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ABSTRACT

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The Peter Principle states that, after a promotion, the observed output of promoted employees tends to fall. Lazear (2004) models this principle as resulting from a regression to the mean of the transitory component of ability. Our experiment reproduces this model in the laboratory by means of various treatments in which we alter the variance of the transitory ability. We also compare the efficiency of an exogenous promotion standard with a treatment where subjects self-select their task. Our evidence confirms the Peter Principle when the variance of the transitory ability is large. In most cases, the efficiency of job allocation is higher when using a promotion rule than when employees are allowed to self-select their task. This is likely due to subjects' bias regarding their transitory ability. Naïve thinking, more than optimism/pessimism bias, may explain why subjects do not distort their effort prior to promotion, contrary to Lazear's (2004) prediction.

JEL Classification: C91, J24, J33, M51, M52

Keywords: promotion, Peter Principle, sorting, experiment

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1. INTRODUCTION

The Peter Principle states that in any hierarchical organization, employees tend to rise to their level of incompetence (Peter and Hull, 1969). Lazear (2004) provides a theoretical model showing that the Peter Principle is a necessary consequence of any promotion rule when outcomes are a function of both a permanent and transitory (i.e., random) ability component. When job-specific transitory ability has expected value equal to zero, those promoted most likely had a favorable "random draw" prior to promotion. A regression-to-the-mean effect drives the Peter Principle, resulting in job allocation inefficiency. While apparently akin to adverse selection, a key difference is that the agents themselves (i.e., workers) can be worse off with incentive compatible wages. Thus, it can be in the interests of both principal and agents to avoid this type of inefficient job sorting.

Peter and Hull (1968) and Lazear (2004) provide a variety of real-world examples of the Peter Principle: the decline of faculty productivity after tenure, the lower quality of movie sequels, a relatively less satisfying second experience at a new restaurant. Using data from a large hierarchical British financial sector firm, Barmby et al. (2006) find some evidence that performance declines after promotion. Two-thirds of the fall in post-promotion performance is attributed to the Peter Principle and one third to reduced incentives. We know of no other empirical tests of the Peter Principle, largely due to selection bias when employees are mobile and the difficulty in measuring "luck".[1] The laboratory offers a distinct advantage in this instance. The aim of this paper is to provide an experimental test of the Peter Principle and to measure its robustness to various manipulations of the promotion rule.

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[1] In Barmby et al. (2006), the importance of luck is proxied by a logit model of promotion depending on the distance between the evaluation rating of the individual and the average rating of his current grade.

In our experiment, agents perform a real effort task, with observable outcome determined by ability and transitory (i.e., random) components. If output reaches a standard, the agent is promoted to a more difficult task in the second production stage. The wage profiles of the easy and difficult tasks are such that high ability agents would earn more performing the hard task. Lazear (2004) predicts that the Peter Principle is a function of the relative size of the transitory component, and so we implement three treatments that manipulate the variance of the random term. In an additional treatment, we allow subjects to self-select into the easy or difficult task in the second stage. This constitutes the core contribution of this paper: a comparison of the job matching efficiency of self-selection versus promotion rules. Indeed, Lazear's model predicts that if employees know their permanent ability but firms do not, it is more efficient for employees to self-select their task rather than use an exogenous promotion standard. Furthermore, since job mismatch reduces worker payoffs, strategic effort distortion may result under a promotion rule, because low (high) ability employees should strategically distort their effort downward (upward) in the first stage to ensure efficient task assignment.

Our laboratory environment successfully recreates the Peter Principle, though only when the variance of the transitory term is quite high. This is no only consistent with Lazear (2004), but it also validates our experimental design. We next show that self-selection into one's task is not necessarily more efficient than promotions, except when the variance of the transitory ability is large. This result may be due to subjects being overconfident with respect to the likely draw of their transitory ability component, thus inducing them to inefficiently self-select into the hard task. Lazear (2004) also hypothesizes an effort distortion prior to promotion, as workers attempt to ensure assignment to their optimal task. We find no evidence of effort distortion, contrary to theoretical predictions. Because agent overconfidence does not appear connected to effort choices (as opposed to job choice), this lack of effort distortion is more consistent with the hypothesis that agents are naïve rather than strategic in this environment..

The reminder of this paper is organized as follows. Section 2 presents the theoretical model. Section 3 develops the experimental design. Section 4 details the results and Section 5 discusses these results and concludes.

2. THEORY

In this section, we present briefly the main characteristics and predictions of Lazear's (2004) model. The firm's productive activity consists of an easy (*E*) and a hard (*H*) task. Every agent's ability consists of a permanent component, *A*, and a transitory or random component in period *t*, ε_t, that is *i.i.d.* over time. The principal cannot observe agents' permanent ability, but only $\hat{A} = A + \varepsilon_t$.[2] The agent's productivity on the easy task is: $Y^E = \alpha + \beta(A + \varepsilon_t)$ and on the hard task is: $Y^H = \gamma + \delta(A + \varepsilon_t)$, with $\alpha > \gamma$ and $\beta < \delta$. Thus, a less able agent will be more productive (i.e., have a comparative advantage) when performing the easy task than when performing the hard task. Where these two productivity profiles cross corresponds to the ability level at which the employer and worker are both indifferent between being promoted or not, assuming incentive compatible piece-rate pay for workers.

Consider the following two-stage game. In the first stage of the game, all agents are assigned to the easy task. At the end of the first stage, the principal only observes $\hat{A}$ and

[2] In Lazear (2004)'s general model, the agent is not better informed than the principal. He also considers the case of asymmetric information, which is what we consider given our interest in the self-selection question.

must decide on each agent's task allocation for the second stage of the game. The principal determines a standard, $A*$, such that if $\hat{A} \geq A*$ the agent is promoted to the hard task, and if $\hat{A} < A*$ he again performs the easy task in the second stage.

Lazear's model generates the prediction that, on average, the agents who are promoted have a lower observable outcome after promotion—the Peter Principle. In more formal terms, the conditional expected value of ε_1 for those agents who have been promoted is given by: $E(\varepsilon_1 | A + \varepsilon_1 > A*) = \int_{-\infty}^{\infty} E(\varepsilon | \varepsilon > A* - A) f(A) dA$, which is greater than zero. In contrast, the expected value of ε_2 is zero. As a consequence, there is a predicted decrease in the expected observed ability of the promoted agents: $A + E(\varepsilon_1 | A + \varepsilon_1 > A*) > A + E(\varepsilon_2 | A + \varepsilon_1 > A*)$. Another intuitive prediction of the model is that the Peter Principle will be stronger when the transitory ability component is high relative to one's permanent ability component.[3]

An extension of the model introduces strategic agent behavior. When agents have perfect information on their permanent ability, they can manipulate their effort to influence their assignment to a specific task. Let e_E and e_H denote the effort levels chosen by an agent on the easy and hard tasks, respectively, with $c(e)$ the associated cost incurred. Since the expected value of the transitory component is 0, agents prefer the hard task if the following holds: $\alpha + \beta(A + e_E) - c(e_E) < \gamma + \delta(A + e_H) - c(e_H)$. The testable implication is that, under a promotion rule, an agent will strategically distort his level of effort downward (upward) if his permanent ability is low (high). Intuitively, the agent distorts

[3] When transitory components exist, Lazear (2004) shows that promotion cutoffs will be adjusted to take into account the Peter Principal. Because we seek to examine agent behavior in this paper, we exogenously impose a promotion standard in our experiments rather than submit them to subject choice.

effort to influence assignment to the task in which he has the comparative advantage. In such cases, allowing agents to self-select their task assignment should be more efficient than a promotion rule.

To sum up, Lazear (2004) delivers several testable implications that we examine:

a) When observable output is a function of permanent and random ability components, average agent output will decrease (increase) following promotion (*non*-promotion).
b) The larger the random component relative to the permanent one, the stronger the Peter Principle (e.g., the larger the decrease in average post- promotion output).
c) If agents know their ability, then effort will be distorted downward (upward) by lower (higher) ability agents prior to a promotion stage. [4]
d) If agents know their ability, allowing agents to self-select their second-stage task will be perfectly efficient and should be more efficient than a promotion tournament.

3. EXPERIMENTAL DESIGN

The game reproduces the main characteristics of Lazear's (2004) model. Since our main interest is in recreating the Peter Principle and examining agents' strategic behavior, subjects are all assigned as agents. In promotion treatments, we use an exogenous standard.

The Task

The cognitive task utilized is a modified version of the task in Baddeley (1968). The easy task involves reading a short sentence that describes the order of three letters. The subjects have to type in these three letters in the order implied by the sentence. For example, "*S follows B, is not preceded by G*" implies the order "*BSG*".[5] We mix passive (e.g., "*is followed by*") and active (e.g., "*precedes*") statements, positive (e.g., "*follows*")

[4] In this framework, effort distortion is aimed at distorting job allocation. In the literature on promotions, there are many other types of distortion where the principals themselves manipulate information given to the agents or competitors (Meyer, 1991; Bernhardt, 1995). We do not consider these other sources of distortion.
[5] Subjects are given a precise rule to use for lettering ordering, and with the rule there is a unique letter ordering for each sentence description (see instructions in Appendix B).

and negative statements ("*does not precede*") across sentences. Sentences are randomly generated from all letters of the alphabet. Once typed, the sentence is auto-checked against the correct answer after each sentence, so that subjects can learn and improve more rapidly. Though Baddeley (1968) indicates only minor learning with repetition of the task, we consider learning as a potential confound in the data and implement a practice stage of both easy and hard tasks. The hard task is similar except that each sentence contains five letters. For example, "*S follows B, is not preceded by G, precedes H, is not followed by J*" implies the ordering "*JBSGH*". The advantage of this task is that no specific knowledge is needed, difficulty can be increased by increasing the number of letters without altering the cognitive focus of the task, and the outcome is easily measurable. Also, effort is distinguished from ability by use of practice rounds, which contain no incentives to distort effort. Finally, our *Calibration* treatment provides an added way to filter learning trends from the data.

Timing of the game and determination of the parameter values

We first utilize a distinct *Calibration* treatment to create wage profiles such that high ability subjects would earn more in the hard task. Thus, this treatment generates data on average subject ability on each task, which is also necessary to determine a cutoff point for promotions. Because it involves several stages of performing the effort task, we also use this treatment to filter the learning component from the data in the other treatments. We use the same time structure in the *Calibration* treatment as in all other treatments.

A treatment consists of four stages. In stages 1 and 2, the subject performs the 3-letter then 5-letter tasks during separate 7 minute periods.[6] These first two stages provide subjects information on their abilities for each task. In stage 3, the subject performs the 3-

[6] In each of the first two stages, subjects practice the task three times as part of the instructions. During this practice phase subjects cannot proceed to the next sentence before providing a correct answer.

letter task again during 7 minutes. The number of correctly solved sentences in stage 3 is our proxy for the subject's ability, and the difference between stage 1 and stage 3 sentences solved is the general learning trend. At the end of stage 3, the subject draws an *i.i.d* random number from a uniform *U*[-12,+12] distribution. This random number corresponds to the task-specific transitory component in the theoretical model. The subject's "observable" outcome in stage 3—the outcome on which payment is based—is the sum of the number of correctly solved sentences and the random draw number.

In stage 4, some subjects are assigned the easy task and others the hard task. In the *Calibration* treatment, the assignment is purely random: half of the subjects are assigned to each task. Those assigned to the easy task again solve 3-letter sentences during a 7-minute period, after which a random draw from *U*[-12,+12] is added to the number of correct sentences solved. Those assigned to the hard task solve 5-letter sentences during the same 7-minute period and then draw an *i.i.d.* random number from a uniform *U*[-4,+4]. The difference in the support of the uniform distributions corresponds to the induced output equivalent of easy to hard tasks: one hard sentence solved is considered as valuable to the employer as three easy sentences. The subject's stage 4 outcome is the sum of the number of correct sentences and the random term. Appendix A summarizes the game's timing.

The payoffs in each of the four stages have been determined as follows. Pilot sessions gave us an informed estimate of average ability at each task: the average subject solved 2.05 times as many 3-letter sentences (65) vs. 5-letter sentences (30). In order to create the differing slopes of each task's wage profile for the *Calibration* treatment, we first induce an output equivalence of 3 easy task units to each 1 hard task unit. That is, subjects are paid a piece rate that is 3 times as large for the hard task, corresponding to $\delta = 3\beta$.

(Accordingly, as mentioned above, the variance of the random component in the easy task is 3 times larger than in the hard task). By manipulating the piece-rates so that the hard task is rewarded at a rate *greater* than the subjects' observed opportunity cost of the hard task, we effectively induce wage profiles of differing slopes.

Fixed payments are added in each stage of the *Calibration* treatment in order to create the desired overlapping wage profiles. In stage 1, the subject receives a fixed payment of 135 points and the piece-rate pays 3 points for each correct 3-letter sentence solved (i.e., the easy task). In stage 2, the fixed payment is 9 points and each correct 5-letter sentence (i.e., hard task) is paid a piece-rate of 9 points. In stage 3, the easy task is again performed with pay structure as in stage 1, while subjects are randomly assigned to either the hard or easy task, with the corresponding pay structure, in stage 4. The *Calibration* treatment allows us to adjust the values of the parameters, examine learning trends, and to determine the promotion cut-off point in the *Promotion* treatments.

Figure 1 is taken from Lazear (2004), showing the final parameterization of our *Promotion* and *Self-Selection* treatments.[7] Outcomes from pilot sessions and *Calibration* treatment generated the average ability estimate of 30 units (hard task equivalent). The final fixed payments utilized with each wage profile create a crossing point at ability=33 units of hard task, which implies an anticipated promotion rate of just under 50%.[8]

[7] A minor change in fixed payments was made for the *Promotion* and *Self-Selection* treatments. The easy task fixed payment was lowered from 135 to 108 points, to increase anticipated promotions to around 50%--this lowered the Fig 1. intercept to output=12 for the easy task (from 15 in the *Calibration* treatment).

[8] This expectation is based on sample estimates from the *Calibration* treatment, but it is not critical to our experiment whether more or less than 50% of subjects have ability beyond the cross-over point. That is, the Peter Principle is fundamentally about whether random components create matching inefficiencies. Our experiment does not test the ability of principals to anticipate this Principle and adjust the promotion cutoff.

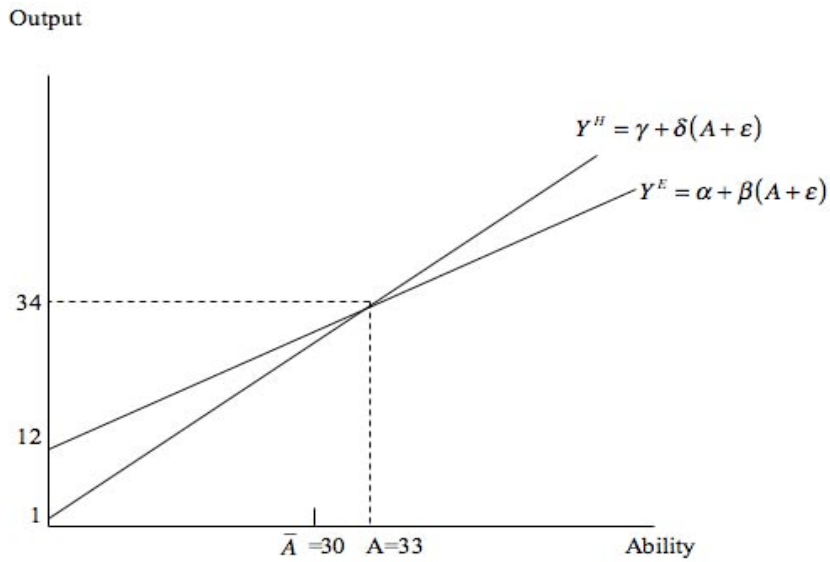


***Figure 1**. Productivity and cut-off point*

The Treatments

We have tested four treatments besides the *Calibration* treatment: the *Promotion-σ_{Low}*, *Promotion-σ_{Medium}*, *Promotion-σ_{High}* treatments, and the *Self-Selection* treatments. The *Promotion-σ_{Low}* treatment constitutes the benchmark of this game. As in the *Calibration* treatment, *Promotion-σ_{Low}* utilizes a random component distributed uniformly on the interval [-12,+12] in the easy task and on the interval [-4,+4] in the hard task.

In contrast with the *Calibration* treatment in which promotion after stage 3 was random, promotion in *Promotion-σ_{Low}* is based on the comparison between the individual's outcome and a promotion standard. The promotion standard is set to 68 easy task units of output, approximately corresponding to the Fig. 1 cross-over point of output=33 units of hard task.[9] The *Promotion-σ_{Low}* treatment is a baseline aimed at recreating the basic Peter

[9] Recall that this easy-hard task correspondence was from the pilot sessions, where average easy task output was 2.05 times that of the hard task output, or (2.05)*33≈68. The analogous correspondence in the *Calibration* treatment stages 3 and 4 was that average easy task output was 2.0 times that of the hard task. It is important to realize that we *cannot* use stages 1 and 2 to generate a hard-to-easy task conversion rate for each individual subject because of the learning confound in those data.

Principle, and its relation to the relative size of the transitory ability component is examined by implementing *Promotion-*σ_{Medium} and *Promotion-*σ_{High}. In *Promotion-*σ_{Medium}, the random component is distributed uniformly on the interval [-24,+24] and [-8,+8] in the easy and hard tasks, respectively. In *Promotion-*σ_{High}, it is distributed uniformly [-75,+75] and [-25,+25] in the easy and hard tasks, respectively. The testable theoretical implication is that the Peter Principle will be strongest in *Promotion-*σ_{High} and weakest in *Promotion-*σ_{Low}. A comparison of stage 3 outcomes in the *Promotion* and *Calibration* treatments allows us to examine the hypothesis that subjects distort effort in anticipation of promotion.

In the *Self-Selection* treatment, the random ability component is distributed as in *Promotion-*σ_{Low}, but the subject chooses her stage 4 task. It is reasonable to assume that she has some knowledge of her ability on each task from the previous stages, and so we assume self-selection should be efficient. The comparison of outcomes in stage 4 of the *Promotion* and *Self-Selection* treatments provides an efficiency comparison of exogenous promotion standards versus self-selection for job sorting.

Finally, in the *Calibration* and *Promotion* treatments, a hypothetical response of preferred stage 4 task is elicited at the beginning of this stage. It was made clear to subjects that their task had already been determined and the hypothetical response would have no bearing on their stage 4 task. Nevertheless, this response may provide some indication of whether subjects correctly assessed their abilities. Treatment details are shown in Table 1.

Table 1. Main treatment characteristics

Treatment	Assignment to Hard task	Variance On Easy task	Variance on Hard task	# of sessions	# of subjects
Calibration	Random	U[-12,+12]	U[-4,+4]	2	38
Promotion-σ_{Low}	Standard	U[-12,+12]	U[-4,+4]	2	37
Promotion-σ_{Medium}	Standard	U[-24,+24]	U[-8,+8]	2	38
Promotion-σ_{High}	Standard	U[-75,+75]	U[-25,+25]	2	37
Self-Selection	Self-Selection	U[-12,+12]	U[-4,+4]	2	40

Experimental Procedures

Ten sessions were conducted at the Groupe d'Analyse et de Théorie Economique (GATE), Lyon, France, with 2 sessions per treatment. 190 undergraduate subjects were recruited from local Engineering and Business schools. 81.05% of the subjects had never participated in an experiment. On average, a session lasted 60 minutes. The experiment was computerized using the REGATE program (Zeiliger, 2000). Upon arrival, the subjects were randomly assigned to a computer terminal, and experimental instructions for the first stage of the game (see Appendix B) were distributed and read aloud. Questions were answered privately. Subjects then practiced the easy task with 3 sentences without payment until they were correctly solved, after which point the 7-minute paid easy task started. At the end of the 7 minutes, we distributed the instructions for the second stage of the game and answered to the participants' questions. Once the practice and 7-minute hard task have been performed, the instructions for both stage 3 and stage 4 were distributed together and read aloud. Knowing the rules for task assignment in stage 4 before starting stage 3 allowed subjects to behave strategically in Stage 3, if desired, in the *Promotion* treatments. After stage 3, subjects were given a brief 3-minute break, with no communication allowed. We used a conversion rate of 100 points for each Euro. At the end of each session, payoffs were totaled for the four stages. In addition, the participants received €4 as a show-up fee. The average payoff was €15.40, and subjects were each paid in private in a separate room.

4. RESULTS

The Peter Principle

Comparing the stage 3 and 4 final outcome levels in the *Promotion* treatments in Table 2 can identify the Peter Principle. In order to compare outcomes across easy and hard tasks, outcome levels in the hard task are normalized to the easy task metric in the following way: we multiply the hard task score by 2.05 (using the conversion rate from our pilot sessions) and the random component by 3 (using the piece-rate conversion).

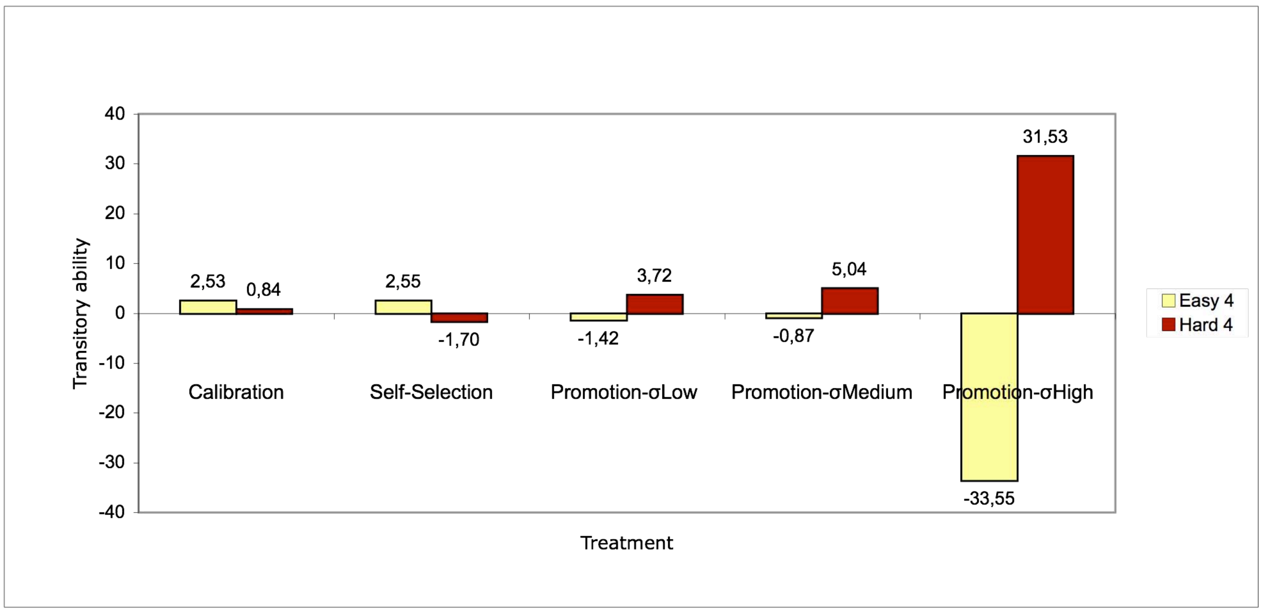
Table 2. Stage-3 and stage-4 average outcomes of promoted/non-promoted subjects

	Non-promoted subjects			Promoted subjects		
Treatment	# obs.	Stage 3 Outcome	Stage 4 Outcome	# obs.	Stage 3 Outcome	Stage 4 Outcome
Calibration	19	62.47	64.47	19	59.53	63.78
Self-Selection	20	64.25	65.15	20	70.65	83.68
Promotion-σ_{Low}	12	55.92	62.58	25	81.04	83.95
Promotion-σ_{Medium}	15	51.80	60.60	23	84.22	85.41
Promotion-σ_{High}	22	27.14	70.91	15	92.13	52.08

Table 2 shows that in *Promotion-*σ_{High}, the post-promotion average outcome (sentences solved plus error term) of the promoted subjects is 43.5% lower than the pre-promotion level (p=.061 Wilcoxon signed-rank test). This treatment provides the cleanest evidence of the Peter Principle. In the other promotion treatments, outcome differences are not statistically significant (p>0.10) pre- and post-promotion. It should, however, be noted that the outcome of the non-promoted subjects increases between stages 3 and 4 by 11.90%, 16.99%, and 161.27%, respectively, in *Promotion-*σ_{Low}, *Promotion-*σ_{Medium}, and *Promotion-*

σ_{High}, which is another implication of the Peter Principle. These differences are all statistically significant (p=0.091, p=0.073, and p=0.001, respectively).

The existence of the Peter Principle is related to the existence of a transitory ability component. As predicted, luck plays a greater role in promotion in the *Promotion* treatments. In *Promotion-*σ_{Low} , 72% of the promoted subjects had a positive transitory component in stage 3, compared to only 42% of the non-promoted subjects (Mann-Whitney, p=.079). In *Promotion-*σ_{Medium} , the respective percentages are 74% and 33.% (p=0.014) and in *Promotion-*σ_{High} , 80% and 9% (p<0.001). Figure 2 displays the average random term drawn in stage 3 for each treatment, sorting subjects according to whether



they are promoted or not in stage 4.

Figure 2. *Average stage 3 transitory ability sorted by task in stage 4*

Figure 2 confirms that in most *Promotion* treatments, the promoted subjects (labeled 'Hard 4') have been luckier than the non-promoted ones, whereas in the *Calibration* treatment the difference is not significant. Their average transitory ability is positive and it increases in its variance. Non-promoted subjects have a negative average transitory ability. According to Mann-Whitney tests, the differences in average transitory components between promoted and non-promoted subjects are significant in *Promotion-*σ_{Low} (p=0.011) and *Promotion-*σ_{High} (p<0.001), while marginally insignificant in *Promotion-*σ_{Medium} (p=0.120). As expected, stage 4 outcomes reflect a reversion-to-mean effect of the transitory component, which completes the Peter Principle.

By next examining the agents' abilities, we can analyze promotion mistakes (i.e., inefficient allocations). Table 3 indicates the stage 3 permanent (i.e., non-transitory) ability of promoted and non-promoted subjects by treatment. Then, it gives the numbers and proportions of subjects who were wrongly promoted despite ability lower than 68 ("mistake 1"), the unlucky subjects who should have been promoted ("mistake 2"), and the subjects who were efficiently allocated in stage 4.

Table 3. Stage 3 ability of promoted/non-promoted subjects and assignment mistakes

	Stage 3-average ability		**Number of mistakes (percentages)**		
Treatment	Non-promoted	Promoted	Mistake 1 (promotion)	Mistake 2 (no-promotion)	Correct decisions
Calibration	59.95	58.68	12 (31.58)	5 (13.16)	21 (55.26)
Self-Selection	61.7	72.35	9 (22.50)	6 (15.00)	25 (62.50)
Promotion-σ_{Low}	57.33	77.32	2 (5.41)	1 (2.7)	34 (91.89)
Promotion-σ_{Medium}	52.67	79.17	4 (10.53)	3 (7.89)	31 (81.58)
Promotion-σ_{High}	60.68	60.60	11 (29.73)	10 (27.03)	16 (43.24)

If permanent ability were observable, only those subjects solving at least 68 sentences in stage 3 would be promoted. We observe that in the *Promotion-*σ_{High} treatment, the average score of the promoted subjects in stage 3 is not significantly different from that of the non-promoted subjects (Mann-Whitney test, $p>0.10$). In addition, it is significantly lower than the promotion standard of 68 (t-test, $p=0.057$). Thus, a fraction of the subjects are inefficiently promoted. Table 3 also indicates that mistake frequency increases in transitory ability variance. As a whole, the *Promotion* treatments data (Tables 2, 3, Figure 2) result from a framework created to generate the principle, so these results are perhaps not surprising. They are, however, an important validation of a rather complex experimental design. We can therefore explore comparison institutions and their predicted effects on effort distortion and self-selection—two key outcome variables not amenable to simulation.

Self-selection vs. Promotions

Our *Self-Selection* treatment allows us to test the hypothesis that one can escape the Peter Principle by letting the individuals select their task. In *Self-Selection*, subjects who chose the hard task in stage 4 *increased* normalized outcomes by a significant 18.44% relative to stage 3 ($p<0.001$) (Table 2). This is partly due to a higher rate of negative transitory components in stage 3 (Fig. 2). In contrast, those who chose the easy task only increased their outcome by an insignificant 1.40% (Table 2, $p>0.10$). This stands in contrast to the clearly observed Peter Principle in *Promotion*-σ_{High}.

The lack of Peter Principle in the *Self-Selection* treatment does not, however, imply that decisions in that treatment are more efficient than with promotion standards. Table 3 indicates that, in *Self-Selection*, 22.50% chose the hard task in stage 4 although their score in stage 3 was below 68, and 15.00% of the subjects made the opposite mistake. The respective proportions of mistakes were 5.41% and 2.70% in *Promotion*-$\sigma_{Low,}$ which has a comparable transitory variance, and the differences are significant (Mann-Whitney tests, $p=0.031$ and $p=0.017$, respectively). In addition, the size of the average error is larger in the *Self-Selection* treatment than in the *Promotion-*$\sigma_{Low,}$ treatment. Indeed, the average type 1-mistake amounts to 9.11 and the average type 2-mistake 8.33 in the *Self-Selection* treatment; the respective values for the *Promotion-*$\sigma_{Low,}$ treatment are 7.50 and 6.00. This casts some doubts on the overall efficiency of self-selection and calls for further comparative analysis of these two job allocation mechanisms.

In particular, we compare the efficiency of promotions, self-selection, and an ad hoc random task assignment. If the proportion of subjects actually promoted in the data for a treatment is *x*, then we simulated random promotion by randomly selecting *x* subjects to be promoted. Therefore, simulated random assignment promotes the same number of subjects as in the data, but not necessarily the same individuals. Efficiency is calculated as the proportion of correct promotions (i.e. promotion when permanent ability is at least 68).[10]

We find that a promotion standard is generally more efficient than a random task assignment. In *Promotion-*σ_{Low} , the efficiency rate is 92% vs. 54% with a random assignment, while in *Promotion-*σ_{Medium} , the respective percentages are 82% and 76%. When the variance of the transitory component is high, however, random assignment can be more efficient than the promotion rule (due to the large Peter Principle at work). Indeed, the efficiency rate is 43% in *Promotion-*σ_{High} and 54% with the simulated random assignments. Self-Selection efficiency is 63%, which is more efficient than the simulated random assignment of 45% in that same treatment. So, while self-selection efficiency is higher than with random task assignment, our results do not support the prediction that task allocations are uniformly more efficient with self-selection rather than promotion standards.

[10] Note that this type of random assignment is distinct from just flipping a coin to determine stage 4 task assignments, as that method necessarily generates a 50% random promotion rate.

This conclusion is also confirmed by the analysis of hypothetical choices. In our *Promotion* treatments, we asked subjects to make a hypothetical choice at the end of stage 3 about which task they would prefer in stage 4. Comparing these hypothetical choices with the actual scores in stage 3 allows us to determine whether the subjects were biased in their self-assessment or not. In Table 4, "optimism" ("pessimism") indicates the proportion of subjects who would choose the hard (easy) task although their score in stage 3 was below (equal or above) 68; "accurate" corresponds to the proportion of subjects who were unbiased and would have efficiently self-selected. In this Table, the data from the *Self-Selection* treatment correspond to the actual subjects' choices.

Table 4. *Self-Selection* task choice and hypothetical choices in *Promotion* treatments

Treatment	Optimistic	Pessimistic	Accurate	Total
Self-Selection	22.50 %	15.00 %	62.50 %	100 %
Promotion-σ_{Low}	13.51 %	13.51 %	72.98 %	100 %
Promotion-σ_{Medium}	26.32 %	18.42 %	55.26 %	100 %
Promotion-σ_{High}	37.84 %	2.70 %	59.46 %	100 %

Table 4 shows that a relatively high proportion of subjects overestimate their ability to succeed at the hard task in stage 4, consistent with non-hypothetical choices in *Self-Selection*. Though Table 4 shows evidence that some job matching errors are due to pessimism, the data indicate that optimism with respect to the transitory component draw is more pervasive. Because hypothetical accuracy in *Promotion-*σ_{Low} is greater than actual accuracy in *Self-Selection* (but with same transitory component variance), we do not believe that mistakes are due to lack of financial incentives for the hypothetical question in the *Promotion* treatments. It is also not the case that less able subjects are unaware of their incompetence because they lack the metacognitive ability to realize it (Kruger and

Dunning, 1999). Indeed, the subjects are informed about their score before they draw their transitory ability component. Therefore, these mistakes are likely attributable to a biased perception of the random ability component. Interestingly, the proportion of optimistic choices increases with the variance of the transitory component, but not that of pessimistic choices. In *Promotion*-σ_{High}, 38% of the subjects would have chosen the hard task although their score was below 68, whereas this proportion is only 14% in *Promotion*-σ_{Low} (Wilcoxon test: p=0.039).

To investigate further the hypothesis that optimism influences self-selection and hypothetical choice errors, we estimate two Probit models in which the dependent variable equals 1 if the actual or hypothetical choice for stage 4 is the hard task. In both models, the explanatory variables include the subject's score and transitory ability in stage 3. In the hypothetical choice model (i.e., *Promotion* treatments), we control for the variance of the transitory ability with a variable equal to 0, 1, or 2 for *Promotion*-σ_{Low} , *Promotion*-σ_{Medium} , and *Promotion*-σ_{High} , respectively. Table 5 reports the results of these estimations.

Table 5. Actual and hypothetical choices in *Self-Selection* and *Promotion* treatments

Probit Models	Actual choice of the hard task *Self-Selection*.		Hypothetical choice of the hard task – *Promotion*.	
	Coeff.	S.E.	Coeff.	S.E.
Score in stage 3	0.035**	0.017	0.029***	0.007
Transitory ability in stage 3	-0.062*	0.032	-0.010*	0.006
Var. of transitory ability	---	---	0.367***	0.144
Constant	-2.345**	1.160	-1.815***	0.482
Nb of observations	40		150	
Log likelihood	-23.113		-84.867	
LR χ^2	9.23		30.44	
Prob>χ^2	0.010		0.000	
Pseudo R^2	0.166		0.152	

Note: *, **, and *** indicate statistical significance at the 0.10, 0.05, and 0.01 level, respectively.

Not surprisingly, Table 5 indicates that both the actual and hypothetical choices of the hard task increase as a function of one's stage 3 score. It also shows that bad luck in stage 3 increases the likelihood of choosing the hard task in stage 4, even though it is common knowledge that the transitory component draws are independent across stages. These results may indicate that the unlucky subjects in stage 3 expect to be more lucky in stage 4, and the *Promotion* treatments model confirms that this optimism may be a function of the transitory ability variance.

These findings are consistent with van den Steen (2004), whose theoretical model shows that, if rational agents differ in their prior beliefs, they are more likely to choose the actions of which they overestimate the probability of success. Here, it means that we are more likely to observe optimistic subjects among those who have chosen the hard task. In other words, according to van den Steen (2004), there is a choice-driven endogenous overconfidence in the data. This suggests that in our game, a process somewhat analogous to the regression-to-the-mean explanation of the Peter Principle is in action when subjects can self-select their task. Therefore, allowing self-selection of one's task may itself generate optimism that reduces job choice efficiency.

When is self-selection likely to be more efficient than using a promotion standard, given that the theoretical assumption of perfect knowledge of one's own abilities may be violated? If we compare the values in Table 4 to the efficiency rates of actual task assignments, we find that the promotion standard performs better than the hypothetical choices when the variance of the transitory ability is low (92% vs. 73%) or medium (82% vs. 55%). In contrast, when this variance is high, the hypothetical choices of the subjects are accurate in 59% of the cases whereas only 43% of the actual promotion decisions are accurate (Wilcoxon test: p=0.010); the 54% efficiency rate with simulated assignment was also lower than with hypothetical choices. We conclude that when the transitory component variance is large relative to permanent ability,[11] it is more efficient to allow self-selection of task assignment rather than use promotions, even though the self-selection choice may generate optimism.

It should be however noted that even in *Promotion*-σ_{High} , it might remain profitable to use promotion standards. Indeed, the decrease in the total outcome of the promoted subjects is compensated by the increase in outcome of the non-promoted ones.[12] Furthermore, the average actual ability of the promoted subjects in stage 4 (68.88) is not significantly different from the standard of 68 (t-test, p>0.10). One might hypothesize that the promotion of low-ability agents provides an extra effort incentive to justify the promotion.[13] As we will see next, we can rule out this interpretation in our data. If one considers all the *Promotion* treatments together, we observe that the subjects who have been promoted by mistake increase on average their (normalized) score by 9.61% from stage 3 to stage 4 (from 56.00 to 61.38). The reason this should not be attributed to an incentive effect is because we observe a similar 10.90% increase for less-able subjects who have not been promoted (from 48.2 to 53.46)—this is more likely a residual learning effect.

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[11] We did not explicitly measure this threshold but we know that the variance must be higher than at least 3 times the average permanent ability –i.e. higher than in the *Promotion*-σ_{Medium} treatment where a promotion standard is still more efficient than hypothetical choice.

[12] The average normalized outcome of promoted subjects decreases from 92.13 in stage 3 to 52.08 in stage 4 (-43.47 %; Wilcoxon test: p=0.061), while the average outcome of the non-promoted subjects increases from 27.14 to 70.91 (+161.27 %; p=0.001).

[13] Koch and Nafziger (2007) provide an interesting principal-agent model in which the Peter Principle emerges from the fact that promotions are used both as a job assignment device and as an incentive. They show that it may be profitable to the principal to promote a less able agent if the latter provides extra-effort after this promotion to outweigh his lack of ability. Such a promotion then induces higher effort.

Effort Distortion

Lazear (2004) hypothesizes that promotions may induce effort distortion by the agents in anticipation of the promotion. This hypothesis requires the assumption that agents know (but principals do not know) their permanent ability and that they have no bias regarding their transitory ability, in which case they know their preferred stage 4 task assignment. Because the transitory component of ability affects promotion decisions, high (low) ability agents have an incentive to distort effort upward (downward) to ensure efficient assignment to the hard (easy) task.

From the experimental data, we take two approaches to examining this hypothesis. If effort is distorted in stage 3, then stage 4 should reflect a non-distorted effort. Low ability agents should have scores (i.e., without considering the error term) in the easy task that are lower in stage 3 than stage 4, while high ability agents should have normalized scores in the hard task that are lower in stage 4 than what they scored in the easy task in stage 3. For the subset of non-promoted subjects, score is on average 9.16 % higher in stage 4 in *Promotion-*σ_{Low} (Wilcoxon test: p=0.028), 8.47 % higher in *Promotion-*σ_{Medium} (p=0.011), and 7.79 % in *Promotion-*σ_{high} (p=0.004). However, this analysis must consider the possibility of a residual learning trend from stage 3 to stage 4. So, a comparison with *Self-Selection*, in which there is no incentive for effort distortion, is helpful. In *Self-Selection*, the non-promoted subjects still performed 8.27 % better in stage 4 (Wilcoxon test: p=0.001). Thus, the evidence is not in favor of effort distortion for low ability subjects, as the entire increase in output in stage 4 is attributable to residual learning. For high ability subjects, similar stage 3 to 4 trends occur in the *Promotion* and *Self-Selection* treatments, so no evidence exists for effort distortion in high ability agents either.

An alternative approach to examining effort distortion is to compare the variance of scores (i.e., non-transitory ability) in stage 3 in the *Promotion* and *Self-Selection* treatments. We find that the standard deviation of scores is 15.83 in the *Self-Selection* treatment and 13.55 in the *Promotion-*σ_{Low} treatment. Contrary to what should be expected from effort distortion, the variance of effort is therefore is smaller, and not larger, when a promotion standard is used. Alternatively, effort distortion in stage 3 of the *Promotion* treatments should lead to a higher variance in scores than in stage 1 of the same treatment, where strategic motivation is not present. Using a difference-in-difference approach, we examine the stage 1-3 difference in standard deviations in the *Promotion* and *Self-Selection* treatments. In *Self-Selection*, the standard deviation of scores in the easy task decreases by 14.06% from stage 1 to stage 3, whereas it decreases by 2.17% in *Promotion-*σ_{Low}, and it increases by 17.89% in *Promotion-*σ_{Medium} and by 8.41% in *Promotion-*σ_{High}. The variance ratio tests, however, fail to reject the null hypothesis that the variances differ from stage 1 to stage 3 of all treatments ($p>0.10$).

In sum, we do not find support for the hypothesis of strategic effort distortion. It is possible that effort distortion may not have the same expected payoff as in Lazear (2006) if beliefs are biased with respect to the transitory ability component. That is, if low ability agents are pessimistic, thinking they will receive a negative transitory ability component, they do need not distort effort downwards to insure assignment to their optimal easy task in the last stage. This interpretation also implies that if high ability subjects are optimistic with respect to the transitory component (i.e., expecting a positive draw), then the expected marginal benefit of distorting effort upwards in *Promotion* treatments is smaller. A comparison of mean abilities in stage 3 for *Promotion* treatment subjects who hypothetically chose the hard over the easy task finds significant ability differences (71 versus 58 respectively—$p<.001$ for the t-test of mean differences). This indicates that subjects who would prefer to be assigned the hard (easy) task were, on average, higher (lower) ability subjects, and not just optimistic (pessimistic) with regards to the transitory component draw. Thus, subjects may have some bias with respect to their transitory ability component, but mostly appear to utilize the information they possess with respect to their own abilities in a rational way. Nevertheless, because effort distortion requires some level of strategic thinking, our results indicate that agents are mostly rational but perhaps naïve. A naïve agent may be capable of understanding that a higher ability increases the odds that the hard task is a more efficient task assignment, but may not think strategically enough to distort effort to guarantee assignment to the right task.

5. CONCLUSION

When transitory ability components affect observable outcomes, the Peter Principle can occur, and performance then declines after a promotion. People are promoted although they are not necessarily competent enough to perform efficiently in their post-promotion job. Lazear (2004) provides a theoretical framework in which the Peter Principle is interpreted as a regression-to-the-mean phenomenon. This model highlights four major testable implications that we examine in this paper:

1) When observable output is a function of both permanent and transitory abilities, the average outcome decreases following promotion.
2) The Peter Principle increases in the relative importance of the transitory component of observable outcomes.

3) Promotion decisions based on an exogenous cutoff point are less efficient than if agents self-select their jobs.
4) Agents distort effort in anticipation of promotions in order to ensure assignment to the efficient task.

To our knowledge, this paper provides one of the first empirical tests of the Peter Principle. In our experiment, we recreate the essential features of Lazear's (2004) framework. We implement a real effort task in both easy and hard forms, where promotion implies assignment to the hard task. In our experiments, the permanent ability component is captured by subject effort, while a random error term simulates transitory ability. The random term is added to the subject's effort task score to yield the observable outcome on which promotion decisions are made. We utilize an exogenously determined standard for promotion. We test three promotion treatments, which manipulate the variance of transitory ability and, in a fourth treatment, subjects self-select their task assignment.

The data are consistent with the fundamental tenant of the Peter Principle. While not significant for relatively small variances of the error term, a larger error component variance generates a clear Peter Principle. Thus, our experimental design is validated by our successfully generation of the Peter Principle in a real effort environment, and we then explore the important subsidiary hypotheses.

Contrary to Lazear's hypothesis, we do not find that self-selection of one's task generates fully efficient job assignment. This is likely due to biased beliefs related to the transitory ability component. Van den Steen (2004) highlights how a random component to outcomes may give rise to systematic biases that could also affect effort. While we find some evidence of optimism/pessimism with respect to the random component (and therefore some evidence of task self-selection errors), we do not find that these biased beliefs are systematically related to effort. In addition, when the variance of the transitory ability component is large enough, the analysis of hypothetical choices suggests that self-selection would be efficiency-improving compared to the use of an exogenous standard, assuming incentive-compatible wages. We also fail to find support for the effort distortion hypothesis. Again, this may result from biased beliefs with regards to transitory ability components, or a lack of strategic behavior from the subjects (i.e., naïve agents).

Though the Peter Principle is considered to apply to a variety of environments, our results have their most clear implications in labor markets, which are the prototypical environments when considering the Peter Principle. Our results suggest that the Peter Principle is more likely to occur in highly volatile environments or in activities where the role of luck may be large compared to talent. This suggests that in these environments, human resource managers should require a longer period of observation before promoting employees, so that they can obtain better information on an employee's permanent ability.

This research offers avenues for further study. In Lazear's (2004) framework, employers are sophisticated and adjust promotion standards in anticipation of Peter Principle effects. Future research could therefore analyze the principals' behavior regarding the optimal adjustment of the promotion standards. It is also the case that different tasks lend themselves more towards delusions about one's ability. Different tasks may be subject to a more or less severe optimism bias in one's self-assessed abilities. Such tendencies would affect the efficiency of self-selection. These qualifications highlight reasons for caution in how one interprets our results. Nevertheless, our results are an important first step in answering some important questions regarding the Peter Principle.

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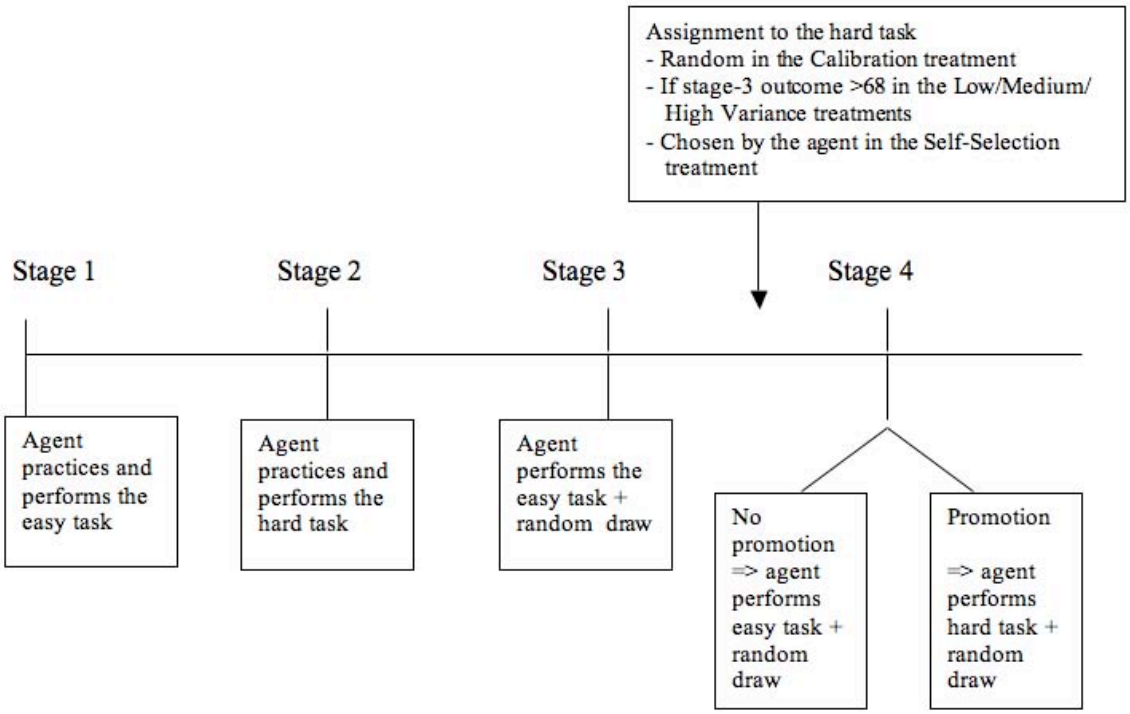
APPENDIX A. Timing of the Game



APPENDIX B – Instructions of the *Promotion*-σ_{Low} treatment

(Other instructions available upon request)

This is an experiment in decision-making. This experiment is separated into different parts. During each part, you will perform the following task. The task involves reading a short sentence that describes the order of letters. Based on the sentence, you must type in the order of the letters that the sentence implies. This task is described in details below. You will earn a specific amount of money for each accurately solved sentence. The earnings in points that you will get in each part will be added up and converted into Euros at the end of the session. The conversion rate is the following:

100 points = 1 Euro

In addition, you will receive 4 € for participating in this experiment. Your earnings will be paid in cash at the end of the session in a separate room. Earnings are private and you will never be informed of anyone else's outcomes or earnings in the experiment.

The instructions related to the first part are described below. The instructions relative to the next parts will be distributed later on.

Part 1

The task involves reading a short sentence that describes the order of 3 letters. Based on the sentence, you must type in the order of the letters that the sentence implies. You must determine the proper order of each 3-letter sentence by doing the following. First, determine the order of the first two letters. Then, keep those two letters together and determine where the third letter should go in relation to the first pair of letters. Using this rule, there is only one possible ordering of the 3 letters for each sentence. You can use indifferently lower case letters or capital letters and you can return. You must validate your answer by clicking the enter key.

Here are some examples of 3-letter sentences followed by the correct letter-order.

Short sentence		Correct order
1)	B is followed by A, precedes H	BAH
2)	X does not precede Y, is not followed by M	MYX

Once you have submitted an answer, a new sentence will automatically appear for you to repeat this task on a different sentence. This will continue for 7 minutes. You will be told on your screen whether you were correct or not after each sentence. If your answer is not correct, you cannot change it; you automatically proceed to the next sentence. You screen indicates the current number of correct letter-orderings and the remaining time.

Your result in units in part 1 is given by the number of correct letter-orderings in this part. For each letter-order that you get correct, you are paid 3 points. In addition, you receive a fixed payoff of 108 points.

Your earnings in points in part 1 = (your result in units in part 1 * 3 points)
+ 108 points

To ensure that you have understood the instructions, you are kindly requested to practice this task by attempting to solve the 3 sentences that will appear on your screen once you have cliked the « practice » button. During this practice, the next sentence will appear only after the current one has been accurately solved. These answers are not taken into account in your result or your earnings.

Please raise your hand if you have any questions. The 7-minute task-period will start after each participant has completed the practice and once we have answered your questions. Communication between participants is forbidden throughout the session.

Part 2

The task involves reading a short sentence that describes the order of 5 letters. Based on the sentence, you must type in the order of the letters that the sentence implies. You must determine the proper order of each 5-letter sentence by doing the following. First, determine the order of the first two letters. Then, keep those two letters together and determine where the third letter should go in relation to the first pair of letters. Then, keep those three letters together and determine where the fourth letter should go in relation to the first three letters. Then, keep those four letters together and determine where the fifth letter should go in relation to the first four letters. Using this rule, there is only one possible ordering of the letters for each sentence.

Here are two examples of 5-letter sentences followed by the correct letter-order.

Short sentence	Correct order
1) A is followed by D, is followed by B, is preceded by C, follows H	HCADB
2) Z does not precede X, does not follow Y, does not follow W, precedes B	XZYWB

Once you have submitted an answer, a new sentence will automatically appear for you to repeat this task on a different sentence. This will continue for 7 minutes.

You will be told on your screen whether you were correct or not after each sentence. If your answer is not correct, you cannot change it; you automatically proceed to the next sentence. You screen indicates the current number of correct letter-orderings and the remaining time.

Your result in units in part 2 is given by the number of correct letter-orderings in this part. For each letter-order that you get correct, you are paid 9 points. In addition, you receive a fixed payoff of 9 points.

Your earnings in points in part 2 = (your result in units in part 2 * 9 points)
+ 9 points

You are kindly requested to practice this task by attempting to solve the 3 sentences that will appear on your screen once you have clicked the « practice » button. During this practice, the next sentence will appear only after the current one has been accurately solved. These answers are not taken into account in your result or your earnings.

You can click the « practice » button as soon as you like. The 7-minute task-period will start after each participant has completed the practice. Your earnings during this part will be added to your previous earnings.

The instructions that have just been distributed describe parts 3 and 4. Part 4 will start immediately after part 3 has been completed and a break of 3 minutes.

Part 3

The task involves reading a short sentence that describes the order of 3 letters, as you already did in part 1 but with new sentences. As before, you must type in the order of the letters that the sentence implies. The sentences appear successively during 7 minutes and you will be told on your screen after each answer if this one is correct or not.

Your result in units in this part depends on two elements:

- The number of correct answers
- A personal random number

Your personal random number is determined as follows. Once you have performed your task, a « random draw » button appears on your screen. Once you click this button, the computer program generates your personal random number. Your number can take any value between -12 and +12, included. Each number has an equally likely chance to be drawn. There is an independent random draw for each participant. Your personal random number is independent of your performance at the task.

Your result in units in part 3 is given by the addition of the number of correct answers and your personal random number in this part.

Your result in units in part 3 = your number of correct answers in part 3 + your personal random number in part 3

For example, if you give 38 correct answers and you draw a random number of -3, then your result is 35 units (i.e. 38-3). If, in contrast, you give 27 correct answers and you draw a random number of +3, then your result is 30 units (i.e. 27+3).

Once you have drawn your number, you are informed on your result in units and on your earnings in points in part 3. For each unit, you get paid 3 points. In addition, you receive a fixed payoff of 108 points.

Your earnings in points in part 3 = (your result in units in part 3 * 3 points) + 108 points

It is important to note that this is **your result** in this part (your number of correct answers + your random number) that will determine the type of task you will perform in part 4.

Part 4

At the beginning of part 4, you are informed on your assignment to 3-letter sentences or to 5-letter sentences during this new part. Your assignment depends on your result in units during part 3.

- If your result in units in part 3 (i.e. your number of correct answers + your random number) amounts to **68 or more**, then you are assigned to solving **5-letter** sentences under the conditions detailed below.
- If your result in units in part 3 is **lower than 68 units**, then you are assigned to solving **3-letter** sentences under the conditions detailed below.

Whatever your assignment is, you will solve sentences during 7 minutes. Your result in units in part 4 is given by the addition of the number of correct answers and your personal random number in this part.

Your result in units in part 4 = your number of correct answers in part 4 + your personal random number in part 4

Here are described the rules for each type of assignment.

◊ **If your result in part 3 assigns you to 3-letter sentences**

Once you have performed your task, you click the « random draw » button that generates your personal random number. Your number can take any value between -12 and +12, included. Each number has an equally likely chance to be drawn. There is an independent random draw for each participant.

You are then informed on your result in units and on your earnings in points in this part. For each unit, you get paid 3 points. In addition, you receive a fixed payoff of 108 points.

Your earnings in points in part 4 if assigned to 3-letter sentences = (your result in units in part 4 * 3 points) + 108 points

◊ **If your result in part 3 assigns you to 5-letter sentences**

Once you have performed your task, you click the « random draw » button that generates your personal random number. Your number can take any value between -4 and +4, included. Each number has an equally likely chance to be drawn. There is an independent random draw for each participant.

You are then informed on your result in units and on your earnings in points in this part. For each unit, you get paid 9 points. In addition, you receive a fixed payoff of 9 points.

Your earnings in points in part 4 if assigned to 5-letter sentences = (your result in units in part 4 * 9 points) + 9 points

Although your assignment to one task or the other depends exclusively on your result in part 3, you will be asked to indicate which task you would prefer being assigned to in part 4 is you would have the choice :

Either the 3-letter sentences, with a random number between -12 et +12, a payoff of 3 points per unit and a fixed payoff of 108 points

Or the 5-letter sentences, with a random number between -4 et +4, a payoff of 9 points per unit and a fixed payoff of 9 points.

You will be requested to submit your answer at the beginning of part 4.
Your earnings during these two parts will be added up to your previous payoffs.

Please raise your hand if you have any questions regarding these instructions. We remind you that communication between participants is still forbidden.