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Worker Characteristics and Wage Differentials:
Evidence from a Gift-Exchange Experiment

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Abstract

There is ample empirical evidence indicating that a substantial fraction of the population exhibits social preferences. Recent work also shows that social preferences influence the effectiveness of incentives in labor relations. Hence when making contracting decisions, employers should take into account that workers are heterogenous with respect to both their productivity and their social preferences. This paper presents causal evidence that they do. In a real-effort experiment, we elicit measures of workers' productivity and trustworthiness and make this information available to potential employers. Our data show that employers pay significant wage premia for both traits. Firms make highest profits with trustworthy workers, in particular with highly productive and trustworthy workers. We also document differences in the strength of gift exchange across worker types. In particular, output and profit levels of trustworthy workers are less dispersed than those of not-trustworthy workers.

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1 Introduction

Firms spend substantial resources to select the “best” candidate for a job. In particular, an increasing fraction of firms uses both ability and personality tests in their hiring processes, see, e.g., Autor and Scarborough (2008). While the rationale for selecting the most “able” candidate is obvious, labor economists have traditionally focused on productivity as the only dimension of ability. So what information about workers might a firm obtain from personality tests and job interviews that is not just a measure of productivity? One important piece of additional information about a potential worker are her social preferences. In particular in the presence of moral hazard, it is valuable for firms to have access to employees that can be motivated by “social incentives” via gift exchange.¹

Despite the importance of information acquisition in real world contracting, our understanding of the impact of the availability of information on the terms of a contract is surprisingly limited. Empirical evidence on the issue is scarce. This paper aims to fill this gap. We provide causal evidence from a laboratory experiment on how principals (employers) use specific pieces of information about agents (workers) when they design contracts in a gift-exchange situation. Specifically, we elicit measures of workers’ productivity and trustworthiness and make this information available to potential employers. Our data show that employers pay significant wage premia for both traits. Thus, individuals seem to understand that there is heterogeneity among workers not only with respect to productivity but also with respect to their social preferences, and they take these social preferences into account when making contract offers.

Research Question When making employment and contracting decisions, firms naturally desire to minimize the risk of hiring an unsuitable candidate. They try to learn about the qualification of a candidate, his education, his family background, etc. before offering an employment contract. As a necessary simplification of reality, we concentrate in our experiment on two dimensions of information that we regard as essential on real world la-

¹Englmaier and Leider (2011) is a recent theoretical treatment of the issue. Dur and Sol (2010) and Mohnen et al. (2008) are examples of experimental studies that highlight that the selection of the workers exhibiting social preferences enhances outcomes in team production situations. We review this literature in more detail below.

bor markets. The first dimension, which we call *productivity*, is an objective assessment of whether the candidate is good at the job he is supposed to accomplish. In the second dimension, a measure of *trustworthiness* is supposed to capture the candidate’s social and reciprocal preferences. We consider these two measures of a worker’s traits the most relevant “skills” in our setting, and we expect that information about these skills matters for firms. In a situation characterized by moral hazard, we expect both elements to play an important part in the effort decision of the agent and hence for the outcome for the principal: Controlling for social preferences, an agent who is more productive at accomplishing a certain task will produce a higher outcome for the principal. Similarly, for given productivity, a reciprocal agent will put in more effort in response to a “generous” wage offer leading to a higher outcome for the principal. We examine if this behavior is anticipated by firms and if it affects wages.

In this article, we concentrate on a contracting situation where information about a worker stems from sources external to the firm-worker relationship. In contrast to earlier studies by Brown et al. (2004) or Bartling et al. (2011), we abstract from information about the worker that arises endogenously in a repeated relationship and can be used for firms to adapt contracts over time. We focus on the trade-off between two pieces of information and their impact on contracting behavior by both principals and agents in a one-shot interaction. The main research question we have in mind is whether these two pieces of information are conditioned upon when writing contracts and to what extent they can be used to predict behavior. Eventually, we evaluate how the presence of certain skill sets and available information about them shapes labor contracts under moral hazard. The high degree of control makes the laboratory an ideal setting to address these questions.

Design Our experiment consists of two parts, which are presented sequentially to subjects such that they do not know what will be the content of the next part. Subjects know in advance, however, that decisions in earlier parts may have an impact on later parts. In the first part, subjects work on a real effort task under a piece rate contract. We use their score in this piece rate task as our measure of productivity. Subsequently, subjects are presented with a binary, neutrally framed, trust game which we use to proxy for social and reciprocal

concerns. In the second part, half of the players are randomly assigned to be employers and the other half to be employees. Subjects play a one-shot gift exchange game where the employer first offers the employee a flat wage and the employee thereafter performs the real effort task from the first part under standard gift-exchange incentives. Before making their wage offers, principals are presented with the information about workers from the elicitation tasks. The level of information provided to firms is our treatment variable. In our main treatment, employers are presented the *productivity* and the *trustworthiness* measure in a binary way (hereafter treatment PT) before submitting wage offers. To control for strategic behavior in the elicitation phases, we run two control treatments where only one piece of information is made available to firms. In treatment “Productivity”(hereafter P) they are only presented the productivity measure, and in treatment “Trustworthiness” (hereafter T) they are only presented the trustworthiness measure. By comparing our treatments P and T to the PT treatment, we can control if the information revelation in the final phase distorts the elicited measures in phases 1 and 2. In P (T) it is communicated to subjects that in the second part only information from the elicitation of productivity (trustworthiness) is possibly made available in later parts of the experiment, whereas in PT this applies for both measures. We do not find any indication of strategic distortions.

Results Along the two dimensions *productivity* and *trustworthiness* we categorize workers into four categories: *high productivity & trustworthy*, *high productivity & not trustworthy*, *low productivity & trustworthy*, *low productivity & not trustworthy*. We document four main findings. 1) Contracts offered by principals systematically vary with the information they have about the agent. Principals tailor their wage offers to employee types, offering more generous contracts to more productive and more trustworthy subjects. 2) Higher wages predict higher output. While trustworthiness and productivity also predict output, they do not have a significant effect once the wage (that already contains information about these traits) is included in the regression. This result is consistent with the interpretation that wages are already set close to their optimal levels conditional on worker characteristics. 3) Firms’ profits vary across worker types. While the remaining three types produce similar output levels, the *high productivity & trustworthy* workers generate significantly higher profits. 4)

Output and profits are much less dispersed for *trustworthy* workers. This result is driven by the fact that *trustworthy* workers in the Gift-Exchange Game produce systematically much closer to their elicited productivity in the piece rate task. The nature of gift exchange varies markedly across the four worker types.

Related Literature Following the arguments by Akerlof (1982), an extensive experimental literature documents incentives and behavior in gift exchange games, see e.g. Fehr et al. (1993), Fehr et al. (1997). This protocol has proven to be a valuable paradigm that captures incentives on real world labor markets in the laboratory. As a major finding of this literature, preferences for fairness and reciprocity serve as a powerful source of motivation to overcome the informational asymmetry between principals and agents on labor markets². These laboratory studies have also been validated in the field; see e.g. Falk (2007) or Bellemare and Shearer (2009). It is now also widely acknowledged that social preferences like reciprocity or inequity aversion potentially do not only shape market outcomes or the result of bilateral bargaining, but have an important effect on the design of optimal incentive schemes as well.³ One additional important empirical finding from both field and laboratory data (e.g. Dohmen et al. (2009)) is that there is substantial heterogeneity with respect to the prevalence of reciprocal inclinations and social preferences among the population, see Fehr and Schmidt (1999) or Fischbacher et al. (2001).

Recent theoretical and experimental work suggests that there are complementarities from matching incentive structures to worker types; e.g. Ichniowski et al. (1997); Englmaier and Leider (2011) or Bartling et al. (2011). However, little work has been done that tests how worker characteristics (amongst them social preferences, if measured) relate to behavior across games and, more to our point, how worker types interact with incentives. One recent important exception is a paper by Cabrales et al. (2010) who design an experiment where in the first phase, all subjects choose a payoff vector and play a self-chosen effort game. From these choices their preference parameters in terms of both outcome preferences and reciprocal inclination are estimated, assuming preferences à la Charness and Rabin (2002).

²For references see Fehr and Gächter (2000), Fehr and Schmidt (2003) or Fehr and Falk (2008).

³See Itoh (2004), Dur and Glazer (2008), Englmaier and Wambach (2010) or Englmaier and Leider (2011) for theoretical treatments and Fehr et al. (2007) for experimental evidence.

In the second phase, it is documented that these estimated preferences predict behavior in a gift exchange game conditional on contract offers. Moreover, contract offers vary systematically with estimated preferences of principals. However, Cabrales et al. (2010) do neither use a real effort task (and hence they do not elicit measures about productivity), nor is information about workers presented to the principals prior to their contract offers. We consider this last feature essential for our understanding of the functioning of real world labor markets. Most closely related to our study, Dohmen and Falk (2011) design a laboratory experiment where they elicit worker characteristics to explain sorting behavior of subjects into variable or fixed-payment incentive schemes. They find strong evidence for worker sorting along multiple dimensions, but claim that "many of the discussed worker attributes are typically unobservable in the hiring process" (p.558). While this is certainly the case for some attributes that are difficult to observe, we argue that proxies for the most important skills of a worker are well available to firms before hiring a worker, e.g. in the form of a curriculum vitae or the results from hiring tests. We therefore complement their analysis by showing how the presence of information about these attributes interacts with incentives on the labor market.⁴

Evidence from the field suggests that firms use available or acquired information about workers and applicants for screening purposes and to tailor incentive schemes in the presence of moral hazard, see e.g. Ichniowski et al. (1997); Huang and Cappelli (2010). In particular in firms' hiring decisions there is evidence for the extensive use of personality tests (see Autor and Scarborough, 2008) and the screening for personality traits like honesty or cooperativeness (see Guion and Gottier, 1965; Sackett and Wanek, 1996; Salgado et al., 2003).⁵

To the best of our knowledge, we are the first to focus on the pure effect of the availability

⁴A related literature on cognitive and non-cognitive skills focuses on the relation between these two skill sets and their interdependence, see e.g. Heckman et al. (2006), Borghans et al. (2008b), and their relationship to labor market outcomes, see e.g. Murnane et al. (1995), Borghans et al. (2008a), Heineck and Anger (2010).

⁵Our paper adds to another strand of literature that assesses the effects of the availability of potentially costly information about an interaction partner on subsequent strategic behavior. Kurzban and DeScioli (2008) show that subjects in public goods game buy information about behavior of others in previous round to adjust their behavior. More recently, Eckel and Petrie (2011) give subjects the possibility to purchase a picture of the interaction partner in a trust game before deciding about trust and trustworthiness. They find that there is informational value in a counterpart's face since many subjects do purchase the picture at nonzero costs.

of information about interaction partners on contracting outcomes in a one-shot moral hazard situation. A controlled laboratory study allows us to exogenously vary the information structure and eliminate effects of worker competition.

The remainder of the paper is structured as follows. Section 2 presents the experimental design. In section 3 we lay out our hypotheses and section 4 presents the results of the experiments. Section 5 discusses our findings and concludes.

2 Experimental Design

The experiment consists of two parts, which are presented sequentially to subjects such that they do not know what will be the content of the next part. Subjects knew in advance, however, that their decisions earlier on may potentially be disclosed to other subjects later on of the experiment. Overall, we ran 12 sessions with a total of 288 subjects in June and July 2011 at the MELESSA laboratory at LMU Munich. The subjects were invited via ORSEE (Greiner, 2004), and the experiment was implemented with zTree (Fischbacher, 2007). Subjects earned experimental points (EP) during the experiment. The exchange rate from EP to Euros was $1\text{EP} = 0.0125 \text{ €}$. The experiment lasted about 60 minutes and subjects earned on average 11.8 €.

2.1 Elicitation of Productivity and Trustworthiness

In a first part of the experiment, measures of both productivity and trustworthiness are elicited from all subjects. We proxy productivity with a measure from a real effort task that consists in matching words and a four-digit code from a list.⁶ A screenshot of the experimental screen of the coding task can be found in the Appendix (Screenshot S.1). Subjects perform this task for 90 seconds and are paid a piece-rate per correct answer of 10 EP that is paid out at the end of the experiment. To discourage guessing there is a penalty for every wrong answer of 10 EP, which is known to subjects. Our measure of productivity therefore consists in the number of correctly matched codes after subtracting

⁶We conduct a trial period before the elicitation task to familiarize subjects with the computer program.

all wrong answers, i.e. the *net correct answers*.

$$\# \text{net correct answers} = \# \text{right answers} - \# \text{wrong answers}$$

There is no particular training required for fulfilling this task and we assume that all subjects put in full effort under the piece rate scheme such that our measure of productivity is as closely related to underlying ability as possible. In the remainder of the analysis, we therefore refer to "productivity" as the number of *net correct answers* in this task. The corresponding payoff from this part is calculated as follows according to the number of net correct answers:

$$10 \text{ EP} * (\# \text{net correct answers})$$

Subjects are presented three screens of 30 seconds each one after another, i.e. a total of 90 seconds, with randomly generated words and codes for every new screen.⁷ We conjecture that intrinsic costs for the task are linear over the interval of 90 seconds, i.e. there are no effects from fatigue or boredom. The resolution of the number of correct answers is given to subjects only at the end of the experiment.

Subsequently, subjects play a standard binary trust game in neutral framing to provide a measure of trust and trustworthiness at the individual level; see Figure 1 for the precise amounts used in the trust game. We make use of the strategy method to get data on both trusting behavior and trustworthiness. Subjects take both decisions for both roles and at the end of the experiment they are matched with another subject, roles are randomly determined and payoffs are realized according to the decisions taken in the respective roles. Behavior in the trust game can be seen as indicative whether individual preferences are characterized by high or low levels of trust and trustworthiness when engaging in an interaction with another person. We focus on the trustworthiness of subjects as this appears as a more relevant proxy of social and reciprocal concerns in the gift exchange game than the initial trusting decision.

--- Include Figure 1 about here. ---

⁷A screenshot of the real effort task can be found in the Appendix (Screenshot S.1).

Importantly, there was no feedback given to subjects about the elicitation procedures (and the resulting payoffs) until the very end of the experiment such that subjects' subsequent behavior in the experiment was not affected. We also elicit all subjects' detailed expectations about productivity and trustworthiness in the population. Since they turn out not to matter for the subsequent experiment, we relegate the description of the experimental protocol and the results on expectations in the Appendix.

2.2 Gift-Exchange Game

In the second phase of the experiment, an experimental gift exchange game is implemented in which the task to be fulfilled is identical to the real effort task in the first part of the experiment. Subjects are randomly allocated to be either a firm or a worker. Overall, there are 12 workers and 12 firms per session. We employ the strategy method, i.e. firms have to submit a binding wage offer for each of the 12 workers such that we obtain the full wage profile firms are submitting for all workers in their market.⁸ After all wage offers have been submitted, every worker is matched randomly to a single firm, i.e. every firm hires only one worker. Workers learn only the wage offer that their matched firm has determined for them before they start working for their firm. There is no possibility for workers to be influenced by offers that the firm has submitted for other workers or by offers that other firms have submitted to them. Subsequently, workers perform the same real effort task from the first part for 90 seconds. The interaction is one-shot to preclude any effects of repetition over time and to focus in the cleanest possible way on the effects of information on contracting behavior.

Workers' performance then determines the payout to the firm according to the following formula

$$\text{firm payoff} = 10 \text{ EP} \cdot (\# \text{net correct answers}) - \text{wage}$$

where $\#$ net correct answers is given by all solved matches minus all wrong matches. Workers are paid their predetermined fixed wage and have non-monetary costs of effort from solving

⁸A screenshot of the wage setting screen can be found in the Appendix (Screenshot S.2).

the task:

$$\text{worker payoff} = \text{wage}.$$

To avoid that subjects can ruin firms by deliberately giving wrong answers we impose a lower limit for the payoff to the firm from the task at 0. This does not preclude firms from making losses if the wage exceeds the revenues generated by their worker. Losses had to be paid from earnings in other parts of the experiment. Given the nature of the task and the fact that new words and codes are randomly generated for every screen, there should be virtually no learning possibilities from doing the task a second time. After the real effort task is completed, there is feedback about the number of correct answers and the payoff to the firm and the worker. Both firms and workers learn only the details from their interaction, but not from the interaction between any other firm-worker pair.

Treatments

Our treatment variation consists in the pieces of information elicited in the first part from the experiment that are made available to firms when submitting their wage profiles. In our main treatment PT, information about *productivity* and *trustworthiness* is available in a binary way. Information about productivity is given to firms in the form of whether a worker has achieved a productivity score in the coding task which is higher than the mean of all subjects in the respective session, or below that mean. Information about trustworthiness is given in the form of the binary decision as trustee in the trust game, i.e. either whether a subject returned trust or not. To preclude framing effects, both pieces of information were given in a neutral way, i.e. in the trust game the actual information was labeled “left” or “right” depending on whether subjects opted for the left or the right branch of the game tree. For the productivity measure, subjects were divided into two groups labeled “blue” or “yellow” which was explained to subjects.

To control for strategic effects in the elicitation phases of our two measures, we conduct two control treatments where we make only one piece of information accessible to firms. In treatment P, information about *productivity* only is available and in treatment T information

about *trustworthiness* only is available, which is told to subjects. Somewhat different from standard control treatments, P and T therefore exclusively serve as a control for the results from the elicitation phases.

We conduct 8 sessions of the main PT treatment (192 subjects) and 2 sessions each (48 subjects) of the two control treatments P and T.

3 Hypotheses

In this section, we sketch a stylized agency model for workers who are heterogeneous with respect to productivity and trustworthiness (assuming stable preferences and productivity types); the model is a simplified version of Englmaier and Leider (2011). A firm hires a worker for a fixed wage w which is binding. The interaction is one shot, effort e is not contractible and there are no contingent contracts. The firm relies on gift exchange to elicit performance. Exerting effort has convex costs of effort $\frac{c(e)}{p}$ for the worker where p with $p > 0$ is the worker's productivity; i.e., a more productive worker has lower (marginal) costs to exert effort. Output π is not contractible but assumed to accrue deterministically according to $\pi(e) = e$. Firm profits are then $\pi - w = e - w$ and the worker's utility $u(w)$ is given by

$$\begin{aligned} u(w) &= w + \eta(w - o)(e - w) - \frac{c(e)}{p} \\ &= w + \eta \cdot w \cdot e - \eta \cdot w^2 - \frac{c(e)}{p} \end{aligned}$$

where η captures the worker's reciprocal inclination, and o is the outside option which we normalize to 0. We abstract from explicitly modeling feelings of negative reciprocity but focus on positive reciprocity between firms and workers, i.e. $\eta \geq 0$ since in our setting as a worker there are no possibilities for punishing the firm other than shirking ($e = 0$).

In order to elicit a positive effort response the worker has to receive a wage "gift", i.e. a wage exceeding his outside option. When the reciprocal worker receives a positive wage gift his utility increases in the firm's profit. For convex costs, the Second Order Condition is globally satisfied, hence the First Order Condition is necessary and sufficient for an optimal

response and implicitly defines the worker's best response e^* :

$$c'(e^*) = \eta \cdot w \cdot p \quad (FOC_{GE}) \quad (1)$$

Compare this with the optimal effort choice of the worker under the piece rate incentives (PR) in part 1 where the worker maximizes his utility $u_{PR} = e - c(e)$ and his optimal effort choice e_{PR}^* is implicitly defined by

$$c'(e_{PR}^*) = 1. \quad (2)$$

Comparing (1) and (2) immediately implies the following Lemma:

Lemma 1 *As long as $1 > \eta \cdot w \cdot p$ it will hold that $e_{PR}^* > e^*$.*

I.e., as long as the wage, the productivity and the reciprocal inclination are not sufficiently large, we will see lower effort levels in the gift exchange situation in the second part than under piece rates in the first part of the experiment.

Now turn attention to the efficacy of gift exchange. Applying the Implicit Function Theorem to the First Order Condition in (1) and noting that the Second Order Condition is globally negative implies that

$$\text{sgn} \left(\frac{\partial e^*}{\partial w} \right) = \text{sgn} \left(\frac{\partial FOC_{GE}}{\partial w} \right) = \text{sgn}(\eta \cdot p) = +1 \quad (3)$$

which is summarized in the next lemma:

Lemma 2 *If $\eta > 0$ there will be gift exchange, meaning that the exerted effort will increase in the wage.*

Now we proceed and analyze whether the efficacy of gift exchange varies across worker types. To do so we check how $\frac{\partial e^*}{\partial w}$ varies when either p or η are varied. The full derivative, derived via the Implicit Function Theorem is given by

$$\frac{\partial e^*}{\partial w} = \frac{\eta \cdot p}{c''(e)}. \quad (4)$$

From this it is straightforward to derive the relevant higher order derivatives, $\frac{\partial^2 e^*}{\partial w \partial \eta}$ and $\frac{\partial^2 e^*}{\partial w \partial p}$:

$$\frac{\partial^2 e^*}{\partial w \partial \eta} = \frac{\eta}{(c''(e))^2} \left(c''(e) - \eta p w \frac{c'''(e)}{c''(e)} \right) \quad , \quad \frac{\partial^2 e^*}{\partial w \partial p} = \frac{p}{(c''(e))^2} \left(c''(e) - \eta p w \frac{c'''(e)}{c''(e)} \right).$$

Contrary to the first intuition it is not obviously the case that there is more gift exchange (meaning a steeper wage-effort relationship) for more reciprocal or more productive agents. In particular, for increasingly convex cost functions, $c'''(e) > 0$, it might be the case that, e.g., more reciprocal subjects react less strongly in response to a given wage increase. Note however, this is a statement in slopes, not levels. I.e., in general more reciprocal (productive) agents will *ceteris paribus* provide higher effort levels. A good way to get an intuition for this result is to think of workers having two modes of working; either they exert a lot of effort or they shirk. So *ceteris paribus* more reciprocal (productive) agents are more likely to be in the high effort mode. Hence, they are less likely to be able to respond to a wage increase by increasing their effort level beyond their peak performance.

Lemma 3 *More reciprocal (higher η) or productive (higher p) workers will react stronger to wage increases if*

$$c''(e) > \eta p w \frac{c'''(e)}{c''(e)} \quad \text{holds.}$$

Finally we can address the optimal wage setting policy for firms conditional on different worker types. Firms maximize profits, $e - w$, by setting w subject to the agents optimal response. Given the experimental setting we disregard the participation constraint and focus on the incentive constraint. We use the First Order Approach and replace the incentive constraint with the First Order Condition from the agent's optimization problem, (1).

$$\begin{aligned} \max_w \quad & e - w \\ \text{s.t.} \quad & c'(e) = \eta \cdot w \cdot p. \end{aligned}$$

Forming the Lagrangian and taking derivatives we get the First Order Condition

$$\frac{\partial e^*}{\partial w} - 1 - \lambda \cdot \eta \cdot p - c''(e) \frac{\partial e^*}{\partial w} = 0$$

which we can rearrange to

$$\frac{\eta \cdot p}{c''(e)} - 1 - (1 + \lambda) \cdot \eta \cdot p = 0 \quad (FOC_{Firm}) \quad (5)$$

as we know from (4) that $\frac{\partial e^*}{\partial w} = \frac{\eta \cdot p}{c''(e)}$.

Now we can again apply the Implicit Function Theorem to derive optimal wage adjustments to different worker types, $\frac{\partial w^*}{\partial p}$ or $\frac{\partial w^*}{\partial \eta}$. Again, as the Second Order Condition is satisfied, we see that

$$\text{sgn} \left(\frac{\partial w^*}{\partial p} \right) = \text{sgn} \left(\frac{\partial FOC_{Firm}}{\partial p} \right) = \text{sgn} \left(\frac{\eta \cdot c''(e) - \eta \cdot p \cdot c'''(e) \frac{\partial e}{\partial p}}{(c''(e))^2} - (1 + \lambda) \cdot \eta \right)$$

and

$$\text{sgn} \left(\frac{\partial w^*}{\partial \eta} \right) = \text{sgn} \left(\frac{\partial FOC_{Firm}}{\partial \eta} \right) = \text{sgn} \left(\frac{p \cdot c''(e) - \eta \cdot p \cdot c'''(e) \frac{\partial e}{\partial \eta}}{(c''(e))^2} - (1 + \lambda) \cdot p \right).$$

We can derive from above that $\frac{\partial e}{\partial p} = \frac{\eta \cdot w}{c''(e)}$ and that $\frac{\partial e}{\partial \eta} = \frac{p \cdot w}{c''(e)}$ and rewrite the above conditions as

$$\begin{aligned} \text{sgn} \left(\frac{\partial w^*}{\partial p} \right) &= \text{sgn} \left(\frac{\eta \cdot c''(e) - \eta \cdot p^2 \cdot w \cdot \frac{c'''(e)}{c''(e)}}{(c''(e))^2} - (1 + \lambda) \cdot \eta \right) \\ \text{sgn} \left(\frac{\partial w^*}{\partial \eta} \right) &= \text{sgn} \left(\frac{p \cdot c''(e) - \eta^2 \cdot p \cdot w \cdot \frac{c'''(e)}{c''(e)}}{(c''(e))^2} - (1 + \lambda) \cdot p \right). \end{aligned}$$

I.e., it is a priori unclear whether firms should set higher wages for more reciprocal or more productive workers. In particular, if the effort costs are very convex, it might be that it is optimal to set rather low wages to those workers. The intuition is clear. If the effort costs are

very convex then it is extremely hard to elicit effort beyond a threshold. Then the benefit of more reciprocal or more productive workers comes from the lower wage that is necessary to bring them to exert effort up to this threshold.

Lemma 4 *Firms will set higher wages for more reciprocal workers only if*

$$\frac{\eta \cdot c''(e) - \eta \cdot p^2 \cdot w \cdot \frac{c'''(e)}{c''(e)}}{(c''(e))^2} - (1 + \lambda) \cdot \eta > 0,$$

and for more productive workers only if

$$\frac{p \cdot c''(e) - \eta^2 \cdot p \cdot w \cdot \frac{c'''(e)}{c''(e)}}{(c''(e))^2} - (1 + \lambda) \cdot p > 0 \quad \text{holds.}$$

The two elicited measures in our experiment are proxying p through the productivity measure and η through behavior in the trust game. As in treatment PT proxies for p and η are available to firms, we expect according effects on offered wages, efforts, and profits. We summarize those in the following hypotheses.

Hypothesis 1 *In treatment PT, we expect above minimum effort. In general, we expect effort to be below the performance in the piece rate treatment. This performance gap will be smaller for more reciprocal workers.*

Hypothesis 2 *In treatment PT, we expect wage offers and performance in the gift exchange game to vary with workers' types. We expect higher output from more reciprocal and more productive workers.*

Hypothesis 3 *In treatment PT, we expect positive gift exchange, i.e. a positive effort response to a wage increase.*

Hypothesis 4 *In treatment PT, we expect higher profits from more reciprocal and more productive workers.*

4 Experimental Results

4.1 Part One: Elicitation of Productivity and Trustworthiness

We start by reporting summary statistics for the coding task performance in part one from treatments PT, P and T. Subjects receive three screens with 15 matches each such that the maximum attainable is 45 correct answers. Only two out of 288 subjects succeeded in giving all 45 answers within 90 seconds correctly such that time was indeed the limiting factor and the way productivity was measured does not harm high productivity subjects. The average number of correct answers given was slightly below 29 with a standard deviation of about 7 answers.

--- Include Table 1 about here. ---

Table 1 shows summary statistics and illustrates that there are no differences across treatments with all treatments being almost identical in terms of the main statistics. We particularly do not find any evidence that subjects in the two control treatments (P and T) behave differently than in our main treatment PT. We can therefore exclude that workers behave strategically to signal to potential future employers as a model of career concerns would predict. The distribution of correct answers in all treatments is symmetric around the mean, but normality is rejected by all conventional tests; see Figure 2.

--- Include Figure 2 about here. ---

As a further robustness check, we regress the coding outcome on a number of socio-demographics to see whether there is explanatory power from gender, age, subject of study or the treatment. We also control for five character traits in the framework of the Big Five Personality Test that we elicited in a control questionnaire at the end of the experiment. Table 2 clearly indicates that there is no effect from gender nor from a quantitative orientation in the subject of study (economics, mathematics, natural sciences) on the performance in the task. Apart from some negative effect of age and the positive impact of the character trait “agreeableness”, there is no significant effect from the four other elicited personality traits from the Big Five

Index on coding task performance either. More importantly as the treatment P/T dummies are insignificant, the regressions confirm that there is no distortion from strategic concerns between the treatments (and hence different levels of information disclosure) on the outcome of the productivity task.

--- Include Table 2 about here. ---

For the second dimension of information, we let subjects play a binary trust game presented to them in a neutral frame. Since we employ the strategy method, we have data on choice behavior in both roles of the trust game for every subject. Table 3 displays the percentage of subjects' behavior in the trust game per treatment.

--- Include Table 3 about here. ---

According to our measure, about one third of all subjects can be considered selfish in the sense that they neither trust others nor do they return trust as a trustee. In a similar vein, about one third of subjects appears to have other-regarding concerns such that they both trust and return trust. The remaining third either trusts but does not return trust or vice versa. Although there is some variation across the treatments, these patterns are quite stable in all three treatments. Subject to the population averages, not to trust is indeed optimal for selfish subjects and cannot be rationalized by choosing to trust for strategic reasons. What is important to note is that about half of all subjects trusted and also about 50 % of all subjects returned trust while about 50 % did not return trust. That is to say that our design succeeds in creating variation across subjects which makes information about other subjects valuable for the firm-worker interaction. If in the population our binary measure of social and reciprocal concerns was distributed less symmetric, the value of the information would clearly decline – if not vanish – when certain character traits were only to show up in small minorities of the underlying population.

--- Include Table 4 about here. ---

Since we deem the decision of returning trust as more indicative of an individual’s concern for reciprocity in the gift-exchange game, we focus in the remainder of the analysis mainly on the behavior of subjects as a second mover in the described trust game. We control for the impact of the same socio-demographics on trustworthiness in a probit regression; the results can be found in Table 4. We find a positive gender and age effect on trustworthiness, but there is no treatment effect between PT and P/T. A quantitative subject of study decreases the probability of returning trust and all personality traits are insignificant with the exception of ”conscientiousness“. We also control for the number of correct answers in part one on the propensity to reciprocate trust, but do not find any effect which confirms that there is no relationship between our measure of productivity and reciprocal behavior in part one. The independence of our two measures is also supported by a nonparametric Spearman rank correlation test that yields $p = 0.82$ for the correlation coefficient; this is illustrated in Figure 3. We summarize these findings in our first result.

--- Include Figure 3 about here. ---

Result 1 *There are no differences across treatments in personal traits for individual productivity and reciprocal concerns. The two measures quantify two distinct dimensions of a person’s characteristics.*

4.2 Part Two: Gift Exchange Game

We subsequently present the results from the firm-worker interaction, where initially, i.e. before the measures are elicited, all workers were told which set of information would be disclosed to firms. This set of information consisted in

- worker productivity from part one (in treatments PT and P) and
- the decision whether to return trust or not in part one (in treatments PT and T).

Firms do not have any experience or knowledge on how workers behave such that we consider firms’ wage policies as the cleanest possible measure of their preferences for information about

workers.

In the analysis of the data from the gift-exchange game, we will focus on the PT sessions where both measures about workers were revealed to firms in a binary way. We will also comment on the results of the two control treatments P and T, but since the set of information firms could condition their wage policy on is smaller, we refrain from directly comparing decisions in P or T with PT.

4.2.1 Firm Behavior

We begin by looking at wage offers received by workers. Wages were bounded to be not negative and not above 250 such that the equal surplus split under maximum efficiency was achievable through a non-maximal wage offer.⁹ Every worker obtained one offer from each of the 12 firms, but just received and saw the relevant wage level for him, that the matched firm had entered for him in the wage setting stage. As a consequence, we can analyze all 12 wage offers directed to a worker through the strategy method, i.e. we have $96 * 12 = 1152$ observations in the PT treatment. The average wage that was submitted to one single worker was 82.5, whereas the average of the actually randomly determined *relevant* wage offer was 80.7. The percentage of workers that returned trust in part one was 52.1 %.

The level of performance¹⁰ in the gift exchange game by subjects is 21.7 and significantly lower than the elicited productivity measures in part one of 28.0 (Wilcoxon signed-ranks test, $p < 0.01$). Hence, there is evidence that subjects do not put in unconditionally full effort levels in the gift-exchange phase. Furthermore, we see that the random attribution of roles to workers and firms has not distorted our two measures in the sense that the sample means of a session (24 subjects) lie close to the means of the workers (12 subjects). To get a deeper understanding on how firms set wages in PT, we run a series of firm fixed effect regressions that predict the wage offer to a specific worker; results are reported in Table 5.

⁹We refer to an outcome where the worker gives the maximum of 45 correct answers and receives a wage of 225, which would yield a payoff of 225 to both the firm and the worker.

¹⁰In what follows, we term “performance” the number of correct answers given by workers in the gift exchange relation with firms, to draw a clear semantic distinction to the measure of “productivity” in part one.

--- Include Table 5 about here. ---

We find a positive impact of being a high productivity and a trustworthy worker on the offered wage level, which is highly significant for both traits in specification I. When comparing the relative sizes, it is immediate to see that employers provide a higher wage premium for the productivity measure than for the trustworthiness measure. The wage premium is more than twice as high for the productivity measure compared to the trustworthiness measure. In specification II, we divide all workers into four categories. To do so, we classify a worker as being of “high productivity” or “low productivity” as well as being “trustworthy” or “not trustworthy”. The left out category is a worker who is neither of high productivity nor trustworthy. We confirm the findings of specification I, i.e. the presence of information about each dimension of a worker increases the wage a firm offers in the first place. All three coefficients are significantly different from each other (t-test between two coefficients, all three tests below $p = 0.0042$) indicating that there is little substitution of the wage premia between the two dimensions. If a worker moves from the lower to the higher category in one dimension, this yields a constant wage premium regardless of her position in the other category. These results support our hypothesis from section 3 for the impact of information on wage setting behavior by firms.

Result 2 *Firms are willing to pay a significant wage premium for both characteristics. The premium for being a high productivity worker amounts to roughly double the premium for being trustworthy.*

We also explore whether a firm’s wage offer is, next to worker characteristics, also affected by the firm’s own characteristics (own coding task performance and behavior in the trust game, gender, age, field of study). All these turn out to be insignificant. Table 6 contains these results.

--- Include Table 6 about here. ---

In the two control treatments P and T, information about one dimension of worker characteristics is not available to firms. When we run identical firm fixed effects regressions for these

two treatments, we find a significant wage premium for productivity in the P treatment, but not for trustworthiness. In the T treatment, there is a positive premium for being trustworthy, but this is not significant. As expected, there is no wage premium for productivity in T. We take this as further evidence that information about workers matters for wage offers, but more so for the productivity dimension than for the measure of trustworthiness; results are reported in Table 7. We next look at worker behavior in terms of effort provision.

--- Include Table 7 about here. ---

4.2.2 Worker Behavior

With respect to effort levels, the question arises of what influences workers most in their decision to provide effort. Along the lines of the gift exchange literature, one can argue that the main driving force will be a high wage offer such that subjects reciprocate by exerting high levels of effort. This notwithstanding, the characteristics of a person in terms of productivity and intrinsic willingness to perform well at a given task can similarly affect actual effort levels. To find out more about which of these rationales helps to explain worker behavior in the working phase, we regress the number of correct answers on the offered wage, both measures of worker characteristics and a set of controls which are reported in Table 8.

--- Include Table 8 about here. ---

We find that the offered wage has a highly significant positive impact on the amount of effort, which we take as a clear sign that gift-exchange considerations play a role in our real-effort experiment. Controlling for the wage which already includes information about worker types, a higher productivity measure from part one does not increase the performance in the interaction between firms and workers. However, reciprocal concerns among workers (i.e. being trustworthy) are predictive for the effort decision. A worker that has returned trust in part one of the experiment, gives on average 4.5 more correct answers than a worker who has not returned trust. While there is an effect in wage levels, there is no effect in slopes as the interaction of the worker characteristics with the wage turns out to be

insignificant (specification III). This is indicative of wages being set optimally by firms with all informational value from types already incorporated into the wage offer. When we control for other socio-demographic characteristics of workers, we do not find significant gender and age, but a significantly negative quantitative dummy in specification III. There is no evidence for a relationship between performance and characteristics in *excess* of the wage. This is underlined by specification II where in the absence of the relevant wage, personal characteristics are highly significant predictors for worker performance – a relationship that vanishes in specification III. We summarize this in our next result.

Result 3 *The wage offer has a significant impact on performance in the gift exchange game. We do not find an additional positive impact from worker’s characteristics on performance in excess of the one already embodied in the wage offer.*

4.2.3 Performance of Wage Contracts in the Gift Exchange Game

Putting the wage setting decision by firms and the worker’s effort decision together, we turn to the analysis of the profitability of firm’s wage policies given the information they have about workers. We pool all contracts concluded in the full information treatment PT and allocate all workers into the four above mentioned broad categories.¹¹

--- Include Table 9 about here. ---

Table 9 shows the key summary statistics for all 96 concluded contracts in the PT treatment. We find that particularly the interaction of both worker characteristics produces high levels of performance from workers (i.e. the number of correct choices given in the gift-exchange relation). These workers give close to 10 correct answers more than all other workers which deliver comparable outputs. Looking at the column “Ratio” leads towards an explanation. Ratio, defined as *performance/productivity*, captures to what extent workers realize their productivity potential as elicited in the piece rate condition in part one. While trustworthy

¹¹All results in this section are identical if we were to include the data from the two control session P and T, but we want to exclude any possible effect that the absence of one piece of information might have both on firms and workers in what follows.

types realize on average 91% of their potential as elicited with the productivity measure in the piece rate task in part one, not trustworthy types achieve only between 60-71% on average. Although they are now in a situation characterized by moral hazard and absent material incentives, they still perform on average very close to their productivity. This is confirmed non-parametrically by Wilcoxon-signed-ranks-tests on the difference between productivity and performance for each worker type separately. While there is no difference between the two for trustworthy types (h0r1: $p = 0.62$, h1r1: $p = 0.97$), we find a significant difference for the not trustworthy types (h1r0: $p < 0.01$, h0r0: $p = 0.04$). Hence low productive but trustworthy workers generate substantial profits because they make up for their low productivity with their “work ethics”.

Wages are increasing in worker characteristics and significantly different according to the worker type identified (Kruskal-Wallis-Test on session averages, $p < 0.01$) and we also find a difference in firms’ profits (Kruskal-Wallis-Test on session averages, $p = 0.02$). The super-additivity of these two characteristics produces highest levels of efficiency and performance only if they both are present at the same time.

Result 4 *Workers that have both characteristics – high productivity and trustworthiness – provide significantly higher levels of effort which leads to higher firm profits. In the absence of at least one of the character traits, efforts and firms’ profits are substantially lower in a firm-worker interaction. Trustworthy workers are more likely to live up to their full productivity potential under moral hazard than not trustworthy workers.*

--- Include Figure 4 about here. ---

Table 9 shows that on average firms reap higher profits when interacting with trustworthy workers. Inspecting the left panel of Figure 4 we see that in addition to an on average higher performance, firms also have to bear less variability in output (performance) when a trustworthy worker is employed. This translates into less dispersed profits, too (right panel of Figure 4). An interaction with a trustworthy worker very likely generates substantial profits, whereas with not trustworthy workers firms run the considerable risk of making losses. The middle panel in Figure 4 shows that the median of the “ratio“ variable for the

two trustworthy types is even above the productivity measure, i.e. above one. For not trustworthy types mean and median are substantially below this threshold. This pattern further qualifies why firms make as much profit with low productive but trustworthy workers as with high productive but not trustworthy workers. While the former are less able, they are able to compensate the ability gap through superior levels of “morale” producing lower variability in output. We sum up these findings in our next result:

Result 5 *Trustworthy workers generate not only higher but also less variable profits for firms.*

--- Include Figure 5 about here. ---

The heterogeneity in the Ratio variable leads to the pattern captured in Figure 5: Performance in our labor market (right panel) is substantially more dispersed than productivity in the piece rate task (left panel). The reason for this is that heterogeneity in the trustworthiness dimension – which is orthogonal to the productivity characteristic – affects behavior under moral hazard. These results are a cautionary tale for us how to interpret ability measures elicited in the labor market. We end this section by trying to better understand what exactly drives these different behavioral responses. Since firms cannot actively choose their worker in our design, but are allocated a worker at random and can only offer different wages, we next proceed to analyzing how the different types of workers react to an offered wage. I.e., we want to understand whether there are differences in the nature of gift exchange across the four worker types.

--- Include Figure 6 about here. ---

In Figure 6 we plot a firm’s profit against the implemented wage separately for the four worker types. It can be seen that for high productivity and trustworthy workers the level of performance is higher than for the others, but surprisingly profit does not increase in the wage for these types. The highly significant constant ($p = 0.01$) together with a virtually flat wage-profit relation for these types further illustrates the unconditional level of worker

performance and firm profits in interactions with high productivity and trustworthy subjects. For the remaining three types, we find a positive wage-profit-relationship, that albeit starts on a substantially lower level. It is only for the low productive and reciprocal types that the wage-profit relationship is significantly positive ($p = 0.014$). For the not trustworthy subjects the slope is not significantly different from 0.

Taken together, the results from the firm-worker interaction suggest that some form of gift-exchange robustly generates above minimum profits across all identified types of workers. For the high productivity and trustworthy subjects it apparently suffices to make them a sizable wage offer. They are inclined to reciprocate and it is not hard for them to deliver the return gift. However, they do not react to a further increase of the wage offer. In contrast, all remaining types exhibit positive wage-effort relations, but only for trustworthy workers this relation is robust. Worker types affect firm profits and are rightly taken into account by firms when writing contracts. Only if information about worker types is available, firms can increase their profits by tailoring the corresponding wage level to a specific worker. Exploiting the interaction between the incentive scheme and worker types therefore crucially depends on the amount of information accessible to firms before writing contracts.

Result 6 *Gift exchange is present for all types, but is most profitable for firms in the interaction with trustworthy types. Optimal wages depend on worker characteristics such that information about workers is valuable for firms to tailor incentives to worker types.*

5 Discussion and Conclusion

This paper presents evidence from a laboratory gift-exchange experiment indicating that firms condition their wage policies on available information about worker productivity and worker's trustworthiness. Firms offer more generous wages to workers who are, according to the elicited measures, more productive and more trustworthy. Our results suggest that workers with higher productivity and high levels of trustworthiness earn wage premia on the labor market.

The availability of information has an impact on final outcomes since firms can adapt con-

tract offers to specific worker types in order to make use of complementarities between wages and types. Our results show that gift-exchange considerations play a role across all types, but they are strongest for trustworthy workers. From the firm's perspective, optimal wages therefore depend on the worker's type; profits are highest for high-productivity workers who are trustworthy. For the firm's optimal contracting strategy, complementarities with respect to different worker characteristics are crucial.

While moral hazard is an important friction that governs contracts and incentives on labor markets, we argue in this paper that there is a crucial interaction between worker heterogeneity and the contractual incompleteness of labor markets. The role of a worker's individual productivity for contracting outcomes has long been acknowledged. We show that if a firm takes heterogeneity with respect to trustworthiness into account, it affects the effectiveness of gift-exchange and hence its profit levels. Since the interaction between worker types and the solution to the moral hazard problem matters, the existence of information about worker characteristics contains an economic value for firms even in the absence of competition for workers. Given the resources spent on information acquisition by firms for the hiring decision, we consider the role and the acquisition of information on labor markets a field of high economic relevance and a promising topic for future research to foster our understanding of the functioning of labor markets.

Appendix: Elicitation of Expectations

In this appendix, we describe in details the elicitation of subjects' expectations about the characteristics in the population in an incentivized manner. After the elicitation of the productivity and the trustworthiness measure, all participants are asked to estimate the number of correct answers in the productivity task reported before. A correct guess of the session average is rewarded with a prize of 100 EP, from which 10 points are deducted for every correct answer that the guess was away from the true value. If the difference between the guess and the true value exceeded 10 answers, subjects earned at worst 0 EP from this part.¹² In a similar vein, we ask subjects how many of the 24 subjects in their session have chosen to reciprocate trust. Subjects are rewarded for the precision of their guesses with a prize of 100 EP if their estimate was correct, and 20 points were deducted for every subject that their guess was away from the true value. Hence, if their guess was more than 5 subjects away from the true value, earnings were 0 EP from this part. Expectations were elicited referring to the current session (24 subjects) of the experiment, which we consider sufficiently large that subjects perceive their impact on the session average small enough to enter their expectation about the whole population.

With no feedback about the choices of other participants, individual expectations about the population are a likely candidate to explain the decision on contract choices later on. The resolution of this part took also place at the end of the experiment, such that subjects entered the gift exchange without any information about the behavior of other subjects in the experiment.

--- Include Table A.1 about here. ---

We report in Table A.1 both subjects' expectations and actual realizations in the three treatments. As far as the number of correct choices in part one is concerned, subjects underestimate the average number of correct outcomes by about 4 answers compared to the true average value. This is confirmed by a highly significant Wilcoxon-signed-rank-test

¹²Subjects could only enter integer guesses and the average was rounded to an integer.

between the own performance and the guess of the average productivity measure ($p < 0.01$). Indeed, out of all 288 subjects, 213 give a lower expectation of the average than their own coding performance in part one, 59 a higher one and 16 subjects consider themselves to be average. Note however that at this point of the experiment, subjects do not know their performance in part one explicitly but only implicitly from remembering how many correct answers they gave.

--- Include Table A.2 about here. ---

When asked about the number of subjects that returned trust in their session, guesses are more accurate and subjects correctly predict that a bit less than half of the participants chose to return trust. To gain a deeper understanding of what drives the formation of expectations, we regress expectations on behavior in part one and include the control treatments P and T. From Table A.2 it is immediate to see that subjects are strongly (positively) influenced by their past behavior, which is suggestive evidence for the false consensus effect. For estimating the average number of correct choices in the elicitation task, the own result is highly significant. In a similar manner, having trusted and returned trust oneself increases one's expectation of the number of subjects that return trust within a session significantly. Socio-demographics do not matter for expectations and we find that there are no effects from a specific characteristic on expectations about the respective other characteristics.

Tables and Figures

Table 1: Descriptive Statistics on the Measure of Productivity

Treatment	Mean	S.D.	N	Median	Min	Max
PT	28.30	6.30	192	28.0	9	45
P	28.89	8.08	48	29.5	0	45
T	29.46	6.06	48	29.5	14	41
Total	28.59	6.58	288	29	0	45

This table contains descriptive statistics for coding performance in the real effort task in Part 1, by treatment.

Table 2: Determinants of Productivity

Dep. Var.: # Correct Choices	I	II
Female	-0.646 (0.813)	-1.050 (0.913)
Age	-0.234*** (0.086)	-0.216** (0.087)
Quant	0.443 (0.917)	0.574 (0.929)
Treatment P	0.967 (1.363)	0.972 (1.367)
Treatment T	1.126 (1.008)	0.983 (1.012)
Big Five (Extraversion)		0.339 (0.339)
Big Five (Agreeableness)		0.874** (0.394)
Big Five (Conscientiousness)		-0.079 (0.354)
Big Five (Emotional Stability)		0.115 (0.343)
Big Five (Openness)		0.124 (0.433)
Constant	34.205*** (2.271)	27.380*** (3.722)
Obs	264	264
R^2	0.04	0.07

This table contains the estimates of OLS regressions predicting the number of correct answers in Part 1. Coefficients show effects relative to answers in the PT treatment. Quant is a dummy for quantitative orientation of studies. All Big Five measures are on a scale from 1 to 7 indicating the strength of the individual personality trait. Due to a server breakdown at the end of one P session, the values of the socio-demographic variables are missing for one session (24 subjects). Standard Errors in brackets. *** represents significance at $p = 0.01$, ** at $p = 0.05$ and * at $p = 0.10$.

Table 3: Descriptive Statistics on the Measure of Trustworthiness

Treatment	no trust no returntrust	trust no returntrust	no trust returntrust	trust returntrust	Total
PT	32.8%	18.2%	13.0%	35.9%	100%
P	35.4%	14.6%	6.3%	43.8%	100%
T	45.8%	16.7%	18.8%	18.8%	100%
Total	35.4%	17.4%	12.9%	34.4%	100%

This tables contains the percentage of subjects trusting and returning trust in the binary trust game in Part 1, by treatment.

Table 4: Determinants of Trustworthiness

Dep. Var.: 1 if Trustworthy	I	II	III
Female	0.319*	0.327**	0.359*
	(0.166)	(0.166)	(0.189)
Age	0.033*	0.036*	0.041**
	(0.019)	(0.019)	(0.020)
Quant	-0.365*	-0.369*	-0.353*
	(0.189)	(0.189)	(0.193)
Treatment P	0.238	0.229	0.279
	(0.281)	(0.282)	(0.286)
Treatment T	-0.307	-0.319	-0.347
	(0.209)	(0.210)	(0.214)
Big Five (Extraversion)			0.063
			(0.071)
Big Five (Agreeableness)			0.073
			(0.082)
Big Five (Conscientiousness)			-0.128*
			(0.074)
Big Five (Emotional Stability)			0.067
			(0.072)
Big Five (Openness)			0.015
			(0.090)
# Correct Choices		0.009	0.007
		(0.013)	(0.013)
Constant	-0.912*	-1.262*	-1.693*
	(0.490)	(0.879)	(0.885)
Obs	264	264	264
Pseudo R^2	0.05	0.05	0.06

This table contains the estimates of Probit regressions predicting trustworthiness in Part 1. Coefficients show effects relative to answers in the PT treatment. Quant is a dummy for quantitative orientation of studies. All Big Five measures are on a scale from 1 to 7 indicating the strength of the individual personality trait with 1 being very weak and 7 being very strong. Due to a server breakdown at the end of one P session, we are lacking the socio-demographic variables of one session (24 subjects). Standard Errors in brackets. *** represents significance at $p = 0.01$, ** at $p = 0.05$ and * at $p = 0.10$.

Table 5: Determinants of the Wage Offer

Dep. Var.:	I	II
Wage offer	PT	PT
1 if high	26.591*** (2.780)	
1 if trustworthy	10.263*** (1.875)	
1 if high and trustworthy		36.932*** (2.707)
1 if high and not trustworthy		26.016*** (2.851)
1 if low and trustworthy		9.798*** (2.130)
Constant	66.100*** (1.352)	66.319*** (1.282)
Obs	1152	1152
R^2	0.13	0.13

This table contains the estimates of linear panel regressions predicting the wage offer with firm fixed effects. Standard errors (clustered on the session level) in brackets. *** represents significance at $p = 0.01$, ** at $p = 0.05$ and * at $p = 0.10$.

Table 6: Determinants of the Wage Offer Including Firm Characteristics

Dep. Var.:	I	II
Wage offer	PT	PT
1 if high productive worker	26.592*** (2.781)	26.602*** (2.787)
1 if trustworthy worker	10.243*** (1.867)	10.254*** (1.868)
1 if high (firm)	-1.182 (7.943)	-3.092 (10.424)
1 if trustworthy (firm)	2.412 (5.114)	7.105 (10.424)
Female (firm)		-14.163 (11.597)
Age (firm)		-0.932 (0.960)
Quant (firm)		6.528 (10.168)
Constant	65.553*** (5.005)	94.415*** (33.857)
Obs	1152	1152
R^2	0.13	0.18

This table contains the estimates of linear panel regressions predicting the wage offer with firm random effects. Standard errors (clustered on the session level) in brackets. *** represents significance at $p = 0.01$, ** at $p = 0.05$ and * at $p = 0.10$.

Table 7: Determinants of the Wage Offer in the Control Treatments P and T

Dep. Var.:	I	II
Wage offer	Treatment P	Treatment T
Worker Productivity	3.204*** (0.021)	0.109 (0.022)
Worker's Trustworthiness	1.068 (1.835)	18.223 (11.048)
Constant	14.431* (1.314)	81.953** (2.986)
Obs	288	288
R^2	0.17	0.04

This table contains the estimates of linear panel regressions predicting the wage offer with firm fixed effects. Firms are shown the productivity measure of Part 1 as a continuous variable. Return trust is a dummy variable for worker behavior in Part 1. Standard errors (clustered on the session level) in brackets. *** represents significance at $p = 0.01$, ** at $p = 0.05$ and * at $p = 0.10$.

Table 8: Determinants of Effort

Dep. Var.: # Correct Choices	I	II	III
	PT	PT	PT
Wage	0.161*** (0.032)		0.188*** (0.055)
1 if productive	0.677 (2.424)	6.007** (2.442)	2.869 (6.136)
1 if trustworthy	4.467** (2.164)	5.788** (2.409)	5.465 (5.173)
Wage \times 1 if productive			-0.028 (0.065)
Wage \times 1 if trustworthy			-0.014 (0.059)
Female			-1.062 (2.263)
Age			-0.147 (0.225)
Quant			-4.971** (2.503)
Constant	6.075** (3.864)	16.149*** (2.037)	9.787 (7.237)
Obs	96	96	96
R^2	0.30	0.11	0.41

This table contains the estimates of OLS regressions predicting the number of correct choices in the gift exchange game. Standard errors (clustered on the session level) in brackets. *** represents significance at $p = 0.01$, ** at $p = 0.05$ and * at $p = 0.10$.

Table 9: Descriptive Statistics for Contracts in the PT treatment

Worker Type	PT treatment				
	Performance	Ratio	Firm's Profits	Wage	Obs
high productivity & trustworthy (h1r1)	29.9	91 %	171.4	102.8	20
high productivity & not trustworthy (h1r0)	20.3	60 %	100.9	97.0	20
low productivity & trustworthy (h0r1)	20.7	91 %	123.9	71.7	30
low productivity & not trustworthy (h0r0)	17.6	71 %	102.3	61.7	26
Total	21.7	79 %	135.9	80.7	96

This table contains descriptive statistics of all contracts for the four different worker types in the PT treatment. Ratio is defined as performance/productivity.

Table A.1: Descriptive Statistics on Expectations

Treatment	# Correct Choices realized	# Correct Choices expected	# Returntrust realized (per session)	# Returntrust expected (per session)
PT	28.3	23.6	11.7	10.7
P	28.9	23.9	12	10.5
T	29.5	26.0	9	9.8
Total	28.6	24.3	11.3	10.5

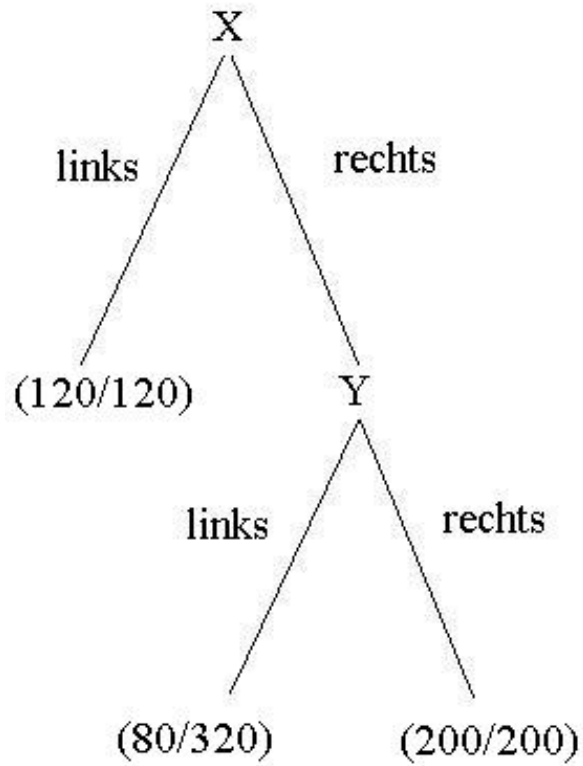
This table reports sample averages of subjects' expectations and of the realizations of productivity and trustworthiness.

Table A.2: Determinants of Expectations

	Dep. Var.: Expected Average of Correct Choices	Dep. Var.: Expected # Subjects Returning Trust
# Correct Choices	0.512*** (0.051)	-0.052 (0.049)
1 if trust	-0.311 (0.688)	2.665*** (0.664)
1 if trustworthy	0.737 (0.698)	6.651*** (0.674)
Female	0.517 (0.649)	0.123 (0.627)
Age	-0.053 (0.071)	-0.021 (0.068)
Treatment P	-0.204 (1.115)	-0.409 (1.076)
Treatment T	1.463* (0.834)	0.429 (0.805)
Constant	10.313*** (2.510)	7.916*** (2.422)
Obs	264	264
R^2	0.31	0.41

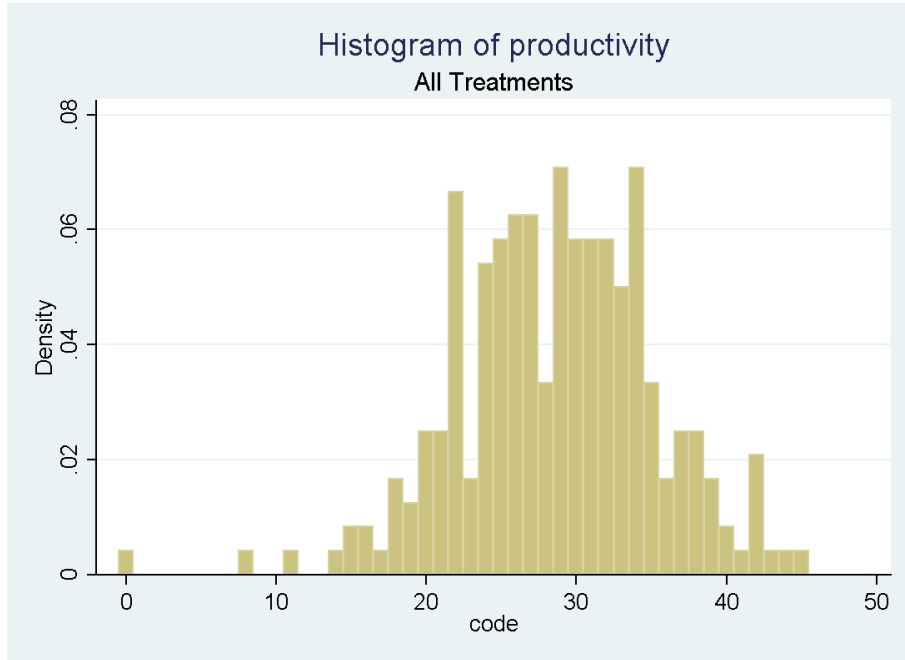
This tables contains estimates of OLS regressions predicting expectations. Coefficients show effects relative to answers in the PT treatment. Trust and returntrust are dummy variables for behavior in part one. Due to a server breakdown at the end of one P session, we are lacking the socio-demographic variables of one session (24 subjects). Standard errors in brackets. *** represents significance at $p = 0.01$, ** at $p = 0.05$ and * at $p = 0.10$.

Figure 1: Trust Game



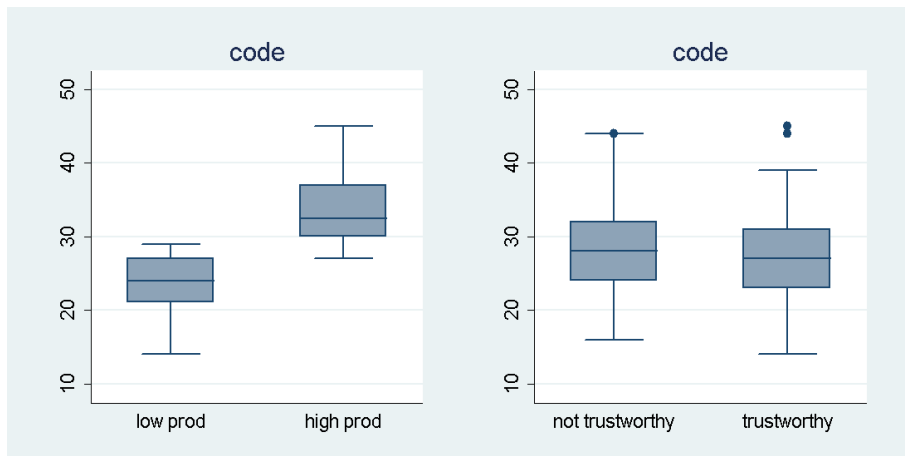
Graphic representation of the trust game as shown to subjects. Subjects had to choose as person X (first mover) and as person Y (second mover), where at each point they could choose between "left" and "right". The corresponding payoffs are given in experimental points (EP), with the first mover's payoff listed first.

Figure 2: Histogram of Productivity in Part 1



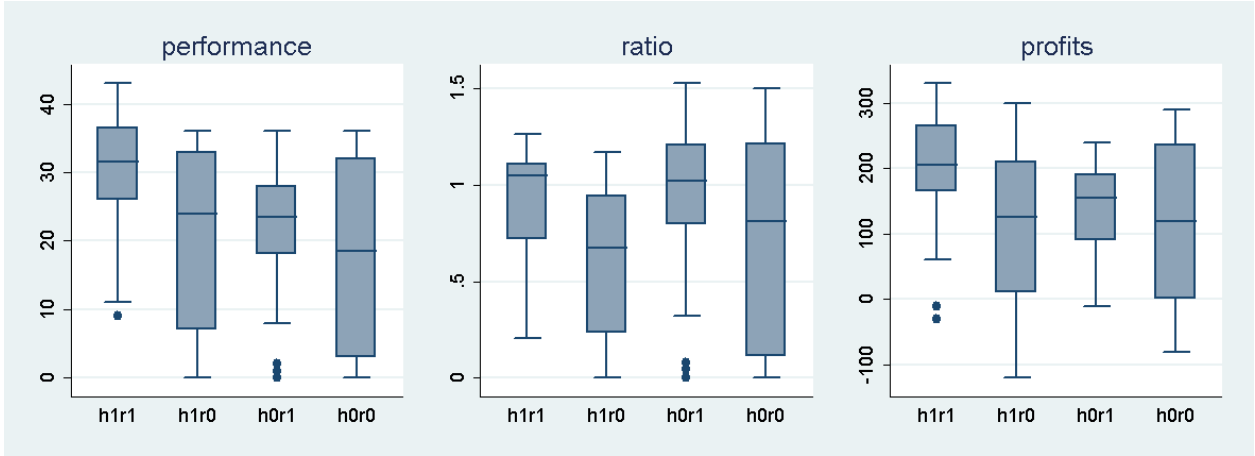
Histogram of productivity measured in Part 1 (number of correct answers); all treatments (PT, P, T) combined.

Figure 3: Boxplot of Productivity in Part 1



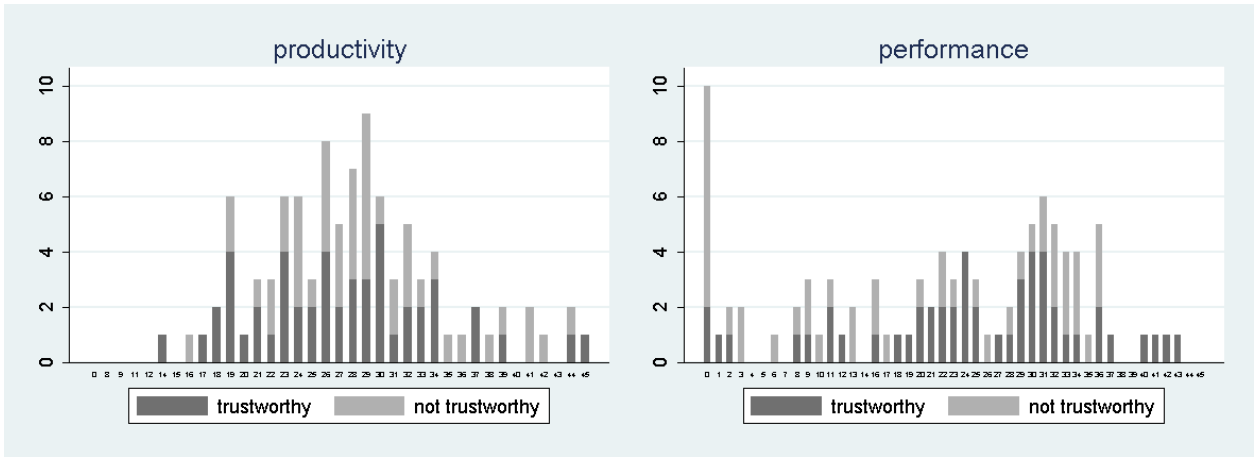
Boxplot of productivity measured in Part 1, stratified by worker characteristics elicited in Part 1, productivity (left panel) and trustworthiness (right panel). Graphs show the median, 25th and 75th as well as 5th and 95th percentile.

Figure 4: Boxplot of Performance, Ratio, and Profits in Part 3



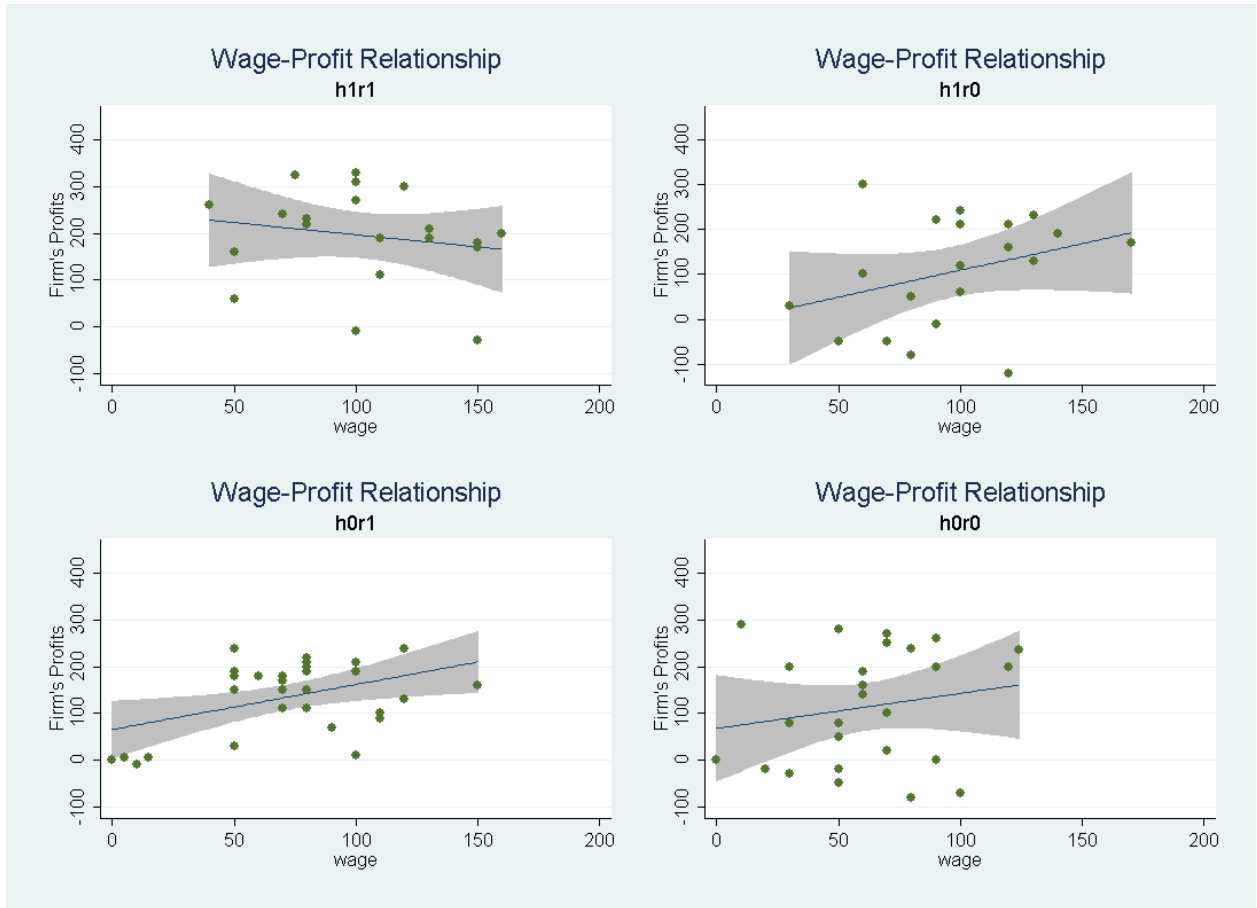
Boxplots of performance, ratio (performance/productivity), and profits measured in Part3, stratified by worker types elicited in Part 1: high productivity & trustworthy (h1r1); high productivity & not trustworthy (h1r0); etc.). Graphs show the median, 25th and 75th as well as 5th and 95th percentile.

Figure 5: Histogram of Productivity and Performance



Histogram of the productivity measure (left panel) and the performance measure (right panel) in treatment PT. Frequencies according to worker trustworthiness.

Figure 6: Wage-Profit Relationships



Scatterplot of the wage-profit relation for four identified types in PT. "h1r1" stands for high productivity and a subject that returned trust. h1r0, h0r1 and h0r0 are defined accordingly. The shaded areas indicate the 95 % confidence interval around the linear regression line.

Screenshots

Screenshot S.1: Real-effort Task

Periode

1 von 1

Verbleibende Zeit [sec]: 26

Schlüssel

Album: 2702 - Baum: 3693 - Katze: 9738 - Blatt: 8376 - Tisch: 4677 - Vogel: 3132 - Auto: 9520 - Jacke: 6115 - Ball: 5437 - Eimer: 7663 - Flora: 8363 - Apfel: 2052 - Tiger: 3968 - Markt: 3750 - Knopf: 4156

Zuordnung

a) 2702 b) 7859 c) 1392 d) 5292	a) 3693 b) 8099 c) 3260 d) 7779	a) 9738 b) 9368 c) 6935 d) 8394	a) 7629 b) 6281 c) 6738 d) 8376	a) 5876 b) 4677 c) 6171 d) 5574
Album <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Baum <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Katze <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Blatt <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Tisch <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)
a) 6168 b) 1441 c) 3132 d) 4924	a) 8053 b) 7659 c) 3698 d) 9520	a) 1795 b) 6115 c) 8885 d) 2705	a) 5437 b) 1800 c) 7231 d) 1626	a) 7663 b) 9518 c) 8127 d) 3992
Vogel <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Auto <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Jacke <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Ball <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Eimer <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)
a) 8363 b) 9016 c) 6315 d) 6748	a) 1447 b) 2052 c) 1086 d) 3986	a) 7167 b) 1707 c) 3968 d) 5014	a) 5807 b) 8865 c) 5942 d) 3750	a) 1273 b) 4156 c) 5898 d) 2596
Flora <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Apfel <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Tiger <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Markt <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Knopf <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)

Screenshot of the real-effort task: The key is shown in the upper half of the screen, the matching is done in the lower half of the screen. Subjects had 30 seconds for each of the three screens.

Screenshot S.2: Wage Entry Screen for Firms

Periode

1 von 1

Verbleibende Zeit [sec]: 27

Teil IV

Ihre Lohnangebote an die Arbeitnehmer

Arbeitnehmer (-,-)	<input type="text"/>
Arbeitnehmer (-,-)	<input type="text"/>
Arbeitnehmer (-,-)	<input type="text"/>
Arbeitnehmer (-,-)	<input type="text"/>
Arbeitnehmer (-,-)	<input type="text"/>
Arbeitnehmer (-,-)	<input type="text"/>
Arbeitnehmer (-,-)	<input type="text"/>
Arbeitnehmer (-,-)	<input type="text"/>
Arbeitnehmer (-,-)	<input type="text"/>
Arbeitnehmer (-,-)	<input type="text"/>
Arbeitnehmer (-,-)	<input type="text"/>
Arbeitnehmer (-,-)	<input type="text"/>

OK

Hilfe
Geben Sie in den Feldern ein, welchen Lohn Sie als Arbeitgeber jedem einzelnen Arbeitnehmer für das Lösen der Zahlencode-Aufgabe zahlen wollen. Die Informationen über den Arbeitnehmer stehen dabei links vom Eingabefeld. Wenn Sie alles eingegeben haben, drücken Sie "Ok".

Wage entry screen for firms: Worker Characteristics are shown in brackets; firms enter one wage for every worker.

Instructions (translated from German)

PT/P/T

This experiment serves the investigation of economic decision making. In the experiment you and other participants¹ of the experiments are asked to make decisions. You can thereby earn money. Your decisions as well as the decisions of other participants determine your earnings from the experiment according to the rules explained below.

The whole experiment approximately lasts **1 hour and 15 minutes and consists of four parts**. First of all, you receive the instructions for part I. Instructions for parts II to IV are handed out to you at the beginning of the respective parts. For each part you are asked to enter your decisions into the computer. The parts are not independent of each other. This implies that decisions taken in one part of the experiment may sometimes (not always) affect other parts of the experiment.

Please raise your hand if you have any questions after reading through the instructions of during the experiment. One of the experimenters will then come to you and answer your questions in private.

While making your decisions, there is a clock counting down in the right upper corner of your computer screen. This clock serves as a guide for how much time it should take you. You may, of course, exceed the time limits. Once time has run out, it is only the pure information screens which will be dismissed as they do not ask you to make any decisions.

Payment

At the beginning of the experiment you receive **4 Euro** for arriving on time. During the experiment you can earn more money by collecting **points**. At the end of the experiment, the points get converted into Euro at the exchange rate of

$$\mathbf{1\ Point = 0.0125\ Euro\ (1.25\ EUROCENT)}$$
$$\text{that is } \mathbf{1\ Euro = 80\ Points}$$

At the end of the experiment the amount of money you earned during the experiment as well as your 4 Euro starting balance will be paid to you in cash.

Anonymity

At no point during or after the experiment you will find out with whom you interact and the identity of other participants. In turn, other participants will not find out your identity and your earnings at any point during or after the experiment. There is a possibility that decisions you took in [PT: parts I and II] [P: part I] [T: part II] are made public to other participants in later parts of the experiment. Please note that your identity remains secret all the same.

It is strictly prohibited to communicate with other participants during the experiment. Furthermore, please note that you may only use the functions of the computer that are part of the experiment. Communication or playing around with the computer results in exclusion from the experiment.

¹ For convenience, we only use male terms in the instructions. They should be considered as being gender neutral.

Part I

During the first part of the experiment you are asked to link terms to the numerical codes corresponding to them. The screen illustrating a representative scenario is displayed below:

Periode		1 von 1		Verbleibende Zeit [sec]: 26	
Schlüssel					
Album: 2702 - Baum: 3693 - Katze: 9738 - Blatt: 8376 - Tisch: 4677 - Vogel: 3132 - Auto: 8520 - Jacke: 6115 - Ball: 5437 - Eimer: 7663 - Flora: 8363 - Apfel: 2052 - Tiger: 3988 - Markt: 3750 - Knopf: 4156					
Zuordnung					
a) 2702 b) 7859 c) 1392 d) 5292	a) 3693 b) 8099 c) 3260 d) 7779	a) 9738 b) 9368 c) 8935 d) 8394	a) 7629 b) 8281 c) 6738 d) 8376	a) 5876 b) 4677 c) 6171 d) 5574	
Album <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Baum <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Katze <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Blatt <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Tisch <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	
a) 6188 b) 1441 c) 3132 d) 4924	a) 8053 b) 7659 c) 3698 d) 9520	a) 1795 b) 6115 c) 8885 d) 2705	a) 5437 b) 1800 c) 7231 d) 1626	a) 7663 b) 9518 c) 8127 d) 3992	
Vogel <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Auto <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Jacke <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Ball <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Eimer <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	
a) 8363 b) 8016 c) 8315 d) 6748	a) 1447 b) 2052 c) 1086 d) 3988	a) 7167 b) 1707 c) 3988 d) 5014	a) 5907 b) 8865 c) 5942 d) 3750	a) 1273 b) 4156 c) 5998 d) 2586	
Flora <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Apfel <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Tiger <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Markt <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	Knopf <input type="radio"/> a) <input type="radio"/> b) <input type="radio"/> c) <input type="radio"/> d)	

The upper part of the screen shows a code key that links specific terms to specific codes. The numerical code always consists of four digits. In the lower part of the screen, you have to assign terms to their respective numerical codes. For each term there are four possible codes, displayed as options a) to d), but **only a single one code among the listed one is correct**. Please click on the correct numerical code for each of the terms. The order of terms as shown in the key code is identical to the order of terms in the assignment task.

There are always 15 terms per screen and you are given 30 seconds per screen. This means that **after 30 seconds there is a new screen that pops up and contains 15 new terms and codes**. In total, you are given 90 seconds for the numerical code tasks, i.e. **three different screens** pop up one after the other.

The order in which you assign terms to their corresponding codes does not play a role. You may skip terms and you may go back to change your old decisions. All terms that you were not able to assign before the screen disappears after 30 seconds do not count for your final payment determination.

For every correct answer you receive **10 points**. For each wrong answer you get a **deduction of 10 points**. You may not run into a loss however, i.e. it's not possible to get minus points and **in the worst case your earnings amount to 0 points** in this part of the experiment. The difference between correct and wrong answers is called correct assignments.

$$\text{Correct assignments} = \# \text{ correct answers} - \# \text{ wrong answers}$$

You will only find out about your performance and thus about the amount of points you earned in this part of the experiment at the very end of the experiment. **Your earnings from this part of the experiment correspond to the sum of all points that you earned by giving correct answers reduced by the points that got deducted for each wrong answer.**

Example 1: You achieve 26 correct answers and 2 wrong answers. Your earnings amount to $(26-2) * 10 = 240$ points.

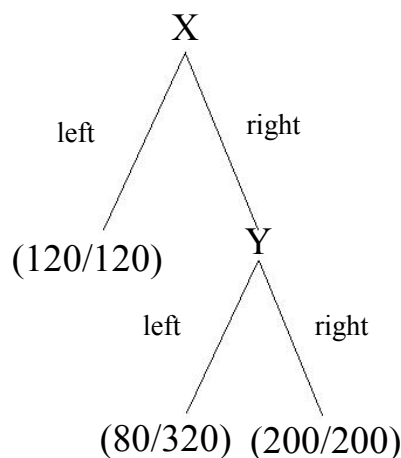
Example 2: You achieve 8 correct answers and 12 wrong answers. Your earnings are 0 points.

There will be a 60 second trial run of the numerical code task before the start of the experiment in order to get familiar with the computer program. The trail run is not part of the experiment and does not influence your final payments.

Part II

(parts and instructions were presented sequentially to subjects)

During the second part of the experiment you are asked to make two decisions, both of which refer to the following situation. Numbers correspond to the earnings in points from this part, and they are labelled in a way such that person X is always referred to first:



Person X chooses between “**left**” and “**right**”. If he decides for “**left**”, person X himself and person Y receive 120 points respectively from this part of the experiment. If he decides on “**right**”, it is person Y who decides on the final earning points in this part. If person Y chooses “**left**”, person X receives 80 points and person Y receives 320 points. If he chooses “**right**”, person X and person Y receive 200 points respectively from this part of the experiment.

You do not know whether you are person X or person Y. The decision is made by the computer at the end of the experiment only. You thus have to make two decisions: The first decision is implemented if you end up becoming person X (“left” or “right”). The second decision is implemented if you end up becoming person Y (“left” or “right”). At the end of the experiment the type of person (X or Y) is randomly assigned to you. Also, there is another participant who is randomly assigned to you and who takes on the respective other type of person.

It is only your decision of your randomly assigned person type that is relevant for your final earnings. This means that if you end up being person X (or Y), the decision that you took as person X (or Y) is relevant only. The final earnings from this part of the experiments are therefore not found out until the end of the experiment.

Please type your decisions into the computer and confirm by clicking OK. As long as you haven't used the OK button yet, you may change your decisions.

Part III

During the third part of the experiment you are asked for your assessment concerning all participants' past behaviour in parts I and II. **The better your estimates for the average outcomes in the first two parts, the more money you earn in part III.** You are asked to estimate two outcomes.

Estimate 1: How did the participants in part I of this experiment perform **on average**? As a reminder, part I dealt with the numerical code task. **Please enter your estimate for the average number of correct assignments of all participants in part I on your screen.** As defined in part I, the number of „correct assignments“ refers to the number of correct answers net of the number of wrong answers.

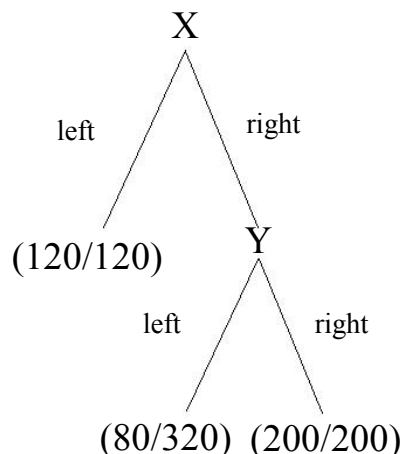
If your estimate is correct, you receive 100 points. Points will be reduced in case your estimate deviates from the true value. 10 points are taken away for each assignment that your estimate deviates from the true average of all participants. For example, in case your estimate deviates from the true value by one correct assignment, you receive 90 points. In case your estimate deviates from the true value by two correct assignments, you receive 80 points, etc. In case your estimate deviates by 10 or more correct assignments, you receive 0 points. You are informed on the true average and on your earnings from this part of the experiment only at the end of the experiment.

Example 1: *You estimate the average to be 20 correct assignments. The true average is 17 correct assignments. Your estimate thus deviates from the true average by 3 correct assignments. Consequently, you earn $100 - 3 \cdot 10 = 70$ points.*

Example 2: *You estimate the average to be 9 correct assignments. The true average is 22 correct assignments. Your estimate thus deviates from the true average by 13 correct assignments. Consequently, you earn 0 points.*

Estimation 2: How many of the 24 participants of this experiment chose **"right"** **being person Y** in part II? Please enter your estimate for the average number of correct assignments of all participants in part I on your screen. **If your estimate is correct, you receive 100 points.** 20 points are taken away for each participant that your estimate deviates from the true number of participants choosing "right". For example, in case your estimate deviates from the true number of participants by one participant, you receive 80 points. In case your estimate deviates from the true number by two participants, you receive 60 points, etc. In case your estimate deviates by 5 or more participants, you receive 0 points. You are informed on the true number of participants and on your earnings from this part of the experiment only at the end of the experiment.

The following figure serves as a reminder of the situation faced in part II:



Example 1: You estimate the number of participants that chose “right” being person Y to be 16. The true number is 14 participants. Your estimate thus deviates from the true number by 2 participants. Consequently, you earn $100 - 2 \cdot 20 = 60$ points.

Example 2: You estimate the number of participants that chose “right” being person Y to be 5. The true number is 12 participants. Your estimate thus deviates from the true number by 7 participants. Consequently, you earn 0 points.

Please note: The decision that participants took being person X does not play a role for this part. Your estimate merely concerns the decision that all 24 participants took being **person Y**. The type of person that is randomly assigned to the participants at the end of the experiment is irrelevant to this part. Your estimate should refer to all participants of this session.

Part IV

During the fourth part of the experiment, the computer randomly assigns a type of person to you. **There are two types of persons, employers and employees.**

Brief overview of part IV of the experiment:

Part IV of the experiment consists of two stages. The stages are structured as following:

1. **Employers and employees sign an employment contract.** In the first stage employers thus state which wage level they are willing to pay to which employee.
2. In the second stage of this part, each employer is randomly assigned to an employee who, once again, is given 90 seconds to solve the numerical code task of part I for the employer. The number of correct answers determines the earnings of the employer. The employee is paid the wage by the employer.

Detailed procedure:

There are 24 participants in this room, i.e. there are exactly 12 employers and 12 employees. On the first screen of this part you are told which type of person you are (employer or employee).

1. Determination of wages

In a first step, employers state which wage level they are willing to pay to which of the 12 employees in return for them solving the numerical code task in stage 2 of this part. For this purpose, employers get two pieces of information about **each employee**: The number of correct assignments (**# correct answers – # wrong answers**) **from part I** of the experiment, and the **decision of the employers that he took being person Y** in the decision situation of part II of the experiment.

Employers state a wage level for each of the 12 employees. Wage levels are entered in a table that looks like the following:

Periode: 1 von 1 Verbleibende Zeit [sec]: 27

Teil IV

Ihre Lohnangebote an die Arbeitnehmer

Arbeitnehmer (-, -)	<input type="text"/>
Arbeitnehmer (-, -)	<input type="text"/>
Arbeitnehmer (-, -)	<input type="text"/>
Arbeitnehmer (-, -)	<input type="text"/>
Arbeitnehmer (-, -)	<input type="text"/>
Arbeitnehmer (-, -)	<input type="text"/>
Arbeitnehmer (-, -)	<input type="text"/>
Arbeitnehmer (-, -)	<input type="text"/>
Arbeitnehmer (-, -)	<input type="text"/>
Arbeitnehmer (-, -)	<input type="text"/>
Arbeitnehmer (-, -)	<input type="text"/>
Arbeitnehmer (-, -)	<input type="text"/>

Hilfe
Geben Sie in den Feldern ein, welchen Lohn Sie als Arbeitgeber jedem einzelnen Arbeitnehmer für das Lösen der Zahlencode-Aufgabe zahlen wollen. Die Informationen über den Arbeitnehmer stehen dabei links vom Eingabefeld. Wenn Sie alles eingegeben haben, drücken Sie "OK".

The order of listed employees is random. **Information on each employee's behaviour in [PT: parts I and II] [P: part I] [T: part II] of the experiment are provided in brackets to the employers.**

[PT: If an employee achieved more correct assignments than the average in part one, he belongs to "group yellow". If he achieved less than the average in part one, he belongs to "group blue".] [P: The number of correct assignments in part one is given in brackets] [T: The decision in part two is given in brackets]

For example, the employee [PT: **(group yellow, right)**, P: **(5)**, T: **(right)**] [PT: achieved more correct assignments than the average in part I] [P: achieved 5 correct assignments in part I] and [PT/T decided on "right" in the decision scenario of part II]. Correspondingly, the employee [PT: **(group blue, left)**, P: **(13)**, T: **(left)**] [PT: achieved less correct assignments than the average in part I] [P: achieved 13 correct assignments in part I] and [PT/T decided on "left" in the decision scenario of part II].

Wage levels should be entered in the box labelled “Your wage offer“. The offer may not be smaller than 0 and exceed 250:

$$0 \leq \text{wage offer} \leq 250$$

Employers may enter a different wage level in each box or the same wage level for everyone or for some of the employees. **Employers have to fill in every box, i.e. they are required to make a wage offer to every employee.**

Employers find out which employee is allocated to them only in the second stage of this part. The allocation is done by the computer. An employer can get allocated to any employee.

Example: An employer offers a wage of 70 to the employee [PT: (group yellow, right), P: (5), T: (right)] and a wage of 130 to the employee [PT: (group blue, left), P: (13), T: (left)]. In case the employer gets allocated to the employee [PT: (group yellow, right), P: (5), T: (right)], the employer is required to pay him a wage of 70 in return for the employee performing the numerical code task.. In case the employer gets allocated to the employee [PT: (group blue, left), P: (13), T: (left)], the employer is required to pay a wage of 130.

While employers enter their wage offers, employees are asked to state their wage expectations and how certain they are on their expectations.

2. Task stage

One employee gets allocated to one employer for each task stage. The employee receives a wage in return for performing the numerical code task. Again, employees are given 90 seconds to this end.

In this setting, the wage corresponds to the one level offered by an employer to the respective employee in the previous stage. Earnings of an employer are determined by the number of correct answers net of wrong answers which is achieved by the employee in the numerical code task.

Employees do not find out to which employer they are allocated. Before starting the numerical code task, employees only find out the wage that they get paid.

Employers are shown the information on the employees that got allocated to them on their screens.

While employees work on the numerical code task, employers are asked to state their expectations on the number of correct answers of their employee and how certain they are on their expectations.

How are earnings determined?

Earnings of employers:

- Earnings of employers depend on the number of correct answers of their respective employee (net of wrong answers) as well as on the wage they offered to pay to their employee. Earnings are determined in the following way:

Earnings of an employer = 10 points * (# correct answers – # wrong answers) – wage

The employer receives 10 points for each correct answer of their employee (net of wrong answers). He is required to pay the wage to the employee from this revenue. Earnings of employers are thus higher the more correct

assignments their employee scores. Earnings of employers are lower the fewer correct assignments their employee score.

In case the employee has given more wrong answers than correct ones, revenues of the employer amount to 0 points in the worst case. But in any case, the employer is required to pay the wage to the employee.

Earnings of employees

- Earnings of employees are the wages that they receive from their respective employer. Earnings are determined in the following way:

Earnings of an employee = wage

Earnings of employees are thus independent of the number of correct and wrong answers in the numerical code task.

Earnings of all employers and employees are determined in the same way. **Consequently, every employee is able to compute the earnings of the employer he works for.**

Please note, that, in principle, it is possible to **incur losses**. You are required to settle losses using your show-up fee or earnings from other parts of the experiment.

Example 1: An employer offers a wage of 110 points to an employee and is allocated to this particular employee. If the employee achieves 21 correct and 2 wrong answers, the employer's earnings amount to:

$$10 * (21 - 2) - 110 = 10 * 19 - 110 = 190 - 110 = 80$$

The employee receives his wage of 110 points.

Example 2 An employer offers a wage of 80 points to an employee and is allocated to this particular employee. If the employee achieves 35 correct and 0 wrong answers, the employer's earnings amount to:

$$10 * (35 - 0) - 80 = 10 * 35 - 80 = 350 - 80 = 270$$

The employee receives his wage of 80 points.

Example 3: An employer offers a wage of 200 points to an employee and is allocated to this particular employee. If the employee achieves 15 correct and 4 wrong answers, the employer's earnings amount to:

$$10 * (15 - 4) - 200 = 10 * 11 - 200 = 110 - 200 = -90$$

This loss has to be settled with earnings from other parts of the experiment or the starting balance. The employee receives his wage of 200 points

You are informed on your earnings as well as the earnings of your partner at the end of part IV on a particular **screen showing your earnings**:

This screen contains the following information:

- Information on the employee from parts I and II of the experiment
- Number of correct assignments (# of correct answers – # wrong answers) of the employee
- Earnings of the employee (wage)
- Earnings of the employer

The experiment does not start until all participants have become familiar with the exact calculations of earnings. For this purpose, we kindly ask you to solve several practice exercises on your screens beforehand. Please raise your hand in case you have any questions.

At the very end of the experiment, the computer calculates your final earnings from parts I to IV and provides you with detailed earning information for each part of the experiment on the screen.

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