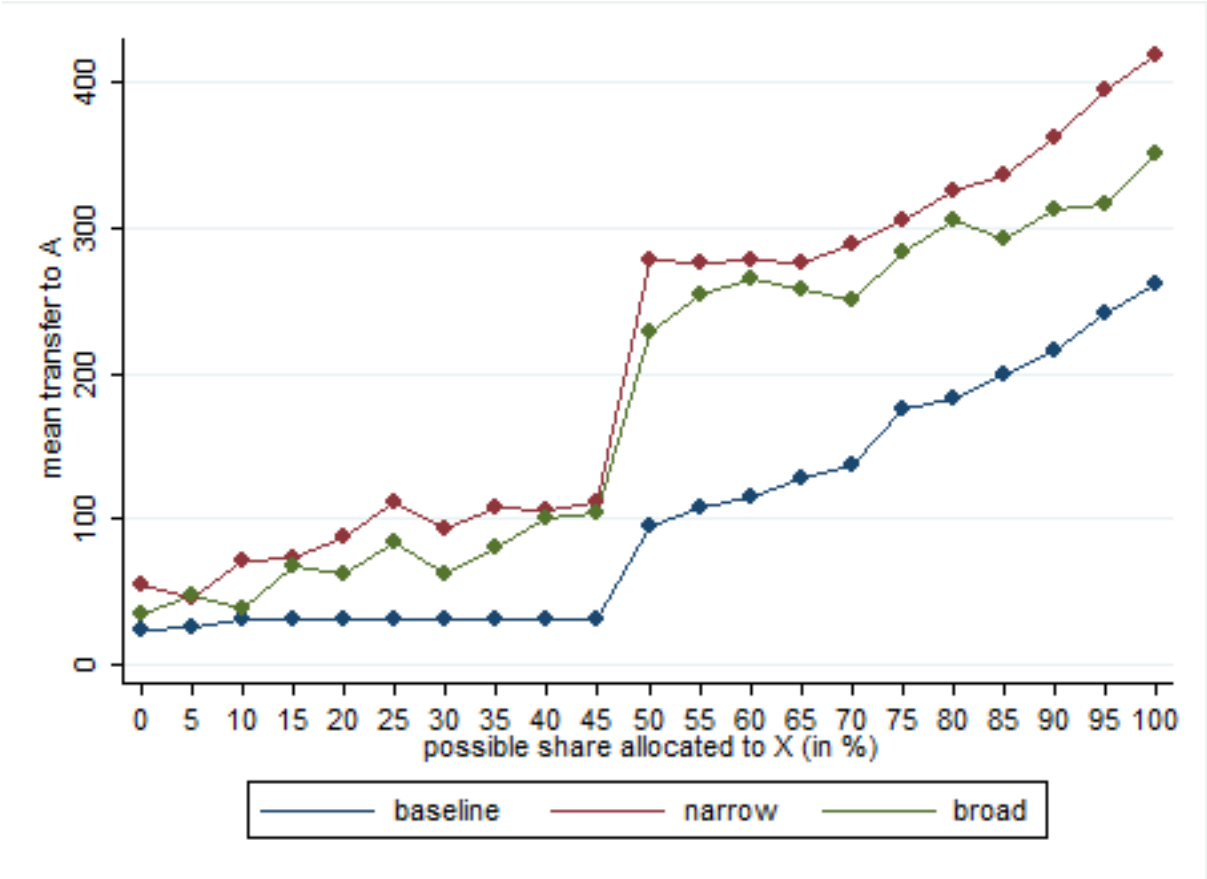

Figure 1

Mean transfers of players X for every possible allocation allocated to her by the impartial decision makers

On the horizontal axis, the possible share of money allocated by As to Xs in part 1 of the experiment is indicated: 0% "possible share allocated to X" correspond to 0 ECU for X and 2000 ECU for Y; 5 % correspond to 100 ECU for X and 1900 ECU for Y, etc. 100% correspond to 2000 ECU allocated to player X and 0 ECU to Y. On the vertical axis, the mean transfers by players X to A in part 2 of the experiment are indicated.

downward trend is therefore stopped artificially. Tobit regressions account for the possibility that (some) subjects might have even taken money instead of giving nothing by controlling for censoring. Moreover, as we have 21 transfer decisions per individual (due to the strategy method), random effects models which take individual specific effects into account are in order.

Table 2

Treatment effects on transfers – comparison of baseline and voice treatments

Random effects Tobit regressions
Dependent variable: transfers in the dictator game

	Model 1	Model 2	Model 3	Model 4	Model 5
Narrow	236.22***	217.71***	198.28***	191.92***	275.86***
	(80.37)	(77.51)	(75.32)	(72.56)	(90.93)
Broad	211.97***	161.87**	155.40**	149.59**	246.63***
	(79.75)	(78.79)	(76.21)	(73.42)	(89.54)
Fair allocation		-0.27***	-0.16	-0.01	-0.01
		(0.10)	(0.11)	(0.12)	(0.11)
Expectation			-0.27**	-0.27**	-0.28***
			(0.11)	(0.11)	(0.10)
Social value orientation score				4.65**	4.20**
				(1.82)	(1.67)
Narrow*Gender					-222.89*
					(133.22)
Broad*Gender					-267.92**
					(131.00)
Gender					-32.25
					(94.38)
Possible allocation part 1	0.34***	0.34***	0.34***	0.34***	0.34***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Constant	-489.98***	-70.59	112.00	-127.96	-72.24
	(59.89)	(166.67)	(178.63)	(195.96)	(198.51)
N	1848	1848	1848	1848	1848
P model	<.001	<.001	<.001	<.001	<.001
Wald Chi2	981.76	985.97	988.88	993.65	1004.88

Random effects Tobit regressions. Standard errors are presented in parentheses. The *narrow* dummy equals 1 for all observations of the *narrow voice* treatment, the *broad* dummy equals 1 for all observations of the *broad voice* treatment, *fair allocation* controls for the allocation players X consider as fair (from 0 ECU for X to 2000 ECU for X), *expectation* controls for players X's expectations about the actual allocation by the impartial decision maker (from 0 ECU for X to 2000 ECU to X), *social value orientation score* controls for differences in players' social value orientation, the *gender* dummy equals 1 for male players, X *possible allocation part 1* controls for possible allocations that can be implemented by the impartial decision maker (from 0 ECU for X to 2000 ECU for X). Significance at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively. Left-censored = 865; right-censored =21.

The treatment dummies *narrow* and *broad* explain treatment differences in comparison to the *baseline* (level effects). In model 1, we control by the variable *Possible allocation part 1* for effects that are due to a particular allocation that might have been implemented by decision makers. In fact, we find that the more money is allocated to players X in part one of the experiment, the more players X transfer in the dictator game (significant coefficient of 0.34). In model 2, the variable *fair allocation* additionally controls for the private statement of players X about a fair allocation. In the third model, we add the variable *expectation* controlling for the allocation expectations by players X. In the fourth model, we further add the participants' *social value orientation score* to control for differences in dictator game transfers due to general individual differences in pro-social behavior. The variable *expectation* and the *social value orientation score* have explanatory power for transfers: the more money players expect for themselves in the initial allocation, the lower is their transfer; the more players are socially-minded, the more they give in the second-part dictator game. But most importantly, the treatment effects (i.e., the dummy coefficients *narrow* and *broad*) are positive and significant in all models. Moreover, the effect sizes of the treatment dummies are substantial, once again indicating large differences between the *baseline* and the *voice* treatments. Hence, the regression analyses provide further support for the main effect of generally higher transfers in the *voice* treatments.

As a final step to test Hypotheses 1a and 1b, we test non-parametrically if the treatment effect is robust for all possible outcomes in the first part. As indicated above, average transfers for every possible allocation are higher in the *voice* treatments than in the *baseline*. These differences are also statistically significant for most of the possible allocations.[12] Non-significant differences in transfers only emerge for extreme allocations: e.g., if players X receive less than 25% of the total amount. Additionally, transfers in the *broad voice* treatment are not significantly higher for very favorable allocations for players X (when they receive 80% or more). Extreme allocations can be considered as highly unfair (for either players X or players Y). Thus, the positive effect of voice on transfers in the dictator game is significant for all allocations, which can be supported as reasonably fair. It even persists for rather unfair allocations to player X, i.e., when player X receives less than 50% (but more than 25%) of the total amount. In particular, in no situation are transfers lower in the *voice* treatments than in the *baseline*.

To sum up, we find strong evidence for a general positive voice effect. Subjects' behavior is in line with a prevailing "value-expressive" function of voice (Hypothesis 1a). We do not find support for Hypothesis 1b: Voice does not have a predominant "instrumental" function in our setting. We state the main result of our paper as follows:

> **Result 1:** Transfers are substantially higher in both *voice* treatments than in the *baseline*. This positive voice effect is largely independent of the actual allocation decision.

[12] See Table 3 in the Appendix VIII.2 for exact values.

Hence, the opportunity for voice in a decision making process positively affects the behavior of subordinates towards an impartial decision maker. In a further explorative data analysis, we unexpectedly find that the treatment effects on transfers reported above are driven by the behavior of female participants in the experiment (see Table 2 Model 5). Indeed, transfers of male participants are not statistically different between treatments (average transfers: *baseline* =100 (sd = 105), *narrow* = 107 (sd = 92), *broad* = 104 (sd = 86); Mann-Whitney tests of average transfers per player X: *narrow* vs. *baseline*: |z|= 0.196, p= 0.8449; *broad* vs. *baseline*: |z|= 0.294, p= 0.7689), whereas transfers of female participants are (average transfers: *baseline* = 107 (sd = 139), *narrow* = 282 (sd = 203), *broad* = 232 (sd = 143); Mann-Whitney tests: *narrow* vs. *baseline*: |z|= 2.503, p= 0.0123; *broad* vs. *baseline*: |z|= 2.736, p= 0.0062). Due to the fact that we had no ex-ante hypothesis on gender differences, and due to problems of reporting and interpreting purely explorative results, we refrain from further elaboration. Nevertheless, we think that, since we are the first – to the best of our knowledge – to report such gender differences in behavior due to voice procedures, it might be interesting for future research to explore the robustness of this finding and possible explanations for it.[13]

IV.2 Extent of Voice

In the following, we analyze whether the extent of voice leads to differences in transfers by comparing the two *voice* treatments. As Figure 1 already suggests, descriptively, differences between the *voice* treatments seem to be small. Indeed, comparing the mean transfers per player X in the dictator game, we do not find significant differences (Mann-Whitney test, |z|=0.250, p=0.8024). This is further supported by a Wald test, which tests whether the coefficients *narrow* and *broad* in the regression analyses presented in Table 2 are significantly different. The test exhibits no significant differences between the *voice* treatments ($p \geq 0.48$ for all models). Finally, we compare transfers in the *voice* treatments for every possible allocation separately. Applying the Mann-Whitney test, none of the transfer comparisons shows significant differences on conventional levels.[14] Hence, we find no evidence for Hypothesis 2:

> **Result 2:** Transfers are not significantly different between the *narrow voice* treatment and the *broad voice* treatment.

The result that the possibility to express oneself in a written message in addition to a numerical statement does not have a significant impact on the subsequent behavior is in line with the findings of

[13] A possible explanation could be that women lay more emphasis on procedural fairness than men (e.g., Sweeney and McFarlin 1997), but the robustness of this finding remains questionable, as other studies do not succeed in showing this difference (e.g., Kulik et al. 1996, Cohen-Charash and Spector 2003, for a meta-study).
[14] See Table 3 in the Appendix VIII.2 for exact values.

Corgnet and Hernán-González (2013).[15] The numerical statement alone seems to be sufficient for the effect of voice.

IV.3 Expectations

Presumably, people expect their voice to matter for the outcome of the decision (e.g., Ong et al. 2012). First we report results of the post-experimental questionnaire to elicit the perceived influence of players X on the decision of players A. Players X indicate on a 11-item Likert style scale to what extent they perceive to have influenced the decision (0 = "no influence at all"; 10 = "very strong influence"). Players X in the *voice* treatments perceive to have a higher influence on the decision of the impartial decision maker than those in the *baseline*, differences being highly significant (Mann-Whitney test *broad* vs. *baseline* $|z|=3.642$, $p=0.0003$; *narrow* vs. *baseline* $|z|=3.989$, $p=0.0001$). Moreover, players X messages towards the impartial decision makers are highly correlated with the expected allocation (Spearman's Rho – *narrow*: $r_S=0.5575$, $p=0.0017$; *broad*: $r_S=0.6781$, $p=0.0000$), which is another indication that the players believe their voice to matter.[16] Since the *baseline* provides no means to players X to influence the decision, perceived higher influence in the *voice* treatments seems obvious. Yet it raises the question if the perceived higher influence on the decision of players A also means that players X expect a more favorable allocation decision in the *voice* treatments than in the *baseline*. On average, players X expect to receive 1228 ECU (sd=271) (of the 2000 ECU) in the *narrow voice* treatment, 1230 ECU (sd=261) in the *broad voice* treatment and 1334 ECU (sd=359) in the *baseline* (Mann-Whitney test – *narrow* vs. *baseline* $|z|=1.652$, $p=0.0985$; *broad* vs. *baseline* $|z|=1.597$, $p=0.1103$). Thus, despite the perceived influence on the allocation decision by players A in the *voice* treatments, players X do not expect higher allocations to themselves:

> **Result 3:** Players X in the *voice* treatments perceive that they have more influence on the decision in the allocation stage than players in the *baseline*. However, at the same time, they do not expect more favorable decisions.

[15] Corgnet and Hernán-González (2013) find similar behavior in agents who could only send a simple statement to the principal, compared to the behavior of those who could chat with the principal for three minutes. Similarly, when studying the effect of voice on the recipient of the message, Andreoni and Rao (2011) report that the numerical statement influences the recipients' behavior, but an additional written message does not.

[16] Averages influence: *narrow*: 5.55, sd=3.56; *broad*: 4.97, sd=3.19; *baseline*: 1.90; sd=2.55. Averages statements of fair allocations towards the impartial decision makers: *narrow*: 1359, sd=298; *broad*: 1260, sd=353.

IV.4 Earnings of Subordinates

Using the strategy method, we show in section IV.1 that players X value the voice opportunity irrespectively of outcomes. Yet it could be that, in the actually realized decisions, players X do profit from the voice procedure in monetary terms. We therefore analyze the actual earnings of players X in the *voice* treatments and in the *baseline*.

In total, players X earn significantly less in the *voice* treatments than in the *baseline* (average earnings part 1 and part 2 *baseline*: 2144, sd=290; *narrow*: 1868, sd=378, *broad*: 1893, sd=311; Mann-Whitney test *broad* vs. *baseline*: |z|=2.652, p=0.0080; *narrow* vs. *baseline*: |z|=2.904, p=0.0037). This difference is due to two facts: First, in the *voice* treatments, players A allocate lower amounts of money to players X in the first part of the experiment than in the *baseline*. On average, players X in the *baseline* receive 1286 ECU (sd=243) (of 2000 ECU to be distributed) in the first part. This is more than the earnings of those in the *broad voice* treatment (average of 1160 ECU, sd=192; Mann-Whitney test *broad* vs. *baseline*: |z|=2.127, p=0.0334) and of those in the *narrow voice* treatment (average of 1183 ECU, sd=267; Mann-Whitney test *narrow* vs. *baseline*: |z|=1.553, p=0.1204). Second, despite the fact that players X in the *voice* treatments receive less favorable allocations in the first part of the experiment, actually realized transfers from the dictator game are significantly higher in the *voice* treatments than in the *baseline* (averages: *baseline*: 142 ECU, sd=180; *narrow*: 315 ECU, sd=247; *broad*: 267 ECU, sd=233; Mann-Whitney test *narrow* vs. *baseline*: |z|=2.993, p=0.0028; *broad* vs. *baseline* |z|=2.378, p=0.0174). Both aspects lead to the fact that players X do not profit from the voice procedures in monetary terms.[17]

IV.5 Voice Effects on Perceived Fairness

Although we focus on subjects' actual behavior, we also aim at understanding if voice shapes subjects' perception of fairness with regard to procedures and outcomes. We again turn our analysis to players X. At the end of the experiment, players X answer questions about the perceived fairness of the procedure, namely (1) how fair they perceive the procedure to be in which the decision about the allocation came about in general, (2) how fair they perceive the procedure to be in which the decision about the allocation has been made from the viewpoint of player X in particular and (3) to what extent they personally feel treated in a fair way in the decision making process. Furthermore, they are asked to state (4) the extent to which they perceive the outcome as fair and (5) the extent to which they accept the decision. All questions have to be answered on an 11-point Likert style scale ranging from 0="not fair at all" to 10="completely fair" (0="not at all" to 10="completely" for question (5)). In line

[17] See Kleine et al. 2014 for a detailed analysis of the effects of voice on allocations.

with procedural justice literature, we predict a positive effect of voice on perceived fairness of the procedure and of the outcomes, as well as on the acceptance of the decision. Figure 2 illustrates the results.

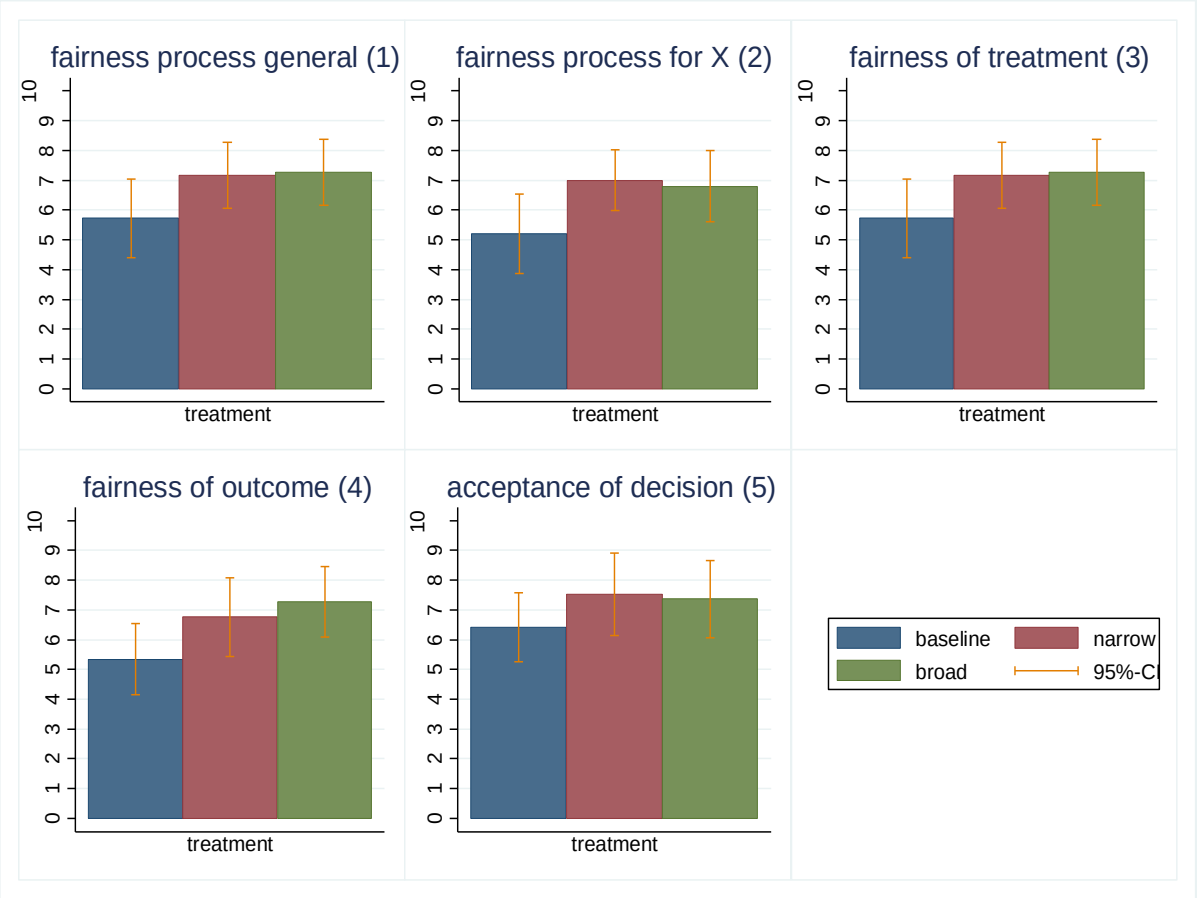

Figure 2

Players X' average ratings of: fairness of the process (1), fairness of the process from the point of view of a player X (2), fairness of the personal treatment (3), fairness of the outcome (4) and extent to which they can accept the decision (5)

The mean ratings are presented on the vertical axis. The scale goes from 0 (not at all) to 10 (completely). On the horizontal axis, one can see the different treatments. "95%-CI" is the 95% confidence interval.

Indeed, the answers to the questions regarding the procedural fairness, i.e., questions (1)-(3), all point to the direction that players X consider the voice procedure as fairer than the no-voice procedure. The results are (weakly) significant for all comparisons between the *broad voice* treatment and the *baseline* and significant for comparisons of ratings concerning question (3) between the *narrow voice* treatment and the *baseline*.[18] The higher fairness ratings in question (1) (general procedural fairness) are remarkable: In fact, the *voice* treatments could be considered as rather unfair in that they provide only one player with the opportunity for voice.[19] However, players X seem to appreciate the voice opportunity when rating procedural fairness. Furthermore, the outcomes are also judged as significantly fairer in the *voice* treatments than in the *baseline*. Finally, players X in the *voice* treatments are somewhat more willing to accept the decision made by players A than those in the *baseline*.[20] We therefore conclude that from the point of view of players X in this setting voice increases perceived fairness in regard to the procedure and the outcomes.

> **Result 4:** Players X in the *voice* treatments generally perceive the procedure and the outcomes as fairer than those in the *baseline* and are more willing to accept the decision by the impartial decision makers.

V. Extension – Voice Effects on Behavior towards Uninvolved Parties?

So far we have shown that voice has a positive effect on generous behavior towards an impartial decision maker. Moreover, the pattern we have observed has shed some light on the underlying mechanisms of how voice affects the players who are granted voice. Yet we make one further attempt at improving our understanding of the mechanisms behind the voice effect. Therefore, we examine the question whether voice effects can only be shown in a direct interaction with the impartial decision maker, or whether it also affects behavior towards third, uninvolved parties. If voice effects are merely due to a positive change in the emotional state or due to an activation of a general sensibility for acting in a fair way (e.g., because participants feel treated more fairly), we conjecture that voice should also lead to more generosity towards uninvolved parties. If no such effect towards uninvolved parties occurs, this would not prove wrong the conjecture that emotional states are influenced by voice. Rather, it would favor the notion that we are correct with our interpretation that voice changes the attitude towards the impartial decision maker (i.e., a directed expression of gratitude for being treated fairly due to voice).

[18] For averages and Mann-Whitney-tests on the fairness questions see Table 6 in the Appendix.
[19] If asked explicitly for the fairness of the process from the viewpoint of player Y, players X do not indicate higher fairness levels in the voice treatments (see Table 6 in the Appendix).
[20] In related questions, ratings about the satisfaction with the outcome conditional on every possible allocation do not differ between treatments.

Therefore, we conducted two additional treatments. In terms of design and instructions, these treatments are identical with the *baseline* and the *narrow voice* treatment described in section II, with the exception that in part two the recipient of the dictator game is an uninvolved third party rather than the impartial decision maker. By assigning the role of the uninvolved party to a charity,[21] we keep the roles in the laboratory constant. We refer to these additional treatments as *baseline-uninvolved* and *narrow voice-uninvolved*.

90 subjects participated in each of the treatments, which were conducted in March 2013. Subjects who participated in the treatments described in section II of this paper were not invited to these sessions. Our analysis is based on 30 independent observations for *baseline-uninvolved* and 29 independent observations for *narrow-uninvolved*. One independent observation from the analysis of the *narrow voice-uninvolved* treatment had to be excluded from the analysis, as one subject erroneously participated in both *uninvolved* treatments. Participants were mainly students from various disciplines (34% majoring in economics) with a mean age of 25.21 (sd=6.15). 48% were female. As in the other treatments described in section II, sessions lasted approximately 90 minutes on average. Participants earned 16.19 EUR on average (sd=3.19).[22]

Again, we focus our attention on the main variable of interest – the transfers in the second part of the experiment.[23] In the *baseline-uninvolved*, players X transfer on average 184 ECU (sd=221) to the uninvolved party (average over all possible allocations). In the *narrow voice-uninvolved* treatment, the average transfer is 197 ECU (sd=234). The Mann-Whitney test that compares average transfers is insignificant ($|z|=0.076$, $p=0.9394$), which confirms that average transfers are very similar across the treatments. Also, when we compare transfers for every possible allocation the impartial decision maker might have implemented in the first part, we find no evidence for significant differences in transfers (for all respective Mann-Whitney tests: $p\geq0.44$).[24] Moreover, we conduct a number of Tobit regressions (see Table 5 as an example in the Appendix VIII.2); in no model specification can we reject the null hypothesis that transfers in both treatments are the same. We therefore state the following result:

> **Result 5:** There is no statistical difference between transfers in the *baseline-uninvolved* treatment and the *narrow-uninvolved* treatment. Hence, in our setting, there seems to be no spillover effects of voice on the behavior towards an uninvolved party.

[21] The chosen charity, "Deutsche Welthungerhilfe e.V.", supports development projects against hunger and poverty worldwide. Subjects were informed about the goal of the charity and that it is certified by the "Stiftung Deutsches Zentralinstitut für soziale Fragen" (German Central Institute for Social Issues).

[22] 16.19 EUR corresponded to 21.06 USD at the time of the experiment.

[23] Since we implemented the uninvolved treatments to test whether voice also affects the subordinate's behavior towards an uninvolved party, we limit our analysis on the transfers of players X. For results on the other variables (expectations, perceived fairness ratings), we refer to the Table 7 in the Appendix. It has to be noted that comparisons of non-incentivized perceived fairness ratings across these treatments do not yield the same results as those in the main experiment. Applying Mann-Whitney tests, all comparisons between *baseline-uninvolved* and *narrow voice-uninvolved* are insignificant. This indicates that the fairness ratings to some extent are influenced by the setting in which players take their decisions.

[24] See Table 4 in the Appendix VII.2 for exact values.

VI. Conclusion

We show that the opportunity for voice increases the attitude of a subordinate towards an impartial authority. In our experiment, the implementation of voice leads to substantively higher transfers to the former decider in a subsequent dictator game. This effect is largely independent of the previous decision by the decision maker. Our results support the notion of a "value-expressive" effect of voice. Participants seem to appreciate the voice procedure not primarily for instrumental reasons. They rather value the mere opportunity to state their opinion in the decision-making process. In that sense, our results for interactions with an impartial decider are distinct from the behavioral effects of voice towards a self-interested decision maker (Corgnet and Hernán-Gonzalez 2013, Ong et al. 2012, Mertins 2010).

We find no differences in transfers between the two *voice* treatments. This indicates that the participation opportunities do not need to be extensive. Even restricted voice improves the relationship between the impartial decision maker and the subordinate. Surprisingly, voice towards an impartial decision maker does not seem to increase subordinates' expectations of favorable outcomes, although it does increase perceived influence on the decision. Further, voice leads to positive effects on subjects' fairness perceptions: subjects in our main treatments who can voice their opinion generally perceive procedures and outcomes as fairer and are more willing to accept these decisions. Finally, we do not find that positive effects of voice on subordinates' generous behavior spill over to interactions with an uninvolved charity. They seem to unfold in direct interactions with the impartial decision maker only.

Of course, important questions remain unanswered. In our setting, only one out of two subordinates has a voice opportunity. This way we can identify pure voice effects. Nonetheless, it would be interesting to test voice effects when more than one subordinate may express herself. Further, it remains open how voice is affected by a more powerful impartial decision maker who may grant or deny voice opportunities at her discretion. As already mentioned above, it might also be interesting for future research to explore the robustness of gender differences in reactions to voice and potential reasons for it. Along these lines, more experimental evidence is needed to understand the importance of voice procedures in economic, social, and legal interactions. Our experimental design may serve as a general framework to address these questions.

VI. References

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VIII. Appendix

VIII.1 Instructions

The instructions for the *baseline* and the *voice* treatments only differ in one regard. In the first experiment, the *baseline* consists only of Steps 1 and 2. In the *voice* treatments, an additional intermediate Step between these two is introduced. Therefore, we report first the full instructions of the *baseline* and afterwards only the new Step 2 of the *voice* treatments.

a) Baseline

General Instructions for Participants

Please begin by reading these instructions carefully. **Communication during the experiments is prohibited.** If you have any questions, please raise your hand. We will then come to you. **Disobeying this rule will lead to exclusion from the experiment and all payments.**

The experiments are conducted anonymously, i.e., nobody is told with which other participant he or she has interacted. The analysis of the experiment results will also be conducted anonymously.

You will take part in several experiments today. You can earn money during the experiments, depending on the decisions you and the other participants make. In the first experiment, we speak not of €, but of Taler. Your income from this experiment is therefore initially calculated in Taler. At the end of the experiment, the Taler earned are **converted into Euro at a rate of 2 Taler = 1 Cent and paid out to you**. In addition, each participant receives a lump sum payment of 4 Euro for showing up today.

The instructions for the individual experiments will be handed out to you just before each respective experiment. On the following pages, we will first describe the exact procedure of the first experiment. Then, there will be more experiments. It will be **impossible** for you to lose your earnings from one of the experiments in a later experiment.

After the final experiment, you will be given a questionnaire. Once you have filled in this questionnaire, the total sum you have earned will be paid to you in cash.

Information on the First Experiment: Part 1

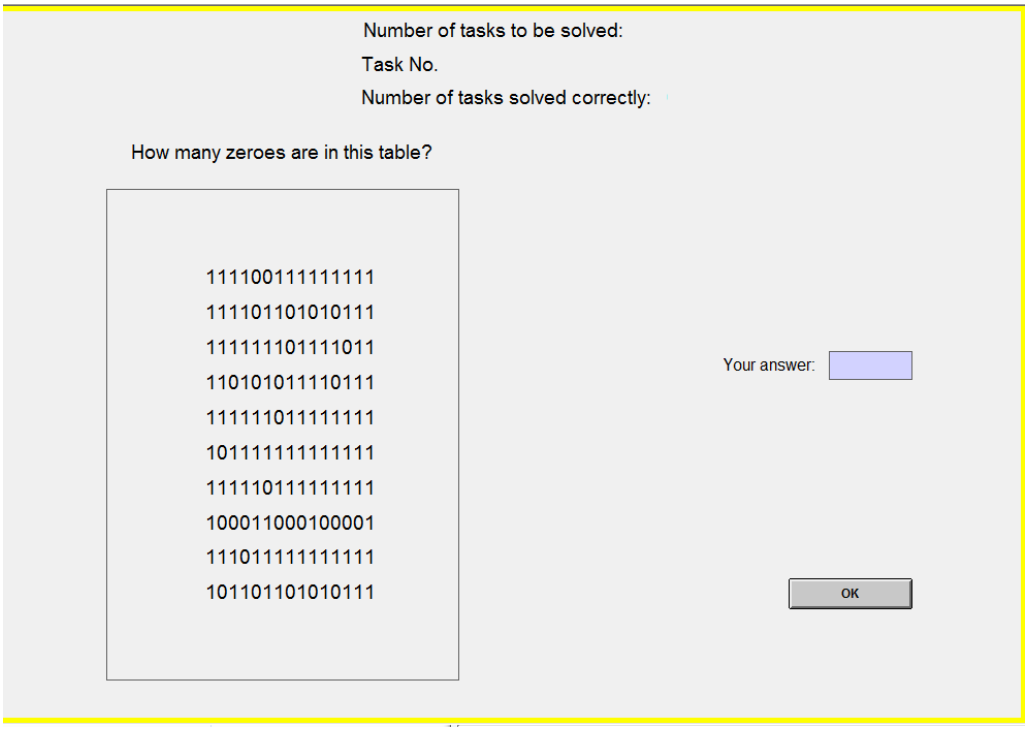
In this experiment, there are three roles: **A**, **X**, and **Y**. At the beginning of the experiment, you are assigned a role **at random**. One participant A, one participant X, and one participant Y form a **group** in this experiment. In this part of the experiment, participant **A** receives a **fixed lump sum of 5 Euro**, which remains unaltered regardless of the decisions taken by A or the other participants. The earnings of participants **X and Y are determined** by the **decisions** made in the course of the experiment. We shall now explain how exactly this works.

This experiment consists of **several parts**. First, we explain and conduct the **first part of the experiment**. You will receive further information separately for the other parts. **Here it is also impossible for you to lose what you have earned in a previous part of the experiment**.

The first part of the experiment consists of **two** steps.

Step 1:

Participants **X** and **Y each solve** a predetermined number of **tasks**. Each task consists of determining the correct amount of zeroes in a table consisting of the numbers 0 and 1. If an incorrect number is given, the participant has up to two more attempts to find the correct number. If the number given is still incorrect after three attempts, the task is considered not completed, and the participant is given a new task. The format of the table (i.e., the number of lines and columns) is the same for all tasks and participants. The tasks are presented to participants X and Y on the screens, as in the following example:



The respective participant is shown new tasks until the predetermined number of tasks that are to be solved has been reached. The number of tasks to be solved and the Taler earned per task correctly solved **are different** for participant **X** and participant **Y**, as the following table shows:

	Number of tasks to be solved correctly	Taler earned per task correctly solved
Participant **X**	**12**	**150**
Participant **Y**	**4**	**50**

In total, thus, participants X and Y together accumulate 2000 Taler in this step. The Taler earned are added up.

In the second step, participant **A** will determine the **definitive distribution** of the 2000 Taler amongst the participants X and Y. Participant A does not solve any tasks.

At the end of this step, all participants state which Taler distribution amongst participants X and Y they would consider fair.

100-Taler increments are possible here. Each participant hence indicates one of the following distributions:

Please note: **This information is not shown to any other participant and has no consequences on the payoffs** – neither on the own payoffs nor on those of the other participants.

Step 2:

Participant **A now decides how to distribute fairly** among participants X and Y the Taler earned by these two participants.

100-Taler increments are possible here. Participant **A** hence opts for one of the following distributions:

This distribution by participant **A** determines the earnings of participants **X** and **Y** in this part of the experiment.

The first part of this experiment ends with participant A making the decision described above.

Participants **X** and **Y** are told about the distribution decided upon by participant **A** and about their earnings from the first part of the experiment **after the end of this experiment**.

It is impossible for you to lose, in a later part of the experiment, the earnings you have accumulated in the first part of the experiment.

You will now be shown some control questions on your screen. After you have answered these questions correctly, the first experiment will begin.

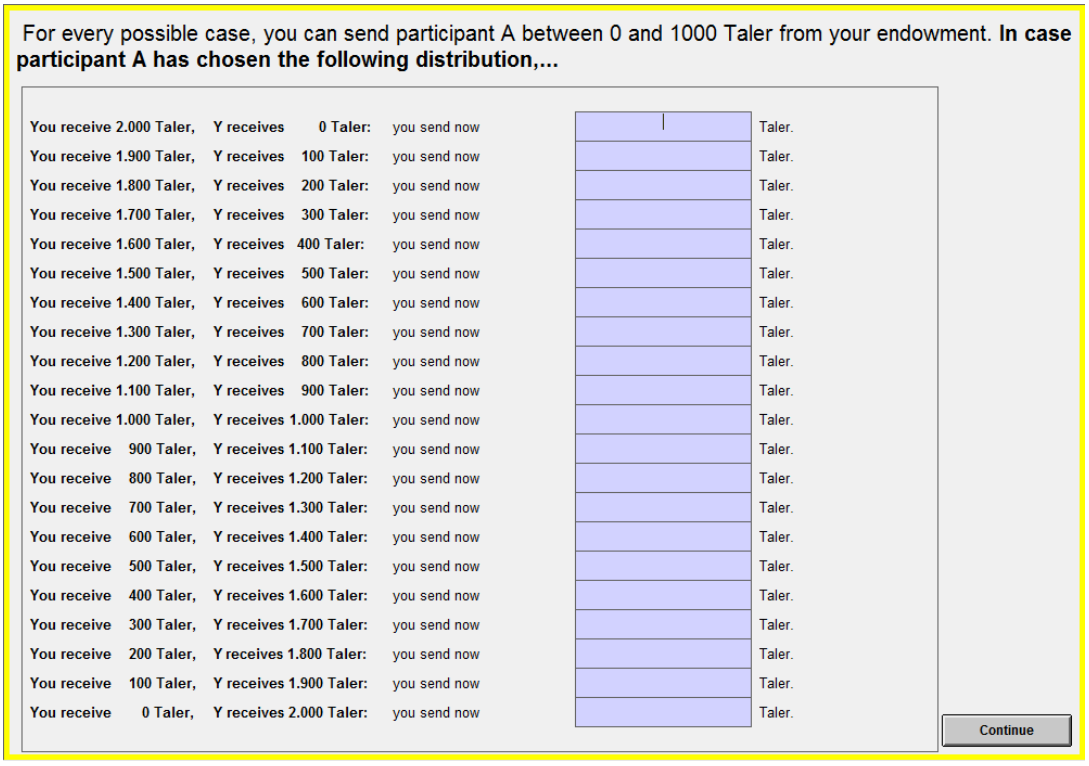
Information on the First Experiment: Part 2

The participants in this part of the experiment have the same roles as in the first part of the experiment. This means that a participant who had role A in the first part of the experiment will also have role A in this part of the experiment; a participant who had role X in the first part of the experiment will also have role X in this part of the experiment; and a participant who had role Y in the first part of the experiment will also have role Y in this part of the experiment. The constellation of the groups also remains the same as in the first part of the experiment. This means participants are always allocated to the same two participants as in the first part of the experiment.

In this part of the experiment, participant **X** receives an endowment of **1000 Taler**. The participant decides how many of these 1000 Taler to send to participant **A** (any full number between 0 and 1000). Each Taler sent is credited to participant A.

Participant X can make the decision on how many Taler to send **dependent on every possible Taler distribution chosen by participant A in the first part of the experiment**. The actual distribution from the first part of the experiment is told to participant **X** only after the experiment has ended. However, X has to decide in **this** part of the experiment how many Taler he or she wishes to send **for every possible distribution**.

In this part of the experiment, participant **X** must therefore fill in the following decision table:



Participant **X** may enter any full number between 0 and 1000 in every line. **The only line that becomes payoff-relevant is the one that corresponds to the distribution actually chosen by A in the first part of the experiment.** The decisions in the other lines do not influence the participants' payments. The lines do not have to add up exactly to 1000 either, as the only decision-relevant line is the one that corresponds to the actual situation.

As participant **X**, please note that at the time of filling in the table you do not yet know which decision participant A has made in the first part of the experiment. **You therefore have to consider your decision very carefully in every line, because any of the lines could become payoff-relevant for you.**

After filling in the table, participants X and Y will be given a brief questionnaire.

Participant **Y** makes no decisions in this part of the experiment and receives **no earnings**. Participant **A** makes no decisions either in this part of the experiment.

The **earnings** from this experiment add up as follows:

Participant **A**	Taler sent by X to A (in the payoff-relevant situation)
Participant **X**	1.000 Taler - Taler sent by X to A (in the payoff-relevant situation)

After filling in the questionnaire, the second part of the experiment ends.

Only after the experiment has ended are participants given information on the **actual distribution decision** of participant **A** in the first part of the experiment and on the sum corresponding to this distribution, which participant **X** has sent to participant A in this part of the experiment.

It is impossible for you to lose, in a later part of the experiment, the earnings you have accumulated in the first and second part of the experiment.

You will now be shown some control questions on your screen. After you have answered these questions correctly, the second part of the experiment will begin.

Information on the First Experiment: Part 3

The participants in this part of the experiment have the same roles and the same group as in the first two parts of the experiment.

In this part, participants **X** and **Y** estimate which distribution they think participant **A** has opted for in the first part of the experiment.

Each participant who correctly estimates the exact distribution chosen by participant A receives 200 Taler. If the estimate is incorrect, albeit straying merely by one decision possibility from participant A's actual decision, then the participant earns 50 Taler. A deviation by one decision possibility means that participant A actually gave participant X 100 Taler more (or less) and participant Y 100 Taler less (or more) than estimated. If the estimate deviates even more from participant A's actual decision, then the participant receives no further earnings from the estimate. The earnings from this part of the experiment are hence calculated in the following manner:

Possible scenario	**Earnings from Estimate**
Correct estimate	200 Taler
Overrated X's earnings by 100 Taler and underrated Y's earnings by 100 Taler	50 Taler
Underrated X's earnings by 100 Taler and overrated Y's earnings by 100 Taler	50 Taler
Stronger deviation (more than 100 Taler) from the estimate	0 Taler

In this part of the experiment, the earnings of a participant who makes an estimate depend only on the correct estimate. No participant can influence in any way, in this part of the experiment, the earnings of another participant. The earnings of a participant in this part of the experiment do not depend on the earnings of another participant in this part of the experiment.

In this part of the experiment, participant **A** makes no decision and receives **no earnings**.

The first experiment ends after these decisions.
All participants are then told the decisions made by the members of their group **that are payoff-relevant to them, as well as their earnings from the individual parts of the experiment.** After this, we would then ask you please to fill in a **brief questionnaire**.

A further experiment will follow. **Here, it will not be possible for participants to interact once again with the same participants from the first experiment**. Further, as before, participants will not be able to lose their earnings from previous experiments in the following experiments.

Information on the Second Experiment

In the following, we would ask you please to make your own decisions. In order to do this, you will be **randomly matched with another participant.** In several distribution decisions, you can give this other participant and yourself points. For this to happen, you will have to choose repeatedly **between two distributions, "A" and "B"**. The points you give yourself are paid out to you at the end of the experiment, at a rate of **250 points = 1 €**. At the same time, you are also randomly matched with **another** experiment participant, who also distributes points to you by choosing distributions. This participant is **not the same participant** as the one to whom you may distribute points. The points given to you are also credited to your account. The **sum** of all points you allocate to yourself and those allocated to you is paid out to you at the end of the experiment, at a rate of 250 points = 1 €.

Please note that the participants matched with you in this part of the experiment are **no members of your group** from the preceding part of the experiment. You are hence matched with other participants in this case.

The individual decision tasks will look like this:

Possibility A:		**Possibility B:**	
Your points	The points of the experiment participant allocated to you	Your points	The points of the experiment participant allocated to you
0	500	304	397

A		B

In this example: If you clicked "A", you would give yourself 0 points and 500 points to the experiment participant allocated to you. If you clicked "B", you would give yourself 304 points and 397 points to the experiment participant allocated to you.

Subsequently we will ask you please to fill in some questionnaires. While you do this, we will prepare your payments.

b) Voice manipulation – additional Step:

In Step 2, participant **X** has the chance to send participant **A** a **message**. If you are a participant **X**, please follow the instructions about this on your screen. The participants **Y** and **A** have no possibility to send a message.

c) Uninvolved Treatments

The instructions of the both uninvolved treatments only differ in the second part of the experiment:

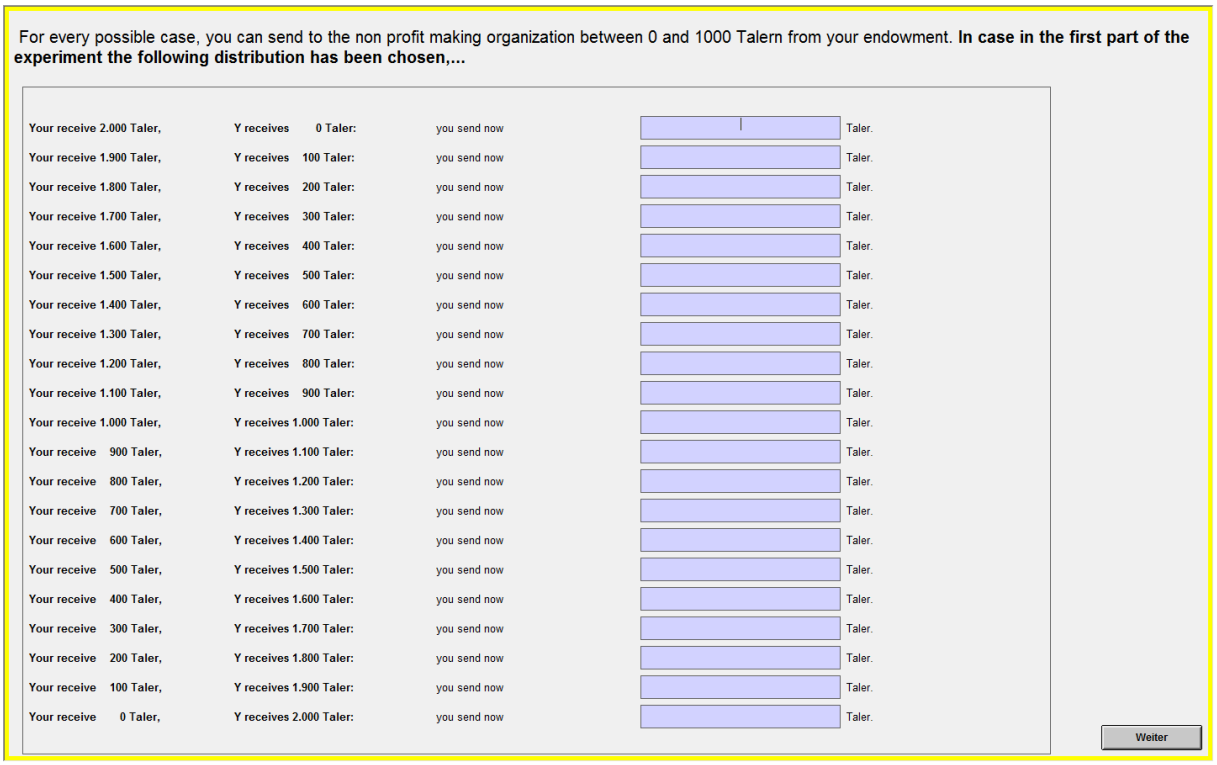
Information on the First Experiment: Part 2

The participants in this part of the experiment have the same roles as in the first part of the experiment. This means that a participant who had role A in the first part of the experiment will also have role A in this part of the experiment; a participant who had role X in the first part of the experiment will also have role X in this part of the experiment, and a participant who had role Y in the first part of the experiment will also have role Y in this part of the experiment. Additionally, the non-profit organization "Deutsche Welthungerhilfe e.V." will be relevant in this part of the experiment. This organization is active in the field of development organization and is officially certified by the German Central Institute for Social Issues.

In this part of the experiment, player **X** receives an endowment of **1000 Taler**. The player decides how many of these 1000 Taler to send to the non-profit institution (any full number between 0 and 1000). Each Taler sent is credited to the non-profit institution.

Player X can make the decision on how many Taler to send **dependent on every possible Taler distribution chosen by player A in the first part of the experiment**. The actual distribution from the first part of the experiment is told to player **X** only after the experiment has ended. However, X has to decide in **this** part of the experiment how many Taler he or she wishes to send **for every possible distribution**.

In this part of the experiment, player **X** must therefore fill in the following decision table:



Participant **X** may enter any full number between 0 and 1000 in every line. **The only line that becomes payoff-relevant is the one that corresponds to the distribution actually chosen in the first part of the experiment.** The decisions in the other lines do not influence the participants' payments. The lines do not have to add up exactly to 1000 either, as the only decision-relevant line is the one that corresponds to the actual situation.

As participant **X**, please note that at the time of filling in the table you do not yet know which allocation decision has been made in the first part of the experiment. **You therefore have to consider your decision very carefully in every line, because any of the lines could become payoff-relevant for you.**

After filling in the table, participants X and Y will be given a brief questionnaire.

Player **Y** and player **A** make no decisions in this part of the experiment and receive **no earnings**.

The **earnings** from this experiment add up as follows:

Participant **X**	1.000 Taler – Taler sent by X to the non-profit institution (in the payoff-relevant situation)

After filling in the questionnaire, the second part of the experiment ends.

Only after the experiment has ended are participants given information on the **actual distribution decision** of player **A** in the first part of the experiment and on the sum corresponding to this distribution, which player **X** has sent to the non-profit institution in this part of the experiment.

It is impossible for you to lose, in a later part of the experiment, the earnings you have accumulated in the first and second part of the experiment.

The Taler sent to the non-profit institution will be converted into Euro at the rate stated above and will be transferred by the experimenters after the end of all experimental sessions. After the end of today's experiments, a web address will be shown to all participants, where proof of the total amount donated in all experimental sessions will be displayed from 17 March 2013 onwards.

You will now be shown some control questions on your screen. After you have answered these questions correctly, the second part of the experiment will begin.

VIII.3 Tables

Table 3

Pairwise treatment comparison of transfers in the dictator game conditional on every possible allocation

Mann-Whitney tests: p-values, |z| in parentheses				
Possible allocation		*baseline* vs. *narrow*	*baseline* vs. *broad*	*Narrow* vs. *broad*
X: 2000	Y: 0	0.0757, (1.776)	0.5432, (0.608)	0.3530, (0.929)
X: 1900	Y: 100	0.0374, (2.081)	0.4766, (0.712)	0.2340, (1.190)
X: 1800	Y: 200	0.0311, (2.156)	0.2190, (1.229)	0.4235, (0.800)
X: 1700	Y: 300	0.0340, (2.120)	0.1382, (1.483)	0.5224, (0.640)
X: 1600	Y: 400	0.0337, (2.123)	0.0501, (1.959)	0.7716, (0.290)
X: 1500	Y: 500	0.0484, (1.974)	0.0753, (1.779)	0.6913, (0.397)
X: 1400	Y: 600	0.0289, (2.185)	0.0301, (2.168)	0.7254, (0.351)
X: 1300	Y: 700	0.0284, (2.191)	0.0130, (2.484)	0.9573, (0.054)
X: 1200	Y: 800	0.0108, (2.549)	0.0059, (2.753)	0.9390, (0.077)
X: 1100	Y: 900	0.0108, (2.549)	0.0133, (2.475)	0.7529, (0.315)
X: 1000	Y: 1000	0.0021, (3.080)	0.0096, (2.590)	0.4416, (0.769)
X: 900	Y: 1100	0.0566, (1.906)	0.0133, (2.476)	0.6550, (0.447)
X: 800	Y: 1200	0.0592, (1.887)	0.0140, (2.458)	0.6733, (0.422)
X: 700	Y: 1300	0.0592, (1.887)	0.0175, (2.376)	0.8068, (0.245)
X: 600	Y: 1400	0.0661, (1.838)	0.0554, (1.916)	0.9313, (0.086)
X: 500	Y: 1500	0.0592, (1.887)	0.0883, (1.705)	0.7865, (0.271)
X: 400	Y: 1600	0.1126, (1.587)	0.1534, (1.428)	0.8221, (0.225)
X: 300	Y: 1700	0.1270, (1.526)	0.2304, (1.199)	0.7215, (0.356)
X: 200	Y: 1800	0.1374, (1.486)	0.2548, (1.139)	0.6809, (0.411)
X: 100	Y: 1900	0.2214, (1.223)	0.3631, (0.909)	0.7760, (0.284)
X: 0	Y: 2000	0.6928, (0.395)	0.7803, (0.279)	0.8984, (0.128)

Table 4

Pairwise treatment comparison of transfers in the dictator game conditional on every possible allocation – uninvolved treatments

Mann-Whitney tests: p-values, |z| in parentheses		
Possible allocation		*Baseline-uninvolved* vs. *narrow-uninvolved*
X: 2000	Y: 0	0.7252, (0.351)
X: 1900	Y: 100	0.5268, (0.633)
X: 1800	Y: 200	0.5566, (0.588)
X: 1700	Y: 300	0.5365, (0.618)
X: 1600	Y: 400	0.5616, (0.580)
X: 1500	Y: 500	0.7367, (0.336)
X: 1400	Y: 600	0.9086, (0.115)
X: 1300	Y: 700	0.8363, (0.207)
X: 1200	Y: 800	0.8303, (0.214)
X: 1100	Y: 900	0.8124, (0.237)
X: 1000	Y: 1000	0.8532, (0.185)
X: 900	Y: 1100	0.7334, (0.341)
X: 800	Y: 1200	0.6400, (0.468)
X: 700	Y: 1300	0.5643, (0.577)
X: 600	Y: 1400	0.9937, (0.008)
X: 500	Y: 1500	0.7945, (0.260)
X: 400	Y: 1600	0.9608, (0.049)
X: 300	Y: 1700	0.9151, (0.107)
X: 200	Y: 1800	0.9346, (0.082)
X: 100	Y: 1900	0.5816, (0.551)
X: 0	Y: 2000	0.4427, (0.768)

Table 5

Treatment effects on transfers – comparison of baseline-uninvolved and narrow voice-uninvolved

Random effects Tobit regression

Dependent variable: transfers in the dictator game

	Model 1	Model 2	Model 3	Model 4	Model 5
Narrow-uninvolved	22.56	41.65	74.68	76.93	88.20
	(111.82)	(106.62)	(103.73)	(97.41)	(131.15)
Fair allocation		-0.36**	0.02	0.18	0.16
		(0.18)	(0.25)	(0.24)	(0.24)
Expectation			-0.53**	-0.53**	-0.54**
			(0.25)	(0.24)	(0.24)
Social Value Orientation				8.23**	7.35**
				(3.19)	(3.17)
Possible allocation part 1	0.22***	0.22***	0.22***	0.22***	0.22***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Narrow- uninvolved*Gender					-12.86
					(191.85)
Gender					-133.72
					(135.43)
Constant	-174.91**	335.22	448.71*	113.91	228.39
	(79.43)	(259.31)	(254.55)	(271.27)	(278.54)
N	1239	1239	1239	1239	1239
P model	<.001	<.001	<.001	<.001	<.001
Wald Chi2	513.11	516.63	520.02	525.61	527.42

Random effects Tobit regression. Standard errors are presented in parentheses. The *narrow-uninvolved* dummy equals 1 for all observations of the *narrow voice-uninvolved* treatment, *fair allocation* controls for the differences in the allocation players X consider as fair (from 0 ECU for X to 2000 ECU for X), *expectation* controls for the differences in players X's expectation about the actual allocation by the impartial decision makers (from 0 ECU for X to 2000 ECU for X), *social value orientation score* controls for differences in players' social value orientation, the *gender* dummy equals 1 for male players, *possible allocation part 1* controls for possible allocations that can be implemented by the impartial decision makers (from 0 ECU for X to 2000 ECU for X). Significance at the 10%, 5% and 1% level is denoted by *, ** and ***, respectively. Left-censored = 432; right-censored = 68.

Table 6

Expectations and perceived fairness in baseline- and voice treatments

		baseline	*narrow*	*broad*	Mann-Whitney tests *baseline* vs. n*arrow*	Mann-Whitney tests *baseline* vs. *broad*
		Mean (sd)	Mean (sd)	Mean (sd)	p-values (|z|)	p-values (|z|)
Expecations	Perceived influence by players X	1.90 (2.55)	5.55 (3.56)	4.97 (3.19)	0.0001 (3.989)	0.0003 (3.642)
	Expectation of players X concerning allocation in part 1	1334 (359)	1228 (271)	1230 (261)	0.0985 (1.652)	0.1103 (1.597)
Perceived fairness by players X	Fairness process general (1)	5.24 (3.31)	5.83 (3.14)	7.10 (3.06)	0.4952 (0.682)	0.0250 (2.241)
	Fairness process for X (2)	5.72 (3.45)	7.17 (2.89)	7.27 (2.98)	0.1267 (1.527)	0.092 (1.685)
	Fairness of treatment (3)	5.21 (3.50)	7.00 (2.69)	6.80 (3.18)	0.0504 (1.957)	0.0749 (1.781)
	Fairness of outcome (4)	5.34 (3.12)	6.76 (3.49)	7.27 (3.15)	0.0638 (1.853)	0.0136 (2.467)
	Acceptance of decision (5)	6.41 (3.04)	7.52 (3.62)	7.37 (3.48)	0.0441 (2.013)	0.1074 (1.610)
	Fairness process for Y	4.52 (3.38)	4.93 (3.29)	5.30 (3.43)	0.6056 (0.516)	0.3249 (0.984)

Perceived influence as well as all *perceived fairness* ratings on a Likert scale from 0 ("not at all") to 10 ("completely"); *expectation* in ECU.

Table 7

Expectations and perceived fairness in baseline-uninvolved and narrow voice-uninvolved

		baseline-uninvolved	*narrow-uninvolved*	Mann-Whitney tests *baseline-uninvolved* vs. n*arrow-uninvolved*
		Mean (sd)	Mean (sd)	p-values (|z|)
Expecations	Perceived influence by players X	1.27 (2.53)	4.28 (3.52)	0.0003 (3.640)
	Expectation of players X concerning allocation in part 1	1223 (310)	1341 (298)	0.117 (1.568)
Perceived fairness by players X	Fairness process general (1)	6.47 (2.98)	5.62 (3.19)	0.3158 (1.00)
	Fairness process for X (2)	5.90 (3.26)	6.45 (3.77)	0.3771 (0.883)
	Fairness of treatment (3)	6.63 (2.89)	6.07 (3.67)	0.7303 (0.345)
	Fairness of outcome (4)	6.60 (3.40)	6.00 (3.99)	0.4675 (0.727)
	Acceptance of decision (5)	7.30 (3.37)	6.72 (4.01)	0.5849 (0.546)
	Fairness process for Y	6.00 (3.01)	6.07 (2.91)	0.9817 (0.023)

Perceived influence as well as all *perceived fairness* ratings on a Likert scale from 0 ("not at all") to 10 ("completely"); *expectation* in ECU.

VIII.4 List of non-incentivized questions to players X

Directly after the dictator game for every possible allocation, separately:

"Please tell us know how happy you are with the respective possible distributions. In case the role A participant has chosen the following distribution, you receive 0 ECU, Y receives 2000 ECU, are you completely happy / not at all happy?"

At the end of the experiment:

"What influence do you think you had on participant A's decision?" (Question on influence in section IV.3)

"In your opinion, how fair is the way in which the decision was made?" (Question 1 in section IV.5)

"How fair do you consider the process from the point-of-view of participant X?" (Question 2 in section IV.5)

"How fairly do you consider yourself treated in the process leading to the decision?" (Question 3 in section IV.5)

"In your opinion, how fair is the distribution, chosen by participant A, between yourself and participant Y?" (Question 4 in section IV.5)

"To what degree do you accept the distribution, chosen by participant A, between yourself and participant Y?" (Question 5 in section IV.5)

"How fair do you consider the process from the point-of-view of a participant Y?" (Question in section IV.5)

"In your opinion, how legitimate is participant A's decision?"

"In your opinion, how legitimate is the fact that the participant decided on the ECU distribution?"

Baseline, only: "How important would it have been to you to have the chance to send participant A a message in which you state the distribution you would have considered as fair?"

Narrow and *broad voice* treatments, only: "How important was it to you to have the chance to send participant A a message in which you state the distribution you would have considered as fair?"

Baseline and narrow voice treatment, only*: "*How important would it have been to you to have the chance to send participant A a message in the guise of a freely formulated text?"

Broad voice treatment, only: "How important was it to you to have the chance to send participant A a message in the guise of a freely formulated text?"

Questions on subjects'general obedience to the law according to Tyler 2006, p. 46.

Questions on subjects' reciprocity taken from the German Socio-Economic Panel Study (SOEP)

Socio-demographics (age, gender, no. of siblings, experience in experiments, field of study, duration of study, native language, latest school grade in mathematics, currently employed for more than 10 hours per week, monthly income)

Questions to be answered free form (e.g., aim of the experiment, something subjects want to tell to the experimenter)