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Peer advice in a tax-evasion experiment

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Abstract

We examine peer effects in a tax-evasion experiment where subjects receive either advice or compliance data from participants with either above-median or below-median compliance rates in a control treatment. Both types of information on peer behavior yield significantly lower compliance rates than in the control group without any kind of information about peers. Receiving advice or compliance data from the pool of low-compliance participants yields the lowest compliance rates. We show that advice has a slightly bigger impact than observing the compliance of others and that subjects focus on payoff maximization rather than on tax morale when giving advice.

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1 Introduction

Imagine you knew how many of your neighbors honestly paid their taxes and how many were indicted for evading them. Or consider a situation where your neighbors gave you some well-meant advice on how much of your income you should actually declare and which parts of income you might easily conceal. Would that influence your tax declaration? This is the main question we pose in this paper, and the firm answer is: Yes, it would. More precisely, the controlled laboratory experiment we are going to present in this paper suggests that your compliance rate would probably go down.

The influence of social interactions with neighbors, colleagues, or peers has been documented for a broad variety of decisions, like binge drinking, smoking and illicit drug use (Lundborg, 2006; Powell et al., 2006), criminal activities (Bayer et al., 2007), or student achievement (Sacerdote, 2001; Zimmerman, 2003). Recently, the influence of social interactions on tax evasion has come under closer scrutiny.[1] Fortin et al. (2007), for instance, have shown in a cleverly designed experiment that fairness plays an important role for tax evasion, but that social conformity does not. That is to say that tax compliance depends on horizontal equity. If taxation is perceived to be unfair (to one's disadvantage, of course) tax compliance can be expected to go down. This latter finding resounds in several empirical studies. For instance, an analysis of survey data suggests that compliance rates depend on the government's reputation and the public perception of the tax system. If a country's government is considered as more efficient and less corrupt and if the tax system is considered as fair, then compliance rates are typically higher (Torgler, 2003). Likewise, democratic participation rights have been documented to increase the level of tax compliance (Pommerehne and Weck-Hannemann, 1996). Frey and Torgler (2007) show in a survey study that an individual's tax morale depends significantly negatively on this individual's estimate about how many of his or her compatriots cheat on taxes if they have the chance to do so. That means that tax morale is higher the more likely people consider their compatriots to pay their due taxes as well. This is clear evidence of peer effects on tax evasion decisions, even though Frey and Torgler's (2007) results are based on individual perceptions and beliefs rather than on hard facts and precise knowledge about the tax compliance in a peer group. The experiment of Fortin et al. (2007) controls carefully for the size of the peer group and the relevant parameters (such as income, tax rates and audit probabilities), thereby also avoiding the well-known identification problems of endogenous social interaction effects (Manski, 1993). Fortin et al. (2007) then study how tax evasion converges within groups, i.e., they let group members revise their tax declarations contingent on their observation of others' tax declarations. This iterative procedure is finished when subjects' changes are only minuscule or after a upper-bound number of iterations is reached. Fortin et al. (2007) observe that about 88% of procedures converge before reaching the upper-bound. Convergence is in almost 50% of cases either on full tax compliance or full tax evasion.

In this paper we also study peer effects on tax evasion behavior. In contrast to Fortin et al. (2007) we do not consider the process of convergence in a group of peers, but rather examine how individuals react when they observe another group. Hence, the social interaction we are considering is unidirectional. This may be interpreted as a stress test of peer effects, as we exclude any kind of iterative process, but rather study a one-shot effect. We also compare the behavior with social interaction to a control condition where no peer group exists, so that

[1] It has long been acknowledged that the main puzzle of tax evasion or compliance is rather why people pay taxes than why they don't (Alm et al., 1992). For determinants of tax compliance other than social interaction effects see the surveys of Andreoni et al. (1998) and Torgler (2002). Psychological arguments – such as psychic costs of evasion – are discussed in Bayer (2006) or in the book by Kirchler (2007).

we can quantify the influence exerted from a peer group on tax compliance. In order to examine whether unidirectional social interaction (from peer group to individual) works differently depending upon the characteristics of the peer group, we study peer effects when the peer group is either a high-compliance group or a low-compliance group. Unlike Fortin et al. (2007) we are also interested in two different sources of peer effects. Like them we consider as one source the effects of providing information on compliance behavior in a peer group. In addition we also consider how receiving advice from a peer group influences behavior. We study tax evasion decision when subjects receive advice on how much income to declare, with the advice stemming from subjects who have participated in the same experiment before. In order to account for the fact that peers who give advice have only little more experience than the advisee, we use the term "naïve advice" (Schotter, 2003). By examining tax compliance with either information on a peer group's compliance or advice from a peer group we can compare the relative effects of both forms of peer influence.

The main advantage of using a laboratory experiment is to control the information available to subjects and to set up identical conditions for the control group and the group of subjects with peer information. Such a degree of control is hardly possible with field data. In particular, it would be a demanding challenge for field studies to truthfully elicit the information available to a given subject when declaring its income. It is methodologically even more challenging to observe the degree of tax evasion in the field. Both aspects can be completely controlled in the laboratory. Furthermore, in the lab we can control causality more precisely. For instance, the field study of Frey and Torgler (2007) shows that people's tax morale depends on their beliefs about their peer's tax morale. Yet, the direction of causality remains an open issue, whereas in the laboratory it is possible to separate causes and consequences by observing tax declarations with and without information on peer group behavior. It does not go without saying, though, that field studies and laboratory experiments should be considered as complementary approaches that – taken together – should enhance our understanding of tax evasion and tax compliance.

We find in our experiment that both sources of peer influence – observing compliance rates as well as receiving advice – significantly reduce tax compliance. This general finding does not depend on the peer group being either a high-compliance or a low-compliance group. Yet, it is important to note that compliance rates are considerably higher and less prone to a downward-trend if the peer group is the high-compliance group. In sum, our findings suggest a rather strong and negative effect of peers on tax compliance. Hence, it may seem a wise policy of tax authorities to hide data on tax compliance, as long as it is not 100%.

The rest of the paper proceeds as follows. Section 2 introduces the tax evasion game as well as the experimental design. Experimental results are presented in Section 3, and Section 4 concludes the paper.

2 The tax evasion experiment

2.1 Experimental design

In all experimental treatments to be introduced below, subjects had to decide on how much of their endowment to declare for taxation in each of a total of 20 periods. A subject's endowment in each period consisted of a fixed and a random component. The fixed component was determined by letting subjects answer 20 trivia questions at the beginning of the experiment. This procedure was chosen in order to induce a higher level of entitlement over endowments. Each correct answer yielded 20 points. Hence the maximum fixed component was 400. Then we added a random number from the uniformly distributed interval

[-100;100] to the fixed component, yielding in sum a subject's true endowment E_T.[2] By this approach we generate some variance in endowments both across subjects as well as within subjects across periods, allowing us to examine how tax compliance depends on a subject's true endowment.

After having been informed about their endowment in a given period, subjects had to declare an endowment E_D. The declared endowment E_D needed not be equal to E_T, and it was taxed with a flat tax rate of $t = 0.3$. The audit probability was set to $p_A = 0.05$. In case of an audit, a subject's true endowment was revealed, and the subject had to pay the evaded tax plus a penalty of twice the evaded tax if $E_D < E_T$. This implies a fine rate of $f = 3$.[3]

The tax revenues from the declared endowments were used for providing a public good in the following way. In each period, every subject was paired with one other subject, thus constituting groups of $n = 2$.[4] The tax revenues from both subjects were summed up, multiplied by an efficiency factor $e = 1.1$, and redistributed in equal parts to both subjects.[5] This yields the following payoff function for a subject i who is paired with subject j. In order to keep the notation simpler we drop period indices.

$$Payoff_i = p_A \cdot \left(E_{Ti} - tE_{Di} - ft(E_{Ti} - E_{Di}) + \frac{et(E_{Di} + E_{Dj})}{n} \right) + (1 - p_A) \cdot \left(E_{Ti} - tE_{Di} + \frac{et(E_{Di} + E_{Dj})}{n} \right)$$

At the end of each period, subjects were told their payoff, the amount of taxed paid, the fine in case an audit had taken place, and the returns from the public good. They were not informed about the declared or true endowment of the matched subject and whether the matched subject was audited or not.

2.2 Experimental treatments

Treatment 1 – *Control*. The 40 subjects in this treatment did not receive any information (like advice or statistics about other subjects' past behavior) when declaring their endowment. Hence, these subjects serve as the control group for the other treatments described below. Note that participants in *Control* were asked at the end of the experiment (without knowing that in advance) to provide advice on i) a suggested percentage of the endowment to be declared and ii) a rationale for the suggested percentage. Subjects in *Control* were informed that their advice would be distributed to participants in future sessions before they would declare their endowment. We also stressed that in order to provide an incentive for meaningful advice we would randomly pick four out of the 40 advice sheets collected in *Control* and pay to the four authors of these advice sheets the same amount of money that would have to be paid to the recipients of the advice sheets.[6] In fact, the additional payments

[2] Since no subject earned less than 100 points from the questionnaire at the beginning of the experiment, negative endowments were not possible.

[3] Note that we focus on the influence of social interaction with peers on tax compliance. Hence, we are not primarily interested in how tax compliance depends on tax rates, audit probabilities or fine rates (see Andreoni et al., 1998, Kirchler, 2007, or Maciejovsky et al., 2007, for evidence on these issues). The parameters chosen in our experiment are motivated by stylized facts from Continental Europe. In Austria and Germany, for instance, the average tax burden is around 30%. The audit probability seems slightly below the 5%-level chosen in the experiment, and fine rates can be up to 300% of evaded taxes (not to speak of the hardly measurable costs of being sent to prison for large-scale tax evasion).

[4] In fact, we set up matching groups of four subjects each, and rematched these four subjects randomly in each period into two pairs. This procedure ensures that we can consider a matching group of four subjects as one statistically independent observation. Subjects were not aware of the size of the matching group, but were only informed that the pairs were randomly rematched after each period.

[5] Possible revenues from the tax audits were not considered for the public goods provision.

[6] As will become clear below, when subjects received advice (in treatment *High-advice* and *Low-advice*) they received each available advice sheet exactly in one period. For paying the authors of the four advice sheets

ranged from € 15.2 to € 18.1 and were paid out after the treatments with advice had been carried out.

For the following four treatments we split the subjects from the *Control*-treatment into two groups by using the median of the average compliance rates over the 20 periods (i.e. the fraction of the true endowment that is declared). Those 20 subjects with above-median average compliance rates constituted the *high-compliance* group, those 20 subjects with below-median average compliance rates constituted the *low-compliance* group.

Treatment 2 – *High-history*. Before declaring their endowment in a given period, the 20 subjects in this treatment were informed about the tax compliance of the *high-compliance* group in the *Control*-treatment in the very same period. More precisely, subjects in *High-history* saw a table that indicated the numbers of subjects from the *high-compliance* group that declared i) 0-33 percent, ii) 34-66 percent, or iii) more than 66 percent of their true income. Additionally the table included iv) the average compliance rate in the *high-compliance* group in a particular period. Please note that participants in the *High-history*-treatment were not informed that the data they were presented originated from a subject pool that had above-median compliance rates. Rather, subjects were told that the data they saw were taken from a group of 20 subjects who had previously participated in this experiment, but who had not received any kind of information from other participants.

Treatment 3 – *Low-history*. The 20 subjects in this treatment were presented data originating from the *low-compliance* group of *Control*, i.e. the group with below-median compliance. In all other aspects this treatment is analogous to the *High-history*-treatment. Appendix A shows all tables that were shown in the respective periods of *High-history* or *Low-history*.

Treatment 4 – *High-advice*. The 20 subjects in this treatment received the advice sheets from the *high-compliance* group of the *Control*-treatment. In each period each participant in *High-advice* received exactly one sheet of advice before making a tax declaration. The order of presenting advice sheets was such that over all 20 periods each participant received all 20 different sheets of advice. At the same time, each of the 20 different advice sheets was distributed in each single round to one participant in the *High-advice*-treatment.

Treatment 5 – *Low-advice*. The 20 subjects in this treatment were presented the advice sheets from the *low-compliance* group of *Control*. In all other aspects this treatment is analogous to the *High-advice*-treatment. Appendix B lists all advices that were used in *High-advice* and *Low-advice* as well as the schedule of how advice sheets were distributed across subjects and periods.

The five different treatments allow an investigation of the influence of advice-giving or the access to summary statistics of peers on the degree of tax evasion. Since we divided the subjects from the *Control*-treatment by the median of overall compliance rates, both the *high-compliance* group and the *low-compliance* group exhibit some variance for single periods, both in compliance rates as well as in the advice put forward (see Appendix A and B).

After the tax evasion experiment we exposed all subjects in all session to the procedure of Holt and Laury (2002) to measure their individual risk attitudes. More precisely, subjects had to make ten choices between a relatively safe and a relatively risky lottery. The probabilities for the different outcomes of the lotteries were systematically varied from 0.1 to 1. In this procedure the number of safe choices (i.e. choosing the relatively safe lottery) can be used as an indicator of risk aversion.

The experiment was run with the help of z-tree (Fischbacher, 2007) and conducted at the University of Innsbruck. In order to facilitate calculations, we gave participants access to a calculator on the screen. In total, we had 120 participants in six different sessions, each of

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selected for payment we summed the profits that the advisees got in the very period where the saw they respective advice sheet.

them lasting approximately 75 minutes. Subjects earned € 18.22 on average, not including the extra payments to the four subjects that were paid for giving advice.

3 Results

3.1 Aggregate data

We start our analysis by presenting in Table 1 the overall average compliance rates, i.e. the percentage of the true endowment that is declared on average across the 20 periods. Note that we split the data from *Control* into the high-compliance group and the low-compliance group. Table 1 reports also the average true endowments and the standard deviation of both true endowments and compliance rates.

Table 1
Average true endowment (points) and compliance rate (percent of true endowment)*

	Control (*N* = 40)			*High-history* (*N* = 20)	*Low-history* (*N* = 20)	*High-advice* (*N* = 20)	*Low-advice* (*N* = 20)
	all	high compliance group	low compliance group				
True endowment	225.22 (82.30)	202.78 (81.71)	247.67 (76.66)	243.53 (81.88)	275.7 (86.86)	251.06 (73.3)	252.95 (73.3)
Compliance rate	64.48 (29.68)	88.53 (22.06)	40.53 (41.35)	41.76 (25.19)	28.22 (21.31)	46.03 (33.07)	13.12 (12.20)

* Standard deviations in parentheses

The main result emerging from Table 1 is that compliance rates decrease with social interaction, no matter whether subjects receive advice or history. Compliance rates in *Control* are significantly higher than in *High-history* ($p = 0.02$; two-sided Mann-Whitney U-test; $N = 15$), *Low-history* ($p = 0.007$; $N = 15$), or *Low-advice* ($p = 0.002$; $N = 15$).[7] The difference to *High-advice* ($p = 0.111$; $N = 15$) falls short of conventional significance, yet if we compare compliance rates in *High-advice* with the compliance rates in the high-compliance group of *Control* (those with above-median compliance), then the difference gets weakly significant ($p = 0.06$; $N = 10$).

Comparing the effects of receiving advice or observing compliance rates of a peer group we find that there is no significant difference between *High-advice* and *High-history* (46.0% vs. 41.8%; $p = 0.75$; $N = 10$). However, compliance rates in *Low-advice* are significantly smaller than in *Low-history* (13.1% vs. 28.2%; $p = 0.009$; $N = 10$), indicating that advice has a stronger impact on behavior when it is given from subjects who evade taxes very much.

[7] Note that for the non-parametric tests used in this subsection we use matching groups of four subjects as one independent unit of observation. For instance, the 40 participants in *Control* constitute 10 independent observations, and the 20 subjects in *High-history* 5 observations. All *p*-values reported in this subsection are based on two-sided tests.

3.2 The development of compliance over time

3.2.1 Graphical illustration

Figure 2 plots the compliance rates which subjects in the treatments with history observed (straight line) and those they have actually chosen (dotted line). Note that while the left-hand side of the figure shows a huge and significant difference between observed and chosen compliance rates in *High-history* (p = 0.007 when comparing the overall averages; Mann-Whitney U-test), on the right-hand side the differences are much smaller and not significant in *Low-history* (p = 0.17). From Appendix A it becomes evident that the tables for *High-history* include only very few subjects who have declared less than a third of their true endowment, while in the tables shown in *Low-history* a majority of subjects did so. Obviously, observing only a few subjects with low compliance rates suffices to drive down compliance rates very strongly even when the peer group is one with above-median compliance group as in *High-history*.

Figure 2
Observed versus actual compliance

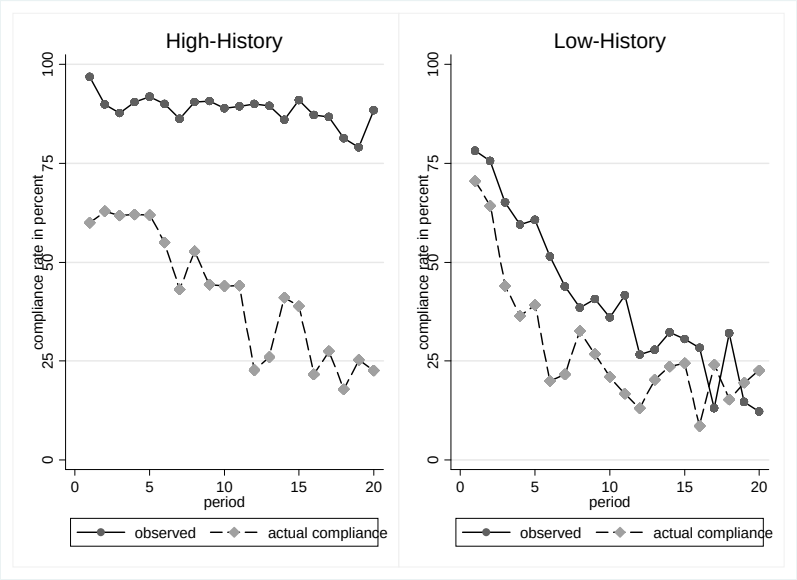
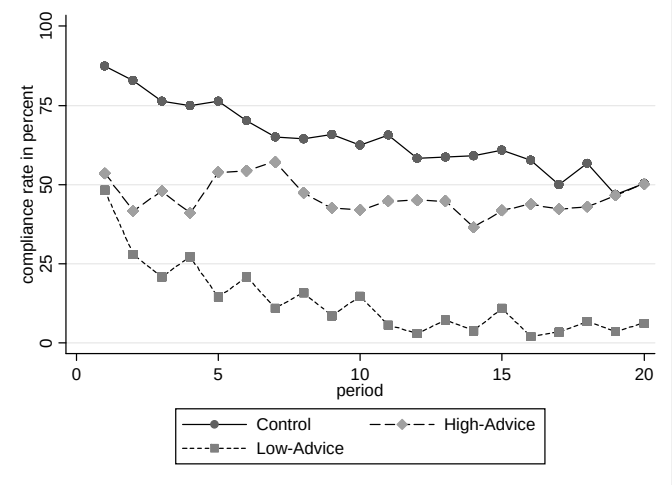


Figure 3 shows that compliance rates in *High-advice* and *Low-advice* and how they compare to the compliance rates in the corresponding groups of the *Control*-treatment. In both treatments, the actual compliance rates are substantially smaller than in *Control* (p < 0.05 when checking overall averages; Mann-Whitney-). However, it is also important to note that in both treatments the actual compliance rate tracks the trend of the compliance rate in the corresponding *Control*-group fairly well, meaning that in *High-advice* compliance rates are basically stable, whereas in *Low-advice* they show also a clear downward trend.

Figure 3
High-advice versus Low-advice



4 Conclusion

Our experiment has shown that providing information about peers reduces tax compliance significantly. We have been able to establish this finding in a tax-evasion experiment by comparing compliance rates of subjects who do not have access to any information about the behavior of others with subjects who are either informed about the compliance rates of a peer group or who receive advice from a peer group that has participated in the experiment before.

As such, our results relate to two hitherto separate strands of literature. First, the literature on the influence of peer group effects has documented the significant impact of peer effects for a wide variety of activities, such as academic achievement (Sacerdote, 2001; McEvan, 2003; Robertson and Symons, 2003; Schneeweis and Winter-Ebmer, 2007), drug abuse among youths (Lundborg, 2006), or criminal activities among detained offenders (Bayer et al., 2007). In general, it seems fair to conclude from this literature that individual behavior often assimilates to the kind of behavior in one's peer group. Second, the literature on naïve decision making (pioneered by Schotter, 2003) has shown that receiving advice from other subjects or observing the behavior of others in experimental games induces subjects in many situations to behave similarly, and in particular in a way they would less likely have behaved without observing others or receiving advice (see Schotter and Sopher, 2003, 2007; Chaudhuri et al., 2006; Kocher et al., 2007).

In our paper we have studied the effects of naïve advice and observational learning in an experimental game. However, the novelty of our design is the fact that we have set up two different peer groups from whom to receive advice or whom to observe. The peer groups differ with respect to their overall compliance rates. The above-median compliance group can be considered the "good" environment for tax morale, whereas the below-median compliance group can be regarded as the "bad" one.

The most striking finding, from our point of view, is the fact that both in the "good" and the "bad" environment subjects evade taxes more often than in a control group without any information on peers. That means that revealing information about peers may be detrimental for tax honesty even if the peer group has rather high compliance rates (of almost 90%, which must be considered as very high in the context of experimental tax evasion games). Hence, it seems a reasonable strategy of tax authorities around the world to prevent the leakage of key data such as compliance rates, audit rates, and so on. In other words, publishing data on tax evasion, benefit fraud or clandestine employment could have severe detrimental effects on tax

morale and, consequently, on tax revenues, because such information may remove other subjects' scruples to evade taxes themselves.

In fact, we attribute the higher degree of tax evasion in our four treatments with information on peers (*High-history, Low-history, High-advice,* and *Low-advice*) to the fact that observing less than full compliance by others or receiving advice that suggests to evade taxes supports the perception that tax evasion is socially accepted – and obviously a widespread phenomenon in a group of peers. As a consequence, subjects' moral scruples to evade taxes are lowered and their focus of attention shifts towards payoff maximization.

The revelation of information about and from peers may also affect tax morale through the channel of conditional cooperation. As Frey and Torgler (2007) have found out, subjects consider tax evasion more acceptable if they expect more others to be tax evaders. In other words, in an environment where most subjects evade taxes no one wants to be the "sucker" who provides the public good of paying taxes honestly while the others do not. A large body of experimental evidence has shown that conditional cooperation is a robust and widespread behavioral regularity that can explain a large part of contributions in public goods experiments (Keser and van Winden, 2000; Fischbacher et al., 2001; Brandts and Schram, 2001; Kocher et al., 2008). In our experiment, the revelation of compliance rates in the peer group (in treatments *High-history* or *Low-history*) may be considered as the best available estimate about the tax honesty of subjects' interaction partners in these treatments. The motive of conditional cooperation may then lead subjects to base their decision about tax compliance rather on the least honest taxpayers in the peer group (who have the lowest compliance rates) than on the average compliance rate in the peer group. By doing so subjects decrease their likelihood of being worse off than their interaction partner. Consequently, we should observe lower compliance rates in these treatments than in the *Control*-treatment where subjects have no information about peers.

In our treatments with naïve advice (*High-advice* and *Low-advice*) we have found that receiving advice has a similar effect of reducing compliance rates as receiving information about compliance rates. In section 4.3 we have shown that putting forward the argument of maximizing one's payoff drives down compliance rates significantly. Hence, subjects respond to the advice they receive. It is important to note then that advisors typically suggest compliance rates that are smaller than the ones they have chosen themselves in the *Control*-treatment (see section 4.2.2). As a consequence, the compliance rates in the treatments with advice become significantly smaller than in the control group without advice from peers.

In sum, our paper has confirmed the influence of information from and about peers in the setting of a tax evasion experiment. As such, our paper complements in a wider perspective the literature on peer pressure and peer effects, and in a more narrow perspective the literature on the determinants of tax evasion. From a practical point of view, our paper raises a few new questions. For instance, future research might want to examine which kind of information from and about peers *in*creases rather than *de*creases tax compliance. Tax authorities might be interested whether including in the communication with taxpayers some specific information about the compliance of peers increases or decreases tax compliance.[8] If tax authorities wanted to include such information, then another open question would concern the appropriate level of aggregation. In our experiment we have not varied the kind of information presented to participants (terciles of compliance rates and overall average), yet it might be worth examining how different statistical presentations of compliance data affect compliance rates.

[8] So far, there is quite some evidence on the influence of tax authorities commenting on the likely audit rates (Slemrod et al., 2001) or adding rental property schedules to increase tax honesty by reducing deduction claims (Wenzel and Taylor, 2004). However, we are not aware of tax authorities including information on tax compliance within a given jurisdiction and the likely effects of such information.

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Appendix A: Tables seen in the History-treatments

Table C1

High-history

	Period 1		**Period 11**
Declared 0-33 percent	0	Declared 0-33 percent	0
Declared 34-66 percent	1	Declared 34-66 percent	2
Declared more than 66 percent	19	Declared more than 66 percent	18
Average declaration (in percent)	96.8	Average declaration (in percent)	89.4
	Period 2		**Period 12**
Declared 0-33 percent	1	Declared 0-33 percent	1
Declared 34-66 percent	1	Declared 34-66 percent	1
Declared more than 66 percent	18	Declared more than 66 percent	18
Average declaration (in percent)	89.9	Average declaration (in percent)	90.0
	Period 3		**Period 13**
Declared 0-33 percent	1	Declared 0-33 percent	1
Declared 34-66 percent	1	Declared 34-66 percent	2
Declared more than 66 percent	18	Declared more than 66 percent	17
Average declaration (in percent)	87.7	Average declaration (in percent)	89.5
	Period 4		**Period 14**
Declared 0-33 percent	1	Declared 0-33 percent	0
Declared 34-66 percent	0	Declared 34-66 percent	4
Declared more than 66 percent	19	Declared more than 66 percent	16
Average declaration (in percent)	90.5	Average declaration (in percent)	86.0
	Period 5		**Period 15**
Declared 0-33 percent	0	Declared 0-33 percent	0
Declared 34-66 percent	1	Declared 34-66 percent	2
Declared more than 66 percent	19	Declared more than 66 percent	18
Average declaration (in percent)	91.9	Average declaration (in percent)	91.0
	Period 6		**Period 16**
Declared 0-33 percent	1	Declared 0-33 percent	1
Declared 34-66 percent	1	Declared 34-66 percent	2
Declared more than 66 percent	18	Declared more than 66 percent	17
Average declaration (in percent)	90.0	Average declaration (in percent)	87.2
	Period 7		**Period 17**
Declared 0-33 percent	2	Declared 0-33 percent	2
Declared 34-66 percent	2	Declared 34-66 percent	1
Declared more than 66 percent	16	Declared more than 66 percent	17
Average declaration (in percent)	86.2	Average declaration (in percent)	86.7
	Period 8		**Period 18**
Declared 0-33 percent	1	Declared 0-33 percent	2
Declared 34-66 percent	1	Declared 34-66 percent	2
Declared more than 66 percent	18	Declared more than 66 percent	16
Average declaration (in percent)	90.5	Average declaration (in percent)	81.3
	Period 9		**Period 19**
Declared 0-33 percent	1	Declared 0-33 percent	2
Declared 34-66 percent	1	Declared 34-66 percent	4
Declared more than 66 percent	18	Declared more than 66 percent	14
Average declaration (in percent)	90.7	Average declaration (in percent)	79.0
	Period 10		**Period 20**
Declared 0-33 percent	2	Declared 0-33 percent	1
Declared 34-66 percent	1	Declared 34-66 percent	1
Declared more than 66 percent	17	Declared more than 66 percent	18
Average declaration (in percent)	88.9	Average declaration (in percent)	88.4

Table C2
Low-history

	Period 1		**Period 11**
Declared 0-33 percent	3	Declared 0-33 percent	9
Declared 34-66 percent	2	Declared 34-66 percent	4
Declared more than 66 percent	15	Declared more than 66 percent	7
Average declaration (in percent)	78.1	Average declaration (in percent)	41.7
	Period 2		**Period 12**
Declared 0-33 percent	4	Declared 0-33 percent	14
Declared 34-66 percent	0	Declared 34-66 percent	2
Declared more than 66 percent	16	Declared more than 66 percent	4
Average declaration (in percent)	75.6	Average declaration (in percent)	26.6
	Period 3		**Period 13**
Declared 0-33 percent	6	Declared 0-33 percent	14
Declared 34-66 percent	2	Declared 34-66 percent	2
Declared more than 66 percent	12	Declared more than 66 percent	4
Average declaration (in percent)	65.1	Average declaration (in percent)	27.9
	Period 4		**Period 14**
Declared 0-33 percent	7	Declared 0-33 percent	13
Declared 34-66 percent	2	Declared 34-66 percent	1
Declared more than 66 percent	11	Declared more than 66 percent	6
Average declaration (in percent)	59.5	Average declaration (in percent)	32.3
	Period 5		**Period 15**
Declared 0-33 percent	5	Declared 0-33 percent	13
Declared 34-66 percent	5	Declared 34-66 percent	3
Declared more than 66 percent	10	Declared more than 66 percent	4
Average declaration (in percent)	60.7	Average declaration (in percent)	30.5
	Period 6		**Period 16**
Declared 0-33 percent	8	Declared 0-33 percent	12
Declared 34-66 percent	4	Declared 34-66 percent	5
Declared more than 66 percent	8	Declared more than 66 percent	3
Average declaration (in percent)	51.4	Average declaration (in percent)	28.4
	Period 7		**Period 17**
Declared 0-33 percent	10	Declared 0-33 percent	15
Declared 34-66 percent	3	Declared 34-66 percent	5
Declared more than 66 percent	7	Declared more than 66 percent	0
Average declaration (in percent)	43.8	Average declaration (in percent)	13.1
	Period 8		**Period 18**
Declared 0-33 percent	12	Declared 0-33 percent	14
Declared 34-66 percent	3	Declared 34-66 percent	0
Declared more than 66 percent	5	Declared more than 66 percent	6
Average declaration (in percent)	38.5	Average declaration (in percent)	32.0
	Period 9		**Period 19**
Declared 0-33 percent	11	Declared 0-33 percent	17
Declared 34-66 percent	1	Declared 34-66 percent	1
Declared more than 66 percent	8	Declared more than 66 percent	2
Average declaration (in percent)	40.7	Average declaration (in percent)	14.6
	Period 10		**Period 20**
Declared 0-33 percent	11	Declared 0-33 percent	16
Declared 34-66 percent	4	Declared 34-66 percent	3
Declared more than 66 percent	5	Declared more than 66 percent	1
Average declaration (in percent)	36.0	Average declaration (in percent)	12.2

Appendix B: Advices (translated from German)

4.1.1 B1: Advices that have been distributed in *High-advice*

4.1.2 Advice 1:

Declare 99 percent of your endowment
Why: Behave strategically, make some (or many) decisions at random in order to be incalculable for the other participants.

4.1.3 Advice 2

Declare 100 percent of your endowment
Why: Close to true endowment, because you are occasionally audited. If you deviate too much from the real endowment, you will pay an extra deduction.

4.1.4 Advice 3

Declare 98 percent of your endowment
Why: - 30% of the endowment and + 110% of half of the sum of the declared endowments, the profit is higher if the endowment is higher.

4.1.5 Advice 4

Declare 50 percent of your endowment
Why: 50 is the golden mean.

4.1.6 Advice 5

Declare 50 percent of your endowment
Why: THE DEDUCTION FROM THE DECLARED endowment is quite low this way – the probability of an audit is only 5%, is thus rather improbable. Also, the refund is with 50% of the real endowment rather secure.

4.1.7 Advice 6

Declare 100 percent of your endowment
Why: because then there is no threat of an extra deduction, no fear of audits, because the declared endowment is true. Otherwise, you could get a huge fine, a secure method is in the long run better.

4.1.8 Advice 7

Declare 89 percent of your endowment
Why: because it is a lower value of the endowment.

4.1.9 Advice 8

Declare 56 percent of your endowment
Why: It would be interesting to have two rounds of questions. That would be fairer, sometimes the questions suit to someone, sometimes they do not.

4.1.10 Advice 9

Declare 90 percent of your endowment
Why: The difference that has to be paid in case of audit is low. To declare little endowment means gaining less from the pot.

4.1.11 Advice 10

Declare 100 percent of your endowment
Why: You can never know how much the others declare and whether you are audited. Therefore, it is better to play it safe and declare 100 percent.

4.1.12 Advice 11

Declare 68 percent of your endowment

Why: The risk of audit is relatively low, nevertheless the risk would keep within a limit. Also, the partner will get something through the refund.

4.1.13 Advice 12

Declare 60 percent of your endowment
Why: The less percent you declare, the more you can gain. The deduction is based on the declared endowment.

4.1.14 Advice 13

Declare 100 percent of your endowment
Why: because then the deduction is bigger. One gets a refund of half of the sum of the deductions times 110 percent. If the deduction is higher, the refund is higher and no extra deductions if you declare 100%.

4.1.15 Advice 14

Declare 60 percent of your endowment
Why: 1.) declare a lower amount of endowment than you have declared, because then you will make less losses in case that points are deducted; 2.) or declare the exact amount of the endowment, but points will be deducted anyway.

4.1.16 Advice 15

Declare 5 percent of your endowment
Why: If the true endowment is a high number (i.e. 423), then one should take a risk and declare a low endowment in order to get less deduction (risk: if you are audited)

4.1.17 Advice 16

Declare 60 percent of your endowment
Why: It is better to keep something in order to compensate losses or deductions. In any case, declare more than half to signalize that you are willing to take a risk.

4.1.18 Advice 17

Declare 70 percent of your endowment
Why: because 30% are deducted, 70% remain.

4.1.19 Advice 18

Declare 100 percent of your endowment
Why: This is secure. If one is a lucky dog, one should sometimes declare only 80%.

4.1.20 Advice 19

Declare 75 percent of your endowment
Why: In case of an audit the extra deductions are not that high.

4.1.21 Advice 20

Declare 100 percent of your endowment
Why: It is better to play it safe than to lose more points if one declares a lower amount.

4.1.22 B2: Advices that have been distributed in *Low-advice*

4.1.23 Advice 1:

Declare 5 percent of your endowment
Why: The probability that you are audited and caught is so low that even in case of a fine, the gains from low declarations is not destroyed. One breaks only rules established by the university, no universal, ethic,…

4.1.24 Advice 2

Declare 1 percent of your endowment
Why: To lose 3 times the deduction at only 5% probability is a bearable risk. You can rip off your opponent. This will bring a lot especially if the other does not follow a similar strategy. Thus, declare as little endowment as possible.

4.1.25 Advice 3

Declare 0 percent of your endowment
Why: Follow this advice especially if you have already been audited. 5% of 20 rounds = 1x. A second audit is extremely improbable. Before, a more cautious strategy is appropriate.

4.1.26 Advice 4

Declare 100 percent of your endowment
Why: Cooperation is everything! The deductions are refunded at 110%. Trust your business partner.

4.1.27 Advice 5

Declare 0 percent of your endowment
Why: The chance of being audited is very low.

4.1.28 Advice 6

Declare 5 percent of your endowment
Why: The ratio between audit and deduction (loss) leaves no big margins. “Honesty does not pay”. “Life is a game”.

4.1.29 Advice 7

Declare 50 percent of your endowment
Why: I would advise to declare at least once or twice the true income. I was audited only once and lost 170 points and declared almost always between 0 and 10. Declare always a low endowment, at a probability of 5% you are not audited more than 2 times. Thus, one makes profit in any case.

4.1.30 Advice 8

Declare 30 percent of your endowment
Why: If you declare little, you will be deducted little. This means your profit increases. If you are caught, the difference between true endowment and 1/3 of the endowment is high, but bearable. Pay close attention to the deductions – you are selected at random. If you have already been selected, the probability that you are selected once again is lower.

4.1.31 Advice 9

Declare 30 percent of your endowment
Why: The less you declare, the less will be deducted!!! If you are audited, the deductions at roughly 30% are bearable.

4.1.32 Advice 10

Declare 0 percent of your endowment
Why: The probability of 5% is minimal. Besides, keep it yourself instead of donating money (to the bad players who all declare 0).

4.1.33 Advice 11

Declare 0 percent of your endowment
Why: The probability that you are caught is only 5%, but you pay 30% in order to secure yourself against an audit.

4.1.34 Advice 12

Declare 1 percent of your endowment

Why: An audit occurs with probability 5% and that is very low. I was audited twice out of 20x and that pays in the end. Why not 0%? – Superstition! With it, you are secure.

4.1.35 Advice 13

Declare 100 percent of your endowment
Why: …this way you will make no losses.

4.1.36 Advice 14

Declare 70 percent of your endowment
Why: extra deduction = 3 * (100%-70%) = 90%, that means I pay at most 90% of my endowment as a fine. At an audit, otherwise, I pay less than I should.

4.1.37 Advice 15

Declare 40 percent of your endowment
Why: Because the probability of an audit is rather low and you can gain something, or save something. To take a risk pays only with higher amounts. At those, I would declare only 1% or 2%. But you could lose a lot too.

4.1.38 Advice 16

Declare 5 percent of your endowment
Why: You are audited with a probability of 5 percent. If you are not audited, which is very likely, then you have good chances to make high profits. (That depends, of course, also on the amounts the other participant declared). If you are audited, you had once bad luck, but in the long run the chance of being audited more often is low.

4.1.39 Advice 17

Declare 1 percent of your endowment
Why: 1%, because the deductions are lower (higher profits).

4.1.40 Advice 18

Declare 10 percent of your endowment
Why: If you are lucky and your partner declares a higher percentage, then you will win.

4.1.41 Advice 19

Declare 0 percent of your endowment
Why: The probability of an audit is very low and so you can profit from the deductions of the other participants. I was never audited.

4.1.42 Advice 20

Declare 20 percent of your endowment
Why: The less endowment you declare the more you gain and if you are audited the deduction does not hurt very much. It would be best to declare only 1% after an audit, because then the probability of an additional audit is very low. In total, it depends on whether you are risk-loving.