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## Because of you I did not give up - How peers affect perseverance

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# Because of you I did not give up - How peers affect perseverance\*

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## Abstract

Various empirical papers have shown that peers affect productivity and behavior in the workplace. However, the mechanisms through which peers influence each other are still largely unknown. In this laboratory experiment we study a situation in which individuals might look at their peers' behavior to motivate themselves to endure in a task that requires perseverance. We test the impact of unidirectional peer effects under individual monetary incentives, controlling for ability and tactics. We find that peers significantly increase their observers' perseverance, while knowing about being observed does not significantly affect behavior. In a second experiment we investigate the motives to self-select into the role of an observing or an observant subject and what kind of peers individuals deliberately choose. Our findings from this treatment provide first insights on the perception of peer situations by individuals and new empirical evidence on how peer groups emerge.

Keywords: grit, perseverance, laboratory experiment, peer effects, real effort  
JEL codes: C91, D03, M50, J24

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# 1 Introduction

Peers affect productivity and behavior in the workplace. Both theoretical papers (for instance Kandel & Lazear (1992)) as well as several empirical studies (such as the papers by Falk & Ichino (2006), Mas & Moretti (2009) and Bandiera et al. (2010)) show that individuals are influenced by others working besides them. However, the exact channels through which individuals influence each other are still unexplored (Charness & Kuhn 2011). Peer effects could, for example, stem from imitation of behavior (Bandura & Walters 1963, Mobius & Rosenblat 2014), an increase in knowledge about the task (Banerjee 1992, Guryan et al. 2009), competitive preferences (O’Keeffe et al. 1984), guilt and shame (Kandel & Lazear 1992), a desire for conformity (Bernheim 1994, Bellemare et al. 2010), self-perception in comparison to others (Deci & Ryan 1985) or the pure joy of working with others (Bandiera et al. 2010). In this paper we apply a novel experimental design that allows us to study one channel of peer effects in isolation: We analyze if and how peers affect individuals’ perseverance on a task.

By focusing on perseverance, our paper moreover contributes to the emerging economic literature on non-cognitive skills. Recent studies in the fields of psychology and economics have shown that perseverance over a longer period of time – an attribute also known as grit – is an important determinant for various life outcomes. It could be shown that grit elicited from questionnaires is a reliable predictor for pupils’ success in the national spelling bee, retention rates of West Point Military Academy cadets and Grade Point Averages (GPA) among Ivy League undergraduates (Duckworth et al. 2007, Eskreis-Winkler et al. 2014). In Gerhards & Gravert (2015) we introduce the first experimental measure of grit in a short-term laboratory real effort task and show that more gritty behavior leads to higher earnings in the experiment – irrespective of subjects’ initial ability. In the present paper we extend our previous work and combine the elicitation of grit with a peer setting in order to study how gritty behavior is influenced by the information about a peer’s performance. This is an important question since to the present day there is no consensus on whether perseverance in the face of challenges is an inherent trait or a malleable attribute that can be learned (Alan et al. 2015).

We base our research design on the following idea: While workers might not know their co-workers’ initial abilities and cannot assess the quality of the output before the termination of a project, they often have a good estimate of how much effort their colleagues exert. Consider for instance graduate students working in the same office: They can observe on a daily basis when their peers come and go and whether they are working on their research projects or browsing the internet. But different fields of research and methods might make it difficult to compare the peers’ progresses and outputs to their own. Nevertheless, observing the other students working longer and more diligently likely affects a student’s perseverance on his or her own project.

A field setting, like the graduate office, might be confounded by several factors that cannot be controlled for. Examples being (self-)selection based on homophilistic preferences (which can cause spurious correlations (Manski 1993)), “reflection problems” of peers simultaneously influencing each other or complementarities between individuals in case their work tasks or payoffs are interrelated. We therefore decided to conduct a controlled laboratory experiment that excludes these and other confounding factors. Our subjects are randomly assigned to treatments and work on a word play task in which they solve anagrams on an individual piece rate basis. As a way to avoid working hard,

subjects can choose to solve easier anagrams or to skip individual hard anagrams. Both avoidance behaviors come at an explicit cost. We compare the subjects' performance and amount of avoidance behavior when working by themselves (Baseline treatment) to a peer situation (Random Matching treatment) where half of the subjects are randomly assigned the role of an "Observer", who is informed about his or her randomly matched "Peer"'s avoidance behavior. The Peer knows that he is observed by another subject, but does not receive any further information about the Observer.

We find, first, that Observers (compared to subjects from the Baseline treatment) significantly reduce their avoidance behavior if they are presented with information about their Peers' respective behavior. Peers' and Baseline treatment subjects' avoidance behavior, on the other hand, does not differ significantly. Second, Peers' and Observers' avoidance depends significantly less on subjects' self-reported grit than it does for subjects from the Baseline treatment who work in isolation. And third, we find that while the Observers' tendency to skip decreases irrespective of the Peer's actual behavior, the Observers' inclination to switch to easy anagrams is significantly positively correlated with their Peer's switching behavior. Interestingly, this effect is even stronger if the Peer is of similar or worse ability. From this we conclude that staying perseverant on a task does not only depend on individuals' own grittiness, but hinges on the peer group composition at hand.

A handful of other papers have looked at peer effects and performance in laboratory settings. The first three papers that we will briefly describe below, consider peer effects in subjects' performance and output in general. The fourth paper focuses on peer effects in perseverance in particular.

Georganas et al. (2015) randomly assign individuals into observer and peer categories and ask them to work on Gill & Prowse's (2012) slider task. The authors find no overall peer effects on performance when subjects work under individual piece rate incentives. They do, however, conjecture that this could be explained by a significant and dominant learning effect that conceals potential treatment effects.<sup>1</sup> Beugnot et al. (2013) test whether the strength of peer effects depend on the actual presence of a peer. In one of their treatments, subjects are informed about a peer's outcome who has worked on the same real task (a math task) in an earlier session. In a second treatment, subjects are continuously informed about the performance of a peer who is simultaneously working in the same room. In both treatments the authors find significant peer effects for men, but not for women. They conclude that their effects are likely driven by rivalry between peers, which they find to be stronger for men than for women. Van Veldhuizen et al. (2015) set out to replicate the main features and findings of Mas & Moretti's (2009) seminal field study in the lab. Groups of four students work in a team to solve a predetermined number of mathematical calculations. By design, the subjects' payoffs do not depend on the other participants, but their workload does. Depending on their role in the team, subjects can either observe others, are observed by others, do both things simultaneously or cannot do either. In contrast to Mas & Moretti (2009), the authors find no difference between being observed and observing a coworker. They do, however, observe heterogeneous peer effects in productivity when subjects are aware of the general productivity level of their peer. Compared to the studies by Georganas et al. (2015), Beugnot et al. (2013) and

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<sup>1</sup>In a team incentive treatment, they find that observed participants increase their productivity a bit faster than the control group. However, this group catches up to them within 3 minutes of the experiment. Possibly, being observed leads to slightly faster learning in this experiment.

van Veldhuizen et al. (2015), our design allows to analyze peer effects in perseverance in isolation, keeping biasing learning effects, competitive motives and interdependencies between subjects at a minimum. First, we restrict our analysis of treatment effects to the final part of the experimental sessions when further learning can largely be ruled out. Second, we inform Observers only about their Peers’ avoidance behavior and not about their performance or output. And third, we consider performance in individual tasks only.

Similar to us, Bonein & Denant-Bomont (2015) study individuals’ perseverance – in particular their self-control – in the presence of a peer. In their experiment subjects work on the slider task and can commit themselves to an output level *ex ante*. The authors find that subjects choose higher penalties for not reaching their goal if they know that their output will be reported to another participant with whom they interact in a later stage of the experiment. They do, however, not find any effects on increased perseverance on the actual task. Besides the apparent differences between their and our experimental setup, it should moreover be noted that Bonein & Denant-Bomont (2015) measure perseverance as a zero/one decision. Once a subject decides to give in to temptation he or she could not return to the experiment. In a sense, their perseverance measure is hence more strict than our measure of “giving up” in the form of skipping or switching to easy anagrams, which can happen multiple times during the session.

The remainder of the paper proceeds as follows. In section 2 we will present the experimental designs and results from our Baseline and Random Matching treatment. Subsequently, we will widen our focus to the conscious decision of selecting peers. First, we will analyze which personal characteristics predict a preference for being observed (i.e. self-selection into the Peer’s role) rather than observing someone else (i.e. self-selection into the Observer’s role). Second, we investigate which type of Peer Observers choose to observe given information about his previous performance. We will provide a detailed description of the experimental design and a discussion of our findings from this additional Chosen Matching treatment in section 3. The paper concludes in section 4.

## 2 Peer effects

### 2.1 Experimental design

During the experimental sessions subjects work on an anagram word play task in which they have to rearrange the letters of English words to form new ones. As an example consider the word “top” that can be rearranged to “pot” and “opt”. We accept all possible anagrams that can be build from a word as a correct solution and introduce two levels of difficulty. “Easy anagrams” consist of 3 to 4 letters, “hard anagrams” comprise 5 to 7 letters. As discussed in detail in Gerhards & Gravert (2015) the anagram task was perceived as rather challenging and did not favor any of the sexes in the sense that men’s and women’s performance does not differ significantly.<sup>2</sup> Irrespective of treatment, all subjects are presented with the same anagrams in the same order.

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<sup>2</sup>The median “challenge” assessment of participants from the Baseline treatment was 4 out of 5 points. Moreover, there are neither significant gender difference in the number of easy anagrams solved in the practice period (Mann-Whitney ranksum test result:  $p = 0.84$ ), nor in the number of hard anagrams solved per hard rounds in the first ( $p = 0.68$ ) or second part of the experiment ( $p = 0.33$ ) in the Baseline treatment.

The experiment starts with a five minute practice round in which subjects are only asked to solve easy anagrams. Performance in this part is not monetarily incentivized. It allows subjects to familiarize themselves with the experimental task. In the main part of the experiment subjects are paid according to their performance and free to choose between solving hard and easy anagrams. For each correctly solved hard anagram subjects earn DKK 5.00. For each solved easy anagram subjects earn DKK 0.50.

The main part of the experiment is divided into two identical parts of 30 minutes each. Part 1 mainly serves us to measure subjects’ tactics on the task, which is an important control variable that we will discuss in detail in section 2.2 below. Part 2 varies across treatments and is hence the working period of interest in our data analysis. Both parts are partitioned into ten rounds of three minutes. In the first round of each part subjects have to work on hard anagrams. We chose this set-up in order to stress the default character of working on hard anagrams and to make sure that the subjects get to know the level of difficulty of hard anagrams. At the beginning of each of the following nine rounds subjects can choose to “stay with the hard anagrams” or to “switch to the easy anagrams” for the coming three minutes.<sup>3</sup> We hence allow subjects to go back to solving hard anagrams after having switched to easy anagrams in the previous round (and vice versa). If subjects decide to switch to easy anagrams at the beginning of a new round, they have to bear a cost of DKK 3. The total cost of switching to easy anagrams for one round hence consists of the explicit switching cost and the implicit cost of reduced earning opportunities.

Each anagram is presented for up to 90 seconds. If a subject enters a correct solution, a new anagram will be displayed immediately. If a subject does not manage to solve an anagram within the given time frame, a new anagram will be generated free of cost. Importantly, when working on hard anagrams, subjects are given the opportunity to “skip” individual anagrams. Then they do not have to wait until the end of the 90 seconds, but a new anagram is generated immediately. Just like switching to easy anagrams, also each skip comes at a cost of DKK 3.00.

Table 1: Monetary incentives

Action	Payoff
Solving a hard anagram	5.00
Solving an easy anagram	0.50
Skipping a hard anagram	-3.00
Switching to easy anagrams	-3.00
DKK 1.00 corresponds to approximately EUR 0.13 or US-Dollar 0.18.	

Table 1 summarizes the monetary incentives. We chose this incentive structure to make switching to easy anagrams clearly monetarily unattractive. Subjects should be able to see without any formal calculation that even for less able individuals switching to easy anagrams is not a monetarily optimal decision. The choice can hence only be rationalized by a strong desire to avoid working hard.<sup>4</sup>

<sup>3</sup>See Appendix C for a screen shot of this decision stage and examples of other screens that were displayed during the experimental sessions.

<sup>4</sup>Indeed, for only 2 out of our 152 subjects from the Baseline and Random Matching treatment we find that their lowest round earnings on easy anagrams in part 1 lay above their highest round earnings on hard anagrams (-3.00 vs. -2.50 and -1.00 vs. 0).

In both parts of the main part, an information box on the computer screen continuously informs subjects about how many hard anagrams they have already skipped since the beginning of the current part and how many hard anagrams they have skipped in the current round. In rounds in which subjects work on easy anagrams, no such information is displayed.

At the end of part 1 subjects receive feedback about their productivity on hard anagrams and payoffs in that part. Furthermore, we ask them without previous announcement to make an incentivized guess which performance quintile they belong to.<sup>5</sup> In particular, we ask them to make this guess based on their number of correctly solved hard anagrams. We reward the correct answer with DKK 20. After subjects enter their guess they are informed about their actual performance quintile and part 2 starts.

### 2.1.1 Treatments

As already mentioned above, the treatments differ only in part 2 of the experimental sessions. In the Baseline treatment, the instructions presented in the beginning of part 2 simply ask the subjects to continue working on the anagram task, as they did before in part 1.<sup>6</sup> Subjects in the Random Matching treatment are presented a new set of instructions on their computer screens. These inform them that half of the subjects in their session will be randomly assigned the role of an Observer (in order to prevent framing effects called “Person A”) and the other half will assume the role of a Peer (called “Person B”). Always one Observer and one Peer are randomly matched for part 2. Both are presented with the same anagrams in the same order.

Observers learn their Peer’s performance quintile from part 1. Moreover and more importantly, Observers are informed about their Peer’s skipping and switching behavior during part 2: When Observers work on hard anagrams, their computer screens display the total number of hard anagrams the matched Peer has skipped since the beginning of that part and whether the Peer is working on hard or easy anagrams in the current round. This information is updated at the beginning of each round, that is, every three minutes. To prevent the emergence of so called “rat races” we do not provide them with further information on the number of correctly solved hard or easy anagrams. Peers receive no information about their Observer’s performance, but are informed about the type of information that the Observer receives about them.

### 2.1.2 Questionnaire

After having finished working on the real-effort task, we ask the subjects to fill out a short questionnaire that comprises two parts. In the task-specific part we ask them how they perceived working on the task. In the survey part we elicit a number of non-cognitive skills and personality traits through non-incentivized survey questions. In particular, we administer the Short-Grit-Scale (Duckworth & Quinn 2009), which comprises eight items that are ranked on a 5 point scale and measure the tendency to sustain effort and maintaining interests over a long period of time.

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<sup>5</sup>In one of our sessions only 6 subjects participated. Therefore we asked them to guess which performance tertile they belonged to instead.

<sup>6</sup>Note that the Baseline treatment presented in this paper corresponds to the data set that we use to analyze further aspects in our companion paper Gerhards & Gravert (2015).

A valid concern when running questionnaires at the end of an experiment is that the behavior during and the outcome of the experiment might influence the answers given in the questionnaire. To address this point, we randomize the order of the experiment and the survey part of the questionnaire in the Baseline treatment at the session level. The task-related questions are always asked at the end of the experiment. As discussed in more detail in Gerhards & Gravert (2015), the sequence of real effort task and survey questions neither significantly affects the answers given in the questionnaire, nor the performance on the task.

### 2.1.3 Procedures

We ran 16 experimental sessions in spring and summer 2014 at the Cognition and Behavior (Cobe) Lab at Aarhus University. Subjects were recruited via the laboratory’s online recruiting website from a subject pool of mostly undergraduate students from all faculties. In total 152 subjects participated, out of which 69 (45 percent) were female. We observe 62 subjects in the Baseline treatment and 90 subjects in the Random Matching treatment (45 Observers, 45 Peers). Most of the subjects in our sample are Danish undergraduates. The participants had on average 9 years of English at school with 3 years being the minimum.

At the beginning of each session consent forms and detailed instructions about the experimental task were distributed to the subjects (available in Appendix B). The subjects had 10 minutes to read the instructions. Afterwards the experimenter asked if there were any questions or if anyone needed more time to read. When all subjects denied, the experiment (programmed in z-Tree (Fischbacher 2007)) started, and all further instructions were provided on the computer screens. At the end of each session either part 1 or part 2 was randomly selected for payments. Average earnings for the 90 minutes sessions amounted to DKK 120, including the reward for a correct guess about the subject’s own performance rank and a DKK 40 show-up fee. The payments were directly transferred to the subjects’ bank accounts.

## 2.2 Results

In the following statistical analysis we will focus on four outcome variables from part 2: subjects’ earnings, their number of solved hard anagrams per round in which they worked on hard, their number of skipped hard anagrams and their tendency to switch to easy anagrams. One can well consider the first variable a consolidated measure of the three latter ones: The number of solved hard anagrams increases subjects’ earnings, while skipping and switching reduces it.

Figure 1 gives a first overview of how the means of our outcome variables evolve over the ten rounds in the Baseline and Random Matching treatments. In general, the line graphs paint a rather positive picture of peer effects: The upper left graph suggests that Peers and Observers in the Random matching treatment realize higher mean earnings than their counterparts from the Baseline treatment who work in isolation. This seems to result from the fact that Random Matching subjects, on the one hand, solve more hard anagrams (see the upper right graph) and, on the other hand, display less avoidance behavior (see the two lower graphs). In particular Observers skip, and Peers switch on average less often than subjects from the Baseline treatment.



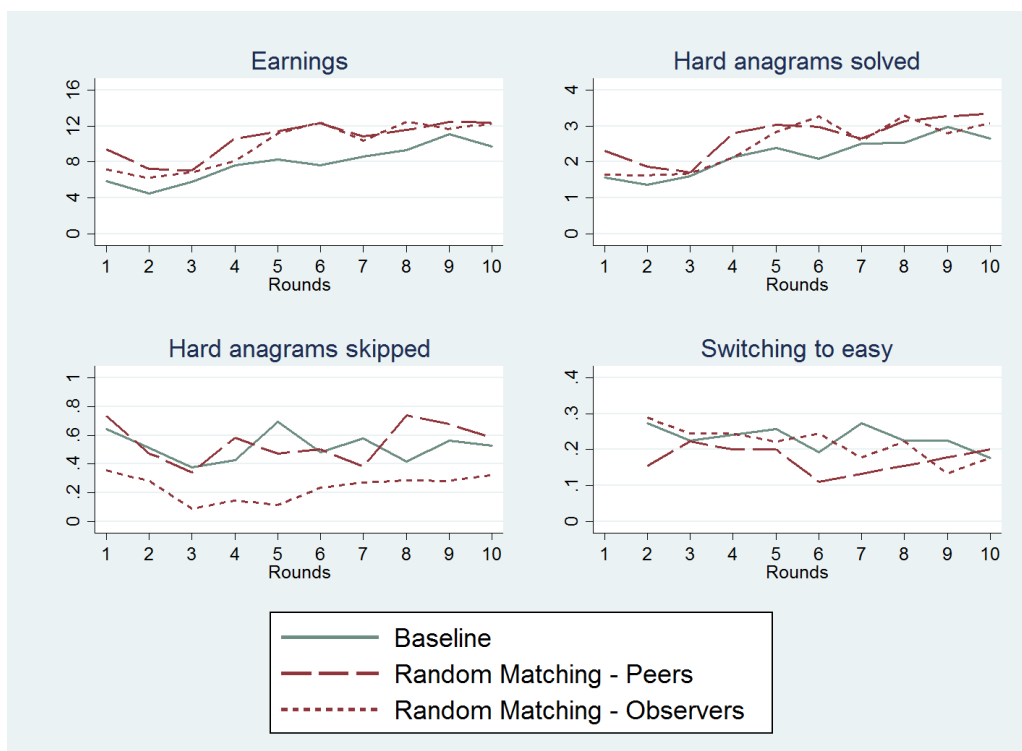


Figure 1: Outcome variables of interest across treatments

In the following regression analysis we are able to control for subject-specific abilities and tactics and go into more depth with regards to the effects of being in a peer situation. We begin by comparing Observers' and Peers' behavior to that of subjects from the Baseline treatment. Subsequently, we move on to study potential heterogeneous treatment effects. And lastly, we will consider the question of which of their Peer's attributes affect Observers most.

### 2.2.1 Observers react to their Peers, but Peers don't seem to care.

Using the pooled OLS models (1) to (3) and the pooled Probit Model (4) in Table 2, we test for general treatment effects and in particular role effects. We regress our four outcome variables on role dummies that take the value 1 for subjects in the Observer and Peer role, respectively. We hence treat the subjects from the Baseline treatment as our reference group. Moreover, in all four specifications in Table 2 we control for subjects' tactics and task ability.

We define tactics as the average round earnings that subjects realized when working on hard anagrams in part 1. These earnings depend on the number of correctly solved and skipped anagrams per round worked on hard. They hence indicate the subjects' capability to find their individual payoff-optimal mix of skipping and solving anagrams, which may well vary across subjects. Some subjects might skip individual hard anagrams that they cannot solve in reasonable time, deliberately accepting the costs of DKK 3, in order to try their luck on the next word that might yield them a payoff of DKK 5. Other subjects might refrain from skipping entirely in order not to reduce their earnings. Our consolidated tactics measure allows us to control for these individually optimal strategies in a unified way.

Table 2: Observers and Peers compared to Baseline subjects

	Hard anagrams...			
	Earnings	... solved	... skipped	Switch to easy
	(1)	(2)	(3)	(4)
Observer	-0.039 (0.829)	-0.236 (0.213)	-0.393*** (0.143)	0.067 (0.234)
Peer	-0.267 (0.908)	-0.055 (0.225)	-0.094 (0.163)	-0.009 (0.236)
Constant	7.380*** (0.731)	1.895*** (0.169)	0.729*** (0.133)	-1.064*** (0.177)
Tactics	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Ability	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
$R^2$	0.406	0.476	0.158	
Pseudo $R^2$				0.128
Number of clusters	152	152	152	152
Observations	1520	1232	1232	1368

Regression 1: pooled OLS, dependent variable: Earnings; Regression 2: pooled OLS, dependent variable: Number of solved hard anagrams (given worked on hard); Regression 3: pooled OLS, dependent variable: Number of of skipped anagrams in hard; Regression 4: pooled Probit, dependent variable: Decision to switch to easy anagrams. Note that all continuous independent variables are standardized. Cluster-robust standard errors are given in parentheses: \*  $p < 0.10$  \*\*  $p < 0.05$  \*\*\*  $p < 0.01$ . All regressions include round dummies.

Ability is defined as the number of solved easy anagrams in the unincentivized practice period. It might hence also reflect the subjects' general motivation to work on the task. Ideally, we would have preferred to define ability analogously to the tactics measure, that is, as the average number of correctly solved anagrams when working on hard anagrams in part 1. However, this measure is by definition correlated with our tactics variable and would hence have led to severe multicollinearity problems in our regressions.

As becomes evident from Table 2, most of the role differences in Figure 1 are not statistically significant once one includes the necessary controls for subjects' tactics and ability. Subjects earnings, their number of solved hard anagrams and their propensity to switch to easy anagrams do not differ across roles and hence across treatments. The coefficient of the Peer dummy is in neither of the regressions significant. We do, however, find that Observers skip significantly fewer hard anagrams than subjects from the Baseline treatment. We summarize our findings as follows:

**Result 1** *Being able to observe another subject significantly reduces the Observers' tendency to skip hard anagrams. In contrast, being aware of the fact that another subject observes one's actions does not significantly affect Peers' avoidance behavior.*

This finding is surprising, as it is in contrast to both Mas & Moretti (2009), who find stronger effects for Peers than for Observers, and to van Veldhuizen et al. (2015), who find that there is no difference in the behavior of Peers and Observers. A potential explanation lies in the fact that Observers and Peers in our study are not dependent on each other – neither in their workload nor in their payoff. In our case it thus seems highly

unlikely that Peers work hard out of guilt or fear of upsetting their team, which might be a driving factor in the two other experiments.

Another noteworthy aspect is the fact that we find significant overall peer effects in skipping, but not in switching. We suspect that the high explicit and implicit costs of switching to easy anagrams reduce subjects' tendency to switch in general and hence leave less room for peers to have an effect. Nonetheless, we continue to consider peer effects in both avoidance behaviors – skipping and switching – in the following steps of our analysis, since the overall null effects might conceal noteworthy heterogeneous treatment effects.

### 2.2.2 Successful subjects perform even better in a peer situation

The question that arises from Result 1 is whether the mean peer effect on skipping (and switching) depends on specific attributes of the Observers. In particular, is the effect driven by high skilled Observers, who get more motivated by being in a peer situation – or rather by low skilled individuals, who imitate their Peers' tactics? To answer this question we will consider interaction effects between individuals' task ability and their role as well as between their tactics on hard anagrams and their role.

Moreover, we will tackle the question of whether subjects can learn from each other to behave more perseverant conditional on their grit level that we elicit in the final questionnaire using Duckworth & Quinn's (2009) scale. This issue is of particular interest since in our companion paper Gerhards & Gravert (2015) we find that self-reported grit is significantly correlated with a lower tendency to skip hard anagrams if subjects work in isolation. It is an open question whether this similarly holds in situations in which peers are present.

The regressions reported in Table 3 resemble those in Table 2. Our dependent variables are again earnings (Model (1)), the number of hard anagrams solved (Model (2)), the number of skipped hard anagrams (Model (3)) and the decision to switch to easy anagrams (Model (4)). We regress these outcome measures on role dummies for Observers and Peers (that is, subjects from the Baseline treatment are again treated as the reference group to both roles), our measures of tactics and task ability and subjects' self-reported grit. Moreover, we include interaction terms of these personal attributes and the role dummies. All continuous variables are standardized in order to facilitate the interpretation of the main and interaction effects.

When it comes to earnings, the positive and significant tactics main effect in Model (1) reveals that a successful tactic in part 1 increases Baseline subjects' earnings in part 2 of the experiment. Interestingly, this positive tactics effect is even stronger for Observers, as indicated by the corresponding positive and highly significant interaction term. Similarly, Observers benefit comparably more from their general task ability than Baseline subjects. Self-reported grit influences neither Baseline subjects', nor Observers' earnings significantly.

The findings on performance on hard anagrams from Model (2) largely replicate our result from Model (1). Successful tactics on hard anagrams positively affect the Baseline subjects' amount of correctly solved anagrams. Moreover, this effect is even stronger for Observers and Peers. We summarize our findings below:

**Result 2** *Being able to observe a Peer performing the same task reinforces the generally positive effects of successful tactics (and task ability) on earnings through an increase in the number of correctly solved hard anagrams.*

Table 3: Heterogeneous treatment effects for Observers and Peers compared to Baseline subjects

	Hard anagrams...			
	Earnings	... solved	... skipped	Switch to easy
	(1)	(2)	(3)	(4)
Observer	0.370 (0.760)	-0.253 (0.190)	-0.353*** (0.128)	0.248 (0.307)
Peer	0.494 (0.763)	-0.003 (0.195)	-0.161 (0.153)	0.210 (0.304)
Tactics	4.695*** (0.707)	0.931*** (0.208)	0.312** (0.147)	-1.398*** (0.409)
Observer x Tactics	3.617*** (1.189)	0.804*** (0.269)	-0.233 (0.171)	0.722 (0.487)
Peer x Tactics	1.638 (1.017)	0.697** (0.301)	0.186 (0.240)	0.875* (0.512)
Ability	-0.265 (0.667)	0.166 (0.188)	-0.017 (0.130)	0.985*** (0.210)
Observer x Ability	2.792* (1.466)	0.413 (0.343)	0.173 (0.182)	-1.021*** (0.293)
Peer x Ability	0.249 (0.930)	0.000 (0.249)	0.217 (0.205)	-0.592* (0.315)
Self-reported grit	0.178 (0.492)	-0.215 (0.155)	-0.370*** (0.134)	-0.008 (0.189)
Observer x Self-reported grit	-0.043 (0.765)	0.061 (0.204)	0.348** (0.148)	-0.077 (0.254)
Peer x Self-reported grit	-0.905 (0.672)	0.161 (0.186)	0.488*** (0.161)	-0.012 (0.244)
Constant	6.833*** (0.658)	1.822*** (0.145)	0.722*** (0.126)	-1.282*** (0.266)
$R^2$	0.445	0.511	0.252	
Pseudo $R^2$				0.166
Number of clusters	152	152	152	152
Observations	1520	1232	1232	1368

Regression 1: pooled OLS, dependent variable: Earnings; Regression 2: pooled OLS, dependent variable: Number of solved hard anagrams (given worked on hard); Regression 3: pooled OLS, dependent variable: Number of of skipped anagrams in hard; Regression 4: pooled Probit, dependent variable: Decision to switch to easy anagrams. Note that all continuous independent variables are standardized. Cluster-robust standard errors are given in parentheses: \*  $p < 0.10$  \*\*  $p < 0.05$  \*\*\*  $p < 0.01$ . All regressions include round dummies.

Model (3) yields the most interesting results. First, it reproduces our finding from Table 2: Being in the Observer role generally reduces subjects’ amount of skipping. The respective “Observer” main effect is highly significant and the size of the coefficient remains largely unaffected by the inclusion of further control variables and interaction terms (compare the corresponding coefficient in Model (3) of Table 2). Moreover, we find that, for Baseline subjects, profitable tactics in part 1 are significantly positively correlated with the number of skips, suggesting that a certain amount of skipping can indeed be explained by money-maximizing, strategical considerations. Lastly, we find that Baseline subjects skip significantly less the more gritty they are. Remarkably however, this effect is significantly less pronounced for Observers and Peers. These subjects from the Random Matching treatment skip fewer hard anagrams, irrespective of their initial level of grit. This suggests that own grit and being in a peer situation can act as substitutes when it comes to reducing avoidance behavior.

Model (4) provides similar insights in the sense that also the Random Matching subjects’ inclination to switch depends less on their personal characteristics than the Baseline subjects’: In particular, we find that the Baseline subjects’ tendency to switch to easy anagrams depends negatively on their tactics on hard anagrams and positively on their task ability (remember, we defined the ability variable as performance on easy anagrams). Observers’ – and to a lesser extend also Peers’ – switching decisions, on the other hand, depend significantly less on their ability. Taken together, our findings from Model (3) and (4) lead to our next result:

**Result 3** *Subjects’ abilities and attributes play less of a role in peer situations in which they can observe each other or are aware of the fact that their performance is reported to a peer.*

Ultimately, our findings from Model (3) suggest that being in a peer situation can work as a substitute for own grit. A potential reason for this could be that Observers become more perseverant by imitating the behavior of their Peer. An alternative explanation could be that merely mentioning the Peer leads Observers to become more perseverant on the task. In the following we will disentangle these two effects – that is the information effect resulting from observing the Peer’s actual behavior and the social facilitation effect caused by the mere presence of the Peer (first discussed by Zajonc (1965)).

### 2.2.3 Similar able Peers affect Observers’ performance most

In the regressions presented in Table 4 we focus on potential determinants of Observers’ avoidance behavior and hence consider only subjects from Random Matching treatment in the role of Observers.<sup>7</sup> As described in section 2.1 above, in rounds in which Observers work on hard anagrams, the computer screen continuously informs them about their Peer’s avoidance behavior. In particular, Observers are informed whether their Peer works on hard or easy anagrams in the present round and how many hard anagrams the Peer has skipped until the previous round.<sup>8</sup> In the pooled OLS Model (1) we estimate the effect

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<sup>7</sup>Note that we can consider only 42 out of our 45 Observers in the regressions in Table 4 since three Observers switched to easy anagrams in every single round in which this was possible. They hence never received information about their Peers’ avoidance behavior.

<sup>8</sup>Since the instructions emphasized that we classify the decision to switch to easy anagrams like a skip of a hard anagram, the latter figure also includes the number of switches.

Table 4: Random Matching Treatment: How Peers affect Observers' behavior

	(1) Skips in hard	(2) Switch to easy
Peer from higher quintile	-0.071 (0.176)	0.770* (0.464)
Peer's skips until present round	-0.072 (0.043)	
Peer's skips x Peer from higher quintile	0.149 (0.099)	
Peer switched in previous round		1.122** (0.514)
Peer switched x Peer from higher quintile		-1.842*** (0.652)
Constant	0.270* (0.150)	-2.019*** (0.595)
Tactics	<i>Yes</i>	<i>Yes</i>
Ability	<i>Yes</i>	<i>Yes</i>
$R^2$	0.186	
Pseudo $R^2$		0.151
Number of clusters	42	42
Observations	317	280

Regression 1: pooled OLS, dependent variable: Number of of skipped anagrams in hard; Regression 2: pooled Probit, dependent variable: Decision to switch to easy anagrams. Note that all continuous independent variables are standardized. Cluster-robust standard errors are given in parentheses: \*  $p < 0.10$  \*\*  $p < 0.05$  \*\*\*  $p < 0.01$ . All regressions include dummy variables for sub-periods.

of the Peer's skipping information on the Observers' amount of skipping. In the pooled Probit Model (2) we study how the Peer's switching decision in the last round affects the Observers' decision to switch to easy anagrams in the present round. Since these information effects might well depend on the similarity (or dissimilarity) of the Observer and Peer, we add a further dummy variable that takes the value one if the Peer was from a higher performance quintile (i.e. solved more hard anagrams correctly in part 1) than the Observer. We additionally interact this "Peer from higher quintile" dummy with both our information variables.

Model (1) reveals that neither the main effect of Peer's skipping in the previous rounds, nor the main effect of Peer's relative ability is significant. Neither of the two variables is able to significantly explain the Observer's tendency to skip hard anagrams. This finding, together with the significantly negative "Observer" main effect from Model (3) in Table 3, suggests that Observers reduce their amount of skipping compared to Baseline subjects due to a general social facilitation effect (Zajonc 1965). Apparently, any kind of information about the Peer leads the Observer to skip significantly less often. The actual behavior of the Peer plays less of a role. The insignificant interaction term "Peer's skips x Peer from higher quintile", moreover, suggests that this holds irrespective of the Peer's relative ability.

However, when we consider the Observers' tendency to switch to easy anagrams, that

is, the more fundamental form of avoidance behavior, Model (2) implies that the Peer’s behavior actually does influence the Observer’s behavior. First, the positive “Peer switched in previous round” main effect indicates that if a similar or less able Peer switched (did not switch) in the previous round, this significantly positively affects the Observer’s decision (not) to switch. Second, as indicated by the negative coefficient of the interaction term “Peer switched x Peer from higher quintile”, the Peer’s switching decision is significantly less correlated with the Observers’ decision if the Peer is from a higher performance quintile. Lastly, and in a similar vein, the positive and significant main effect of “Peer from higher quintile” suggests that observing a better Peer staying with hard anagrams leads the Observer to switch to easy more often. We summarize our findings from Table 4 as follows:

**Result 4** *The Observers’ tendency to skip is neither significantly correlated with their matched Peers’ relative task ability, nor with their actual skipping behavior. Observers do, however, imitate their Peer’s switching behavior, if they are matched to a similar or less able Peer. If the Peer is out of reach ability-wise, his presence and his actual behavior have less of an effect on the Observer’s switching decisions.*

### 3 Self-selection into the Observer and Peer Roles

The fact that the Peer’s impact on their Observer differs depending on the relative ability of the Peer raised two further research questions.

First, we got interested in the question of what kind of Peer Observers would like to observe if they had a choice. According to the social comparison model by Falk & Knell (2004) Observers should on average choose Peers that are similar to them. This conclusion results from balancing two competing motivations: On the one hand, an upwards comparison can inspire individuals to work harder (self-improvement). On the other hand, a downwards comparison can make them feel better about themselves (self-enhancement). Thus, as own ability rises, the reference level for social comparison rises as well. Battaglini et al. (2005) extend the ideas of Falk & Knell (2004) and consider social comparison in situations that require perseverance in particular. They predict that the ideal peer is someone who has a slightly worse self-control problem than oneself, because this makes his successes more encouraging, and his failures less discouraging. Based on these theoretical models we conjecture that Observers in our experiment choose Peers who are equally or slightly less able than themselves. Note that this hypothesis is also in line with our Result 4, stating that similar or worse Peers had the strongest effects on switching behavior for Observers.

Second and more generally, we wondered which factors and personal characteristics are able to explain a desire to voluntarily self-select into the Observer and Peer role. The questionnaire data from the Random Matching treatment might provide a first hint. Since we exogenously and randomly assigned Observers and Peers to their roles in this treatment, this data allows us to cleanly identify potential differences in task perception depending on role. In particular, we asked all subjects from the Random Matching treatment (1) how much they enjoyed working on the task, (2) how challenging and (3) how exhausting they perceived the task and, finally, (4) how much effort they provided during the task. Answers were given on 5-point scales. At the aggregate, ranksum tests do not reveal significant differences between the answers provided by Observers and Peers

( $p = .43$ ,  $p = .25$ ,  $p = .47$  and  $p = .25$ , respectively). But the picture changes if we restrict the group of Observers to those 22 subjects who were matched to a Peer from a higher performance quintile. These Observers report to having enjoyed the task significantly less ( $p = .07$ ) and having perceived the task as a greater challenge ( $p = .03$ ) than the 45 Peers. From this one could conjecture that subjects in the Chosen Matching treatment will exhibit a tendency to avoid the Observer role – in particular if they expect to observe a Peer from a better performance quintile, for instance, due to their own rather low ability. However, on the other hand, one could also argue that subjects dislike and hence rather avoid the position in which they are observed by others, that is, the Peer role. The fact that only avoidance behavior and not positively connoted productivity is reported to the Observer, might reinforce this effect. It is hence an empirical question, which of the two effects prevails, i.e. which role is preferred on average.

In order to tackle the questions of peer selection and role choice, we designed a third treatment, the Chosen Matching treatment, that we will describe in the following.

### 3.1 Experimental Design: Chosen Matching Treatment

In the summer and fall 2014 we invited 90 additional subjects to the lab using the same recruiting procedure as for the Baseline and Random Matching Treatment. The only difference to the Random Matching treatment is the intervention that we introduce between part 1 and part 2: After guessing and being informed about their performance quintile in part 1, all subjects have to make two choices for part 2. First, we ask them to indicate whether they want to assume the role of an Observer or of a Peer (labeled Person A and Person B, similar to the Random Matching treatment). We inform subjects that the computer allocates the desired roles under the premise that within each performance quintile always one of the subjects assumes either role. This procedure is strategy-proof, as the subjects' best strategy is still to state their preferences truthfully, and yet it gives us the flexibility to allocate the experimental roles in an efficient way.<sup>9</sup>

Subsequently, we ask the subjects to state from which performance quintile they want to observe a Peer in case they are assigned the role of an Observer. Using these procedures it is well possible that more than one Observer observes a Peer. For all participants who actually assume the role of a Peer, we elicit their belief on somebody choosing to observe them or not. However, at this stage the Peers are not informed whether and how many subjects observe them. Only after they have finished working the anagram task, in the end of part 2 they receive information on whether their guesses about actually being observed was correct or not – and whether they will earn additional DKK 20 for their correct guess.

### 3.2 Results: Chosen Matching Treatment

In the following, we first consider subjects' peer choices. That is, in the words of Falk & Knell (2004) we focus on the question of whether rather the self-enhancing (downward) or the self-improving (upward) comparison motivation determines subjects' Peer choices.

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<sup>9</sup>In fact, 29 out of 90 subjects in that treatment were allocated their roles randomly: 6 out of 28 subjects, who wanted to be an Observer had to assume the Peer role, 23 out of 62 subjects, who stated a preference for the Peer role had to take on the role of an Observer. The remaining 61 subjects could assume the roles they initially stated as their preferred choice.



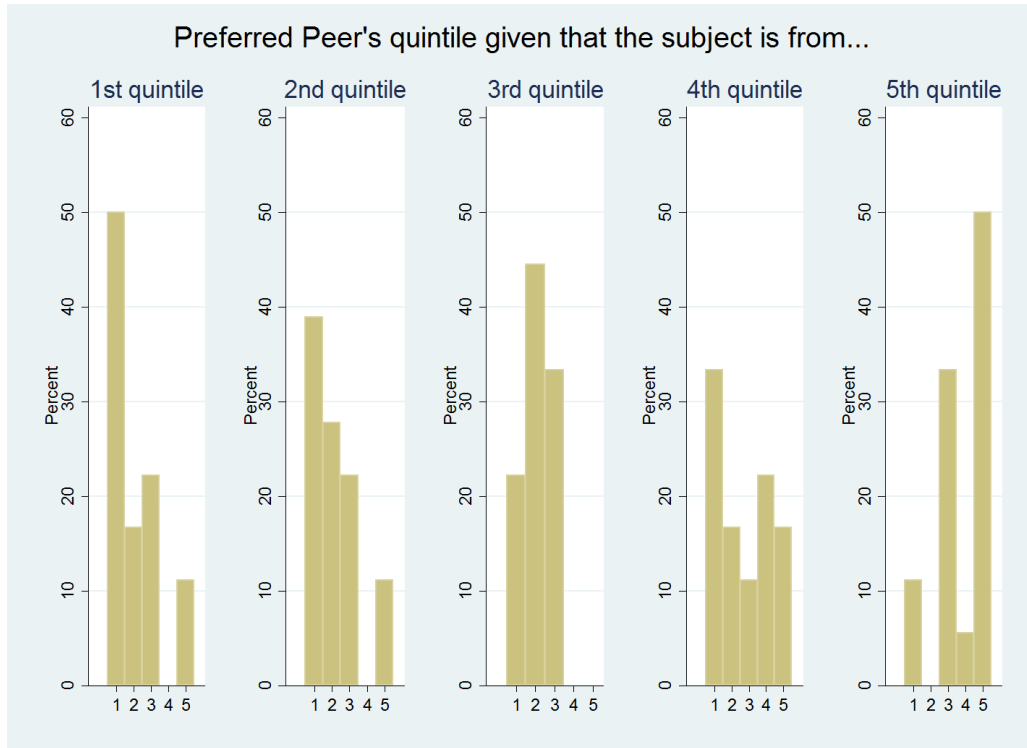


Figure 2: Choice of Peer quintile

Figure 2 shows for each of the subjects' performance quintile separately, their preferred Peer's performance quintile. As evident, in all five quintiles, at least fifty percent of the subjects prefer to be informed about the avoidance behavior of a Peer from their own or a better performance quintile. This suggests that, on average, the self-improvement comparison motivation seems to prevail in our task. However, another non-negligible fraction of our subjects also wishes to observe the avoidance behavior of a (slightly) worse peer, which is in line with Battaglini et al.'s (2005) prediction.

Next we consider subjects' role choices. Interestingly, we find that 62 out of the 90 subjects want to assume the Peer role, that is, they want to be the ones being observed. Using a regression analysis we focus on the determinants of this choice. In individual Probit models, we regress the subjects' decision to become a Peer (=1, vs. an Observer =0) on their average performance on hard anagrams in part 1, their amount of skipping and switching in part 1, overconfidence (regrading their performance quintile in part 1) as well as on their self-reported levels of narcissism, grit, altruism and gender that we elicit in the questionnaire. It turns out that only two of these variables have significant effects on the desire to assume the Peer role. Subjects' average performance on hard anagrams is positively, their inclination to switch to easy anagrams is negatively correlated with their wish to be observed. The corresponding regression table is reported in Appendix A.

Still puzzled by the finding that about two thirds of the subjects wanted to take on the Peer role, we further consulted their statements in the final questionnaire where we asked them to explain why they selected a specific role. Interestingly, 41 out of the 62 wannabe Peers stated to having chosen that role in order to avoid any information that might have distracted them from their work. One of those subjects, for instance, stated: *"I did not want to observe anybody else, as I thought it would be annoying while working."*

These subjects were hence consciously self-selecting out of a situation in which they would receive information about peers. Since we did not provide them with a neutral “No peer option”, they could only self-select into the Peer role. Only three of the remaining 21 subjects who wanted to assume the Peer role stated that they wanted to do so in order to provide a good example for their Observer. One of them for instance wrote that she *“felt very confident in the experiment, so [she] figured that someone else, might ben[e]fit from seeing, what/how [she] was doing.”*

## 4 Conclusion

Although the literature on peer effects has been growing over the last couple of years, the precise mechanisms how peer effects arise are still a black box. In this paper we uncovered some new puzzle pieces that help us to better understand how individuals are affected by peers. Instead of measuring peer effects in quantitative productivity, we study a how subjects’ behavior in a rather qualitative dimension – perseverance – is affected by the presence of a peer.

We measure perseverance in an word play task in which subjects can skip anagrams or switch to easier anagrams in order to avoid working on the default task, that is, solving hard anagrams. After working independently in part 1 of the experiment subjects in our Random Matching treatment are randomly assigned the role of an Observer or a Peer and randomly matched to each other. Observers receive information about the skipping and switching behavior (i.e. the reverse of perseverance) of their Peer. Peers are only informed that this behavior is reported to the Observer.

We find that Observers’ skipping behavior is significantly decreased when they receive information about a Peer. This effect is independent of the Peer’s actual behavior, which suggests a general social facilitation effect. Only when considering the switching behavior of Observers who are matched to a similar or worse Peer, we find that Observers react to the actual behavior of their Peer. In particular, Observers seem to act according to the motto “if he can do it, so can I”: When their Peer stayed with hard anagrams in the previous round, the Observer has a higher propensity to stay with hard, too. If, conversely, the Peer is out of reach ability-wise, his presence and his actual behavior have less of an effect on the Observer’s switching behavior. For Peers, being observed has no effect on their behavior. On average, they do not behave significantly different than subjects from our Baseline treatment who work in isolation.

To understand which role subjects would prefer if given a choice and what type of Peer they would want to observe, we ran a third treatment in which we allocate roles based on the subjects’ stated preferences. We find that most subjects prefer to observe a similar or slightly more able Peer in case they have to assume the role of an Observer. In general, however, the majority of subjects prefers to assume the Peer role. Survey evidence suggests that most of these subjects would prefer to avoid the peer situation entirely. They only chose the Peer role as a means to avoid being distracted, even if this meant that their avoidance behavior was observed by others.

This finding warrants further study, as it shines new light on how to interpret peer effects. Individuals might prefer to work alongside a peer in a tedious task such as Falk & Ichino’s (2006) folding letters into envelopes task – and they do so potentially in order to experience social facilitation rather than in order to learn from observing a peer. If they

are, however, confronted with a mentally more challenging task that requires a high level of concentration, they might prefer to work in isolation in order not to get distracted by peer information.

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## A Additional tables

Table A.1: Chosen Matching Treatment: Determinants of Peer role choice

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Solved hard anagrams in part 1	0.183*							
	(0.100)							
Skipped hard anagrams in part 1		0.019						
		(0.037)						
Switches to easy anagrams in part 1			-0.108**					
			(0.053)					
Self-reported narcissism				0.102				
				(0.528)				
Self-reported grit					-0.073			
					(0.234)			
Self-reported altruistic inclination						0.016		
						(0.052)		
Overconfident regarding performance quintile in part 1							-0.279	
Female							(0.294)	
								-0.111
								(0.280)
Constant	0.114	0.446***	0.810***	0.445	0.724	0.379	0.587***	0.541***
	(0.242)	(0.164)	(0.213)	(0.283)	(0.759)	(0.407)	(0.172)	(0.186)
Pseudo $R^2$	0.033	0.003	0.037	0.000	0.001	0.001	0.008	0.001
Observations	90	90	90	90	90	90	90	90

Probit regressions with robust standard errors that are given in parentheses: \*  $p < 0.10$  \*\*  $p < 0.05$  \*\*\*  $p < 0.01$ .

## B Instructions

**Welcome and thank you for taking part in our experiment.**

In this experiment you can, depending on the decisions you make, earn money in addition to the 40kr you receive as a show-up fee. Your earnings will be added to your show up fee and transferred to your Nem-ID account after the session.

You are not allowed to communicate during the session. If you have questions, please raise your hand. A member of the research team will come to you and answer them in private. Violation of this rule will lead to the exclusion from the session and all payments.

Please take your time to read the instructions carefully before the start of the experiment!

The whole experiment will take about 1.5 hours.

### **Instructions**

The experiment consists of 4 parts. The first part consists of a questionnaire. You will receive the instructions for the subsequent parts of the experiment after you have filled out the questionnaire.

Figure 1: First Page Instructions

### Instructions

The experiment consists of 4 parts. A practice round of 5 minutes and two payment-relevant parts of 30 minutes each where you will be asked to work on a task. The last part consists of a questionnaire. You will get paid, if you complete all 4 parts.

#### Description of the task

You will be asked to solve anagrams consisting of 5 to 7 letters. An anagram is a word formed by reordering the letters of another word.

Example of an anagram:

section -> notices

**For each solved anagram you will earn 5kr.** You have 90 seconds to work on each anagram. You can only move to the next anagram by entering a correct solution and clicking the **“Submit”** button. If you submit a wrong answer, the anagram will remain the same until you solve it or the time runs out. Submitting a wrong answer is not penalized. If you have not solved the anagram within the time limit a new anagram will be generated at no cost.

You have the possibility to skip an anagram, if you do not know the answer. If you click the **“Skip”** button a new anagram will be generated and **3kr will be deducted from your total earnings.**

The two payment-relevant parts of 30 minutes are divided into 10 sub-periods of 3 minutes each. After every sub-period you have the opportunity to switch to easy anagrams with 3 to 4 letters by clicking the **“Switch to Easy”** button. **For each solved easy anagram you will earn 0.5kr.** As with the hard anagrams you have 90 seconds to solve it until the time runs out and a new anagram will be generated. When **switching from hard to easy anagrams 3kr will be deducted**, as this is equivalent to skipping a hard anagram. Stay with the hard anagrams by clicking the **“Stay with Hard”** button leads to no deduction.

Anagrams might have more than one solution. We will accept any correct solution.

Figure 2: Second Page Instructions



While you work on the task several pieces of information will be displayed on the screen: A countdown for the 3 minutes sub-period that you currently work in, a countdown for the current anagram and the number of anagrams you have skipped in the current sub-period of the experiment. Please note that as you approach the end of a three minute sub-period, the sub-period countdown determines how much time you have left to work on the displayed anagram (and not the countdown for the current anagram).

In the first 5 minutes of the experiment you have the chance to familiarize yourself with the task by solving easy anagrams in a **practice round which is not payment relevant!** After 5 minutes you will be told that the payment relevant part begins.

At the end of the second payment-relevant part, either the first or the second part will be randomly selected to calculate your payment. Your earnings will depend on your performance in the chosen part. It is therefore in your best interest to always act as if it is the part that counts.

**Payoff example:**

Consider the case that a person solved 30 hard and 10 easy anagrams, skipped 2 hard anagrams and switched 2 times to the easy anagrams in the part of the experiment that was randomly selected for payment.

The person hence earns  $30 \cdot 5kr + 10 \cdot 0,50kr - 2 \cdot 3kr - 2 \cdot 3kr = 150kr + 5kr - 6kr - 6kr = 143kr$  additional to the 40kr show-up fee.

Please note: Those sample numbers are randomly chosen and must not necessarily correspond to actual behavior of previous participants in this experiment!

Figure 3: Third Page Instructions

# C Screenshots of the experiment

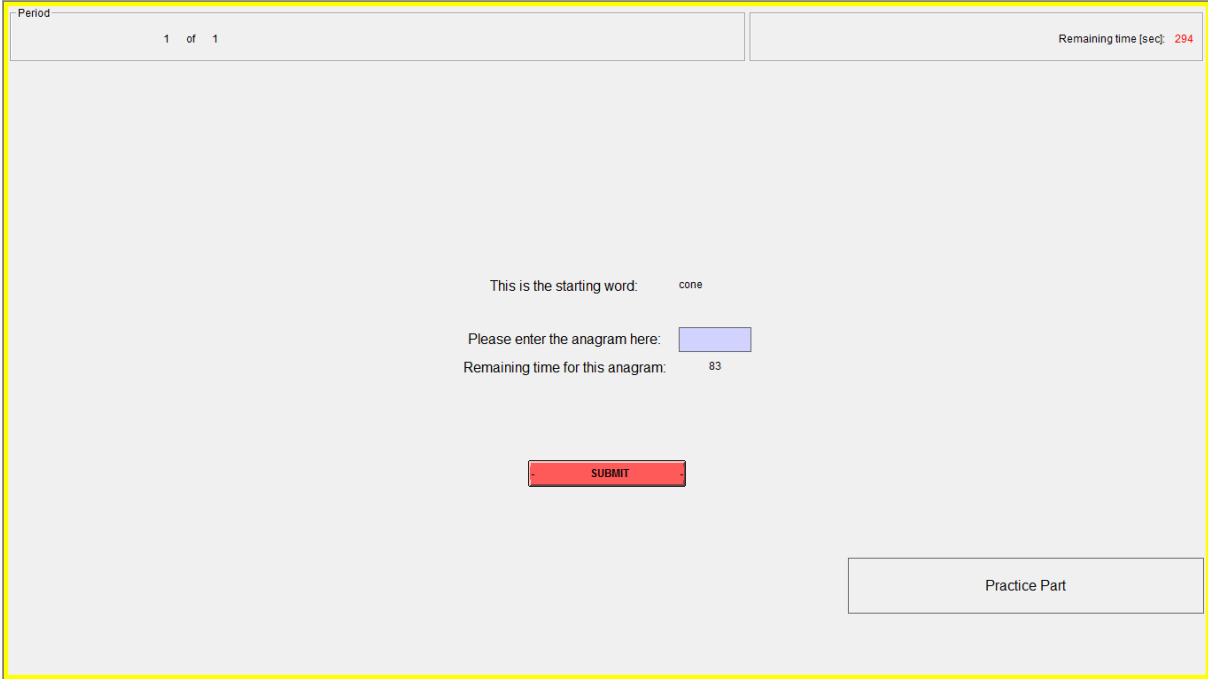


Figure 4: Practice round

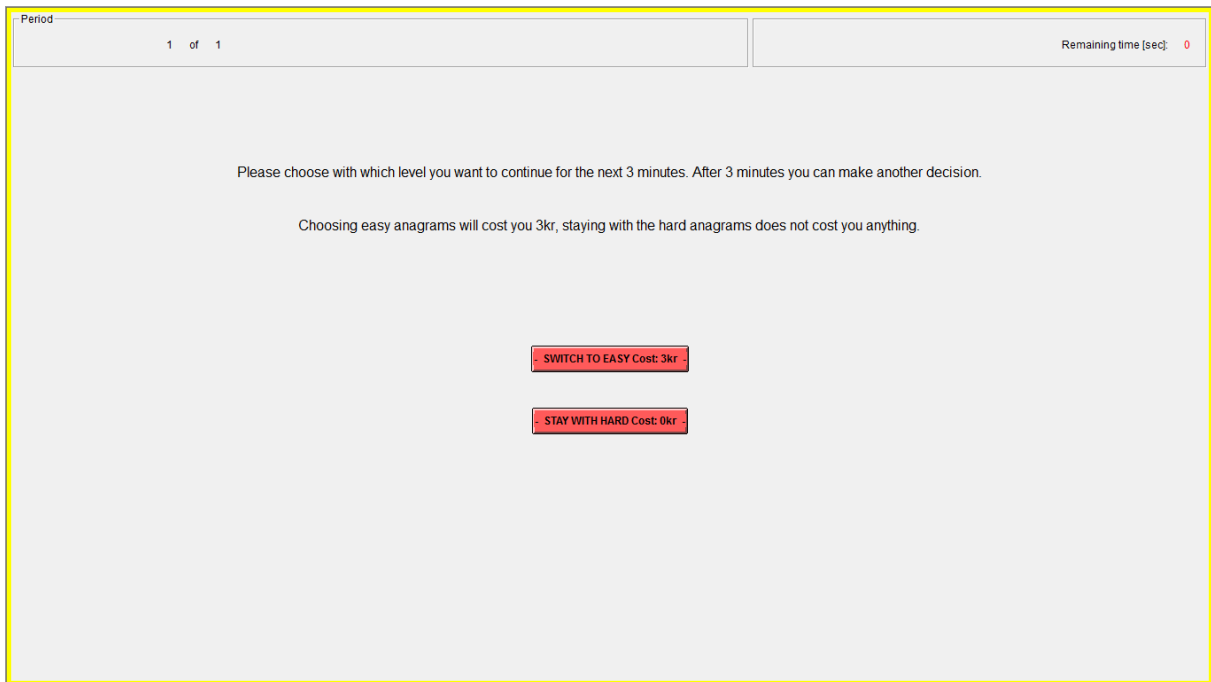


Figure 5: Decision screen

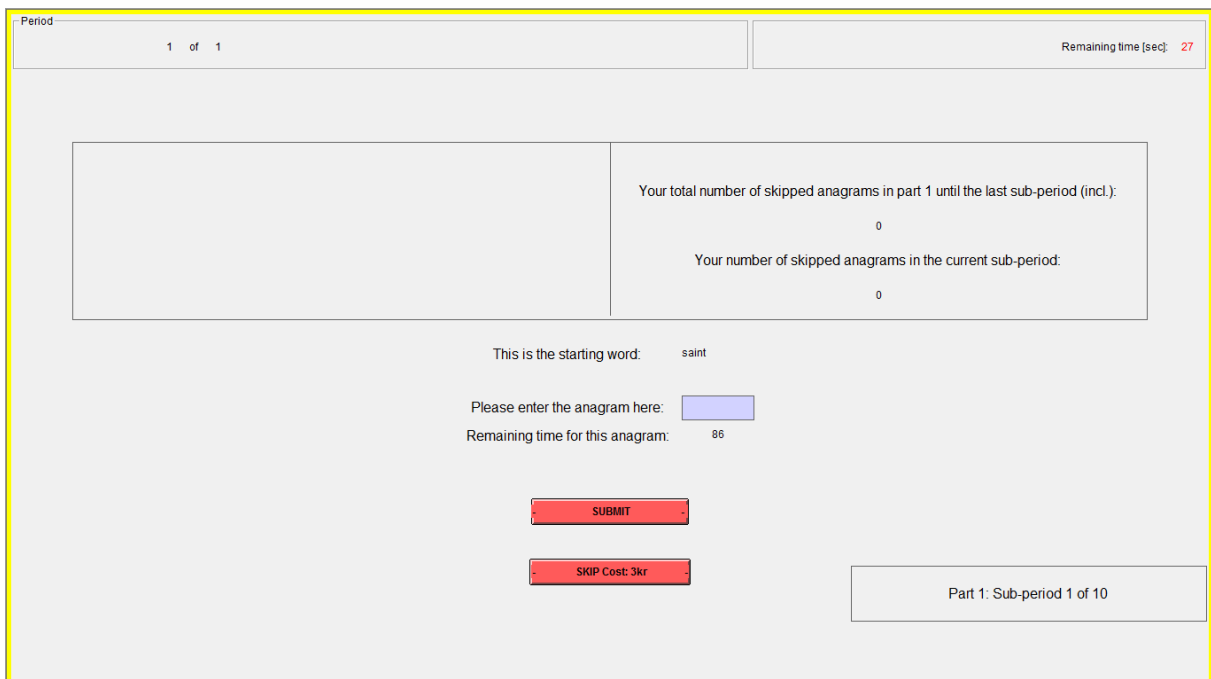


Figure 6: Working on hard anagrams

Period 1 of 1 Remaining time [sec]: 175

This is the starting word: slaw

Please enter the anagram here:

Remaining time for this anagram: 84

Part 2: Sub-period 5 of 10

Figure 7: Working on easy anagrams

Period 1 of 1 Remaining time [sec]: 17

In this part you have correctly solved **0** hard anagrams, **0** easy anagrams and skipped **0** anagrams.

Your total earnings from this part are **0kr**.

There are 9 other participants who have worked on the same task.

All of you 10 participants will be sorted into 5 groups with 2 participants each, based on how many **hard anagrams** you solved correctly. Each of these 5 groups is called a quintile.  
In which quintile do you think you are based on your **number of solved hard anagrams** compared to the other participants?  
(First quintile are the most correctly solved anagrams, fifth quintile the least correctly solved anagrams)

You will receive **20kr extra**, if you guess correctly.

My guess:

- First Quintile
- Second Quintile
- Third Quintile
- Forth Quintile
- Fifth Quintile

**SUBMIT**

Figure 8: Expectation about own ability in part 1

Period

1 of 1

Remaining time [sec]: 13

Please get ready for part 2.

The task in part 2 is identical to the task in part 1.

Click "Start" when you are ready.

START

Figure 9: Instructions for part 2: Baseline treatment

Period

1 of 1

Remaining time [sec]: 30

Please get ready for part 2.

The task in part 2 is identical to the task in part 1.

In part 2 you will be **randomly** matched with another participant.

Both of you will work on the same task as in part 1 and the order of anagrams to solve is the same for both of you.

One of you assumes the role of "Person A", the other one is "Person B". Independent of the role, you will receive information about your own progress on the task, similar as in part 1.

**Person A will additionally receive information about the progress of Person B.**

On the top of his/her screen Person A will see:

- If Person B is currently working on easy or hard anagrams.
- The total amount of skipped hard anagrams (including the number of switches to easy anagrams) by Person B in part 2.

**Person B does not receive any information about Person A at any time.**

On the next screen you will be informed whether you are Person A or Person B.

Importantly, your payoffs only depend on your own actions. There are no joint payoffs.

Click "I understand the instructions" when you have read the instructions and have no further questions. If anything is unclear, please raise your hand.

I understand the instructions

Figure 10: Instructions for part 2: Random Matching treatment

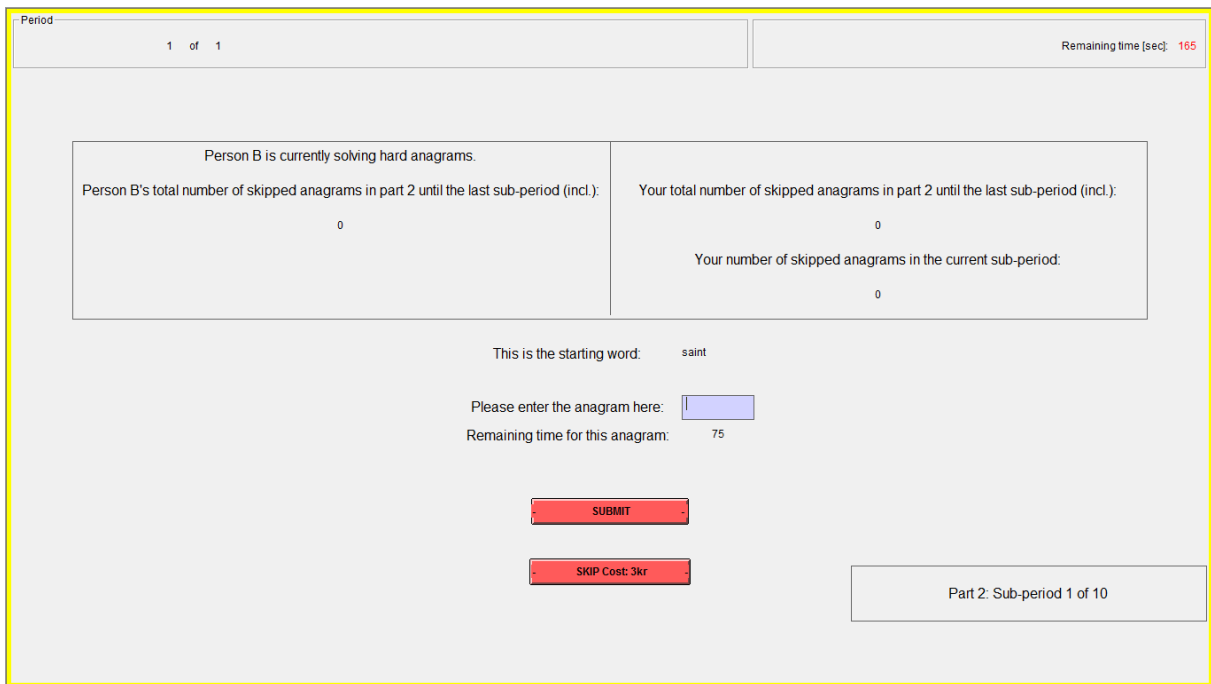


Figure 11: Observer's screen in part 2 (when working on hard anagrams)

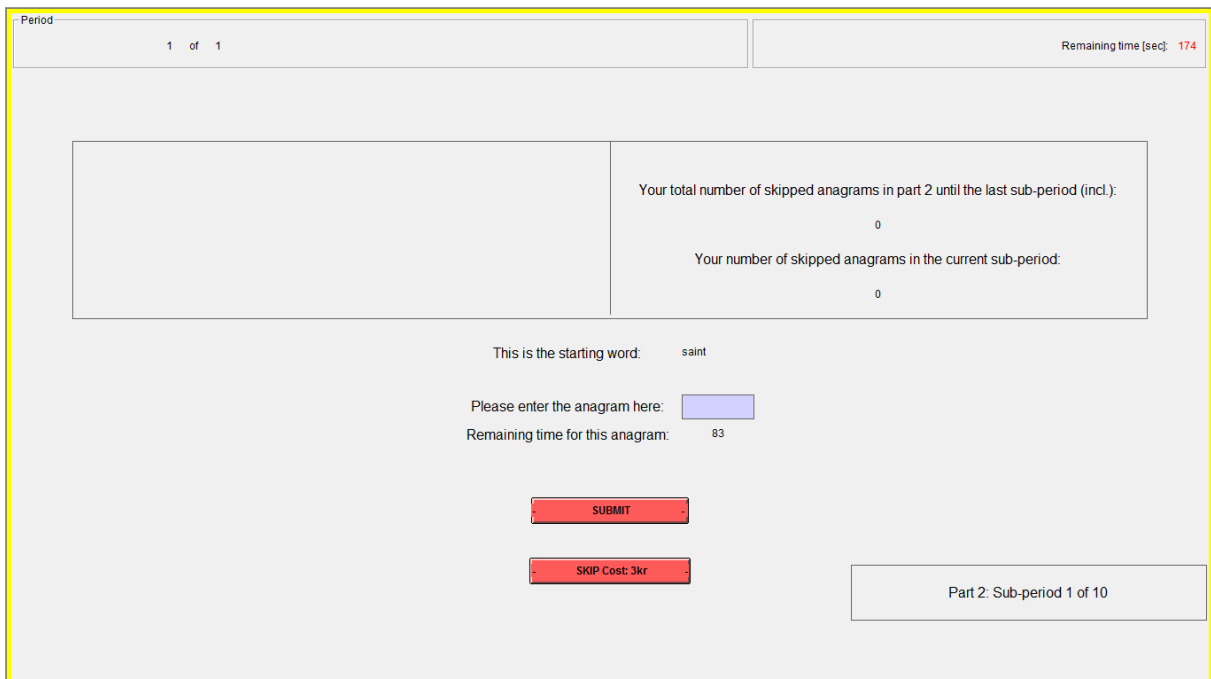


Figure 12: Peer's screen in part 2 (when working on hard anagrams)



Period

1 of 1

Remaining time (sec): 28

Please get ready for part 2.

The task in part 2 is identical to the task in part 1.

In part 2 you will be matched with another participant.

Both of you will work on the same task as in part 1 and the order of anagrams to solve is the same for both of you.

One of you assumes the role of "Person A", the other one is "Person B". Independent of the role, you will receive information about your own progress on the task, similar as in part 1.

**Person A will additionally receive information about the progress of Person B.**

On the top of his/her screen Person A will see:

- If Person B is currently working on easy or hard anagrams.
- The total amount of skipped hard anagrams (including the number of switches to easy anagrams) by Person B in part 2.

**Person B does not receive any information about Person A at any time.**

**On the next screen you can state your preference about which role - Person A or Person B - you would like to take.**

If you choose to take the role of Person A, you are also asked to choose from which quintile you want to observe a Person B in part 2.

**Note:** To make sure that a Person A can observe a Person B from any quintile that he/she wants to observe, one participant from each quintile has to take the role of Person A.

That means, if you and the other participant from your quintile want to take different roles, these preferences will be implemented.

If, on the other hand, both of you want to take the same role, the roles will be randomly allocated by the computer.

To increase the chance to get your desired role, you have to state your true preferences, as they will be implemented with a fair degree of certainty.

**Some final remarks:**

More than one Person A can observe a Person B, that means that even if all Person As want to observe the same Person B, this is possible.

If you take the role of Person B, you will not be informed whether someone chose to observe you.

Importantly, your payoffs only depend on your own actions. There are no joint payoffs.

Click "I understand the instructions" when you have read the instructions and have no further questions. If anything is unclear, please raise your hand.

I understand the instructions

Figure 13: Instructions for part 2: Chosen Matching treatment

Period 1 of 1 Remaining time [sec]: 27

**State your choice**

You can now state your preference about which role you want to take. Either Person A, this means you will be able to observe a Person B or Person B, this means you could be observed by Person As.

My preference:  Person A  
 Person B

**SUBMIT**

Figure 14: Preferred role

Period 1 of 1 Remaining time [sec]: 22

**State your choice**

Remember that you were in quintile 3 (persons from the first quintile solved the most hard anagrams correctly, persons from the fifth quintile solved the least hard anagrams correctly).

Please indicate from which quintile you want to observe a Person B, if you will actually take the role of Person A in the following part.

First Quintile  
 Second Quintile  
 Third Quintile  
 Forth Quintile  
 Fifth Quintile

**SUBMIT**

Figure 15: Preferred peer