Pride and persistence: social comparisons in production

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Abstract

Work is ordinary and necessary for most people, but some people work excessively ("work persistence"), seemingly driven by internal forces. We theoretically and experimentally investigate the role of relative performance incentives in causing or exacerbating work persistence. In our setting, agents perform a task over two stages. In the first stage, they can earn prizes, which are allocated either randomly or according to relative performance. Afterwards, they have the opportunity to continue working in a second stage, with payment by piece rate and no competition against others. Our theoretical model of motivated belief updating predicts that agents adjust their beliefs asymmetrically: they attribute their relative performance more to their productivity if they win a prize, and more to luck if they lose. This bias leads winners of the first-stage prize to increase their effort in the subsequent piece-rate stage, but with no corresponding decrease in work effort by losers. Results from a real-effort experiment confirm these predictions: winners' effort in the piece-rate stage is roughly 30 percent higher when earlier bonus prizes had been allocated by performance, compared to when those prizes had been allocated randomly. Losers' effort is also higher – not lower – though this difference is not significant.

Keywords: work persistence; workaholism; tournament; motivated beliefs; asymmetric belief updating; experiment

JEL codes: C92, D44, D72, D82, J31

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1 Introduction

Excessive involvement with work – driven by internal motivations rather than job requirements – is often referred to as *workaholism* (Fassel, 1990; Spence and Robbins, 1992; Porter, 1996; Schaufeli et al., 2008).¹ Since the phenomenon of workaholism was first identified (to our knowledge, by Oates, 1971), it has attracted much attention in the popular press, and the term itself has become a colloquialism.

Although workaholism has been found to be closely related to overabundant labour supply and to adversely affect social welfare (e.g., Nishiyama and Johnson, 1997; Robinson et al., 2001; Andreassen et al., 2013), the attention given to it in economics is relatively scant. Adopting a model of self-signalling, Bénabou and Tirole (2004) argue that individuals may adhere to an exceedingly rigid rule that results in workaholism, because the fear of appearing weak to themselves leads workers to choose a degree of self-restraint to put their willpower to the test. Müller and Schotter (2010) observe workaholic behaviours in an experimental study, and attribute this finding to the possibility that subjects behave in a loss-averse manner.

In this study, we investigate workaholism by developing a simple theoretical model of *work persistence*, which we then test in a lab experiment. We use the term "work persistence" instead of "workaholism" in describing our own study, as our experiment will involve comparisons of work effort across settings, rather than to workers' welfare-maximising levels (which we will not be able to observe). In our model, workers' self-image is related to their perceived productivity relative to the population. As they receive relevant information, they interpret it in a biased manner. Specifically, workers who receive positive performance information will *overestimate* its value as a signal of their productivity, while those receiving negative information will *underestimate* its signalling value. This motivated updating of beliefs implies that the former group of agents will work excessively in subsequent tasks, but without a corresponding decrease in work by the latter group. Results from the experiment support this conjecture.

In our setting, the performance information received by workers is the outcome of a relative comparison: they either earn a bonus prize or they do not, depending on how their performance compared with that of another worker. (The setting is thus similar to a tournament, except that our workers are unaware of the tournament incentives at the time they undertake the task.) Economists (e.g., Lazear and Rosen, 1981; Malcomson, 1984; Waldman, 2012) have long observed that many

¹ According to Sussman et al. (2012), approximately 10 percent of the general US population may be workaholics. Estimates in other studies are even higher (Andreassen et al., 2012). A prevalence of workaholism has been observed especially among management-level workers and in specific sectors (Andreassen et al., 2012; Taris et al., 2012).

labour markets are organised as tournaments, particularly with regard to internal competition for prizes such as bonuses or promotions. The existing tournament literature has focussed on agents' *extrinsic* motivation, before prizes are paid out and when effort can influence the chance of receiving a prize (e.g., Rosen, 1986; Moldovanu and Sela, 2001; Hvide, 2003; Müller and Schotter, 2010). By contrast, our study explores whether and how competition for prizes may shape agents' *intrinsic* motivation, which then drives them to continue working excessively – even after all prizes are paid out.

Our model has one firm and two workers. Workers care about their own income and about their self-image, with the latter influenced by their relative productivity. A more positive self-image is associated with a higher intrinsic motivation, and therefore a lower cost of subsequent effort.

The firm offers each worker a two-stage incentive scheme. Prior to the first stage, each worker is uninformed about their productivity, either in an absolute sense or relative to the other worker. In the first stage, workers compete for a fixed prize, and then the results are announced. In the second stage, each worker (who now has gained some information about their productivity) works under a piece-rate incentive system, with no competitive incentives at all.

The main result of the model is that self-image concerns, combined with biased belief updating, lead workers to distort their second-stage effort provision. Winners – those receiving the prize based on their first-stage outcome – will raise their effort, in order to maintain their current perception of themselves.² The bias also prevents losers (those not receiving the prize) from lowering their efforts in the second stage; rather they reassert their self-image by *maintaining* their efforts. This drives the workers in aggregate – winners and losers combined – to work excessively, relative to the monetary incentives they face.

Despite its intuitive appeal, such a bias is difficult to test in the field, as establishing performance incentives wherein everything else remains equal is nearly impossible in real workplaces. In particular, as Gibbons and Roberts (2013) stress, "incentive pay may cause selection, and the productivity effects of this selection can be as important as the productivity effects of the incentives themselves" (p. 95). For example, an extensive literature on asymmetric learning in labour markets (e.g., Waldman, 1984; Bernhardt, 1995; DeVaro and Waldman, 2012) view promotion tournaments as a signalling device since the promoted worker will have come from the high end of the ability distribution. To sidestep

 $^{^{2}}$ In some of our exposition, including here, we abuse terminology slightly, since "raising" and "lowering" effort imply a change over time. In our theoretical model and experimental environment, by contrast, the relevant comparisons are not over time, but rather are counterfactual between treatments.

this problem, we design a tightly controlled experiment in which we vary how subjects are rewarded – based either on performance rank or on luck – but we do so in a way that largely eliminates selection effects (as detailed below).

The experiment, like the theoretical model, comprises two stages. In the first stage, subjects undertake a simple but tedious "real effort" task.³ They must complete a fixed, known number of units of the task in order to go on to the next stage.

After the first stage ends, subjects are informed about the possibility of receiving bonus prizes. In our *Random* treatment, a die roll determines whether a subject receives the prize for the first-stage task; thus earning the prize is due entirely to luck, and subjects are aware of this. In our *Comparison* treatment, subjects are told (truthfully) that they are paired with another, anonymous, subject, and that they receive the prize if they had completed the first-stage task faster than their matched rival. Unbeknownst to the subjects, half are randomly chosen to be matched to an extremely slow rival from a previous session, and the other half to an extremely fast rival. This minimises selection effects, since earning the bonus from the first-stage task is nearly unrelated to productivity, even in the Comparison treatment. (See Section 2.3 for a further discussion of these features of our experimental design.)

In the second stage, subjects undertake the same real-effort task as in the first stage, but under different incentives. The second-stage payment is based entirely on a known piece rate. Subjects choose how long they wish to continue working, and can stop when they wish, at which time they are paid and can leave the session. There are no differences between the treatments in how the second stage is conducted. This allows us to identify the effect of self-image, by comparing second-stage effort provision between our Random and Comparison treatments with income effects and selection effects arguably controlled.⁴

We observe strong evidence of work persistence in the experimental data. Overall, subjects provide approximately 20 percent more effort in the *second stage* when success in the *first stage* is seen as attributable to performance (in our Comparison treatment) than when it is seen as attributable to luck (in our Random treatment). "Winners" of the first stage prize work significantly longer

³ We use the zero-counting task of Abeler et al. (2011). This task is well suited for our experiment for three reasons, all previously noted by Abeler et al. First, the output and effort of a subject can be easily measured (tables completed and time spent, respectively). Second, a subject's individual productivity θ is unlikely to change substantially across time because the task is straightforward (that is, little learning is involved). Third, we can minimise the possibility of voluntary effort, altruism or reciprocity towards the experimenters because the completed tables per se are obviously worthless for us.

⁴ Nonetheless, our analysis includes instrumental-variable models that account for the minor extent of selection in our Comparison treatment. As demonstrated in Section 3, our results are unaffected by whether or not these methods are used.

during the second stage in our Comparison treatment than winners in our Random treatment. If beliefs were updated in a symmetric way, we would then expect the opposite result for the remaining subjects ("losers"): working less in the second stage in the Comparison treatment than in the Random treatment. In fact, our data indicate nearly the opposite: losers in the Comparison treatment actually work *more* in the second stage than their counterparts in the Random treatment, though this increase is not significant. These results suggest not only that self-image concerns are important in guiding choices of work effort, but also that self-image is updated in a motivated way in response to new information.

1.1 Related literature

Our study makes at least three contributions. First, it complements the economics literature about excessive work, variously referred to as workaholism (Bénabou and Tirole, 2004; Müller and Schotter, 2010), work addiction (Corgnet et al., 2020), and overprovision of work effort (Dohmen and Shvartsman, 2023), and which we are calling "work persistence". Bénabou and Tirole (2004) view workaholism as a compulsive behaviour. They propose a model of self-control wherein people see their own choices as signals of their desire. They claim that workaholism represents a costly form of self-signalling in which "the individual is so afraid of appearing weak to himself that every decision becomes a test of his willpower" (Bénabou and Tirole, 2004, p. 851). Müller and Schotter (2010) are the first (to our knowledge) to associate workaholism with relative-performance incentives. In their experiment, they find that high-ability subjects in a tournament appear unable to stop themselves from working. This result is attributed to the possibility that winning subjects behave in a loss-averse manner in a tournament; high-ability subjects work excessively for fear of not winning the prize. Dohmen and Shvartsman (2023) and Corgnet et al. (2020) examine the connection between excessive work and uncertainty under absolute-performance incentives; the former find that workers work harder in settings with subjective uncertainty (less performance feedback), while the latter find that workers work harder in settings with objective uncertainty (a piece rate given by a probability distribution instead of a known amount). Our study is most similar to that of Müller and Schotter, in that we also examine the connection between excessive work and relative-performance incentives, but our focus is on ex post effort: distinct from behaviour during the time the performance comparisons are being made.

Second, our study connects to the literature on rank-order tournaments. Following the seminal work of Lazear and Rosen (1981), a large volume of literature has investigated settings where every

agent chooses an *ex ante* (i.e., before all prizes are paid out) effort level (e.g., Lazear, 1999; Hvide, 2003; Goltsman and Mukherjee, 2011; Zabojnik, 2012; Imhoff and Krakel, 2016; Boudreau et al., 2016). Santos-Pinto (2010) considers positive self-image in a tournament model, and the effect of biased beliefs, but again focuses on *ex ante* effort provision. Huffman et al. (2022) observe positive correlations between managers' (positively) biased recollections about their past tournament results and overconfident beliefs about their future performance in similar tournaments. In contrast to these studies, we highlight the role played by relative-performance incentives in shaping the intrinsic incentives of agents even after all prizes are paid out. Our experimental evidence shows that the rank-order incentives boost agents' *ex post* effort provision.

Third, this study is related to the literature about asymmetric belief updating. A growing body of evidence from behavioural economics suggests that people have difficulty in forming unbiased opinions about their own abilities (e.g., Svenson, 1981; Gervais and Odean, 2001; Malmendier and Tate, 2005; Englmaier, 2006; Burks et al., 2013). One proposed reason for this phenomenon is that people systematically under-update when they perceive negative signals but apply Bayesian rules when they recognise positive ones. Behavioural-theory models of such asymmetric updating have been suggested by Bénabou and Tirole (2002) and others. Empirically, Eil and Rao (2011) and Möbius et al. (2022) report evidence suggesting that subjects defend their beliefs about their IQ by judging positive signals as more informative than negative ones. Möbius et al. (2022) further find that such biases are mitigated when subjects update about an event not related to their ability, indicating that they are motivated (preference-based) rather than cognitive biases. Zimmerman (2020) observes that positive signals about IQ are persistently incorporated into beliefs, while negative signals have only a transitory effect. Chew, Huang and Zhao (2020) go further, arguing that individuals may forget bad signals or reinterpret them as good ones, or fabricate good signals from nothing. Our experiment adds to this stream of research by finding significant asymmetry between good news and bad news in their impact on effort choices.

2 Theory and experiment

For both the theoretical model and the experiment, we consider a two-stage setting with one (nonstrategic) firm and two workers, called i and j, who perform the same task for the firm.

2.1 Model

Workers' initial wealth is normalised to zero. Worker *i*'s output y_i in a given stage is a function of absolute productivity $\theta_i > 0$ and effort level $l_i \ge 0$:

$$y_i = \theta_i l_i$$

(and similarly for worker j). Both workers' absolute productivities θ_i and θ_j are exogenous and initially unknown. They are independently drawn from the same continuous distribution, and remain unchanged for all stages. The effort level l can be thought of as time spent working; there is no analogous choice of work intensity.

Each worker's stage utility is a function of individual wealth (up to but not including the current stage) $\omega \ge 0$, individual income (from the current stage) $m \ge 0$, self-image concerns s, and effort level l. Specifically, worker i has the stage-utility function

$$U_{i}(\omega_{i} + m_{i}, s_{i}, l_{i}) = \frac{(\omega_{i} + m_{i})^{\gamma_{i}}}{\gamma_{i}} - \frac{l_{i}^{\beta_{i}}}{\beta_{i}(1_{i} + s_{i})}.$$
(1)

The first term of the right-hand side of (1) represents the worker's utility from material gains $(\omega_i + m_i)$, and the second term represents the cost of effort. Here, $\gamma_i \in (0, 1]$ and $\beta_i > 1$ are parameters. Because $\gamma_i \leq 1$, marginal utility of material gains is non-increasing (and strictly decreasing if $\gamma_i < 1$), and because $\beta_i > 1$, the cost of effort is convex. Workers are myopic: they ignore any impacts on later stages when making decisions about their behaviour in earlier stages.⁵

The self-image parameter s_i in (1) captures worker *i*'s "ego utility" (Bénabou and Tirole, 2002; Kőszegi, 2006), which is influenced by their relative productivity $\tilde{\theta}_i$. Specifically, $s_i = k_i \tilde{\theta}_i$, where $k_i \in [0, k_{max}]$, with $0 < k_{max} < 1.6$ We have

$$\widetilde{\theta}_{i} = \begin{cases} 1 & if \quad \theta_{i} > \theta_{j} \\ 0 & if \quad \theta_{i} = \theta_{j} \\ -1 & if \quad \theta_{i} < \theta_{j} \end{cases}$$
(2)

 $^{^{5}}$ We make this myopia assumption in the model to correspond to our experimental setting, where subjects only receive details about the first-stage prize and second-stage task after their decisions for the first stage have been made. Assumptions about this kind of myopia – where individuals partly or completely fail to account for how current decisions affect the future decision-making setting – are common in the behavioural- and experimental-economics literatures. See, e.g., O'Donoghue and Rabin (1999), DellaVigna and Malmendier (2006), Hey and Lotito (2009), and Méder et al. (2017) (some of these studies use the term 'naive' rather than 'myopic').

⁶ This last inequality ensures that $s_i > -1$, so that the last term in (1) is always defined.

for worker *i*. In the case that information about θ_j is unavailable, $\tilde{\theta}_i = 0$.

The parameter k_i characterises the connection between s_i and $\tilde{\theta}_i$: the worker's interpretation of the revealed output rank. When $k_i = 0$, meaning that the worker believes s_i is unaffected by $\tilde{\theta}_i$, we have $s_i = 0$ regardless of $\tilde{\theta}_i$. When $k_i > 0$, a high $\tilde{\theta}_i$ entails that s_i is high, while a low $\tilde{\theta}_i$ entails that s_i is low. In principle, we could allow for the possibility that $k_i < 0$ – for cases where the worker views the signal as negatively correlated with productivity – but as we view negative k as unlikely, we impose the assumption that $k_i \geq 0$.

The marginal cost of effort is decreasing in s_i , meaning that workers with higher self-image concerns are intrinsically more motivated to exert effort. This is mathematically equivalent to Auriol and Renault's (2008) formulation (see their Equation 2), though other ways of modelling self-image concerns also exist in the literature (e.g., as a multiplier of wages by Besley and Ghatak, 2008).

2.2 Two-stage incentive scheme

At the first stage, the firm establishes relative-performance incentives. Namely, the worker with the higher first-stage productivity receives a prize of π money units (in the event of a tie, which occurs with probability zero in equilibrium, one of the workers is randomly chosen to receive the prize). There is also a (possibly zero) absolute-performance component to earnings, $v \ge 0$. So, worker *i*'s first-stage earnings are $m_i = \pi_i + v_i$ if the worker receives the prize, and $m_i = v_i$ if not. Including both a relative- and an absolute-performance component to earnings makes our model correspond more closely to the experimental setting – which contains both of these components – and reflects the ubiquity of non-competitive aspects to remuneration in real workplaces, even highly competitive ones.

At the beginning of the first stage, workers do not know their own absolute productivity θ_i , nor that of the other worker; all that they know is that θ_i and θ_j are drawn from the same distribution. We therefore assume $\tilde{\theta}_i = 0$ (and thus $s_i = 0$) initially for all i.

Note that, since θ_i and θ_j are drawn from the same distribution, each worker is identical before θ is realized. Also, as noted above, s = 0 in the first stage for both workers. As a result, in equilibrium each worker exerts the same (in expectation) first-stage effort \hat{l} and wins the prize with probability one-half. At the end of the first stage, each worker observes their first-stage output \hat{y} and infers their own θ . They also can infer their relative productivity $\tilde{\theta}$ from the revealed output rank. Specifically, from (2) above, $\tilde{\theta}_i$ is equal to 1 if worker *i* is the winner, and -1 if *i* is the loser.⁷

⁷ Setting $\tilde{\theta}_i$ (and thus s_i) initially to zero simplifies the analysis substantially, as it ensures that beliefs about relative productivity in the second stage are based entirely on the first-stage result. Our experimental task – counting zeros in

In the second stage, a piece-rate formula ties a worker's income to their output. Worker *i* exerts effort l_i , which yields the (observable and verifiable) second-stage output y_i , and the worker earns $m_i = \alpha y_i$, where $\alpha > 0$ is the known piece rate. If the worker receives the performance prize as a winner in the first stage, then when the second stage begins, they possess wealth $\omega_i = \pi_i + v_{i,winner}$. By contrast, $\omega_i = v_{i,loser}$ if the worker lost in the first stage. We make the following assumption:

Assumption 1 For a given worker *i*, v_i is weakly larger if the worker is a winner; *i.e.*, $v_{i,winner} \geq v_{i,loser}$.

This would follow from the absolute-performance component of payment being unrelated to productivity (e.g., salary), but would also follow from the amount being strictly increasing in productivity, such as a piece rate, performance reviews by supervisors, and so on.

To illustrate the potential impacts of the relative-performance incentives on the workers' effort provision at the piece-rate stage, we consider three cases.

Case 1: Workers without self-image concern. For this benchmark case, we suppose that there are no self-image concerns that would influence workers' behaviour, i.e., $s_i = 0$. Then, substituting $\omega_i = \pi + v_{winner}$, $m_i = \alpha y_i = \alpha \theta_i l_i$ and $s_i = 0$ into the utility function (1), we have the winner's problem:

$$\max_{l_i} \left\{ \frac{(\pi + \upsilon_{i,winner} + \alpha \theta_i l_i)^{\gamma_i}}{\gamma_i} - \frac{l_i^{\beta_i}}{\beta_i} \right\}.$$
(3)

The winner's first-order condition can be expressed as

$$\alpha \theta_i (\pi + \upsilon_{i,winner} + \alpha \theta_i l_i)^{\gamma_i - 1} = l_i^{\beta_i - 1}, \tag{4}$$

which solution implicitly yields the winner's optimal effort provision $l_{i,winner}^*$.⁸

an array of zeros and ones – is sufficiently artificial that subjects were arguably unlikely to have heterogeneous prior beliefs about their relative productivity (see Section 2.3). However, we note that incorporating heterogeneous priors could change the model's predictions, depending on the nature of this heterogeneity (e.g., are priors correct on average or are workers overconfident) and how new information is handled (how much weight is put on priors relative to winning or not winning the prize, whether priors are updated based on absolute productivity when no prizes are awarded, etc.). Details are available from the corresponding author upon request.

⁸ In (3) and (4), we have implicitly assumed that θ_i remains constant for a given worker from stage 1 to stage 2 (as we do not include time subscripts); such an assumption would be unwarranted if there were (for example) learningby-doing. Our model is not sensitive to this assumption. The occurrences of θ throughout Cases 1-3 refer to the stage 2 value; the stage 1 value only appears indirectly, via s. All that is required is that changes over time in θ do not depend systematically on the treatment or on whether the worker wins the prize; this lack of dependence follows from our experimental design, since subjects are not aware of the treatment or prize until after stage 1 has finished. Also, in Section 3, we will observe that there is no significant evidence of learning-by-doing in the experimental data.

Likewise, substituting $\omega_i = v_{i,loser}$, $m_i = \alpha y_i$ and $s_i = 0$ into (1) yields the loser's problem:

$$\max_{l_i} \left\{ \frac{(\upsilon_{i,loser} + \alpha \theta_i l_i)^{\gamma_i}}{\gamma_i} - \frac{l_i^{\beta_i}}{\beta_i} \right\}.$$
 (5)

The loser's first-order condition is

$$\alpha \theta_i (v_{i,loser} + \alpha \theta_i l_i)^{\gamma_i - 1} = l_i^{\beta_i - 1}, \tag{6}$$

which solution implicitly yields the loser's optimal effort provision $l^*_{i,loser}$.

It is straightforward to show that winners exert weakly less effort in the second stage than losers, other things equal:

Proposition 1 Suppose Assumption 1 holds, and there are no image concerns. Then, a worker's second-stage effort will be weakly less after winning the bonus than after not winning it: for worker i, $l_{i,winner}^* \leq l_{i,loser}^*$.

The intuition behind this result is simple.⁹ The prize awarded at the end of the first stage (weakly) reduces the winner's marginal utility of additional income in the subsequent piece-rate stage.

Case 2: Workers with unbiased self-image concern. Now, suppose that workers have self-image concerns, and that they update their self-image in an unbiased way as they receive new information. In this situation, k (the variable relating relative productivity and self-image) can be viewed as a fixed positive parameter; we assume $k_i = \overline{k_i} \in (0, k_{max})$. For a worker who won the prize (so that $\tilde{\theta_i} = 1$), the utility maximisation problem becomes

$$\max_{l_i} \left\{ \frac{(\pi + \upsilon_{i,winner} + \alpha \theta_i l_i)^{\gamma_i}}{\gamma_i} - \frac{l_i^{\beta_i}}{\beta_i (1 + \overline{k}_i)} \right\}.$$
(7)

For a worker who did not win the prize (so that $\tilde{\theta}_i = -1$), the utility maximisation problem becomes

$$\max_{l_i} \left\{ \frac{(\upsilon_{i,loser} + \alpha \theta_i l_i)^{\gamma_i}}{\gamma_i} - \frac{l_i^{\beta_i}}{\beta_i (1 - \overline{k}_i)} \right\}.$$
(8)

Following the procedures similar to those in Case 1, we find the first-order condition for the winner

$$\alpha \theta_i (\pi + \upsilon_{i,winner} + \alpha \theta_i l_i)^{\gamma_i - 1} = \frac{l_i^{\beta_i - 1}}{1 + \overline{k_i}},\tag{9}$$

⁹ The proofs of all theoretical results in this paper are in Appendix A. Note that Assumption 1 is used in the proof of Proposition 1, but not for any other theoretical results.

and for the loser

$$\alpha \theta_i (v_{i,loser} + \alpha \theta_i l_i)^{\gamma_i - 1} = \frac{l_2^{\beta_i - 1}}{1 - \overline{k_i}},\tag{10}$$

which as before yield the winner's optimal effort provision $l_{i,winner}^{UB}$ and the loser's $l_{i,loser}^{UB}$ implicitly.

It is straightforward to show that these unbiased image concerns lead to *higher* second-stage efforts by winners and *lower* efforts by losers, compared to when there are no image concerns (that is, Case 2 as compared to Case 1):

Proposition 2 Other things equal, winners exert higher second-stage efforts under unbiased image concerns $(l_{i,winner}^{UB} \ge l_{i,winner}^*)$ compared to the case of no image concerns, whereas losers exert correspondingly lower efforts $(l_{i,loser}^{UB} \le l_{i,loser}^*)$.

Intuitively, positive feedback about s decreases the marginal cost of effort and hence reinforces the intrinsic motivation in the ego-driven worker, while negative feedback about s does the opposite.

Case 3: Workers with biased self-image concern. We continue to suppose that workers have self-image concerns, but now they are *biased* in how they update their beliefs in response to new information. By this, we mean that the variable k_i , rather than being an exogenous parameter as above, acts as a choice variable. We will see below that a worker who is successful (wins the prize) will assign a high value to k_i (overestimating the prize as a signal of productivity), while an unsuccessful worker will assign a low value to k_i (under-estimating its signal).

In this situation, the winner's problem is

$$\max_{k_i, \ l_i} \left\{ \frac{(\pi + \upsilon_{i,winner} + \alpha \theta_i l_i)^{\gamma_i}}{\gamma_i} - \frac{l_i^{\beta_i}}{\beta_i (1 + k_i)} \right\},\tag{11}$$

and the loser's problem is

$$\max_{k_i, \ l_i} \left\{ \frac{(\upsilon_{i,loser} + \alpha \theta_i l_i)^{\gamma_i}}{\gamma_i} - \frac{l_i^{\beta_i}}{\beta_i (1 - k_i)} \right\}.$$
(12)

Observing (11) and (12), we notice that given the effort level l_i , the winner can increase utility by adjusting their beliefs about k_i upward while the loser can increase utility by adjusting their beliefs about k_i downward. Intuitively, if a worker outperforms, they have an incentive to make a stronger positive connection between relative standing and self-image concerns in their mind. If the worker underperforms, they will underestimate the connection in order to protect their self-image. So, in equilibrium, the winner is inclined to believe $k_i = k_{max}$ (performance is maximally attributable to ability) while the loser is inclined to believe $k_i = 0$ (performance is unrelated to ability).

This logic allows us to simplify (11) and (12) to

$$\max_{l_i} \left\{ \frac{(\pi + \upsilon_{i,winner} + \alpha \theta_i l_i)^{\gamma_i}}{\gamma_i} - \frac{l_i^{\beta_i}}{\beta_i (1 + k_{max})} \right\}$$
(13)

and

$$\max_{l_i} \left\{ \frac{(\upsilon_{i,loser} + \alpha \theta_i l_i)^{\gamma_i}}{\gamma_i} - \frac{l_i^{\beta_i}}{\beta_i} \right\}.$$
 (14)

The corresponding first-order conditions are

$$\alpha \theta_i (\pi + \upsilon_{i,winner} + \alpha \theta_i l_i)^{\gamma_i - 1} = \frac{l_i^{\beta_i - 1}}{1 + k_{max}}$$
(15)

for winners and

$$\alpha \theta_i (v_{i,loser} + \alpha \theta_i l_i)^{\gamma_i - 1} = l_i^{\beta_i - 1} \tag{16}$$

for losers, which implicitly yield the winner's and loser's optimal effort provisions $l_{i,winner}^{BI}$ and $l_{i,loser}^{BI}$ respectively.

By arguments similar to those underlying Proposition 2, it is easy to show that winners' secondstage effort is higher than in Case 2, and losers' is equal to that in Case 1.

Proposition 3 Winners' second-stage efforts are higher under biased image concerns than under unbiased ones $(l_{i,winner}^{BI} \ge l_{i,winner}^{UB})$. Losers' efforts are the same under biased image concerns as when there are no image concerns $(l_{i,loser}^{BI} \ge l_{i,loser}^*)$.

Thus we have

$$l_{i,winner}^{BI} \ge l_{i,winner}^{UB} \ge l_{i,winner}^*$$

and

$$l_{i,loser}^{BI} = l_{i,loser}^* \ge l_{i,loser}^{UB}.$$

This means that both winners' and losers' second-stage efforts (and therefore the average effort across both types) are at least weakly highest in Case 3.¹⁰

¹⁰ We have assumed that the biased image concerns are only reflected in k, but it is possible that bias can appear elsewhere, such as in beliefs about θ in the second stage. Namely, even though workers can infer their absolute productivity from their first-stage output, they may view this as a sample from an underlying distribution, and believe that the sample understates their true ability. The sign of the effect of a biased estimate of θ on second-stage effort is ambiguous, so such a bias could either reinforce or counteract the effect of k seen in Proposition 3. (Details available from the corresponding author upon request.)

2.3 Experimental design and procedures

We test the model and its implications with a lab experiment, in which we vary the basis for awarding subjects the first-stage prize (output rank or luck). The experiment took place at Hebei University of Economics and Business (Shijiazhuang, China); it was computerised using the z-Tree platform and conducted in Chinese. A total of 164 subjects, from a variety of majors, participated (see Table C for session information). There were no exclusion criteria, but most students were undergraduates (82.3 percent) and majoring in economics or business (79.9 percent). The average age of subjects was 20.4 years, and 64.0 percent were female.

Because our experimental design includes an endogenous session duration (subjects decide when to stop working in the second stage), we took measures to avoid subjects being influenced by others' decisions. In particular, within a session, we led the subjects into the lab in different batches, with a 10-minute interval between successive batches, and randomly assigned a cubicle to each. This meant that subjects' "neighbours" in the room could have begun the session (and hence the second stage) earlier or later, weakening any perceptions of "peer pressure" based on another subject choosing to finish or not to finish.

Before the experiment started, subjects were told that they were not allowed to communicate with each other during the experiment, and that their payoffs were denominated in lab money ("talers"). Then, subjects answered a set of control questions (see Appendix D.1), including a series of smallstakes lottery choices for assessing attitudes toward risk (Holt and Laury, 2002) and losses versus gains (Gächter et al., 2007) and a brief questionnaire on age, gender, and college major.

The main part of the experiment had two stages. First, the subjects read the instructions for the first stage (see Appendix D.2). The instructions specified that there were two stages, and that they would receive instructions for the second stage after they finished the first stage. During the first stage, each subject worked on Abeler et al.'s (2011) simple but tedious "zero-counting" task. They were given 30 tables, each with 150 randomly ordered zeros and ones. For each table, they were asked to count the number of zeros and enter it; if correct, they went on to the next table. For each subject, the elapsed time was displayed on the screen throughout the task, so each was informed about their own time spent in counting the 30 tables. Our measure of individual (absolute) productivity is this counting speed: the number of correctly completed tables per minute during this stage, or equivalently, 30 divided by the time spent working. Subjects' payoffs for the first stage had two components. First, subjects incurred a cost of 1 taler for each minute required (rounded down to the nearest taler) to

complete the 30 units of the task; this was subtracted from a lump-sum participation payment of 100 talers.¹¹ Second, a bonus prize was received by half of the subjects, though subjects were not informed of this possibility until they received the second-stage instructions.¹²

At the end of the first stage, subjects received instructions for the second stage (see Appendix D.3). The second stage began with subjects being informed of the first-stage result: whether or not they had earned the bonus of 90 talers. In the Random treatment, they were told, "we give you a chance to earn the first-stage bonus by playing dice. Please roll the virtual die under the supervision of one experimenter. If the die shows 1, 3, or 5, you will earn zero. If the die shows 2, 4, or 6, you will earn 90 talers". Clearly, which subjects earned the first-stage prize would be attributable to luck in this treatment.

In the Comparison treatment, the instruction was "we give you a chance to earn the first-stage bonus by performance comparison. Specifically, we paired you with another participant (your rival) prior to the first stage. If you spent more time in completing the first-stage task than your rival did, you will earn zero; otherwise, you will earn 90 talers". The instructions did not specify how the pairings were determined. In fact, we purposely selected the "rivals" from a previous pilot session: one rival was quick (finishing the task in 15 minutes and 40 seconds), whereas the other was slow (finishing in 50 minutes and 54 seconds). A given subject was very likely to win if paired with the slow rival, and to lose if paired with the fast rival; indeed, it turns out that only 3 out of 82 (3.7 percent) of subjects in this treatment either beat the fast rival or lost to the slow rival. As one of these two rivals was randomly assigned to each subject, the underlying reason some subjects earned the first-stage prize in the Comparison treatment was therefore nearly the same as that in the Random treatment, minimising the selection issues normally present under rank-order incentives.

We are aware that this aspect of the design may be regarded as deception by some readers, even though it falls into the category of limited disclosure (not providing information that may be relevant to subjects) rather than lying to subjects (stating information that is false).¹³ Many experimental

¹¹ In principle, it was possible for subjects to have negative real-money earnings at the end of the first stage, by taking longer than 100 minutes to complete the task. However, the task was calibrated to make this very unlikely. In fact, subjects on average took only 27 minutes to complete the first-stage task, and the slowest subject finished in 58 minutes.

 $^{^{12}}$ Withholding this information until after the first stage ended was done to ensure that behaviour *in the first stage* is not affected by the presence or absence of relative-performance incentives. In particular, this allows our measure of first-stage productivity (completed tables per minute) to be considered exogenous to the treatment. However, it does raise the possibility of unintended consequences for our second stage, where there is no bonus prize but subjects may have expected there to be one. We discuss the implications of such beliefs in Section 4.

¹³ During the experimental sessions, subjects were told that they could ask questions about the details of the experiment, which would be answered privately. No subject asked for clarification of how they were paired in the Comparison treatment, though we would have answered such a question truthfully. We acknowledge that this aspect of our procedures takes advantage of subjects' lack of awareness that such a matching could take place.

economists tend to support limited disclosure to subjects, especially if it is essential to the design and does not involve outright lying, as is the case here (Hertwig and Ortmann, 2008; Charness et al., 2022).

Subjects then moved to the second stage. The task they undertook was the same as before, but with two changes to the surrounding setting. First, the incentive scheme was changed: subjects received a known and certain piece rate (3 talers per correct answer), with no competition against other subjects. Second, subjects could decide how much and for how long they wanted to work, up to a maximum of 90 minutes. When they wanted to leave, they could click a button on the screen to signal that the experiment was over. As noted above, subjects began this second stage at different times, which should reduce pressure to stop working if other subjects recently stopped, or to continue working if others are continuing.

When a subject chose to finish the experiment, that subject was paid and left. Talers were converted to Chinese yuan (CNY) following the exchange rate of 1 CNY per 10 talers (rounded to the nearest 0.1 CNY).¹⁴ There was a show-up fee of 10 CNY; as noted above, this was sufficient to offset any losses in the first stage. Subjects' earnings averaged 38.2 CNY, and ranged from 7.8 to 80.4 CNY. Sessions took approximately two hours on average, including the time for instructions.

2.4 Hypotheses

Both of our experimental treatments satisfy Assumption 1 in Section 2.2, meaning that the equalities and inequalities in Propositions 1-3 will hold. Using time spent on the second-stage real-effort task as our measure of effort provision, we can test whether subjects process information about their relative productivity in a biased manner by comparing the average second-stage effort provision across treatments and groups.

A slight complication is that while our propositions are expressed as within-person comparisons (e.g., a worker with biased image concerns who wins the first-stage prize would work harder in the second stage in the Comparison treatment than the same person would in the Random treatment), our experiment involves between-person comparisons (whether subjects who win the first-stage prize work harder in the Comparison treatment than other subjects in the Random treatment who also won the prize). However, we can still use these propositions to formulate hypotheses for the experiment, for two reasons. First, the standard experimental practice of random assignment to treatments ensures that

¹⁴ In 2019, the minimum hourly wage in Shijiazhuang was around 16 CNY (2.27 US dollars).

ex ante, distributions of individual-specific parameters (e.g., β and γ) should be similar between our Random and Comparison treatments, and between winners and losers within each treatment. Second, our regressions (see Table 2 and accompanying discussion) will include controls for demographic and attitudinal variables, so that to the extent that any between-subject heterogeneity is captured by these variables, estimated coefficients and marginal effects can be used to assess our hypotheses even if between-treatment heterogeneity was not fully eliminated.

As described in Section 2.2, each subject in the Random treatment has s = 0 in the second stage. Thus, if subject *i* was a winner in the first stage, *i* should exert the second-stage effort $l_{i,winner}^*$, while if *i* was a loser, *i* should exert $l_{2,loser}^*$. In the Comparison treatment, a subject *i* with unbiased self-image concerns should exert the second-stage effort $l_{i,winner}^{UB}$ or $l_{2,loser}^{UB}$ (as a winner or loser respectively), while if *i* has biased self-image concerns, the corresponding second-stage efforts are $l_{i,winner}^{BI}$ and $l_{2,loser}^{BI}$. The hypotheses below are based on subjects with biased self-image concerns, and are stated as directional alternative hypotheses when appropriate (so that the corresponding null hypotheses should be clear from context).¹⁵

Hypothesis 1 In stage 2, time spent working in the Random treatment is higher for losers than winners.

Hypothesis 1 is a test for an income effect. In the Random treatment, winners and losers are drawn randomly, and thus should have similar productivity on average, and because subjects understand that winners are randomly chosen, self-image concerns should not come into play. Hence, and as demonstrated in Proposition 1, winners in that treatment should work weakly less than losers, due to the prize's effect on marginal utility of money (i.e., because $\gamma \leq 1$).

The next hypothesis is a test for self-image concerns:

Hypothesis 2 In stage 2, winners in the Comparison treatment spend more time working than their counterparts in the Random treatment.

The reasoning behind Cases 2 and 3 in Section 2.2 implies that compared to the Random treatment, winners in the Comparison treatment exert more effort in the second stage. The difference between Cases 2 and 3 is that when self-image concern is unbiased (Case 2), losers in the Comparison treatment will exert *less* effort in the second stage compared to their counterparts in the Random treatment, while

 $^{^{15}}$ We state these hypotheses in terms of strict inequalities where applicable. However, it should be noted that in the (unlikely but possible) case that the maximum or minimum second-stage effort level (90 minutes and 0 minutes, resp.) turns out to be binding for a large number of subjects, the inequalities would be weak instead of strict.

biased self-image concerns (Case 3) mean that losers would maintain their efforts in the Comparison treatment. Thus, our final hypothesis is a test of *biased* self-image concerns:

Hypothesis 3 In stage 2, losers in the Comparison treatment spend at least as much time working as their counterparts in the Random treatment.

3 Experimental results

Table 1 presents descriptive statistics from the experiment, including the average time spent working in stage 2 (effort) and the corresponding number of completed tables (output): for each treatment overall, and separately according to the first-stage outcome. (Figure 1 below provides more disaggregated information.)

Treatment	Dandom		Companian	
Treatment:	Random		Comparison	
Stage 1 productivity (units	1.17(0.33)		1.19(0.33)	
completed/minutes worked)				
Stage 2 effort (minutes	58.62(28.81)		71.55(21.99)	
worked)	× ,			
Stage 2 output (units	72.52(42.71)		93.23(42.84)	
completed)				
Stage 2 productivity	1.17(0.37)		1.23(0.39)	
Stage 1 outcome:	Winner	Loser	Winner	Loser
Stage 1 productivity	1.17(0.35)	1.16(0.31)	1.25(0.38)	1.14(0.28)
Stage 2 effort	56.03(26.63)	61.34(31.04)	75.10(18.74)	68.33(24.35)
Percent with maximum	19.0	30.0	43.6	20.9
stage 2 effort (90 minutes)				
Stage 2 output	68.33(41.41)	76.93(44.12)	100.95(42.52)	86.23(42.39)
Stage 2 productivity	1.18(0.40)	1.17(0.35)	1.29(0.37)	1.18(0.40)
Observations	42	40	39	43

Table 1: Descriptive statistics, by treatment and first-stage outcome

Notes: Standard deviations are in parentheses.

As before, we use the terms "winner" and "loser" to mean those who won, or failed to win, the prize at the end of the first stage. Recall from Section 2.3 that in the Random treatment, winners and losers were determined by random draws, making assignment of this role exogenous. In the Comparison treatment, subjects were randomly matched to either a very slow or a very fast subject from a pilot session, and were winners or losers as they outperformed or did not outperform their assigned rival. It was therefore possible that some subjects selected to be "winners" did not actually win (due to being even slower than the slow rival), and similarly for those selected as "losers". This happened for 1 of 38 subjects (2.6 percent) selected to be "winners" and 2 of 44 (4.5 percent) selected

to be "losers". Therefore, while our design did not completely eliminate selection effects, it did reduce them dramatically. Our regression analysis will include robustness checks using instrumental variables (see below for details), but it turns out that our results are not materially affected by whether or not these are used.

Before examining our main research questions, we briefly discuss some of the other results shown in Table 1. First, there is no significant difference between treatments in first-stage productivity (p > 0.20for pooled first-stage winners and losers, two-tailed robust rank-order test), consistent with our random assignment of subjects to treatments.¹⁶ There are also no significant first-stage productivity differences between first-stage winners and losers in the Random treatment (p > 0.20), nor in the Comparison treatment (p > 0.20); this last finding supports our assertion that our implementation of the relativeperformance incentives minimises the issues of selection common in experimental studies.

Next, we observe that productivity tends to increase from stage 1 to stage 2, which is consistent with learning-by-doing (though other explanations for this increase exist, since incentives were also changing from stage 1 to stage 2). However, the increase is small in magnitude in all groups, and is never significant (sign test and Wilcoxon signed-ranks test, p > 0.05 for all groups and p > 0.10 for all groups except for pooled winners and losers in the Comparison treatment). Also, the amount of the increase does not vary significantly between treatments, or between winners and losers within a treatment (robust rank-order test, p > 0.10 for all comparisons).¹⁷

Finally, while all subjects chose to work a positive number of minutes in the second stage (the minimum time spent in this stage was just under 3 minutes), some appear to have been constrained by the second-stage maximum of 90 minutes. However, most subjects (more than two-thirds overall) chose to work less than the maximum, suggesting that the experiment should have sufficient power to detect treatment effects. In our regressions below, we will include Tobit specifications to account for the right-censoring.

We move to our main interest: the analysis of subjects' effort choices, that is, time spent working on the second-stage task.¹⁸ Table 1 indicates clearly higher efforts in the Comparison treatment than in the Random treatment, both overall and for winners and losers separately. The differences are

¹⁶ For sake of comparison, the "slow rival" matched to subjects chosen to be winners in our Comparison treatment had a productivity of 0.59, and the "fast rival" matched to would-be losers had a productivity of 1.92.

¹⁷ Strictly speaking, these non-parametric tests are not appropriate for the Comparison-Winner and Comparison-Loser groups, since these groups' members are determined endogenously. However, the lack of significance continues to hold if we redefine these groups based on whether they were paired with a slow or fast rival, respectively (p > 0.10 for all comparisons).

¹⁸ Results involving second-stage output (tables completed) are broadly similar to those discussed here involving effort; details are available from the corresponding author.

substantial in magnitude: efforts in the Comparison treatment are nearly 20 percent higher overall than in the Random treatment, and the difference is roughly 30 percent for first-stage winners and 10 percent for first-stage losers.¹⁹ Non-parametric tests confirm these treatment effects, with differences between the treatments in aggregate significant (p < 0.01 for pooled winners and losers, two-tailed robust rank-order test). The differences are also significant for winners alone (Comparison winners versus Random winners, p < 0.01). The differences for losers are not significant (p > 0.20), but here it is equally important that there is no difference in the other direction (let alone a significant difference), as would be implied by unbiased self-image concerns (recall Section 2.4). This means that the lack of support for unbiased self-image concerns in our data is not due to a lack of power to detect a difference, but rather suggests that there actually is no difference in the direction predicted by unbiased self-image concerns. Rather, this lack of significant differences is consistent with biased self-image concerns, i.e., with Hypothesis 3.

Figure 1 displays four scatterplots of first-stage log productivity and second-stage log effort, along with fitted OLS trend lines. In the top-left panel, these correspond to the Random and Comparison treatments (grey and red lines, respectively). On average, the red scattered points lie above the grey ones, indicating higher effort in the Comparison treatment. The treatment effect appears to be larger at higher levels of productivity, though we will see in Table 2 below that the interaction between treatment and productivity is not significant. The bottom-left and bottom-right panels indicate similar effects for first-stage winners and losers separately.

Figure 1: Scatterplots of first-stage productivity and second-stage effort. Individual red circles and grey diamonds represent individual subjects; solid red and grey lines represent associated OLS trend lines. Horizontal dashed lines mark the natural logarithm of the time limit $(\ln(90))$.

[Figure 1 here]

Examination of the top-right panel of Figure 1 suggests that income effects in the Random treatment are negligible, as there do not appear to be any meaningful differences in second-stage effort between winners and losers of the first-stage prize. (Below, we examine this result further using regressions.)

As discussed in Case 2 in Section 2.2, unbiased self-image concerns imply that if being informed of their output rank after the first stage makes winners in the Comparison treatment increase their

¹⁹ Midpoint formulas (difference between A and B, divided by average of A and B) used for computing percent differences.

subsequent effort provision (i.e., $l_{winner}^{UB} > l_{winner}^*$), then losers should correspondingly reduce theirs (i.e., $l_{loser}^{UB} < l_{loser}^*$). Since our data do indeed show that winners in the Comparison treatment work more in the second stage than winners in the Random treatment, losers in the Comparison treatment ought to work *less* in the second stage than winners in the Random treatment. However, as noted already, there is no significant difference observed in that direction, and indeed, the sign of the difference is the opposite (though, again, not significant). Our data therefore do not support unbiased self-image concerns; rather they are consistent with subjects' updating their beliefs about k in a biased manner, as discussed in Case 3 of Section 2.2, and consistent with Hypothesis 3.

Table 2 continues the examination of these treatment effects. In columns I-III and V, we report

	(I)	(II)	(III)	(IV)	(V)	(VI)	
	Tobit	Tobit	Tobit	inst. vars.	Tobit	inst. vars.	
Coefficient estimates							
Comparison	0.433^{**}	0.654^{**}			0.584^{*}	0.498^{*}	
	(0.203)	(0.331)			(0.335)	(0.265)	
Win			0.091	0.017	-0.068	-0.037	
			(0.227)	(0.173)	(0.315)	(0.272)	
Comparison x Win					0.360	0.112	
					(0.353)	(0.306)	
log(Prod)	0.692^{**}	0.245	0.672^{**}	0.316	0.662^{**}	0.318	
	(0.281)	(0.385)	(0.297)	(0.216)	(0.289)	(0.227)	
$Comparison \ge \log(Prod)$		0.928^{***}					
		(0.356)					
Marginal effects of Compe	arison var	iable					
Average effect	0.273^{**}	0.468^{**}			0.468^{**}	0.553^{**}	
	(0.120)	(0.189)			(0.185)	(0.244)	
Winners					0.567^{**}	0.610^{**}	
					(0.236)	(0.310)	
Losers					0.379^{*}	0.498^{*}	
					(0.211)	(0.265)	
Marginal effects of Winner variable							
Average effect			0.057	0.017	0.054	0.020	
			(0.142)	(0.173)	(0.125)	(0.161)	
Comparison					0.137	0.076	
					(0.089)	(0.158)	
Random					-0.052	-0.037	
					(0.243)	(0.272)	
Demographic variables?	No	Yes	Yes	Yes	Yes	Yes	
$\mathrm{Adj/Pseudo-}R^2$	0.037	0.108	0.047	0.103	0.106	0.207	

Table 2: Second-stage effort (time working), estimated coefficients and marginal effects

Notes: N = 164. Clustered standard errors (by session) in parentheses. Demographic variables are: age, loss aversion, risk aversion (numerical); female, postgraduate student, business/econ student (indicators). * (**, ***) Significant at 10% (5%, 1%) level.

results of Tobit regressions, where the dependent variable is the natural logarithm of second-stage effort

(time spent working).²⁰ The main explanatory variables are a dummy for the Comparison treatment, a dummy for winning the prize (in either the Random or Comparison treatment) and the natural logarithm of productivity (log(Prod)). (Recall that productivity is defined as the subject's number of correctly completed tables per minute in the first stage.) Column I shows that the effect of the Comparison dummy is positive and significant, indicating more second-stage effort in the Comparison treatment than in the Random treatment. Column II indicates that this effect is robust to whether we include the interaction between the Comparison dummy and productivity, along with demographic and attitudinal variables, in the regression.²¹

In columns IV and VI, we account for the endogeneity of our Win variable in the Comparison treatment, using instrumental-variables (linear) regressions. The instrument we use is an indicator for whether the subject was paired with the slow or fast rival; this variable is exogenous, highly correlated with our Win variable, and has no effect on our dependent variable except via the Win variable (see Table 4 in Appendix B for first-stage estimation results). The similarity of columns IV and VI to (respectively) columns III and V confirms our main results.

Result 1 Subjects in the Comparison treatment spend significantly more time working in the second stage than their counterparts in the Random treatment.

The insignificant coefficient of Win, and corresponding insignificant marginal effects (columns III-VI) confirm that winners' effort provision does not differ from losers' in either the Comparison or the Random treatment. That is, there is no significant income effect (the data do not support Hypothesis 1).

Result 2 There are no significant differences between winners' and losers' second-stage efforts.

Models V and VI allow us to estimate the marginal effects of the Comparison treatment separately for winners and losers. The positive and significant marginal effect on winners indicates that firststage winners in the Comparison treatment increase their second-stage effort significantly more than their counterparts in the Random treatment, and statistically confirms the graphical evidence from

 $^{^{20}}$ OLS regressions, presented in Appendix B, yield similar conclusions, aside from somewhat smaller estimated effect sizes, due to Tobit models' accounting for the downward-censoring of subjects who worked for the upper bound of 90 minutes (about 28 percent of subjects did so) but would have preferred to work longer.

²¹ The demographic and attitudinal variables are left out of the table for space reasons, as their effects are tangential to our main research questions. For information, we mention that the estimated marginal effects of the female and postgraduate student indicators are significantly positive (indicating more time spent on the task in stage 2), while the business/economics student indicator has a significantly negative marginal effect. The age, risk aversion and loss aversion variables have no significant effects. These results are suggestive for future research, though we note that they are not corrected for multiple statistical comparisons.

the bottom-left panel of Figure 1. Moreover, the corresponding marginal effect on losers implies that first-stage losers in the Comparison treatment also increase their second-stage effort provision (relative to their counterparts in the Random treatment), but this increase is not significant at the 5 percent level. The results for both winners and losers are consistent with the predictions derived from the assumption of biased beliefs about k (biased self-image).

Result 3 Winners in the Comparison treatment exert significantly more second-stage effort than their counterparts in the Random treatment.

Result 4 Losers in the Comparison treatment exert more second-stage effort than their counterparts in the Random treatment, but the difference is not significant.²²

With these experimental results in mind, we can re-assess the theoretical model from Section 2. Our Result 1 (differences in aggregate behaviour between Comparison and Random treatments) highlights that self-image matters; that is, the self-image concern parameter s is positive in our Comparison treatment, which in turn implies that the parameter k (relationship between s and the signal of relative productivity) is nonzero for a substantial portion of our subjects. Our Result 2 (lack of a significant income effect) suggests that the parameter γ (exponent of income in the utility function) is close to 1. Our Result 3 (more second-stage effort for winners in the Comparison treatment, compared to the Random treatment) suggests that for winners, k > 0. This positive value is consistent with winners' viewing their winning the prize as a positive signal of productivity, with a corresponding effect on their self-image. Finally, our Result 4 (more second-stage effort in the Comparison treatment) indicates that $k \leq 0$ for losers; since we cannot reject the null that k = 0, the result is consistent with losers' viewing failing to win the prize as unrelated to productivity.

4 Discussion

We theoretically and experimentally examine *work persistence*. Workers make effort decisions under a two-stage incentive scheme; they compete for a fixed prize in the first stage, and face a piece-rate system in the second stage. Our theoretical model allows for motivated belief updating: workers may interpret information from their first-stage relative performance asymmetrically as a signal of their underlying productivity. Workers who "won" (received the prize) in the first stage *overestimate*

 $^{^{22}}$ Note that Results 3 and 4 together imply Result 1. We have kept them as distinct results to aid our assessment of the model, at the end of this section.

would the usefulness of this positive signal, and thus overestimate their productivity. Those who "lost" (did not receive the prize) would *underestimate* the usefulness of this negative signal, and thus also overestimate their productivity. As a result, workers in aggregate (winners and losers combined) should tend to work excessively afterwards under the subsequent piece-rate system.

We test for work persistence with an experiment that implements the theoretical setting. In the first stage, subjects are assigned a task with a fixed workload and a fixed potential prize, which half of them will earn. In the second stage, they perform the same task but choose how many units of the task they undertake under a piece-rate system, with no competition against others.

Our treatments differ only in the feedback subjects receive between stages. In our Random treatment, subjects are informed that a die roll determines who wins the prize. In the Comparison treatment, subjects are informed that they were assigned to a rival at the beginning of the experimental session, and only those who outperformed their rivals are rewarded. However, these rivals were chosen from previous sessions: one rival was very quick and the other was very slow (so respectively, the subject was either almost certain to lose or almost certain to win). The rival was assigned randomly, making this treatment nearly identical to the Random treatment except for the relative performance framing, and minimising issues of selection and endogeneity.

The experimental data provide support for our "work persistence" hypotheses. Overall, subjects in the Comparison treatment work significantly harder in the second stage than their counterparts in the Random treatment. This result is seen in both winners and losers (those in the Comparison treatment work harder than their counterparts in the Random treatment), though the difference is only significant for winners. Our results are robust to controlling for the (minor) selection issues in the Comparison treatment.

These results therefore support the model of motivated belief updating we developed. The model posits that social comparisons activate self-image concerns. When subjects receive feedback from a relative-performance incentive, their beliefs about their own productivity are updated in a biased, asymmetric manner. Winners in the Comparison treatment receive positive feedback, which they over-attribute to their own above-average productivity. This "boost" in their self-image lowers the subjective cost of working more, leading them to exert extra effort in the second stage.

If losers in the Comparison treatment reacted to their (negative) feedback in an analogous way, they would respond by exerting lower effort in the second stage. However, they do not do so. Instead, consistently with our model, they largely dismiss the negative signal – attributing the loss to factors like bad luck rather than below-average productivity. In this way, losers preserve their self-image. Consequently, they maintain a similar level of effort in the second stage.

In the Random treatment, by contrast, subjects know that the bonus prize is awarded via a die roll, so that both winners and losers understand the result is not based on their performance. Hence they do not update their beliefs about their productivity after learning the prize outcome, meaning that their self-image does not change, and they do not raise or lower their effort in the second stage. Combining these implications, winners in the Comparison treatment work harder than winners in the Random treatment, while losers in the Comparison treatment work no more or less hard than those in the Random treatment – exactly what the model predicted.

Our results complement the existing literature about excessive work (e.g., Bénabou and Tirole, 2004; Müller and Schotter, 2010). Like Müller and Schotter (2010), we associate excessive work with relative-performance incentives, but our emphasis is on how self-serving attribution bias induces expost work persistence behaviours (i.e., after the relative-performance incentives are no longer present), in contrast to the other studies' focus on behaviour while relative-performance incentives are still in effect. Our study is also germane to the general literature on tournaments (e.g., Lazear and Rosen, 1981; Lazear, 1999) and highlights how tournaments shape agents' need to work, even after all prizes have been awarded. Finally, our study contributes to the literature about motivated beliefs (e.g., Eil and Rao, 2011; Möbius et al., 2022) by finding significant asymmetry between good news and bad news in the domain of real-effort choices.

We close by mentioning some limitations and possible extensions of this study. Firstly, while we believe our theoretical model incorporates self-image into effort choice in a natural way, other model structures may yield different theoretical implications. In particular, our model assumes that the benefit of money and the disutility of labour are additively separable, and that the disutility of labour is reduced as self-image improves. Secondly, we have implemented motivated belief updating as overestimating the value of positive signals and underestimating that of negative signals. Other authors have argued for different explanations: e.g., as noted in Section 1.1, Zimmerman (2020) proposes that positive signals are more persistently incorporated into beliefs than negative signals, while Chew, Huang and Zhao (2020) suggest that negative signals may be ignored or reinterpreted as good signals.

Thirdly, our model and experiment are one-shot; it is possible that work persistence would be attenuated if workers repeatedly faced relative-performance incentives followed by absolute-performance incentives. This conjecture is in the spirit of Gervais and Odean (2001), who argue that agents become better with experience at assessing their own abilities, but take too much credit for favourable outcomes when they are inexperienced. Along the same lines, workers faced the same task in both stages of our setting, which may have highlighted the value of the first-stage outcome as a signal for the second stage. This value might have been reduced, or even reversed, if the second-stage task were different in nature from the first-stage task.

Fourthly, other explanations can be proposed for our experimental results. One might be called an "expected surprise" explanation. As noted in footnote 12, our withholding of information about the first-stage prize (and how it is determined) until the end of the first stage could lead some subjects to expect there to be a similar second-stage prize. Such an expectation, in turn, would imply that subjects in the Comparison treatment may expend more second-stage effort than those in the Random treatment, since the former would believe the prize to be output-based and the latter would believe it to be luck-based. However, this explanation cannot characterise the differences we observe between winners and losers. In the experiment, winners in the Comparison treatment increase their effort more than losers do, while an expectation of a second-stage prize ought to cause losers to increase their effort more (since their first-stage result left them "out of the money" and needing to improve to get the presumed prize, in contrast to winners who may believe that maintaining their previous effort would continue to suffice).

According to a second, "social context" explanation, rank-based incentives from stage 1 of the Comparison treatment make the task more meaningful because of the added social context (Ryan and Deci, 2000), increasing subjects' intrinsic motivation to work on the task in stage 2 of that treatment. A third, "procedural justice" explanation posits that subjects in the Random treatment would view a bonus paid based on pure luck as procedurally unfair, leading them to exert less effort in the second stage of that treatment. Either of these explanations would characterise the differences we observe between pooled winners and losers in the Comparison treatment and pooled winners and losers in the Random treatment. However, they would not explain the heterogeneity in treatment effects we observe between winners and losers. The social context explanation would seem to apply equally to winners and losers, predicting comparable treatment effects, while the procedural justice explanation would imply a larger effect on losers than winners (as with our "expected surprise" explanation above), since aversion to unfavourable inequity is typically substantially stronger than aversion to favourable inequity. (This is the rationale behind the formulation of behavioural models of inequity aversion such as those of Fehr and Schmidt, 1999, and Bolton and Ockenfels, 2000.) As noted above, we observe a larger effect on winners than on losers in our experimental data, suggesting that at most, these alternatives only partially describe what motivates our subjects.

Lastly, and related to the previous point, our experiment was designed as a behavioural test of biased belief updating (where we use "behavioural" in the traditional sense of using incentivised actions to understand decision making, rather than the more recent sense implicit in the term "behavioural economics"). We acknowledge that a limitation of our approach is that even when the data support our theory, other explanations (those mentioned above, or others) can be proposed, and it is difficult to definitively conclude that one is correct. Collecting decision-process and other data in addition to the behavioural data could have shed light on how and why subjects made the effort choices we observed. Eliciting beliefs under a proper scoring rule could provide evidence of where subjects thought they belonged in the population distribution of productivity in the task, and whether and how this changed after prizes were awarded. A post-experiment survey could get subjects' own views of whether the incentive system was fair, whether the task was worthwhile, and so on. Face reading and neuroeconomic techniques such as magnetic resonance imaging could capture information about subjects' emotions when they receive information and make decisions. We encourage future research along these lines to further improve our understanding of why people work excessively.

Declarations

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A Proofs

Proof of Proposition 1: Consider the equation

$$l_i^{\beta_i-1} = \alpha \theta_i [x\pi + xv_{i,winner} + (1-x)v_{i,loser} + \alpha \theta_i l_i]^{\gamma_i-1}.$$
(17)

Note that when x = 1 and x = 0, (17) reduces to the first-order conditions for winners and losers respectively (see (4) and (6) in the main text), and recall from Section 2 that $v_{i,winner} \ge v_{i,loser}$, $\beta_i > 1 \ge \gamma$, and $\alpha, \theta_i > 0$.

Differentiating implicitly (with respect to l_i and x), we have

$$(\beta_i - 1)l_i^{\beta_i - 2}dl_i = \alpha \theta_i (\gamma_i - 1)[x\pi + x\upsilon_{i,winner} + (1 - x)\upsilon_{i,loser} + \alpha \theta_i l_i]^{\gamma_i - 2}[(\pi + \upsilon_{i,winner} - \upsilon_{i,loser})dx + \alpha \theta_i dl_i]$$

which simplifies to

$$\frac{dl_i}{dx} = \frac{\alpha \theta_i (\gamma_i - 1)(\pi + \upsilon_{i,winner} - \upsilon_{i,loser}) [x\pi + x\upsilon_{i,winner} + (1 - x)\upsilon_{i,loser} + \alpha \theta_i l_i]^{\gamma_i - 2}}{(\beta_i - 1)l_i^{\beta_i - 2} - \alpha \theta_i (\gamma_i - 1) [x\pi + x\upsilon_{i,winner} + (1 - x)\upsilon_{i,loser} + \alpha \theta_i l_i]^{\gamma_i - 2}}$$
(18)

On the right-hand side of (18), the numerator is non-positive (since $\gamma_i \leq 1$ and $v_{i,winner} \geq v_{i,loser}$). The first term of the denominator is positive (since $\beta_i > 1$), and the second term is non-positive, so the entire denominator is strictly positive, making the entire right-hand-side non-positive. So l_i is weakly decreasing in x, meaning that $l_{i,winner}^* \geq l_{i,loser}^*$. (Note that this inequality is strict if $\gamma_i < 1$.)

Proof of Proposition 2: For winners, consider the equation

$$\alpha \theta_i (\pi + \upsilon_{i,winner} + \alpha \theta_i l_i)^{\gamma_i - 1} = \frac{l_i^{\beta_i - 1}}{1 + k_i}.$$
(19)

This equation yields the first-order conditions for winners in Case 1 (when $k_i = 0$) and in Case 2 (when $k_i = \overline{k}_i$), and as noted above, we have $\beta_i > 1 \ge \gamma$ and $\alpha, \theta_i > 0$.

Differentiating implicitly (with respect to l_i and k_i), we have

$$(\alpha\theta_i)^2(\gamma_i-1)(\pi+\upsilon_{i,winner}+\alpha\theta_i l_i)^{\gamma_i-2}dl_i = (\beta_i-1)l_i^{\beta_i-2}[1+k_i]^{-1}dl_i + l_i^{\beta_i-1}[1+k_i]^{-2}dk_i,$$

which can be written as

$$\frac{dl_{i,winner}}{dk_i} = \frac{l_i^{\beta_i - 1}}{(\beta_i - 1)[1 + k_i]l_i^{\beta_i - 1} - (\alpha\theta_i)^2(\gamma_i - 1)[1 + k_i]^2(\pi + \upsilon_{i,winner} + \alpha\theta_i l_i)^{\gamma_i - 2}}.$$

The numerator of the right-hand side is positive, as is the first term of the denominator. The second term of the denominator is non-positive (since $\gamma_i \leq 1$), making the entire denominator, and thus the entire right-hand side, positive.

Hence we have

$$\frac{dl_{i,winner}}{dk_i} > 0.$$

Since $k_i = 0$ gives us Case 1 and $k_i = \overline{k_i} > 0$ gives us Case 2, we can conclude that l_i is higher for winners in Case 2 compared to Case 1: $l_{i,winner}^{UB} > l_{i,winner}^*$.

The corresponding proof for losers is similar. Consider the equation

$$\alpha \theta_i (\pi + \upsilon_{i,winner} + \alpha \theta_i l_i)^{\gamma_i - 1} = \frac{l_i^{\beta_i - 1}}{1 - k_i},\tag{20}$$

which yields the first-order conditions for losers in Case 1 (when $k_i = 0$) and in Case 2 (when $k_i = \overline{k_i}$).

Differentiating implicitly,

$$(\alpha\theta_i)^2(\gamma_i-1)(\upsilon_{i,loser}+\alpha\theta_i l_i)^{\gamma_i-2}dl_i = (\beta_i-1)l_i^{\beta_i-2}[1-k_i]^{-1}dl_i + l_i^{\beta_i-1}[1-k_i]^{-2}dk_i,$$

which can be written as

$$\frac{dl_{i,loser}}{dk_i} = \frac{l_i^{\beta_i - 1}}{(\alpha \theta_i)^2 (\gamma_i - 1)[1 - k_i]^2 (\upsilon_{i,loser} + \alpha \theta_i l_i)^{\gamma_i - 2} - (\beta_i - 1)[1 - k_i] l_i^{\beta_i - 1}}.$$

The numerator of the right-hand side is positive, as is the second term of the denominator. The first term of the denominator is non-positive (since $\gamma_i \leq 1$), making the entire denominator, and thus the entire right-hand side, negative.

Hence we have

$$\frac{dl_{i,loser}}{dk_i} < 0.$$

Since $k_i = 0$ gives us Case 1 and $k_i = \overline{k_i} > 0$ gives us Case 2, we can conclude that l_i is lower for losers in Case 2 compared to Case 1: $l_{i,loser}^{UB} < l_{i,loser}^*$. Proof of Proposition 3: For winners, the first-order condition in Case 3 is

$$\alpha \theta_i (\pi + \upsilon_{i,winner} + \alpha \theta_i l_i)^{\gamma_i - 1} = \frac{l_i^{\beta_i - 1}}{1 + k_{max}},\tag{21}$$

which is just (19) above with $k_i = k_{max}$. From the proof of Proposition 2, $dl_{i,winner}/dk_i > 0$. Since $l_{i,winner}^{UB}$ is based on $k_i = \overline{k}_i < k_{max}$ and $l_{i,winner}^{BI}$ is based on $k_i = k_{max}$, we have $l_{i,winner}^{BI} > l_{i,winner}^{UB}$.

For losers, the first-order condition in Case 3 is

$$\alpha \theta_i (v_{i,loser} + \alpha \theta_i l_i)^{\gamma_i - 1} = l_i^{\beta_i - 1}, \tag{22}$$

which is (20) above with $k_i = 0$, and thus identical to the corresponding condition for $l_{i,loser}^*$, meaning that $l_{i,loser}^{BI} = l_{i,loser}^*$.

B Additional regression results

	(VII)	(VIII)	(IX)	(X)
Coefficient estimates	()	()	()	
Comparison	0.345^{*}	0.505		0.486
1	(0.161)	(0.272)		(0.281)
Win			0.060	-0.013
			(0.178)	(0.282)
Comparison x Win				0.142
-				(0.311)
log(Prod)	0.395^{*}	0.204	0.419^{*}	0.407*
	(0.168)	(0.289)	(0.192)	(0.189)
Comparison $x \log(Prod)$	· · · ·	0.433		
		(0.325)		
Marginal effects of Comp	arison var	iable		
Average effect	0.345^{*}	0.561^{*}		0.557^{*}
5	(0.161)	(0.250)		(0.251)
Winners				0.629^{*}
				(0.309)
Losers				0.486
				(0.280)
Marginal effects of Winne	er variable			
Average effect			0.060	0.057
6			(0.178)	(0.164)
Comparison			· · · ·	0.129
-				(0.148)
Random				-0.013
				(0.282)
Demographic variables?	No	Yes	Yes	Yes
$\widetilde{Adj/Pseudo} R^2$	0.077	0.207	0.099	0.204

Table 3: OLS results corresponding to Table 2

Notes: N = 164. Clustered standard errors (by session) in parentheses. Demographic variables are: age, loss aversion, risk aversion (numerical); female, postgraduate student, business/econ student (indicators). * (**, ***) Significant at 10% (5%, 1%) level.

Table 4: First-stage estimation results for instrumental-variable regressions in Table 2

Dependent variable: Win	(IV)	(VI)
Paired with slow	4.178^{***}	4.184***
rival (indicator)	(0.492)	(0.496)
Constant	-1.973^{***}	-1.971^{***}
	(0.299)	(0.297)

Notes: N = 164. Coefficient estimates, with clustered standard errors (by session) in parentheses. * (**, ***) Significant at 10% (5%, 1%) level.

C Session information

Session	Treatment	Number of	Number of	Number of
		subjects	winners	losers
1	Random	13	6	7
2	Random	22	11	11
3	Random	18	8	10
4	Comparison	19	11	8
5	Comparison	19	11	8
6	Comparison	27	18	9
7	Comparison	17	3	14
8	Random	29	15	14

D Experimental instructions

Below are the instructions of the experiment translated into English.

Thank you for participating! Please switch off your mobile phone and do not talk to other participants during the experiment. If you have any questions, raise your hand, and one of the instructors will answer your question. For your arrival on time, you receive 10 yuans that will be paid to you at the end of the experiment. In this classroom, different participants start and complete their tasks at different times.²³ So, some participants will come in and go out now and then. Please focus on your own tasks, and ensure that your decisions are NOT influenced by the others. Your answers in the experiment stay completely anonymous. The computer stores all the information you have given for analysis only.

In this experiment you will be asked to carry out several tasks for which you can earn a number of talers. These talers will be translated into your payment at the end of the experiment at the exchange rate of:

D.1 Questionnaire and lottery decision tasks

Before the experiment starts, you are asked to fill in a short questionnaire and two lottery decision sheets.

Questionnaire. There are several questions about your gender, date of birth and college major. Please take your time and fill the questionnaire in truthfully. The answers you give have no impact on your payments, but they are important for our scientific analysis.

The first lottery. Your screen will show you 6 rows. Each row shows you one lottery. You have to decide on either rejecting or accepting a lottery.²⁴ In each lottery, the losing prize is varied between 10 and 35 Talers, and the winning price is fixed at 30 talers. After the experiment, the computer will choose one of these six lotteries for pay. If you have accepted the lottery, a virtual coin will be tossed to determine your payoff. If you have rejected a lottery, both winning and losing prizes are zero.

 $^{^{23}}$ Here, we try to eliminate the effect of peer pressure. For example, if we let all participants start at the same time, in the second stage, some of them may not stop working until someone else leaves the classroom.

²⁴ According to Gächter et al. (2007), the number of rejected lotteries measures the degree of loss aversion—a subject with higher loss aversion should reject more lotteries.

Example 1 Suppose the computer picks Lottery No.3. If you have rejected Lottery No.3, you earn zero; if you have accepted Lottery No.3, the computer will randomly select a number N between [0, 1]. When $N \leq 0.5$, you will lose 20 talers; otherwise, you will earn 30 talers.

Lottery	Accept	Reject
1. You lose 10 talers with probability 50%; you win 30 talers with probability 50%.		
2. You lose 15 talers with probability 50%; you win 30 talers with probability 50%.		
3. You lose 20 talers with probability 50%; you win 30 talers with probability 50%.		
4. You lose 25 talers with probability 50%; you win 30 talers with probability 50%.		
5. You lose 30 talers with probability 50%; you win 30 talers with probability 50%.		
6. You lose 35 talers with probability 50%; you win 30 talers with probability 50%.		

The following is the decision sheet you need to fill in.

The second lottery. Your screen will show you 10 rows. In each row, two options are displayed: Option A and B. You need to decide which of the two options you prefer. After the experiment, the computer will randomly pick one of the 10 rows. For that row, the computer then randomly determines your earnings for the Option (A or B) you chose.

Example 2 Suppose the computer picks Row No.3 and you prefer Option A. Then, the computer will randomly select a number N between [0, 1]. When $N \leq 0.3$, you will earn 20 talers; otherwise, you will earn 16 talers.

Example 3 Suppose the computer picks Row No.7 and you prefer Option B. Then, the computer will randomly select a number N between [0, 1]. When $N \leq 0.7$, you will earn 38.5 talers; otherwise, you will earn 1 taler.

The following is the decision sheet you need to fill in.



D.2 The instruction for the first stage

The experiment consists of two stages. You can only enter the second stage after completing the tasks specified at the first stage. Now, please read the experimental instruction for the first stage carefully.

Task description At this stage, you need to correctly count the number of zeros in a series of tables.

The following figure shows the work screen you will use later. Enter the number of zeros into the box on the right side of the screen. After you have entered the number, click the OK-button. If you enter the correct result, a new table will be generated. If your input was wrong, you have two additional tries to enter the correct number into the table. You therefore have a total of

three tries to solve each table. You only have to enter the correct answer once in three chances to be judged as correct. If all three inputs are incorrect on the same table, a new table will then be generated but the number of completed tables won't increase.

Figure 2: Work screen of counting zeros in the first stage

[Figure 2 here]

Task requirements At this stage, you must correctly count 30 tables before you can proceed to the second stage of the experiment. We do not have a time restriction for the 30-table task. Nevertheless, once the 30-table task starts, we will deduct 1 taler from your final payment every minute you spend in the task, i.e. the less time consumed, the less talers will be deducted.

Example 4 If you complete the 30-table task in 9 minutes and 35 seconds, you will be deducted 9 talers.

Example 5 If you complete the 30-table task in 25 minutes and 46 seconds, you will be deducted 25 talers.

Please raise your hand after completing the 30-table task. We will provide you the instruction for the second stage.

D.3 The instruction for the second stage

Now, you are at the second stage. Please read the following instruction for this stage carefully.

• Subjects in the random treatment read the following sentences:

As you have completed the 30-table task at the first stage, we give you a chance to earn the firststage prize by playing dice. Please roll the virtual dice under the supervision of one experimenter. If the dice shows 1, 3, or 5, you will earn zero. If the dice shows 2, 4, or 6, you will earn 90 talers.

Regardless of whether you earn the first-stage prize or not, you are eligible to enter the second stage of the experiment. The second-stage payment depends on how many tables do you solve correctly at this stage. You will receive 3 talers for each table you solved correctly.

• Subjects in the tournament treatment read the following sentences:

As you have completed the 30-table task at the first stage, we give you a chance to earn the firststage prize by performance comparison. Specifically, we paired you with another participant (your rival) prior to the first stage. If you spent more time in completing the first-stage task than your rival did, you will earn zero; otherwise, you will earn 90 talers.

Regardless of whether you earn first-stage prize or not, you are eligible to enter the second stage of the experiment. The second-stage payment depends on how many tables do you solve correctly at this stage. You will receive 3 talers for each table you solved correctly.

Task description At this stage, you need to correctly count the number of zeros in a series of tables which is similar with that at the first stage. The following figure shows the work screen you will use later. However, it should be noted that there are two differences between the first and second-stage tasks.

(1) At this stage, there is an immediate reward for each table solved correctly. Enter the number of zeros into the box on the right side of the screen. After you have entered the number, click the OK-button. If you enter the correct result, you will get 3 talers and a new table will be generated. If your input was wrong, you have two additional tries to enter the correct number into the table. If you enter three times a wrong number for a table, 3 talers will be subtracted from your earnings and a new table will then be generated.

(2) At this stage, there is no requirement for the number of tables should be solved, nor deduction according to the minutes you used. In other words, you are free to choose how many tables you want to solve and how long it will take. However, the maximum working time is 90 minutes. When time is up, the computer will automatically end the task and the experiment is over. If you want to stop working before the 90-minute deadline, please click the red button "the whole experiment is over".

Figure 3: Work screen of counting zeros in the second stage

[Figure 3 here]

Example 6 Suppose you correctly solve 60 tables, but miscounted 2 tables, in the second stage. Your second-stage payment is $(60-2) \times 3 = 174$ Talers.

Total payment You will be paid anonymously after the experiment. Your total earnings in *talers* = Lottery payment + first-stage payment + second-stage payment + showup fee. Your total earnings in China yuan = Total earnings in talers $\times 0.1$.