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Co-employment of permanently and temporarily employed agents[#]

Werner Güth⁺, Martin G. Kocher[§], and Vera Popova⁺

Abstract: One-shot interaction and repeated interaction often co-exist in the real world. We study possible behavioral effects of this co-existence in a principal-agent setting, in which a principal simultaneously employs a permanent and a temporary agent. Our experimental results indicate that there is “discrimination” between the two agents and that the available information for agents determines the extent of this discrimination, even though the theoretical solution of the game implies equal treatment of agents. Discrimination is, thus, a consequence of reciprocity. Agents that are discriminated against react negatively by withholding effort.

JEL classification: C72, C91, D21, J31

Keywords: principal-agent problem; permanent and temporary employment; fairness; wage discrimination

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1 Introduction

Repeated interaction often co-exists with one-shot interaction in the same group. Work teams or clubs, for instance, may consist of permanent members and temporary members with a much shorter interaction horizon. Firms employ workers with permanent contracts and temporary workers often even for the same job and in the same department. University hire permanent faculty alongside non-tenured faculty. In this paper, we study the effects of the co-existence of repeated interaction and one-shot interaction. We first model an appropriate principal-agent situation. Based on the theoretical predictions from our model, we conduct a series of controlled laboratory experiments.

In our model, principals employ two agents and can determine a fixed wage and a piece rate for each of them. One agent is a permanent agent that stays with the same principal for a commonly known number of periods; the other agent is a temporary agent that is assigned to another principal in every period. After being informed about the contract(s) offered by the principal, agents determine their costly effort levels. Our treatments are implemented in a way that – according to the theoretical benchmark of money-maximizing agents – there should not be any discrimination¹ between the two agents by the principal.

The tenure of a relationship has been shown to strongly influence the terms of contracts on labor markets (Brown et al., 2004). There is ample evidence for gift-exchange games with incomplete contracts that wages exceed the marginal product of labor (Fehr et al., 1993; Fehr et al., 1998a, b; Fehr and Falk, 1999) even in a one-shot setting because of reciprocity. However, the effect is usually much stronger in a repeated interaction.² In a repeated interaction, a possible reputation effect comes on top of the originally described fair wage-effort effect (Akerlof, 1982; Akerlof and Yellen, 1990).

¹ We use the term “discrimination” throughout the paper without negative connotation. It simply indicates whether principals offer different contract terms to the different agents.

² For an overview of the “early” experimental literature on the gift-exchange game, see Gächter and Fehr (2000). It is important to note that there is some discussion on the robustness of the gift-exchange results (Levitt and List, 2007). However, even if gift-exchange is not always robust (e.g., Hannan et al., 2002; Charness et al., 2004), most field studies show a certain level of it (a recent overview is provided by Cooper and Kagel, 2009). In our study, we are not going to interpret the level of gift-exchange, but rather the difference in gift-exchange across different experimental treatments and agent types.

Most experimental studies on principal-agent relationships use simple fixed wage environments, i.e. entirely incomplete contracts. A couple of papers employ partly incomplete contracts that use a fixed wage and possible bonuses and fines. In Fehr et al. (2007) paying the bonus is at the discretion of the principal after the agent has chosen the effort, whereas bonuses and fines are enforceable in Gächter et al. (2009).

Not many experimental papers study complete contract designs. Anderhub et al. (2002) investigate behavior within a principal-agent experiment that allows for a large class of linear contracts. They find a high degree of incentive-compatible behavior, but also fair sharing and reciprocity. A similar result is provided by Güth et al. (1998) in a dynamic principal-agent environment. Keser and Willinger (2000) present evidence that subjects in the role of agents choose payoff-maximizing action, while subjects in the role of principals offer non-optimal contracts. Anderhub et al. (2003) study the relationship between contract duration and firm-specific investments in a principal-agent setting. In contrast to the game-theoretic prediction for their setup, they observe reduced investments in short-term contracts compared to long-term contracts.

We are, however, not aware of any paper studying whether and how the *co-existence* of permanent and temporary agents – which is obviously widespread on modern labor markets – affects these results.³ More specifically, we ask two sets of main research questions: (i) Do principals discriminate between the two types of agents, and if so which type is favored? How do agents react to such discrimination? (ii) Which role does the level of information on contract details among agents play; or in other words: Do principals condition their contract offers on whether agents know the contracts of their co-workers or not?

Our main findings indicate a significant degree of discrimination. It is stronger where the two agents are not informed about contract details before exerting effort and weaker where information is given. It is important to note that there is no reason to discriminate according to the standard theoretical solution of our principal-agent game. Thus, discrimination is purely behavioral in the sense that only reciprocity or, more general, social preferences can explain its existence. Put differently, reciprocity among the principal and the permanently employed agent causes discrimination within the firm. The latter

³ Alewell et al. (2007) analyze the co-employment of hired and rented hands in a principal-agent setting. In their model rented hands are employed due to unexpected higher demand, and there are no long-term relationships.

observation confirms that at least part of the social concerns that are typically observed in principal-agent games in the laboratory are strategic and not intrinsic. Agents respond strongly to contractual incentives, no matter whether they are permanently employed or only temporarily. As expected, for temporary agents, being discriminated against has a strong and significant effect on effort provision: they “punish” principals by reducing their effort when being discriminated against in fixed wages and reward principals for equal treatment in piece rate wages. Anticipating such responses, it is optimal for principals not to discriminate, although the negative effect of discrimination on principal’s profits is small. Hence, those principals who do not discriminate seem to have an additional non-monetary preference for non-discrimination.

Our paper is also related to a small but increasing set of experimental papers that take multiple (*homogeneous*) agents into account. Cabrales and Charness (2003) study the optimality of different 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. Cabrales and Charness (2003) thereby contemplate the common situation where contracts must be negotiated with a union and, then, be approved by the workers. The paper focuses on the characteristics of the chosen contract menus and evaluates them against social preference models. Maximiano et al. (2007) 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. In a similar vein but with a focus on horizontal comparisons rather than vertical ones, Charness and Kuhn (2007) 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.

Altmann et al. (2009) extend the setting of Brown et al. (2004) to one in which firms can employ no, one or two agents. They are interested in the interaction of contract completeness with unemployment and find that firms pay high wages but offer fewer vacancies than possible and efficient from a standard viewpoint. Finally, Kocher et al. (2010) study contract standardization for the same job within a given firm. More specifically, they 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, they observe in contrast to theoretical predictions that contract standardization leads to significantly higher wages, effort levels and a higher market efficiency.

Our research questions regarding the effects of contract disclosure are related to a much larger body of literature on social comparison.⁴ While Maximiano, Sloof and Sonnemans (2007) as well as Charness and Kuhn (2007) provide some evidence that the vertical comparison (between the principal and the single agent) seems to be more important than the horizontal comparison (between agents), there is also some counter-evidence in other contexts. Knez and Camerer (1995), for instance, report a modest effect in a three-person ultimatum game with one proposer and two responders, who receive (and observe) different offers given their different outside options. In a standard ultimatum game, Bohnet and Zeckhauser (2004) provide evidence that information on average offers influences decisions of responders. In another experimental setting that is more similar to our labor market Güth et al. (2001) show that horizontal fairness matters at least as far as principals are concerned. Principals prefer to submit more equitable offers when work contracts are observable to both agents. In a similar vein, Alewell and Nicklisch (2009) study the effect of social comparison for wage acceptance in multilateral ultimatum games. The results of their laboratory experiments indicate that the availability of information concerning ultimatum offers to other responders influences acceptance behavior.

Principal-agent settings have been studied extensively in field experiments over the last couple of years. While the picture is not fully conclusive concerning the persistency of gift-exchange, the basic result that reciprocity plays a role on many labor markets has been confirmed quite generally (Cooper and Kagel, 2009). As with laboratory experiments, we are not aware of any field experiment that observes permanent agents and temporary agents working alongside each other.⁵ There is, however, some related evidence from natural field data providing results in favor of the wage compression effect for workers. Management's and worker groups' interests seem to be aligned on that issue (Lazear,

⁴ Festinger (1954) and Deutsch (1985) are the classic references from social psychology, but the literature is huge.

⁵ There are many field studies that analyze the interaction between a principal and an agent. See, for instance, Gneezy and List (2006).

1989). Even in the absence of fairness considerations firms choose more equitable wage structures to prevent uncooperative behavior among workers. Outside the laboratory it is, however, very difficult to control for the effort costs of agents, the relevant interaction horizons and the information conditions, whose consequences we are particularly interested in. Furthermore, selection into types is often endogenous in the field, whereas an experiment solves the problem by assigning subjects randomly to treatments.⁶

The remainder of the paper is organized as follows: Section 2 introduces the principal-agent model in more detail. In Section 3, we describe the experimental design and derive some behavioral predictions. Section 4 presents our experimental results, and Section 5 concludes the paper and discusses implications of our findings.

2 The basic principal-agent setup

The principal (P) of a firm employs two workers, a permanent agent (PA) and a temporary agent (TA), to produce a good which is sold at the positive price p . A contract offer $\{w_i, s_i, \tilde{e}_i\}$ to an agent $i = \{PA, TA\}$ in a given period $t \in \{1, 2, \dots, N, \dots, T\}$ consists of a non-negative fixed wage w_i , a piece rate s_i (with $0 \leq s_i \leq p$) and a desired non-binding effort level $\tilde{e}_i \geq 0$.⁷ Knowing at least her own contract or, depending on the treatment, both contracts, an agent decides on the effort level that she wants to exert. The cost of effort function $c_i \cdot e_i^2 / 2$ for the production of one unit of a good is convex in effort, where c_i denotes the positive cost parameter of an agent. The timing of the interaction in each period is standard: first, P simultaneously offers a contract to both PA and TA . Then, both agents exert effort simultaneously.⁸

⁶ Torgler et al. (2009) provide an interesting empirical study on social comparison among players in football teams.

⁷ For notational convenience we suppress the time index whenever it is not necessary.

⁸ Since both agents are assumed to have no outside option and the fixed payments are restricted to be non-negative, we assume that agents automatically accept the contracts proposed to them by P . At most, they may “reject” the contract by exercising zero effort. Non-negativity of w_i also excludes internalizing the positive externalities of effort through $s_i = p$ in exchange for a negative payment w_i . Such a franchising solution is assumed to be infeasible due to exogenous restrictions. In our setup, s_i is interpreted as a share of pe_i .

The resulting period payoffs of the three players are given by equations (1) to (3).

$$\pi_p = (p - s_{PA}) \cdot e_{PA} - w_{PA} + (p - s_{TA}) \cdot e_{TA} - w_{TA} \text{ for the principal} \quad (1)$$

$$\pi_{PA} = w_{PA} + s_{PA} \cdot e_{PA} - \frac{c_{PA}}{2} \cdot e_{PA}^2 \text{ for the permanent agent} \quad (2)$$

$$\pi_{TA} = w_{TA} + s_{TA} \cdot e_{TA} - \frac{c_{TA}}{2} \cdot e_{TA}^2 \text{ for the temporary agent} \quad (3)$$

The temporary agent is only employed for one single period by the same principal, whereas the permanent agent is employed for N periods with $1 < N < T$, where T is the finite number of periods played in a session. Note that the two agents – except for possible behavioral effects from the comparison on what they individually earn when being co-employed – are structurally unrelated. Since N and T are finite, the theoretical solution to this interaction is straightforward. Assuming common knowledge of rationality and purely selfish preferences yields a stationary equilibrium for the multi-stage game, with the equilibrium of the base game being played repeatedly. An agent's optimal effort is $e_i^* = s_i / c_i$. Anticipating optimal efforts the principal's optimal contract offer to each of the two agents is $\{w_i^* = 0, s_i^* = p/2, \tilde{e}_i\}$ with any \tilde{e}_i , because \tilde{e}_i is just cheap talk. Substituting yields the solution play:

$$e_i^* = \frac{p}{2c_i}, s_i^* = \frac{p}{2}, w_i^* = 0, \tilde{e}_i \text{ for } i = PA, TA \quad (4)$$

3 Experimental design and behavioral predictions

3.1 Experimental treatments and parameters

Our interest is in discrimination between long-term and short-term agents, the possible reaction to such discrimination, the behavioral effects of a short-term relationship on a concurrent long-term relationship in a principal-agent setting, and the role of information. The two treatments that we implemented are, therefore, straightforward:

Treatment INFO: In each phase of the experiment, the permanent agent repeatedly interacts with one principal, whereas the temporary agent is randomly assigned to another principal in every new period. The two agents have the same effort-cost parameter and are informed about the contract details of their co-workers before exerting effort.

Treatment NOINFO: In each phase of the experiment, the permanent agent repeatedly interacts with one principal, whereas the temporary agent is randomly assigned to another principal in every new period. The two agents have the same effort-cost parameter, but are NOT informed about the contract details of their co-workers before exerting effort.

Parameter ranges were chosen as follows: $w \in [0,100]$, $s \in [0,4]$, $e \in [0,100]$, $\tilde{e} \in [0,100]$. Numbers with up to two decimals were allowed. Furthermore $p = 4$, $c = 0.05$, $N = 10$, and $T = 20$ in all treatments. All parameters are common knowledge among participants in the experiment. The feedback conditions after each period are standard: Principals are informed about both workers' effort choices and their own profit; agents only receive feedback on their own profits, but not on the profits of co-workers. Thus, even in treatment INFO an agent cannot compare payoffs, since the effort choice of the other agent is not revealed.

The chosen parameters imply the optimal contract of

$$e_i^* = 40, s_i^* = 2, w_i^* = 0, \tilde{e}_i \text{ for } i = PA, TA \quad (5)$$

Consequently, $\pi_p = 160$ for the principal, and $\pi_{PA} = \pi_{TA} = 40$ for each of the agents in equilibrium. The socially optimal solution would imply $e_i^* = 80$ for $i = PA, TA$ regardless of s and w . Due to the quadratic costs of effort, a higher than equilibrium effort is always more beneficial for the principal than for the agent. A principal can aim to elicit such a higher effort level from a reciprocal agent through a raise in the wage w and/or a raise in the piece-rate s .

3.2 Hypotheses

According to the standard theoretical benchmark, principals do not offer differentiated contracts to the two types of agents in any of our treatments. The only relevant contract component, the piece rate, does not depend on the characteristics of the worker but only on the exogenous price, and agents best-respond to the piece rate.

*H1. Principals will offer equal contracts to both agents **PA** and **TA** with contract terms corresponding to the theoretical benchmark.*

H2. Agents will best-respond to offered piece rates, ignoring fixed wages and desired effort level.

However, evidence from principal-agent experiments show that many people do not behave according to the standard theoretical benchmark. Previous experimental findings inspired different attempts to capture other-regarding concerns. Deriving point-predictions using social preference models is difficult and somewhat arbitrary in our context. It depends critically on how one defines reference groups, e.g. whether the reference group for an agent includes only the co-worker in the treatment with information or also the principal. We, therefore, focus on qualitative predictions when we discuss other-regarding concerns.

Neither opportunistic, nor inequity-averse players should be affected by the interaction horizon and the information condition. More interesting types of players are those who behave reciprocally out of strategic motives. Brown et al. (2004) and Fehr et al. (2007) have shown for a somewhat different setting⁹ with only fixed wages that opportunistic agents initially have an incentive to mimic fair agents, when the proportion of fair principals is large enough in the population until the second to last period of a repeated interaction.

In the role of temporary agents such strategic cooperators would behave completely opportunistically, whereas in the role of permanent agents they would act reciprocally. Consequently, fair-minded and opportunistic principals may offer generous contracts, e.g. offer a positive fixed wage or a piece rate $s > 2$ to permanent agents. In order to be an equilibrium such a strategy combination requires that the number of fair-minded agents and strategic agents together is sufficiently large. Furthermore, if principals assume that the number of fair-minded agents is small enough (so that most agents will not react negatively to discrimination), strategic principals may offer different contracts to the two agents even in the INFO treatment. Hence, discrimination should be smaller in the treatment with disclosure, without necessarily ruling out discrimination.

⁹ In these settings, the kind of relationship (permanent or transitory) was endogenously determined and not exogenously varied as in our experiment.

aH1. In line with the fair-wage hypothesis (Akerlof and Yellen, 1990), principals offer, on average, more generous contract conditions than predicted by the theoretical benchmark. They use the fixed wage to induce higher effort levels in the spirit of gift exchange, because its signal is more obvious.

aH2: Principals discriminate against temporary workers in NOINFO, but less so in INFO.

aH3: Agents on average behave reciprocally. For temporary agents this may mean that they shirk in case of discrimination.

3.3 Experimental procedures

The computerized experiment was conducted at the laboratory of the Max Planck Institute in Jena (Germany). It was programmed using z-Tree (Fischbacher, 2007). A total of 108 participants (all undergraduate students at the University of Jena, invited using the organizational software ORSEE by Greiner, 2004) took part in the four sessions of the experiment (two per treatment). The same number of participants (54 per treatment) faced either the scenario in which (1) all workers received information about the coworker's contract (INFO), or the scenario (2) in which workers *did not* receive any information about the coworker's contract (NOINFO). We used matching groups consisting of nine participants.

Sessions proceeded in the following way: upon entering the laboratory, participants were randomly assigned to cubicles and provided with written instructions. After sufficient reading time, the instructions (see Appendix) were also read out aloud by the experimenter. Participants were, then, asked to answer a set of control questions to make sure that the rules of the experiment have been fully understood. Any incorrect entries were corrected and all remaining questions clarified before the experiment started. Roles of agents were described in neutral terms ("type A" and "type B").

Each session extended over a total of 20 periods, which were grouped into two phases of ten periods each. The course of action in a specific period followed the procedure described above. Sessions lasted for about 60 minutes. At the end of each session, the accumulated period profits in experimental currency units (ECU) were converted into euro at the pre-announced rate of 200 ECU = € 1.00. Average earnings were € 7.63 per subject.

In addition to their income from the experiment, participants received a fixed show-up bonus of € 2.50.

4 Experimental results

In reporting our experimental results, we proceed as follows: We first analyze the extent of discrimination between permanent and temporary agents by the principal (Section 4.1). Then, we proceed to discuss the behavior of agents in our two treatments (Section 4.2). Section 4.3 provides a more extensive treatment of the determinants of principals' profits.

4.1 First overview and evidence for discrimination

4.1.1 Descriptive overview

We start by reporting descriptive results for our two treatments: information about co-workers contracts (INFO) and no information on co-workers contracts (NOINFO). Table 1 gives a first overview of the results. On the average level, there appears to be a difference between the two treatments in terms of the fixed wage, but it is not significant on standard levels if we compare completely independent observations on the matching group level. Comparing the two types of agents within a treatment, in NOINFO permanent agents receive significantly higher fixed wages ($p = 0.046$) and piece rate wages ($p = 0.075$) than temporary agents (Wilcoxon signed-rank tests¹⁰). This is not the case in the INFO treatment.

Table 1: Choices of principals and agents

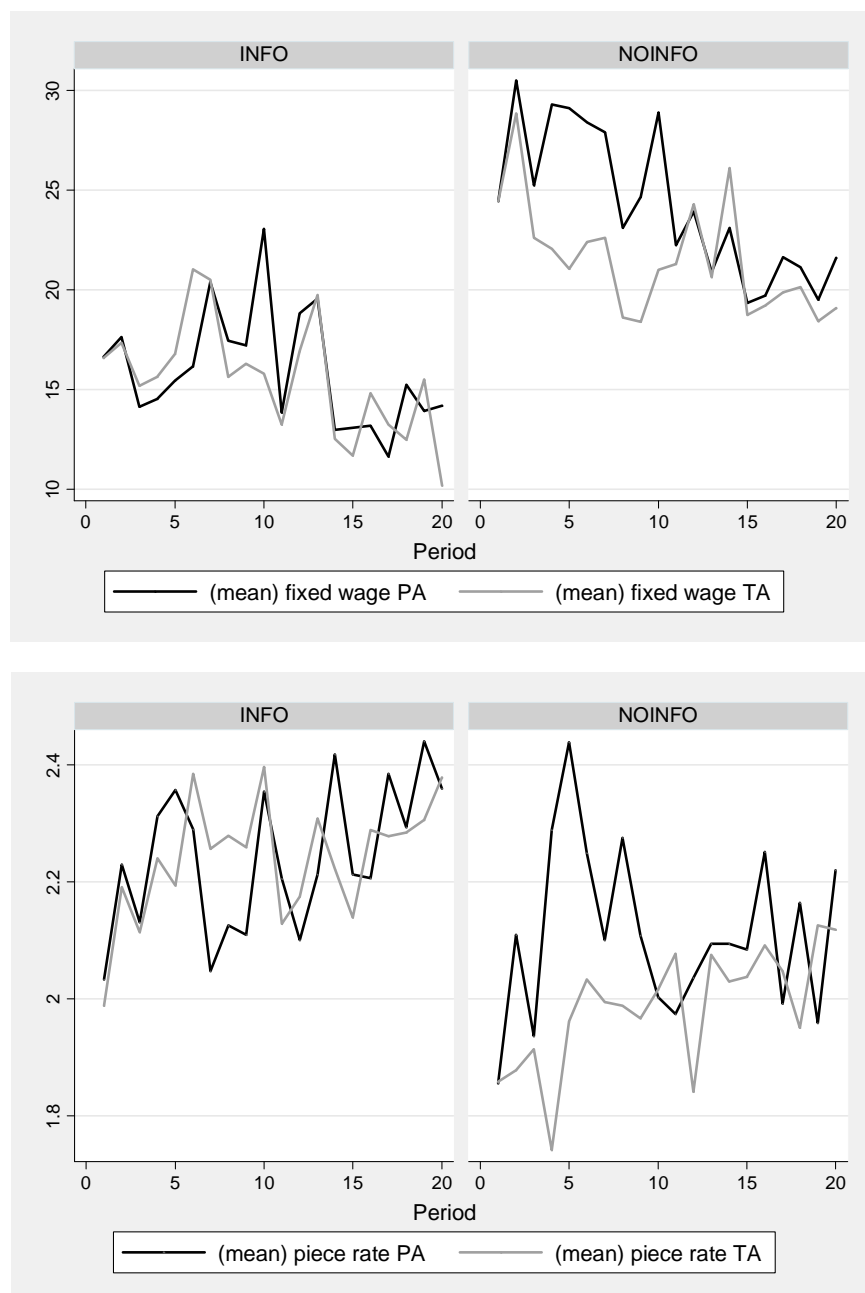
		Fixed wage		Piece rate		Actual effort		Desired effort	
		PA	TA	PA	TA	PA	TA	PA	TA
INFO	<i>Mean</i>	15.94	15.54	2.24	2.24	42.69	43.74	63.41	65.33
	<i>Median</i>	9.00	5.75	2.00	2.00	42.00	40.00	60.00	62.00
	<i>Std. Dev.</i>	21.36	21.09	0.70	0.67	18.64	15.67	21.14	22.14
NOINFO	<i>Mean</i>	24.23	21.48	2.11	1.99	40.09	38.79	48.18	49.28
	<i>Median</i>	20.00	19.50	2.00	2.00	40.00	40.00	50.00	50.00
	<i>Std. Dev.</i>	19.98	19.88	0.79	0.83	18.85	18.37	25.93	27.36

Note: Std. Dev. = standard deviation

¹⁰ All non-parametric tests in this paper are based on averages on the level of statistically completely independent matching groups. Tests are always two-sided.

Whereas the chosen piece-rate parameter s and exerted effort levels e are pretty close to the equilibrium solution (with $s^* = 2$ and $e^* = 40$), on average, the fixed wage w deviates significantly and persistently over the 20 periods from the theoretical prediction of zero in both treatments (Wilcoxon rank-sum test, $p = 0.002$). Although declining over time, it stays above 10 in INFO and above 15 in NOINFO even in period 20. Time trends are shown in Figure 1.

Figure 1: Time trends of compensation in the two treatments



On average over all periods, piece rate wages in INFO are significantly larger than 2 for both types of agents (Wilcoxon rank-sum test, $p = 0.040$). In NOINFO piece rate wages are higher than 2 for permanent agents (Wilcoxon rank-sum test, $p = 0.040$) and not different from 2 for temporary agents. In the following we will take a closer look at the discrimination of principals between the two agents on the level of a single principal.

4.1.2 Evidence of discrimination and its determinants

Table 2 provides evidence for discrimination against the temporary agent on the level of the individual principal. We distinguish between three different cases: (i) the permanent agent is treated favorably either in the fixed wage or the piece rate and not treated unfavorably in the other (i.e., $w_{PA} \geq w_{TA}$ and $s_{PA} \geq s_{TA}$ with one strict inequality); (ii) the temporary agent is treated favorably either in the fixed wage or the piece rate and not treated unfavorably in the other (i.e., $w_{PA} \leq w_{TA}$ and $s_{PA} \leq s_{TA}$ with one strict inequality); and (iii) both agents are treated equally (i.e., $w_{PA} = w_{TA}$ and $s_{PA} = s_{TA}$). Figures in Table 2 do not add up to 100% because there is a residual category.

Table 2: Discrimination by contract offers

	NOINFO	INFO
Permanent agent is favoured	143 (40%)	85 (24%)
Temporary agent is favoured	53 (15%)	46 (13%)
Equal treatment	111 (31%)	198 (55%)
Total decisions	360	360

Table 2 reveals that making contracts of co-workers public within firms leads to more equal treatment in terms of contracts. Whereas 55% of all contract offers for the two agents are identical in the treatment INFO, for the case without information about co-workers contracts, in treatment NOINFO, the relevant figure is only 31%. Without any horizontal information on contracts among agents, the permanent agent is favored in 40% of all cases. The distributions over the three categories in the two treatments are highly significantly different from each other (χ^2 -test; $p < 0.001$).

To take a closer look at the determinants of contracts chosen by principals, we linearly regress contract components on a treatment dummy for NOINFO (i.e., we take INFO as the reference treatment, also in later regressions), a period indicator and a dummy for the type of agent. Table 3 provides the results: Permanent agents receive significantly higher

fixed and piece rate wages than temporary agents. Principals require significantly less effort from permanent agents. In NOINFO fixed wages are higher and piece rate wages are lower than in INFO. More experienced principals offer higher piece rate wages and lower fixed wages.

Table 3: Determinants of contracts

<i>Dependent variable</i>	<i>Fixed wage</i>	<i>Piece rate</i>	<i>Desired effort</i>
<i>Independent variables</i>	<i>I</i>	<i>II</i>	<i>III</i>
NOINFO dummy	0.62*** (0.14)	-0.29* (0.16)	0.11 (0.16)
Period	-0.02*** (0.00)	0.01** (0.00)	0.01 (0.00)
Period 1 of phases 1, 2	-0.09 (0.06)	-0.16** (0.07)	-0.06 (0.06)
Permanent agent dummy	0.08** (0.03)	0.07* (0.04)	-0.08* (0.04)
Constant	-0.61*** (0.10)	0.11 (0.12)	-0.58*** (0.12)
N	1351	1351	1351

Note: Linear OLS regressions with individual- and matching group-specific fixed effects. The columns show standardized coefficient estimates with their corresponding standard errors in parentheses. The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level.

In Table 4, we directly investigate the determinants of discrimination. More specifically, we assess the propensity for discrimination by employing a logit regression where the dependent variable is equal to one if the fixed wage (piece rate) of agent i is lower than the fixed wage (piece rate) of agent j , her co-worker, and zero otherwise. Cases in which $w_i = w_j$ and/or $s_i = s_j$ are coded as zero. We, furthermore, control for time and, of course, dummies for the treatment and the decision maker in the role of permanent agent.

According to the results, the propensity for discrimination in fixed wages is significantly higher in NOINFO. Permanent agents are less likely to be discriminated against with respect to both fixed and piece rate wages, and the effect is mainly driven by the NOINFO treatment (see model IV). Discrimination is also less likely in the first period of a new phase in which both agents the temporary and the permanent agent do not have any history with the relevant principal.

Table 4: Determinants of discrimination

<i>Dependent variables</i>	<i>Propensity for discrimination in...</i>			
	<i>fixed wages</i>		<i>piece rate</i>	
<i>Independent variables</i>	<i>I</i>	<i>II</i>	<i>III</i>	<i>IV</i>
NOINFO dummy	0.85*	1.02**	0.32	0.61
	(0.46)	(0.50)	(0.42)	(0.44)
Period	-0.00	-0.00	0.02	0.02
	(0.01)	(0.01)	(0.01)	(0.01)
Period 1 in phases 1 and 2	-0.65**	-0.65**	-0.59**	-0.59**
	(0.26)	(0.26)	(0.29)	(0.29)
PA dummy	-1.00***	-0.78**	-0.81***	-0.45
	(0.21)	(0.32)	(0.19)	(0.28)
PA dummy * NOINFO	-	-0.37	-	-0.61*
		(0.41)		(0.37)
Constant	-1.02***	-1.12***	-1.13***	-1.31***
	(0.38)	(0.39)	(0.35)	(0.35)
N	1440	1440	1440	1440

Note: Logit random effects regressions with individual-specific random effects and matching group-specific fixed effects. The columns show standardized coefficient estimates with their corresponding standard errors in the subsequent column. The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level.

In summary, principals treat the two agents differently in both treatments, but less so in the treatment with public information about contract offers. This result is consistent with our alternative hypotheses aH2. The average values of the piece rate wages correspond to the theoretical benchmark. However, principals offer fixed wages that are significantly above zero to both agents in order to induce higher levels of effort (consistent with aH1). Overall, we find support for our alternative hypotheses aH1 and aH2.

4.2 Behavior of agents and reciprocity

4.2.1 Determinants of effort choices

Table 1 shows that actual effort levels are, on average, close to the equilibrium prediction under standard assumptions of $e^* = 40$. However, note that the individually optimal effort choice is a function of the offered piece rate wage. Hence, one has to take contract offers into account when assessing the response of agents. This is done in Table 5. We regress actual effort on the three contract components, a dummy variable for treatment NOINFO, a time trend and four dummy variables for equality and discrimination in fixed and piece rate wages, interacted with INFO, since only in INFO participants can compare contract conditions. Discrimination in fixed (piece rate) wages is captured by a dummy variable that takes the value of one, if the fixed wage (piece rate) of an agent i is lower than the

fixed wage (piece rate) of the co-worker agent j and zero otherwise. Both i and j can be permanent and/or temporary agents. The dummy for equality in the fixed wage (piece rate) takes the value one, if wages are the same and zero otherwise. Panel A of Table 5 presents the regression results for permanent agents and panel B for temporary agents.

Table 5: Determinants of effort choice and reciprocity

Panel A – Permanent agents:

<i>Dependent variable: actual effort</i>	<i>I</i>	<i>II</i>	<i>III</i>	<i>IV</i>	<i>V</i>
Fixed wage (w_{PA})	0.20*** (0.03)	0.19*** (0.03)	0.19*** (0.03)	0.19*** (0.03)	0.19*** (0.03)
Piece rate wage (s_{PA})	0.70*** (0.02)	0.68*** (0.03)	0.68*** (0.03)	0.70*** (0.03)	0.70*** (0.03)
Desired effort level (\tilde{e}_{PA})	-	0.05 (0.03)	0.04 (0.03)	0.05 (0.03)	0.05 (0.03)
NOINFO dummy	-	-	0.05 (0.28)	-	0.57** (0.28)
Period	-	-	0.01 (0.00)	0.01 (0.00)	0.01 (0.00)
Period1 in phases 1, 2	-	-	0.05 (0.08)	0.06 (0.08)	0.06 (0.08)
$w_{PA} - w_{TA}$ (if < 0) dummy * INFO	-	-	-	-0.04 (0.12)	-0.04 (0.12)
$w_{PA} = w_{TA}$ dummy * INFO	-	-	-	0.11 (0.10)	0.11 (0.10)
$s_{PA} - s_{TA}$ (if < 0) dummy * INFO	-	-	-	0.18 (0.13)	0.18 (0.13)
$s_{PA} = s_{TA}$ dummy * INFO	-	-	-	-0.02 (0.10)	-0.02 (0.10)
Constant	0.07 (0.20)	-0.08 (0.20)	-0.14 (0.21)	-0.23 (0.22)	-0.57*** (0.22)
<i>N</i>	720	720	720	720	720

Note: Linear random effects regressions with individual-specific random effects and matching group-specific fixed effects. The columns show standardized coefficient estimates with their corresponding standard errors in the subsequent column. The symbols *, **, *** indicate statistical significance at the 10%, 5%, and 1% level.

The effort choice of permanent agents is positively and significantly determined by fixed and piece rate wages with the impact of the piece rate coefficient being more than three times larger than the impact of the fixed wage coefficient. Note that the table provides standardized coefficients. Hence, the size of the coefficients is directly comparable. Permanent agents seem insensitive to differential treatment by principals, which is not surprising, since normally they are treated more favorably. Hence,

discrimination against them occurs infrequently and coefficients regarding discrimination are far from being significant.

Panel B – Temporary agents:

<i>Dependent variable: actual effort</i>	<i>I</i>	<i>II</i>	<i>III</i>	<i>IV</i>	<i>V</i>
Fixed wage (w_{TA})	0.12 ^{***} (0.02)	0.11 ^{***} (0.03)	0.12 ^{***} (0.03)	0.10 ^{***} (0.03)	0.10 ^{***} (0.03)
Piece rate wage (s_{TA})	0.79 ^{***} (0.02)	0.79 ^{***} (0.02)	0.79 ^{***} (0.03)	0.79 ^{***} (0.03)	0.79 ^{***} (0.03)
Desired effort level (\tilde{e}_{TA})	-	0.00 (0.03)	0.00 (0.03)	0.00 (0.03)	0.00 (0.03)
NOINFO dummy	-	-	0.48 ^{***} (0.17)	-	0.29 (0.21)
Period	-	-	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
Period 1 in phase 1, 2	-	-	-0.02 (0.07)	-0.03 (0.07)	-0.03 (0.07)
$w_{TA} - w_{PA}$ (if < 0) dummy * INFO	-	-	-	-0.18* (0.11)	-0.18* (0.11)
$w_{TA} = w_{PA}$ dummy * INFO	-	-	-	-0.16 (0.11)	-0.16 (0.11)
$s_{TA} - s_{PA}$ (if < 0) dummy * INFO	-	-	-	0.12 (0.12)	0.12 (0.12)
$s_{TA} = s_{PA}$ dummy * INFO	-	-	-	0.31 ^{***} (0.10)	0.31 ^{***} (0.10)
Constant	-0.21* (0.12)	0.23 ^{**} (0.12)	-0.26 ^{**} (0.12)	-0.41 ^{**} (0.18)	-0.09 (0.18)
<i>N</i>	720	720	720	720	720

Note: Linear random effects regressions with individual-specific random effects and matching group-specific fixed effects. The columns show standardized coefficient estimates with their corresponding standard errors in the subsequent column. The symbols *, **, *** indicate statistical significance at the 10%, 5%, and 1% level.

Similar to permanent agents, temporary agents respond positively to higher fixed and piece rate wages (see Table 5, Panel B). The difference in the impact of piece rate wages compared to fixed wages is also present and even larger in magnitude than for permanent agents. In contrast to permanent agents, temporary agents react more sensitively to discrimination. They “punish” principals by reducing their effort when being discriminated against in fixed wages, but only weakly significantly so, and “reward” principals for equal treatment in piece rate wages. The effects of such reward and punishment on effort exerted are larger in magnitude than the effect created by the fixed wage. Hence, discrimination

has a strong impact on effort provision. This is consistent with our alternative hypothesis aH3.

4.2.2 Voluntary cooperation by agents

Table 6: Propensity and intensity of voluntary cooperation by agents

Panel A – Permanent agents:

<i>Dependent variables</i>	<i>Propensity for voluntary cooperation</i>		<i>Intensity of voluntary cooperation</i>	
	I	II	III	IV
<i>Independent variables</i>				
Fixed wage (w_{PA})	0.53*** (0.11)	0.55*** (0.11)	0.23*** (0.08)	0.26*** (0.08)
Piece rate wage (s_{PA})	-0.16 (0.11)	-0.15 (0.11)	0.04 (0.08)	0.05 (0.08)
Desired effort level (\tilde{e}_{PA})	0.03 (0.12)	0.02 (0.12)	0.04 (0.08)	0.04 (0.08)
NOINFO-dummy	2.17*** (0.54)	-0.08 (0.72)	1.01** (0.49)	-0.996 (0.63)
Period	-0.003 (0.02)	-0.004 (0.02)	-0.01 (0.01)	-0.01 (0.01)
Period 1 of phases 1, 2	0.01 (0.31)	0.02 (0.31)	0.06 (0.21)	0.08 (0.22)
$w_{PA} - w_{TA}$ (if < 0) dummy * INFO	-	-0.30 (0.45)	-	0.12 (0.34)
$w_{PA} = w_{TA}$ dummy * INFO	-	0.06 (0.37)	-	0.20 (0.25)
$s_{PA} - s_{TA}$ (if < 0) dummy * INFO	-	0.01 (0.47)	-	0.03 (0.32)
$s_{PA} = s_{TA}$ dummy * INFO	-	-0.35 (0.36)	-	-0.33 (0.27)
Constant	-2.73*** (0.51)	-1.39** (0.66)	0.02 (0.42)	0.18 (0.54)
<i>N</i>	720	720	198	198

Note: Logit random effects regressions (I, II), and linear random effects regressions (III, IV) with individual-specific random effects and matching group-specific fixed effects. The columns show standardized coefficient estimates with their corresponding standard errors in parentheses. The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level.

Table 6 provides evidence on the propensity and the intensity of voluntary cooperation by agents. Agents voluntarily cooperate with the principal when they choose effort levels that are higher than optimal, for the offered piece rate and the individual cost parameter. The variable “propensity for voluntary cooperation” takes the value one, if actual effort is higher than optimal effort and zero otherwise. “Intensity of voluntary cooperation” is the

positive difference between actual and optimal effort (given a specific piece rate). Again we run separate regressions for permanent agents (Table 6, Panel A) and temporary agents (Table 6, Panel B).

Panel B – Temporary agents:

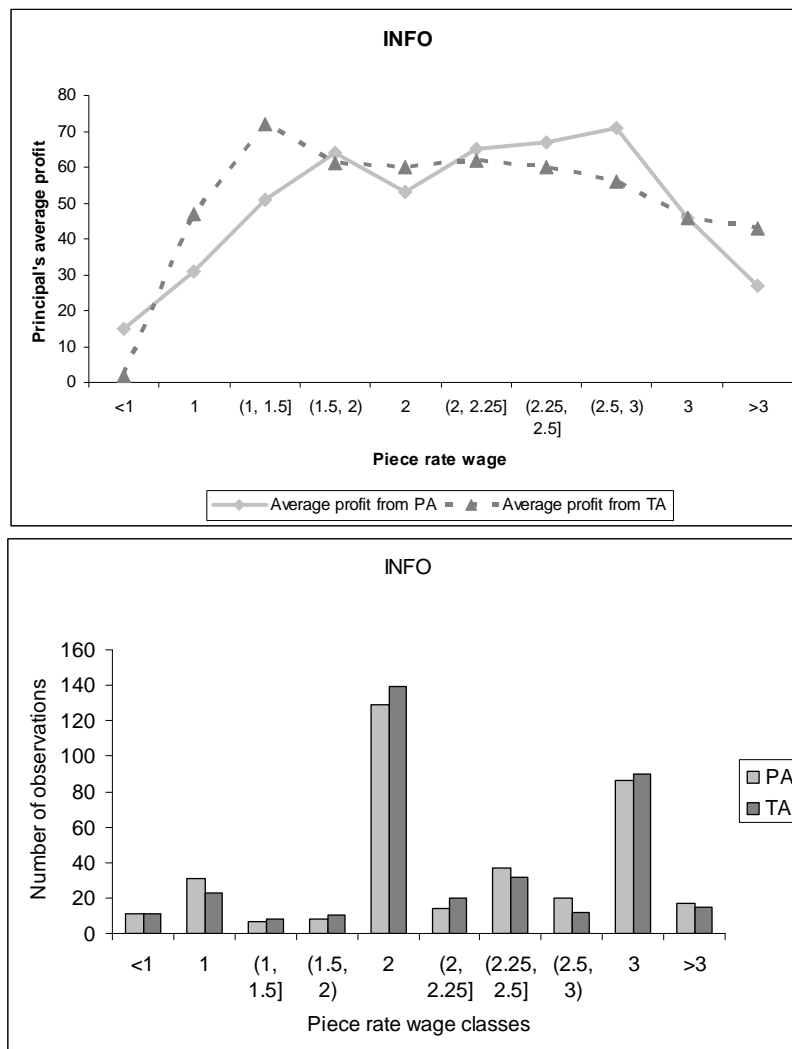
<i>Dependent variables</i>	<i>Propensity for voluntary cooperation</i>		<i>Intensity of voluntary cooperation</i>	
<i>Independent variables</i>	<i>I</i>	<i>II</i>	<i>III</i>	<i>IV</i>
Fixed wage (w_{TA})	0.48*** (0.12)	0.45*** (0.12)	0.10 (0.09)	0.10 (0.09)
Piece rate wage (s_{TA})	0.02 (0.11)	0.03 (0.11)	-0.09 (0.09)	-0.09 (0.09)
Desired effort level (\tilde{e}_{TA})	-0.02 (0.12)	-0.01 (0.12)	0.00 (0.09)	-0.01 (0.09)
NOINFO-dummy	22.58 (14.48)	-0.04 (0.94)	0.23 (0.61)	0.43 (0.78)
Period	-0.00 (0.02)	-0.01 (0.02)	-0.01 (0.02)	-0.01 (0.02)
Period 1 of phases 1, 2	0.31 (0.31)	0.31 (0.31)	-0.26 (0.24)	-0.26 (0.25)
$w_{TA} - w_{PA}$ (if < 0) dummy * INFO	-	-0.82 (0.51)	-	0.10 (0.41)
$w_{TA} = w_{PA}$ dummy * INFO	-	-0.03 (0.46)	-	0.08 (0.38)
$s_{TA} - s_{PA}$ (if < 0) dummy * INFO	-	0.07 (0.50)	-	-0.09 (0.43)
$s_{TA} = s_{PA}$ dummy * INFO	-	-0.33 (0.46)	-	0.14 (0.38)
Constant	-23.89 (14.48)	-1.23 (0.80)	-0.49 (0.47)	-0.71 (0.66)
<i>N</i>	720	720	186	186

Note: Logit random effects regressions (I, II), and linear random effects regression (III, IV) with individual-specific random effects and matching group-specific fixed effects. The columns show standardized coefficient estimates with their corresponding standard errors in parentheses. The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level.

Fixed wages have a positive effect on the probability for cooperation of permanent agents (see Panel A). Further, permanent agents are more likely to cooperate in treatment NOINFO. As can be seen in Panel B, fixed wages also influence the propensity for cooperation of temporary agents. All other variables here are not significant.

4.3 Determinants of profits

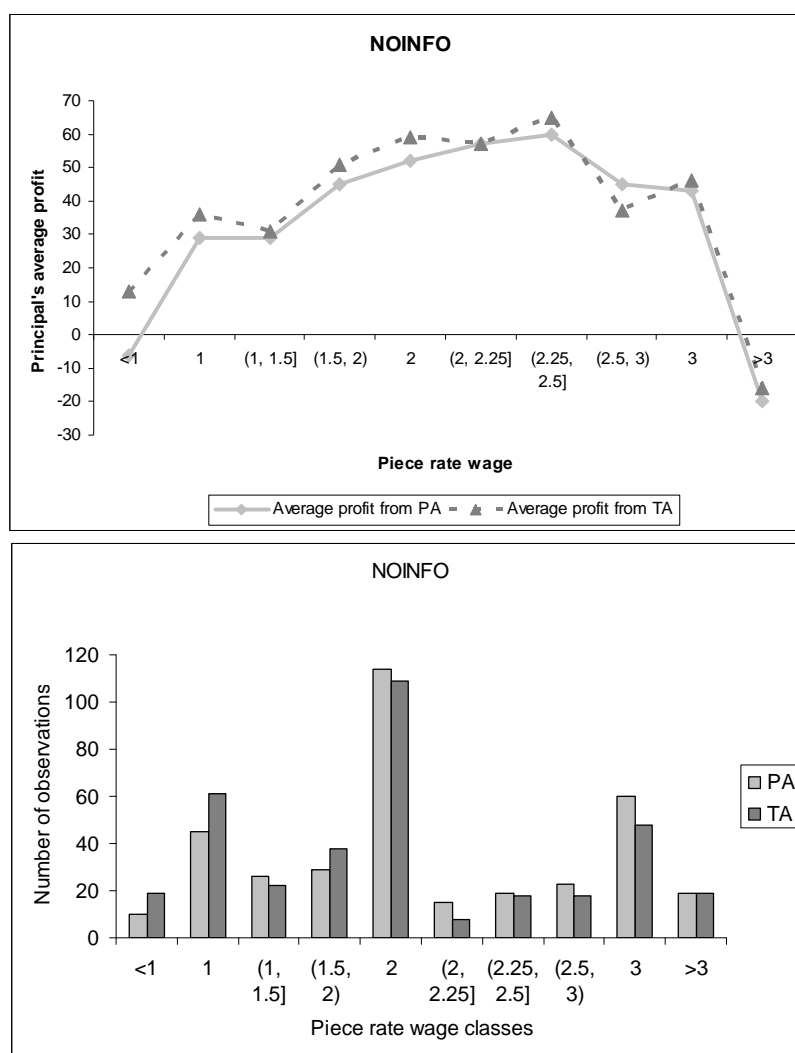
Figure 2 – Panel A: Piece rate wage with principals’ profits (upper part) and number of observations (bottom part) in treatment INFO



We finally take a look on behaviorally optimal contracts, i.e. what principals should have done, given the actual response of agents. Not surprisingly, the ex-post optimal piece-rate wage is around 2, which is the theoretical prediction under standard assumptions. Interestingly, the ex-post payoff function of principals around this prediction is quite flat, i.e., negative or especially positive deviations from the optimal contract design do not decrease actual payoffs as strongly as expected for sequentially rational agents. Note that some of the categories contain a small number of observations; hence, the small spikes should not be over-interpreted. However, in treatment INFO piece rates above 2 offered to permanent agents seem to be more beneficial than the optimal contract. Figures 2 and 3 as

well as Table 7 provide an overview of the average profits of principals, contingent on fixed wages and piece rates for the two types of agents.

Figure 2 – Panel B: Piece rate wage with principals’ profits (upper part) and number of observations (bottom part) in treatment NOINFO



In both treatments it would have been optimal for principals to offer a fixed wage of zero. However, the average profit of principals is rather constant up to a fixed wage of 20. Obviously, principals faced both reciprocal and non-reciprocal agents, but the number of reciprocal agents was not large enough to render a positive fixed wage profitable ex post. Nevertheless, many principals chose higher fixed wages, especially in the NOINFO treatment (see Figure 3, Panel B). In this treatment, the distinction between reciprocal and non-reciprocal agents becomes apparent: there are more observations of permanent agents

receiving a fixed wage of zero than temporary agents, namely those permanent agents that have not responded reciprocally before. Conversely, there are many more permanent than temporary agents who receive a fixed wage > 20 . These are the pairs of principals and permanent agents that have established a reciprocal relationship.

Figure 3 – Panel A: Fixed wage with principals' profits (upper part) and number of observations (bottom part) in treatment INFO

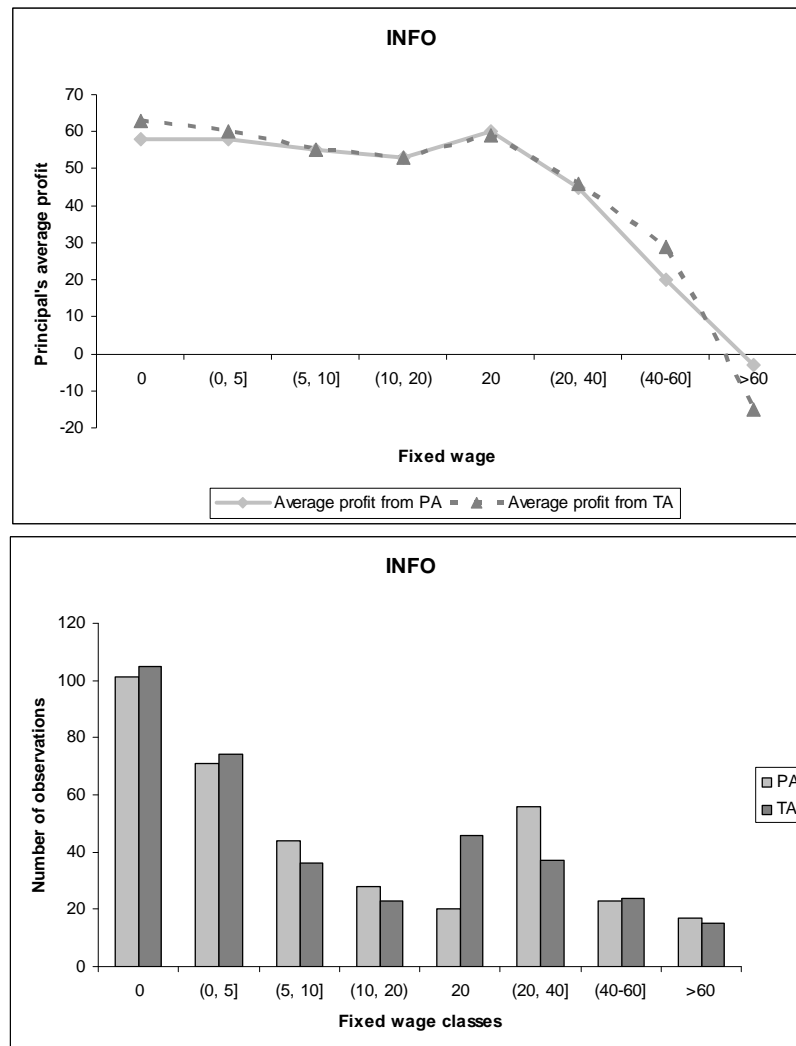
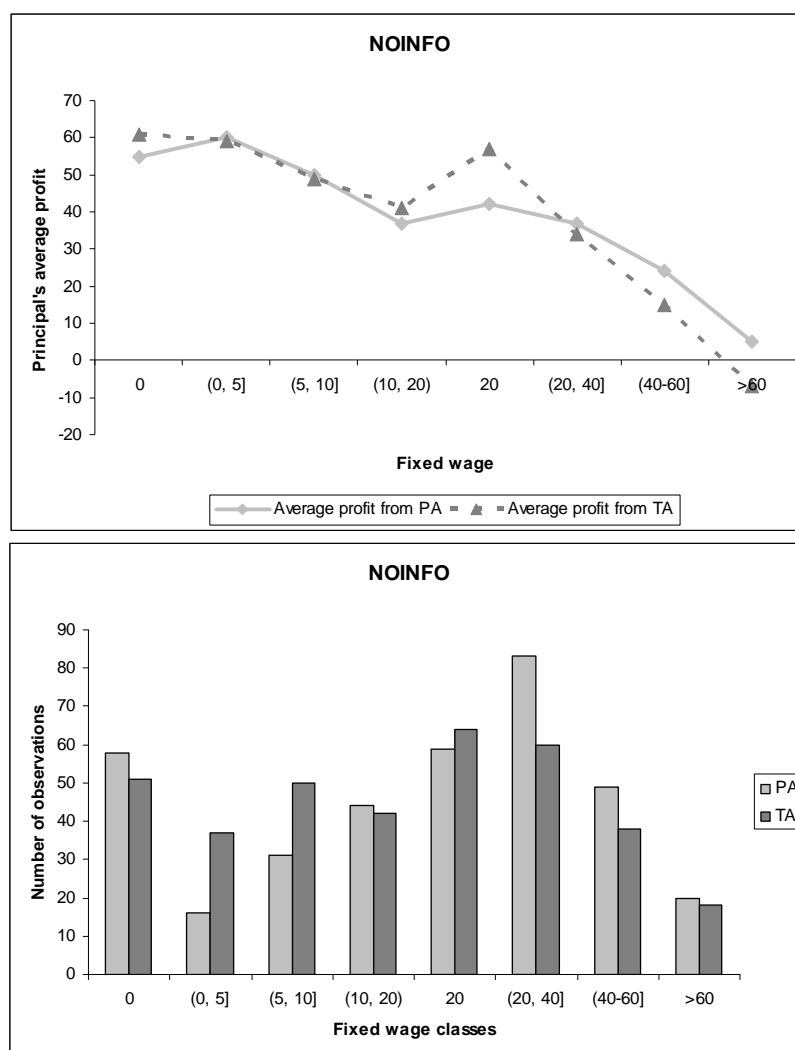


Figure 3 – Panel B: Fixed wage with principals’ profits (upper part) and number of observations (bottom part) in treatment NOINFO



Regressions in Table 7 confirm these results. In addition, they show that principals who pay different piece rates for the two agents in the INFO treatment earn significantly less *ceteris paribus* from their interaction with temporary agents. In other words, temporary agents react negatively to discrimination, and the reaction is sufficiently strong to reduce the profit of the principal significantly, both for a discrimination dummy as well as for the degree of discrimination as independent variables (see models III and IV).

In summary, principals would have fared better, on average, by offering the theoretically optimal contract. However, this does not preclude that very profitable reciprocal relationships emerged between principals and (permanent) agents. Most

importantly, discrimination between permanent and temporary agents leads to a reduction of principals' profits in the INFO treatment, everything else equal. However, the effect is not significant for discrimination in fixed wages.

Table 7: Determinants of profits of principals

Principal profits per period from interaction with permanent and temporary agent

<i>Dependent variable: profit of principal</i>	<i>From interaction with permanent agent</i>		<i>From interaction with temporary agent</i>	
	<i>I</i>	<i>II</i>	<i>III</i>	<i>IV</i>
<i>Independent variables</i>				
Fixed wage permanent agent	-0.48 ^{***} (0.05)	-0.47 ^{***} (0.05)	-	-
Piece rate wage permanent agent	-0.15 ^{***} (0.04)	-0.15 ^{***} (0.04)	-	-
Desired effort level permanent agent	0.11 ^{**} (0.05)	0.10 ^{**} (0.05)	-	-
Inequality of fixed wages dummy *INFO dummy	-0.24* (0.13)	-	-0.12 (0.13)	-
Inequality of piece rate wages dummy *INFO dummy	-0.13 (0.13)	-	-0.24* (0.13)	-
NOINFO dummy	0.52 (0.33)	0.88 ^{***} (0.32)	-1.13 ^{***} (0.31)	-0.15 (0.30)
Period	0.01 ^{**} (0.01)	0.01 ^{**} (0.01)	0.02 ^{***} (0.01)	0.02 ^{***} (0.01)
Period 1 of phases 1, 2	0.07 (0.11)	0.07 (0.10)	-0.01 (0.10)	-0.00 (0.10)
Inequality of fixed wages (absolute value) * INFO dummy	-	-0.03 (0.05)	-	0.01 (0.05)
Inequality of piece rate wages (absolute value) * INFO dummy	-	-0.15 ^{***} (0.05)	-	-0.14 ^{***} (0.05)
Fixed wage temporary agent	-	-	-0.68 ^{***} (0.05)	-0.68 ^{***} (0.05)
Piece rate wage temporary agent	-	-	-0.15 ^{**} (0.04)	-0.15 ^{***} (0.04)
Desired effort level temporary agent	-	-	0.06 (0.05)	0.06 (0.05)
Constant	-0.70 ^{***} (0.24)	-0.86 ^{***} (0.24)	0.41* (0.24)	-0.58 ^{***} (0.22)
<i>N</i>	720	720	720	720

Note: Linear random effects regressions with individual-specific random effects and matching group-specific fixed effects. The columns show standardized coefficient estimates with their corresponding standard errors in the subsequent column. The symbols *, **, *** indicate statistical significance at the 10%, 5%, and 1% level.

5 Discussion and conclusion

Although co-employment of permanent and temporary workers becomes more and more common, possibly generating new and stronger intra-firm conflicts, we are not aware of other attempts to study experimentally the effects of parallel short-term and long-term relationships in a principal-agent setting. In order to explore the impact of horizontal contract transparency in a setting where a principal faces one permanent and one temporary agent, we compare a situation in which agents know the contract details of both with a condition in which this is not the case.

The experimental implementation of our setup provides several results. First, we find that principals discriminate between the two types of agents, even though the benchmark solution under standard assumptions implies no discrimination. On average, permanent agents in the treatment without disclosure of contract details receive significantly higher fixed wages and significantly higher piece rate wages than temporary agents. Second, making contracts of co-workers public within firms before effort determination leads to more equal treatment of agents, i.e., less discrimination. Third, agents react to unfair treatment by principals. They “punish” principals for being discriminated, on average, and “reward” principals for equal treatment in piece rate wages. Fourth, discrimination between permanent and temporary agents, on average, leads to a reduction of principals’ profits in the treatment with disclosure, everything else equal. Fifth, in the spirit of gift-exchange, the fixed wage rather than the piece rate is the contract component by which principals trigger reciprocity, as in experiments where no piece rate is available.

It is important to remember that discriminatory contracts in our setup are neither a consequence of money-maximizing behavior nor a consequence of a taste for discrimination. They exist because of reciprocal behavior together with reputational concerns. The number of reciprocal agents in the experiment is obviously too small to induce principals to offer generous contracts in one-shot interactions. Hence, temporary agents, on average, receive worse offers than permanent agents for whom reputational concerns in the repeated interaction suggest reciprocal behavior even of completely selfish agents (see Kreps et al. 1982). This effect is attenuated by information disclosure within the firm, probably because of a distaste of reciprocal agents for discrimination or the expectation of principals regarding such distaste.

Two practical implications could be mentioned: First, our results provide an additional explanation for why temporary workers are paid less in companies than permanent workers. Principals invest less in the voluntary cooperation (reciprocity) of temporary workers. Second, information disclosure seems a good strategy against discrimination in such settings. If one wants to reduce discrimination, a policy of transparency could do the trick, at least to a considerable extent.

In contrast to Charness and Kuhn (2007) our results show that co-workers contract details matter quite a bit. However, this difference in findings is not surprising. In our case, co-worker's contract details are much more salient than in their case. Discrimination is obviously unfair, and agents are willing to "punish" contract structures that they perceive as unfair and "reward" contract structures they perceive as fair. Regarding the effects of unequal treatment of workers within the same firm, our results are in accordance with those of Kocher et al. (2009), who show that unequal treatment in a principal-agent framework with incomplete contracts leads to lower levels of wages, effort and efficiency than equal treatment, when equal treatment is feasible. Here, we show that discrimination has a small, but negative effect on principals' profits.

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Appendix: Experimental instructions (Originally in German; not necessarily for publication; for referees' convenience)

Experimental instructions

Welcome and thank you for participating in this experiment. For having shown up on time you will receive 2.50 euros. If you read these instructions carefully, you may earn more. In this experiment all amounts are denominated in ECU („experimental currency units“). In the end of the experiment earnings will be converted at the exchange rate $200 \text{ ECU} = 1.00 \text{ euro}$ and paid to you in cash. During the experiment, you are not allowed to talk to other participants. Whenever you have questions, please raise your hand. An experimenter will come to you and answer your questions individually. If you do not obey these rules, we will have to exclude you from the experiment. No payment will be provided in this case.

The setting

In the experiment, three participants interact in a labor market environment. These three include a producer P , a worker of type A , and a worker of type B . At the beginning of the experiment, you will be assigned one of the above roles. You will keep the given role during the entire experiment. Altogether, the experiment comprises two phases, each including ten periods. At the beginning of each phase, you will be randomly matched with two other participants to form a group of size three. Producer P and worker A interact with each other throughout an entire phase, i.e., during the whole ten periods. To the contrary, worker B is randomly re-matched with another producer P and worker A in every period. Note that it is possible that B encounters the same P and A whom he/she has encountered before.

Your task

Your task in the experiment depends on the role that is assigned to you. You will be informed about your actual role when the experiment starts. Even though you will be assigned to one particular role, we urge you to carefully study the characteristics of all four roles as this may help you to perform well in economic terms in the experiment.

Producer P

You are in charge of a firm in the manufacturing business which produces finished goods that are sold at the market. Both the market price and the demand for your goods are exogenously determined and will remain constant during the entire experiment. The price of the product is 4.00 ECU. Since your firm is considered to be small, you are always able to sell your entire production.

To manufacture goods, you rely on labor which you can contract by directly hiring worker *A* and/or worker *B*. You may hire a worker by proposing a contract in which you can specify a (non-negative) fixed wage component and a piece rate wage component. Additionally, you can (but don't have to) specify the amount of work effort you wish worker *A* and/or *B* to exert in case of their employment.

Please note that the amount of goods produced depends on the work effort workers *A* and/or *B* exercise, if you contracted them to work for you. If you contract neither one, your production will be zero and you will not realize any profit. Increasing production translates into increasing revenues from which your costs (both fixed and piece rate) for contracting *A* and/or *B* are deducted. Be aware of the risk that if your costs surpass your revenues, you will realize a net loss in the given period.

Your payoff in each period is as follows:

$$\begin{aligned}
 \text{Revenue} &= \text{price of the product} * (\text{work effort } A + \text{work effort } B) \\
 \text{Costs} &= \text{fixed wage } A + \text{piece rate wage } A * \text{work effort } A \\
 &+ \text{fixed wage } B + \text{piece rate wage } B * \text{work effort } B \\
 \text{Profit } P &= \text{revenue} - \text{costs}
 \end{aligned}$$

Worker type A

Producer *P* proposes a contract to you that comprises a (non-negative) fixed and/or a piece rate wage component. You receive the fixed wage component irrespective of your exerted effort. On the contrary, you only benefit from the piece rate wage component if you chose to exert work effort. The effort level stands for the number of goods that you produce in a given period. *P* might inform you about the amount of work effort he/she wishes you to exert. However, you do not have to follow this recommendation.

Note that work effort is not for free. Increasing effort levels translate into progressively increasing effort costs. For your convenience, during the training phase effort costs will be immediately posted on your computer screen, once you have entered a particular effort level.

Your payoff in each period is as follows:

$$\begin{aligned}
 \text{Profit } A &= \text{fixed wage} \\
 &+ \text{piece rate wage} * \text{work effort} \\
 &- \text{costs for work effort}
 \end{aligned}$$

Worker type *B*

The rules that hold for worker *A* also apply for you. However, there is one exception: while worker *A* interacts for one phase – thus ten periods – with the same producer, you will be assigned to a (possibly) new producer in every period.

Your payoff in each period is as follows:

$$\begin{aligned} \text{Profit } B &= \text{fixed wage} \\ &+ \text{piece rate wage} * \text{work effort} \\ &- \text{costs for work effort} \end{aligned}$$

Course of events

In each of the experiment's twenty periods there is the same course of events. At first, *P* proposes a contract to *A* and/or to *B*. [*Participants in INFO read:* Subsequently, *A* and *B* are informed about the own contract specifications and those of the other worker.] [*Participants in NOINFO read:* Subsequently, *A* and *B* are informed about the own contract specifications.] Finally, each worker decides how much effort he/she wants to exert. Depending on the chosen work effort and the particular contract specifications, the profits for *P*, *A*, and *B* are computed. At the end of the period, you will be informed about your own payoff. Only producer *P* is additionally informed about the work effort chosen by workers *A* and/or *B*.

Your payoff at the end of the experiment

Your final payoff is the sum of your twenty periodic payoffs plus the show-up fee of € 2.50. If you conclude the experiment with a negative final payoff, we will ask you to do some administrative work for one hour at the Max-Planck-Institute of Economics in Jena.

Questionnaire and Training

Before the experiment starts, we kindly ask you to complete the questionnaire that is displayed on your computer screen. The questions are asked to ensure that you have fully understood the rules of the experiment. After completing the questionnaire, you will have the possibility to familiarize yourself with the experiment during ten training periods. During this time, you will act alone. Hence, you will not be informed about the decisions of other participants and vice versa. Note that the training periods will not affect your final payoff.