

JENA ECONOMIC RESEARCH PAPERS



2008 – 011

Effects of Profitable Downsizing on Collective Bargaining

by

Sven Fischer
Werner Güth
Christoph Köhler

www.jenecon.de

ISSN 1864-7057

The JENA ECONOMIC RESEARCH PAPERS is a joint publication of the Friedrich Schiller University and the Max Planck Institute of Economics, Jena, Germany. For editorial correspondence please contact m.pasche@wiwi.uni-jena.de.

Impressum:

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

© by the author.

Jena Economic Research Papers 2008-011

Effects of Profitable Downsizing on Collective Bargaining

Sven Fischer[1], Werner Güth[2] and Christoph Köhler[3]

this version: 15 December 2007

Abstract

We experimentally test how acceptance thresholds react to the decision of the proposer in a three party ultimatum game to exclude one of two responders with veto power from the game. We elicit responder acceptance thresholds in case the proposer decides to exclude one of them, what increases the available pie, and in case he doesn't exclude him despite strong monetary incentives. We find that on the aggregate level the proposer's decision has no effect on acceptance thresholds. However, if the proposer excludes one responder, the distribution of thresholds becomes bimodal, indicating a polarization in behavior.

Keywords: bargaining, experiment, labor markets

[*JEL*: C91, J52]

[1] *Corresponding author.* Department of Economics, University College London, Gower Street, London WC1E 6BT, United Kingdom, Email: Sven.Fischer@ucl.ac.uk

[2] Max Planck Institute of Economics, Kahlaische Strasse 10, 07745 Jena, Germany, Tel: +49/3641/686621, Fax: +49/3641/686667, Email: Gueth@econ.mpg.de

[3] Friedrich-Schiller-University Jena, Institute for Sociology, Economic and Social Structure Group

1 Introduction

Fast technological progress and increasing global market integration requires firms to swiftly adjust their working routines and labor force. Especially large corporations are often under pressure by their share holders to increase profitability by downsizing the labor force. While this pressure is strongest in cases of unprofitability, it also hits companies which are rather profitable but can become even more efficient by reducing the number of employees. A prominent example in Germany was the *Deutsche Bank AG*, which in early 2005 started a restructuring initiative planning to fire about 6500 employees worldwide despite a rate of return on equity 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, recent figures show that profitability increased to 19.5% in 2006 (see, Deutsche Bank Aktiengesellschaft 2007).

Great parts of the German public, and here especially union representatives, reacted with outrage to the downsizing decision by *Deutsche Bank*. But how does it affect the employer–employee relation when, for the sake of profitability, part of the labor force is dismissed? International justice research in the tradition of Kahnemann and Knetsch (see, e.g. Kahneman et al., 1986) has established that layoffs are seen as unfair if not economically necessary to save the company and remaining employees from bankruptcy (see, e.g., 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.

Our stylized game, representing the problem in a collective bargaining framework, allows to capture the effects of downsizing for the sake of profitability on wage demands by the remaining workforce. By implementing it experimentally we explore whether and how behavior differs from what common (and known) opportunism predicts.

Our main research questions are:

- How do wage demands of the remaining workforce react to the decision of the firm to fire their colleagues, and how does this compare to the reaction to the decision, despite monetary incentives, not to do so?
- To what degree can these effects be related to (indirect) reciprocity or emotions?

The remainder of this paper is organized as follows: Section 2 introduces and discusses the model. Section 3 describes the exact experimental procedure. The experimental findings are described in section 4. Section 5 concludes.

2 The Model

We reduce the rather rich real world process of downsizing and wage bargaining to a three party ultimatum game (Güth and van Damme, 1998). By doing so we, of course, exclude effects resulting from repeated interaction and (indirect) communication.[4]

[4]Alternatively we could use the Nash demand game with procedurally symmetric bargaining power where, however, on behalf of both roles demands are likely to depend on beliefs concerning the demands of the other party. The ultimatum game allows us to elicit

In the three party ultimatum game the interacting parties are the employer X, the employee Y not threatened by unemployment and the employee Z threatened to be fired. The game tree is plotted in figure 1. In a first stage the employer decides whether to fire employee Z and make him live on a positive unemployment benefit of u. In the following stage the employer starts to bargain with the remaining employee(s) about their share of the firm's surplus. For simplicity we fix the surplus to a divisible monetary amount $p > 0$ in case of no firing and to rp with $r > 1$ in case of firing. By setting $r > 1$ and $u \ll p/3$, downsizing increases productivity but leaves Z much less than an equal share of p.

After deciding about the number of responders (and, thus, the size of the pie), X chooses how much to offer the remaining employee(s). Here y (z) with $y + z \leq p$ or $y \leq rp$, respectively, indicates his offer to Y (Z, if applicable). The employer is not confined to identical offers to both employees when Z is not fired. If Z has been fired only Y decides whether to accept or reject the offer. Otherwise both employees decide independently, knowing not only their own but also their colleague's wage offer. Here both employees have veto power.

Clearly, this one-shot bargaining model cannot fully capture real world collective wage bargaining. It, however, provides an easily understood scenario, allowing to elicit acceptance thresholds of responders. Since perceived intentions behind the employer's firing (or keeping) decision may affect responder behavior we introduce a second treatment condition RAN, which

acceptance thresholds, which are independent of beliefs, although, beliefs concerning what offers (the proposer believes) the responder will accept, may still be relevant.

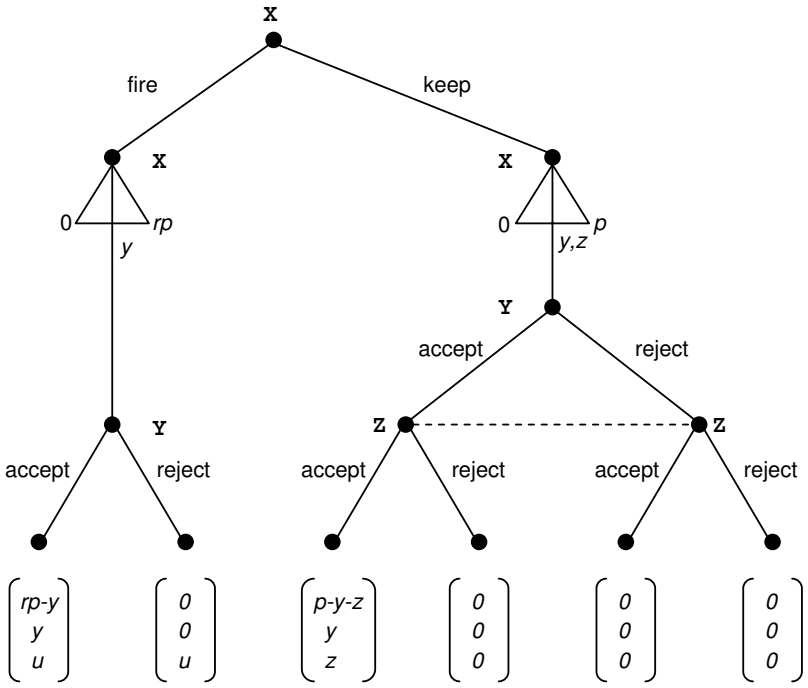


Figure 1: Game Tree Treatment EMP

differs from EMP only in that the $fire$ or $keep$ decision is replaced by a random draw with a probability of firing P_f equal to the observed relative frequency of the $fire$ decision in the EMP condition. With RAN we try to rule out reciprocal motives of responders.[5]

Although there exist plenty of data on ultimatum behavior we also ran a control treatment CON which is a simple two person ultimatum game with the same procedure and parametrization as in the $fire$ subgame (excluding dummy Z). By this we want to measure how differently the firing decision in EMP or the firing outcome in RAN affects behavior of the remaining employee Y compared to Y's behavior in the CON - Treatment.

[5]However, we are not able to exclude potential reciprocity towards the experimenter.

2.1 Benchmark Solutions and Behavioral Hypotheses

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 will consider perfect equilibria assuming opportunistic agents which are rational and know that others are rational, too.[6] A selfish and rational responder accepts any offer yielding at least his conflict payoff. As $r > 1$ this implies that (expecting rational behavior of employees) the employer will fire Z allowing to distribute $rp > p$ among himself and Y. The only perfect equilibrium is therefore that X fires Z and offers minimal amounts to Y alone or Y and Z and that both Y and Z accept any (positive) offer.

Previous three person ultimatum bargaining experiments largely focused on versions of the game where only one responder has veto power.[7] Here, the so-called dummy Z usually receives little amounts. We are not aware of a three person ultimatum experiment with veto power on behalf of all two responders.[8]

From the existing literature on Ultimatum games[9] we know that offers of below 30% (and sometimes even higher ones) are frequently rejected and significant offers are made, the modal offer often being 50% of the available pie. It has widely been argued that this behavior reflects equity preferences.[10] Incorporating equity preferences into our game does not yield clear predictions.

[6] Higher orders of rationality are not required in this game.

[7] See, e.g., Kagel and Wolfe (2001) and Güth et al. (2007).

[8] Büchner et al. (2004) study a similar but more complex and, thus, quite different ultimatum game.

[9] For a survey see Camerer (2003).

[10] For a discussion, see, e.g. Charness and Rabin (2002).

It depends on how and on whether the size of u is relevant or not. If Z is fired, X and Y basically play a standard ultimatum game where Y will most likely be offered a significant amount (see Camerer (2003), for references on confirming evidence). On the other hand, in the keeping subgame, roles Y and Z are interchangeable. Unless other considerations lead to carry over effects from the $fire/keep$ decision, (expected) payoffs of Y and Z should be equal. Thus, firing Z should lead to much more inequality in payoffs than keeping him, irrespective of how inequality is measured.[11] Despite the clear monetary and efficiency[12] incentives to fire Z, inequality aversion may inspire keeping Z and offering significant amounts to both Y and Z.

Reciprocity in turn tries to evaluate expected or observable actions as "kind" or "unkind" and predicts that one reciprocates accordingly.[13] Besides this direct reciprocity one also observes behavior which can be described as indirect reciprocity. Here a reciprocal action is directed towards one party in response to a "kind" or "unkind" action of this party towards a third one.[14]

How does reciprocity, and here more importantly indirect reciprocity, affect behavior in our firing game? Let us assume that firing Z constitutes an action which is widely considered as "unkind" towards Z and "potentially kind" towards Y.[15] Vice versa, keeping Z (and offering him more than u) constitutes a "kind" action towards Z and again an only "potentially unkind"

[11] Note that this effect is, furthermore, strengthened by $r > 1$.

[12] Firing Z increases the available pie and increases the total surplus by the additional transfer u.

[13] See, e.g. Rabin (1993) for normal form games and Dufwenberg and Kirchsteiger (2004) for extensive games.

[14] See, e.g. Seinen and Schram (2006) or Greiner and Levati (2005)

[15] Note that this qualification of the two actions is different from the one used, e.g. in Dufwenberg and Kirchsteiger (2004).

action towards Y.[16] In the keeping subgame where Z is not fired, Z may reciprocate this kind action by being willing to accept less. On the other hand, Y considering this action to be unkind towards him may reciprocate by forcing conflict. This tendency may be offset of some higher offer y to "buy" Y's acceptance. If this is the case, one should observe lower acceptance thresholds of Z than of Y. However, these effects on behalf of Y may be confounded with indirect reciprocity. Y may be willing to reward X for his "kind" action towards Z. Looking at experimental evidence so far, it appears that indirect reciprocity is weaker than direct reciprocity. We, thus, formulate

Hypothesis 1 In the keeping subgame, due to primarily positive reciprocity on behalf of Z and the potential negative reciprocity of Y, acceptance thresholds of Y are higher than those of Z.

Turning to the firing subgame one immediately sees that, again, we may have two confounding reactions on behalf of Y. He may interpret the firing decision as kind towards him and reciprocate positively. In this case, acceptance thresholds of Y will be smaller than those of a responder in an equivalent two party ultimatum game (with pie rp), i.e. treatment CON.

If, however, the observable moral outcry in our real world example has any effect on behavior, we should observe negative indirect reciprocity, reflected in the willingness to induce conflict with high acceptance thresholds, on behalf of Y. In this case acceptance thresholds of responders Y in EMP should be higher than those observable in treatment CON. More importantly, however,

[16]The proposer may still be able to offer Y the same amount he would offer him after firing Z.

such reactions should not occur in treatment RAN. We summarize these considerations in

Hypothesis 2 Acceptance thresholds of Y in the firing subgame EMP differ from those in RAN and CON, whereas those in RAN and CON are the same.

An important aspect of reciprocity is the intentionality of the decision. In the RAN treatment, we replace intentionality by a random draw, which explains the latter part of hypothesis 2. While we can formulate some vague hypotheses concerning behavior of non opportunistic responders, this is even more difficult for proposers: Reciprocal proposers will base their offers on their own second order beliefs (what does the proposer think the responder expects to be offered). However, as a rather conservative hypothesis we predict offers made in the firing subgame:

Hypothesis 3 Proposer offers in the firing subgame of treatment EMP differ from those in RAN, which in turn are the same as in CON.

Thus, we assume that proposers correctly anticipate reactions on behalf of responders as stated in hypothesis 2 and react to it, accordingly.

3 Experimental Design

We ran three different treatment conditions in a between subjects design: EMP (employer) which is the game presented in figure 1, RAN (random) which is as EMP except for the first decision of X being replaced by a random draw with a probability of firing P_f equal to the observed relative frequency

of firing decisions in EMP of 0.85; and control treatment CON. In order to obtain sufficient information on behalf of responders we employed the strategy method. By using the positional order protocol[17] we, nevertheless, guaranteed a "hot" decision environment, which we further strengthened by informing the responders that the proposer had just made his decision(s). In detail treatment EMP proceeded as follows:

1. X decides whether to '*keep*' or '$fire$' employee Z.

2. X decides how much he offers to his remaining employees where his set of possible offers depends on his first decision. Following '*keep*' he can choose among $(y, z) \in \{0, 2, 4, 6, 8\}^2$ for Y and Z, and after '$fire$' he can chose 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 get informed that "*X has already decided whether to fire Z*" and that "*X has already submitted his wage offer(s). However, you will not be informed about his decisions before the end of this round*".[18]

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

    - Similarly Z is asked the same questions, of course only for the case that he is not fired.

[17] It is known that employers have already decided before responders react, without, however, informing responders about the earlier employer decision.

[18] A translation of the instructions is reproduced in the appendix. The original version in German can be obtained from the corresponding author upon request.

4. Decisions are matched and all players are informed about all relevant decisions and all resulting payoffs. In case that Z got fired he obtains $u = 1$.

The set of possible offers are reduced to five and seven values, respectively, in order to reduce the length of the strategy vector the responders have to submit. The chosen sets include the possibility for equal offers (i.e. (6,6,6) and (10,10)) in order to render unequal offers less acceptable.[19] Furthermore, the sets include offers giving more to the responder(s) than to the proposer. While such offers are rarely observed, they are, nevertheless, possible and should be included (see, e.g., Güth et al., 2003; Fischer, 2005, for evidence of rejecting too generous offers).

To see whether experience influences behavior subjects are rematched in a strangers design and play the same game once again in the same role.[20] Only one randomly drawn round is paid, where points earned in that period are converted to € at €1,50 = 1 *point*. The three treatment conditions EMP, RAN and CON are compared between subjects, i.e., a subject either plays EMP, RAN or CON.[21] As we want to elicit "emotional" reactions, instructions relied on the loaded labor market terminology used in the description above.

In total we ran six sessions, two for each of the three treatments with 30 subjects in every session except for one in treatment CON, where only

[19] Falk et al. (2000) compare behavior in several mini ultimatum games which only differ in the set of alternative offers the proposer can choose from (sets are overlapping). They 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.

[20] Thus no one interacts with someone twice.

[21] The alternative within subjects comparison may result in order or demand effects.

27 participated. For the first round we thus have 20 independent group observations per treatment and for the second round we have 6.[22]

4 Results

Including admission and payment, sessions of treatments EMP and RAN lasted for about 45 minutes, those of CON for about 30 minutes. Average earnings in EMP and RAN were €11.40 for X, €8.25 for Y and €3.68 for Z with standard deviations of 5.4, 3.6 and 0.93, respectively.

In EMP, proposers in 85% of all cases chose to $fire\ Z$. In detail, in the first round 80% of all proposers fired Z, in the second 90%, which indicates no repetition effect.[23]

4.1 The $fire$ subgame

We first take a look at responder acceptance thresholds in the $fire$ 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[24] do not find significant ($p > .025$) differences.[25] As a further control we tested whether the frequency of observed changes in subjects' thresholds between the first and second repetition is sig-

[22]For a perfect strangers rematching we need at least 27. Thus, over the two rounds in each session there are two matching groups size 9 and one size 12.

[23]A Fisher test does not reject the null hypothesis of equal firing probabilities with $p = 0.6614$.

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

[25]If not mentioned otherwise the level of significance is set to $\alpha = 0.025$ throughout.

nificantly higher than an error rate of 5%, which also could be rejected in all treatments.

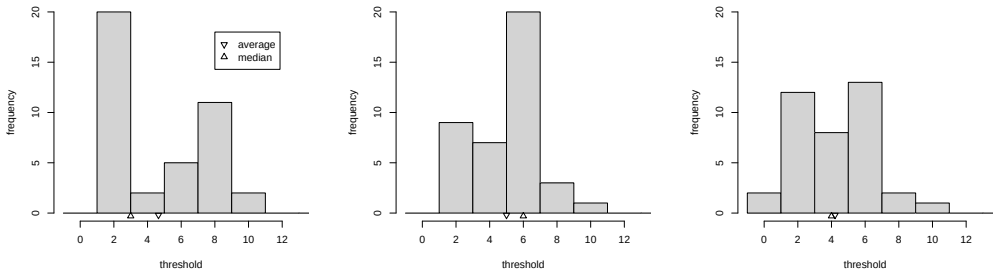
Looking at the distributions in figure 2 there are obvious differences. While the distributions for CON and RAN are rather similar, EMP clearly indicates different behavior. This observation is supported by separate Fisher tests, testing the hypothesis of equal frequencies of discrete threshold classes versus the general alternatives of differences in distribution. In detail, the distribution of thresholds in EMP significantly differs from that in RAN with $p < 0.001$ and from that in CON with $p = 0.0016$. With $p = 0.5799$ the comparison between RAN and CON is insignificant. The differences in distribution and the discreteness of the data make it difficult to test for differences in location. Wilcoxon rank sum tests,[26] however, indicate that, despite the differences in distribution, the differences in location are insignificant. This can also be inferred from the rather negligible differences in average thresholds, indicated by the upside down triangles in figure 2.

But how do thresholds differ. Starting with the plot for CON and going to the left, the distributions become gradually more and more bimodal. This is reflected in the considerable increase of the variance from 5.36 and 4.10 in treatments CON and RAN, respectively, to 8.49 in treatment EMP. Again pooling data over repetitions, Ansari-Bradeley tests prove that the dispersion of thresholds in treatment EMP is significantly larger than in RAN (p-value< 0.001) and CON (p-value=0.002). The dispersions of thresholds in treatments RAN and CON, however, do not differ significantly (p-

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

value=0.784).[27]

Observation 1 After the firing decision by the proposer, acceptance thresholds of responders Y in treatment EMP are significantly more diverse than in treatments RAN or CON. Whereas distributions in RAN and CON are more centered, in EMP thresholds are either very small or rather large. The threshold levels, however, do not differ significantly.



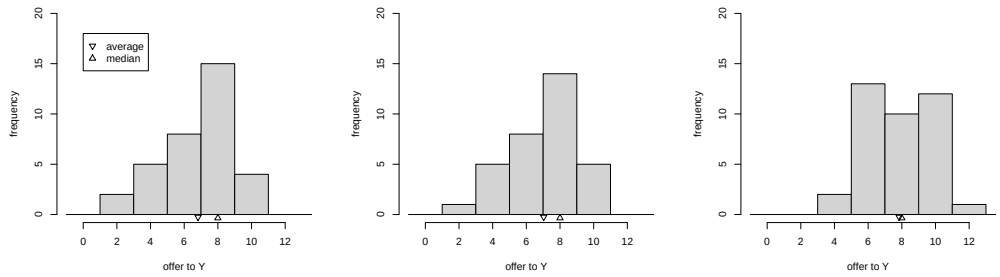
(a) EMP (b) RAN (c) CON

Figure 2: Thresholds Responder Y after $fire$ and in CON

Do proposer offers differ significantly between treatments? Figure 3 gives the distribution of offers to Y in treatments EMP and RAN after firing and in treatment CON. Distributions for treatments EMP and RAN are almost identical. This observation is confirmed by Fisher exact tests and Wilcoxon rank sum tests. The distribution of treatment CON, however, differs slightly. A Fisher exact test comparing the frequencies of the discrete offers between treatment EMP and CON combined over the two rounds rejects the null of equal distributions with $p = 0.0489$, pointing to a weakly significant effect.

[27] Qualitatively the same statements hold when comparing only data from the first or second round.

A Wilcoxon rank sum test, however, can not reject the null of no differences in location (two sided, $p = 0.08101$).[28] A look at the rather small differences in averages and medians supports this finding. We conclude



(a) EMP (b) RAN (c) CON

Figure 3: Offers to Y after $fire$ and in CON

Observation 2 Offers to responder Y in treatments EMP and RAN after $fire$ and in treatment CON do not differ significantly.

4.2 The *keep* subgame

The relative frequencies of accepted offers by Y and Z in the *keep* subgame are plotted in figure 4. In the three dimensional plots the height of the bars represents the relative frequency of how often an offer y to Y and z to Z was accepted. Figure 4a gives acceptance rates by Y, figure 4b those by Z, separately for treatments EMP and RAN. Again the plots show the combined data for the two rounds.

In these plots two comparisons are of interest. The first is the comparison of acceptance rates by Y with that by Z within each treatment. Obviously

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

such a comparison requires a transformation of the data such that the acceptance rate of an offer $(x,y,z) = (x,\upsilon,\theta)$ by responders Y is compared to the acceptance rate of the offer (x,θ,υ) by responders Z. In the plots this is equivalent to mirroring the x- and y-axis. The plots suggest that there are no significant differences. To test whether in every case indexed by $c \in \{1,\dots 25\}$ (offer (x,υ,θ) to Y and (x,θ,υ) to Z) the acceptance rate r_c^Y of Y equals that of Z (r_c^Z), we ran four Mantel and Haenzel tests,[29] one for each treatment/period combination. In detail we tested the null H_0*:* $r_1^Y = r_1^Z$, $\dots$, $r_c^Y = r_c^Z$, $\dots r_{25}^Y = r_{25}^Z$ towards the general alternative that H_1*:* $r_c^Y \neq r_c^Z$ *for at least one* c. Not surprisingly, the null hypothesis could not be rejected throughout.

Observation 3 In the *keep* subgame, acceptance behavior of Y and Z does not differ significantly.

The second comparison of interest is that between treatments. Does the intentionality of the *keep* decision influence acceptance behavior? Again, we ran 'Mantel and Haenzel' tests, separately for each role and period. With only one weakly significant exception there are no significant differences: In role Z in the first round we can reject the null hypothesis of equal acceptance probabilities in both treatments EMP and RAN with $p = 0.0256$.

Looking at the individual acceptance rates of Z in the first period, this is due to two main differences: Compared to treatment RAN, in treatment EMP responders Z have a slightly higher tendency to reject very small offers and, furthermore, tend to ignore rather small offers y towards Y which in

[29] For details, see Hollander and Wolfe (1999).

RAN are rejected more often. This higher "sensitivity" to one's own offer on the one hand and lower "sensitivity" towards small offers to the other responder, however, is not present in the second round. We, thus conclude

Observation 4 Acceptance behavior of responders in the *keep* subgame does not differ significantly between treatments EMP and RAN.

For a more elaborate picture of how responders in the *keep* subgame react to the composition of the offer vectors, we ran mixed effects logit regressions of the acceptance of an offer on various covariants, as shown in table 1. In these estimations we assume that there are unobservable subject wise random effects. In detail, in our logit estimation the expected probability of acceptance of an offer $\mathbf{o}_{ci} = (o_{ci}, o_{cj})$ (where $i,j = \{Y, Z\}$, $i \neq j$, $c \in \{1, \ldots, 25\}$, $o_{ci} = y_c$ for i in role Y and $o_{ci} = z_c$ for i in role Z) by subject i is defined by

$$\text{logit}^{-1}\left(\text{Prob}(i \text{ accepts } \mathbf{o}_{ci})\right) = x'_{ci}\beta + \zeta_i + u_{ci} \ , \tag{1}$$

where x'_{ci} is a matrix of covariants, ζ_i is a subject wise random effect,[30] and u_{ci} is a white noise term.

First look at model (1) in the first column. The estimation includes the two dummies D_Z for responders Z and D_{P2} for the second round. Observe that both dummies are insignificant, what confirms our previous results. Dummy $D(o_j = 0)$ has ones for offers in which the respective other responder gets nothing. Estimation (2) excludes the two insignificant and uninformative

[30]The subject wise random effects are assumed to be homoscedastic with expected value equal zero and to be uncorrelated with x.

(Akaike information criterion) control dummies and is the best representation of the data we could find. The effects can be described as follows:

1. The acceptance probability significantly increases with the offer (o_i), one receives.

2. Given several vectors with identical offers to oneself, the probability of acceptance increases in the offer to the other player ($o_i o_j$).[31]

3. Acceptance significantly decreases if the other player is offered nothing ($D(o_j = 0)$) as opposed to something.

In addition to the effects listed in table 1, we tested for effects resulting from zero offers to oneself (dummy $D(o_i = 0)$), offers where the other would obtain more or the same as oneself (dummies $D(o_i < o_j)$ and $D(o_i = o_j)$) as well as for the relevant interaction effects of all variables. All these were insignificant and did not contribute to the accuracy of the model according to the Akaike information criterion.

In the *keep* subgame we have very few observations of proposer offers. On average (combining both rounds, there are six observations) Y and Z were offered 4.67 (standard deviation σ=2.15) and 5.0 (σ=1.67), respectively. In treatment RAN, Z is not being fired by the random mechanism in 7 cases and average offers to Y and Z are 4.57 (σ=1.51) and 4.29 (σ=1.38), respectively.

[31]Note that the coefficients in table 1 are not the marginal effects. However, as the effect on o_i is positive and, as the effect on o_j is almost zero, we can conclude that the marginal effect on $o_i o_j$ is also significantly positive.

Table 1: GLMM Logit of Acceptance in *keep* Subgame

	Acceptance of offer $\mathbf{o}_{ci}$			
	(1)		(2)	
	coef.	(*t-value*)	*coef.*	(*t-value*)
(intercept)	-1.948**	(-3.63)	-1.856**	(-4.02)
D_Z	-0.074	(-0.17)	—	
D_{P2}	0.329	(1.40)	—	
o_i	0.562**	(6.08)	0.577**	(6.46)
o_j	-0.131	(-1.38)	-0.133	(-1.45)
$o_i o_j$	0.094**	(3.75)	0.096**	(3.96)
$D(o_j = 0)$	-2.725**	(-3.50)	-2.840**	(-3.70)
σ_u; σ_ζ	1.650;	2.038	1.734;	1.953
Deviance	2749		2757	

NOTE: Those interaction effects absent in the table were insignificant and did not contribute to the accuracy of the estimation according to the Akaike (AIC) information criterion. Null deviance: 5366; *: p-value< 2.5%; **: p-value< 1%.

4.3 Acceptance in $fire$ vs. $keep$

Let us finally explore how the threshold of Y after *keep* (t_k^Y) compares to that after $fire$ (t_f^Y). For a comparison we rather look at the relative than the absolute threshold, defined as $s_k^Y = {}^{t_k^Y}/_{p}$ and $s_f^Y = {}^{t_f^Y}/_{rp}$, respectively. In order to make thresholds comparable, for the *keep* subgame we define Y's thresholds as the smallest offer he is willing to accept independently of the offer towards Z.[32] Assuming inequality aversion or any other kind of other regarding preferences, one may expect that the relative threshold after *keep* is smaller than after $fire$ due to the larger group $\{X,Y,Z\}$ versus $\{X,Y\}$ and

[32]Define again an offer as the vector (x, y, z) with $x + y + z = p$. A responder i of role Y rejecting all offers $((p - \tilde{y} - \tilde{z}),\tilde{y}, \tilde{z})$ for all $\tilde{y} < t_k^i$ and all possible $\tilde{z}$ but accepting at least one offer with $y = t_k^i$ is assumed to have threshold t_k^i even if there exists another offer $\left((p - t_Y - \tilde{z}),t_k^i, \tilde{z}\right)$ he rejects.

the smaller pie p versus rp. respectively. The two boxplots in figure 5a represent the distributions of the differences in relative thresholds of responders Y in subgame $fire$ minus the one in subgame $keep$ separate for treatments EMP and RAN (combined over the two rounds). In both treatments, differences for most subjects are positive. Wilcoxon signed rank tests confirm that the relative threshold of Y in subgame $fire$ is significantly higher than the one in subgame $keep$ (EMP: p-value=0.0163; RAN: p-value=0.0001).

When investigating the differences in relative thresholds, it appears that the relative threshold of Y in subgame $fire$ (denoted by s_f^Y) can be described as follows:

$$\begin{aligned} s_f^Y &= s_k^Y + s_f^Y(1 - \tilde{s}_{kY}^X - s_k^Y) \\ &\approx s_k^Y + s_f^Y s_k^Y \end{aligned} \tag{2}$$

The intuition behind this formula for Y's threshold share is:

1. Y insists on getting at least the same share as in $keep$ (s_k^Y)

2. Share $\tilde{s}_{kY}^X$ stands for what Y judges to be a sufficient share for X in the $keep$ subgame. Therefore the remainder $(1 - \tilde{s}_{kY}^X - s_k^Y)$ is what Y thinks to be a reasonable share for Z in the keep subgame, which after the firing decision is left to be shared between Y and X. We now assume that as roles Z and Y in $keep$ are interchangeable it holds that $\tilde{s}_{kY}^Z = (1 - \tilde{s}_{kY}^X - s_k^Y) \approx s_k^Y$

3. Y intends to share this remainder left by the exclusion of Z the same way he intends to share the entire pie rp.

The two boxplots of figure 5b show the distributions of individual differences $s_f^Y/(1+s_f^Y) - s_k^Y$, which should be zero according to equation (2), for treatments EMP and RAN, respectively. Obviously differences are either equal or close to zero, what is confirmed by Wilcoxon signed rank tests which only find insignificant differences between the two transformed relative thresholds (EMP: p-value=0.1989; RAN: p-value=0.8713).

The alternative behavioral hypothesis that the share of Z is divided equally between X and Y in the fire subgame, could not be confirmed by the data.

5 Discussion

Our three party ultimatum game allows the proposer to exclude one of the responders (Z). We were able to test how excluding Z, or abstaining from doing so despite monetary incentives, affects acceptance behavior of the remaining responder. Theoretically we established two main possible reactions to X's decision: Direct reciprocity on behalf of both responders, but in opposite directions, and indirect reciprocity of Y.

Looking at the aggregate level, we found no reactions, neither to excluding nor to keeping Z. More specifically, we can distinguish between the following non-effects:

1. Treatment indifference of responder Y's acceptance thresholds:

   - Thresholds of responders Y in the two party ultimatum game are the same, irrespective whether this two party game resulted from

exclusion of Z, a random exclusion of Z or non-existence of Z.

- If X does not exclude Z, acceptance thresholds of responders Y are identical to those of Z.

2. Treatment indifference of Z's acceptance thresholds: Intentionality of the decision not to exclude Z does not result in different acceptance behavior of Z.

Although we do not observe reactions in acceptance thresholds of Y, this does not imply that responders Y are insensitive towards what Z is being offered. Acceptance behavior in the three person ultimatum game significantly depends on what is offered to the other responder. Here, the strongest reaction can be observed if the other responder is offered nothing. Note, however, that given the high rate of rejections of zero offers, this has nearly no outcome effect, as conflict already results from the rejection of the subject who was offered nothing.

While these non-results hold with respect to the aggregate level of thresholds, there is one significant effect concerning the distribution of Y thresholds. If the two party ultimatum game results from an intentional exclusion of Z, responders Y can be divided into two groups: subjects with a very low threshold of 2 (50% of all) on the one hand and subjects with a rather high one of about 8. This indicates that the intentionality of the firing decision has opposing effects on different subjects.

Explaining this effect conclusively is rather difficult. As already described, direct and indirect reciprocity work in opposite directions: Interpreting the firing of Z as an unkind action towards Z, responder Y reciprocates by a

high willingness to induce conflict and to lower X's (expected) payoff. Direct reciprocity on the other hand interprets the decision as "kind" towards Y, which Y reciprocates by lower thresholds. Our result, thus, suggests that responders Y are either direct or indirect reciprocators.

A similar explanation could be that the $fire$ decision signals that the proposer is selfish to what some responders might react by lowering their acceptance threshold. This effect may be related to reciprocity. When evaluating an action as "*kind*" or "*unkind*", the expected proposer type, e.g. from attributing motives to why X has fired Z and how Y wants to react to that, may be relevant. In this sense high acceptance thresholds could be interpreted as Y attempting to punish a greedy proposer X whereas low acceptance thresholds of Y-participants after firing Z could express yielding to greedy proposers.

But how about our real world example? Clearly, one has to be cautions when translating the results of our stylized interaction to field situations. Still, our results indicate that (1) refraining from downsizing one's labor force will not be rewarded and (2) downsizing results in a polarization of the remaining workforce. Whereas in our experiment this has no effect on the aggregate level of acceptance thresholds, in a different institutional framework this polarization may very well affect outcomes. More specifically, in a centralized bargaining framework it would matter which kind of Y is more represented in the negotiation process: The high reacting or low reacting type.

References

Büchner, S., L. G. Gonzalez, W. Güth, and M. V. Levati (2004). Incentive contracts versus trust in three-person ultimatum games: An experimental study. *European Journal of Political Economy 20*, 673–694.

Camerer, C. F. (2003). *Behavioral Game Theory: Experiments in Strategic Interaction*. Princeton, New Jersey: Princeton University Press.

Charness, G. and D. I. Levine (2000). When are layoffs acceptable? *Industrial and Labor Relations Review 53*, 381–400.

Charness, G. and M. Rabin (2002). Understanding social preferences with simple tests. *The Quarterly Journal of Economics 117*(3), 817–869.

Deutsche Bank Aktiengesellschaft (2006). Jahresbericht 2005.

Deutsche Bank Aktiengesellschaft (2007). Jahresbericht 2006.

Dufwenberg, M. and G. Kirchsteiger (2004). A theory of sequential reciprocity. *Games and Economic Behavior 47*(2), 268–298.

Falk, A., E. Fehr, and U. Fischbacher (2000). Testing theories of fairness – intentions matter. Working Paper No. 63, Institute for Empirical Research in Economics – University of Zurich.

Fischer, S. (2005). Inequality aversion in ultimatum games with asymmetric conflict payoffs: A theoretical and experimental analysis. *Discussion Papers on Strategic Interaction 36-2005*.

Frankfurter Allgemeine Zeitung (2005). Deutsche bank macht 2,5 milliarden euro gewinn. February 3, 2005.

Gerlach, K., D. Levine, G. Stephan, and O. Struck (2006). The acceptability of layoffs and pay cuts: Comparing north america with germany. *IAB Discussion Paper 1-2006*. forthcoming: Cambridge Journal of Economics.

Greiner, B. and V. M. Levati (2005). Indirect reciprocity in cyclical networks: An experimental study. *Journal of Economic Psychology 26*(5), 711–731.

Güth, W., C. Schmidt, and M. Sutter (2003, may). Fairness in the mail and opportunism in the internet: A newspaper experiment on ultimatum bargaining. *German Economic Review 4*(2), 243–265.

Güth, W., C. Schmidt, and M. Sutter (2007). Bargaining outside the lab – a newspaper experiment of a three-person ultimatum game. *The Economic Journal 117*(518), 449–469.

Güth, W. and E. van Damme (1998). Information, strategic behavior, and fairness in ultimatum bargaining: An experimental study. *Journal of Mathematical Psychology 42*(23), 227–247.

Hollander, M. and D. A. Wolfe (1999). *A general theory of equilibrium selection in games*. New York: John Wiley & Sons, Inc. 2nd edition.

Kagel, J. H. and K. W. Wolfe (2001). Tests of fairness models based on equity considerations in a three-person ultimatum game. *Experimental Economics 4*, 203–219.

Kahneman, D., J. L. Knetsch, and R. Thaler (1986, sep). Fairness as a constraint on profit seeking: Entitlements in the market. *The American Economic Review 76*(4), 728–741.

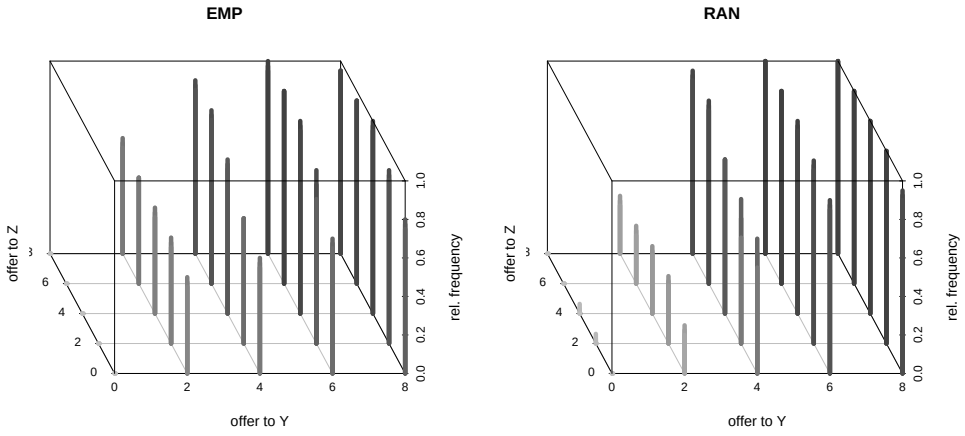
Kreps, D. M. and R. Wilson (1982). Sequential equilibria. *Econometrica 50*(4), 863–894.

Rabin, M. (1993). Incorporating fairness into game theory and economics. *The American Economic Review 83*(5), 1281–1302.

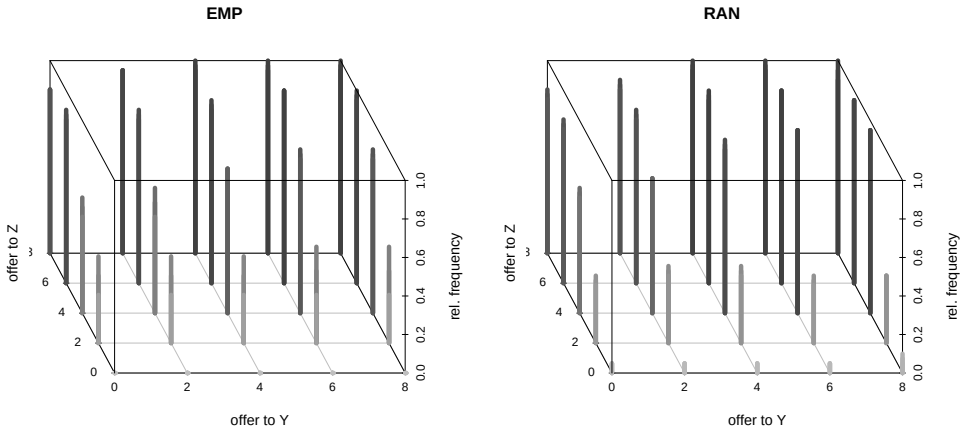
Seinen, I. and A. Schram (2006). Social status and group norms: Indirect reciprocity in a repeated helping experiment. *European Economic Review 50*(3), 581–602.

Selten, R. (1975). Reexamination of the perfectness concept for equilibrium points in extensive games. *International Journal of Game Theory 4*(1), 25–55. Reprinted in Kuhn, Harold W. (ed.) (1997). *Classics in Game Theory*, pp. 317-54. Princeton University Press, Princeton, NJ.

Streitberg, B. and J. Röhmel (1984). Exact nonparametrics in apl. *ACM SIGAPL APL Quote Quad 14*(4), 313–325.

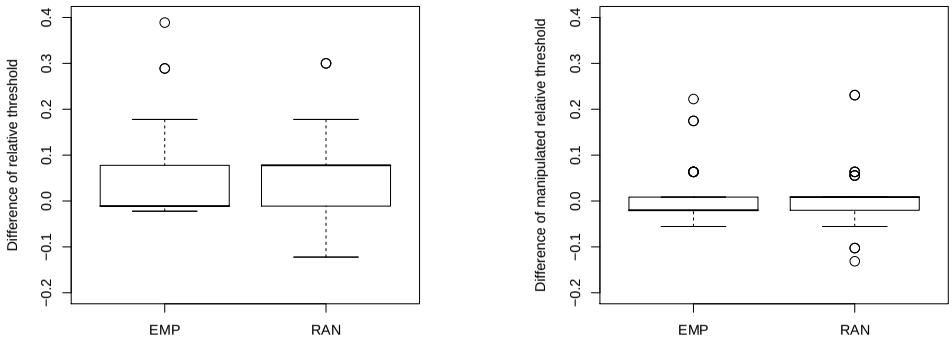


(a) Acceptance Frequencies Y



(b) Acceptance Frequencies Z

Figure 4: Acceptance Frequencies of Offers after *keep* in Treatments EMP and RAN



(a) Δ relative thresholds of Y (b) Δ transformed thresholds of Y

Figure 5: Differences of relative thresholds of Y between treatments

Jena Economic Research Papers 2008-011