The Robustness of Laboratory Gift Exchange: A Reconsideration*

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Abstract

We report a gift exchange experiment in which we systematically vary the following experimental design and implementation characteristics: the choice of equilibrium (interior versus corner point), the extent of potential efficiency gains, and the choice of frames (abstract versus employer-worker). We also employ a matching mechanism that has been shown to best preserve the nature of one-shot interactions (rotation).

Much of the observed play of our participants, especially responders, is at or close to equilibrium. Our results therefore stand in stark contrast to much of what has been reported in the literature. Specifically, we find little evidence for positive reciprocity. We do find more evidence for negative reciprocity which, however, is relatively inexpensive in our setting.

Our results, together with other evidence we discuss, suggest that laboratory gift exchange is highly sensitive to the parametrization of the model and the way the model is implemented; they question the wide-spread belief that trust and reciprocity are robust phenomena.

Keywords: laboratory gift exchange, anomalies, robustness

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1 Introduction

Numerous studies have shown that many people trust and reciprocate positively (e.g., Berg et al. 1995; Ortmann et al. 2000; Fehr et al. 1993; Fehr et al. 1997; Maximiano et al. 2007a,b). Surveying the research on trust and reciprocity, Van der Heijden et al. (2001) assert that "by now there is much experimental evidence that people engage in reciprocal exchange. People are observed to return favors even in the absence of binding pre-commitments" (p. 280; see also Fehr and Gächter 1998). The evidence has been interpreted as showing that "the exclusive reliance on selfishness and, in particular, the neglect of reciprocity motives may lead to wrong predictions and to wrong normative inferences" (Fehr et al. 1997, p. 833). Relatedly, it has been argued that financial incentives are likely to undermine the intrinsic motivation that people allegedly are endowed with.[1] The seemingly robust findings on trust, reciprocity, and intrinsic motivation suggest that *homo economicus* is a myth and that the incentive compatible design of institutions and organizations may be less imperative than some economists have argued (e.g., Smith 1759 and Smith 1776, especially book V; or Tirole 2006).

The well-documented (e.g., Fehr and Gächter 1998), now classic, results of laboratory gift exchange have captured economists' attention because they contradict canonical theory's predictions[2] for one-shot or finitely repeated interactions.[3] Below we argue that most classic gift exchange studies have features that

[1] It is not without a certain irony that, as economists start talking about intrinsic motivation, a sentiment is growing rapidly among psychologists that it is a myth rather than a reality (Eisenberger and Cameron 1996; Eisenberger et al. 1999; see also Hertwig and Ortmann 2001, p. 396).

[2] New theoretical developments (e.g., McKelvey and Palfrey 1995, 1998; Anderson et al. 1998, 2001; Goeree and Holt 2001; see also earlier Reny 1992 for similar arguments) have incorporated noise into explanatory models of experimental data. Canonical theory, such as that found in standard micro graduate textbooks (e.g., Kreps 1990 and Mas-Colell et al. 1995), does not. While game theory does allow formally for all kinds of preferences, in the present context canonical theory is meant to be understood as rational income maximization.

[3] The experimental results on gift exchange are, however, quite in line with game theoretic predictions for indefinitely repeated interactions. Hoffman et al. (1996, p. 300) suggested that most people conceptualize important aspects of real life in such a manner and accordingly import into the laboratory decision rules that evolved for contexts where seemingly trusting or reciprocal behavior may be observationally equivalent to selfish behavior. Fundamentally, this view questions whether one-shot and finitely repeated games of the social dilemma kind are at all implementable in the laboratory (see also Ortmann and Hertwig 2000; Harrison and Rutström 2001; Samuelson 2005; Binmore 2007).

do not give the canonical theory for one-shot and finitely repeated games its best shot. Specifically, much of the literature features corner point equilibria that allow only for deviations consistent with trust and positive reciprocity and thus systematically bias results in that direction whenever subject behavior is noisy (as much of experimental participant behavior surely is).[4] In addition, the typical corner equilibrium tends to be unattractive because it yields only minimal payoffs for the subjects and hence gives them substantial incentives to move away from the equilibrium. This effect is often reinforced by dramatic potential efficiency gains. For example, in Fehr et al. (1993) the achievable efficiency gains were up to 1100% and were still 300% at the maximal possible effort. Furthermore, offering a higher wage implied small risk and could not lead to negative payoffs in Fehr et al. (1993), because the wage cost paid by the employer were equal to the product of wage and effort. Hence an employer had a substantial incentive to initiate cooperation at an above equilibrium wage-effort combination, without running the risk of being exploited. And, since the cost to the worker of providing effort was trivial, workers had a (subjective) incentive to reciprocate (especially if the matching mechanism did not best preserve the one-shot nature of the interaction.) Importantly, laboratory gift exchange studies have often been implemented in problematic ways as regards, for example, such aspects as framing, anonymity, subject pools, and matching schemes.

The robustness of laboratory gift exchange to such parametrization and implementation issues has, until recently, received little attention.[5] For the present context, robustness is understood to encompass the following three meanings: One, which we shall call third-degree robustness below, simply denotes the variability within one cell of the design matrix (e.g., Table 1 below), i.e., robustness to replication. For gift exchange to be a notable phenomenon, it should be essentially immune to replication. Another meaning, which we shall call second-degree robustness below, is concerned with the stability of experimental results to variations in experimental procedures such as framing, nature of the task, anonymity, subject pools,

[4] That subject behavior is noisy is generally accepted. It is this fact that has prompted insightful probabilistic choice models that have been able to rationalize systematic patterns of "anomalous" subject behavior, for example, in public good provision experiments that feature corner point equilibria (Holt and Laury forthcoming; Goeree et al. 2002). Interestingly, in their review of results from public good provision experiments with interior Nash equilibria (Laury and Holt forthcoming), the authors do not find persuasive evidence that moving equilibria away from corner points always produces behavior closer to the predictions of canonical game theory.

[5] See our discussion of related literature in Section 5; see also Gächter and Fehr (2002).

and other implementation details. For gift exchange to be a notable phenomenon, it should be essentially immune to implementation details, too. Finally, the meaning that we shall call first-degree robustness below, refers to sensitivity towards parametrization characteristics such as the nature of the equilibrium (corner versus interior), the degree of possible efficiency gains, the degree of asymmetry between the surplus that employers and workers can capture, the risk to the employer of being exploited when trusting, and the cost to the worker of reciprocating. While such kind of parametric variation should be expected to lead to some variation in gift exchange, its very existence should not fundamentally be undermined for gift exchange to be a robust phenomenon. If it were, then the spotlight ought to be trained on issues of external validity (and calibration).

One way to think about the distinction between first-degree robustness and second-degree robustness is whether it entails changes of the parameters of the underlying game that is being used to implement the experiment: second-degree robustness is tied to the (lack of) variability in results that comes from implementation details typically not part of the underlying model, while first-degree robustness is tied to the (lack of) variability in results that comes from changes in the parameters of the game. These two kinds of robustness are linked to the issue of the generalizability, or external validity, of laboratory results (Levitt and List 2007). We return to the issue in our concluding discussion. The distinction between first-degree and second-degree robustness is not always straightforward: the choice of matching scheme (which in principle is an implementation issue), can, for example, affect the equilibrium. Likewise, when implementing real-effort tasks (e.g., Brüggen and Strobel 2007; Gneezy and List 2006) actual effort costs are unknown and hence equilibrium predictions might be affected.

Most things we could learn about robustness in the past we had to learn across studies. Namely, Fehr et al. (1993) and Fehr et al. (1997) studied one-sided auctions while, for essentially the same parameterizations, Fehr and Falk (1999) studied double auctions and Falk et al. (1999) and Gächter and Falk (2002) used a matching scheme that best preserves the nature of one-shot interactions ("rotation matching"; see Kamecke 1997); there are no qualitative differences in trust and reciprocity across these studies. Fehr et al. (1998a) systematically compare a market with partners treatment and a market with an excess supply of workers, and again find no difference in behavior. Falk et al. (1999) and Gächter and

Falk (2002) compare rotation matching and partners matching and find, not surprisingly, higher efforts and an endgame effect in the latter.

Addressing the issue of subject pools, the studies by Gächter and Falk (2002) and Falk et al. (1999) compare Austrian and Hungarian subjects; while the authors find somewhat higher effort in Hungary, they replicate the findings from earlier studies of positive reciprocity on the part of Austrian subjects. Similar results have been found by various studies in the USA, the Netherlands, and Switzerland. Relatedly, Fehr et al. (1998a) study trust and reciprocity of student and military subjects and find no difference here either. In an earlier, now well-known study, Fehr et al. (2002) investigated the impact of high stakes on subjects in Russia and, somewhat surprisingly, find trust and reciprocity alive and well.

A number of frames have also been used (e.g, buyer-seller in Fehr et al. 1993, and Fehr et al. 1997, and employer-worker in Fehr, et al. 1998b) and again the results do not seem to differ across different frames. For other treatments, see Gächter and Fehr (2002). The bottom line is that none of these variations and robustness tests destroyed trust and reciprocity in a fundamental manner. In contrast, recent investigations that systematically vary the aspects of the first-degree and second-degree robustness of laboratory gift exchange do question the belief that trust and reciprocity are robust phenomena as defined here. Interesting recent examples include: List (2006), Gneezy and List (2006), Pereira et al. (2006), Healy (2007), Maximiano et. al. (2007a,b). We will address these recent studies below in our discussion section 5.

Here we report a gift exchange experiment in which we systematically vary the following experimental design and implementation characteristics. First, we compare interior and corner point equilibria. Second, we systematically vary the efficiency gains. Third, we systematically vary the framing of the laboratory decision problem. We also employ a matching mechanism that has been shown to best preserve the nature of one-shot strategic situations. We therefore address issues of both first-degree and second-degree robustness. In essence, we give the canonical theory for one-shot and finitely repeated games a good shot at proving itself.

The paper is structured as follows: In Section 2 we present our model of gift exchange. In Section 3 we present our experimental design and implementation and in Section 4 we present our results. Section 5 provides a brief interpretation of our results and relates them to the literature. In Section 6 we proffer some concluding remarks.

2 Our model of gift exchange

Gift exchange games are sequential principal-agent games in which the first mover (a principal such as an employer) can propose to the second mover (an agent such as a worker) an incomplete contract. The key characteristic of this contract is that a generous offer on the part of the principal, if reciprocated, will lead to Pareto improvements. In one-shot games, reciprocal behavior would contradict canonical game theory's reliance on selfishness. Likewise, generous offers would be inconsistent with rational expectations of selfishness.[6]

For ease of exposition, we use employer - worker interaction to explain our model in which a principal chooses a wage and suggests an effort. While neither principal (employer) nor agent (worker) can adjust a wage that the principal has decided to offer, the agent can adjust his effort. Both gross revenue and effort cost are increasing in effort, typically in such a manner that there are efficiency gains. The wage partly determines the transfer from the employer to the worker.

A key element of all gift-exchange games is the cost function of effort (Rigdon 2002). Typically, marginal costs of effort are assumed to be increasing. Here we follow this widely used and intuitive assumption. Specifically, we use the following two cost schedules $c_1$and c_2.

e	1.0	1.2	1.4	1.6	1.8	2.0	2.2	2.4	2.6	2.8	3.0
$c_1(e)$	0	1	2	4	6	9	12	16	20	25	30
$c_2(e)$	0	1	2	4	6	9	12	15	19	23	27

Note that these cost schedules differ only for high effort choices, and even there do so only slightly. As we will see presently, the choice of either cost schedule has no effect on the equilibria but they do allow us, in conjunction with a productivity parameter, to construct high and low potential efficiency gains.

Payoffs for workers and employers in the *interior equilibrium* treatments are given by

$$U = w\left(\min\left(1 + \frac{e-1}{2}, 1.5\right)\right) - c(e)$$

and

[6] In finitely repeated games, by standard backward induction arguments, both generous offers and reciprocal behavior would be inconsistent with common knowledge of rationality and selfishness.

$$\Pi = em - w\left(\min\left(1 + \frac{e-1}{2}, 1.5\right)\right)$$

where $m \in \{50, 80\}$[7]

A couple of comments are in order: First, m is a multiplier that scales employers' return on workers' effort. It is useful to think of m as a productivity parameter. Second, the (gross) payoff function for workers is increasing in the wage w throughout and in effort for $e \in [1.0, 2.0]$. Specifically, it is linear in effort with slope $\frac{w}{2}$ for $e \in [1.0, 2.0)$ and constant for $e \geq 2$. Thus, the marginal (gross) payoff function is first positive at $\frac{w}{2}$ and then drops to zero.[8] Since marginal costs are positive and increasing, the payoff maximizing effort for the workers is (weakly) monotonic in wage but never exceeds 2.

Specifically, the best-reply schedule of workers is

$$e^*(w) \begin{cases} = 1.0 & \text{for } w < 10 \\ \in \{1.0, 1.2, 1.4\} & \text{for } w = 10 \\ = 1.4 & \text{for } 10 < w < 20 \\ \in \{1.4, 1.6, 1.8\} & \text{for } w = 20 \\ = 1.8 & \text{for } 20 < w < 30 \\ \in \{1.8, 2.0\} & \text{for } w = 30 \\ = 2.0 & \text{for } w > 30 \end{cases}$$

Note that the best-reply schedule of workers is the same for cost schedules c_1 and c_2. Since higher wages yield higher effort (given selfishness of workers), the profit maximizing wage offer exceeds the minimal wage for m sufficiently large. This is particularly true for the values of m we have chosen here, $m = 50$ and $m = 80$, which both induce interior equilibria. In fact, given the values of m we have chosen our configuration yields the same two subgame-perfect equilibria, namely $w^* = 20, e^* = 1.8$ (if workers choose for $w = 20$ the

[7] The admissible values for w were $w \in [0, 100]$ if $m = 50$ and $w \in [0, 200]$ if $m = 80$.

[8] A commentator suggested that the transfer function $w\left(\min\left(1 + \frac{e-1}{2}, 1.5\right)\right)$ looked somewhat scary and wondered whether our participants understood the payoff function. In Section 3.2 we explain how we insured that they did. The specific form of the payoff function is motivated simply by our attempt to introduce interior equilibria in wages and efforts; other ways to introduce interior equilibria in effort are possible (e.g, Pereira et al. 2006) but arguably not any more persuasive. We note that our payoff functions are economically not unrealistic. They reflect profit-sharing agreements where workers receive a bonus (that is some percentage of the base wage) that depends on profits but is capped at some level.

maximal effort from the available set of best replies $\{1.4, 1.6, 1.8\}$) and (otherwise) $w^* = 21, e^* = 1.8$.[9] The equilibrium payoffs in the first case are

$$\begin{aligned} U &= 1.4w - c(e) = 28 - 6 = 22 \text{ and} \\ \Pi &= 1.8m - 1.4w = 90 - 28 = 62 \text{ for } m = 50 \text{ and} \\ &= 144 - 28 = 116 \text{ for } m = 80 \end{aligned}$$

and in the second case are

$$\begin{aligned} U &= 1.4w - c(e) = 29.4 - 6 = 23.4 \text{ and} \\ \Pi &= 1.8m - 1.4w = 90 - 29.4 = 60.6 \text{ for } m = 50 \text{ and} \\ &= 144 - 29.4 = 114.6 \text{ for } m = 80. \end{aligned}$$

The equilibria clearly favor employers.[10] Employers, however, bear all the risk of initiating cooperative outcomes.

Note that the equilibrium effort is below the maximally inducible effort 2.0 which requires a wage of 30 (or 31 if the worker chooses the lower effort from the set of best replies when indifferent). An equal split of the maximal joint payoff is achieved at $w = 60, e = 3.0$ for $m = 50$ (yielding $U = \Pi = 60$) and at $w = 89, e = 3.0$ for $m = 80$ (and cost schedule c_2) (yielding $U = \Pi = 106.5$).

Since a major goal of our study was to better understand the effects of potential efficiency gains, our interior equilibrium treatments (while featuring the same interior equilibria) are distinguished accordingly: For the treatments with high efficiency gains we used the larger multiplier $m = 80$ and the flatter cost curve c_2.[11] This implies efficiency gains at the equilibrium effort (i.e. when the effort is increased from 1.8 to 2.0) of 433% (since the employer gains 16 at a cost of 3 for the worker) and at the maximal effort (i.e. when the effort is increased from 2.8 to 3.0) of 300%. For the treatments with low efficiency gains we used

[9] This multiplicity is caused by our restriction to integer wages.

[10] This, arguably, is a desirable feature. Equilibria that favor employees seem at odds with reality.

[11] We chose two cost schedules that, to recall, were identical in the lower effort range (including and slightly exceeding the equilibrium effort) but differed slightly in higher effort choices. The first fact guaranteed that the equilibria would not be affected. The second fact allowed us to increase the difference between our high and low efficiency gain treatments.

the smaller multiplier $m = 50$ and the steeper cost curve c_1; for this configuration the efficiency gains are 233% at the equilibrium effort and 100% at the maximal effort.

In yet another treatment we induced an equilibrium near the corner point (which for the sake of economy of expression we call corner point equilibrium or corner equilibrium). Specifically, we replaced the transfer function $\left(w\left(\min\left(1+\frac{e-1}{2}, 1.5\right)\right)\right)$ with one that is constant in effort for $e > 1.0$. The payoff functions for workers and employers were thus given by

$$U = wn - c_1(e)$$

and

$$\Pi = 50e - wn$$

with $n = 1.0$ for $e = 1.0$ and $n = 1.5$ for $e > 1.0$. Thus, workers' marginal (gross) payoff function is 0 for $e \geq 1.2$. The best-reply schedule for workers is thence

$$e^*(w) \begin{cases} = 1.0 & \text{for } w < 2 \\ \in \{1.0, 1.2\} & \text{for } w = 2 \\ = 1.2 & \text{for } w > 2 \end{cases}$$

which yields the (subgame-perfect) equilibria $w^* = 2, e^* = 1.2$ (if workers choose for $w = 2$ the maximal effort from the available set of best replies $\{1.0, 1.2\}$) and (otherwise) $w^* = 3, e^* = 1.2$. The equilibrium payoffs in the first case are

$$\begin{aligned} U &= 1.5w - c(e) = 3 - 1 = 2 \text{ and} \\ \Pi &= 1.2m - 1.5w = 60 - 3 = 57 \end{aligned}$$

and in the second case are

$$\begin{aligned} U &= 1.5w - c(e) = 4.5 - 1 = 3.5 \text{ and} \\ \Pi &= 1.2m - 1.5w = 60 - 4.5 = 55.5. \end{aligned}$$

These equilibria are even more biased in favor of employers, but also imply high risks for them. On the one hand, punishing an employer for a low offer by rejecting it (which yields a payoff of 0 for both players) becomes relatively inexpensive for a worker. On the other hand, exploiting an employer who has offered a high wage becomes relatively expensive for an employer, since the best-reply effort of the worker is lower. For example if the employer offers $w = 60$ (which coupled with $e = 3.0$ would still lead to a fair split $U = \Pi = 60$) and the worker chooses the best reply $e = 1.2$ then the payoffs are $U = 89, \Pi = -30$, whereas in the interior equilibrium low efficiency treatment the best reply is $e = 2.0$ which leads to payoffs $U = 81, \Pi = 10$.

3 Experimental design and implementation

3.1 Experimental design

Drawing on our model of gift exchange, we developed treatments along three dimensions, namely the nature of the equilibrium (interior vs. corner), efficiency gains, and frames. (A fourth dimension was added by our use of two subject pools, one of them on a site which has produced many of the classic gift exchange data.)

For the interior equilibria we chose two realizations of efficiency gains (low and high, from here on denoted L and H) and two realizations of frames, namely one frame using abstract descriptors and another one using employer-worker interactions (from here on A and EW). The arguments in favor of real-world frames are spelled out in Ortmann and Gigerenzer (1997) and Harrison and Rutström (2001).

In contrast, for the corner-point equilibrium we chose only the low efficiency gains and the abstract frame. We consider corner equilibria the most striking feature of the classic gift-exchange experiments. The present treatment was hence designed both as a benchmark of sorts and as a treatment meant to explore how the presence of a corner-point equilibrium affects attempts to induce reciprocity. We chose the equilibrium to be slightly off the corner point of zero wage and minimal effort in order to keep a fundamental aspect of our interior equilibria treatments, namely that employers can induce a somewhat higher effort from a rational and selfish worker by paying a positive wage. This feature is dramatically reduced but not eliminated in our corner-point treatment.

All in all, we conducted 13 sessions. For details of the design see Table 1; details of the exact implementation of the design follow in Section 3.2 below.

Frame	Interior equilibrium		"Corner"
	low eff gains [L]	high eff gains [H]	equilibrium [C]
abstract [A]	3 (B)	2(Z)	2(B)
empl-wrkr [EW]	4(2B, 2Z)	2(Z)	

Table 1: Number of sessions for the individual treatments. Z=Zurich, B=Berlin

3.2 Experimental implementation

All sessions were conducted in the experimental laboratories of the economics department at Humboldt-University Berlin (B) or the Institute for Empirical Research in Economics at the University of Zurich (Z). The exact breakdown is indicated in Table 1. Subjects were recruited in line with the standard procedures in the two labs. The Berlin subject pool was predominantly economics and business administration students; the Zurich participants were from a wide variety of fields. For the treatment EW-L two sessions each were conducted in Berlin and Zurich. While both wage offers and efforts were somewhat higher in Zurich, the differences are far from statistically significant (see Tables 5 and 9 in Section 4 for details.) Hence in the descriptive statistics we report pooled data from the Zurich and Berlin sessions in this treatment.

Participants seated themselves (Berlin) or were seated randomly (Zurich). Excess subjects were paid a show-up fee that was in line with the conventions in the respective labs, i.e. DM 10 (EUR 5.11) in Berlin and CHF 10 (about EUR 6.50) in Zurich.

Instructions (which were in German - for a sample translation see the appendix; for the complete set of instructions see http://home.cerge-ei.cz/Ortmann/instructions.html) were then read aloud. Questions were answered in the usual manner (e.g., privately or by repetition of the relevant passage of the instructions). At the end of the instructions we asked a dozen control questions to identify subject confusion. An experimental session was started only after all subjects had answered all questions correctly. Each set of instructions included a flow diagram that illustrated the sequencing of decisions and summarized the key parameters of the session. The relation between the effort and the transfer from the employer to the worker was explained with the aid of a wage multiplier that depended on the effort. This multiplier was included in the table below the flow diagram that presented the cost schedule. (For a sample of such a flow diagram see http://home.cerge-ei.cz/Ortmann/instructions.html.)

The experimental software was developed and the experiments were run in z-Tree (Fischbacher 2007). The experimental software included a profit calculator that was displayed on the left side of the computer screen and that allowed subjects to calculate both workers' and employers' payoffs for all admissible wage-effort pairs. There was no restriction on the time subjects could use the calculator. Subjects used the calculator extensively and intensively.[12]

Each experimental session employed 12 subjects which were randomly assigned to one of the two roles (by seating themselves or being seated) and kept these roles throughout. The number of subjects was limited by the number of seats in the Berlin laboratory. Each of the subjects in a session was matched with each participant in the other role ("rotation matching") which has been shown to best preserve the one-shot nature of the interaction by precluding any indirect reputation or spill-over effect (Kamecke 1997). We explained to subjects that their behavior in any one period could not affect any future interactions. For the exact wording, see the second paragraph of the sample instructions. We ran the maximum number of periods (6) possible under this matching procedure.

In each period each employer had to make an offer to the worker they were matched with for that period; they could enter that wage offer and an effort they suggested to the worker on the right hand side of the computer screen. Wage offers had to be integers between 0 and 100 (for $m = 50$) or 0 and 200 (for $m = 80$). After all employers had made their offers, the workers were informed about their wage offer and the effort suggested by the employer they were matched with; they were then asked to enter their decision whether to accept or reject the offer and, in case of acceptance, their effort level. A rejection led to zero payoffs for employers and workers. When all workers had made their choice, all players were informed

[12] Specifically, individual employers used the calculator in at least 2 periods (mean 5.49, 52 of 78 employers used the calculator in all six periods). In the periods in which the calculator was used, individual employers on average made 16.9 computations per period (median 12, maximum 123). The average time that an employer used the calculator was 109.5 seconds per period (median 86 seconds, maximum 548 seconds). Workers used the calculator in more periods. While one worker never did, the others did so in at least 4 periods, 68 of 78 in all six periods (mean 5.76). On the other hand, workers performed fewer computations per period (mean 9.9, median 9, maximum 33) and used the calculator for a shorter time (mean 67.2 seconds, median 57 seconds, maximum 379 seconds). The reason seems to be that most workers did not perform any computations for a wage other than that chosen by the employer.

Frame	Interior equilibrium		"Corner"
	low eff gains [L]	high eff gains [H]	equilibrium [C]
abstract [A]	31.4 (108)	37.4 (72)	22.9 (72)
	1.78 (102)	1.68 (64)	1.33 (56)
empl-wrkr [EW]	32.3 (144)	51.4 (72)	
	1.73 (137)	1.84 (70)	

Table 2: Average wage offers (top) and average efforts in case of acceptance (bottom) by treatment. Low efficiency gains correspond to $m = 50$ and cost schedule c_1, high efficiency gains to $m = 80$ and cost schedule c_2. Number of data points on which averages are based in parentheses.

Frame	Interior equilibrium		"Corner"
	low eff gains [L]	high eff gains [H]	equilibrium [C]
abstract [A]	32.49 (102)	40.3 (64)	27.5 (56)
	12.5 (6)	14.5 (8)	6.94 (16)
empl-wrkr [EW]	33.24 (137)	52.21 (70)	
	12.86 (7)	23 (2)	

Table 3: Average wage offers by treatment, separated into those accepted (top) and those rejected (bottom). Low efficiency gains correspond to $m = 50$ and cost schedule c_1, high efficiency gains to $m = 80$ and cost schedule c_2. Numbers of observations on which averages are based in parentheses.

about the choices in their pair and about their own payoff. No subject was ever informed about the choices of any other employer or worker.

4 Results

Descriptive statistics. In Table 2 (which uses as a template Table 1) we report for each treatment in the top row the average wage offers across all experimental sessions (and in parentheses the number of data points on which the averages are based) and in the bottom row the average effort in case of acceptance (and in parentheses the number of data points on which the averages are based).

In Table 3 we condition the wage offer data on whether an offer was accepted or rejected; in each cell the first row denotes the acceptance and the second the rejection case. The distribution of wage offers and efforts chosen (in case of acceptance) is shown in Figures 1 and 2, respectively.

We observe the following facts.

Wage offers are somewhat above the equilibrium, in particular:

1. The majority of wage offers are clustered slightly above the equilibrium offer for all interior equilibria treatments except the EW-H one. The majority of wage offers in the EW-H treatment lie substantially above the equilibrium.

2. In the C treatment, the majority of wage offers are below the wage offers in the interior equilibrium treatments. However, there is a non-negligible number of very high wage offers as well. Consequently, average wage offers in treatment C are substantially above those predicted by the corner point equilibrium.

   The interaction of high efficiency gains and the EW frame clearly influences wage offers. In treatment C, wage offers are much more dispersed. Specifically, we observe these facts:

3. The distribution of wage offers is, for the interior equilibria treatments, clearly affected by the efficiency gains. In the abstract frame, this effect materializes only as a relatively low number of high wage offers for the A-H treatment. In contrast, for EW-H the whole wage offers distribution is shifted to the right relative to that in the EW-L frame. These trends are also reflected in the average wage offers. In the abstract frame, the relatively small number of very high wage offers translates into an increase of approximately 20 % only. In contrast, for EW-H, average wage offers are about 60 % higher than in EW-L. Using the variance in the individual sessions as independent observations, the variance of the treatments with high efficiency gains is clearly and significantly higher than that in the treatments with low efficiency gains (Mann-Whitney test, $p < 0.01$).

4. The distribution of wage offers seems, for the interior equilibria, also affected by the frame. For low efficiency gains, we observe for the EW frame a higher share of offers both below and substantially above the equilibrium, resulting in nearly identical averages for EW and A frames. In contrast, for high efficiency gains, we again observe a substantial upward shift of the whole distribution, resulting in about 40 % higher average wages in EW.

5. Together, high efficiency gains and the EW frame lead to a substantial shift upward in wage offers relative to the A-L treatment, with high efficiency gains (A-H vs A-L) and the EW frame (EW-L vs A-L) alone having much less of an impact.

6. Returning to the C treatment, we see a more dispersed set of wage offers (low equilibrium wages cause a higher number of low wages, but also low equilibrium efficiency leads to more high wage offers). Using the variance in the individual sessions as independent observations, the difference between the corner equilibrium treatment and the other treatments with low efficiency gains just misses significance (Mann-Whitney test, $p = 0.14$) which, given that there are only two sessions in the C treatment, strikes us as remarkable.

   Effort choices are close to the best replies to wage offers, and workers sometimes react to low wage offers with rejections. Specifically, we observe these facts:

7. As Table 3 shows, in all treatments (interior and corner) rejections of wage offers are triggered by comparatively low wage offers.

8. In contrast to wage offers, there are no discernible differences in effort choices across interior equilibrium treatments. Since, furthermore, efforts are clustered at equilibrium and maximal best-reply, the average effort choices are close to the equilibrium in all treatments.

9. The only difference that might qualify as discernible are the effort choices in EW-H which overwhelmingly are at the maximal best-reply effort and likely result from the higher wage offers in EW-H. See also Table 8 below.

10. Returning once more to the C treatment, we note that virtually all effort choices are at the equilibrium and that the number of rejections is substantially higher than in the other treatments.

Table 4 shows the average payoffs for employers and workers by treatment (in Experimental Currency Units and excluding show-up fees to keep the Berlin and Zurich data comparable).

Statistical analysis. Observations within sessions are not independent. To analyze whether the treatment variables have significant influence on wage offers and effort choices, we estimate random-effects regression models with the sessions as independent units of observations. Table 5 reports the coefficients for dummy variables for high efficiency gains, employer-worker frame, the interaction effect of high efficiency gains and employer-worker frame, the corner point equilibrium and Zurich sessions. The left column refers to the

Frame	Interior equilibrium		"Corner"
	low eff gains [L]	high eff gains [H]	equilibrium [C]
abstract [A]	246	419	118
	221	264	180
empl-wrkr [EW]	231	421	
	226	394	

Table 4: Average payoffs by treatment for employers (top) and workers (bottom) in Experimental Currency Units. ECUs were exchanged in the L treatments at a rate of 1 ECU=0.10 DM (Berlin) or 1 ECU=0.10 CHF (Zurich) and in the H treatments at a rate of 1 ECU=0.05 CHF. Participants in Zurich were paid a show-up fee of 10 CHF in addition.

analysis for wage offers, the middle column to effort choices, and the right column to excess efforts, i.e. differences between efforts and best-reply efforts.

The only significant influence on effort choices is the corner equilibrium. In line with the theoretical prediction, effort is substantially and significantly lower than in the interior equilibrium treatments. Furthermore, the corner equilibrium has a positive impact on the difference between effort and best-reply effort (probably because negative differences were restricted to 0.2 and negative reciprocity was hence executed by rejections.) Confirming the descriptive statistics, the only significant determinant of wage offers is the interaction of the extent of efficiency gains with the frame: High efficiency gains coupled with the employer-worker frame extract significantly larger wage offers. High efficiency gains or the employer-worker frame by themselves do not have a significant or substantial impact. This is also confirmed by a separate analysis testing directly for the impact of high efficiency gains in the different frames (see Table 6), which shows a significant effect of high efficiency gains only in the EW frame. We particularly note that the dummy variable Zurich has neither substantial nor significant influence on wage offers, efforts or excess efforts (see Table 5 and Table 7 which provides a direct test for the influence of the Zurich dummy in the only treatment where sessions were run in both Berlin and Zurich, namely the treatment with employer-worker frame and low efficiency gains).[13]

Trust and reciprocity. As we have seen, wage offers are somewhat higher than equilibrium would dictate. We emphasize that this could be trust in positive reciprocity or, similar to what we typically observe in

[13] Dropping the Zurich dummy does not change any level of significance with respect to the threshold values $p = 0.01$, $p = 0.05$, $p = 0.1$ in the analyses presented in Table 5.

	Wage Offer	Effort	Effort - Best Reply Effort
Constant	31.38 (8.390)**	1.789 (21.664)**	-0.068 (-1.196)
High	2.329 (0.265)	-0.258 (-1.331)	-0.161 (-1.202)
EW Frame	-0.991 (-0.168)	-0.130 (-0.994)	-0.090 (-1.005)
High $\times$ EW	14.963 (1.706)$^{+}$	0.300 (1.548)	0.142 (1.066)
Corner-Point	-8.449 (-1.429)	-0.465 (-3.497)**	0.194 (2.099)*
Zurich	3.722 (0.575)	0.142 (0.995)	0.083 (0.843)

Table 5: Random-effects regression for wage offers (column 2), efforts (column 3) and differences between effort and best-reply effort (column 4). The rows list coefficients for the following dummy variables: high efficiency gains (High), employer-worker frame (E-W frame), the interaction effect of high efficiency gains and employer worker frame (High x E-W), corner point equilibrium (Corner), and Zurich sessions (Zurich), z-statistics are given in parentheses. $+$ = significant at $p = .1$ $*$ = significant at $p = .05$, $**$ = significant at $p = .01$.

	Dependent Variable	Abstract Frame	EW Frame
High	Wage Offer	6.051 (0.863)	19.153 (6.092)**
	Effort	-0.117 (-0.787)	0.113 (1.036)
	Effort $-$ Best Reply Effort	-0.078 (-0.876)	0.023 (0.337)

Table 6: Random-effects regression for the impact of the dummy variable "High" (for high efficiency gains) on wage offers (row 2), efforts (row 3), and on the difference between effort and best-reply effort (row 4). The regressions are run separately for the treatments with abstract frame (column 3) (not including the corner-point treatment) and the treatments with employer-worker frame (column 4). z-statistics are given in parentheses. $+$ = significant at $p = .1$ $*$ = significant at $p = .05$, $**$ = significant at $p = .01$.

ultimatum games, it could be an attempt to prevent negative reciprocity. Of course, it could also reflect altruism or inequality aversion given that the equilibrium payoffs (which subjects had time to evaluate) favored the employer. Little such “generous” behavior is found on the worker side. Table 8 shows, for each treatment, the relative number of effort choices that are equal to, above, or below workers’ best replies to actual wage offers as well as the numbers of rejections. (In case of a wage offer that let the worker be indifferent, i.e. 10, 20, or 30, we counted any of the efforts in the set of best replies as equal to the best reply.)

As Table 8 shows, in all treatments the vast majority of effort choices (60%) is at the best reply and more effort choices are below (22%) the best reply than are above (10%). Since the best reply is always in the lower half of the range of possible efforts, random errors should produce deviations towards choices above the best reply rather than below. Using selfishness of the worker as a benchmark, positive reciprocity

	Wage Offer	Effort	Effort − Best Reply Effort
Zurich	3.722 (1.167)	0.142 (0.970)	0.083 (0.864)

Table 7: Random-effects regression for the impact of the dummy variable "Zurich" (for Zurich sessions) on wage offers (column 2), efforts (column 3), and on the difference between effort and best-reply effort (column 4) in treatment EW-L. z-statistics are given in parentheses. $+$ = significant at $p = .1$ $*$ = significant at $p = .05$, $**$ = significant at $p = .01$.

	Below BR	Equal BR	Above BR	Rejection
A-L	23 (21%)	70 (65%)	9 (8%)	6 (6%)
A-H	22 (31%)	32 (44%)	10 (14%)	8 (11%)
EW-L	44 (31%)	80 (56%)	13 (9%)	7 (5%)
EW-H	15 (21%)	48 (67%)	7 (10%)	2 (3%)
Subtotal	104 (26%)	230 (58%)	39 (10%)	23 (6%)
C-A	0 (0%)	49 (68%)	7 (10%)	16 (22%)
Total	104 (22%)	279 (60%)	46 (10%)	39 (8%)

Table 8: Absolut and relative numbers of effort choices below (column 2), at (column 3), or above the worker's best reply (column 4) and of rejections (column 5)

would imply effort choices above the best reply in reaction to high wage offers, while negative reciprocity would lead to effort below the best reply in the case of low wage offers. Figure 3 shows the average deviation of effort from the best reply dependent on the worker's payoff implied by best-reply effort for the given wage offer.

As can be seen from Figure 3 there is a positive relation between the effort - best reply difference and workers' payoff (at best-reply effort). Since the difference is, however, rarely positive, our result seems in line with negative rather than positive reciprocity.[14]

[14] Note that in the interior equilibrium treatments negative reciprocity is relatively cheap at low wages (where only it applies). For example, for wages 11, 21, and 31, choosing an effort of 1.0 instead of the best reply effort (1.4, 1.8, or 2.0, respectively) costs the worker 0.2, 2.4, or 6.5, respectively, but the employer loses 17.8, 31.6, or 34.5, respectively, in the treatments with low efficiency gains, and even 29.8, 55.6, or 64.5, in the treatments with high efficiency gains. The relative costs of negative reciprocity by rejections (instead of best-reply effort) are, for non-trivial wages, higher, e.g. at wages 11 or 21 the costs for the worker are 11.2 or 23.4 and the loss for the employer (i.e. the punishment) is 56.8 or 60.6 (low efficiency gains treatment) and 98.8 or 114.6 (high efficiency gains treatment). Positive reciprocity at high wages is somewhat more expensive than negative reciprocity (just because we did not allow dramatic efficiency gains). Since the transfer is independent of the effort for $e \geq 2.0$, the costs and benefits of positive reciprocity are independent of the wage for $w > 30$. For $e \geq 2.0$, in the low efficiency gains treatments, increasing the effort by 0.2 always yields a benefit of 10 for the employer. The marginal costs for

In fact, the preceding analysis underestimates the role of negative reciprocity since rejections of low wage offers are a form of negative reciprocity. The last column of Table 8 shows the number of rejections per treatment and Table 3 compares the average wage offers that are accepted with those that are rejected. In all treatments, average rejected offers are less than half of the average accepted wage offers, indicating that rejections are indeed a negatively reciprocal reaction to low wage offers. Only in treatment C-A are the positive differences between effort and best reply more substantial than the negative differences. This, however, is the result of the best reply being bounded by 1.2 in this treatment. Negative reciprocity could (almost) only be exercised by rejections in this treatment and the number of rejections is by far the highest in C-A (22% compared to 6% in the other treatments).[15][16]

Table 9 shows the coefficients for a random-effects regression model for the dependence of the excess effort (i.e. the difference between effort and best reply) on the wage offer, the suggested effort as well as treatment dummies. In each cell the top line refers to all treatments, the bottom line to an analysis excluding the corner-point equilibrium treatment. The left column refers to the complete data, the middle column to the data restricted to wage offers below or equal to 30 (because up to 30 the best reply is increasing in the wage offer) and the right column to wage offers larger than 30.[17] Table 10 shows the coefficients for Wage Offer and Suggested Effort in the corresponding analysis for the individual treatments.

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the worker increase from 3 (when choosing 2.2) to 4 (when increasing the effort to 2.4 or 2.6) and eventually to 5. In the high efficiency gains treatment, the marginal benefit for the employer is 16, and the marginal costs for the worker do not exceed 4.

[15] The relative costs for negative reciprocity by choosing an effort 1.0 instead of the best reply effort (which is generally 1.2) is much higher than in the interior equilibrium treatments (for wages of 3, 11, or 21, the costs for the worker are 0.5, 4.5, or 9.5, respectively and the loss for the employer 8.5, 4.5 or even -0.5). Rejections, in comparison, are more efficient as punishment. For the same wages, the costs for the worker are 3.5, 15.5, or 30.5, and the loss for the employer 55.5, 43.5, or 28.5. Due to lower marginal effort costs for lower effort, positive reciprocity is cheaper in that range than in the interior equilibrium treatments. Hence it is consistent with traditional economic reasoning that compared to the interior equilibrium treatments, we see slightly more positive reciprocity in the corner treatment and negative reciprocity exhibited by rejections instead of lower effort.

[16] Rejections in C-A are a common form of negative reciprocity as they are not concentrated in a small number of subjects. Among the twelve workers in this treatment, only five never reject an offer, while one does so once, five do so twice and one actually rejects in five of the six periods.

[17] Dropping the Zurich dummy has no substantial effects. For all wage offers (left column), High is then significant at

	Treatments	All Wage Offers	Wage Offers ≤ 30	Wage Offer > 30
Constant	all	-0.457 (-6.832)**	-0.095 (-0.775)	-0.425 (-3.201)**
	exclud. C-A	-0.471 (-6.341)**	-0.109 (-0.748)	-0.412 (-3.515)**
Wage Offer	all	0.007 (6.145)**	-0.009 (-2.140)*	0.009 (4.555)**
	exclud. C-A	0.007 (5.559)**	-0.010 (-1.977)*	0.009 (5.028)**
Suggested Effort	all	0.071 (2.143)*	0.074 (1.639)	0.040 (0.682)
	exclud. C-A	0.083 (2.341)*	0.093 (1.671)$^{+}$	0.040 (0.773)
High	all	-0.172 (-2.242)*	-0.036 (-0.304)	-0.342 (-2.614)**
	exclud. C-A	-0.169 (-2.053)*	-0.044 (-0.372)	-0.338 (-3.092)**
EW Frame	all	-0.075 (-1.478)	-0.039 (-0.521)	-0.132 (-1.500)
	exclud. C-A	-0.075 (-1.370)	-0.038 (-0.522)	-0.133 (-1.791)$^{+}$
High $\times$ EW Frame	all	0.028 (0.359)	0.003 (0.019)	0.078 (0.634)
	exclud. C-A	0.030 (0.363)	-0.010 (-0.061)	0.080 (0.774)
Corner-Point	all	0.246 (4.510)**	0.123 (1.465)	0.190 (1.727)$^{+}$
Zurich	all	0.034 (0.608)	-0.091 (-1.041)	0.136 (1.463)
	exclud. C-A	0.033 (0.540)	-0.089 (-1.017)	0.136 (1.759)$^{+}$

Table 9: Random-effects regression for the difference between effort and best-reply effort. The rows list coefficients for impact of the wage offer, the suggested effort and the dummy variables for high efficiency gains (High), employer-worker frame (E-W Frame), the interaction effect of high efficiency gains and employer worker frame (High x E-W), corner point equilibrium (Corner), and Zurich sessions (Zurich). In each cell the top row refers to regressions including all treatments, the bottom row to those excluding the corner-point equilibrium treatment. z-statistics are given in parantheses. $+$ = significant at $p = .1$, $*$ =significant at $p = .05$, $**$ = significant at $p = .01$.

	Treatment	All Wage Offers	Wage Offers <= 30	Wage Offer > 30
Wage Offer	A-L	0.012 (4.443)**	0.0001 (0.007)	0.012 (3.926)**
	A-H	0.005 (1.902)$^{+}$	-0.027 (-1.969)*	0.012 (2.408)*
	EW-L	0.011 (3.526)**	-0.002 (-0.247)	0.012 (2.538)*
	EW-H	0.005 (2.465)*	-0.038 (-2.825)*	0.007 (2.507)*
	C-A	0.010 (2.545)*	-0.001 (-0.359)	0.047 (1.054)
Suggested Effort	A-L	-0.030 (-0.490)	-0.095 (-1.014)	-0.003 (-0.037)
	A-H	0.075 (0.681)	0.205 (1.449)	-0.171 (-0.853)
	EW-L	0.086 (1.460)	0.095 (1.115)	0.092 (1.153)
	EW-H	0.090 (0.915)	-0.047 (-0.230)	0.014 (0.121)
	C-A	-0.024 (-0.218)	-0.007 (-0.320)	-0.329 (-0.407)

Table 10: Random-effects regression for the difference between effort and best-reply effort. The rows list the coefficient for the variables wage offer (upper part) and suggested effort (lower part) for the various treatments. First row: Abstract-low, second row: Abstract-high, third row: Employer-worker-low, fourth row: Employer-worker-high, fifth row: Corner-point. z-statistics are given in parentheses. $+$ = significant at $p = .1$, $*$ =significant at $p = .05$, $**$ = significant at $p = .01$.

Note that Wage Offer has a significant but small positive impact on excess efforts in all treatments. To increase the excess effort by one step (i.e. 0.2) requires to increase the wage offer by about 20. Interestingly, the impact is negative (or essentially zero) for wage offers below 30 in all treatments which implies that the increase in effort is smaller than (or roughly equal to) the increase in best-reply efforts. This negative impact might look surprising at first, but note that for very low wage offers, the best-reply effort is equal or close to the minimum possible effort. Hence there is little or no room for negative reciprocity through below best-reply efforts (and only rejections remain as a means of negative reciprocity). As wages increase, best-reply efforts increase and hence it becomes possible to deviate below the best-reply effort. For wages below 30, workers are apparently not satisfied and increase their efforts less than the best-reply efforts. Suggesting a higher effort has a slight positive effect, which does not, however, show a consistent pattern across treatments.

To summarize our results on worker behavior, we observe that there is some reciprocal relation since efforts increase in wage offers at least for parts of the wage range. In contrast to previous studies, however,

$p = 0.01$ instead of $p = 0.05$, but for wage offers > 30, it is only significant at $p = 0.05$ instead of $p = 0.01$. Furthermore, without the Zurich dummy, suggested effort is not significant any more at $p = 0.1$ for wage offers ≤ 30, and E-W Frame is not significant any more at $p = 0.1$ for wage offers > 30. There are no other changes with respect to the significance thresholds.

negative reciprocity appears to dominate positive reciprocity. Indeed, even wage offers above the equilibrium wage are followed in only 11% by efforts larger than the best reply, but in 21% by efforts below the best reply and in 4% by rejections. What might contribute to this result is that at equilibrium wage-effort combinations, employers receive substantially higher payoffs than workers, and hence also for wages slightly above the equilibrium they are better off. Thus if workers are driven by payoff comparisons as, for example, the model by Fehr and Schmidt (1999) suggests, this could explain why they are reciprocating high wages by high efforts only when this does not lead to disadvantageous inequality.

So can the Fehr-Schmidt model rationalize our data? We consider the low-efficiency gains, high-efficiency gains and corner-point treatments in turn and look at wage ranges where the Fehr-Schmidt model makes clear predictions without reverting to specific assumptions about the joint distribution of the parameters of the model. For the low-efficiency gains treatments, for $w > 40$ efforts above the (selfish) best reply do not lead to disadvantageous inequality.[18] In particular, the Fehr-Schmidt model predicts that in this wage range efforts should never be below the best reply, offers should never be rejected and for $\beta > \frac{3}{13}$ workers would choose $e > BR$. According to the rough parameter estimates that Fehr and Schmidt (1999) present, $\beta > \frac{3}{13}$ would hold for 70% of workers.[19] In our data, we find 42% of effort choices above the best reply and 4% below the best reply. Looking at the other side of the wage range, for $w < 36$ the Fehr-Schmidt model predicts that efforts are never above the best reply and $e < BR$ for $\alpha \geq \frac{1}{4}$, which should hold according to the Fehr-Schmidt estimates for 70% of workers.[20] We indeed find no efforts above the best reply and 35% of choices below the best reply. Hence the Fehr-Schmidt model is broadly consistent with the fact that efforts are above the best reply only when workers are advantaged and below the best reply (almost) only

[18] To be more precise, there is some effort level larger than the (selfish) best reply that does not imply disadvantegous inequality, while even higher efforts might still lead to disadvantageous inequality. The crucial observation is that the Fehr-Schmidt model predicts for inequality averse subjects efforts somewhat above the (selfish) best reply.

[19] Blanco et al. (2007) provide (nearly) point estimates for the parameters of the Fehr and Schmidt model and also find about 70% among their participants with $\beta > \frac{3}{13}$.

[20] The estimates by Blanco et al. (2007) of the α parameter are in line with those by Fehr and Schmidt. However, since they do not collapse the distribution to a few mass points as Fehr and Schmidt do (in particular, all with $\alpha < \frac{1}{2}$ are assigned $\alpha = 0$ in Fehr and Schmidt), in Blanco et al. (2007) the number of participants with estimated $\alpha \geq \frac{1}{4}$ is even larger than 70%.

when they are disadvantaged. We do, however, find both less positive and less negative reciprocity than the Fehr-Schmidt estimates suggest.

Similar patterns hold in the high-efficiency gains and corner point treatments. In the high-efficiency gains treatment, $w > 62$ should never lead to $e < BR$ or rejections and to $e > BR$ if $\beta > \frac{3}{19}$. Consistent with this prediction, offers are never rejected and efforts never below the best reply, but only 21% are above the best reply in contrast to the 70% to be expected according to Fehr and Schmidt. Similarly, for $w < 55$ we should never see $e > BR$ and $e < BR$ or rejections for $\alpha > \frac{1}{3}$, so in 70%. While 38% of $e < BR$ and 10% rejections are broadly consistent with this prediction, we also find 8% $e > BR$, suggesting that payoff inequalities are not necessarily the standard which workers use to determine a "fair wage" that they reciprocate by efforts above the best reply.[21] In the corner-point treatment, for $w > 23$ Fehr-Schmidt predicts no efforts below best reply or rejection and $e > BR$ for $\beta > \frac{1}{9}$. Our data indeed shows no case of $e < BR$ or rejection, but only 29% $e > BR$. For $w < 11$, Fehr-Schmidt predicts no effort above the best reply and $e < BR$ or rejections for $\alpha \geq \frac{1}{2}$.[22] Results are relatively close to this prediction with 55% rejections and only 4% $e > BR$.

To summarize, the Fehr-Schmidt model appears to capture some of the essence of the reciprocity we observe, but we find substantially less reciprocity (both positive and negative) than their estimates based on previous experiments suggests.[23] How can we explain that the Fehr-Schmidt model appears to rationalize much of our worker data, but predicts substantially more reciprocity? The possible explanations are first, that we clearly implemented one-shot interactions between specific employer-worker pairs which eliminated

[21] Equal payoffs appear, however, to be a better standard than equilibrium wages as a benchmark for reciprocity.

[22] More precisely, since rejections reduce inequality more effectively, for $w > 3$ we should only see rejections, as indeed we do.

[23] Careful readers might have observed that the estimates of parameter distributions that Fehr and Schmidt present are meant as distributions across subjects whereas here we look at the shares of decisions that conform with certain predictions. This, however, would only be a problem, if the distribution of certain wage offers was biased in such a way that the reciprocal subjects got fewer wage offers in the relevant ranges. With our matching scheme, it is hard to conceive how this could have occured and indeed we have confirmed that the crucial wage offers are rather evenly spread across subjects. If anything, there is a bias in favor of reciprocity as workers who got relatively frequently high or low offers tended to reciprocate more than the average subject.

any strategic reciprocal actions, and second, that the provision of the profit calculator promoted payoff maximization (although it also dramatically facilitated payoff comparisons).

While the above analysis suggests that the Fehr-Schmidt model captures quite well that equal payoffs are a benchmark for reciprocity, it is not that clear that aversion towards inequality is really what drives the reciprocal actions. First, as seen above, wage offers that imply disadvantegeous payoff inequalities even at best reply are still followed by $e > BR$ in 8% in the high-efficiency gains treatments.[24] More importantly, among the 46 efforts choices above the best reply observed in all treatments more than a quarter (12) are substantially higher than the inequality minimizing level, causing disadvantageous inequalities between 27 and 177. Another 14 reduce the advantegous inequality but do not minimize it subject to a constraint of not causing disadvantegous inequality.[25] Thus, payoff equality seems to play some role as a standard to evaluate the fairness of wages and as the aim of reciprocal actions, but not necessarily the dominant one in either case.[26][27] Finally, note that the average payoffs are relatively equal for workers and employers in three treatments and also differ substantially less than the equilibrium payoffs in the remaining treatments (see Table 4 above). This could be seen as evidence in favor of inequality aversion. Note, however, that most wage-effort combinations result in rather large payoff differences.[28] The relative equality in average payoffs

[24] More precisely, these are eight choices by six different workers. There are 24 workers in total in the high-efficiency-gains treatments.

[25] In nine of these cases the marginal cost curve is constant at the effort choice and hence the results are not consistent with any possible Fehr-Schmidt parameters.

[26] Note again, that this is in spite of the provision of a profit calculator that eliminated all problems of calculating payoff equalizing effort.

[27] In a more direct way, without reverting to inequality aversion, which does not find much support in pure distribution games (see Engelmann and Strobel, 2004), the model by Cox et al. (2007) captures that reciprocity can be triggered by deviations from payoff equality. This model is based on agents that are basically altruistic, but also reciprocal. Reciprocity is triggered by deviations from a benchmark payoff, which could, e.g., be the equilibrium payoff or the equal payoff. Our data suggest that in the low-efficiency games treatment the equal payoff is the appropriate standard, while in the high-efficiency gains treatments it might be lower.

[28] The average absolute payoff difference is 25.2, the absolute payoff difference is smaller than 10 in 162 cases (this includes the 39 rejections that result in equal (0) payoffs), but ≥ 10 in 306 cases and even ≥ 20 or ≥ 40 in 194 and 108 cases, respectively.

results from two counter-balancing effects: general wage-effort combinations close to the equilibrium that advantage employers and partial exploitation of high wage offers, that substantially advantage workers.[29]

A related question is how well models of reciprocity such as Dufwenberg and Kirchsteiger (2004) or Falk and Fischbacher (2006) capture the behavior of our subjects. We first note that since effort choices below best response are Pareto-dominated, even efforts somewhat above the (selfish) best response are to be considered unkind according to Dufwenberg and Kirchsteiger (2004). Similarly, most wages that we observe would be unkind. Thus, the observed behavior could potentially be consistent with a reciprocity equilibrium where reciprocity is primarily of the negative kind. Since the model crucially depends on first-order and second-order beliefs, it is, however, difficult to assess how well the model captures underlying motivation. Occasional high wage offers seem to be indicative of some players holding wrong beliefs, inconsistent with equilibrium. Moreover, we have clear, albeit indirect, evidence against the relevance of these reciprocity models. Assessing the opponent's kindness crucially depends on the alternative options that were available to the opponent. The data on the use of the profit calculator does not indicate that players were thoroughly investigating these counter-factuals. Specifically, while 68 of the 78 workers use the calculator in all six periods, only 8 workers ever enter a wage other than that chosen by the employer, 7 among them only one other wage, and 6 of these only in one period. Indeed, some of these computations appear to result from typos. Only one worker uses the calculator to extensively explore the realm of outcomes that would have been possible if the employer had chosen another wage. Ignoring counterfactual wages is consistent with standard game theory, whereas this observation casts doubts on the validity of reciprocity models like those of Dufwenberg and Kirchsteiger (2004) and Falk and Fischbacher (2006) in this context. While it is possible that workers might have evaluated the outcome of counter-factual wages without the use of the calculator, this seems very unlikely because workers use the calculator quite extensively for the given wage. Thus while reciprocity models might capture some aspects of our choice data, the evidence we have on subjects' decision process is inconsistent with that suggested by the reciprocity models.

[29] One might be tempted to conclude that hence this exploitation of occasional high wage offers can be explained by a desire to equalize total payoffs. This conclusion, however, does not survive closer scrutiny, as total payoffs within some sessions differed quite substantially.

The crucial question for the robustness of gift exchange is whether reciprocity is sufficiently strong to make high wage offers worthwhile. Figure 4 shows the profits of employers by wage brackets.

Figure 4 illustrates that the optimal wage in the low efficiency treatment is slightly above the equilibrium wage. In contrast, in the high efficiency treatments wages that are substantially above the equilibrium tend to be profitable. (The noise, especially of the EW-H data, is due to differences in the distribution of wage offers. Also contributing to the variance in payoffs at the lower end of the wage offers is the number of sessions per treatment.) Raising the wage to the equilibrium wage increases the profit more strongly than predicted because lower wages are sometimes answered by negative reciprocity. For the same reason, it pays to increase the wage even slightly above the equilibrium. This is confirmed by Table 11 which shows coefficients for Wage Offer in a random-effects regression for the employer's payoff, by treatment and by wage bracket (top row: all wage offers; second row: offers smaller than 40, which is above the equilibrium wage but below 60, the wage required for equal payoffs at maximal effort; third row: offers above 20, the equilibrium wage in all treatments except for C-A; bottom row: offers between 20 and 40.) Employer's payoff is significantly increasing in Wage Offer for low wage offers (except for EW-H) but decreasing for high wage offers. Note that the positive coefficient on Wage Offer for the range 20 - 40 suggest that it pays to raise offers somewhat above the equilibrium. Note also that for the corner point equilibrium the optimal wage offers lie substantially above the equilibrium but below that for other treatments. Last but not least we note that in this treatment the high wage offers lead to negative payoffs for employers.

Apparently, employers learn that it is not profitable to offer very low or very high wages. Figure 5 shows the distribution of wage offers in the first two and the last two periods, aggregated for all interior equilibria treatments. It is clearly shown that the number of wage offers below the equilibrium and those above 50 decreases substantially towards later periods. The corresponding effect is even more striking in the corner-point treatment, as shown in Figure 6. While wage offers are quite spread out in the first two periods, they cluster between 10 and 19 (i.e. somewhat, but not substantially above the equilibrium) in the last two periods. Furthermore, if we include the period as independent variable in the analysis for wage offers, efforts, and excess efforts presented in Table 5, we see that it has a small but significant negative

	Wage Offers	All Treatments	Excluding C-A
Wage Offer	all	-0.205 (-2.908)**	-0.115 (-1.475)
	$w < 40$	0.926 (5.481)**	0.857 (4.495)**
	$w > 20$	-0.593 (-6.695)**	-0.541 (-6.224)**
	$20 < w < 40$	0.434 (1.155)	0.397 (1.056)
		EW-H	EW-L
Wage Offer	all	-0.505 (-3.005)**	0.033 (0.317)
	$w < 40$	-0.414 (-0.404)	0.812 (4.502)**
	$w > 20$	-0.698 (-3.460)**	-0.386 (-3.079)**
	$20 < w < 40$	2.317 (0.555)	0.501 (1.505)
		A-H	A-L
Wage Offer	all	-0.037 (-0.186)	-0.068 (-0.555)
	$w < 40$	1.779 (2.561)**	0.783 (4.202)**
	$w > 20$	-0.619 (-2.922)**	-0.586 (-4.705)**
	$20 < w < 40$	0.577 (0.350)	0.150 (0.468)
		C-A	
Wage Offer	all	-0.588 (-4.162)**	
	$w < 40$	1.162 (2.859)**	
	$w > 20$	-0.490 (-0.597)	
	$20 < w < 40$	(insufficient obs.)	

Table 11: Coefficients for wage offer in random-effects regression for employer's payoff. Coefficients for the variable wage offer are shown for regressions including all wage offers (first row in each cell), restricting to wageoffers less than 40 (second row), wage offers larger than 20 (third row), wage offers between 20 ad 40 (fourth row). The cells on top show results across all treatments (left) or all except the corner-point treatment (right), the lower part for the individual treatments. z-statistics are given in parantheses. $+ =$ significant at $p = .1$, $* =$significant at $p = .05$, $** =$ significant at $p = .01$.

impact on all variables.[30] Hence there is no tendency towards gift-exchange, but rather a slight decrease of it over time similar to what Gneezy and List (2006) find.

5 Discussion

We interpret our results as follows: First, as summarized in Facts # 1 - 2 above, we see attempts at eliciting gift exchange on the part of employers in both interior and corner point scenarios. These offers typically are "small commitments" (Watson 2002).

Second, as evidenced by Facts # 7 - 10, Table 8, and Figure 3, in all treatments workers typically maximize their payoffs given wage offers. Contrary to what has been observed in many other experiments, our subjects choose best-reply actions. Particularly noteworthy is that workers show little positive reciprocity. They do exhibit some negative reciprocity towards comparatively low wage offers. We note that doing so is relatively cheap for them.

Third, the employers' small commitments are therefore largely unsuccessful in eliciting efforts above the workers' best reply but they are rational in that their absence increases negative reciprocity. While the wages are somewhat above the equilibrium wage (20 or 21), they only marginally exceed the wage (30 or 31) necessary to induce the maximal best reply (2.0). As evidenced by Figure 3, larger commitments rarely increase the effort and are almost never profitable (Figure 4).

Regarding the corner point equilibrium, we find, fourth, that attempts to elicit gift exchange are more pronounced than for the interior equilibria (Fact # 2); worker behavior, however, is hardly affected (Fact # 10). This causes the wage data to be more noisy in the corner point treatments than in the interior equilibrium treatment with low efficiency gains (Fact # 6). The added noise seems to result from the both proposers and responders not being satisfied with the inefficient outcome of the corner-point equilibrium.

Fifth, we find that efficiency gains interact with framing in important ways (Fact # 5). As evidenced by Fact # 4, framing the situation as an employer-worker relationship does not have a substantial impact

[30] In particular, for wage offer: coefficient for variable "period": $-.0954$, $p = 0.029$, for effort: $-.0294$, $p = 0.015$, for effort − best-reply effort: $-.0257$, $p = 0.009$. The effects are slightly smaller, but still significant, if we exclude the corner-point treatment ($p = 0.051$ in regression for wage offer, $p < 0.05$ in the other two cases). Coefficients and levels of significance for the other variables are only minimally affected by including the variable "period".

in low-efficiency-gains treatment but does for the-high-efficiency-gains treatment. Similarly, as evidenced by Fact # 3, high efficiency gains have a small effect in the abstract frame but a substantial effect in the employer-worker frame. Interestingly, though, the preceding statements hold for wage offers only. Effort choices seem to be unaffected by both the extent of efficiency gains and framing, given wage offers. Of course, this result might not hold if potential efficiency gains were even more dramatic than they are in our parametrization right now.

Our results suggest, in sum, that laboratory gift exchange is much less robust than is often asserted (e.g., Fehr and Gächter 1998; Van der Heijden et al. 2001; List 2006; Levitt and List 2007 [31]). Clearly, the subjects in our experiment did not engage in much reciprocal exchange. And they did, for the most part, not return favors (meaning little positive reciprocity). And while they punish more often, they apparently do so because it is not very costly to them. Gift-exchange behavior, quite in line with economic intuition, thus appears rather price-sensitive.

Recall that we are concerned with first-degree robustness, i.e., the sensitivity to parametrization characteristics such as the nature of the equilibrium (corner versus interior), the degree of possible efficiency gains, the degree of asymmetry between the surplus that employers and workers can capture, the risk to the employer of being exploited when trusting, and the cost to the worker of reciprocating, as well as second-degree robustness, i.e., the stability of experimental results to variations in experimental procedures such as framing, anonymity, and subject pools.

Specifically, we developed our treatments along four dimensions: the nature of the equilibrium (first-degree robustness), efficiency gains (first-degree robustness), subject pools (second-degree robustness) and frames (second-degree robustness); we also chose a matching mechanism that has been shown to best preserve the one-shot nature of the strategic interaction between employers and workers. Our design and implementation thus aimed at important facets of first-degree and second-degree robustness. We chose our characteristics of gift exchange experiments because they seemed to be among the most important

[31] We hasten to stress that the latter authors themselves have a more differentiated view of these issues. Specifically, Van der Heijden et al. (2001) explore the robustness of a repeated experimental gift exchange game with respect to matching (partners vs strangers) and game form (normal vs extensive). And List (2006) provides an intriguing study that compares (lack of) gift exchange in real-world settings with gift exchange iu carefully calibrated lab settings.

contributors to the results of laboratory gift exchange. We believe, and our belief seems to be confirmed by the interaction effects of efficiency gains and framing documented above, that testing for first- or second-degree robustness one at a time is potentially misleading.

That said, Charness and Kagel and their collaborators have, in parallel work, stress-tested second-degree robustness of laboratory gift exchange with intriguing results. Drawing on a standard corner-point design, Charness et al. (2004) find that the degree of gift exchange is "surprisingly sensitive to an apparently innocuous change - whether or not a comprehensive payoff table is provided in the instructions." (p. 189) Specifically, they find that, for US undergraduate students, the presence of a payoff table reduces gift exchange sharply.[32]

Also drawing on a standard corner point design, Hannan et al. (2002) find that US "undergraduates provide substantially less effort than MBAs" (p. 923). They interpret their finding as resulting from previous work experience (and hence different priors or understandings) that MBAs bring to the laboratory. A similar argument has been made more generally by Harrison and Rutström (2001; see also Ortmann and Gigerenzer 1997, or Samuelson 2005). It is interesting to note that the frames being used in these two studies were of the employer-worker kind. Hannan et al. also investigate first-degree robustness by studying the effects of different efficiency gains. For both undergraduates and MBAs they find higher wage offers for higher productivity firms but no difference in the wage-effort relation. This is roughly in line with our results.

Brüggen and Strobel (2007) assess second-degree robustness by comparing chosen effort and real effort. They find gift exchange to be alive and well in both treatments. Also considering real effort, Gneezy and List (2006) study the effect of duration in two tasks (entering data in a library and raising funds for a research center). In their experiments, they find that gift exchange is a transitional phenomenon

[32] The authors correctly call for a similar study with European students to better understand that effect. While we did not provide such a payoff table (our experimental sessions were conducted during July 2000 - June 2001; theirs were conducted in May 2001), the Charness et al. results suggest that our provision of a payoff calculator may be partially responsible for the comparatively low degree of trust, and positive reciprocity, in our data. It is intriguing to speculate what such a payoff matrix would have done to the choice behavior of our subjects. Also, had we supplied a best-reply button, we would likely have seen choices even closer to equilibrium.

that lasts not much longer than a typical experimental session (i.e., for a couple of hours). An important methodological problem with these two studies – explicitly discussed by Gneezy and List (2006) – is that true effort costs are not known and hence the second-degree robustness issue of effort type is confounded with (unknown) first-degree parametric variation.

List (2006) tries very hard to keep parametric variation at a minimum across all treatments in order to carefully calibrate laboratory experiments with real-world markets (trading cards and ticket stubs). He finds that in his replication of laboratory experiments gift exchange is alive and well even in the lab treatments calibrated to the real-world markets; he argues persuasively that the gift exchange he finds, both in the lab and the real-world treatments, is driven by reputational concerns rather than social preferences. Relatedly, Healy (2007) tests experimentally the conjecture that selfish workers in lab experiments might have an incentive to pretend to be other-regarding initially since firms would take defection as a signal about the type of workers they are dealing with. This perceived type correlation would then make lower wage offers fall back also on the selfish types. By and far, Healy's experiment supports this story. In a sense, our experiment provides even stronger evidence for his explanation of gift exchange because our matching does not allow for group reputation (and, for that matter, individual reputation) effects and we find little seemingly other-regarding behavior. In terms of our proposed categorization of types of robustness, Healy (2007) does find by and far second-degree robustness (i.e., there are differences in the respective treatments but gift exchange as such survives) but, as his model predicts, he does find clear violations of first-degree robustness (i.e., for some treatments gift exchange disappears.)

In another interesting recent laboratory gift-exchange study that addresses both first-degree and second-degree robustness, Rigdon (2002) explores what the effects are of nontrivial costs of effort and increased social distance (here, anonymity) between subject and experimenter. She points out that the costs of effort in classic studies such as Fehr et al. (1993) and Fehr et al. (1997), but also in Fehr and Gächter (2002) were trivial and that laboratory workers had to report their effort choices to the experimenters. Rigdon (2002), within a corner-point equilibrium design, finds that nontrivial costs and increased social distance induce actual effort levels that are significantly below desired ones. Her result contradicts much of what has been reported about the reality of gift exchange in the laboratory.

Brandts and Charness (2004) vary the competitive balance between workers and employers where the wage offers are made in a one-sided oral auction. As such their article is an example of a test of first-degree robustness. They find that the gift-exchange is indeed robust with respect to this variation as wages and the wage-effort relation is little affected by the competitive pressure.[33] Furthermore, Brandts and Charness consider the effects of a minimum wage in a treatment with excess supply of workers. With a minimum wage efforts conditional on wages are indeed somewhat lower, indicating limitations of first-degree robustness. Finally, Brandts and Charness conduct one-shot gift-exchange experiments with exogenous matching of an equal number of firms and workers. They find some remaining gift exchange, but lower wages and in particular lower efforts than in their repeated interaction treatments. This is consistent with an interpretation of the relatively low levels of gift exchange in our experiments as being partly driven by the matching we employ that implements a sequence of one-shot encounters without any scope of repetition.

Maximiano et al. (2007 a,b) provide tough and relatively clean tests of first-degree robustness, and – to their own surprise – do not manage to exorcise gift exchange. In Maximiano et al. (2007a), they compare bilateral (one worker per firm) and multilateral (four workers per firm) interaction and find gift exchange alive and well in both treatments. Moreover, the difference in treatments are not statisticially significant. In Maximiano et al. (2007b), they compare a standard bilateral gift exchange game with two tri-lateral ones (with ownership and control of the firm being separated). Again they find gift exchange immune to this first-degree robustness test.

Fehr and Gächter (2002) have constructed an interior equilibrium by allowing employers to include bonuses and punishments into the contract. They find that, compared to a corner-point control treatment, excess effort (voluntary contribution in their terminology) is substantially reduced – a results which seems roughly in line with ours. They also find an interesting interaction with the framing because this effect is much stronger for the punishment treatment than the bonus treatment. Pereira et al. (2006) address

[33] This, however, appears to be an interesting puzzle. Whereas gift-exchange is robust with respect to this variation, models that provide an explanation for gift-exchange in the first place predict that there should actually be a difference between treatments. Brandts and Charness (2004) conclude that outcome-based models are consistent with their results, but this is true only for the wage-effort relation, but not for the wages themselves. If we have gift-exchange and it pays for firms to offer high wages, then it pays to pay more if we have to compete for workers, unless every firm pays the maximum wage anyway.

first-degree robustness by constructing both interior (in effort) and corner equilibria. In the former they too find nearly twice as many negatively reciprocal acts than positively reciprocal acts, being in line with our results though they find more positive reciprocity than we do. They conclude that trust and reciprocity survive in their interior equilibrium treatment. Given that on balance they find more negative than positive reciprocity, this strikes us as a curious interpretation.

The preceding articles provide further evidence that both first-degree and second-degree robustness of gift exchange are more fragile than one might conclude from previous accounts. It is noteworthy that models such as Bolton and Ockenfels (2000), Fehr and Schmidt (1999), and Charness and Rabin (2002), which have been proposed to explain experimental results of gift exchange and related social dilemma scenarios within a game theoretic framework, are not insensitive to variations in parameterizations (e.g., differential efficiency gains).[34] Hence, experimental results that question first-degree robustness can partially be rationalized by these models. They are, however, insensitive to issues of implementation and hence experimental results rejecting second-degree robustness suggest that these models do not tell the complete story. In particular, they are unable to explain the important interaction effects that we identified above.

In summary then of this review, and recalling our initial categorization of robustness tests into those testing robustness to replication (third degree), robustness to implementation of the underlying game (second degree), and robustness to changes in the underlying game itself (first degree), we find that results are surprisingly mixed. For example, while Maximiano et al.'s (2007a,b) results find support for the first-degree robustness of gift exchange, the results by Healy (2007), Rigdon (2002) and ourselves seem to strike a blow to that claim. Likewise, while Brüggen and Strobel (2007) find support for second-degree robustness, the results by Gneezy and List (2006) and List (2006) seem to question decisively that claim. Moreover, our results suggest strongly interesting interaction effects between changes in the underlying game and the implementation of that game. To the extent that the reality of laboratoy (and real-world) gift exchange seems to require robustness of all three degrees, the accumulating evidence suggests clear limits to that reality. This indeed then very squarely trains the spotlight on issues of external validity (and

[34] Indeed, the evidence in Engelmann and Strobel (2004, 2007) suggest the importance of that key facet of first-degree robustness.

calibration), a topic which – with one laudable exception (List 2006) – has been neglected in that literature, notwithstanding occasional claims to the contrary.

Friedman (1998) has demonstrated for the Monty Hall problem how experimenters can systematically construct, and deconstruct, alleged choice anomalies – a fact well-known to experimental psychologists and, in fact, the source of considerable and very public contentiousness in that field (e.g., Kahneman and Tversky 1996 and Gigerenzer 1996). Our work and recent work by others suggest that laboratory gift exchange can be systematically affected by changing design and implementation characteristics. As there are conditions that make it more likely that experimental results confirm the existence of *homo reciprocans* (namely, (unattractive) corner point equilibria, dramatic potential efficiency gains, employer-worker frame, etc.), there are also conditions that make it more likely that experimental results are in line with *homo economicus.*

6 Concluding remarks

Much of the observed play of the participants in our gift-exchange experiments is at or close to equilibrium. This result is very likely not a subject pool effect as some of our sessions were conducted on a site which has produced many of the classic gift-exchange data. In a narrow sense, this suggests that *homo economicus* is very much alive in our experiment. This result stands in stark contrast to much of what has been reported in the literature, with few exceptions (Rigdon 2002; Gneezy & List 2006; List 2006). In particular, we find little evidence for positive but substantial evidence for negative reciprocity. The occurrence of negative reciprocity itself, however, is not consistent with homo economicus but it comes relatively cheap. Furthermore, wages above equilibrium and the sensitivity to the interaction of efficiency gains and framing are not consistent with homo economicus.

Our results (together with other recent studies) suggest that laboratory gift exchange is highly sensitive to changes in the underlying game (parametrization) as well as its implementation, namely the nature of the equilibrium, the degree of possible efficiency gains, the degree of asymmetry between the surplus that employers and workers can capture, the risk to the employer of being exploited when trusting, and the cost to the worker of reciprocating, as well as framing and anonymity.

While exclusive reliance on selfishness and the neglect of reciprocity motives may indeed lead to wrong predictions and to wrong normative inferences (as Fehr et al. 1997 argue), so will the belief – now apparently widely held – that people trust, reciprocate, and are intrinsically motivated. As Tirole (2006) makes abundantly clear (for example in his discussion of micro-finance institutions), different assumptions about trust, reciprocity, and intrinsic motivation lead to very different desiderata for the internal organizations of institutions. Drawing on experimental results of gift-exchange for the design of institutions crucially requires tests of its external validity. The latter requires both first-degree and second-degree robustness (and obviously third-degree robustness), as it requires gift-exchange to survive given parametrizations corresponding to real-world scenarios as well as implementations of real-world settings. Our own results as well as a number of those discussed above suggest that taking the necessary first-degree and second-degree robustness as given is far from always warranted.

There are clearly scenarios – like ours – where the belief that people are motivated by trust, reciprocity, and intrinsic motivation is unwarranted and where canonical game theory works reasonably well. To our minds, our results prompt two related questions: First, what is the relative importance of the parametrization and implementation characteristics supporting the view of *homo reciprocans* and *homo economicus*, respectively? Second, what constitutes a "realistic" parametrization and implementation? While we realize that this question is bound to be a contentious one, keeping in mind the "parallelism postulate" (Plott 1987) strikes us as imperative because of the policy implications that the laboratory gift-exchange research program entails.

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A Instructions

All instructions were in German (for both the German and Swiss subjects). The complete set of instructions can be accessed at http://home.cerge-ei.cz/Ortmann/instructions.html. The following is a translation of the instructions of the employer-worker frame with low efficiency gains, with the German original inserted after each paragraph. The instructions for the other treatments were essentially the same. We only substituted a different multiplier for the treatments with high efficiency gains. For the abstract frame, words like employer, worker, and basic wage were substituted with participant X, participant Y, and payment component, respectively. The instructions for the corner point equilibrium treatment were identical to those in the abstract frame except for the different multiplier (and corresponding changes in the examples).

Instructions [Instruktionen]

Welcome to our experiment. Please read the following instructions carefully. **Should you have trouble understanding something while we go over the instructions or during the experiment, please raise your hand**. Please do not engage in any form of communication with the other participants in this experiment from now on; we shall ask you to leave the experiment and not pay you if you don't comply. [Willkommen zu unserem Experiment. Bitte lesen Sie die folgenden Instruktionen sorgfältig durch. Falls Sie etwas jetzt oder im Laufe des Experiments nicht verstehen, heben Sie bitte die Hand. Stellen Sie bitte von jetzt an die Kommunikation mit anderen Teilnehmern des Experiments ein; wir werden Sie andernfalls bitten, das Experiment zu verlassen und werden Sie außerdem von der Auszahlung ausschließen.]

You will participate in **six rounds**. In each round, you will be matched with another participant, i.e., you will never encounter anyone that you encountered in an earlier round. Likewise, you will not be able to indirectly influence participants in the experiments that you will encounter in later rounds. In addition, no participant will be informed about the choices of other participants in earlier rounds. Whatever you do in a round, will hence not have an effect on interactions in later rounds. [Das Experiment besteht aus sechs Runden. In jeder Runde treffen Sie auf einen anderen Teilnehmer, d.h. Sie werden niemals mit jemandem interagieren, mit dem Sie bereits in einer früheren Runde interagiert haben. Auch können Sie Teilnehmer des Experiments, mit denen Sie später interagieren werden, nicht indirekt beeinflussen, und außerdem erfährt kein Teilnehmer etwas über das Verhalten anderer Teilnehmer in vorhergehenden Runden. Was immer Sie in einer Runde tun, kann also spätere Interaktionen nicht beeinflussen.]

Participants will be assigned one of two roles, employer or employee. These roles will be assigned randomly and remain the same throughout the experiment, that is, in every round you are either an employer or employee. **You will be informed about your role before the experiment proper starts**. [Es gibt zwei verschiedene Rollen, Arbeitgeber und Arbeitnehmer. Die Rollen werden am Anfang zufällig zugeteilt und stehen für das ganze Experiment fest, d.h. Sie sind entweder in allen Runden ein Arbeitgeber oder in allen Runden ein Arbeitnehmer. Über Ihre Rolle werden Sie vor Beginn der ersten Runde informiert.]

In each round, **each employer offers to the employee that has been assigned for that round a contract**. The contract consists of a pair of numbers, namely a base wage w and a desired effort e. [In jeder Runde bietet zunächst jeder Arbeitgeber dem für die Runde zugeteilten Arbeitnehmer einen Arbeitsvertrag an. Dieser Vertrag besteht aus zwei Werten, einem Grundlohn g und einer gewünschten Arbeitsanstrengung a.]

Once the employer has offered a contract with base wage w and desired effort e, **the employee that has been assigned for that round decides whether to accept or reject the contract**. **If the employee accepts the contract**, then he will have to choose her or his actual effort. (In this decision he may ignore the desired effort.) **If the employee rejects the contract**, then both employer and employee receive a payoff of 0 ECU (Experimental Currency Unit). [Nachdem ein Arbeitgeber den Vertrag mit Grundlohn g und gewünschter Arbeitsanstrengung a angeboten hat, entscheidet der für die Runde zugeteilte Arbeitnehmer, ob er den Vertrag annimmt oder den Vertrag ablehnt. Nimmt der Arbeitnehmer den Vertrag an, so muss er im nächsten Schritt die tatsächliche Arbeitsanstrengung a wählen. (In dieser Entscheidung ist er nicht an den Vorschlag des Arbeitgebers gebunden.) Lehnt der Arbeitnehmer den Vorschlag ab, so erhalten sowohl Arbeitgeber als auch Arbeitnehmer für die jeweilige Runde eine Auszahlung von 0 ECU (Experimental Currency Unit).]

The two choices the employee has, and the consequences of a decision to go with one of the options, are illustrated on the attached **SEQUENCING OF DECISIONS** sheet. Please study this sheet carefully now. [Die zwei Optionen des Arbeitnehmers, und die Konsequenzen der Entscheidung für eine dieser Optionen, sind auf dem beiliegenden VERLAUFSDIAGRAMM zusammengefasst. Bitte schauen Sie sich dies Verlaufsdiagramm jetzt genau an.]

[Experimenter repeats paragraphs 4 and 5.][Experimenter wiederholt Paragraph 4 und 5.]

[Experimenter: "Any questions at this time?"]["Haben Sie im Moment Fragen?"]

The actual effort e (selected by the employee), and not necessarily the desired effort (proposed by the employer) determines the gross profit of the employer. (This gross payoff is 50*e ECUs.) The net payoff results from a multiplier m that is dependent on e and that transforms the base wage w into the wage l = w*m. The wage l = w*m is the amount that the employer has to pay to the employee. The net payoff of the employer is therefore (50*e - l = 50*e - w*m) ECUs. [Die tatsächliche (vom Arbeitnehmer gewählte) Arbeitsanstrengung a, und nicht notwendigerweise die vom Arbeitgeber vorgeschlagene, bestimmt den Bruttogewinn des Arbeitgebers. (Dieser Bruttogewinn ist 50*a ECUs.) Der Nettogewinn ergibt sich aus einem von a abhängigen Lohn-Multiplikator m, der den Grundlohn g in den Lohn l=g*m transformiert. Diesen Lohn l=g*m ECUs muss der Arbeitgeber an den Arbeitnehmer zahlen. Der Nettogewinn des Arbeitgebers ist damit (50*a – l = 50*a – g*m) ECUs.]

The actual effort e (selected by him) causes the employee a disutility of c and results in a net wage of (l - k = w*m - c) ECUs. The relationship of effort, multiplier, and cost are illustrated for some values of e, m, and c on the attached SEQUENCING OF DECISIONS sheet and for all values summarized in the Table on the bottom of this sheet. Please take another look at the SEQUENCING OF DECISIONS sheet now. [Die tatsächliche (von ihm gewählte) Arbeitsanstrengung a verursacht dem Arbeitnehmer Kosten k und damit einen Nettolohn von (l - k = g*m - k) ECUs. Diese Zusammenhänge sind für einige wenige Werte auf dem beiliegenden VERLAUFSDIAGRAMM illustriert und für alle Werte von a, m, und k in der Tabelle (im VERLAUFSDIAGRAMM unten) zusammengefasst. Bitte schauen Sie sich dieses VERLAUFSDIAGRAMM jetzt noch einmal an.]

[Experimenter: “Any questions at this time?”][“Haben Sie im Moment Fragen?”]

Please note that decisions can lead to losses. However, both employers and employees can always decide in such a way that they are guaranteed not to make losses. [Bitte beachten Sie, dass Entscheidungen zu Verlusten führen können. Allerdings können Sie sowohl als Arbeitgeber als auch als Arbeitnehmer immer so entscheiden, dass Sie Verluste mit Sicherheit ausschließen.]

Some **examples**: [Einige Beispiele:]

1. Employer *proposes*: w = 30, e = 3.0. Employee *rejects*. Employer and employee both receive 0 ECU.

   [Arbeitgeber *bietet an*: g = 30, a = 3,0. Arbeitnehmer *lehnt ab*. Arbeitgeber und Arbeitnehmer erhalten also beide 0 ECU.]

2. Employer *proposes*: w = 20, e = 1.6. Employee *accepts* and *selects* e = 1.6.

   Employer hence receives 50*e - w*m = 50*1.6 - 20*1.3 = 80 - 26 = 54 ECUs.

   Employee receives w*m - c = 20*1.3 - 4 = 26 - 4 = 22 ECUs.

   [Arbeitgeber *bietet an*: g = 20, a = 1,6. Arbeitnehmer *nimmt an* und *wählt* a = 1,6.

   Arbeitgeber erhält also 50*a - g*m = 50*1,6 - 20*1,3 = 80 - 26 = 54 ECUs.

   Arbeitnehmer erhält g*m - k = 20*1,3 - 4 = 26 - 4 = 22 ECUs.]

3. Employer *proposes*: w = 55, e = 2.8. Employee *accepts* but *selects* e = 1.4.

   Employer hence receives 50*e - w*m = 50*1.4 - 55*1.2 = 70 - 66 = 4 ECUs.

   Employee receives w*m - c = 55*1.2 - 2 = 66 - 2 = 64 ECUs.

   [Arbeitgeber bietet an: g = 55, a = 2,8. Arbeitnehmer nimmt an, wählt aber a = 1,4.

   Arbeitgeber erhält also 50*a - g*m = 50*1,4 - 55*1,2 = 70 - 66 = 4 ECUs.

   Arbeitnehmer erhält g*m - k = 55*1,2 - 2 = 66 - 2 = 64 ECUs.]

After each round, each employer is informed whether the employee assigned for that round has accepted the contract and, if so, which effort e he has chosen. Additionally, both employer and employee are informed privately about their payoffs. No participant will be informed about other employers' or employees' decisions. [Nach jeder Runde wird jeder Arbeitgeber darüber informiert, ob der ihm für die Runde zugeteilte Arbeitnehmer den Vertrag angenommen hat und, wenn ja, welche Arbeitsanstrengung a der Arbeitnehmer gewählt hat. Außerdem werden sowohl Arbeitgeber als auch Arbeitnehmer privat jeweils über ihre Auszahlungen informiert. Kein Teilnehmer wird Informationen über eine Entscheidung anderer Arbeitgeber oder Arbeitnehmer erhalten.]

At the end of the experiment the sum of your payoffs from all six rounds will be converted into DM and then paid. 1 ECU corresponds to 0.10 DM. [Am Ende des Experiments wird die Summe ihrer Auszahlungen aus allen sechs Runden von ECUs in DM umgerechnet und ausbezahlt. 1 ECU entspricht dabei 0,10 DM.]

To ensure that all participants of the experiment have understood these instructions correctly, please answer now the following questions: [Um sicherzugehen, dass alle Teilnehmer des Experiments die Instruktionen richtig verstanden haben, beantworten Sie uns nun bitte die folgenden Beispielfragen:]

1. The employer *offers*: w = 10, e = 3.0.

   Employee *rejects*.

   => Employer receives ... ?

   => Employee receives ... ?

   Employee *accepts* and *selects* e = 1.0.

   => Employer receives ... ?

=> Employee receives ... ?

[Arbeitgeber *bietet an*: g = 10, a = 3,0.

Arbeitnehmer *lehnt ab*.

=> Der Nettogewinn des Arbeitgebers ist : ... ?

=> Der Nettolohn des Arbeitnehmers ist: ... ?

Arbeitnehmer *nimmt an* und *wählt* a = 1,0.

=> Der Nettogewinn des Arbeitgebers ist: ... ?

=> Der Nettolohn des Arbeitnehmers ist: ... ?]

2. The employer *offers*: w = 10, e = 2.0.

   Employee *rejects*.

   => Employer receives ... ?

   => Employee receives ... ?

   Employee *accepts* and *selects* e = 1.0.

   => Employer receives ... ?

   => Employee receives ... ?

   [Arbeitgeber bietet an: g= 10, a = 2,0.

   Arbeitnehmer lehnt ab.

   => Der Nettogewinn des Arbeitgebers ist : ... ?

   => Der Nettolohn des Arbeitnehmers ist: ... ?

   Arbeitnehmer *nimmt an* und *wählt* a = 1,0.

   => Der Nettogewinn des Arbeitgebers ist : ... ?

   => Der Nettolohn des Arbeitnehmers ist: ... ?]

3. The employer *offers*: w = 52, e = 3.0.

   Employee *rejects*.

   => Employer receives ... ?

   => Employee receives ... ?

   Employee *accepts* and *selects* e = 3.0.

   => Employer receives ... ?

   => Employee receives ... ?

   [Arbeitgeber *bietet an*: g = 52, a = 3,0.

   Arbeitnehmer lehnt ab.

   => Der Nettogewinn des Arbeitgebers ist : ... ?

   => Der Nettolohn des Arbeitnehmers ist: ... ?

   Arbeitnehmer *nimmt an* und *wählt* a = 3,0

   => Der Nettogewinn des Arbeitgebers ist : ... ?

   => Der Nettolohn des Arbeitnehmers ist: ... ?]

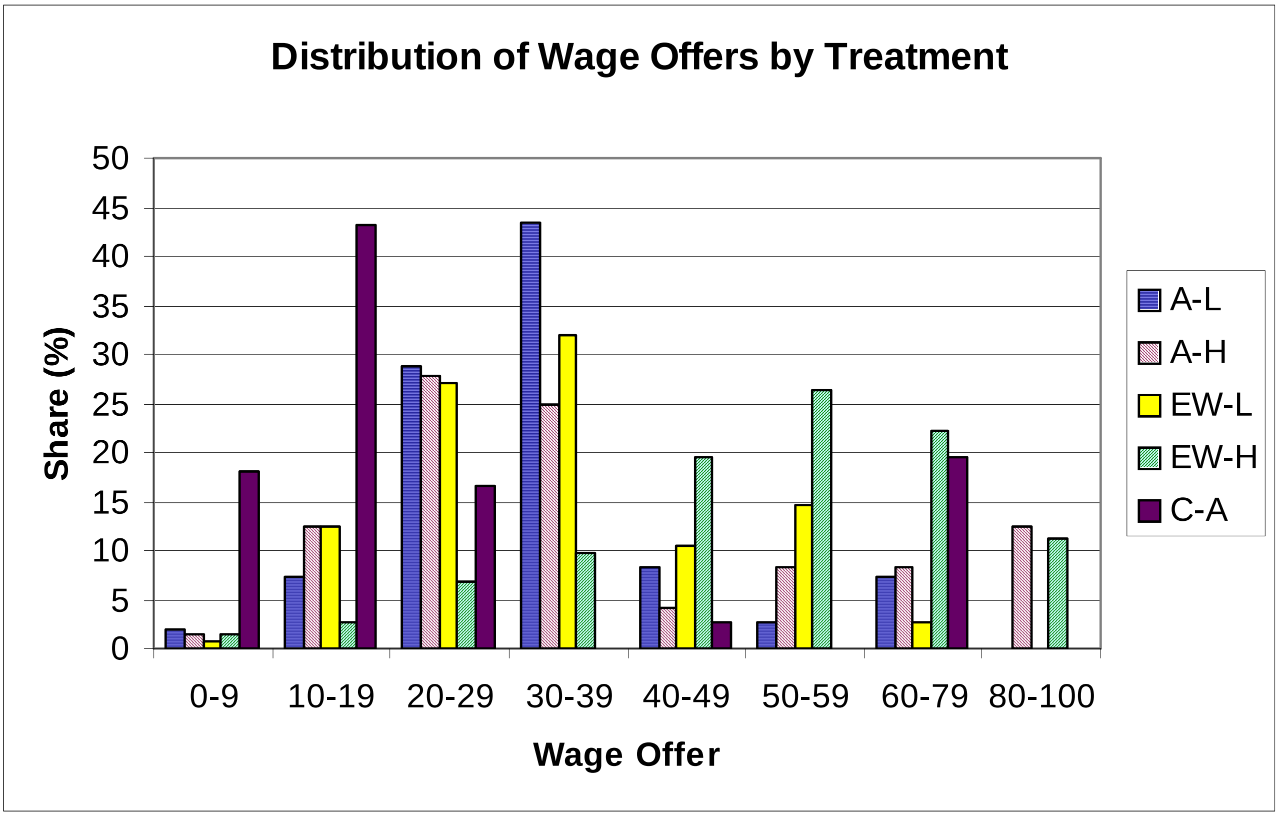

Figure 1: Distribution of wage offers by treatment (in percent of proposed contracts).

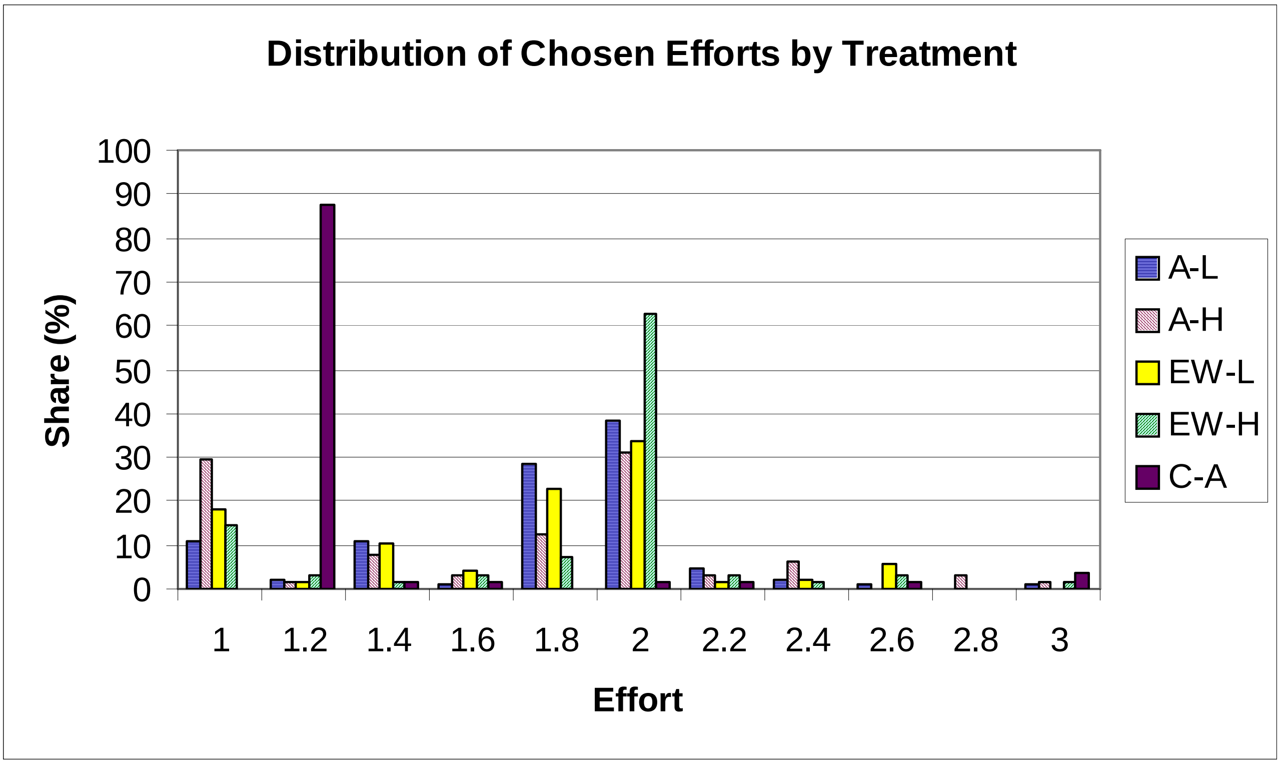

Figure 2: Distribution of chosen efforts by treatment (in percent of accepted contracts).

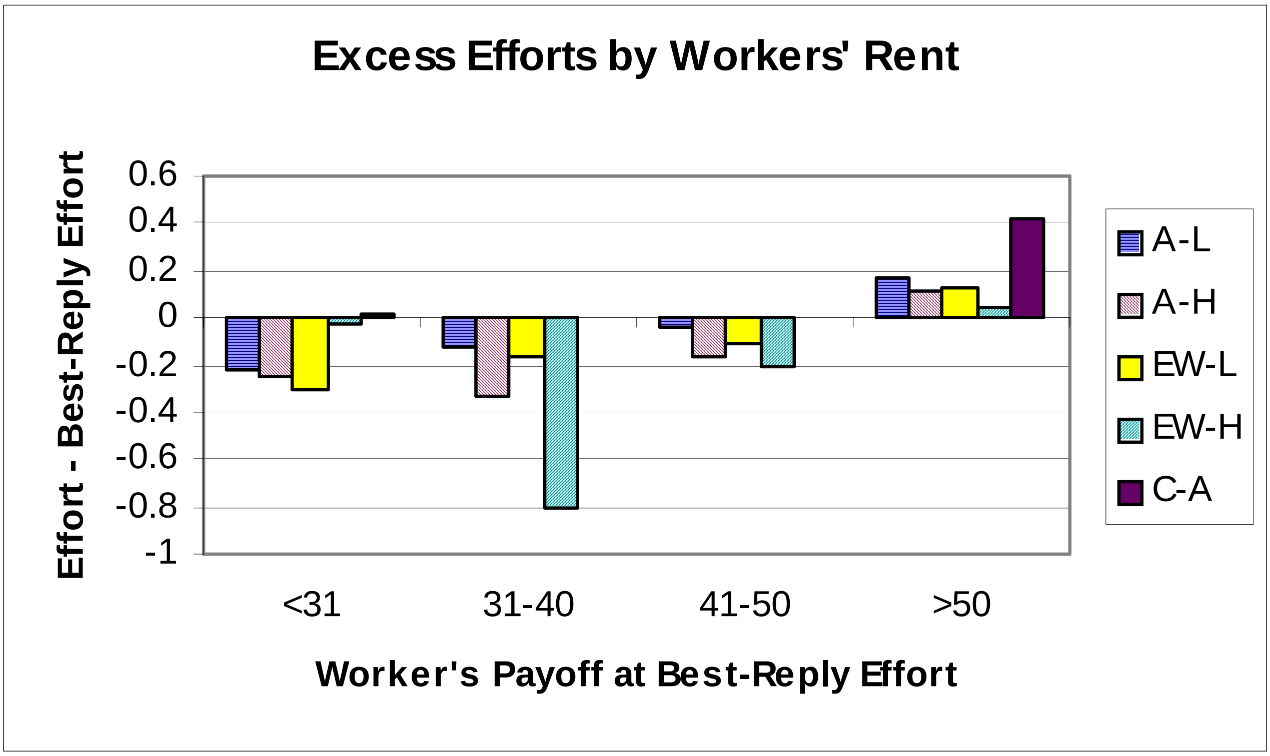

Figure 3: Difference between chosen effort and best reply effort for the given wage offer by treatment and worker's payoff at best reply effort.

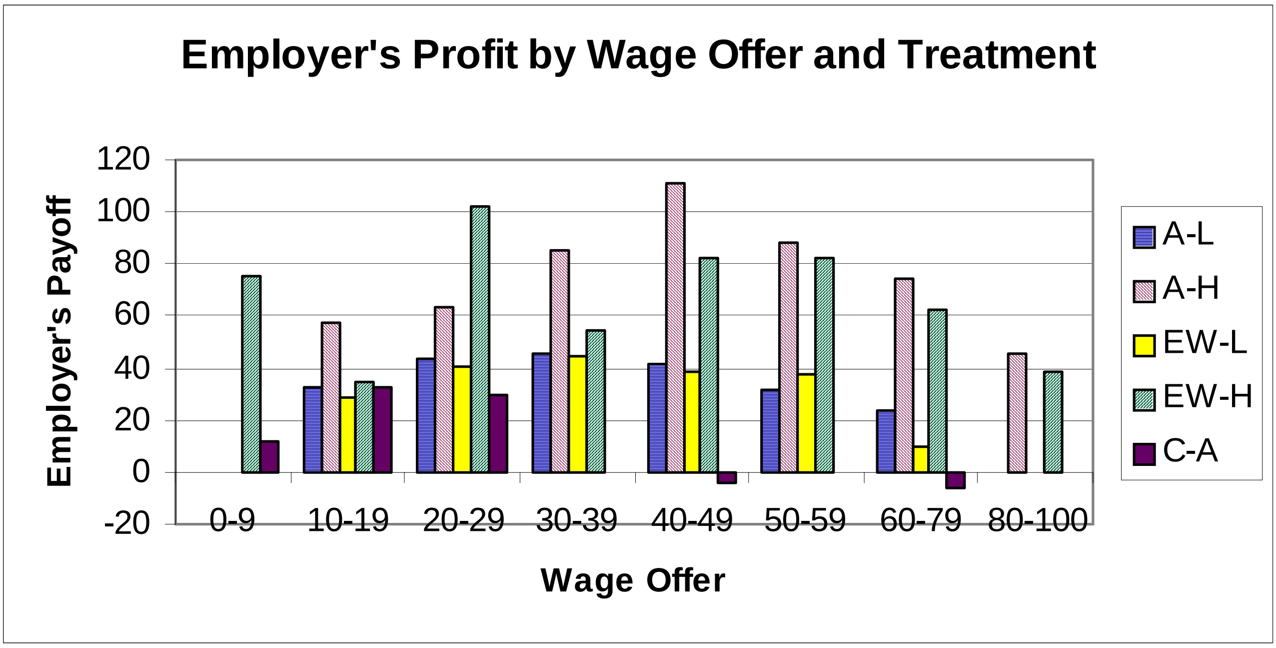

Figure 4: Employer's average profits by treatment and wage bracket.

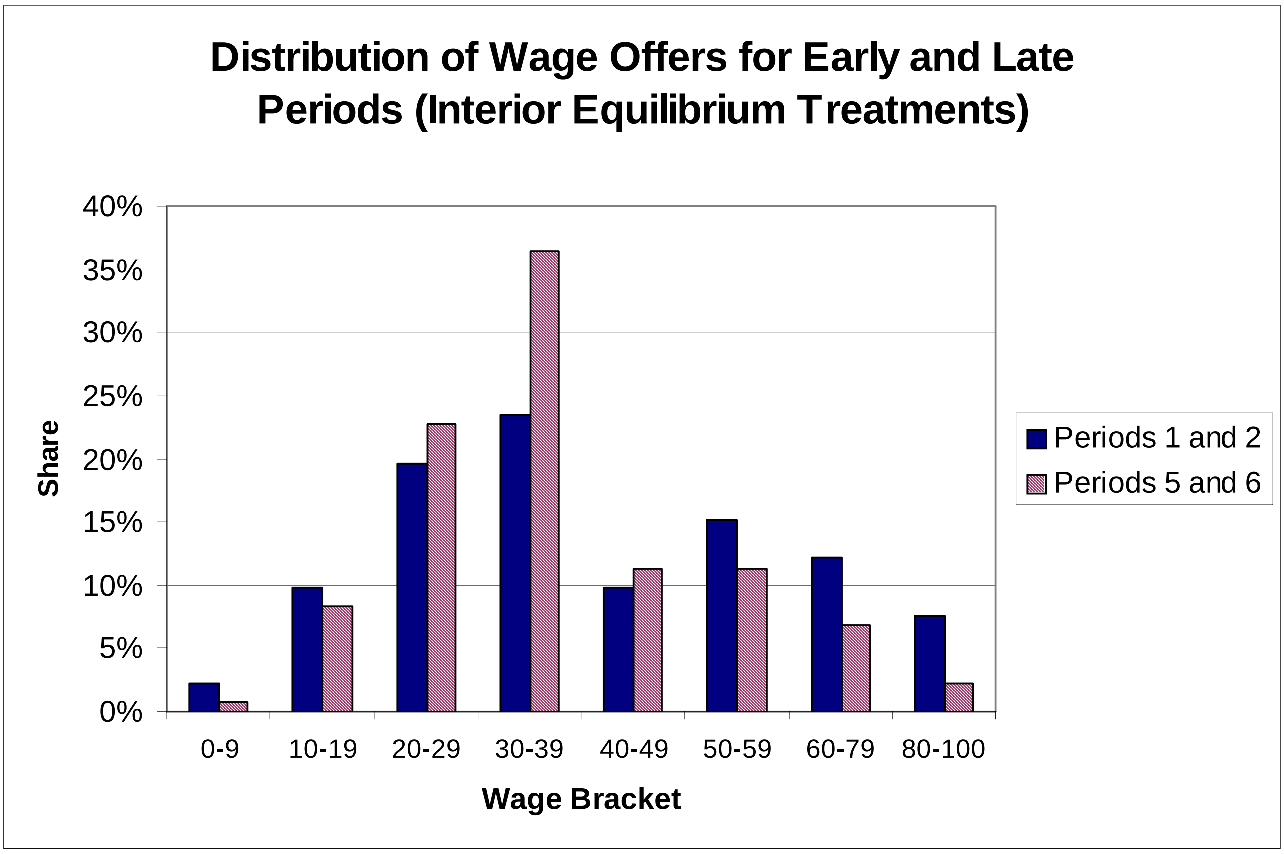

Figure 5: Distribution of wage offers in the first two and the last two periods for all interior equilibrium treatments.

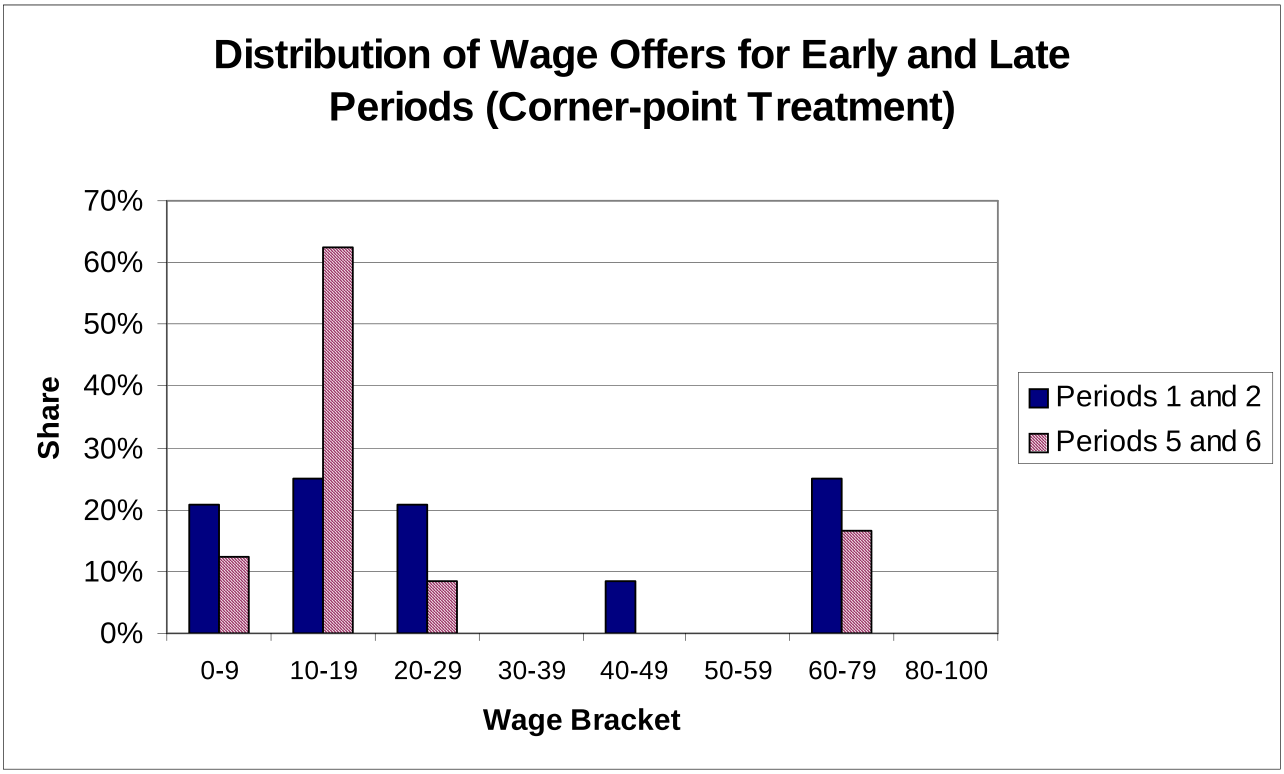

Figure 6: Distribution of wage offers in the first two and the last two periods for the corner-point equilibrium treatment.