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IN SEARCH OF WORKERS' REAL EFFORT RECIPROCITY – A FIELD AND A LABORATORY EXPERIMENT

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Abstract

We present a field experiment to assess the effect of own and peer wage variations on actual work effort of employees with hourly wages. Work effort neither reacts to an increase of the own wage, nor to a positive or negative peer comparison. This result seems at odds with numerous laboratory experiments that show a clear own wage sensitivity on effort. In an additional real-effort laboratory experiment we show that explicit cost and surplus information that enables to exactly calculate employer's surplus from the work contract is a crucial pre-requisite for a positive wage-effort relation. This demonstrates that employee's reciprocity requires a clear assessment of the surplus at stake.

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1. Introduction

In efficiency wage models, a wage increase in excess of the market clearing wage can induce an increase in the productivity of labor and, thus, be beneficial to a profit-maximizing firm. The perhaps most popular explanation for the positive correlation of wage and productivity is the fair wage-effort model, in which workers increase (reduce) the effort exerted, if the wage paid is greater (smaller) than the wage they perceive as the fair wage (Akerlof 1982; Summers 1988; Akerlof and Yellen 1990). The original reasoning for such reciprocal behavior of workers is based on the notion of cognitive dissonance reduction (Adams and Rosenbaum 1962; Akerlof 1982) which basically introduces a disutility from being over-paid or under-paid (i.e. receiving a wage above or below the perceived fair wage, respectively). To avoid the disutility of cognitive dissonance, workers can either adjust their beliefs concerning the fair wage (Adams 1963) or adjust their effort level in order to align the fair and the received wage (Akerlof 1982). To complicate matters even further, a number of different parameters have been suggested in the literature that may influence the wage level that is perceived as fair. These include the own past wage, the wage of other individuals in a "reference set" (e.g. a peer group or a group of superiors), and the general work environment. However, the extent, the direction, and the interplay of these factors all remain open issues for empirical research.

In numerous experiments, work situations have been modeled where subjects are assigned to the roles of the employer and the employees, respectively, and effort exertion is simulated by effort level choices. These studies (see Fehr, Kirchsteiger Riedl 1993, Fehr, Kirchler, Weichbold, Gächter 1998, Fehr and Falk 1999, Charness 2004) provide the robust observation that an increased wage is reciprocated by an increased effort. Fahr and Irlenbusch (2000) and Gneezy (2004) identify a positive wage effort relation also in laboratory experiments with real effort exertion.[1] In this paper, we report a field and a laboratory experiment that extend these studies in two dimensions. First, we increase the authenticity of the work situation by experimenting with employees who receive real work contracts, produce valuable output and perform natural work tasks.[2] Second, we systematically vary the peer

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[1] Charness, Fréchette, and Kagel (2004) report significantly reduced levels of employee reciprocity in a setup in which detailed payoff tables replace the typical payoff function representation. Gneezy and List (2005) report an experiment which casts doubt on the long term persistence of a positive wage effort relation. We will return to these studies in the discussion at the end of this paper.

[2] There are a number of other studies using field experiments in a labor context. Erev, Bornstein and Galili (1993) compare piece-rate payments in an inter-group competition. Dickinson (1999) and Fehr and Götte (2002) study labor supply under variation of piece rate wages. Harrison and List (2004) propose a taxonomy of field

wage and the employer's surplus information to identify which information is crucial for observing a positive wage-effort relation.

The results of our field experiment are surprisingly clear. We find no treatment effects whatsoever. Neither wage increases nor information on peer wages have a significant influence on the workers' effort. These results seems at odds with the numerous laboratory experiments that show a positive wage-effort relationship. To address the question, whether the information on employer's cost and surplus, which is available in the laboratory experiments, but is absent in the field, is the cause for our negative findings, we conducted an additional laboratory experiment.[3] In the laboratory experiment, we study a similar work situation as in the field experiment with real – not simulated – effort exertion and a variation of the wages. The extension of our design compared to the existing literature is the controlled variation of information on the employer's surplus from the work relation. This allows us to assess the impact of the availability of employer surplus information on employee's reciprocal behavior. Previous work indicates that asymmetric information may have a negative impact on cooperation. Mitzkewitz and Nagel (1993), for example, show that rate of acceptance in ultimatum games is significantly affected by asymmetric information on the size of the cake. Irlenbusch and Sliwka (2005) show that efficiency in gift exchange games drops when the effort choice of the employee cannot be perfectly observed by the employer.

Our laboratory experiment gives support to the conjecture that a positive wage-effort relationship is only observed in the presence of employer surplus information. Comparing all treatments from both our experiments, we find that only in the case of full information, wage increases can be profitable as predicted by the fair wage-effort hypothesis. We conclude that mutual surplus information is crucial for reciprocal employment relationships in real effort settings. We postpone the discussion of our results and the comparison to the literature to the concluding section of this paper.

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experiments and provide an overview over the recent studies. Some studies use data from "natural experiments," i.e. from variations that occur in the field without the manipulation by an experimenter. Camerer, Babcock, Loewenstein, and Thaler (1997), for example, analyze cabdrivers' labor supply with varying demand conditions.

[3] We are grateful for the discussion by Ernst Fehr and Reinhard Selten at the ENDEAR Workshop 2003, Amsterdam, in which the idea was expressed that employer surplus information may be decisive for the degree of observed employer reciprocity.

2. A field experiment with real effort

In the field experiment, student workers were hired by a research institution of the University of Bonn (Sonderforschungsbereich 303). The subjects performed a real work task that required a substantial amount of effort. They typed the abstracts of research papers that were not yet available electronically into an internet database in order to enhance the accessibility of research output.[4] The work environment was a typical university office. Student workers were contacted by a project manager who was neither a student nor part of the local staff[5]. In total, 103 students of the University of Bonn, most of them with majors in law or economics, were recruited for our field experiment.[6]

Each subject had to work for two hours on two separate occasions of one hour each. The two working hours were approximately one month apart. On the first occasion, the typists were asked to arrive 20 minutes before the work hour in order to receive instructions and a brief training. The instructions (ca. 10 – 15 minutes) not only explained the technical details of the job, but also explained the value of the work for the university[7]. It was made absolutely clear that the job was to be taken very seriously. No reference was made to the fact that wages in some treatments would be varied in the second occasion. The instructions also explained that the project was being used for a pilot study on the division of work load.

Following the instructions, each typist went through a brief training (ca. 5 – 10 minutes) that consisted of typing an abstract similar to those actually used in the data set.[8] Immediately following the training, each typist received a folder containing the abstracts, keywords, and JEL-classification numbers that had to be entered, and the first hour of work commenced. After one hour sharp, the project coordinator terminated the first session, making sure that all data was saved. On the second occasion, no further instructions were given and the second hour of work proceeded in the same way as on the first occasion.

Each typist performed the job alone in a typical university office on a typical office computer. In addition to the normal items found in such an office, some up-to-date

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[4] Abstracts of the papers that were still available at the time of the experiment can be found at http://www.wiwi.uni-bonn.de/sfb303/sonderforschungsbereich.eng.html

[5] We employed a primary school teacher, who had just graduated and who had no obvious connection to the laboratory of experimental economics. None of the experimenters had contact to the typists.

[6] There were 42 female and 61 male typists.

[7] The instructions are contained in Appendix B1. It was explained to the typists that they would be able to see the results of their work (i.e. the typed abstracts) on the institute's research website.

[8] All typists were trained using the same abstract.

newspapers and magazines were placed in the room. Furthermore, the computer was configured in such a way that the icons for some of the standard computer games provided by the operating system were placed close to the icons for the abstract typing software. During the working hours the typists were left unobserved. The typists knew that the project coordinator was next door, but since they were told to call the project coordinator in case of problems, the setup provided no indication of unexpected controls during the work hour.

Typists were promised a flat fee of DM 10 ($\approx$ Euro 5) for showing up on time, plus an hourly wage of DM 20 for each hour of typing. This hourly wage was slightly above average in the typical wage distribution for students' jobs, which at the time was in the range from DM 15 to DM 22. The typists were told they would receive their payment only after having completed both working hours. In total, subjects were paid a flat fee of DM 50 to DM 58 depending on the treatment.

2.1. Treatments of the field experiment

For the first hour of work, the typists in all treatments received a previously announced wage, which was in the range of wages for comparable student jobs. Before the second hour of work, the typists – in some treatments – were informed about an unexpected pay raise and – in some treatments – about the pay raise of a peer group. To avoid contamination, the second work hours were scheduled after all subjects had completed their first work hour. This left almost one month time between the first and the second work hour for most subjects, which made the variations in wages more plausible. In those treatments, in which the wage was varied, the project coordinator informed each typist of the new wage before the typist started to work. The wording used by the project coordinator to inform the typists was fixed by the experimenters beforehand and always presented exactly in the same manner.

Treatment F0 was our control treatment in which the typists received no pay raise and no information on peer groups. In treatment F10, typists received a 10 percent pay raise, but received no information on peer groups. In treatment F40$_{peer}$, typists received a 40 percent pay raise and were informed that the typists in the previous group F10 had received a lower pay raise of 10 percent. Finally, the group F10$_{peer}$ received a 10 percent pay raise and was informed that the previous group F40$_{peer}$ had received a substantially higher pay raise of 40 percent. Over all treatments, we had 103 independent observations. Table 1 summarizes the four treatments of our field experiment.

Note that we control for subjects' fixed effects. The within-subject design ensures that individual differences (e.g. in typing capabilities) are cancelled out by using each subject's first-hour performance as a control for the second-hour performance. Since all subjects were treated equally in the first hour, any cross-treatment difference in the change of subjects' performance must be due to the corresponding treatment variation. If we, however, find no significant treatment differences concerning the change in performance, then we must conclude that the treatment variation has had no discernible effect on subjects' behavior.

Table 1. Treatments in the field experiment

Treatment	F0	F10	F40peer	F10peer
Show-up fee (in DM)	10	10	10	10
Wage 1st hour (in DM)	20	20	20	20
Wage 2nd hour (in DM)	20 (+0%)	22 (+10%)	28 (+40%)	22 (+10%)
Information on peer reference group	no	no	yes smaller increase of preceding group: +10%	yes greater increase of preceding group: +40%
Number of typists	24	25	23	31

2.2. Hypotheses for the field experiment

According to a positive wage-effort relationship we expect subjects in F10 to exhibit higher efforts than subjects in F0. The higher wage as well as the favorable peer comparison should lead to even higher efforts in F40peer compared to F10. If peer comparison plays a role, the unfavorable peer information in F10peer should lead to lower efforts than in F40peer and in F10, where the same pay increase is made, but without the peer comparison.

2.3. Results of the field experiment

We base our data analysis on two measures we observe for every typist: the usable output (i.e. the number of correctly typed words) and the erroneous output (i.e. the number of incorrectly typed words). Misspelled words and deviations from the original text (omissions or additions) were counted as errors. Multiple errors in the same word were counted as a single error. Two independent secretaries checked all the abstracts, counting the correct words as well as the errors. The counts were double-checked when necessary. Usable output is an immediate measure of *output quantity*, but it is insufficient as an indicator of productivity, because it

does not capture the cost of correcting erroneous production.[9] Thus, the evaluation of a typist's productive effort should also take into account the *output quality* (i.e. the ratio of usable output to total production).

Table 2 shows the averages of output quantity, error, and output quality observed in each working hour of each of the four treatments. Additionally, the average change of each measure from the first to the second working hour is reported. In all four treatments, we find an experience effect: Subjects' output quantity in the second work unit is higher than in the first unit. Errors, however, rise as well causing a decrease in output quality over time. All changes are not significant, though.

Table 2: Treatment averages in the field experiment

		F0	F10	F40peer	F10peer
Output quantity (usable output) (in words per minute)	1st hour	16.887	17.301	15.592	16.929
	2nd hour	17.521	17.453	16.051	17.211
	2nd minus 1st hour *	.634	.152	.459	.282
Errors (in words per minute)	1st hour	.373	.290	.268	.367
	2nd hour	.412	.300	.303	.379
	2nd minus 1st hour *	.040	.010	.035	.011
Output quality	1st hour	.978	.983	.984	.979
	2nd hour	.978	.982	.982	.978
	2nd minus 1st hour *	-.001	-.002	-.002	-.001

*) The reported average of all differences may slightly deviate from the difference of the averages, due to rounding errors.

To analyze the impact of wage increase and peer information, we base our investigation on the following measures, (1) output difference: second hour minus first hour quantity; (2) quality difference: second hour minus first hour quality. For each treatment, the vertical line in Figure 1 visualizes the range of output changes, and the horizontal line marks average output.

We find no treatment effects whatsoever.[10] The increase in own wage has no effect on the provided work effort measured by correct output or output quality. There is also no effect of the availability of peer wage information on work output, neither when the own wage is

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[9] There were real costs of correction, because the abstracts were corrected by secretaries before being published on the internet.

[10] The results of the statistical comparisons are given in Table A1 in Appendix A.

higher nor when it is lower than the peer group's wage.[11] Hence, as far as our student typists are concerned, increasing wages is not at all a profitable action for the employer. There also seems to be no need for wage compression (i.e. reducing the distribution spread of wages in the company) or for wage secrecy, both of which are sometimes believed to increase employer profits.
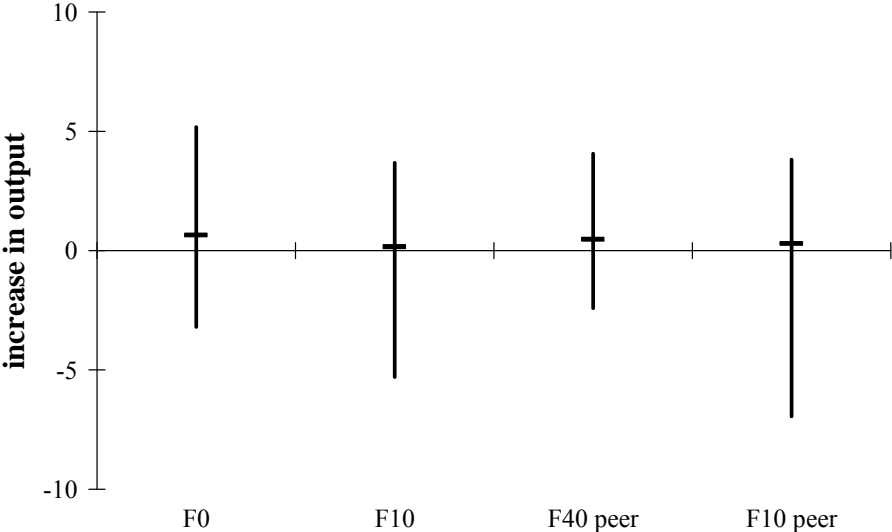

Figure 1: Increase of work output in the field experiment

3. A laboratory experiment with real effort

We conducted a laboratory experiment to analyze whether information about relevant model parameters in labor relations may be the cause for observed differences in exerted efforts. We study a real-effort work situation in the laboratory that closely resembles our field experiment's setup. In contrast to the field experiment, however, we control the information of the workers on the employer's surplus. We distinguish between a situation in which the

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[11] As a referee helpfully suggested, we could have also run an F40 treatment to check the effect of a strong own wage increase without a concurrent peer effect. Note, however, that we did not observe an increased reciprocal response in the F40peer treatment, in which both positive motives (strong own wage increase and positive peer wage comparison) work together. Hence, we had no reason to hypothesize that one of the two motives alone would have a strong effect and economized on the number of treatments.

employee is completely informed about the employer's surplus from work effort and a situation in which no such information is available.[12]

We implemented a work task that was simple to perform, that was physically not overly demanding, and that asked for no special knowledge. Moreover, we required the output of the work task to be easily measurable and to allow for considerable variance. Performing a similar task as in Falk and Ichino (2006), our subjects folded a letter and enveloped it.[13]

In total, 59 subjects participated in our laboratory experiment. The experiment was conducted at the Erfurt Laboratory for Experimental Economics (elab), University of Erfurt. Upon arrival, subjects were seated in separate cubicals containing a pile of letters, a pile of envelopes, and written instructions on the experiment (see appendix B.2).[14] After having read the instructions, a monitor demonstrated the work process and answered questions. The work task was divided into two units of 15 minutes each. Between the two units, there was a break of 5 minutes in which the monitor collected the filled envelopes. At the beginning of the second unit, subjects received the instructions for the second unit. After the experiment, subjects were paid their wage. Total average session time was 45 minutes.

3.1. Treatments of the laboratory experiment

Our research hypothesis on the influence of surplus information demands to study variations of the two treatments F0 and F10 of the field experiment in the laboratory experiment. Because we have no explicit hypothesis on peer comparisons, we do not replicate the related treatments.

In the baseline treatment L0, subjects were paid the same amount for the first and the second work unit, while in treatment L10 subjects received a wage increase of ten percent in the second work unit. Both treatments were conducted under two information conditions: no information (L0 and L10) and information on the employer's surplus from work effort (L0surplus and L10surplus). Table 3 summarizes the four treatments of our laboratory experiment.

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[12] Our distinction between the "field" and "laboratory" experiment is mainly based on the fact that subjects in the field experiment experienced a regular work environment, while the subjects in the lab experiment performed their task (which was also real and valuable) in a typical laboratory setting.

[13] Subjects were informed that the letter was part of an information mailing for an upcoming research conference.

[14] The formulation of the instructions was as close as possible to the instructions of the field experiment.

The information on the employer's surplus from the employee's work effort was implemented by exactly specifying the employer's opportunity costs. The instructions explicitly stated that the experimenter had to pay Euro 0.06 in case of outsourcing the task. This cost was determined in a pre-session in which students were asked to do the same work task. The unit cost of a letter was calculated based on the wage of university student employees.

Table 3. Treatments in the laboratory experiment

Treatment	L0	L10	L0surplus	L10surplus
Show-up fee	1.50 Euro	1.50 Euro	1.50 Euro	1.50 Euro
1st work unit wage (15 min)	2.50 Euro	2.50 Euro	2.50 Euro	2.50 Euro
2nd work unit wage (15 min.)	2.50 Euro (+0%)	2.75 Euro (+10%)	2.50 Euro (+0%)	2.75 Euro (+10%)
Information on employer's surplus	no	no	yes	yes
Number of subjects	10	10	17	19

To facilitate the calculation of the employer's surplus, the instructions of the treatments L0surplus and L10surplus included a table of the employer's average cost per letter, depending on the number of envelopes filled per work unit (see table 4). In addition, it was explicitly mentioned that, compared to outsourcing, the employer's break even was at a production of 41 (45) letters per subject per work unit in L0surplus (L10surplus, respectively).

Table 4. Average cost of production (displayed in the surplus information treatments only)

Number of letters	15	20	25	30	35	40	45	50	55	60	65	70	75	80	85	90
L0surplus Average cost per letter in Cents in	16.7	12.5	10.0	8.3	7.1	6.3	5.6	5.0	4.5	4.2	3.8	3.6	3.3	3.1	2.9	2.8
L10surplus average cost per letter in Cents (2nd work unit)	18.3	13.8	11.0	9.2	7.9	6.9	6.1	5.5	5.0	4.6	4.2	3.9	3.7	3.4	3.2	3.1

3.2. Results of the laboratory experiment

Work output is measured by the number of filled envelopes. Due to the simplicity of the task, we did not observe any output waste. Hence, there is no difference in the measurement of output quantity and quality. As in the field experiment, we correct for individual capabilities

of the workers. We, therefore, base our evaluation on the difference in work output of the two work units, i.e. before and after the wage increase. Table 5 shows that in the no-surplus information treatments L0 and L10, average output increases by 10 and 9.9 envelopes, respectively. In the information treatments L0surplus and L10surplus, our experimental workers filled an excess of 6.2 and 12.9 envelopes, respectively in the second work hour.

Table 5. Treatment averages in laboratory experiment

	L0	L10	L0surplus	L10surplus
Output quantity work unit 1	31.1	40.5	37.5	43.3
Output quantity work unit 2	41.1	50.4	43.8	56.2
Absolute increase in output	10.0	9.9	6.2	12.9
Relative increase in output in percent	35.3	25.4	16.3	32.2

In all four treatments, we find an experience effect: Subjects' output quantity in the second work unit is significantly higher than in the first unit (in each treatment $p \leq .001$, Wilcoxon signed rank test, two-tailed). Nonetheless, the question is whether in addition to the experience effect also a treatment effect exists. To analyze the impact of surplus information and wage increase on output, we compare the output increases across treatments. For each treatment, the vertical line in Figure 2 visualizes the range of output changes, and the horizontal line marks average output.
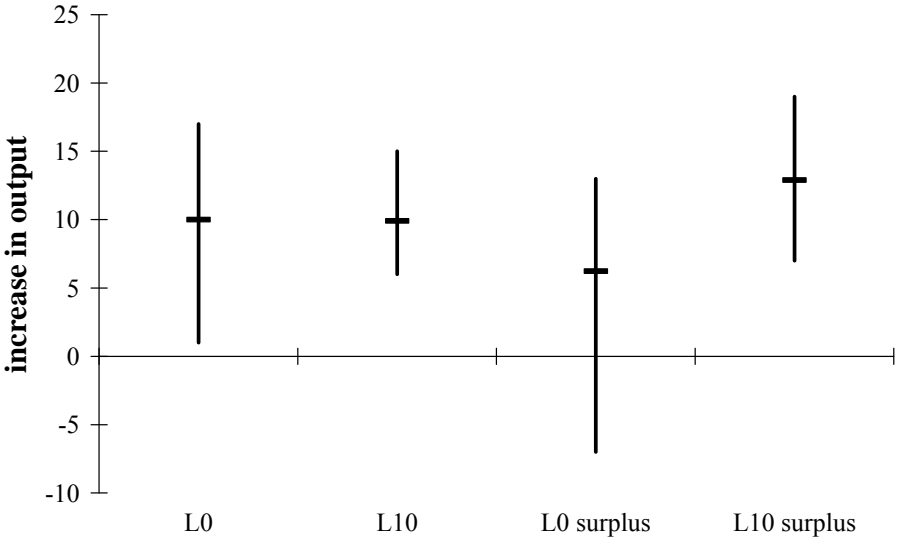

Figure 2: Increase of work output in the laboratory experiment

The impact of a wage increase in the absence of surplus information is analyzed by comparing L0 to L10. As expected from our field experiment, we find no significant difference in the work output increase (p=.836, two-sided, Mann Whitney U-test). Obviously we have replicated the finding of our field experiment that in the absence of surplus information, a positive wage-effort relation is not observed.

If surplus information is provided, the picture is clearly different. The wage increase from the first to the second work unit leads to an increase in output of only 6.2 envelopes in L0surplus. In L10surplus, however, the absolute output increase more than doubles. This difference is highly significant (p=0.001, Mann Whitney U-test, two-sided). Hence, with explicit surplus information, we observe a strong positive wage-effort relationship.[15]

Before reaching a final conclusion, it is important to check whether surplus information per se has an impact on the increase in output. This can be analyzed by comparing treatment L0 with L0surplus and treatment L10 with L10surplus. The first comparison reveals that surplus information in absence of a wage increase does not yield a significant output increase (p=0.235, Mann Whitney U-test, two-sided). Only when wages are raised, surplus information leads to a significantly higher work output (p=0.043, Mann Whitney U-test, two-sided).

An important question for the employer is whether in case of a positive wage effort relation, the increased output compensates for the higher wage expenses. Obviously, in the absence of surplus information (comparison L0 to L10) the higher wage is not recouped. In case of surplus information, however, the wage increase by 0.25 cents pays off if at least four additional envelopes are produced. Beyond the experience effect (also observed in L0surplus), however, we observe an average increase by 6.7 envelopes. Hence, in case of surplus information the expenses for the wage increase are more than compensated by the output increase.

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[15] One of the referees correctly pointed out that a stronger wage increase may have led to a reciprocal effect even without surplus information. Obviously, testing many different levels of wage increases would go beyond the scope of this paper. But, we are rather confident in our conjecture that our results are robust, because (i) in the surplus information setting, we already observe a significant reciprocal response at a 10% wage increase and (ii) in the no information setting, we have the evidence from our field experiments, suggesting that not even strong wage increase will result in increased reciprocal responses.

4. Discussion and Conclusion

Our study provides two major insights into the empirical relevance of the fair wage-effort model. First, our laboratory experiment supplies further support for reciprocal behavior in labor relationships. Wage increases lead to a substantially increased output in our real effort setting. In fact, we find that wage hikes can be profitable for the employer. This positive wage-effort relationship, however, is only observed when there is unambiguous information on all parties' cost and benefits. In lack of employer surplus information, employees do not respond reciprocally, i.e. with increased output to increased wage.

This latter finding is the second major insight from our study, for which we not only find evidence in our real-effort laboratory experiment, but also in our extensive field experiment. There, neither variations of own wages, nor variations of peer income information lead to changes in the work output. We conclude that reciprocal employee behavior can provide a financial benefit for employers, but only if there is sufficient information about the distribution of surplus.

Our results confirm and expand the findings reported in the literature in a number of ways. To start with, the fact that we observe a positive wage to effort correlation when there is sufficient surplus information is consistent with the results of laboratory studies that have found this effect repeatedly and persistently both with real effort (Fahr and Irlenbusch 2000; Gneezy 2004) and without (Fehr, Kirchsteiger, Riedl 1993; Fehr, Kirchler, Weichbold, Gächter 1998; Fehr and Falk 1999; Charness 2004).

Furthermore, our result concerning the lack of workers' sensitivity to peer wage comparisons seems in line with the laboratory findings by Charness and Kuhn (2005). We are cautious in comparing the two studies, however, because the experiments are different in several ways. Charness and Kuhn (2005) examine three-player games with one "employer" and two "workers" with unequal productivities. The employer is informed of both productivity levels. The workers are not informed about each other's productivity and, hence, cannot exactly derive the employer's surplus. In both private and public wage settings, workers' effort levels are positively correlated to own wages, but neither to peer wages nor to peer wage differentials. Thus, while the lack of sensitivity to peer wages is in line with our

results, the strong own wage effect when there is imperfect surplus information seems not to be.[16]

A possible explanation for the fact that Charness and Kuhn (2005) observe own wage effects, while we do not, is that the workers in that setup have two clues that may help guess the magnitude – but not the exact amount – of the employer's surplus. The first clue is the knowledge workers have on how increasing their own effort affects the employer's surplus. Although they know that the productivity of their co-worker is different, they are likely to expect that it will of the same order of magnitude.[17] The second clue comes from the fact that the employers are also students participating in the experiment. This makes it most likely that their surplus, which basically amounts to their earnings from the experiment, will be of the same order of magnitude as the average earnings of all students in the lab. Taking these clues together, it is clear that the imperfect information that the workers in the Charness and Kuhn (2005) setup have is substantially different from that in our setup. The workers in our setup basically have no clues on how changes in their effort affects university's surplus. It is truly not self-evident how to judge the value of the work they performed for the university.

In this respect the setup of our field experiment is probably closer to the setup used by Gneezy and List (2006) in their "library" experiment. In that experiment, students were hired, who typed in bibliographic data into the computer system of a university library. The students were promised an hourly wage of $12. Upon arrival, however, half of the subjects were informed that they will receive 67 percent more than announced, i.e. $20 per hour. Gneezy and List (2006) find that the subjects who were told that they will be given more than announced were significantly more productive in the first 90 minutes of the task, but not after that.

While the informational setting, the type of task, and the result that long term positive own wage effects are not observed resemble our study, there are also a number of remarkable differences between the two studies. First, we measure the effect of *actual* changes in the own wage, whereas Gneezy and List (2006) measure the effect of a *fictitious* wage increase, i.e. they surprise workers by paying them more than announced, but without ever having paid the lower amount. It is interesting that surprising workers with a higher starting wage than

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[16] We thank an anonymous referee for pointing this out.

[17] In fact, worker II was two times as productive as worker I. For example, the employer's surplus increased by $0.05 or $0.10, if worker I or worker II increased their effort level from "low" to "medium," correspondingly.

promised has a short-run positive effect on effort, while actually increasing the wage – as in our experiment – does not.

Second, Gneezy and List (2006) use an across subject design, while we use a difference-in-differences design. Varying the wage, we compare the within-subject changes of effort levels across treatments. In this way we avoid confounding effects that may result from differences in the subjects' individual capabilities.[18]

Third, the duration of the work time in the study by Gneezy and List (2006) is longer than in our experiment. This allows them to observe changes in behavior over time, such as the drop in effort observed in the high wage groups. Obviously, it is possible that our subjects would have also reduced their effort over time in a longer experiment. However, since we did not observe different effort levels in the first place, we have no reason to believe that time dependent changes in effort would be correlated to the wage level in our setting.

Fourth, the workers in Gneezy and List's (2006) "charity" experiment, in which surplus information was available, were paid such high wages that inducing a positive outcome for the employer was obviously not viable. The "low" wage group was paid $10 an hour and collected $2 donations on average. The "high" wage group was paid $20 and collected $3 donations on average. Hence, subjects in both the "low" and the "high" wage settings probably recognized quickly that they are being overpaid by an incredibly high margin. Thus, one possible interpretation of the observed decline in reciprocal behavior over time is as follows. Perhaps the subjects in the "high" wage treatment started off trying to achieve a fair outcome, but were then frustrated to see that they could not even come close to the break-even point of the employer, no matter how much effort they invested. Having recognized the discrepancy between the required and the feasible effort levels, they were discouraged and did not attempt to reciprocate any longer. Our setup was different, because we made sure that the break-even point was attainable for most subjects, by running pretests to calibrate the parameter in our setup. Hence, workers could choose to induce positive surplus levels for the employer, even though doing so required a non-negligible amount of effort.[19]

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[18] In across-subject designs the confounding effects of individual attributes are avoided by randomly distributing the subjects across treatments. Gneezy and List (2006), for example, randomly assigned 10 out of 19 (13 out of 23) subjects to the high wage group in their library (charity) experiment.

[19] The break-even point was explicitly mentioned in the instructions. See the instructions in the appendix.

Given the numerous differences between our setup and the ones used by Gneezy and List (2006), it remains unclear what exactly drives the differences in the results. Further research will be necessary to clarify these issues one by one.

Note, however, that our fundamental observation that reducing the workers' information on employer surplus, reduces the extent of reciprocal effort (at least after an opening phase), seems be supported by the results of the experiments discussed so far. The question remains, whether increasing the information will increase the reciprocal response by workers even more. We can check for an increased information effect in the experiment presented by Charness et al. (2004). Subjects in that experiment are given detailed payoff tables that allow them to exactly see the employer's and worker's payoffs for every possible combination of wage and effort choices. This information is more detailed than what has been provided in most other studies, because subjects usually only had common knowledge of the payoff functions, but had to do the calculations themselves.

Comparing the results of the treatment with payoff tables to that without, Charness et al. (2004) find that the workers tend to choose lower efficiency levels with more detailed information than without. A closer look at the results reveals that the main difference between the two treatments occurs at very high wages, at which inducing fair outcomes becomes (almost) impossible for workers no matter how much effort they exert. At wages of 60 or more, the worker earns more than the firm, even when workers choose the maximum effort level. Hence, workers with detailed information may be reciprocating less at such high wage levels than the workers without detailed information simply because they recognize that a fair outcome is not attainable, no matter how hard they work. In a way, it seems as though this may be connected to the observation by Gneezy and List (2006), who find that the overpaid workers in their charity experiment give up on reciprocation, once they notice that inducing positive payoffs for the employer is not a feasible option.

Taking all the evidence together, it seems clear that employer surplus information plays an important role in reciprocal effort choice. If workers have no information on the magnitude of the employer's benefit from their effort, they will hesitate to respond to higher wages with higher effort levels. In other words, efficiency wages seem to require a clear surplus sharing agreement between workers and employers. Interestingly, the work by Charness et al. (2004) and by Gneezy and List (2006) seems to indicate that workers not only need the surplus information to ensure that they are not being underpaid, but also to check whether they are being overpaid. When overpaid, workers seem to reduce their effort levels,

as if they take the overpayment is a signal for an altruistic gift of the employer without the expectation of a reciprocal response. Clearly, this effect needs a more thorough examination.

Our study implies that a firm's internal profit information policy may have a positive impact on work output. This seems to be due to the fact that if employees are not able to assess how the gains from trade are distributed, they do not have a reference point for being reciprocal. In long-term relationships, however, the negative effect of missing surplus information may be mediated by experience, as employees improve their information over time. It may still be hard to assess the exact value of a single action, but employees may develop a general idea for the impact of increased effort level on the firm's performance. Obviously, cooperation in long-term labor relationships is partially due to repeated game effects, but as Gächter and Falk (2002) show, reciprocity and repeated game effects reinforce one another in labor relationships. Hence, in addition to the usual repeated game considerations, the accumulation of information on the distribution of income can be another reason why the fair wage-effort model is more suitable for long-term than for short-term labor relationships.

It is evident that our experiments only allow us to draw qualitative conclusions on the validity of the fair wage-effort hypothesis, and not to assess the distribution of the corresponding parameters in real labor markets. We will have to leave this task to future empirical research.

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Appendix A: Statistical analysis

Table A1: Statistical comparison of output measures

Change in variable	F0 vs. F10	F10 vs. F40peer	F10 vs. F10peer	F40peer vs. F10peer
Output difference				
usable output	.290	.656	.734	.893
output quality	.744	.499	.961	.424
Output ratio				
usable output	.366	.553	.719	.835
output quality	.744	.526	.961	.424

The entries denote the p-values of the two-tailed Mann-Whitney U-Test

There is also no difference between treatments according to the Kruskal-Wallis test for both the difference [2nd minus 1st hour] in usable output ($p = .827$) and output quality ($p = .881$).

Appendix B: Instructions

B1. Field Experiment

The *Economics Research Center „Information and Coordination of Economic Activities" (SFB 303)* has been existing during the past 15 years. Most professors and assistants of the Bonn economics department are collaborating in the SFB 303. Within these years a huge amount of working papers have been written reporting on the research done in the Economics Research Center. To make research results accessible to other scientists in a more effective way than before, the abstracts of all research papers shall be available via Internet by mid-2000. Recently submitted papers do not raise any problems because the author electronically submits the abstract together with the paper. Difficulties arise, however, with older discussion papers either providing no abstract at all or at least no typed version. Most authors have left the University of Bonn already. In order to give electronic access also to these abstracts we initiated the project *Electronic Abstract Documentation*.

Your task

You will receive a folder containing abstracts of older papers the authors did not submit electronically. In addition, you will find abstracts which have been created by extracting the important results of the research paper. Your task is to enter the abstracts into the word processor from which they will be transferred to the Internet. We hope you understand that the SFB 303 wants to present itself in the Internet by correctly typed abstracts only. Therefore please take care to enter the abstracts from the folder without any mistakes.

Organization

This is a pilot study on the division of work load. Many students collaborate in this study for two working hours each. Everybody works for these two hours at two different occasions as already has been arranged with you. In the following, you will be introduced to the task you are going to perform, and you will have the opportunity to

familiarize yourself with the word processor by typing an abstract for training. Introduction and training must not exceed **20 minutes.**

After having completed the training phase, you have *60 minutes time* to enter the abstracts from the file. You will be notified by the project coordinator when the time is over. Please stop typing even if you did not finish the abstract you are working on because we otherwise will not be able to keep our time schedule.

Payment

You will be paid after having completed the second working hour. You will receive DM 10 for the instruction and training phase plus a wage for each working hour of DM 20 for typing.

If you have any questions or if any technical problems arise please *immediately* call the project coordinator whom you find in the room next to this office.

Information to typists for the 2nd working hour

Treatment F0

This is a reminder that the payment for the second working hour is DM 20.

Treatment F10

We are happy to inform you that due to a favorable development in the funding of the project we are able to remunerate your experience. We therefore can raise the payment for the second working hour by 10% to DM 22.

Treatment F40peer

We are happy to inform you that due to a favorable development in the funding of the project we are able to remunerate your experience. As you may have heard already, we could previously raise the payment for the second hour of work by 10% to DM 22. Unexpectedly, additional funding became available for the project. Therefore we decided to once more raise the payment for the second working hour, namely by all in all 40% to DM 28.

Treatment F10peer

We are happy to inform you that due to a favourable development in the funding of the project we are able to remunerate your experience. As you may have heard already we could previously raise the payment for the second hour of work by 40% to DM 28. Unexpectedly, less funding became available for the project. We are sorry that for this reason we can raise the payment for the second working hour only by 10% to DM 22.

B2. Laboratory Experiment

——— Those parts of the instruction that are written in *italics* are relevant for the surplus information treatment only. ———

This year, the department of Microeconomics is organizing an international conference on experimental economics. For this reason, a large amount of information letters has to be mailed to people interested in the conference.

Your task

You will receive a pile of information sheets as well as a pile of envelopes. Your task is to fold the information sheets twice, put them into the envelopes and close the envelopes. We hope you understand that the department wants to present itself by correct information letters only. Therefore we ask you to carefully put the sheets into the envelopes.

The Organization

This is a pilot study on the division of work load. Many students collaborate in this study for two working units of 15 minutes each. Between the working units, there is a break of 5 minutes. After the introduction, you will be introduced into the task you are going to perform. Introduction and training must not exceed **10 minutes.**

Payment

You will be paid after having completed the second working unit. You will receive € 1,50 for the instruction plus a wage of € 2,50 for each working unit.

Surplus for the department

If the department had the task being completed by someone else, costs of 6 Euro Cents per letter would occur. The department decided, however, to hand this task over to you on a flat fee basis. The table below shows the costs per letter depending on the number of completed letters per work unit the department encounters by paying you you € 2,50. In order to create a surplus for the department, you would have to complete more than 41 letters in one work unit (i.e. in 15 minutes).

number of letters	15	20	25	30	35	40	45	50	55	60	65	70	75	80	85	90
cost per letter in Centss	16,7	12,5	10,0	8,3	7,1	6,3	5,6	5,0	4,5	4,2	3,8	3,6	3,3	3,1	2,9	2,8

Any further questions?

If you have any questions, please ask the project coordinator.

Information on the wage for the 2nd working unit

Treatments L0 and L0surplus

To remind you: the payment for the second working hour is € 2,50.

If the department had the task being completed by someone else, costs of 6 Euro Cents per letter would occur. The table below shows the costs per letter depending on the number of completed letters per work unit the department encounters by paying you € 2,50. In order to create a surplus for the department you would have to complete more than 41 letters in one work unit (i.e. in 15 minutes).

number of letters	15	20	25	30	35	40	45	50	55	60	65	70	75	80	85	90
cost per letter in Cents	16,7	12,5	10,0	8,3	7,1	6,3	5,6	5,0	4,5	4,2	3,8	3,6	3,3	3,1	2,9	2,8

Treatments L10 and L10surplus

We are able to remunerate your experience and therefore can raise the payment for the second working unit by 10% to € 2,75.

By raising your payment higher costs occur to the department.

If the department had the task being completed by someone else, costs of 6 Euro Cents per letter would occur. The table below shows the costs per letter depending on the number of completed letters per work unit the department encounters by paying you € 2,75. In order to create a surplus for the department you would have to complete more than 45 letters in one work unit (i.e. in 15 minutes).

number of letters	15	20	25	30	35	40	45	50	55	60	65	70	75	80	85	90
cost per letter in Cents	18,3	13,8	11,0	9,2	7,9	6,9	6,1	5,5	5,0	4,6	4,2	3,9	3,7	3,4	3,2	3,1