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Exploring the Effects of Unequal and Secretive Pay*

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Abstract

We experimentally test whether intentional and observable discriminatory pay of symmetric agents in the Winter (2004) game causes low paid agents to reduce effort. We control for intentionality of wages by either allowing a principal to determine wages or by implementing a random process. Our main observations are that discrimination has no negative effect on efforts and principals do not shy away from using discriminatory pay if it is observable. Rather, with experience discrimination enhances efficiency as it facilitates coordination among agents. The only evidence for reciprocity is that subjects receiving a low payment from a principal (discriminatory or not) exert significantly less effort.

Keywords: wage discrimination, experimental study, envy, reciprocity, pay secrecy

JEL Classification: C72, C91, D21, J31

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1 Introduction

According to the fair wage-effort hypothesis by Akerlof and Yellen (1990) and also to common perception, being offered a lower renumeration for the same task than an other identically qualified worker may cause agents to withhold effort. However, ample evidence for unequal payment of equally qualified workers exists. A prominent case is that of Lily Ledbetter, who discovered when nearing retirement that throughout her career her male colleagues were earning much more.[1] Discriminatory pay, i.e. a differential in payments which can not be justified by aspects relevant for productivity, is well documented[2] and banned by law today in many countries - at least if it is based on aspects like sex, age or race.

For example, in a survey of some German banks Kampkötter and Sliwka (2009) find that externally hired employees earn up to 20% more than their internally promoted colleagues. The authors argue that this is due to risk preferences on behalf of the employees.

In this study we test in a controlled laboratory experiment whether and how unequal remuneration of symmetric workers affects efforts. We argue that especially intentional negative wage discrimination results in a withdrawal of effort, and that employers correctly anticipate this and, thus, shy away from discrimination if it is observable.

Our working horse is the principal agent game by Winter (2004) with one principal, two agents, and an interdependent productivity function with increasing returns to scale. The treatments varied along two dimensions: intentionality of the wage offer and information about the other agent's wage. We control for intentionality by either asking the principal to choose both wages or by determining them randomly.

We observed that principals neither shied away from discrimination nor that agents reacted negatively to it. Quite contrarily, over time low paid workers learned to exert effort when being offered discriminatory wages. Yet, low wages triggered low effort rates in all treatments but the lowest ones if wages were determined by the principal and not by a random process.

Already Solow (1979) argues that working efforts do not only depend

[1]see article in the New York Times on Jan. 30, 2009: "Obama Signs Equal-Pay Legislation" by S. Stolberg. The Supreme court had previously overruled her compensation claims for mere technical reasons. Now the Lilly Ledbetter Fair Pay Act extends employee's rights in case of pay discrimination by extending the possibilities to file discrimination suit. Previous legislation allowed only for filing a discrimination suit within 180 days of the date that the employer first paid her less than her peers. The new legislation restarts the six-month clock every time the worker receives a paycheck. The case gained prominence via the attention that President Obama devoted to that case during his campaign. In fact the Lilly Ledbetter Fair Pay Act was the frist bill that the new president signed into law on January 29, 2009.

[2]See Olivetti and Petrongolo (2008) and Oostendrop (2009) for recent cross country studies on gender wage gaps.

on monetary incentives but also on working morale. In a more elaborate model, relating to Akerlof (1982) and based on psychological and sociological concepts, Akerlof and Yellen (1990) argue that workers withdraw effort if their actual wage falls short of their "fair" wage. Here, the perception of what constitutes a fair wage may relate to the wages colleagues receive. As shown in many experimental and empirical studies, fairness consideration - or more generally other regarding preferences - influence individual behavior considerably (for a survey on other regarding preferences see Cooper and Kagel, 2009). In labor relations the most illustrative example is the efficiency wage model which is not only supported by laboratory experiments (see, e.g. Dohmen et al. (2009)) but also by some (quasi) field experiments.[3]

Various papers explore the effects of wage discrimination in the laboratory. Charness and Kuhn (2007) run gift exchange experiments in which a principal can offer wages to his two agents individually. In one treatment workers can observe the wage of the other employee, in the other they can't. While efforts do not react significantly to observability of unequal pay there is evidence that employers shy away from paying different wages if these are observable. However, in Charness and Kuhn (2007) workers are asymmetric in their productivity, which may justify different wages and makes it unlikely that other regarding preferences or norms fully apply. Contrary to this, we analyze how information about co-worker's pay affects individual effort choices in situations in which pay is unequal despite symmetry among workers.

In his theoretical paper Winter demonstrates that under certain conditions, unequal pay of identical, simultaneously-moving agents may be the optimal wage mechanism for the principal. More specifically, the discriminatory wage mechanism is the cheapest one guaranteeing full effort by all simultaneously moving agents in equilibrium in a situation in which this is efficient.

To understand the intuition behind this equilibrium, note that in the principal agent game Winter analyzes, not only the expected project return itself but also expected individual rewards, (paid only in case of the project's success) exhibit increasing marginal returns to scale in the number of effort investing agents.[4] Here the cheapest full effort inducing mechanism is to offer a fully discriminatory reward scheme with the following characteristics: One worker is promised a high reward which is high enough to render full contribution a dominant strategy irrespective of whether other agents contribute or not. Since the wage mechanism is public knowledge the remaining agents can infer that the first will contribute. It is then sufficient to offer a second agent enough to render full effort dominant given at least one other

[3]See, Fehr et al. (2009) for a review.

[4]More specifically, workers are offered a reward which is only paid in case the project is successful. The probability that the project is a success in turn is a convex increasing function in the number of contributing workers.

agent contributes and so on. Given the increasing returns to scale nature of rewards in this game, every $(k+1)$-th agent is offered less than the k-th one for all $k \in 1, ..., n-1$. Still, note that this wage mechanism is only one of many equilibria in the entire game.

While several experimental studies have analyzed effects of (knowledge about) unequal pay on individual efforts, to our knowledge non has considered this in a case where workers (or agents) are symmetric and where reciprocity towards a principal, embodied by an other experimental subject, is possible.

Niklisch (2008) also use a gift-exchange framework but with a real effort task. In the online computer game world of warcraft, participants were invited to complete a task against virtual payment. Wages were offered based on experience and discrimination showed no effect. In Güth et al. (2008) a principal has the possibility to offer separate wages to permanent and short term employees. When wages are private knowledge, principals discriminate. When wages are public, less discrimination is observed.

The mechanism suggested by Winter (2004) requires full information on all sides. In a real-effort field experiment Hennig-Schmidt et al. (2010) find evidence for this information requirement. While they find no treatment effect in the real effort task, they do observe a response to increased wage offers in a laboratory experiment, however only if the payoff structure (including the payoffs to the principal) is commonly known.

In a different line of literature Goerg et al. (2008) and Klor et al. (2009) investigate the effects of unequal pay on team production. They base their experimental design on Winter (2004). However, both studies omit the principal. We are particularly interested in the role of the principal and the decision on and response to discrimination. We test for possible effects of reciprocity. How the knowledge on the options available to the principal and the wage offer to the co-worker affect effort decision. And how the effects of interdependence on the return to the other agent affect the decision.

The remainder of the paper is organized as follows. The next section introduces the model, section 3 outlines the experimental design, results are presented in section 4, before section 5 concludes.

2 Model

We consider a simplified version of Winter (2004) with one principal and only two agents. The principal has a project that returns profit π if it is successful and zero otherwise.[5] Success only occurs with certainty if both agents exert effort which is binary, i.e. $e = \mathbf{1}$ or $e = \mathbf{0}$ and induces effort costs of ec with $c > 0$. If only one agent exerts effort the probability of

[5] In the experiment we replace stochastic outcomes by expected values to avoid complications due to risk preferences.

success reduces to $\alpha \in [0, 1]$ and if both are lazy it reduces further to α^2. In other words, the project has a convex production technology, so that – in a comparative statics view – the increase in expected return of an additional agent exerting effort exceeds that of his predecessor. The effort choice itself is made simultaneously. More specifically, the sequential game proceeds as follows:

First the principal offers wage w_i to each agent $i = 1, 2$. Both agents learn each other's wage[6] before they decide independently their effort level $e_i \in \{0, 1\}$. Wages are only paid if the project is successful. Effort choices are not observable in the sense that the principal can not condition his wage on effort, but it influences the likelihood of success.

We define by k the number of agents choosing $e_i = 1$, by $p = \alpha^{2-k}$ the probability of success, where $\alpha \in [0, 1]$. Thus, the expected payoffs of the principal (V_P) and of each agent i (V_{Ai}) are:

$$V_P = \alpha^{2-k}(\pi - w_1 - w_2) \tag{1}$$
$$V_{Ai} = \alpha^{2-k} w_i - e_i c \ , \tag{2}$$

where $\pi > 2c > 0$ and $\alpha \in [0, 1]$. Furthermore, we restrict c to

$$\pi \geq \frac{c\,(1+\alpha)}{\alpha\,(1-\alpha)}, \tag{3}$$

the reason for which we will justify later. Thus, contingent on their wage offer and assuming risk neutrality the agents face the following game:

	1	**0**
1	$w_2 - c$ $\pi - w_1 - w_2$ $w_1 - c$	αw_2 $\alpha(\pi - w_1 - w_2)$ $\alpha w_1 - c$
0	$\alpha w_2 - c$ $\alpha(\pi - w_1 - w_2)$ αw_1	$\alpha^2 w_1$ $\alpha^2(\pi - w_1 - w_2)$ $\alpha^2 w_1$

In each cell, the lower left payoff refers to the row player (agent 1), the middle one to the principal, and the upper right belongs to the collum player (agent 2). Note that the principal moves prior to the agents by selecting a wage scheme.

Analysis of Equilibrium Behavior

Assuming risk neutral agents and common knowledge of rationality, this game has several possible equilibria. We first look at the agents' individual

[6] In one treatment the offer to the other agent will not be observable.

best response functions. It is easy to see that for a very small wage of $w_0 < c/(1-\alpha)$ no effort **0** is the dominant strategy. For an intermediate low wage w of $c/(1-\alpha) \leq w < c/\alpha(1-\alpha)$ it is a best response to exert effort **1** if the other agent also chooses **1** and play **0** if the other is lazy, too. Finally, if w increases even further to $w \geq c/\alpha(1-\alpha)$ effort **1** becomes dominant irrespective of the choice of the other agent. We, thus, define the following threshold wages

$$w_l = \frac{c}{(1-\alpha)} \tag{4}$$

$$w_h = \frac{c}{\alpha(1-\alpha)} \tag{5}$$

Let us look at a truncation of the game by eliminating all wage offers other than w_l and w_h from the action space of the principal. For now we only look at the sub-game between the agents. If an agent receives w_h, an effort of **1** constitutes a weakly dominant strategy if he expects the other to be lazy and a strictly dominant strategy if he expects him to exert effort. Thus, if both receive w_h, in equilibrium both exert effort $e = 1$.

If both only receive w_l two equilibria are possible. Either both exert full effort or both exert no effort. A real mixed equilibrium only exists for intermediate wages w_m with $w_l < w_m < w_h$.[7]

If agents are discriminated, effort by both constitute the only equilibrium. Given the dominance of effort for the high paid agent, the low paid agent can confidently belief that the first agent will exert full effort. The second agent then best responds by playing **1** as well, despite receiving w_l only.

Next, we turn to the principal's choice, here every wage combination can constitute an equilibrium offer. The restriction in equation (2) points at her participation constraint. For any $\pi > \frac{2c}{(1-\alpha)}$ it is beneficial for the principal to offer w_l if this results in effort by all agents. As Winter, we are, however, primarily interested in the asymmetric wage offer. This is reflected in condition 3, the participation constraint for the principal in a discriminating equilibrium.

As already Winter (2004) pointed out this payment schedule is the cheapest full effort inducing mechanism available to the principal. This asymmetric equilibrium, advising the principal to discriminate between agents is at the focus of our investigation. We explore the (possibly emotional) response of agents to intentional discrimination. Do they reciprocate discrimination by being less likely to exert effort or are they driven by efficiency considerations? Do principals shy away from discrimination when it is observable? Or would they take advantage of the asymmetric payment schedule to facilitate coordination? Does sensitivity for justice play a role in choices?

[7] In a mixed strategy equilibrium **1** is chosen with probability $q = \frac{c}{w(1-\alpha)^2} - \frac{\alpha}{(1-\alpha)}$. Note, that for a wage offer of $w = w_l \rightarrow q = 1$, and for $w = w_h \rightarrow q = 0$.

3 Experiment

In the experiment we implemented the simplified version of the Winter (2004) model discussed above. In addition we replaced expected by deterministic payoffs to avoid complications due to risk preferences and used the following parametrization:

$$\alpha = \frac{1}{2}, \quad c = 9, \quad w_L = 21, \quad w_H = 42, \quad \pi = 100$$

Note, that the numbers were chosen to make wage offers sufficiently high to avoid indifference, i.e. $w_H > \frac{c}{\alpha(1-\alpha)}$ and $w_L > \frac{c}{(1-\alpha)}$. As a result, weakly dominant relations in the model are replaced by strict dominance.

A detailed payoff table is attached in the instructions in the appendix. The parametrization was chosen to induce principals to offer a discriminatory wage. More specifically, we set α, π, w_L and w_H such that an offer of (w_L, w_L) induces a very unfair distribution of final payoffs irrespective of effort choices. On the other hand, an offer of (w_H, w_H) leaves very little for the principal herself. This indeed let to a sufficient number of (w_H, w_L) or (w_L, w_H) offers (i.e. 52% in the FIP and 46% in the PIP treatment).[8]

3.1 Design & Treatments

Experiments were carried out at the Max Planck Experimental Lab in Jena with university students in July 2009. A total of 256 students participated in eight sessions and one pilot session. The experiment was programmed using ztree (Fischbacher, 2007) and invitation of participants was managed using ORSEE (Greiner, 2004) which guaranteed that no subject participated in more than one session. A translation of the German instructions is attached in the appendix. Participants were given time to read the instructions in private before instructions were read aloud to induce common knowledge.

After reading the instructions and before the actual experiment, participants could simulate the experiment playing all three roles for five minutes.[9] Only then roles were assigned and remained fixed throughout the experiment. Participants interacted over 15 rounds in a random stranger design which guaranteed that no one interacted with someone they interacted with in the previous round, what was common knowledge. Only one randomly selected round was paid at the end. Thus, subjects could not hedge over rounds and discrimination - if it occurred - was salient.

With one exception in which only 27 invited subjects showed up, 30 participants interacted in every session. In two treatments we had one additional participant acting as a helper. Her/his task was to carry out random

[8] Treatments will be described in detail below.

[9] For this a special program was used, relying on the same screens.

draws. The role of the helper and the computers in the laboratory were assigned by lot at the entrance.

Our research question is whether, and to what extend pay discrimination of symmetric agents affects effort choices. In particular, we wanted to test the impact of intentions and the impact of information about discrimination. We employed a 2x2 between subjects factorial design to answer this question. More specifically, the structure of the experiment is as follows. All subjects receive the same instructions detailing the game, parameters and payoff functions, illustrated by payoff tables. Since we wanted to test for negative reciprocal reactions, the instructions are written in a labor market frame. In our baseline treatment called "full info principal" (FIP), each round of the experiment proceeded as follows:

1. The principal chooses a wage combination from $(w_1, w_2) \in \{w_L, w_H\}^{\times 2}$.
2. Both agents are informed about (w_1, w_2) and independently make an effort choice $e_i \in \{0, 1\}$
3. Principals are asked for their expectation on the effort decision of both agents. Agents are asked for their expectation on the other agent's effort choice.
4. Payoffs are calculated and subjects are informed about all decisions and final payoffs.

The beliefs of participants on other players' choices were elicited by asking: Please indicate on a scale from 0 to 100 how sure you are that the other worker will exert effort? The elicited beliefs were not incentivized as this would have complicated the experiment and to avoid complications due to the known interaction of scoring rules with risk preferences (see Offerman et al. (2009) for a discussion).

The design was varied along two dimensions. In one dimension we varied information, while in the other we manipulated intentionality of wage offers. Under full information **(FI)** each agent receives information on both his own and the other agent's wage, while under partial information **(PI)** he is only informed about his own wage.[10] The second dimension is intentionality of the wage offer. Here we varied whether the principal **(P)** or a random mechanism **(R)** determined the wage combination. Thus, we had four treatments which we will refer to as: full info principal (FIP), full info random (FIR), partial info principal (PIP), and partial info random (PIR). All treatments were compared between subjects, i.e. every subject only confronted one of the four treatments.

[10] During the game an agent only knew his own wage and at the end of the round he was only informed about whether the other agent has exerted effort and his own payoff.

	principal	random
full info	FIP	FIR
partial info	PIP	PIR

In treatments FIR and PIR the decision of the principal was replaced by a random move. Here each wage combination (differentiating (w_L, w_H) and (w_H, w_L)) occured with likelihoods r_{LL}, r_{LH}, r_{HL} and r_{HH} (with $\sum_{i,j\in\{L,H\}} r_{ij} = 1$) equal to the observed frequencies of these offers in treatment FIP and PIP, respectively. In the PIR treatment, workers were only informed about their own wage offer.

The random draw itself was carried out by the helper subject using an urn with 100 numbered chips. The helper only received instructions about the random draws and did not know the content of the experiment.[11] To obtain sufficient variation for the random move we combined a randomization by computer with a proper randomization by the helper. More specifically, at the beginning of each round participants first saw a table matching each chip numbered from 1 to 100 to a wage combination. The number of lots assigned to a wage combination was equivalent to it's commonly known likelihood. Every matching table was randomly and independently created for every group and round by the computer software, what subjects knew. The helper subject publicly drew a numbered chip from a bowl with 100, announced the number and entered it into the computer. This number then determined for each group individually the wage combination.

After all 15 repetitions the round relevant for payment was determined by a random draw in front of the subjects. The experiment was concluded by a questionnaire, where justice sensitivity of the subjects of elicited. In particular we used the Schmitt et al. (2005) scale to elicit sensitivity for victim, observer and beneficiaries.

4 Results

We ran a total of 8 session in the computer laboratory of the *Max Planck Institute* in Jena, two for each of the four treatment combinations. Subjects were students recruited from the *Friedrich Schiller Universität Jena* and no one participated in more than one session. With one exception, in every session 30 subjects interacted with each other over 15 repetitions in a random stranger rematching which guaranteed that no one interacted with someone in two consecutive rounds. This matching procedure was common knowledge. Unfortunately in one session for treatment combination *PIR* only 27 subjects participated in the experiment itself. Each session lasted between about 55 and 70 Minutes including admission and payment of all

[11]He (or she) had to leave the room while the instructions were read aloud to the participants.

subjects. On average subjects earned in total[12] €9.91 with minimum €4.50, maximum €23.40 and standard deviation 4.67.

4.1 Wage Offers

We first look at the wage combinations principals offered to the agents in treatments *FIP* and *PIP*. The barplots in Figure 1 show the relative frequency of each wage combination separately for every period and treatments FIP and PIP. In addition the frequency over the entire experiment, i.e. periods 1 to 15, are shown in the last bar to the right. In both treatments the most common wage combination was the $(21, 21)$ offer, followed by the two discriminating offers. The combination which paid both the high wage was chosen rather rarely. Averaged over all repetitions, there is no significant[13] treatment effect.[14] In treatment FIP the frequencies for combinations $(21, 21)$, $(21, 42)$, $(42, 21)$, and $(42, 42)$ are 45.67%, 26.33%, 26% , and 2% , and in treatment PIR the numbers are 50.33%, 24.33%, 22.33% , and 3% , respectively.

What, however, is striking is a repetition effect in treatment FIP. With experience principals tend to chose a discriminating offer more often. This is especially the case in the last six periods what results in significant differences in offers between FIP and PIP.[15]

Observation 1 *Observability of discrimination does not reduce the frequency of discriminatory offers. On the contrary, in later rounds proposers tend to discriminate more when the wage of the other worker is observable.*

4.2 Effort Choices - Descriptive Analysis

We now turn to our main variable of interest, the effort choices made by the agents. We essentially employed a $2 \times 2 \times 2 \times 2$ design with the following four dimensions of which the first two were determined endogenously:

1. Size of payment: low vs. high.

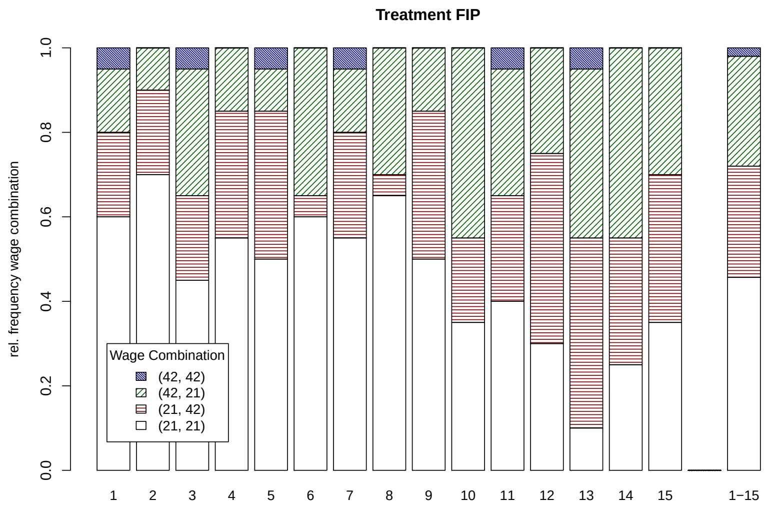
2. Discrimination: being paid the same or a different wage.

[12]This includes the €4.00 subjects received for the post-questionnaire.

[13]If not mentioned otherwise, significance levels are set to threshold $\alpha = 2.5\%$ throughout the following analysis and p-values above that level will not be reported.

[14]Both, a Pearson's Chi-squared test and a Fisher's exact test can not reject the null of equal frequency distributions between treatments.

[15]Looking only at the last three or five rounds, using a Fisher's exact test, there is a significant ($p = .0135\%$ and $p = .0110\%$, respectively) difference between treatments which is not the case if one looks at all previous rounds combined either over multiples of three or five rounds. Even more specifically, one sided Fisher tests comparing only the frequency of equal offers versus discriminatory ones in the last three or five rounds find significantly more discrimination in treatment FIR ($p = .0023$ and $p = .0012$, respectively).



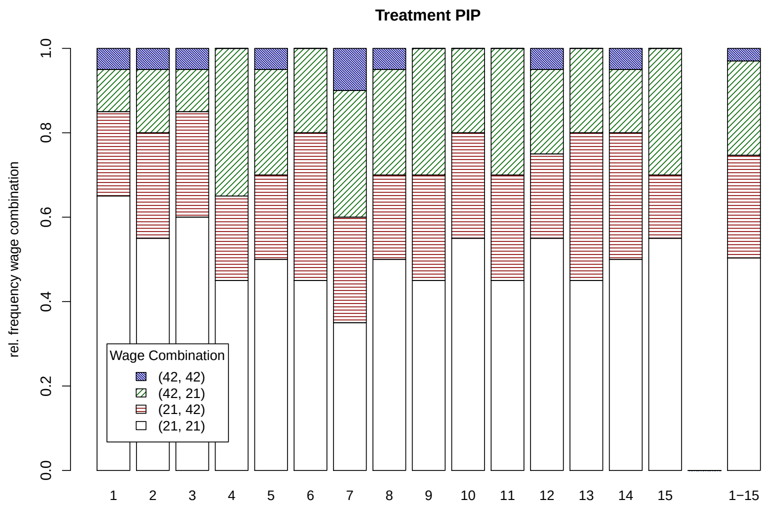


Figure 1: Frequency of wage combinations in *FIP* and *PIP*

3. Observability of discrimination.

4. Intentionality of payment: Wage is determined either by the principal or a random process.

To understand behavior better, we will first analyze effort decisions separately along these dimensions before running a more detailed regression analysis including all dimensions simultaneously in the subsequent section.

Figure 2 shows the relative frequencies of effort by wage combination, role, and treatment. The heights of the bars represent the relative frequency of effort and the numbers at the bottom indicate how often this treatment/wage combination occurred. For example, in treatment PIP, agents $A2$ confronted $(21, 42)$ a total of 73 times and exerted effort in 85% of those cases. Note, however, that in PIP and PIR an agent did not know the wage of the other. Thus, in the example an $A2$ could not distinguish between $(21, 42)$ or $(42, 42)$. This is reflected in very similar frequencies of effort for cases which are indistinguishable for the agents. Fisher exact tests confirm this, as the null hypothesis of equal frequencies can not be rejected. Also, effort choices are invariant with respect to names, more specifically, $A1$ and $A2$ subjects who face the same situation (from their individual perspective) behave identically.[16] Thus, we will treat the two agent roles interchangeably.

Effects of the Wage Size

We first look at whether and to what extend agents react to the wage size. This is important as it shows whether subjects understood the fundamentally different incentives rendered by low vs. high payment. A strong effect is obvious in treatment PIP, what is confirmed by Fisher exact tests for all repetitions but round 2 and 5. In treatment PIR this effect is much less pronounced, but still significant.[17] In the full information treatments this comparison becomes a bit more difficult as here the reaction to getting a small vs. high wage potentially interacts with information effects. For now we ignore this interaction and only compare frequencies of effort choices conditional on an agents own wage. With only a few exceptions,[18] effort frequencies are significantly higher if an agent receives 42 instead of 21 in every round of treatment FIP and FIR.

While this suggests that reactions go at least in the right direction the question remains whether dominance of effort for a wage of 42 is mirrored

[16]Fisher exact tests on the frequency of effort choices could not find significant differences.

[17]The effect is significant throughout if one always combines data of three consecutive rounds.

[18]In treatment FIP (FIR) the effort frequencies do not differ significantly in the first and last (first) repetition only.

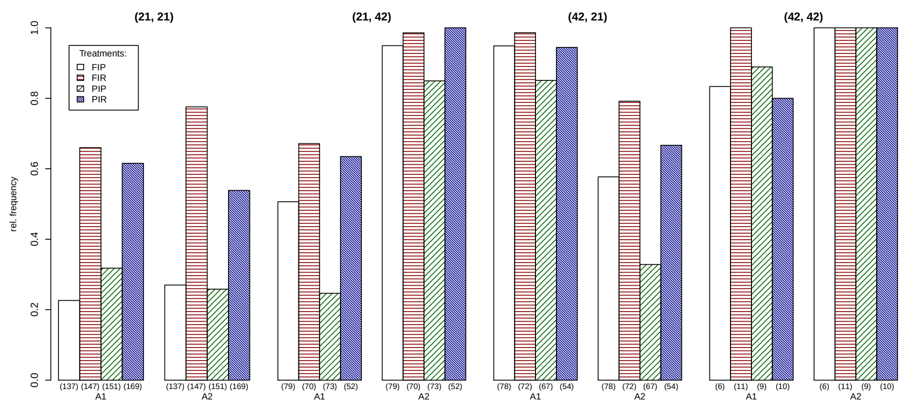


Figure 2: Frequency of Full Effort by Wage Combination, Role, and Treatment.

NOTE: Contrary to this illustration, in treatments PIP and PIR agents only know their own wage.

in the data. We compared actual effort frequencies of agents who received a wage of 42 to a baseline frequency of 97.5%, separately for each treatment and repetition.[19] One sided binomial tests can not reject the null of a 97.5% effort frequency (against the alternative of a lower rate) in every repetition in treatments FIR, FIP and PIR. Surprisingly, however, in periods 2, 5, 6, 11, 14 of treatment PIP the effort frequency is significantly smaller. Over all periods and at a 2.5% confidence level we find that in PIP the true rate of rational behavior is below 91.06%.

Observation 2 *In all treatments agents are more likely to exert effort if they receive the high wage. Also, in FIR, FIP and PIR effort rates of agents with the high wage are not significantly smaller than 97.5%. In PIP, however, effort rates are significantly lower, indicating irrational behavior.*

Intentionality of Wages

We now turn to the question whether agents react to how the wage is determined. Let us first analyze the private information treatments. Comparing effort frequencies of agents that receive a wage of 21 in treatment PIP with treatment PIR reveals a strong and significant negative reciprocity towards a subject decider: In treatment PIP in only 28.7% of all cases agents exert effort compared to 59.5% in PIR.[20] For offers of 42 a similar effect can be observed, i.e. higher effort rates in treatment PIR than in PIP. However, this effect is insignificant throughout.

We now turn to the full information treatments. Again we observe a significant negative reciprocity effect if an agent is offered only 21. This is especially true for combination $(21, 21)$ for which the frequency of effort is significantly higher when the wage combination was determined by the randomization device (FIR) rather than by a human principal (FIP).[21] A similar significant effect holds if an agent is negatively discriminated, i.e. for cases in which a subject gets 21 and the other 42. However, this effect is not significant for individual rounds. In moving bands of five consecutive rounds this effect is significant for early rounds only and becomes insignificant starting over the combined rounds 7 to 11. This is due to a gradual increase in efforts under discriminatory pay in treatment FIP with experience. This can be seen in Figure 3a which plots relative frequencies of effort of agents who received 21 and were informed that the other agent received

[19] Clearly, a hard nosed theorist would compare actual frequencies to a rate of 100%. This hypothesis must clearly be rejected for many rounds. We, however, want to allow for a minor error rate of 2.5%.

[20] Looking at individual repetitions this effect is significant in eight rounds, and, if one always combines data from three consecutive rounds the effect is significant throughout.

[21] Looking at individual periods, in eleven of the fifteen repetitions a Fisher exact test rejects the null hypothesis of equal effort frequencies ($p < .001$ for data combined over all periods).

42, separately for every period in treatments FIP and FIR. Quite contrary to this and as can be seen in Figure 3b, efforts for wage combination (21, 21) gradually decrease over time.

We summarize

Observation 3 *A subject receiving a low payment is more likely to exert effort if his wage was determined by the randomization device rather than by another subject.*

and

Observation 4 *In treatment FIP under negative discriminatory pay agents become significantly more likely to exert effort with experience.*

Discrimination and Information Effects

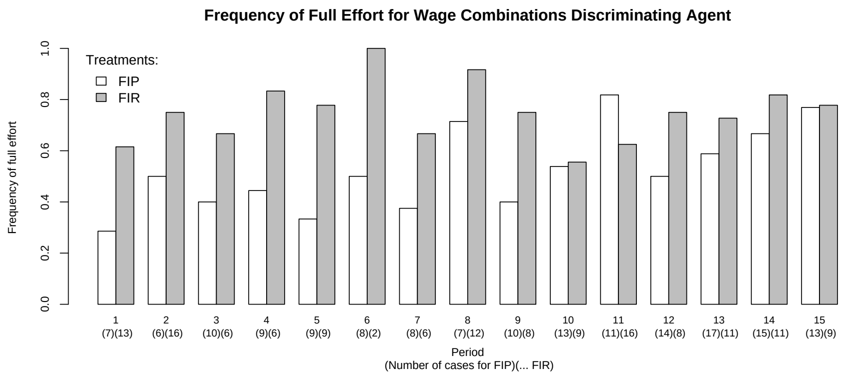
We now turn to the question whether it makes a difference that an agent who gets 21 knows that the other gets 42 or not. According to our parametrization a risk neutral agent receiving a low wage should exert effort whenever he believes that the likelihood of the other agent exerting effort exceeds 0.7143. We already established that in all treatments except for PIP agents receiving 42 contributed with a frequency not significantly different from 0.975. But also in treatment PIP effort rates are significantly higher than 0.7143 throughout. The same holds if one only looks at effort frequencies of the high paid agents for wage combinations $(21, 42)$ and $(42, 21)$ in the full information treatments. Thus, a low paid agent who knows that the other agent receives 42 should always contribute.

From Figure 1 one can already see that in both FI treatments effort rates of the low paid agents under discriminatory pay are significantly below 97.5% in every round. But are agents who receive a low wage more likely to exert effort if they know that the other agent receives a high payment?

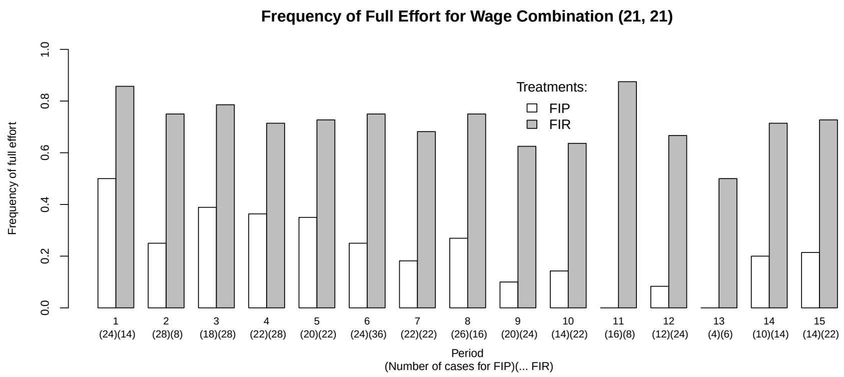
We look at the FI treatments and compare the frequency of effort in case of the symmetric $(21, 21)$ combination with that for cases in which the agent is negatively discriminated. Strikingly, although insignificant ($p = 0.0505$), in FIR the effect is reversed as in 71.77% of the symmetric case $(21, 21)$ agents exert effort, compared to only 62.16% in the asymmetric one.

In FIP on the other hand, the effect is significant ($p < 0.0001$) and in the expected direction with 24.82% in the symmetric case, compared to 54.14% in the asymmetric. A closer inspection of individual rounds confirms our previous observation that this effect evolves with experience: For individual rounds data, this effect is weakly significant ($p < 0.05$) in rounds 10 to 15 and highly significant ($p < 0.025$) starting in rounds 8 and 9 if one combines data from two consecutive rounds.

Observation 5 *Known negative discrimination increases efforts in treatment FIP with increasing experience of subjects, but not in treatment FIR.*



(a) negative discrimination



(b) (21, 21) offers

Figure 3: Frequency of Full Effort in FIP and FIR for Selected Wage Combinations.

This observation is a central result of our paper. In treatment FIP low paid agents learn that high payment of the other agent increases incentives to contribute. Strikingly, such learning is not present in FIR.

We can make similar comparisons between full information and partial information treatments. We compared effort rates in case of negative discrimination in treatment FIP with effort rates for a low payment in treatment PIP. Looking at data combined over all periods, observable negative discrimination in FIP results with 54.14% in significantly more effort than in PIP with only 28.73% ($p < 0.001$). Looking at data from individual rounds this is only significant for the last 5 periods. A similar comparison between FIR and PIR does not show any significant differences (73.24% vs. 65.09%)

Similarly, how do effort rates in case of wage combination (21, 21) in FI treatments compare to those for a low payment in the PI treatments? Interestingly, there are no significant differences, neither in the $--P$, nor in the $--R$ treatments.

There are also some further experience effects. Effort rates for $(21, 21)$ offers in FI, and for low offers in the PI treatments decrease over time.

Observation 6 *In addition to observation 5 we observe the following information effects:*

1. *Effort rates for* $(21, 21)$ *offers in an* $FI - x$ *treatment are not significantly different from those for low offers in the equivalent* $PI - x$ *treatment.*

2. *Effort rates of negatively discriminated agents in* FIR *and in early rounds in* FIP *are not significantly different from those of low paid agents in the equivalent* PI *treatment.*

3. *In later rounds, effort rates of negatively discriminated agents in* FIP *are significantly higher than those of low paid agents in* PIP*.*

4. *Effort rates for* $(21, 21)$ *offers in the* FI*, and for low offers in the* PI *treatments, decrease with experience.*

4.3 Effort Choices - Regression Analysis

Following the rather descriptive analysis from the previous section we now turn to a more controlled analysis. So far results indicate that agents reciprocate a low payment coming from a principal with less effort and that in FIP with experience low paid agents become more likely to exert effort if the other agent receives 42. Thus, so far we have little reason to believe that subjects actually reciprocate intentional negative discriminatory pay.

Table 1 lists the estimates of linear probability models (LPM) regressing the effort decisions on several regressors.[22] Significant coefficients ($p <$ .025) are highlighted by a * and p-values are reported in parentheses. All models include a random effect on every subject,[23] whose relevance was confirmed by Lagrangian multiplier tests. Furthermore, every estimation includes fixed effects for every period and for role A_2 (not reported). Models were estimated using heteroscedasticity robust generalized least squares.

All regressors denoted by D are dummy variables. Variable D_P indicates that the offer was made by the principal, D_I that the wage of the other agent was observable, D_{21} that the agent received a low wage, and $D_{w1 \neq w_2}$ that the two wages differed. Variable t stands for the period and $E[e_j = 1]$ for the reported belief that the other agent will exert effort. Combinations of variables indicate interaction effects. Note the equivalence between the four main dimensions of our experiment and the first four variables in the estimations.

Let us first look at model (1) in Table 1. Most of our previous results are confirmed: A low paid worker is generally significantly less likely to exert effort (D_{21}). If in addition the low wage was determined by the principal the tendency to exert effort is reduced further ($D_P D_{21}$). As before, the mere fact of being discriminated (be it observable or not) has no effect irrespective of whether it is positive or negative discrimination ($D_{w1 \neq w_2}$ and $D_{21} D_{w1 \neq w_2}$). The same holds if one only looks at observable discrimination. Interestingly, the partial effect of observable negative discrimination ($D_I D_{21} D_{w1 \neq w_2}$) is positive, though insignificant.

With respect to our main research question, the coefficient we are most interested in is that on $D_P D_I D_{21} D_{w1 \neq w_2}$ as it measures the effect of the joint occurrence of intentionally determined and observable negative discrimination above the sum of all individual sub-effects. A negative coefficient would proof our hypothesis of a negative reciprocity to discrimination. However, our estimation result shows that it is positive. Thus, indicating that subjects react optimally to negative discrimination by increasing their tendency to exert effort. Although we already observed that the coefficient for observable negative discrimination in general is positive, again it is only significant if the discrimination came from the principal. This confirms our previous observations that in FIP agents learn to react more rational to discrimination. Something we do not find in FIR.

The interaction effect of $D_P D_I D_{21} D_{w1 \neq w_2}$ with variable t in model (2) illustrates the learning effect.[24] Including $D_P D_I D_{21} D_{w1 \neq w_2} t$ in the regression

[22]Generalized linear mixed effects estimations were infeasible due to convergence problems. As LPMs deliver good approximations and robust estimates, and the share of predictions outside the unit interval never exceeded 2.95% , we decided to use LPM instead.

[23]The standard deviation of the random effects is reported as σ_{re}

[24]Note that the regressions include dummies for every period, making the inclusion of

Table 1: Regression Analysis of Effort Decisions

regressor	(1)	(2)	(3)	(4)	(5)
D_P (principal)	-.0671	-.0672	-.0751	-.0749	.0002
	(.379)	(.381)	(.063)	(.065)	(.996)
D_I (Info)	.0945	.1066	.0368	.0387	.0395
	(.220)	(.167)	(.373)	(.351)	(.599)
D_{21}	-.3444*	-.3432*	-.3510*	-.3514*	-.3323*
	(.000)	(.000)	(.000)	(.000)	(.000)
$D_{w_1 \neq w_2}$	.0335	.0370	.0149	.0161	.0173
	(.551)	(.510)	(.443)	(.408)	(.366)
$D_P D_I$	-.0824	-.0934	–	–	–
	(.516)	(.466)			
$D_P D_{21}$	-.2441*	-.2435*	-.2354*	-.2347*	-.2522*
	(.000)	(.000)	(.000)	(.000)	(.000)
$D_P D_{w_1 \neq w_2}$	-.0181	-.0192	–	–	–
	(.721)	(.705)			
$D_I D_{21}$	.0474	.0348	.0931*	.0915*	.0677
	(.504)	(.624)	(.021)	(.024)	(.087)
$D_I D_{w_1 \neq w_2}$	-.0758	-.0889	–	–	–
	(.276)	(.203)			
$D_{21} D_{w_1 \neq w_2}$	.0095	.0081	–	–	–
	(.871)	(.891)			
$D_I D_{21} D_{w_1 \neq w_2}$	.0196	.0325	–	–	–
	(.814)	(.698)			
$D_P D_I D_{21}$	-.0706	-.0618	-.1479*	-.1516*	-.0808
	(.538)	(.592)	(.015)	(.013)	(.168)
$D_P D_I D_{w_1 \neq w_2}$	.1089	.1242	–	–	–
	(.327)	(.268)			
$D_P D_I D_{21} D_{w_1 \neq w_2}$	.2045	-.0737	.2664*	.0049	-.0994
	(.078)	(.598)	(.000)	(.956)	(.256)
$D_P D_I D_{21} D_{w_1 \neq w_2} t$	–	.0294*	–	.0292*	.0231*
		(.000)		(.000)	(.004)
$E[e_j = 1]$	–	–	–	–	.0035*
					(.000)
σ_{re}	.2379	.2388	.2414	.2421	.2220
σ_u	.3418	.3405	.3415	.3402	.3311
R^2	.2808	.2856	.2830	.2856	.3457
% correctly predicted	74.13	74.89	74.15	74.89	76.49

NOTE: Linear probability models including random effect on each subject (standard deviation reported as σ_{re} - relevance of random effects confirmed by Lagrangian multiplier tests) and fixed effects for every repetition (not reported) and role A_2. Estimated using a heteroscedasticity robust generalized least square method. Values in parentheses are p-values. * significant at 2.5% level. "(.000)" means $p < .001$

reduces the effect on $D_P D_I D_{w1 \neq w_2}$ substantially, making it even negative, though still insignificant. As expected the coefficient on $D_P D_I D_{w1 \neq w_2} t$ itself is significant and positive, confirming the learning effect.

Models (3) and (4) are the reduced versions of (1) and (2) which still include the main effects but are reduced by exclusion of insignificant and non explanatory variables in order to account for potential efficiency loss. Observe that in model (3) the coefficient on $D_P D_I D_{w1 \neq w_2}$ is now positive and significant. Another result is that now $D_I D_{21}$ is significant indicating that in treatments with feedback efforts for a low wage are significantly higher. As $D_P D_I D_{21}$ is negative and the joint coefficient $D_P D_I D_{21} + D_I D_{21} = -0.0548$ is insignificant ($p = .326$), this effect is, however, restricted to treatment FIR.

We can use an additional variable to control for aspects which may drive behavior. In model (5) we included the reported subjective probability that the other agent will contribute ($E[e_j = 1]$). The propensity to exert effort increases significantly with increasing expectation that the other exerts effort. Interestingly now the intercept effect $D_P D_I D_2 1D(w_1 \neq w_2)$ becomes negative, indicating that at least in early rounds there is a negative reciprocal reaction to discrimination observable. Still, this effect remains insignificant. The effects on $D_P D_I D_{21}$ and $D_I D_{21}$ now become insignificant which indicates that they were primarily due to different expectations.

We also tested for interaction effects of the belief with the two exogenous treatment dummies D_P and D_I which turned out to be insignificant. Unfortunately, due to simultaneity we have reasons to assume that the belief variable is endogenous. Unfortunately we couldn't find nor construct a truly exogenous and relevant (not weak according to Hansen's J-statistic) instrument to control for endogeneity.

As a final test for whether there exists a negative reaction to discrimination we ran Probit estimations of effort decisions in treatment FIP and FIR only. The results of these estimations which again include random effects on each subject and fixed effects for every period, are reported in Table 2. Although we find some evidence of a negative reaction in early rounds in variable $D_{21} D_{w_1 \neq w_2}$ in models (1) and (2), this remains again insignificant.

Furthermore the results in Table 2 confirm our previously observed experience effects. In FIP the propensity to exert effort under negative discrimination increases significantly over time,[25] whilst the tendency to exert effort under equal but low pay significantly decreases over time.[26] No such experience effect can be found in treatment FIP.

Observation 7 *The regression analysis confirms that there is no negative reaction to observable discrimination. On the contrary, with experience low*

regressor t itself obsolete.

[25]Joint significance of $t + D_{21} t + D_{21} D_{w_1 \neq w_2} t = .0845$ at $p = .007$.

[26]Joint significance of $t + D_{21} t = -.1025$ at $p = .001$.

Table 2: Probit Regression of Effort Decisions

treatment	*FIP*		*FIR*	
regressor	(1)	(2)	(3)	(4)
Intercept	1.025	-.0145	5.436	2.950
	(.129)	(.986)	(.985)	(.983)
D_{21}	-1.164	-1.090	-3.632	-2.791
	(.091)	(.195)	(.990)	(.984)
$D_{w1 \neq w2}$	.2693	.7630	-3.335	-2.048
	(.686)	(.349)	(.991)	(.988)
$D_{21}D_{w1 \neq w2}$	-.7305	-1.6648	2.477	1.087
	(.327)	(.064)	(.993)	(.994)
t	.0773	.0649	.2927	.3852
	(.089)	(.187)	(.063)	(.058)
$D_{21}t$	-.1798*	-.1438*	-.3706*	-.4406
	(.001)	(.014)	(.021)	(.033)
$D_{21}D_{w1 \neq w2}t$	.1807*	.1371*	.0943	.0603
	(.000)	(.001)	(.029)	(.186)
$E[e_j = 1]$	–	.0212*	–	.0244*
		(.000)		(.000)
log $\mathfrak{L}$	-253	-228	-184	-165
% corr. predicted	74.2%	77.2%	79.4%	79.5%

NOTE: Probit models including random effect on each subject (standard deviation reported as σ_{re} - relevance of random effects confirmed by Lagrangian multiplier tests). Reported coefficients are parameter estimates from restricted maximum likelihood estimation. Values in parentheses are p-values. * significant at 2.5% level. "(.000)" means $p < .001$

paid agents become more likely to exert effort if they know that the other agent is paid highly. This, however is only true in treatment FIP and not in FIR.

In addition we wanted to test whether effort choices in one period are not mere reactions to current wages but also to experienced wages in the past, especially discriminatory ones. We constructed a net-discrimination variable which accumulates experienced discrimination in previous periods by adding -1 or +1 for every time an agent received less, respectively more than the other agent.[27] This regressor turned out to be insignificant not only in the partial-, but also in the full information treatments.

4.4 Beliefs

In the previous section, we observed that submitted beliefs are correlated to the effort decision. However, this should not be understood as an indication of rationality. Overall, in only 64% of all cases an agent exerted effort after submitting a belief of effort of the other agent of 71.43% or higher. This ratio is much higher in treatments where the randomization device defined the wages. More specifically in our four treatments FIP, FIR, PIP and PIR the ratios are 57%, 80%, 48% and 71%, respectively.

We now look at belief formation. In Table 3 we report the results of the regression analysis of reported beliefs about the likelihood that the other agent will exert effort. Here D_{j42} indicates that the other agent was offered $w = 42$, what, however, was only known to the agent for $D_I = 1$, i.e. in full information treatments.

As rationality predicts, knowing that the other agent receives 42 increases the expectation of effort significantly ($D_I D_{j42}$). Interestingly, in treatments in which the principal decides, expectations are significantly lower (D_P), indicating a general expectation of some sort of negative reciprocity.

If in full information treatments the other agent receives 42, the expectation is reduced if oneself receives 21 (coefficient for $D_I D_{21} D_{j42}$). This effect can be explained by a lack of understanding of the dominance of effort for $w = 42$: Subjects obviously believed that their own low wage reduces the other agent's confidence in them exerting effort and that this in turn reduces the other one's tendency to exert effort himself.

Again we observe an effect which is restricted to the case where the other agent receives 42 and oneself only 21 in treatment FIP only: The expectation that the other agent will exert effort increases significantly with experience ($D_P D_I D_{21} D_{j42} t$). A similar learning effect could not be observed for any other situation.

[27]For a brief description of experienced discrimination, see section 4.5.

Table 3: Regression Analysis of Reported Belief

regressor	coefficient (*p*-value)	regressor	coefficient (*p*-value)	regressor	coefficient (*p*-value)
D_P	-18.66*	$D_I D_{21}$	3.95	$D_P D_I D_{21} D_{j42}$	20.02*
	(.001)		(.181)		(.000)
D_I	2.78	$D_I D_{j42}$	29.35*	$D_P D_I D_{21} D_{j42} t$	1.80*
	(.469)		(.000)		(.000)
D_{21}	-2.18	$D_P D_I D_{21}$	-13.99*		
	(.191)		(.000)		
D_{j42}	-1.41	$D_I D_{21} D_{j42}$	-18.77*		
	(.400)		(.000)		
σ_{re}	17.83	σ_u	23.21	R^2 (overall)	.2075

NOTE: Linear random effects model estimated via GLS. Relevance of Subject wise random effect confirmed by LM test. * significant at 2.5% level. "(.000)" implies $p < .001$

4.5 Further Results

Learning among Principals

Table 4 reports the parameter estimates from multinomial logit regressions of the wage combination chosen by the principal, where the base case is the $(21, 21)$ offer.[28] The first three columns are the effects on the three other wage combinations in treatment FIP, the last three columns are those for PIP. Model (1) and (2) already reveal that there is a significant repetition effect on the discriminatory offers in FIP but not in PIP. In FIP the tendency to offer a discriminatory combination increases significantly with variable period. Note that as the frequency of (42, 42) offers remains constant, this equally implies that (21, 21) is offered less often with experience.

Despite this significant increase with the number of repetitions, the question is whether this is driven by learning from experience, i.e. whether principals adapt to observed effort choices. In model (3) and (4) we added two variables which summarize each principal's experience with respect to his agent's cooperativeness in previous rounds. Here variable $\overline{D_{(21,21)}e}$past is the average number of agents matched with the principal who exerted effort in case of wage (21, 21) in previous rounds. Similarly $\overline{D_{w1 \neq w_2} e}$ past measures the same for discriminating wage offers. Both variables are zero in case the principal has never offered this wage combination before. Observe

[28] All estimations include fixed effects on each subject, measuring the individual's overall tendency to offer a particular combination.

Table 4: Multinomial Logit Regression of Wage Offers

	FIP			*PIP*		
	$(21,42)$	$(42,21)$	$(42,42)$	$(21,42)$	$(42,21)$	$(42,42)$
model		(1)			(2)	
Period	0.200*	0.234*	-0.007	0.030	0.046	-0.076
	(.0473)	(.0522)	(.1123)	(.0427)	(.0426)	(.0895)
pseudo R^2		.4242			.4653	
model		(3)			(4)	
Period	0.203*	0.218*	0.015	-0.001	0.002	-0.102
	(.0606)	(.0618)	(.1550)	(.0539)	(.0500)	(.1080)
$\overline{D_{(21,21)}}$epast	-0.603	-0.835	-1.556	-1.004	-0.850	-0.697
	(.6137)	(1.104)	(.8711)	(.7392)	(.6900)	(1.611)
$\overline{D_{w1\neq w_2}}$epast	-0.078	0.244	-0.026	0.564	0.883	0.469
	(.4644)	(.4197)	(1.613)	(.5500)	(.5193)	(.9619)
pseudo R^2		.4287			.4723	

NOTE: Parameter estimates of multinomial logit regressions (heteroscedasticity robust standard errors). Estimations included fixed effects for every subject. Pseudo R^2 include subject effects. * significant at 2.5% level.

that for both treatments no learning from experienced effort choices can be found, and that the period effect in treatment FIP remains significant.

Observation 8 *Principals fundamentally learn to discriminate in treatment FIP. Learning is not adaptive as it is not driven by experience.*

Alternation of Discriminatory Pay

In the experimental procedure we decided to fix roles and to allow identification of the two A roles. This allows a discriminating principal to offer equal payoffs in expectation to both A's by balancing the frequency of combination (21, 42) and (42, 21) over the 15 repetitions. On the aggregate level this is clearly the case with a total of 79 offers of $(21,42)$ and 78 of $(42,21)$, in treatment FIP and 73 vs. 67 in PIP. Looking on the individual level, however, this becomes less clear. Allowing for a difference of 1 between the frequencies of the two discriminatory offers,[29] only 9 of the 20 subjects in FIP held a balance of offers compared to 12 of 19 in treatment PIP. Allowing for a difference of 2 the number increases to 15 in both treatments. Note,

[29]This is to account for the uneven number of rounds.

however, that most subjects who held a balance only reached that balance in late rounds. In each of the two treatments there is only one subject each who alternated the discriminating offers whenever they made one. Allowing for one break in the alternation, the number only increases to 4 and 6 in FIP and PIP, respectively.

Individual Experience of Discrimination

A related question is how agents experienced discrimination over the 15 repetitions. Only two agents, both in PIR, never experienced a discriminating wage. For every period from 2 to 15 we identified all agents who up to that round experienced discrimination at least once. Of those we calculated the share who experienced both discriminatory wages equally often. This share varies between 11% and 20% and in the very last round it is about 12% in all four treatments. We can conclude that especially in early rounds discrimination was salient and that even after 15 repetitions the overwhelming majority experienced an overall discriminatory pay.

Efficiency and Payoffs

In our experiment full effort is efficient and, thus, our results on effort choices indicate what treatments yielded the highest efficiency. As efficiency effects are very strong this directly determines results on payoffs for both roles.

Observation 9 *With respect to efficiency and payoffs there are two main effects:*[30]

1. *It yields higher efficiency and higher payoffs for all roles, if the wage is determined by the random mechanism rather than by the principal.*

2. *If wages are determined by the principal and workers have ample opportunity to learn, it is more efficient and yields higher payoffs for everyone if coworkers are informed about each other's payment.*

Questionnaire

At the end of the experiment we asked subjects to answer a set of questions. Some of these questions were designed to measure fairness preferences. As we did not find any significant reciprocity effect other than the negative reciprocity towards low payment in general (and by a subject principal in detail), we refrain from analyzing this data in this paper.

One general question we asked was how someone behaved and why. It is difficult to put answers to this question in simple categories, but there are

[30]Efficiency outcomes are compared by Fisher exact tests comparing the frequency distributions of 0, 1 or 2 total effort in the groups. Payoffs were compared by Wilcoxon rank sum tests.

some common themes to them. Overall the most often mentioned motives which drove behavior were cooperation among workers, self interest, frustration about the behavior of others and envy (or outright spite) towards a very high income of the principal. These motives were more or less mentioned equally often in all treatments.

Other aspects mentioned in the answers such strategic considerations like the conditioning of ones own effort choice on how likely the other agent was believed to exert effort or the riskiness of certain decisions. Here a strong difference between treatments is observable. With 18% and 16% compared to only 2% and 3% of all meaningful answers, a conditioning of ones's own strategy was mentioned much more often in treatments FIP and PIP than in FIR and PIR. Also, the treatments in which the wage was determined by the principal had a much higher frequency of answers which described or explained an advantage of a discriminatory payment (23% and 11% vs. 0% and 0%) - not all of which were given by principals. Similarly, 7% of answers in each of the treatments in which the principal decides, indicate that the subject fundamentally understood the incentives of the game. This was not the case for any answer in the other two treatments. Equally, these treatments show a much higher occurrence of answers which allow to conclude that some form of adaptive or experiential learning took place. (30% of meaningful answers in FIP and 11% in PIP compared to only 7% in FIR and 0 in PIR).

5 Conclusions

Our study reports the first controlled laboratory study that explores the effects of intentional wage discrimination on symmetric agents. In particular we argue that if discrimination of symmetric agents is perceived as unfair then such unfair offers would be reciprocated by less effort. Based on existing evidence we, furthermore, expected that principals shy away from discrimination if it is observable.

We adopted the principal agent game by Winter (2004) which gives incentives to discriminate, as discrimination facilitates coordination among workers. In our experiments we controlled for behavior along four dimensions: Size of remuneration, discrimination, information about discrimination and intentionality of discrimination. Hereof the first two dimensions were compared within subjects and either determined endogenously by the principal or by a random draw conducted by a helper subject who was an outsider to the experiment itself. The latter two dimensions were controlled for exogenously in a between subjects comparison. With respect to intentionality we argue that the random process resembles an environment in which discrimination is not intentional and, thus, the hypothesized reciprocity should not be observable (see, e.g. the results by Bolton et al., 2005). Furthermore, we asked agents to submit their expectation that the other agent will exert effort.

Our main observation is that principals neither shy away from discrimination, nor that subjects react negatively to it. However, agents react negatively to receiving a low wage - irrespective of wether discriminatory or not - especially if it was determined by a principal. It is important to stress that especially in treatment FIP, i.e. in the case of observability of the other agent's wage, this indicates reciprocal behavior: Given the high effort rates by high paid agents, a negatively discriminated agent very likely harms not only the principal and the other agent but also himself by not exerting effort. The effect is much higher in FIP than in FIR what implies that at least the additional effect in FIP can not be due to equity preferences which should influence both treatments the same. This emphasizes the role of intentional discrimination – when the wage offers are determined by a participating subject. In line with those findings is the lack of negative reciprocity observed in Hennig-Schmidt et al. (2010) and Niklisch (2008) where the wage offers were determined by the experimenter.

Under full information and intentional discrimination (FIP) low paid agents learn over time to exert effort and principals increasingly make discriminating offers. This supports experimentally the theoretical predictions of Winter (2004), that discrimination may enhance efficiency as a coordination device. Similar experimental observations were made by Goerg et al. (2008), who find that discriminatory pay in a Winter (2004) game enhances coordination and overall effort. At the same time effort rates for the $(21, 21)$ combination decrease even further. We don't observe similar learning in FIR where the wage is determined by the random process. We conjecture two alternative explanations for this difference: One reason could lie in the fact that in FIP discrimination is intentional which may initiate a more thorough analysis of the game and, thus, learning. Alternatively the answer may lie in the high effort rates in FIR already at the beginning of the experiment. There is little room for learning, whereas in FIP agents learn over time that under discriminatory pay they should not reciprocate a low payment by exerting no effort.

One may argue that such reasoning is strengthened by equity concerns induced by the asymmetric payoff structure of our experimental parametrization. Clearly, equity concerns may influence behavior. However, they can only affect behavior equally in all four treatments and therefore our comparative effects are independent of equity preferences. Also, the fact that we do observe reciprocal behavior towards low payment by a principal implies that equity concerns do not crowd out reciprocity.

The main conclusion one can draw from our results is that, if discrimination of symmetric agents can enhance coordination in teams, then negative discrimination itself has no adverse effect on working morale. This is an important qualification of our main result. Although agents are symmetric, our game gave strong incentives to discriminate. Discriminatory pay was optimal, as it is the cheapest wage combination inducing full effort (under observability of wages) among all workers and full effort was efficient. Rather, keeping wages a secret hinders coordination. Moreover, though overall principals did not offer worse wages than under observability, the partial information on wages may have caused negative responses.

Even though one must be cautious when making policy advise based on merely experimental observations, we think our results indicate the following with respect to performance pay settings. The first implication suggests that it is beneficial to justify low payment by outside factors. Anecdotal evidence from the real world suggests that this is actually done. Low payment is often excused to result from market pressures, obligations from contractual agreements with unions or to restructuring initiatives suggested and planned by external consultants. Often discriminatory pay appears random as well, with team roles being attributed at random and being mostly attached to meaningless titles but still determining the size of bonuses and premiums.

The second implication is that, if discrimination can enhance coordination in teams and a "randomization" of wages is not possible, discrimination should be observable. At least in situations with a similar incentive structure to the Winter game, discriminatory pay enhances coordination among team members, efficiency, and profits.

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6 Appendix: Experimental Instructions

Comment: These are the instructions translated from German for all four treatments. When paragraphs differ this is indicated by

- **R:** Random,
- **P:** Principal,
- **PI:** partial info
- **FIR:** full info random,
- **PIR:** partial info random.

Anything unmarked was part of the instructions in all treatments.

Welcome to the experiment! Please cease any conversation with other participants, turn off your cell phone and read the following instructions carefully. If something is not clear, please raise your hand and we will come to you and answer your questions individually.

R: One of you has been assigned the role of supervisor. His or her task is to carry out the random moves. He did not get a copy of the instructions and will not participate in the actual experiment.

The instructions are identical for all (**R:** other) participants. During the experiment you remain anonymous. This means that no participant will learn about your identity.

The experiment consists of two phases. In the first phase you have the opportunity to familiarize yourself with the software and the rules of the experiment in a non binding way. In the second phase you will interact in 15 repetitions (rounds) with other participants. You can earn money in each of these 15 rounds. How much money you earn will depend on (**R:** a random move,) your own decisions, and those of other participants. However, only one round will actually be paid. At the end of the experiment a random draw will determine which round is relevant for payment. For each participant the earnings are then calculated according to the earnings in that round.

During the experiment all sums of money are listed in ECU (for Experimental Currency Unit). Your earnings during the experiment will be converted to Euro at the end and paid to you in cash. The exchange rate is 3 ECU = 1 Euro.

There are two roles in the experiment. One third of all participants will be assigned the role of the employee M, the remaining two thirds will be assigned the role of a worker A.[31] The roles will be assigned at random and

[31] "Arbeitgeber M" and "Arbeiter A" in German.

remain unchanged throughout the experiment. In each round three participants interact: one *M* and two *A* (called *A1* and *A2*). The composition of the three participants will be newly assigned at random in each round. We will make sure that nobody is assigned to a participant with whom he has interacted in the previous round.

Course of a round

P: The employer *M* starts by deciding how much both workers *A* should receive **if <u>both</u> exert full effort.** The employee *M* can choose for each worker between a low and a high amount: 21 or 42 ECU. He can choose the amount individually for each worker.

R: A random move decides how much both workers *A* should receive **if <u>both</u> exert full effort.** The mechanism of the random move will be explained in detail below. For each worker there is either a high or a low amount: 21 or 42 ECU.

Taken together for the two workers there are four possibilities: (high, high) (low, low) (low, high) and (high, low). A high amount makes effort more attractive to the worker but is more costly to M.

Then, both workers (*A1* and *A2*) learn their own as well as the other workers amount (**PI:** only their own. . .), and then individually decide whether to exert effort. Effort increases the payoffs of all, but creates costs of 9 ECU to the worker. Depending on the selected amounts for *A1* and *A2* (noted as L1 and L2 each of which could either be 21 or 42) and on how many workers decide to exert effort, the payoffs are calculated as follows.

The employee M receives:

If no worker exerts effort:	$\frac{100-L_1-L_2}{4}$
If one worker exerts effort:	$\frac{100-L_1-L_2}{2}$
If both workers exert effort:	$100 - L_1 - L_2$

A worker with amount L receives:

If neither he nor the other worker exert effort:	$\frac{L}{4}$
If only he exerts effort, but the other not:	$\frac{L}{2} - 9$
If the other worker exerts effort, but he himself does not:	$\frac{L}{2}$
If both workers exert effort:	$L - 9$

How much each receives depending on L1 and L2 and on who exerts effort is listed in detail in the table at the end of the instructions.

FI: After each round all three participants learn which action each participant chose and which payoff each of them would receive in case this round is selected for payment. ("round income")

PI: After each round all three participants learn which worker exerted effort and their **own** payoff in case this round is chosen for payment. ("round income") The employee M is also informed about the income of both workers.

R: Random move

The random move determines the amounts for both workers in every round. The mechanism works as follows: In a bowl there are 100 wooden chips, numbered 1 to 100. Each chip corresponds to a particular combination of both amounts. At the beginning of a round the supervisor draws a chip from the bowl. The number on the chip determines the amount for worker *A1* and *A2* according to a table as shown in figure 1.

—

FIR: This table shows in the second and third column the amounts for *A1* and *A2*. The first column assigns each combination of amounts to a set of chip numbers. For example if the number 52 is drawn, *A1* will receive 21 (low) and *A2* will receive 42 (high).

The table is newly and independently created at the beginning of each round for each group of three (consisting of *M, A1* and *A2*). Thus it can vary from round to round and group to group. In figure 2, for example, the same chip number 52 leads to a combination of amounts of (low) 21, (low) 21.

The frequencies in each table for each combination of amounts is constant: altogether 46 chip numbers lead to (21, 21); 26 chip numbers lead to (21, 42); 26 chip numbers lead to (42, 21); and 2 chip numbers lead to (42, 42).

—

PIR: There are four columns in this table for *M*, for each possible combination of the amounts for *A1* and *A2*. Each number from 1 to 100 is assigned to one particular combination. For example, if the chip with number 55 is drawn, *A1* will be offered 21 (low) and *A2* will be offered 42 (high).

For the two *A* participants in a particular group of three (each consisting of one *M, A1*, and *A2*) the assignment of chip numbers for amounts L1 and L2 is identical to the one for *M*. However, *A1* and *A2* will only see their own amount which is determined by the draw. Figure 2 shows an example of that screen for *A1* and Figure 3 the corresponding figure for *A2*. In Figure 2 you can see that, as it does in the table for for *M*, chip 55 determines 21 (low) for *A1*. Correspondingly in Figure 3 chip 55 determines 42 (high) for *A2*.

Thus, workers *A1* and *A2* only see their specific summary of the table of *M*.

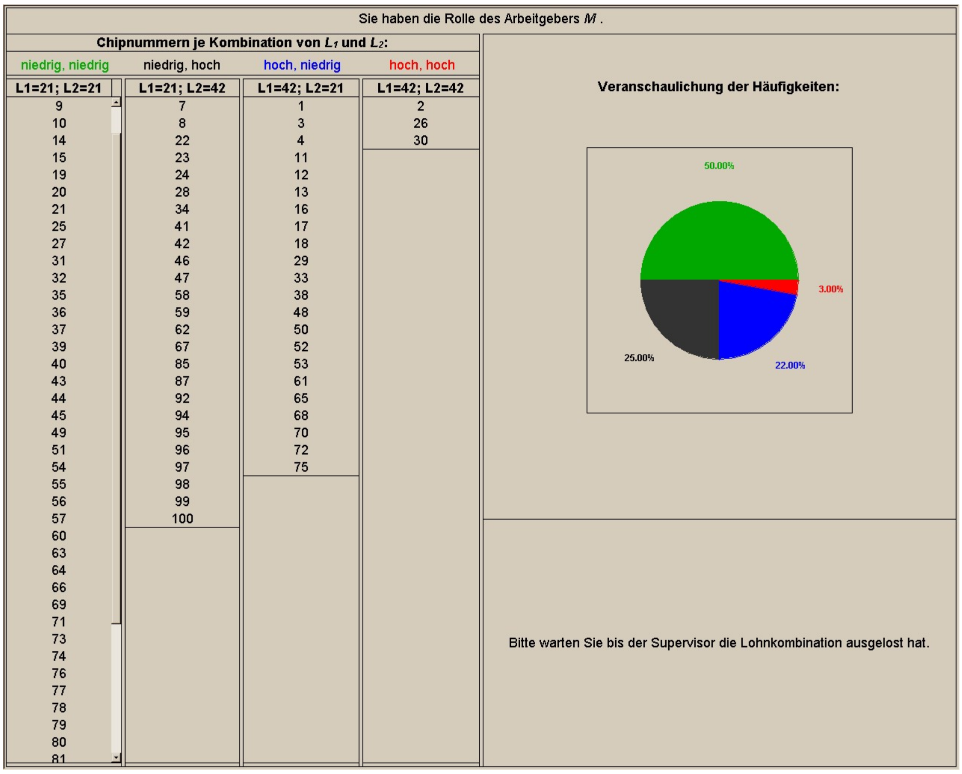


Figure 4: Example for a table of the principal in PIR (not taken from actual instructions).

The table is newly and independently created at the beginning of each round for each group of three (consisting of *M*, *A1* and *A2*). Thus it can vary from round to round and group to group. In figure 4, for example, the same chip number 55 leads to a combination of amounts of (low) 21, (low) 21.

The frequencies in each table for each combination of amounts is constant: altogether 50 chip numbers lead to (21, 21); 25 chip numbers lead to (21, 42); 22 chip numbers lead to (42, 21); and 3 chip numbers lead to (42, 42).

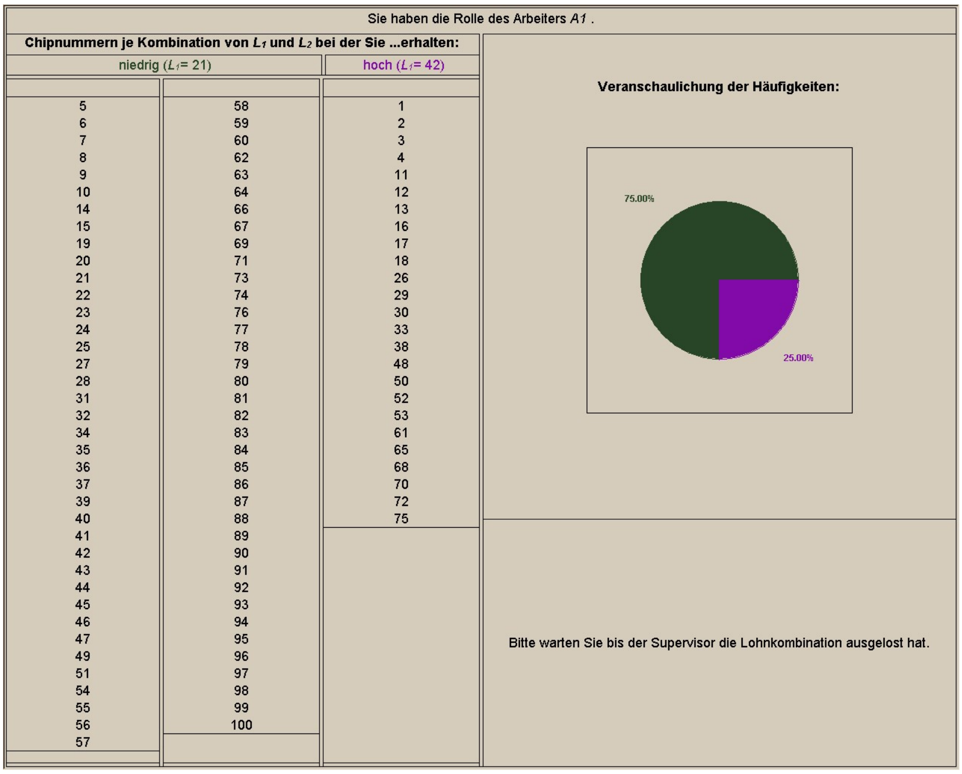
—

6.1 Procedure

(Identical in all treatments)

Next you will be given the opportunity to test the software in all roles for five minutes. You will simulate all three participants. Nothing you are doing during these five minutes will affect your payment. Also, no other participant will be informed about your input.

The five minutes testing phase is followed by the second phase. You will first

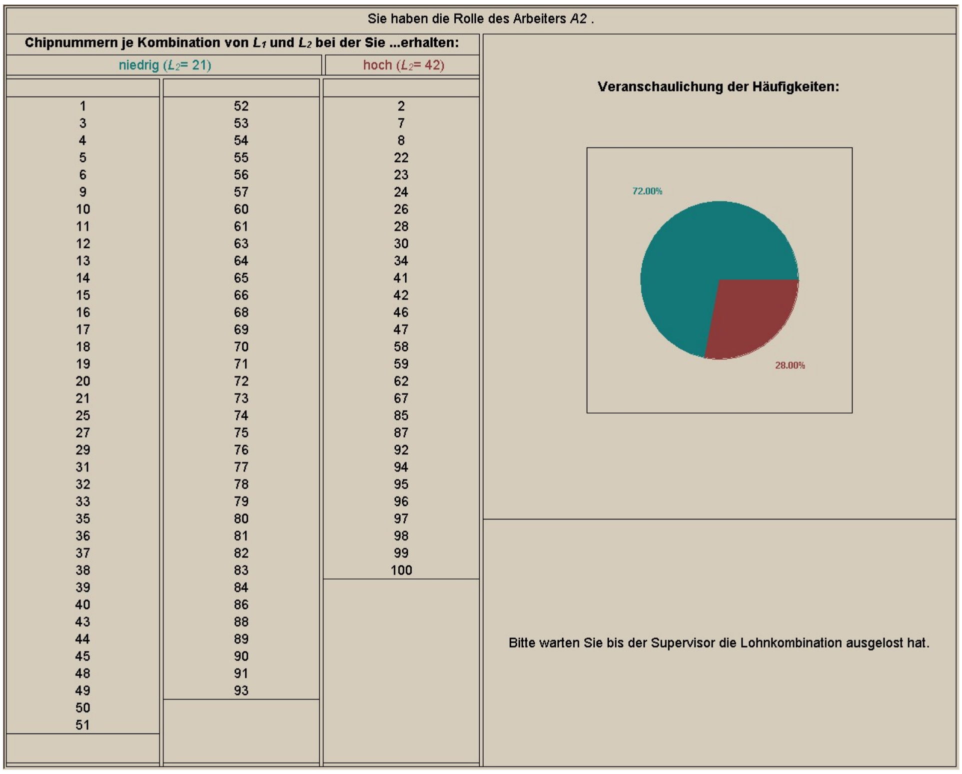


Sie haben die Rolle des Arbeiters *A1* .

Chipnummern je Kombination von L_1 und L_2 bei der Sie ...erhalten:

niedrig (L_1= 21)		hoch (L_1= 42)
5	58	1
6	59	2
7	60	3
8	62	4
9	63	11
10	64	12
14	66	13
15	67	16
19	69	17
20	71	18
21	73	26
22	74	29
23	76	30
24	77	33
25	78	38
27	79	48
28	80	50
31	81	52
32	82	53
34	83	61
35	84	65
36	85	68
37	86	70
39	87	72
40	88	75
41	89	
42	90	
43	91	
44	92	
45	93	
46	94	
47	95	
49	96	
51	97	
54	98	
55	99	
56	100	
57		

(a) Agent A1



Sie haben die Rolle des Arbeiters *A2* .

Chipnummern je Kombination von L_1 und L_2 bei der Sie ...erhalten:

niedrig (L_2= 21)		hoch (L_2= 42)
1	52	2
3	53	7
4	54	8
5	55	22
6	56	23
9	57	24
10	60	26
11	61	28
12	63	30
13	64	34
14	65	41
15	66	42
16	68	46
17	69	47
18	70	58
19	71	59
20	72	62
21	73	67
25	74	85
27	75	87
29	76	92
31	77	94
32	78	95
33	79	96
35	80	97
36	81	98
37	82	99
38	83	100
39	84	
40	86	
43	88	
44	89	
45	90	
48	91	
49	93	
50		
51		

(b) Agent A2

Figure 5: Example for tables of the agents in PIR (not taken from actual instructions). Equivalent to situation pictured in table 4

learn your role (M or A) before you run through 15 rounds as described above. At the beginning of each round you are assigned to two other participants. Under no circumstances you will meet the same participants you interacted with in the previous round.

Between your decisions we will ask for your expectations regarding the decision of the other participants. This has no impact on your payoffs nor will it be revealed to any other participant.

At the end we will ask you to answer in a short questionnaire.

If you have read and understood everything, please click "continue" on your computer screen.

6.2 Payoff Table

Offer	A1	A2	**Payoff in ECU**		
A1(low), A2(low)	no	no	14.50	5.25	5.25
	no	yes	29.00	10.50	1.50
	yes	no	29.00	1.50	10.50
	yes	yes	58.00	12.00	12.00
A1(low), A2(high)	no	no	9.25	5.25	10.50
	no	yes	18.50	10.50	12.00
	yes	no	18.50	1.50	21.00
	yes	yes	37.00	12.00	33.00
A1(high), A2(low)	no	no	9.25	10.50	5.25
	no	yes	18.50	21.00	1.50
	yes	no	18.50	12.00	10.50
	yes	yes	37.00	33.00	12.00
A1(high), A2(high)	no	no	4.00	10.50	10.50
	no	yes	8.00	21.00	12.00
	yes	no	8.00	12.00	21.00
	yes	yes	16.00	33.00	33.00

Examples

- Assume A1 gets a "low" and A2 a "high" offer If both exert effort, M gets 37 ECU, A1 12 ECU and A2 33 ECU. If none exerts effort, M receives 9.25 ECU, A1 5.25 ECU and A2 10,50 ECU. etc...
- Assume both A1 and A2 receive a "high" offer. If none exerts effort, M receives 4ECU, A1 10,50ECU and A2 10,50ECU If A1 exerts effort but A2 not, then M receives 8ECU, A1 12ECU and A2 21ECU etc.
- Assume both A1 and A2 receive a "low" offer. If none exerts effort, M receives 14,50ECU, A1 5,25ECU and A2 5,25ECU If A1 exerts effort but A2 not, then M receives 29ECU, A1 1,50ECU and A2 10,50ECU etc.