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Abstract

Recent research has shown that the presence of peers can increase individual output both in the lab and the field. This paper tests for negative side effects of peer settings. We investigate whether peer settings are particularly prone to cheating even if they do not provide additional monetary benefits of cheating. Participants in our real effort experiment had the opportunity to cheat when declaring their output levels. Although cheating did not have different monetary consequences when working alone than when working in the presence of a peer, we find that cheating is a more severe problem in peer settings.

Keywords: cheating, peer effects, organizational design, personnel economics, experimental economics

JEL: J20, J30, M50

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1 Introduction

Recent research indicates the existence of positive peer effects on work effort and individual output levels (e.g., Falk and Ichino 2006, Mas and Moretti 2009, Bäker and Mechtel 2013, Beugnot et al. 2013). These studies show that individual output is, on average, higher when working in the presence of one or more peer(s) as opposed to working alone.[1] We will refer to this as a *positive peer effect* throughout the paper. Interestingly, positive peer effects can be found even when there are no peer-specific monetary rewards or task interdependencies between workers (Falk and Ichino 2006, Bäker and Mechtel 2013).[2]

Given the existence of positive peer effects on output, from a management perspective it appears to be promising to rely on peer settings (e.g. open-plan offices or requiring workers to work on site instead of at home) when designing organizations. However, in almost all situations principals cannot (fully) observe agents' effort/output levels and output quality. The resulting moral hazard problem opens the floor for agents to behave in a way not desirable from the principal's point of view. If peer settings create some pressure to perform, they could not only increase productive effort but also moral hazard in terms of cheating.[3] In this case, implementing such organizational structures would superficially increase output to the cost of negative side effects such as worse product quality. Such a result would then question the overall benefits of peer settings.

With this study we aim to shed more light on peer effects, opening the floor for cheating behavior in a real effort experiment. Following Nargin et al. (2002), we understand cheating as a form of shirking, i.e. reducing effort and not *behaving* according to the rules.[4] In our experiment, conditions differ with respect to whether cheating is possible or not.[5] To

[1]Within this paper, we focus on peer effects in terms of differences in average output levels between single and peer settings. Hence, we are not primarily interested in productivity spillovers between low and high productive workers, but focus on the question whether it, on average, pays for principals to rely on peer settings instead of letting agents work alone.

[2]See Chan et al. (2014) for compensation systems having an impact on peer effects.

[3]While empirical evidence on the reasons for the existence of positive peer effects is scarce, theoretical explanations refer to Social Comparison Theory (Festinger 1954) or peer pressure induced by peer settings, which could then induce cheating. With regards to cheating, several ways seem possible: (1) manipulating oneself (i.e. doping, see e.g. Preston and Szymanski 2003, Kräkel 2007, Schermer 2008), (2) manipulating others (i.e. sabotage, see e.g. Preston and Szymanski 2003, Dato and Nieken 2014), (3) manipulating the evaluator or principal (i.e. influence activities, see e.g. Milgrom and Roberts 1988, Kräkel 2007) or (4) manipulating one's output. Our study contributes to this last category of cheating. The agent manipulates the quality of output by reducing his or her (unobservable) effort. An example could be a researcher not proofreading an article though asked to do so by his or her co-authors, a journalist not checking the accuracy of facts, a sales person not presenting all the selling points of a product or a teacher not correcting pupils' exams for spelling mistakes.

[4]Lying, in turn, would refer to not telling the truth (see e.g. Croson et al. 2003).

[5]As in Ariely et al. (2008) and Pascual-Ezama et al. (2013), participants' real effort task is to solve riddles. Please see Section 3.1.1 for a detailed description of the task.

analyze how peer settings influence cheating, we run these conditions as peer and individual sessions (as done by Falk and Ichino 2006). In the former, two participants work in the same room on their own tasks, while each participant works alone in a separate room in the latter condition. Comparing output and cheating levels between session types allows us to identify whether peer settings are more prone to cheating behavior, i.e. whether higher average output levels in peer settings are caused by cheating.

As previous studies have shown that the degree of undesirable behavior such as cheating also depends on monetary incentives (see, for example, Harbring and Irlenbusch 2011), we implement two different payoff schemes to test for the sensitivity of our results. In the first, subjects receive a piece rate. In the second, participants receive a fixed wage and have to be present for a given period of time (similar to the setup chosen by Falk and Ichino 2006). This research design allows us to identify whether potential cheating effects depend on the payoff scheme.

Our results show that peer settings are indeed more prone to cheating and that the higher average output levels in peer settings are in large parts driven by cheating. For conditions where cheating is not possible, we find that the presence of a peer increases individual output only moderately (6% in the piece rate scenario, 7% under the fixed wage). The difference in absolute output levels between single and peer sessions when cheating is not possible is not statistically significant.[6] However, for conditions in which cheating is possible, we find that the number of completed tasks is significantly higher in peer sessions (11% under the piece rate, 13% in the fixed wage scenario).

Given our experimental design, we can clearly identify that the increase in output is to a large extent driven by cheating behavior. Working in the presence of a peer appears to cause participants to feel a certain pressure to perform. However, under a piece rate compensation scheme it does not increase productive effort but cheating. Even under a fixed wage, the difference in average output levels between individual and peer sessions is considerably driven by cheating.

This finding casts some doubt on the desirability of implementing peer settings – at least when other motivational instruments can be applied and cheating is possible. Whereas previous studies have shown that monetary incentives potentially induce undesirable behavior

[6]At first glance, this result seems to contradict previous studies. However, as in the piece rate scenario the average reservation wage in both conditions is almost zero, the participants of our experiment were on average highly motivated (and therefore highly productive) workers. In the fixed wage scenario, average output levels are around 15% smaller, but the subjects were obviously far from being unproductive. Falk and Ichino (2006), Mas and Moretti (2009), and Bäker and Mechtel (2013) show that the overall positive effects of peer settings are mostly driven by low productive workers who increase their output levels in the presence of peers. Given that participants in our experiment are rather productive, the absence of strong positive peer effects is not surprising and fits the existing evidence.

(e.g., Cadsby et al. 2010, Conrads et al. 2013, Danilov et al. 2013, Gilpatric 2011, Harbring and Irlenbusch 2011), our results suggest that peer settings can trigger this kind of behavior even in the absence of explicit incentives. Our study therefore contributes to the literature on peer settings in that it points to the existence of negative side effects that have not yet been identified before. For principals, this means that (1) the apparently positive effects of peer settings could be considerably driven by cheating and (2) this might yield unrecognized costs in the form of a lower quality which might only be detected in the long run.

The remainder of the paper is organized as follows. In Section 2, we describe the related literature. Section 3 presents the experimental design and sets up a small theoretical model to derive our hypotheses. Results are described and discussed in Section 4, before Section 5 concludes.

2 Related Literature

Peer effects have mainly been studied in the context of the educational system with the aim of e.g. giving policy implications as to whether to apply ability tracking in school (e.g. Hanushek et al. 2003, Kim et al. 2008, Lavy et al. 2012). However, recently some studies have dealt with peer effects in the work place (Mas and Moretti 2009) or analyzed peer effects in (field) experiments (Falk and Ichino 2006, Bäker and Mechtel 2013). These recent studies point to the existence of positive peer effects on individual output for work place settings. In addition to analyzing whether peer effects exist and whether they are indeed positive, some studies try to shed more light on specific factors that foster or hinder the appearance of peer effects such as gender or race of peers.

An important aspect to be considered when judging the results of studies on peer effects is the interdependence in tasks or compensation between peers. For example, the study by Mas and Moretti (2009) analyzed supermarket cashiers. While they were paid independently in the form of an hourly wage, their tasks were not independent, because work (checking of goods) that was done by one cashier did not have to be done by another, i.e. one hard working cashier could reduce the workload of her peers. Consequently, peer effects in this setting might in part be due to a desire to help one's coworkers. Similarly, peer effects found based on field data from study groups or school classes might be driven by interdependence of tasks (studying together might facilitate learning) if not compensation.

One way to isolate effects arising from peers' monetary or task interdependence from "pure" peer effects is conducting experiments which exclude both types of interdependence. Falk and Ichino (2006) conducted a field experiment where subjects had to prepare letters for mailing either with another subject working on the same task in the same room or not.

Participants earned a fixed hourly wage and worked on the task for four hours. Consequently, there was neither task nor monetary interdependence and still Falk and Ichino find that participants in the peer settings had a significantly higher output on average, i.e. completed more letters per hour.

What drives this effect? Social Comparison Theory (Festinger 1954) tells us that individuals base their opinion of themselves on – among other things – comparisons with other individuals of their reference group. In peer settings, the peers are likely to form the reference group and it is, thus, important to measure up or outperform them to maintain a positive self-perception. That is social processes might indirectly and potentially unintentionally induce a competitive mind frame in peer settings. The management toolbox contains instruments to actively foster such a competitive setting, for example tournament compensation under which employees compete for a bonus (see e.g. Lazear and Rosen 1981). The multitude of studies on worker behavior under tournament compensation schemes tells us that it is a powerful motivational instrument, but that it also induces unwanted behavior, such as rat races or unproductive behavior in the form of sabotage or cheating (e.g., Cadsby et al. 2010, Gilpatric 2011, Harbring and Irlenbusch 2011).[7] If peer settings induce a similar competitive mind frame (even in the absence of monetary consequences) then it appears plausible that they also induce adverse behavior.

While there is a vast body of literature looking at determinants and consequences of cheating or fraud, evidence on cheating in social settings or personal interactions is rather scarce. The existing studies contain elements of monetary and/or task interaction, thereby impeding the measurement of pure peer effects in cheating. They report that under team compensation individuals cheat more than under individual compensation, because under the latter they cannot "help" their colleagues by cheating (Conrads et al. 2013, Danilov et al. 2013, see Briggs et al. 2013 for a theoretical analysis, see Erat and Gneezy 2012 for higher likelihood of lying when it only helps others).[8] In tournament settings where cheating only helps oneself, the experimental findings by Schwieren and Weichselbaumer (2010) show positive effects of competition on cheating, which seems to be driven by the rather unproductive who do not want to be seen as the low performers or want to increase their chances of winning.[9]

Using field data on vehicle emission tests, Pierce and Snyder (2008) find that the degree

[7]Using data on cheating behavior of teachers in schools, Jacob and Levitt (2003) show that an increase in performance incentives leads to increased cheating behavior even without a competitive or team setting.

[8]An exception to these findings is Waller and Bishop (1990) who find higher cheating under a piece rate compensation scheme than under an implicit team compensation. This might be due to the fact that the compensation was not communicated as a team compensation and cooperation could not be guaranteed.

[9]Kandel and Lazear (1992) argue that in teams, peer pressure induces the low productive team members to increase their effort. However this effort is generally assumed to be productive.

of (un)ethical behavior of inspectors working for different organizations is influenced by the norms of the employing organizations, i.e. organizational norms and managers influence fraud behavior. This finding is in line with the experimental results by Jones and Kavanagh (1996) who find an influence of managers' (un)ethical behavior on employees' (un)ethical behavior. However, they also find evidence for peers' (un)ethical behavior on employees' (un)ethical behavior. This ties in with the results of a number of studies which show that own cheating behavior positively depends on perceptions of others' cheating behavior, i.e. the acceptability of cheating (see Ichino and Maggi 2000, Carrell et al. 2008, Megehee and Spake 2008, and O'Fallon and Butterfield 2008), and negatively depends on the penalty for cheating (see Megehee and Spake 2008 and O'Fallon and Butterfield 2008) that might be imposed by e.g. the manager or the organization in general. With respect to the effect of peers, some articles argue that they serve as a reference point and thereby influence behavior (e.g. Trevino 1986). However, Gould and Kaplan (2011) make a case for peers *learning* to cheat from their (high performing) peers.

To formally derive our hypotheses, in section 3.2 we will present a stylized theoretical model based on insights from the literature presented above.

3 Experimental Setup and Theoretical Predictions

3.1 Experimental Setup

3.1.1 Experimental Design

Following Falk and Ichino (2006), we implement a real effort task in both an individual setting as well as a peer setting. The only difference between the two settings is that in the peer setting participants work in the presence of another participant working on the same task. They can see each other and are allowed to communicate, but work individually on their own task. We did not conduct a field experiment but chose to implement a laboratory setting instead, allowing us to vary the possible degree of cheating which would have been very complicated in a field setting (and may be perceived as artificial by the subjects) for a given level of experimental control.

To generate conditions that differ in the potential for cheating, we rely on the real effort task used by Ariely et al. (2008) and Pascual-Ezama et al. (2013). Participants have to solve riddles: They receive a sheet of paper with a sequence of 850 randomly drawn letters. Within this sequence, there are ten instances of two consecutive letters "p". The participants' task is to find these ten. We create our two conditions by varying whether cheating is possible.[10]

[10] Ariely et al. (2008) have a third condition, in which cheating is possible but cannot be observed ex-

In the "no cheating" condition (NC), each solved riddle is checked for correctness before the participant may begin with working on the next riddle. Thus, cheating is not possible. In the "cheating" condition (C), solved riddles are not checked for correctness but simply collected on a pile. Thus, in the aftermath of the experiment, it was possible to check how often participants cheated, i.e. whether they declared to have successfully solved a riddle while in fact they did not solve it correctly.[11]

Table 1 shows our 2x2 design. Applying a between-subjects design, participants are randomly assigned to one of the four session types. For the sake of brevity, we use the following notation for the different session types throughout the remainder of the paper: S_j, where $S = NC, C$ refers to no cheating and cheating sessions, respectively, and $j = i, p$ refers to individual and peer sessions, respectively.

	Individual sessions	Peer sessions
No cheating possible (NC)	1 (NC_i)	3 (NC_p)
Cheating possible & observable (ex-post) (C)	2 (C_i)	4 (C_p)

Table 1: Experimental design: four session types.

3.1.2 Compensation Schemes

As stated in the introduction, we run these four conditions for two different compensation schemes: a piece rate and a fixed wage. With respect to the first, we use Ariely et al.'s (2008) and Pascual-Ezama et al.'s (2013) setup of a linearly decreasing piece rate as it offers an interesting setting for observing the strength of the motivational effect induced by peer settings. Any peer effect that can be observed in addition to the strong motivational instrument of a piece rate speaks for peer settings being a very strong motivator. Implementing a declining piece rate allows us to measure task performance in how many tasks they decided to work on, or phrased differently: when they quit working. Thus we can compare reservation wages between the four experimental conditions. In accordance with Ariely et al. (2008), participants earn 55 cents for the first riddle, 50 cents for the second riddle, and so on. That is, the piece rate declines by 5 cents per completed riddle. The eleventh riddle is the last one to pay any monetary amount different from zero.

post. We exclude this condition from our analysis because it likely differs in more than one aspect from our condition without the possibility to cheat.

[11] Experimenters always waited some seconds until they handed the next riddle sheet to the subjects in the cheating condition. We thus guarantee that the short delay caused by checking the riddles for correctness in the no cheating condition does not influence our outcome measures.

In comparison, a fixed wage c.p. provides less motivation to generate output, thereby potentially strengthening the motivational effects caused by peer settings because the initial level of output is expected to be lower under that compensation scheme. We implement the fixed wage in accordance with the setup by Falk and Ichino (2006). Participants have to work for 25 minutes on the task (a time window that was chosen to be most similar to the average time worked under the piece rate) and receive a fixed wage amounting to the average wage obtained under the piece rate compensation scheme (5.80 Euro).

3.1.3 Experimental Procedure

The procedure of the experiment was as follows (see Appendix for detailed instructions):

(1) Written instructions on the task, the procedure and the compensation are presented on paper and read out loud by the experimenter. Any questions are answered publicly (in peer sessions). The instructions contain an example riddle. For the piece rate a table depicting the amount of money earned per completed riddle as well as the cumulated amount of money for any number of completed riddles is given, for the fixed wage the table shows that total compensation is the sum of the show-up fee (2.50 Euro) and a fixed wage (3.30 Euro). At several points within the instructions, participants are informed that they can stop solving riddles at any point in time. Under the piece rate scheme, participants are free to leave the room upon deciding to stop solving riddles (see Ariely et al. 2008).

(2) The experimenter hands over the first riddle, and in case of the piece rate announces the piece rate for the first riddle (55 cents). Participants start working on it.

(3) Once a riddle is completed, the experimenter acts according to the experimental condition (checking the riddle or simply collecting it) and asks whether the participant wants to work on another riddle (in case of the piece rate the applicable piece rate is stated).

(4) In the piece rate scenario, once a participant declines working on another riddle, the experiment ends and (s)he fills out a short questionnaire. In the fixed wage scenario, a participant in total has to be present for 25 minutes even if (s)he does not work on additional riddles. After this working period of 25 minutes is over, the experiment ends and (s)he fills out the questionnaire.

(5) The participant is paid privately according to the number of solved riddles (piece rate) or the fixed wage of 3.30 Euro (fixed wage) plus in any case a show-up fee of 2.50 Euro.

3.2 Theoretical Predictions

To illustrate potential effects of the presence of a peer on individual output, we summarize the previously discussed results from the literature in a simple principal-agent model. Our research question and experimental design focus on the role of the agents. Deviating from the experimental setup, in which all participants are agents and no principal is present, in our theoretical model we include a (passive) principal. Our theoretical predictions focus on the agents' behavior and are therefore still valid if we use a framing that ignores the principal and only assumes that agents have some reason to produce a positive output level in the fixed wage scenario. Given this flexibility in the theoretical considerations, in the experiment we explicitly decided against implementing a classical principal-agent setting as this might have caused difficulties in the analysis. As we know from the literature, principal-agent settings are often accompanied by strategic considerations of both sides, social preferences, aspects of gift exchange and reciprocity, etc. Adding a principal-agent setting to the experiment might have made it difficult to cleanly identify basic behavioral effects caused by the presence of peers, as they could as well interact with other factors that are typical for principal-agent settings. Our experimental design therefore allows a very clean test of the hypotheses derived from the model.

For our theoretical analysis, we assume an agent's utility to depend on three components. First, utility depends positively on the agent's wage w which might depend on her output (piece rate scenario) or not (fixed wage scenario). Second, producing output is costly to the agent who does not want to exert effort. Utility costs of producing output are given by the function $c(o_k, \overline{o})$, where o_k denotes agent k's output level and $\overline{o}$ captures the average output level of her peers. Suppose $\frac{\partial c}{\partial o_k} > 0$, $\frac{\partial^2 c}{\partial o_k^2} > 0$, and $\frac{\partial^2 c}{\partial o_k \partial \overline{o}} < 0$ to hold. The latter follows the theoretical approach of Falk and Ichino (2006) and reflects their idea that the disutility of producing output is lower when average production is higher. As Falk and Ichino, we are not interested in identifying the basic mechanisms behind peer effects here, but just aim to derive predictions about what we should see in the experimental results if positive peer effects are at work.

In our experiment, subjects have the possibility to cheat in some conditions. Cheating means that they tell the experimenter that they have correctly solved their task, while in fact they did not do so (which cannot be observed by the experimenter during the session). The possibility to cheat reduces agents' marginal costs of producing output. We thus impose a weighting factor δ on effort costs c which takes the value of 1 whenever cheating is not possible. If cheating is possible, $0 < \delta < 1$ holds, indicating that disutility of producing one marginal unit of output is smaller than in the case where cheating is not possible.[12]

[12] Note that we do not explicitly model the agent's actual decision for/against cheating. $\delta < 1$ simply means that agents cheat to *some* extent. We do not focus on the question of whether δ might be endogenous in the way that it might be smaller in peer sessions than in individual sessions due to a higher perceived social pressure to produce output in peer sessions (see, e.g., Falk and Ichino 2006; Mas and Moretti 2009), yielding a higher amount of cheating. We discuss this point in more detail after presenting our hypotheses.

For the piece rate scenario, these two components of an agent's utility are sufficient to derive hypotheses about differences in output levels depending on whether cheating is possible or not and whether an agent works in the presence of a peer or not. However, in the fixed wage scenario, agents would not have a monetary incentive to work at all as their payment does not depend on the output level. We, therefore, add a third component to the agent's utility function: $a(o_k)$ with $a' > 0$. This function might reflect a number of things that could cause a positive effect of produced output on the agent's utility level.[13] We do in no respect focus on what causes $a(o_k)$, but just need it as a technical matter in order to guarantee positive output levels in the fixed wage scenario to be able to derive hypotheses on the effects of (1) peer settings and (2) introducing the possibility to cheat on agents' output levels.

Agent k's utility function has the following appearance under a piece rate compensation scheme:

$$U_k = w(o_k) - \delta c(o_k, \overline{o}) + a(o_k), \tag{1}$$

with $w' > 0$. In our experiment, $w'' < 0$ holds. However, all of our hypotheses that we are about to derive in the following remain if we assume $w'' = 0$. Under the fixed wage compensation scheme, the first term in equation 1 does not depend on o_k and thus $w' = 0$ holds.

We will base our analysis on the piece rate compensation scheme. As will be shown later, all hypotheses derived from the model also hold for the fixed wage. The agent chooses the output level o_k^* that maximizes her utility. The first order condition for an interior solution is

$$\frac{\partial U_k}{\partial o_k} = w'(o_k) - \delta c'(o_k, \overline{o}) + a'(o_k) \stackrel{!}{=} 0.$$

Given our assumptions about the functional forms ($w'' < 0$, $c'' > 0$, $a'' \leq 0$), the second order condition is obviously fulfilled.

Within the setup of our experiment, four cases are possible: subjects work (i) alone without the possibility to cheat ($o_{i,NC}^*$), (ii) alone with the possibility to cheat ($o_{i,C}^*$), (iii) in the presence of a peer without the possibility to cheat ($o_{p,NC}^*$), or (iv) in the presence of a peer with the possibility to cheat ($o_{p,C}^*$). The first order conditions for these four possible

[13] For instance, an agent might reciprocate in the way of a gift exchange: the principal pays her a fixed wage and she thus experiences an increase in utility whenever she produces more output (which would increase the principal's profits). Alternatively, $a(o_k)$ might represent altruism against the principal whose profits increase in the output level. $a(o_k)$ might as well be interpreted as compliance to a social norm saying that producing a high level of visible output is an adequate reaction to receiving a fixed wage. Given the above reasoning, it appears not to be plausible to assume $a'' > 0$. We therefore assume $a'' \leq 0$. Our hypotheses do not depend on whether $a'' < 0$ or $a'' = 0$ holds.

cases read:

$$w'(o_k) - c'(o_k, 0) + a'(o_k) = 0, \tag{2}$$
$$w'(o_k) - \delta c'(o_k, 0) + a'(o_k) = 0, \tag{3}$$
$$w'(o_k) - c'(o_k, \overline{o}) + a'(o_k) = 0, \tag{4}$$
$$w'(o_k) - \delta c'(o_k, \overline{o}) + a'(o_k) = 0. \tag{5}$$

Based on these FOCs, we can derive our hypotheses. Comparing equations 2 and 3, it turns out that equation 3 cannot be fulfilled for $o^*_{i,NC} = o^*_{i,C}$ as $\delta < 1$ holds in equation 3. Hence, optimal effort must be higher when cheating is possible ($o^*_{i,C}$) than when cheating is not possible ($o^*_{i,NC}$), as this increases c', decreases w', and decreases a' (if $a'' < 0$ holds). Comparing equations 4 and 5, it turns out that equation 5 cannot be fulfilled for $o^*_{p,NC} = o^*_{p,C}$ as $\delta c'(o_k, \overline{o}) < c'(o_k, \overline{o})$. We thus end up with $o^*_{p,NC} < o^*_{p,C}$. The possibility to cheat reduces the marginal costs of producing output, yielding higher output levels in both individual and peer sessions.[14] Thus, our first hypothesis is:

Hypothesis 1 *Individual output is higher when cheating is possible than when cheating is not possible (holding the peer/individual dimension constant).*

Comparing equations 2 and 4, our model yields the same behavioral prediction as derived by Falk and Ichino (2006). Equation 4 cannot be fulfilled for $o^*_{i,NC} = o^*_{p,NC}$ and $\overline{o} > 0$ as $c'(o_k, \overline{o}) < c'(o_k, 0)$ due to $\frac{\partial^2 c}{\partial o_k \partial \overline{o}} < 0$. Hence, $o^*_{p,NC}$ must be higher than $o^*_{i,NC}$ as this increases c', decreases w', and decreases a' (if $a'' < 0$ holds). Similarly, comparing equations 3 and 5 reveals that equation 5 cannot be fulfilled for $o^*_{i,C} = o^*_{p,C}$ due to $\delta c'(o_k, 0) > \delta c'(o_k, \overline{o})$ for $\overline{o} > 0$. Thus, $o^*_{i,C} < o^*_{p,C}$ holds. The presence of a peer reduces the marginal costs of producing output (positive peer effect), yielding higher output levels in both cheating and no cheating conditions. Our second hypothesis reads:

Hypothesis 2 *Individual output is higher in peer sessions than in individual sessions (holding the cheating/no cheating dimension constant).*

Hypotheses 1 and 2 have highlighted the basic effects of the possibility to cheat and the peer presence. Our hypothesis of main interest compares the impact of the possibility to cheat between individual and peer conditions. Comparing equations 2 and 3 with 4 and 5, it is obvious that $(o^*_{i,C} - o^*_{i,NC}) < (o^*_{p,C} - o^*_{p,NC})$ results. On the one hand, the possibility

[14] Imposing symmetry between the peers yields $\overline{o}_{p,NC} < \overline{o}_{p,C}$ which in addition strengthens the result that output is higher in the peer condition when cheating is possible than in the peer condition when cheating is not possible.

to cheat increases optimal output in both the individual and peer condition due to $\delta < 1$. On the other hand, due to $\overline{o}_{p,NC} < \overline{o}_{p,C}$ (see footnote 14), there is an additional increase of optimal individual output in the peer condition. Combining the above reasoning, we derive the third hypothesis:

Hypothesis 3 *Individual output is highest in the peer sessions where cheating is possible.*

Hypotheses 1 to 3 also apply to the fixed wage compensation scheme. In that case, $w' = 0$ holds. However, all further theoretical reasoning presented above is still valid as can easily be seen from the FOCs.

So far, our hypotheses focus on individual output levels in the different conditions. As described earlier, we do not focus on the individual *decision* to cheat, but assume that there is *some* level of actual cheating behavior whenever cheating is possible (reflected by $\delta < 1$). However, it might obviously be possible that the perceived social pressure in peer settings induces a higher level of *actual* cheating. If that was the case, δ (reflecting the actual degree of cheating) would be smaller in peer sessions than in individual sessions. Imposing $\delta_p < \delta_i$ in our theoretical model would exactly generate this result. In that case, all hypotheses regarding individual output levels presented above would remain as they are. However, we abstained from assuming $\delta_p < \delta_i$ in our theoretical analysis as the resulting hypothesis "More cheating in peer sessions than in individual sessions" would obviously directly depend on this assumption. Nonetheless, we will use our experimental data to see whether the actual amount of cheating is indeed higher in peer sessions than in individual sessions.

4 Results

4.1 Descriptive Statistics

Subjects in the piece rate setting were students at the University of Tübingen, subjects in the fixed wage scenario were students at the University of Trier. In total, 167 students participated in the piece rate experiment, and 151 students in the fixed wage experiment. Subjects were recruited from the student body of the University of Tübingen via an email newsletter and from the pool of experimental subjects of the Trier Laboratory for Experimental Economics. The allocation of subjects to conditions was random. In order to minimize the probability that the two subjects in a peer session knew each other beforehand, participants in the same session never had the same subject of study.

Table 2 displays the number of observations for the four different session types per compensation scheme. For individual sessions, the number of observations equals the number of sessions. For the peer sessions, the number of sessions equals half the number of observations.

	Individual sessions	Peer sessions
Piece Rate		
No cheating possible (NC)	36	70
Cheating possible & observable (ex-post) (C)	25	36
Fixed Wage		
No cheating possible (NC)	33	50
Cheating possible & observable (ex-post) (C)	24	44

Table 2: Number of observations per session type.

Figure 1 shows the distribution of the number of completed riddles per session type for the piece rate experiment. It is evident that the majority of participants completed exactly 11 riddles, i.e. worked as long as it paid a positive piece rate. The average reservation wage for the no cheating conditions is 10 cents in individual sessions and 7 cents in peer sessions. For the cheating conditions, the average reservation wage is 7 cents and 5 cents for individual and peer sessions, respectively. However, as Figure 1 shows, there is also substantial variation in the number of completed riddles, ranging from the theoretical minimum of 1 to 20, the maximum number of riddles provided. Looking at the distributions, they vaguely resemble a normal distribution, but results from t-tests (see below) should be interpreted with caution. We therefore additionally run Wilcoxon rank sum tests.

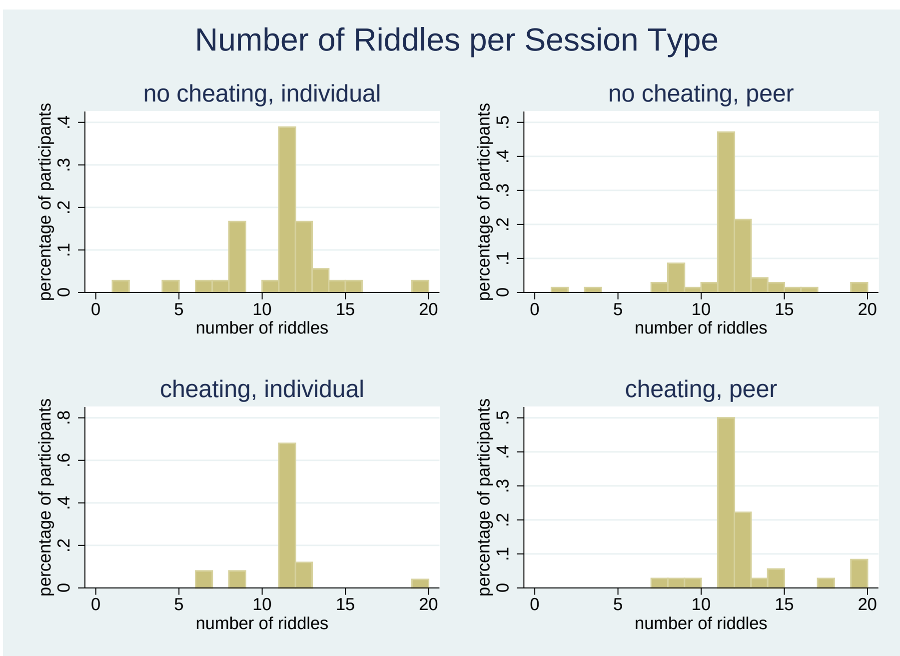

Figure 1: Piece rate: Histograms of output per session type.

Figure 2 shows the distribution of the number of completed riddles per session type for the fixed wage experiment. Other than under the piece rate, there is much more dispersion in the number of completed riddles under the fixed wage.

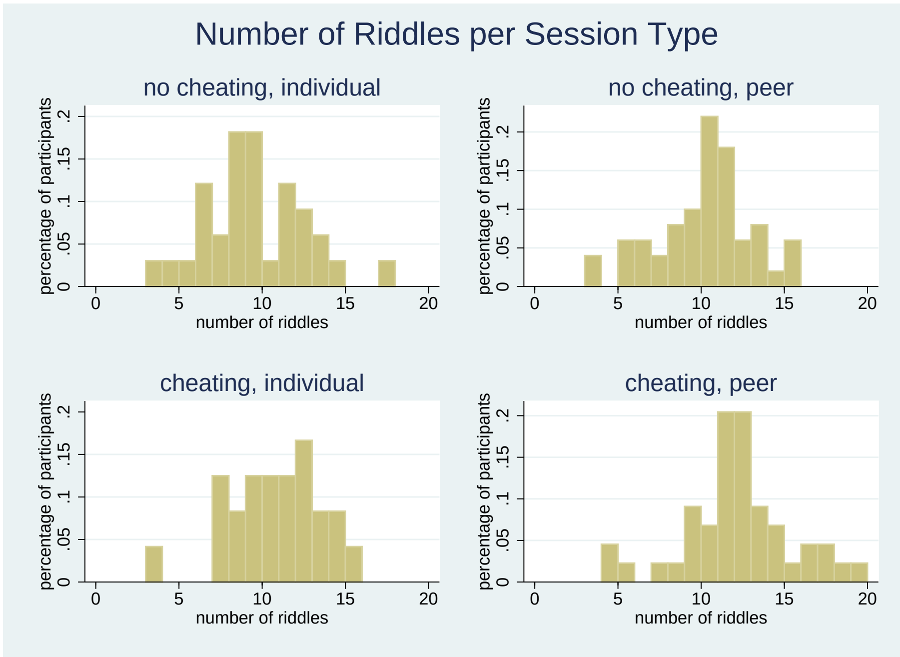

Figure 2: Fixed wage: Histograms of output per session type.

In the post-experimental questionnaire we collected data on personal characteristics, specifically gender, year of birth and personality as measured via the NEO-FFI by Costa and McCrae (1989). In addition we recorded the time (in minutes) that participants needed to complete the first riddle as an inverse measure for ability. Table 3 displays the mean values of these variables separately for our four experimental conditions and the two compensation schemes. This allows us to assess whether participants differed systematically between conditions and compensations schemes, which might then influence our results. The last column in Table 3 contains the results from a Kruskal-Wallis test on equality of population of different samples. It states whether the participants in the four conditions (per compensation scheme) differ systematically in the respective characteristic. Overall we can see that this is only the case for the year of birth and conscientiousness in the fixed wage scheme. We also compare the two compensation schemes per experimental condition using t-tests and Wilcoxon ranksum tests. The results are given in the rows under the mean values of the respective characteristic. Again we hardly find significant differences between compensation schemes. The only exceptions are gender in the no cheating peer condition, where the share of male participants is higher under the fixed wage. We also find differences for conscientiousness in the two cheating conditions. In the individual cheating condition, participants in the fixed wage scheme were less conscientious than in the piece rate scheme, in the peer cheating condition the opposite holds true. Overall, the reported differences in the observable characteristics between conditions are small in economic terms. For the following results, these very few differences should thus have a limited impact. However, we will in addition to the (non-)parametric tests also estimate regression models in order to explicitly account for all control variables at hand.

4.2 (Non-)Parametric Tests and Regression Results

To test whether peer effects exist, we first compare individual sessions and peer sessions (holding the cheating/no cheating dimension constant) with respect to the average number of completed riddles. Table 4 shows the average number of completed riddles for the piece rate compensation scheme, and Table 5 for the fixed wage compensation scheme. The last two columns in both tables show results of two-sided t-tests and Wilcoxon rank sum tests, checking for significant differences between individual and peer sessions. The last two rows in both tables show results for differences between cheating and no cheating conditions.

The results from the fixed wage experiment support Hypothesis 1 as they show that average output is higher when cheating is possible (both in individual and peer sessions). However, for the piece rate a mixed picture emerges with respect to Hypothesis 1: average output levels are (not) higher when cheating is possible in peer (individual) sessions.

With respect to Hypothesis 2, looking at the 'no cheating' conditions (first row in Tables 4 and 5), unexpectedly we find no evidence for statistically significant peer effects – independently of the compensation scheme. However, for the cheating conditions we find significant positive peer effects: the number of completed riddles is significantly higher in peer than in individual sessions. For the piece rate compensation scheme this finding might be explained by the fact that the piece rate is already causing high motivation so that the peer pressure induced by a peer setting does not lead to an additional increase in productive effort (i.e. working more) when cheating is not possible. Yet, also for a setting with a lower level of subjects' motivation due to a lack of monetary incentives (i.e. the fixed wage), we find no significant peer effects when cheating is not possible.[15] While these results are not in line with Hypothesis 2 and the previous literature on peer effects, we find the expected peer effects when analyzing the sessions where cheating was possible. That is, average output is significantly higher in peer sessions than in individual sessions for the cheating condition.

Supporting Hypothesis 3, for both compensation schemes the highest average output level can be found for the peer condition where cheating is possible. Taken together, these findings suggest that independently of the compensation scheme, peer pressure induced by

[15] As can be seen from comparisons of Tables 4 and 5, average output levels are lower under the fixed wage – an effect that perfectly corresponds to the existence of a positive motivation effect caused by the piece rate.

	No cheating, individual	No cheating, peer	Cheating, individual	Cheating, peer	Kruskal-Wallis
Ability (inverse)					
piece rate	2.61	2.53	2.4	2.39	n.s.
fixed wage	2.61	2.66	2.75	2.59	n.s.
t-test	n.s.	n.s.	n.s.	n.s.	
Wilcoxon rank sum test	n.s.	n.s.	n.s.	n.s.	
Male					
piece rate	0.17	0.26	0.28	0.17	n.s.
fixed wage	0.27	0.40	0.25	0.25	n.s.
t-test	n.s.	*	n.s.	n.s.	
Wilcoxon rank sum test	n.s.	*	n.s.	n.s.	
Year of Birth					
piece rate	1989	1989	1990	1988	n.s.
fixed wage	1989	1989	1991	1988	**
t-test	n.s.	n.s.	n.s.	n.s.	
Wilcoxon rank sum test	n.s.	n.s.	n.s.	n.s.	
Conscientiousness					
piece rate	0.73	0.74	0.77	0.75	n.s.
fixed wage	0.73	0.70	0.65	0.82	***
t-test	n.s.	n.s.	**	**	
Wilcoxon rank sum test	n.s.	n.s.	**	**	
Agreeableness					
piece rate	0.70	0.74	0.71	0.70	n.s.
fixed wage	0.73	0.71	0.71	0.73	n.s.
t-test	n.s.	n.s.	n.s.	n.s.	
Wilcoxon rank sum test	n.s.	n.s.	n.s.	n.s.	
Openess					
piece rate	0.74	0.75	0.68	0.70	n.s.
fixed wage	0.72	0.74	0.68	0.72	n.s.
t-test	n.s.	n.s.	n.s.	n.s.	
Wilcoxon rank sum test	n.s.	n.s.	n.s.	n.s.	
Neuroticism					
piece rate	0.60	0.58	0.57	0.62	n.s.
fixed wage	0.64	0.60	0.59	0.61	n.s.
t-test	n.s.	n.s.	n.s.	n.s.	
Wilcoxon rank sum test	n.s.	n.s.	n.s.	n.s.	
Extraversion					
piece rate	0.73	0.71	0.72	0.68	n.s.
fixed wage	0.66	0.73	0.65	0.70	n.s.
t-test	n.s.	n.s.	n.s.	n.s.	
Wilcoxon rank sum test	n.s.	n.s.	n.s.	n.s.	

Notes: * significant at 10%; ** sign. at 5%; *** sign. at 1%.

Table 3: Means for Control Variables.

	# of riddles			
	Individual sessions	Peer sessions	t-test	Wilcoxon
No cheating (NC)	10.47	11.09	n.s.	n.s.
Cheating (C)	10.84	12.08	*	**
t-test	n.s.	*		
Wilcoxon rank sum test	n.s.	n.s.		

Notes: * significant at 10%; ** sign. at 5%; *** sign. at 1%.

Table 4: Piece rate: Average output per session type.

	# of riddles			
	Individual sessions	Peer sessions	t-test	Wilcoxon
No cheating (NC)	9.09	9.76	n.s.	n.s.
Cheating (C)	10.33	11.68	*	*
t-test	n.s.	***		
Wilcoxon rank sum test	*	***		

Notes: * significant at 10%; ** sign. at 5%; *** sign. at 1%.

Table 5: Fixed wage: Average output per session type.

a peer setting does not necessarily lead to an additional increase in productive effort (i.e. working more). However, peer settings lead to an increase in average output when cheating is possible. This result suggests that the observed increase in output in peer sessions is – at least in parts – driven by actual cheating.

Given the ex-post observability of cheating behavior, next we will check whether the observed increases in output when cheating is possible are indeed driven by cheating. While it is not possible to tell which of the participants cheated in the peer sessions, we can calculate the percentage of riddles that participants cheated on (similar as in Fischbacher and Föllmi-Heusi 2013, Gill et al. 2012) separately for individual sessions and peer sessions.

For the piece rate we find that the share of cheating (i.e. the number of incorrectly solved riddles that were handed in by the participants as being correctly solved) is 3.3% in individual sessions and 7.3% in peer sessions. The fact that cheating is more than twice as common in peer sessions supports our conjecture that higher average output levels in sessions where cheating is possible are caused by increased cheating and not increased productive effort. When we correct average output levels in the cheating sessions for the share of wrong solutions, it turns out that the observed positive peer effects on output levels are driven by cheating. Average output in individual sessions where cheating is possible equals 10.84 riddles (see Table 4). Correcting for the share of 3.3% wrong solutions, we end up with 10.48 corrected units of output – which perfectly corresponds to average output in the no cheating individual condition (10.47). The same holds for peer sessions. Correcting average output (12.08) when cheating is possible for the share of 7.3% wrong answers gives a number of 11.2 corrected units of output – once again very close to the value of 11.09 that we observe for peer sessions when cheating is not possible.

In line with our findings for the piece rate scheme, for the fixed wage we find that the share of cheating is higher in peer sessions (7.2%) than in individual sessions (4.4%). When we correct average output levels in the cheating sessions for the share of wrong solutions, it turns out that the positive peer effects on output levels are (as for the piece rate compensation scheme) driven by cheating. According to Table 5, average output in individual sessions where cheating is possible equals 10.33 riddles. Correcting for the share of 4.4% wrong solutions, the average number of correct units of output is 9.88. For peer sessions, correcting average output (11.68) when cheating is possible for the share of 7.2% wrong answers gives a number of 10.84 corrected units of output. Thus, the positive peer effect on average output when cheating is possible shrinks from 1.35 riddles (including incorrectly solved riddles) to 0.96 riddles. A considerably large part of what we would, based on the previous literature, call "positive peer effect" thus vanishes once we correct the results for actual cheating. However, in contrast to the piece rate scheme, there remains a positive effect on productive effort.

In addition to comparing the average number of completed riddles across individual and peer sessions, we also run Ordinary Least Squares (OLS) regressions with the number of completed riddles as dependent variable. Table 6 shows the corresponding results for different estimation models. Our main explanatory variables are dummy variables for the session types: C_i is a dummy for the individual cheating sessions, C_p is a dummy for the peer cheating sessions, and NC_p is a dummy capturing the peer no cheating sessions. The reference category is individual no cheating sessions (NC_i). We run the estimations of Model 1 separately for observations under the piece rate (column 1), the fixed wage (column 2) and pooled for both schemes (column 3), where in the latter we include a dummy for observations under the fixed wage (fixed wage=1). Model 2 further includes the number of minutes needed for completing the first riddle as an inverse measure of ability, gender, year of birth, and the Big Five personality measures of neuroticism, openness to experiences, agreeableness, conscientiousness and extraversion. They were measured using the short item version of the NEO-FFI by Costa and McCrae (1989). Again, the estimations are run separately and pooled for the compensation schemes (columns 4 to 6).

As can be seen from columns 1 and 4, under the piece rate compensation scheme the possibility to cheat does not increase the number of riddles in individual sessions (the coefficients of the dummy variable C_i are positive but statistically insignificant). Referring to the fixed

	Model 1			Model 2		
	(1)	(2)	(3)	(4)	(5)	(6)
	Piece rate	Fix wage	Pooled	Piece rate	Fix wage	Pooled
C_i	0.368	1.242	0.792	0.386	1.437**	0.777
	(0.728)	(0.794)	(0.536)	(0.773)	(0.698)	(0.509)
C_p	1.611**	2.591***	2.121***	1.715**	2.222***	2.036***
	(0.721)	(0.796)	(0.540)	(0.767)	(0.763)	(0.556)
NC_p	0.613	0.669	0.662	0.572	0.589	0.682
	(0.626)	(0.756)	(0.481)	(0.635)	(0.646)	(0.468)
Ability (inverse)				0.178	-0.925***	-0.453***
				(0.192)	(0.177)	(0.162)
Male				-0.327	0.373	-0.187
				(0.613)	(0.656)	(0.454)
Year of birth				0.0455	0.0903	0.0543
				(0.0377)	(0.0766)	(0.0371)
Conscientiousness				1.500	3.556**	1.949*
				(1.363)	(1.519)	(1.048)
Agreeableness				2.684	-2.310	0.355
				(2.307)	(1.716)	(1.396)
Openness				0.981	0.647	0.836
				(1.261)	(1.612)	(1.024)
Neuroticism				0.750	0.00430	0.332
				(1.042)	(1.483)	(0.945)
Extraversion				0.0231	2.662**	1.387
				(1.172)	(1.277)	(0.889)
Fixed wage			-0.977***			-0.885**
			(0.364)			(0.361)
Constant	10.47***	9.091***	10.28***	-84.63	-171.4	-100.0
	(0.530)	(0.540)	(0.417)	(74.73)	(152.3)	(73.63)
Observations	167	151	318	163	150	313
R^2	0.038	0.095	0.082	0.084	0.315	0.152

Notes: Coefficient estimates from ordinary least squares estimations. * significant at 10%; ** sign. at 5%; *** sign. at 1%. The dependent variable is the number of riddles solved (i.e. handed in) by subject k. C_i and C_p represent dummy variables that take the value of 1 whenever a subject took part in an individual/a peer session where cheating was possible. NC_p is a dummy variable that takes the value of 1 whenever a subject took part in no cheating peer session. *Ability (inverse)* captures the number of minutes participant k needed for completing the first riddle as an inverse measure of ability. The dummy variable *Male* takes the value of 1 whenever participant k was male and is 0 otherwise. *Year of birth* captures participant k's age. *Fixed wage* is a dummy variable that takes the value of 1 in all fixed wage sessions in the pooled estimations (columns 3 and 6). The additional variables capture the Big Five personality measures of neuroticism, openness to experiences, agreeableness, conscientiousness, and extraversion. They were measured using the short item version of the NEO-FFI by Costa and McCrae (1989) and normalized to values between 0 and 1. Some subjects did not provide information on all items asked in the questionnaire. We therefore end up with 163 (150) observations for the piece rate (fixed wage) when including all control variables. Heteroskedasticity-robust Huber-White standard errors are in brackets. Standard errors are clustered at the session level.

Table 6: OLS regression results. Dependent variable: number of riddles per participant.

wage scheme, column 2 replicates the findings from the t-tests presented above. However, controlling for inverse ability, gender, year of birth, and the Big Five personality inventory, column 5 reveals that the possibility to cheat increases the number of riddles in individual sessions under the fixed wage (C_i is statistically significant at the 5% level). This latter finding yields some support for our hypothesis 1. Also, a comparison of the coefficients and significance levels of C_p and NC_p supports hypothesis 1 that output is higher when cheating is possible.

Irrespective of the compensation scheme, the coefficient of the C_i dummy variable is considerably smaller than the coefficient of C_p. This supports our notion that participants cheat less in individual sessions than in peer sessions[16]. Comparing the coefficients of C_i, C_p, and NC_p, our estimation results support hypothesis 3. In sum, we find that the results of the (non-)parametric tests are confirmed when controlling for factors such as ability, gender, age, and the Big Five personality inventory.

4.3 Discussion

Overall, our experimental results show that positive peer effects on output are highly driven by cheating behavior. Individual output levels are highest in peer sessions where cheating is possible – both under the piece rate and the fixed wage compensation scheme. In this section, we will augment our analysis and present both insights on the pooled sample and gender specific cheating effects and a discussion on the potential limitations of our study.

4.3.1 Pooled Sample

We now estimate our econometric models for the pooled sample of piece rate and fixed wage sessions. Columns 3 and 6 of Table 6 are based on this pooled sample, including the dummy variable *fixed wage* which takes the value of 1 for the fixed wage sessions and is 0 otherwise. The estimation results based on this full sample support the previously discussed results; the coefficient of *fixed wage* again reveals that average output levels are significantly lower for the fixed wage conditions. We find mixed evidence regarding Hypothesis 2 in the pooled sample: the coefficient of the no cheating peer condition dummy variable NC_p is positive but insignificant, whereas the coefficient of C_p is larger and highly significant. This supports Hypothesis 3 and suggests that output is higher in peer sessions than in individual sessions (without the possibility to cheat) only when cheating is possible. Given the larger share of actual cheating in peer sessions, these results show that peer settings should be looked

[16] We can of course not identify actual cheating in the estimations, but the share of cheated solutions is higher in peer sessions, see the discussion above.

at carefully. In situations where the quality of individual output is not directly observable for principals, peer settings potentially induce cheating. Observing higher average output levels in peer settings than in individual working situations might then not necessarily be an indicator for a higher productive effort, but also for cheating.

4.3.2 Gender Effects

In recent years, a growing literature tests for gender differences in behavior. Although it is not the main focus of this paper, our data allow to test whether female and male participants react differently to peer settings and the possibility to cheat. Existing empirical evidence on gender differences in undesirable behavior such as lying and sabotage is mixed. Some scholars find that women are less prone to lying than men (Ross and Robertson 2000, Dreber and Johannesson 2008, Pruckner and Sausgruber 2008, Ellingsen et al. 2009, Conrads et al. 2013, 2014), whereas other studies find the opposite result (Tyler and Feldman 2004, Tyler et al. 2006) or domain specific or no significant gender differences at all (Lewis 1993, DePaulo et al. 1996, Rowatt et al. 1998, Cadsby et al. 2010, Childs 2012, Erat and Gneezy 2012, Houser et al. 2012, Belot and Schröder 2013, Abeler et al. 2014, Muehlheusser et al. 2015). Dato and Nieken (2014) find that men are more likely to engage in sabotage in tournaments than women, whereas Balafoutas et al. (2012) do not find significant gender differences.

To test whether (a) a peer setting and (b) the possibility to cheat influence behavior of female and male subjects differently, we estimated the models presented in Table 6 separately for these two subsamples. Table 7 shows the corresponding regression results. The coefficient of the dummy variable that captures the peer settings in which cheating was possible (C_p) is statistically significant in all estimations for the male subsample, irrespective of the payment scheme.[17] The C_p coefficients for the subsample of female participants have the expected sign, but remain insignificant for the piece rate. However, we find statistically significant coefficients for the female subsample under the fix wage payment scheme. All these coefficients turn out to be considerably smaller than those for the male subsample. As gender differences are not our main focus and sample sizes (especially for the male subsample) are small, we do not go into a deeper analysis here. However, we conclude that the overall effects of the possibility to cheat in peer settings reported in Table 7 tend to be driven by male participants. Albeit not possible with our data, this result suggests that it would be particularly interesting to focus on gender differences in cheating in future research. On the

[17] In columns (1) and (3) of Table 7, the C_p coefficient is significant at the 10%-level. Given the small sample size and the large absolute value of the coefficient, we interpret this as a sufficiently significant result to reject H0: $C_p = 0$.

	Model 1				Model 2			
	Piece rate		**Fix wage**		**Piece rate**		**Fix wage**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Male	Female	Male	Female	Male	Female	Male	Female
C_i	1.690	0.289	1.444	1.125	1.469	0.311	1.526	1.265
	(1.881)	(0.793)	(1.188)	(0.978)	(1.995)	(0.856)	(2.170)	(0.807)
C_p	4.167*	1.100	4.929***	1.761*	5.032*	1.220	4.286**	1.518**
	(2.125)	(0.741)	(1.490)	(0.921)	(2.546)	(0.756)	(1.813)	(0.757)
NC_p	2.944	0.144	1.311	0.592	3.077	0.0640	1.202	0.275
	(1.832)	(0.632)	(1.004)	(0.947)	(1.955)	(0.634)	(0.945)	(0.842)
Ability (inverse)					-0.330	0.232	-0.674	-1.052***
					(0.493)	(0.215)	(0.403)	(0.188)
Year of birth					0.130	0.0138	0.131	0.0737
					(0.109)	(0.0389)	(0.233)	(0.0746)
Conscientiousness					-0.627	2.319	3.288	3.533*
					(3.164)	(1.476)	(3.072)	(2.057)
Agreeableness					3.922	2.219	-0.353	-3.050
					(6.836)	(2.534)	(3.104)	(2.171)
Openness					-1.322	1.187	-1.562	1.756
					(3.350)	(1.435)	(2.547)	(1.747)
Neuroticism					1.699	0.479	-1.848	0.899
					(3.415)	(1.153)	(3.085)	(1.910)
Extraversion					1.945	-0.302	1.523	3.321**
					(2.565)	(1.472)	(2.609)	(1.633)
Constant	8.167***	10.93***	7.889***	9.542***	-253.5	-21.41	-250.9	-138.9
	(1.722)	(0.505)	(0.385)	(0.712)	(218.1)	(77.43)	(463.9)	(148.6)
Observations	37	130	46	105	36	127	45	105
R^2	0.171	0.024	0.259	0.048	0.267	0.072	0.410	0.318

Notes: Coefficient estimates from ordinary least squares estimations. * significant at 10%; ** sign. at 5%; *** sign. at 1%. The dependent variable is the number of riddles solved (i.e. handed in) by subject k. C_i and C_p represent dummy variables that take the value of 1 whenever a subject took part in an individual/a peer session where cheating was possible. NC_p is a dummy variable that takes the value of 1 whenever a subject took part in no cheating peer session. *Ability (inverse)* captures the number of minutes participant k needed for completing the first riddle as an inverse measure of ability. *Year of birth* captures participant k's age. The additional variables capture the Big Five personality measures of neuroticism, openness to experiences, agreeableness, conscientiousness, and extraversion. They were measured using the short item version of the NEO-FFI by Costa and McCrae (1989) and normalized to values between 0 and 1. Some subjects did not provide information on all items asked in the questionnaire. We therefore end up with 163 (150) observations for the piece rate (fixed wage) when including all control variables. Heteroskedasticity-robust Huber-White standard errors are in brackets. Standard errors are clustered at the session level.

Table 7: OLS regression results for female and male subsamples. Dependent variable: number of riddles per participant.

one hand, this holds with respect to the actual degree of individual cheating. However, on the other hand, given that Muehlheusser et al. (2015) report that the gender specific degree of lying in team decision making depends on a team's composition, it might in addition be worthwhile to investigate whether (fe)male cheating depends on the gender of the peer.

4.3.3 Potential Limitations

Our study is subject to several potential limitations which we aim to discuss briefly in this section. We use a laboratory experiment setup, meaning that some aspects of typical work situations will not be represented: First, experiment participants were not in a long-term social relationship with each other. In fact, we took measures to insure that participants in the same peer session did not know each other beforehand. Such anonymity is scarce in real life work situations – it might for example be found in call centers with constantly changing composition of employees. In fact, social relationships between coworkers could reinforce peer pressure exerted by the peers: It might be more important to measure up to coworkers who you know than to anonymous peers. That is, the pressure to perform might be greater in real life, causing us to potentially underestimate the effects of peer settings on cheating. However, second, in real life peers might also be more motivated to monitor each other and to frown upon and punish cheating (e.g. Ouchi 1979) as cheating potentially has negative consequences for the firm and therefore also individual workers. Another common caveat of experiments is, third, that they do not allow for long-term employment perspectives. In a real work setting, workers might abstain from cheating so as not to risk getting fired. On the other hand, they might be more likely to cheat if e.g. a promotion system is in place and promotion is based on relative output (see section 2 for the connection between tournament incentives and undesirable behavior). Lastly, our findings might depend on task type (see e.g. Bäker and Mechtel 2013). While the task itself was not too complicated, it required some concentration. Peer settings in general present a (potential) distraction to workers (Sanders et al. 1978, Baron 1986). The higher the cognitive load of the task, the less suitable a peer setting should be. Peer settings might thus have a stronger effect on cheating when tasks become more complex or cognitively demanding, as (i) distractions might make it harder to produce "real" output. Also (ii) given the complex nature of the task agents might feel that cheating will be perceived as an oversight rather than intentional, thus rendering cheating behavior more acceptable. The acceptability of cheating also differs between (organizational) cultures. Consequently, peer settings might have a much stronger "unproductive" effect in some cultures than in others.

5 Conclusions

Peer settings inducing positive peer effects have been deemed a cheap instrument to increase productivity. Real life examples of such peer settings that a firm might implement or abstain from doing so are open-plan offices, information systems that update employees about the achievements of their coworkers and themselves on a regular basis, or simply the decision of whether to let agents work at home or have them come into the office. The aim of our study was to analyze whether behavioral effects induced by peer settings are indeed always "positive" from the employers' point of view in the sense that they increase productive effort. An alternative and much less desirable explanation would be that peer settings provoke adverse behavior such as cheating and sabotage, as has been found for competitive settings, e.g. rank-order tournaments.

Our results challenge previous findings regarding positive peer effects. In our experiment, average output levels are higher in peer settings than in individual settings in experimental conditions in which cheating is possible, but not in those in which it is not possible. We find that cheating is more pronounced in peer settings than in individual settings. The observed increase in output levels is driven by actual cheating behavior in peer settings. This is a first indication that peer settings might induce cheating.

The potential implications of our study for management are straightforward: Having shown that peer settings increase counterproductive behavior (and only weakly increase productive effort), managers are well advised to reconsider peer settings as a cheap tool for increasing performance. While they cause no harm if cheating and potentially other counterproductive behavior such as sabotage are not feasible, they provide incentives to engage in said adverse behavior if possible. This holds even if there is no monetary interdependence between peers in our setting – in contrast to tournaments or team compensation settings.

Based on our results, future research might address two issues. First, it would be particularly interesting to focus on underlying channels that drive our results. Given that we find more pronounced output effects for male than for female subjects when cheating is possible, it might be interesting to investigate these differences in more detail. As discussed in the paper, peer settings possibly induce pressure to perform – which is why they might induce cheating even in the absence of monetary incentives. If peer settings trigger a competitive mindset, gender differences in competitiveness (see, e.g., Niederle and Vesterlund 2007) might explain the results presented in this paper. Future research might, thus, focus on differences in competitiveness as explanation for cheating. Second, it might be worthwhile to move the analysis from the lab to the field. Although it is difficult to provide causal evidence on peer effects in the field, implementing a field experiment might yield additional insights regarding the robustness of our core result.

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Appendix

No cheating condition

Instructions:

Welcome to this experiment and thank you for your participation. You will receive 2.50 Euro for arriving on time. Please turn off your mobile phones. Please read the instructions – which are identical for everyone – carefully. In case you have any questions, please raise your hand, so that we can assist you.

The amount of money you will have earned throughout the experiment will be paid out in cash at the end of the experiment. The payoff will be made in private so that no other participants will know your payoff. Your task in this experiment is to solve riddles, such as:

```
elapmkklogfygnasahyhzxfcaktrnzwatwsrudxkhbcynbehuu
fmcrksyjdscehtrhsmxzzmqjliuynifcwuoxnjmufryiyryilj
kouzafuhmsxpycxhqdmwnpplzoqqpputnlbwpkigsgpptdzikw
giexwehcxjxofbnsstdfmrpaseflsrphellwfaqmgxtyrkzkmp
zdmgkxjqigjfukaeeckotlsqczibliyrwcdpturpjnlczbcxkq
gotstuurrpbaarttkgguqqgbgauppjreqppcxluhemrzbrtzrw
yqwzzzututtifsltmlhejhinejqsyrbdsoubulrpaeypbjuksc
lzpxoxmmkgiedjbiqceunepddzselqkmqlocztmoffutiuymyp
ggtgwpnadrqbhlqgabsxhiodwqrxlfgyucwhkycjcziuqtqqdz
bphzlcygcghihgsbqzotpshduxuuffsdaijeibnddkqrppxffi
ufyrdgfcqqiwwgkebgadeohqwjreuqwreyippaxlcsijuuwqfb
ieokzuhguxczjyhppksdxcqasbwbrcyrhixkbcsungasiznbas
zkjlytegqliybefdqonmqqwzxiceemhxneewggzjaahyycdkwf
xwatbcpntowiluqattgoaiqfnqouhmmhhtmkgxdrppunoibold
qtjwbfppdkhhfbohuljoyosnbhyzaqaxwdwwgtncchnioueaep
zryujtejyokglnpkwkmgrluiwbjjozzcqdwraetxscmzzyialh
bblqoymbxwheqebsktakszcfgzfrbdizuujlrxycdhckukrgdd
```

In the beginning of the experiment you are handed out a sheet of paper which contains ten pairs of the letter P which are printed side by side (pp). In order to solve the task of this sheet, all of these ten pairs must be found and highlighted.

Your payoff depends on the number of solved sheets. You receive the most money (55 cents) for the first sheet, for the second it is less (50 cents), even less for the third (45 cents), and so on (in 5 cent decrements). An overview over the exact payoffs depending on the amount of solved sheets can be found in the payoff table at the end of the instructions. You can end the experiment anytime at any self-determined point of time in the experiment. Before you are handed out a new sheet an experimenter is going to tell you how much money you can earn by solving this specific sheet.

It is up to you how many sheets you are going to solve. The only rules are:

1. Before you start, put your name on the top of the sheet.

2. You can work at only one riddle at once.
3. Do not start with a new riddle before you have completed the one you are working on.
4. After completing a sheet you hand it to the experimenter who checks it and files it in a folder.
   If you want to solve another riddle afterwards, the experimenter is going to hand one out to you.

If you do not want to solve another sheet, please tell the experimenter. This is when the experiment ends. You fill in a short questionnaire about your person and then you receive your payoff.
Payoff table:

Sheet	Payoff	Accumulated Payoff
1	0.55 Euro	0.55 Euro
2	0.50 Euro	1.05 Euro
3	0.45 Euro	1.50 Euro
4	0.40 Euro	1.90 Euro
5	0.35 Euro	2.25 Euro
6	0.30 Euro	2.55 Euro
7	0.25 Euro	2.80 Euro
8	0.20 Euro	3.00 Euro
9	0.15 Euro	3.15 Euro
10	0.10 Euro	3.25 Euro
11	0.05 Euro	3.30 Euro
12+	0.00 Euro	3.30 Euro

+ 2.50 Euro for arriving on time.

Cheating condition

Instructions:

Welcome to this experiment and thank you for your participation. You will receive 2.50 Euro for arriving on time. Please turn off your mobile phones. Please read the instructions - which are identical for everyone - carefully. In case you have any questions, please raise your hand, so that we can assist you.

The amount of money you will have earned throughout the experiment will be paid out in cash at the end of the experiment. The payoff will be made in private so that no other participants will know your payoff. Your task in this experiment is to solve riddles, such as:

```
e l a p m k k l o g f y g n a s a h y h z x f c a k t r n z w a t w s r u d x k h b c y n b e h u u
f m c r k s y j d s c e h t r h s m x z z m q j l i u y n i f c w u o x n j m u f r y i y r y i l j
k o u z a f u h m s x p y c x h q d m w n p p l z o q q p p u t n l b w p k i g s g p p t d z i k w
g i e x w e h c x j x o f b n s s t d f m r p a s e f l s r p h e l l w f a q m g x t y r k z k m p
z d m g k x j q i g j f u k a e e c k o t l s q c z i b l i y r w c d p t u r p j n l c z b c x k q
g o t s t u u r r p b a a r t t k g g u q q g b g a u p p j r e q p p c x l u h e m r z b r t z r w
y q w z z z u t u t t i f s l t m l h e j h i n e j q s y r b d s o u b u l r p a e y p b j u k s c
l z p x o x m m k g i e d j b i q c e u n e p d d z s e l q k m q l o c z t m o f f u t i u y m y p
g g t g w p n a d r q b h l q g a b s x h i o d w q r x l f g y u c w h k y c j c z i u q t q q d z
b p h z l c y g c g h i h g s b q z o t p s h d u x u u f f s d a i j e i b n d d k q r p p x f f i
u f y r d g f c q q i w w g k e b g a d e o h q w j r e u q w r e y i p p a x l c s i j u u w q f b
i e o k z u h g u x c z j y h p p k s d x c q a s b w b r c y r h i x k b c s u n g a s i z n b a s
z k j l y t e g q l i y b e f d q o n m q q w z x i c e e m h x n e e w g g z j a a h y y c d k w f
x w a t b c p n t o w i l u q a t t g o a i q f n q o u h m m h h t m k g x d r p p u n o i b o l d
q t j w b f p p d k h h f b o h u l j o y o s n b h y z a q a x w d w w g t n c c h n i o u e a e p
z r y u j t e j y o k g l n p k w k m g r l u i w b j j o z z c q d w r a e t x s c m z z y i a l h
b b l q o y m b x w h e q e b s k t a k s z c f g z f r b d i z u u j l r x y c d h c k u k r g d d
```

In the beginning of the experiment you are handed out a sheet of paper which contains ten pairs of the letter P which are printed side by side (pp). In order to solve the task of this sheet, all of these ten pairs must be found and highlighted.

Your payoff depends on the number of solved sheets. You receive the most money (55 cents) for the first sheet, for the second it is less (50 cents), even less for the third (45 cents), and so on (in 5 cent decrements). An overview over the exact payoffs depending on the amount of solved sheets can be found in the payoff table at the end of the instructions. You can end the experiment anytime at any self-determined point of time in the experiment. Before you are handed out a new sheet an experimenter is going to tell you how much money you can earn by solving this specific sheet.

It is up to you how many sheets you are going to solve. The only rules are:

1. You can work at only one riddle at once.
2. Do not start with a new riddle before you have completed the one you are working on.

3. After completing a sheet you hand it to the experimenter who adds it to the other completed sheets.
   If you want to solve another riddle afterwards, the experimenter is going to hand one out to you.

If you do not want to solve another sheet, please tell the experimenter. This is when the experiment ends. You fill in a short questionnaire about your person and then you receive your payoff.
Payoff table:

Sheet	Payoff	Accumulated Payoff
1	0.55 Euro	0.55 Euro
2	0.50 Euro	1.05 Euro
3	0.45 Euro	1.50 Euro
4	0.40 Euro	1.90 Euro
5	0.35 Euro	2.25 Euro
6	0.30 Euro	2.55 Euro
7	0.25 Euro	2.80 Euro
8	0.20 Euro	3.00 Euro
9	0.15 Euro	3.15 Euro
10	0.10 Euro	3.25 Euro
11	0.05 Euro	3.30 Euro
12+	0.00 Euro	3.30 Euro

+ 2.50 Euro for arriving on time.

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