Relational contracts with individual and standardized wages[#]

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Abstract

Wage standardization within a firm for the same job is a common phenomenon in collective contracts. In this paper, we analyze whether such standardization has any effects on incomplete contract markets compared to a situation where each agent receives an individual contract. In a gift-exchange experiment with a market on which principals can offer contracts and employ several agents, we observe that contract standardization leads to significantly higher wages, effort levels and a higher market efficiency. Since existing theories fail to account for the difference, we provide an explanation that is consistent with our findings. The paper also discusses the implications of our results.

JEL classification: C72**,** C91, C92 D21, J31, J50

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1 Introduction

On many markets, especially on labor markets, contracts are incomplete because obligations can only be specified imprecisely, or informal agreements and social norms play a major role. In case there is no third party that is able to enforce these relational contracts, principals and agents face a serious incentive problem because the stipulated quality or effort levels cannot be enforced in such an environment.

Although we do not have a lot of field evidence for such situations, we know from quite a few controlled laboratory experiments that, on average, a considerable extent of gift-exchange takes place between the principal and the agent (for an overview, see Fehr and Gächter, 2000). Despite the theoretical solution that predicts minimal wages and minimal effort levels for selfish money-maximizing players as a consequence of the incentive problem, non-minimal wages and effort levels that are increasing with wages are typically observed (e.g., Fehr, Kirchsteiger and Riedl, 1993; Fehr et al., 1998; Fehr and Falk, 1999; Charness, 2000; Fehr and Gächter, 2000; Hannan, Kagel and Moser, 2002; Fehr, Kirchsteiger and Riedl, 1998).[1] This is even the case for one-shot interactions, but the effects are typically stronger for repeated encounters of principals and agents. In case of repeated interaction, reputation building and the prospect of above-minimal rents foster reciprocal behavior and contract renewal. Even selfish players may, then, have an incentive to choose non-minimal efforts. Different theoretical approaches either adhering to the assumption of selfishness (e.g., Fudenberg and Maskin, 1986) or assuming the existence of social preferences for a subset of players (e.g., Fehr and Schmidt, 1999; Brown, Falk and Fehr, 2004) are consistent with these experimental results. The first theoretical treatments of gift-exchange situations under the name *efficiency wage hypothesis* date back to Gintis (1976) and Akerlof (1982).

In this paper we assess whether and in which way the extent and persistence of gift-exchange in principal-agent relations is influenced by contractual limitations. Specifically, we compare whether standardized contracts are conducive to reciprocal gift-exchange compared to individualized contracts or not. It is obvious that contract standardization for the same job or the same product is a widespread phenomenon on many markets. On labor

[1] In the following, we will refer to the labor market analogy of the gift-exchange game and use the terms wage and effort. Of course, the wage can also be a price and the effort a quality level on an incomplete product market.

markets, all collective contracts share this property but it is also important in the context of medical services like, for instance, visits to doctors. Studying the impact of such standardization is, thus, an important issue in economics.

For our purpose, we have to extend the usual experimental principal-agent relation that is based on a one-on-one matching of principals and agents to a multiple agents design. Our experimental setup builds upon Brown, Falk and Fehr (2004), who analyze gift-exchange between single principals and single agents in a rather flexible environment that allows us to assess the impact of institutional changes on several important market parameters.

In our experiment four principals – repeatedly, but finitely – interact with 16 agents on an experimental market. Principals can offer a wage level privately to any of the agents or publicly to all agents on the market. Agents can accept any offer, but not more than one. Principals are allowed to employ a maximum of three agents in each period. This restriction implies that labor demand is the short side of the market. We will simply denote a principal and the agents that she[2] employs a *firm*. After principals and agents have been matched through the market, agents have to choose an effort level. The higher the effort level, the higher is the cost for the agent, but the higher is also the principal's profit from the interaction. We introduce two straightforward treatments to this modified gift-exchange game: (i) an individual contract treatment, henceforth abbreviated by *IC*, and (ii) a standardized contract treatment, henceforth *SC*. In the IC treatment, principals can offer any contract that is within the given parameter range to any or all of the agents. In the SC treatment, the first accepted contract simply serves as a blueprint for all other contracts that a principal offers in a specific period. In other words, she is legally bound to offer the same contract to all employees. All other features of the markets are kept constant.

Our particular interest in this context is in the behavioral impact of different contractual specifications or limitations. Although we do not discuss the origin of these limitations, it is easy to envision that a law or some legally binding agreements are the source of the limitations. Standardization is, of course, only one possible contractual specification, but in our view very important both from a theoretical and from an empirical perspective. Whether principals would offer the same contract to each of their agents in any case even if they were be able to individualize, whether standardization would tend to

[2] Inspired by Cabrales and Charness (2003) but using a different assignment of gender, we assume throughout the paper that the principals are female and the agents are male.

result in lower levels of employment because non-reciprocal types are dismissed and not replaced, and whether the institutional limitation of standardization would have any effects on market efficiency, are obviously very relevant questions on labor markets and analogously on product markets with non-enforceable quality. Our experiment is able to answer all of these questions straightforwardly.

Note that in our setup agents are completely identical in their observable characteristics. They can only differ in their a priori unobservable inclination for reciprocity and their track record as an employee with regard to exerted effort levels from period 2 on. Since these conditions are hardly met by data from real markets, conducting an experiment has the advantage of sustaining control over many variables that are next to being unobservable in the field. Note further that without too much curtailment, what we denote a firm can also be considered an industry. Therefore, our results can, mutatis mutandis, be applied to collective contracts on incomplete labor markets, even though one has to bear the important restriction in mind that we do not model collective bargaining explicitly. Any application of our results to unionized industries with collective bargaining and collective contracts has to take that simplification into account. Examples for industries with predominantly incomplete labor contracts to which our setup is, in principle, applicable are manifold. On a general level, the prime example that we have in mind is the service sector that is characterized by a considerable degree of contractual incompleteness.

Our experiment provides clear and convincing evidence that the institutional difference between individual and standardized contracts that seems to be minor at first sight has a significant and economically relevant effect on market efficiency and other market parameters. For instance, it increases overall market efficiency by about 27%. Although many theories both with and without the incorporation of social preferences would not predict any effect of standardizing contracts within the firm, we surprisingly observe significantly higher average wages and effort levels with standardized contracts than with individual contracts.

Since there is no difference between the two treatments in the first period of play, it is rather obvious that the difference cannot arise from underlying behavioral determinants like for instance different interaction effects of the treatment variable with the risk attitude of principals. The most promising explanation for the stark difference is that principals react differently to the outcome of the very first period in the two treatments and in

subsequent periods. With individual contracts, reciprocal behavior of agents is usually reinforced by at least as or even more generous contract offers in period $t+1$, and non-reciprocal behavior is punished by dismissals. In a world with standardized contracts, a principal cannot target individual performance. If she wants to reinforce reciprocal behavior for single agents, she also has to offer equally generous contracts to all other employees. Although, the principal is able to decide about employment after each period and, therefore, can dismiss single agents, she has to offer the same generous contracts to any new agent or forgo the potential profit from employing an additional agent. These generous contracts that are forced upon principals by the institution of standardization seem to trigger higher levels of effort and, thus, achieve a higher market efficiency. Note, however, that it is the agents that predominantly profit from standardized contracts, while the payoffs of principals are not significantly higher with standardized contracts.

The important implication of our findings is that individualized contracts for the same job and under equal prerequisites might have detrimental effects on the efficiency of markets as compared to standardized wages because principals actually overuse the instrument of differentiation from an ex post perspective. Standardized contracts or even collective wages on the industry level may be able to induce higher levels of efforts on markets with incomplete contracts and may lead to a higher overall market efficiency. Again, this also applies, for instance, to the standardization of prices for the same medical service.

The remainder of the paper is organized as follows: Section 2 briefly presents related experimental studies as well as the details of the market and our experimental design. Then, we present our theoretical predictions in Section 3. Section 4 offers a brief overview of our basic experimental results. In Section 5, we consider and discuss several explanations for the treatment difference in greater detail. Section 6 concludes the paper and discusses its implications.

2 Existing evidence, experimental design and procedure

2.1 Existing evidence and related experimental studies

As already briefly mentioned in the introduction, experimental evidence of gift-exchange or reciprocal behavior in incomplete contract markets is quite solid and

conclusive. Almost all studies, however, focus on assessing the nature of reciprocity in the interaction of a single principal with a single agent. We are aware of only a handful of experiments that take multiple agents into account. None of these consider the question of contract standardization.

The most closely related papers are Cabrales and Charness (2003), Charness and Kuhn (2004) and Maximiano, Sloof and Sonnemans (2004). Cabrales and Charness study the optimality of different precast contracts offered by one principal simultaneously to two differently productive agents in a hidden information context. In their experiment, contracts have to be accepted by both agents. They thereby contemplate the common situation where contracts must be negotiated with a union and, then, be approved by the workers. If either agent rejects, both the agents and the principal receive symmetric reservation payoffs. Cabrales and Charness focus on the characteristics of the chosen contract menus and evaluate them against social preference models; however, they cannot focus on the impact of standardized contracts evaluated against individual contracts because contracts could not be targeted individually in their experiment.

Maximiano, Sloof and Sonnemans (2004) compare a standard bilateral gift-exchange game with a setting where each principal is matched with four agents. They observe almost equal levels of reciprocity in one-shot interactions, even though the principal earns much more than the agents if she employs four agents, leading them to conclude that (i) gift-exchange is rather robust to increases in the size of the workforce and (ii) intention-based reciprocity rather than outcome-oriented social preferences seems to be the main driving force behind gift-exchange.

Finally, Charness and Kuhn (2004) test predictions from social preference models on whether the behavior of two workers in the same firm is affected by the respective co-worker's wage. Contrary to their expectations that a wage compression effect should be observable, their experiments indicate that workers' effort choices are highly sensitive to their own wages, but largely unresponsive to co-workers' wages. Their experiment comes closest to our research question, but it only analyzes wage comparison and refrains from implementing contractual limitations like we do.

2.2 Experimental design

As laid out above we implement two different treatments. The two treatments share the following characteristics:[3] In each session, four subjects were assigned to the role of principals and 16 to be agents. Each participant received a unique identification (ID) number that remained the same throughout the experiment. Therefore, participants, though anonymous, were identifiable during the whole experiment, and repeated interaction was possible.[4] In each period (for the sake of succinctness, we suppress time indices), principals could offer contracts $\{w, \tilde{e}\}$ during a three-minute trading phase (stage one of every period), where w denotes the transfer or wage rate and $\tilde{e}$ the desired effort level. There was no limit regarding the number of contract offers within one trading period.[5] However, as soon as three contracts were accepted by agents, all standing contract offers of that principal were deleted immediately. In the experiment, w had to be an integer from the interval [1, 100]. Our market corresponds to a continuous one-sided auction where principals were the contract makers. They alone could offer contracts to agents, who in turn could accept any offer in real time but could not offer contracts to principals themselves. The duration of a contract was always one period. Thus, each period started with all the agents being unemployed.

After the trading phase, those agents who had accepted a contract had to submit an effort level e out of the set of feasible effort levels {1,2,….,10} (stage 2 of every period). The desired effort level of the principal was not binding.

Material period payoffs for the agent amounted to:

$$p_A = \begin{cases} w - c(e) & \text{if a contract was concluded} \\ 5 & \text{if no contract was concluded} \end{cases} \quad (1)$$

The principal's material period payoff is given by:

$$p_P = \begin{cases} a \cdot \left(\sum_{i=1}^{N} e_i \right) - \left(\sum_{i=1}^{N} w_i \right) & \text{if a contract was concluded} \\ 0 & \text{if no contract was concluded} \end{cases} \quad \text{with } i = \{1,2,...,N\} \quad (2)$$

[3] The basic setup follows Brown, Falk and Fehr (2004) in most characteristics. Of course, we extended their design to allow for multiple agents and the implementation of different contracts.

[4] The analysis of long-term relationships was the main focus of the study by Brown, Falk and Fehr (2004). We retained this feature to be able to detect any possible interaction effects of our treatment variable with the tenure of contracts.

[5] It was, of course, permissible to offer the same contract more than once.

where a is a productivity parameter and i counts the number of employed agents. In the experiment we set a = 10. Agents have to bear a cost of effort denoted by $c(e)$ that is increasing in efforts with increasing marginal costs according to the schedule in Table 1. Obviously, the efficient effort level is the maximum effort $e = 10$, as the marginal costs of effort are at most 3, and the marginal revenue is always equal to $a = 10$. If an agent does not conclude a contract, he receives a lump-sum payment of 5 tokens (the experimental currency unit) that constitute an unemployment benefit. Every agent could not accept more than one contract in a given period.

Insert Table 1 around here

Regarding the production function of the principal we decided to use a linear additive aggregation of individual efforts because such a setting seems to be most straightforward to start with, sufficiently general and easiest to convey to participants.[6]

A very important feature of the trading phase concerns the way contract offers could be submitted. Principals could decide whether they want to make private or public offers. For private offers, a principal had to indicate the agent's ID number with whom she wanted to trade. Only this agent was informed about the offer. Public offers were posted on the market screen and all agents as well as other principals were informed about them. Payoff functions, the cost of effort, the number of principals and agents, and the number of periods were common knowledge in all sessions.

Note that it was easy to establish a long-term relationship by offering contracts privately to the agent with the same ID number in any period. To indicate which agents had already concluded a contract, principals were informed about whether a specific agent was already employed or still unemployed at any time during the trading phase. Standing

[6] Other functions that can range from a weakest-link production function ($N \min\{e_i\}$) to a production function that incorporates teamwork advantages among agents (e.g., $N\sum_{i=1}^{N} e_i$) or even a production function assuming individual effort levels to be complements ($\prod_{i=1}^{N} e_i$) might be interesting. Luhan (2006) shows for a weakest-link production function that markets quickly collapse and approach the theoretic solutions derived under the assumptions that players are selfish money-maximizers.

offers whether private or not could be accepted at any time by those agents who were still unemployed.

We implemented two treatments that differ only with regard to a seemingly small institutional change. In the individual treatment (IC), principals can offer any contract that is within the parameter range to any agent privately or to the market. There are no other restrictions on contract offers. In contrast, there is a standard contract for each principal in every period in the standardized contract treatment (SC). The first accepted contract in this treatment simply serves as a blueprint for all other contracts that a specific principal offers in a trading phase, i.e. a specific period. In other words, she is legally bound to offer the same contract to all (potential) employees. As soon as the first contract of a specific principal was accepted, all standing contract offers of this principal that differed from the accepted contract were deleted, and, subsequently, she was only allowed to offer the same contract to other agents, either privately or publicly. All other features of the market remained unchanged.

The peculiar way we implement standardization is a consequence of the following arguments. First, we did not want to impose any standard contract ourselves because our main interest is in the endogenous evolution of exactly those market parameters that would have been imposed in this case. Second, we wanted to avoid a rather complicated collective bargaining procedure between principals and agents within a firm. Such a bargaining protocol would have rendered the direct comparison between the two treatments impossible and would have made the experiment much more complex for the participants. Therefore, we opted for the relatively simple mechanism that the first accepted contract serves as a blueprint for all following contracts within a firm in a given period. While this is clearly a simplification of reality if one aimed to assess the impact of collective bargaining, we believe that it is the most natural starting point and the only meaningful way to assess our research questions. It is important to note here that we do not claim to picture a collective bargaining situation. Our focus is on the mere effect of standardized, i.e. equal wages versus individual wages. One obvious alternative – letting principals offer only one blueprint contract from the beginning of each trading phase on – was discarded by us because it would have differed too profoundly from the IC treatment. Note finally that the payoff functions are completely identical for the two treatments for both principals and agents.

Each session consisted of 15 identical periods.[7] Upon arrival subjects received detailed instructions.[8] After reading them aloud, subjects were asked to solve several exercises in order to ensure that they completely understand the interaction. Having solved the exercises publicly, we answered any remaining questions privately. This part of the experiment usually took about half an hour. Then, the experiment started. At the end of each period, subjects were informed about contracts, payoffs and ID numbers of the principal and all employed agents within their firm. Thus, agents were not only provided with information concerning their contracts, but also received information regarding the contracts of their co-agents as well as the total payoff of the principal.[9] Since we wanted to ensure that subjects follow the development of market parameters, we asked them to write down the contractual feedback information on a documentation sheet after each period. At the end of the experiment subjects were asked to fill in a short questionnaire regarding socio-economic characteristics and were informed about their total earnings converted into euro. The money was paid to each participant privately and in cash.

The experiment was computerized, programmed and conducted with the help of the software z-tree (Fischbacher, 1999). For each treatment five sessions were run, with none of the 199 subjects participating in more than one of those 10 sessions.[10] Subjects were students from the University of Innsbruck and the Medical University of Innsbruck with various subject backgrounds. A session lasted slightly more than 90 minutes, and participants earned on average € 16.57.

[7] Two practice periods without any financial incentives were played in each session before the start of the experiment in order to make subjects familiar with the market interaction. ID numbers were reshuffled after the practice periods to avoid any transfer of reputation effects from the practice phase to the experiment. Subjects were fully aware of that.

[8] The full text of the instructions can be found in the Appendix. The conversion rate between tokens and euros was stated clearly in the instructions.

[9] There is a growing literature on the effects of wage comparison on contracts. In this paper, we wanted to avoid any direct influence und decided to inform agents about their co-agents wages only after effort submission.

[10] In one SC session, there were only 15 agents due to one participant who did not show up.

3 Predictions

There are several theoretical approaches that prove to be useful in order to derive theoretical predictions for our experimental setup. Let us start by assuming that all players are risk-neutral, rational and selfish, and that this is common knowledge. Since the interaction horizon is finite and known by all players, we can confine the analysis to one period and apply backward induction: Agents should choose the lowest admissible effort level e = 1 because effort exertion is costly. As a consequence, principals will offer only contracts (w = 5, $\tilde{e}$), where $\tilde{e}$ is obviously irrelevant theoretically.[11] Twelve trades should take place, i.e. each principal should employ the maximum of three agents, and four agents should remain unemployed. The resulting subgame-perfect equilibrium is the same for the IC and the SC treatments. Thus, we should not observe any difference in the market outcomes between the two implemented treatments.

The theoretical solution based on the assumption of common knowledge of rationality and selfishness entails a huge efficiency loss. In equilibrium $p_A = 5$ and $p_P = 3 \cdot 10 - 3 \cdot 5 = 15$. Within a firm, this amounts to $3p_A + p_P = 30$. It is obvious that these payoffs are small compared to the Pareto-optimum. Since marginal costs of effort are always lower than marginal revenue for the permissible parameter ranges, the latter would be reached at the maximum effort level e = 10 and the maximum number of employed agents. Wages can be disregarded here because they only constitute a re-allocation of the generated rents. Given our parameters, $3p_A + p_P = 246$ is the Pareto-optimum. Again, there is no difference between the two treatments.

We now consider the case where rationality or selfishness of all players is not common knowledge. The important general conclusion is that now a lot of different outcomes and equilibria may occur because reputation building can become a profitable strategy. For instance, even completely selfish agents might have a strategic incentive to mimic agents with social preferences. Models like in Kreps et al. (1982) or social preference models (e.g., Rabin, 1993; Levine, 1998; Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Dufwenberg and Kirchsteiger, 2005; Falk and Fischbacher, 2005) would be consistent with such a strategic reasoning. Using the model of Fehr and Schmidt (1999), Brown, Falk and Fehr (2004) show for the one-on-one principal-agent matching that if

[11] For w = 5 agents are indifferent to accept the offer. Since we require the wage rate to be an integer value, w = 6 can also be optimal. From empirical purposes the difference is of minor importance.

sufficiently many fair subjects are present, there is an equilibrium in which all agents exert high efforts in the first 14 periods, while in period 15 only the fair-minded agents submit non-minimal effort levels. The basic idea is straightforward: Since the existence of fair agents makes it profitable for principals to offer generous contracts, selfish agents have an incentive to disguise their real type and mimic the fair agents until the end of the next to last period.

It is quite straightforward to extend their analysis to the multiple agent framework that we use. For the sake of succinctness we only present an upshot of the arguments here.[12] If players are outcome-oriented, they will take into account that each principal can employ up to three agents.[13] Thus, in order to induce the same level of reciprocity in terms of efforts, wage offers have to be significantly higher than in the one-on-one gift-exchange setting. The size of the difference depends, of course, on the distribution of the inequity aversion parameters within the firm. Furthermore, one has to distinguish whether agents care for the material payoffs of the co-agents, i.e. co-workers, or not. Although these differences between the one-on-one and the multiple agent gift-exchange game makes the task for inequity averse players much more demanding and it is difficult to judge empirically whether certain outcomes arise from ex post correct beliefs or by mere chance, similar equilibria as described above for the one-on-one interaction can be derived. If we, however, assume that players do not care about the outcome but apply intention-based reciprocity arguments (in the spirit of Rabin (1993) and the subsequent literature), then we would again expect no difference between the one-on-one game and the game with multiple agents because the relation between the principal with other agents is simply neglected when only intentions play a role.

Regardless of whether the theoretical predictions between the one-on-one game and the game with multiple agents actually differ or not, our focus is, of course, on whether there is a difference in theoretical prediction between our IC treatment and our SC treatments. It is obvious that some of the equilibria that are discussed above are not feasible in the SC setting because they encompass different contract offers to different agents. Apart from that restriction, all equilibrium contract offers in the IC treatment with the same level of wages are also equilibria in the SC treatment. Thus, also the set of

[12] Maximiano, Sloof and Sonnemans (2004) provide a more detailed discussion for a strategically similar situation.

[13] If they simply ignore that fact, the predictions would be identical to the one-on-one setting.

models that drops the assumption of common knowledge of rationality and selfishness would not predict any difference between our IC treatment and our SC treatment.

In summary, the theoretical models discussed above would predict no difference between our two treatments. It is, hence, safe to formulate the hypothesis that the results in terms of efficiency, wages, effort levels and other important market parameters should be identical for the two treatments. Recall, however, that we have to deal with multiple equilibria. From a behavioral perspective we cannot exclude that the institutional difference between our two treatments renders different equilibria more likely. [14]

4 Basic results

We start by presenting an overview of the general results from the experiment and will postpone a more detailed discussion and attempts of explanation to Section 5. Table 2 provides some descriptive statistics of the most important market variables. We display the overall means with standard deviations in parentheses. All observations are measured in tokens and percent, respectively. Significance tests have been conducted using the mean values of each experimental session, thereby using five independent observations on each variable per treatment, which is a very conservative approach. We start be applying a two-sided Mann-Whitney-U-test to compare the two treatments in order to avoid making assumptions about functional form or error distribution.

Insert Table 2 and Figure 1 around here

It becomes immediately obvious that several important market variables are significantly different between the IC and the SC treatments, contrary to our theoretical predictions. Wages are by 29.71% and significantly higher in the SC treatment than in the IC treatment. Figure 1 shows that mean wages in the SC treatment are in all 15 periods well above those of the IC treatment, with the notable exception of the very first period. In both treatments wages display an upward trend over time that appears to be stronger in the SC than in the IC treatment. While SC wages were well above 40 tokens from the fourth period on, reaching their peak of 55.15 in round 13, the lower IC wages range between 31

[14] If we had not had such a feeling, we would, of course, not have conducted the experiment.

and 41. We find a common last round effect, but wages stay above 40 and 30 tokens, respectively.

Given the higher wages, it is not surprising that effort levels are also higher in markets with standardized contracts than in markets with individual contracts. The difference of 29.08% between the two treatments for efforts matches the 29.71% wage difference almost perfectly. Again, the difference is significant; we observe an upward trend and a rather strong last-round effect.

Insert Figure 2 around here

With regard to unemployment, we do not find a significant difference in the IC treatment and in the SC treatment. While there is a little bit more variance in the number of contracts offered in the SC treatment, the overall level of unemployment is close to the minimal number of four agents in both treatments. Principals are slightly more reluctant to make offers in the SC treatment, but the difference is not significant. Figure 3 gives an overview of these results.

Insert Figure 3 around here

Obviously, a very important variable is market efficiency. With higher wages and efforts, and unemployment rates almost equal to those of the IC treatment, it is clear that the overall market efficiency is significantly higher in the SC treatment. The first row of Table 2 shows aggregate mean payoffs per period generated within a market, i.e. token earnings of all 20 players. In the SC treatment these payoffs are on average 124.96 tokens or 23.68% higher than in the IC treatment. While in the IC treatment nearly 48% of the maximal efficiency is forfeit, this loss reduces to 35% in the SC treatment. Figure 4, which displays average per period payoffs, conveys a clear impression of the situation: While in the IC treatment average payoffs per period lie in the range of 30 to 40 tokens, profits in the SC treatment are significantly higher over periods 1 – 14, ranging between 40 and 50 tokens. Finally, note that both principals and agents earn more in the SC treatment than in the IC treatment, but the difference is only significant for the agents because the variance of per period payoffs is much larger among principals than among agents. Figure 5 give an

impression of the distribution of payoffs in the two treatments that remains roughly the same across treatments.

Insert Figures 4, 5 and 6 around here

The last significant difference between the two treatments concerns the share of private offers. Not surprisingly, this is higher in the SC treatment than in the IC treatment. Figure 7 depicts the time trend of offers in that respect. Targeting specific agents in order to induce higher levels of reciprocity seems to be one of the differences that the institution of standardized contracts causes. However, overall tenure is not significantly higher in the SC treatment, though higher on average and also higher for all but two periods.

Finally, figures 7 and 8 provide a nice overview of what would have been an optimal strategy ex post. Principals in the IC treatment earned most with wage offers between 61 and 70 tokens; for principals in the SC treatment the ex post optimum lies in the wage-range of 51 to 60 tokens, but the difference between the two treatments with regard to the optimum contract offer is small in absolute terms. Agents would have fared best by exerting the maximum effort in both treatments. While there is no difference between the profit of agents in SC and IC for higher levels of efforts, lower levels of effort lead to higher profits for agents in the SC treatment than in the IC treatment.

Insert Figures 7 and 8 around here

In order to examine our results further, we ran several regressions that allow us to assess the extent of reciprocity in a more rigorous manner. The regressions basically reproduce our findings based on non-parametric statistics but they also offer new insights. The dependent variable is the effort level. We use Tobit regression to take into account the censoring of the effort levels at $e = 1$ and $e = 10$. Models (1) – (4) in Table 3 are without session fixed effects; models (5) – (8) reproduce the same estimations with session fixed effects. We start with an interpretation of models (1) – (4).

Insert Table 3 around here

We start out with a model (model (1)) that accounts for the following independent variables: wage, period and period squared, a dummy for private offers and a dummy for our SC treatment. As expected, wages and the use of private contracts have a significantly positive impact on efforts chosen. The variables *period* and $period^2$ account for a supposedly non-linear time trend in all estimated models. Indeed the time trend describes an inverted U-shape, with efforts rising in earlier periods and declining later on.

If we include the tenure of the relationship (model (2)) between a principal and an agent as an independent variable, the significance levels of all other variables remain unchanged and the magnitude of the effects is quite similar. Since one might suspect that incorporating the variable tenure might create an endogeneity bias, it is important to note that this does not seem to be the case. The variable itself is, as expected, highly significant.

We, of course, considered a few other variables. First, including desired efforts as an independent variable does not change any significance levels. Since its behavioral effect on actually chosen efforts is, however, quite strong, it slightly affects the magnitude of the coefficients of the wage level.[15] Co-agents wages and wage differentials within a firm cannot (and actually do not) have an impact because agents get the information only after they have decided on effort levels.[16]

Regarding the treatment dummy, the results are surprising at first sight. Our estimations in models (1) and (2) indicate that there is no independent effect of the SC treatment on effort levels. Ceteris paribus, efforts contingent on wages are indeed not lower or higher in the SC treatment than in the IC treatment. One immediate conclusion that we will take up in the next section again is that agents are clearly not more reciprocal or more generous by choosing higher effort levels in the SC treatment compared to the IC treatment. The institutional difference seems to be caused by the behavior of principals, i.e. in the context of wage setting.

The treatment dummy as in models (1) and (2) only reveals differences of the overall effort levels and does not come out significant. To find out whether agents show differences in reciprocation in markets with and without standardized contracts, we have to estimate the sensitivity of efforts on wages for each treatment separately. To this end, we

[15] This is another indication that our results are rather robust. Estimations with the desired effort level as an independent variable are available upon request.

[16] Whether they have an impact on behavior in the following period, is difficult to test in our setup because the assignment of principals and agents usually change across periods.

introduce an interaction dummy variable called *SC-wage* in models (3) and (4) that captures the slope of the wage-effort function. It is immediately apparent that the introduction of this variable renders the pure treatment dummy now significantly positive, although the results for all other variables remain almost unchanged. The interaction dummy is significant negative, but its magnitude is negligible.

The estimations that take into account session fixed effects are very similar with regard to the influence of the wage, period, tenure and private contracts. However, they are a bit more inconclusive regarding the effects of standardization. The SC dummy is both insignificantly positive (model (4)), weakly significantly (model (7)) and highly significantly positive (model (8)). The interaction term looses significance. Nevertheless, we believe that it is safe to conclude that there is not much difference in the nature of reciprocity across the two treatments.

For the wage variable, however, the estimations in Table 3 show that reciprocity plays an important role on incomplete labor markets. Hence, we confirm existing findings from similar experiments. It is comforting to note that our findings regarding important market characteristics are qualitatively and often even quantitatively[17] in line with the results from Brown, Falk and Fehr (2004). Although they analyze a one-on-one gift-exchange market and they only have seven principals and ten agents, which makes it impossible to compare the two experiments directly, it is a re-assuring fact that the results from different setups exhibit very similar characteristics. The general conclusions from gift-exchange markets, for instance, regarding reciprocity seem to be very robust with regard to many changes in the market setup.

It is not surprising, however, that the quantitative level of reciprocity tends to be lower in our experiment. This is due to the fact that each principal can employ up to three agents and, thus, can earn triple the amount of principals in the usual one-on-one setting. As a consequence, the level of reciprocity of individual agents tends to be less pronounced but the reduction effect is much smaller than required to equalize the advantage for the principal of being able to employ up to three agents.

The important question now is why we observe this marked difference in wages, effort levels and market efficiency but not in terms of reciprocity between the two treatments, although no theoretic approach has predicted it. The institutional difference

[17] We could compare all market variables, even those not reported in Brown, Falk and Fehr (2004), because the authors provided us with their raw data. We gratefully acknowledge that.

between the IC treatment and the SC treatment must induce an important behavioral reaction. The next section offers an explanation for our findings.

5 Explanations for the difference

Let us briefly recap the stylized facts from our experiments: On a labor market with incomplete contracts between a principal and several agents, individual contracts fare worse than standardized contracts in several respects. The treatment with individual contracts leads to significantly lower wages, effort levels and a significantly lower market efficiency than the treatment with standardized wages within the firm, while the unemployment rate remains unchanged. Remarkably, no theory that we are aware of – neither with the assumption of selfish and rational players nor by relaxing the selfishness assumption – is able to explain our observations. This section aims to propose an explanation that is consistent with our findings.

First, it is important to know before proceeding whether wages or efforts are the initial trigger that, ultimately, leads to a superior result in the SC treatment. This is not apparent in a situation that is characterized by reciprocal gift-exchange. A very important finding to start out with is that neither wages nor efforts are statistically different in the first period of the two treatments. Table 4 gives a first impression of the emergence of the difference between the IC treatment and the SC treatment by comparing averages of important variables in the first period and in the second period of each treatment. While there is no difference between the treatments in the first period, several variables are significantly different in the second period, especially the market volume, i.e. the sum of all rents, wages, efforts, desired efforts and the profit of agents.[18] Note also that, although there is a further small increase in the difference between wages and efforts in IC and SC in later periods according to Figure 1 and Figure 2, the strongest effect seems to be in period 2.

Insert Table 4 around here

[18] Significance tests can be found in the Table.

Recall that our regression analysis in the previous section has indicated that there is hardly any difference in overall reciprocity between the two treatments and that, therefore, wages are the driving forces for the superior outcome in the SC treatment. Obviously, something important happens between these two periods that is caused by the institutional difference between our two treatments and that is induced by the wage offers of principals. Thus, it seems plausible that our explanation must take the dynamics of the events into account.

Corroborating evidence for our claim that it is not the nature of reciprocity that is different in the two treatments but that the difference is due to a change in accepted wage offers of principals comes from Figure 9, which displays mean effort levels conditional on wages in steps of ten tokens. Reciprocal behavior is almost identical in the two treatments according to the bars in Figure 9, but lower wage offers are much more prominent in the IC treatment than in the SC treatment. Whereas in the former, 22.58% of all accepted offers are in the range 0 and 10 tokens, only 8.38% of the accepted offers in the SC treatment fall in this category. About 57% of all accepted wage offers are below 30 tokens in the IC treatment, while around two thirds are above 30 tokens in the SC treatment.

Insert Figure 9 around here

Apparently, the institution of standardization causes principals to offer higher wages, on average, in period 2, which in turn triggers higher effort levels by agents, leaving the degree of reciprocity contingent on the wage levels, however, unchanged.

Assume that principals evaluate their three contracts – most of them actually had three in the experiment – after the first period in terms of profits either against an expectation (signaled through the desired effort level) or in relative terms against each other. Denote the three desired effort level by $\tilde{e}_i$ and suppose that $\tilde{e}_1 > \tilde{e}_2 > \tilde{e}_3$. In the IC treatment an average principal will try to employ those agents again that fulfilled her expectations ($\tilde{e}_i \leq e_i$) or, alternatively, those who performed best in terms of reciprocity relative to other agents within her firm. She will usually offer them a contract privately that is at least as generous as the previous one in order to sustain the beneficial relationship. Obviously, a principal will, however, if she is rational not employ a shirking agent again. Since employing three agents is profit maximizing ex post (and almost all principals have realized that fact already from the first period on), principals will replace shirking agents

by other agents in period 2. For this end, contract offers will either be made privately, directed to a random agent, or publicly. At least in the latter case, principals will tend to use not too generous contracts in order to be on the safe side as long as a multi-period relationship is not established, yet. Thus, average wages even fall slightly in the IC treatment over the first two periods.

In contrast, in the SC treatment, principals cannot target specific contracts to specific players unless they choose not to employ the maximum number of agents, which would be detrimental in terms of profits. Of course, a rational principal will not offer a generous contract to a shirking agent again in the SC treatment, just as in the IC treatment. Suppose, for the sake of the argument, that at least one agent within a firm behaved reciprocal ($\tilde{e}_1 \leq e_1$) and at least one shirked $\tilde{e}_3 > e_3$. If the principal wants to reward the reciprocal agent, she has to offer an at least as generous contract to this agent. The difference in the SC treatment compared to the IC treatment is, however, that she has to offer that generous contract to anybody else she is going to employ. While the principals in the IC treatment could both reward reciprocal agents and offer less generous contracts to "new" agents in order to test their inclination of reciprocating, this is impossible in the SC treatment. If she wants to reward a single agent in the SC treatment, she has to offer the same generous contract to anybody else she is going to employ or decide not to employ the maximum number of three agents. Since the latter was clearly not the case, on average, principals must have offered generous contracts to more agents than they would have if they could have offered individual contracts.

Are our data consistent with this line of reasoning? Several statistical analyses provide rather conclusive overall evidence. Let us start by comparing contracts in greater detail. Table 5 provides results for disaggregated private and public offers that were actually accepted by agents. It shows that for private offers, the SC treatment starts out with much lower levels of wages than the IC treatment, although the difference is not significant on the session level with five independent observations. The important change appears in the second period. While private offers, on average, stay almost the same in the IC treatment, we see a surge of wages in accepted private offers to an average of $w = 52.11$ in the SC treatment. Consistently, this is the region of wage offers that turned out to be profit maximizing for principals ex post.

For the public offers on the market, a similar effect is observed. Here the level of wages in the SC treatment is already slightly higher in the first period, but far from being

significant. While the average wage in accepted public offers fall, on average, for the IC treatment, indicating that principals get more reluctant to offer generous contracts to unknown agents, it increases substantially for the SC treatment so that the difference between the two treatments in the second period is significant. This is consistent with our explanation that principals in the SC treatment are triggered to offer more generous contracts even to unknown agents in order to be able to reward and re-employ other highly reciprocal agents that they have already employed in period 1. Consequently, we observe a smaller difference between wages in accepted private offers and public offers in period 2 of the SC treatment (18.06 tokens) than in the IC treatment (23.53 tokens).[19] Note finally, that there is only a small difference between average actually accepted wages and the average of all offered wages either publicly or privately. The same holds true for the first offer on the market in a specific period and the first accepted contract in that period.[20] Hence, our conclusions also hold for offers and not only for accepted offers.

Insert Table 5 around here

Another important piece of information is the wage dependent on whether an agent is re-employed by the same principal or not. Since the strongest treatment effect seems to emerge in the second period, we confine our analysis to it. Table 6 gives a descriptive overview. For both treatments, wages of tenured agents are higher in the second period than for non-tenured agents. This is not surprising and confirms the findings of Brown, Falk and Fehr (2004). It is also consistent with our explanation that both private and public wages in accepted contract offers are not different for tenured agents, i.e. agent that are employed by the same principal in both period 1 and period 2. The important information from Table 6 regards accepted contracts of agents in period 2 that have been employed by a different principal in period 1 or have been unemployed in period 1. For those agents, wages in accepted public offers are significantly higher in the SC treatment (32.18 tokens) than in the IC treatment (24.99 tokens). Wages in accepted private offers are also much higher but not significantly (50.83 tokens in SC versus 38.75 tokens in IC). Clearly,

[19] Our conclusions are confirmed when we use individual data instead of session average. Results are available upon request.

[20] We are happy to provide detailed results on request.

principals offer more generous contracts to agents in period 2 that have not been employed by them in period 1 in the SC treatment than in the IC treatment.

Insert Table 6 around here

Let us now take a closer look at efforts. According to our explanation there should not be a lot of a difference between the developments of effort levels of tenured agents. But do the higher wages in SC trigger higher effort levels as well? The average first-period effort level of agents who were not re-employed in round 2 by the same agent, i.e. they were fired, is 3.32 for the IC treatment and 3.47 for the SC treatment. However, in the second period average effort levels of those agents increase to 4.99 in the SC treatment, while it increases only to 4.19 in the IC treatment. Obviously, the higher wage rates trigger higher effort levels.

As a final confirmation of our explanation we disentangle the reactions of principals on differently reciprocal agents. Again we confine ourselves for the sake of succinctness to the transition from period 1 to period 2. Table 7 provides especially interesting evidence. We compare the development of wages from period 1 ($w(t\text{-}1)$) to period 2 ($w(t)$) for three types of agents: (i) the one with the lowest effort level within the firm (min), (ii) the one with the medium effort level within the firm (med), and (iii) the one with the highest effort level within the firm (max). In the IC treatment we see a very strong differentiation in wages contingent on efforts in the previous period. Whereas the average wage of those agents who provided the highest effort within their firm in period 1 rise from 35.67 tokens in period 1 to an astonishingly high 59.17 tokens, the wages of the agents with the median and the minimum effort level within the firm in period 1 rises only by 1.40 and 0.15 tokens, respectively.

Insert Table 7 around here

In the SC treatment where principals can only decide not to re-employ a specific agent but have to offer the same contract to new agents as to highly reciprocal we do not observe such a differentiation. The wage difference between the first and the second period contingent on effort levels in the first period is very similar and does not depend systematically on whether one has provided the highest effort within the firm in period 1

($w(t) - w(t\text{-}1) = 12.11$), the median effort ($w(t) - w(t\text{-}1) = 6.71$) or the minimum effort ($w(t) - w(t\text{-}1) = 8.95$). Table 7 disentangles the result for agents that are employed by the same principal in the two periods and agents who are employed by a different principal. Although the number of observations become rather small for certain categories, the overall picture is quite clear. Obviously, there is not much difference between the two treatments for re-employed agents, but there is a very stark difference similar to the one described above for the aggregated data for agents who change the principal. Hence, the driving forces behind our results are the newly employed agents and, consequently, the inability to distinguish contracts with regard to them. Summarized in a simple statement, even agents that chose low levels of efforts in the first period get a second change in the SC treatment, whereas they are put under scrutiny (by offering them trust contracts to a smaller degree) in the IC treatment. It is interesting to note here that the principals in the SC treatment exert their limited power – they cannot target contracts but only dismiss shirking agents – to a surprisingly great extent. None of the 20 agents that provided the minimum effort within their firm in period 1 is re-employed by the same principal, but 15 of them are newly employed by a different principal.

All these pieces seem to be consistent with our explanation. An important question remains, however, to be answered: Why did principals in the IC treatment not simply offer the same contract to each agent and thereby copy the more efficient SC treatment? At first sight, one might expect that principals underestimated the degree of reciprocity among agents and, therefore, refrained from offering the same (generous) contract to each agent. A closer look at the data suggests an alternative explanation: From Table 2 it is obvious that principals do not earn significantly more in the SC treatment than in the IC treatment. Although the average profits of principals are higher in the SC treatment than in the IC treatment, there is quite a lot of variance. If principals are risk averse to a certain degree, their utility might not necessarily be increased by offering the same uniform contracts in the IC treatment as in the SC or at least by not much.[21] The agents who profit a lot in terms of payoffs from uniform contracts in an IC situation that copies the SC treatment cannot implement them because they are not the contract makers. It is, therefore, not that

[21] If they actually could improve their earnings significantly, we would expect a convergence between the results from the IC and the SC treatments in later rounds due to learning effects in the IC treatment. This is clearly not the case.

surprising as it may seem that principals do not copy the superior SC treatment in the IC treatment, which would be obviously easy to do.

Other explanations for our main result regarding the difference between the SC and the IC treatment, though possibly appealing at first sight, are hardly consistent with our data. For instance, if the SC treatment with its uniform contracts would create more security or a feeling of mutual trust among principal and agents that might foster reciprocity (according to the following simple argument: as an agent I know that I am not the sucker within the firm already before I determine efforts), we should already observe a positive effect on wages and effort levels already in the first period. This is clearly not the case.

A failure to find the ex-post optimal contracts in terms of payoffs in the IC treatment is also hardly an explanation for our findings because it would predict that the wedge between the results from the two treatments should decline in the course of the experiment. With the exception of an endgame effect that is stronger for the SC treatment, this does not seem to happen. If principals were just more cautious or more considerate in the IC treatment than they are by institutional force in the SC treatment, we would observe a smaller number of offers in the first period. Table 4 clearly refutes such a hypothesis.

6 Conclusion

We have studied the impact of individual and standardized contracts on markets with incomplete enforcement. A principal can either offer individualized contracts or standardized contracts to several agents that determine a costly effort level after accepting a contract. Our gift-exchange experiment provides clear and convincing evidence that the institutional difference between individual and standardized contracts that seems to be minor at first sight has a significant and economically relevant effect on wages, effort levels and market efficiency. Standardized contracts lead to superior results compared to individual contracts.

Since there is no difference between the two treatments in the first period of play, it is rather obvious that the difference cannot arise from underlying behavioral determinants like for instance different interaction effects of the treatment variable with risk aversion of principals. We, therefore, propose an explanation that takes the dynamics of the relationship between principals and agents into account. When deciding upon re-

employment in round $t+1$, it is impossible to target individual performance in a world with standardized contracts. If the principal wants to reinforce or reward reciprocal behavior for single agents, she has to offer equally generous contracts to all other employees. These generous contracts that are forced upon principals by the institution of standardization trigger higher levels of effort and, thus, achieve a higher market efficiency. Note, however, that it is the agents that predominantly profit from standardized contracts, while the payoffs of principals are not significantly higher in the SC treatment.

A similar reasoning and conclusion apply to product or service markets with incomplete contracts over the quality level of a good. Here, the buyer is the principal and the seller the agent who decides on the provided quality after seeing the price. Our results lend support to the view that on such markets standardized prices would lead to more efficient markets than with individually determined prices.

The important implication of our findings for labor markets is that individualized contracts for the same job and under equal prerequisites might have detrimental effects on the efficiency of markets as compared to standardized wages because principals actually overuse the instrument of differentiation from an ex post perspective. In this paper, we were interested in the pure effect of the same wage for the same job that cannot be contracted upon completely. In such a case, uniform contracts clearly fare better than individually targeted contracts. Real-world applications would be manifold in the service industries. Actually, universities are a very good example in that respect. Different wages for different groups of employees that have the same job description seem to be detrimental in terms of market efficiency. Such discrimination is quite common on public universities in Europe because employment laws for the public sector change quite rapidly, and different lecturers or professor have quite different contracts in terms of wages and pension claims. This is obviously suboptimal in terms of achieving high effort levels. But there are also lots of examples of private firms in a more or less incomplete contract environment that offer different and usually less generous contracts to younger employees than older employees actually hold.

Does this mean that collective wages are superior to individual wages and unions actually increase market efficiency? Several limitations must necessarily be pointed out. First, we do not model any bargaining between principals and agents. Second, and more importantly, the stark difference between the two treatments shows up from the second period on. It is, thus, not inherent to the institutional background but a consequence of a

repeated interaction. While some re-shuffling and flexibility is certainly present in many labor markets, there are also labor markets that are rather rigid. Whether our results apply to these latter cases, can be disputed. Third, we of course implement a specific situation with regard to productivity and effort costs that might not prevail for each different cost of effort function in the field. Especially the latter limitation calls for more evidence. We believe that especially field experiments would be a useful remedy to circumvent the limitations of an, in the end always, artificial setup in the laboratory.

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Tables and figures

Figure 1

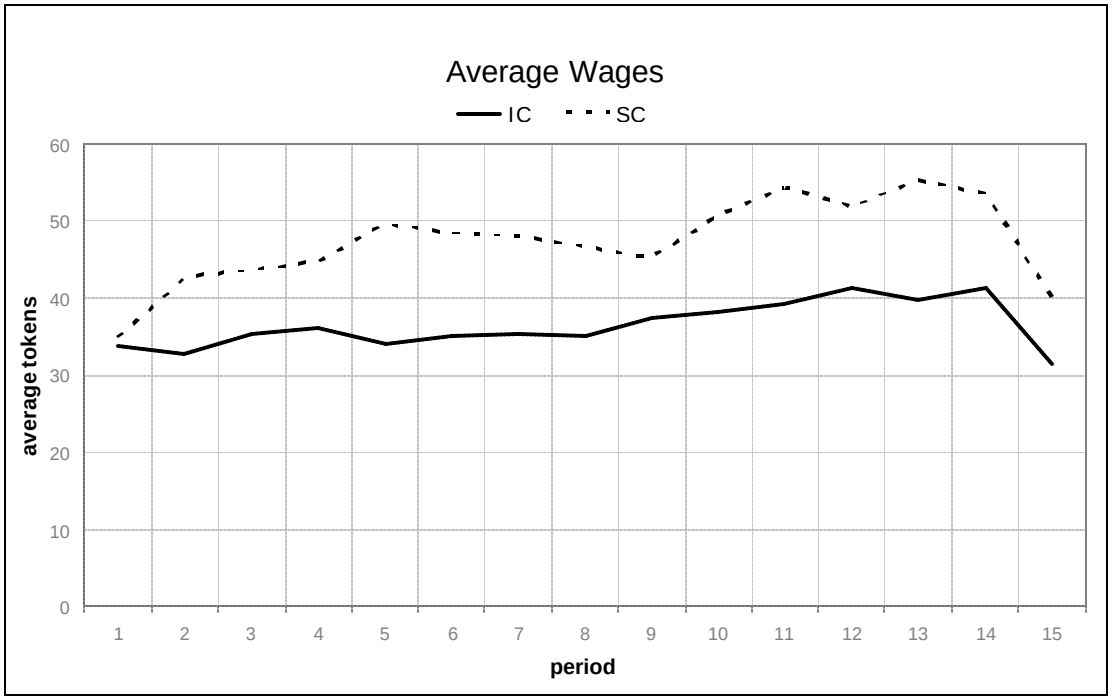


Figure 2

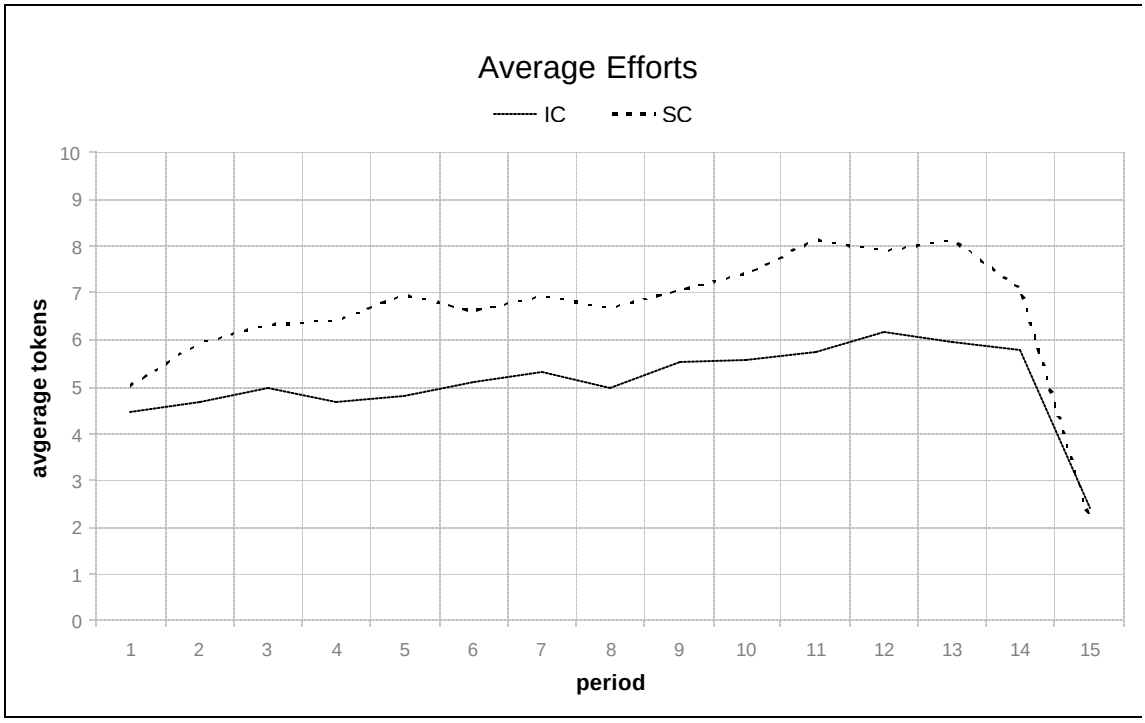


Figure 3: Average Numbers of offers and Trades

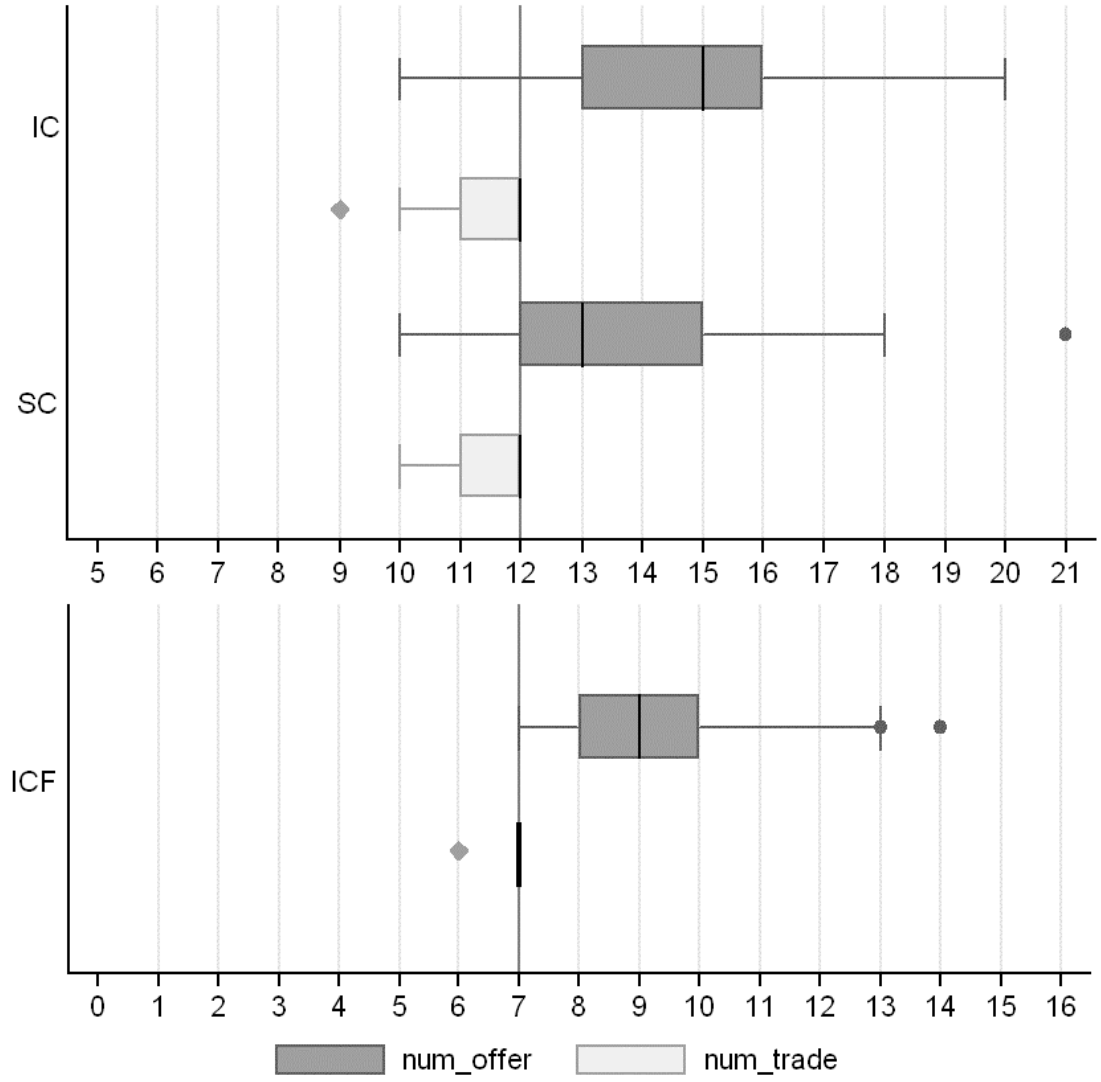


Figure 4

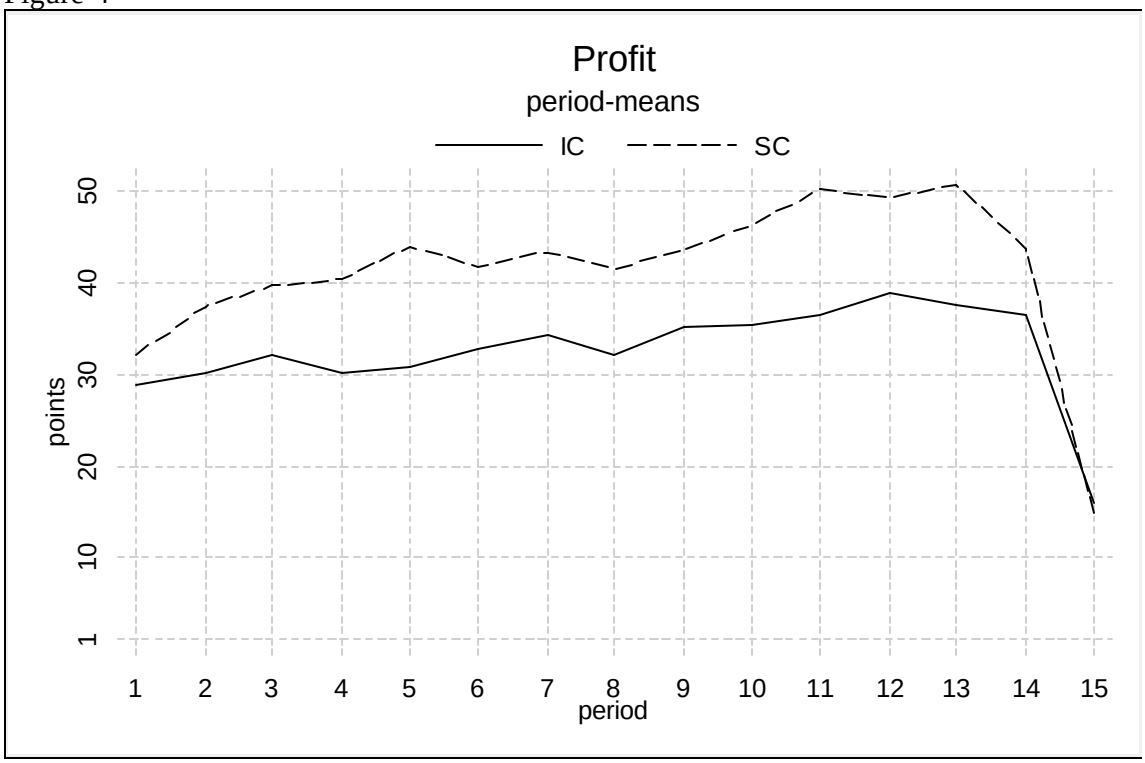


Figure 5

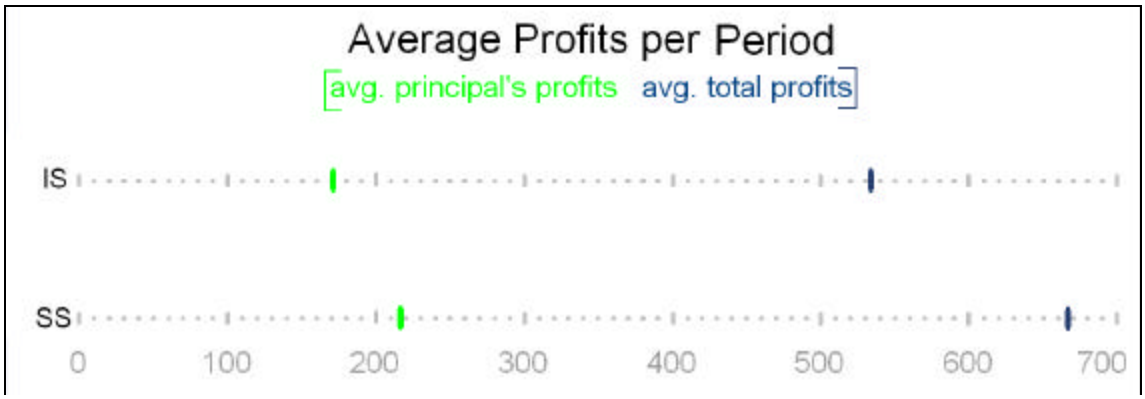


Figure 6

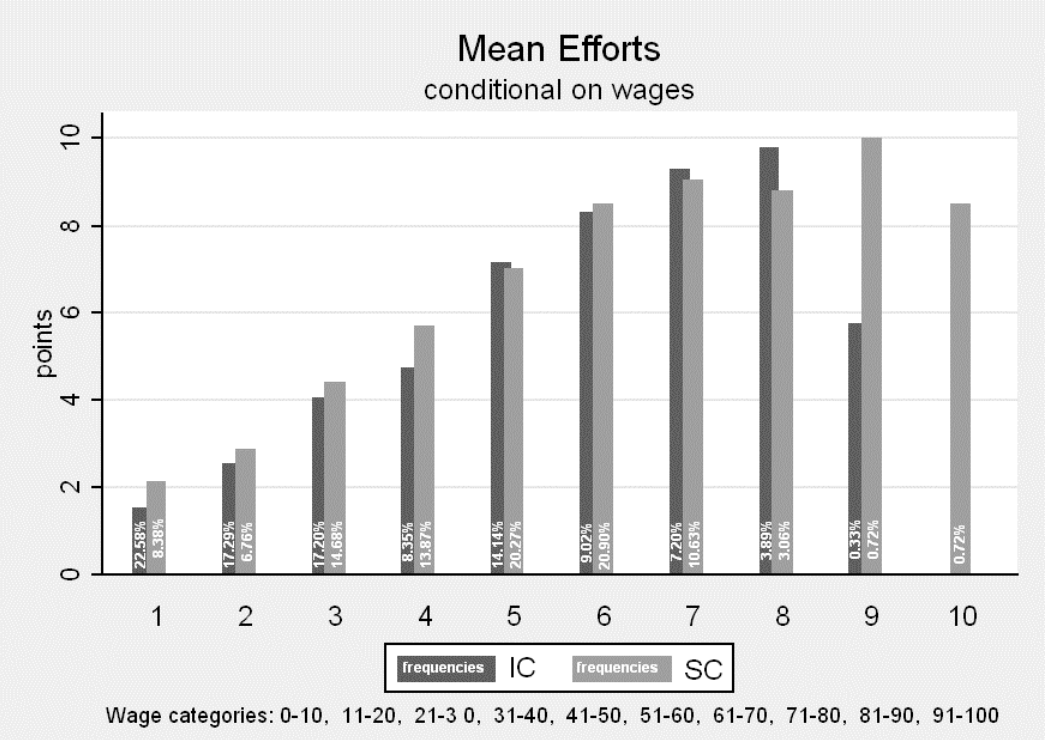


Figure 7

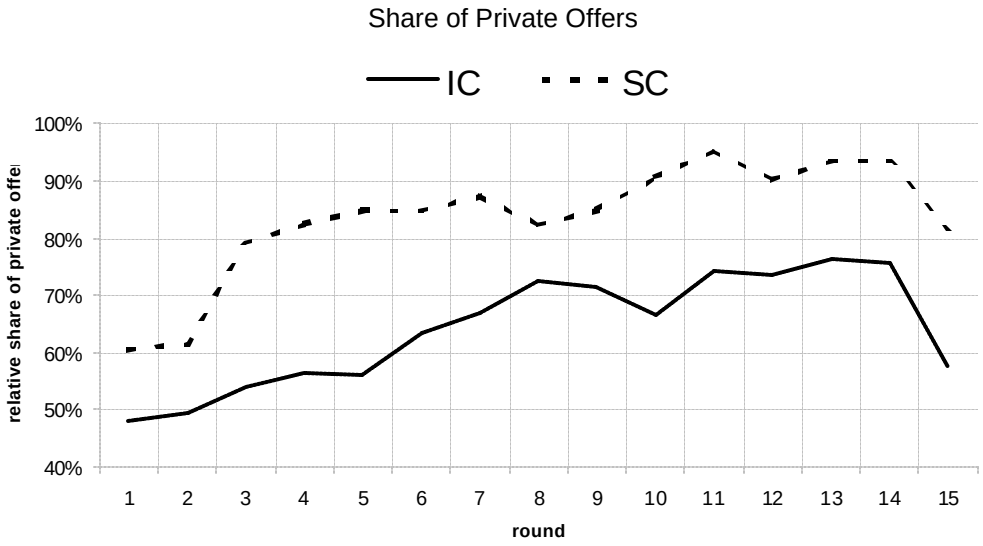


Figure 8

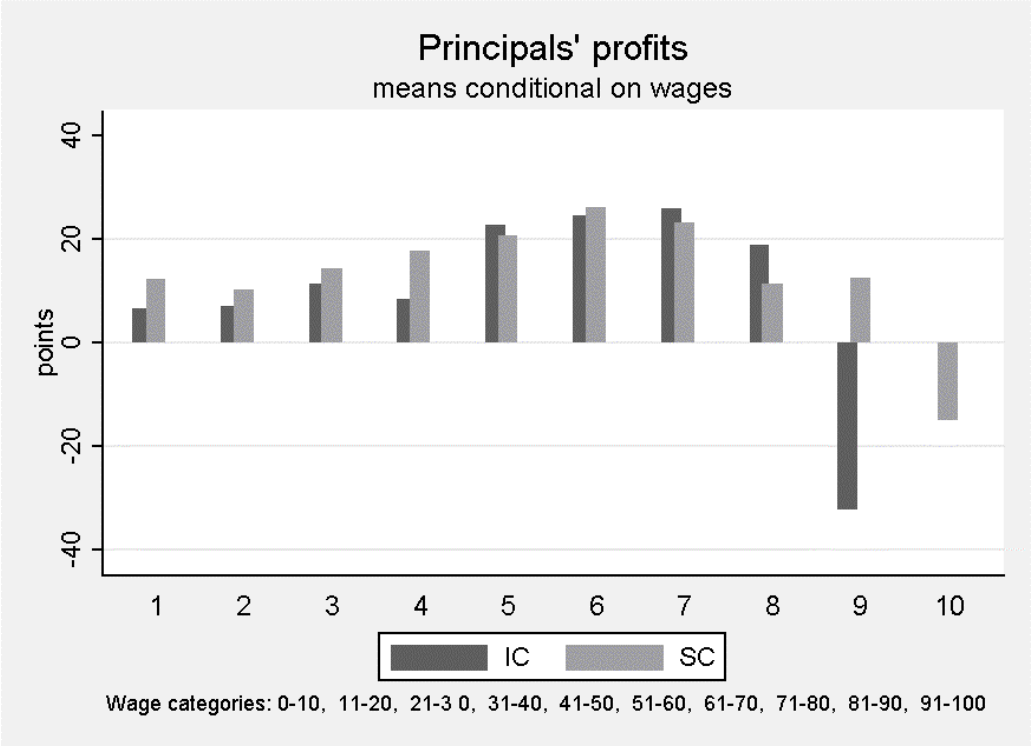


Figure 9

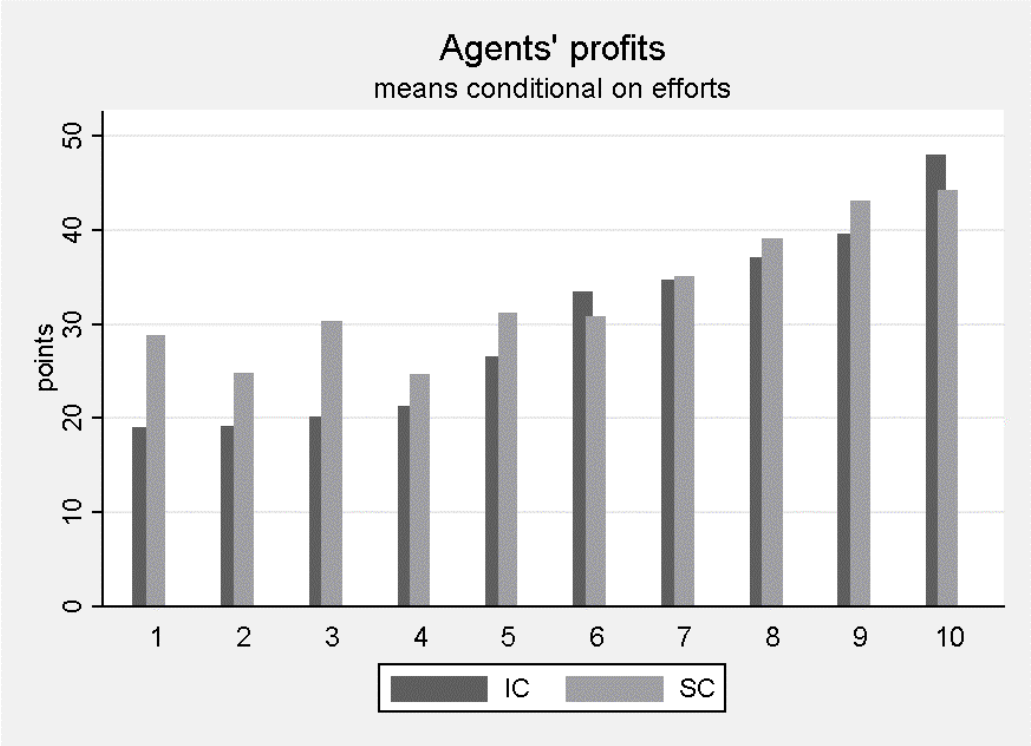


Table 1

Cost of effort

Effort *e*	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Cost *c(e)*	0	1	2	4	6	8	10	12	15	18

Table 2: Mean value of important variables in the experiment

Variables	***IC***	***SC***	***P (Mann-Whitney) two-sided***
market volume	**527.65** (128.81)	**652.61** (94.32)	0.076
share principal	0.27 (0.07)	0.28 (0.06)	0.465
wage	**36.35** (8.17)	**47.15** (4.25)	0.047
effort	**5.09** (1.50)	**6.57** (0.80)	0.047
desired effort	7.01 (0.95)	7.91 (1.07)	0.117
number of contracts	11.68 (0.23)	11.57 (0.49)	0.754
number of offers	14.64 (2.05)	13.47 (1.18)	0.602
share of private offers	**0.66** (0.16)	**0.83** (0.16)	0.076
profit principal per contract	14.58 (6.91)	18.54 (5.36)	0.347
profit principal	42.59 (20.50)	53.96 (16.52)	0.465
profit agent	**29.13** (5.06)	**37.06** (3.36)	0.028
average tenure	2.09 (1.48)	2.68 (0.77)	0.602

Note: We display the overall means with standard deviations in parentheses. All observations are measured in tokens and percent, respectively. Significance tests have been conducted using the mean values of each experimental session, thereby using five independent observations on each variable per treatment. Bold numbers indicate a significant difference between the two treatments on the 10% level.

Table 3: Determinants of Effort and Treatment Differences between IC, and SC

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Wage	0.162***	0.152***	0.171***	0.159***	0.162***	0.154***	0.167***	0.158***
	(35.38)	(32.41)	(28.62)	(26.07)	(35.29)	(32.62)	(27.77)	(25.64)
Period	0.185***	0.162***	0.202***	0.177***	0.210***	0.187***	0.220***	0.195***
	(3.51)	(3.12)	(3.81)	(3.37)	(4.04)	(3.62)	(4.20)	(3.75)
Period²	-0.021***	-0.024***	-0.022***	-0.025***	-0.022***	-0.025***	-0.023***	-0.025***
	(6.02)	(6.86)	(6.23)	(7.01)	(6.42)	(7.04)	(6.54)	(7.11)
Tenure		0.203***		0.200***		0.165***		0.163***
		(6.90)		(6.76)		(5.50)		(5.44)
Private	1.920***	1.689***	1.874***	1.656***	1.654***	1.514***	1.621***	1.490***
	(10.13)	(8.91)	(9.86)	(8.71)	(8.50)	(7.78)	(8.28)	(7.61)
SC dummy	0.102	0.1	0.908**	0.743**	0.142	-0.973***	-0.945*	-0.982**
	(0.68)	(0.67)	(2.44)	(2.01)	(0.44)	(3.03)	(1.87)	(1.96)
SC-wage dummy			-0.019**	-0.015*			-0.011	-0.009
			(2.36)	(1.90)			(1.37)	(1.07)
Constant	-1.945***	-1.482***	-2.303***	-1.778***	-1.200***	-0.889***	-1.444***	-1.082***
	(8.69)	(6.51)	(8.45)	(6.40)	(3.84)	(2.83)	(4.00)	(2.98)
Observations	1952	1952	1952	1952	1952	1952	1952	1952
Log likelihood	-3496.63	-3494.83	-3520.48	-3517.69	-3463.45	-3462.88	-3478.6	-3477.66

Absolute value of t statistics in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 4: Mean value of important variables in the experiment in periods 1 and 2

Period-means	***IC***		***SC***		*Wicoxon z IC*	*Wicoxon z SC*	*two tailed t-Test p Per1*	*two tailed t-Test p Per2*
	Per1	***Per2***	***Per1***	***Per2***				
marketvolume	444.400	455.200	510.600	577.400	0.685	0.225	0.401	**0.015**
share principal	0.265	0.340	0.340	0.329	*0.345*	*0.500*	0.245	0.863
wage	33.900	32.838	34.582	42.614	893.000	0.138	0.877	**0.011**
effort	4.465	4.730	4.977	5.938	893.000	0.138	0.506	**0.044**
desired effort	6.767	6.612	6.759	7.686	0.786	0.225	0.991	**0.022**
number of contracts	11.400	11.200	11.800	11.400	0.670	0.157	0.397	0.667
number of offers	15.000	15.400	14.200	13.000	0.891	0.157	0.509	**0.082**
share of private offers	47.49%	50.61%	60.52%	60.56%	0.586	0.893	0.130	0.527
profit principal per contract	10.745	14.459	15.191	16.765	0.345	0.345	0.284	0.564
profit principal	122.000	158.200	181.200	191.800	0.345	0.500	0.232	0.431
profit agent	28.310	26.600	27.759	33.823	0.786	0.225	0.857	**0.017**
average tenure	0 - klar!	0.388	0 - klar!	0.355	*0.043*	*0.056*	*kein Test*	0.805

Table 5: Accepted private and public wage offers in the two treatments

Treatment			Mean	St. Dv.	Obs.
private offers	IC	Per 1	48,793	3,72	5
		Per 2	47,190	2,57	5
		total	44,242	3,94	5
	SC	Per 1	39,360	6,25	5
		Per 2	52,114	5,19	5
		total	50,089	1,48	5
t-Test p: (two tailed)	IC vs. SC: Per 1		0,2310		
	IC vs. SC: Per 2		0,4193		
	IC vs. SC: total		0,2022		

Treatment			Mean	St. Dv.	Obs.
public offers.	IC	Per 1	25,526	3,55	5
		Per 2	23,656	1,53	5
		total	19,038	4,69	5
	SC.	Per 1	29,557	4,89	5
		Per 2	34,054	3,64	4
		total	31,914	4,41	5
t-Test p: (two tailed)	IC vs. SC: Per 1		0,5232		
	IC vs. SC: Per 2		0,0245		
	IC vs. SC: total		0,0299		

Table 6: Accepted private and public offers contingent on tenure or not

	Period-means	***IC Per2***	***SC Per2***	*two tailed t-Test p*
no tenure	wages no tenure	31.105	40.173	0.205
		(11.80)	(15.55)	
	wages tenure	38.958	48.286	0.293
		(17.99)	(6.16)	
	wage public offer	24.988	32.179	0.000
		(4.33)	(10.00)	
	wage private offer	38.750	50.833	0.186
		(14.36)	(16.65)	
tenure	*wage public offer*	19.167	50.000	-
		(8.79)	*one obs.*	
	wage private offer	50.833	47.857	0.565
		(7.59)	(7.02)	

Based on session averages. Standard deviations in parenthesis.

Table 7: Wage changes from period 1 to period 2

Wage changes from period 1 to period 2

	Effort rank within firm in Period t-1			
		min	med	max
IC	**N**	20	15	6
	w(t-1)	27.45	36.93	35.67
	w(t)	27.60	38.33	59.17
	w(t) - w(t-1)	0.15	1.40	23.50
		min	**med**	**max**
SC	**N**	20	17	9
	w(t-1)	34.65	35.18	34.78
	w(t)	43.60	41.88	46.89
	w(t) - w(t-1)	8.95	6.71	12.11

Wage changes from period 1 to period 2 *same employer*

	Effort rank within firm in Period t-1			
		min	med	max
IC	**N**	10	9	2
	w(t-1)	29.4	43.88889	59.5
	w(t)	27.8	48.33333	62.5
	w(t) - w(t-1)	-1.60	4.44	3.00
		min	**med**	**max**
SC	**N**	9	9	5
	w(t-1)	43.66667	43.66667	47.6
	w(t)	48	45.77778	54.4
	w(t) - w(t-1)	4.33	2.11	6.80

Wage changes from period 1 to period 2 *different employers*

	Effort rank within firm in Period t-1			
		min	med	max
IC	**N**	10	6	4
	w(t-1)	25.5	26.5	23.75
	w(t)	27.4	23.33333	57.5
	w(t) - w(t-1)	1.90	-3.17	33.75
		min	**med**	**max**
SC	**N**	11	8	4
	w(t-1)	27.27273	25.625	18.75
	w(t)	40	37.5	37.5
	w(t) - w(t-1)	12.73	11.88	18.75

Appendix A: Instructions for the SC treatment (Originally in German; not for publication – will be provided on the websites of the authors)

The aim of this experiment is the investigation of economic decision behaviour. During the experiment you and the other participants will be asked to make decisions. You will earn money in doing so. The amount of your payoff is determined by your own decision as well as by those made by the other participants in accordance to the rules on the following pages.

2 types of participants
There are two types of participant: type A and type B. You will be randomly assigned to one of these roles. Your role (A or B) is displayed on upper range of your screen throughout the experiment. Your role will stay the same (A or B) throughout the whole experiment.

Earnings
At the beginning of the experiment you will receive an initial endowment of 3 EURO. During the course of the experiment you can earn a further amount of money by gaining points. The amount of points that you gain during the experiment depends on your decisions and the decisions of other participants. All points that you gain during the course of the experiment will be exchanged into Swiss Francs at the end of the experiment. The exchange rate will be:

1 point = 0.03 EURO (3 Eurocent)
therefore 1 Euro = 33,33 points

At the end of the experiment you will be paid the money that you earned during the experiment in addition to your endowment of 3 EURO, privately and in cash.

Duration
The experiment will take approx. 150 minutes. It is divided into 15 periods. In each period you have to make decisions, which you will enter on a computer screen.

Documentation
The last page of these instructions contains a documentation sheet. At the beginning of the experiment you have to fill in your identification number displayed on the screen. In each round you will fill in certain information (see below) into the corresponding rows. Please hand in this sheet at the end of the experiment.

Anonymity
The identities of the participants that have interacted with you will at no point be revealed to you. Neither will the other participants, neither during nor after the experiment, learn which role you were assigned or how much you have earned.

Please note that communication between participants is strictly prohibited during the experiment. In addition we would like to point out that you may only use the computer functions which are required for the experiment. Communication between participants and unnecessary interference with computers will lead to immediate exclusion from the experiment.

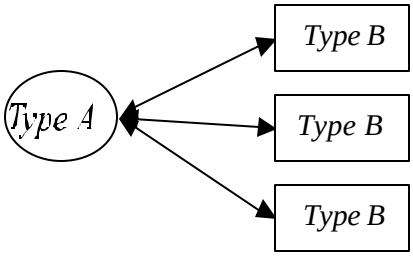
An Overview of the Experimental Procedures
In each period of the experiment every Type A participant can conclude trades with three participants of type B. Type B earns a profit if he receives a transfer which exceeds his costs. Type A earns a profit if the transfer he sends is less than what the factor is worth to him. The costs that type B has to bear and the earnings of type A are determined by the factor chosen by type B.

The experiment lasts for 15 periods. In each period the procedures are as follows:

1. Each period starts with a trading phase which lasts 3 minutes. During this phase participants of type A can submit offers, which can be accepted by participants of type B. When submitting an offer a buyer has to specify three things:
    - which transfer he offers to send,
    - which factor he *desires*,
    - and finally, which type B he wants to submit the offer to.

Type A participants can submit two types of offers; private offers and public offers. *Private offers* are submitted to one type B only and can only be accepted by that particular participant. *Public offers* are submitted to all type B participants and can be accepted by any type B.

Type A participants can - in each period - submit as many offers as they like. Submitted offers can be accepted at any time during the trading phase. Each type B participant can at most conclude one trade in each period. Each type A participant can at most conclude three trades per period. As there are 16 type B and 4 type A participants, several type Bs will not trade in each period.



2. Following the trading phase each type B who has concluded a trade determines which factor he will send. *Type B is not obliged to send the factor desired by the type A*. Once every participant of type B has chosen a factor each participant's earnings in the current period are determined. After this the next period starts.

   The points gained during all 15 periods will be summed up at the end of the experiment, exchanged into EURO and paid together with your endowment in cash.

As a reference to the real world one can think of the experiment as a labor market : Type A is the employer offering job contracts, containing the wage (transfer) and the desired effort i.e. working hours etc. (factor). Each employer can hire 3 employees (see figure 1). After being employed (having accepted the offer) the workers decide whether to accomplish the desired effort or not.

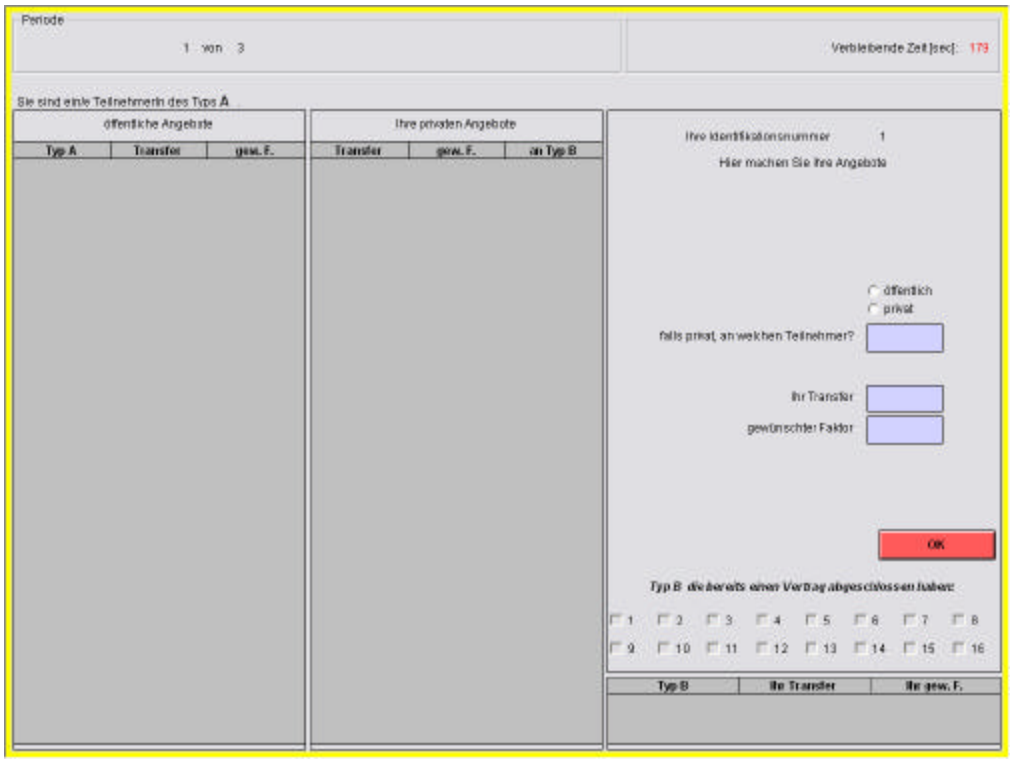
The Experimental Procedure in Detail

There are 4 type A and 16 type B participants in the experiment. **Your role will stay the same (A or B) throughout the whole experiment.** In the following we describe in detail how you can make your decisions in each period.

The trading phase

TYPE A

Each period starts with a trading phase. During the trading phase each Type A can conclude a trade with up to three type B participants. In order to do so each type A can submit as many offers as he wishes.
In each trading phase participants of Type A see the following screen:



In the top left corner of the screen the current period of the experiment is displayed. In the top right corner of the screen the time remaining in this trading phase is displayed in seconds. Each period lasts 3 minutes (= 180 seconds). When this time is up the trading phase is over. Hereafter, no further offers can be submitted or accepted in this period.

Once the above screen is displayed the trading phase has started. Type A participants have the possibility to submit offers to the sellers. In order to do so you have to enter *three specifications* on the right hand side of the screen:

a) First the participants of type A have to specify whether to submit a public or a private offer:

- **Public offers**
  Public offers will be communicated to all participants in the market. All type B participants see all public offers on their screens. A public offer can therefore be accepted by any type B participant. The type A participants will also see all public offers submitted by other type A participants.
  To submit a public offer, click on the field "public", using the mouse cursor.
- **Private offers**
  A private offer is submitted to one particular type B participant only. Only this type B is informed about the offer and only this participant can accept the offer. No other type B or type A participant will be informed about that offer.
  To submit a private offer, click on the field “private” using the mouse cursor. The type B participant, the offer shall be submitted to, has to be specified in the field below. Each of the 16 type Bs has an identification number (type B 1, type B 2, ..., type B 16). Each type B keeps his identification number throughout the whole experiment. To submit an offer to a specific type B enter the number of that participant (e.g., "4" for type B 4).

b) After having specified to whom to submit an offer, the type A participants must determine which transfer is offered.
This is entered in the field "Your transfer". The transfer offered is a number between 0 and 100:

0 = transfer offered = 100

c) Finally type A participants have to specify which factor they desire. This is entered in the field "Desired factor".
The desired factor is a number between 1 and 10:

1 = desired factor = 10

After having completely specified the offer, it is submitted by clicking the "OK" button. As long as the "OK" button has not been clicked the offer can be changed. When confirmed by the "OK" button the offer will be displayed to all type B participants it was submitted to.

The header "public offers" is located on the top of the left side column on the screen. All public offers in the current trading phase are displayed here. Your public offers as well as those of all other participants of type A will be displayed. It is displayed which participant submitted the offer, which transfer was offered and which factor is desired. All type A participants also have an identification number, which they keep throughout the whole experiment.

In the middle of the screen below the header "Your private offers" all *your* private offers submitted in the current trading phase are listed. It is displayed which transfer was offered, which factor is desired and which participant each offer was submitted to.

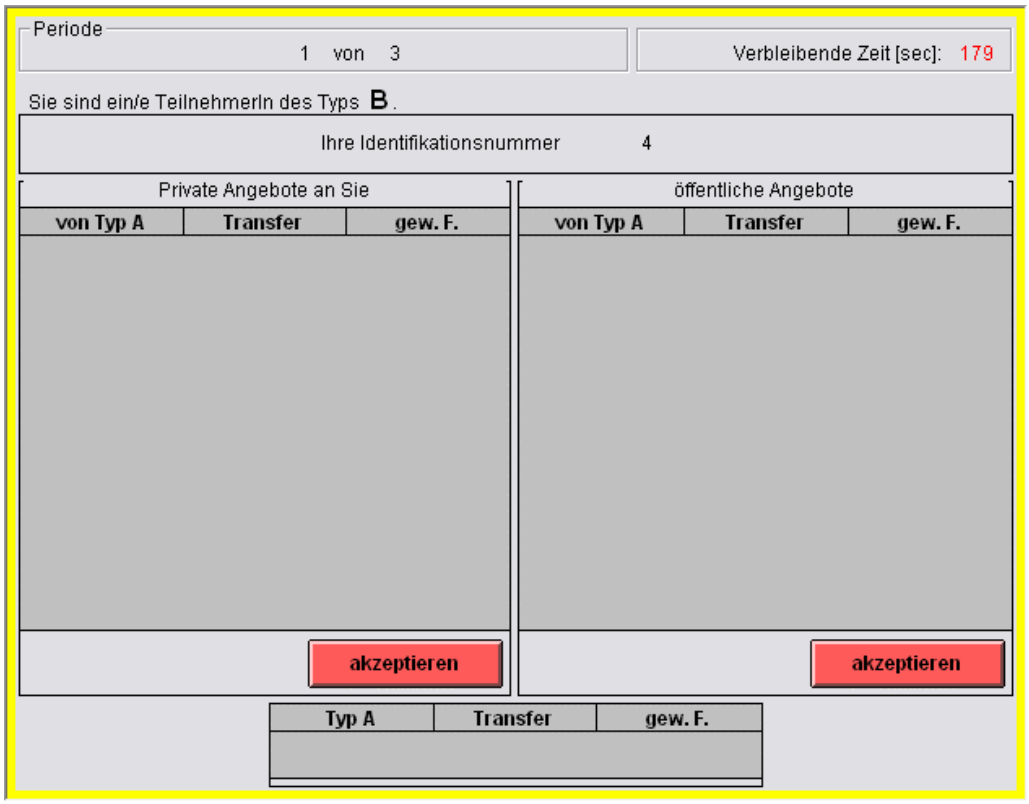
Each Type A can submit as many offers as she wishes public or private, no matter if they are of identical or differing content. Each offer can be accepted at any time during the trading phase.

In any given period each participant of type A can conclude at most three trades. Once an offer has been accepted, type A will be notified which type B accepted which of his offers. In the bottom right corner of the screen the identification number of the type B participant will be displayed as well as the offered transfer and desired factor. As type A participants can conclude only three trades in each period all other offers will be automatically cancelled after the third offer has been accepted, and this type A will not be able to submit any further offers during this period.
In any given period each participant of type B can conclude at most one trade. The type A participants will be continuously informed which type B have not yet accepted an offer. On the right bottom of the screen there are 16 fields, each field for one of the 16 type B participants. Once a type B has accepted an offer an “x” will appear in the field next to the participant’s identification number. It is not possible to submit private offers to a participant of type B who has already concluded a trade.

TYPE B

In each trading phase participants of Type B see the following screen:

In the top left corner of the screen the current period of the experiment is displayed. In the top right corner of the screen the time remaining in this trading phase is displayed in seconds. Each period lasts 3 minutes (= 180 seconds). When this time is up the trading phase is over. Hereafter, no further offers can be submitted or accepted in this period.



Once the above screen is displayed the trading phase has started. Participants of type B can now accept offers submitted by the type A participants. There are two types of offers which you can accept:

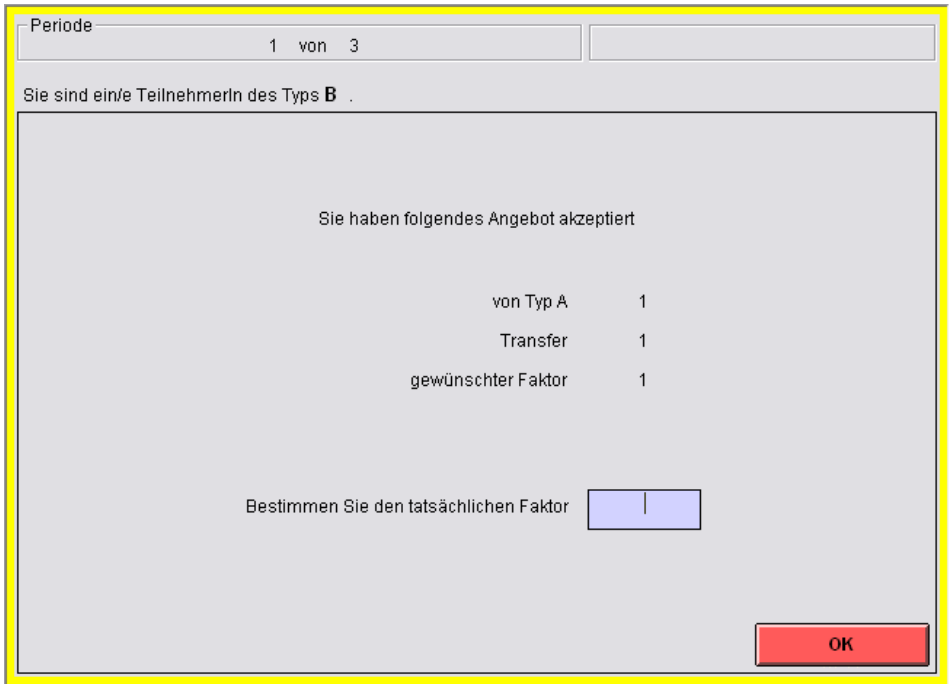
- **Private offers**
  Only the Type B the offer was addressed to will be able to see and accept a private offer. No other type A or B is informed about these offers. If *you* receive private offers in the Role of type B, they will appear on the left side of your screen, below the header "Private offers to you". The offer contains the following information: the identification number of the type A who submitted the offer, the transfer which he offers and which factor he desires. To accept a private offer, a click on the respective row in which the offer is displayed marks the offer and it will be highlighted. The offer is accepted by a click on the button "accept" at the bottom of the screen. As long as the "accept" button has not been clicked the choice can be altered.

- **Public offers.**
  All type B participants are informed about these offers and can accept them. Public offers appear on the right side of the screen (pictured in figure 3), below the header "Public offers". The offer again contains the identification number of the type A who submitted the offer, the transfer which he offers and which factor he desires. This information is also displayed to all other participants of type A and type B. To accept a public offer the same procedure as with private offers is applied: A click on the respective row in which the offer is displayed marks the offer and it will be highlighted. The offer is accepted by a click on the button "accept" at the bottom of the screen. As long as the "accept" button has not been clicked the choice can be altered.

As soon as the "accept" button is pressed the accepted offer is displayed in the bottom row of the screen.

- Once all 4 participants of type A have concluded three trades or after 3 minutes have elapsed, the trading phase is over.
- **No buyer is obliged to submit offers, and no seller is obliged to accept an offer.**

Determination of the Actual Factor
Following the trading phase, all participants of type B who have concluded a trade determine which factor they send to "their" type A participants. The factor desired by the type A *is not binding!* The type Bs can choose exactly the factor desired the type A, but also a higher or lower factor. The factor is entered in the following screen:



In order to determine the actual factor, the value for the factor is entered in the field "Determine the actual factor". By pressing the "OK" button the choice is confirmed As long as the "OK" button has not been clicked the choice can be altered.
The desired factor is a number between 1 and 10:

1 = desired factor = 10

While type B enters he factor, type A specifies on a separate screen, which factor he expects to receive, and how certain he is about these expectations.

How are incomes calculated?
Income Type A:

- If no trade was concluded, participants of type A receive an income of 0 points in that period.
- If at least one of type A's offers was accepted, the income depends on the transfer sent and on the *factor chosen by type B*. The income is calculated as follows:

Income type A= 10*(factor1 + factor2 + factor3)
– (transfer1 + transfer2 + transfer3)

The principal receives the sum of all transfers (at most three) which are multiplied by the factor 10. He has to pay the sum of all transfers sent.

As can be seen see from the above formula the income of type A is higher, the higher the factor chosen by the participants of type B. At the same time the income is higher, the lower the transfer(s) sent.

Income Type B:

- If no trade was concluded, participants of type B receive an income of 5 points in that period.
- If type B accepted an offer, the income depends on the transfer sent and on the ***costs*** *of the factor chosen by type B*. The income is calculated as follows:

Income type B = transfer – costs of the factor

The costs are higher, the higher the factor chosen. The costs for each factor possible are displayed in the table below:

factor	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**
costs of the factor	0	1	2	4	6	8	10	12	15	18

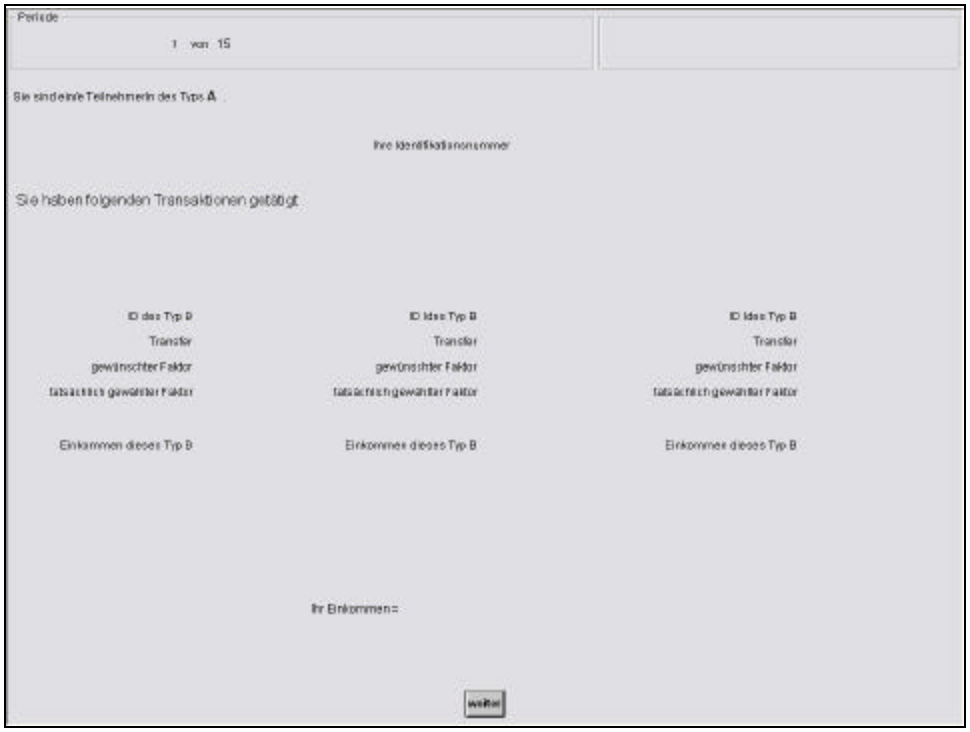
The income of type B is therefore higher, the lower the factor chosen and the higher the transfer(s) received.

The income of all Type A and type B participants are determined in the same way. Each participant can therefore calculate his own income and the income of the players connected with him in that period. Furthermore, each participant is informed about the identification number of his trading partner in each period.

Please note that you can incur losses in each period. These losses have to be paid from the initial endowment or from earnings made in other periods.

Information
You will be informed about your income and the income of the participants connected to you on an "income screen". On the screen (see below) the following will be displayed:

- The ID(s) of the participants you concluded a trade with,
- the offered transfer,
- the desired factor,
- the actual factor chosen,
- the income of each participant involved and
- your income in this period.



Please copy all the information into the documentation sheet at the end of this text!

After the income screen has been displayed, the period is over. Thereafter the trading phase of the next period starts. Once you have finished studying the income screen please click on the "continue" button.

The experiment will not start until all participants are perfectly acquainted with all procedures. In order to ensure that this is the case we kindly ask you to solve the exercises on the following pages.

Additionally *2 trials of the trading phase* will be conducted, so that you can get accustomed to the computer. During the trial phases no money can be earned. After the trial phases the experiment is started, which will last for 15 periods.

Control Questionnaire
Please solve the following exercises completely. If you have questions ask the experimenter. Wrong answers will not have any effect on the experiment or your payoffs.

Exercise 1
Type A did not make an offer during the trading phase. What is his income in this period?
Income type *A* =

Exercise 2
Type B did not accept an offer during the trading phase. What is his income in this period?
Income type *B* =

Exercise 3:
An offer containing a transfer 30 and a desired factor of 9 is accepted. *Type B* chooses a factor of 9.
Income type *A*=
Income type *B* =

Exercise 4:
An offer containing a transfer 60 and a desired factor of 9 is accepted. *Type B* chooses a factor of 6.
Income type *A*=
Income type *B* =

Exercise 5:
An offer containing a transfer 10 a desired factor of 2 is accepted. *Type B* chooses a factor of 5
Income type *A*=
Income type *B* =

Exercise 6:
An offer containing a transfer 40 and a desired factor of 4 is accepted. *Type B* chooses a factor of 5.
Income type *A*=
Income type *B* =

Exercise 7:
During the trading phase *Type A* made several offers. None of theses offers was accepted. What is his income in this period?
Income type *A*=

Exercise 8:
The following offers were accepted:
Trade 1: Transfer 40 desired factor 7; chosen factor 5.
Trade 2: Transfer40 desired factor 7; chosen factor 8.
Trade 3: Transfer 40 desired factor 7; chosen factor 4.
Income type $B_{(Handel1)}$ =
Income type $B_{(Handel2)}$ =
Income type $B_{(Handel3)}$ =
Income type *A* =

When you have finished the exercises we recommend you take a second look at the exercises and the solutions provided.