

Pay transparency, fairness perceptions and work effort: Experimental evidence from China

In a field experiment in China, we informed randomly selected workers that they earned less than others for the same work, without providing any explanation. The informed workers perceived their pay as less fair, but, surprisingly, increased their average output without reducing quality. A post-experiment survey revealed that workers had benign explanations for the pay difference, with 58% attributing it to higher quality of the better-paid workers. Our results are validated by a follow-up survey of 1100 people of working age in China whom we briefed about our findings and asked for their explanations. 57% believed that the informed workers perceived their higher-paid peers to be better workers and aspired to match them. When asked what they would do in a similar situation, 75% replied that they would work harder as well. Our findings question the predictions of fairness theory and suggest the importance of cultural norms in shaping reactions to pay transparency.

1 Introduction

Growing awareness of income inequality fuels public demand for equity which is met by policies that promote coworker pay transparency, among other measures. While pay transparency does reduce pay inequality (Obloj and Zenger, 2022; Cullen, 2024), it may also have performance consequences, especially for the lower-paid workers, which are the focus of this study. Specifically, we are interested in what happens to the effort of lower-paid workers once they learn that their peers earn more for the same work. We believe this question is sufficiently topical given that policies promoting pay transparency are likely to reveal information about coworkers' pay.

Intuitively, being informed of earning less than peers could negatively affect one's attitudes and performance. Earlier empirical studies found lower morale, happiness and job satisfaction reported by lower-paid workers after learning their position in the earnings distribution, relatively high earners being unaffected (Pfeffer and Langton, 1993; Luttmer, 2005; Clark and Oswald, 1996; Card et al., 2012). Furthermore, there is evidence showing that informed lower-paid workers tend to provide less effort than their equally earning but uninformed peers (Cohn et al., 2014; Gächter and Thöni, 2010; Nosenzo, 2013; Breza, Kaur, and Shamdassani, 2018; Cullen and Perez-Truglia, 2022), or reduce work quality (Greiner, Ockenfels, and Werner, 2011).¹

A combination of arguments drawn from theories of relative deprivation and fairness would predict pay transparency to have negative performance effects for the informed lower-paid workers, and thus rationalize the above findings. Relative deprivation theory defines one as relatively deprived if one wants but does not have an object X (e.g., higher pay rate), perceives X as feasible to obtain and observes other having it (e.g., thanks to pay transparency) (Runciman, 1966). Relative deprivation causes feelings of anger and resentment, resulting in a variety of negative behavioral reactions (Smith et al., 2012), including worse performance at work (Geurts, Buunk, and Schaufeli, 1994; Schreurs et al., 2020; Fan, Deng, and Li, 2023).

Theory of fairness posits the existence of inequity aversion (Fehr and Schmidt, 1999), implying a displeasure from receiving less than peers, which may result in actions to correct inequity even at one's own cost. Consistent with this theory are numerous experimental studies of behavior in the ultimatum game: the proposer shares a valuable resource between herself and a receiver, both get the allocated shares if the receiver accepts the offer and nothing otherwise (Oosterbeek, Sloof, and Van De Kuilen, 2004; Rand et al., 2013; Ho and Su, 2009). Contrary to the simple economic intuition that the receiver should accept whatever offer from the proposer, offers that are "too unequal" are frequently rejected. In the employment context, which is closer to our study, this behavior is captured in the "fair wage hypothesis" (Akerlof and Yellen, 1990) predicting workers to withdraw effort or quit altogether if their wage falls below what they consider as fair.

Fairness and relative deprivation theories agree in predicting negative performance consequences of pay transparency for lower-paid workers, but there is one important mediator between a pay difference revealed through pay transparency and a reaction to it that fairness theory adds: fairness perceptions. Specifically, the effect of pay transparency would depend on how (un)fair one's pay is perceived to be, which perception is influenced by context- and reference-dependent social comparisons (Falk, Fehr, and Zehnder, 2006) as well as personality traits ("fairness concerns") that vary across individuals (Abbink, Irlenbusch, and Renner, 2000). For instance,

¹Three are a few exceptions to the general finding of negative performance effects of pay transparency for low earners. Charness and Kuhn, 2007 find no effect. Liu-Kiel et al. (2013) find that informed power-paid workers worked harder than equally earning uninformed earners. Paying piece rather than hourly rates, they explain their finding through the loss aversion argument: workers earning a lower piece rate choose to produce more output to bridge the total income gap with those earning a higher piece rate.

one's pay would be perceived as less, or more, fair if pay transparency shifts one's reference point above or below one's previously held beliefs. Holding fairness concerns fixed, higher pay awarded to a more productive worker would be perceived as less unfair and would trigger a weaker reaction. In a given context, a given pay level would be perceived as less fair by lower-earning workers with stronger fairness concerns.

There is some empirical evidence to some of the above arguments. Bae et al. (2022) find that pay transparency improves pay fairness perceptions of blue-collar public sector workers in Korea who actually earned more than those in similar jobs in the private sector. In Breza, Kaur, and Shamdasani (2018), negative effects of pay transparency on lower-earning workers' performance disappear when they can observe that their higher-earning peers are more productive than themselves.

However, there is no study as yet on how fairness perceptions mediate reactions to pay transparency and how fairness concerns affect fairness perceptions. Another research gap is a dearth of evidence on the effects of pay transparency in China. Research on performance consequences of pay transparency has been done across the globe, but, to our knowledge, there is only one study involving Chinese participants, Liu-Kiel et al. (2013).

Our study fills these gaps by carrying out a real-effort field experiment in China (details in Section 2) in which we informed randomly selected workers that there were others receiving a higher pay rate. Our experiment produced results that are mostly inconsistent with fairness theory predictions. While the informed perceived their pay as less fair than equally earning but uninformed workers did, they increased their average output without reducing its quality. There were no protests about the pay or walk-outs. The fairness concerns we measured before the treatment were not related to pay fairness perceptions and did not moderate the treatment effects.

Although we did not provide any explanation for the pay difference, a post-experiment survey found that 58% of the lower-paid workers believed that higher-paid workers were better (this was not the case, since we assigned workers into the high and low pay conditions randomly), other explanations being benign as well. There is no relationship between the explanations the workers had for the pay difference and their reaction to our pay transparency treatment, possibly because nearly all explanations were benign.

Our findings are not easy to dismiss on the grounds of flawed study design or lack of external validity. With the many precautions we took in our procedures, our experiment meets the standards of similar studies in the pay transparency literature. A follow-up survey of China's working age population ($N = 1100$) lends external validity to some of our most surprising results. Specifically, when we briefed the survey respondents about our experiment and findings and asked them to provide their own explanations, 57% stated that the informed workers believed their higher-paid peers to be better workers and tried to match them by working harder

(58% of our workers had a similar explanation). Furthermore, when we asked the respondents what they would do in a similar situation, 75% said they would work harder.

The consistency between the main findings from our experiment and the China’s working population survey as well as the respondents’ predominant choice to work harder in a similar situation question the predictions of fairness theory in the context of China and call for more attention to cultural norms shaping reactions to pay transparency. We discuss how one cultural norm present in China – maintaining face – may explain our findings at the end of the paper.

2 Study Design

In our study design, we strove to combine the rigorous control of a laboratory experiment with the realism of a genuine working environment. This section describes and discusses the many choices we had to make to achieve this ambition. It is important to mention that our experimental design was reviewed and approved by the relevant Ethics Committee.

2.1 Context: worker recruitment, job task, and payment

We ran a real-effort field experiment to capture the causal effect of pay transparency free of possible confounds in an realistic yet controlled work environment. Our experiment took place on two weekends, 22nd-23rd October and 29th-30th October 2016, on campus of Shaoyang University in Hunan, China. The participants, henceforth workers, were Shaoyang students hired through regular job adverts (text in Appendix A 8.1) placed about a month before the start of the experiment. The advert asked interested persons to register online by October 10th 2016. There was no candidate selection, either stated in the advert or actually done, nor did we state any specific requirements for the job. We did not inform the workers that they were part of an experiment until the post-treatment survey in February 2017. Instead, we advertised a one-off, three-hour long job of digitizing paper questionnaires, paid per hour (not per questionnaire) and before the work started. The paper questionnaires were used in an earlier and unrelated research project (example in Appendix A 8.2). Each questionnaire is 9 pages long, contains 102 fields, and took 6.2 minutes to digitize, on average.

The job advert promised “attractive pay” without specifying the exact rate. The usual hourly rate for similar jobs on campus at the time was 20 RMB, which is what we paid to the majority of the workers once they showed up for work. A randomly selected 18 workers were paid a substantially higher rate, 35 RMB per hour, which was done to enable our treatment, as we explain next. The main focus of this study is on the regularly-paid (20 RMB per hour) workers.

Following the emerging trend in the empirical social science research to pre-register their research design (Miguel, 2021), we decided to pre-register our experiment with the American

Economic Association's registry for randomized control trials (registration ID AEARCTR-000XXXX [redacted for anonymity]). This way we committed ourselves to the experimental design and focal outcomes and moderators as we envisaged before seeing the data.

2.2 Treatment, outcome variables and moderators

Our treatment was to inform randomly selected regularly paid workers of being paid less for the same job than the other, higher-paid ones, who got paid 35 RMB/hour. The workers in the control condition were paid the same hourly rate as the treated workers (20 RMB/hour), but were not informed of the earnings of others. The higher-paid workers were not informed of the others' pay either, but we do not include them in the control group because they got a different pay rate.

We made it possible for the treated workers to find out about the others' pay via payslips they got and were asked to sign during the pre-scheduled 15-minute break in the middle of their work session, so as to confirm the receipt of their payment given to them before they started working. The control group workers got the same payslips at the same time during their work session, except that the payslips for the control group contained less detail on the payment. Specifically, as an example payslip in Appendix 8.3 shows, the treated workers saw both the regular (20 RMB/hour) and high (35 RMB/hour) hourly rates next to each other, with the regular hourly rate, applicable to them, circled, with an indication that it was us, the researchers, who determined the applicable pay rate.

As per our pre-registered experimental design, our key performance outcomes are: 1) the length of time it takes to digitise a paper questionnaire, and 2) the number of empty fields in a digitized questionnaire. We believe these variables are adequate measures of work effort. Our third key outcome is pay fairness perceptions measured in a survey after the experiment (details in Section 2.7). Our key pre-registered moderator variable is fairness concerns measured with a pre-experiment survey presented in Section 2.6. Additionally, in a post-experimental analysis, we estimated the treatment effect on the quality of work as measured by the number of mistakes per questionnaire discovered upon inspecting a random sample of questionnaires (details in Section 4.1).

2.3 Allocation into experimental conditions

After the registration deadline of October 10th expired, we randomly allocated the 514 workers who registered with us into the regular ($n = 492$) and high pay ($n = 22$) groups. Those ended up in the regular pay group were randomly assigned into the treatment and control conditions. We then put the workers allocated into the same condition into groups of three to six. Each group would work in a specified three-hour time slot and room, and was informed accordingly.

We used twelve seminar rooms on campus (example photo in Appendix 8.4) to host the groups during their work sessions scheduled in three time slots, 8am-11am, 11:30am-2:30pm, and 3pm-6pm, on four days. This gives a total of 12 rooms X 4 days X 3 time slots = 144 work sessions. However, 172 registered workers did not show up for work, so we ended up with 342 workers in 132 work sessions, 153 workers in 47 treatment sessions, 171 in 79 control sessions, and 18 in 6 high-pay (35 RMB/hour) sessions. Furthermore, owing to technical problems, we had to exclude observations from three workers in the treatment (2% of the total) and ten (6% of the total) in the control conditions, which leaves us with the usable sample of 150 workers in the treatment and 161 in the control conditions.²

Why did we end up with more control than treatment work sessions (79 vs. 47)? Wanting to minimize information spillovers between workers in different conditions in a communication-intensive college environment, we decided to form fewer treatment work sessions and schedule more of those later into the experiment (all high-pay sessions were therefore scheduled on the last day, October 23rd). Our efforts paid off: only 6% of workers who took part in a post-experimental survey stated that they had known of different pay rates being paid before coming to work. Excluding those workers does not change our results.

Table 1 reports descriptive statistics for $N = 514$ workers we randomly allocated into the treatment and control conditions at the registration stage, and for those $N = 172$ who did not show up. Looking at all the registrations, an average worker is 19.4 years old and is on their 1.6th year of study. The workers come from a variety of study majors. There is a slight over-representation of female workers (54.5% of registrations). Turning to those who showed up vs. no-shows, female, more junior and non-art major workers were statistically significantly more likely to show up. However, the differences between the shows and no-shows along these dimensions are small, and we control for those in our statistical analysis.

Focusing on the workers in the control vs. treatment conditions, there are no significant differences in their observable characteristics, with the exception of the average group size per work session, which is larger in the treatment (3.19) than control condition (2.04). This is because we planned fewer treatment work sessions, to minimize information spillovers, which meant more workers per session. Our results are robust to controlling for differences in group size per work session.

²We cannot ascertain the exact nature of the technical problems, because they became known after the affected work sessions were over and we retrieved the output the workers produced, but they manifested themselves by the absence of output either in the pre- (5 cases) or post-treatment (5 cases) periods, or both (2 cases), and in one case in the control condition the recorded entry time was implausibly long. The problems did not concentrate in any specific work sessions. It could be that some workers simply produced no output. However, we have not heard from room managers of anyone visibly idle during the work sessions they supervised, nor were there any complaints about technical problems from the workers.

2.4 Personnel

We hired room managers and assistants to help administer the experiment so as to ensure smooth operations and prevent information spillovers. Room managers prepared the rooms for work sessions, greeted the workers and showed them to their desks, announced instructions, prevented mobile phone use or other communications during work, and reported any out-of-protocol situations such as complaints, questions, walk-outs or technical issues (there were none). Assistants collected workers at the entrance to the buildings and guided them to and from their designated rooms in a manner that prevented interaction between workers who had completed their sessions and those just arriving. Assistants also distributed payment envelopes to each room before the work sessions started, and brought the payslips to be signed by the workers during their work session break.

The room managers and assistants were recruited separately from the workers, and outside of the university, to minimize the possibility of them knowing each other. Before starting the experiment, we provided training and detailed operational and communication instructions for them. During the work sessions, we monitored the experiment from a separate office, maintaining communication with room managers and assistants via their phones set to silent mode. We had no direct contact with the workers before, during or after the experiment.

2.5 Procedures during work sessions

Each work session lasted three hours and was structured as follows. Before the work began, room managers prepared the designated rooms by labeling desks with numbers corresponding to the assigned workers, recording each computer IP address on the forms we provided, and opening the online data entry form. As workers arrived at their scheduled times, assistants took them to the designated work rooms. At the entrance, room managers inspected workers' IDs, ensured that the workers' mobile phones were switched off, and showed each worker to their assigned desk.

At the beginning of the work session, but prior to the start of work itself, room managers read the instructions for the workers that we prepared. The instructions introduced us to the workers as “researchers from Hong Kong and Germany who hired you”, explained the task, the house rules, and the schedule of the work session. Importantly, the room managers asked the workers on our behalf to participate in a survey unrelated to the work they were about to do. We provide details of this survey, referred to as Q1 in what follows, in the next section.

After the workers completed survey Q1, assistants entered the room to announce the hourly pay rates, 20 RMB per hour in both treatment and control conditions. They then distributed individual sealed envelopes containing the total payment in cash (60 RMB), requesting each worker to count the money to ensure the amount was correct. Thereafter, the first work period

began, lasting 75 minutes and ending with a pre-scheduled 15-minute break, during which the treatment, as described in section 2.2, was administered. After the break, the second, post-treatment 75-minute work period began during which the workers resumed their usual tasks. At the end of the work session, the room managers thanked the workers for coming and the assistants escorted them out of the building so as not to cross paths with the newly arriving workers.

To minimize the possibility that the treated workers may not have noticed the pay differences in the payslip, we took two measures. First, we instructed the room managers to ask the workers to verify the information on their payslips and sign and return them during the 15-minute break, thereby increasing the chance of the stated pay differences being noticed. Second, the room managers in the treatment condition were asked to make the following announcement after the assistants' departure: "I have noticed there are differences in pay rates. I have just texted the researchers. They confirmed that your pay rate is 20RMB per hour, and that some other workers do earn 35RMB per hour for the same data entry job that you are doing." At the same time, but in the control condition, the room managers gave a generic announcement informing that the signed payslips had been fully completed and returned.

In anticipation to possible queries from the treated workers about pay differences, we prepared a special protocol for the room managers which instructed them to confirm, if asked, that different pay rates were indeed given for the same job and to equally qualified workers, and that the room managers were not part of the research team and did not know the reasons for the pay differences. No questions were asked.

2.6 Pre-experiment Survey Q1: Eliciting fairness concerns

Survey Q1 was administered in both treatment and control groups upon the workers' arrival to the designated rooms, lasted 10 minutes and focused on eliciting workers' fairness concerns, which is the key moderator variable in this study. The text of survey Q1 is in Appendix 8.5.

Our measure of fairness concerns is based on Abbink, Irlenbusch, and Renner, 2000's "moonlighting" game. Specifically, the workers were asked to imagine the following situation. There is an anonymous distributor and two recipients, one of whom is the focal worker. The distributor allocates 20 RMB between the focal worker and the other, anonymous, recipient, while always retaining a fixed amount of 10 RMB. There are three allocation scenarios, presented in a random order: 1 – 10 RMB for each recipient, 2 – 14 RMB for the focal worker, 6 RMB for the other recipient, and 3 – 6 RMB for the focal worker, 14 RMB for the other recipient. The workers were asked to specify how much they would like to reward or punish the distributor in each scenario, at a personal cost. For example, punishing or rewarding the distributor with 3 RMB costs the workers 1 RMB deductible from their total payment (there was no payment for not rewarding or punishing). One of the three scenarios was randomly selected as the basis for

payment. For example, if a worker chose to punish the distributor with 3 RMB at the cost of 1 RMB in scenario 3, and if scenario 3 ended up being selected for payment, the worker would get $6 - 1 = 5$ RMB sent to them later on.

Figures 1-3 report distributions of the workers' choices of costly rewards or punishments in the three scenarios of the moonlighting game. Many workers choose to reward or punish the distributor despite incurring personal costs, especially in the unequal allocation scenarios 2 and 3. The actions of many workers are consistent with self-serving distributional fairness concerns as defined in Ubeda, 2014. Thus, 58% choose to punish the distributor in the other-favoring allocation scenario 3 (6 RMB for the focal worker, 14 RMB for the other recipient), and 46% choose to reward in the self-favoring allocation scenario 2 (14 RMB for the focal worker, 6 RMB for the other recipient). Yet, there is also a significant fraction of the workers who seem to exhibit another, more strictly egalitarian, type of fairness concerns, rewarding the distributor for equal allocation in scenario 1 (25%) and punishing for unequal allocation, whether favoring self or not, in scenarios 2 or 3. Fairness-neutral workers are relatively few: although 65%, 18% and 32% do nothing in scenarios 1, 2 and 3 respectively, only 9% do nothing in all three scenarios. These preliminary findings are consistent with a large empirical literature documenting that workers do have fairness concerns and take them into account when making decisions (Fehr, Goette, and Zehnder, 2009; Ho and Su, 2009; Oosterbeek, Sloof, and Van De Kuilen, 2004; Benjamin, 2005; Fehr and Fischbacher, 2004).

2.7 Post-experiment survey Q2: Measuring workers' pay fairness perceptions and collecting explanations for different pay rates

After the experiment, we invited workers who showed up for the job to collect their certificates of participation in early February 2017. Upon arrival, we asked them to complete a post-experiment survey, labeled Q2 in what follows. Of the final sample of 311 workers, 260 (83%) showed up to collect their certificates and participated in survey Q2 whose text is in Appendix 8.6. The survey consisted of ten questions and was primarily focused on measuring pay fairness perceptions, but we also used the opportunity to learn the workers' opinions about the reasons for different pay rates given to different workers for the same job.

The survey was conducted on computers and did not allow revisiting previously answered questions. In the first five questions, we asked the workers to evaluate various aspects of their work experience on a scale from 1 to 7, without reminding of pay differences. The evaluations covered five aspects: the difficulty and attractiveness of the work, work conditions, previous data entry experience (a placebo test), and, most importantly, how fair the pay was.

The remaining part of Q2 consists of open-ended questions. Most importantly, we reminded of paying different rates for the same work and asked whether the workers had heard about differ-

ent pay rates before coming to work (question 8), and if so, where they heard this information from (question 9). Only 6% of the respondents in Q2 said that they had known of different pay rates being paid before coming to work. The final question 10 asked the workers to share their opinions, in free text, as to why different workers received different hourly pay rates for the same work.

2.8 Estimation method

We estimate the average and mediated/moderated treatment effects of our interest using the ANCOVA regression (McKenzie, 2012) where the post-treatment focal outcome for worker i (Y_{i1}) is regressed on the worker's treatment status dummy (T_i), pre-treatment outcome (Y_{i0} , where available) and controls for individual and contextual characteristics such as gender, study year and major, work session size, time and date:

$$Y_{i1} = \beta \cdot T_i + \gamma \cdot Y_{i0} + \text{controls}_i + \text{error}_i \quad (1)$$

For instance, to estimate the average treatment effect on the length of time to process a questionnaire (β), we regress the average post-treatment processing time for a given worker on his/her treatment dummy, pre-treatment average processing time, and controls.³ We cluster errors at the work session-date-time level. The approach we take is a straightforward extension of the popular difference-in-difference estimator (Fredriksson and Oliveira, 2019),

$$Y_{i1} - Y_{i0} = \beta \cdot T_i + \text{controls}_i + \text{error}_i \quad (2)$$

One can see that the difference-in-difference regression (2) is a specific case of the ANCOVA regression (1) with the restriction $\gamma = 1$ which is relaxed in the ANCOVA regression we use.

We estimate the mediation and moderation relationships with an extended version of regression (1) where a variable M_i added to the controls (mediation) and interacted with the treatment dummy T_i (moderation):

$$Y_{i1} = \beta \cdot T_i + \delta \cdot T_i \times M_i + \alpha \cdot M_i + \gamma \cdot Y_{i0} + \text{controls}_i + \text{error}_i \quad (3)$$

³We cannot implement the ANCOVA estimator for pay fairness perceptions which we measure only after the treatment, in survey Q2. In this case, we estimate regression (1) without the pre-treatment outcome Y_{i0} .

3 Experimental Results

Our experiment produced several results which we present in this section.

Result 1: Treated workers perceive their pay as less fair.

Pay fairness perceptions, measured in the post-experiment survey Q2, is among the key pre-registered outcomes of our experiment. Table 2 (column 1) shows that, compared to the control group receiving the same hourly pay rate (20 RMB), treated subjects perceive their pay as less fair. The treatment effect estimate is not only statistically significant but also substantial in magnitude, amounting to over a quarter of the standard deviation in pay fairness perceptions.

The treatment effects on other perceptions, such as task difficulty or working conditions, which were not meant to be affected by our treatment, or on data entry experience, which could not have been affected, are indeed insignificant, both statistically and magnitude-wise (Specifications 2-5). The treatment affecting fairness perceptions but not other aspects of work is consistent with our theoretical expectations, and also presents a useful manipulation check, suggesting that the workers did notice the treatment and react to it.

Result 2: Treated workers tend to work harder.

Specifications (1) and (4) in Table 3 show the average treatment effects on the other key pre-registered outcomes: the average length of time it took the workers to process a questionnaire and the average number of empty fields in the questionnaires they processed. Surprisingly, treated workers tend to process questionnaires faster and leave fewer empty fields than those in the control group. The average treatment effects are statistically significant at conventional levels and substantial in magnitude, amounting to 17% of the standard deviation of the corresponding pre-treatment outcomes. They are also robust to variations in regression specifications, and specifically to the exclusion of the workers who heard of the pay differences before coming to work (Tables 11-12). As we noted earlier, there were no protests or walk-outs in any of the 132 work sessions we organized, and, as will be shown later (Section 4.1), higher output in the treatment condition does not come at the expense of lower quality.

Result 3: Treatment effects on worker performance are unrelated to fairness perceptions (survey Q2) or fairness concerns (survey Q1).

A further result from Table 3 (Specifications 2-3 and 5-6) is that pay fairness perceptions neither mediate nor moderate the treatment effects on the worker performance measures, implying that performance responses to our treatment are independent of how (un)fair the treated workers perceive their pay to be. We also see no relationship between the magnitude of the treatment effects and the strength of the workers' fairness concerns measured as the workers' decision in the other-favoring scenario 2 of the moonlighting game in the pre-treatment survey Q1. Contrary to the standard fairness theories mentioned in the introduction, more fairness-concerned

treated workers do not put in lower effort (Table 4). This is not because the workers are not concerned about fairness: as we reported earlier (Section 2.6), many choose to punish unequal allocation of resources by the distributor in the moonlighting game, even at a personal cost.

Moreover, there is no correlation between fairness concerns measured in Q1 and pay fairness perceptions measured in Q2, and no stronger negative treatment effect on pay fairness perceptions of the more fairness-concerned workers (specification 1 in Table 4). The latter results reveal an interesting disconnect between worker perceptions of the situation presented in the moonlighting game in the pre-treatment survey Q1 and the treatment they faced later on, even though the two are similar. In both situations, money is distributed by an external force to the disadvantage of the focal workers. Yet, the workers who choose to punish the unfair distributor in Q1 do not choose to punish the researchers (us) by withdrawing effort. This disconnect suggests important contextual factors are present in the real work situation (our treatment) but absent in the stylized, though payment-relevant, situation played out in Q1. We begin probing into these factors by asking the workers for their explanations why we paid different hourly rates for the same work.

Result 4: Workers come up with various benign explanations for pay differences.

In the post-experiment survey Q2, we reminded the participants about the pay differences and asked them to specify in free text why, in their opinion, different workers got different hourly pay rates for the same work (to recall: we never gave an explanation ourselves). An application of text analysis revealed the following groups of explanations provided by the workers: “better workers”, “different job”, “choices made in survey Q1”, “experimental treatment”, “don’t know”, and “other”. The procedure produced 305 explanations from the texts written by 260 workers who showed up for Q2, with some workers giving more than one explanation.

Figure 4 shows the distributions of the explanation groups for the treated and control workers. The explanations are predominantly benign, with the “better workers” and “different jobs” being by far the leading explanations. The correct “experimental treatment” and the agnostic “don’t know” explanations are rare. There is no difference between the distributions of the explanation groups for the treated and control workers (p -value = 0.8). In addition, there is no correlation between the workers’ explanations and their fairness concerns (pre-treatment Q1) or pay fairness perceptions (Q2). The specific explanations many workers give for the pay difference in the absence of any information from us about the actual reason suggests that they tried to make sense of the experience they had working for us (Maitlis and Christianson, 2014), in different but benign ways.

Result 5: Workers’ explanations for the pay difference do not moderate the treatment effects.

Table 5 reports treatment effects on the key outcomes estimated on the subsamples of workers

citing a specific explanation: “better worker”, “different job”, and “other”. The explanation-specific treatment effects are not significantly different from each other (p -value=0.3). This no-result does not necessarily contradict Breza, Kaur, and Shamdasani, 2018 who found performance effects of pay transparency to be moderated by exposure to information about the productivity of the higher-paid workers. Nor does it suggest that sensemaking with respect to the pay difference is unimportant: given that the workers’ explanations are predominantly benign, there may be not enough contrast in the explanations to identify moderation statistically.⁴ In other words, there may be a macro-level factor, for instance culture, that is shared by most if not all the workers and shapes their perceptions of and responses to pay differences. We address this possibility in a later, post-experimental analysis which we report in the next section.

Result 6: Treatment effects are (mildly) heterogeneous.

Table 6 reports coefficients on the interactions of the treatment dummy and a selection of contextual characteristics other than fairness concerns, reported above, which was our pre-registered moderator. The treatment effects on performance are independent of the worker’s gender, study cohort, major and the date of the working session. Interestingly, treated workers with worse baseline performance respond to treatment more positively. Treated female workers perceive their pay as less unfair than treated male workers.

⁴Another possible reason for the lack of moderation could be low reliability of the explanations measured in Q2. Workers may have forgotten their actual experiences and come up with plausible but random explanations when asked. What speaks against this possibility, however, is the robust difference in pay fairness perceptions between the treated and control workers measured in the same survey and before they were reminded of our treatment.

4 Post-experimental analysis

In this section, we describe two post-hoc, not pre-registered analyses we undertook to better understand our surprising experimental results.

4.1 Quality check

One possibility for the treated workers to punish us for paying them less than peers may have been to worsen the quality of their output, as in Greiner, Ockenfels, and Werner (2011). To test this possibility, we performed a quality check by taking a random sample of 407 questionnaires (6% of the total processed) digitized by 89 different workers (29% of the total headcount) and calculating the number of mistakes in each questionnaire. A mistake is defined as any discrepancy between the paper questionnaire and its digital version. For this task, we hired six research assistants whom we specially trained to detect mistakes but did not tell about our experiment.

Budgetary and time constraints prevented us from surveying each processed questionnaire for mistakes. It took longer to detect mistakes than to digitize an average questionnaire, but even assuming that detecting mistakes takes the same time as digitizing, our six assistants would have to spend nearly 130 hours each to survey all the questionnaires. Therefore, it is important to ensure that our quality check subsample is representative of the entire sample.

Table 7 reports descriptive statistics and treatment effects for the subsamples selected and not selected for the quality check. The two subsamples are not significantly different from each other in terms of all the average characteristics except the number of empty fields per questionnaire, which is somewhat lower in the quality check subsample. The broad similarity of the average characteristics of, and the treatment effects on, the selected and not selected subsamples suggests that the selected subsample is representative of our entire sample.

The average number of mistakes per questionnaire in the quality check subsample is 1.9, which is about 2% of the total number of positions. Table 8 reports the treatment effects on the number of mistakes from the ANCOVA regressions estimated at the questionnaire and worker (average) level, with and without the usual controls plus controls for the length and the number of empty fields which may be correlated with mistakes. The absence of a significant effect on the number of mistakes in either specification leads us to conclude that the treatment effect on the quantity of output occurred without detriment to quality.⁵

⁵One could argue that our treatment effect estimates are not significant only because of the limited size of our quality check subsample. To address this argument, we performed simulations in which we assumed the same magnitude of the treatment effects and the quality check subsample equal to the entire sample. Our simulation results produce the implied standard errors of the treatment effect estimates that are about half the size of the original standard errors reported in Table 8. This further implies that, assuming we would get the same treatment effects on the entire sample, they would still be statistically insignificant in three out of four specifications.

4.2 Survey of China's working-age population

In an attempt to externally validate our experimental results, we had a commercial survey company run an online survey of working age population in China in April 2021. 1100 people aged 18 to 68 participated in the survey, their descriptive statistics are in column (1) of Tables 9 and 10. Because the survey sample is over-represented by the young (average age 28), low-earning and currently in university education, we cannot claim its results to be representative of the views of China's working population. However, the survey sample is close in its characteristics to the worker sample in our experiment, which allows us to draw comparisons between the two samples.

In the survey questionnaire, we briefed the participants about our experiment and its findings, and asked them to choose from a drop-down menu up to three explanations to our results that they thought were most likely, or to provide their own explanation if they found ours unsatisfactory. The text of the explanations provided by us is in Appendix 8.8. The total number of explanations chosen by 1100 survey participants was 2661, or 2.42 per average participant. Figure 5 shows distribution of the explanations given by survey participants. The leading explanation provided for our main results, labeled “better workers” for brevity, is: “*They [treated workers] worked harder because they believed that those earning more than them were better workers and they wanted to match them in performance*”. Its popularity is broadly consistent with the results of our earlier survey Q2 where “better workers” was also the leading explanation. The other popular explanations are workers’ showing “high motivation signal”, hoping their efforts would still be rewarded somehow (“reward efforts”), and avoiding losing the chance for better pay in future that might come from working harder (“chance for better pay”. Again, as in survey Q2, the explanations are mostly benign, except for the offered but infrequently chosen “emphasize injustice” and “distance from negative feelings”. (The own, free-text explanations are few and can be related to the explanations we provided.)

Columns (2)-(7) in Table 9 list the coefficients from multinomial logit regressions of the explanations chosen by the survey participants on their demographic and socio-economic characteristics and attitudes towards pay inequality that we asked in the same survey. The most popular “better workers” explanation is more likely to be chosen by the participants who believe unequal pay is up to the employer and by those who tend to reward fair behavior, and is less frequent among the middle income earners (80-150k RNB per year) relative to the baseline income group (< 30k RNB per year). There is no significant relationship between choosing this explanation and all other respondent characteristics we measure. The relatively few significant correlations between explanations and respondent characteristics, as well as the absence of a discernible pattern in the correlations, allow us to conclude that the distribution of the chosen explanations is fairly stable across different demographic, income and social groups.

We next asked the survey participants to imagine themselves in the same situation as the in-

formed workers in our experiment and tell us what they would do: work harder, work less hard, or not change effort upon learning of being paid less than peers for the same work. Surprisingly, 75% of the participants said they would work harder, 13% would not change effort, and only 12% would reduce effort. To relate the stated effort choices to participant characteristics, we run multinomial logit regressions whose coefficients are reported in columns (2)-(3) of Table 10. As before, not many coefficients are significant, and no pattern in the correlations is transparent. Rich and/or single individuals are less likely to change effort, while those who think unequal pay is up to the employer are more likely to decrease or increase effort. Participants who are religious, come from lower income regions (Shaoyang is one of those) and profess to reward fair behavior are more likely to increase effort, and so are those who chose “better workers”, “reward efforts” and “distance negative emotions” explanations.

5 Discussion and conclusion

To summarize our results, informing workers of being paid less than others for the same job reduces their perceived pay fairness but, surprisingly, increases their effort without detriment to quality. The positive effects of pay transparency on the informed workers' output disagree with the existing evidence from outside China and contradict the predictions of fairness theory that we used as guidance in designing our study. The lack of correlation between pay fairness perceptions and fairness concerns, and the lack of mediation or moderation of the treatment effects by fairness is not consistent with fairness theory either. Therefore, an alternative explanation, suitable for the Chinese context, is needed for our results.

One possible explanation for some of our results is the culture of high power distance in China (Hofstede, 2001). If the informed workers perceived their job as connected to Shaoyang University (which was not the case), they might be inclined to accept lower pay without protest and rationalize it benignly out of deference to the authority of the University. Indeed, we observe that the workers' explanations were benign, and that there were no protest or sabotage or even questions about pay. However, while the high power distance argument could explain no treatment effect on output, it cannot explain the positive effects we observed.

Another possible explanation comes from face negotiation theory originating from communication studies (Ting-Toomey, 1982). This theory aims to explain the diverse ways individuals across cultures manage rapport and navigate conflicts (Ting-Toomey and Kurogi, 1998; Ting-Toomey, 2005; Oetzel, Meares, et al., 2003). The theory suggests that people constantly strive to maintain and negotiate "face", a concept closely linked to the Chinese notion of *mianzi* and linked to respect, honor and social standing. According to face negotiation theory, a person's face is influenced by moral values, social norms and accepted social roles.

During conflicts or tensions a person's face may be threatened, prompting them to engage in "facework", or activities to preserve or restore face. There are different strategies of facework. Face negotiation theory argues that cultural dimensions, as outlined by Hofstede, 2001, significantly affects facework strategy, a claim supported by several cross-cultural comparison studies (Oetzel, Ting-Toomey, et al., 2001; Oetzel and Ting-Toomey, 2003; Oetzel, Garcia, and Ting-Toomey, 2008). In individualistic cultures that prioritize personal rights and independence, such as many Western societies, self-face (one's own image) is emphasized, leading individuals to adopt direct, defensive, and often dominating facework strategies to protect their self-image. Effort withdrawal after learning of being paid less would be consistent with defensive facework strategy to protect self-face.

In contrast, collectivist cultures, prevalent in many Eastern societies including China, prioritize the group over the individual. These cultures emphasize a concern for other-face (the image of the other party), which may collectively contribute to mutual face (the shared image of both parties). Consequently, people in these cultures often use obliging, compromising, or avoiding

facework strategies that align their face with others to maintain harmony. Not reducing effort is consistent with avoiding facework strategy, and increasing effort speaks to obliging facework strategy.

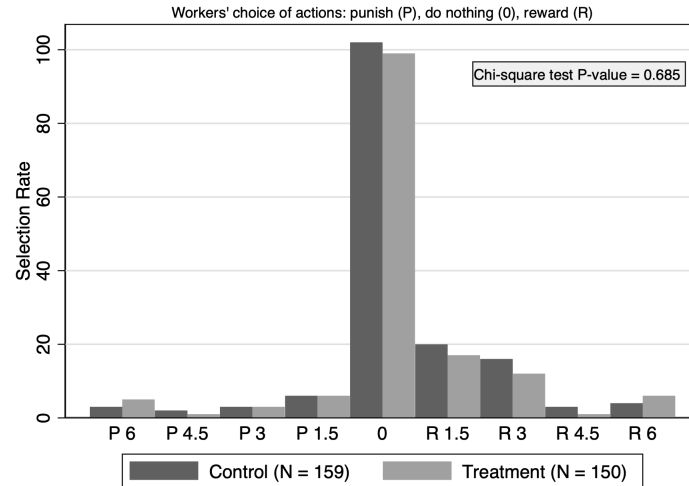
Furthermore, face negotiation theory highlights that cultures with low power distance generally value equality, favoring informal, horizontal-based facework that respects symmetrical relations and balanced power dynamics, again leading to defensive facework strategies. On the other hand, high power distance cultures are more likely to employ formal, vertical-based facework, upholding hierarchical structures and respecting authority in conflict resolution, which makes avoiding or obliging facework strategy more suitable.

Our experimental findings align with face negotiation theory in the following way: When workers in the treatment group became informed about receiving lower wages than others, a form of workplace conflict arose between them and the authority (us) who decided on wages. This conflict threatened the informed workers' self-face, as they had to reconcile their respect for authority with the reality of being paid unfairly low wages relative to the high-earning workers. The defensive facework strategies to restore face being culturally less appropriate, the informed workers chose avoiding or obliging strategy, which is our experiment was to increase work effort.

In a collectivist culture, one's attempt to restore self-face also involves actions that enhance other-face. This may have pushed the informed workers to choose obliging vs. avoiding strategy by increasing effort — an action that not only avoided open conflict with the authority but also aligned their facework activity with the collective good. This of course is a theoretical hypothesis that needs more development and validation in future studies.

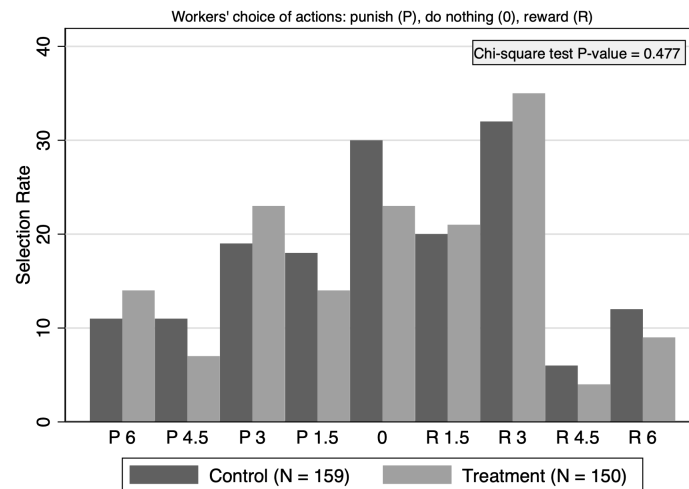
6 Figures

Figure 1: Distribution of worker choices in the equal allocation scenario of the moonlighting game



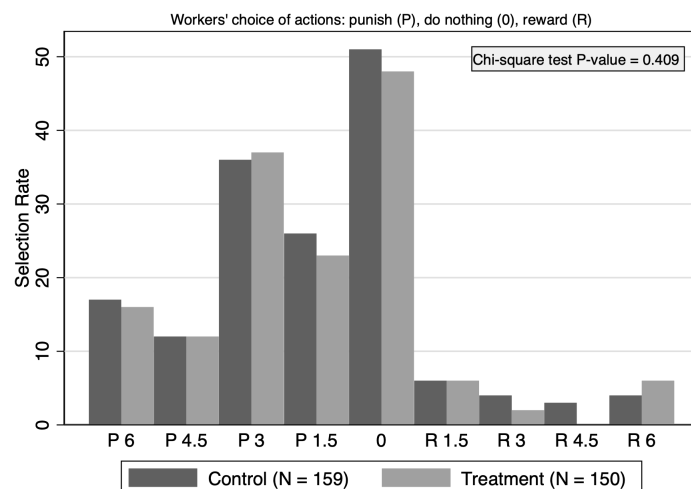
Note: The graph demonstrates the distribution of workers' choices (punish, do nothing, reward) in the equal allocation scenario from the pre-treatment survey (Q1). In total 309 workers participated in Q1. The p-value of the chi-squared test for equality of treatment and control distributions is 0.685, indicating no statistically significant difference between the distributions of the treatment and control groups (Section 2.6).

Figure 2: Distribution of worker choices in the self-favoring allocation scenario of the moonlighting game



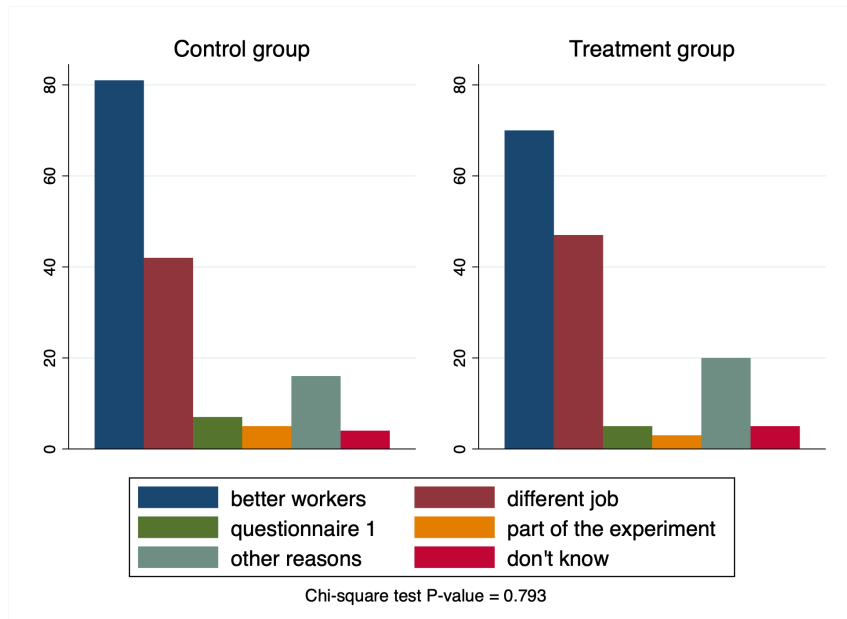
Note: The graph demonstrates the distribution of workers' choices (punish, do nothing, reward) in the self-favoring allocation scenario from the pre-treatment survey (Q1). In total 309 workers participated in Q1. The p-value of the chi-squared test for equality of treatment and control distributions is 0.477, indicating no statistically significant difference between the distributions of the treatment and control groups (Section 2.6).

Figure 3: Distribution of worker choices in the other-favoring allocation scenario of the moon-lighting game



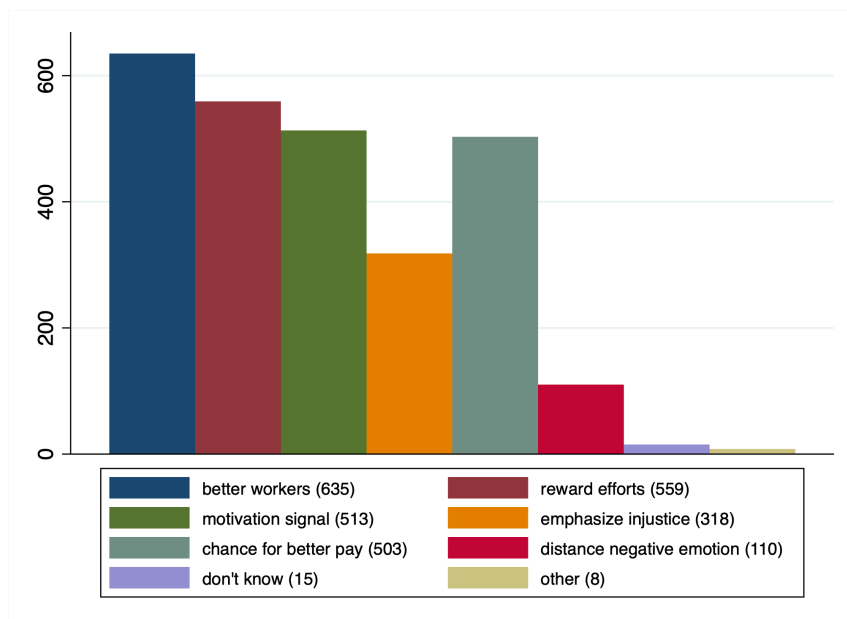
Note: The graph demonstrates the distribution of workers' choices (punish, do nothing, reward) in the other-favoring allocation scenario from the pre-treatment survey (Q1). In total 309 workers participated in Q1. The p-value of the chi-squared test for equality of treatment and control distributions is 0.409, indicating no statistically significant difference between the distributions of the treatment and control groups (Section 2.6).

Figure 4: Distributions of the explanation groups for the treated and control workers



Note: The graph demonstrates the distributions of the explanation groups by treated and control workers. In total 305 explanations were provided in a free text written by 260 workers who participated in the post-experiment survey Q2, with some workers giving more than one explanation. The p-value of the chi-squared test for equality of treatment and control distributions is 0.793, indicating no statistically significant difference between the distributions of the explanation groups for the treated and control workers.

Figure 5: Distributions of explanations provided by participants of China's working-age population



Note: This graph shows the distribution of the explanations provided by participants of China's working-age population for why the informed workers tended to work harder after learning about being paid less than some other peers for the same job.

7 Tables

Table 1: **Descriptive statistics of the study sample**

	Registered (1)	Showed up and included in the study sample (2)				No Shows (3)	
	Mean (s.d.)	Treatment group	Control group	T-test (T)-(C) P-value	High-paid group	Mean (s.d.)	T-test (2)-(3) P-value
Age	19.441 (1.222)	19.343 (1.080)	19.456 (1.302)	0.402	19.305 (0.944)	19.542 (1.304)	0.189
Female	0.545 (0.498)	0.573 (0.496)	0.602 (0.491)	0.461	0.500 (0.514)	0.407 (0.501)	0.028**
Study year	1.573 (0.722)	1.493 (0.653)	1.609 (0.717)	0.036*	1.278 (0.461)	1.663 (0.804)	0.048**
Major Art	0.175 (0.380)	0.160 (0.368)	0.168 (0.375)	0.796	0.056 (0.236)	0.215 (0.412)	0.091*
Business and Economics	0.192 (0.395)	0.173 (0.380)	0.211 (0.409)	0.233	0.278 (0.461)	0.198 (0.399)	0.837
Engineering and Tech	0.389 (0.488)	0.440 (0.498)	0.348 (0.478)	0.019**	0.389 (0.502)	0.378 (0.486)	0.713
Social Science and Law	0.226 (0.418)	0.227 (0.420)	0.248 (0.433)	0.525	0.278 (0.461)	0.203 (0.404)	0.394
Others	0.015 (0.124)	0.000 (0.000)	0.025 (0.156)	0.006**	0.000 (0.000)	0.006 (0.076)	0.206
Session size	-	3.191 (1.393)	2.038 (1.055)	0.000***	6.667 (2.275)	-	-
N	514	150	161		18	172	

Note: This table reports the demographic characteristics of the study sample. Column (1) provides information on those workers who were registered ($N = 514$). Column (2) reports information about our study sample, workers who showed up to their working sessions, divided by treatment conditions, and excluding observations (13) due to technical issues ($N = 329 = 150 + 161 + 18 - 13$). Column (3) includes the data of workers who did not show up for the work ($N = 172$). All the clarifications are provided in Section 2.3.

Table 2: **Treatment effects for perception outcomes**

	(1)	(2)	(3)	(4)	(5)
	Fair pay	Hard work	Good job	Good conditions	Experience
Treatment	-0.271*	0.121	-0.099	-0.034	-0.113
	(0.137)	(0.112)	(0.135)	(0.118)	(0.147)
N	260	260	260	260	260

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses are clustered by work session.

The estimates are expressed in standard deviations of the outcome variables.

Controls include session size, cohort, major, gender, time, and date of the session.

Note: This table shows the effect of the treatment on the perception outcomes measured in the post-experiment survey Q2 (Section 2.7): perception of fair pay, difficulty and usefulness of tasks, working conditions and also control measurement, previous data entry experience. $N = 260$ workers participated in Q2.

Table 3: **Treatment effects on the key performance outcomes**

Sample	Length			Empty fields		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Q2 participants		All	Q2 participants	
Treatment	-0.173** (0.077)	-0.166** (0.082)	-0.160** (0.080)	-0.173* (0.092)	-0.127 (0.090)	-0.120 (0.075)
Fair pay			0.031 (0.050)			0.065 (0.061)
Treatment X Fair pay			-0.085 (0.071)			-0.072 (0.077)
Observations	311	260	260	311	260	260
R-squared	0.498	0.542	0.544	0.489	0.569	0.570

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Regression estimator: ANCOVA (McKenzie, 2012).

Standard errors in parentheses are clustered by work session.

The estimates are expressed in standard deviations of the outcome variables.

Controls include session size, cohort, major, gender, time, and date of the session.

Note: This table shows the treatment effects on the the key performance outcomes: the average length of time it took the workers to process a questionnaire and the average number of empty fields in the questionnaires they processed. $N = 311$ is our study sample of workers. $N = 260$ workers participated in the post-experiment survey (Q2) and provided their pay fairness perceptions.

Table 4: **Treatment effects on the key outcome variables moderated by fairness concerns**

	(1) Fair pay	(2) Length	(3) Empty fields
Treatment	-0.271* (0.137)	-0.173** (0.077)	-0.173* (0.092)
Fairness	-0.064 (0.064)	-0.028 (0.041)	-0.077 (0.059)
Treatment X Fairness	0.089 (0.081)	-0.020 (0.050)	0.072 (0.071)
Observations	259	309	309
R-squared	0.133	0.519	0.550

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses are clustered by work session.

The estimates are expressed in standard deviations of the outcome variables.

Controls include session size, cohort, major, gender, time, and date of the session.

Note: This table reports moderation by fairness concerns. $N = 259$ in column (1): 260 workers participated in the post-treatment survey (Q2), where pay fairness perception was measured, excluding 1 worker, who participated in Q2, but did not respond in the pre-experiment survey (Q1), where fairness concerns were elicited. $N = 309$ in columns (2)-(3): 311 workers from our study sample minus 2 workers who did not respond in the pre-experiment survey (Q1).

Table 5: Treatment effects on the key outcome variables estimated on the subsamples of workers citing a specific explanation for pay differences

	(1) Length	(2) Empty fields	(3) Fair pay
“Better worker”	-0.174 (0.170)	-0.141 (0.198)	-0.237 (0.439)
“Different job”	0.027 (0.156)	0.274 (0.224)	0.025 (0.352)
“Other”	-0.326 (0.274)	-0.120 (0.181)	-0.147 (0.392)
N	260	260	260
P-value	0.299	0.138	0.734
R-squared	0.568	0.580	0.152

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses are clustered by work session.

The estimates are expressed in standard deviations of the outcome variables.

Controls include session size, cohort, major, gender, time, and date of the session.

Note: This table shows treatment effects on the key outcome variables length, empty fields and fair pay estimated on the subsamples of workers citing a specific explanation: “better worker”, “different job”, and “other”. The explanation-specific treatment effects are not significantly different from each other. $N = 260$ workers, who participated in the post-experiment survey (Q2).

Table 6: **Treatment effects heterogeneity on the key outcome variables**

	(1)	(2)	(3)	(4)	(5)
	Female	Cohort	Major	Date	Baseline Performance
Length	-0.169 (0.136)	-0.053 (0.088)	-0.014 (0.022)	0.116 (0.107)	-0.286* (0.145)
Empty Fields	0.230 (0.232)	0.057 (0.123)	0.012 (0.032)	0.093 (0.145)	-0.351* (0.155)
Fair Pay	0.621** (0.286)	-0.141 (0.162)	-0.040 (0.044)	-0.029 (0.200)	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses are clustered by work session.

Controls include session size, gender, major, study year, date and time.

Note: This table shows the interactions of the treatment dummy and contextual characteristics: gender, study cohort, major and the date of the working session, and also baseline performance of the workers.

Table 7: Descriptive statistics and treatment effects on the subsamples selected and not selected for quality check

	(1) Selected Sample	(2) Not Selected	(3) P-value (1) - (2)
N workers	89	222	
N questionnaires	407	6386	
Treatment	0.516 (0.500)	0.492 (0.500)	0.843
After	0.560 (0.497)	0.611 (0.488)	0.295
Length	5.494 (2.146)	5.804 (2.600)	0.157
Empty fields	1.150 (1.941)	1.659 (3.292)	0.007**
Female	0.506 (0.501)	0.570 (0.495)	0.532
Fairness concerns (1 to 5)	2.118 (1.367)	2.205 (1.510)	0.642
Study year	1.590 (0.540)	1.578 (0.715)	0.883
Major Art	0.086 (0.281)	0.161 (0.367)	0.132
Business and Economics	0.177 (0.382)	0.198 (0.399)	0.656
Engineering and Tech	0.541 (0.499)	0.390 (0.488)	0.115
Social Science and Law	0.179 (0.384)	0.239 (0.426)	0.393
Others	0.017 (0.130)	0.012 (0.111)	0.779
Treatment effects:	Coefficient (std. error)	Coefficient (std. error)	P-value (1) - (2)
Fair pay	-0.153 (0.286)	-0.377 (0.185)	0.396
Length	-0.366 (0.333)	-0.491 (0.267)	0.680
Empty fields	-0.492 (0.373)	-0.287 (0.191)	0.564

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses are clustered by work session.

Note: Treatment effects on the selected and non-selected samples were estimated in the same way as the treatment effects on the entire sample.

Table 8: Treatment effects on the number of mistakes per questionnaire

	Questionnaire-level analysis		Worker-level analysis	
	(1)	(2)	(3)	(4)
Treatment	0.328 (0.439)	0.470 (0.711)	0.839 (0.756)	0.232 (1.104)
Controls:		+		+
N	228	228	83	83
R-squared	0.024	0.130	0.019	0.198

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses are clustered by work session.

Controls include: length, empty fields, cohort, major, gender, time, and date of the session.

Note: This table reports the treatment effects on the number of mistakes per questionnaire in the quality check sample. estimated with the ANCOVA regression that we use throughout our study. In columns 1 and 2, the unit of observation is individual questionnaire. There were 228 questionnaires in the quality check sample that were digitised in the treatment period, hence $N = 228$ in columns 1 and 2. In columns 3 and 4, the unit of observation is individual worker, so that the mistakes per questionnaire are averaged for each workers in the periods before and after the treatment. In the quality check sample, questionnaires digitised before and after treatment were available for 83 workers, hence $N = 83$ in columns 3 and 4.

Table 9: Correlations between reasons provided and respondents' characteristics (relative to the base category)

	(1) Freq. or Mean (s.d.)	(2) Better workers	(3) Motivation signal	(4) Chance for better pay	(5) Emphasize injustice	(6) Reward efforts	(7) Distance negative emotions
Single/Divorced (base category Attached)	517 583	-0.034 (0.182)	0.152 (0.182)	0.216 (0.183)	-0.158 (0.204)	-0.031 (0.185)	0.300 (0.320)
Advanced degree (b.c. Undergraduate)	89 827	-0.046 (0.241)	0.191 (0.233)	-0.130 (0.236)	-0.104 (0.270)	-0.036 (0.251)	-0.682 (0.511)
High school/ Secondary/below	184	0.128 (0.179)	-0.010 (0.174)	0.160 (0.175)	-0.329 (0.204)	0.296* (0.176)	-0.050 (0.291)
30k-80k (b.c. Below 30k)	220 394	-0.203 (0.221)	-0.060 (0.222)	-0.333 (0.221)	0.572** (0.245)	0.267 (0.220)	-0.477 (0.335)
80k-150k	332	-0.397* (0.239)	0.014 (0.243)	-0.268 (0.238)	0.638** (0.276)	0.378 (0.239)	-0.918** (0.393)
150k-300k	125	-0.063 (0.293)	-0.034 (0.290)	-0.037 (0.287)	0.801** (0.333)	-0.279 (0.291)	-0.341 (0.471)
Over 300k	29	0.168 (0.472)	0.584 (0.456)	0.185 (0.457)	0.855 (0.521)	-0.824 (0.504)	-0.367 (0.747)
Non-communist (b.c. Communist)	379 721	-0.033 (0.149)	-0.290** (0.147)	-0.229 (0.148)	-0.033 (0.167)	-0.016 (0.148)	-0.057 (0.238)
Religious (b.c. Non-religious)	250 850	-0.077 (0.151)	-0.121 (0.151)	-0.028 (0.150)	0.131 (0.163)	-0.039 (0.149)	0.507** (0.225)
Exp. 3-5 years (b.c. 0-3 years)	142 446	0.087 (0.274)	0.185 (0.268)	0.261 (0.274)	-0.106 (0.287)	0.192 (0.270)	-0.222 (0.434)
5-8 years	253	-0.103 (0.288)	-0.074 (0.284)	0.297 (0.287)	-0.119 (0.316)	-0.066 (0.286)	0.227 (0.448)
Over 8 years	259	-0.121 (0.349)	-0.219 (0.345)	0.196 (0.342)	-0.549 (0.397)	0.250 (0.345)	-0.350 (0.608)
Managers (b.c. Students)	365 334	0.383 (0.303)	0.180 (0.301)	0.325 (0.301)	-0.788** (0.341)	-0.150 (0.304)	0.979* (0.532)
Entry level employee	126	0.138 (0.260)	0.386 (0.260)	0.324 (0.259)	-0.061 (0.280)	-0.296 (0.259)	0.707* (0.408)
Employee without managerial duties	242	0.349 (0.269)	0.093 (0.269)	0.049 (0.265)	-0.436 (0.305)	0.056 (0.271)	0.588 (0.446)
Other occupation	33	0.518 (0.439)	-0.136 (0.429)	-0.134 (0.434)	-0.522 (0.500)	-0.155 (0.436)	1.076* (0.591)
Middle income areas (b.c. Higher income)	139 614	0.096 (0.194)	-0.059 (0.195)	0.273 (0.192)	-0.077 (0.219)	0.013 (0.192)	0.022 (0.327)
Lower income areas	347	-0.094 (0.140)	-0.048 (0.138)	-0.002 (0.139)	0.101 (0.153)	0.235* (0.139)	0.166 (0.230)
Female (b.c. Male)	683 417	-0.109 (0.130)	-0.044 (0.129)	0.176 (0.130)	-0.085 (0.144)	0.069 (0.129)	-0.016 (0.213)
Age	28.161 (7.512)	0.004 (0.016)	0.023 (0.017)	-0.017 (0.017)	-0.005 (0.020)	-0.012 (0.016)	0.002 (0.028)
Unequal pay's up to employer (scale 1 to7)	4.526 (1.329)	0.112** (0.048)	0.039 (0.046)	-0.045 (0.048)	-0.047 (0.053)	0.108** (0.048)	-0.037 (0.083)
Reward fair behavior (scale 1 to7)	5.272 (1.119)	0.145** (0.058)	0.016 (0.057)	0.046 (0.058)	-0.048 (0.064)	-0.063 (0.058)	0.022 (0.113)
Punish unfair behavior (scale 1 to7)	4.872 (1.243)	-0.008 (0.053)	0.024 (0.052)	-0.062 (0.052)	0.090 (0.061)	0.017 (0.051)	-0.151* (0.089)
N	1100	1100	1100	1100	1100	1100	1100

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table list descriptive statistics of the respondents in the China's working age population survey, and shows how the respondents' characteristics are related to their stated reasons for why our treatment produced a positive effort response. Specifically, it displays the coefficients and standard errors from the multinomial logit regressions of each reason on the characteristics listed in the table. For instance, the coefficient -0.397^* in front of the category "80k-150" in column 2 implies that the respondents with annual income in above group are 40% less likely to cite "better worker" as the reason in comparison to the base category earning less than 30k per year.

Table 10: Correlations between choice of effort and respondents' characteristics

	(1) Freq. (b.c.) or Mean (s.d.)	(2) Would work less hard (12% of the sample)	(3) Would work harder (75% of the sample)
		Coefficient (std. error)	Coefficient (std. error)
Single/Divorced (base category Attached)	517 (583)	-0.670* (0.382)	-0.655** (0.293)
Advanced degree (b.c. Undergraduate)	89 (827)	0.586 (0.439)	-0.086 (0.345)
High school/ Secondary/below	184	-0.229 (0.355)	-0.093 (0.271)
30k-80k (b.c. Below 30k)	220 (394)	0.453 (0.472)	-0.037 (0.387)
80k-150k	332	-0.262 (0.546)	-0.287 (0.423)
150k-300k	125	-0.848 (0.644)	-0.851 (0.503)
Over 300k	29	-1.831* (0.977)	-1.443** (0.678)
Non-communist (b.c. Communist)	379 (721)	0.171 (0.284)	-0.220 (0.222)
Religious (b.c. Non-religious)	250 (850)	0.202 (0.328)	0.672** (0.267)
Exp. 3-5 years (b.c. 0-3 years)	142 (446)	0.285 (0.544)	0.063 (0.425)
5-8 years	253	-0.267 (0.566)	-0.396 (0.452)
Over 8 years	259	-0.692 (0.696)	-0.409 (0.540)
Managers (b.c. Students)	365 (334)	0.244 (0.635)	0.631 (0.485)
Entry level employee	126	-0.177 (0.549)	0.202 (0.423)
Employee without managerial duties	242	0.028 (0.541)	-0.077 (0.432)
Other occupation	33	0.573 (0.792)	0.228 (0.657)
Middle income areas (b.c. Higher inc.)	139 (614)	-0.139 (0.368)	-0.237 (0.278)
Lower income areas	347	0.185 (0.294)	0.414* (0.225)
Female (b.c. Male)	683 (417)	-0.395 (0.263)	-0.305 (0.205)
Age	28.161 (7.512)	0.032 (0.040)	0.050 (0.033)
Unequal pay's up to employer (scale 1 to 7)	4.526 (1.329)	0.210** (0.098)	0.215*** (0.067)
Reward fair behavior (scale 1 to 7)	5.272 (1.119)	-0.113 (0.113)	0.212** (0.088)
Punish unfair behavior (scale 1 to 7)	4.872 (1.243)	0.077 (0.111)	0.028 (0.083)
Better workers	635	-0.509* (0.273)	0.712*** (0.205)
Motivation signal	513	0.264 (0.264)	0.122 (0.194)
Chance for better pay	503	0.281 (0.257)	0.235 (0.196)
Emphasize injustice	318	0.296 (0.278)	-0.118 (0.216)
Reward efforts	559	-0.075 (0.274)	0.468** (0.209)
Distance negative emotions	110	0.594 (0.458)	0.850** (0.380)
N	1100		

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table reports how the respondents' characteristics in the China's working age population survey are related to their choices of effort in the hypothetical situation similar to that faced by the informed workers in our experiment. As in Table 9, columns 2 and 3 list the coefficients from the multinomial logit regression of the effort choice on the respondents' characteristics. As in Table 9, columns 2 and 3 list the coefficients and standard errors from the multinomial logit regression of the effort choice on the respondents' characteristics.

References

- Abbink, Klaus, Bernd Irlenbusch, and Elke Renner (2000). "The moonlighting game: An experimental study on reciprocity and retribution". In: *Journal of Economic Behavior & Organization* 42.2, pp. 265–277.
- Akerlof, George A and Janet L Yellen (1990). "The fair wage-effort hypothesis and unemployment". In: *The Quarterly Journal of Economics* 105.2, pp. 255–283.
- Bae, Kwang Bin et al. (2022). "Does knowing other workers' wage level promote employees' pay fairness perception? Evidence from a randomized survey experiment". In: *Public Personnel Management* 51.4, pp. 407–429.
- Benjamin, Daniel J (2005). "A theory of fairness in labor markets". In: *Harvard University*.
- Breza, Emily, Supreet Kaur, and Yogita Shamdasani (2018). "The morale effects of pay inequality". In: *The Quarterly Journal of Economics* 133.2, pp. 611–663.
- Card, David et al. (2012). "Inequality at work: The effect of peer salaries on job satisfaction". In: *American Economic Review* 102.6, pp. 2981–3003.
- Charness, Gary and Peter Kuhn (2007). "Does Pay Inequality Affect Worker Effort? Experimental Evidence". In: *Journal of Labor Economics* 25.4, pp. 693–723.
- Clark, Andrew E and Andrew J Oswald (1996). "Satisfaction and comparison income". In: *Journal of Public Economics* 61.3, pp. 359–381.
- Cohn, Alain et al. (Aug. 2014). "Social Comparison and Effort Provision: Evidence from a Field Experiment". In: *Journal of the European Economic Association* 12.4, pp. 877–898.
- Cullen, Zoë (Feb. 2024). "Is Pay Transparency Good?" In: *Journal of Economic Perspectives* 38.1, pp. 153–80.
- Cullen, Zoë and Ricardo Perez-Truglia (2022). "How Much Does Your Boss Make? The Effects of Salary Comparisons". In: *Journal of Political Economy* 130.3, pp. 766–822.
- Falk, Armin, Ernst Fehr, and Christian Zehnder (2006). "Fairness Perceptions and Reservation Wages: The Behavioral Effects of Minimum Wage Laws". In: *The Quarterly Journal of Economics* 121.4, pp. 1347–1381. (Visited on 03/11/2024).
- Fan, Yafan, Tongbo Deng, and Min Li (2023). "Decent work and workplace deviance among Chinese physicians: A relative deprivation perspective". In: *Psychology Research and Behavior Management* 16, pp. 1379–1390.
- Fehr, Ernst and Urs Fischbacher (2004). "Third-party punishment and social norms". In: *Evolution and human behavior* 25.2, pp. 63–87.
- Fehr, Ernst, Lorenz Goette, and Christian Zehnder (2009). "A Behavioral Account of the Labor Market: The Role of Fairness Concerns". In: *Annual Review of Economics* 1.1, pp. 355–384.
- Fehr, Ernst and Klaus M Schmidt (1999). "A theory of fairness, competition, and cooperation". In: *The quarterly journal of economics* 114.3, pp. 817–868.
- Fredriksson, Anders and Gustavo Magalhães de Oliveira (2019). "Impact evaluation using Difference-in-Differences". In: *RAUSP Management Journal* 54, pp. 519–532.

- Gächter, Simon and Christian Thöni (2010). "Social comparison and performance: Experimental evidence on the fair wage–effort hypothesis". In: *Journal of Economic Behavior & Organization* 76.3, pp. 531–543.
- Geurts, Sabine A., Bram P. Buunk, and Wilmar B. Schaufeli (1994). "Social Comparisons and Absenteeism: A Structural Modeling Approach". In: *Journal of Applied Social Psychology* 24.21, pp. 1871–1890.
- Greiner, Ben, Axel Ockenfels, and Peter Werner (2011). "Wage transparency and performance: A real-effort experiment". In: *Economics Letters* 111.3, pp. 236–238.
- Ho, Teck-Hua and Xuanming Su (2009). "Peer-induced fairness in games". In: *American Economic Review* 99.5, pp. 2022–2049.
- Hofstede, Geert (2001). "Culture's consequences: Comparing values, behaviors, institutions, and organizations across nations". In: *International Educational and Professional*.
- Liu-Kiel, Hong et al. (2013). "A cross-cultural real-effort experiment on wage-inequality information and performance". In: *The BE Journal of Economic Analysis & Policy* 13.2, pp. 1095–1120.
- Luttmer, Erzo FP (2005). "Neighbors as negatives: Relative earnings and well-being". In: *The Quarterly journal of economics* 120.3, pp. 963–1002.
- Maitlis, Sally and Marlys Christianson (2014). "Sensemaking in organizations: Taking stock and moving forward". In: *Academy of management annals* 8.1, pp. 57–125.
- McKenzie, David (2012). "Beyond baseline and follow-up: The case for more T in experiments". In: *Journal of Development Economics* 99.2, pp. 210–221.
- Miguel, Edward (2021). "Evidence on research transparency in economics". In: *Journal of Economic Perspectives* 35.3, pp. 193–214.
- Nosenzo, Daniele (2013). "Pay secrecy and effort provision". In: *Economic inquiry* 51.3, pp. 1779–1794.
- Obloj, Tomasz and Todd Zenger (2022). "The influence of pay transparency on (gender) inequity, inequality and the performance basis of pay". In: *Nature Human Behaviour* 6.5, pp. 646–655.
- Oetzel, John, Adolfo J Garcia, and Stella Ting-Toomey (2008). "An analysis of the relationships among face concerns and facework behaviors in perceived conflict situations: A four-culture investigation". In: *International Journal of Conflict Management* 19.4, pp. 382–403.
- Oetzel, John, Mary Meares, et al. (2003). "Interpersonal conflict in organizations: Explaining conflict styles via face-negotiation theory". In: *Communication Research Reports* 20.2, pp. 106–115.
- Oetzel, John and Stella Ting-Toomey (2003). "Face concerns in interpersonal conflict: A cross-cultural empirical test of the face negotiation theory". In: *Communication research* 30.6, pp. 599–624.

- Oetzel, John, Stella Ting-Toomey, et al. (2001). "Face and facework in conflict: A cross-cultural comparison of China, Germany, Japan, and the United States". In: *Communication monographs* 68.3, pp. 235–258.
- Oosterbeek, Hessel, Randolph Sloof, and Gijs Van De Kuilen (2004). "Cultural differences in ultimatum game experiments: Evidence from a meta-analysis". In: *Experimental economics* 7, pp. 171–188.
- Pfeffer, Jeffrey and Nancy Langton (1993). "The Effect of Wage Dispersion on Satisfaction, Productivity, and Working Collaboratively: Evidence from College and University Faculty". In: *Administrative Science Quarterly* 38.3, pp. 382–407. (Visited on 03/10/2024).
- Rand, David G et al. (2013). "Evolution of fairness in the one-shot anonymous ultimatum game". In: *Proceedings of the National Academy of Sciences* 110.7, pp. 2581–2586.
- Runciman, W.G. (1966). *Relative deprivation and social justice: A study of attitudes to social inequality in twentieth-century England*. Routledge and Keagan Paul Publishers.
- Schreurs, Bert et al. (2025/01/12 2020). "Perceived overqualification and counterproductive work behavior: testing the mediating role of relative deprivation and the moderating role of ambition". In: *Personnel Review* 50.3, pp. 1038–1055.
- Smith, Heather J. et al. (2012). "Relative Deprivation: A Theoretical and Meta-Analytic Review". In: *Personality and Social Psychology Review* 16.3. PMID: 22194251, pp. 203–232.
- Ting-Toomey, Stella (1982). "Toward a theory of conflict and culture." In.
- (2005). *The matrix of face: an updated face-negotiation theory*. In W. B. Gudykunst (Ed.), *Theorizing about intercultural communication* (pp. 71–92).
- Ting-Toomey, Stella and Atsuko Kurogi (1998). "Facework competence in intercultural conflict: An updated face-negotiation theory". In: *International journal of intercultural relations* 22.2, pp. 187–225.
- Ubeda, Paloma (2014). "The consistency of fairness rules: An experimental study". In: *Journal of Economic Psychology* 41, pp. 88–100.

8 Appendices

8.1 Example of the job advert used to recruit workers for the experiment (Section 2.1)



有偿问卷输入工作招聘

一边赚钱，一边帮我们做研究！来自香港和德国两所大学的研究人员，现正寻求同学帮助，把问卷调查数据由纸质问卷输入到电脑。三个小时的工作可赚取有吸引力的时薪。工作将安排在十月十五／十六日或二十二／二十三日这两个周末的其中一天，时间会在上午，下午或傍晚，工作地点在七里坪校区。

有兴趣吗？

扫描二维码报名：



网上报名：

问题查询

Note: The translation of the text in the advert is below. "Paid questionnaire entry job recruitment. Earn money while helping us do research! The researchers, from two universities in Hong Kong and Germany, are now seeking help from fellow students to input the survey data from paper questionnaires into computers. Earn an attractive hourly wage for three hours of work. The work will be arranged on one of the two weekends, October 22nd/23rd or October 29th/30th, in the morning, afternoon or evening, and the work location will be at the Chiliping campus."

8.2 Representative page from of the paper questionnaire (Section 2.1)

心理健康问题

填写须知:

- (1) 本表每一问题都只有“强烈同意”、“同意”、“中立”、“不同意”和“强烈不同意”，五种可供选择答案。
- (2) 对于每一个问题或陈述，请将你认为符合你想法的选项圈起来。如果认为“强烈同意”选1，“同意”选2，“中立”选3，“不同意”选4和“强烈不同意”选5。
- (3) 这些问题主要反映你的心情和感受，与学习成绩无关。
- (4) 答案没有对错、好坏之分，请根据你的直觉作答。

(圈出符合你想法的答案)

题号	问题	答案
示范题		
A	繁重的功课总是让你觉得不开心。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
10 陈述 (一)		
a	你夜里睡觉的时候，总想着明天的功课。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
b	老师提问的时候，你总担心自己被老师抽中。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
c	听说要考试就心里紧张。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
d	成绩不好时，你心里感到很不开。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
e	成绩不好时，你总是担心会退。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
f	考试的时候，原来知道的知识想不起来你会感到很焦虑。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
g	考试结束之后，没有出成绩之前总是感到放心不下。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
h	考试之前，你总是担心考不好。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
i	你希望考试能顺利通过。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
j	在没有完成任务之前，你总是担心完不成任务。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意

3

题号	问题	答案
11 陈述 (二)		
a	当着大家读课文时，总是担心会读错。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
b	觉得学校公布的学习成绩总是不大可靠。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
c	觉得自己比别人更担心学习。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
d	做梦时，梦到自己考试没考好。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
e	做梦时，梦到因学习成绩不好受到家长的训斥。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
f	觉得同学在背后说自己的坏话。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
g	受到父母批评时，久久不能忘记。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
h	在玩游戏或者体育运动等竞技活动中输给对方后，不想再干了。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
i	别人在背后议论自己时，感到很厌烦。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
j	在大家面前被老师提问时，感到脸红。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意

题号	问题	答案
12 陈述 (三)		
a	如果你担任班干部，你会感到很紧张。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
b	总是感到好像有人在注意你。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
c	在学习和工作时，如果有人注意你，你会感到很紧张。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
d	受到批评时你会感到不开心。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
e	如果老师批评你，你心里会感到不安。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
f	有时候大家都在笑，你却不想笑。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
g	觉得在同学家里玩不如在自己家里好玩。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
h	大家在一起的时候，你会感到自己很孤单。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
i	与其和同学一起玩，自己一个人玩更开心。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意
j	同学在交谈的时候，你不想加入。	1=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意

4

Note: This is a scanned and anonymised copy of a typical page from a typical hand-filled questionnaire the workers had to digitise. Each questionnaire had 102 positions printed on 9 pages.

8.3 Example of the payslip (Section 2.2)

工资单

资方 : 香港岭南大学与德国歌德大学科研团队
工作 : 邵阳大学学生问卷数据录入
工作描述 : 在网上表格中录入心理健康问卷数据

学生姓名 : _____
学生出生日期 : _____
学生证号码 : _____
工作日期 : _____
工时总数 : 3 小时
时薪 (由研究人员设定) : 35 元人民币 20 元人民币
总工资 (由研究人员设定) : 105 元人民币 60 元人民币

学生签署 : _____

Payslip

Sponsor: Lingnan University and Goethe University
Job: Shaoyang University Questionnaire Data Entry
Job Description: Questionnaire data entry in an online form.

Student Name : _____
Student Date of Birth : _____
Student ID number : _____
Date of work : _____
Total number of hours worked : 3 hours
Hourly rate
(set by researchers) : 35 RMB 20 RMB
Total salary
(set by the researcher) : 105 RMB 60 RMB

Signature of student : _____

Note: This is an example of a payslip for the treated workers translated into Chinese and English languages.

8.4 Example photo of the room for the working session (Section2.3)



Note: This is a photo example of the working session room.

8.5 Text of the pre-experiment survey Q1 translated into English (Section 2.6)

Thank you for participating in our data entry work. Before working, please answer the following questions first. Your answers will be used in our other research projects, but the project work with you today as unrelated. We promise that your information will only be used for research purposes and will not be disclosed to others.

1. Your name:
2. Your student ID:
3. Your date of birth:
4. Your gender:
5. Your profession:
6. Your grade:

Please note that your answer below will allow you to earn money. Please read the instructions carefully before answering the following questions. There is no right or wrong answer. Different people have different feelings about the following questions and their answers are also different.

Suppose there is a distributor who is about to split the sum of 20 yuan between you and the other recipient, the distributor will receive another 10 yuan. The distributor can split the 20 yuan equally between you and the other recipient, or it can favour either you or the other recipient. The distributor, you and the other recipient are all strangers, you do not know each other and will never meet.

After the distributor makes the distribution decision, you can reward or punish the distributor's decision according to your feelings, and increase or decrease the money the distributor receives. When you reward or punish the distributor, you need to pay one-third of the relevant amount as a fee. For example, if you decide to reward the distributor with 1.5 yuan, then you need to pay 0.5 yuan. Or if you decide to punish the distributor 3 yuan, you have to pay 1 yuan. Of course, you can choose not to make any rewards or punishments, then you do not need to pay any fees.

There are three scenarios below. In each scenario, the distributor will allocate 20 yuan to you and the other recipient in different ways. Please tick the option in each scenario to decide whether you will reward or punish the distributor and the related amount. We will randomly select a scenario later (the three scenarios have equal chances) and determine the amount of money you can get according to your choice. For example, suppose you don't make any rewards or punishments in Scenario One, in Scenario Two you reward the distributor with 3 yuan, and in Scenario Three you punish the distributor with 1.5 yuan. If scenario 2 is randomly selected, you will get $14 - (1/3) * 3 = 13$ yuan. We will inform you of the money collection arrangements later.

Scenario 1: You have 10 yuan, the other recipient 10 yuan, the distributor 10 yuan.

- Punish the distributor 6 yuan (you pay 2 yuan)

- Punish the distributor 4.5 yuan (you pay 1.5 yuan)
- Punish the distributor 3 yuan (you pay 1 yuan)
- Punish the distributor 1.5 yuan (you pay 0.5 yuan)
- Do nothing (you pay 0 yuan)
- Reward the distributor 1.5 yuan (you pay 0.5 yuan)
- Reward the distributor 3 yuan (you pay 1 yuan)
- Reward the distributor 4.5 yuan (you pay 1.5 yuan)
- Reward the distributor 6 yuan (you pay 2 yuan)

Scenario 2: You have 14 yuan, the other recipient 6 yuan, the distributor 10 yuan.

- Punish the distributor 6 yuan (you pay 2 yuan)
- Punish the distributor 4.5 yuan (you pay 1.5 yuan)
- Punish the distributor 3 yuan (you pay 1 yuan)
- Punish the distributor 1.5 yuan (you pay 0.5 yuan)
- Do nothing (you pay 0 yuan)
- Reward the distributor 1.5 yuan (you pay 0.5 yuan)
- Reward the distributor 3 yuan (you pay 1 yuan)
- Reward the distributor 4.5 yuan (you pay 1.5 yuan)
- Reward the distributor 6 yuan (you pay 2 yuan)

Scenario 3: You have 6 yuan, the other recipient 14 yuan, the distributor 10 yuan.

- Punish the distributor 6 yuan (you pay 2 yuan)
- Punish the distributor 4.5 yuan (you pay 1.5 yuan)
- Punish the distributor 3 yuan (you pay 1 yuan)
- Punish the distributor 1.5 yuan (you pay 0.5 yuan)
- Do nothing (you pay 0 yuan)
- Reward the distributor 1.5 yuan (you pay 0.5 yuan)
- Reward the distributor 3 yuan (you pay 1 yuan)
- Reward the distributor 4.5 yuan (you pay 1.5 yuan)
- Reward the distributor 6 yuan (you pay 2 yuan)

8.6 Text of the post-experiment online survey Q2 translated into English (Section 2.7)

Thanks again for helping us with data entry in October this year.

We would like to learn about your experiences while working for us. Please rate on the scale of 1 (not at all) to 7 (very much) the following statements about your experiences:

1. The work was difficult
2. You already had data entry experience before doing work for us
3. Your experience of working for us contributed to the development of your professional skills and is useful
4. The work room had adequate size and was well-lit and well-aired
5. The pay was fair

Now, can you answer the following questions in your own words.

1. How many people you are in regular contact with (e.g., friends, classmates, etc.) do you know to have registered to do the data entry work for us?
2. Of these people, how many did their work BEFORE you?
3. Did you hear BEFORE coming to work for us that we were paying different rates to different people for the same work?
4. If your answer to the previous question is YES, where did you hear about it from?
5. In your opinion, what were the reasons for different pay rates given to different people for the same work?

8.7 Robustness check for treatment effects on work effort outcome variables

Table 11: Treatment effects on the performance outcome length with different sets of control variables

	(1)	(2)	(3)	(4)	(5)
Lag.length	0.539*** (0.099)	0.586*** (0.117)	0.545*** (0.101)	0.510*** (0.090)	0.517*** (0.093)
Treatment	-0.214*** (0.078)	-0.207** (0.090)	-0.210*** (0.079)	-0.183** (0.075)	-0.173** (0.077)
Controls:					
Session size	+	+	+	+	+
Female			+		+
Major			+		+
Study cohort			+		+
Date + time				+	+
Exclude heard		+			
N	311	244	311	311	311
R-squared	0.448	0.501	0.464	0.484	0.498

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Regression estimator: ANCOVA (McKenzie, 2012).

Standard errors in parentheses are clustered by work session.

The estimates are expressed in standard deviations of the outcome variables.

Note: This table shows the treatment effects on the work effort outcome variable Length, the average length of time it took the workers to process a questionnaire with different sets of control variables.

$N = 311$ is our study sample, $N = 244$: 260, who participated in the post-experiment survey (Q2), minus 16, who mentioned that they have heard of pay differences before coming to work.

Table 12: **Treatment effect on the performance outcome empty fields with different sets of control variables**

	(1)	(2)	(3)	(4)	(5)
Lag.empty fields	0.717*** (0.127)	0.763*** (0.130)	0.701*** (0.118)	0.720*** (0.127)	0.702*** (0.118)
Treatment	-0.207** (0.089)	-0.178* (0.097)	-0.160** (0.080)	-0.230** (0.098)	-0.173* (0.092)
Controls:					
Session size	+	+	+	+	+
Female			+		+
Major			+		+
Study cohort			+		+
Date + time				+	+
Exclude heard		+			
N	311	244	311	311	311
R-squared	0.432	0.481	0.487	0.434	0.489

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Regression estimator: ANCOVA (McKenzie, 2012).

Standard errors in parentheses are clustered by work session.

The estimates are expressed in standard deviations of the outcome variables.

Note: This table shows the treatment effect on the work effort outcome variable Empty fields, the average number of empty fields in the questionnaires workers processed.

$N = 311$ is our study sample, $N = 244$: 260, who participated in the post-experiment survey (Q2), minus 16, who mentioned that they have heard of pay differences before coming to work.

8.8 Text of survey of China's working-age population (Section 4.2)

Hello and thank you for taking time to participate in our survey! We are an international team of researchers, and we'd like to share with you a finding from our recent study. Please read the description below, which should not take longer than five minutes. With questions following afterwards, the whole survey should take about 10 to 12 minutes.

Description:

A large number of university students in China were recruited to do a one-off data entry job lasting three hours for a pre-specified hourly pay rate. The work was individual and there was no interaction or communication between the workers while working. There were no bonuses additional to the hourly pay and no further employment opportunities beyond the duration of the job, all of which was clearly communicated.

While most of the people hired for the job received a standard pay rate typical for this type of job in the given region, some ended up receiving a higher rate by a random chance, independently of skill, ability, motivation or any other personal characteristics.

Now, let's focus on the people who received the typical pay rate. At the start of the three-hour working session, as they arrived at their workplaces, they knew only their pay rate, which they accepted. But, later in the session, some of them found out that there were others doing the same work for a higher pay rate. For convenience, let us label these people "informed". The other people, earning the same standard pay, did not know that someone else was earning more than them. Let us label these people "uninformed".

After the informed people had found out that others received more for the same work, they tended to work harder, on average, producing more output as compared to the uninformed who didn't know other people's wages.

Questions:

1. Now we would like to ask for our opinion on why the informed people worked harder after learning of their pay relative to others. We have thought of several possible explanations, described below. Please choose up to three explanations from the list below that you think are most likely, or provide your own if you feel none of ours are good enough.

Menu options:

- They worked harder because they believed that those earning more than them were better workers and they wanted to match them in performance.
- They worked harder because, although the job was one-off and pay was fixed in advance, they hoped that their efforts might still be rewarded somehow.
- Working hard even though the pay is relatively low sends a strong signal of integrity and high motivation. The informed people wanted to send this signal to the managers who observed their performance.
- They perceived unequal pay for the same work as unjust and wanted to do something to

achieve equal pay. They chose to work harder to emphasize how unjust it was to pay them less than others.

- They believed the only chance for a better pay was to work harder. They took this chance, however small, to avoid the feeling of regret from not using it.
 - They worked harder to distance themselves from negative emotions caused by learning of being paid less than others.
 - None of the explanations above sounds convincing to be, but I have my own, namely...
 - None of the explanations above sounds convincing to me, and I cannot think of any other explanations.
2. If YOU found yourself in a similar situation as the informed people, how would you react: worked harder, worked less hard, did not change effort.

Now, please tell us a little more about yourself. All your information will be treated with utmost anonymity and will not be revealed to third parties.

3. Gender
4. Age
5. Family status: single, married or living with a partner
6. Education in years:
7. Work experience in years
8. Occupation type: in education, not employed or in education, self-employed, entry level employee, professional employee with managerial duties, professional employee without managerial duties, senior manager
9. Occupation sector (if employed): business sector, public sector
10. Do you consider yourself religious: yes, no
11. On a 1 to 7 scale, indicate your agreement with the following statements (1 completely disagree, 7 completely agree):
12. "Unequal pay for the same work is up to the employer and should be socially acceptable"
13. "I am generally willing to reward fair behavior, even if it is costly to me"
14. "I am generally willing to punish unfair behavior, even if it is costly to me"