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Catrine Jacobsen
Marco Piovesan

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Authors: Catrine Jacobsen, Marco Piovesan

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Department of Food and Resource Economics (IFRO)
University of Copenhagen
Rolighedsvej 25
DK 1958 Frederiksberg DENMARK
www.ifro.ku.dk/english/

Tax me if you can:
An artefactual field experiment on dishonesty

Catrine Jacobsen[a] and Marco Piovesan[b]

University of Copenhagen

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Abstract

In this paper, we test whether increased salience of a tax charge increases dishonesty using a version of the *die-under-cup* paradigm. Participants earn money in proportion to the outcome reported and, thus, have an incentive to over-report. We find a significant increase in high outcomes in the presence of a tax frame suggesting that participants use the tax as an excuse to rationalize their dishonest act. In addition, we tested whether adding an explanation for the adoption of the tax would increase honesty. We find evidence for reversed dishonesty with participants reporting significantly more low outcomes. These results warn policy makers about the non-trivial relationship between taxation charges and dishonesty.

[a] Catrine Jacobsen, Institute of Food and Resource Economics, University of Copenhagen. cj@ifro.ku.dk.
[b] Corresponding author: Marco Piovesan, Dept. of Economics, University of Copenhagen. Marco.Piovesan@econ.ku.dk.

1. Introduction

Big corporate scandals such as Enron, Halliburton or WorldCom only account for a small part of the dishonesty we observe in the real world. Many ordinary people cheat on taxes, over-report the number of hours worked, inflate travel expenses and commit insurance fraud (Ariely, 2012; Gino, Ayal, & Ariely, 2009b; Mazar & Ariely, 2006). Traditional models in the economics of crime (Allingham & Sandmo, 1972; Becker, 1968; Jensen & Meckling, 1976) suggest that individuals act dishonestly when the benefits of wrongdoing (more money, a better position) outweigh the costs (paying a fine, losing a job). These models imply that more honest behavior can be achieved by lowering the benefit or increasing the cost of the dishonest act. For instance, a "dishonesty tax" imposed as a charge on certain reports which are susceptible to overestimation can be used to discourage dishonesty in the same way as a sin tax is used to discourage the consumption of alcohol, tobacco, candy, soft drinks, fast food and gambling (O'Donoghue & Rabin, 2003, 2006).

Imagine the following example: agent A hires agent B (a consultant, a contractor or a research assistant) to perform a task that can take from one to six hours. Agent A knows how much time, on average (3.5 hours), is required to complete the task, but she does not observe (or can verify) the true number of hours worked on the task. Agent B has to report the hours worked and agent A pays compensation (say 10 dollars) for each hour reported. Imagine, moreover, that agent A introduces a charge, which is deducted from the total compensation to dis-incentivize over-reporting (for instance a $5 charge for reporting 4 hours, $10 for 5 hours and $15 for 6 hours equivalent to a 50% marginal charge). Of course, agent B still has an incentive to report six hours' work, but the marginal benefit of reporting more hours than three is lower. Should agent A frame this deduction as a tax? And does

including an explanation of why this charge has been added change behavior regarding the hours reported?

In this paper, we investigate the effect of including a marginal compensation charge on dishonesty. In particular, we are interested in studying whether framing the charge as a tax and making this salient increases over-reporting, and whether an explanation about why the tax has been introduced increases compliance. Our research hypothesis is that including a tax framing increases dishonesty since it provides a justification or rationalization for performing a dishonest act in order to cover the 'loss' imposed by the tax charge. In fact, the literature in behavioral economics and social psychology has shown that people cheat more when a justification for the act is possible, even if such justification is unknown to anyone other than that person (Bandura, 1999; Bandura, Barbaranelli, Caprara, & Pastorelli, 1996; Shu, Gino, & Bazerman, 2011). For instance, having other people benefit from the dishonest act (Erat & Gneezy, 2012; Gino & Pierce, 2010; Gneezy, 2005; Moore & Gino, 2013), or watching one's peers acting dishonestly tend to increase dishonesty (Fosgaard, Hansen, & Piovesan, 2013; Gino, Ayal, & Ariely, 2009a; Gino, Ayal, et al., 2009b; Gino, Gu, & Zhong, 2009). Even rolling a die more than once is sufficient justification for lying about the outcome of the first roll since the subsequent outcomes are rationalized as being just as likely to have occurred first (Shalvi, Dana, Handgraaf, & De Dreu, 2011). If people can rationalize their dishonest actions as being morally acceptable, they can disengage themselves morally and convince themselves that it is actually acceptable to cheat (Ayal & Gino, 2013; Zhang, Gino, & Bazerman, 2014).

Our artefactual field experiment was conducted in a local Copenhagen shopping center. We used a modified version of the die-under-cup paradigm (Fischbacher & Föllmi-Heusi, 2013): we asked participants to roll a single die once in private and report the outcome on a report sheet. As in the example above, the payoff depended on the reported outcome. We found that a tax frame and increased saliency increases dishonesty compared to a baseline treatment and that adding an explanation as to why the tax charge was introduced, decreases over-reporting. Interestingly, even with the explanation, we found evidence of participants reporting dishonestly, but in a reversed manner, an effect that was first explored by Utikal and Fischbacher (2013) in a similar experiment conducted with nuns. Overall, our study reveals that dishonesty does not only depend on the payoffs and is, therefore, not simply a gain driven behavior. Rather, the saliency of a charge and the framing used has a direct influence on the level of dishonesty people exhibit. These results shed light on the negative effect of using a taxation charge to discourage particular types of behavior.

The remainder of this paper is organized as follows. In Section 2, we present some relevant literature on tax aversion and dishonesty. Section 3 describes our experiment and treatment manipulations, and in Section 4, we present our results. Section 5 discusses our findings, their implications and the future directions for research.

2. Taxes and dishonesty

Our study contributes to the debate on attitudes and behavior towards taxes. Kallbekken, Kroll, and Cherry (2011) found that simply framing a Pigouvian instrument as a tax significantly reduces voting

support. Thus, although the public supports the intention to penalize undesirable behavior, they oppose taxation as the instrument. The philosopher, John Stuart Mill, (1848, bk. 5, ch. 6) argued that, "An Englishman dislikes, not so much the payment, as the act of paying. He dislikes seeing the face of the tax-collector, and being subject to his peremptory demand". If Mill is right, this means that it is not the act of paying a tax or charge *per se* that people dislike, rather it is being subject to the visible demand of a tax that creates aversive behavior. Similarly, Chetty et al. (2009) found that disclosing a sales tax (i.e. increasing the saliency of the sales tax related to a product) had a direct influence on consumption. Explicitly, adding the sales tax to the price tags reduced consumption significantly compared to the normal situation where only the pre-tax price was disclosed. People, thus, do not seem to take the tax into account when purchasing. This finding supports the claim that the visibility and saliency of the tax charge as a part of the total price has an important influence on behavior. It is, however, unclear whether it also makes individuals more dishonest. Along these lines of thought, Verbeke and Coeck (1997) find that an environmental taxation influences firms' willingness to respect the environmental policies and creates devastating incentives for cheating behavior in the long run. In the light of this literature, it is, therefore, not surprising that when people have the active opportunity to evade taxes, they do so to increase personal gain (Doerrenberg & Duncan, 2014).

In our study, participants cannot evade taxes since these are imposed as charges on the payoffs, but they can over-report their outcome to attain a higher net benefit (payoff after the tax charge). The question, however, remains whether the framing of the payment together with increased saliency directly influence such behavior. Therefore, this paper contributes to the existing literature by testing whether the saliency and framing of a tax charge in itself fosters dishonesty compared to when a

similar deduction is incorporated in a payoff. Additionally, this paper investigates whether providing an explanation for a tax reduces dishonest behavior. Greenberg (1990) found that exposing employees to a pay-cut with no explanation led to increased employee theft and dishonest actions, while providing an explanation for the pay-cut maintained honesty among the employees. Furthermore, Hallsworth, List, Metcalfe, and Vlaev (2014) found that drawing attention to the social norms and collective responsibility in a tax payment reminder increased compliance significantly. This paper, therefore, also contributes to knowledge about the use of explanations to modulate compliance behavior towards taxes.

Finally, our results also contribute to the literature on dishonesty and truth-telling in economics. Using a cheap-talk sender-receiver game, where the receiver can lie, Gneezy (2005) shows that an aversion towards lying exists and that this aversion is sensitive to the cost a lie can incur on others. Fischbacher and Föllmi-Heusi (2013) show that the majority of people lie, but just a little bit to preserve their positive self-concept. External reputation is also important for the decision about whether to lie: Hao and Houser (2013) demonstrate that dishonesty significantly decreases when this decision must be revealed before the act. In addition, the perceived level of fairness is a key factor in the decision to act dishonestly: for instance, Houser, Vetter, & Winter (2010) show that individuals who believe they are being treated unfairly are more likely to cheat on a subsequent coin task. The experimental investigation into dishonesty collected evidence not only in the lab, but also in the field. For instance, Pruckner and Sausgruber (2013) showed the effect of a moral and legal reminder on payments of newspaper purchases in Austria where 2/3 of people do not pay for the newspaper when they have the opportunity. Interestingly, Abeler, Becker, and Falk (2014) found almost no evidence of

lying when people participated in an experiment over the phone. However, they speculated that others in the same situation would have lied. All these papers shed light on the mechanism behind the decision to lie (and cheat) but, to the best of our knowledge, no one has looked at the effect of a tax frame on this decision. Therefore, we make a step forward in this direction and test the effect that frames and the saliency of deductions have on the probability of reporting the outcomes of dice throw honestly.

3. Experiment

We conducted our field experiment in a shopping center in Copenhagen during a regular weekday. We randomly approached customers and once they agreed to participate we provided them with a die, a cup, a closed envelope with the instructions and a report sheet[1]. Participants had to roll a die once, in private (i.e. no one other than the participant observed the actual outcome), and report the outcome using the report sheet in the envelope. The participants then had to show the report sheet to the researcher to receive their payment according to the rules explained in the instructions, but they could also keep the report sheet or throw it out. Upon receiving the money, each participant had to fill out a receipt form and place it in a closed box. These forms contained the payment details, a treatment identifier (unknown to the participants) and a couple of questions about gender and nationality. In total, we collected 149 observations (83 females), which were randomly assigned to

[1] *Randomization worked as follows: The sealed envelopes were arranged in bulks of 10 such that the first ten participants were given envelopes with the Baseline Treatment (BT), the next ten were given envelopes containing the Tax Framing Treatment (TT) and the following ten the Explanation Treatment (ET) and so forth. A research assistant switched the receipt forms containing treatment identifiers for each ten envelope given out. The different treatments were therefore cycled throughout the day, which should have prevented any bias related to time of day (and people travelling to the shopping center at different times of the day).*

three treatments: 50 observations in a Baseline Treatment (BT), 50 observations in a Tax Framing Treatment (TT) and 49 observations in an Explanation Treatment (ET). The distribution of females in the three treatments was 25, 31 and 27 respectively (see table 1 below for an overview of participant distribution). 121 people were born in Denmark (or born elsewhere, but were Danish citizens). All participants were able to read or understand Danish.

Table 1. Participant details

Condition	Males	Females	Natives	Non natives	No nationality stated
BT	23	27	40	9	1
TT	19	31	41	6	3
ET	24	25	40	8	1
Total	66	86	121	23	5

A sign clearly stated that the experiment was being run by the University of Copenhagen. No details about the tax, who would receive it or what it was to be used for, were explicitly stated. The reason for this was that not every participant was exposed to a treatment including the tax framing. Moreover, we wanted to keep the instructions as similar as possible to make sure the effects reflect the framing of a tax and not additional explanations about taxation. This means we cannot with certainty know whether subjects believed the tax was in fact transferred to the government or simply hypothetical (as was the case). However, those participants who asked were informed that the university had financed the experiment. No participants asked about the tax or the purpose of it. It is important to note that the type of tax focused on in the experiment is a charge and not an income tax, which was evident from the Danish word 'afgift' used (equivalent to that of a tax charge). Simply put, a charge (or 'afgift') is a one-time fee one has to either add to a payment, such as a sales tax or a

sin tax, or deduct from a payoff (as is the case in this experiment), such as a gambling tax imposed on any winnings earned from gambling.

Figure 1. Instructions.

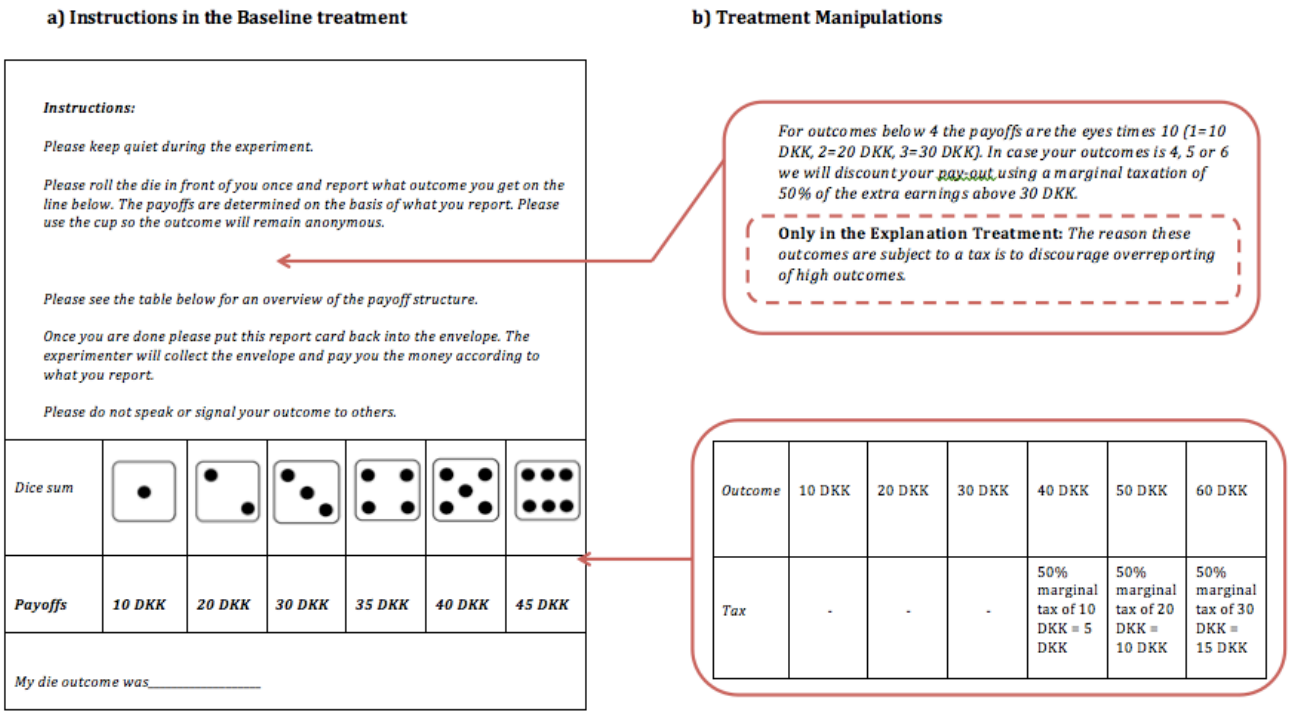


Figure 1: *Illustration of additions to the treatment manipulation.* ***a)*** *shows the illustrations of the Baseline Treatment (BT). These instructions were present in all treatment variations.* ***b)*** *the boxes indicate which treatment manipulations had been added to the baseline instructions. The solid boxes include details that were added in the Tax Frame Treatment (TT). In the Explanation Treatment (ET) the additional information from the dashed box was added together with details from the solid boxes.*

Figure 1 reports the instructions of our Baseline Treatment and our treatment manipulations. In BT a table indicates the payoff structure for each of the six possible outcomes (1=10DKK[2], 2=20DKK, 3=30DKK, 4=35DKK, 5=40DKK, 6=45DKK) below the instructions. Note that for outcomes above 3, the payoffs only increase by 5 DKK, instead of 10 DKK. In TT the payoff structure is the same, but the *framing* is different. We add two sentences to the instructions and two extra rows to the payoff table

[2] 10 DKK corresponds approximately to $1.5.

specifying the lower gain increments for outcomes above 3 as a result of a marginal deduction of a tax charge. ET is identical to TT, except for one sentence added to the instructions, which states why the tax was imposed (the statement reads: '*The reason these outcomes are subject to a tax is to discourage over reporting of high outcomes*'). Finally, it is important to note that participants have to report their outcome below the table, at the bottom of the sheet, which provides a subtle *nudge* to read the entire table and take note of the payoff structure.

4. Results

First, we test whether our participants have reported their outcomes dishonestly. Since we cannot observe the true outcome and compare this with the reported one, we can only speculate and test whether the distribution of reported outcomes is significantly different from the (expected) uniform distribution. A non-parametric Kolmogorov–Smirnov[3] test shows that there is evidence of cheating in TT *(K-S z= 1.556, P=0.016, two-tailed),* but not in BT[4] *(K-S z= 0.849, P=0.478, two-tailed)*. Figure 2 reports the two distributions (panel a and b). Participants in the BT treatment reported rolling 3.52 on average (median = 4), whereas participants in the TT treatment reported rolling 3.94 on average (median = 4). A Wilcoxon Rank Sum test (or Mann-Whitney medians test) confirms that the distribution of outcomes reported in the TT treatment is (marginally) significantly higher than those in the BT treatment (*z =-1.322, P=0.092)*. A closer look at these two distributions (see Table 2) suggests

[3] Since Kolmogorov-Smirnov assumes continuous distributions and the Chi-squared test requires a minimum of five observations per category, we calculated the p-value for the Kolmogorov-Smirnov test based on the discrete distribution, which results in more conservative p-values (Conover, 1972).

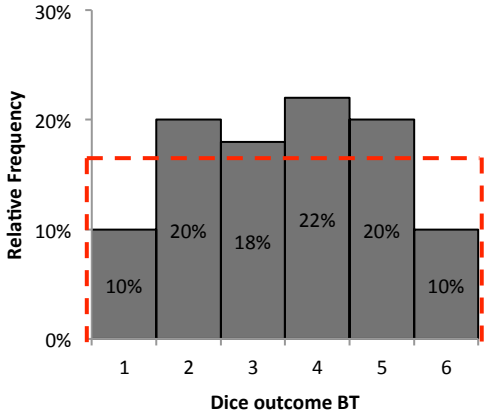
[4] This result is in line with the finding reported by Abeler et al. (2014) that shows people generally act honestly when asked to flip a coin in complete anonymity over the phone.

that this difference in reported outcomes is mainly driven by the increase in the number of "6s" reported in TT (+120%, see Table 2). We see the opposite trend in the number of "1s" reported (-40%) compared to BT, while the rest of the outcomes remain somewhat stable between the two treatments. We, therefore, further investigated the difference in extreme outcomes (i.e. "1s" and "6s") in isolation between the two treatments. A Fisher's Exact test shows that this variation of less "1s" reported in TT is not significantly lower than that reported in BT *(P=0.357, one-tailed)*, but the variation of more "6s" reported in TT compared to BT is (marginally) significant *(P=0.086, one-tailed)*[5]. The 40% decline in "1s" is, therefore, within the expected range of variation, whereas the 120% increase in "6s" between BT and TT confirms that more people chose to report the maximum outcome in TT, which supports the claim that more people reported dishonestly in TT.

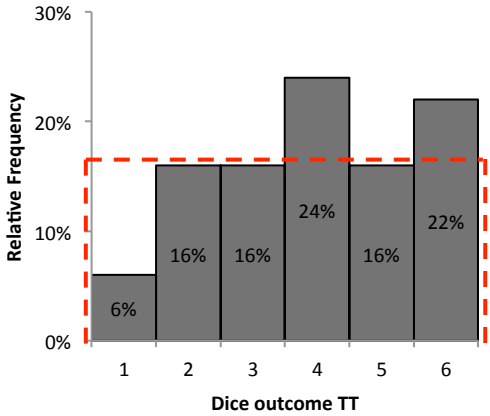
[5] In this analysis, we used a dummy variable in order to compare the number of "1s" reported (dummy=1) with any other numbers reported (dummy=0), and a dummy of "6"(dummy=1) compared to any other number (dummy=0).

Figure 2. Distribution of reported outcomes in BT, TT and ET.

a) Distribution of outcome in BT



b) Distribution of outcome TT



c) Distribution of outcome ET

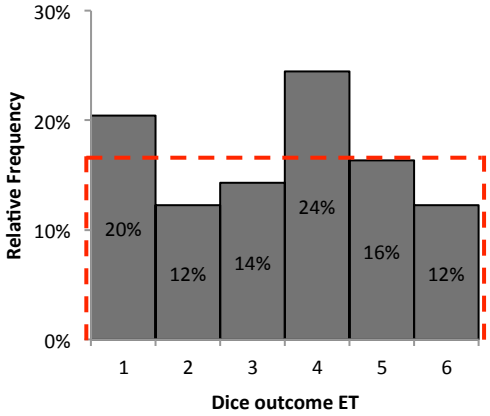


Figure 2: *Distributions.* ***a)*** *Distribution of outcomes for the baseline treatment (BT) showing no significant difference from a uniform.* ***b)*** *Distribution of the tax-frame treatment (TT) showing a clear left-skewed trend.* ***c)*** *Distribution of the Explanation treatment (ET) showing a right-skewed trend. The red dashed line indicates the level of the uniform distribution. All distributions are illustrated in relative frequency to show the proportional distribution of outcomes relative to the amount of observations in each treatment. This allows for an easier comparison.*

Table 2. Change in distribution of outcomes from BT to TT.

Outcome	BT	TT	Change (in %) from BT to TT
	5	3	-40%
	10	8	-20%
	9	8	-11%
	11	12	+9%
	10	8	-20%
	5	11	+120%

Comparing the distribution of outcomes reported in TT and ET allows us to study the effect of adding the explanation (see Figure 2, panel b and c). A Kolmogorov-Smirnov test also shows a significant deviation from the uniform distribution found in ET *(K-S z= 1.429, P=0.034, two-tailed).* However, the average reported outcome in ET is only 3.41 (median = 4), and is, therefore, below the expected of 3.5. Again a Wilcoxon Rank Sum test (or Mann-Whitney) confirms that the distribution of outcomes reported in the ET treatment is (marginally) significantly lower than that reported in the TT treatment (*z =-1.536, P=0.062)*. This difference suggests that the deviation from the uniform distribution observed in ET is skewed to the right and not to the left as in TT. In other words, despite the subtle and rather minor change in treatment manipulation between TT and ET, participants seem to display a different behavior. If we look at the change in the reported outcomes from TT to the ET (see Table 3) there is a clear polarity difference from observing many people reporting "6" (22%) in the TT treatment to observing many people reporting "1" (20%) in the ET treatment. We, therefore, tested

whether the proportions of the extreme outcomes of "1" and "6" occurring in the two treatments differed between ET and TT. For this we also ran a Fisher's Exact test and found that the difference in the proportions of "1" reported in ET and TT is indeed significant *(P=0.033, one-tailed)*, but the number of "6s" reported was not *(P=0.154, one-tailed).* This result suggests a notable reaction to the inclusion of an explanation causing a change in the number of extreme outcomes reported from more "6s" to more "1s".

Table 3. Change in distribution of outcomes from TT to ET.

Outcome	TT	ET	Change in % From TT to ET
	3	10	+233%
	8	6	-25%
	8	6	-13%
	12	12	+0%
	8	8	+0%
	11	6	-45%

Finally, to confirm these findings, we ran a multinomial probit model on the outcomes of "1" and "6" and "other", which supports the results that participants in TT reported more "6s" compared to participants in BT (Table 4, outcome "six", regression 1) and that participants in ET reported significantly more "1s" compared to participants in TT (Table 4, outcome "one" regression 1). These results remain significant when controlling for gender and nationality (Table 4, regression 2). In particular, neither gender nor nationality has any significant effect on the outcomes reported.

Table 4. Regressions

Outcome "One"				
	Regression 1: simple		*Regression 2: Controlling for gender and nationality*	
Treatment	Coefficient	z	Coefficient	z
BT	0.19 (0.50 [0.70])	0.39	0.13 (0.51) [0.80]	0.25
ET	0.86* (0.47) [0.07]	1.82	0.82* (0.48) [0.09]	1.71
Constant[a]	-1.85*** (0.38) [0.00]	-4.87	-3.20*** (0.89) [0.00]	-3.59
Nationality[b]	-	-	0.37 (0.38) [0.33]	0.97
Gender	-	-	0.69 (0.44) [0.21]	1.55
Outcome "Six"				
Treatment	Coefficient	z	Coefficient	z
BT	-0.65 (0.43) [0.13]	-1.51	-0.66 (0.44) [0.13]	0.52
ET	-0.33 (0.42) [0.43]	-0.79	-0.33 (0.42) [0.43]	2.13
Constant[a]	-1.00*** (0.27) [0.00]	-3.61	-1.24 (0.78) [0.11]	1.87
Nationality[b]	-	-	-0.02 (0.36) [0.96]	-1.76
Gender	-	-	0.23 (0.45) [0.61]	0.94

[a] TT is set as the base value to make the comparison between the conditions easier to interpret.
[b] Nationality = Danish vs. non-Danish participants. This parameter, therefore, compares Danes to non-Danes.
*Note: The dependent variable is discrete outcomes bulked as either "1", "6" or "other". Column (1) refers to the beta values of each independent variable; Column (2) refers to the z value of each independent variable in the multinomial probit model. Standard errors in parentheses; p-values in square brackets; *** p < 0.01, ** p < 0.05, * p < 0.1.*

5. Discussion

In this paper, we study the effect of different framings on dishonesty. Our artefactual field experiment reveals that a tax frame of an imposed charge/deduction increases dishonesty. When the payoff structure is framed as a result of a tax charge, participants significantly increase the average outcome-value reported and particularly the number of "6s" reported. Interestingly, we found no evidence for dishonesty in the Baseline Treatment even though the payoffs and circumstances for acting dishonestly were identical to those in the Tax Framing Treatment. This phenomenon might be a result of a missing justification for dishonesty, which has also been suggested by Shalvi et al. (2011). When

people have no apparent justification for dishonest behavior, it is reduced as we see in the Baseline Treatment. In the original Fischbacher and Föllmi-Heusi (2013) set-up, people could roll the dice more than once, providing means for justifying dishonest behavior. This was not the case in our experiment, where people were only allowed to roll once. This result also lends support to recent findings by Abeler et al. (2014), which suggests a general preference for honesty when similar set-ups are tested in the field. However, because both the Abeler et al. (2014) experiment and our experiment employ only one coin toss and a single die roll, it is not clear whether this finding is due to the adjusted set-up or the fact that the experiment was conducted in the field.

In our experiment, the tax framing and saliency of the deducted charge primarily influenced the number of extreme outcomes reported, and significantly more people reported "6" thereby maximizing gains. In a similar set-up to ours, Shalvi et al. (2011) found that people avoid major lies when reporting the outcomes of dice rolls, and thus avoid reporting extremes. When a dishonest act threatens one's positive self-image too much by e.g. reporting an extreme outcome that maximizes gains, people prefer to report the truth. Our results, therefore, suggest that framing a payment or charge as a tax is sufficient justification to shift preferences from moderate dishonesty (i.e. avoiding major lies) towards gain maximizing behavior. However, because a tax charge also benefits the government (i.e. the government increases its income when more people pay the tax charge), the observed dishonest behavior might be altruistically driven. By over-reporting the die outcome, a participant increases both his income and the public good via the imposed charge and, thus, acts like "a good citizen". However, since people are generally loss averse, it is likely that the salience of the imposed tax loss is what people focus on, and also what stimulates people to act more dishonestly. All

together, these results provide empirical evidence that the increased salience of a payment together with a tax frame make people more dishonest (as evident from the difference between the Baseline Treatment and the Tax Framing Treatment).

Interestingly, adding an explanation as to why the tax was introduced proved successful in decreasing over-reporting, but not dishonesty per se as was the case in Greenberg (1990). Indeed, we find evidence that the explanation does not eliminate dishonesty, but instead "reverses" it: our participants, on average, report a number close to that which would be expected, but the number of "1s" reported significantly increased. This result lends support to a novel type of disadvantageous lying behavior also explored by Utikal and Fischbacher in 2013. Our explanation is phrased such that it draws attention to an expectation of over-reporting associated with high outcomes, which may, therefore, have induced a preference for reporting a disadvantageous outcome not in line with the selfish assumption of gain maximization ("1"). By excessively reporting "1", people thus seem to display a behavior in which they willingly forego entitled gains otherwise acquired had they reported the true outcome. The importance of signaling non-selfish behavior thus becomes more important than obtaining a possible gain. Perhaps including an explanation reminds people of their reputation or social appearance influencing the honesty of people as argued by Hao and Houser (2013) and, as in our case even leads to disadvantageous lying behavior. Finally, some of the difference found between the Tax Framing Treatment and the Explanation Treatment might be a result of people simply wanting to conform to the message displayed in the explanation (i.e. not over reporting) and hence shifting the norm. This does, however, not explain the increased amounts of "1" reported. Therefore, similar to the way defaults function as a powerful policy making tool to nudge certain behavior (Altmann,

Falk, & Grunewald, 2013), policy makers and tax authorities may utilize the information of introducing an explanation like the one applied here as a means to decrease over reporting and enhance compliance.

This experiment does not uncover underlying drivers to the observed behavior. We therefore suggest that future research should focus on whether saliency of other types of deductions reflects similar behavior, or if this is an actual reaction to the fact that the deduction was framed as a tax. Furthermore, future directions lie in understanding what exactly drives the explanation effect, by e.g. testing different types of explanations.

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