UNIVERSITÄT PASSAU
Wirtschaftswissenschaftliche Fakultät

Do just deserts and competition shape patterns of cheating?

Susanna Grundmann

Diskussionsbeitrag Nr. V-79-20

Volkswirtschaftliche Reihe ISSN 1435-3520

PASSAUER DISKUSSIONSPAPIERE

Herausgeber:
Die Gruppe der volkswirtschaftlichen Professoren
der Wirtschaftswissenschaftlichen Fakultät
der Universität Passau
94030 Passau

Do just deserts and competition shape patterns of cheating?

Susanna Grundmann

Diskussionsbeitrag Nr. V-79-20

Volkswirtschaftliche Reihe ISSN 1435-3520

Adresse des Autors/der Autoren:

Susanna Grundmann
Wirtschaftswissenschaftliche Fakultät
Universität Passau
94030 Passau

Telefon: +49 851 509 2553
Telefax: +49 851 509 2552
E-Mail: susanna.grundmann@uni-passau.de

Do just deserts and competition shape patterns of cheating?

Susanna Grundmann

January 2020

Abstract

Previous research has shown that people accept inequalities resulting from differences in performance but aim at reducing those resulting from differences in luck, corresponding to the fairness principle of just deserts. But will just deserts also intrinsically prevent people from cheating for their personal gain in a situation in which cheating can be disguised? And will competitive pressure crowd-out such an intrinsic motivation? I investigate these questions in a lab experiment based on Grundmann and Lambsdorff (2017). Subjects earn income and report a tax rate, which they determine by rolling a die under a cup, creating an incentive to cheat. Treatments vary whether the size of income is based on performance or luck and whether there is competition for a high income. In the luck treatments, just deserts would imply that subjects aim at reducing inequalities, such that cheating should decrease with income. If income is based on performance, the opposite should be true. The results show that cheating increases with income in the performance and the luck treatments, such that lucky subjects as well as high performers do not aim at reducing inequality. Just deserts thus do not intrinsically prevent subjects from cheating for their personal gain. Competition has no systematic effect on cheating.

JEL Classification: H26, C91

Keywords: Cheating, tax morale, laboratory experiment, income, real-effort task, distributive justice, just deserts, competition.

1 Introduction

The question of what constitutes a fair distribution of resources has been an object of debate for centuries and has produced a wide range of different views on distributive justice (Konow 2003). One prominent view argues that a fair distribution should take entitlements or deserts into account (Konow 2003: 1189). For example, while writing on the American Dream, James Truslow Adams (1943: 415) called for "opportunity for each according to […] ability or achievement". In a similar vein, Miller (1976: 106) writes that "rewards must be proportional not to actual contributions but to that part of […] contribution which can be attributed to […] own abilities and efforts". Adams' and Miller's quotes touch upon the notion of just deserts – the idea that everyone should get what they deserve (Galeotti et al. 2017, Frohlich et al. 2004, Hoffmann and Spitzer 1985). The concept of just deserts is captured by a liberal egalitarian (Cappelen et al. 2007) fairness ideal[1], which approaches distributive justice from the perspective of the source of income. It distinguishes between inequalities arising due to factors beyond individual control such as luck and factors within individual control such as effort, productivity and choices. Individuals should not be held responsible for factors beyond their control. Inequalities in income resulting from these factors are therefore not justified and should be equalised through redistribution. On the other hand, individuals should be held responsible for factors within their control, such that resulting inequalities are justified (Cappelen et al. 2007).

This fairness ideal forms a middle way between the two more extreme views of egalitarianism and libertarianism. An egalitarian view does not hold individuals responsible for any factors determining their income and aims at equalising all inequalities regardless of how they came about. Libertarianism, on the other hand, would hold individuals responsible for all factors determining their income and grant everyone exactly what they produced, thus resisting any redistribution (Cappelen et al. 2007). Experimental evidence shows that there seems to be considerable heterogeneity in the fairness ideals people hold (Cappelen et al. 2010), but that a majority of subjects hold others responsible for their individual productivity and effort and not for factors beyond their control (Almas et al. 2016, Bartling et al. 2018, Cappelen et al. 2010). Consequently, people are also more generous towards a recipient if they appear to be poor due to circumstances and not own lack of effort (Fong 2007). Different perceptions of the degree to which income inequalities depend on effort and on luck can even help explain differences between the Unites States

[1] Similar concepts include the accountability principle (Konow 1996, 2000), equality of opportunities (Roemer et al. 2003) and choice egalitarianism and meritocratism (Cappelen et al. 2010).

and Europe in the extent of the welfare state (Alesina et al. 2001, Alesina and Angeletos 2005, Bénabou and Tirole 2006).

The above-mentioned evidence shows support for the idea of just deserts. This paper examines whether people also exhibit behaviour in line with just deserts in a situation in which they can disguise their actions. To do this, I analyse cheating behaviour in a tax environment, in which individual cheating cannot be detected.[2] This allows me to assess whether people follow just deserts intrinsically in their decision to cheat. I run a laboratory experiment and develop a theoretical model from which hypotheses on predicted behaviour are derived. The basic design of the experiment builds on a study by Grundmann and Lambsdorff (2017). Subjects in my experiment work on a real effort task and receive a small piece rate in order to ensure that they have an incentive to exert effort. This small income is tax free. After the task, subjects receive their main income (called bonus), the determination of which depends on the treatment. Subjects must pay taxes on the bonus. The tax rate is determined by rolling a die under a cup, and must then be reported by subjects. This creates an incentive to cheat and ensures that cheating can only be detected at the aggregate level. Collected taxes are redistributed evenly.

Treatments vary along two dimensions. The first dimension varies whether the bonus is based on luck or performance, allowing me to examine whether just deserts explain cheating patterns.[3] Based on the predictions of the model, I expect subjects to be guided by just deserts in their decision which tax rate to report. This means that in the performance treatments, subjects will have no preference for reducing inequality. Subjects will cheat to the extent that the monetary gain compensates the moral costs of cheating. This is more strongly the case for higher bonus levels. Thus, reported tax rates are predicted to decrease with higher bonus levels in the performance treatments. On the other hand, in the luck treatments, subjects will experience disutility from inequality. The decision which tax rate to report will not only be guided by the financial gains and moral costs of cheating, but also by the desire to reduce inequality through redistribution. This makes reporting low tax rates more costly for subjects with a higher bonus and less costly for subjects with a lower bonus. Reported tax rates are thus predicted to increase with the bonus in the luck treatments.

[2] This paper only analyses intrinsic motivations for honesty and does not take extrinsic deterrence factors such as the probability of detection and punishments into account, which are usually included in experiments on tax compliance (for recent surveys see Alm 2018 or Malézieux 2018) and are also relevant in the standard model on tax compliance by Allingham and Sandmo (1972).

[3] The range of possible bonus levels is held fixed between rounds and subjects, but the way in which the bonus is determined varies. In contrast, Grundmann and Lambsdorff (2017) vary income between rounds and subjects but hold the way in which income is earnt fixed (piece-rate).

The second dimension along which treatments vary is whether there is competition for a high bonus. Competitive pressure might crowd-out preferences for just deserts and honesty. Competitions usually constitute constant-sum games, meaning that if one person wins, others necessarily do not win. Therefore, the winner knows that if she hadn't received the prize, someone else would have. This might increase the focus on the own individual gain and translate into more cheating. For example, Schwieren and Weichselbäumer (2010: 242) find that "competition draws the attention from the well-being of the group towards the individual […]. Consequently, individuals may find themselves less bound to adhere to standards of fairness but may find it legitimate to gain their personal share by cheating." My model predicts that competition increases the utility of own payoff through a stronger focus on individual wellbeing. Therefore, I expect reported tax rates to be lower in the competition treatments.

I find that subjects cheat more when they receive a higher bonus. This effect is present in the performance treatments and in the luck treatments. Subjects in the luck treatments do not aim at reducing inequality. Thus, I find no evidence for just deserts in a situation in which cheating can be disguised as a random event. This implies that considerations of distributive justice do not intrinsically prevent subjects from cheating for their own benefit. Instead, subjects react identically to a high income generated by luck and by performance. This implies that subjects feel entitled to their high income even if this was generated by factors beyond their control. This would be more in line with a libertarian fairness ideal. Competition has no overall effect such that reported tax rates are not systematically smaller in the competition treatments. Across all treatments, the rich always cheat more. The findings can be relevant for tax authorities with limited resources for investigating tax returns when deciding on which type and size of income to audit.

The remainder of this paper is organised as follows. Section 2 discusses relevant literature while section 3 presents the experimental design. In section 4, I develop the theoretical model and derive the hypotheses. Section 5 briefly describes experimental procedures and in section 6, I present the results. Section 7 discusses the results and concludes.

2 Related literature

A variety of experimental studies shows that those with more money behave more self-servingly in contexts such as tax compliance (Anderhub et al. 2001, Coricelli et al. 2010, Coricelli et al. 2014, Durham et al. 2014, Fochman et al. 2018, Grundmann and Lambsdorff 2017, Heinemann and Kocher 2013), public goods games (Buckley and Croson 2006, Cherry et al. 2005, Rapoport et al. 1989) or giving games (Erkal et al. 2011).[4] The observed behaviour might be explained by a cost-benefit calculation regarding financial incentives and moral costs, meaning that subjects act self-servingly when the financial gains that can be derived from such behaviour outweigh the moral costs this behaviour incurs (Kajackaite and Gneezy 2017). In contrast to low income, higher income would lead to a more favourable cost-benefit calculation, inducing subjects to act self-servingly.[5]

On the other hand, the intrinsic cost of a lie will also influence lying behaviour (Gneezy et al. 2018). Indeed, many subjects only lie partially (Fischbacher and Föllmi-Heusi 2013, Gino et al. 2009, Shalvi 2011b), cheat more if there are self-justifications available (Shalvi et al. 2015, Shalvi et al. 2011a) and engage in self-serving actions more frequently in situations with moral wiggle room, in which responsibility for outcomes cannot be clearly attributed to them (Batson et al. 1997, Dana et al. 2007, Regner 2017). This suggests that such behaviour reduces the cost of self-serving actions. Relatedly, Mazar et al. (2008) show that subjects cheat within a range that allows them to maintain an honest self-concept.

The source of income is likely to influence this connection between self-serving behaviour and income. Indeed, many experimental studies finds that earnt income creates entitlements. For example, if subjects have to earn the money to be divided, they are less generous towards others than if they receive the money as windfall or randomly (Carlsson et al. 2013, Carpenter et al. 2010, Cherry et al. 2002, Jakiela 2015, Oxoby and Spraggon 2008).[6] Having to earn the right to a role has similar effects (Hoffman et al. 1994). However, subjects are also more generous the stronger the recipients' entitlements (Cappelen et al.

[4] Alm et al. (1992), Eckel et al. (2007) and Rapoport and Suleiman (1993) find no or opposite effects.
[5] Gneezy et al. (2013), Gibson et al. (2013), Sutter (2009) and Gneezy (2005) find that subjects are more likely to lie if the monetary gain from lying increases. Abeler et al. (2016) and Weisel and Shalvi (2015) find that raising the stakes does not increase cheating while Gerlach et al. (2019) find that evidence is mixed.
[6] Cherry et al. (2005) find no difference in contributions for earnt and windfall income in a public goods game. In tax compliance experiments, Boylan and Sprinkle (2001) and Boylan (2010) find higher levels of compliance for earnt than for windfall income and Kirchler et al. (2009) and Muehlbacher and Kirchler (2008) find a positive relation between the hypothetical level of effort and compliance. While these results might imply that subjects place less value on their earnt income, this need not be the case. All tax compliance experiments mentioned include audits and fines. Thus, subjects might be more risk-averse in evading taxes on earnt income, because this can lead to an even larger loss of earnt income due to having to pay fines after being caught (Kirchler et al. 2009). Losing income through fines might then be worse for earnt income than for windfall income.

2013b, Cappelen et al. 2017, Fahr and Irlenbusch 2000, Frohlich et al. 2004, Oxoby and Spraggon 2008).

Entitlements seem to play an important role in respect to the question of what constitutes a fair distribution. A large body of literature has analysed distributive justice experimentally and has examined the question whether subjects perceive differences resulting from effort, choices, luck and performance as fair. Most evidence suggests that people more willing to accept inequalities if these are a result of performance, effort or choices and less likely to do so if they are a result of luck (Akbas et al. 2016, Almas et al. 2016, Bartling et al. 2018, Cappelen et al. 2013a, Gee et al. 2017, Konow 2000, Lefgren et al. 2016).[7] Even though some studies find that subjects apply distributive justice in a self-serving way (Deffains et al. 2016, Rodriguez-Lara and Moreno-Garrido 2012, Rutström and Williams 2000), most people seem to generally consider it fairer if income differences are a result of performance rather than of luck. Thus, the question arises whether subjects will also follow these fairness considerations in their decision to cheat.

Gravert (2013) as well as Kajackaite (2018) examine cheating behaviour in relation to luck and performance with differing results. Gravert (2013) finds that subjects are more likely to increase their compensation through stealing if their compensation was based on performance than if it was based on luck. Kajackaite (2018) finds that subjects lie more about the outcome of a luck task than of a performance task, thus increasing their payoff more strongly for the luck task. My experiment differs from Gravert (2013) and Kajackaite (2018) in an important way. Subjects in my experiment cheat at the expense of other subjects and not only the experimenter. This means that in Gravert (2013) and Kajackaite (2018), subjects might have perceptions of what they deserve as a fair payoff from the experimenter. But as there is no interaction with other subjects, these experiments do not capture just deserts in this sense. In this respect, my experiment is more similar to Galeotti et al. (2017), who contrast norms of equality and equity and show that if compensation deviates from just deserts, subjects who were disadvantaged by inequitable payoffs cheat in order to restore equitable payoffs.

The payoff someone receives often does not only depend on own performance or luck but may also be determined relative to others. A large body of literature shows that people are concerned about their relative earnings, status and ranking (e.g. Bolton 1991, Bolton and Ockenfels 2000, Charness et al. 2014, Clark et al. 2010, Fehr and Schmidt 1999, Robson 1992). Additionally, outperforming another subject even induces a Joy-of-Winning that

[7] Ku and Salmon (2013) are an exception, as they find more acceptance for inequalities arising from luck than from performance.

increases subjects' utility (Sheremeta 2010) and affects reward-related brain areas (Dohmen et al. 2011). It is therefore not surprising that many studies find contestants try to improve their chances of winning a competition by cheating or sabotaging others (Balafoutas et al. 2012, Carpenter et al. 2010, Dato and Nieken 2014, Deutscher et al. 2013, Harbring et al. 2007, Harbring and Irlenbusch 2008, Preston and Szymanski 2003, Schwieren and Weichselbäumer 2010) and that cheating increases with the intensity of competition (Conrads et al. 2014, Feltovich 2018). Several studies even show that competition also has a general detrimental effect on subsequent pro-sociality (Buser and Dreber 2016, Grosch et al. 2017, Ter Meer 2014). Competing for income with others may therefore increase overall cheating and possibly weaken the effect of just deserts.

The effect of winning a competition on subsequent honesty and pro-sociality is not entirely clear. Schurr and Ritov (2015) show that winners of a competition cheat more in a subsequent task than losers and relate this to the entitlement subjects feel through winning a competition. Siniver and Yaniv (2018), on the other hand, show that winners of a real-life lottery subsequently behave more honestly than losers. Buser and Dreber (2016) also find competition losers to be less cooperative in a subsequent public goods game than winners.

Finally, there might be an interaction between competition and performance and luck. Hoffman and Spitzer (1985) show that dictators behave more self-interestedly if they win the right to their role by outperforming another subject in a game than by a coin flip. This raises the question whether cheating might be increased by beating a rival or by the mere experience of winning. My experiment thus varies treatments along the dimensions performance-luck as well as competition-no competition.

3 Experimental design

The experiment lasts for 16 rounds and subjects play in groups of four. The groups are randomly re-matched at the beginning of each round. Each round consists of three phases: in the first phase (working phase), subjects work on a real effort task. In the second phase (bonus phase), subjects receive a bonus, the determination of which depends on the treatment. In the third phase (tax phase), subjects pay taxes on the bonus.

Working Phase

In the working phase, all subjects work on a real effort task consisting of the slider task by Gill and Prowse (2012) and earn a small piece rate of 5 Taler for each slider they correctly place on position 50. In each round, 21 sliders are displayed, and the length of the task is either 40 seconds or 80 seconds and alternates each round. Subjects are informed about how many sliders they placed correctly after finishing the task.

Bonus Phase

In the bonus phase, subjects receive a bonus, which either amounts to 100 Taler, 200 Taler, 300 Taler or 400 Taler. The way in which the bonus is determined depends on the treatments and will be explained in more detail below.

Tax Phase

After subjects are informed about the size of their bonus, they have to file a tax declaration in the tax phase. Subjects have to pay taxes on the bonus, but not on the income they receive by working on the real effort task. The tax rate is determined by rolling a die in a sealed cup. This approach is implemented in Grundmann and Lambsdorff (2017) and is commonly applied in studies on cheating (e.g. Fischbacher and Föllmi-Heusi 2013, Gächter and Schulz 2016, Shalvi et al. 2011a). In my experiment, each number on the die corresponds to a certain tax rate, as shown in table 2. Subjects are required to enter the tax rate in their tax declaration and then submit it. Deducted taxes of each group are redistributed evenly among the four group members. Subjects do not receive any feedback on redistribution until the end of the experiment.

Table 1 – number on die and corresponding tax rate

Number on die	Tax rate (%)
1	10
2	20
3	30
4	40
5	50
6	60

This die-rolling paradigm ensures that individual cheating cannot be detected and that self-serving actions can be disguised as a random event. The decision whether or not to comply with the rolled number therefore depends only on subjects' intrinsic motivation. While individual cheating cannot be detected, I can compare the resulting distribution of reported tax rates to the statistical distribution to detect dishonesty. If all subjects report their tax rates honestly, the mean tax rate should be 35% and each tax rate should be reported equally often. Outside of the laboratory, tax evaders might decide to underreport their income instead of claiming a wrong tax rate. However, both actions result in the same effect of reducing the deduction from the gross income. Further, there are many examples of cheating on the applicable tax rate. For a more detailed discussion of this paradigm see Grundmann and Lambsdorff (2017).

After the tax declaration phase, subjects are asked for their expectation regarding the average number of correctly placed sliders of the three other group members. The elicitation of expectations is incentivised, and subjects receive 5 Taler for a correct expectation rounded to the closest integer.

Treatments

I implement four treatments that vary along two dimensions. The first dimension refers to whether a subject's bonus depends on luck or on her performance in the slider task. The second dimension refers to whether there is competition for a high bonus in a sense that exactly one player from each group receives each bonus. An overview of the treatments is given in table 2.

Table 2 – overview of treatments

	No competition	Competition
Luck	Luck-NC	Luck-C
Performance	Perf-NC	Perf-C

In treatment Luck-NC, each subject receives a virtual urn each round with one black ball, one dark grey ball, one light grey ball and one white ball inside. The computer draws a ball for each subject from her urn every round and the bonus the subject receives depends on the colour of the ball. If a black ball is drawn by the computer, the subject receives 400 Taler. A dark grey ball renders 300 Taler, a light grey ball 200 Taler, and a white ball 100 Taler. The bonus is thus independent of subjects' performance in the real effort task and of the bonus of other subjects.

In treatment Luck-C, each group receives a virtual urn each round with one black ball, one dark grey ball, one light grey ball and one white ball inside. The computer draws a ball for each subject (without replacement) from the group's urn every round and the bonus each subject receives again depends on the colour of the ball. The subject for whom the black ball is drawn receives 400 Taler. The subject for whom the dark grey ball is drawn receives 300 Taler, the subject for whom the light grey ball is drawn receives 200 Taler and the subject for whom the white ball is drawn receives 100 Taler. The bonus thus is independent of subjects' performance in the real effort task but dependent on the ball that is drawn for other subjects, meaning that one subject's good fortune is another's loss.

In treatment Perf-NC, the bonus subjects receive depends on how well they perform in the slider task each round. Each round, three thresholds are communicated to subjects before they start working on the slider task. If subjects reach the highest threshold, they receive

400 Taler. If they reach the medium but not the highest threshold, they receive 300 Taler. If they reach the lowest threshold, they receive 200 Taler and if they don't reach any threshold, they receive 100 Taler. The thresholds vary by length of task and over the course of the experiment so as to ensure that the number of subjects receiving the different bonus levels is approximately comparable to other treatments. For this reason, I used data on the performance in the slider tasks of all sessions that were conducted before the first session of this treatment in order to determine appropriate thresholds.[8] Thresholds are calibrated in a way that if subjects perform exactly as in the preceding sessions, each bonus occurs in 24.9% to 25.1% of cases. All thresholds for each round can be found in table 6 in appendix B. The bonus in treatment Perf-NC is thus dependent on subjects' performance in the real effort task but independent of other subjects' performance.

In treatment Perf-C, subjects' bonuses depend on their performance relative to the other subjects in their group. This means that the best-performing subject receives 400 Taler, the second-best performing subject receives 300 Taler, the third-best performing subject receives 200 Taler and the worst-performing subject receives 100 Taler.[9] The bonus is thus dependent on subjects' performance in the real effort task and also dependent on other subjects' performance.

A few design choices should be explained in more detail. Subjects work on the real effort task in all treatments, regardless of whether the bonus depends on their performance in the working phase or not. This has two advantages. First, paying a piece rate for the real-effort task ensures that subjects have incentives to exert effort in all treatments, also in those in which the bonus does not depend on performance. I can thus rule out that differences between treatments can be explained simply by the fact that subjects had to exert effort in some treatments and not in others. Second, this approach allows me to control for the effect of doing well in the real effort task independently from receiving a high bonus. While the two are linked in the performance treatments, they are not in the luck treatments. Thus, analysing the effect of increased performance in the luck treatments allows me to control for whether subjects self-select into high performance and cheating.

The alternation of the length of the working time for the real effort task allows me to control for whether working on a task for a longer time influences cheating in the tax phase. Even though longer tasks lead to a higher payoff from the piece rate, this compensation is small compared to the bonus. The size of the bonus is not influenced by the length of the task, so

[8] Two sessions of Luck-NC, one session of Luck-C and two sessions of Perf-C had been conducted up to that point.

[9] If there is a tie, this is resolved by taking into account the position of the slider that was placed closest to 50 but did not reach 50. Of the subjects that are tied, the subject whose slider is closest to 50 obtains the higher relative position.

that subjects need to exert more effort in longer tasks for the same benefit from the bonus. I will thus control for whether the required effort had an influence on cheating. A second reason for implementing different task lengths is that this creates some variation and subjects cannot as easily compare their performance in one round to the next. Further, it might contribute to a variation of which subjects receive which bonus in the performance treatments as some subjects might be fast at first while others may catch up in the 80 sec tasks.

4 Model and hypotheses

Pure self-interest would predict that all subjects report the lowest possible tax rate of 10% in all rounds and all treatments given that individual cheating cannot be detected and has no adverse consequences. Based on the literature, I expect subjects to depart from this solution. In the following section, I will develop a theoretical model and derive alternative hypotheses from the model.

An individual i will report a tax rate y_i' that maximises utility given by

$$U_i = B_i(1 - y_i')\alpha_c - \varphi(\dots) - \beta(\dots) \qquad (1)$$

$B_i\ (1 - y_i')$ represents the monetary payoff, which increases with the received bonus $B_i = \{100, 200, 300, 400\}$ and decreases with the reported tax rate $y_i' = \{0.1, 0.2, 0.3, 0.4, 0.5, 0.6\}$. The utility from the monetary payoff also depends on the competition term $\alpha_c \geq 1$, which will be discussed in more detail later.

The second term of the utility function, $\varphi(\dots)$, denotes the costs of lying. The costs of lying are dependent on the aversion to lying $\varphi \geq 0$ and the extent of the lie given by the difference between the observed tax rate y_i and the reported tax rate y_i':

$$\varphi(y_i - y_i')^2 \qquad (2)$$

The aversion to lying φ captures the intrinsic disutility an individual experiences from telling a lie.[10] In general, this can be classified into three types (Kajackaite and Gneezy 2017). If $\varphi = 0$, lying bears no costs and, disregarding the third term of the utility function,

[10] My model does not include a term for disutility experienced from possible concerns for how honest a report will be perceived, which many models do (Abeler et al. 2016, Dufwenberg and Dufwenberg 2018, Gneezy et al. 2018, Kajackaite and Gneezy 2017). The fear of being perceived as a liar might not be as large in my setting, given that subjects play for 16 rounds and a dishonest image can be avoided if subjects report low tax rates for some tasks and counteract this by reporting high tax rates for others. Also, each new die roll eradicates the evidence of each preceding die roll. Nevertheless, the term φ can be assumed to also include reputational concerns as well as the disutility experienced from the psychological costs of lying (Shalvi et al. 2011b) and the threat a lie poses to subjects' own self-concept (Mazar et al. 2008).

individuals would always report the lowest tax rate $y_i' = 0.1$. If $\varphi = \infty$, lying costs are infinitely high, such that individuals would never lie and always report $y_i' = y_i$.
If $0 < \varphi < \infty$, lying bears positive but finite costs. For sake of parsimony, I will assume fixed positive costs of lying such that $0 < \varphi < \infty$. The costs of lying increase with the extent of the lie $(y_i - y_i')$, as shown in equation (2).

Finally, the utility function includes costs for inequality, $\beta(\dots)$. Subjects acting in accordance with just deserts would accept inequalities resulting from factors within individual control and want to equalise those resulting from factors beyond individual control (Cappelen et al. 2007). If payoffs result from factors beyond individual control, just deserts would be adhered to perfectly if the payoffs after taxation are equal.[11] The costs of inequality are given by

$$\beta(B_i(1 - y_i') - B_{-i}(1 - y'_{-i}))^2 \qquad (3)$$

with $B_{-i} = \{100, \dots, 400\}$ denoting the average bonus received by the other subjects in the group and $y'_{-i} = \{0.1, \dots, 0.6\}$ denoting the average tax rate reported by the other subjects. The costs are assumed to increase with the differences in payoffs after taxes. Thus, the higher the remaining inequality, the higher the costs. If subjects strive for equality, receiving a higher bonus would make it necessary to report a higher tax rate in order to avoid inequality.

Inserting equations (2) and (3) into (1) results in the complete utility function:

$$U_i = B_i(1 - y_i')\alpha_c - \varphi(y_i - y_i')^2 - \beta(B_i(1 - y_i') - B_{-i}(1 - y'_{-i}))^2 \qquad (4)$$

Maximising (4) with respect to the reported tax rate yields the first derivative

$$\frac{dU_i}{dy_i'} = -B_i\alpha_c + 2\varphi(y_i - y_i') + 2\beta B_i\big(B_i(1 - y_i') - B_{-i}(1 - y'_{-i})\big) = 0 \qquad (5)$$

Solving (5) for y_i' reveals the optimal reported tax rate given by

$$y_i' = \frac{\varphi y_i + \beta B_i\left(B_i - B_{-i}(1 - y'_{-i})\right) - 0.5 B_i\alpha_c}{\varphi + \beta B_i^2} \qquad (6)$$

In the context of my experiment, acting according to just deserts would mean that in the luck treatments, subjects should try to equalise payoffs through redistribution. In the performance treatments, on the other hand, the bonus is determined by subjects' performance and effort in the task and is within subjects' control. The arising inequality in these treatments will find justification, given that those who performed better earn more.

[11] This is achieved if the amount of income subjects are left with after they have paid taxes is equal. This is the case because taxes are redistributed evenly in the end.

Subjects will thus feel entitled to their bonus and will not aim at equalising payoffs. Therefore, I assume $\beta = 0$ in the performance treatments and $\beta > 0$ in the luck treatments.

In the performance treatments, the optimal reported tax rate is thus given by

$$y_i' = \frac{\varphi y_i - 0.5 B_i \alpha_c}{\varphi} \qquad (6.1)$$

Equation (6.1) shows that for given φ, y_i and α_c, the reported tax rates will always decrease with the bonus, because B_i enters the numerator negatively. The larger the bonus, the more strongly the moral costs of cheating are compensated by a higher gain from cheating, and thus the lower the reported tax rate. In the performance treatments, reported tax rates that decrease with the bonus are in line with just deserts. If a subject in the performance treatments receives a low bonus, they will accept that they do not deserve more and the gains from cheating will not or only to a small extent compensate the moral costs cheating would bear. If a subject receives a high bonus, they will feel entitled to their high payoff and feel they deserve it. The gains from cheating will compensate the moral costs of cheating to a large extent. None of these subjects would strive for equalising payoffs.[12]

In the luck treatments, subjects acting in line with just deserts will suffer from inequality, because the bonus levels are determined by factors beyond individual control. Subjects will therefore strive for reducing inequality. As mentioned above, this implies $\beta > 0$ in the luck treatments. I will simplify equation (6) and set $\varphi = 0$ for the time being. Under these assumptions, the optimal reported tax rate in the luck treatments is given by

$$y_i' = \frac{\beta B_i \big(B_i - B_{-i}(1 - y_{-i}')\big) - 0.5 B_i \alpha_c}{\beta B_i^2} \qquad (6.2.1)$$

Equation (6.2.1) can be rearranged such that

$$y_i' = 1 - \frac{\beta B_{-i}(1 - y_{-i}') + 0.5\alpha_c}{\beta B_i} \qquad (6.2.2)$$

Equation (6.2.2) shows that for given B_{-i}, y_{-i}' and $\alpha_c$the reported tax rates will always increase with the bonus. This is the case because the bonus B_i only enters the denominator, which means that the term being subtracted from 1 is smaller the larger B_i.

Just Deserts Hypothesis: *The mean reported tax rate will decrease with the received bonus in the performance treatments and will increase with the received bonus in the luck treatments.*

[12] The negative relation between the bonus and reported tax rates means that subjects with different bonus levels will pay similar absolute amounts of taxes, which would uphold the income distribution.

I can drop the simplification of $\varphi = 0$ in the luck treatments and instead set $\varphi > 0$. Mathematical simulations show that an increase from the lowest to the highest bonus will also lead to an increase in the reported tax rate in the luck treatments for $\varphi > 0$, for all cases in which β is large enough to be relevant compared to φ.

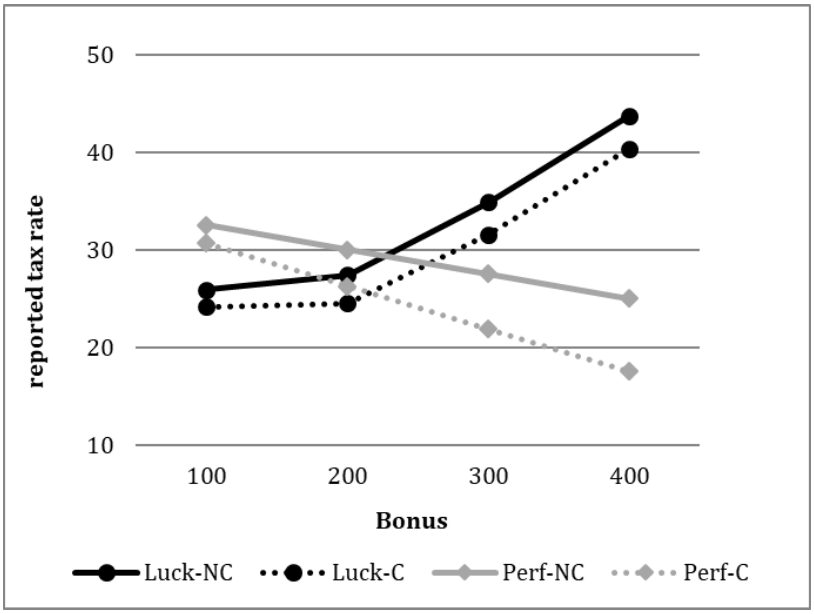
The second hypothesis concerns the effect of competition. As described earlier, competition has a negative effect on pro-sociality and increases the focus on the individual. Competition is thus assumed to increase the utility individuals derive from their own financial gain. In the no-competition treatments, I will assume a neutral weight $\alpha_c = 1$, such that the financial gain is only dependent on the bonus and the reported tax rate. For the competition treatments, I will assume $\alpha_c > 1$, such that individuals place a larger weight on their financial gain. The financial gain will thus more easily compensate the costs of cheating. In equation (6), α_c enters the numerator negatively, such that competition will lead to lower reported tax rates.

Competition hypothesis: *The mean reported tax rate will be lower in the competition treatments than in the no-competition treatments for all bonus levels.*

I will assume a weight for the aversion to lying of $\varphi = 2000$ and will further assume that a subject on average observes a tax rate of $y_i = 0.35$. In the luck treatments, I will assume a weight for the disutility of inequality of $\beta = 0.015$ and assume that a subject expects the other three subjects to receive a bonus $B_{-i} = 250$ and report a tax rate $y'_{-i} = 0.35$ on average.[13] In the competition treatments, I will assume $\alpha_c = 1.75$. Inserting the values for $\alpha_c, \beta, \varphi, B_{-i}, y'_{-i}$ and y_i into equation (6) allows me to plot a graphical illustration of the predictions derived by the model for all bonus levels for each treatment as displayed in figure 1.

[13] While it is reasonable to use the average of all possible bonuses in Luck-NC, one might argue that a subject knows exactly how high the average bonus of the other three players is given her own bonus in Luck-C. However, she might at the same time expect the average reported tax rate to increase with the average bonus. Thus, a subject in Luck-C might expect the same average payoff after taxes of others as a subject in Luck-NC. For sake of parsimony, I will thus also assume $B_{-i} = 250$ and $y'_{-i} = 0.35$ in Luck-C.

Fig. 1 – theoretical prediction of reported tax rates by treatment and bonus



5 Experimental procedures

The 12 sessions of the experiment were conducted computer-based at the PAULA laboratory at the University of Passau using z-Tree (Fischbacher 2007) and Orsee (Greiner 2015). Sessions included 16-20 subjects each such that, in total, 224 subjects participated. One treatment was played per session (between-subject design). The experiment was run between October 2018 and February 2019. Altogether, the experiment lasted 50-60 minutes. 55.4% of the participants were female. Participants were on average 22.6 years old. The most frequent fields of study included cultural studies, law, economics as well as media and communication studies.

All instructions can be found in appendix A. After reading the instructions, subjects were redirected to a screen with incentivised comprehension questions. Subjects earned 1 Taler per question if they answered the question correctly in the first try. 25 Taler converted into 1 Euro. At the end of the experiment, one round was randomly drawn, and this round determined subjects' payoff. They do not receive any feedback on the other rounds at any time. Subjects were informed about their final payoff in Taler and Euros at the end of the experiment. After the end of the experiment, participants answered a demographic questionnaire on their age, gender, field of study and stated whether they had already filed a real tax declaration and their political affiliation, which they could also choose not to state. Participants earned an average payoff of 11.54 EUR. Payoffs ranged between 4.00 EUR and 21.10 EUR. Participant exited the lab one by one and each received their payoff in private outside of the laboratory from a person that did not know the content of the experiment. This was communicated to subjects before the experiment started.

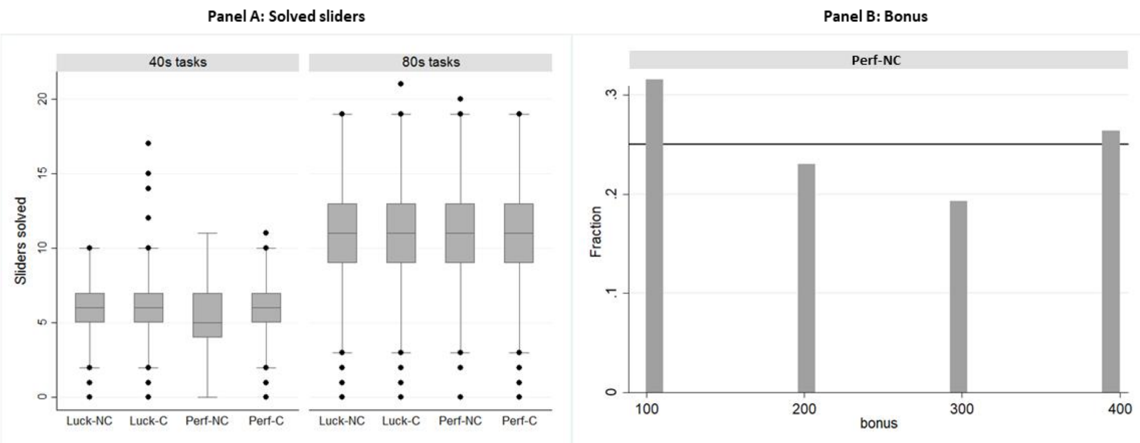
6 Results

In this section, I will first give an overview over the data by presenting aggregate results in the working phase, the bonus phase and the tax declaration phase. Second, I will analyse possible treatment differences and provide results for the hypotheses, focussing first on the just deserts hypothesis and then on the competition hypothesis. Finally, I will provide robustness checks.

Data overview

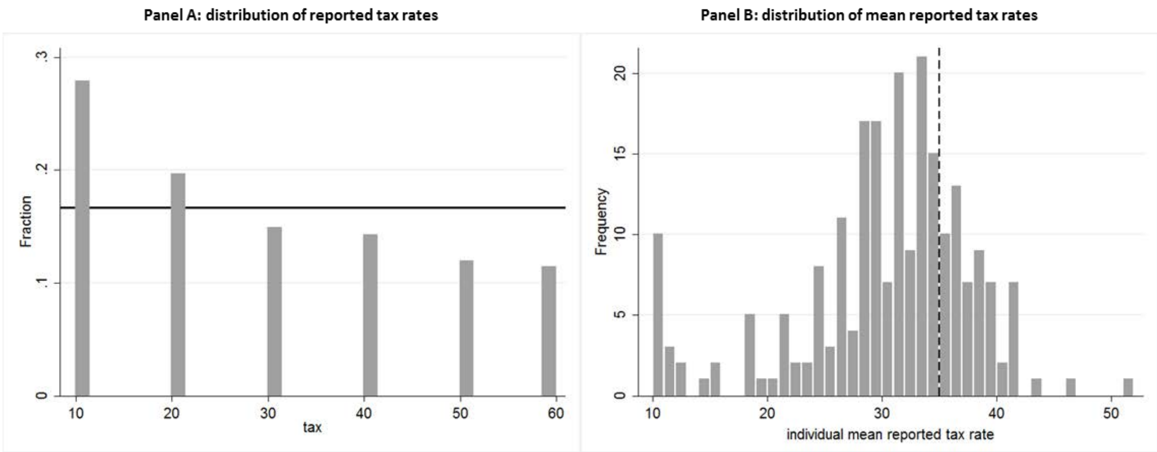
In the working phase, subjects placed an average of 5.7 sliders correctly in the 40 sec tasks and 10.8 sliders correctly in the 80 sec tasks. Panel A in figure 2 displays boxplots for the correctly placed sliders by treatments for the 40 sec and the 80 sec tasks and reveals only small differences between the treatments. The figure shows that the median is identical (11 sliders) for all treatments in the 80 sec tasks and only slightly lower for Perf-NC (5 sliders) than for the other treatments (6 sliders) in the 40 sec tasks. This means that the effort exerted was similar across treatments. By design, each bonus necessarily occurred equally often in the competition treatments. In treatment Luck-NC, the share of each bonus ranges between 23.2% and 26.5%. A t-tests reveals that the mean bonus of 245.4 in treatment Luck-NC is not significantly smaller than 250 (p=0.103). Panel B in figure 2 shows the distribution of bonuses for treatment Perf-NC. In this treatment, shares lie between 19.2% and 31.5%. The mean bonus of 240.4 is significantly smaller than 250 (t-test, p<0.01).

Fig. 2 – Boxplots for sliders by treatments and working time and histograms for bonuses in Perf-NC



Panel A in figure 3 displays the overall distribution of mean reported tax rates, which is similar to the distribution of outcomes observed in other experiments using the die-rolling paradigm (Fischbacher and Föllmi-Heusi 2013, Gächter and Schulz 2016, Grundmann and Lambsdorff 2017, Shalvi et al. 2011a). The figure exhibits that lower tax rates were reported more often than higher tax rates. One-sided binomial tests reveal that the share with which tax rates were reported is significantly different from 16.6% at the 1% level for all tax rates. The mean reported tax rate across all treatments, subjects and rounds equals 29.7%, which is significantly different from 35% (t-test, $p<0.01$). Panel B in figure 3 shows the distribution of individual mean reported tax rates rounded to the closest integer. 10 subjects (4.5%) cheated to the full extent and reported a tax rate of 10% in all 16 rounds.[14] The other subjects were either responsive to the circumstances, cheating in some occasions but not in others, or cheated to some extent but not fully, or reported their tax rates honestly. It is important to note, however, that even a mean reported tax rate of 35% or larger does not necessarily mean that subjects were entirely honest. Despite having reported high and low tax rates, subjects might not have reported them in the task they rolled them for.

Fig. 3 – overall distribution of reported tax rates and distribution of individual mean reported tax rates



There are some gender differences with regard to the slider tasks, bonuses and reported tax rates. On average, men solved about one slider more in the tasks than women. Given these differences, women reached the highest bonus of 400 slightly less often in the performance treatments than men. 47.0% of bonuses of 400 in treatment Perf-NC were reached by women. In treatment Perf-C, this number equals 47.9%. On average, men reported significantly lower (Mann-Whitney tests, $p<0.01$) tax rates (28.5%) than women (30.7%).[15] Given these observed differences, all regressions will control for gender.

Just Deserts Hypothesis

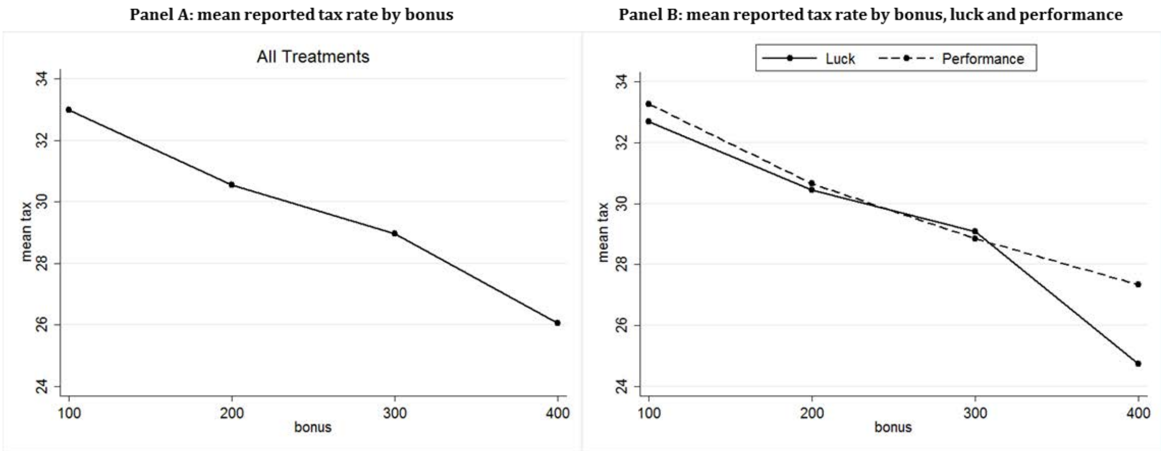
Panel A in figure 4 displays the mean reported tax rates for each bonus pooled for all treatments. The figure exhibits a negative relationship between the bonus and the mean reported tax rate. While the mean reported tax rate equals 33.0% for a bonus of 100,

[14] Between 1 and 4 subjects per treatment reported a tax rate of 10% in all rounds such that the number of payoff maximising subjects is distributed fairly evenly across treatments.

[15] The finding that women cheat less is in line with the literature (e.g. Abeler et al. 2016, Bühren and Kundt 2014, Coricelli et al. 2010, Fortin et al. 2007, Frey and Torgler 2007, Grundmann and Lambsdorff 2017, Pántya et al. 2016).

subjects on average reported a tax rate of 26.1% for a bonus of 400. The mean reported tax rates of all bonus levels are significantly different from 35% (t-tests, p<0.01 for all bonus levels) and also significantly different from each other (Mann-Whitney tests: 1 vs 2, p<0.01; 2 vs 3: p=0.051; 3 vs 4: p<0.01).

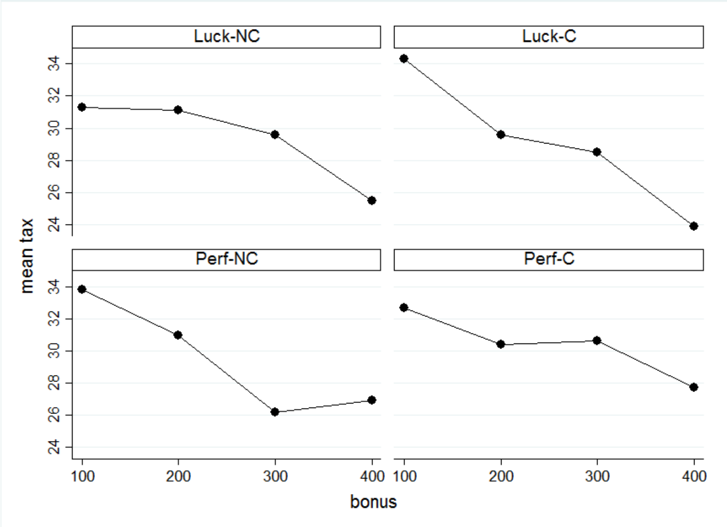
Fig. 4 – mean reported tax rate by bonus



Panel B directly compares the effect of luck and performance by splitting up the data into both luck treatments (Luck-NC and Luck-C) and both performance treatments (Perf-NC and Perf-C). In the luck and the performance treatments, reported tax rates decline for higher bonuses. Only comparing the bonuses of 100, 200 and 300, there is hardly any difference at all between the luck and performance treatments. For the bonus of 400, the reported tax rate is significantly lower for the luck treatments than for the performance treatments (24.7% vs 27.3%, Mann-Whitney test, p=0.013), which is contrary to what the just deserts hypothesis would have expected. Overall, the mean reported tax rates for luck (29.3) and performance (30.1) show only small differences and a Mann-Whitney test reveals that the difference between the treatments is only marginally significant (p=0.083).

Figure 5 shows the mean reported tax rates by bonuses for each treatment separately. Reported tax rates decline with higher bonuses in all treatments. This effect seems most pronounced in treatment Luck-C, which is not in line with what the just deserts hypothesis predicted. All in all, it appears that subjects across treatments did not act in accordance with just deserts. Subjects in the luck treatments, in which the bonus was not determined by factors within subjects' control, did not aim at equalising payoffs through redistribution. Instead, they also reported lower tax rates for higher bonuses.

Fig. 5 – mean reported tax rate by bonus and treatment



Just Deserts Result: *The mean reported tax rate decreases with the bonus in the performance treatments, but also in the luck treatments. This is not in line with just deserts.*

The graphical analysis is supported by the results of random effects regressions in table 4.[16] Regression 1 uses the reported tax rate as the dependent variable and only the bonus as the independent variable. The coefficient for the bonus equals -2.19 is significant at a 1% level, supporting the graphical analysis that higher bonuses lead to lower reported tax rates. Regression 2 further includes some control variables, first of all a variable indicating the number of correctly placed sliders in the real effort task (effort) in order to distinguish between effort in the task and the bonus received. The regression further includes a dummy variable indicating whether a subject was female, to account for the observed gender differences. It also includes subjects' age and a dummy indicating whether the observation stems from a round in which subjects' had to work for 80 seconds. Finally, the period is included in order to control for effects such as learning or exhaustion over the course of the experiment. All further regressions on the reported tax rate will also include these variables as controls. The influence of the control variables on the reported tax rate will be discussed in further detail later. The coefficient for the bonus in regression 2 equals -2.31 and remains significant at the 1% level, revealing that the negative effect of the bonus is robust to including these control variables.

Regression 3 displays results for the effect of the bonus separately for luck and performance treatments and thus also includes a dummy for the performance treatments in addition to the control variables mentioned above. The variable Luck treatments*Bonus uses data from both luck treatments and reveals that the bonus has a significantly negative effect (-2.42,

[16] I also ran ordered logit, ordered probit and fixed effects regressions to check robustness. These did not render different findings and are thus not displayed here.

p<0.01) on the reported tax rate. The variable Performance treatments*Bonus uses data from both performance treatments. The coefficient is also negative and significant (-2.11, p<0.01). Although the effect of the bonus seems to be larger for the luck treatments, a Wald-test reveals that the difference is not significant (p=0.632).[17]

Table 3 – Random Effects regressions results

Reported tax rate	1 Overall	2 Overall	3 Luck vs Performance	4 Treatments
Bonus	-2.19*** (0.31)	-2.31*** (0.33)		
Luck treatments*Bonus			-2.42*** (0.41)	
Performance treatments*Bonus			-2.11*** (0.52)	
Luck-NC*Bonus				-1.88*** (0.50)
Luck-C*Bonus				-3.04*** (0.67)
Perf-NC*Bonus				-2.85*** (0.70)
Perf-C*Bonus				-1.58** (0.66)
Effort		0.21 (0.15)	0.18 (0.15)	0.20 (0.15)
Female		2.20** (1.08)	2.15** (1.08)	2.13** (1.08)
Age		0.29* (0.15)	0.29* (0.15)	0.29* (0.15)
80 sec tasks		-2.13** (0.95)	-1.99** (0.95)	-2.04** (0.93)
Period		-0.18*** (0.07)	-0.17** (0.07)	-0.17*** (0.07)
Performance treatments			-0.06 (1.87)	
Luck-C				2.50 (2.65)
Perf-NC				2.39 (2.63)
Perf-C				0.19 (2.28)
Constant	35.1*** (0.92)	33.6*** (1.65)	33.7*** (1.94)	32.5*** (2.08)
N	3584	3584	3584	3584
R^2	0.022	0.030	0.031	0.033

Standard errors in parentheses, * p<0.10, ** p<0.05, *** p<0.01
Notes: *Bonus* divided by 100 before entering the regression. *Age*: subjects' age minus 18

[17] I ran a Power-Test to estimate the necessary sample size, which revealed a necessary sample size of 492 observations per group for detecting differences between the effects of high and low bonuses in the combined performance and luck treatments. My sample size is thus sufficiently large to detect differences, should they exist. Further details on the power test can be found in appendix B.

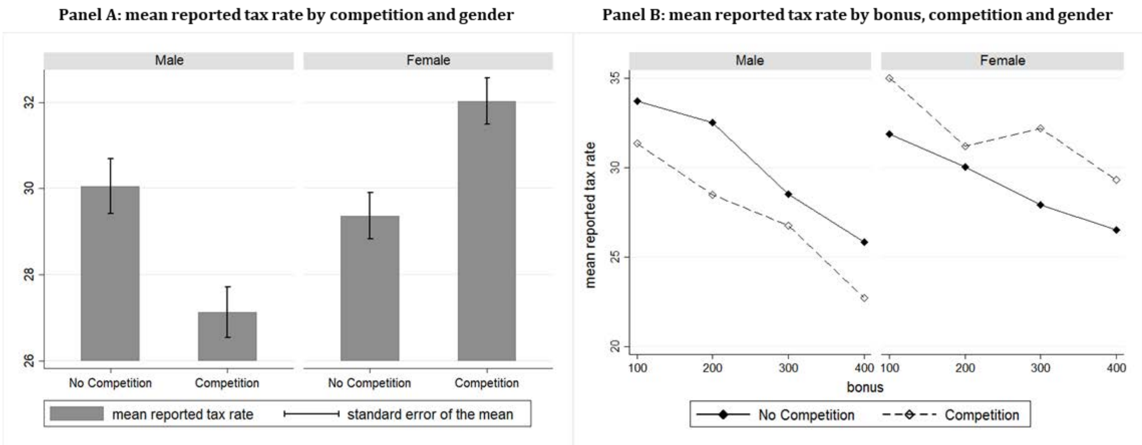
Regression 4 shows the effect of the bonus for each treatment separately, including the usual control variables as well as dummies for the treatments. The results show that the coefficient for the bonus is negative in all treatments and significant at a 1% level for all treatments apart from Perf-C, in which it is significant at a 5% level. Comparing Luck-NC and Perf-NC, performance seems to increase the negative effect of the bonus on the reported tax rate, but the difference is not significant (Wald-test, p=0.256). Looking at treatments Luck-C and Perf-C, the effect of the bonus becomes weaker for performance, which is again contrary to the expected effect. The difference is, however, also not significant (Wald-test, p=0.109). Supporting the graphical analysis, the effect of the bonus is largest in size for treatment Luck-C, though not significantly different from the other treatments.

Competition Hypothesis

The competition hypothesis predicted that competition would lead to more overall cheating. The overall level of cheating is very similar in all treatments. The mean reported tax rate equals 29.5 in Luck-NC, 29.1 in Luck-C, 29.9 in Perf-NC and 30.3 in Perf-C. While the mean reported tax rate is marginally lower in treatment Luck-C than in Perf-C at the 10%-level (Mann-Whitney test, p=0.083), a Kruskal-Wallis test (p=0.310) as well as pairwise Mann-Whitney tests reveal no other significant differences between the treatments.

To further analyse the competition hypothesis, I combine both no-competition treatments (Luck-NC and Perf-NC) and both competition treatments (Luck-C and Perf-C). The mean reported tax rate for the no-competition treatments equals 29.7 and takes a value of 29.8 for the competition treatments. This minor difference is not significant (Mann-Whitney test, p=0.706). While this seems to contradict the competition hypothesis, splitting the data by gender offers more detailed insights. Panel A in figure 6 shows the mean reported tax rate by male and female participants in the competition and no-competition treatments. In the no-competition treatments, men report a tax rate of 30.1% and women report a tax rate of 29.4% on average. This difference is not significant (Mann-Whitney test, p=0.505). The effect of competition differs strongly between women and men. Compared to the no-competition treatments, men report significantly lower tax rates of 27.1% in the competition treatments (Mann-Whitney test, p<0.01). For women, the mean reported tax rate significantly increases to 32.0% in the competition treatments (Mann-Whitney test, p<0.01). Thus, the finding that there is no overall difference is driven by the fact that the predicted negative effect only occurs for men and is cancelled out by the opposite effect for women.

Fig. 6 – mean reported tax rate by competition



Given the observed negative effects of the bonus on the reported tax rate, one might wonder whether the gender effect is driven by different average bonus levels. Men, on average, reach higher bonuses than women in the competition treatments (257.5 vs 243.5, Mann-Whitney test, $p<0.01$), which might explain the gender differences in these treatments. However, men do not exhibit significantly different average bonus levels between the competition and the no-competition treatments (Mann-Whitney test, $p=0.511$) and women receive marginally significantly higher bonuses in the competition treatments (Mann-Whitney test, $p=0.088$). The effect thus seems to be driven by the experience of competition and cannot be explained by the received bonuses. This can be observed in more detail in panel B of figure 6. Panel B shows the mean reported tax rates by bonus for the competition and the no-competition treatments for women and men separately. The graphs are all negatively sloped. However, while men report lower tax rates in the competition treatments than in the no-competition treatments for all bonus levels, the effect is opposite for women, who report higher tax rates for all bonus levels in the competition treatments. Competition thus appears to induce more cheating for men and make women act more honestly while preserving the overall effect of the bonus.

Table 4 offers further insights. Regression 1 includes all data and regresses the reported tax rate on a dummy for whether the data stems from a competition treatment, controlling for the bonus, effort, gender, age, 80 sec tasks and the period. The coefficient for competition is small (0.33) and does not have a significant influence ($p=0.741$). Regression 2 uses only data for the subsample of male subjects. Here, the variable for competition is larger and negative with a value of -2.87, but marginally misses significance ($p=0.101$). The regression results thus do not fully support the graphical analysis. Finally, regression 3 uses only data for the subsample of female subjects. The results exhibit that women report significantly higher tax rates in the competition treatments. The coefficient for competition takes a value of 2.86 and is significant at the 1% level.

Overall, the competition hypothesis finds no support, although there is some evidence that men act in the way predicted by the model.

Competition Result: *Overall, there is no difference in the mean reported tax rate between the competition treatments and the no-competition treatments. There is some evidence that compared to the no-competition treatments, men report lower tax rates in the competition treatments. Women report significantly higher tax rates in the competition treatments than in the no-competition treatments.*

Table 4 – Random Effects regression results

Reported Tax Rate	1 All	2 Male	3 Female
Competition	0.33	-2.87	2.86***
	(0.99)	(1.75)	(1.04)
Bonus	-2.31***	-2.84***	-1.89***
	(0.33)	(0.51)	(0.43)
Effort	0.21	-0.16	0.22
	(0.15)	(0.23)	(0.18)
Female	2.21**		
	(1.08)		
Age	0.29*	0.55***	0.01
	(0.15)	(0.18)	(0.20)
80 sec tasks	-2.13**	-2.31	-1.77
	(0.95)	(1.57)	(1.11)
Period	-0.18***	-0.13	-0.20**
	(0.07)	(0.10)	(0.09)
Constant	33.5***	35.3***	34.6***
	(1.48)	(2.11)	(1.91)
N	3584	1600	1984
R^2	0.030	0.055	0.022

Standard errors in parentheses, * p<0.10, ** p<0.05, *** p<0.01
Notes: *Bonus* divided by 100 before entering the regressions. *Age*: subjects' age minus 18.

Robustness checks: Expectations

After subjects had reported their tax rate each round, they had to state their expectation regarding the average number of correctly placed sliders of the other three players in the preceding working phase. This allows me to conduct several robustness checks. First, in the luck treatments, subjects might self-servingly believe their own performance in the slider tasks to be better than that of the others if they receive a high bonus and thus feel entitled to cheat. Or they might try to justify their preceding cheating by looking for an entitlement for their high bonus. In any case, this would create confounds, as the bonus and the slider task would not have been perceived as completely independent from one another. To control for this, I created a variable indicating subjects' relative expectation. This was done by subtracting the expectation about others' mean number of correctly placed sliders from subjects' own number of correctly placed sliders. A positive relative expectation then means that subjects thought they were better than others and vice versa. A correlation coefficient of -0.01 (p=0.659) reveals no notable correlation between the bonus and the relative expectations in the luck treatments. Running a regression of the relative expectations on the bonus and controlling for effort and gender also reveals no significant effect of the bonus on the relative expectation in the luck treatments (see regression 1 in table 5). I can thus conclude that subjects in the luck treatments correctly distinguished between their performance in the slider task and their bonus. Including the relative expectations in regression 2 in table 3 shows no significant influence on the reported tax rate and also hardly has any influence on the effect of the bonus (see regression 2 in table 5

).

Secondly, the relative expectations can be used for a robustness check in treatment Perf-NC. While subjects should expect an average bonus of 250 in treatment Luck-NC, subjects might have biased expectations about the average bonus of others in treatment Perf-NC. Such biased expectations may influence their behaviour in the tax reporting phase. To test this, I calculated the expected average bonus of others for each subject for every round in each session of treatment Perf-NC. For this, I used subjects' expectations regarding the average of other players' correctly placed sliders and compared them to the target values in each round in order to derive the expected average bonus. Taking the mean of the expected average bonus yields a value of 246.1, which is not significantly different from 250 (t-test, p=0.279). Thus, on average, subjects did not have biased expectations regarding the average bonus of others. The expected average bonus of others is, however, dependent on the own received bonus. A correlation coefficient of 0.49 (p<0.01) between the own bonus and the expected average bonus of others reveals that subjects also expected others to be better when they themselves were. I therefore ran regression 4 in table 3 using only data for Perf-NC and included the expected average bonus of others (see regression 3 in table 5). The results show that subjects' expectations have no significant influence on the reported tax rate (p=0.793) and hardly any impact on the effect of the bonus.

Robustness checks: Control variables

A possible caveat to my findings might be self-selection in the sense that some subjects come to the lab with the determination to earn as much money as possible. These subjects would put in a lot of effort in the slider task in order to maximise the piece-rate earnings from the task and also make use of the possibility to cheat and report especially low tax rates. This could be problematic in the performance treatments, as the observed effect would then be driven by self-selection and not by higher bonus levels.[18] My design allows me to control for this possibility. First of all, the results show that those with a higher bonus also cheat more in the luck treatments, which is an indication that self-selection does not play a role. A more thorough way to check this is by looking at how productive subjects were in the slider tasks in the luck treatments. A self-selection effect would imply that very productive subjects cheat more, regardless of their bonus, in order to maximise their payoff. Productivity in the slider tasks is measured by the total number of sliders each subject solved correctly.[19] Running a regression including data for the two luck treatments and using each subject's mean reported tax rate as dependent variable and the total number of correctly solved sliders as the independent variable and controlling for gender and age reveals that the variable for the sum of correctly solved sliders is positive and significant at the 5% level (see regression 4 in table 5). This is driven by one subject who managed to solve a total of 37 sliders more than the second-best subject. Excluding this subject renders a positive, but insignificant (p=0.173) coefficient for the sum of solved sliders. In any case, self-selection cannot explain the fact that those with a higher bonus cheat more and it rather seems that more productive subjects were more honest.

Subjects are paid a small piece rate for the slider tasks, meaning that they were compensated slightly more for longer tasks. However, the main part of the total earnings for each task is made up of the bonus and the working time has no influence on the bonus. This means that in the 80 sec tasks, subjects had to work for the double length of time but earnt just about the same as in the 40 sec tasks. In all regressions in which all observations are included, the coefficient for the 80 sec tasks is significant and negative. This implies that subjects might have perceived the longer working time as unfair and might have felt that cheating was more justified, given that they had to exert more effort for similar earnings.[20] The coefficient for the period is also negative and significant in all regressions in which all observations are included, implying that subjects become more dishonest over time. Subjects' age has a significant and positive effect on the reported tax rate in all regressions in which all observations are included, which is in line with survey evidence finding that tax morale increases with age (Barth et al. 2013, Frey and Torgler 2007).[21]

---

[18] Erkal et al. (2011) show that those who earn the highest income in a contest give less than those who receive the second highest income and further show that this effect is driven by self-selection. Others find more productive workers to cheat more (Gill et al. 2013) or to be less pro-social (Buser and Dreber 2016) which might be driven by those who are more motivated by money.

[19] The total number of solved sliders in the luck treatments ranges from 79 to 248 with a mean of 133.5 sliders.

[20] Grundmann and Lambsdorff (2017) find that subjects cheat more in longer tasks. Bühren and Kundt (2014) show that subjects cheat more if they had to work hard than if they had to work moderately. In a survey, Barth et al. (2013) find that respondents are more likely to say they consider it justified to evade taxes if they work long hours with a low hourly wage.

[21] On the other hand, Abeler et al. (2016) find no effect of age in a meta-study on cheating.

Table 4 – Further regressions

	1 Luck treatments **Relative Expectation**	2 All treatments **Reported tax rate**	3 Treatment Perf-NC **Reported tax rate**	4 Luck treatments **Mean reported tax rate**
Bonus	-0.04	-2.21***	-2.72**	
	(0.03)	(0.34)	(0.94)	
Effort	0.26***	0.34**	-0.09	
	(0.02)	(0.17)	(0.40)	
Relative Expectation		-0.24		
		(0.15)		
Exp. bonus of others			0.28	
			(0.79)	
Total solved sliders				0.07**
				(0.03)
Female	-0.39*	2.09*	-0.61	2.77*
	(0.21)	(1.09)	(2.41)	(1.52)
Age		0.29*	0.58***	-0.02
		(0.15)	(0.18)	(0.29)
80 sec tasks		-2.63***	-3.33	
		(0.99)	(2.38	
Period		-0.19***	-0.12	
		(0.67)	(0.13)	
Constant	-1.81***	32.86***	35.21***	19.2***
	(0.22)	(1.74)	(3.54)	(4.33)
N	1792	3584	832	112
Overall R^2	0.268	0.032	0.056	
Adjusted R^2				0.031

Regressions 1, 2 and 3 are random effects, regression 4 is OLS.
Standard errors in parentheses (clustered by subject in regression 1, 2 and 3), * p<0.10, ** p<0.05, *** p<0.01
Notes: *Bonus* and *Expected bonus of others* divided by 100 before entering the regressions. *Age*: subjects' age minus 18.

A possible concern relates to the external validity, given that the subjects are students and might behave differently than the average taxpayer. In order to address this concern, subjects were asked whether they had already filed a real tax declaration, which 36.2% had. Including a dummy variable indicating this in regression 2 in table 3 does not produce a significant coefficient (p=0.901) and also does not impact the variable for the bonus. Running regression 2 separately for experienced taxpayers does not change the effect of the bonus. This contributes to the external validity of my findings.[22] Cappelen et al. (2010) and Cappelen et al. (2016) find that people who are politically more right-winged tend to hold people responsible for more factors that determine their income than people who are politically more left-winged, which then leads to different redistributive preferences. In the context of my experiment, this might imply that politically more left-winged subjects would aim at equalising payoffs more strongly, especially in the luck treatments, but possibly also in the performance treatments. I therefore ran regression 4 table 3 separately for more left-

[22] Dishonesty in the die-in-the-cup task has been shown to be linked with rule violations in societies (Gächter and Schulz 2016) and with fare dodging in public transport (Dai et al. 2018), supporting the external validity of this task.

winged and more right-winged subjects to assess whether there are any noteworthy differences between subjects with different political preferences in regard to the treatments. This was not the case. Including dummy variables for subjects' field of study in regression 2 in table 3 has no notable impact on the effect of the bonus. Controlling for the time of day of sessions also has no impact on the main results. Given that tax-paying experience, political preferences, fields of study and time of day have no impact, the results are not displayed.

7. Discussion and Conclusion

The size of the bonus has a significantly negative effect on the reported tax rate in all treatments. Not only the subjects in the performance treatments reported lower tax rates for higher bonuses, but subjects in the luck treatments acted in this way, too. Even though the size of the bonus was clearly not determined by a factor within subjects' control in the luck treatments, subjects did not aim at reducing inequality. Taken together, subjects across treatments did not act in accordance with just deserts. The effect of an increased bonus on cheating was largest in treatment Luck-C, which deserves some further attention. Compared to Luck-NC, subjects with the highest bonus in Luck-C knew for sure that they alone had the highest bonus in their group. They possibly wanted to seize the opportunity to make use of their good fortune, being reluctant to share this with others. On the other hand, while subjects receiving the highest bonus in Perf-C might have been confident to be able to win the highest bonus again, subjects in Luck-C might have seen their good fortune as temporary and might have wanted to capture their momentary luck. Both of these possible reasons might have led to a stronger effect of the bonus in treatment Luck-C.

My model assumed an overall desire for equalising payoffs in the luck treatments and an overall desire not to do so in the performance treatments, thus an overall agreement on a just deserts fairness ideal. However, fairness ideals might be heterogeneous across subjects. Some subjects might be egalitarian types, with a preference for equalising payoffs regardless of the source of income. Others might be libertarian types, deeming all factors that determine income relevant for a fair distribution.[23] But even heterogeneous fairness ideals cannot explain my results. Assuming that types are distributed evenly across treatments, the presence of just deserts types would induce opposing effects of the bonus

[23] While my design does not allow me to identify types, I can take a look at how many subjects in the luck treatments act in accordance with just deserts. 29 out of 112 subjects (26%) show a positive correlation coefficient between tax rates and bonus levels, which is significant for 3 of these subjects. Thus, only a minority of subjects acts in accordance with just deserts in the luck treatments.

in the luck and performance treatments, because these types would act like egalitarians in the luck treatments and like libertarians in the performance treatments.

Another possibility is that subjects applied fairness principles in a self-serving way, applying the fairness ideal that suits them most from a self-serving perspective (Deffains et al. 2016, Rodriguez-Lara and Moreno-Garrido 2012, Rutström and Williams 2000). However, while this would explain that subjects receiving a high bonus act like libertarians in all treatments, it does not explain that subjects with a low bonus act this way, too. If a self-serving bias was to explain the data, low earners should have also cheated to a large extent, self-servingly applying an egalitarian fairness ideal.

Instead, subjects do not seem to distinguish between income received due to performance or due to luck. Overall, they seem to have acted like libertarians, resisting an equalisation of payoffs regardless of the source of income. Other studies estimate the share of libertarians to be below $\frac{1}{3}$ (Almas et al. 2016, Cappelen et al. 2007, Cappelen et al. 2010). In contrast to these studies, in my design, subjects can disguise their self-serving actions as a random event and know that individual cheating cannot be detected. This seems to have induced subjects to follow their own personal interests and not try to reduce inequality, even if they were not responsible for their high bonus and thus should not have felt entitled to it. However, subjects also do not make full use of the possibility to cheat but instead seem to be driven to a certain degree by a desire for honesty. When subjects receive a low bonus, the financial gain seems to compensate the intrinsic costs of lying only to a limited extent, leading to higher reported tax rates. On the other hand, receiving a high bonus induces more cheating, implying that the financial gain from a larger lie over-compensates the increased costs of lying. This logic does not seem to be counteracted by a desire for equalising payoffs, implying that subjects felt entitled to their high bonus in all treatments. Therefore, I find no evidence for just deserts in a situation which allows subjects to disguise their self-serving behaviour as a random event. Just deserts might be a fairness ideal that people think they should hold or would like to appear to hold, but which they cast aside as soon as they think they can get away with it. This interpretation is in line with the effects of moral wiggle room reported by Dana et al. (2007) and Regner (2017).

Future research could possibly explore whether and how the design could be modified such that subjects act according to just deserts. On the one hand, it might be interesting to explore the effects if subjects are not affected by the reported tax rates themselves. This would be the case if one subject reported tax rates for a subject in another group, for example. On the other hand, it might even be possible to induce subjects to act according to just deserts if they make decisions for themselves. This could possibly be achieved by asking subjects upfront what behaviour or system of taxation would be fair, hereby possibly inducing a commitment to less self-serving behaviour.

There is some evidence that men report lower tax rates in the competition treatments than in the no-competition treatments, implying that competition might crowd-out honesty. This effect, however, marginally misses significance in the regressions. The finding that women are more honest in the competition treatments than in the no-competition treatments might be seen in the light of studies on gender effects in the reaction to and preference for competition (Croson and Gneezy 2009, Gneezy et al. 2003, Gneezy et al. 2009, Niederle and Vesterlund 2007, Saccardo et al. 2018). From a policy perspective, this would mean that it is important to pay attention to possible diverging effects of competitive incentives on cheating for women and men. The findings suggests that tax authorities should pay special auditing attention to income earnt by men in a competitive environment.

All in all, tax authorities would be well advised to audit higher incomes thoroughly, regardless of the way in which this income is achieved. For these incomes, the temptation to cheat is largest. Policy makers could further try to devise programmes especially aimed at encouraging or nudging high-earning individuals to pay their taxes honestly. Despite the fact that just deserts do not shape patterns of cheating, this does not necessarily mean that people will not support redistributive policies that follow this ideal. Possibly, redistributive policies based on such a fairness ideal will induce the least resistance, but still need to be enforced externally, especially if a situation allows people to disguise their self-serving actions.

Acknowledgements

I am especially grateful to Johann Graf Lambsdorff for his continuous support and advice as well as many suggestions, ideas and comments during the entire project. A special thank also goes to Katharina Werner and Marcus Giamattei for their helpful comments and support. I am grateful to participants of the GfeW conference 2018 in Paderborn and the Thurgau Experimental Economics Meeting 2019. I would also like to thank the Bavarian Graduate Programme in Economics for supporting this research through the funds for the "BGPE Doctoral Student" status.

References

Abeler, Johannes; Nosenzo, Daniele; Raymond, Collin (2016): Preferences for Truth-Telling. *CESifo Working Paper* No. 6087.

Adams, James Truslow (1943): The Epic of America. Boston: Little, Brown, and Company.

Akbas, Merve; Ariely, Dan; Yuksel, Sevgi (2014): When Is Inequality Fair? An Experiment on the Effect of Procedural Justice and Agency. *SSRN Journal*. DOI: 10.2139/ssrn.2474368.

Alesina, Alberto; Angeletos, George-Marios (2005): Fairness and Redistribution. *American Economic Review* 95 (4), 960–980. DOI: 10.1257/0002828054825655.

Alesina, Alberto; Glaeser, Edward; Sacerdote, Bruce (2001): Why Doesn't the United States Have a European-Style Welfare State? *Brookings Papers on Economics* (2), 187–277. DOI: 10.3386/w8524.

Allingham, Michael G.; Sandmo, Agnar (1972): Income tax evasion: a theoretical analysis. *Journal of Public Economics* 1 (3-4), 323–338. DOI: 10.1016/0047-2727(72)90010-2.

Alm, James (2018): WHAT MOTIVATES TAX COMPLIANCE? *Journal of Economic Surveys* 15 (2), p. 245. DOI: 10.1111/joes.12272.

Alm, James; Jackson, Betty R.; McKee, Michael (1992): Estimating the determinants of taxpayer compliance with experimental data. *National Tax Journal* 45 (1), 107–114.

Almas, Ingvild; Cappelen, Alexander W.; Tungodden, Bertil (2016): Cutthroat Capitalism Versus Cuddly Socialism: Are Americans More Meritocratic and Efficiency-Seeking than Scandinavians? *SSRN Journal*. DOI: 10.2139/ssrn.2879358.

Anderhub, Vital; Giese, Sebastian; Güth, Werner; Hoffmann, Antje; Otto, Thomas (2001): Tax Evasion with Earned Income - An Experimental Study. *FinanzArchiv* 58 (2), 188–206.

Balafoutas, Loukas; Lindner, Florian; Sutter, Matthias (2012): Sabotage in Tournaments: Evidence from a Natural Experiment. *Kyklos* 65 (4), 425–441. DOI: 10.1111/kykl.12000.

Barth, Erling; Cappelen, Alexander W.; Ognedal, Tone (2013): Fair Tax Evasion. *SSRN Journal*. DOI: 10.2139/ssrn.2262192.

Bartling, Björn; Cappelen, Alexander W.; Ekstrrm, Mathias; Sorensen, Erik; Tungodden, Bertil (2018): Fairness in Winner-Take-All Markets. *SSRN Journal*. DOI: 10.2139/ssrn.3175189.

Batson, C. Daniel; Kobrynowicz, Diane; Dinnerstein, Jessica L.; Kampf, Hannah C.; Wilson, Angela D. (1997): In a very different voice: Unmasking moral hypocrisy. *Journal of personality and social psychology* 72 (6), 1335–1348. DOI: 10.1037/0022-3514.72.6.1335.

Bénabou, R.; Tirole, J. (2006): Belief in a Just World and Redistributive Politics. *The Quarterly Journal of Economics* 121 (2), 699–746. DOI: 10.1162/qjec.2006.121.2.699.

Bolton, Gary E. (1991): A Comparative Model of Bargaining: Theory and Evidence. *American Economic Review* 81 (5), 1096–1136.

Bolton, Gary E.; Ockenfels, Axel (2000): ERC: A Theory of Equity, Reciprocity, and Competition. *American Economic Review* 90 (1), 166–193. DOI: 10.1257/aer.90.1.166.

Boylan, Scott J. (2010): Prior Audits and Taxpayer Compliance: Experimental Evidence on the Effect of Earned Versus Endowed Income. *The Journal of the American Taxation Associatio*n 32 (2), 73–88. DOI: 10.2308/jata.2010.32.2.73.

Boylan, Scott J.; Sprinkle, Geoffrey B. (2001): Experimental Evidence on the Relation between Tax Rates and Compliance: The Effect of Earned vs. Endowed Income. *The Journal of the American Taxation Association* 23 (1), 75–90. DOI: 10.2308/jata.2001.23.1.75.

Buckley, Edward; Croson, Rachel (2006): Income and wealth heterogeneity in the voluntary provision of linear public goods. *Journal of Public Economics* 90 (4-5), 935–955. DOI: 10.1016/j.jpubeco.2005.06.002.

Bühren, Christoph; Kundt, Thorben C. (2014): Does the Level of Work Effort Influence Tax Evasion? Experimental Evidence. *Review of Economics* 65 (2), p. 341. DOI: 10.1515/roe-2014-0203.

Buser, Thomas; Dreber, Anna (2016): The Flipside of Comparative Payment Schemes. *Management Science* 62 (9), 2626–2638. DOI: 10.1287/mnsc.2015.2257.

Cappelen, Alexander W.; Halvorsen, Trond; Sørensen, Erik Ø.; Tungodden, Bertil (2017): Face-saving or fair-minded: What motivates moral behavior? *Journal of the European Economic Association* 15 (3), 540–557. DOI: 10.1093/jeea/jvw014.

Cappelen, Alexander W.; Hole, Astri Drange; Sørensen, Erik Ø.; Tungodden, Bertil (2007): The Pluralism of Fairness Ideals: An Experimental Approach. *American Economic Review* 97 (3), 818–827. DOI: 10.1257/aer.97.3.818.

Cappelen, Alexander W.; Konow, James; Sørensen, Erik Ø.; Tungodden, Bertil (2013a): Just Luck: An Experimental Study of Risk-Taking and Fairness. *American Economic Review* 103 (4), 1398–1413. DOI: 10.1257/aer.103.4.1398.

Cappelen, Alexander W.; Moene, Karl O.; Sørensen, Erik Ø.; Tungodden, Bertil (2013b): NEEDS VERSUS ENTITLEMENTS-AN INTERNATIONAL FAIRNESS EXPERIMENT. *Journal of the European Economic Association* 11 (3), 574–598. DOI: 10.1111/jeea.12000.

Cappelen, Alexander W.; Fest, Sebastian; Sørensen, Erik Ø.; Tungodden, Bertil (2016): Choice and personal responsibility: What is a morally relevant choice? *Working Paper*.

Cappelen, Alexander W.; Sørensen, Erik Ø.; Tungodden, Bertil (2010): Responsibility for what? Fairness and individual responsibility. *European Economic Review* 54 (3), 429–441. DOI: 10.1016/j.euroecorev.2009.08.005.

Carlsson, Fredrik; He, Haoran; Martinsson, Peter (2013): Easy come, easy go. *Experimental Economics* 16 (2), 190–207. DOI: 10.1007/s10683-012-9326-8.

Carpenter, Jeffrey; Liati, Allison; Vickery, Brian (2010): They Come To Play. *Rationality and Society* 22 (1), 83–102. DOI: 10.1177/1043463109358486.

Charness, Gary; Masclet, David; Villeval, Marie Claire (2014): The Dark Side of Competition for Status. *Management Science* 60 (1), 38–55.

Cherry, Todd L.; Frykblom, Peter; Shogren, Jason F. (2002): Hardnose the Dictator. *American Economic Review* 92 (4), 1218–1221. DOI: 10.1257/00028280260344740.

Cherry, Todd L.; Kroll, Stephan; Shogren, Jason F. (2005): The impact of endowment heterogeneity and origin on public good contributions. Evidence from the lab. *Journal of Economic Behavior & Organization* 57 (3), 357–365. DOI: 10.1016/j.jebo.2003.11.010.

Conrads, Julian; Irlenbusch, Bernd; Rilke, Rainer Michael; Schielke, Anne; Walkowitz, Gari (2014): Honesty in tournaments. *Economics Letters* 123 (1), 90–93. DOI: 10.1016/j.econlet.2014.01.026.

Coricelli, Giorgio; Joffily, Mateus; Montmarquette, Claude; Villeval, Marie Claire (2010): Cheating, emotions, and rationality: an experiment on tax evasion. *Experimental Economics* 13 (2), 226–247. DOI: 10.1007/s10683-010-9237-5.
Coricelli, Giorgio; Rusconi, Elena; Villeval, Marie Claire (2014): Tax evasion and emotions: An empirical test of re-integrative shaming theory. *Journal of Economic Psychology* 40, 49–61. DOI: 10.1016/j.joep.2012.12.002.
Croson, Rachel; Gneezy, Uri (2009): Gender Differences in Preferences. *Journal of Economic Literature* 47 (2), 1-27.
Dai, Zhixin; Galeotti, Fabio; Villeval, Marie Claire (2018): Cheating in the Lab Predicts Fraud in the Field: An Experiment in Public Transportation. *Management Science* 64 (3), 1081-1100.
Dana, Jason; Weber, Roberto A.; Kuang, Jason Xi (2007): Exploiting moral wiggle room: experiments demonstrating an illusory preference for fairness. *Economic Theory* 33 (1), 67–80. DOI: 10.1007/s00199-006-0153-z.
Dato, Simon; Nieken, Petra (2014): Gender differences in competition and sabotage. *Journal of Economic Behavior & Organization* 100, 64–80. DOI: 10.1016/j.jebo.2014.01.012.
Deffains, Bruno; Espinosa, Romain; Thöni, Christian (2016): Political self-serving bias and redistribution. *Journal of Public Economics* 134, 67–74. DOI: 10.1016/j.jpubeco.2016.01.002.
Deutscher, Christian; Frick, Bernd; Gürtler, Oliver; Prinz, Joachim (2013): Sabotage in Tournaments with Heterogeneous Contestants: Empirical Evidence from the Soccer Pitch. *The Scandinavian Journal of Economics* 115 (4), 1138–1157. DOI: 10.1111/sjoe.12036.
Dohmen, Thomas; Falk, Armin; Fliessbach, Klaus; Sunde, Uwe; Weber, Bernd (2011): Relative versus absolute income, joy of winning, and gender. Brain imaging evidence. *Journal of Public Economics* 95 (3-4), 279–285. DOI: 10.1016/j.jpubeco.2010.11.025.
Dufwenberg, Martin Jr.; Dufwenberg, Martin Sr. (2016): Lies in Disguise – A Theoretical Analysis of Cheating. *CESIFO Working Paper No. 6208*
Durham, Yvonne; Manly, Tracy S.; Ritsema, Christina (2014): The effects of income source, context, and income level on tax compliance decisions in a dynamic experiment. *Journal of Economic Psychology* 40, 220–233. DOI: 10.1016/j.joep.2012.09.012.
Eckel, Catherine; Philip J. Grossman and Angela Milano (2007): Is More Information Always Better? An Experimental Study of Charitable Giving and Hurricane Katrina. *Southern Economic Journal* 74 (2), 388–411. DOI: 10.2307/20111974.
Erkal, Nisvan; Gangadharan, Lata; Nikiforakis, Nikos (2011): Relative Earnings and Giving in a Real-Effort Experiment. *American Economic Review* 101 (7), 3330–3348. DOI: 10.1257/aer.101.7.3330.
Fahr, René; Irlenbusch, Bernd (2000): Fairness as a constraint on trust in reciprocity: earned property rights in a reciprocal exchange experiment. *Economics Letters* 66 (3), 275–282. DOI: 10.1016/S0165-1765(99)00236-0.
Fehr, E.; Schmidt, K. M. (1999): A Theory of Fairness, Competition, and Cooperation. *The Quarterly Journal of Economics* 114 (3), 817–868. DOI: 10.1162/003355399556151.
Feltovich, Nick (2018): The interaction between competition and unethical behaviour. *Experimental Economics* 22 (1), 101–130. DOI: 10.1007/s10683-018-9578-z.
Fischbacher, Urs (2007): z-Tree: Zurich toolbox for ready-made economic experiments. *Experimental Economics* 10 (2), 171–178. DOI: 10.1007/s10683-006-9159-4.
Fischbacher, Urs; Föllmi-Heusi, Franziska (2013): LIES IN DISGUISE-AN EXPERIMENTAL STUDY ON CHEATING. *Journal of the European Economic Association* 11 (3), 525–547. DOI: 10.1111/jeea.12014.
Fochmann, Martin; MMller, Nadja; Overesch, Michael (2018): Less Cheating? The Effects of Prefilled Forms on Compliance Behavior. *SSRN Journal*. DOI: 10.2139/ssrn.3206410.
Fong, Christina M. (2007): Evidence from an Experiment on Charity to Welfare Recipients: Reciprocity, Altruism and the Empathic Responsiveness Hypothesis. *Economic Journal* 117 (522), 1008–1024. DOI: 10.1111/j.1468-0297.2007.02076.x.

Fortin, Bernard; Lacroix, Guy; Villeval, Marie-Claire (2007): Tax evasion and social interactions. *Journal of Public Economics* 91 (11), 2089–2112. DOI: 10.1016/j.jpubeco.2007.03.005.

Frey, Bruno S.; Torgler, Benno (2007): Tax morale and conditional cooperation. *Journal of Comparative Economics* 35 (1), 136–159. DOI: 10.1016/j.jce.2006.10.006.

Frohlich, Norman; Oppenheimer, Joe; Kurki, Anja (2004): Modeling other-regarding preferences and an experimental test. *Public Choice* 119, 91-117.

Gächter, Simon; Schulz, Jonathan F. (2016): Intrinsic honesty and the prevalence of rule violations across societies. *Nature* 531, 496 EP -. DOI: 10.1038/nature17160.

Galeotti, Fabio; Kline, Reuben; Orsini, Raimondello (2017): When foul play seems fair: Exploring the link between just deserts and honesty. *Journal of Economic Behavior & Organization* 142, 451–467. DOI: 10.1016/j.jebo.2017.08.007.

Gee, Laura K.; Migueis, Marco; Parsa, Sahar (2017): Redistributive choices and increasing income inequality. Experimental evidence for income as a signal of deservingness. *Experimental Economics* 20 (4), 894–923. DOI: 10.1007/s10683-017-9516-5.

Gerlach, Philipp; Teodorescu, Kinneret; Hertwig, Ralph (2019): The truth about lies: A meta-analysis on dishonest behavior. *Psychological bulletin* 145 (1), 1–44. DOI: 10.1037/bul0000174.

Gibson, Rajna; Tanner, Carmen; Wagner, Alexander F. (2013): Preferences for Truthfulness: Heterogeneity among and within Individuals. *American Economic Review* 103 (1), 532–548. DOI: 10.1257/aer.103.1.532.

Gill, David; Prowse, Victoria (2012): A Structural Analysis of Disappointment Aversion in a Real Effort Competition. *American Economic Review* 102 (1), 469–503. DOI: 10.1257/aer.102.1.469.

Gill, David; Prowse, Victoria; Vlassopoulos, Michael (2013): Cheating in the workplace. An experimental study of the impact of bonuses and productivity. *Journal of Economic Behavior & Organization* 96, 120–134. DOI: 10.1016/j.jebo.2013.09.011.

Gino, Francesca; Ayal, Shahar; Ariely, Dan (2009): Contagion and differentiation in unethical behavior: the effect of one bad apple on the barrel. *Psychological science* 20 (3), 393–398. DOI: 10.1111/j.1467-9280.2009.02306.x.

Gneezy, Uri; Niederle, Muriel; Rustichini, Aldo (2003): Performance in Competitive Environments: Gender Differences. *The Quarterly Journal of Economics* 118, 1049-1074.

Gneezy, Uri (2005): Deception: The Role of Consequences. *American Economic Review* 95 (1), 384–394. DOI: 10.1257/0002828053828662.

Gneezy, Uri; Leonard, Kenneth L.; List, John A. (2009): Gender Differences in Competition: Evidence from a Matrilineal and a Patrilineal Society. *Econometrica* 77 (5), 1637-1664.

Gneezy, Uri; Kajackaite, Agne; Sobel, Joel (2018): Lying Aversion and the Size of the Lie. *American Economic Review* 108 (2), 419–453. DOI: 10.1257/aer.20161553.

Gneezy, Uri; Rockenbach, Bettina; Serra-Garcia, Marta (2013): Measuring lying aversion. *Journal of Economic Behavior & Organization* 93, 293–300. DOI: 10.1016/j.jebo.2013.03.025.

Gravert, Christina (2013): How luck and performance affect stealing. *Journal of Economic Behavior & Organization* 93, 301–304. DOI: 10.1016/j.jebo.2013.03.026.

Greiner, Ben (2015): Subject pool recruitment procedures: organizing experiments with ORSEE. *Journal of the Economic Science Association* 1 (1), 114–125. DOI: 10.1007/s40881-015-0004-4.

Grosch, Kerstin; Marcela Ibanez; Angelino Viceisza (2017): Competition and prosociality: A field experiment in Ghana. *GlobalFood Discussion Papers* (No. 113).

Grundmann, Susanna; Graf Lambsdorff, Johann (2017): How income and tax rates provoke cheating – An experimental investigation of tax morale. *Journal of Economic Psychology* 63, 27–42. DOI: 10.1016/j.joep.2017.10.003.

Harbring, Christine; Irlenbusch, Bernd (2008): How many winners are good to have?: On tournaments with sabotage. *Journal of Economic Behavior & Organization* 65 (3), 682–702. DOI: 10.1016/j.jebo.2006.03.004.

Harbring, Christine; Irlenbusch, Bernd; Kräkel, Matthias; Selten, Reinhard (2007): Sabotage in Corporate Contests - An Experimental Analysis. *International Journal of the Economics of Business* 14 (3), 367–392.

Heinemann, Friedrich; Kocher, Martin G. (2013): Tax compliance under tax regime changes. *International Tax and Public Finance* 20 (2), 225–246. DOI: 10.1007/s10797-012-9222-3.

Hoffman, Elizabeth; McCabe, Kevin; Shachat, Keith; Smith, Vernon (1994): Preferences, Property Rights, and Anonymity in Bargaining Games. *Games and Economic Behavior* 7 (3), 346–380. DOI: 10.1006/game.1994.1056.

Hoffman, Elizabeth; Spitzer, Matthew L. (1985): Entitlements, Rights, and Fairness. An Experimental Examination of Subjects' Concepts of Distributive Justice. *The Journal of Legal Studies* 14 (2), 259–297. DOI: 10.1086/467773.

Jakiela, Pamela (2015): How fair shares compare: Experimental evidence from two cultures. *Journal of Economic Behavior & Organization* 118, 40–54. DOI: 10.1016/j.jebo.2015.02.017.

Kajackaite, Agne (2018): Lying about luck versus lying about performance. *Journal of Economic Behavior & Organization* 153, 194–199. DOI: 10.1016/j.jebo.2018.07.010.

Kajackaite, Agne; Gneezy, Uri (2017): Incentives and cheating. *Games and Economic Behavior* 102, 433–444. DOI: 10.1016/j.geb.2017.01.015.

Kirchler, Erich; Muehlbacher, Stephan; Hoelzl, Erik; Webley, Paul (2009): Effort and Aspirations in Tax Evasion: Experimental Evidence. *Applied Psychology* 58 (3), 488–507. DOI: 10.1111/j.1464-0597.2009.00403.x.

Konow, James (1996): A positive theory of economic fairness. *Journal of Economic Behavior & Organization* 31 (1), 13–35. DOI: 10.1016/S0167-2681(96)00862-1.

Konow, James (2000): Fair Shares. Accountability and Cognitive Dissonance in Allocation Decisions. *American Economic Review* 90 (4), 1072–1092. DOI: 10.1257/aer.90.4.1072.

Konow, James (2003): Which Is the Fairest One of All? A Positive Analysis of Justice Theories. *Journal of Economic Literature* 41 (4), 1188–1239. DOI: 10.1257/002205103771800013.

Ku, Hyejin; Salmon, Timothy C. (2013): Procedural fairness and the tolerance for income inequality. *European Economic Review* 64, 111–128. DOI: 10.1016/j.euroecorev.2013.09.001.

Lefgren, Lars J.; Sims, David P.; Stoddard, Olga B. (2016): Effort, luck, and voting for redistribution. *Journal of Public Economics* 143, 89–97. DOI: 10.1016/j.jpubeco.2016.08.012.

List, John A.; Sadoff, Sally; Wagner, Mathis (2011): So you want to run an experiment, now what? Some simple rules of thumb for optimal experimental design. *Experimental Economics* 14 (4), 439–457. DOI: 10.1007/s10683-011-9275-7.

Malézieux, Antoine (2018): Review – A Practical Guide to Setting up your Tax Evasion Game. *Journal of Tax Administration* 4 (1), 107-127.

Mazar, Nina; Amir, On; Ariely, Dan (2008): The Dishonesty of Honest People: A Theory of Self-Concept Maintenance. *Journal of Marketing Research* 45 (6), 633–644. DOI: 10.1509/jmkr.45.6.633.

Miller, David (1976): Social Justice. Oxford: Oxford University Press.

Moffatt, Peter G. (2016): Experimetrics. Econometrics for experimental economics. London, New York: Macmillan Education Palgrave.

Muehlbacher, Stephan; Kirchler, Erich (2008): Arbeitsaufwand, Anspruchsniveau und Steuerehrlichkeit. *Zeitschrift für Arbeits- und Organisationspsychologie A&O* 52 (2), 91–96. DOI: 10.1026/0932-4089.52.2.91.

Niederle, Muriel; Vesterlund, Lise (2007): Do women shy away from competition? Do men compete too much? *The Quarterly Journal of Economics* 122, 1067-1101.

Oxoby, Robert J.; Spraggon, John (2008): Mine and yours: Property rights in dictator games. *Journal of Economic Behavior & Organization* 65 (3-4), 703–713. DOI: 10.1016/j.jebo.2005.12.006.

Pántya, József; Kóvacs, Judit; Kogler, Christoph; Kirchler, Erich (2016): Work performance and tax compliance in flat and progressive tax systems. *Journal of Economic Psychology* 56, 262-273.

Preston, Ian; Szymanski, Stefan (2003): Cheating in Contests. *Oxford Review of Economic Policy* 19 (4), 612-624.

Rapoport, Amnon; Bornstein, Gary; Erev, Ido (1989): Intergroup competition for public goods: Effects of unequal resources and relative group size. *Journal of personality and social psychology* 5 (56), 748–756.

Rapoport, Amnon; Suleiman, Ramzi (1993): Incremental Contribution in Step-Level Public Goods Games with Asymmetric Players. *Organizational Behavior and Human Decision Processes* 55 (2), 171–194. DOI: 10.1006/obhd.1993.1029.

Regner, Tobias (2017): Reciprocity under moral wiggle room: Is it a preference or a constraint? *Experimental Economics* 21 (4), 779–792. DOI: 10.1007/s10683-017-9551-2.

Robson, Arthur J. (1992): Status, the Distribution of Wealth, Private and Social Attitudes to Risk. *Econometrica* 60 (4), 837-857.

Rodriguez-Lara, Ismael; Moreno-Garrido, Luis (2012): Self-interest and fairness. Self-serving choices of justice principles. *Experimental Economics* 15 (1), 158–175. DOI: 10.1007/s10683-011-9295-3.

Roemer, John E.; Aaberge, Rolf; Colombino, Ugo; Fritzell, Johan; Jenkins, Stephen P.; Lefranc, Arnaud; Marx, Ive; Page, Marianne; Pommer, Evert; Ruiz-Castillo, Javier; San Segundo, Maria Jesus; Tranaes, Torben; Trannoy, Alain; Wagner, Gert G.; Zubiri, Ignacio (2003): To what extent do fiscal regimes equalize opportunities for income acquisition among citizens. *Journal of Public Economics* 87, 539-565.

Rutström, E. Elisabet; Williams, Melonie B. (2000): Entitlements and fairness: an experimental study of distributive preferences. *Journal of Economic Behavior & Organization* 43 (1), 75–89. DOI: 10.1016/S0167-2681(00)00109-8.

Saccardo, Silvia; Pietrasz, Aniela; Gneezy, Uri (2018): On the Size of the Gender Difference in Competitiveness. *Management Science* 64 (4), 1541-1554.

Schurr, Amos; Ritov, Ilana (2016): Winning a competition predicts dishonest behavior. *Proceedings of the National Academy of Sciences* 113 (7), 1754–1759. DOI: 10.1073/pnas.1515102113.

Schwieren, Christiane; Weichselbaumer, Doris (2010): Does competition enhance performance or cheating? A laboratory experiment. *Journal of Economic Psychology* 31 (3), 241–253. DOI: 10.1016/j.joep.2009.02.005.

Shalvi, Shaul; Dana, Jason; Handgraaf, Michel J.J.; Dreu, Carsten K.W. de (2011a): Justified ethicality: Observing desired counterfactuals modifies ethical perceptions and behavior. *Organizational Behavior and Human Decision Processes* 115 (2), 181–190. DOI: 10.1016/j.obhdp.2011.02.001.

Shalvi, Shaul; Gino, Francesca; Barkan, Rachel; Ayal, Shahar (2015): Self-Serving Justifications. *Current Directions Psychological Science* 24 (2), 125–130. DOI: 10.1177/0963721414553264.

Shalvi, Shaul; Handgraaf, Michel J. J.; Dreu, Carsten K.W. de (2011b): Ethical Manoeuvring: Why People Avoid Both Major and Minor Lies. *British Journal of Management* 22, S16-S27. DOI: 10.1111/j.1467-8551.2010.00709.x.

Sheremeta, Roman M. (2010): Experimental comparison of multi-stage and one-stage contests. *Games and Economic Behavior* 68 (2), 731–747. DOI: 10.1016/j.geb.2009.08.001.

Siniver, Erez; Yaniv, Gideon (2018): Losing a real-life lottery and dishonest behavior. *Journal of Behavioral and Experimental Economics* 75, 26–30. DOI: 10.1016/j.socec.2018.05.005.

Sutter, Matthias (2009): Deception Through Telling the Truth?! Experimental Evidence from Individuals and Teams. *Economic Journal* 119 (534), 47–60. DOI: 10.1111/j.1468-0297.2008.02205.x.

Ter Meer, Janna, (2014): The indirect effect of monetary incentives on deception. No 05-04, *Cologne Graduate School Working Paper Series*, Cologne Graduate School in Management, Economics and Social Sciences.

Weisel, Ori; Shalvi, Shaul (2015): The collaborative roots of corruption. *Proceedings of the National Academy of Sciences* 112 (34), 10651–10656. DOI: 10.1073/pnas.1423035112.

Appendix A: Instructions

Oral instructions (translated from German)

[Oral instructions were read pre-recorded prior to the first session and played out lout before the experiment started.]

Welcome to the experiment! Before the experiment starts, you will receive some general instructions: Please listen carefully and don't click on the button "Start Experiment" until the end of these instructions.

The experiment aims at gaining insights on human behaviour. All participants are here in this room and are participating in the same experiment.

You can earn money in this experiment. The amount depends on your decisions as well as the decisions of the other participants. In the experiment, you will earn money in the currency Taler. 25 Taler equal 1€. In any case, you will receive at least 3.50€ for participating in this experiment.

All participants are anonymous and cannot communicate with one another. The decisions you make cannot be traced to you personally.

The disbursement of payoffs will also be carried out anonymously: no other participant will be able to see how much money you receive and the experimenters won't find this out either. The person who will pay out the money at the end of the experiment does not know the experiment and, therefore, cannot infer your decisions from your payoff.

Please note that you may have to wait for other participants during the experiment. Please remain seated quietly at your desk during the entire course of the experiment and do not talk to other participants. If you do not comply with these rules, you may be excluded from the further participation in this experiment.

If you have any questions please raise your hand. Someone will then come to your desk.

Please click on "Start Experiment" now.

[End of oral instructions]

Written instructions (translated from German)

On screen:

Thank you for your participation in this experiment!

You now have 300 seconds to read the instructions lying on your desk. Please read the instructions carefully. After you have read the instructions, you will be asked comprehension questions and you can earn money for answering them correctly. After the 300 seconds have passed, you will be directly redirected to the comprehension questions. The remaining time is displayed at the top right edge of the screen.

Please place the slider on 50 when you are asked to do so in the instructions.

[Slider displayed here, test below displayed when slider was placed on 50]

You have placed the slider correctly. Please now read the rest of the instructions.

On paper:

Instructions are identical across treatments apart from Step 2: bonus. For this part, all versions are given.

<u>General instructions</u>

The experiment lasts for 16 rounds. At the beginning of each round, you are randomly matched to three other players. These can be different players in each round.

Each round consists of three steps that will be explained in more detail in the following:

<u>Step 1: task</u>

In each round, your tasks consists of placing sliders on position 50.

For each slider placed on position 50, you will receive a work income of 5 Talers. The working time equals 80 seconds in the first round, 40 seconds in the second rounds, 80 seconds in the third round, 40 seconds in the fourth round and so on. The remaining working time will always be displayed at the top right edge of the screen during the task. The working time is always the same for all players.

On your screen, you can see a slider. Please place the slider on position 50 as practise. The current position is displayed on the right.

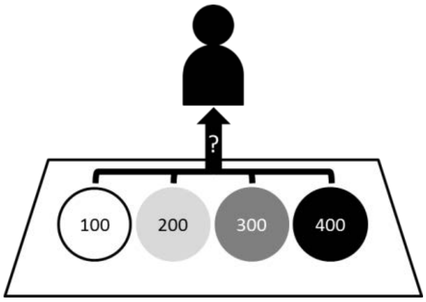
<u>Step 2: bonus</u> *(each treatments' version given separately)*

In addition to your work income, you will receive a bonus after working on the task which amounts to either 400 Taler, 300 Taler, 200 Taler or 100 Taler.

As already mentioned, you will be randomly matched to three other players each round. These can be different players each round.

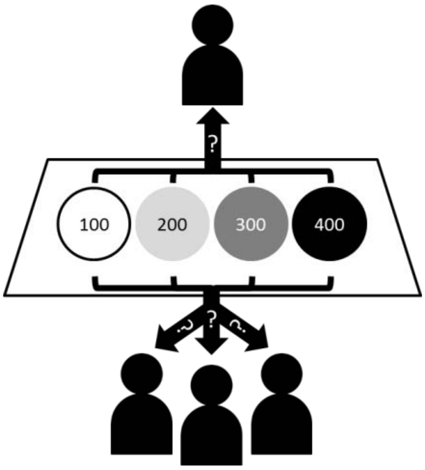
Luck-NC: The computer places a black ball, a dark grey ball, a light grey ball and a white ball in an urn. After that, the computer randomly draws one of the four balls from the urn. If the black ball is drawn, you receive 400 Taler as a bonus. If the dark grey ball is drawn, you receive 300 Taler as a bonus. If the light grey ball is drawn, you receive 200 Taler as a bonus. If a white ball is drawn, you receive 100 Taler as a bonus. All four balls are in the urn in each round.

You and the three other players each receive an own urn from which a ball is randomly drawn. Independent of which ball is drawn for you, it is thus possible that the three other players each receive the same, a higher or a lower bonus, as displayed in the picture below.



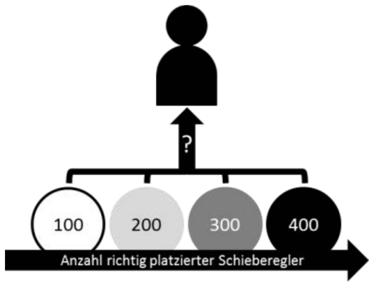
Luck-C: The computer places a black ball, a dark grey ball, a light grey ball and a white ball in an urn. After that, the computer randomly draws one of the four balls from the urn each for you and the three other players. The player for whom the black ball is drawn receives 400 Taler as a bonus. The player for whom the dark grey ball is drawn receives 300 Taler as a bonus. The player for whom the light grey ball is drawn receives 200 Taler as a bonus. The player for whom the white ball is drawn receives 100 Taler as a bonus. All four balls are in the urn in each round.

You and the three other players together receive one urn, from which a ball is randomly drawn. Thus, each round, one player receives the bonus of 400 Taler, one player receives the bonus of 300 Taler, one player receives the bonus of 200 Taler and one player receives the bonus of 100 Taler, as displayed in the picture below.



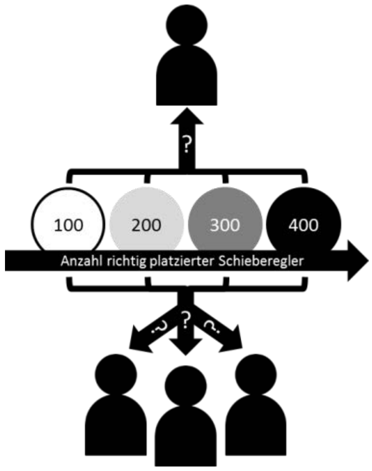
Perf-NC: Your bonus is connected to your performance in the task. Each round, there are three target values. If you place at least as many sliders correctly as indicated by the highest target value, you receive 400 Taler as a bonus. If you place at least as many sliders correctly as indicated by the middle target value, you receive 300 Taler as a bonus. If you place at least as many sliders correctly as indicated by the lowest target value, you receive 200 Taler as a bonus. If you do not place at least as many sliders correctly as indicated by the lowest target value, you receive 100 Taler as a bonus. In each round, you will be informed about the size of the target values right before the start of the task. The size of the target values varies depending on the length of the task and over the rounds.

The same target values apply to you and the three other players. Independent of whether you reach the target values, it is thus possible that the three other players each receive the same, a higher or a lower bonus, as displayed in the picture below.



Perf-C: Your bonus is connected to your performance in the task. The player who places the most sliders correctly receives 400 Taler as a bonus. The player who places the second-most sliders correctly receives 300 Taler as a bonus. The player who places the third-most sliders correctly receives 200 Taler as a bonus. The player who places the least sliders correctly receives 100 Taler as a bonus. In case of a tie, the distance to 50 of the best not correctly placed slider is decisive.

Thus, each round, one player receives the bonus of 400 Taler, one player receives the bonus of 300 Taler, one player receives the bonus of 200 Taler and one player receives the bonus of 100 Taler, as displayed in the picture below.



Step 3: tax declaration

You have to pay taxes on the bonus. For this, you fill in a tax declaration at the end of each round. You determine the tax rate that you have to pay for your bonus in a specific round by rolling the provided die.

For this, roll the die in the sealed cup and look through the hole in the top of the cup to see which number you rolled. Then look up the tax rate in the table provided below and enter this into the tax declaration:

Number on die	Tax rate
1	10%
2	20%
3	30%
4	40%
5	50%
6	60%

The table above will be displayed to you while you fill in the tax declaration.

The resulting tax will be deducted from your bonus in the respective round. The taxes deducted from you and the taxes deducted from the three other players are divided by four in the respective round and evenly redistributed to you and the three other players.

Please note that you only have to pay taxes on your bonus and not on the work income earnt directly in the task.

Payoff

At the end of the experiment, one round that will determine your payoff in euros is randomly drawn. Your payoff for the randomly drawn round is determined as follows:

Payoff round = earnt work income in the task + bonus – tax for bonus + share of redistributed taxe

Appendix B: Further details

Power Test

I ran a Power-Test using conventional values for α (0.05) and β (0.80) to estimate the necessary sample size. Regarding the just deserts hypothesis, the effect I test for is the diff-in-diff in the reported tax rate between high and low bonuses between the luck and performance treatments. The diff-in-diff is calculated using the mean reported tax rate for the bonus levels of 100 and 200 in the performance treatments and subtracting the mean reported tax rate for the bonus levels of 300 and 400 in the performance treatments. The same difference is then calculated for the luck treatments and the resulting value is subtracted from the calculated value in the performance treatments. For the estimated effect size, I refer to my model and to the effect found in Grundmann and Lambsdorff (2017). Using the values displayed in figure 1, the model would predict a total effect size of around 16. Grundmann and Lambsdorff (2017) find an effect size of 0.7 between their highest and lowest category of income. As they use the number on the die and not the reported tax rate, this would translate to a difference in the reported tax rate of 7. Using this value for the performance treatments and assuming its strength to be equal for the luck treatments, would predict a total effect size of $7 - (-7) = 14$. I conservatively test for around half the effect size derived from the model and from Grundmann and Lambsdorff (2017), so for an estimated effect size of 7.5. For the standard deviation, I refer to Grundmann and Lambsdorff (2017) and use their value of 1.7 again multiplied by 10, resulting in a value of 17. The number of observations is balanced between the luck and performance treatments, so that I use a sampling ration of 1. The power test reveals that the necessary sample size per group equals 82. Given that I have several observations per subjects, which might not be completely independent, I need to adjust the necessary sample size (List et al. 2011, Moffatt 2016). This is done by multiplying the calculated sample with a “variance inflation factor” (List et al. 2011: 451) of $1 + (t - 1) * \rho$, with t representing the number of observations per subject and $\rho = \frac{var\,(u_i)}{var\,(u_i)+var\,(e_{it})}$ representing the intra-subject correlation. The factor $var\,(u_i)$ denotes the between-subjects variation and $var\,(e_{it})$ captures the within-subject variation. I assume a between-subject variation $var\,(u_i) = \frac{1}{3}$ of total variation. Employing this adjustment results in a necessary sample size of $82 * \left(1 + \frac{(16-1)}{3}\right) = 492$ per group. The combined luck and performance treatments each entail 1792 observations. Assuming an equal distribution between high and low bonuses results in 896 observations per group, allowing me to conclude that the sample size was sufficiently large and the probability of finding an effect, should it exist, was high.

Regarding the competition hypothesis, I refer to the model and the values displayed in figure 1. This allows me to roughly derive an expected difference in the mean reported tax rate of around 4 between the competition and the no-competition treatments. Using the same values for α, β, the standard deviation and the sampling ratio as before yields a sample size of 285 observations per group. Multiplying this number with the variance inflation factor yields a necessary sample size of $285 * \left(1 + \frac{(16-1)}{3}\right) = 1710$. Thus, the sample size of 1792 observations each in the combined no-competition treatments and the combined competition treatments is sufficiently large.

Threshold values for slider task

Table 5 – Threshold values in slider task for different bonuses in treatment Performance-NC

Period	Working time	Number of correctly placed sliders necessary for bonus of...		
		200	300	400
1	80	5	7	9
2	40	4	5	6
3	80	9	10	12
4	40	5	6	7
5	80	10	11	13
6	40	5	6	7
7	80	10	12	14
8	40	5	6	7
9	80	10	12	13
10	40	6	7	8
11	80	10	12	14
12	40	6	7	8
13	80	11	13	14
14	40	6	7	8
15	80	10	13	15
16	40	6	7	8

Volkswirtschaftliche Reihe der Passauer Diskussionspapiere

Bisher sind erschienen:

V-1-98 Gerhard Rübel, Can adjustments to working hours help reduce unemployment?

V-2-98 Martin Werding, Pay-as-you-go Public Pension Schemes and Endogenous Fertility: The Reconstruction of Intergenerational Exchange

V-3-98 Carsten Eckel, International Trade, Direct Investment, and the Skill Differential in General Equilibrium

V-4-98 Reinar Lüdeke, Das Staatsbudget und intergenerationelle Umverteilung, Das Staatsvermögen als Instrument intergenerativer Verteilungspolitik und der "generational accounting"-Ansatz: Alter Wein in neuen (höherwertigen) Schläuchen?

V-5-98 Anja Klüver und Gerhard Rübel, Räumliche Industriekonzentration und die komparativen Vorteile von Ländern - eine empirische Studie der Europäischen Union

V-6-98 Klaus Beckmann und Elisabeth Lackner, Vom Leviathan und von optimalen Steuern

V-7-98 Martin Werding, The Pay-as-you-go Mechanism as Human Capital Funding: The "Mackenroth hypothesis" Revisited

V-8-98 Reinar Lüdeke und Klaus Beckmann, Social Costs of Higher Education: Production and Financing. The Case of Germany (1994)

V-9-98 Gerhard Rübel, "Faire" Löhne und die Flexibilität von Arbeitsmärkten in einem Zwei-Sektoren-Modell

V-10-98 Klaus Beckmann, Notizen zum Steueranteil von Rentenversicherungsbeiträgen im Umlageverfahren

V-11-98 Christian Jasperneite und Hans Joachim Allinger, Trendwende am westdeutschen Arbeitsmarkt? - Eine ökonometrische Analyse

V-12-98 Christian Jasperneite und Hans Joachim Allinger, Langfristige Perspektiven für den westdeutschen Arbeitsmarkt: Was sagen die Gesetze von Okun und Verdoorn?

V-13-98 Hans Joachim Allinger und Christian Jasperneite, Saisonbereinigung von Arbeitsmarktdaten bei aktiver Arbeitsmarktpolitik

V-14-99 Reinar Lüdeke und Klaus Beckmann, Hochschulbildung, Humankapital und Beruf: Auswertung einer Längsschnittsbefragung Passauer Absolventen 1988 - 1998

V-15-99 Gerhard Rübel, Volkseinkommenssteigerung durch ausgabenfinanzierte Steuersenkung - Eine Umkehrung des Haavelmo-Theorems für offene Volkswirtschaften

V-16-99 Silke Klüver, Konzentrationsursachen in der europäichen Versicherungsbranche - eine empirische Untersuchung

V-17-99 Reinar Lüdeke, Familienlastenausgleich, Elternleistungsausgleich und die Neufundierung der umlagefinanzierten Altersversorgung

V-18-99 Anja Klüver und Gerhard Rübel, Industrielle Konzentration als Kriterium für die Geeignetheit eines einheitlichen Währungsraums – Eine empirische Untersuchung der Europäischen Union von 1972 bis 1996

V-19-00 Carsten, Eckel, Fragmentation, Efficiency-seeking FDI, and Employment

V-20-00 Christian Jasperneite, Understanding Hysteresis in Unemployment: The German Case

V-21-00 Jörg Althammer, Reforming Family Taxation

V-22-00 Carsten Eckel, Labor Market Adjustments to Globalization: Unemployment versus Relative Wages

V-23-00 Klaus Beckmann, Tax Competition through Tax Evasion

V-24-01 Klaus Beckmann, Steuerhinterziehung, begrenzte Rationalität und Referenzabhängigkeit: Theorie und experimentelle Evidenz

V-25-01 Klaus Beckmann, Solidarity, Democracy, and Tax Evasion: an Experimental Study

V-26-04 Michael Fritsch, Udo Brixy und Oliver Falck, The Effect of Industry, Region and Time on New Business Survival - A Multi-Dimensional Analysis

V-27-04 Gerhard D. Kleinhenz, Bevölkerung und Wachstum - Die Bevölkerungsentwicklung in Deutschland als Herausforderung für Wirtschafts- und Sozialpolitik

V-28-04 Johann Graf Lambsdorf, The Puzzle with Increasing Money Demand - Evidence from a Cross-Section of Countries

V-29-04 Frauke David, Oliver Falck, Stephan Heblich und Christoph Kneiding, Generationsgerechtigkeit und Unternehmen

V-30-04 Roland Engels[†], Zur mikroökonomischen Fundierung der Geldnachfrage in allgemeinen Gleichgewichtsmodellen

V-31-05 Johann Graf Lambsdorff, Between Two Evils – Investors Prefer Grand Corruption!

V-32-05 Oliver Falck, Das Scheitern junger Betriebe – Ein Überlebensdauermodell auf Basis des IAB-Betriebspanels

V-33-05 Raphaela Seubert - On the Nature of the Corrupt Firm: Where to Situate Liability?

V-34-05 Johann Graf Lambsdorff – Consequences and Causes of Corruption – What do We Know from a Cross-Section of Countries?

V-35-05 Stephan Heblich - Arbeitszeitflexibilisierung Revisited

V-36-05 Oliver Falck und Stephan Heblich - Das Konzept der eigenverantwortlichen Generation zur Bewältigung des demographischen Wandels

V-37-05 Florian Birkenfeld, Daniel Gastl, Stephan Heblich, Ferry Lienert, Mascha Maergoyz, Oksana Mont und Andrius Plepys - Product ban versus risk management by setting emission and technology requirements – the effect of different regulatory schemes taking the use of trichloroethylene in Sweden and Germany as an example

V-38-05 Johann Graf Lambsdorff - Determining Trends for Perceived Levels of Corruption

V-39-05 Oliver Falck - Mayflies and Long-Distance Runners: The Effects of New Business Formation on Industry Growth

V-40-05 Johann Graf Lambsdorff und Christian Engelen - Hares and Stags in Argentinean Debt Restructuring

V-41-05 Johann Graf Lambsdorff und Mathias Nell – Let Them Take Gifts, and Cheat Those Who Seek Influence

V-42-06 Hans Joachim Allinger – Bürgerversicherung und Kopfpauschale haben vieles gemeinsam – Anmerkungen zur Diskussion einer Reform der gesetzlichen Krankenversicherung

V-43-06 Michael Schinke und Johann Graf Lambsdorff - Insider Trading among Central Bankers – a Treatise on Temptation and Policy Choice

V-44-06 Johann Graf Lambsdorff und Hady Fink - Combating Corruption in Colombia: Perceptions and Achievements

V-45-06 Oliver Falck und Stephan Heblich - Corporate Social Responsibility: Einbettung des Unternehmens in das Wirtschaftssystem

V-46-06 Johann Graf Lambsdorff und Luka Bajec - There Is No Bank Lending Channel!

V-47-06 Christian Engelen und Johann Graf Lambsdorff - Das Keynesianische Konsensmodell

V-48-07 Stephan Heblich - Eigenverantwortliche Individuen und Pro-Aktive Unternehmen

V-49-07 Christian Engelen und Johann Graf Lambsdorff - Das Keynesianische Konsensmodell einer offenen Volkswirtschaft

V-50-07 Christian Engelen und Johann Graf Lambsdorff - Fairness in Sovereign Debt Restructuring

V-51-07 Johann Graf Lambsdorff und Björn Frank - Corrupt Reciprocity - an Experiment

V-52-07 Mathias Nell - Strategic Aspects of Voluntary Disclosure Programs for Corruption Offences - Towards a Design of Good Practice -

V-53-07 Mathias Nell - Contracts Induced by Means of Bribery - Should they be Void or Valid?

V-54-08 Michael Pflüger – Die Neue Ökonomische Geographie: Ein Überblick

V-55-08 Florian Birkenfeld und Shima'a Hanafy - Wie zentral sind die Abschlussprüfungen an deutschen Schulen wirklich?"

V-56-08 Florian Birkenfeld - Kleine Klassen und gute Luft - Warum sind die Grundschulen auf dem Land besser?

V-57-08 Johann Graf Lambsdorff – The Organization of Anticorruption – Getting Incentives Right!

V-58-09 Oliver Farhauer und Alexandra Kröll - Verfahren zur Messung räumlicher Konzentration und regionaler Spezialisierung in der Regionalökonomik

V-59-09 Oliver Farhauer und Alexandra Kröll - Die Shift-Share-Analyse als Instrument der Regional- und Clusterforschung

V-60-10 Johann Graf Lambsdorff – Deterrence and Constrained Enforcement – Alternative Regimes to Deal with Bribery

V-61-10 Johann Graf Lambsdorff – Who Accepts Bribery? Evidence from a Global Household Survey

V-62-10 Oliver Farhauer und Alexandra Kröll – What We Can and What We Can’t Say About Employment Growth in Specialised Cities

V-63-11 Johann Graf Lambsdorff, Manuel Schubert und Marcus Giamattei – On the Role of Heuristics - Experimental Evidence on Inflation Dynamics

V- 64-12 Manuel Schubert und Johann Graf Lambsdorff – On the Costs of Kindness - An Experimental Investigation of Guilty Minds and Negative Reciprocity

V-65-12 Manuel Schubert – Deeds rather than Omissions - How Intended Consequences Provoke Negative Reciprocity

V-66-12 Agnes Binagwaho, Renate Hartwig, Denyse Ingeri und Andrew Makaka – Mutual Health Insurance and its Contribution to Improving Child Health in Rwanda

V-67-15 Marcus Giamattei – Cold Turkey vs. Gradualism – Evidence on Disinflation; Strategies from Laboratory Experiment

V-68-15 Marcus Giamattei, Johann Graf Lambsdorff – classEx – an Online Software for Classroom Experiments

V-69-15 Johann Graf Lambsdorff – Preventing Corruption by Promoting Trust – Insights from Behaviorial Science

V-70-16 Lukas Schötz – Power and Rents – political influence on rent extraction. Quasi-experimental evidence for Bavaria

V-71-16 Lukas Schötz – Mayor Games in Bavaria: Self Selection of Local Politicians is not influenced by constitutionally defined remuneration increases

V-72-16 Johann Graf Lambsdorff, Marcus Giamattei, Katharina Werner, Manuel Schubert-Emotion vs. Cognition – Experimental Evidence on Cooperation from the 2014 Soccer World Cup

V-73-16 Katharina Werner – Whom do People Trust after a Violent Conflict? - Experimental Evidence from Maluku, Indonesia

V-74-16 Katharina Werner, Johann Graf Lambsdorff – Emotional Numbing and Lessons Learned after a Violent Conflict – Experimental Evidence from Ambon, Indonesia

V-75-17 Ramona Molitor – Publicly Announced Speed Limit Enforcement and Its Impact on Road Safety – Evidence From the German Blitzmarathons

V-76-17 Johannes Lorenz – Population Dynamics of Tax Avoidance with Crowding Effects

V-77-19 Katrin Huber – The role of the career costs of children for the effect of public child care on fertility and maternal employment

V-78-19 Markus Fritsch, Andrew Adrian Yu Pua, Joachim Schnurbus – Revisiting habits and heterogeneity in demands