An Experimental Study of Incentive Contracts for Short- and Long-term Employees*

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Abstract

Motivated by real-world observations of different contract offers, we conduct a lab experiment to examine a principal's contract choice and agent effort in both long- and short-term employment relationships, implemented as one-shot and repeated games. We find that a piece-rate contract has the strongest incentive effect on short-term agents' effort and is the principals' dominant choice. Nevertheless, the bonus contract works almost as well as the piece-rate contract for long-term relationships, but not so well for short-term relationships. In addition, the bonus contract's effect on effort is mainly driven by the bonus component, suggesting that a fixed wage alone is not an effective mechanism to improve workers' performance.

Keywords: Incentive Contract; Repeated Game; Experiment

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1. Introduction

One of the pervasive trends in the American workplace in recent decades is the increase in the number of temporary workers (Hatton, 2011). These temporary workers are also known as contingent or contract workers. In the United States, since the recession in 2008, the number of temporary workers has increased to nearly 2.7 million-the highest on government record since 1990. At the same time the hiring of regular employees has stagnated (Associated Press, 2013). Large companies, such as Walmart, have been hiring only temporary workers in many of their US stores, and temporary workers have to reapply for their jobs after 180 days (Smith, 2013). Moreover, the emergence of nontraditional labor markets, such as online markets, also generates an increasing demand for temporary workers (Hatton, 2011).

The relationship between an employer and regular employees is often characterized as long-term and mutually invested, whereas the relationship between an employer and temporary workers is generally described as short-term, transactional, and lacking commitment (Lepak and Snell, 1999). Thus, the management of regular versus temporary workers also differs in the selection, training, compensation, and benefits (Koene and Riemsdijk, 2005). Many studies in the US have shown that temporary workers are paid lower wages and receive fewer benefits than regular long-term employees (Segal and Sullivan, 1997; Hippie, 2001). However, there is little evidence of whether firms provide different pay schemes for short- and long-term employees.

Fortunately, the Global Investment Survey conducted in China by the World Bank provided some initial evidence for pay scheme differences between short- and long-term employees in Chinese manufacturing firms. Between 2005 and 2006, as part of the World Bank's Global Investment Climate Project, the World Bank's China Division and the Enterprise Survey Division of China's National Bureau of Statistics jointly conducted a survey of 12,400 manufacturing firms from 120 cities in China. The sample firms were selected via stratified random sampling, thus representing the population. The sample firms covered all 31 sub-categories of the manufacturing industry from textile, clothing, and furniture to computer and electronic equipment manufacturing. According to the survey, 62% firms reported that they hired both temporary and regular production workers in their operations. They also reported the percentage of various pay forms used in the total compensation for temporary and regular employees. Based on the data, different pay contracts were identified. For example, "piece-rate only" refers to companies which only implement piece-rate contracts for the corresponding type of workers (e.g., piece-rate was 100% of the total pay).

Using the data from this survey, we found different pay schemes were being offered to regular and temporary workers. The five most popular contracts are: fixed-wage only, fixed-wage plus a bonus, piece-rate only, fixed-wage plus a piece-rate, fixed-wage combined with both a bonus and a piece-rate, and a group labeled as "others."[1] As shown in Table 1a, the distribution of companies by payment scheme is significantly different for regular and temporary workers (p=0.000, chi-square test). In particular, the dominant payment scheme is different: 38% of the surveyed companies use the "piece-rate only" contract for their temporary production workers, whereas 33% of them choose the "fixed-wage with a bonus" contract for regular workers. Regarding wages and working hours for temporary and regular workers, companies choosing the "fixed-wage with a bonus" offer significantly higher wages and workers work fewer hours compared to companies using other pay schemes. In Table 1b, we present the same data for the labor-intensive textile industry, in which "piece-rate only" is a more popular pay scheme than in other manufacturing industries for both regular and temporary workers.[2] Similar to the pattern shown in Table 1a, companies in the textile industry are much more likely to use "piece-rate only" for temporary workers than for regular workers (48% vs 17%).

The survey data shows that firms are inclined to offer a fixed wage with a bonus to regular workers and a piece-rate to temporary workers. Motivated by this empirical observation, we conduct a real-effort lab experiment to study the principals' contract choice, wage offers and agents' effort, under a controlled experiment setting, so as to confirm whether the patterns observed from the firm survey can hold in the experiment.

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[1] The survey and data are available from the authors. The accurate definition of different payment schemes was not given in the questionnaire. It is possible that the respondents did have different understandings of what a payment scheme meant. Nevertheless, since these payment schemes are popular in business, there is usually a common understanding of their meaning. For example, in China's business context, a bonus is a performance-based payment that is given to employees after performance evaluation is conducted at the end of a year, quarter, or month.

[2] The contract choice depends on both product feature and the degree of cooperation required in the production. The piece-rate contract is only applicable when the work is quantitative in nature and cooperation is not critically needed. We acknowledge that sometimes, in order to improve cooperation between experienced and in-experienced employees, firms prefer a bonus contract over fixed payment and piece-rate. We thank an anonymous referee for pointing this out.

Apart from being motivated by survey data, our study is also motivated by the extensive literature on the comparison of piece-rate and fixed-wage pay schemes, e.g. Lazear (2000), Paarsch and Shearer (2000), Freeman and Kleiner (2005). More recent studies by Fehr et al. (2007) and Casari and Cason (2013) extended the contract comparison to include a bonus contract that contains a fixed amount of the upfront wage and a bonus after principals observe the agent's performance. In both Fehr et al. (2007) and Casari and Cason (2013), subjects are randomly rematched so that they will not repeatedly interact with other players. Our study implements both random-rematch and fixed-match protocols, and our novelty lies in comparing the behaviors under these two matching protocols. Relating to the survey evidence, we consider random-match a proxy of a short-term relationship between a principal and an agent, and fixed-match long-term relationship.

Corroborating the survey evidence, in the experiment we find that principals converge to choosing a piece-rate contract in randomly rematched games (short-term relationships), even though the principals do attempt to choose the bonus contract in early rounds. In fixed-match games (long-term relationships), principals are almost equally likely to choose the bonus contract or piece-rate contract even in the later rounds. The "success" of the bonus contract is mainly due to cooperation between principals and agents in repeated interactions in terms of high wages and high effort.

The rest of the paper is organized as follows. First, we review the relevant literature in Section 2. In Section 3, we describe the theoretical analysis and experimental design. Experimental results are reported in Section 4. Finally, in Section 5 we discuss the results and conclude the paper.

2. Related Studies

Our study is closely related to the literature on piece-rate pay schemes. The piece-rate is a popular form of an incentive pay. Compared with fixed wages, the piece-rate has an incentive effect on workers' productivity, as well as a sorting effect of attracting and retaining high-ability workers (Lazear, 1986). In management literature, piece-rates are used whenever the nature and observability of the task are allowed, while in contract theory literature there is no consensus. Carroll (2015) and Carroll and Meng (2016) provided another theoretical explanation for the popularity of piece-rates in practice. In a standard moral hazard model, under the assumption of non-observability of effort and given the principal's limited knowledge about what agents can or cannot do, piece-rates (i.e., the linear contract referred to by Carroll (2015) and Carroll and Meng (2016)) can guarantee a worst-case payoff for the principal. In repeated games, an implicit, unenforceable bonus contract can be chosen over the explicit, enforceable contract such as piece-rate, because when players interact repeatedly, cooperation can emerge if the cost of damaging the long-term relationship is greater than the immediate benefit (MacLeod and Malcomson, 1989; MacLeod, 2007). Compared to perfectly enforced contracts, self-enforcing relational contract can sustain by reputation and hence can substitute for the perfect enforcement mechanisms, to some degree (Levin, 2003, 2006).

Lab experiments have provided strong evidence for the incentive and sorting effects of piece-rates. More productive workers are more likely to choose piece-rate plans and higher productivity under the piece-rate scheme is a result of both the incentive and sorting effects (Cadsby et al., 2007). Dohmen and Falk's (2011) experiment provided evidence of multidimensional sorting in that piece-rate and fixed-wage schemes attracted individuals with different risk attitude, self-assessment, and gender. Larkin and Leider (2012) conducted a lab experiment and found that overconfident individuals are more likely to self-sort into a piece-rate plan that pays a higher piece-rate at a higher output level (i.e., a convex scheme). Another stream of lab experiments compared piece-rate and tournament schemes. For example, Bull et al. (1987) found that the performance variance is larger in the tournament treatment compared to the piece-rate treatment. Gneezy et al. (2003) showed that men and women performed similarly in the piece-rate treatment, while in the tournament treatment where competition is stronger, women performed worse than men.

The effect of piece-rates on productivity has also been well-documented in studies using firm payroll data (Paarsch and Shearer, 2000; Bender et al., 2012), in randomized field experiments (Shearer, 2004; Shi, 2010; Heywood et al., 2013), and in field research taking advantage of natural experiments when firms change their compensation schemes from a piece-rate to a fixed-wage or vice versa (Lazear, 2000; Franceschelli et al., 2010). Shearer (2004) randomly selected tree-planting workers who had been working under piece-rates to instead work under fixed wages and found that workers' productivity was 20 percent higher under the piece-rate plan. The productivity premium associated with piece-rates was similarly documented in Lazear (2000) who observed a productivity increase when windshield installing workers experienced a pay scheme change from salary to piece-rates and in Franceschelli et al. (2010) where pay scheme changed from a salary to piece-rate and vice versa in a textile company.

Despite its advantages, the piece-rate scheme has been criticized for its potential to lower quality and increase injuries as employees focus on speed and quantity under the piece-rate contract (Paarsch and Shearer, 2000; Freeman and Kleiner, 2005; Bender et al., 2012). Using data from shoe manufacturing, Freeman and Kleiner (2005) showed that productivity was higher under a piece-rate scheme, but firm profit was lower because of higher labor and material costs. Bender et al. (2012) provided evidence for a strong relationship between piece-rates and workplace injuries based on survey data from several countries in Europe. However, some field experiment research reported less quality damage under piece-rates. Shi (2010) conducted two field experiments in a tree-thinning firm and found that workers' productivity was 20-23 percent higher under piece-rates, while the quality of their work did not drop. Heywood et al.'s (2013) field experiment showed that, in addition to a large productivity gain, with sufficient monitoring the quality of work can be assured under piece-rates.

Compared to prior experimental studies, we compare piece-rates with bonuses under two circumstances: (1) a short-term relationship between a principal and an agent, (2) a long-term relationship between them in a spot-implementable manner. Our study still focuses on the effect of different pay schemes on productivity; it does not consider the sorting effect, quality or work injuries under different schemes. We acknowledge that this would limit the applicability of our research findings to more general settings, an issue we encourage future research to further explore. Moreover, our study differs from Fehr and Schmidt (2000) and Fehr et al. (2007), and Casari and Cason (2013), in that we examine contract choices, offered wages, and agent effort under both short- and long-term relationships. Regarding experimental studies with repeated games, Eriksson and Villeval (2008) found that, compared to players who were randomly rematched in each round, principals in repeated interactions offered a higher wage and agents exhibited higher average effort. Brown et al. (2004) allowed principals to choose whether to form a long-term relationship with trading agents and observed that under successful long-term relationships the offered wages and the agents' effort were higher. Our study is different from theirs in a few ways. First, compared to Eriksson and Villeval (2008), we examine the principals' contract choice, while they study the agents' choice. Second, compared to Brown et al. (2004), we do not endogenize the relationship choice but rather focus on employers' contract choice in different relationships.

3. Experimental Design

In this section, we first present the theoretical framework which guides our experimental design, then we describe the experimental procedure in details.

3.1 Theoretical Framework

A key assumption in contract theory models is whether effort is observable. In the case where effort is unobservable, it gives rise to the classical moral hazard problem, where the principal can relate pay to observable performance which is "only a noisy signal of effort" (Bolton and Dewatripont, 2005, p.129) or the principal can spend on monitoring to induce effort (Shapiro and Stiglitz, 1984). In our model, performance/output is observable and we assume the noise in the performance function is small, such that effort is almost fully observable based on output. When we designed the experiment to be consistent with this theoretical setup, we chose an experiment task in which individual ability or other environmental uncertainties have little influence on output; hence, it required little monitoring of action.

In a long-term contract, unobserved effort would induce the dynamic moral hazard problem (Bolton and Dewatripont, 2005, p.419). Under this assumption, Holmstrom and Milgrom (1987) proposed that a simple linear contract (such as a piece-rate or commission-based pay) is still optimal or nearly optimal when agents are risk-averse with a negative exponential utility function. In the Holmstrom and Milgrom (1987) model, incentives are binding. When incentives are non-enforced, the long-term contract is an informal, relational contract (MacLeod, 2007). MacLeod and Malcomson (1989) proved that an implicit contract can be self-enforcing in the repeated labor contract if there is sufficient surplus from continued employment. Relating to relational contract theory literature, we consider both enforceable and non-enforceable incentives in our model.

We study three contracts. At the beginning of the game, the principal chooses a contract type. An agent chooses effort $e \geq 0$ after observing the principal's contract choice. If the agent expends effort e, he generates a gross profit $v(e)$ for the principal that is strictly increasing and concave with respect to e, i.e., $v'(e) > 0$ and $v''(e) \leq$

0. The agent also incurs a cost $c(e)$ where $c'(e) > 0$ and $c''(e) \geq 0$. In our study, we simply assume that $v(e) = 2e$ and $c(e) = \frac{1}{2}ke^2$ for analytical tractability. However, even without imposing these assumptions on the functional form, the directional comparison results still remain the same.

(1) Piece-Rate Contract (P): The principal and agent's monetary payoffs are given by $M_P = e$ and $M_A = e - \frac{1}{2}ke^2$, respectively. To focus on the principal's choice of different contracts, we simplify the piece-rate contract by keeping the incentive rate fixed ($1 per unit of effort); hence, we do not allow principals to choose different incentive rates under the piece-rate contract. We set this incentive rate deliberately so that if principals offer $w = e$ under the fixed-wage contract or $w + b = e$ under the bonus contract, then the payoffs would be the same as those under the piece-rate contract. In our case, piece-rate contract is formal and enforcing.

(2) Fixed-Wage Contract (F): the principal offers an unconditional fixed wage $\$ w \geq 0$. The principal and the agent's monetary payoffs are given by $M_P = 2e - w$ and $M_A = w - \frac{1}{2}ke^2$, respectively.

(3) Bonus Contract (B): the principal offers an unconditional fixed wage $\$w \geq 0$ and may pay a bonus $\$ b \geq 0$ after observing the agent's effort e.[3] The principal and the agent's monetary payoffs are given by $M_P = 2e - w - b$ and $M_A = w + b - \frac{1}{2}ke^2$, respectively. The principal's bonus payment is unenforceable. Therefore, different from piece-rate contract, the bonus contract is implicit and unenforceable.

Assuming that both the principal and the agent are self-interested and only care about their own material payoffs, we first consider a one-shot game, representing a short-term relationship. In a one-shot game, the principal and the agent interact once. When facing a fixed-wage contract, a self-interested agent has no incentive to invest any effort. Thus, the principal should not pay any wage at the beginning. When using a bonus contract, the principal will not pay any bonus after the agent extends effort, and then the agent should not invest any effort expecting that no bonus will be paid at the end.

[3] Since the principal can give any amount of bonus after observing agent output, there is no structure as to how the bonus amount is linked to output. Therefore, there is no specific pay structure in our bonus plan.

Consequently, the principal will not pay a fixed wage in the first place. Therefore, the principal's expected monetary payoff of using a fixed wage and a bonus contract will be 0. When facing a piece-rate contract, the agent will choose the optimal effort $e^* = \frac{1}{k}$ which maximizes his payoff function $M_A = e - \frac{1}{2}ke^2$. With the piece-rate contract, the principal earns $M_P = e^* = \frac{1}{k}$. Therefore, a piece-rate would generate the highest expected payoff for the principal out of the three contract types; hence, it is the optimal contract choice.

Then, we consider a finitely-repeated setting where the relationship between the principal and the agent lasts for multiple rounds. In each round, the principal writes a new contract, choosing between a fixed-wage/bonus contract and a piece-rate contract. Thus, we consider a particular type of long-term contract where the long-term relationship is spot-implemented. As argued above, the piece-rate contract is the dominant contract in a one-shot game. By backward induction, choosing a piece-rate contract in every round is the unique subgame perfect equilibrium strategy for a finitely-repeated game. In summary, we expect that the piece-rate contract will be the dominantly chosen pay scheme in both short-term and long-term relationships.

3.2 Experimental Procedure

Based on our theoretical analysis, we implemented a 2x2 factorial design (Table 2). We examine the relational effect by comparing individual behavior between pairs with a random-rematching protocol and a fixed-matching while testing the bonus effect by investigating the behavioral differences between treatments where the fixed-wage contract is either with or without a bonus component, i.e., piece-rate vs. fixed-wage contracts and piece-rate vs. bonus contracts.

At the beginning of each session with the 12 subjects, half of the subjects were randomly assigned as players A, while the other half-as players B. The role of each player was fixed until the end of the experiment. Each player B was asked to participate in a real-effort slider task, which was adapted from Gill and Prowse (2012). For each slider that was finished, her/his matched player A received two tokens in that round. This was a common knowledge between both players.

We choose the slider task as it involves little randomness (Gill and Prowse, 2012), and we can interpret a subject's points score in the slider task as effort exerted. This matches our theoretical analysis in which we assume effort is observable, and therefore there is no moral hazard or information asymmetry, and in this case, the piece-rate should be dominantly chosen. It is worthwhile to note that some studies have shown that the slider task is underpowered in that the task's responses to different incentives are inelastic. For example, Araujo et al. (2016) implement a between-subject laboratory experiment in which the piece-rate incentive size is varied between treatments, such as a half-cent, two-cents, and eight-cents per slider.[4] They find that though the incentive rate increases by 1500%, subjects' performance only increases by 5%. In contrast with Araujo et al. (2016), the incentive size in our experiments is fixed, set by the experimenter. We do not intend to examine the effort of the agents under different incentive rates, but focus on comparing the agents' effort between piece-rate and fixed-wage contracts.

Before the B-players participated in the real-effort task, the A-players had the option to choose between a piece-rate and a fixed-wage with (out) a bonus contract. If the A-players opted for the fixed-wage contract, they also chose the amount, which was a non-negative integer up to 100.[5] In contrast, if the A-players chose a piece-rate contract, the B-players would receive one token for each unit of work they finished. After the A-players chose a contract, each B-player was informed of her/his player A's contract choice and the amount of the upfront wage if the fixed-wage contract was chosen. Then the B-players started working on the slider task. At the end of the task in each round, the number of sliders player B finished was revealed to her/his player A. In the bonus treatment, conditional on the fixed-wage contract, there was a third stage in which the A-players were asked to give the B-players an additional number of tokens. The amount of this bonus was also a non-negative integer up to 100.

To examine the relational effect, we implemented different matching protocols to mimic long- and short-term relationships. In the random-rematch treatment, players A and B were randomly rematched in each round,[6] while in the fixed-match treatment,

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[4] Conditional on piece-rate, we pay subjects 1 token per slider. As 6 tokens=1 RMB in our experiment and 1 USD=7 RMB in 2013, the incentive size is roughly 2 cents per slider.

[5] Before the experiment, we conducted two separate sessions and implemented the same piece-rate contract for 20 paying rounds and the maximum number of sliders finished was 35. In the experiment instruction, we provided summary statistics as an example of the agents' productivity. We admit that providing such a table may decrease the variance of agents' effort between treatments, and we thank an anonymous referee for calling this to our attention. Additionally, by allowing the maximum wage to be 100, we made sure there was no cap on the agents' effort.

[6] In the theoretical analysis, we model the short-term relationship as a one-shot game. In the experiment, in order to keep the same number of game rounds and get sufficient observations, we used the random-rematch protocol to mimic one-shot games.

after players A and B were matched at the beginning of the first round, they remained matched with each other until the end of the experiment. The experiment had 20 paying rounds and one practice round at the beginning. Sample instructions are included in Appendix B. After the experiment, we gave each participant a post-experiment survey that collected demographic and personality traits information such as risk- and loss-aversion. The post-experiment questionnaire is also included in Appendix B.

In total, we conducted 16 independent computerized sessions at the Economic Science and Policy Experimental Laboratory at Tsinghua University from March 2013 to June 2013, with a total of 192 subjects. All the subjects are students recruited via email from a subject pool for economics experiments. Each subject participated in only one session. We used a z-Tree (Fischbacher, 2007) to program our experiments. Each session lasted approximately one and a half hours, with the first 15 minutes used for instructions. The exchange rate was 1 RMB per six tokens.[7] In addition, each participant was paid a 10 RMB show-up fee. The average amount that participants earned was 98 RMB, including the show-up fee.[8] Data is available from the authors upon request.

4. Results

Throughout the analysis, we treat each pair of principals and agents in the fixed-match treatment as one independent observation, whereas each session with 12 subjects in the random-rematch treatment is one independent observation. Therefore, we have 24 independent observations per fixed-match treatment and 4 independent observations per random-rematch treatment. Standard errors are clustered at the independent observation level to control for potential interdependency in individual decisions across rounds and subjects. Second, we report the two-sided p-values for the entire analysis and use the 5% statistical significance level as the cutoff.

4.1 The Principals' Decisions

Figure 1 shows the proportion of principals choosing the piece-rate by treatment. The *x*-axis indicates the number of rounds. We provide the data by treatment and session in

[7] The currency exchange rate is: 1 USD= 7 RMB.

[8] On average, the students earn 100 RMB per hour for jobs such as a tutor or intern.

Appendix A. In contrast to our theory prediction, we observe a significant bonus effect. In the upper two no-bonus treatments, a vast majority of the principals (over 80%) choose the piece-rate contract. Pooling all sessions and rounds data in a treatment together, we find that, on average, the proportion of piece-rate is significantly higher in the no-bonus than the bonus treatments (random-rematch: 92 vs. 68, p=0.000; fixed-match: 85 vs. 60, p=0.005, two-sided tests of proportion). Our findings are consistent with prior studies (Fehr and Schmidt, 2000; Fehr et al., 2007; Casari and Cason, 2013) which found, with an unenforceable bonus, principals become more likely to choose the implicit contract than the baseline without such a promise (i.e. Fixed wage only). Moreover, Figure 1 shows the relational effect for the two bonus treatments. In the case of the random-rematch treatment, the percentage of principals choosing the piece-rate fluctuates between 40% and 60% in the first 10 rounds, gradually increases after round 10, and finally converges to almost 100% at the end of the experiment. In contrast, for the fixed-match treatment, the percentage of principals choosing the piece-rate remains around 50%. The test of proportion shows that only the difference between the bonus-random and the bonus-fixed treatments in rounds 11-20 is significant (bonus-random vs. bonus-fixed: 84 vs. 63, p= 0.008).

To further analyze the relational effect on contract choice, we run a Probit regression where the dependent variable is equal to one if the principal chooses the piece-rate and zero otherwise. We focus on the two bonus treatments, since the proportion of principals choosing the fixed-wage contract is fairly low in the two no-bonus treatments. We control for the agent's effort in the previous round in all specifications. Column 1 of Table 3 shows that the overall relational effect is not significant. Column 2 further includes the interaction of the fixed-match dummy with the second 10 rounds dummy. The coefficient estimate for this interaction term is negative and significant, confirming the difference in the principals' contract choice between the fixed-match and the random-rematch treatments over time, as shown in Figure 1. In column 3, we examine to what extent the principals' contract choice is correlated over time. The results show that a principal's contract choice is significantly influenced by her choice in the past three rounds, with the effect decreasing as the time lag increases. Based on the Probit estimates, we compute the marginal effects (Table 3). The effect size suggests that a principal is roughly 30% more likely to choose the piece-rate if she has chosen it in the last two rounds than those who have not.[9] Column 3 also shows when the contract choice in the previous rounds is controlled for, the agent's effort in the previous round has a significantly negative effect on the contract choice of piece-rate, suggesting the higher effort in the previous round would make the principal less likely to choose the piece-rate.

Figure 2 shows the upfront wage, bonus, and the total wage in the four treatments under the fixed-wage and bonus contracts. For comparison, we also show the wage under the piece-rate contract, which is equal to the amount of effort. In all four treatments, the wage under the piece-rate contract is always higher than that under the fixed wage with(out) bonus contract.

The upper two graphs show that in the no-bonus treatments, the wage is higher for a fixed-match agent than a random-rematch one. The lower two graphs show that in the bonus treatments, the total wage is also higher for a fixed-match agent. Moreover, the bonus for a

fixed-match agent is higher, but not for the upfront wage. Finally, the graphs for the fixed-match treatments show that in the last round, the offered wage (the upper right panel) and the offered bonus (the lower right panel) dropped This is consistent with findings in the finitely repeated games, e.g., Prisoner's Dilemma (Embrey et al., 2017) and public goods game (Lugovskyy et al., 2017). Appendix Table A1 confirms that offered wages are higher in the fixed-match treatments, as can be seen from the positively significant estimates for the fixed-match dummy for the no-bonus and the bonus treatments. Additionally, Appendix Tables A2-3 show that the higher total wages under the fixed-treatment treatment is not driven by the upfront wage but by a higher bonus. In Appendix Table A3, the positive and significant estimate for the fixed-match dummy becomes insignificant after controlling for the number of finished sliders, suggesting that the significant long-term relational effect on the bonus amount is driven to a large extent by the agents' high effort.

4.2 Agents' Effort

In this section, we examine the treatment effects on the agents' effort. Figure 3 depicts average effort in each round in the four treatments. For comparison, we show average

[9] Although not reported for brevity's sake, we include more lag variables and find that the contract choice is significantly influenced by the choice lagged by six rounds. Beyond that the effect becomes insignificant.

effort under both piece-rate and fixed-wage with(out) bonus contracts. When piece-rate is chosen, there is no difference in the effort level between treatments. On average, agents finish 27 sliders per round under the piece-rate incentive, with a minimum of 5 sliders and a maximum of 46 sliders. This is consistent with the performance of agents under a piece-rate scheme observed by other studies. For example, in Araujo et al. (2016), the average performance is 26.7 sliders, with a minimum of 10 sliders and a maximum of 46 sliders. In all four treatments, agents' effort is higher under piece-rate than under the fixed wage with(out) bonus (with the mean 21.7, minimum 0 and maximum 41), and the difference is significant except for the bonus fixed-match treatment. Notably, under the bonus random-rematch treatment (the lower left panel), average effort declines over time, similar to the observation for the implicit bonus contract in Casari and Cason (2013). In the bonus fixed-match treatment (the lower right panel), agents' effort dropped dramatically in the last few rounds, which is consistent with the finding that cooperation rates fell in finitely repeated games toward the end of the interaction. Appendix Table A4 corroborates the patterns observed from Figure 3. Agents' effort is significantly higher under the bonus treatments than the no-bonus treatments. The average effort is also higher in the fixed-match treatment with bonus. Moreover, in all specifications, the upfront wage's estimated coefficients are positive and significant. This suggests that agents generally act reciprocally, exerting higher effort when being offered a higher upfront wage. When we control for the total wage, the treatment effect becomes much smaller and insignificant, suggesting that the agents' effort varies mostly with the total wage so that the difference in the effort level between treatment can be almost entirely explained by the treatment differences in the upfront wage and the bonus.

4.3 Parameter Estimates and Efficiency

Lastly, we utilize the experiment data to obtain parameter estimates for our stylized model and further calculate the different contracts' efficiency. First, we estimate the parameters in the cost function $c(e) = \frac{1}{2}ke^2$. Under the piece-rate contract, the agents maximize their payoff $M_A = e - \frac{1}{2}ke^2$ and they will choose $e^* = \frac{1}{k}$. Hence, only the parameter k needs to be estimated. To estimate k, we use data from two other sessions of the experiment in which all the participants act as agents and are offered only a piece-rate contract.[10] The additional experiment data is labeled as the "piece-rate only" sample. Following Gill and Prowse (2012), we assume that the parameter $k = \kappa + \pi$ where κ denotes the common component of k and π is the random component following a Weibull distribution with a scale parameter ϕ and a shape parameter φ. The log-likelihood is then

$$\ln\mathcal{L}(e_1, e_2, \cdots, e_n) = \ln f(e_1, e_2, \cdots, e_n | \kappa, \phi, \varphi) = \sum_{i=1}^{n} \ln\left[\frac{\varphi}{\phi}\left(\frac{\frac{1}{e_i} - \kappa}{\phi}\right)^{\varphi-1} e^{-\left(\frac{\frac{1}{e_i}-\kappa}{\phi}\right)^{\varphi}}\right]$$

$$= \sum_{i=1}^{n}\left(\ln\varphi - \varphi\ln\phi + (\varphi - 1)\ln\left(\frac{1}{e_i} - \kappa\right) - \left(\frac{\frac{1}{e_i} - \kappa}{\phi}\right)^{\varphi}\right)$$

We use the "piece-rate only" sample to conduct a maximum likelihood estimation. The estimate for κ and the two Weibull distribution parameters are reported in Table 4. The estimate for κ, 0.022, is significantly different from zero, suggesting that the cost of effort function exhibits significant convexity. This estimate is also similar to that in Gill and Prowse (2012), which is 0.019, based on their preferred specification (Gill and Prowse, 2012, Table 3). Then, using the estimates of the cost parameter, we calculate each contract's efficiency under different treatments. We define efficiency as the sum of the principals' and the agents' realized payoff (column 3 in Table 5). Columns 1 and 2 show the average payoff for principals and agents, respectively.

Consistent with prior relational effects results, we find that conditional on the bonus contract, efficiency is much higher in the fixed-match treatment than in the random-rematch treatment (37.525 vs. 30.760). However, this difference is much smaller under the piece-rate contract. This suggests that, when the bonus contract is chosen, the two parties benefit from the fixed-matched partnership and repeated interactions, while this effect is not evident under piece-rate incentives. Moreover, except for the bonus contract under the fixed-match treatment, efficiency under the fixed-wage or bonus contract is always much lower than efficiency under the piece-rate contract. In contrast,

[10] This piece-rate is specified in the same way as in our main experiment. The participants are asked to do the same slider task for 20 rounds. The other experiment setup is also the same as the main experiment. There are 60 subjects in total.

efficiency under the bonus contract in the fixed-match treatment is close to that under the piece-rate contract (37.525 vs. 39.891).

5. Discussion

The result of our experiment stands in contrast to the theory prediction in Section 3. This result might not be surprising, considering the differences in the contract choice between one-shot and repeated games could relate to whether the incentive is enforceable or not. In our case, a piece-rate is enforceable while a bonus is not. It has been shown that non-enforceable incentives can work in repeated interactions because of concerns regarding reputation (e.g., Macleod and Malcomson, 1989; Levin, 2003; 2006; Macleod, 2007). Under certain conditions, non-enforceable contracts are even more efficient than enforceable incentives in repeated games (Ben-Ner and Putterman, 2009). Given the setup of our theoretical model and experiment, we attempt to derive a specific condition under which cooperation exists in finitely repeated interactions, and we examine whether the actions of subjects in our experiment meet this condition.

The theory of infinitely repeated games offers an explanation of cooperation in a long-term relationship. If the end of horizon is approaching very slowly, backward induction may not enter into the strategic calculations of players at the beginning. Osborne and Rubinstein (1994) argued if the end of horizon is sufficiently distant, people might ignore the existence of the horizon until its arrival is imminent. The classical Folk Theorem of infinitely repeated game predicts that any outcome that Pareto dominates the minimax point can arise as a Nash equilibrium if the players are sufficiently patient. The fundamental motivation behind behavior in repeated games is the trade-off between current and future incentives. In the settings that we studied, when principals ignore the end of a relationship (which is more likely to happen at the beginning of the horizon) they might choose fixed-wage or bonus contracts expecting agents to expend no less effort than what agents would under the piece-rate mechanism, and in turn they pay more to the agents.

In our experiment data, a significant proportion of the principals chose bonus contracts over piece-rate. In the following, we derive the condition for the existence of Folk Theorem equilibrium for the bonus treatment setting. Let (w, e, b) be the stationary equilibrium in an infinitely repeated game, that is, $w_t = w, e_t = e, and\ b_t = b$ in every period. On this equilibrium path, the principal and agent earn $u_P = 2e - w - b$ and $u_A = w + b - \frac{1}{2}ke^2$ respectively, in each period. For such an equilibrium to exist, the following conditions should be satisfied:

From the agent's perspective, if he deviates from the actions that sustain the desired outcome or invests in effort other than e, the principal will not award any bonus and will revert to the one-shot Nash equilibrium (i.e., choose a piece-rate contract for the remainder of the game). This can be expressed as,

$$b - \frac{1}{2}ke^2 + \frac{\delta}{1-\delta}(w + b - \frac{1}{2}ke^2) \geq \frac{\delta}{1-\delta}\frac{1}{2k}$$

where δ is the discounting factor. The left-hand side gives the agent's expected future payoff at a point in time he chooses his effort, and the right-hand side gives the maximum payoff he can get after deviating from the equilibrium path.

From the principal's perspective, if he does not offer bonus b, the agent will not cooperate in the future for any bonus contract, and therefore, he would have to revert to a piece-rate contract for the remainder of the game. This can be expressed as,

$$-b + \frac{\delta}{1-\delta}(2e - w - b) \geq \frac{\delta}{1-\delta}\frac{1}{k}$$

where the left-hand side gives the principal's expected future payoff at a point in time he chooses the bonus, and the right-hand side gives the maximum future payoff he can get if he does not give the bonus.

To sum up, the condition for the existence of a stationary equilibrium can be given as

$$\frac{1}{2}ke^2 - \delta\left(w - \frac{1}{2k}\right) \leq b \leq \delta\left(2e - w - \frac{1}{k}\right).$$

In the extreme case where the players are very patient, that is, when δ is close to 1, the above condition becomes $w + b - \frac{1}{2}ke^2 \geq \frac{1}{2k}$ *and* $2e - w - b \geq \frac{1}{k}$ (*). This implies that the single-period equilibrium payoffs for both the principal and the agent in an infinitely long-run relationship are no less than their one-period Nash equilibrium payoffs. Therefore, we can expect in a long-run relationship, if the players are sufficiently patient and the end of horizon is sufficiently distant, a cooperation between principal and agent (i.e., choosing a non piece-rate contract) could appear. When approaching the end of the horizon, such cooperation might collapse because of the endgame effect.

Relating this to our data, in Table 6, we present the number of principals who chose a bonus contract and the number of principals who meet the cooperative equilibrium conditions in each round of the bonus treatment. Although a significant proportion of principals chose a bonus contract instead of a piece-rate contract, only a few met the cooperative conditions. Furthermore, if the subjects are purely self-interested, it should be expected that in the last round of the bonus treatment, the principals who chose the bonus contract would not give any bonus. Our data shows that at the end of the game, four out of seven principals who chose the bonus contract still gave a bonus. This suggests there may be other concerns, such as fairness that might underlie the behavior of subjects.

6. Conclusion

Using a real-effort experiment, we examine both the relational and the bonus effects on principals' and agents' behavior. We have several main findings. First, the presence of a bonus option decreases the likelihood of principals choosing the piece-rate contract, while the dynamics in choosing the bonus contract are different for principals under the two partnerships. In the randomly-rematched partnership, a considerable proportion (48%) of principals choose the bonus contract over the piece-rate contract in the first 10 rounds; however, in the second 10 rounds this proportion decreases and drops to almost zero at the end. In contrast, in the fixed-matched partnership, the proportion of principals choosing the bonus contract over the piece-rate contract remains significant (42% in the first 10 rounds and 37% in the second 10 rounds).

We also find a higher total wage offered by the principals under fixed-match and bonus treatments. Such a higher total wage is mainly driven by a higher bonus, while the upfront wage is not significantly higher than that under the randomly-rematched partnership. This finding suggests an important difference in the principals' wage offers between long- and short-term relationships with employees. Principals under a short-term relationship may not honor their promises of offering a high bonus; however, to induce agent effort, they cannot offer too low an upfront wage in the short-term relationship.

Turning to the workers' side, we find that agents exert higher effort in both fixed-match and bonus treatments. We also observe when a bonus is combined with the fixed-matched partnership, the average effort level under the bonus contract is almost as high but does not exceed the effort level under the piece-rate contract. The reason could be that, in practice, players engage in partial cooperation where the effort level does not reach the first-best level, but such an effort level is still regarded as being cooperative enough by principals. As a result, the principals continue choosing the bonus contract.

Our experiment results have practical implications. The piece-rate contract provides a strong incentive for worker effort and productivity, as shown by our study and many previous studies. However, the piece-rate is not implementable in some circumstances. In such cases, the bonus contract provides an alternative method to motivate workers. It is effective for long-term workers but may not be as effective for short-term workers. For long-term workers, the effort level and efficiency under the bonus contract are almost as high as those under the piece-rate contract. The findings of our experimental study may shed light on both the firms' pay-scheme designs as well as on employees' motivation. For future work, it would be interesting to test the contract choice in a more natural setting and using other types of tasks. Moreover, since the long-term contract used in our experiment is spot-implemented, our experimental findings may only apply to this particular type of long-term contract, and more studies should be conducted to examine other types of long-term contracts.

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TABLE 1A: DISTRIBUTION OF COMPANIES WITH DIFFERENT PAYMENT SCHEMES:

ALL 31 MANUFACTURING INDUSTRIES

Compensation Type	Regular employee			Temporary employee		
	Percentage of employees	Monthly wage	Monthly working hours	Percentage of employees	Monthly wage	Monthly working hours
	（1）	（2）	（3）	（1）	（2）	（3）
Fixed-Wage Only	15.64	166.5	175.3	14.74	119.1	171.9
Fixed-Wage+Bonus	**33.18**	203.3	170.8	10.47	142.1	168.7
Piece-Rate Only	10.11	132.6	179.0	**38.40**	112.5	177.1
Fixed-Wage+Piece-Rate	14.12	146.4	176.6	11.54	119.9	174.8
Fixed-Wage+Bonus+ Piece-Rate	18.21	161.3	176.1	8.11	129.8	174.0
Others	8.74	162.6	177.3	16.74	118.0	175.7
Observations	7628	7628	7628	7628	7628	7628

Notes: Monthly wages are in US Dollars. The dominant payment scheme is bolded. The monthly wage offered to employees by the surveyed firms were much higher than the minimum wage (270 to 580 RMB Yuan, roughly 40-90 US Dollars) in 2005.

TABLE 1B: DISTRIBUTION OF COMPANIES WITH DIFFERENT PAYMENT SCHEMES:

TEXTILE INDUSTRY

Compensation Type	Regular employee			Temporary employee		
	Percentage of employees	Monthly wage	Monthly working hours	Percentage of employees	Monthly wage	Monthly working hours
	（1）	（2）	（3）	（1）	（2）	（3）
Fixed-Wage Only	13.24	140.1	184.6	8.07	131.4	184.8
Fixed-Wage+Bonus	19.48	153.1	179.6	4.11	121.0	178.1
Piece-Rate Only	17.20	118.0	180.7	**48.10**	108.2	180.5
Fixed-Wage+Piece-Rate	18.42	127.1	179.6	11.57	110.7	181.1
Fixed-Wage+Bonus+ Piece-Rate	**19.94**	142.7	178.3	7.91	123.1	174.2
Others	11.72	125.4	179.6	20.24	108.2	180.0
Observations	657	657	657	657	657	657

Notes: Monthly wages are in US Dollars. The dominant payment scheme is bolded.

TABLE 2: EXPERIMENTAL DESIGN

Contract Choice	Game Structure	Treatment Name	Number of Subjects
Piece-Rate vs. Fixed-Wage	Random-Rematch	NoBonus-Random	12x4
Piece-Rate vs. Fixed-Wage	Fixed-Match	NoBonus-Fixed	12x4
Piece-Rate vs. Bonus	Random-Rematch	Bonus-Random	12x4
Piece-Rate vs. Bonus	Fixed-Match	Bonus-Fixed	12x4

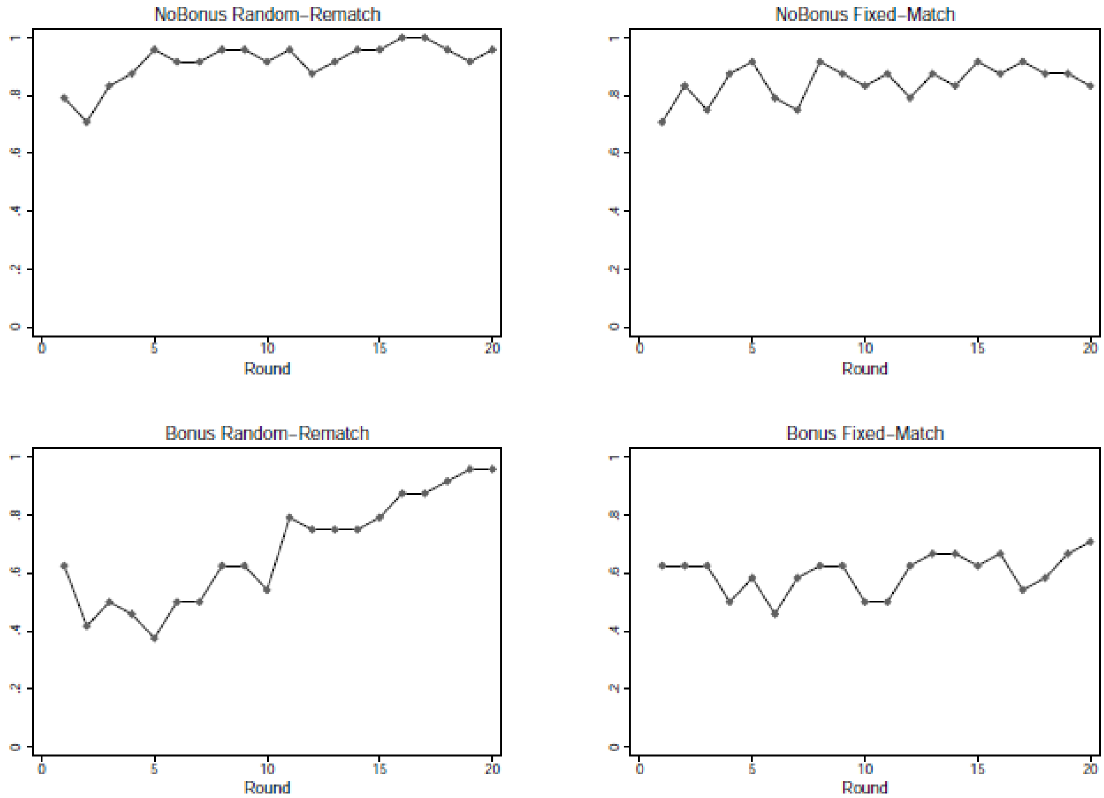


FIGURE 1: THE PROPORTION OF PRINCIPALS CHOOSING THE PIECE-RATE BY TREATMENT OVER TIME

TABLE 3: PROBIT REGRESSION ANALYSIS FOR PRINCIPAL'S CHOICE OF PIECE-RATE IN BONUS TREATMENTS

	(1)	(2)	(3)
Fixed-Match	-0.084	0.065	-0.068*
	(0.073)	(0.072)	(0.038)
Second 10-Round Dummy	0.208***	0.364***	
	(0.045)	(0.056)	
Second 10-Round Dummy*Fixed-Match		-0.321***	
		(0.080)	
Contract Choice Lag1			0.355***
			(0.042)
Contract Choice Lag2			0.335***
			(0.061)
Contract Choice Lag3			0.170***
			(0.048)
Agent's Effort in t-1	-0.003	-0.003	-0.010***
	(0.004)	(0.004)	(0.003)
Observations	960	960	816
Pseudo R^2	0.04	0.06	0.33

Notes: Marginal effects are reported. Standard errors in parentheses adjusted for 48 clusters. Significant at: *** 1%, ** 5%, * 10%. A round dummy indicates the observations of the second 10 rounds with the first 10 rounds as the base group.

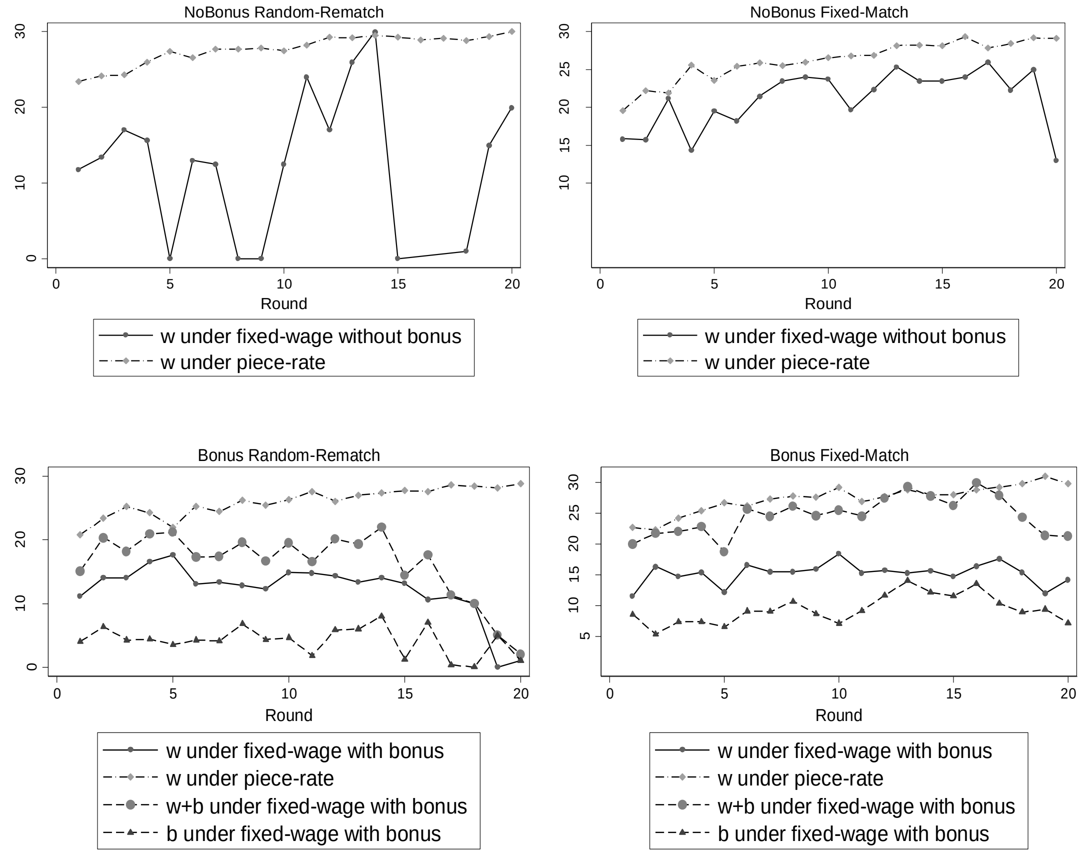


FIGURE 2: OFFERED WAGES AND BONUSES BY TREATMENT OVER TIME

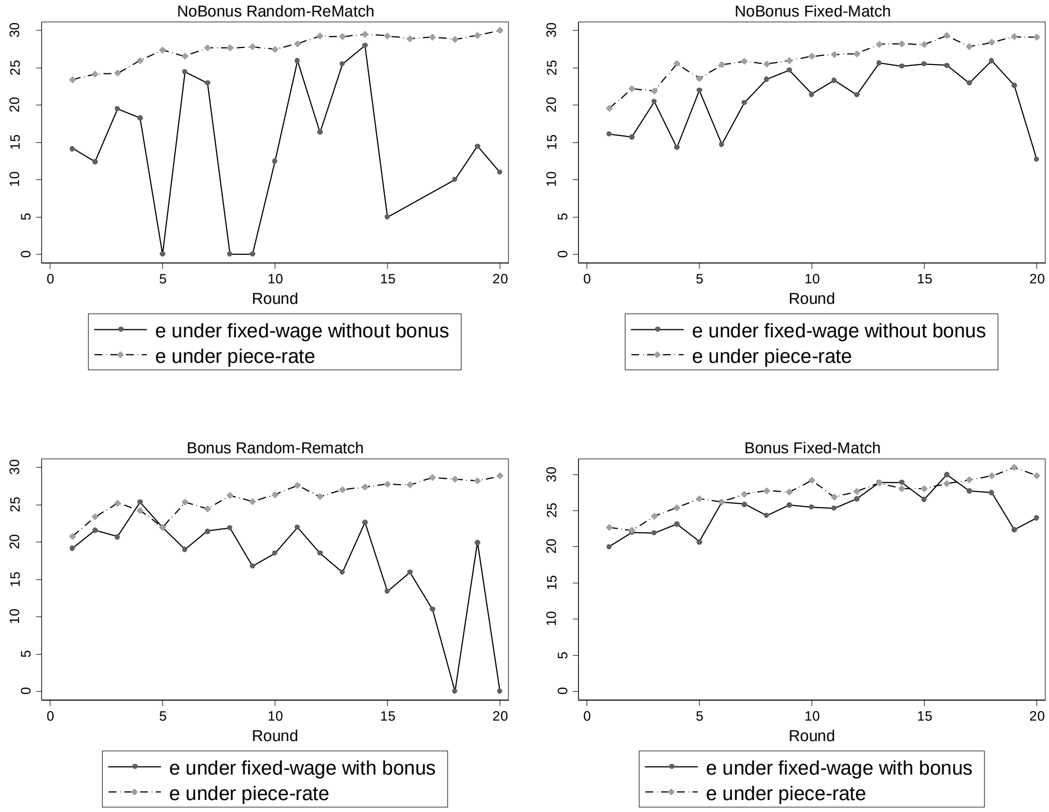


FIGURE 3: AGENT EFFORT BY TREATMENT OVER TIME

TABLE 4: PARAMETER ESTIMATES

	κ	SE	ϕ	SE	φ	SE
Piece-rate only	0.022***	0.000	0.019***	0.010	1.904***	0.157

Notes: Bootstrap standard error (SE) estimates are reported with 500 replications. The significance level is based on z-tests. * 10%; ** 5%; *** 1%.

TABLE 5: AVERAGE EFFICIENCY

		Principal's Payoff (1)	Agent's Payoff (2)	Efficiency (3)
Random-Rematch	Piece-rate	27.834	12.348	40.182
	Fixed-wage	20.483	9.420	29.902
Fixed-Match	Piece-rate	26.323	12.520	38.842
	Fixed-wage	20.616	11.772	32.389
Random-Rematch	Piece-rate	26.552	12.377	38.929
	Bonus	21.673	9.087	30.760
Fixed-Match	Piece-rate	27.392	12.498	39.891
	Bonus	26.037	11.488	37.525

TABLE 6: SUBJECTS' COOPERATION BEHAVIOR

period	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
# cooperation	0	1	0	3	0	3	1	1	0	1	1	0	1	1	0	2	3	3	2	0
# choosing the bonus contract	9	9	9	12	10	13	10	9	9	12	12	9	8	8	9	8	11	10	8	7

Note: cooperation is defined based on condition (*). Calculation assumes the cost of parameter, κ= 0.0389; the discounting factor δ=0.99.

Appendix A: Additional data by treatment and session

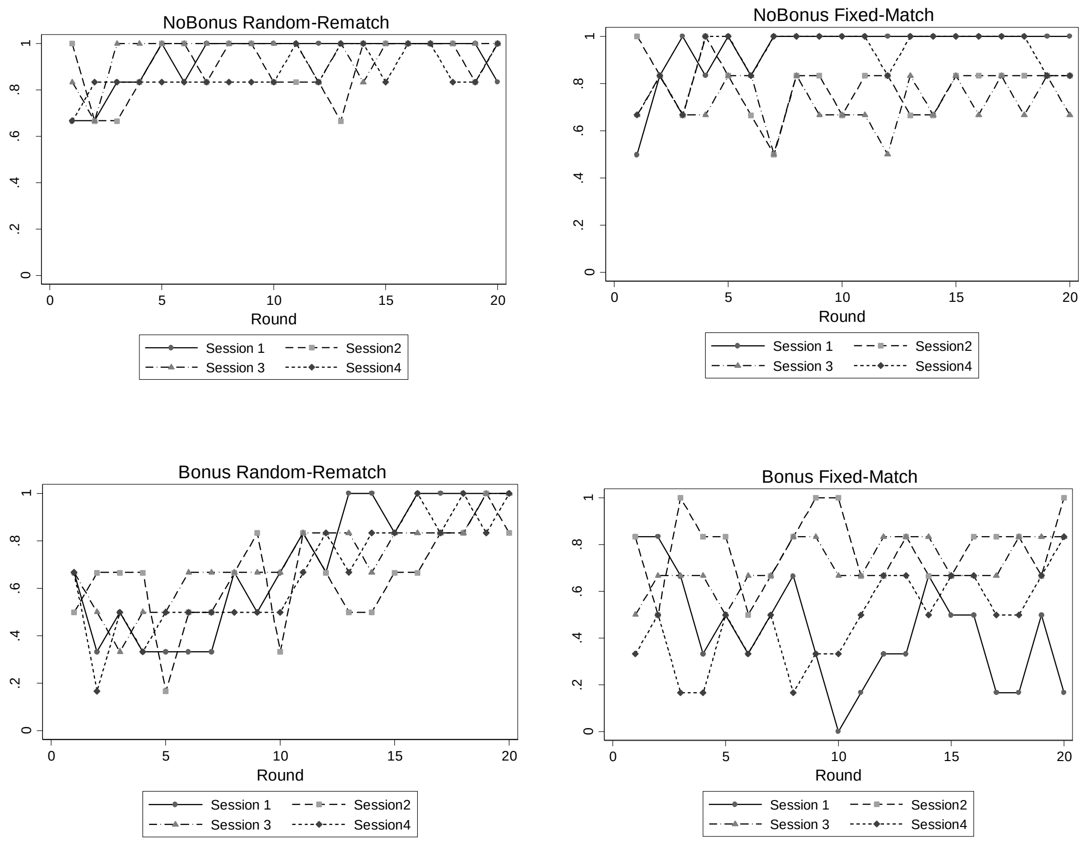


FIGURE A1: THE PROPORTION OF PRINCIPALS CHOOSING THE PIECE-RATE BY TREATMENT AND SESSION

APPENDIX TABLE A1: OLS REGRESSION ANALYSIS FOR THE TOTAL WAGE AMOUNT

	Random-Rematch (1)	Fixed-Match (2)	No-Bonus (3)	Bonus (4)
Bonus	3.447	0.684		
	(2.773)	(1.504)		
Fixed-Match			6.249**	5.418***
			(2.523)	(1.265)
Agent's Effort in t-1	0.299***	0.692***	0.743***	0.363***
	(0.094)	(0.126)	(0.117)	(0.115)
Second 10-Round Dummy	-2.135	-0.820	-1.402	-0.890
	(1.484)	(0.689)	(1.449)	(0.860)
Constant	8.564**	6.110**	-0.988	10.149***
	(3.341)	(2.887)	(2.791)	(2.709)
Observations	180	250	102	328
R^2	0.117	0.306	0.435	0.221

Notes: Standard errors in parentheses are adjusted for 39 clusters for column (1), 32 clusters for column (2), 27 clusters for column (3) and 44 clusters for column (4). Significant at: *** 1%, ** 5%, * 10%.

APPENDIX TABLE A2: OLS REGRESSION ANALYSIS FOR THE UPFRONT WAGE AMOUNT

	Random-Rematch (1)	Fixed-Match (2)	No-Bonus (3)	Bonus (4)
Bonus	-0.918	-8.050***		
	(2.789)	(2.113)		
Fixed-Match			6.249**	1.289
			(2.523)	(1.653)
Agent's Effort in t-1	0.242***	0.573**	0.743***	0.248
	(0.087)	(0.272)	(0.117)	(0.179)
Second 10-Round Dummy	-1.555	-2.351	-1.402	-2.009*
	(1.262)	(1.423)	(1.449)	(1.134)
Constant	9.541***	9.485	-0.988	8.609**
	(3.425)	(6.032)	(2.791)	(4.252)
Observations	180	250	102	328
R^2	0.069	0.254	0.435	0.069

Notes: Standard errors in parentheses are adjusted for 39 clusters for column (1), 32 clusters for column (2), 27 clusters for column (3), and 44 clusters for column (4). Significant at: *** 1%, ** 5%, * 10%. A round dummy indicates the observations of the second 10 rounds with the first 10 rounds as the base group.

APPENDIX TABLE A3: OLS REGRESSION ANALYSIS FOR THE BONUS AMOUNT

	Bonus Treatment	
	(1)	(2)
Fixed-Match	4.129**	2.367
	(1.578)	(1.530)
Number of Finished Sliders		0.349***
		(0.070)
Agent's Effort in t-1	0.115	0.008
	(0.113)	(0.110)
Second 10-Round Dummy	1.119	1.582*
	(0.982)	(0.928)
Constant	1.539	-2.952
	(2.719)	(3.689)
Observations	328	328
R^2	0.123	0.296

Notes: Standard errors in parentheses are adjusted for 44 clusters. Significant at: *** 1%, ** 5%, * 10%. A round dummy indicates the observations of the second 10 rounds with the first 10 rounds as the base group.

APPENDIX TABLE A4: OLS REGRESSION ANALYSIS FOR THE AGENT'S EFFORT

	Random-Rematch	Fixed-Match	No-Bonus	Bonus	Random-Rematch	Fixed-Match	No-Bonus	Bonus
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bonus	3.138*	7.131***			-0.329	1.670		
	(1.568)	(1.496)			(1.430)	(1.014)		
Fixed-Match			-0.045	3.901**			-0.045	0.035
			(1.864)	(1.566)			(1.864)	(0.914)
Upfront Wage	0.588***	0.446***	0.731***	0.368***				
	(0.093)	(0.108)	(0.078)	(0.100)				
Total Wage					0.802***	0.779***	0.731***	0.813***
					(0.082)	(0.055)	(0.078)	(0.059)
Total Wage in t-1	0.184*	0.095*	0.002	0.181***	0.150*	0.041	0.002	0.106**
	(0.099)	(0.051)	(0.065)	(0.056)	(0.083)	(0.042)	(0.065)	(0.043)
Second 10-Round	-2.458	2.525**	1.011	0.201	-2.164	1.012	1.011	-0.608
Dummy	(1.699)	(1.187)	(1.206)	(1.255)	(1.440)	(0.694)	(1.206)	(0.850)
Constant	8.193***	9.084***	5.044**	13.729***	5.090**	3.484**	5.044**	4.357***
	(2.089)	(2.584)	(2.077)	(1.791)	(2.136)	(1.536)	(2.077)	(1.279)
Observations	180	250	102	328	180	250	102	328
R^2	0.217	0.365	0.606	0.225	0.453	0.723	0.606	0.575

Notes: Standard errors in parentheses are adjusted for 39 clusters for column (1) and (5), 32 clusters for column (2) and (6), 27 clusters for column (3) and (7), 44 clusters for column (4) and (8). Significant at: *** 1%, ** 5%, * 10%. A round dummy indicates the observations of the second 10 rounds with the first 10 rounds as the base group.

Appendix B: Experiment Instructions (Bonus-Fixed Treatment)

This is an experiment in decision-making. You will make a series of decisions in the experiment, followed by a post-experiment questionnaire. **Please note that you are not being deceived and everything you are told in the experiment is true.**

Each of you has been assigned an experiment ID, i.e. the number on your index card. The experimenter will use this ID to pay you at the end of the experiment.

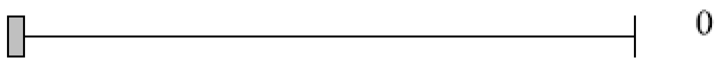
Rounds: The experiment consists of 21 rounds of games including one practice round. After the practice round, the payment you earn in each round will cumulate toward your final payment.

Roles: This experiment has 12 participants, six of whom are player As and the others are player Bs. Your assigned role will be the same for all rounds. Therefore, if you are a player A, you will always be a player A. Similarly, if you are a player B, you will always be a player B.

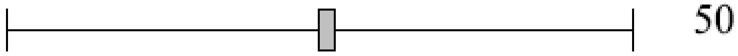
Grouping: At the beginning of the experiment, a player A is randomly grouped with a player B in the room. Each player A will play with the same player B until the end of the experiment.

Player B's Task: In each round, all players B needs to undertake an identical task described below.

The task lasts 120 seconds and consists of a screen with 48 sliders. As shown below, each slider is initially positioned at 0. The slider can be moved as far as 100. The number to its right shows its current position. You can use the mouse in any way you like to move the slider, and readjust the position of the slider as many times as you wish. You may now practice by moving the slider below.



To complete one piece of the slider task correctly, you will need to position the slider **at exactly 50,** as shown in the example below. Note the number to its right shows the correct position "50". Each time that you undertake the task, the "number of sliders completed" will be the number of sliders correctly positioned at exactly 50 at the end of the 120 seconds. Are there any questions?



Before we start experiment, please look at a sample screenshot below. The screen shot contains 48 sliders. The upper right corner shows the remaining time is 104 seconds. Three sliders are currently positioned at 50. So the box on the top of the screen shows that "currently, number of sliders complete is 3".

Additionally, for every slider that player B finishes, player A will receives 1.5 tokens.

Player A's Choice :

In each round, player A first chooses the amount of tokens given to B. Player B will be told A's choice before she starts the slider task. A can choose between the following two schemes:

Option 1: Player A first chooses a fixed amount X. After B finishes slider task, then choose another fixed amount Y. Note, B will only know the amount of X but not know Y.

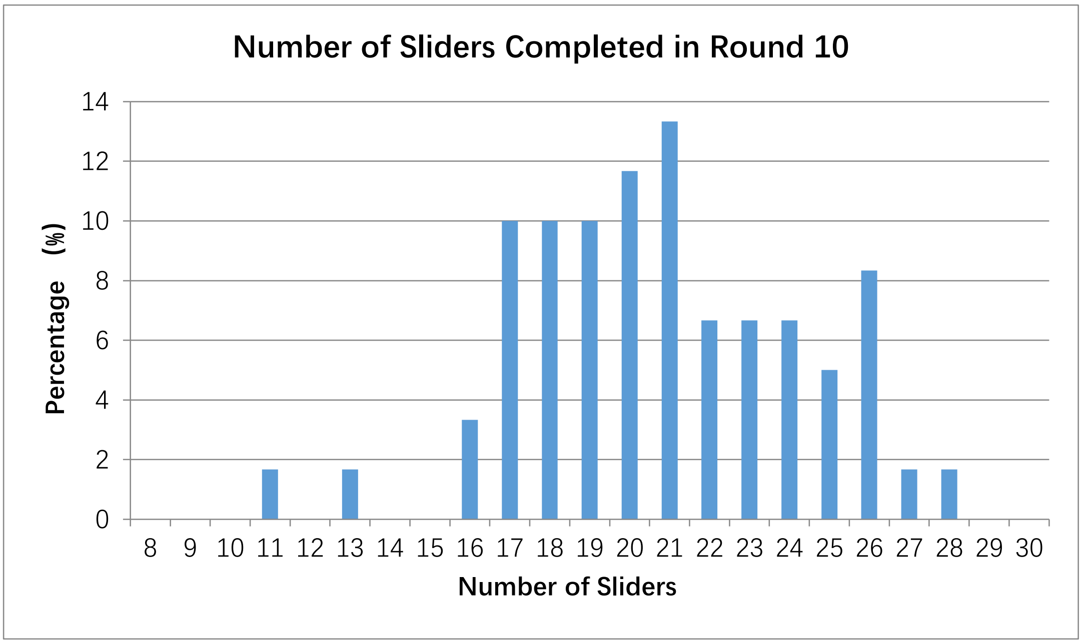
Option 2: For every slider B finishes, she gets 1 token.

To help Player A to evaluate player B's relative skill in the task. We previously ran sessions in which 60 participants undertook the same slider task for 20 rounds and earned 1 token for each completed slider – the same as option 2 above. The average number of finished sliders across 20 rounds is 20.[11]

Table below presents the summary statics in each round.

Round	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**13**	**14**	**15**	**16**	**17**	**18**	**19**	**20**
Median	12.5	16	17	18	18	19.5	19	20	20	20.5	20	21	20	21	21	21.5	21	22	23	23
Min	1	8	7	10	11	5	7	11	12	11	11	13	13	11	9	11	15	14	15	14
Max	23	23	24	26	28	27	31	27	28	28	29	31	29	30	31	35	34	30	32	29

We also provide you with the histogram below. It shows the distribution of the numbers of sliders completed by those participants in round 10. The horizontal axis represents the number of sliders completed. The height of each column shows the percentage of participants who completed that particular number of sliders in round 10. The higher a column is, the more people who completed that number of sliders. For instance, 13% participants completed 21 sliders.



[11] We provide this table in all treatments.

Payoffs: Assume the number of sliders that player B finishes is Z, each player's payoff in each round is below:

- ✓ When player A chooses option 1

  A's payoff=Z*2- X -Y

  B's payoff=X+Y

- ✓ When player A chooses option 2

  A's payoff= Z*2-Z= Z

  B's payoff=Z

Cumulative Payoff: Your cumulative payoff will be the sum of your payoff in all rounds.

Feedback: At the end of each round, you will receive the following feedback on your screen about the round including (1) player A's decision; (2) player B's number of completed sliders; (3) Your payoff this round, and (4) your cumulative payoff up to this round.

History: Player A's decision, Player B's number of completed sliders, your payoff in each round and your cumulative payoff will be displayed in a history box.

Exchange Rate: At the end of the experiment, the tokens you earned will be converted to Chinese yuan at the rate of **1 yuan = 6 tokens.**

Please do not communicate with one another during the experiment or use your cell phones. No food is allowed in the lab either. If you have a question, feel free to raise your hand, and an experimenter will come to help you.

Post-experiment survey:

1. Gender
   A. Male
   B. Female
2. Ethnic Background
   A. Han
   B. Other

3. Age： ______

4. College Grade/ Year：

A. Freshman Year

B. Sophomore

C. Junior

D. Senior

E. >4 Years

F. Graduate student

5. Would you describe yourself as (Please choose one)

A. Optimism

B. Pessimism

C. Neither of above

6. Which of the following emotions did you experience during the experiment? (Select all that apply).

A. Anger

B. Anxiety

C. Confusion

D. Contentment

E. Fatigue

F. Happiness

G. Irritation

H. Mood swings

I. Withdrawal

7. In general, do you see yourself as someone who is willing, even eager, to take risks, or as someone who avoids risks whenever possible? [7 point likert] 1 I avoid risks as much as possible ……7 I am very willing to take risks.

8. Under the circumstance that the risk bring same amount of income and loss, do you think the negative effect of loss is larger than the positive effect of income? [7 point likert] 1 Equally large……7 The negative effect is far larger than positive effect

9. In general, how competitive do you think you are? [7 point likert] 1 I am not competitive at all…….7 I am very competitive

10. When you use computer in daily life, which of the following statements is true?

A. I use mouse and touchpad equally often.

B. I use mouse more often than touch pad.

C. I use touchpad more often than mouse.

If you are the player A, please answer question 11-12. If you are the player B, please answer question 13.

11. If you are a Player A, and you chose the plan "Pay X token to B before the task and Y tokens after the task ", please choose the possible reasons why you chose this plan (Select all that apply)

A. this plan is easier

B. this plan provide more incentive to B

C. this plan is fairer for B

D. this plan is more challenging for B

E. under this plan, there is less risk of my income

F. other reason

12. If you are a Player A, and you chose the plan "For every successful slider that player B finishes, she will get 1 token", please choose the possible reasons why you chose this plan (Select all that apply)

A. this plan is easier

B. this plan provide more incentive to B

C. this plan is fairer for B

D. this plan is more challenging for B

E. under this plan, there is less risk of my income

F. other reason

13. If you are a Player B, please choose the effort under different pay plan. Please choose a number between 0-100 (0 is the lowest, 100 is highest) to represent your effort.

When A choose plan "Pay X token to B before the task and Y tokens after the task", your effort is __________

When A choose "For every successful slider that player B finishes, she will get 1 token", your effort is_____