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Galchenko, Valeria; Zubanov, Nick; Wong, Ho Lun; Zhou, Xiang

### Working Paper Unequal pay, fairness perceptions, and work effort: Experimental evidence from China

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# Unequal pay, fairness perceptions, and work effort: Experimental evidence from China

Valeria Galchenko, University of Konstanz, valeria.galchenko@uni-konstanz.de Nick Zubanov, University of Konstanz, nick.zubanov@uni-konstanz.de Ho Lun Wong, Wheaton College, alex.wong@wheaton.edu Xiang Zhou, Xiangtan University, hoverchow@163.com

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#### About the authors

Valeria Galchenko is a PhD candidate at the University of Konstanz, working under the supervision of Nick Zubanov. She holds an M.Sc. degree in Science-Intensive Technologies and the Economics of Innovation from ITMO University, Saint Petersburg, Russia. Her main interests lie in organizational economics, human resource management, personnel economics, and experimental economics.

Nick Zubanov holds the Chair of Organizational Economics at the University of Konstanz. He does empirical research focusing on human resources in organizations, and more specifically on incentives, communications and employee turnover. He held professorships at the universities of Tilburg, Rotterdam, and Frankfurt and was awarded a PhD from the University of Birmingham.

Ho Lun (Alex) Wong is an Associate Professor of Economics at Wheaton College. He is also an Affiliate Fellow at the Pan Sutong Shanghai-Hong Kong Economic Policy Research Institute, Lingnan University. His primary research areas are development economics and the economics of education, with additional interests in health economics, labor economics, and public economics.

Xiang Zhou is an assistant professor in the Department of Economics at the Business School of Xiangtan University. His primary research field is labor economics. He earned his Ph.D. in Management from the University of Birmingham, UK. Following his Ph.D., he worked as a postdoctoral researcher in the Department of Economics at Jinan University. Earlier in his career, he held positions at Lingnan University and the Chinese University of Hong Kong.

# Unequal pay, fairness perceptions, and work effort: Experimental evidence from China

Valeria Galchenko, University of Konstanz, Germany Nick Zubanov, University of Konstanz, Germany Ho Lun Wong, Wheaton College, IL, USA Xiang Zhou, Xiangtan University, China

In a field experiment in China, we informed randomly selected workers that others received a higher wage for the same work. 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 did not communicate reasons for the pay difference, a post-experiment survey revealed that workers developed their own, predominantly benign, explanations, the leading one 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. 57% believed that the informed workers perceived their higher-paid peers to be better workers and aspired to match them. When asked what they would do in a similar situation, 75% replied that they would work harder as well. Our results hint at the importance of culture in moderating behavioral responses to unequal pay, not all of them necessarily negative.

### 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 and Zenger, 2022; Cullen, 2024), but it may also have consequences for organizational performance. A review by Bamberger, 2021 identifies three dimensions of pay transparency, distinct in their performance effects: outcome (or level of pay), process (pay setting rules), and communications about pay within firms. Our study is about performance effects of pay level transparency, and asks what would happen to lower-paid workers' performance when they find out about earning less than peers.

Intuitively, knowing of being paid lower than peers could be frustrating and negatively affect one's attitudes and performance. Evidence from empirical studies on performance effects pay level transparency we surveyed, listed in APPENDIX A, supports this intuition: after learning of their position in the earnings distribution, lower-paid workers become less happy (Luttmer, 2005) and less satisfied with their pay (Cohn, Fehr, Herrmann, et al., 2014) and job (Pfeffer and Langton, 1993; Clark and Oswald, 1996; Card et al., 2012; Schnaufer et al., 2022), while relatively high earners react little. Furthermore, informed lower-paid workers tend to work less hard than their equally earning but uninformed peers (Geurts, Buunk, and Schaufeli, 1994; Gächter and Thöni, 2010; Nosenzo, 2013; Cohn, Fehr, Herrmann, et al., 2014; Breza, Kaur, and Shamdasani, 2018; Schreurs et al., 2020; Cullen and Perez-Truglia, 2022; Gutierrez, Obloj, and Zenger, 2025), are less likely to collaborate (Pfeffer and Langton, 1993), especially with higherpaid coworkers (Bamberger and Belogolovsky, 2017), and may reduce work quality (Greiner, Ockenfels, and Werner, 2011). The few exceptions are Charness and Kuhn, 2007, who found no effect of unequal pay on individual effort choices, Bartling and 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.

Our study advances the literature on performance effects of pay level transparency, empirically as well as theoretically. Starting with empirical contributions, we provide new evidence on the effects of pay level transparency from China. Surprisingly little is known about how pay transparency affects performance in this major economy, Liu-Kiel et al., 2013 being, to our knowledge, the only other study involving Chinese participants. 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 transparent pay practices and outcomes. Thus, a recent survey by Aon, 2024 of 350 HR managers in thirteen locations throughout Asia, including China, found that 77% of the respondents based in China consider pay transparency important, even though little is known about its potential performance effects.

Our real-effort field experiment involving 329 student workers at Shaoyan University in Hunan, China, produced several discoveries. In it, we paid different hourly rates to different workers for the same job of digitizing paper questionnaires, at the same time and location, not informing the workers of this difference when the work commenced. Then, in the middle of the work session, though a routine administrative procedure, we informed randomly selected lower-paid workers of the higher rate we had paid to 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 58% of the lower-paid believed that higher-paid workers were better. Other explanations elicited from the workers were benign as well, and there were no significant differences in performance by explanation provided.

Our puzzling findings are not easy to dismiss on the grounds of flawed research design or lack of external validity. We took many precautions in our procedures to ensure our experiment met or exceeded the current research design standards of the relevant literature. Moreover, a followup 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, 57% stated that the informed workers believed their higher-paid peers to be better workers and tried to match them by working harder (58% of our workers had a similar explanation). Furthermore, when we asked the respondents what they would do in a similar situation, 75% said they would work harder.

Turning to theoretical contributions, our results contradict fairness theory (Adams, 1963; Fehr and Schmidt, 1999; Folger and Cropanzano, 2001), the leading theoretical framework in pay level transparency research, that predicts negative reactions of lower-paid workers to what they would perceive as unfair treatment. However, fairness theory itself and most of its empirical applications originated in Western cultures (Breza, Kaur, and Shamdasani, 2018's study of Indian workers is an exception), and it is not clear whether its predictions would work in cultures different from the West, such as China. It is theoretically possible for culture to moderate the effects of pay level transparency. For instance, the link between individual attitudes/perceptions and behaviors is weaker in high power distance cultures (Daniels and Greguras, 2014), which could render unfair pay decisions by the authority more acceptable. Also, a culture strong on long-term orientation may shift emphasis away from satisfying momentary needs to observing longer-term interests, which may not be served best by instant retribution.

After examining different theoretical arguments that could explain our results, we find a combination of high power distance and long-term orientation, characteristic of Chinese culture, to be the most plausible explanation. Hence our proposal for the inclusion of the culture factor in the theoretical models linking pay level transparency and organizational performance, along with some practical suggestions on how this could be implemented. We conclude with outlining implications of our work for management practice and its limitations.

### 2. Study Design

In our study design, we strove to combine the rigorous control of an experiment with the realism of a genuine working environment. This section describes the steps we took to achieve this ambition. We pre-registered our study design with the American Economic Association's registry for randomized control trials (registration ID AEARCTR-000XXXX [redacted for anonymity]), thereby committing ourselves to the research plan, focal outcomes and moderators as we envisaged before seeing the data. It is also important to mention that our experimental design was reviewed and approved by the relevant Ethics Committee. We paid at- or above-the-market wages to the workers, who were under no formal or implied obligation to work for us.

#### 2.1. Guiding theory

Like most of the related research we surveyed, our study was informed by a combination of arguments from theories of equity (Adams, 1963) and fairness (Fehr and Schmidt, 1999; Folger and Cropanzano, 2001), which in what follows we label as "fairness theory" for brevity. Fairness theory argues that one perceives the outcome of a social exchange as more, or less, fair depending on how the outcome compares with one's contribution, as well as with the outcomes and contributions of the other parties, including peers: the more equitable to the peers', the fairer. People are averse to unfairness, and will attempt to restore fairness by punishing the perpetrator of unfair action, even at a personal cost. The willingness to pay for restoring fairness depends on "fairness concerns", a personality trait the varies across individuals and can be elicited through incentivized experiments (Cohn, Fehr, and Goette, 2015; Abbink, Irlenbusch, and Renner, 2000), as we do later.

The proposition of fairness theory that fairness-concerned individuals will apply costly punishment to restore fairness is supported by the results of numerous experiments with the "ultimatum game": the distributor shares a valuable resource between herself and a receiver, both get the allocated shares if the receiver accepts the offer, but nothing otherwise (Oosterbeek, Sloof, and Van De Kuilen, 2004; Rand et al., 2013; Ho and Su, 2009). Contrary to the simple economic intuition that the receiver is better off from any nonzero offer from the proposer and should therefore always accept, offers that are too unequal are frequently rejected.

In the employment context, which is closer to our study, this behavior is captured in the "fair wage hypothesis" (Akerlof and Yellen, 1990) stating that workers will withdraw effort or quit altogether if their wage falls below what they consider fair. Restated for the context of pay level transparency, this argument would have lower-paid workers perceive their wage as less fair and act punitively towards the boss who sets the wages, unless they perceive the higher-paid peers' wages as the reward for their better performance (Breza, Kaur, and Shamdasani, 2018). Summing up, fairness 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?) and moderated by their fairness concerns (willingness to punish the unfair boss) and

reasons for unequal pay. This simple conceptual model guided our experimental design in ways we describe next.

#### 2.2. Context: worker recruitment, job task, and payment

We ran a real-effort field experiment to capture the causal effect of pay transparency free of possible confounds in an realistic yet controlled work environment. Our experiment took place on two weekends, 22nd-23rd October and 29th-30th October 2016, on campus of Shaoyang University in Hunan, China. The participants, henceforth workers, were Shaoyang students hired through regular job adverts (text in APPENDIX B) 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 C) were used in a separate and unrelated research project. Each questionnaire is 9 pages long, contains 102 fields, and took 6.2 minutes to digitize, on average.

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

#### 2.3. Treatment and key variables

Our treatment was to inform randomly selected regularly-paid workers of being paid less for the same job than the other, higher-paid ones, who got paid 35 RMB/hour. The uninformed workers in the control condition were paid the same hourly rate as the informed workers (20 RMB/hour), but were not notified 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.

We communicated the information about the others' pay to the informed workers 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 the payslips for the control group contained less detail. Specifically, as an example payslip in APPENDIX D shows, the informed workers saw both the regular (20 RMB/hour) and high

(35 RMB/hour) hourly rates next to each other, with the regular 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 high pay rate.

As per our pre-registered experimental design, our key performance outcomes are: 1) the length of time spent digitizing a paper questionnaire, and 2) the number of empty fields in a digitized questionnaire. We believe these variables are adequate measures of work effort. Our third key outcome is pay fairness perceptions, measured in a survey after the experiment, which we also treat as a mediator in some specifications. Our key pre-registered moderator variable is fairness concerns measured in a pre-experiment survey. Additionally, in post-experimental analysis, we estimated the treatment effect on the quality of work as measured by the number of mistakes per questionnaire discovered upon inspecting a random sample of questionnaires.

# 2.4. Allocation into experimental conditions, and worker descriptive statistics

After the registration deadline of October 10th expired, we randomly allocated the 514 workers who registered with us into the regular (N = 492) and high pay (N = 22) groups. Those ended up in the regular pay group were randomly assigned into the treatment (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, and was informed accordingly. We used seminar rooms (example photo in APPENDIX E) 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 work sessions planned. However, 172 registered workers did not show up for work, so we ended up with 342 workers in 132 work sessions, 153 workers in 47 treatment sessions, 171 in 79 control sessions, and 18 in 6 high-pay (35 RMB/hour) sessions. Furthermore, owing to technical problems, we had to exclude observations from three workers in the treatment and ten in the control conditions (2% and 6% of the treatment and control samples, respectively), which leaves us with the usable sample of 150 workers in the treatment and 161 in the control conditions.<sup>1</sup>

Why did we end up with more control than treatment work sessions (79 vs. 47)? To minimize information spillovers between workers in different conditions in a communication-intensive college environment, we decided to form fewer treatment work sessions and schedule more of

<sup>&</sup>lt;sup>1</sup>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.

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 1 reports descriptive statistics for N = 514 workers we randomly allocated into the treatment and control conditions at the registration stage, and for those N = 172 who did not show up. Looking at all the registrations, an average worker is 19.4 years old and is on their 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). Turning to those who showed up vs. no-shows, female, more junior and non-art major workers were statistically significantly more likely to show up. However, the differences between the shows and no-shows along these dimensions are small, and we control for those in our statistical analysis.

	(1) Registered	Showed u	(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)	0.402	19.305 (0.944)	19.542 (1.304)	0.189	
Female	$0.545 \\ (0.498)$	0.573 (0.496)	0.602 (0.491)	0.461	0.500 (0.514)	0.477 (0.501)	0.028**	
Study year	1.573 (0.722)	1.493 (0.653)	1.609 (0.717)	0.036**	1.278 (0.461)	1.663 (0.804)	0.048**	
Major Art	0.175 (0.380)	0.160 (0.368)	0.168 (0.375)	0.796	0.056 (0.236)	0.215 (0.412)	0.091*	
Business and Economics	0.192 (0.395)	0.173 (0.380)	0.211 (0.409)	0.233	0.278 (0.461)	0.198 (0.399)	0.837	
Engineering and Tech	0.389 (0.488)	0.440 (0.498)	0.348 (0.478)	0.019**	0.389 (0.502)	0.378 (0.486)	0.713	
Social Science and Law	0.226 (0.418)	0.227 (0.420)	0.248 (0.433)	0.525	0.278 (0.461)	0.203 (0.404)	0.394	
Others	0.015 (0.124)	0.000 (0.000)	0.025 (0.156)	0.006**	0.000 (0.000)	0.006 (0.076)	0.206	
Session size	-	3.191 (1.393)	2.038 (1.055)	0.000***	6.667 (2.275)	-	-	
N	514	150	161		18	172		

Table 1: Descriptive statistics

*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.

Focusing on the workers in the control vs. treatment conditions, there are no significant

differences in their observable characteristics, with the exception of the average group size per work session, which is larger in the treatment (3.19) than control condition (2.04). This is because we planned fewer treatment work sessions, to minimize information spillovers, which meant more workers per session. Our results are robust to controlling for differences in group size per work session.

#### 2.5. Personnel

We hired room managers and assistants to help administer the experiment so as to ensure smooth operations and prevent information spillovers. Room managers prepared the rooms for work sessions, greeted the workers and showed them to their desks, announced instructions, prevented mobile phone use or other communications during work, and reported any out-of-protocol situations such as complaints, questions, walk-outs (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 session started, and brought the payslips to be signed by the workers during their work session break.

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

#### 2.6. Procedures during work sessions

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

At the beginning of the work session, but prior to the start of work itself, room managers read the instructions for the workers that we prepared. The instructions introduced us to the workers as "researchers from Hong Kong and Germany who hired you", explained the task, the house rules, and the schedule of the work session. Importantly, we instructed the room managers to inform the workers that the job 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. We provide details of this survey, labeled Q1, in the next section. After the workers completed survey Q1, assistants entered the room to announce the applicable hourly pay rate. They then distributed individual sealed envelopes containing the total payment in cash, requesting each worker to count the money to ensure the amount was correct. Thereafter, the first work period began, lasting 75 minutes and ending with a prescheduled 15-minute break, during which the treatment, as described above, was administered. After the break, the second, post-treatment 75-minute work period began during which the workers resumed their usual tasks. At the end of the work session, the room managers thanked the workers for coming and the assistants escorted them out of the building so as not to cross paths with the newly arriving workers.

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

In anticipation to possible queries from the 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.

#### 2.7. Pre-experiment Survey Q1: Eliciting fairness concerns

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

Our measure of fairness concerns is based on Abbink, Irlenbusch, and Renner, 2000's "moonlighting" game, also used in Cohn, Fehr, and 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 1-3 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 as defined in Ubeda, 2014. Thus, 58% choose to punish the distributor in the other-favoring allocation scenario 3 (6 RMB for the focal worker, 14 RMB for the other recipient), and 46% choose to reward in the self-favoring allocation scenario 2 (14 RMB for the focal worker, 6 RMB for the other recipient). Yet, there is also a significant fraction of the workers who seem to exhibit another, more strictly egalitarian, type of fairness concerns, rewarding the distributor for equal allocation in scenario 1 (25%) and punishing for unequal allocation, whether favoring self or not, in scenarios 2 or 3. Fairness-neutral workers are relatively few: although 65%, 18% and 32% do nothing in scenarios 1, 2 and 3 respectively, only 9% do nothing in all three scenarios. These preliminary findings are consistent with the existing evidence that people do have fairness concerns that affect their economic decisions (Fehr, Goette, and Zehnder, 2009; Ho and Su, 2009; Oosterbeek, Sloof, and Van De Kuilen, 2004; Fehr and Fischbacher, 2004).



20

0

P 4.5

Р3

Control (N = 159)

P 1.5

P 6



*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.

0

R 1.5

R 3

Treatment (N = 150)

R 4.5

R 6

## Figure 2: 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.





*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.

# 2.8. 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 postexperiment survey, labeled Q2 in what follows. Of the final sample of 311 workers, 260 (83%) showed up to collect their certificates and participated in survey Q2 whose text is in APPENDIX G.

Survey Q2 was conducted on computers and did not allow revisiting previously answered questions. In the first five questions, we asked the workers to evaluate various aspects of their work experience on a scale from 1 to 7, without reminding of pay differences. The evaluations covered five aspects: the difficulty and attractiveness of the work, work conditions, previous data entry experience (a placebo test), and, most importantly, how fair the pay was.

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

#### 2.9. 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 ( $T_i$ ), pretreatment outcome ( $Y_{i0}$ , where available) and controls for individual and contextual characteristics such as gender, study year and major, work session size, time and date:

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

For instance, to estimate the average treatment effect on the length of time to process a questionnaire ( $\beta$ ), we regress the average post-treatment processing time for a given worker on his/her treatment dummy, pre-treatment average processing time, and controls. <sup>2</sup> 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 and Certo, 2021; Li et al., 2024),

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

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 and Certo, 2021) or for dynamic feedback from pre- to post-treatment performance outcomes. In fact, the estimated  $\gamma$  in our regressions is 0.5-0.7, depending on specification (Tables H2-H3 in APPENDIX H), significantly different from  $\gamma = 1$  assumed in the standard difference-in-difference regression (restriction test *p*-values between 0.07 and 0.001).

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

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

#### 3. Experimental Results

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

Pay fairness perceptions, measured in the post-experiment survey Q2, is among the key preregistered outcomes of our experiment. Table 2 (column 1) shows that, compared to the unin-

<sup>&</sup>lt;sup>2</sup>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  $Y_{i0}$ .

formed, 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, amounting to over a quarter of the standard deviation in pay fairness perceptions. This result is robust to variations in regression specification (Table H1 in APPENDIX H).

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

Table 2: Average treatment effects on worker perceptions

\* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01

Standard errors in parentheses are clustered by work session.

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

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

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

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

#### **Result 2: Informed workers tend to work harder.**

Columns 1 and 4 in Table 3 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 H2-H3 in APPENDIX H). 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.

	Length			Empty fields		
	(1)	(2)	(3)	(4)	(5)	(6)
Sample	All	Q2 part	icipants	All	Q2 part	icipants
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

Table 3: Average treatment effects on performance outcomes

\* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01

Standard errors in parentheses are clustered by work session.

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

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

*Notes:* This table reports the average treatment effects on the key, 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, columns (3) and (6) show treatment effects mediated and moderated by pay fairness perceptions (insignificant in all cases). 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.

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

A further result from Table 3 (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. Contrary to fairness theory, 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 3).

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 4). 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 withdrawing effort. 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. We begin probing into these factors by asking the workers for their explanations why we paid different hourly rates for the same work.

	=		
	(1)	(2)	(3)
	Fair pay	Length	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

Table 4: Treatment effects moderated by fairness concerns

\* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01

Standard errors in parentheses are clustered by work session.

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

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

*Notes:* This table reports the average treatment effects and moderation effects by fairness concerns measured in the pre-experiment survey Q1. 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.

#### **Result 4: Workers give various benign explanations for the pay difference.**

In the post-experiment survey Q2, we reminded the workers about the pay difference and asked them to describe why, in their opinion, different workers got different hourly pay rates for the same work (to recall: we never gave an explanation ourselves). An application of text analysis revealed the following groups of explanations provided by the workers: "better workers", "different job", "choices made in survey Q1", "experimental treatment", "don't know", and "other". We elicited 305 distinct explanations from the texts written by 260 workers who showed up for Q2, 1.17 explanations per worker on average.

Figure 4 shows the distributions of the explanation groups for the informed workers in the treatment group and uninformed workers in the control group. The explanations are predominantly benign, with the "better workers" and "different jobs" being by far the leading explanations. The correct "experimental treatment" and the agnostic "don't know" explanations are rare. There is no difference between the distributions of the explanation groups for the informed and control workers (p-value = 0.8). In addition, there is no correlation between the workers' explanations and their fairness concerns or pay fairness perceptions. The specific explanations

informed workers gave for the pay difference they experienced in the absence of any information from us about the actual reason suggests that they tried to make sense of the experience they had working for us (Maitlis and Christianson, 2014), in different but benign ways.





*Notes:* The graph plots the distributions of the explanations for pay differences provided by informed (treated) and uninformed (control) workers in the post-experiment survey Q2. In total, 260 workers submitted 305 explanations. The *p*-value of the chi-squared test for equality of treatment and control distributions is 0.793, indicating no statistically significant difference between the two groups.

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

Table 5 reports treatment effects estimated on the subsamples of workers citing a specific explanation: "better worker", "different job", and "other". The explanation-specific treatment effects are not significantly different from each other (p-values from 0.14 to 0.73, depending on the outcome). This no-result does not necessarily contradict Breza, Kaur, and 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. <sup>3</sup> In other words, there may be a macro-level factor (culture?) that is

<sup>&</sup>lt;sup>3</sup>Another possible reason for the lack of moderation could be low reliability of the explanations measured in Q2. Workers may have forgotten their actual experiences and come up with plausible but random explanations when asked. What speaks against this possibility, however, is the robust difference in pay fairness perceptions between the informed and uninformed workers measured in the same survey and before they were reminded of our treatment.

shared by many workers and shapes their perceptions of, and responses to, unequal pay.

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

 Table 5: Average treatment effects estimated on the subsamples of workers citing a specific explanation for pay differences

\* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01

Standard errors in parentheses are clustered by work session.

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

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

*Notes:* This table shows average treatment effects estimated on the subsamples of workers citing a specific explanation for the pay difference: "better worker", "different job", 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.

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

Table 6 reports coefficients on the interactions of the treatment dummy and a selection of contextual characteristics other than fairness concerns, reported above, which was our pre-registered moderator. The treatment effects on performance are independent of the worker's gender, study cohort, major, 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.

	(1)	(2)	(3)	(4)	(5)
	Female	Cohort	Major	Date	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** (0.286)	-0.141 (0.162)	-0.040 (0.044)	-0.029 (0.200)	

Table 6: Treatment effect heterogeneity

\* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01

Standard errors in parentheses are clustered by work session.

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

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

*Notes:* This table shows the coefficients on the interactions of the treatment dummy and contextual characteristics: gender, study cohort, major, working session date, and baseline performance, where available.

### 4. Further Analysis

In this section, we describe 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, and Werner (2011). To test this possibility, we performed a quality check by taking a random sample of 407 questionnaires (6% of the total processed) digitized by 89 different workers (29% of the total headcount) and calculating the number of mistakes in each questionnaire. A mistake is defined as any discrepancy between the original paper questionnaire and its digital version. For this task, we hired six research 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 7 reports descriptive statistics and treatment effects for the subsamples selected and not selected for the quality check. The two subsamples are not significantly different from each other in terms of all the average characteristics except the number of empty fields per questionnaire, which is somewhat lower in the quality check subsample. The broad similarity

of the average characteristics of, and the treatment effects on, the selected and not selected subsamples suggests that the selected subsample is representative of our entire sample.

	(1)	(2)	(3)
	Selected Sample	Not Selected	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
C	(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)	

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

 $\overline{p < 0.10, ** p < 0.05, *** p < 0.01}$ 

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.

The average number of mistakes per questionnaire in the quality check subsample is 1.9,

about 2% of the total number of positions. Table 8 reports the treatment effects on the number of mistakes from the ANCOVA regressions (1) estimated at the questionnaire and worker (average) level, with and without the usual controls plus controls for the length and the number of empty fields which may be correlated with the number of mistakes. The absence of a significant effect on the number of mistakes in either specification leads us to conclude that the treatment effect on the quantity of output occurred without detriment to quality.<sup>4</sup>

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

Table 8: Average treatment effects on the number of mistakes per questionnaire

\* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01

Standard errors in parentheses are clustered by work session.

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

*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 worker 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.

#### 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 9 and 10. 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. However, the survey sample is close in its characteristics to the worker sample in our experiment, allowing us to draw comparisons between the two samples.

<sup>&</sup>lt;sup>4</sup>One could argue that our treatment effect estimates are not significant 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. Our simulation results produced the implied standard errors of the treatment effect estimates that are about half the size of the actual standard errors reported in Table 8. Even with these simulated standard errors, the treatment effects would still not reach statistical significance in three out of four specifications shown in Table 8.

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 1100 survey participants was 2661, or 2.42 per average participant. Figure 5 shows distribution of the explanations given by survey participants. The leading explanation for our main results, 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 earlier survey Q2 where "better workers" was also the leading explanation. The other popular explanations are workers' showing "high motivation signal", hoping their efforts would still be rewarded somehow ("reward efforts"), and avoiding losing the chance for better pay in future that might come from working harder ("chance for better pay". Again, as in survey Q2, the explanations are benign, except for the offered but infrequently chosen "emphasize injustice" and "distance from negative feelings". (The own, free-text explanations are few and can be grouped with the explanations we offered to the participants to choose from.)

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 9. 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 10. 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.





*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.

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

 Table 9: Descriptive statistics of China's working-age population survey respondents, and their correlations with the explanations they provided

\* 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.

<b>I</b>	(4)		
	(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		

## Table 10: Correlations between choice of effort and China's working-age population survey respondents' characteristics

\* 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.

#### 4.3. Results for the highly-paid workers

To complete our analysis, we now turn to the small group of N = 18 randomly selected highly-paid and uninformed workers whom we paid a substantially above-market hourly rate, 35 RMB/hour. Table 11 reports the differences in the average performance outcomes between the highly-paid and the regularly paid, also uninformed, workers (our control group) estimated from equation (1). The estimates from Table 11 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 highly-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 and 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, and 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.

Table 1	1: Differences in I	key performance out	comes between ui	ninformed high-paid wor	rkers
	(35 RMB/hour)	) and uninformed reg	ularly-paid worke	ers (20 RMB/hour, the co	ontrol
	group)				
_		(1)			-

	(1)	(2)	(3)
	Fair pay	Length	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 session size (all specifications).

*Notes:* This table reports the differences in the average performance outcomes between the high-paid uninformed workers (N = 18) and the regularly 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 control sessions and 18 in high-paid sessions. For column 1, the sample size is 147, with 131 control group workers and 16 high-paid workers participating in Q2.

### 5. Discussion

Surprisingly little is known about the effects of pay level transparency in China, especially given the wealth of the existing literature on the topic. 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 finding with 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 (Fehr and 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 and 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 and Thöni, 2010; Nosenzo, 2013; Cohn, Fehr, Herrmann, et al., 2014; Breza, Kaur, and 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, and 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 question some of the key predictions of fairness theory that guided most of the research on pay level transparency, including our study. To be precise, our findings of workers having fairness concerns over resource distribution (survey Q1 results, Figures 1-3) and perceiving their pay as less fair when informed of the others' higher pay (Result 1, Table 2) are consistent with fairness theory. However, the theory is inconsistent with our findings of no correlation between pay fairness perceptions and fairness concerns, no mediation of the treatment effects by pay fairness perceptions, and no moderation by fairness concerns, let alone the more surprising finding of a positive treatment effect on output. 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? Questioning the universal applicability of fair-

ness theory, our findings require explanations, not only to rationalize the results of this specific study but also to guide future research on pay level transparency in organizations.

#### 5.1. Failed experiment?

Before we get on with this task, 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 would internally invalidate our findings, especially in comparison with other published experimental studies we consulted. 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 1). Our sample is large enough, thus obviating statistical power concerns. 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 information spillovers (workers knowing of pay differences 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 H2-H3). 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 basic 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 and 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, through abductive reasoning, 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, defined as "the extent to which the less powerful members of organizations and institutions accept that power is distributed unequally" (Hofstede, 2011, p. 9), is a key dimension in Hofstede's framework. 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 and employers (Tyler, 2000), and weakens the link between attitudes/perceptions and observed behaviors (Daniels and Greguras, 2014), including reactions to unfair treatment (Fischer and 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).<sup>5</sup> 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 the authority (us or the University)

<sup>&</sup>lt;sup>5</sup>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*.

that they believed decides on their employment and pay. Such beliefs are congruent with longterm 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, and 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 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 authorities, 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 and Miller, 1978). Correlated with power distance and long-term orientation, BJW in China are also stronger than in many Western societies (Reich and 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 (Figure 4) 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 and 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, and 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 and Yee-kwong Chan, 2003), and hard work and prosocial behavior do improve one's face (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 going 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 close to significant difference in the treatment effects for the informed workers who stated "better workers" 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 for 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 small project to fill a relatively narrow research gap (testing fairness theory in the context of China), our study ends up asking a broader question: how does culture affect perceptions of unequal pay and behavioral 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, such as Prolific, 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 and Bamberger, 2014). Indeed, most of the literature on pay level transparency is experimental (APPENDIX A). 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: "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 certainly do not 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 fairness theory that guided the design of our study, we did not collect information on the culture constructs we have ended up relying on in explaining our unexpected results. We therefore cannot at this point offer a thorough test of our proposition that culture matters for performance effects of pay level transparency. However, the failure of fairness theory to predict our results and circumstantial evidence from surveys are consistent with this proposition.

Second, there are usual (for experiments) concerns about external validity of the results. Is there something special about the context in which our experiment took place? 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? To our knowledge, the location of our experiment and our worker sample are representative of many Chinese higher educational institutions. The China working-age population survey results are consistent with our experimental findings, lending them a degree of external validity. However, as with other studies, replication is the ultimate test for external validity.

A related concern is that most of our student workers were too young to have substantial labor market experience, including the experience of unequal pay. There are other studies that relied on data from young people in their early 20s (e.g., Nosenzo, 2013, Liu-Kiel et al., 2013, Cohn, Fehr, Herrmann, et al., 2014). True, young workers may not be representative of working-age

population, but it is not clear what difference to expect between the reactions of your and more mature workers 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.

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## Appendices

1

# APPENDIX A. Summary of empirical studies on pay level transparency

<u> </u>					<i>C</i> 1
Study	Experiment	Treatment/Key regressor	Focal outcome	Kesuits Lower-paid workers	for the Higher-paid workers
Pfeffer and Lang- ton (1993)	No	Salary variation within a given academic department	Research productivity (self-reported)	Negative	-
		-	Collaboration (self-reported)	Negative	-
			Job satisfaction	Negative	Negative
Clark and Oswald (1996)	No	Reference income level	Job satisfaction	Nega	tive
Luttmer (2005)	No	Neighbors' earnings	Happiness (self- reported)	Negative	Negative
Charness and Kuhn (2007)	Yes	Employer choosing wages of both workers in a two-worker team	Work effort	No effect	No effect
Gächter and 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 and von Siemens (2011)	Yes	Paying unequal piece rates within teams	Willingness to partici- pate in team production	No effect	No effect
			Effort	No effect	No effect
Greiner, Ocken- fels, and 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
		1 7	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
				Continued	on next page

Study	Experiment	Treatment/Key regressor	Focal outcome	Results	for the
		2 0		Lower-paid workers	Higher-paid workers
		Employer choosing wages of one worker in a two-worker team, the other determined randomly	Work effort	Negative	Negative
Belogolovsky and Bamberger (2014)	Yes	Pay secrecy in performance- based wage	Individual task perfor- mance	Negative	
		Pay secrecy in performance- based wage	Continuation intention	Negative	
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 work- ers in a team	Work output	Nega	tive
			Pay satisfaction	Negative	
Bamberger and Belogolovsky (2017)	Yes	Pay transparency in performance- based wage	Amount of help work- ers afford to highest- paid peers	No effect on average <sup>6</sup>	-
Breza, Kaur, and Shamdasani (2018)	Yes	Paying different wages, depend- ing on productivity, informing of different wages BUT not provid- ing the reason	Turning up for work	Negative	Negative
	_		Work output	Negative	No effect
		Paying different wages, depend- ing on productivity, informing of pay differences AND providing the reason	Turning up for work	No effect	No effect
			Work output	No effect	No effect
Cullen and Perez- Truglia (2022)	Yes	Informing about the average salary over a random sam- ple of five managers (vertical comparisons)	Average hours worked	Positive	-
		•emparisens)	Work performance	Positive	-
		Informing about the average salary over a random sam- ple of five peers (horizontal comparisons)	Average hours worked	Negative	No effect
			Work performance	Negative	No effect
Schnaufer et al. (2022)	Yes <sup>7</sup>	Undermet pay expectations re- vealed through pay transparency	Job satisfaction	Negative	-
Gutierrez, Obloj, and Zenger (2025)	<ul> <li>No</li> <li>Providing access to information on academic salaries in public universities in U.S. states</li> </ul>		Research productivity index	Negative	Positive

<sup>&</sup>lt;sup>6</sup>More strongly negative effect for less individualistic and less prosocially motivated workers. <sup>7</sup>Survey experiment (vignette)

# APPENDIX B. Example of the job advert used to recruit workers for the experiment



#### 有偿问卷输入工作招聘

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



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

### **APPENDIX C. Representative page of the paper** questionnaire

#### 心理健康问题

<u>填来须知:</u> (1) 未表每一问题都只有"强烈同意"、"同意"、"中立"、"不同意"和"强烈不同意",五种可供选择答案。 (2) 对于每一个问题或陈述,请将你认为符合你想法的选项围起来,如果认为"强烈同意"选 1、"同意"选 2、"中立"选 3、 "不同意"这4 和"强烈不同意"则选 5。 (3) 这些问题主笑风映你的心情和感受,与学习成绩无关。 (4) 客爱没有对相、好坏之分,请根据你的直觉作答。

(圖出符合你想法的答案)	
题号 问题 答案	
示范题	
A 家重的功课总是让你觉得不开心。 I=强烈同意(2)=同意 3=中立 4=不同意 5=强烈不同意	
10 陈述(一)	
a 你夜里睡觉的时候,总想着明天的功课。 1= 11 11 11 12 12 12 13 13 14 14 14 15 15 15 15 15 15 15 15 15 15 15 15 15	t
b 老师在提问的时候,你总担心自己被老师抽中. 1=强烈同意 2=同意 3=中立 4=不同意 (3=强烈不同意	t
c 听说要考试就心理紧张. ①=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同能	t
d 成绩不好时,你心里感到很不开心. ①=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意	t
c 成绩不好时,你总是提心吊胆。 ①= 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意	t
f 考试的时候,原来知道的知识想不起来你会感到很焦 1=强烈同意 Q=同意 3=中立 4=不同意 5=强烈不同意	
虑.	
g 考试结束之后,没有出成绩之前总是感到放心不下。 ①= 强烈同意 2= 同意 3= 中立 4= 不同意 5= 强烈不同意	t
h 考试之前,你总是担心考不好。 ①=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同题	t
i 你希望考试能顺利通过. ①=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同和	ŧ 👘
j 在没有完成任务之前,你总是担心完不成任务。 1=强烈同意 2=同意 3=中立 4=不同意 (5=强烈不同)	\$

题号	问题	答案			
11	陈述(二)				
a	当着大家读课文时,总是担心会读错。	1=强烈同意 2=同意 3=中立 4=不同意 5-强烈不同意			
b	觉得学校公布的学习成绩总是不大可靠。	1=强烈同意 2=同意 3=中立 4=不同意 3=强烈不同意			
C	觉得自己比别人更担心学习。	①=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意			
d	做梦时,梦到自己考试没考好。	①=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意			
e	做梦时,梦到因学习成绩不好受到家长的训斥。	①=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意			
f	觉得同学在背后说自己的坏话。	1=强烈同意 2=同意 3=中立 4=不同意 ③=强烈不同意			
g	受到父母批评时,久久不能忘记。	①=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意			
h	在玩游戏或者体育运动等竞技活动中输给对方后,不 想再干了。	1=强烈同意 2=同意 3=中立 4=不同意③=强烈不同意			
i	别人在背后议论自己时,感到很厌烦。	①=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意			
j	在大家面前被老师提问时,感到脸红。	1=强烈同意 2=同意 3=中立 4=不同意 (5)=强烈不同意			
题号	问题	答案			
题号 12	问题 陈述 ( 三 )	答案			
题号 12 a	问题 <u>陈述(三)</u> 如果要你担任班干部,你会感到很紧张。	答案 1= 强烈同意 2=同意 3=中立 4=不同意合上强烈不同意			
题号 12 a b	问题 防述(三) 如果要你担任班干部,你会感到很紧张。 总是感到好像有人在注意你。	答案 1=强烈同意 2=同意 3=中立 4=不同意(5)=强烈不同意 1=强烈同意 2=同意 3=中立 4=不同意(5)=强烈不同意			
题号 12 a b c	问题 防运(三) 如果要你担任班干部,你会感到很紧张。 总是感到好像有人在注意你。 在工作和学习时,如果有人注意你,你会感到很紧 张。	答案 1=强烈同意 2=同意 3=中立 4=不同意()=强烈不同意 1=强烈同意 2=同意 3=中立 4=不同意()=强烈不同意 ()=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意			
题号 12 a b c d	问题 防速(三) 如果要你担任班干部,你会感到很紧张。 总是感到好像有人在注意你。 在工作和学习时,如果有人注意你,你会感到很紧 张。受到批评时你会感到不开心。	答案 1= 强烈同意 2=同意 3=中立 4=不同意()=强烈不同意 1= 强烈同意 2=同意 3=中立 4=不同意()=强烈不同意 ()=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意 ()=强烈同意 2=同意 3=中立 4=不同意 5=强烈不同意			
题号 12 a c d	问题 防迷(三) 如果要你语任班干部,你会感到很紧张。 总是感到好像有人在注意你。 在工作和学习时,如果有人注意你,你会感到很紧 张。 受到批评时你会感到不开心。 如果老师批评你,你心里会感到不安。	答案 1 = 强烈同意 2 = 同意 3 = 中立 4 = 不同意(5)- 强烈不同意 1 - 强烈同意 2 = 同意 3 = 中立 4 = 不同意(5)- 强烈不同意 0 = 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 0 = 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 0 = 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意			
题号 12 a b c d f	问题 <u>防途(三)</u> 如果要你拒任班干部,你会感到很紧张。 合是感到好像有人在注意你。 在工作和学习时,如果有人注意你,你会感到很紧 张。 受到批评时你会感到不开心。 如果老师批评你,你心里会感到不安。 有时微大家都在笑,你起不想笑。	答案 1 = 强烈同意 2 = 同意 3 = 中立 4 = 不同意 (5)= 强烈不同意 1 = 强烈同意 2 = 同意 3 = 中立 4 = 不同意 (5)= 强烈不同意 (0)= 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 (0)= 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 (0)= 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 (0)= 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意			
题号 12 a b c d f g	问题 防途(三) 如果要你担任班干部,你会感到很紧张。 总是感到好像有人在注意你。 在工作和学习时,如果有人注意你,你会感到很紧 张。 受到批评时你会感到不开心。 如果考示师批评你,你心里会感到不安。 有时候大家都在笑,你却不想笑。 觉得在同学家里玩不如在自己家里好玩。	答案 1 = 强烈同意 2 = 同意 3 = 中立 4 = 不同意(2)-强烈不同意 1 = 强烈同意 2 = 同意 3 = 中立 4 = 不同意(2)-强烈不同意 (2)=强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 (2)=强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 (2)=强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 (2)=强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 (3)=强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 (3)=强烈同意 2 = 同意 3 = 中立 4 = 不同意 6)=强烈不同意			
题号 12 a b c f f g h	问题 防途(三) 如果要你担任班干部,你会感到很紧张。 总是感到好像有人在注意你。 在工作和学习时,如果有人注意你,你会感到很紧 张。 受到批评时你会感到不开心。 如果考研批评你,你心里会感到不安。 有时强大家都在笑,你却不想笑。 觉得在同学家里玩不如在自己家里玩玩。 大家在一起的时候,你会感到自己很孤单。	答案 1 = 强烈同意 2 = 同意 3 = 中立 4 = 不同意(2)= 强烈不同意 1 = 强烈同意 2 = 同意 3 = 中立 4 = 不同意(2)= 强烈不同意 (2)= 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 (2)= 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 (2)= 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 (2)= 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 (3)= 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 (3)= 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 (3)= 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 (3)= 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意			
题号 12 a b c f f g h i	问题 <u>防途(三)</u> 如果要你拒任班干部,你会感到很紧张。 合是感到好像有人在注意你。 在工作和学习时,如果有人注意你,你会感到很紧 张。 受到防护时你会感到不开心。 如果老师抗冲时你,你心里会感到不安。 有时像大家都在笑,你却不想笑。 觉得在同学家里玩不如在自己家里好玩。 大家在一起的时候,你会感到自己很孤单。 与其和同学一起玩,自己一个人玩理开心。	答案 1 = 强烈同意 2 = 同意 3 = 中立 4 = 不同意 (5)= 强烈不同意 1 = 强烈同意 2 = 同意 3 = 中立 4 = 不同意 (5)= 强烈不同意 (0)= 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 (0)= 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 (0)= 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 (1)= 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 1 = 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 1 = 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 1 = 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 1 = 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 1 = 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意 1 = 强烈同意 2 = 同意 3 = 中立 4 = 不同意 5 = 强烈不同意			

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

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# APPENDIX D. Example of the payslip for the informed workers

Payslip

#### 工资单 Sponsor: Lingman University and Goethe University Job: Shaoyang University Questionnaire Data Entry 资方 : 香港岭南大学与德国歌德大学科研团队 Job Description: Questionnaire data entry in an online form. 工作 : 邵阳大学学生问卷数据录入 工作描述 : 在网上表格中录入心理健康问卷数据 Student Name Student Date of Birth : 学生姓名 Student ID number : : 学生出生日期 Date of work : Total number of hours worked : 3 hours 学生证号码 工作日期 Hourly rate (set by researchers) : :3小时 35 RMB 20 RMB 工时总数 时薪(由研究人员设定) :35元人民币 20元人民币 Total salary 总工资(由研究人员设定) :105元人民币 60 元人民币 (set by the researcher) : 105 RMB 60 RMB 学生签署 Signature of student

Note: This is an example of the payslip for the informed workers, translated into Chinese and English.

# APPENDIX E. Photo of a typical room where working sessions took place



### APPENDIX F. Text of the pre-experiment paper survey Q1 translated into English

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

- Your name:
- Your student ID:
- Your date of birth:
- Your gender:
- Your study major:
- Your grade:

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

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

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

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

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

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

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

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

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

- Reward the distributor 3 yuan (you pay 1 yuan)
- Reward the distributor 4.5 yuan (you pay 1.5 yuan)
- Reward the distributor 6 yuan (you pay 2 yuan)
- Scenario 3: You have 6 yuan, the other recipient 14 yuan, the distributor 10 yuan.
- Punish the distributor 6 yuan (you pay 2 yuan)
- Punish the distributor 4.5 yuan (you pay 1.5 yuan)
- Punish the distributor 3 yuan (you pay 1 yuan)
- Punish the distributor 1.5 yuan (you pay 0.5 yuan)
- Do nothing (you pay 0 yuan)
- Reward the distributor 1.5 yuan (you pay 0.5 yuan)
- Reward the distributor 3 yuan (you pay 1 yuan)
- Reward the distributor 4.5 yuan (you pay 1.5 yuan)
- Reward the distributor 6 yuan (you pay 2 yuan)

### APPENDIX G. Text of the post-experiment online survey Q2 translated into English

Thanks again for helping us with data entry in October this year. We would like to learn about your experiences while working for us.

Please rate on the scale of 1 (not at all) to 7 (very much) the following statements about your experiences:

- The work was difficult

- You already had data entry experience before doing work for us

- Your experience of working for us contributed to the development of your professional skills and is useful

- The work room had adequate size and was well-lit and well-aired

- The pay was fair

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

- How many people you are in regular contact with (e.g., friends, classmates, etc.) do you know to have registered to do the data entry work for us?

- Of these people, how many did their work BEFORE you?

- Did you hear BEFORE coming to work for us that we were paying different rates to different people for the same work?

- If your answer to the previous question is YES, where did you hear about it from?

- In your opinion, what were the reasons for different pay rates given to different people for the same work?

# APPENDIX H. Robustness checks for the average treatment effects

Table H1: Average treatment	t effects on worker pay fairnes	s perceptions with different sets of
control variables		

	(1)	(2)	(3)	(4)	(5)
Treatment	-0.315**	-0.301**	-0.273**	-0.335**	-0.271*
	(0.123)	(0.127)	(0.127)	(0.137)	(0.137)
Controls:					
Session size	+	+	+	+	+
Female			+		+
Major			+		+
Study cohort			+		+
Date + time				+	+
Exclude heard		+			
N	260	244	260	260	260
R-squared	0.025	0.023	0.087	0.034	0.095

\* 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.

Notes: This table shows the average treatment effects on worker pay fairness perceptions. There were N = 260 workers who took part in the post-experiment survey Q2, and N = 244 workers who did not hear of the pay difference before coming to work.

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

Table H2: Average treatment effects on the length of time per questionnaire with different sets of control variables

\* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01Standard errors in parentheses are clustered by work session. The estimates are expressed in standard deviations of the outcome variables.

Notes: This table shows the average treatment effects on the length of time it took the workers to process a questionnaire with different sets of control variables. There were N = 311 workers in the control and treatment groups together, N = 260 workers who took part in the post-experiment survey Q2, and N = 244 workers who did not hear of the pay difference before coming to work.

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

Table H3: Average treatment effects on empty fields with different sets of control variables

\* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01Standard errors in parentheses are clustered by work session. The estimates are expressed in standard deviations of the outcome variables.

Notes: This table shows the average treatment effects on the number of empty fields in the processed questionnaires. There were N = 311 workers in the control and treatment groups together, N = 260workers who took part in the post-experiment survey Q2, and N = 244 workers who did not hear of the pay difference before coming to work.

# APPENDIX I. Text of the online survey of China's working-age population

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

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

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

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

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

Questions:

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

Menu options:

- They worked harder because they believed that those earning more than them were better workers and they wanted to match them in performance.

- They worked harder because, although the job was one-off and pay was fixed in advance, they hoped that their efforts might still be rewarded somehow.

- Working hard even though the pay is relatively low sends a strong signal of integrity and high motivation. The informed people wanted to send this signal to the managers who observed their performance.

- They perceived unequal pay for the same work as unjust and wanted to do something to achieve equal pay. They chose to work harder to emphasize how unjust it was to pay them less than others.

- They believed the only chance for a better pay was to work harder. They took this chance, however small, to avoid the feeling of regret from not using it.

- They worked harder to distance themselves from negative emotions caused by learning of being paid less than others.

- None of the explanations above sounds convincing to be, but I have my own, namely...

- None of the explanations above sounds convincing to me, and I cannot think of any other explanations.

2. If YOU found yourself in a similar situation as the informed people, how would you react:

- worked harder

- worked less hard

- did not change effort.

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

3. Gender

- 4. Age
- 5. Family status
- 6. Education (in years)
- 7. Work experience (in years)
- 8. Occupation
- 9. Do you consider yourself religious?

10. On a 1 to 7 scale, indicate your agreement with the following statements (1 completely disagree, 7 completely agree):

- Unequal pay for the same work is up to the employer and should be socially acceptable.

- I am generally willing to reward fair behavior, even if it is costly to me.
- I am generally willing to punish unfair behavior, even if it is costly to me.