Mobility and productivity in a dual labor market: an experiment*

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Abstract

We propose an experiment where participants receive one of two contracts involving a piece-rate payment for performing a real-effort task. The differences in piece-rate levels aim to capture earnings differentials between formal and informal markets to study how the reallocation rules of these contracts, capturing labor mobility, affect the workers' effort supply. We use a tournament structure where the worst-performer of the best contract and the top-performer of the worst contract enter into a contest, whose outcome is defined by the completed transcriptions in a real-effort task. We find that these contests, regardless of a low or high mobility rule based on effort, increase the participants' productivity. We also find that low mobility rules have a larger effect on a sample of workers when combined with a meritocratic initial allocation of the contracts. By contrast, students react more to rules evoking high labor mobility. We also find that the most significant increase in productivity comes from participants who retain the best contract after the contest, suggesting that perceptions of downward mobility are dominant in altering effort supply.

Keywords: Contract allocation; Labor mobility; Meritocracy; Dual Labor Market; Labor Productivity.

JEL Classification Codes: C90, J24, J41, M5, O17

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1 Introduction

Two hypotheses may explain the co-existence of formal and informal labor markets: exit and exclusion (De Soto, 1989; Perry et al., 2007; Fernández et al., 2017; Pham, 2022; Arango and Flórez, 2021). The exit hypothesis pertains to the choice of the optimal relationship with the state, affected by the perceptions of efficiency and the degree of satisfaction (Maloney, 2004). On the other hand, exclusion refers to structural factors that drive workers away from the formal economy without a deliberate choice. This exclusion is associated with market failures, such as those caused by moral hazard (Bardey et al., 2015), and labor market rigidities. Although exclusion is not a deliberate choice, the perception of labor mobility–through equality of opportunities–may affect aggregate outcomes (Marrero and Rodríguez, 2013). At the individual level, perceptions of labor mobility affect the effort provision in the short-run and, by altering aspirations, the intention to accumulate human capital in the long-run (Dalton et al., 2016).

The study of mobility in dual labor markets using naturally occurring data is not trivial. The main issue is that structural conditions of a labor market, such as fiscal conditions or the demand for industry-specific human capital, simultaneously affect mobility and productivity. Moreover, measuring the perceptions of labor mobility at the individual level could become a cumbersome task due to selection issues (e.g., a correlation between positive perceptions of mobility and observability of a given individual) and measurement error problems.

We designed and conducted a proctored online experiment to measure differences in productivity resulting from perceptions of labor mobility in a controlled setting. We use a tournament structure to allocate two types of contracts, whose difference in piece-rate payments reflect the gap in remuneration between formal and informal labor markets (La Porta and Shleifer, 2014). Since our primary purpose is to study the perceptions of labor mobility, we compare different rules for contract reassignment based on current effort provision. We employ a real-effort task that does not require any previous knowledge. Participants must transcript several codes, from numbers to letters, whose correspondence changes after every correctly solved code. Since the task is the same for all participants, and the number of completed tasks defines the commonly known mobility rule between labor markets, we argue that the task provides a good signal of the participants and their competitors' effort, which translates into our measure of productivity.

We argue that the perception of labor mobility based on productivity may transform the incentives to provide effort. If the perception of labor mobility is high, workers might incorporate a premium in the piece-rate in their expected future payment. This premium is associated with a transition from the informal to the formal market. By contrast, a scenario with limited mobility between markets, or a scenario where mobility is not correlated with effort, will limit the incentives to the current piece-rate payment. Moreover, differences in relative payments for a similar task may also reduce productivity (Breza et al., 2018), making it harder for participants in the informal sector to perceive that increasing effort provision can lead them to a better contract. These perceptions of mobility will interact with our different tournament rules for contract reassignment.

In our tournament, participants belong to a group of four contestants who have a limited amount of time to complete as many tasks as possible. Two of them belong to the formal market, labeled "Contract A"; and the other two participants to the informal market, labeled "Contract

B". Participants with Contract A get paid twice as much per completed task. When the task time ends, the worst-performer with Contract A and the top-performer with Contract B enter a contest that reassigns contracts. In our treatment with low labor mobility, the contest has perfect discrimination. The player with the most completed tasks automatically receives Contract A for the next round, leaving Contract B for the other player. In our treatment with high labor mobility, the contest has noisy discrimination. The probability of winning the contest is proportional to the number of completed tasks. We compare both treatments with a baseline scenario. Here, the contracts from all four players are randomly reallocated every round, regardless of their performance. Although labor mobility is high, it is unrelated to effort provision.

Another advantage of our tournament approach to dual labor markets is studying sorting based on abilities. Although the productivity gap between formal and informal markets is known (La Porta and Shleifer, 2014), it is not clear whether formal markets effectively select the most productive workers. Another possibility is that expectations about low mobility opportunities and gaps in remuneration could lead to an aspirations trap that induces an effort reduction. For this reason, we also study the initial contract allocation by comparing a merit-based assignment (using productivity from a practice round) with a random assignment. The interplay of both treatment arms might be important because low mobility could perpetuate random allocations, and meritocracy could signal tournament attributes (e.g., perception of initial fairness).

We conducted the experiment in a proctored online format with two samples: students with previous experience in the laboratory and workers without any prior experience in this type of studies. The two samples were restricted to participants from Colombia, a country with a self-employment rate of 51%, the highest among OECD members, and whose labor market is characterized by its low mobility from the informal to the formal sector (Prada, 2012).

We find that the productivity was higher in treatments where contract reallocation involved tournaments. Moreover, the effect of merit-based initial allocations is intertwined with mobility rules: If contracts are randomly reallocated, merit-based allocations decrease productivity. By contrast, merit-based allocations that were followed by reallocations with contests increase productivity. The reported effects were also heterogeneous by samples. The contest with low labor mobility (i.e., with perfect discrimination) has a larger and positive effect on productivity in the workers' sample, whereas students become more productive when the contest has a higher perception of labor mobility (i.e., with noisy discrimination).

Experiments have vastly contributed to the understanding of the shadow economy from the tax evasion perspective (Alm, 2012; Alm and Malézieux, 2021). The offered controlled environment gives an opportunity to understand monitoring and sanctioning rules better. We contribute to this experimental literature from a different perspective, by studying exclusion in tournament environments mimicking dual labor markets. Our study sheds light on two elements. First, on the understanding of effort provision for different payment and transition rules between formal and informal markets. Second, on the sorting of individuals in competitive environments.

Regarding the first element, natural experiments, field experiments, as well as lab and lab-in-the-field experiments, have contributed to the vast evidence connecting relative payments and effort (Cohn et al., 2014; Cullen and Perez-Truglia, 2022; Senik, 2021; Card et al., 2012). The general finding is that the chosen effort level is sensitive to their wages and is also affected by the information about co-workers' wages when they do not differ in productivity. In more controlled settings, the social comparison in effort provision typically involves three-person gift-exchange games (Charness and Kuhn, 2007; Gächter and Thöni, 2010; Nosenzo, 2013). Recent experiments, using real effort tasks and piece-rate payments, show that subjects' effort is affected by the information about relative wage changes (Bracha et al., 2015; Rojas-Fallas and Williams, 2020) and the timing of wage increases (Sliwka and Werner, 2017). In the domain of tax compliance, one example of social comparison involves differential tax rates. Bazart and Bonein (2014) report that these differences trigger negative reciprocity and reduce tax compliance. We contribute to this literature by letting participants reduce these payment differentials through effort provision in a controlled environment where contract reallocation depends on this provision, but differentially according to our treatments.

Regarding the second element, experiments have contributed to understanding contract selection and sorting in competitive environments within the firm. Departing from Lazear and Rosen's theoretical contribution (1981) on performance-based payments, multiple natural and field experiments have explored how productivity is affected by these schemes (Bandiera et al., 2007; Leuven et al., 2011; Delfgaauw et al., 2014). More controlled settings have shown that contract selection is affected by productivity (Cadsby et al., 2007; Eriksson et al., 2009), but also by other factors associated with the perception of how competitive the environment is (that are unrelated to productivity). The most well-known is gender (Gneezy et al., 2003), but risk-aversion and selfishness could also affect this sorting (Dohmen and Falk, 2011). We contribute to this literature by showing that labor mobility leads to productivity-based sorting when rules involve effort provision. Moreover, we report that initial contract allocations based on meritocracy tend to foster effort provision in competitive environments.

At the risk of being too obvious, we validate the importance of mobility rules between labor markets in fostering effort provision. Much less obvious is our finding that participants who almost lose Contract A increase their productivity in response to this "alarm." Since the perceived threats of losing the privileged contract seem to be determinant in effort provision, understanding downward mobility as an alarm of exclusion could help in the broader comprehension of the co-existence of formal and informal markets.

2 Experimental Design

2.1 The tournament

We grouped participants in teams of four and instructed them to complete as many tasks as possible within the time limit of 120 seconds. Two participants received Contract A, and the other two received Contract B within each team. Both contracts employ a piece-rate payment, but in Contract A the participants get paid twice as much for each correctly solved task. At the end of each round, participants within a group were lexicographically ranked using two arguments. The first argument, contract type, guarantees that players holding Contract A were ranked first and second, and those with Contract B remained third and fourth. The second argument was the number of correctly solved tasks during the current round, a measure we will refer to as the participants'

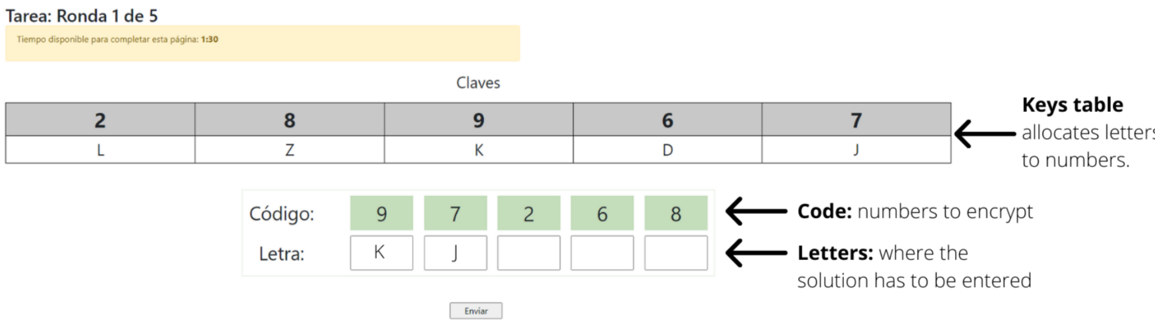

Figure 1: Graphical description of the encryption task.

productivity. Hence, in theory, we could have participants ranked third or fourth with more correctly solved tasks than those in the top two ranking positions. The participants ranked second and third face a contest in which they compete based on their productivity. Hereafter, we will call this contest a "playoff." The winner will obtain Contract A for the next round, and the loser will get Contract B. In the following subsection, we describe the implementation of this competition.

The task employed in the tournament is a modification of the encryption task proposed by Erkal et al. (2018), and adapted by Benndorf et al. (2019) with a double-randomization to reduce learning. In our task, participants have to encrypt a combination of five randomly generated numbers into letters. Figure 1 displays an example of this task. Participants observe a table on top with the correspondence from numbers to letters. Below, participants have a box displaying the numbers to encrypt and the cells to write the corresponding letter. The task is counted as correct when all the letters are correctly entered. After a correct task, the double-randomization occurs: the correspondence between numbers and letters shown in the top table is redrawn, and so is the ordering of the numbers in the bottom box.

We modify the original encryption task and limit the employed letters to five: Z, D, J, K, L. The reason is that typing abilities of our samples of students and workers might differ considerably. Hence, limiting the number of letters that participants have to type might reduce pre-existent heterogeneities between samples, which in turn may affect effort provision due to uncontrolled factors (Dechenaux et al., 2015). The chosen consonants are below the five vowels in a QWERTY keyboard, the standard in Colombia. The reason is that, in a related experiment, we vary whether the transcription involved vowels or consonants, so we kept the same letters for comparability of baseline productivity.

Participants play this tournament for five rounds, plus an initial practice round without direct payoff consequences. At the end of each round, contracts are reallocated based on the playoff outcome, and groups are reshuffled. This reshuffling is stratified based on the ranking positions *after* the playoff. In other words, when the participant with Contract B wins the playoff, she is now ranked second and receives Contract A. The loser of Contract A is now ranked in third place and receives Contract B. In each round, our reshuffling procedure is stratified by contract type. We thus guarantee two players with Contract A and two players with Contract B per group.

2.2 Treatments

Our treatments involve variations in two dimensions. The reallocation of contracts, capturing labor mobility; and in the initial assignment of contracts, capturing any path-dependent outcomes that might result important in combination with labor mobility.

Let us start with contract reallocation. The treatment *Playoff-Perfect* refers to the case where the playoff between players ranked second and third is directly defined by their round's productivity. We borrow the term "perfect" from contest theory (Szymanski, 2003). The playoff outcome is deterministic because it can *perfectly* discriminate the player exerting the highest effort (as in a standard auction where the highest bid wins). In our case, we assume that the player with the highest effort also has higher productivity.

The treatment *Playoff-Noisy* refers to the case where the odds of winning the playoff are proportional to each player's productivity. The playoff outcome is stochastic–or noisy–because as long as a player has a positive productivity, she has a positive probability of winning. For instance, if the player ranked second solved eight tasks and the player ranked third solved 12 tasks–a possible ranking given its lexicographic nature–the former player will have a 40% chance of winning and the latter the remaining 60% chance. Generally speaking, the odds of winning the playoff for player i, who has a productivity ϕ_i, and faces player j with ϕ_i are:

$$P_i(\phi_i, \phi_j) = \frac{\phi_i}{\phi_i + \phi_j} \tag{1}$$

In the *Playoff-Perfect* treatment, the tie-breaking rule dictates that if both contestants have the same productivity, the winner of Contract A is randomly decided. Note that in this case, the allocation rule in *Playoff-Perfect* and *Playoff-Noisy* becomes equivalent.

We contrast these treatments with a baseline condition that we define as *Random*. In this treatment, *all* participants are randomly reassigned to a contract in every new round. After this description, the immediate question is why all, and not only the participants ranked second and third, are randomly reassigned. The reason is twofold. First, this condition eliminates any future effect of being ranked first or last. Hence, it lets us see the pure effect of the piece-rate payment. Second, a treatment condition that would have looked "closer" to the tournaments with playoff, where the random allocation only involved the players ranked second and third, is already captured in the other treatments by every outcome in which their productivity is identical.

We now move to the exogenous variation in the initial allocation of contracts. We have a *Merit*(-based) treatment, in which we use the participants' productivity during the practice round to assign contracts in round 1. Participants were sorted by productivity and then divided into two categories, as holders of Contract A and B. Then, we created groups of four participants, ensuring that there were two participants with Contract A and another two with Contract B in each group. In the *Merit* treatment, the instructions for the practice round mentioned that, although there was no direct payment, their performance would affect the game in the future. We contrast this condition with a *Luck*(-based) treatment, in which we randomly assigned contracts for round 1. Table 1 summarizes the six conditions emerging from our 3×2 between-subjects design.

Table 1: Description of treatments

Treatment arms		Allocation rules	
Reallocation	**Initial allocation**	Rounds 2-5: Reallocation rules	Round 1: Contract A's allocation based on
Playoff-Perfect	Merit	Deterministic playoff between 2nd-3rd	Round 0's productivity
Playoff-Noisy	Merit	Stochastic playoff between 2nd-3rd	Round 0's productivity
Random	Merit	Random reallocation between 1st to 4th	Round 0's productivity
Playoff-Perfect	Luck	Deterministic playoff between 2nd-3rd	Randomness
Playoff-Noisy	Luck	Stochastic playoff between 2nd-3rd	Randomness
Random	Luck	Random reallocation between 1st to 4th	Randomness

2.3 Payments, sampling and implementation

We randomly select one of the five tournament rounds for payment. The piece-rate payments were COP 3,000 and 1,500 in Contracts A and B, respectively. Participants also received a fixed payment of COP 10,000 in all treatments, conditional on completing the activity.

Participants completed a survey at the end of the experiment. It included an incentivized measure of risk-aversion using the staircase procedure proposed by Falk et al. (2018). Each session lasted approximately 60 minutes[1] and participants received, on average, COP 36,939. This average payment is equivalent to roughly 1.2 times a daily minimum wage by the time we conduct the experiment.[2] We processed payments via bank transfer.

The experiment was programmed in oTree (Chen et al., 2016) and conducted online. We obtained approval from the Ethics Committee at Universidad del Rosario in Bogotá. We conducted the sessions targeting students in September 2021. They were invited using the Rosario Experimental and Behavioral Economics Lab - REBEL students subject pool. We conducted the sessions targeting workers in November 2021. They were invited via social media (i.e., Facebook and Twitter) to complete a pre-selection survey. We use this survey to validate the participants' work status, obtain their consent to be contacted by e-mail, and use their bank account information for payment. The Ethics Committee also approved this survey. We invited participants between 18 and 27 years old that were holders of a bank account in their name. Although sessions in September and November targeted students and workers, respectively, about 20 to 24% of the participants in one type of session self-identified with the other group.

3 Hypotheses

We preregistered our hypotheses in AsPredicted (#75078, `https://aspredicted.org/~tg28tdFYyU`). We start with the comparison, in terms of productivity, between the degrees of mobility induced

[1]We had 6 (2.8 %) participants with early dropouts from the activity. We replaced these participants with bots, whose productivity was manually adjusted to be ten sequences.

[2]This amount was equal to approximately USD 9.65 by September 2021.

in our dual labor markets:

Hypothesis 1 (H1): Productivity is higher in the *Playoff-Noisy* condition compared to the *Playoff-Perfect* condition. Both playoff treatments induce larger productivity than the *Random* treatment.

To provide some intuition for H1, we introduce the idea of the contest success function–CSF (Tullock, 2001; Chowdhury and Sheremeta, 2011), mapping efforts (i.e., productivity in our case) into probabilities of winning. Eq. 1 represents a case in which the odds of winning are linear for the *Playoff-Noisy* condition. However, suppose each effort term in Eq. 1 is exponentiated to a value greater than one. In that case, the winning probabilities are higher for the contestant providing more effort. In the extreme case, our *Playoff-Perfect* condition, this contestant wins with certainty. If participants are heterogeneous, and we argue that the two contract types create this heterogeneity, the perfect discrimination of effort in the latter condition reduces the incentives to provide effort. A similar argument applies when comparing with the *Random* condition. Since participants do not have future incentives from preserving the current contract, effort (i.e., productivity) drops.

Our second hypothesis is related to the other treatment arm. We argue that an initial allocation based on productivity creates a degree of entitlement to Contract A. As a consequence, participants provide more effort to keep this contract.

Hypothesis 2 (H2): *Merit*(-based) initial allocation leads to higher productivity compared to the *Luck*(-based) allocation.

We will also explore the cross-treatment effects between the contest type dictating mobility and the initial allocation, although we do not have clear hypotheses *ex ante*. We finally hypothesize over the effect that switching contracts may have on the participants' effort provision. In essence, we argue that contract promotion has a larger impact than the corresponding demotion. This difference is caused by the gap in piece-rate incentives and the realization of upward mobility.

Hypothesis 3 (H3): Promoted workers are more productive than demoted workers once they switch contracts.

4 Results

4.1 Descriptive statistics

We asked for some demographic information at the end of the experiment that allowed us to characterize our two samples. We will call "students" those whose occupation is only students or unemployed people studying. The participants who report being workers (paid or unpaid), and those unemployed who are not students, will be called "workers" hereafter. Students were on average 20.6 years old (std. dev. 1.8), 73% of them self-identified as females, and 20% reported an average personal income greater than the minimum wage. Ninety-four percent of participants reported using a laptop and 6% a desktop computer, 39% reported utilizing a mouse, and 61% the laptop's mouse pad during the activity. Ninety-two percent perceived that their internet connection was good during the experiment. When we asked them about social security, 59% reported having contributory health insurance, and 13% of them make pension contributions.

Regarding the worker sample, they were on average 21.9 years old (std. dev. 1.9), 63% of them self-identified as females, and 67% reported an average personal income greater than the minimum wage. Ninety-one percent of participants reported using a laptop and 9% a desktop computer, 60% reported utilizing a mouse, and 40% a mouse pad during the activity. Ninety-two percent perceived that their internet connection was good during the experiment. When we asked them about social security, 75% reported having contributory health insurance, and 53% reported making pension contributions. Tables A.1 and A.2, in the Appendix, validate that the assignment of conditions is balanced in these observable characteristics.

The average productivity was 10.49 completed tasks per round (std. dev. 2.39). Participants in the *Playoff-Noisy* condition completed 10.63 tasks (std. dev. 2.56), in the *Playoff-Perfect* condition completed 10.56 tasks (std. dev. 2.49), and in the *Random* condition completed 10.27 tasks (std. dev. 2.08). Regarding the initial allocation, in the *Merit* condition they completed 10.57 tasks (std. dev. 2.48), a similar value to the 10.39 tasks (std. dev. 2.30) completed on the *Luck* condition. We found that students completed 10.54 tasks per round (std. dev. 2.15), a value slightly larger than for the workers' population (10.45 tasks, std. dev. 2.57). We employed a transcription task with double randomization to reduce learning between rounds. However, we found that the average number of completed tasks in the first paid round was 10.15 (std. dev. 2.08), whereas, in the last round, it increased to 11.46 (std. dev. 2.20).

Since we are interested in mobility between contracts, we compute transition probabilities between contracts A and B in consecutive rounds. We call A-A and B-B those keeping the respective contracts, and B-A and A-B the promoted and demoted participants. Table 2 reports these transitions. The differences between the *Merit* and *Luck* conditions are not statistically significant (Chi-squared test, p-value 0.512). However, we find some differences by reallocation treatments. For some intuition, look at the transition probabilities in the *Random* reallocation condition. Any listed outcome is equally likely since all contracts are randomly reassigned between rounds. This is why all the reported values are very close to 25%. By contrast, in the treatments with tournaments, promotions and demotions are less likely, as they range between 10.6 and 10.9 percent. What is more interesting is that transition probabilities do not differ between *Playoff-Perfect* and *Playoff-Noisy* (Chi-squared test, p-value 1.000). Although, in theory, the *Playoff-Noisy* had higher labor mobility, the piece-rate incentives made it very common that the player with Contract A that was entering the playoff had an advantage. The main consequence of this outcome is that the treatment effects discussed in the rest of this section correspond to the higher perception of mobility evoked by the *Playoff-Noisy* with respect to the *Playoff-Perfect* condition, rather than by an *effective* difference in contract mobility between these treatments.

4.2 Effect of the reallocation rules

We conduct an OLS analysis with the number of completed tasks as the dependent variable, explained by our treatment arms and other covariates listed in Table 3. We report the results for the pooled sample and the samples of workers and students separately. Odd columns report the coefficients without interactions between treatments, and even columns add these interactions.

For the full sample, productivity increases in about 0.33 completed tasks in the treatments with playoff competitions. Nonetheless, these coefficients are non-distinguishable from each other.

Table 2: Transition probabilities by treatment conditions

TRANSITION	Initial allocation		Reallocation			All
	Luck	Merit	Random	Perfect	Noisy	
A-A	33.08	35.88	25	39.02	39.19	34.54
A-B	16.41	14.12	24.63	10.61	10.81	15.22
B-A	16.67	14.12	24.63	10.98	10.81	15.34
B-B	33.84	35.88	25.75	39.39	39.19	34.9

However, by computing the treatment effects separately for workers and students, we find that each sample is more affected by one of the treatment conditions. The productivity increase associated with the *Playoff-Perfect* condition is driven by the workers, whereas this productivity increase for the *Playoff-Noisy* condition is driven by the students. We also confirm that the higher piece-rate incentives from Contract A induce higher productivity among these participants. To put treatment effects in perspective, the increase in productivity from any playoff competition dwells between one-third to one-fifth of the effect from doubling the piece-rate incentives. We find that women are more productive in the workers' sample. This result is aligned with the more recent evidence on women's performance in competitive environments in non-WEIRD countries (Gneezy et al., 2009; Dariel et al., 2017), as opposed to the early evidence where woman's performance deteriorates under competition (Gneezy et al., 2003; Niederle and Vesterlund, 2007).

Note that the initial allocation does not have any effect. Column 4 reveals a different story for workers after interacting our treatment arms. Merit-based allocations decrease productivity for the random reallocation of contracts in about 0.75 completed tasks. Still, it seems to increase productivity for the treatments with playoffs in about 0.3 to 0.4 completed tasks. Nonetheless, the sum of the merit coefficient with each interaction is not statistically significant. Column 6 reveals that the initial allocation does not affect productivity, and by interacting this treatment arm with contract reallocation, the predictive power of both treatments is gone.

Summing up, we find partial evidence for H1 and H2. Regarding H1, contests capturing labor mobility increase productivity with respect to a random reallocation of contracts. However, we do not find evidence that the *Playoff-Noisy* condition induces greater productivity than the *Playoff-Perfect* condition. The overall effects seem to be similar regardless of the type of playoff. Regarding H2, the *Merit* condition by itself does not predict higher productivity. However, the reason is that its effect depends on its interaction with labor mobility. A merit-based initial allocation is counterproductive when future contract allocations do not depend on current effort. By contrast, this merit-based allocation increases productivity when labor mobility is high. In other words, H2 is only supported when mobility rules provide dynamic incentives to hold Contract A in the future.

A separate look at the workers' and students' samples reveal that the effects of *Merit* are driven by the former group. Since the workers are expected to be more heterogeneous in abilities, we conjecture that the initial conditions are more important for them. This reasoning could explain the interaction effects between *Merit* and the reallocation treatments. By contrast, students might infer less heterogeneity in their abilities and are more used to competitive environments. Therefore,

Table 3: OLS results for the determinants of productivity by type of participant.

	Number of completed tasks in 120 seconds					
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	All		Workers		Students	
Initial allocation						
Merit	0.034	-0.409*	0.011	-0.748**	0.136	0.071
	(0.128)	(0.213)	(0.200)	(0.351)	(0.165)	(0.264)
Reallocation						
Perfect	0.313**	0.069	0.427*	-0.140	0.060	0.217
	(0.154)	(0.203)	(0.233)	(0.327)	(0.190)	(0.253)
Noisy	0.341**	-0.126	0.238	-0.438	0.495**	0.237
	(0.157)	(0.249)	(0.230)	(0.383)	(0.206)	(0.317)
Merit × Perfect		0.454		1.082**		-0.313
		(0.303)		(0.496)		(0.373)
Merit × Noisy		0.846***		1.196**		0.481
		(0.308)		(0.472)		(0.405)
Contract A	1.702***	1.707***	1.809***	1.810***	1.527***	1.509***
	(0.124)	(0.124)	(0.180)	(0.183)	(0.162)	(0.161)
Worker	-0.173	-0.174				
	(0.131)	(0.130)				
Women	0.299**	0.273**	0.448**	0.397**	0.024	-0.039
	(0.139)	(0.138)	(0.190)	(0.187)	(0.196)	(0.200)
Constant	9.756***	10.012***	9.013***	9.557***	10.343***	10.456***
	(0.282)	(0.288)	(0.423)	(0.457)	(0.347)	(0.358)
Observations	1,020	1,020	570	570	450	450
R-squared	0.242	0.248	0.248	0.259	0.272	0.278
p-values for F-tests on linear combinations of coefficients						
Perfect - Noisy = 0	(0.811)		(0.403)		(0.032)	
Merit + Merit × Perfect = 0		(0.826)		(0.338)		(0.358)
Merit + Merit × Noisy = 0		(0.064)		(0.161)		(0.079)

Additional controls in all models: age, type of computer, mouse, risk parameter and quiz score. Round fixed effects included. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

they react more to the tournament system with higher mobility, the *Playoff-Noisy* condition.

4.3 Sorting and labor mobility

We learned that the higher piece-rate payment from Contract A induces greater productivity compared to Contract B. In this section, we explore two "dynamic" components from our contracts. First, we analyze the degree of sorting induced by our labor mobility rules. We aim to understand to which extent the productivity gap between contracts is due to better coders consistently holding Contract A. Second, following H3, we study whether labor mobility increases productivity among the promoted participants more than the equivalent decrease among the demoted ones.

We explore these questions using an OLS model. The dependent variable is the number of completed tasks in round t, and the independent variables of interest correspond to "contract transitions" between $t-1$ and t. Here, A-A captures the additional productivity from participants ranked first in $t-1$, holding Contract A in t, with respect to players ranked last (B-B, the excluded group). The interaction between A-A and the playoff captures the difference in productivity between those ranked first in $t-1$, and those ranked second that kept Contract A in t by winning the playoff. The variable A-B captures demoted participants, and B-A the promoted ones. Finally, the interaction between B-B and playoff captures the difference in productivity between those ranked third in $t-1$ that lost the playoff and those ranked fourth in $t-1$.

Table 4 reports the coefficients of this regression exercise. Each column corresponds to a different reallocation treatment. The sorting is evident in Columns 1 and 2, involving the perfect and noisy playoff. Let us start with Column 1, for the *Playoff-Perfect* condition. B-B participants completed on average 9.4 tasks, whereas A-A participants completed 4.5 additional tasks (an increase of 48%). A-A players who went through the playoff are slightly less productive in t compared to A-A players (-0.9 tasks), whereas those that were promoted (B-A) completed 1.5 fewer tasks than A-A players. The distance between A-A players and those demoted (A-B) is much larger since the latter completed around 2.7 fewer tasks.

In Column 2, for the *Playoff-Noisy* condition, we observe similar–though smaller–differences between contract transitions. A-A players are 41% more productive than B-B players, those with Contract A that went through the playoff are slightly less productive than those who do not (-0.6 tasks), whereas the promoted players completed roughly one fewer task than the A-A players. On the other hand, the demoted players completed 1.5 fewer tasks than the A-A players. We also have evidence of sorting in this treatment condition. However, the smaller differences in productivity with respect to A-A players suggest that the noisy playoff effectively induces a higher degree of mobility.

Finally, Column 3 serves as a placebo test. Recall that in the *Random* reallocation treatment *all* the participants switch contracts regardless of their current productivity or contract type. Hence, by construction, we should not observe any sorting: A-A players should not differ from B-A players, and A-B players should not differ from B-B players (whose productivity is captured in the constant). The results validate this placebo test. None of the coefficients capturing contract transitions predict productivity.

Overall, these results provide partial support for H3: B-A participants are more productive

Table 4: OLS results for the effect of *contract reallocation* on the current productivity

	Completed tasks in the current round				
	(1)	(2)	(3)	(4)	(5)
	All participants			Playoff participants	
VARIABLES	Playoff-Perfect	Playoff-Noisy	Random	Playoff-Perfect	Playoff-Noisy
Contract transition					
A-A	4.544***	3.372***	0.287	1.961***	0.765*
	(0.359)	(0.355)	(0.312)	(0.413)	(0.459)
A-B	1.778***	1.476***	0.388	0.128	-0.573
	(0.388)	(0.397)	(0.301)	(0.371)	(0.537)
B-A	3.012***	2.354***	0.389	1.280***	0.295
	(0.414)	(0.439)	(0.300)	(0.373)	(0.553)
A-A $\times$ Playoff	-0.925***	-0.610*			
	(0.325)	(0.361)			
B-B $\times$ Playoff	1.368***	1.865***			
	(0.350)	(0.407)			
Merit	-0.016	0.425*	-0.359	0.142	0.146
	(0.202)	(0.252)	(0.232)	(0.277)	(0.346)
Worker	-0.044	-0.409	-0.281	-0.006	-0.558*
	(0.212)	(0.263)	(0.221)	(0.296)	(0.335)
Constant	9.414***	8.286***	12.114***	10.779***	10.060***
	(0.398)	(0.415)	(0.527)	(0.522)	(0.516)
Observations	256	292	268	124	143
R-squared	0.555	0.418	0.135	0.392	0.305
p-values for F-tests on linear combinations of coefficients					
A-A = B-A	(0.000)	(0.042)	(0.752)	(0.068)	(0.317)
B-A = A-B	(0.002)	(0.078)	(0.996)	(0.003)	(0.080)

Additional controls in all models: age, type of computer, mouse, risk parameter and score in the instructions quiz. Round fixed effects included. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

than A-B participants in treatments with playoffs, even though they have just switched contracts, but not on the *Random* reallocation treatment. We validate that piece-rate incentives are fundamental, even among very similar participants, as they explain the differences in productivity between promoted and demoted participants.

An unexpected result observed in Table 4 is the effect of not losing Contract A in the playoff. These A-A participants are more productive than the B-A participants. This result is presented more clearly in Columns 4 and 5, where we block sorting effects by excluding participants ranked first and last in each group. Here, A-A means being second and winning the playoff, and the constant captures players ranked third that lost the playoff. For the *Playoff-Perfect* condition, the B-A coefficient is larger than the A-B coefficient, confirming H3. However, our main interest dwells on comparing the A-A and the B-A coefficients. Since the former is larger, we confirm that participants keeping Contract A after the playoff increase their effort in the following round. The most straightforward interpretation for this pattern is that being close to losing this contract was a "threat" that raised their productivity. In the *Playoff-Noisy* condition, we also validate H3. However, entering the playoff did not increase the productivity among those that "almost lose" Contract A. Hence, this alarm for demotion appears when mobility chances are low.

We performed an additional regression exercise in which the dependent variable is the difference in the participants' productivity between periods t and $t-1$, explained by the held contracts in periods t and $t-1$. This regression "in differences" is more demanding, as any predictor variable that results statistically significant depends on within-subjects variation in productivity. Table 5 reports the results for each reallocation treatment. We find that contract transitions are not statistically significant in the *Playoff-Perfect* and (as expected) in the *Random* conditions. We argue that the differences in productivity reported in Table 4 reveal the lower mobility in the *Playoff-Perfect* condition, leading to a higher sorting. Consequently, productivity gaps across ranking positions remain stable over time. In the *Playoff-Noisy* condition, we confirm that players holding Contract A after winning the playoff increased their productivity with respect to themselves in the past. By contrast, players losing the playoff while holding Contract B decrease their productivity in the following round.

Moreover, we find that the A-B and B-A coefficients are non-significant in the playoff treatments. Hence, promoted and demoted players do not alter their productivity sufficiently to predict that switching contracts affects their performance regarding the previous round. We reconcile these results with those displayed in Table 4 by confirming that our tournament structure effectively sorts participants based on their productivity. However, most of the differences when switching contracts do not come from a boost (or a drop) in their motivation that changes their performance, but rather from the differences when they are compared with other participants.

4.4 Robustness checks and additional results

Measuring the effect of "labelling" the reallocation rules

We mention in Section 2 that the three reallocation rules become identical when the players ranked second and third completed the same number of transcriptions. Although we only observe this outcome about 6% of the time, because Contract A typically induced higher productivity, we

Table 5: OLS results for the effect of *contract reallocation* on the productivity change.

	Tasks in the current round - Tasks in the previous round					
	(1)		(2)		(3)	
VARIABLES	Playoff-Perfect		Playoff-Noisy		Random	
Contract transition						
A-A	-0.305	(0.366)	-0.835**	(0.326)	-0.067	(0.274)
A-B	0.128	(0.386)	0.075	(0.506)	-0.166	(0.195)
B-A	-0.279	(0.361)	-0.530	(0.340)	0.150	(0.198)
A-A $\times$ Playoff	0.322	(0.243)	1.107***	(0.293)		
B-B $\times$ Playoff	-0.217	(0.361)	-0.532*	(0.295)		
Merit	-0.259	(0.195)	0.024	(0.219)	0.050	(0.210)
Worker	0.060	(0.200)	-0.064	(0.216)	0.133	(0.146)
Constant	0.217	(0.374)	0.586*	(0.334)	-0.074	(0.452)
Observations	256		292		268	
R-squared	0.069		0.084		0.046	

Additional controls in all models: age, gender, type of computer, mouse, risk parameter and quiz score. Round fixed effects included. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

use these 59 observations to study what happened in the following round. Table A.3 reports the coefficients for this regression. Qualitatively, the results are similar to Table 3, but some of the variables lost their statistical significance due to the reduced sample size. We thus focus on the two statistically significant variables. First, the *Playoff-Perfect* variable reveals higher productivity (1.7 additional tasks) in the following round (see model 1). Second, when we include cross-treatment effects, *Merit* becomes more negative (-1.4) and statistically significant. We argue that our results hold qualitatively for this particular scenario, where the only difference between treatments is how the contract reallocation is labeled.

Aggregate effects

A complementary question is how reallocation rules affect the group's productivity, measured as the number of completed tasks from the four members in a given round. It is not clear whether the effects of labor mobility are sufficiently large to increase the group's total number of completed tasks, despite the productivity drops caused by demotions and lack of mobility for holders of Contract B. We report in Table A.4, in the Appendix, the OLS coefficients from this regression. Following model 1, merit-based allocations increase the group's productivity in 1.8 tasks. However, as we previously show, the effects of *Merit* are intertwined with reallocation rules. Following model 2, in the *Playoff-Perfect* and *Playoff-Noisy* conditions, merit-based allocations increase the group's productivity in 2.1 and 4.4 tasks, respectively. We thus argue that labor mobility increases total productivity, conditional on a meritocratic initial allocation. The combined effects of *Merit* and playoffs are mostly driven by the workers' sample (see models 3-4), whereas the effect of merit-based allocations is driven by students (see model 5).

Differences between formal and informal workers

We conducted a regression similar to Table 3 in which we include as predictors the categorical variables indicating *(i)* whether participants were in the contributive health system, *(ii)* were currently contributing to their pension scheme, and *(iii)* whether their monthly earnings were above the minimum wage. These three variables are good proxies of being a formal worker. Table A.5 in the Appendix reports these results. We find that contributions to health and pension schemes predict a higher number of completed tasks for the sample of workers, but not in the sample of students. Our interpretation of this result is that formal workers are more likely to have higher productivities, revealing that pre-existing differences in typing skills do matter in this sample, even if the employed task was novel to everyone and minimized learning.

5 Conclusions

We designed and conducted an online experiment in which participants receive one of two types of contracts, paying different piece-rates for solving the same transcription task. We vary the initial allocation and reallocation rules of both contracts to study how labor mobility affects productivity, defined as the number of correctly solved tasks within a time limit. Our *Playoff-Perfect* and *Playoff-Noisy* treatments enable labor mobility based on rewarding the best performers holding Contracts A and B. The reward for the top-performer with Contract A was to keep this contract for the next round. The reward for the top-performer with Contract B was to enter a contest, giving her a chance to switch contracts with the bottom-performer holding Contract A. In the *Playoff-Perfect* condition, expected mobility was low because the contestant with the higher productivity was the winner. Since holders of Contract A typically were more productive due to the higher piece-rate incentive, they had an advantage in this type of playoff. In the *Playoff-Noisy* condition, expected mobility was high because the odds of winning the playoff were proportional to the number of completed tasks, giving a higher chance to the contestant with Contract B. Both reallocation rules are compared to baseline condition in which contracts were reallocated every round, regardless of the participants' productivity.

We find that both treatments with playoffs encourage participants to provide more effort. However, the effects of each playoff type seems to affect more one type of population. The effects among workers are driven by the *Playoff-Perfect* treatment, whereas the effects among students are driven by the *Playoff-Noisy* treatment. Moreover, the effects for workers are dependent on the rule dictating the initial contract allocation. Merit-based allocation increases productivity, as long as productivity is rewarded through labor mobility rules. We argue that workers rely more on signals hinting less competitive contests (i.e., perfect contest discrimination and the meritocratic contract allocation) for two reasons. First, they may expect a higher dispersion in productivity between participants compared to the sample of students. Second, there is more heterogeneity in how competitive is their environment compared to University students. For this reason, the latter are more likely to increase their effort when contests offer more mobility opportunities.

The evident following question is how these results are informative outside the controlled environment offered by the laboratory. Perhaps the most important conclusion from our experiment is that the perception of mobility is important in the provision of effort, which ultimately increases productivity. If exclusion partly explains the existence of dual labor markets, signals that this exclusion can be overcome are fundamental to encourage such efforts.

Our tournament structure was useful to introduce and convey contract mobility in dual labor markets. In addition, we learned about how being close to lose Contract A also increased productivity, one more advantage of perceiving "downward" mobility. However, the tournament format comes at the cost of implicitly assuming that labor mobility is a zero-sum game: for each promoted participant to Contract A, another participant is demoted to Contract B. Future research could introduce new rules for contract reallocation. For instance, by promoting holders of Contract B, without demoting bottom-performers of Contract A, if a productivity threshold is met.

In this paper, we focus on the exclusion from the primary labor market, leaving aside the study of self-selection into the secondary labor market. This self-selection has been explored on a related and broader literature on tax compliance. Future experiments may combine exclusion and voluntary exit. For instance, to explore whether the perception of exclusion can serve as a self-deception mechanism to opt for the secondary labor market without incurring in self-image costs from tax avoidance. Imagine a setting where piece-rate payments depend on the productivity ranking, and the existence of a flat tax rate makes profitable to select the primary labor market only if one expects a minimum ranking position. Participants may self-deceive, arguing that the reason for going directly to the secondary market, without taxes, was their fear of ending up very low in the ranking.

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Appendix

A Additional Tables

Table A.1: Balance between treatments: Luck and Merit conditions

	(1) Obs.	(2) Mean	(3) Std. Dev	(4) Mean Luck	(5) Mean Merit	(6) p-value Diffs. (4) vs (5)
Age	207	21.35	(1.98)	21.21	21.48	0.33
Female	205	0.67	(0.47)	0.63	0.71	0.20
Risk taker (staircase)	207	14.03	(7.4)	14.48	13.62	0.40
Quiz score	205	1.33	(0.64)	1.36	1.31	0.59
Minimum Wage	207	0.46	(0.5)	0.41	0.51	0.17
Education Level	207					0.62
High school		0.22	(0.42)	0.25	0.19	
Technical/Technological		0.09	(0.28)	0.08	0.09	
Undergraduate		0.60	(0.49)	0.60	0.60	
Posgraduate		0.09	(0.29)	0.07	0.11	
Occupation	207					0.41
Only student		0.38	(0.49)	0.36	0.40	
Unemployed		0.04	(0.2)	0.07	0.02	
Full-time		0.20	(0.4)	0.16	0.24	
Part-time		0.17	(0.38)	0.18	0.17	
Self-employed		0.15	(0.36)	0.17	0.13	
Unpaid worker		0.01	(0.12)	0.01	0.02	
Other		0.03	(0.18)	0.04	0.03	
Health Contribution	207	0.68	(0.47)	0.71	0.66	0.45
Pension Contribution	207	0.64	(0.48)	0.66	0.63	0.69
Desktop PC (0=*Laptop*)	207	0.08	(0.27)	0.08	0.07	0.86
Mouse	207	0.51	(0.5)	0.46	0.55	0.24
Quality internet connection	207	0.92	(0.27)	0.95	0.90	0.17

Willingness to take risks elicited using Falk et al.'s (2018) staircase procedure.

Table A.2: Balance between treatments: Random, Playoff-Perfect, Playoff-Noisy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Mean			p-value Diff.	
	Obs.	Mean	St. Dev	Random	Playoff-Perfect	Playoff-Noisy	(4) vs (5)	(4) vs (6)	(5) vs (6)
Age	207	21.35	(1.98)	21.22	21.64	21.22	0.23	0.98	0.22
Female	205	0.67	(0.47)	0.69	0.69	0.64	0.94	0.60	0.55
Risk taker (staircase)	207	14.03	(7.4)	14.36	13.88	13.88	0.71	0.71	1.00
Quiz score	205	1.33	(0.64)	1.42	1.37	1.22	0.65	0.08	0.16
Minimum Wage	207	0.46	(0.5)	0.46	0.47	0.46	0.94	0.97	0.90
Education Level	207						0.68	0.3	0.46
High school	207	0.22	(0.42)	0.16	0.20	0.30			
Technical/Technological	207	0.09	(0.28)	0.09	0.11	0.07			
Undergraduate	207	0.60	(0.49)	0.67	0.58	0.55			
Posgraduate	207	0.09	(0.29)	0.07	0.12	0.08			
Occupation	207						0.57	0.32	0.6
Only student		0.38	(0.49)	0.36	0.39	0.39			
Unemployed		0.04	(0.2)	0.04	0.06	0.03			
Full-time		0.20	(0.4)	0.22	0.18	0.20			
Part-time		0.17	(0.38)	0.15	0.14	0.23			
Self-employed		0.15	(0.36)	0.12	0.20	0.14			
Unpaid worker		0.01	(0.12)	0.03	0.02	0.00			
Other		0.03	(0.18)	0.07	0.02	0.01			
Health Contribution	207	0.68	(0.47)	0.64	0.61	0.78	0.67	0.06	0.02
Pension Contribution	207	0.64	(0.48)	0.66	0.68	0.59	0.76	0.45	0.29
Desktop PC (0=*Laptop*)	207	0.08	(0.27)	0.07	0.12	0.04	0.37	0.39	0.08
Mouse	207	0.51	(0.5)	0.45	0.52	0.55	0.44	0.21	0.65
Quality internet connection	207	0.92	(0.27)	0.97	0.88	0.92	0.05	0.19	0.43

Willingness to take risks elicited using Falk et al.'s (2018) staircase procedure.

Table A.3: OLS results for the determinants of productivity participants ranking 2-3

	Number of completed tasks in 120 seconds			
VARIABLES	(1)		(2)	
Initial allocation				
Merit	-0.240	(0.739)	-1.352**	(0.668)
Reallocation				
Perfect	1.655***	(0.613)	0.276	(1.157)
Noisy	1.148	(0.864)	0.336	(1.246)
Merit × Perfect			2.299	(1.435)
Merit × Noisy			1.750	(1.746)
Contract A	0.514	(0.515)	0.432	(0.490)
Worker	0.746	(0.510)	0.970	(0.633)
Female	0.581	(0.518)	0.780	(0.609)
Risk taker (staircase)	0.026	(0.037)	0.047	(0.040)
Constant	8.918***	(1.315)	8.773***	(1.308)
Observations	59		59	
R-squared	0.209		0.266	

Additional controls in all models: age, gender, type of computer, mouse, risk parameter and quiz score. Round fixed effects included. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.4: OLS results for the determinants of group productivity by type of participants.

	Number of completed tasks in 120 seconds per group					
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	All		Workers		Students	
Initial allocation						
Merit	1.792***	-1.499	-0.210	-4.931***	2.983***	2.529*
	(0.605)	(1.026)	(0.917)	(1.239)	(0.839)	(1.409)
Reallocation						
Perfect	0.468	-1.443	0.355	-1.997	0.698	0.581
	(0.744)	(1.235)	(1.030)	(1.308)	(1.212)	(1.921)
Noisy	0.334	-3.039**	0.218	-6.187***	2.593***	1.953
	(0.782)	(1.324)	(1.428)	(2.171)	(0.944)	(1.616)
Merit $\times$ Perfect		3.623**		4.618**		0.195
		(1.440)		(2.134)		(2.022)
Merit $\times$ Noisy		5.954***		9.821***		1.238
		(1.415)		(2.102)		(1.885)
Constant	39.816***	42.086***	32.730***	39.628***	46.733***	47.042***
	(2.289)	(2.384)	(3.365)	(3.638)	(3.739)	(3.767)
Observations	265	265	127	127	138	138
R-squared	0.319	0.363	0.380	0.484	0.404	0.406
p-values for F-tests on linear combinations of coefficients						
Merit + Merit $\times$ Perfect = 0		(0.034)		(0.848)		(0.060)
Merit + Merit $\times$ Noisy = 0		(0.000)		(0.004)		(0.012)

Additional controls in all models [group means]: share of women, age, type of computer, mouse, risk parameter and quiz score. Round fixed effects included. Additional control in models 1-2: share of workers per group. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.

Table A.5: OLS results for the determinants of productivity by type of participant - SOCIAL SECURITY.

	Number of completed tasks in 120 seconds					
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	All		Workers		Students	
Initial allocation						
Merit	0.038	-0.340	0.015	-0.685*	0.120	0.117
	(0.128)	(0.219)	(0.201)	(0.361)	(0.172)	(0.269)
Reallocation						
Perfect	0.315**	0.152	0.447*	-0.021	0.032	0.181
	(0.153)	(0.208)	(0.231)	(0.330)	(0.191)	(0.259)
Noisy	0.306*	-0.137	0.018	-0.645*	0.479**	0.337
	(0.160)	(0.253)	(0.240)	(0.391)	(0.210)	(0.323)
Merit × Perfect		0.299		0.895*		-0.282
		(0.321)		(0.530)		(0.372)
Merit × Noisy		0.799**		1.188**		0.275
		(0.313)		(0.482)		(0.413)
Contract A	1.670***	1.678***	1.709***	1.723***	1.489***	1.478***
	(0.124)	(0.124)	(0.182)	(0.182)	(0.163)	(0.162)
Worker	-0.299**	-0.315**				
	(0.152)	(0.153)				
Health Contribution	0.039	0.064	0.370*	0.411*	-0.224	-0.213
	(0.139)	(0.140)	(0.219)	(0.219)	(0.195)	(0.198)
Pension Contribution	0.438***	0.416**	0.697***	0.636***	-0.415	-0.345
	(0.165)	(0.172)	(0.215)	(0.226)	(0.254)	(0.259)
Wage above minimum	-0.099	-0.049	-0.101	-0.034	-0.406*	-0.368*
	(0.140)	(0.144)	(0.194)	(0.205)	(0.214)	(0.219)
Constant	9.714***	9.926***	8.802***	9.253***	10.630***	10.663***
	(0.291)	(0.299)	(0.448)	(0.477)	(0.367)	(0.379)
Observations	1,020	1,020	570	570	450	450
R-squared	0.249	0.254	0.271	0.281	0.286	0.289
p-values F tests						
Perfect - Noisy = 0	(0.952)		(0.069)		(0.030)	
Merit + Merit × Perfect = 0		(0.856)		(0.557)		(0.537)
Merit + Merit × Noisy = 0		(0.042)		(0.126)		(0.232)

Additional controls in all models: gender, age, type of computer, mouse, risk parameter and instructions quiz score. Round fixed effects included. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

B Experimental Protocol: Translated Version

Below you will find the translated version of the protocol. Variations between treatments are written in brackets, and are color-coded:

- **Initial allocation of contracts:** We use *purple* for the *Merit*(-based) condition, and *teal* for the *Random* condition.
- **Contract reallocation:** We use *gray* for the *Random* condition, *brown* for the *Playoff-Perfect* condition, and *orange* for the *Playoff-Noisy* condition.

General Instructions

In this activity, your task consists of encoding sequences of *numbers* into *letters*. Your payoff depends on luck and the number of correctly solved sequences by you and other participants. You will participate in a practice round and then in a tournament with five rounds.

- **Practice: Round 0.** The purpose of this round is to familiarise yourself with the task. You will have 120 seconds to encode as many *sequences* as you can from numbers to letters.
- **Tournament: Rounds 1 to 5.** In each round, you will have 120 seconds to encode as many *sequences* as you can, from numbers to letters. Your round payoff depends on the number of correctly-solved sequences and the type of contract you have.

There are two types of contracts: *Contract A* and *Contract B*. The task is the same in both contracts, but the piece-rate in *Contract A* is twice the value of the piece-rate in *Contract B*. You will be part of a group with three other people, four people in total. Two of them will receive *Contract A* and the other two *Contract B*.

Why we call it a "Tournament"?

Your contract could be changed at the end of each round, depending on your performance and luck. The implication is that:

- If you have *Contract A*, you can be demoted to *Contract B*.
- If you have *Contract B*, you can be promoted to *Contract A*.

After the practice round, we will explain in detail the rules for switching contracts.

Additional bonus

Before the end of the activity, we will present you with five situations in which you can earn an additional payoff, depending on your decisions and luck.

Payoffs for this activity

Your payoff for the entire activity corresponds to the round's earnings in ONE round, randomly chosen from the five tournament rounds, plus COP 10,000 for completing the activity and the survey. Keep in mind that you could have an additional gain.

Please chat with us if you need help. An instructor will contact you individually to help.

To begin the activity, please introduce the initial letters of your first name and last name, followed by your birth date. For instance, if your name is Lina Ríos and you were born on 11 February 1995, you will have to type LR11021995. This tag is important to ensure your participation in the rest of the activity and the payment assignment. [Participant code]

Task Description

You will see a number sequence that you have to translate into a series of letters (Z, D, J, K, L). Each **NUMBER has its own LETTER**, you will have to type the corresponding letter as an answer in each box.

Each translation of 5 numbers into their respective letters will be called a sequence. Each time you complete a sequence, *click* the button "Send". Should the translation have any mistake, the computer will ask you to correct it. If there are no mistakes in the translation, the sequence will be complete.

When a sequence is complete, the correspondence between NUMBERS and LETTERS will change. This means that each translation is different from the previous one.

How to perform the translation?

Below you can see the screen you will observe during the translation task.[See Figure 1]

You can find a table with the keys to translate numbers into letters in the upper part. You can find a table with 5 numbers and 5 empty boxes in the bottom part. You will have to type the LETTER that corresponds to each NUMBER.

We want to remind you that there are only five letters in the translation, and they will be the same in all the rounds. The following image shows the location of these five letters. You can see below and check with your keyboard.

You will have 120 seconds to complete correctly as many sequences as you can. The first time, you will perform the task for practice purposes.

Please, press "next" to start. Once you *click*, time will start running for the practice round.

Practice Round

You will dispose of 120 seconds to practice with the task you have to perform in the following rounds.

In the *Tournament* rounds, your performance and luck can influence the allocation of your contract. For this reason, it is important to practice and try to complete as many sequences as you can. At the end of this round, half of the participants will be assigned to *Contract A* and the other half to *Contract B*.

How will be the initial allocation of contracts for the Tournament?

[According to your performance in this practice round. The first half of the participants, who completed the highest number of sequences, will receive *Contract A*. Participants ranked in the other half will receive *Contract B* / Randomly. In other words, your likelihood of receiving either *Contract A* or *Contract B* is the same. Use this practice round to familiarise yourself with the task and test your skills.]

Tournament

Following, you will participate in a tournament of 5 rounds. In each round, you will be part of a group of four (4) people: two (2) will have a "*Contract A*" and two (2) will have a "*Contract B*." The other three (3) members of your group will change round after round. There will always be two *Contract A* and two *Contract B* in each group.

In each round, you will have 120 seconds to complete as many sequences as you can. The payment for each completed sequence will depend on the contract you were assigned to at the end of the previous round:

- *Contract A*: receive $3,000 per completed *sequence*.
- *Contract B*: receive $1,500 for completed *sequence*.

Ranking and probability of receiving a Contract A in the next round

This is a tournament because it ranks the participants with *Contract A* and *Contract B* in their group [, and defines who will receive the contracts for the next round / , and defines who will receive the contracts for the next round].

Ranking	Position and Contract	Likelihood Contract A
1	First position of participants with Contract A	[50% / 100 % / 100 %]
2	Second position of participants with Contract A	[50% / Playoff / Playoff]
3	First position of participants with Contract B	[50% / Playoff / Playoff]
4	Second position of participants with Contract B	[50% / 0 % / 0 %]

If two participants with the same contract have the same number of completed *sequences*, the first and second places in the contract are assigned randomly.

According to the third column of the table, [**the likelihood to switch contracts is the same for all participants**. That is, it is equally likely to receive either *Contract A* or *Contract B*. / the participant Ranked 1st retains *Contract A*, and the participant Ranked 4th retains *Contract B*. On the other hand, participants ranked 2nd and 3rd may switch contracts in the playoff. / the participant Ranked 1st retains *Contract A*, and the participant Ranked 4th retains *Contract B*. On the other hand, participants ranked 2nd and 3rd may switch contracts in the playoff.]

What is the playoff?

[The allocation of *Contract A* depends on the number of completed sequences made by the participants ranked 2nd and 3rd. **The one who completed more *sequences* among the participants in the Playoff gets *Contract A***. If both participants get the same number of complete sequences, the probability of winning *Contract A* is the same for both participants (50%). / The allocation of *Contract A* depends on the number of completed sequences made by the participants in the Ranking **2** and **3**. Obtaining *Contract A* depends on a draw. **The higher the number of sequences with respect to the other participant in the playoff, the higher the likelihood of winning *Contract A***. If both participants complete the same number of sequences, the likelihood of winning Contract A is the same for both participants (50%). The exact equation is as follows:

$$\text{Probability Contract A} = \frac{\text{Your sequences}}{\text{Your sequences } + \text{The contestant's sequences}}.$$

]

Comprehension quiz

Questions:

1. [You complete 6 sequences and the other participants also complete 6 sequences. / You complete 6 sequences and the other participant in the playoff completes 4 sequences. / You complete 6 sequences and the other participant in the playoff completes 4 sequences.] Your probability of receiving Contract A in the next round will be:

0% , 30%, **50%**, **60%**, 90% ó **100%**

2. [You complete 6 sequences and the other participants also complete 6 sequences. / You complete 6 sequences and the other participant in the playoff also completes 6 sequences. / You

complete 6 sequences and the other participant in the playoff also completes 6 sequences.] Your probability of having Contract A in the next round is:

0% , 30%, **50%**, 60%, 90% ó 100%

C Experimental Protocol: Original Version (Spanish)

Las variaciones en el protocolo están escritas en corchetes, y están codificadas por colores:

- **Asignación inicial de contratos:** Usamos el color *púrpura* para la condición basada en el *Merito*, y el color *verde azulado* para la condición *Aleatoria*.
- **Reasignación de contratos:** Usamos el color *gris* para la condición *Aleatoria*, *pardo* para la condición *Repechaje-Perfecto*, y *naranja* para la condición *Repechaje-Ruidoso*.

Instrucciones Generales

En esta actividad su tarea consiste en pasar secuencias de *números* a *letras*. Su pago depende del azar, y de la cantidad de secuencias correctas que usted y otros participantes realicen. Usted participará en una ronda de práctica, y luego en un Torneo de 5 rondas.

- **Práctica: Ronda 0.** El propósito es que se familiarice con la tarea. Usted tendrá 120 segundos para pasar la mayor cantidad de *secuencias* que pueda de números a letras.
- **Torneo: Rondas 1 a 5.** En cada ronda, usted tendrá 120 segundos para pasar la mayor cantidad de *secuencias* que pueda de números a letras. Su pago en cada ronda depende del número de secuencias correctas que haga y del tipo de contrato que tenga.

Hay dos tipos de contrato: *Contrato A* y *Contrato B*. La tarea es la misma con ambos contratos, pero el pago por *secuencia* correcta con el *Contrato A* es el doble que con el *Contrato B*. Usted estará en un grupo con otras tres personas, cuatro en total. Dos recibirán el *Contrato A* y los otros dos el *Contrato B*.

¿Por qué le llamamos Torneo?

Al final de cada ronda podrá cambiar de contrato, según su rendimiento y la suerte. Esto quiere decir que:

- Si tiene el *Contrato A*, puede ser relegado a tener el *Contrato B*.
- Si tiene el *Contrato B*, puede ser promovido a tener el *Contrato A*.

Luego de la ronda de práctica explicaremos en detalle los cambios de contrato.

Ganancia adicional

Antes de finalizar se le presentarán 5 situaciones en las que dependiendo de sus decisiones y del azar podrá obtener una ganancia adicional.

Pagos de la actividad

Su pago por toda la actividad corresponde a las ganancias en UNA de las cinco rondas del Torneo elegida al azar y $10.000 pesos por completar la actividad y responder la encuesta. Recuerde que podrá tener una ganancia adicional.

Por favor, escribanos por chat si necesita ayuda. Uno de los monitores se comunicará con usted y tratará de ayudarlo de manera individual.

Para iniciar por favor ingrese las iniciales de su primer nombre y apellido seguido de su fecha de nacimiento. Por ejemplo, si usted se llama Lina Ríos y usted nació el 11 de febrero de 1995, debe ingresar LR11021995. Escriba todo en mayúscula. Este código es importante para asegurar su participación en el resto de la actividad y la realización de los pagos. [Código de participante]

Descripción de la tarea

A continuación, usted va a ver una secuencia de números que debe traducir a una serie de letras (Z, D, J, K, L). A cada **NÚMERO le corresponde una LETRA**, debe escribir esta letra como respuesta en cada casilla.

A cada traducción de 5 números en sus respectivas letras le llamaremos secuencia. Cada vez que complete una secuencia, haga *clic* en el botón "Enviar". Si la traducción tiene errores, el computador le pedirá que los corrija. Si la traducción no tiene errores, la secuencia quedará completa.

Al terminar una secuencia cambiarán los NÚMEROS y las LETRAS asociadas. Es decir, cada traducción es diferente a la anterior.

¿Cómo hacer la traducción?

Debajo puede ver la pantalla que observará durante la tarea de traducción [Vea la figura 1]

En la parte de arriba encuentra una tabla con las claves para traducir a letras. En la parte de abajo encuentra una tabla con 5 números y 5 casillas vacías. Usted debe escribir la LETRA que corresponde a cada NÚMERO.

Queremos recordarle que sólo hay cinco letras en la traducción, y serán las mismas en todas las rondas. La siguiente imagen muestra la ubicación de esas cinco letras. Esto lo puede ver debajo y confirmar en su teclado (Ver imagen B).

Usted dispondrá de 120 segundos para completar correctamente todas las secuencias que pueda. Esta vez resolverá la tarea a modo de prueba.

Por favor oprima "Siguiente" para comenzar. Una vez haga *clic* comenzará a correr el tiempo de prueba.

Ronda de Práctica

Usted tendrá 120 segundos para probar la tarea que deberá realizar en las siguientes rondas.

En las rondas del *Torneo* su desempeño y la suerte puede que influyan en la asignación de su contrato. Por esto es importante que practique y trate de completar el mayor número de secuencias que pueda. Al finalizar esta ronda, la mitad de los participantes serán asignados al *Contrato A* y la otra mitad al *Contrato B*.

¿Cómo será la asignación inicial de contratos para el Torneo?

[Según su rendimiento en esta ronda de práctica. La primera mitad de los participantes, quienes completaron el mayor número de secuencias, recibirán el *Contrato A*. Los participantes clasificados en la otra mitad recibirán el *Contrato B* / Al azar. Es decir, es igual de probable que reciba el *Contrato A* o el *Contrato B*. Aproveche esta ronda de práctica para familiarizarse con la tarea e ir poniendo a prueba sus capacidades.]

Torneo

Usted ahora participará en un torneo por 5 rondas. En cada ronda hará parte de un grupo de cuatro (4) personas: dos (2) tendrán un *Contrato A* y dos (2) un *Contrato B*. Los otros tres (3) miembros de su grupo irán cambiando ronda tras ronda. Siempre habrá dos personas con el *Contrato A* y dos con el *Contrato B* en cada grupo.

En cada ronda tendrá 120 segundos para realizar el mayor número de *secuencias* que pueda. El pago por cada *secuencia* correcta dependerá del contrato al que fue asignado en la ronda anterior:

- *Contrato A*: recibe $3.000 por *secuencia* correcta.
- *Contrato B*: recibe $1.500 por *secuencia* correcta.

Ranking y probabilidad de tener un Contrato A en la siguiente ronda

Este es un torneo porque hace un ranking de los participantes con *Contrato A* y *Contrato B* de su grupo [, y define quién tendrá los contratos la próxima ronda / , y define quién tendrá los contratos la próxima ronda].

Ranking	Puesto y contrato	Probabilidad Contrato A
1	Primer puesto de los participantes con Contrato A	[50% / 100 % / 100 %]
2	Segundo puesto de los participantes con Contrato A	[50% / Reclasificación / Reclasificación]
3	Primer puesto de los participantes con Contrato B	[50% / Reclasificación / Reclasificación]
4	Segundo puesto de los participantes con Contrato B	[50% / 0 % / 0 %]

Si dos participantes con el mismo contrato tienen el mismo número de *secuencias* completas, el primer y segundo puesto del contrato se asignan al azar.

Según la tercera columna de la tabla,[**la probabilidad de cambiar de contrato es la misma para todos los participantes.** Es decir, es igual de probable que reciba el *Contrato A* o el *Contrato B*. / El participante en el Ranking **1** mantiene el *Contrato A*, y el participante en el Ranking **4** mantiene el *Contrato B*. Los participantes en el Ranking **2** y **3** pueden cambiar de contrato en la *Reclasificación*. El participante en el Ranking **1** mantiene el *Contrato A*, y el participante en el Ranking **4** mantiene el *Contrato B*. Los participantes en el Ranking **2** y **3** pueden cambiar de contrato en la *Reclasificación*.

¿En qué consiste la Reclasificación?

[La asignación del *Contrato A* depende del número de secuencias correctas que hayan realizado los participantes en el Ranking **2** y **3**. **Se queda con el *Contrato A* el que haya completado más *secuencias* entre los participantes en la Reclasificación.** Si ambos hacen la misma cantidad de secuencias correctas, la probabilidad quedarse con el *Contrato A* es la misma para ambos los participantes (50%). [La asignación del *Contrato A* depende del número de secuencias correctas que hayan realizado los participantes en el Ranking **2** y **3**. Quedarse con el *Contrato A* depende de un sorteo. **Entre más secuencias correctas respecto a las del otro participante en la Reclasificación, más chances de ganar.** Si ambos hacen la misma cantidad de secuencias correctas, la probabilidad quedarse con el *Contrato A* es la misma para ambos los participantes (50%). Así se ve la fórmula exacta:

$$\text{Probabilidad Contrato A} = \frac{\text{Sus tareas}}{\text{Sus tareas } + \text{Tareas del otro}}.$$

]

Preguntas de control

Preguntas:

1. [Usted hace 6 secuencias, y los otros participantes hacen 4 secuencias cada uno. / Usted hace 6 secuencias, y el otro participante hace 4 secuencias. / Usted hace 6 secuencias, y el otro participante hace 4 secuencias.] Su probabilidad de tener el Contrato A en la siguiente ronda es:

   0% , 30%, **50%**, **60%**, 90% ó **100%**

2. [Usted hace 6 secuencias, y los otros participantes también hacen 6 secuencias cada uno. / Usted hace 6 secuencias, y el otro participante también hace 6 secuencias. / Usted hace 6 secuencias, y el otro participante también hace 6 secuencias.] Su probabilidad de tener el Contrato A en la siguiente ronda es:

   0% , 30%, **50%**, 60%, 90% ó 100%