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Sven Fischer
Werner Güth

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Friedrich Schiller University Jena
Carl-Zeiss-Str. 3
D-07743 Jena
www.uni-jena.de

Max Planck Institute of Economics
Kahlaische Str. 10
D-07745 Jena
www.econ.mpg.de

Effects of exclusion on social preferences

Sven Fischer*and Werner Güth†

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In three party ultimatum games the proposer can first decide whether to exclude one responder, what increases the available pie. The experiments control for intentionality of exclusion and veto power of the third party. We do not find evidence for indirect reciprocity of the remaining responder after the exclusion of the other. Similarly, not excluding the second responder is only insignificantly reciprocated by it. Overall, we find little evidence that intentional exclusion has substantial effects on behavior.
Keywords: Exclusion, bargaining, ultimatum game, social preferences, experiment
[*PsychINFO*: 3000, 3600, 3660] [*JEL*: C91, J52]

1. Introduction

In many situations of human interaction, one of three or more parties may decide to exclude another from a jointly profitable endeavor. For example, two children may decide to exclude a third from their play. Similarly, business partners who in the past used to work together may decide to exclude some former partner(s). Assuming other regarding concerns, a robust finding in experimental psychology and economics, the decision to exclude a third party clearly should affect behavior of the remaining ones. If, on the other hand, despite strong incentives, a party is not excluded, this should matter as well.

In labor relations, exclusion is an important aspect. Especially multinational firms are frequently restructured and relocated what often renders part of the workforce redundant. Consequently, wage negotiations with unions are frequently held behind the backdrop of layoffs either in other parts of the corporation or of less organized parts of the workforce within the same unit. Furthermore, such layoffs may occur at times when the corporation overall makes huge profits. Judging by statements voiced by labour

*Corresponding author: Max Planck Institute for Research on Collective Goods, Kurt-Schumacher-Str. 10, D-53113 Bonn, Germany; fischer@coll.mpg.de; Tel: +49(0)228-91416-53
†Max Planck Institute of Economics, Kahlaische Str. 10, 07745 Jena, Germany; gueth@econ.mpg.de; Tel: +49(0)3641-686-621

representatives, such layoffs affect wage negotiations even if they concern more or less unrelated parts of the workforce.

One example is the restructuring initiative of *Deutsche Bank AG* in early 2005. *Deutsche Bank* planned to fire about 6500 employees worldwide despite a rate of return on equity[1] of 16.2% in the same year (see Frankfurter Allgemeine Zeitung 2005 and Deutsche Bank Aktiengesellschaft 2006). Although *Deutsche Bank*'s net labor force was reduced much less and increased again in 2006, figures show that profitability increased to 19.5% in 2006 (see Deutsche Bank Aktiengesellschaft 2007).

Several representatives of the German public, especially unions, reacted with outrage to the downsizing decision by *Deutsche Bank*. But how does it actually affect employer–employee relations when, for the sake of profitability, part of the labor force is dismissed? International justice research (see, for example, Kahneman et al., 1986) established that layoffs are seen as unfair if they are not economically necessary to prevent bankruptcy (see, for example, Charness and Levine, 2000; Gerlach et al., 2006). Surveys also indicate that part of the remaining workforce reacts with conflict-seeking behavior, sick leaves, and even sabotage. In the context of the *Deutsche Bank* example this raises the question how behavior of the (remaining) workforce is affected by downsizing (threats).

In a set of experiments we explore how the decision to exclude a third party (or not to exclude despite its profitability) affects behavior. By modifying the three person ultimatum game we allow for exclusion of one party by the proposer. Exclusion increases the total available pie and renders it profitable. To control for intentionality of exclusion, we contrast this (baseline) game with treatments in which a random draw excludes the third party with a probability equal to the observed frequency in the baseline treatment. We, furthermore, vary veto power in case of no exclusion: in one set of experiments there are two responders with veto power, in a second set only the party that cannot be excluded can veto. Furthermore, we contrast behavior to results from a standard ultimatum game with equivalent parameters.

Of course, the ultimatum game does not mimic a particular real world interaction. It is rather a workhorse allowing us to measures individual reactions and preferences of responders who are not troubled by strategic uncertainty.

While in one experiment there are reactions to exclusion, they are rather mild and difficult to interpret. Overall we find no evidence for indirect reciprocity in the sense that the remaining responder tries to punish the proposer after excluding the third party. Quite to the contrary, the proposer has little to fear from excluding a responder. Still, if the third party is not excluded, the other responder reveals consideration for the third party's payoff by rejecting 0-offers for the third party. If, on the other hand, the proposer decides not to exclude, he only gains little. While the party, which he could have excluded, lowers its acceptance threshold, this effect is only small and insignificant.

In a related study Güth and Paul (2011) capture the downsizing example more realistically by a "one-principal and two-agents"-framework where the principal could get rid of the less productive agent. Treatments differed in the profitability of downsizing as well

[1] We are, of course, aware of the fact that this high rentability is partly due to the low equity ratio in the banking industry.

as in whether later downsizing could (not) be anticipated by the agents. One surprising result is that downsizing does not depend on its profitability and that the employee, who cannot be fired, does not react much to the threat or fact of downsizing. This seems to confirm our finding that there is little solidarity in the labor force what questions some of the arguments used by trade unionists when trying to discourage downsizing attempts of employers.

More generally, downsizing is a form of ostracism (see, for instance Masclet, 2003; Cinyabuguma et al., 2005; Kerr et al., 2009; Maier-Rigaud et al., 2010). What is, however special is that ostracism is usually seen as a form of punishing norm deviators whereas downsizing is just an attempt to improve a firm's profitability and not at all a sanctioning of underperforming agents. The exclusion of innocent parties from payments is, however, also true for experimental studies where one party dictatorially decides whether or not another party keeps or obtains its payment (see, for example, Brennan et al., 2008). Here the major finding is that, in spite of some other regarding concerns, most participants are very much self-centered in that they are bothered by own risk or delay but hardly ever by similar complexities of other's payoffs.

The remainder of this paper is organized as follows: We first introduce and discuss the model in section 2 and derive behavioral hypotheses in section 3. Section 4 describes the experimental procedure. The experimental findings are reported in section 5. Section 6 concludes.

2. The Model

We analyse behavior in a modified three party ultimatum game with one proposer X and two responders Y and Z. The game tree of treatment $XV2$, our baseline treatment, is plotted in Figure 1. In the first stage the proposer decides whether to exclude Z what reduces him to a base payment of u. After (not) excluding Z the proposer plays an ultimatum game with the remaining responder(s) where the available "pie", a divisible monetary amount, is $p > 0$ in case of not firing Z and rp with $r > 1$ in case of firing Z:[2] proposer X first chooses how much to offer the remaining responder(s). Here y (z) with $y + z \leq p$ or $y \leq rp$ indicates her offer to Y (Z, if applicable) about which Y and (if applicable) Z are then informed about. The proposer is not confined to identical offers to both responders when Z is not excluded. If Z is excluded, only Y decides whether to accept or reject the offer y. With respect to veto power in the subgame after *keep*, we compare two different treatments. In the baseline treatments V2 of Figure 1, both Y and Z can veto the offer (y,z), in treatments V1 only Y can veto. Thus, we vary the degree of involvement of Z from a mere dummy in $V1$ to a strategically involved party in $V2$.

Clearly, this one-shot bargaining model cannot fully capture real world bargaining. However, it provides an easily understood scenario and allows to elicit acceptance thresholds of responders free of any strategic considerations.[3] Our 2×2 factorial design has

[2]Because of $r > 1$ and $u \ll p/3$, firing increases the total available pie but leaves Z much less than an equal share of p.

[3]Game theoretically this means to experimentally implement the normal form of games as far as respon-

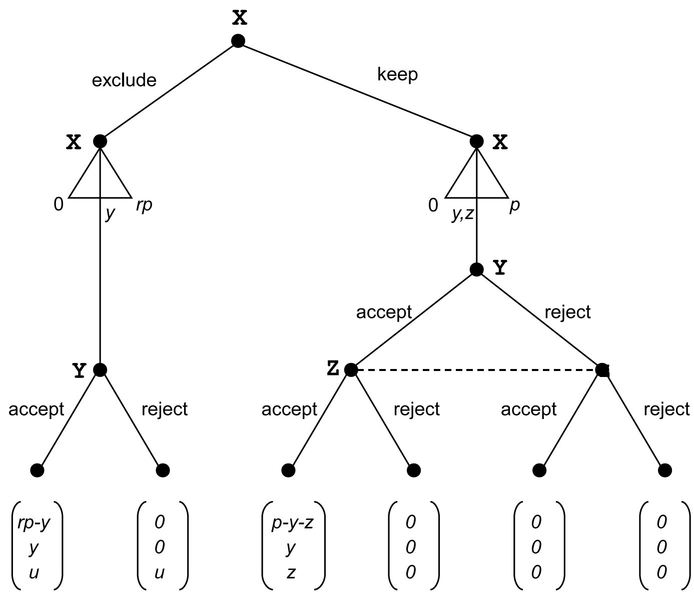

Figure 1: Game Tree Treatment XV2

the following two dimensions: (no) intentionality of the decision to exclude Z and (no) veto power of Z. Intentional firing (or keeping) may affect responder behavior. We are especially interested in how responders react to the intentional decision to exclude or keep Z. In all $X\star\star$ treatments the decision to exclude Z was made by a subject in role X. In $R\star\star$ treatments this choice by X was replaced by a random draw excluding Z with a commonly known probability equal to the observed frequency in the equivalent $X\star\star$ treatment.

In treatments $XV2$ and $RV2$ both responders are endowed with veto power in the subgame after *keep*. So we can measure Z's reaction to not being excluded. However, if Z has veto power, then not excluding him results in greater strategic uncertainty of the proposer. Thus, excluding Z may be more acceptable for Y in $V2$ which is why we introduced treatments $V1$. An overview of the four main treatments is given in Table 1.

Although there exist ample data on ultimatum behavior, we also ran a control treatment CON as a simple two person ultimatum game with the same procedure and

der behavior is concerned. For proposers we do not use the strategy method to avoid counterfactual considerations of proposer X (like having to choose y only in spite of playing *keep* and (y, z) in spite of *exclude*).

Table 1: Main Treatments

		veto power	
		Y & Z	Y only
intentionality	X decides	$XV2$	$XV1$
	random draw	$RV2$	$RV1$

parametrization as in the subgame after *exclude*. By comparing Y's acceptance thresholds in the subgame after *exclude* in $XV2$ and $XV1$ with the acceptance thresholds in CON, we can directly measure effects of exclusion.

3. Benchmark solutions and behavioral hypotheses

We now turn to a brief game theoretic analysis of our games which, however, can hardly inform us about how subjects actually behave. Like most bargaining games, our continuous game has infinitely many equilibria of which only one is sequentially rational (Selten, 1975; Kreps and Wilson, 1982). As a first benchmark solution, we consider perfect equilibria assuming opportunistic agents who are rational and know that others are rational, too. A selfish and rational responder accepts any offer yielding at least his conflict payoff. As $r > 1$ the proposer will exclude Z what allows him to distribute $rp > p$ between himself and Y. Thus, the only perfect equilibrium is that X excludes Z and offers minimal amounts to Y alone or Y and Z (in *keep*), and responders accept any (positive) offer.

In standard ultimatum games[4] offers below one third (and sometimes even higher ones) are frequently rejected and significant offers are made with the modal offer usually being 50% of the available pie. It has been widely argued that this behavior reflects equity preferences.[5] Existing studies on three party ultimatum game behavior focus on versions of the game where only one responder has veto power. The third subject, the so-called dummy, usually receives small amounts and responders with veto power are willing to accept very small offers to Z.[6] We are not aware of a three person ultimatum experiment with two responders independently exercising veto power.

But what about behavioral models? With respect to our main research question, we turn our attention to reciprocity although in a rich model like ours its likely effects are far from obvious. Actually, the ambiguity and flexibility of reciprocity concerns in our scenario may question rather than support the way how reciprocity theory has

[4]For a summary see, for example, Camerer (2003) or Holt (2007).

[5]For a discussion see, for example, Charness and Rabin (2002).

[6]Kagel and Wolfe (2001) find that acceptance only mildly and non-monotonically reacts to the size of the offer to the third party. This, however, may be due to the random veto power in their experimental design creating ex ante procedural equality among the receivers. Güth and van Damme (1998) however find that offers to Z crucially depend on whether Y is informed about them (see also Güth et al., 2007).

been formalized. According to theories of reciprocal behavior, expected or observable actions are evaluated as "kind" or "unkind" and reciprocated accordingly.[7] Besides such direct reciprocity one also observes indirect reciprocal behavior. Here a reciprocal act is directed towards a party in response to a "kind" or "unkind" act of this party towards a third one.[8] Stanca (2009) gives a good overview of experimental results on indirect reciprocity. Using the same definitions, we argue that strong indirect social and strong direct reciprocity is influencing decisions in our games. In our one shot experiments reciprocity can only be "strong" according to Stanca's definition as it can only be due to intrinsic motivation and not because of long run strategic considerations like reputation building as in repeated interactions.

Acceptance thresholds in the "*exclude* subgame"

Let us first look at responder Y's behavior in the "*exclude* subgame" as influenced by indirect reciprocity due to X's exclusion of Z. The only way of sanctioning X is by demanding more of the available pie, i.e., risking conflict. But what about direct reciprocity? Excluding Z leaves more of the pie for both X and Y and, thus, can be judged kind towards Y. However, this reasoning is incomplete. Suppose Y actually interprets exclusion of Z as kind towards himself. He could reciprocate by conceding more to X if exclusion was intentional rather than random. However, this contradicts the interpretation of the intentions behind the exclusion: the reciprocal act would leave X better off than in the absence of reciprocity, thus rendering the exclusion of Z a selfish act by X. Due to this ambiguity of reciprocity concerns, whether an offer in subgame *exclude* is kind or unkind may not depend on whether the decision to exclude Z came from X or nature. In our view, if we compare Y's acceptance thresholds between treatments $XV1$ and $RV1$, the effect of indirect reciprocity is more obvious, what explains our

Hypothesis 1 *Responder Y reacts to X's intentional exclusion of Z with a larger acceptance threshold in treatment $XV1$ than in $RV1$.*

With respect to ⋆V2 treatments,another complication has been pointed out by one of our anonymous referees. We mainly considered $\star V2$ treatments to measure Z's reactions. However, Z's veto power introduces additional strategic uncertainty that is eliminated if Z is excluded. Excluding Z may, thus, be judged a kind act towards Y - an act that is kind irrespective of the later offer. Contrary to our previous argument, exclusion of Z can therefore bee interpreted to be 'kind' (or 'good' or 'reasonable') what Y can reciprocate by conceding more to X. Thus, responder Y would directly reciprocate X's decision to *keep* or *exclude* Z in treatment $XV2$ but not in treatments $XV1$. In case of the random exclusion in $RV2$, however, there is no room for reciprocity:

Hypothesis 2 *Acceptance thresholds of responder Y in subgame exclude are smaller in $XV2$ than in $XV1$ and $RV2$.*

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[7] See, for example, Rabin (1993) for normal form games and Dufwenberg and Kirchsteiger (2004) for extensive games.

[8] See, for example, Seinen and Schram (2006) or Greiner and Levati (2005)

Responder Y's decision task in treatment CON and in the subgame after *exclude* in treatments $RV2$ and $RV2$ is pretty identical as far as reciprocity is concerned:

Hypothesis 3 *Responder Y's acceptance thresholds are the same in CON and in the exclude subgames of treatments $RV2$ and $RV1$.*

Acceptance thresholds in the *keep* subgame

Let us now turn to the subgame after *keep* where Z's behavior may reflect direct reciprocity. Clearly, keeping Z is kind towards him. Not only because he is likely to receive more but also because it gives him veto power in $XV2$ which in itself may be valuable. Obviously, this act can only be kind and not driven by selfishness of X. Direct reciprocity, thus, predicts that if the decision to *keep* Z is intentional, Z reciprocates by accepting less:

Hypothesis 4 *Acceptance thresholds of Z players are smaller in $XV2$ than in $RV2$.*

But what about Y's threshold. Indirect reciprocity predicts that Y will be more willing to accept lower offers if the decision to *keep* Z is intentional. At the same time *keep* may be bad news for Y himself. Still, the decision to *keep* Z can only be driven by the desire to leave Z with more than u and, thus, can not really be interpreted unkind towards Y.

Hypothesis 5 *In subgame keep, responders Y accept lower offers to themselves in treatment $XV1$ than in $RV1$.*

Again, in the $\star V2$ treatments the strategic uncertainty associated with Z's veto power may induce different reciprocal reactions:

Hypothesis 6 *Acceptance thresholds of responder Y in the keep subgame are higher in $XV2$ than in $XV1$, whereas in treatments $RV2$ and $RV1$ they do not differ.*

Inequality Aversion

Inequality aversion argues that people do not only care about their own payoff but also how it compares to those of others (see, for example, Bolton and Ockenfels, 2000; Fehr and Schmidt, 1999). Unfortunately, equity preferences can neither account for intentions, as measured with our comparison of X vs. R treatments, nor for differences in veto power. They first and foremost predict that inequality averse proposers will be less inclined to exclude Z and that responders' thresholds are likely to be positive and sensitive to how (un)equal the offered payoffs are. Exact predictions crucially depend on the formalization and the distribution of inequality aversion in the population.[9]

[9]For the exact parameterization of our experiment one can easily apply the Fehr and Schmidt (1999) model to our experimental game, using the distribution of aversion parameters suggested by them as "*consistent with the large experimental evidence we have on the ultimatum game*" (Fehr and Schmidt, 1999, , p. 843). See , however, Fehr and Schmidt (2010); Binmore and Shaked (2010).

Models of inequality aversion predict that, if in subgame *exclude* Y cares about Z's payoff (and how his own compares to it) his acceptance threshold is smaller than in CON. (See the formal derivation based on F&S in A).

Hypothesis 7 *Acceptance thresholds of responder Y in subgame exclude are smaller than those in CON.*

Note that this hypothesis contradicts hypothesis 3.

Veto Power

Finally we take a closer look at potential behavioral effects of changes in veto power. If Z has veto power, Y does not need to 'speak' for Z:

Hypothesis 8 *Acceptance behaviour of Y in the keep subgame reflects more concern for Z's payoffs in treatments $XV1$ and $RV1$ than in $XV2$ and $RV2$.*

4. Experimental Design

We ran the five different treatment conditions in a between subjects design, i.e. each subject participated in only one treatment. In the random treatments $RV1$ and $RV2$ the probability of being fired was 85 and 67.5%, respectively. In order to obtain sufficient information from responders, we employed the strategy method.[10] By using the positional order protocol, we nevertheless tried to guarantee a "hot" decision environment that we strengthened by informing responders that the proposer had just made her decision(s). In more detail, treatment $XV2$ proceeded as follows:

1. X decides whether to '*keep*' or '*exclude*' responder Z.

2. X decides how much she offers to her remaining responders[11] where the set of possible offers depends on her first decision. Following '*keep*' she can choose among $(y, z) \in \{0, 2, 4, 6, 8\}^2$ for Y and Z, and after '*exclude*' she can choose among $y \in \{0, 2, 4, 6, 8, 10, 12\}$. The monetary pie is set to $p = 18$ and $r = 10/9$, thus, $rp = 20$.

3. Both Y and Z are informed that "*X has already decided whether to exclude Z*" and that "*X has already submitted her wage offer(s). However, you will not be informed about her decisions before the end of this round.*"[12]

   - Y is then asked to state for every possible offer y (for the case that Z is excluded) and combination of (y, z) offers (for the case that Z is not excluded) whether he would accept or reject it.

[10] In play method we would not obtain enough data points for many - often unreached - histories even at considerably higher costs. In some treatments, for example, proposers decided to keep Z in less than 15% of the cases. Also note that, for example, Oxoby and McLeish (2004) find no difference between strategy and play method in Ultimatum games. Also, see the survey by Brandts and Charness (2011).

[11] Note again, that we do not use the strategy method for proposers to avoid counterfactual considerations.

[12] A translation of the instructions and the original version in German are available upon request.

- Similarly, Z is asked the same questions, of course only when he is not excluded.

4. Decisions are matched and all players are informed about all relevant decisions and all resulting payoffs. In case Z is excluded, he obtains $u = 1$.

Treatments $\star V1$ differed from $\star V2$ only in subgame *keep* where Z had no veto power.[13] Finally, in treatments $RV2$ and $RV1$ the decision to exclude was made by a random draw with known probability equal to the excluding frequency in $XV1$ and $XV2$, respectively.

We limited the sets of possible offers to five and seven values, respectively to reduce the length of the strategy, the responders have to submit. The chosen sets include the possibility for equal offers (i.e., (6,6,6) and (10,10)) to render unequal offers less acceptable.[14] Furthermore, the sets include offers giving more to the responder(s) than to the proposer. While such offers are rare, they are nevertheless possible and should be included (see, for example, Güth et al., 2007, for rejections of too generous offers).

With respect to our hypothesis 7, we need to check wether despite the reduced strategy set we are likely to observe the predicted differences. In appendix A.2 we use widely adopted parameters of the F&S model[15] and apply F&S preferences to our experiment. Results indicate that we are likely to observe higher thresholds of responder Y in CON than in subgame *exclude*.

With respect to inequality aversion we are also interested to check, whether with our parametrization we are likely to observe concerns for others who receive less. More specifically, will we observe cases where Y rejects an offer because it leaves considerably less for X - or similarly, because it offers too little to Z? When applying the same F&S model version to subgame *keep* (see, appendix A.2) one predicts:

Hypothesis 9 *Acceptance of offers by responder Y in subgame keep reflects concern for Z but not for X.*

To see whether experience influences behavior, subjects were rematched in a perfect stranger design and played the same game once again in the same role.[16] Only one randomly drawn round was paid, where points earned in that period were converted to € at 1 *point* = €1.50.

Instructions relied on a neutral frame. The instructions, for example, did not use the term "exclude". Instead subjects were informed that "*[f]irst, X decides whether Z should have power of decision, i.e., whether the round should already end for Z.*"

To make sure that subjects understood the instructions, and to create common knowledge about it, we invited an excess number of subjects and asked all to answer a set of control questions. Subjects making mistakes were excluded from participation. All subjects received €3 for answering the control questions, no additional show up fee was paid.

[13] To keep Z participants busy we asked them a hypothetical question.
[14] Falk et al. (2000) find that the set of alternatives has crucial influence on behavior. An unequal split is less acceptable if the proposer had the option to offer an equal one.
[15] See, however, the discussion in Binmore and Shaked (2010) and Fehr and Schmidt (2010).
[16] Thus no one interacted with someone twice.

We ran ten sessions, two for each of the five treatments. In each of our four main treatments 60 subjects participated, and in treatment CON a total of 38. For the first round we, thus, have 20 independent group observations per treatment in $XV1$, $RV1$, $XV2$ and $RV2$.

5. Results

Including admission and payment, sessions of treatments $XV2$ and $RV2$ lasted for about 45 minutes, those of CON for about 30 minutes. Average earnings in all treatments except for CON were €12.13 for X, €8.88 for Y, and €4.48 for Z (including the €3 fee).

Throughout the following analysis, significance levels will be set to $\alpha = 10\%$. In $XV2$ ($XV1$), proposers in 85% (67.5%) of all cases excluded Z. According to Fisher exact tests, firing frequencies neither differed significantly between repetitions[17] within each treatment nor between treatments.[18]

5.1. The *exclude* subgame

We first take a look at responder acceptance thresholds in the *exclude* subgame and in treatment *CON* which are shown, combined over the two repetitions, in the histograms of figure 2. We tested for repetition effects by comparing distributions of acceptance thresholds between repetitions within each treatment. Wilcoxon signed rank tests[19] do not find significant differences.[20] Due to the discreteness and skewness of the data, questioning the applicability of the Wilcoxon rank sum test, we furthermore tested whether frequencies of discrete thresholds differ between repetitions using the Fisher exact test, what could also be rejected. The following comparisons between treatments therefore rely on pooled data.

Table 2 reports the results of Wilcoxon rank sum tests[21] (above diagonal), and Fisher exact tests[22] (below diagonal) comparing two treatments at a time, as well as some descriptive statistics (last columns to the right). All comparisons were also made by Kolmogorove Smirnov and t-tests, whose results are qualitatively similar to the reported ones. Acceptance thresholds are generally considerably higher in the two random treatments $RV1$ and $RV2$, and with respect to $XV1$ they are even significantly larger. With hypothesis 1 in mind we summarize:

Observation 1 *Y's acceptance thresholds in the exclude subgame are larger in random treatments than in treatments where the offer is made by the proposer. This effect is significant for $XV1$ and $RV1$ data.*

[17] With $p = .6614$ in $XV2$ and $p = .5006$ in $XV1$.

[18] With $p = .1136$ for the comparison between $XV2$ and $XV1$ with data combined over both repetitions.

[19] The test corrected for ties, using the shift-algorithm by Streitberg and Röhmel (1984).

[20] Wilcoxon signed rank tests comparing acceptance thresholds between the first and second repetition in treatments $XV2$, $RV2$, $XV1$, and $RV1$ with p=.7728, .2031, .9930, and 1.0000, respectively.

[21] Distribution of averages over periods per subject.

[22] We use the frequencies of choices over both rounds, thus treating the two observations of one subject as independent.

With respect to Hypothesis 2 we observe:

Observation 2 *Though insignificant, Y's threshold after Z was excluded is larger in $XV2$ than in $XV1$ but does not differ between $RV2$ and $RV1$.*

Note that, except for equality between $RV2$ and $RV1$, both observations reject our hypotheses, even contradicting their direction. However, Hypothesis 3 is confirmed:

Observation 3 *Acceptance thresholds of responders Y in the exclude subgame of $RV1$ and $RV2$ do not differ significantly from those in CON. Furthermore, they do not differ significantly in size from CON in $XV1$ and $XV2$.*

Note that this contradicts our hypothesis 7 derived from inequality aversion.

Looking at distributions in figure 2 there is a striking difference. The distribution of thresholds in $XV2$ is bimodal what is reflected in significant differences in distributions according to the Fisher exact test (see Table 2). Ansari-Bradeley tests confirm that the dispersion of thresholds in treatment $XV2$ is significantly larger than in any other treatment.[23] No other treatment comparison finds significant differences in dispersion.

Observation 4 *Acceptance thresholds of responders Y in subgame exclude are more dispersed in $XV2$ than in any other treatment. More specifically, in $XV2$ the distribution is bimodal.*

Table 2: Comparisons of Y's acceptance thresholds after *exclude* between treatments

	Treatment								
Treatment	$XV2$	$RV2$	$XV1$	$RV1$	CON		Median	Mean	Var
$XV2$	–	.2956	.2983	.1943	.3506	*Wilcoxon rank sum*	3	4.65	8.49
$RV2$	**.0001**	–	**.0385**	.3840	.1222		6	5.00	4.10
$XV1$	**.0003**	**.0008**	–	**.0333**	.2731		4	3.80	6.32
$RV1$	**.0149**	.1274	**.0626**	–	.1010		6	5.30	6.27
CON	**.0016**	.5799	.1343	.1919	–		4	4.21	5.36
	Fisher exact test								

NOTE: Reported values (other than median, mean and variance) are p-values: Below diagonal from Fisher exact tests; above diagonal from Wilcoxon rank sum tests.

Considering the differences we find in acceptance thresholds, is this also reflected in offers? Figure 3 shows the distribution of offers after exclusion of Y and in treatment CON.[24] Distributions for treatments $XV2$ and $RV2$ are almost identical, and comparing all treatments, medians are always identical and means differ only slightly. This

[23] Comparison with treatments $XV2$ ($p = 8.8e-6$), $XV1$ ($p = .025$), $RV1$ ($p = .007$), and CON ($p = .004$). Qualitatively, the same statements hold when comparing only data from the first or second round.

[24] Again we could not find differences between the first and second period.

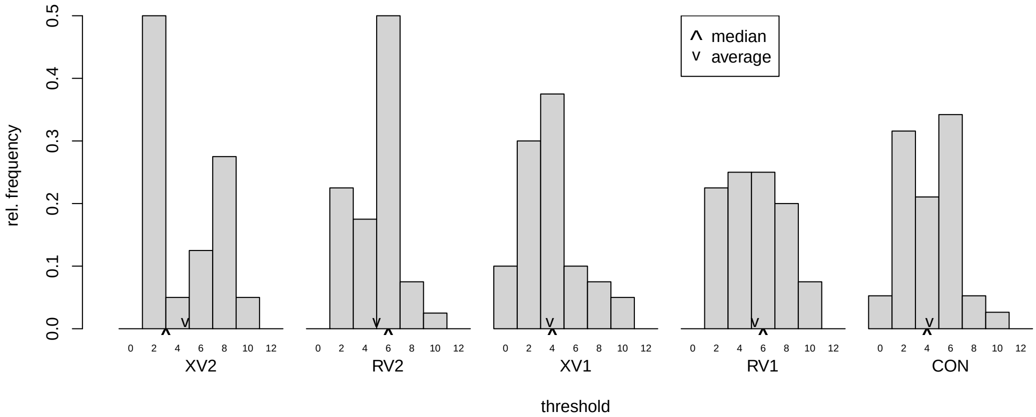

Figure 2: Thresholds Responder Y after *exclude* and in CON

observation is confirmed by Fisher exact tests and Wilcoxon rank sum tests that do not find any significant differences.

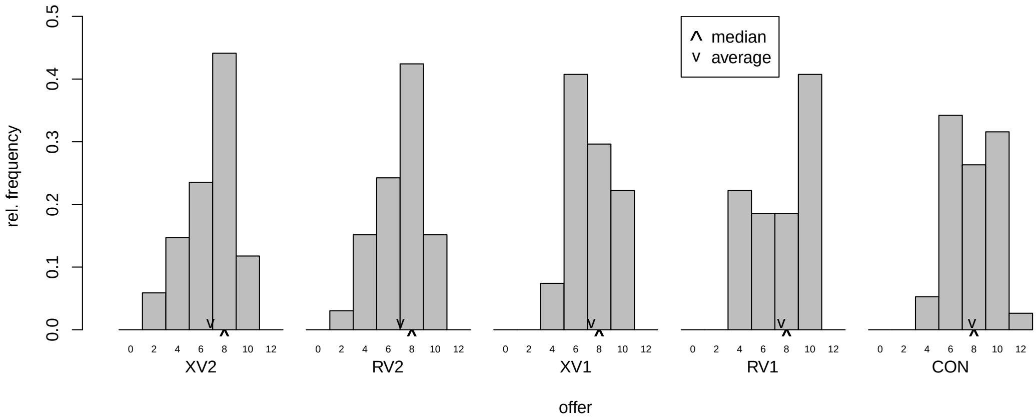

Figure 3: Offers to Y after *exclude* and in CON

Observation 5 *Proposer offers to responder Y after exclude do neither differ between treatments, nor with respect to offers in the control treatment CON.*

5.2. The *keep* subgame

We now turn to responder behavior in the *keep* subgame, where we first look at responder Z in treatments $XV2$ and $RV2$. Ignoring for now the offer to Y, we analyze the smallest offer to Z he is willing to accept. The boxplot of figure 4 shows the distribution of these acceptance thresholds separately for each treatment. The median in XV2 is smaller than in RV2, what supports our hypothesis 4. However, again, differences are not significant - neither in location (Wilcoxon rank sum test: $p = .658$), nor in distribution (Fisher: $p = .4709$).[25] We also ran a panel probit estimation of Z's acceptance of offers depending on the `own` offer, the offer to Y (`other`), and two dummies: `DR` indicating the random treatment and `DP2` for period 2 data, and relevant interaction effects. We report the marginal effects[26] of the estimation results and their bootstrap standard errors (in parentheses) in the first column (H4) of Table 3.[27] While DR has a positive marginal effect on acceptance, it remains insignificant.

Observation 6 *The smallest offer responder Z is willing to accept does not differ between $XV2$ and $RV2$*

How does Y react to the intentional decision not to exclude Z? The boxplots in figure 4(b) show the distributions of the smallest accepted offer to himself (ignoring the offer to Z). The plots indicate significant differences. Indeed, Wilcoxon rank sum test (and t-tests), find that the lowest accepted offers in $XV1$ ($XV2$) are significantly smaller than in $RV1$ ($RV2$).[28] This result is confirmed by model H5 of our Probit estimations in Table 3. Intentionality of the decision to keep Z, significantly ($p = 0.068$) increases the probability that Y accepts an offer by 12.66% points what confirms[29] our hypothesis 5.

Observation 7 *Y's acceptance thresholds are significantly smaller in subgame keep of treatment $XV1$ than $RV1$.*

Does veto power of Z in the $V2$ treatments affect reciprocity of responder Y? Models H6a and H6b in table 3 relate to this hypothesis 6. Both, figure 4 and estimation H6a and H6b show:

Observation 8 *Acceptance thresholds of responder Y in the keep subgame are insignificantly smaller in $XV2$ than in $XV1$. In line with our hypothesis, however, they do not differ significantly between $RV2$ and $RV1$.*

Does refusing veto power to Z increase Y's consideration for Z's payoff (Hypothesis 8)? The estimation H8 in Table 3 used data from all treatments except control CON. The results show that with every additional payoff for Z, the probability of acceptance by Y increases by 1.88% points averaged over all treatments. In treatments where Z has veto power, this insignificantly increases by 0.15% to 2.03%, contradicting our hypothesis.

[25] As there are no differences between periods we, again, combined data from both rounds.

[26] The interaction effects makes it impossible to draw meaningful conclusions from regression coefficients.

[27] We also ran estimations interacting DP2 with other regressors. As results did not differ, we report the results of the scarcer and, thus, more efficient models.

[28] As there were no differences between period 1 and period 2 data we combined the data by taking the average for each subject. The respective $p - values$ are .0777 and 0.0389.

[29] In a similar regression, with only 1.33% points the same difference between $XV2$ and $RV2$ turns out to be insignificant.

Table 3: Probit Estimations of Acceptance in *keep*

Model:	H4	H5	H6a	H6b	H6
Treatments:	$XV2$ & $RV2$	$XV1$ & $RV1$	$XV1$ & $XV2$	$RV1$ & $RV2$	$X\star\star$ & $R\star\star$
Role:	Z	Y			
(Intercept)	0.0017	0.0027	0.0021	0.0002	0.0028
	(.0155)	(.0139)	(.0214)	(.0004)	(.0071)
DR	0.0353	-0.1266*			
	(0.0790)	(.0695)			
DV2			-0.0414	0.0716	0.0152
			(.0597)	(.0516)	(.0437)
own	0.0724***	0.0554	0.0917	0.0356*	0.0723***
	(.0261)	(.0445)	(.0569)	(.0183)	(.0166)
other	0.0166**	0.0179***	0.0184***	0.0194***	0.0188***
	(.0074)	(.0046)	(.0045)	(.0054)	(.0031)
DP2	0.0224	-0.0080	0.0162	0.0084	0.0123
	(.0446)	(.0127)	(.0267)	(.0132)	(.0120)
Dran:own	0.0477	-0.0602			
	(0.0483)	(.0845)			
DV2:own			0.0108	0.0150	0.0197
			(.0746)	(.0337)	(.0278)
Dran:other	0.0039	0.0103			
	(.0143)	(.0091)			
DV2:other			0.0113	-0.0072	0.0015
			(.0086)	(.0112)	(.0068)
N(Obs); N(Subj)	2000; 40	2000; 40	2000; 40	2000; 40	4000; 80
log$\mathfrak{L}$; σ^2_{RE}	-548; 1.45	-458;3.27	-505; 2.93	-473; 1.52	-437; 1.59

NOTE: Panel Probit estimations with random effect per subject. Reported values are the average marginal effects in the sample. Standard errors (in parentheses) are the empirical distribution errors from 50 panel bootstrap estimations. Variables: `DR` dummy for treatments with random draw, `DV2`: Dummy for veto power of Z, `own` own payoff, i.e. either y or z, `other`: offer to other responder, `DP2` dummy for period 2 decisions. Significance: $^{***}p \leq .01$, $^{**}p \leq .05$, $^{*}p \leq .10$. The effect reported as (Intercept) is the probability of acceptance of an offer of (0,0) in an $X**$ treatment in period 1.

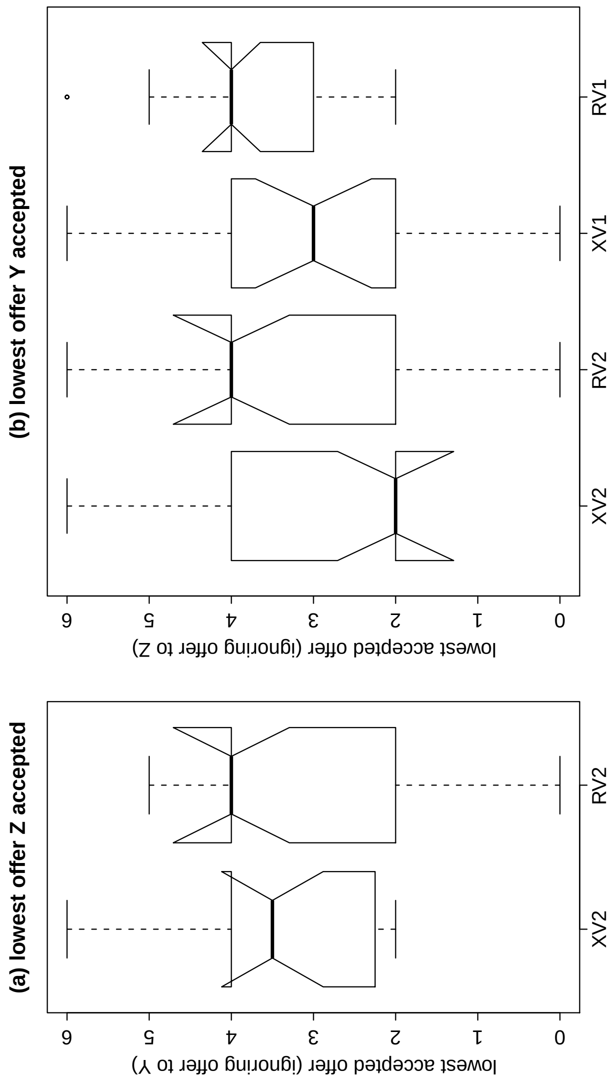

Figure 4: Lowest Accepted Offers in *keep* subgame

`Note:` Notches give an approximate 95% confidence interval for the median. Due to the skewness of the distributions, some notches reach outside hinges.

Observation 9 *Concern for Z, exhibited by Y, does not depend on whether Z has veto power or not.*

5.3. Inequality Aversion in *keep*

Suppose responders are not only concerned about their own payoff but also compare it to those of others. We test for the presence of such preferences in responder Y with a simple Probit estimation (see column 1 in table 4) of acceptance on his own payoff (`own`), the absolute difference to the average payoff in case Y receives less (`disadv`), and

Table 4: Probit: Inequality Aversion of Y

	1	2	3
Subgame(s):	*exclude*	& *fire*	*keep*
(Intercept)	-1.4399***	-0.4728**	-2.5022***
	(.2396)	(0.2264)	(.2067)
own	0.7020***	0.5238***	1.0177***
	(.0302)		(.0342)
disadv	-0.1925***		
	(.0225)		
adv	-0.7258***		
	(.0678)		
YlessX		-0.1333***	
		(.0100)	
YmoreX		-0.0350	
		(.0443)	
YlessZ			-0.0329*
			(.0184)
YmoreZ			-0.3978***
			(.0246)
DP2	0.0978	0.0971	0.1057
	(0.0677)	(.0671)	(.0697)
N(Obs.); N(Subj.)	5120; 80	5120; 80	4000; 80
log$\mathfrak{L}$; σ^2_{RE}	-1166; 2.57	-1178; 2.37	-916.4; 2.48

NOTE: Panel Probit estimations with random effect per subject on data from $XV1$, $RV1$, $XV2$, and $RV2$, Subject Y decisions only. Variables: `own`: offer to Y, `disadv`: difference to average if Y received less, `adv`: difference to average if Y received more, `YlessX`: difference to payoff for X when Y received less, `YlessX`: difference when Y received more, `YlessZ` and `YmoreZ` are defined equivalently, `DP2` dummy for Period 2 decisions. Reported values are the coefficients and standard errors (in parentheses). Significance: $^{***}p \leq .001$, $^{**}p \leq .01$, $^{*}p \leq .05$, $'p \leq .10$.

the absolute difference in case he receives more (`adv`).[30] As both comparison effects are significantly negative, inequality aversion seems confirmed.

However, a closer look at our data indicates that in line with our Hypothesis 9 the effect of advantageous discrimination only holds in comparison to Z's payoff. More specifically, responders Y often reject an offer in *keep* if they receive much more than Z - especially if Z receives nothing - but they hardly ever reject an offer where they receive much more than X. To disentangle comparison effects we ran two additional estimations. Model 2 of table 4 only tests for comparisons with the payoff of the proposer. Here the comparison

[30]Note that due to an identification problem we ca not run a similar regression for Z: For Z we only have data from *keep* (in XV2 and RV2). As the sum of payoffs is always constant in *keep*, only two of the three reactions to motives are identified.

effect only holds if Y receives less but not if he receives more than X. In model 3 where we test for comparison effects with Z only, both effects turn out significant.

Observation 10 *Ceteris paribus responders Y are less likely to accept an offer if they receive less than the average payoff and if they receive much more than Z.*

6. Discussion

In a three party ultimatum game, a proposer can exclude a third party Z before deciding how to distribute a fixed amount of money, where the total available pie increases after exclusion. With this environment, where a selfish proposer has strict incentives to exclude Z, we study especially how socially minded responders react to an intentional exclusion of Z. As a proxy for social preferences, we use responder's acceptance thresholds as these are free of strategic considerations. We control for intentionality by replacing the proposer decision to exclude Z with a random draw.

Our results are very mixed and difficult to interpret. For instance, it is far from obvious what effect intentional exclusion of Z has on Y. The lack of significant differences in acceptance thresholds in $XV2$ and $XV1$, compared to CON, indicates that there are no differences on the aggregate level. On the other hand, the significantly lower thresholds in $XV1$ in comparison to $RV1$ show that intentional exclusion has an effect which, however, is difficult to explain. Positive direct reciprocity to the exclusion of Z appears to be the cause. However, as explained in section 3, the act to exclude Y can only be interpreted as kind towards Y if it is followed by a "generous" offer. Accepting less out of reciprocity contradicts the supposed kindness. We are therefore reluctant to interpret the smaller thresholds in $X\star\star$ treatments in comparison to $R\star\star$ as resulting from direct reciprocity.

An alternative interpretation is that an intentional exclusion of Z signals a particular "tough" type to what some Y react by lowering their acceptance threshold. This effect of yielding to a tough proposer happens and should be identical in $XV1$ and $XV2$. However, we observe on average higher acceptance thresholds in $XV2$ than in $XV1$ and, furthermore, in $XV2$ the distribution is bimodal. We are unable to explain either effect: For $XV2$ we predicted an additional effect of positive direct reciprocity to the elimination of strategic uncertainty what should result in smaller thresholds.

One unequivocal result, however, is the absence of indirect negative reciprocity of Y when reacting to the intentional exclusion of Z. Similarly, we do not find increased consideration for Z by Y in subgame *keep* of the $V1$ treatments. But there is evidence that Y responders actually care for Z's payoff although, to some extent this has no consequences, as it is primarily concerned with rare zero offers to Z. Zero offers that must be unlikely after the decision to *keep* Z. Still, it is puzzling to see that while Y responders do care for Z's payoff, they obviously do not bother if he is excluded.

But what about Z? All we find is a rather weak positive reaction to not being excluded. All these results combined imply for the proposer that excluding Z has little to no negative effects for himself, whereas keeping Z on board has at most minor positive effects. This suggests that in many situations it is cheaper to exclude than hoping to benefit from not doing so. However, further research is necessary to resolve some of the puzzles in our experimental results.

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A. Fehr and Schmidt (1999) Preferences

In this section we apply Fehr and Schmidt (1999) (F&S) preferences to our experimental game. We start with the subgame after *exclude* before we turn to the subgame after *keep*.

A.1. Subgame after *exclude*

In subgame *exclude*, responder Y's F&S valuation of the acceptance of distribution (x,y,u) with $x = p - y$, is defined by:

$$V = y - \frac{\alpha}{2}\left[\max(p-2y,0) + \max(u-y,0)\right] - \frac{\beta}{2}\left[\max(2y-p,0) + \max(y-u,0)\right],$$

where α and β are two aversion parameters with $0 \leq \beta \leq \alpha$ and $\beta < 1$. With respect to the lowest offer y^t that responder Y is willing to accept, which for small u will always satisfy $y^t < p/2$, this reduces to:

$$V = \begin{cases} y^t - \frac{\alpha}{2}(p - 3y^t + u), & \text{for } y^t < u; \\ y^t - \frac{\alpha}{2}(p - 2y^t) - \frac{\beta}{2}(y^t - u), & \text{otherwise.} \end{cases} \tag{1}$$

Rejection on the other hand always results in valuation

$$V = -\frac{\alpha u}{2}.$$

After some rearranging we obtain the acceptance threshold:

$$y^t = \begin{cases} \frac{\alpha p}{3\alpha+2}, & \text{for } \alpha < \frac{2u}{p-3u} \text{ (and } 3u \neq p); \\ \frac{\alpha p}{2\alpha-\beta+2} - \frac{(\alpha+\beta)u}{2\alpha-\beta+2}, & \text{otherwise.} \end{cases} \tag{2}$$

In the standard ultimatum game, however, valuation of an accepted offer equals

$$V = y - \alpha \max(p-2y,0) - \beta \max(2y-p,0)$$

and rejection always results in $V = 0$. Thus, the smallest accepted offer equals

$$y^t_{CON} = \frac{\alpha p}{2\alpha + 1} \tag{3}$$

It is easy to see that $y^t_{CON} > y^t$ for $0 \leq \beta \leq \alpha$ and $\beta < 1$.

A.2. Numeric Application of Fehr and Schmidt (1999)

In this section we apply the following widely used distribution of parameters α and β, suggested by Fehr and Schmidt (1999) to our experiments:

pop. share	α	β
30%	0	0
30%	0.5	0.25
30%	1	0.6
10%		4

In Table 5 we list the resulting distribution of the smallest accepted offer by Y. The numeric predictions turns out to be in line with Hypothesis 7. Furthermore, Wilcoxon rank sum tests on simulated distributions (with number of observations equivalent to the ones in our experiments) find significant ($p = .043$) differences.

Table 5: Predicted Acceptance Thresholds of Y

y	subgame *exclude*	*CON*
0	30%	30%
2	0%	0%
4	30%	0%
6	30%	30%
8	10%	30%
10	0%	10%
12	0%	0%
av. y^t	3.8	5.2

We can also apply the distribution of aversion parameters to subgame *keep*. The resulting frequencies of acceptance of each offer are listed in Table 6.

Observe that we do find consideration of Y for the payoff of Z in two cases.

1. While only 10% reject an offer of 4 to Y and 4 or more to Z, 40% reject an offer of 4 to Y and less than 4 to Z.

2. While all accept an offer of 6 to Y and 4 or more to Z, 10% reject an offer of 6 and less than 4 to Z.

Despite some strong differences in payoffs between X and Y we, however, do not find consideration by Y for X.

Table 6: Predicted Frequency of Acceptance in Subgame *keep*

Offer			share
x	y	z	accept
18	0	0	30%
16	0	2	30%
14	0	4	30%
12	0	6	30%
10	0	8	30%
16	2	0	30%
14	2	2	30%
12	2	4	30%
10	2	6	30%
8	2	8	30%
14	4	0	60%
12	4	2	60%
10	4	4	90%
8	4	6	90%
6	4	8	90%
12	6	0	90%
10	6	2	90%
8	6	4	100%
6	6	6	100%
4	6	8	100%
10	8	0	100%
8	8	2	100%
6	8	4	100%
4	8	6	100%
2	8	8	100%

B. Experimental Instruction

This section gives the original experimental instructions of our baseline treatment $XV2$ translated to English. Whenever instructions of other treatments differed we indicate this.

Instructions

Welcome! Please stop communicating with other participants, switch off your mobile and read the following instructions carefully. If you have any question, please do not ask loudly, raise your arm and wait for one of the supervisors to come and help you. Should you not comply with this rule we will have to exclude you from the experiment and all payments.

The instructions are identical for all participants. In the experiment you will remain anonymous. This means that no participant will learn anything about your identity.

The experiment consists of two repetitions (rounds). In every round you can earn money. How much you earn depends on your own decisions, those of others participants and random draws. However, only one round will be paid: At the end of the experiment a lot decides which round is relevant for payment. The money you earned in that round will be paid to you in Euro.

There are three roles. One third of all participants decides in role X, an other third in role Y and the remaining ones in role Z. In every round three participants each interact: one X, one Y and one Z. In the second round participants will be mixed again such that no one interacts in round two with someone they interacted with in the first round. The roles will be assigned at random at the beginning of the experiment and remain unchanged throughout.

Sequence of a round

Basic principle

First X decides whether the round ends for Z. [$R\star\star$**:** . . . a random draw decides whether the round ends for Z. The probability that it ends equals [$RV1$**:** 85%] [$RV2$**:** 67.5%]]

. . . The remaining procedure depends on that decision [outcome]:

- <u>X [$R\star\star$**:** The random draw] decides the round does not end for Z:</u>
  If X [$R\star\star$**:** The random draw] decides that the round does not end for Z, then X can decide about a total amount of €18. First X determines how he intends to divide €18 amongst himself, Y, and Z. His offers y and z indicate how much he is willing to give to Y and Z, respectively. Both, y and z, can accept any of the following values: 0, 2, 4, 6, or 8€.

  Then, both Y and Z simultaneously decide [$\star V1$**:** Then Y decides] whether they accept [he accepts] this offer or not. Depending on this decision payoffs are determined as follows:

Y and Z accepts offer by X	Y or Z or both reject offer by X
[$\star V1$**:** Y accepts offer by X	Y rejects offer by X]
X obtains 18-y-z	X obtains 0
Y obtains y	Y obtains 0
Z obtains z	Z obtains 0

- X [$R\star\star$**:** The random draw] decides the round does end for Z:
  If X [$R\star\star$**:** The random draw] decides that the round ends for Z, then the round ends for Z and he obtains 1 €. For X and Y the round continues as follows:

  X can decide about a total amount of 20 €. First X decides how he intends to divide 20 €amongst himself and Y. His offers y indicates how much he is willing to give to Y. Hereby, y can accept any of the following values: 0, 2, 4, 6, 8, 10, or 12€.

  Then, Y decides whether he accepts this offer or not. Depending on this decision payoffs are determined as follows:

Y accepts offer by X	Y rejects offer by X
X obtains 20-y	X obtains 0
Y obtains y	Y obtains 0
Z obtains 1	Z obtains 1

After reading these instructions we will test whether all participants have understood them with a set of control questions on your computer screen. Please answer them as good as possible. If you cannot answer the questions correctly we may have to exclude you from participation. In any case you obtain 3€for answering this set of questions.

Control Questionnaire

1. Suppose X [$R\star\star$: the random draw] has decided the round does not end for Z.
   Participant X offered y=8 to Y and $z = 8$ to Z.
   Both, participant Y and Z rejected that offer.
   [$\star V1$**:** Participant Y rejected that offer.]
   What are the round incomes of X, Y, and Z?

2. [only in $\star V2$ treatments!]
   Suppose X [$R\star\star$: the random draw] has decided the round does not end for Z.
   Participant X offered y=2 to Y and $z = 8$ to Z.
   Participant Y accepted but Z rejected that offer.
   What are the round incomes of X, Y, and Z?

3. [only in $\star V2$ treatments!]
   Suppose X [$R\star\star$: the random draw] has decided the round does not end for Z.
   Participant X offered y=8 to Y and $z = 6$ to Z.
   Participant Y rejected but Z accepted that offer.
   What are the round incomes of X, Y, and Z?

4. Suppose X [$R\star\star$: the random draw] has decided the round does not end for Z.
   Participant X offered y=6 to Y and $z = 8$ to Z.
   Both participants Y and Z accepted that offer.
   [$\star V1$**:** Participant Y accepted that offer.]
   What are the round incomes of X, Y, and Z?

5. Suppose X [$R\star\star$: the random draw] has decided the round does end for Z.
   Participant X offered y=12 to Y.
   Participants Y accepted that offer.
   What are the round incomes of X, Y, and Z?

6. Suppose X [$R\star\star$: the random draw] has decided the round does end for Z.
   Participant X offered y=12 to Y.
   Participants Y rejected that offer.
   What are the round incomes of X, Y, and Z?

Detailed Procedure

The detailed procedure of the experiment deviates from the above description in the following aspects:

After X [$R\star\star$**:** ...the random draw ...] decides, whether the round ends for Z and [$R\star\star$**:** X decides] what offer he makes the remaining participant(s), neither Y nor Z learn how he has decided. Instead both (Y and Z) are asked how they [$\star V1$**:** ...Y is asked how he...] would decide in each possible case.

Thus, Y has to fill out two tables: In the first he indicates for the case that the round has ended for Z and for each possible offer y (0, 2, 4, 6, 8, 10, 12) by X whether he accepts or rejects (see Image 1). In the second table he indicates for the case that the round has not ended for Z and for each of the twenty-five possible combinations of offers (y, z) by X to Y and Z, whether he accepts or rejects (see Image 2).

Z only needs to fill out one table that resembles the second for participant Y. He also indicates for the case that the round has not ended for him and for each of the twenty-five possible combinations of offers (y, z) by X to Y and Z, whether he accepts or rejects (see Image 3). [Paragraph deleted in $\star V1$**:** treatments]

After all participants of one group made their decisions everyone is informed about the relevant decisions and round incomes are calculated.