Unequal pay, fairness perceptions, and performance: Experimental evidence from China

In a field experiment in China, we paid different hourly rates for the same work and informed randomly selected lower-paid workers that others received more. Compared to the uninformed but equally paid workers, the informed perceived their pay as less fair, but, surprisingly, increased their output without reducing quality. Although we provided no reason for the pay difference, a post-experiment survey revealed that workers developed their own, predominantly benign, explanations, the leading one (53%) being higher quality of the better-paid workers. We validated our experimental results with a follow-up survey of 1100 people of working age in China whom we briefed about our findings and asked for their explanations. 58% believed that the informed workers perceived their higher-paid peers to be better workers and aspired to match them, and 75% stated that they would also work harder in the same situation. Our results question several theoretical predictions on reactions to unequal pay and hint at the importance of culture in moderating these reactions.

1 Introduction

Growing awareness of income inequality fuels public demand for equity, which is met by government legislation and corporate policies that promote pay transparency. Pay transparency does reduce pay inequality (Obloj, Zenger, 2022; Cullen, 2024), but it may also have consequences for organizational performance, which may differ by context or dimension of pay transparency.[1] In this study, we focus on pay level transparency, asking what would happen to lower-paid workers' performance when they find out about earning less than peers.

Our data come from China – a context that received surprisingly little attention in the pay level transparency literature so far (Liu-Kiel, et al. (2013) is the only exception). Pay level transparency is not widespread in China (Alterman, et al., 2021), as firms are not legally required to disclose salaries of their workers except senior executives (Tse, 2023). However, like in the rest of the world, there is a growing demand for more transparency in pay matters. For instance, according to a recent survey by Aon (2024), 77% of China-based HR professionals consider pay transparency important, but only 25% say their firms have proactively disclosed some aspects of their pay policies. Empirical evidence on performance effects of pay level transparency in China would give the debate on this prominent – and sensitive – topic a firmer scientific basis.

Intuitively, knowing of being paid less than peers could be frustrating and negatively affect one's work performance. Evidence from empirical studies on performance effects of pay level transparency, originating mostly from outside China and summarized in Table 1, supports this intuition: after learning of their position in the earnings distribution, lower-paid workers become less happy (Luttmer, 2005; Perez-Truglia, 2020), less satisfied with their pay (Cohn, Fehr, Herrmann, et al., 2014) and job (Pfeffer, Langton, 1993; Clark, Oswald, 1996; Card, et al., 2012; Schnaufer, et al., 2022) and more willing to quit (Dube, Giuliano, Leonard, 2019), while relatively high earners react less strongly. Furthermore, informed lower-paid workers tend to work less hard than their equally earning but uninformed peers (Geurts, Buunk, Schaufeli, 1994; Gächter, Thöni, 2010; Nosenzo, 2013; Cohn, Fehr, Herrmann, et al., 2014; Breza, Kaur, Shamdasani, 2018; Schreurs, et al., 2020; Cullen, Perez-Truglia, 2022; Gutierrez, Obloj, Zenger, 2025), are less likely to collaborate (Pfeffer, Langton, 1993), especially with higher-paid coworkers (Bamberger, Belogolovsky, 2017), and may reduce work quality (Greiner, Ockenfels, Werner, 2011). The few exceptions are Charness, Kuhn (2007), who found no effect of unequal pay on individual effort choices, Bartling, von Siemens (2011) who found the same in the team context, and Liu-Kiel, et al. (2013) who found that informed workers earning a lower piece rate worked harder than equally earning uninformed workers. [2]

[1] (Bamberger, 2021) identifies three dimensions of pay transparency: outcome (or level of pay), process (pay setting rules), and communications about pay within firms. They may have different performance effects: higher pay level transparency (our focus) could frustrate lower-paid workers, while more transparency in the process of pay setting and pay communications may render pay policies more justifiable and the resulting pay inequality more acceptable.

[2] In a lab experiment without pay inequality, Chen, et al. (2022) paid the same flat wage to all and made it

Given the large evidence that pay level transparency worsens lower-paid workers' attitudes and performance, why do a new study in China rather than extrapolate the existing results to Chinese context? The theoretical propositions underlying performance effects of pay level transparency come from inequity theory (Adams, 1963; Adams, 1965) and its later developments (Fehr, Schmidt, 1999; Folger, Cropanzano, 2001). The gist of the argument is: people are averse to inequitable outcomes they perceive as unfair, such as unequal pay for the same work, and will take restorative action towards the authority who they deem responsible for these outcomes, for instance, by reducing work effort that benefits the firm that pays unequally. However, the theory itself and most of its empirical applications originated in Western cultures (Liu-Kiel, et al. (2013)'s study of American vs. Chinese students and Breza, Kaur, Shamdasani (2018)'s study of Indian workers are only two exceptions), and it is not clear to what extent its predictions would hold in cultures different from the West, such as China.

Why could China be a special case for pay level transparency research? Fairness concerns are universal, but culture could still moderate how they shape behaviors (Oosterbeek, Sloof, Van De Kuilen, 2004), including reactions to unequal pay. For instance, high power distance can weaken the link between individual perceptions and behaviors (Daniels, Greguras, 2014) and thus render unequal pay less consequential for the actions of the workers who might perceive it as unfair. Also, a culture strong on long-term orientation may shift one's emphasis away from satisfying momentary psychological needs, such as restoring equity, to observing longer-term interests which may not be best served by instant retribution. Chinese culture is uniquely high on both power distance and long-term orientation dimensions worldwide (The Culture Factor, 2025).

Our real-effort field experiment involving 329 student workers at Shaoyang University in Hunan, China, produced several discoveries. We paid different hourly rates to different workers for the same job of digitizing paper questionnaires, not informing the workers of the pay difference before the work commenced. Then, in the middle of the three-hour work session, though a routine administrative procedure, we informed randomly selected lower-paid workers of the higher rate we had paid to some other workers. The informed lower-paid workers perceived their pay as less fair than did equally earning but uninformed ones, but, surprisingly, they increased their average output without protest, quitting or reducing output quality. Although we provided no explanation for the pay difference, a post-experiment survey found that 53% of the lower-paid believed that higher-paid workers were better quality, and 37% thought the work they did was different. Other explanations elicited from the workers were also benign, and we found no significant performance differences by explanation provided.

Our findings, puzzling though they are, cannot be dismissed on the grounds of flawed research design or lack of external validity. We took many precautions in our procedures to

transparent to the treated participants that their wages were the same (no pay transparency in the control condition). They found a negative effect of *equal pay* transparency on labor input and positive, though statistically insignificant, on work output.

ensure our experiment met or exceeded the current research design standards of the relevant literature, and our results survive numerous robustness checks. Moreover, a follow-up survey of China's working-age population ($N = 1100$) lends external validity to some of our discoveries. Specifically, when we briefed the survey respondents about our experiment and findings and asked them to provide explanations, 58% stated that the informed workers believed their higher-paid peers to be better workers and tried to match them by working harder (53% 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.

Like most of the pay level transparency studies, ours is guided by theoretical arguments on inequity and fairness, but our findings largely contradict these arguments. After examining several alternative explanations, we conclude that a combination of high power distance and long-term orientation, characteristic of Chinese culture, is the most plausible explanation. Hence our proposal for the inclusion of the culture factor as a boundary condition in the theoretical models linking pay level transparency and organizational performance, along with some practical suggestions on how this could be implemented. We outline further contributions of our study to pay transparency research and practice, and its limitations in conclusion.

2 Study design and preliminary findings

We strove to combine the rigorous control of an experimental setting with the realism of a genuine work environment. This section describes the steps we took to achieve this ambition. Before going into details, it is important to note that we pre-registered our study design with the American Economic Association's registry for randomized control trials (registration ID AEARCTR-000XXXX [redacted for anonymity]), and secured an approval from the relevant Ethics Committee.

2.1 Guiding theory

Following the existing literature on pay level transparency, in designing our study, we took guidance from inequity theory (Adams, 1963; Adams, 1965) and later theoretical developments building on it and producing somewhat different versions of "a theory of fairness" (Fehr, Schmidt, 1999; Folger, Cropanzano, 2001). Inequity theory defines a social exchange relationship, such as paid employment, as inequitable when the perceived outcome-to-input ratio varies across the agents involved in the relationship, for instance, when workers earn different wages for the same work (Adams, 1963). Perceived inequity is discomforting, especially when it is disadvantageous to the focal agent (e.g., lower, rather than higher pay), and motivates action to reduce or eliminate it, for instance, by reducing work effort or withdrawing altogether (Adams, 1965; Akerlof, Yellen, 1990).

Building on inequity theory, Folger, Cropanzano (2001)'s theory of fairness explicates the

process through which people form perceptions of inequity that lead to actions. A given inequity can be perceived as more, or less, unfair depending on who is responsible for the observed inequity, what reasons they had for their decision, and whether they could or should have decided otherwise. Inequity is perceived as more unfair and may lead to stronger restorative action when it is attributed to the deliberate decision made by an authority whose reasons are unclear or unconvincing and and who could have decided more equitably. Thus, Folger, Cropanzano (2001)'s theory links inequity with decision making in organizations, the context where much of pay inequality originates, and implies that performance effects of unequal pay are mediated through pay fairness perceptions and moderated by reasons for pay inequality.

Fehr, Schmidt (1999)'s theory of fairness focuses on fairness as "self-centered inequity aversion", which it treats as a personality trait that differs between individuals and, similarly to Folger, Cropanzano (2001), contexts. That is, a given inequity will be perceived as more, or less, unfair depending on the focal individual's aversion to it. A direct implication of this theory for our study is moderation of performance effects of unequal pay by individual-specific inequity aversion. Holding the organizational context fixed, inequity aversion, also referred to as "fairness concerns" (the term we will use in this study), is measured as the willingness to pay for punishing the perpetrator of inequitable action, which can be elicited through incentivized experiments (Cohn, Fehr, Goette, 2015; Abbink, Irlenbusch, Renner, 2000), as we do later.[3]

Our theoretical framework combines the above theoretical arguments into what we label as "fairness theory" for brevity (there are multiple fairness theories). This theory would predict a relationship between pay level transparency and work effort by the lower-paid workers that is mediated by their fairness perceptions (how fair is wage?, Folger, Cropanzano (2001)) and moderated by their fairness concerns (how far am I willing to punish the authority acting unfairly?, Fehr, Schmidt (1999)) and reasons for unequal pay (why did the authority act this unfair way?, Folger, Cropanzano (2001)). This simple conceptual model, pictured in Figure 1, guides our experimental design in ways we describe next.

Insert Figure 1 about here

2.2 Context: worker recruitment, job task, and pay rates

We designed a real-effort field experiment to capture causal effects of unequal pay free of possible confounds in a realistic yet controlled work environment. Our experiment took place on two

[3]Empirical evidence for varying inequity aversion is offered by the results from numerous experiments with the "ultimatum game": the distributor shares a valuable resource between self and a receiver, both get the allocated shares if the receiver accepts the offer, but nothing otherwise (Oosterbeek, Sloof, Van De Kuilen, 2004; Rand, et al., 2013; Ho, Su, 2009). Contrary to the naive economic intuition – the receiver is better off from any nonzero offer from the proposer and should therefore always accept – offers that are too unequal are frequently rejected. However, the frequency of offer rejection varies across individuals and competitive and cultural contexts.

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 online APPENDIX A) 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 three-hour long job of digitizing paper questionnaires, paid per hour (not per questionnaire) and before the work started. The paper questionnaires (example in APPENDIX B) were used in a separate and unrelated research project. Each questionnaire contains 102 fields and took 6.2 minutes to digitize, on average.

The job advert offered "attractive pay" without specifying the exact rate. The usual rate for similar jobs on campus at the time was 20 RMB/hour, which is what we paid to the majority of the workers once they showed up for work. A random sample of 18 workers were paid a substantially higher rate, 35 RMB/hour. The small size of the higher-paid worker sample reflects the focus of our study on the effects of pay level transparency for lower-paid (20 RMB/hour) workers; indeed, we implemented the higher-pay condition primarily in order to avoid having to lie to the informed workers about the pay difference.

2.3 Treatment, outcomes, and moderators

Our treatment was to inform randomly selected lower-paid workers (20 RMB/hour) of being paid less for the same job than the other, higher-paid ones (35 RMB/hour). The uninformed workers in the control condition were paid the same rate as the informed workers, but were not informed about 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 which may have affected their work performance (it did, as we show later).

We informed the treated workers of the higher rate we paid to others via the payslips that we asked them to sign during the pre-scheduled 15-minute break in the middle of their work session, to confirm the receipt of their payment. The uninformed workers in the control group got the same payslips and at the same time during their work session, except that their payslips contained information about their pay rate only. As the example payslip in APPENDIX C shows, the informed workers saw both the lower (20 RMB/hour) and higher (35 RMB/hour) hourly rates next to each other, with the lower rate they received encased in a red box, and with a text message saying that it was us, the researchers, who determined the applicable pay rate. The uninformed workers got to sign the same payslip, except there was no information about the higher pay rate. We took extensive measures (described below) to ensure the informed workers did notice the pay difference.

As per our pre-registered experimental design, our focal outcomes are: 1) the length of time

spent digitizing a paper questionnaire, 2) the number of empty fields in a digitized questionnaire, and 3) pay fairness perceptions, measured in a post-exoeriment survey, which we also treat as a mediator in some specifications. Additionally, in post-experimental analysis, we analyze the quality of work as measured by the number of mistakes per questionnaire discovered upon inspecting a random sample of questionnaires. To aid interpretation of regression results, we standardized each variable by subtracting its pre-treatment mean and dividing by pre-treatment standard deviation, so that the treatment effects can be quantified in terms of pre-treatment variation in the focal outcome.

Our pre-registered moderator variables are fairness concerns, measured in a pre-experiment survey, and reasons for the pay difference stated by workers in the post-experiment survey that also measured pay fairness perceptions. In that survey, workers were asked to state, in free text, why they thought the pay difference had been implemented. We applied natural language processing to classify the explanations stated by workers into common themes, the indicators for which we later used as moderators.

2.4 Allocation into experimental conditions, and descriptive statistics

After the registration deadline of October 10th expired, we randomly allocated the 514 workers who registered with us into the lower- (20 RMB/hour, $N = 492$) and higher-pay (35 RMB/hour, $N = 22$) groups. Those ended up in the lower pay group were randomly assigned into the treatment (informed) and control (uninformed) 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. We used seminar rooms (example photo in APPENDIX D) on campus 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. In total, we had twelve rooms booked for three time slots on four days = 144 planned 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 higher-pay (35 RMB/hour) sessions. Owing to technical problems, we had to exclude observations from three workers in the treatment and ten in the control conditions, which left us with the usable sample of 150 workers in the treatment and 161 in the control conditions, plus 18 workers in the higher-pay group.[4]

Why did we end up with more control than treatment work sessions (79 vs. 47)? To minimize

[4]We 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.

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 30th). 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 2 reports descriptive statistics for the 514 workers who registered to work for us, and for the 172 workers who did not show up. Looking at all the registrations, an average worker is 19.4 years old and is on their second 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). Female, younger and non-art major workers were statistically significantly more likely to show up for work. We control for worker characteristics in regression analysis.

Insert Table 2 about here

Focusing on the workers in the control vs. treatment conditions, there are small differences in the average study year and study major structure, and a bigger difference in the average group size per work session: 3.19 in the treatment vs. 2.04 in the control condition. 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 and other worker and work session characteristics. Importantly, the allocation of workers into treatments is random with respect to their pre-treatment work output.

2.5 Experimental procedures

To help us administer our experiment smoothly and without uncontrolled information spillovers, we externally hired room managers and assistants whom we trained before the work sessions began. Room managers prepared the work sessions, received the workers, announced instructions, prevented mobile phone use or other communications during work, and reported any out-of-protocol situations such as complaints, questions, walk-outs (there were none), or technical issues. 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.

Each work session lasted three hours. At the start, 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, we instructed the room managers to inform the workers that their employment was one-off and would not be continued.

Next, the room managers asked the workers on our behalf to participate in a survey unrelated to the work they were about to do (details in the next section). After the workers completed the survey, assistants entered the room to announce the applicable hourly pay rate and hand out individual sealed envelopes containing the total payment in cash, asking each worker to count the money and 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 (informing the lower-paid workers that other received more) was administered. After the break, the second, post-treatment 75-minute work period began. At the end of the work session, the room managers thanked the workers and the assistants escorted them out of the building so as not to cross paths with the newly arriving workers, thus eliminating uncontrolled information spillovers.

We took measures to ensure that the informed workers noticed the pay difference in their payslips. 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 treated work sessions were asked to make the following announcement right after the assistants' departure: "I have noticed there are differences in pay rates among the workers doing the same work as you. The researchers you are working for confirm 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.

To answer questions from the informed 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. However, no questions were asked.

2.6 Pre-experiment Survey Q1: Eliciting fairness concerns

Survey Q1 was administered in the treatment and control groups upon the workers' arrival to the designated rooms, lasted ten minutes, and focused on eliciting workers' fairness concerns, a key moderator in our theoretical framework. The text of survey Q1 is in APPENDIX E.

Our measure of fairness concerns comes from Abbink, Irlenbusch, Renner (2000)'s "moonlighting" game, and was also used by Cohn, Fehr, Goette (2015) for the same purpose. 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: scenario 1: 10 RMB for each recipient; scenario 2: 14 RMB for the focal worker, 6 RMB for the other recipient; and scenario 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 deductable from their total payment (there was no payment for not rewarding or punishing). One of the three scenarios was randomly selected 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 was selected for payment, the worker would get $6 - 1 = 5$ RMB sent to them later on.

Figures 2-4 report the 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 (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 the existing evidence that people do have fairness concerns which affect their economic decisions (Fehr, Goette, Zehnder, 2009; Ho, Su, 2009; Oosterbeek, Sloof, Van De Kuilen, 2004; Fehr, Fischbacher, 2004).

Insert Figures 2-4 about here

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

After the experiment, in early February 2017, we invited the workers to pick up personal certificates confirming that they worked for us. Upon arrival, we asked them to complete a post-experiment survey, labeled Q2 in what follows. Of the final sample of 329 workers (311 in the treatment and control groups, plus the 18 higher-paid workers), 276 (84%) showed up to collect their certificates and participated in survey Q2 whose text is in APPENDIX F.

The first five questions in Q2 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 remainder of Q2 consists of two questions asked only to the workers in the control or treatment group. We reminded of paying different rates for the same work and asked whether the workers had heard about different pay rates before coming to work, and if so, where they heard this information from. 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 asked the workers to share their opinions, in free text, as to why different workers received different hourly pay rates for the same work. We use their answers, processed with an AI-powered text classification algorithm, as moderators in later analysis.

2.8 Estimation method

We estimate the average and mediated/moderated treatment effects of our interest from the analysis-of-covariance (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:

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

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 pre-treatment average processing time, both standardized, the treatment dummy, and controls. [5] We cluster errors at the work session-date-time level. The approach we take is a straightforward extension of the difference-in-difference estimator increasingly used in empirical management studies that exploit (quasi-)random variation in data to estimate causal effects of interest (Li, Certo, 2021; J Li, et al., 2024),

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

One can see that the difference-in-difference regression (2) is a special case of the ANCOVA regression (1) with the restriction $\gamma = 1$, which is relaxed in (1). It is important to relax this restriction to allow for possibly different trends in the control and treatment groups (Li, Certo, 2021) or for dynamic feedback from pre- to post-treatment performance outcomes. In fact, the estimated γ in our regressions is 0.5-0.7, depending on specification (Tables G1-G3 in APPENDIX G), significantly different from $\gamma = 1$ assumed in the standard difference-in-difference regression (restriction test $p-$values between 0.07 and 0.001).

[5] We 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}.

We estimate the mediation and moderation relationships with an extended version of regression (1):

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

where the variable M_i added to the controls (mediation) and interacted with the treatment dummy T_i (moderation).

3 Experimental results

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

Table 3 (column 1) shows that, compared to the uninformed, the informed workers in the treatment group perceive their pay as less fair, even though the pay rate in the control and treatment groups is the same (20 RMB/hour). The treatment effect estimate is not only statistically significant but also substantial in magnitude, 0.271 of the standard deviation in pay fairness perceptions. This result is robust to variations in regression specification (Tables G1-G3 in APPENDIX G).

Insert Table 3 about here

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, statistically as well as magnitude-wise (columns 2-5). The treatment affecting fairness perceptions but not other aspects of work is a useful manipulation check showing that the workers noticed the treatment and reacted to it.

Result 2: Informed workers tend to work harder.

Columns 1 and 4 in Table 4 show the average treatment effects on the key pre-registered worker performance outcomes: the average length of time to process a questionnaire and the average number of empty fields in the questionnaires processed. Surprisingly, informed workers tend to process questionnaires faster and leave fewer fields empty than do uninformed workers in the control group. The average treatment effects are mostly statistically significant and substantial in magnitude, amounting to 12-17% of the standard deviations of the corresponding pre-treatment outcomes, depending on specification. They are also robust to variations in regression specifications, and, importantly, to the exclusion of the workers who heard of the pay differences before coming to work (Tables G1-G3 in APPENDIX G). As we noted earlier, there were no protests or walk-outs in any of the 132 work sessions we organized, and, as we will show later, higher output does not come at the expense of lower quality.

Insert Table 4 about here

Result 3: Treatment effects on performance are independent of the workers' pay fairness perceptions or fairness concerns.

A further result from Table 4 (columns 2-3 and 5-6) is that pay fairness perceptions, measured in the post-treatment survey Q2, neither mediate nor moderate the treatment effects on worker performance, implying that performance responses to our treatment are independent of how (un)fair workers perceive their pay to be. We also see no moderation of the treatment effects by workers' fairness concerns, elicited in the pre-treatment survey Q1. More fairness-concerned informed workers do not put in lower effort. This is not because the workers are not concerned about fairness: the results from survey Q1 demonstrate that many choose to punish unequal allocation of resources by the allocator in the moonlighting game, even at a personal cost (Figure 4).

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 (column 1 in Table 5). These 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 reducing work performance. This disconnect suggests important contextual factors are present in the real work situation but absent in the stylized, though payment-relevant, situation played out in Q1.

Insert Table 5 about here

Result 4: Workers' explanations for the pay difference are mostly benign, the majority referring to better quality of the higher-paid peers.

We begin probing into these factors by asking the workers "In your opinion, what were the reasons for different pay rates given to different people for the same work?" in survey Q2 (to recall: we gave no explanation ourselves). Following studies using texts as data (e.g., Blondeel, Everaert, Opdecam, 2025), we designed a rule-based multi-label text classification algorithm to parse the workers' answers into snippets containing keywords for the reasons for the pay difference, and to identify common themes in these snippets. We ran our algorithm in ChatGPT 5.1 and verified the consistency and replicability of the results manually. The complete implementation protocol is in APPENDIX H.

Table 6 lists the pay difference explanations text classification results. Our algorithm extracted a total of 358 snippets from the texts written by the 260 workers who participated in

Q2, and classified the snippets into eleven themes, including "don't know" stated by 5.4% of the workers, and "other" (not classified elsewhere, 3.5%). The leading themes are "Efficiency/capacity/ability", referring to better performance of the higher-paid workers and mentioned by 38.8% of the workers, and "Workload/hours" (26.9%), emphasizing higher input from those workers. Closely related but less frequent themes are "Work attitude" (16.2%), "Job content/difficulty" (13.5%), and "Work quality" (10%). Other themes are rare. Some workers mentioned more than one theme; hence the total tally exceeding 100%. Some themes differ in the number of counts across the treatment conditions (e.g., "Work attitude" is mentioned more frequently by the control group workers), but these themes are relatively rare and it is difficult to evaluate statistical significance of the observed count differences.[6]

For later analysis, we aggregated the themes into three groups, as shown in Table 6: "better worker", "different work" and "other", related to the characteristics of the higher-paid workers, the work they did, and other factors the workers believed to be relevant for pay. 137 workers (53%) mentioned at least one of the "better worker" themes at least once, 97 workers (37%) mentioned one of the themes in the "different work" group, 30 workers (12%) mentioned both, and 56 (22%) mentioned "other" themes not related to the workers or the work, the counts similar across the treatment conditions. Thus, even in the absence of information from us, a large majority of the workers seem to have made sense of earning less than peers (Maitlis, Christianson, 2014), in different but benign ways mostly related to quality of the higher-paid workers or the work they did.

Insert Table 6 about here

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

Table 7 reports treatment effect estimates specific to the theme groups formed to cover the entire sample but without overlaps: "better worker" only, "different work" only, "better worker AND different work", and "other" (none of the above). The theme group-specific treatment effects are not significantly different from each other (p-values from 0.169 to 0.437, depending on the outcome). This no-result does not necessarily contradict Breza, Kaur, Shamdasani (2018) who found performance effects of pay transparency to be strongly 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. [7] In other words, there may be a macro-level factor (culture?) that is shared by many workers and shapes their perceptions of, and responses to, unequal pay.

[6] Multiple themes being mentioned by the same worker precludes the use of the χ^2-test which assumes one count per worker.

[7] Another possible reason for the lack of moderation could be low reliability of the explanations measured in

Insert Table 7 about here

Result 6: Some treatment effects differ by gender and baseline performance.

Table 8 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 (regression (3)). The treatment effects on performance are independent of the worker's gender, study cohort, major, or working session. Interestingly, informed workers with worse baseline (pre-treatment) performance respond to treatment more strongly positively. Informed female workers perceive their pay as less unfair than informed male workers.

Insert Table 8 about here

4 Further Analysis

In this section, we report on three post-hoc, not pre-registered analyses we undertook to better understand and validate our surprising experimental results.

4.1 Quality check

One possibility for the informed workers to punish us for paying them less than peers may have been to worsen the quality of their output, as found in Greiner, Ockenfels, Werner (2011). To test it, we collected information on the number of mistakes in each of randomly selected 407 questionnaires (6% of the total processed) digitized by 89 different workers from the control or treatment groups (29% of the total headcount those groups). A mistake is defined as any discrepancy between the original paper questionnaire and its digital version. We hired six assistants whom we trained to detect mistakes but did not tell anything 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 9 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

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 informed and uninformed workers measured in the same survey and before they were reminded of our treatment.

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.

Insert Table 9 about here

The average number of mistakes per questionnaire in the quality check subsample is 1.9, or about 2% of the total number of positions. Table 10 reports the treatment effects on the number of mistakes from the ANCOVA regressions (1) estimated at the questionnaire (columns 1-2) and worker (column 3-4) levels, with and without the usual controls plus controls for the length and the number of empty fields, which may be correlated with the number of mistakes, and the assistant fixed effects (some assistants may have been better at detecting errors than others). 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.[8]

Insert Table 10 about here

4.2 Survey of China's working-age population

In an attempt to externally validate our experimental results, we hired a survey company to run an online survey of working age population in China we designed in April 2021. 1100 people aged 18-68 participated in the survey, their descriptive statistics are in column 1 of Tables 11 and 12. Since it was done online, there is an over-representation of the young (average age 28), low-earning and currently in university education, so we cannot claim our survey to be nationally representative. On the other hand, the survey sample is closer to our worker sample, allowing us the better to compare the two samples.

Insert Table 11 about here

[8] One could argue that the treatment effect estimates on the number of mistakes are insignificant just 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 but made the quality check subsample equal in size to the entire sample. The simulated standard errors of the treatment effect estimates are about half the size of the actual standard errors reported in Table 10, but even then the treatment effects would still not reach statistical significance in three out of four specifications shown in Table 10.

In the survey questionnaire (text in APPENDIX I), we briefed the participants about our study and its findings and asked them to choose from a drop-down menu of eight options up to three explanations to our findings that they thought were most likely, or to provide their own explanation if they found ours unsatisfactory. The total number of explanations chosen by the respondents was 2661, or 2.42 per respondent. Figure 5 shows distribution of the explanations given by survey participants. The leading explanation for our main results, chosen by 635 respondents (58% of the sample) and labeled "better workers" for brevity, is: "*They* [informed 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 consistent with the results of our survey Q2 where "better worker" 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 benign, except for the offered but less frequently chosen "emphasize injustice" and "distance from negative feelings". (The own, free-text explanations are few and can be grouped with the explanations we offered to the participants to choose from.)

Insert Figure 5 about here

To relate the participants' explanation choices to their personal characteristics, we ran multinomial logit regressions whose results are reported in columns 2-7 in Table 11. 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 RMB per year) relative to the baseline income group (< 30k RMB 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 informed workers in our study and tell us what they would do in their stead: work harder, work less hard, or not change effort. 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 12. Again, 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.

Insert Table 12 about here

4.3 Results for the higher-paid workers

To complete our analysis, we now turn to the small group of 18 randomly selected higher-paid and uninformed workers whom we paid a substantially above-market hourly rate, 35 RMB/hour. Table 13 reports the differences in the average performance outcomes between the higher- and lower-paid workers (the control group), both uninformed, estimated from equation (1). The estimates from Table 13 can be interpreted as the effects of a treatment consisting of paying a higher wage under zero pay level transparency. This treatment is not focal to our present study, hence our decision to employ relatively few workers for such a generous pay rate. Still, it produces interesting results. The uninformed higher-paid workers perceive their pay as more fair and make fewer omissions in their digital output than do their lower-paid peers from the control group (the effect on the length is negative but statistically insignificant). These results are broadly consistent with gift-exchange theories (Dodlova, Yudkevich, 2009; Esteves-Sorenson, 2018), which in our setting would predict a reciprocal response to a clearly above-market wage perceived by the workers as a "gift" from the employer, and are similar to the recent finding in Gutierrez, Obloj, Zenger (2025) on a much larger sample of U.S. academics. Importantly, the results also suggest that workers care about pay level in making their effort decisions, lending extra internal validity to our experimental design that assumes that effort choice is related to pay.

Insert Table 13 about here

5 Discussion

Ours is one of only two studies on performance effects of pay level transparency in China. We find that informing workers of being paid less than others for the same work and in the same "firm" we set up reduced their pay fairness perceptions but increased their output without detriment to quality. The other study, Liu-Kiel, et al. (2013), observed the same effect on output but in a different setting, with piece rather than hourly rate of pay. Liu-Kiel, et al. (2013) explained their result through inequity aversion: workers earning a lower piece rate choose to produce more output to bridge the total income gap with those earning a higher piece rate.

This explanation is consistent with a version of fairness theory that defines fairness in terms of equality in outcomes, not treatments (e.g., Fehr, Schmidt, 1999), but most of the literature, including our study, operates with flat wages or hourly rates not linked to performance, making it impossible to restore equity in outcomes through higher effort.

Comparing our findings with the rest of the literature, the negative treatment effect on perceived pay fairness we report is broadly consistent with the negative attitudinal consequences of pay level transparency found before (e.g., Pfeffer, Langton, 1993; Card, et al., 2012). However, the positive effects of our treatment on work performance are in contrast with most of the previous results from outside China (e.g., Gächter, Thöni, 2010; Nosenzo, 2013; Cohn, Fehr, Herrmann, et al., 2014; Breza, Kaur, Shamdasani, 2018), and the differences are substantial in magnitude as well as direction. For instance, while our treatment brings an improvement in the outcome variables of between 0.12 and 0.17 of their pre-treatment standard deviations (s.d.), Breza, Kaur, Shamdasani (2018) report that introducing pay level transparency reduced the output of lower-paid workers by 0.33 s.d. Even more dramatically, Cohn, Fehr, Herrmann, et al. (2014) find that cutting the wage of one team member and informing both workers reduced the lower-paid worker's output by 34%. This latter effect may be due to lower as well as unequal pay, so adjusting for the effect of the pay cut (14.5% lower output, Table 3, p. 890), the implied net effect of unequal pay on output is -20%, or -0.53 s.d.

Our results support some of the predictions of fairness theory but question others. Finding workers to express concerns over the fairness of resource distribution in survey Q1 (Figures 2-4) and to perceive their pay as less fair when informed of the others' higher pay (Result 1, Table 3) are consistent with fairness theory. However, a robustly positive effect of exposure to unequal pay on the lower-paid workers' output cannot be explained by fairness theory in its current version. Put differently, why would the same workers, who penalized the imaginary distributor for unfair action in the pre-experimental moonlighting game in survey Q1 choose not only not to punish us for paying them perceivedly unfair wages, but to reward us with more output? Our findings require explanation beyond what fairness theory can offer.

5.1 Failed experiment?

Before we proceed to discussing alternative explanations, we examine the possibility that our unexpected findings could have resulted from flawed research design. Although, like other studies, ours has its limitations, we do not see fundamental flaws in the experimental design that could internally invalidate our findings or account for the treatment effect of the opposite sign to what we expected. Randomly allocating workers into the treatment and control conditions permits a causal interpretation of the regression results. The randomization procedure rendered the treatment and control samples balanced in the baseline, pre-treatment characteristics (Table 2). Our sample is large enough for statistical power not be an issue. This together with a successful "manipulation check" of the informed workers perceiving their pay as less fair suggests

that our treatment did not go unnoticed and its outcomes are not a mere statistical noise.

We brought the likelihood of workers knowing about the pay difference prior to the treatment to a minimum through careful work session scheduling and choreographing the movement of different work groups around the buildings to avoid any interaction, and our results are robust to excluding the 6% of the sample who had prior knowledge of pay differences (Tables G1-G3 in APPENDIX G). Other potential confounds, including self-selection, peer effects or work room/session/time characteristics, were excluded by design or carefully controlled for in the statistical analysis. Thus, we consider our experimental design to be solid and its results internally valid. The similarities between the China working-age population survey findings and our experimental results lend them some external validity as well.

5.2 Implications for research

The main implication of our results for research on pay level transparency is: we need a framework that incorporates shared beliefs, values and norms – in short, "culture" – which influences how workers make sense (Harris, 1994; Maitlis, Christianson, 2014) of the pay information they receive and act upon it. Interpreting earning less than peers as unfair and punishable treatment is one, but not the only possible reading of coworker pay information. Such a framework is currently lacking. In this section, we discuss potential explanations to our findings rooted in or related to culture, and ways to test the implied theoretical propositions in future research.

In short, we argue that the most plausible explanation to our results is a culture that combines high power distance and long-term orientation. Chinese culture is strong on both these dimensions. It is worth noting that, although we have chosen to frame our discussion in terms of Hofstede's cultural dimensions, the logic of our proposed explanation does not hinge on this specific framework. Like any influential theory, it has been criticized (e.g., Magala, et al., 2024; Fang, 2003; McSweeney, 2002), and alternative typologies of culture have been proposed (e.g., Witte, 2012). Our arguments can be restated, with little adaptation, for any typology that identifies deference to authority ("power distance") and the importance of future benefits vs. present costs ("long-term orientation") as shared within a culture but varying between cultures. The notion of culture also does not have to be confined to national culture and can be thought of as a system of beliefs, values and norms shared by any organized group of people (e.g., firms, as in Harris, 1994).

5.2.1 Power distance and long-term orientation

Power distance is defined as "the extent to which the less powerful members of organizations and institutions accept that power is distributed unequally" (Hofstede, 2011, p. 9). Chinese culture stands out in this dimension internationally. Purely for an illustration, and without going into measurement intricacies, China scores 80 out of 100 on Hofstede's power distance index, or in the top 20% of the distribution worldwide, with the global average at 59 (The Culture Factor,

2025). High power distance culture increases acceptance of decisions by authorities, including teachers or employers (Tyler, 2000), and weakens the link between attitudes/perceptions and observed behaviors (Daniels, Greguras, 2014), including reactions to unfair treatment (Fischer, Smith, 2006). Thus, in application to our study, high power distance would result in little correlation between pay fairness perceptions and effort, and no protest or sabotage by the informed lower-paid workers against our decision to pay them less than others, which is indeed what we observed.

Furthermore, at the individual level, high power distance culture enhances the perceived benefit from building rapport with powerful individuals or institutions to gain personal favors (Fu, et al., 2018).[9] In the context of our study, compared to the uninformed, the informed workers may have had an extra reason to try to build rapport with us, having learned through the treatment of our power to set the pay rate high or low as we please. Working harder for our benefit was a straightforward way for them to do us a favor (or *renqing*), which is one way to start a relationship in China (Hwang, 1987), or they simply may have wished to get noticed as motivated workers with a strong work ethic who would be a good choice for future jobs (recall the "high motivation signal" explanation, the third most popular in the China's working-age population survey.)

"Future" is indeed the key condition for the above argument to hold. Even though we told the workers, through room managers, that their working for us was one-off and would not continue, informed workers might still have believed that similar jobs, possibly paying well above the market rate, would come up later, and that showing good performance would help secure their future employment through a privileged relationship with us (or the University). Such beliefs are congruent with long-term orientation, another cultural dimension that "stands for the fostering of virtues oriented towards future rewards, in particular, perseverance and thrift" (Hofstede, 2001, p. 359). Chinese culture is particularly strong on this dimension, scoring 87 out of 100 on Hofstede's long-term orientation index, or in the top 5% of the distribution worldwide, with the global average at 45.5 (The Culture Factor, 2025). The strong long-term orientation dimension in Chinese culture also echoes in the popularity of explanations like "reward efforts" and "chance for a better pay" that the China's working-age population survey participants gave to our findings.

Our findings are harder to explain either through power distance or long-term orientation alone. Negative performance effects of transparently unequal pay have been found in cultural contexts with high power distance, such as India (Breza, Kaur, Shamdasani, 2018), or strong in long-term orientation, such as Germany (e.g., Cohn, Fehr, Herrmann, et al., 2014), but none of these contexts is nearly as strong on both cultural dimensions as is China. It thus appears that power distance and long-term orientation are complementary to each other and a combination of

[9] One may be tempted to refer to these relations as *guanxi*, a popular term in China denoting a network of intertwined personal and business connections cultivated by its members for mutual benefit (Hwang, 1987). However, given the informality and off-work connections guanxi usually implies, it is debatable how accurately this term can describe vertical relations in a university context. We therefore opt for a more neutral *rapport*.

high power distance and strong long-term orientation, unique for China, is necessary to explain our results. Indeed, and again purely for illustration purposes, if we adopt the product of the power distance and long-term orientation indices as a measure capturing the complementarity of these two cultural dimensions, China would rank in the top 5% on this measure, with India (top 30%) and Germany (just above the median) well behind. In sum, the simplest theoretical argument consistent with our findings is this: *long-term orientation enhances the perceived benefits from establishing a good rapport with the authority, the unfairness of whose current actions is more acceptable in high power distance cultures.*

We next turn to other possibilities which we deem more demanding theoretically or less plausible empirically. To maintain the focus on the theoretical explanations applicable to pay level transparency, we note but abstract from theories that deal with procedural or communication aspects of pay transparency, for example, uncertainty management theory (Alterman, et al., 2021).

5.2.2 Beliefs in a just world

The beliefs in a just world (BJW) theory argues that people need to believe that one eventually gets what one deserves (Lerner, Miller, 1978). Correlated with power distance and long-term orientation, BJW in China are also stronger than in many Western societies (Reich, Wang, 2015; Wu, et al., 2011), suggesting that BJW may be a relevant context-specific factor for the outcomes of our experiment. A large body of research, surveyed in Furnham (2003), demonstrated that BJW serve as a coping mechanism and a source of positive motivation when experiencing ostensibly unfair events. Our treatment may indeed have been felt as unfair, thus possibly leading the informed workers to have recourse to BJW. Our observations of no protest or sabotage among the informed workers and predominantly benign explanations for pay differences (Table 6) are consistent with BJW as well as with high power distance. The "better worker"-type explanations, most frequently given in both post-experiment worker (Q2) and China's working-age population surveys, are also consistent with BJW.

BJW are correlated with optimism and tendency to expect compensatory outcomes in the future for current actions (Gaucher, et al., 2010; Dalbert, Donat, 2015). This correlation is consistent with the proposition that, if the informed workers believed that similar jobs, possibly paying more, might come later, their higher effort would be an investment to secure future benefits. In this respect, BJW would work very similarly to long-term orientation. However, even if the workers did hold strong BJW, it is unclear whether BJW reflect a genuine conviction that drives corresponding action or serve "merely" as a coping mechanism without going much beyond statements. (For instance, victim blaming, a special case of BJW, is not linked to actual criminal behavior.) This ambiguity, in our view, limits the possibility to explain our results through BJW as compared to power distance combined with long-term orientation.

5.2.3 Saving face

We observed that being paid less than others is interpreted not only as unfair treatment but also as a signal that the others may be better workers. Believing that higher-paid workers are better could not only eliminate negative reactions to unequal pay by the lower-paid workers (Breza, Kaur, Shamdasani, 2018), but even increase their effort in an attempt to restore "face" (*mianzi*) – the respect and deference an individual can claim based on their social position and conduct, influenced by both their actions and the expectations of others (Ho, 1976). Maintaining face is very important in Chinese society (Leung, Yee-kwong Chan, 2003) and can be achieved through hard work (Brunner, et al., 1990). Learning to be less capable compared to peers than previously thought can lead to a loss of face, but working harder helps restore and maintain face, especially when one can demonstrate strong work ethic through higher effort.

There are, however, three concerns with this explanation. First, it is doubtful whether one's face could be lost or maintained depending on one's performance in a menial, three-hour long, one-off job whose results were never to be made public. (Leaving a positive impression on the boss seems more probable.) Second, it is not clear to what extent trying to match supposedly better workers in performance was for the sake of saving face rather than improving one's standing with the authorities in pursuit of better-paid jobs in future, although the two objectives may be related. Finally, we do not see significant difference in the treatment effects for the informed workers who mentioned "better worker" as the reason for pay difference versus the workers who provided other explanations. Thus, while we cannot definitely rule out saving face as a possible explanation to our results, it seems less credible in our specific setting, but might be relevant for future research studying richer and longer-lasting employment relationships than the one we created for our experiment.

5.3 Future research

Though conceived as a project to fill a narrow research gap (reactions to unequal pay in China), our study ends up asking a broader question: how does culture affect perceptions of unequal pay and reactions to it? Fairness theory, currently the predominant theoretical framework in the field, is silent on the role of culture. Yet, the discrepancy between the results from China and other countries suggests that culture should be taken into account in theorizing the impact of pay level transparency on worker performance. Our study calls for conceptualizing power distance and long-term orientation as mutually reinforcing moderators of the theoretical relationship between pay level transparency and performance, direct or mediated through fairness concerns, if one wishes to build on fairness theory.

Empirically, one can measure naturally occurring attitudes towards power distance and future with suitable scales. As an alternative, and complementary, approach, the theoretical boundary conditions imposed by these constructs could be tested through artificial manipulations in research design. For instance, if we had a chance to run a similar experiment but on a greater

scale, we would introduce treatments where workers would receive messages varying in the implied likelihood of jobs from us coming up in future, as well as the implied degree of association between us and the Authority (University). As a word of caution, our experiment was quite difficult to implement as is, but perhaps online experimental platforms could offer easier and more scalable solutions.

It is important to study performance effects of pay level transparency experimentally because of multiple confounders likely to occur in observational data, one example being workers' self-selection into jobs and firms offering pay transparency environments best fitting their preferences (e.g., Card, et al., 2012, Belogolovsky, Bamberger, 2014). Indeed, most of the literature on pay level transparency is experimental (Table 1). This said, there are relatively few field experiments, possibly because of high monetary costs and institutional and ethical restrictions involved. While lab experiments are inexpensive and offer the ultimate control, not quite achievable in the field, the advantage of field experiments is a greater degree of realism, which is useful for constructs like power distance to have a practical meaning for the subjects.

5.4 Practical implications

An immediate implication from our results for management practitioners is that pay level transparency is not necessarily detrimental to performance. In fact, it may improve or reduce performance, depending on how workers make sense of the earnings of their peers. The message of earning less than peers can be interpreted in two distinct ways. The first and seemingly more intuitive interpretation is one rooted in feelings of unfairness: "I am being treated unfairly compared to others, and I will respond by punishing my boss for this unfair treatment." This reaction seems prevalent in egalitarian Western cultures, where notions of fairness are deeply ingrained and often align with fairness theory, which is also an intellectual product of the West.

Conversely, the second interpretation offers a more constructive perspective: "Although I am being treated unfairly, I could potentially earn more in the future if I focus on building the right relationship with my boss, rather than punishing them." This more optimistic view has its own logic rooted in cultures strong on power distance and long-term orientation, such as Chinese culture. In such environments, workers may see pay disparities as an opportunity rather than a source of grievance.

We do not go as far as to recommend that every firm in China or elsewhere should reveal pay information to coworkers. Much depends on workers' perceptions of pay setting rules (is it really better workers who receive higher pay? How do I become a "better worker" in the eyes of the pay-setting authority?) and the expected length of the employment relationship (will it last long enough for me to take advantage of better-paid opportunities?). There is also plenty of variation in individual attitudes within what can be labeled as "national culture". However, a refusal on the grounds of perceived unfairness should not be a knee-jerk reaction to proposals for greater pay level transparency; rather, they should be considered taking worker beliefs,

organizational culture, employment relationship, and pay-setting practices into account.

5.5 Limitations

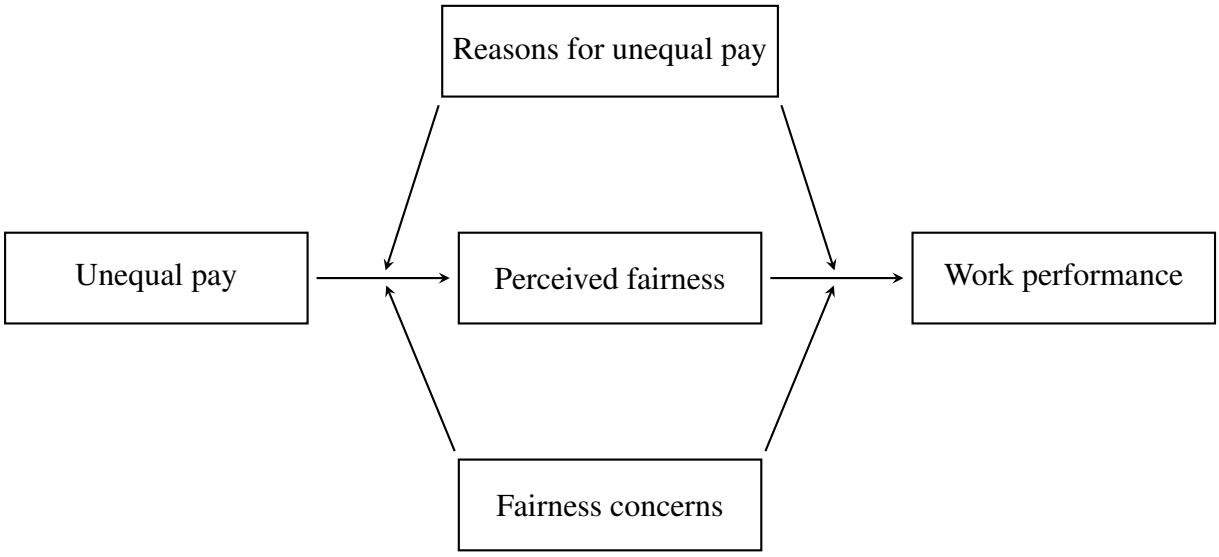
Our work has produced important discoveries that contribute to better understanding of performance effects of pay level transparency, but it has several limitations to note. First, although we did measure constructs pertaining to the theory that guided the design of our study (fairness perceptions and concerns), we did not collect information on the culture constructs we ended up relying on in explaining our unexpected results. We therefore cannot thoroughly test our proposition that culture matters for performance effects of pay level transparency.

Second, usual for experimental studies, there are concerns about external validity of our results. Is there something special about the context in which our experiment took place? Would any of the results be different if we paid below- vs. above-market rates instead of usual (20 RMB/hour) vs. above-market? Could our results be reproduced in real organizations with workers working full-time for years and pay differences likely greater and more important for their livelihood than what we managed to create experimentally? The China working-age population survey results are consistent with our experimental findings, lending them a degree of external validity. To our knowledge, no published field experimental study would pay clearly below-market rates. The location of our experiment and our worker sample are representative of many Chinese higher educational institutions. However, as with other studies, replication (in a comparable context) is the ultimate test for external validity.

A related concern is that, similar to other experimental studies of pay level transparency (e.g., Nosenzo, 2013, Liu-Kiel, et al., 2013, Cohn, Fehr, Herrmann, et al., 2014), most of our student workers were too young to have substantial labor market experience, including the experience of unequal pay. True, young workers may not be representative of working-age population, but it is not clear what difference age should make to the reactions to unequal pay. Mature workers are more aware of their employment rights and thus may be more likely to question unequal pay for the same work. On the other hand, young workers, who often do part-time, temporary jobs without long-term career opportunities, may have had more freedom to express a negative reaction to unequal pay than mature workers bound by more comprehensive contracts. Exploring the possible age gradient in reactions to pay transparency requires another study.

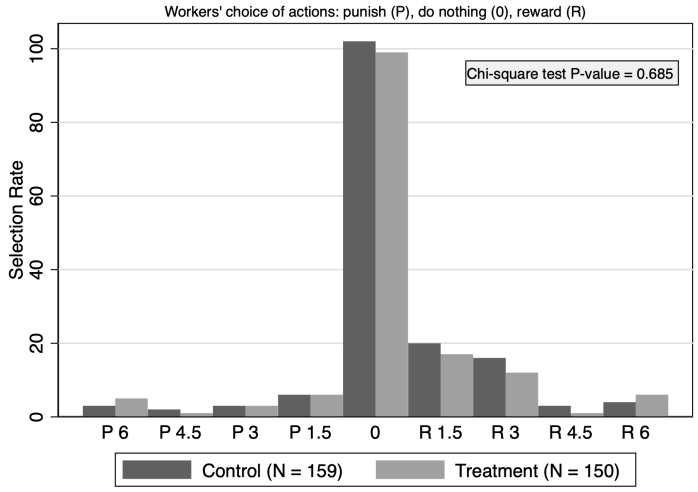
6 Figures

Figure 1: **Conceptual model**



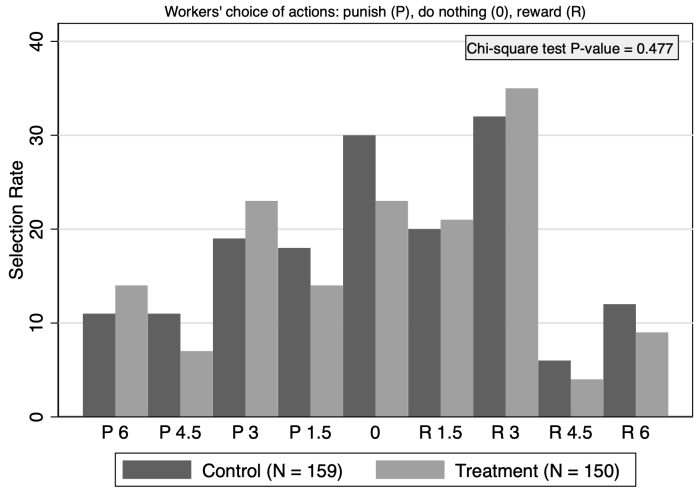
Notes: This diagram depicts the conceptual model guiding our study, based on "fairness theory" – a synthesis of inequity theory (Adams, 1963; Adams, 1965) and theories of fairness developed by Fehr, Schmidt (1999) and Folger, Cropanzano (2001). According to fairness theory, performance reactions to unequal pay are mediated by perceived pay fairness and moderated by fairness concerns and perceived reasons for unequal pay.

Figure 2: **Distribution of workers' choices in the equal allocation scenario of the moonlighting game**



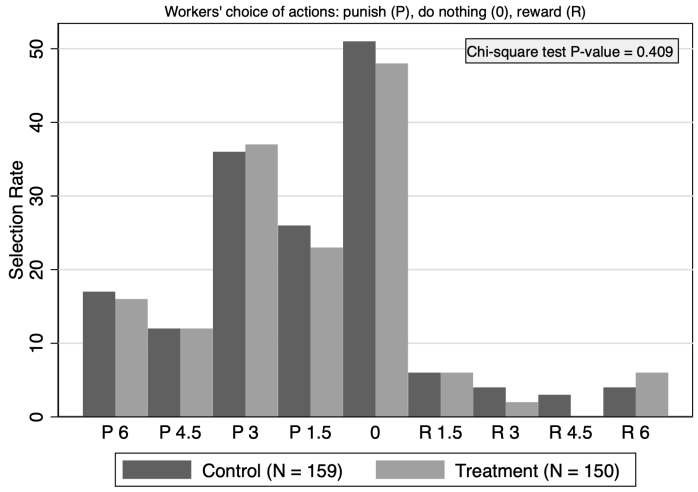
Notes: This graph demonstrates the distribution of workers' choices in the equal allocation scenario in the pre-treatment survey Q1: punish (P[amount]), do nothing (0), reward (R[amount]). In total 309 workers participated in Q1. The $p-$value of the chi-squared test for equality of the treatment and control distributions is 0.685, indicating no statistically significant difference between the two groups.

Figure 3: **Distribution of workers' choices in the self-favoring allocation scenario of the moonlighting game**



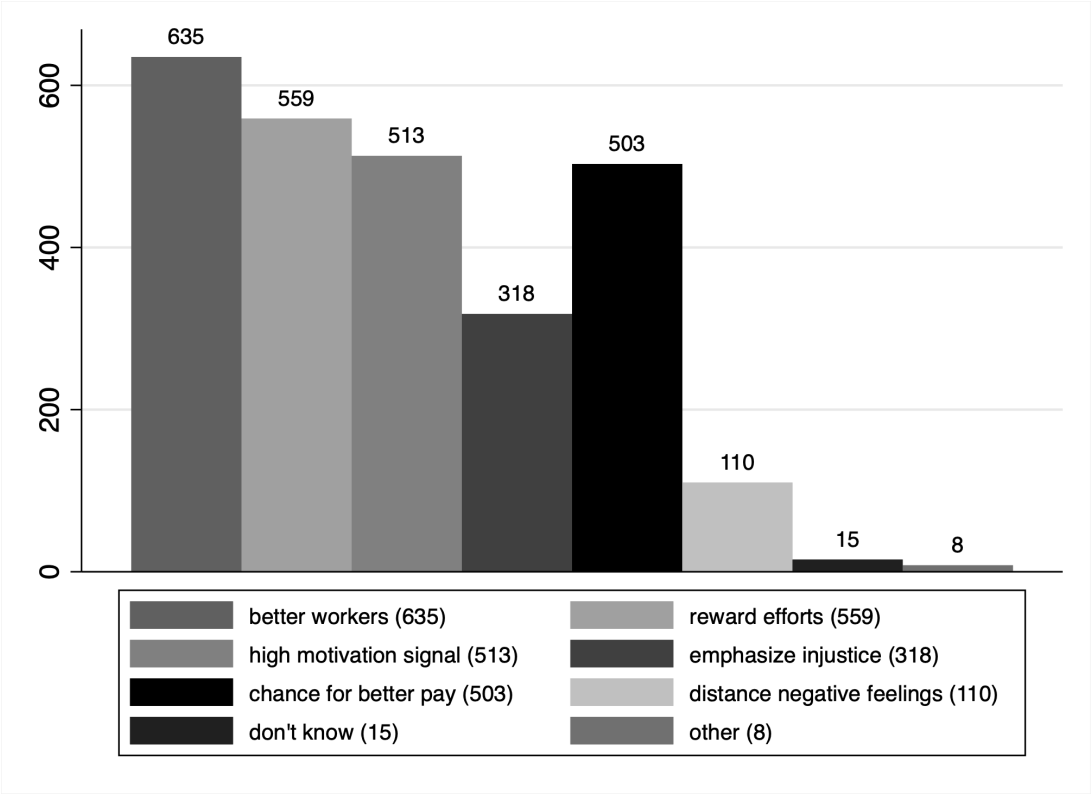
Notes: This graph demonstrates the distribution of workers' choices in the self-favoring allocation scenario in the pre-treatment survey Q1: punish (P[amount]), do nothing (0), reward (R[amount]). In total, 309 workers participated in Q1. The $p-$value of the chi-squared test for equality of treatment and control distributions is 0.477, indicating no statistically significant difference between the two groups.

Figure 4: **Distribution of workers' choices in the other-favoring allocation scenario of the moonlighting game**



Notes: This graph demonstrates the distribution of workers' choices in the other-favoring allocation scenario in the pre-treatment survey Q1: punish (P[amount]), do nothing (0), reward (R[amount]). 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 two groups.

Figure 5: **Distribution of the explanations provided by participants of China's working-age population survey**



Notes: This graph displays the distribution of explanations chosen by participants of China's working-age population survey in response to the question: "Why did informed workers tend to work harder after learning that they were paid less than some peers for the same job?" The following explanations were offered to the survey participants who could choose up to three most applicable in their opinion:
- "better workers": Informed workers believed that those earning more were better workers and worked harder to match their performance.
- "reward efforts": Although the job was one-off and the pay was fixed in advance, informed workers hoped that their efforts might still be rewarded in some way.
- "high motivation signal": Working hard despite relatively low pay sends a strong signal of integrity and high motivation, particularly to managers observing their performance.
- "emphasize injustice": Workers chose to work harder to highlight how unjust it was that they were paid less than others.
- "chance for better pay": They believed that working harder was their only chance for better pay, however small, and took the opportunity to avoid future regret.
- "distance from negative feelings": They worked harder to distance themselves from the negative emotions caused by learning that they were paid less than others.

7 Tables

Table 1. Summary of empirical studies on pay level transparency

Study	*Experiment*	*Treatment/Key regressor*	*Focal outcome*	*Results for the*	
				Lower-paid workers	*Higher-paid workers*
Pfeffer, Langton (1993)	No	Salary variation within a given academic department	Research productivity (self-reported)	Negative	-
			Collaboration (self-reported)	Negative	-
			Job satisfaction	Negative	Negative
Clark, Oswald (1996)	No	Reference income level	Job satisfaction	Negative	
Luttmer (2005)	No	Neighbors' earnings	Happiness (self-reported)	Negative	Negative
Charness, Kuhn (2007)	Yes	Employer choosing wages of both workers in a two-worker team	Work effort	No effect	No effect
Gächter, Thöni (2010)	Yes	Employer choosing wages in a two-worker team (Studies 1 and 2)	Work effort	Negative	No effect
		Wages determined randomly (Study 3)	Work effort	No effect	No effect
Bartling, von Siemens (2011)	Yes	Paying unequal piece rates within teams	Willingness to participate in team production	No effect	No effect
			Effort	No effect	No effect
Greiner, Ockenfels, Werner (2011)	Yes	Pay transparency in hourly rates	Output quantity	No effect	Positive
			Output quality	No effect	Positive
		Pay transparency in piece rates	Output quantity	Positive	Positive
			Output quality	Negative	Negative
Card, et al. (2012)	Yes	Providing access to information on coworkers' pay	Job satisfaction	Negative	No effect
			Job search intention	Positive	No effect
Liu-Kiel, et al. (2013)	Yes	Pay transparency in piece rates, Germany	Work output	Positive	-
		Pay transparency in piece rates, China	Work output	Positive	-
Nosenzo (2013)	Yes	Employer choosing wages of both workers in a two-worker team	Work effort	Negative	No effect
		Employer choosing wages of one worker in a two-worker team, the other determined randomly	Work effort	Negative	Negative
Belogolovsky, Bamberger (2014)	Yes	Pay secrecy in performance-based wage	Individual task performance	Negative	
		Pay secrecy in performance-based wage	Continuation intention	Negative	

Continued on next page

Study	*Experiment*	*Treatment/Key regressor*	*Focal outcome*	*Results for the*	
				Lower-paid workers	*Higher-paid workers*
Cohn, Fehr, Herrmann, et al. (2014)	Yes	Reducing the wage of one of the two workers in a team	Work output	Negative	No effect
			Pay satisfaction	Negative	No effect
		Reducing the wage of both workers in a team	Work output	Negative	
			Pay satisfaction	Negative	
Bamberger, Belogolovsky (2017)	Yes	Pay transparency in performance-based wage	Amount of help workers afford to highest-paid peers	No effect on average[10]	-
Breza, Kaur, Shamdasani (2018)	Yes	Paying different wages, depending on productivity, informing of different wages BUT not providing the reason	Turning up for work	Negative	Negative
			Work output	Negative	No effect
		Paying different wages, depending on productivity, informing of pay differences AND providing the reason	Turning up for work	No effect	No effect
			Work output	No effect	No effect
Dube, Giuliano, Leonard (2019)	No	Peer wages	Employee quits	Positive	Negative
Perez-Truglia (2020)	No	Individual pay information made public through income tax records available online	Happiness	Negative	Positive
			Life satisfaction	Negative	Positive
Cullen, Perez-Truglia (2022)	Yes	Informing about the average salary over a random sample of five managers (vertical comparisons)	Average hours worked	Positive	-
			Work performance	Positive	-
		Informing about the average salary over a random sample of five peers (horizontal comparisons)	Average hours worked	Negative	No effect
			Work performance	Negative	No effect
Schnaufer, et al. (2022)	Yes[11]	Undermet pay expectations revealed through pay transparency	Job satisfaction	Negative	-
Gutierrez, Obloj, Zenger (2025)	No	Providing access to information on academic salaries in public universities in U.S. states	Research productivity index	Negative	Positive

[10] More strongly negative effect for less individualistic and less prosocially motivated workers.
[11] Survey experiment (vignette)

Table 2. Descriptive statistics

	(1) Registered	(2) Showed up and included in the study sample				(3) No Shows	
	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)	0.410	19.305 (0.944)	19.542 (1.304)	0.189
Female	0.545 (0.498)	0.573 (0.496)	0.602 (0.491)	0.603	0.500 (0.514)	0.477 (0.501)	0.028**
Study year	1.573 (0.722)	1.493 (0.653)	1.609 (0.717)	0.140	1.278 (0.461)	1.663 (0.804)	0.048**
Major Art	0.175 (0.380)	0.160 (0.368)	0.168 (0.375)	0.855	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)	0.400	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)	0.097*	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)	0.653	0.278 (0.461)	0.203 (0.404)	0.394
Others	0.015 (0.124)	0.000 (0.000)	0.025 (0.156)	0.052*	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)	-	-
Length	-	6.702 (1.856)	6.982 (2.059)	0.209	6.572 (1.788)	-	-
Empty fields	-	1.614 (1.522)	1.907 (2.432)	0.207	1.016 (0.955)	-	-
N	514	150	161		18	172	

Notes: This table reports descriptive statistics for all the workers who registered to work for us (column 1), who showed up for work (column 2), and no shows (column 3). Column 2 is further divided into the treatment conditions and the highly-paid worker group. P-values of the $t-$ tests comparing group means are also reported.

Table 3. Average treatment effects on worker perceptions

	(1) Fair pay	(2) Hard work	(3) Good job	(4) Good conditions	(5) 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 work session size, date and time, and worker's study year, major, and gender.

Notes: This table shows the average treatment effects on worker perceptions measured in the post-experiment survey Q2 and estimated with regression (1): fairness of pay, difficulty and usefulness of tasks, working conditions, and previous data entry experience. $N = 260$ workers participated in Q2.

Table 4. Average treatment effects on performance outcomes

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

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

Standard errors in parentheses are clustered by work session.

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

Controls include work session size, date and time, and worker's study year, major, gender, and baseline performance.

Notes: This table reports the average treatment effects on the the pre-registered performance outcomes: the average length of time to process a questionnaire, and the average number of empty fields in a questionnaire processed. Columns (1) and (4) show the treatment effects on the entire sample, columns (2) and (5) show the treatment effects on the subsample of Q2 participants, all estimated with regression (1). Columns (3) and (6) show treatment effects mediated and moderated by pay fairness perceptions (insignificant in all cases), as per regression (3). There were a total $N = 311$ workers in the control and treatment conditions, and $N = 260$ workers participated in the post-experiment survey Q2 which measured pay fairness perceptions.

Table 5. Treatment effects moderated by fairness concerns

	(1) Fair pay	(2) Length	(3) Empty fields
Treatment	-0.271*	-0.173**	-0.173*
	(0.137)	(0.077)	(0.092)
Fairness	-0.064	-0.028	-0.077
	(0.064)	(0.041)	(0.059)
Treatment X Fairness	0.089	-0.020	0.072
	(0.081)	(0.050)	(0.071)
N	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 work session size, date and time, and worker's study year, major, gender, and baseline performance, wherever available.

Notes: This table reports the average treatment effects and moderation effects by fairness concerns measured in the pre-experiment survey Q1 and estimated with regression (3). There are $N = 309$ workers in the control and treatment groups for whom fairness concerns were elicited in Q1, and $N = 259$ workers with elicited fairness concerns who participated in Q2. Two workers did not participate in Q1.

Table 6. Themes emerging from the workers' stated explanations for the pay difference

Group: Theme: *"Examples"*	Treatment group	Control group
Better worker:	67	70
Efficiency / capacity / ability	51	50
"work capacity", "ability", "faster"		
Work quality	10	16
"quality", "accuracy"		
Work attitude	15	27
"diligent", "responsible", "devotion"		
Experience / education	7	1
"work experience", "seniority", "education attainment"		
Different work:	53	44
Workload / hours	34	36
"work hours", "time spent", "workload"		
Job content / difficulty	23	12
"job specification", "difficulty", "work nature"		
Other:	28	28
Survey / experiment design	7	13
"questionnaire", "experiment", "research needs"		
Employer factors / fairness	13	5
"personal relationship", "discrimination", "ideology"		
Randomness / luck	7	8
"random", "surprise", "weather"		
Don't know	7	7
Other (not matching any of the above themes)	4	5
N workers taking part in survey Q2	129	131

Notes: This table lists the 11 themes identified through the application of our rule-based multi-label text classification algorithm to the free-text answers the workers gave to the question "In your opinion, what were the reasons for different pay rates given to different people for the same work?" in survey Q2. For each theme, we provide a few examples of text snippets corresponding to it. For later analysis, we grouped the themes into three groups: "better worker", "different work", and "other". For each theme or group of themes, we compute the number of workers who mentioned it in their answers.

Table 7. Treatment effects by explanation for the pay difference

Reason	(1) Length	(2) Empty fields	(3) Fair pay
"Better worker"	-0.181	-0.202	-0.208
(41.2% of workers)	(0.123)	(0.169)	(0.167)
"Different work"	0.026	0.086	-0.058
(25.8%)	(0.124)	(0.157)	(0.250)
"Different work" AND "Better worker"	-0.176	-0.208	-0.815**
(11.5%)	(0.198)	(0.254)	(0.343)
Other	-0.306	-0.127	-0.344
(21.5%)	(0.200)	(0.195)	(0.278)
N	260	260	260
$P-$value test effects equal	0.437	0.394	0.169
R-squared	0.555	0.577	0.140

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

Standard errors in parentheses are clustered by work session.

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

Controls include work session size, date and time, and worker's study year, major, gender, and baseline performance, wherever available.

Notes: This table shows average treatment effects estimated with regression (3) for the subgroups of workers citing a specific explanation for the pay difference: "better worker" only, "different work" only, "better worker"&"different work", and "other". The explanation-specific treatment effects are not statistically significantly different from each other. There were $N = 260$ workers who participated in the post-experiment survey Q2 and shared their explanations for the pay difference.

Table 8. Treatment effect heterogeneity

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

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

Standard errors in parentheses are clustered by work session.

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

Controls include work session size, date and time, and worker's study year, major, gender, and baseline performance, wherever available.

Notes: This table shows the coefficients from regression (3) on the interactions of the treatment dummy and contextual characteristics: gender, study cohort, major, working session date, and baseline performance, where available. Most of these interactions are insignificant, suggesting little treatment effect heterogeneity in terms of observable characteristics.

Table 9. Descriptive statistics and average 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	0.492	0.843
	(0.500)	(0.500)	
After	0.560	0.611	0.295
	(0.497)	(0.488)	
Length	5.494	5.804	0.157
	(2.146)	(2.600)	
Empty fields	1.150	1.659	0.007**
	(1.941)	(3.292)	
Female	0.506	0.570	0.532
	(0.501)	(0.495)	
Fairness concerns (1 to 5)	2.118	2.205	0.642
	(1.367)	(1.510)	
Study year	1.590	1.578	0.883
	(0.540)	(0.715)	
Major Art	0.086	0.161	0.132
	(0.281)	(0.367)	
Business and Economics	0.177	0.198	0.656
	(0.382)	(0.399)	
Engineering and Tech	0.541	0.390	0.115
	(0.499)	(0.488)	
Social Science and Law	0.179	0.239	0.393
	(0.384)	(0.426)	
Others	0.017	0.012	0.779
	(0.130)	(0.111)	
Treatment effects:	Coefficient (std. error)	Coefficient (std. error)	P-value (1) - (2)
Fair pay	-0.120	-0.297	0.396
	(0.225)	(0.145)	
Length	-0.180	-0.244	0.675
	(0.165)	(0.133)	
Empty fields	-0.215	-0.124	0.555
	(0.162)	(0.083)	

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

Standard errors in parentheses are clustered by work session.

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

Notes: Average treatment effects on the selected and non-selected samples were estimated in the same way as the treatment effects on the entire sample, from equation 1.

Table 10. Average treatment effects on the number of mistakes per questionnaire

	Questionnaire-level analysis		Worker-level analysis	
	(1)	(2)	(3)	(4)
Treatment	0.328	0.470	0.839	0.232
	(0.439)	(0.711)	(0.756)	(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, work session size, date and time, and workers' study year, major, gender, and baseline performance.

Notes: This table reports the average treatment effects on the number of mistakes per questionnaire in the quality check sample. In columns 1 and 2, the unit of observation is individual questionnaire. There were 228 questionnaires in the quality check sample that were digitized 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 digitized before and after treatment were available for 83 workers, hence $N = 83$ in columns 3 and 4.

Table 11. Descriptive statistics of China's working-age population survey respondents, and their correlations with the explanations they provided

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

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

Notes: This table lists 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 12. Correlations between choice of effort and China's working-age population survey 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		

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

Notes: This table lists the coefficients and standard errors from the multinomial logit regression of the survey participants' hypothetical effort choices on their characteristics, "no change in effort" is the baseline category.

Table 13. Differences in key performance outcomes between uninformed higher-paid workers (35 RMB/hour) and uninformed lower-paid workers (20 RMB/hour, the control group)

	(1) Fair pay	(2) Length	(3) Empty Fields
High-paid workers	0.485**	–0.241	-0.205**
	(0.188)	(0.173)	(0.087)
N	147	179	179

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

Standard errors in parentheses are clustered by work session.

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

Controls include baseline performance (columns 2 and 3) and workers' study year, major, gender and session size (all specifications).

Notes: This table reports the differences in the average performance outcomes between the higher-paid uninformed workers ($N = 18$) and the lower-paid uninformed workers (our control group), estimated from equation 1. For columns 2 and 3, the total sample size is 179, comprising 161 workers in the control group and 18 higher-paid workers. For column 1, the sample size is 147, with 131 control group workers and 16 higher-paid workers participating in Q2.

References

Abbink K, Irlenbusch B, Renner E (2000) The moonlighting game: An experimental study on reciprocity and retribution. *J. Econ. Behav. Organ.* 42(2):265–277.

Adams JS (1963) Toward an Universtanding of Inequity. *J. Abnorm. Psychol.* 67:422–436.

Adams JS (1965) Inequity In Social Exchange. Berkowitz L (Academic Press), 267–299.

Akerlof GA, Yellen JL (1990) The fair wage-effort hypothesis and unemployment. *Q. J. Econ.* 105(2):255–283.

Alterman V, Bamberger PA, Wang M, Koopmann J, Belogolovsky E, Shi J (2021) Best not to know: Pay secrecy, employee voluntary turnover, and the conditioning effect of distributive justice. *Acad. Manag. J.* 64(2):482–508.

Aon (2024) *2023-2024 Asia Pay Equity Survey Report*. [Online; accessed 20-February-2025].

Bamberger P (2021) Pay transparency: Conceptualization and implications for employees, employers, and society as a whole. *Oxford Research Encyclopedia of Business and Management* (Oxford University Press).

Bamberger P, Belogolovsky E (2017) The dark side of transparency: How and when pay administration practices affect employee helping. *J. Appl. Psychol.* 102(4):658–671.

Bartling B, von Siemens FA (2011) Wage inequality and team production: An experimental analysis. *J. Econ. Psychol.* 32(1):1–16.

Belogolovsky E, Bamberger P (2014) Signaling in secret: Pay for performance and the incentive and sorting effects of pay secrecy. *Acad. Manag. J.* 57(6):1706–1733.

Blondeel E, Everaert P, Opdecam E (2025) A practical guide to implementing ChatGPT as a secondary coder in qualitative research. *Int. J. Account. Inf. Syst.* 56:100754.

Breza E, Kaur S, Shamdasani Y (2018) The morale effects of pay inequality. *Q. J. Econ.* 133(2):611–663.

Brunner JA, Chen J, Sun C, Zhou N (1990) The role of guanxi in negotiations in the Pacific Basin. *J. Glob. Mark.* 3(2):7–24.

Card D, Mas A, Moretti E, Saez E (2012) Inequality at work: The effect of peer salaries on job satisfaction. *Am. Econ. Rev.* 102(6):2981–3003.

Charness G, Kuhn P (2007) Does pay inequality affect worker effort? Experimental evidence. *J. Labor Econ.* 25(4):693–723.

Chen X, Fan X, Tang CS, Chen Y.-J (2022) A theoretical and experimental evaluation of pay transparency policies and payment schemes. *HKUST Bus. Sch. Res. Pap.* (2022-073).

Clark AE, Oswald AJ (1996) Satisfaction and comparison income. *J. Public Econ.* 61(3):359–381.

Cohn A, Fehr E, Goette L (2015) Fair wages and effort provision: Combining evidence from a choice experiment and a field experiment. *Manag. Sci.* 61(8):1777–1794.

Cohn A, Fehr E, Herrmann B, Schneider F (2014) Social comparison and effort provision: Evidence from a field experiment. *J. Eur. Econ. Assoc.* 12(4):877–898.

Cullen Z (2024) Is pay transparency good? *J. Econ. Perspect.* 38(1):153–80.

Cullen Z, Perez-Truglia R (2022) How much does your boss make? The effects of salary comparisons. *J. Polit. Econ.* 130(3):766–822.

Dalbert C, Donat M (2015) Belief in a Just World. Wright JD *International Encyclopedia of the Social Behavioral Sciences (Second Edition)* ().

Daniels MA, Greguras GJ (2014) Exploring the nature of power distance: Implications for micro- and macro-level theories, processes, and outcomes. *J. Manage.* 40(5):1202–1229.

Dodlova M, Yudkevich M (2009) Gift exchange in the workplace. *Hum. Resour. Manag. Rev.* 19(1):23–38.

Dube A, Giuliano L, Leonard J (2019) Fairness and Frictions: The Impact of Unequal Raises on Quit Behavior. *Am. Econ. Rev.* 109(2):620–63.

Esteves-Sorenson C (2018) Gift exchange in the workplace: Addressing the conflicting evidence with a careful test. *Manag. Sci.* 64(9):4365–4388.

Fang T (2003) A critique of Hofstede's fifth national culture dimension. *Int. J. Cross Cult. Manag.* 3(3):347–368.

Fehr E, Fischbacher U (2004) Third-party punishment and social norms. *Evol. Hum. Behav.* 25(2):63–87.

Fehr E, Goette L, Zehnder C (2009) A behavioral account of the labor market: The role of fairness concerns. *Annu. Rev. Econ.* 1(1):355–384.

Fehr E, Schmidt KM (1999) A theory of fairness, competition, and cooperation. *Q. J. Econ.* 114(3):817–868.

Fischer R, Smith PB (2006) Who cares about justice? The moderating effect of values on the link between organisational justice and work behaviour. *Appl. Psychol.* 55(4):541–562.

Folger R, Cropanzano R (2001) Fairness theory: Justice as accountability. ()

Fu X, Lv Y, Yang Z, Yu X, Wang R (2018) Chinese adolescents' power distance value and prosocial behavior toward powerful people: A longitudinal study. *PLoS One* 13(12):1–10.

Furnham A (2003) Belief in a just world: Research progress over the past decade. *Pers. Individ. Differ.* 34(5):795–817.

Gächter S, Thöni C (2010) Social comparison and performance: Experimental evidence on the fair wage–effort hypothesis. *J. Econ. Behav. Organ.* 76(3):531–543.

Gaucher D, Hafer CL, Kay AC, Davidenko N (2010) Compensatory rationalizations and the resolution of everyday undeserved outcomes. *Pers. Soc. Psychol. Bull.* 36(1):109–118.

Geurts SA, Buunk BP, Schaufeli WB (1994) Social comparisons and absenteeism: A structural modeling approach. *J. Appl. Soc. Psychol.* 24(21):1871–1890.

Greiner B, Ockenfels A, Werner P (2011) Wage transparency and performance: A real-effort experiment. *Econ. Lett.* 111(3):236–238.

Gutierrez C, Obloj T, Zenger T (2025) Pay transparency and productivity. *Strateg. Manag. J.*

Harris SG (1994) Organizational culture and individual sensemaking: A schema-based perspective. *Organ. Sci.* 5(3):309–321.

Ho DY (1976) On the concept of face. *Am. J. Sociol.* 81(4):867–884.

Ho T.-H, Su X (2009) Peer-induced fairness in games. *Am. Econ. Rev.* 99(5):2022–2049.

Hofstede G (2001) Culture's consequences: Comparing values, behaviors, institutions, and organizations across nations. *Int. Educ. Prof.*

Hofstede G (2011) Dimensionalizing cultures: The Hofstede model in context. *Online Read. Psychol. Cult.* 2(1).

Hwang K.-k (1987) Face and favor: The Chinese power game. *Am. J. Sociol.* 92(4):944–974.

Lerner MJ, Miller DT (1978) Just world research and the attribution process: Looking back and ahead. *Psychol. Bull.* 85(5):1030–1051.

Leung TK, Yee-kwong Chan R (2003) Face, favour and positioning–a Chinese power game. *Eur. J. Mark.* 37(11/12):1575–1598.

Li CH, Certo ST (2021) The application of the Difference-in-Differences model in strategic management research. *Acad. Manag. Proc.* (1).

Li J, Jiang H, Shen J, Ding H, Yu R (2024) Using the difference-in-differences design with panel data in international business research: progress, potential issues, and practical suggestions. *J. Int. Bus. Stud.* 55(8):949–961.

Liu-Kiel H, Cadsby CB, Schenk-Mathes HY, Song F, Yang X (2013) A cross-cultural real-effort experiment on wage-inequality information and performance. *BE J. Econ. Anal. Policy* 13(2):1095–1120.

Luttmer EF (2005) Neighbors as negatives: Relative earnings and well-being. *Q. J. Econ.* 120(3):963–1002.

Magala S, Erten C, Bell RM, Claes M.-T, Yazici S, Karabag A (2024) *Hofstede Matters*. (Routledge).

Maitlis S, Christianson M (2014) Sensemaking in organizations: Taking stock and moving forward. *Acad. Manag. Ann.* 8(1):57–125.

McKenzie D (2012) Beyond baseline and follow-up: The case for more T in experiments. *J. Dev. Econ.* 99(2):210–221.

McSweeney B (2002) Hofstede's model of national cultural differences and their consequences: A triumph of faith-a failure of analysis. *Hum. Relat.* 55(1):89–118.

Nosenzo D (2013) Pay secrecy and effort provision. *Econ. Inq.* 51(3):1779–1794.

Obloj T, Zenger T (2022) The influence of pay transparency on (gender) inequity, inequality and the performance basis of pay. *Nat. Hum. Behav.* 6(5):646–655.

Oosterbeek H, Sloof R, Van De Kuilen G (2004) Cultural differences in ultimatum game experiments: Evidence from a meta-analysis. *Exp. Econ.* 7:171–188.

Perez-Truglia R (2020) The effects of income transparency on well-being: Evidence from a natural experiment. *Am. Econ. Rev.* 110(4):1019–1054.

Pfeffer J, Langton N (1993) The effect of wage dispersion on satisfaction, productivity, and working collaboratively: Evidence from college and university faculty. *Adm. Sci. Q.* 38(3):382–407.

Rand DG, Tarnita CE, Ohtsuki H, Nowak MA (2013) Evolution of fairness in the one-shot anonymous ultimatum game. *Proc. Natl. Acad. Sci. USA* 110(7):2581–2586.

Reich B, Wang X (2015) And justice for all: Revisiting the global belief in a just world scale. *Pers. Individ. Differ.* 78:68–76.

Schnaufer K, Christandl F, Berger S, Meynhardt T, Gollwitzer M (2022) The shift to pay transparency: Undermet pay standing expectations and consequences. *J. Organ. Behav.* 43(1):69–90.

Schreurs B, Hamstra MRW, Jawahar IM, Akkermans J (2020) Perceived overqualification and counterproductive work behavior: Testing the mediating role of relative deprivation and the moderating role of ambition. *Pers. Rev.* 50(3):1038–1055.

The Culture Factor (2025) *Country Comparison Tool*. [Online; accessed 23-March-2025].

Tse FY (2023) Learning about pay at work: A labour process approach to pay transparency. *Work Employ. Soc.* 37(5):1147–1164.

Tyler TR (2000) Multiculturalism and the willingness of citizens to defer to law and to legal authorities. *Law Soc. Inq.* 25(4):983–1019.

Ubeda P (2014) The consistency of fairness rules: An experimental study. *J. Econ. Psychol.* 41:88–100.

Witte AE (2012) Making the case for a post-national cultural analysis of organizations. *J. Manag. Inq.* 21(2):141–159.

Wu MS, Yan X, Zhou C, Chen Y, Li J, Zhu Z, Shen X, Han B (2011) General belief in a just world and resilience: Evidence from a collectivistic culture. *Eur. J. Pers.* 25(6):431–442.

Online Appendices A - I

Appendices A - I are available online: https://osf.io/ug9ma/overview?view_only=2ae7ed9ee4ec4d0c9c420472e4c953e5