Arbeitskreis Quantitative Steuerlehre
Quantitative Research in Taxation – Discussion Papers

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arqus Discussion Paper No. 164

May 2014

www.arqus.info

ISSN 1861-8944

THE EFFECT OF TAX PRIVACY ON TAX COMPLIANCE—

*AN EXPERIMENTAL INVESTIGATION**

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In this paper, a tax game with audit costs as a public bad is designed to investigate the impact of public disclosure on tax evasion experimentally. Three different types of tax privacy are tested, ranging from complete privacy to full disclosure. We expect to observe two different effects: first, a contagion effect, arising when an individual observes non-compliance of other individuals and therefore reduces his own tax compliance; second, a shame effect of increased tax compliance due to the anticipated shame of being declared a tax evader. We find evidence of increasing tax evasion with reduced tax privacy if information is disclosed anonymously. Our results also indicate that the shame effect is not strong enough to override the contagion effect when both effects are present. Our results are of particular importance for fiscal policy because public disclosure may lead to more evasion instead of less, due to motivational crowding-out of tax morale.

Keywords: Tax privacy · Tax evasion · Public bad · Social norm · Conditional cooperation · Economic experiment

JEL Codes: H24 · H26 · H30

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* We thank Friedel Bolle, René Fahr, Martin Fochmann, Axel Möhlmann, and seminar participants at the Freie Universität Berlin, University of Göttingen, University of Hannover, and University of Paderborn for helpful comments and suggestions.

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I Introduction

Death is certain, but paying taxes is definitely not—at least, not for everyone. According to the Internal Revenue Service, the tax gap in the United States amounts to $385 billion for tax year 2006 alone, mainly due to underreported income (IRS, 2013). Non-compliance reduces both public revenue and the availability of public services and also discriminates against honest taxpayers (Alm, 2012). Therefore, fighting tax evasion is an important issue for policy agendas. A number of countries (e.g., Greece and New Zealand) publicly list tax evaders to combat tax evasion. Others (e.g., Finland, Iceland, Norway, and Sweden) disclose all tax return information. However, the majority of countries treat tax information confidentially.

The main reason to disclose tax compliance information publicly is to deter people from evading taxes by threatening them with the shame of being announced as tax evaders. In addition to the imposition of monetary penalties, shame should be effective as a non-monetary sanction. However, it is far from obvious that a strategy of tax publicity is a successful instrument for fighting tax evasion. Previous research has demonstrated that social norms have a considerable impact on tax evasion (Cullis et al., 2012). Individuals comply as long as they believe that compliance is the social norm (Alm, 2012). Gino et al. (2009) show that the observation of unethical behavior of another person is potentially contagious because it may change the social norms regarding dishonesty. Therefore, at the same time that publishing information could be a deterrent to tax evasion (via the shame effect), it could also destroy the social norm of compliance. This effect would be in keeping with a strand of literature that shows that taxpayers are conditionally cooperative, i.e., people are willing to comply as long as others do (e.g., Frey and Torgler, 2007; Traxler, 2010). Due to these potentially opposing effects—increasing shame on the one hand, risk of contagion on the other—the overall effect of public disclosure on tax compliance remains unclear.

Our study investigates different levels of publicized tax evasion to determine whether the shame effect or the contagion effect dominates as an overall behavioral response. Using a lab experiment enables a controlled variation of tax privacy as a treatment variable. We design a specific tax game with a baseline treatment of no public disclosure. In one treatment, individual tax information is disclosed publicly in an anonymous manner; therefore, only the contagion effect may arise. In another treatment, public disclosure occurs by displaying the pictures of the players next to their tax information, introducing both shame and contagion effects.

Overall, we find evidence of increasing tax evasion with reduced tax privacy if information is disclosed anonymously. In the case of full public disclosure, i.e., when both the contagion and shame effects are present, shame is not strong enough to override contagion. Regarding tax policy these findings imply that lawmakers should be extremely cautious about implementing universal tax return publicity because more evasion could result due to motivational crowding-out of tax morale.

The remainder of this paper is organized as follows. In section II, we give a brief literature review with its implications for our study. In section III, the tax game and the experimental design of the study are described. The results are provided and discussed in section IV, and section V concludes.

II Tax evasion theory

The economic theory of tax evasion is primarily based on the work of Allingham and Sandmo (1972), which assumes that each individual maximizes expected utility after taxes, applying a certain audit probability and penalty level. However, it has been shown that actual tax compliance often differs from the predictions of this model (e.g., Dhami and al-Nowaihi, 2007), and the model has therefore been modified in several aspects (see for a recent review Pickhardt and Prinz 2014). With respect to tax publicity, the following two modifications seem particularly important. First, Erard and Feinstein (1994) account for moral sentiments, particularly guilt and shame, and empirically show that sentiments can be important determinants of compliance. According to Markel (2001, p. 2179) shame can be defined as “the emotion one feels when subjected to public degradation, whereas guilt is the emotion one feels after consciously becoming aware of wrongdoing over which one feels responsible”. Kirchler (2007) notes that “anticipated shame becomes (…) a cost factor in evaluating one's likely advantages and disadvantages of tax evasion.” In this vein, Erard and Feinstein (1994) assume in their model that individuals experience utility-reducing shame when they evade taxes and are subsequently audited. Shame reduces the benefits of evasion and decreases its occurrence. If tax publicity increases shame, disclosure could help to reduce evasion. Second, Traxler (2010) incorporates tax morale, as internalized social norm of tax compliance, into the Allingham and Sandmo (1972) standard model. Taxpayers are assumed to conditionally cooperate because their level of evasion depends on others' compliance. The results imply that strategies that increase belief in high compliance levels reduce tax evasion. Consequently, publishing information about actual tax evasion could alter this belief in a high compliance level and thus destroy the corresponding social norm. This effect would conform to the observation of the contagion effect of unethical behavior in Gino et al. (2009) or the widely discussed broken window hypothesis (Wilson and Kelling, 1982) for which there is also some experimental evidence in the context of tax compliance (Lefebvre et al. 2013).

Hence, tax publicity might trigger two opposing effects simultaneously—the shame effect and the contagion effect. The sum of the overall effect of tax publicity is theoretically unclear; therefore, its positive or negative impact is a matter for empirical investigation. Despite its great policy importance, direct evidence on the effects of tax privacy is scarce. To our knowledge, there exists neither a theoretical study that incorporates both these effects nor an empirical study of public tax disclosure that simultaneously evaluates the contagion and shame effects on tax compliance. However, there are three experimental and two empirical studies on the effects of tax publicity.

Using a between-subject one-shot tax compliance game, Bosco and Mittone (1997) examine the effect of tax audit publicity on tax evasion. Subjects earned taxable income in a real-effort task and subsequently decide how much taxes to evade given a uniform tax rate and audit probability. In the tax publicity (anonymity) treatment subjects are informed in advance that the audit process is public (anonymous). The findings show, however, no deterrent effect of tax publicity. Rather, a positive effect of tax audit publicity on evasion is observed suggesting that subjects in this experiment do not link feelings of shame with being announced as tax evader.

Laury and Wallace (2005) investigate the impact of tax confidentiality experimentally in a between-subjects design. Individuals decide how much of a given income to report to the tax authority under two different treatments. Subjects are informed about the tax rate, the (exogenous) audit probability, and the relevant fine. In the first treatment, full confidentiality is employed. In the second treatment, only partial confidentiality is used, and 25% of the subjects' decisions are disclosed to all other participants. However, the decisions are anonymous and cannot be tracked to the actual person making the decision. The results show that reported income is typically higher under the partial confidentiality treatment; however, this difference is significant only in five out of twenty periods. Moreover, when controlling for demographic variables (particularly gender, marital status, student of economics, raised in North America) the treatment effect becomes insignificant. By and large, the results of this study are ambiguous.

Coricelli et al. (2010) use a within-subjects design to study the impact of tax publicity on compliance. Subjects, in groups of eight players, decide individually how much income to declare. The declared income is subject to a uniform tax rate. Again, taxpayers are informed about audit probability and fines. The treatment variable is the publication of individual pictures. In half of the trials, if an audit reveals that a player underreported his income, a picture of the detected evader is displayed on the group members' screens. The results show that tax publicity reduces both the number of evaders and the amount of tax evaded. The risk of being "named and shamed" as an evader diminishes the probability that an individual will evade taxes by 8.2%.

In addition to these experimental studies, two archival studies were recently published. Hasegawa et al. (2012) use Japanese data where disclosure of both individual and corporate income tax information was mandatory from 1950 until 2004. These data show no evidence that companies reduced declared taxable income after the disclosure requirement was abolished in 2004. However, companies prefer to avoid public disclosure and therefore decreased reported taxable income to be below the threshold beyond which disclosure is required. In contrast to Hasegawa et al. (2012), Slemrod et al. (2013) report evidence that public disclosure of tax returns on the Internet increases reported income. These authors' estimate of the effect of public disclosure on tax compliance based on a quasi-experimental study using data from Norwegian income tax statistics is an increase in reported income of 3%. However, in both archival studies taxable income must serve as a proxy for tax compliance because true evasion remains unknown. Only in the controlled environment of a lab experiment tax evasion is fully observable.

Overall, evidence on the effect of tax publicity is rare and the data available are ambiguous. While Laury and Wallace (2005) find only a weak effect from tax publicity and Hasegawa et al. (2012) as well as Bosco and Mittone (1997) find no deterrent effect from tax publicity, the results of Slemrod et al. (2013) and Coricelli et al. (2010) indicate that abolishing tax privacy laws could increase tax compliance. Moreover, the consequences of different sorts of tax publicity are unclear. For example, the weak results of Laury and Wallace (2005) could be due to the anonymous form in which tax return information is announced in these studies. There, it can be expected that anticipated shame is no deterrent because participants remain anonymous. If, however, a person can be identified by the other participants—e.g., by displaying photos of the subject as in the experiment by Coricelli et al. (2010)—shame should be exacerbated. This might explain why, in contrast to Laury and Wallace (2005), Coricelli et al. (2010) find a strong positive effect of disclosure on compliance.

Interestingly, neither Laury and Wallace (2005) nor Coricelli et al. (2010) implements a public good or any refund of taxes. This neglects the fact that public good games provide a standardized opportunity for studying social interactions within groups (Frey and Torgler, 2007). Outside of such a public good context, externalities may not arise; hence, there is no need for social norms (Huck et al., 2012). This possibility raises the question of whether these experiments underestimate the contagion effect.[4] Conditional cooperators do contribute as long as others contribute, but without a public good context, there is neither necessity nor an opportunity to cooperate. Without a direct public good context, it is unclear whether participants perceive their payment of taxes as contributions to a

[4] In contrast to Laury and Wallace (2005) and Coricelli et al., Bosco and Mittone (1997) redistribute taxes in their experiment. However, due to their one-shot design it is impossible to examine a potential contagion effect.

public good or simply as a cost. If one's own income is not affected by the decisions of other group members, rules of reciprocity or conformity may seem less important. Therefore, the contagion effect loses its bite. To answer this question, we conduct a tax experiment that is designed to investigate the responses to different levels of tax publicity in a public good context, which also allow us to draw conclusions about whether a shame or contagion effect dominates overall contributions.

III Experimental design

We adapt a standard public good game with repetition, inverting the public good by introducing audit costs as a public bad. The rationale behind this is that evasion requires the state to implement a costly tax audit administration that must be equally shared by all subjects. The costs of this administration reduce the endowment of each individual upfront, where each of the N subjects bears 1/N of the costs. In a world without evasion, there would be no need for tax audits. Thus, by complying with the tax law, subjects reduce the costs of tax audits, i.e., they reduce the public bad. However, if some subjects do not contribute their share of taxes, the state will need to increase the audit probability. That, in turn, increases the public bad. This idea is implemented in the following way:

A group of six players receives an aggregate endowment of 450 cents income in each round (each individual receives 75 cents).[5] Given a nominal tax rate of 30%, the group in total must pay income taxes of 135 cents (450×0.3) in each round. All taxes paid are removed from the game (there is no refund or compensation) and only tax evasion determines the public bad in the form of changing audit costs. In the case of equal contribution, every member of the group would have to pay 22.5 cents, but subjects can decide individually how much tax they wish to pay. Hence, subjects are given the chance to evade taxes, where the individual evasion is given by

$$E_{i,t} = \max \; [22.5 - \tau_{i,t}; 0], \qquad (1)$$

[5] Cent is the experimental currency and for every cent subjects earn they receive a euro-cent as payoff.

with $\tau_{i,t}$ being the tax paid by subject *i* in round t, and $E_{i,t}$ being the resulting evasion.

Total evasion determines the administration cost incurred in the next round. To simplify matters, the cost of audit administration is equal to the sum of taxes evaded by all subjects of one group in the previous round given by

$$C_t = \sum_{i=1}^{6} E_{i,t-1}, \tag{2}$$

with C_t being the cost of round t, and $E_{i,t-1}$ being the evasion of individual *i* in round t-1. This cost may be perceived as damage done that must be borne equally by all subjects, i.e. $C_{i,t} = {}^{C_t}/_{6}$. As long as there is no individual tax evasion, the cost of audit administration remains zero. Therefore, by paying taxes, subjects reduce the implemented public bad. If some subjects choose to ride completely free or not fully contribute their sixth of the demanded tax, the cost of audit administration increases. Because cost must not be negative, i.e., $C_t \geq 0$, the overpayment of taxes is not refunded. Audits to combat tax evasion occur with a given probability that is dependent on the tax evasion present in the previous round. Thus, audit probability is endogenous and defined by

$$p_t = 0.005 \times C_t, \tag{3}$$

with p_t being the audit probability of round t, and C_t being the cost from (2). An overview of these relations between taxes paid, cost, and audit probability is provided in Table 3 of Appendix A.

Audits occur at the individual level based on the probability given in (3). That is, even though audit probability is equal for all subjects within the same group for a given period ($p_t = p_{i,t}$) actual audits may occur only for a subgroup. After every round, it is determined whether declared taxes are sufficient for each subject. If an audit occurs and tax evasion is detected (i.e., if $E_{i,t} > 0$), the subject has to pay immediately the amount $E_{i,t}$ plus a penalty of 50% of $E_{i,t}$. Hence,

$$\theta_{i,t} = \begin{cases} 1.5 \times E_{i,t} & \text{if an audit occurs,} \\ 0 & \text{otherwise} \end{cases}, \tag{4}$$

with $\theta_{i,t}$ being the sanction for subject *i* in round *t*. Note also that we follow the recommendation of Alm (2010) and describe the game in neutral language to avoid subjects using individual scripts when interpreting loaded terms (i.e., instead of the term "tax," the term "fee" is used).

In each period, subjects' experimental income is equal to the original endowment of 75 cents less taxes, costs, and sanctions. The overall wealth equals the sum of the 20-period earnings and the payoff from the experiment is given by

$$\pi_i = \sum_{t=1}^{20} \left(75 - 22.5 + E_{i,t} - \frac{C_t}{6} - \theta_{i,t}\right) \tag{5}$$

In a between-subjects design, we implement three treatments that differ in their degree of tax privacy. In our baseline treatment (no information treatment), subjects are only aware of the group's overall tax gap, but have no information on individual tax evasion. This setting reflects tax privacy as it is in most countries. The degree of tax evasion is (at least roughly) known, but individual misconduct is not. Due to the missing information on individual evasion, we expect the potential for a contagion effect to be low in the baseline treatment. In the second and third treatments, there is no tax privacy: all subjects are directly informed about the individual behavior of their group members after each round—everyone knows who evades taxes and to what extent. The difference between treatments two and three is the type of publication. In the second treatment (partial information treatment) only subject numbers are stated, but subjects did not know which number belongs to which group member. This treatment corresponds to the partial confidentiality treatment in Laury and Wallace (2005). Moreover, because desks are individually separated, other subjects and their screens are not visible. In particular, subjects do not know about others' earnings and each subject receives his or her payoff not within the laboratory but in a separate office in absence of other participants. Feelings of shame should not arise under the anonymous disclosure used in this treatment. However, because subjects are provided with information regarding the individual behavior of the other subjects, a contagion effect could result. In the third treatment (full information treatment), photos of subjects are paired with each subject's tax behavior.[6] This treatment is equivalent to the picture treatment in Coricelli et al. (2010). Due to individual disclosure, both shame and contagion effects can be expected. By comparing both the partial and the full information treatments, we can identify whether the shame or contagion effect predominates when ceding tax privacy. By comparing baseline and partial information treatment, we can identify if contagion affects evasion.

The experimental procedure is as follows: After entering the laboratory, subjects are randomly assigned to their group.[7] The subjects remain in the same group and are provided the same treatment throughout the whole experiment. Subjects are given instructions (see Appendix A) after being seated and as much time as they require to understand the procedure. Only after all subjects confirm that they fully understand the experimental instructions and do not have any remaining questions does the tax game begin.

Each experimental session consists of 20 rounds. Screenshots of the different stages of the experiment are provided in Appendix C. At the beginning of each round, subjects are informed about the taxes evaded in the previous round and about the resulting consequences, i.e., the group's total cost according to (2) and the current audit probability according to (3).[8]

Then, subjects are informed about their endowment (75 cents), their share of the group's total costs, their endowment after costs are deducted, the required amount of taxes (22.5 cents), and the

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6 These pictures were taken before the experiment inside the laboratory. After the experiment was finished, all photos were deleted in the presence of the participants.

7 20 sessions were conducted with 13 sessions made up of 2 groups and another 7 sessions made up of one group. In 5 of the 7 sessions with only one group, individuals were assigned to the full information treatment. Because of the type of disclosure in this treatment, subjects would have recognized their group mates anyway. In the other 2 sessions with only one group all subjects were assigned to the no information treatment. Whenever there was a group assigned to the partial information treatment, there were another six subjects in the laboratory. This is important as fewer subjects in the lab may lead to an additional source of shame, in particular in the partial information treatment. We checked for this additional shame effect by excluding the two single groups from the no information treatment. The treatment effects are not affected.

8 Since there was no round 0, costs and audit probability for round 0 were given exogenously with $C_1 = 80$ and $p_1 = 0.40$.

current audit probability. At this point, subjects must decide (without time restrictions) how much in taxes they wish to contribute.

After deciding their contribution, subjects are informed of the group's total result. Subjects are also informed whether their contributions are subject to an audit[9] and, if applicable, on the amount of penalties they must pay.

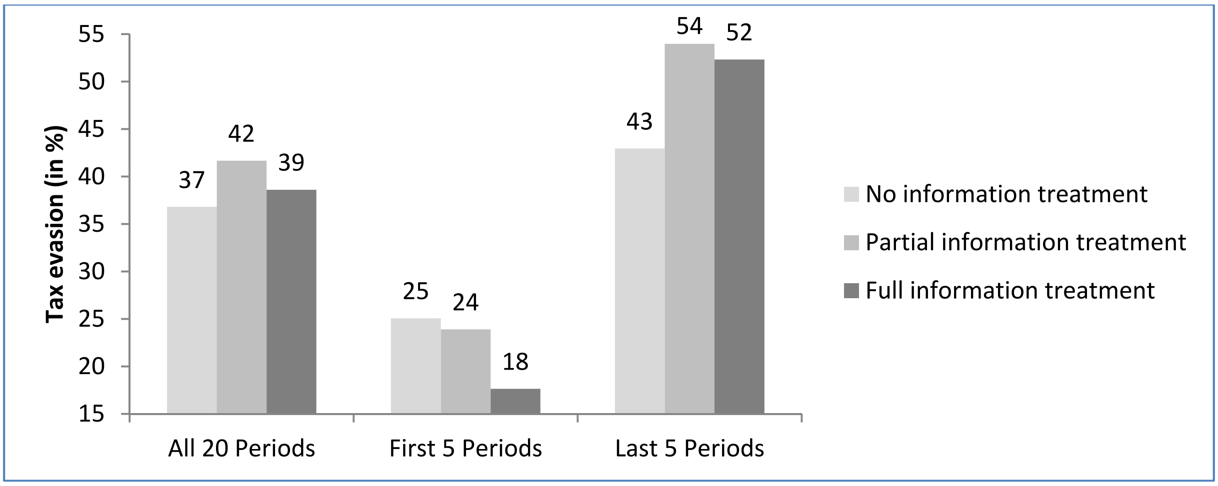
At the end of each round, subjects are informed about their results from the current round as well as their total wealth so far. When all 20 rounds are completed, subjects must answer a short questionnaire that seeks information regarding demographic variables including age, gender, and area of expertise. Then, individual risk aversion is measured using lottery decisions based on the procedure of Holt and Laury (2002). An extract of the full questionnaire is given in Appendix B.

IV Results

The overall result is an increase of non-compliance in the absence of tax privacy if individual information is disclosed anonymously. Our results from the partial information treatment indicate that this result is due to the contagion effect. The results from the full information treatment show that shame does not weaken this effect significantly. Tax evasion is not lower in the full information treatment than in the baseline treatment, suggesting that the shame effect is not strong enough to override the contagion effect when both effects are present.

[9] For each subject a uniformly distributed random number between 0 and 1 was drawn. If this number did not exceed the current audit probability, an audit occurred.

Figure 1: Average individual tax evasion in the three treatments



A Descriptive statistics

Descriptive statistics are shown in Table 1. A total of 198 undergraduate and graduate students participated in the computer-based experiments, programmed and conducted under z-Tree (Fischbacher 2007). The experiment took place in the ViaLab of the European University Viadrina Frankfurt (Oder). Subjects earned an average of €9.99 (std. dev. €1.05). They were on average 22 years old and 85% were German. Of all subjects 58% are females and 68% are students of economics. Their average risk aversion (according to the measure by and Laury, 2002) amounts to 5.2. On average across all 20 periods, audit probability amounts to about 25%. Audit probability is lower in the first five periods (just under 20%), whereas it is higher in the last five periods (about 30%). Subjects were randomly assigned to the three treatments. 72 subjects were assigned to both the no information and the partial information treatments and another 54 subjects were assigned to the full information treatment. There are no significant differences between the three treatments with respect to age, risk aversion, percentage of economic students as well as audit probability and final profit.[10]

Overall, tax evasion is higher in the absence of tax privacy, as shown in the three outer left bars of Figure 1 and Table 1. In the no information treatment, the average tax evasion across all 20 periods is

[10] T-tests were conducted for the variables audit probability, final profit, age and risk aversion, whereas chi squared-test were conducted for the remaining variables. All p-values are greater than 68%.

8.28 cents (37% of the required taxes), whereas tax evasion amounts to 9.38 cents (42%) in the partial information treatment. This difference in average tax evasion is highly significant (two-sample t-test, $p < 0.01$).[11] Receiving information on individual non-compliance causes conditionally cooperative taxpayers to reduce their contributions, and thus the contagion effect is positive. Tax evasion is higher when information on other subjects does not contain personal information. In the full information treatment, tax evasion amounts to 8.69 cents (39%). The difference stemming from the shame effect is not significant in comparison to the evasion observed in the partial information treatment (two-sample t-test, $p = 0.15$), and the shame effect compensates for the contagion effect as tax evasion in the full information treatment is not significantly higher than in the baseline treatment ($p = 0.29$).

Nevertheless, it is standing to reason that contagion should take some time to develop whereas shame might affect behavior at most in the beginning periods. To this end, Figure 1 shows average tax evasion in the first and the last five periods of the experiment, respectively. In the first five periods—shown in the middle three bars—we expect shame causing lower tax evasion in the full information treatment. Indeed, tax evasion is 3.97 cents (18% of the required taxes) in the full information treatment, whereas tax evasion amounts to 5.64 cents (25%) in the no information treatment and 5.38 cents (24%) in the partial information treatment. Tax evasion is significantly lower in the full information treatment (pairwise t-tests, $p = 0.02$ and $p = 0.04$, respectively). There is no significant difference between the no information and the partial information treatment ($p = 0.64$). On the other hand, the three outer right bars of Figure 1 show the average tax evasion in the last five periods. In the no information treatment tax evasion is 9.66 cents (43%), whereas tax evasion amounts to 12.15 cents (54%) in the partial information treatment and 11.77 cents (52%) in the full information treatment. Tax evasion in the no information treatment is significantly lower than in the other two treatments (pairwise t-tests, $p < 0.01$ and $p = 0.02$, respectively), whereas there is no significant difference between the partial and the full information treatment ($p = 0.65$).

[11] We tested for difference in tax evasion between all three treatments and, hence, performed multiple testing. All p-values are adjusted according to Holm (1979).

These results indicate that shame actually has an immediate impact on tax evasion while contagion does not occur until later periods. When the contagion effect is fully evolved the shame effect that is obvious in the first periods is no longer visible.

Subjects may differ in their intrinsic willingness to pay taxes (e.g. Alm and Torgler 2006). On the one hand, there may be highly self-motivated subjects who should not be affected by the treatment as they are expected to always pay their taxes—or at least a large part of it. On the other hand, there may be subjects seeking only their personal best. Depending on the audit probability, these subjects are more likely to evade a large part of their tax obligation. Hence, they should be more sensitive to the given treatment. This would be in line with the results of a strand of literature showing the interaction of moral attitudes and economic incentives (e.g. Reckers et al. 1994). Therefore, we explicitly examine the behavior of the latter ("high evasion") group. Whenever tax evasion amounts to at least 20 cents (note that the tax obligation is 22.5 cents in each period) this is defined as "high tax evasion".[12]

Qualitatively, the results are almost identical to the results we obtained so far as shown in Figure 2. Across all 20 periods there are significant more high tax evasions in the partial information treatments than in the other two treatments.[13] In the first five periods, there are significantly less occurrences of high tax evasion in the full information treatment than in the no information treatment (chi-square test, $p = 0.04$) and the partial information treatment (chi-square test, $p = 0.08$) while the difference between the no information and the partial information treatment is not significant. The rare appearances of high tax evasion in the first five periods in the full information treatment are a strong indicator for the immediate impact of shame on subjects'

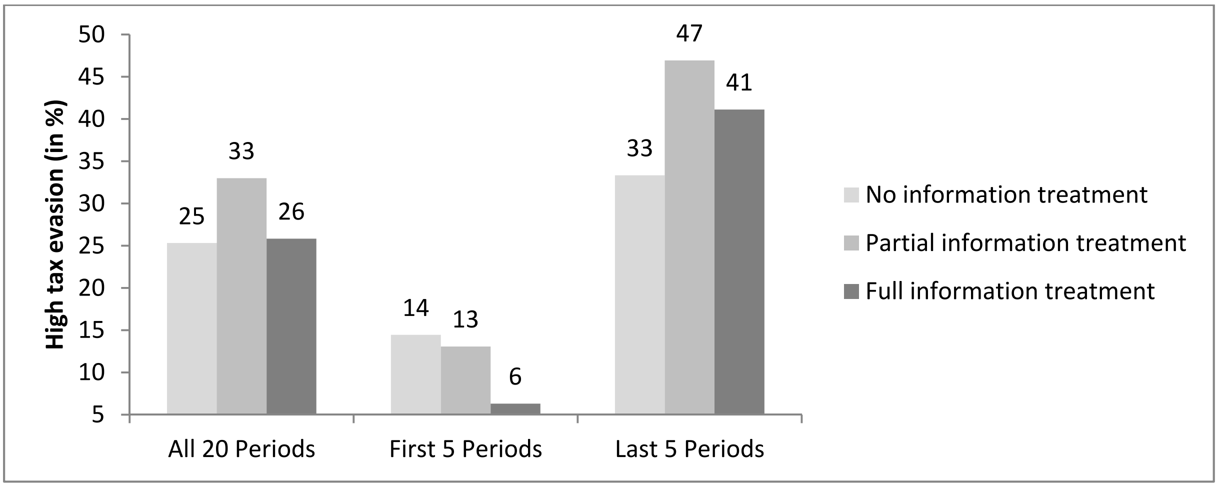
[12] This definition may seem arbitrary. However, all results presented in the following remain unchanged for every definition of "high tax evasion" between at least 15 cents (2/3 of tax obligation) and 22.5 cents (full evasion).

[13] The differences between the no information and the partial information treatment as well as the difference between the full information and the partial information treatment are significant (chi-square test, $p < 0.01$ and $p = 0.01$,rrespectively). There is no significant difference between the no information and the full information treatment.

behavior.[14] In the last five periods, there are significantly less high tax evasions in the no information treatment (33% vs. 47% and 51%, chi-square test, $p < 0.01$ in both cases) but the difference between the partial and the full information treatment is not significant.

In sum, descriptive results point towards a shame effect only in the early periods that is dominated by a contagion effect in later periods regardless whether we use average or high evasion as dependent variable.

Figure 2: Average quantity of high tax evasion in the three treatments



[14] In contrast, the occurrence of full tax compliance—accordingly specified as tax evasion of at most 10%—is uniformly distributed across the treatments. Only in the first five periods, there are significantly more occurrences of full tax compliance in the full information treatment (pairwise *t*-tests, $p < 0.01$ each), further indicating the immediate impact of the shame effect.

Table 1: Descriptive Statistics

	No information treatment			Partial information treatment			Full information treatment		
		Periods			Periods			Periods	
	All	First five	Last five	All	First five	Last five	All	First five	Last five
Tax evasion (in %)	36.78 (41.40)	25.07 (35.75)	42.95 (44.02)	41.66 (44.15)	23.90 (34.37)	53.98 (46.16)	38.61 (41.73)	17.64 (28.27)	52.33 (44.54)
High tax evasion (in %)	25.28 (43.48)	14.44 (35.20)	33.33 (47.21)	32.99 (47.03)	13.06 (33.74)	46.94 (49.98)	25.83 (43.79)	6.30 (24.33)	41.11 (49.29)
Audit Probability (in %)	23.09 (20.09)	17.50 (16.07)	28.99 (21.73)	25.39 (21.38)	19.07 (16.00)	29.23 (22.73)	25.44 (20.56)	17.49 (15.87)	31.56 (23.26)
Final Profit (in €)	9.94 (1.06)			10.03 (1.24)			9.99 (0.72)		
Females	62.5			52.8			57.4		
Age	22.26 (2.59)			22.17 (2.33)			22.07 (2.33)		
Economic Students	63.9			70.8			70.4		
Risk Aversion	5.26 (2.63)			5.38 (1.83)			5.02 (2.31)		
Subjects	72			72			54		

Values shown above are means with standard deviations in parentheses. High tax evasion covers every tax evasion of at least 20 (of at most 22.5) Cents. Subjects' final profit is their total experimental wealth accumulated over 20 rounds. Risk aversion was measured based upon Holt and Laury (2002). Subjects were given 10 paired lottery-choice decisions. Risk aversion is the average number of decisions subjects made in favor of the risk free alternative. A more detailed overview on the measure of risk aversion, as well as the other variables, is given in Appendix B. The last row simply shows the number of subjects per treatment.

B Panel analysis

As noted in Section 4.1, a total of 198 students participated in experimental sessions comprising 20 rounds each, resulting in 198×20=3960 observations. Subjects cannot evade less than 0 or more than 22.5. Hence, tax evasion is bounded between 0 and 22.5 Therefore, we apply a two-sided censored random effects Tobit type I panel model on our data. Hence, our model is given by

$$E_{it}^{*} = \beta' x_{it} + v_i + \varepsilon_{it}$$

where $i = 1, \dots, 198$ and $t = 1, \dots, 20$. The random effects v_i are $N \sim (0, \sigma_v^2)$ i.i.d and $\varepsilon_{it} \sim N(0, \sigma_\varepsilon^2)$ i.i.d is independent of v_{it}. The observed variable is

$$E_{it} = \begin{cases} 0, & \text{if } E_{it}^{*} \leq E_{it} \\ E_{it}^{*}, & \text{if } 0 < E_{it}^{*} < 22.5 \\ 22.5, & \text{if } E_{it}^{*} > 22.5 \end{cases}$$

The regressor matrix is given by x_{it} and includes indicator variables for the partial and the full information treatment, gender, subject of study, risk aversion and income. The no information treatment (full anonymity) is the baseline treatment. In the partial information treatment, public disclosure is anonymous; in the full information treatment, real public disclosure occurs. The other model variables are briefly described in section 4.1. As in section 4.1 we look at the overall results as well as the first and last five periods only. The results are given in the first three columns of Table 3.

In addition to the Tobit model, we apply a random effects Probit model to examine high tax evasion. This model is given by

$$\Pr\left(Eh_{it} = 1 | x_{it}\right) = \phi(\beta' x_{it} + v_i + \varepsilon_{it}),$$

where Eh equals 1 in the case of high tax evasion. ϕ is the standard normal cumulative distribution function. The random effects v_{it} are defined as in the Tobit model, whereas ε_{it} is $N \sim (0, 1)$ i.i.d. and again independent of v_{it}. The regressor matrix is again given by x_{it} and includes the same variables as in the Tobit model.

The descriptive results are largely confirmed by the Tobit and the Probit regression: across all 20 periods, tax evasion increases with decreasing tax privacy. The coefficients of both the partial and the full information treatments are positive, but only the coefficient of the partial information treatment is significant. Because disclosure is anonymous in the partial information treatment, the strong effect of this treatment can only stem from the simple observation of others' behavior, i.e., a contagion effect. As in our descriptive results, the difference in tax evasion between the baseline and the full information treatments is not significant, indicating that shame is not strong enough to override the contagion effect. This is further supported by the fact that the difference between the two information treatment coefficients is not significant (Tobit: Wald, $p = 0.41$, Probit: Wald p=0.50). Similarly, for the first and the last five periods the multivariate results match the descriptive results. The existence of a shame effect can be shown only in the first five periods. However, in contrast to the descriptive results, the effect is significant only for high tax evasion, i.e., in the Probit model. On the other hand, in line with the descriptive results during the last five periods, only contagion can be shown as tax evasion in both, the partial and the full information treatment is higher than in the no information treatment but the difference in tax evasion between the partial and the full information treatment is not statistically significant (Wald $p = 0.89$).

The impact of control variables is in all models as expected from the previous literature. Audit probability has a highly significant negative impact on tax evasion. Male subjects evade more taxes than female subjects, which comports e.g. with Kastlunger et al. (2010) or Bazart and Pickhardt (2011), and economics students are less compliant than others, as has been shown before by Cullis et al. (2012). Tax evasion increases with decreasing risk aversion and risk aversion decreases with higher income—together, these effects lead to more tax evasion at higher incomes.[15] These patterns prevail even if the public resource is crowded out, and all must share the burden of the public bad.[16]

[15] Some subjects switched more than once between the risk free and the risky alternative. To check our results for robustness we included an indicator variable into our model. The results regarding the impact of the treatment variables remain unchanged.

[16] In this respect, the last period is a special case as there is no follow-up period and, thus, no public bad as a consequence of tax evasion. However, the results remain qualitatively unchanged even when the last period is excluded from the analysis.

Table 2: Multivariate Analysis

	Tobit (all 20 Periods)		Tobit (first five Periods)		Tobit (last five Periods)		Probit (all 20 Periods)		Probit (first five Periods)		Probit (last five Periods)	
Intercept	12.45 (4.30)	***	7.94 (3.70)	**	7.53 (7.38)		-0.50 (0.36)		-0.98 (0.51)	*	-0.56 (0.88)	
Partial information treatment	4.84 (2.09)	**	0.39 (1.80)		8.14 (2.98)	***	0.38 (0.22)	*	-0.12 (0.34)		0.60 (0.30)	**
Full information treatment	2.67 (2.54)		-2.02 (1.92)		7.73 (2.95)	***	0.26 (0.20)		-0.87 (0.39)	**	0.68 (0.27)	**
Audit probability	-67.64 (3.42)	***	-35.09 (4.25)	***	-82.57 (6.09)	***	-5.66 (0.29)	***	-5.41 (1.10)	***	-6.70 (0.66)	***
Female	-4.62 (2.23)	**	-3.96 (1.70)	**	-5.46 (2.81)	*	-0.51 (0.22)	**	-0.66 (0.30)	**	-0.66 (0.22)	***
Student of economics	3.82 (1.90)	**	2.59 (1.57)	*	3.14 (2.71)		0.32 (0.20)		0.45 (0.32)		0.19 (0.26)	
Risk aversion	-1.30 (0.56)	**	-1.05 (0.44)	**	-1.55 (0.53)	***	-0.10 (0.05)	**	-0.12 (0.07)	*	-0.14 (0.05)	**
Income	0.03 (0.00)	***	0.04 (0.01)	***	0.04 (0.01)	***	0.00 (0.00)	***	0.01 (0.00)	***	0.00 (0.00)	***
Log likelihood	-7441.77		-1993.72		-1644.70		-1476.37		-298.56		-401.44	

Dependent variable: tax evasion (Tobit models) and high tax evasion (Probit models).
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
All reported standard errors are robust using the bootstrap method with 50 replications.

V Conclusions

To determine the effect of tax privacy on tax compliance, we designed a novel tax game with tax privacy as the treatment variable and tax administration cost as public bad. Tax privacy ranged from full confidentiality to full publicity, combining the different forms of tax publicity from Laury and Wallace (2005) and Coricelli et al. (2010). Theoretically, two opposing effects—a contagion effect and a shame effect—can occur in response to public disclosure. We investigated these effects at three different levels of tax privacy.

In the partial information treatment, individual tax information is publicly disclosed in an anonymous manner. The only difference from the baseline treatment of full tax privacy is that the individual behavior of the other subjects in one's group is observable. Because subjects remain anonymous in this treatment, the shame effect is not expected, and the only impact on compliance results from the contagion effect. In contrast, in the full information treatment, public disclosure of all individuals is employed: tax evaders' photos are shown, which presents the potential for the shame effect. The shame effect only arises if subjects' contributions are known and linked to each subject. Overall, we find higher tax evasion with public disclosure if information is published anonymously. This indicates that the pure observation of deviant behavior could destroy the social norm of compliance and lead to one's own non-compliance. There is a crowding out of intrinsic motivation to pay taxes. Additionally, the shame effect appears to be too small to override the contagion effect when both are present simultaneously. Although shame is an effective deterrent in early periods of the experiment, we do not observe an effect in later periods suggesting that the feelings of shame diminish over the course of the experiment because subjects observe the non-compliance of other participants.

These findings are of particular importance to fiscal policy: general public disclosure could lead to more, instead of less, evasion. However, concrete predictions are difficult to make because they also depend on the current state of existing social norms. Because a system of social norms typically has multiple equilibria, also "history and chance" determine where the system settles (Cooter 1998). Moreover, our experiment is subject to some limitations and, therefore, the results should be interpreted with caution. First, while Alm et al. (1992) do not find any impact from terminology used (loaded vs. neutral) on tax compliance, other studies provide some evidence that subjects are more compliant in a tax, as opposed to a neutral, context (Baldry, 1986; Wartick at al., 1999; Durham et al., 2014). This could potentially influence the relative strength of the different effects. Second, the shame effect could also be more effective in natural settings where long-term economic consequences often result.[17] However, the work of Coricelli et al. (2010) indicates that accounting for emotions in the laboratory is possible and that emotion is the main driver of the shame effect. There is no direct reason why emotion should not be present in the laboratory, but stronger financial consequences might increase the importance of fear in the tax evasion decision. The broader question here concerns the impact an initiative might have on social behavior where the motivational crowding literature (i.e., Benabou and Tirole, 2006; Bolle and Otto, 2010) stresses the importance of the current state for predicting potential changes.

In contrast to Laury and Wallace (2005) and Coricelli et al. (2010), tax publicity leads to higher tax evasion in our study (at least) in the case of anonymous disclosure. This strong difference might be due to the missing public good context in these studies, which leads to an underestimation of the contagion effect. Outside a public good or redistribution context, there exists neither a necessity nor an opportunity to cooperate. Within this standard of cooperative behavior, the effects of tax publicity remain ambiguous with no sustained effect of copying positive behavior, as can be observed, for instance, in a fundraising context (Andreoni and Petrie, 2004). In addition to the negative impact of shame on tax evasion, there is an opposing contagion effect that might, overall, lead to less compliance.

[17] In addition, shame may be more effective if not all subjects but e.g. only subjects that evaded most are "named and shamed". Note also, that "naming and shaming" could have different effects on firms than on individuals. In particular, those firms where reputation matters a lot and the market penalizes deviant behavior could be affected by "naming and shaming". E.g., Hitz et al. (2012) show the effectiveness of a 'name and shame' mechanism in an accounting context.

Appendix A Experimental instructions

Thank you very much for participating in this experiment on the evaluation of group behavior. Overall, the experiment will last about one hour. Your payoff depends both on your own decisions and on the decisions of the other players. In the following, we will give you a short overview on how the experiment is carried out. If you have any questions, please do not hesitate to contact the experimental supervisor.

1. Initially, you are randomly assigned to one out of two groups. Both groups are made up of six players each. Throughout the whole experiment, the groups' composition remains unchanged.
2. There will be a total of 20 rounds. At the beginning of each round, every group member receives an endowment of 75 cents. Thus, the group's total endowment is 450 cents for every round.
3. In each of the 20 rounds, you must pay fees on your endowment. You may decide on the amount of your fees yourself. However, in each round your group's total fees should sum up to 135 cents. In the case of a uniform distribution, every member of your group would have to pay 22.5 cents.
4. If your group's total fees amount to less than 135 cents, the consequences for the following round are twofold. First, there will be a cost for your group in the amount of the underpaid fee. Secondly, the amount of the underpaid fee will determine the probability of your individual fee being audited in the next round. If you are audited, and it is found that you paid less than the required 22.5 cents, your underpaid fee plus a penalty of half of your less underpaid fees will be collected automatically.
5. The consequences resulting from underpaid fees by your group are given in Table 3. The table reads as follows: There will not be any cost in the next round, if you and your group members paid at least the required fee of 135 cents in total. Moreover, your individual fee in the following round will not be subject to an audit. For instance, there will be a cost of 6 cents in the following round if the group's total fees only sum up to 129 cents. These costs will be split equally among every group member. Thus, in the next round, there will be a deduction of 1 cent from the endowment of every member of your group. The corresponding audit probability for the next round amounts to 3%.
6. At the beginning of the first round, a hypothetical total fee for the fictional round 0 will be determined randomly. This fee will be between 1 and 135 cents. The cost and audit probability for the first round will be determined according to the table above. In all later rounds, cost and audit probability are determined on the basis of the actual total fees paid by your group in the previous round.

Table 3: Consequences of less paid fees

Total fees	Total costs	Audit probability (in %)	Total fees	Total costs	Audit probability (in %)	Total fees	Total costs	Audit probability (in %)	Total fees	Total costs	Audit probability (in %)
135	0	0.0	101	34	17.0	67	68	34.0	33	102	51.0
134	1	0.5	100	35	17.5	66	69	34.5	32	103	51.5
133	2	1.0	99	36	18.0	65	70	35.0	31	104	52.0
132	3	1.5	98	37	18.5	64	71	35.5	30	105	52.5
131	4	2.0	97	38	19.0	63	72	36.0	29	106	53.0
130	5	2.5	96	39	19.5	62	73	36.5	28	107	53.5
129	6	3.0	95	40	20.0	61	74	37.0	27	108	54.0
128	7	3.5	94	41	20.5	60	75	37.5	26	109	54.5
127	8	4.0	93	42	21.0	59	76	38.0	25	110	55.0
126	9	4.5	92	43	21.5	58	77	38.5	24	111	55.5
125	10	5.0	91	44	22.0	57	78	39.0	23	112	56.0
124	11	5.5	90	45	22.5	56	79	39.5	22	113	56.5
123	12	6.0	89	46	23.0	55	80	40.0	21	114	57.0
122	13	6.5	88	47	23.5	54	81	40.5	20	115	57.5
121	14	7.0	87	48	24.0	53	82	41.0	19	116	58.0
120	15	7.5	86	49	24.5	52	83	41.5	18	117	58.5
119	16	8.0	85	50	25.0	51	84	42.0	17	118	59.0
118	17	8.5	84	51	25.5	50	85	42.5	16	119	59.5
117	18	9.0	83	52	26.0	49	86	43.0	15	120	60.0
116	19	9.5	82	53	26.5	48	87	43.5	14	121	60.5
115	20	10.0	81	54	27.0	47	88	44.0	13	122	61.0
114	21	10.5	80	55	27.5	46	89	44.5	12	123	61.5
113	22	11.0	79	56	28.0	45	90	45.0	11	124	62.0
112	23	11.5	78	57	28.5	44	91	45.5	10	125	62.5
111	24	12.0	77	58	29.0	43	92	46.0	9	126	63.0
110	25	12.5	76	59	29.5	42	93	46.5	8	127	63.5
109	26	13.0	75	60	30.0	41	94	47.0	7	128	64.0
108	27	13.5	74	31	30.5	40	95	47.5	6	129	64.5
107	28	14.0	73	62	31.0	39	96	48.0	5	130	65.0
106	29	14.5	72	63	31.5	38	97	48.5	4	131	65.5
105	30	15.0	71	64	32.0	37	98	49.0	3	132	66.0
104	31	15.5	70	65	32.5	36	99	49.5	2	133	66.5
103	32	16.0	69	66	33.0	35	100	50.0	1	134	67.0
102	33	16.5	68	67	33.5	34	101	50.5	0	135	67.5

7. At the beginning of each round, every player will be informed of the amount of your group's total fees paid in the previous round. This determines the current audit probability according to the table above. (The following applies only in the partial and the full information treatments: Every player is informed of the amount of fees paid by every single group member.)

8. At the end of the 20 rounds, your individual income for every round is summed up. The payoff of your participation in this experiment corresponds to your total income.
9. Subsequent to the 20 rounds, there will be a lottery. Afterward, you have to answer a short questionnaire. The resulting payoff for your participation in this experiment may be picked up during the office's opening hours.

The experiment starts as soon as all participants are ready.

Good luck!

Appendix B Questionnaire (extract)

1. Age (in years)
2. Gender (male/female)
3. Student of economics (yes/no)
4. Semester
5. Risk aversion (see Table 4)

Table 4: The ten paired lottery-choice decisions (Holt and Laury, 2002)

Please choose always the one alternative A or B you prefer. One of the ten lotteries you choose will be selected randomly. You will be credited the respective result.

Option A	Option B	Expected payoff difference
10/10 of € 0.525	1/10 of € 0.75, 9/10 of € 0.41	€ 0.08
10/10 of € 0.525	2/10 of € 0.75, 8/10 of € 0.41	€ 0.05
10/10 of € 0.525	3/10 of € 0.75, 7/10 of € 0.41	€ 0.01
10/10 of € 0.525	4/10 of € 0.75, 6/10 of € 0.41	€ -0.02
10/10 of € 0.525	5/10 of € 0.75, 5/10 of € 0.41	€ -0.05
10/10 of € 0.525	6/10 of € 0.75, 4/10 of € 0.41	€ -0.09
10/10 of € 0.525	7/10 of € 0.75, 3/10 of € 0.41	€ -0.12
10/10 of € 0.525	8/10 of € 0.75, 2/10 of € 0.41	€ -0.16
10/10 of € 0.525	9/10 of € 0.75, 1/10 of € 0.41	€ -0.19
10/10 of € 0.525	10/10 of € 0.75, 0/10 of € 0.41	€ -0.23

Appendix C Screenshots from the experiment

Below are screenshots from the experiment. The experiment was conducted in German, hence, everything shown below was translated into English.

Figure 3: Information stage

Required minimum fee by your group in the preceding period (in cent)	135.0
Actual total fees by your group in the preciding period (in cent)	86.0
Resulting total costs in the current period (in cent)	49.0
Resulting audit probability in the following period (in %)	24.5

Figure 4: Contribution entry

Your endowment (in cent)	75.0
Your share of costs from the preciding period (in cent)	8.2
Endowment after deduction of costs (in cent)	66.8
Your nominal share in group's total fee (in cent)	22.5
Audit probability (in %)	24.5
Your fee (in cent)	17

Figure 5: Audit stage (No information treatment)

Your fee (in cent)	17.0
Your group's total fees (in cent)	66.5
Was your fee subject to an audit?	No
Your penalty (in cent)	0.0

Figure 6: Audit stage (Partial and full information treatment)[18]

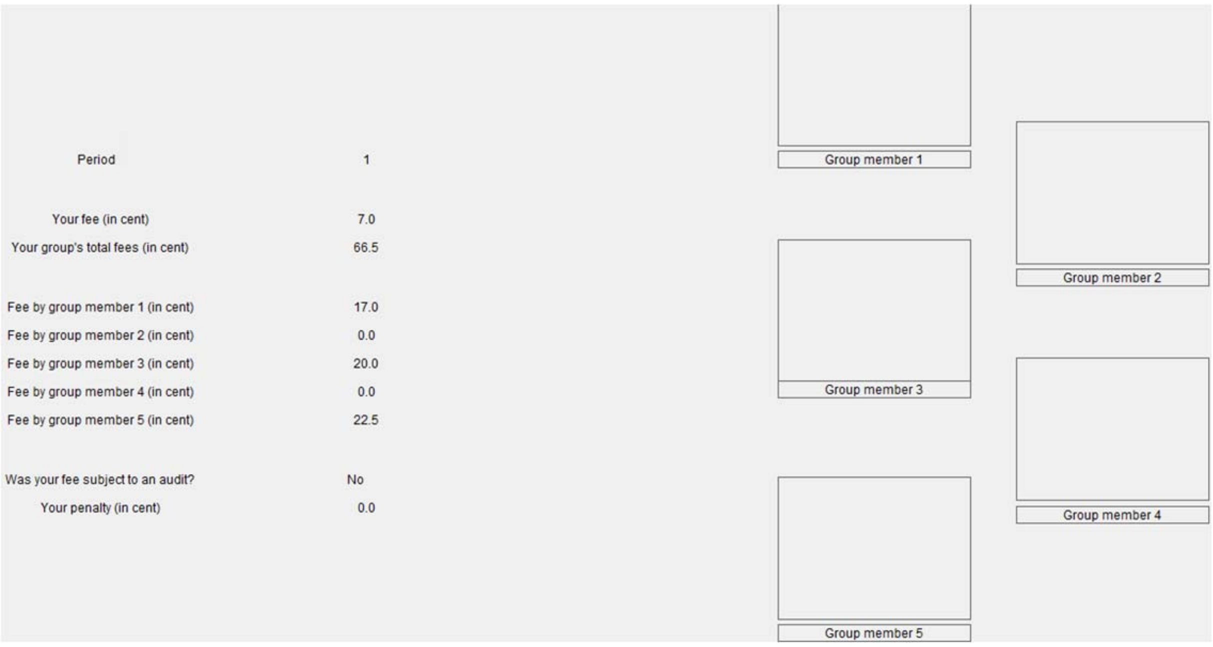
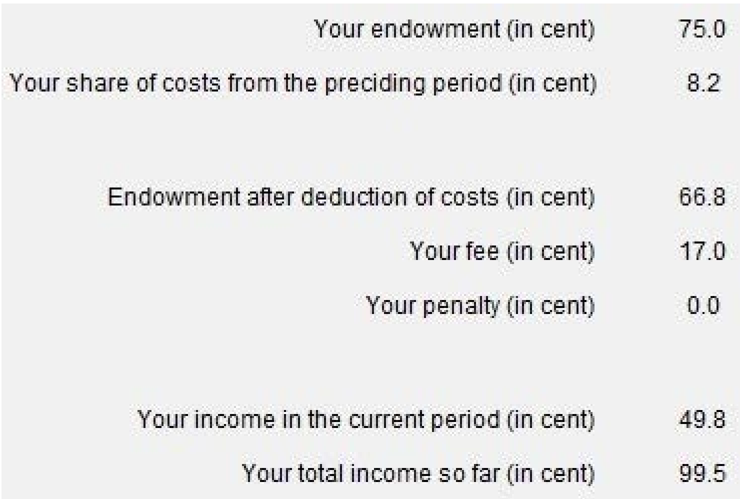


Figure 7: Profit Display



The screenshot shown reflects the situation in the no information treatment, where no individual information is available. In both the partial and the full information treatment individual taxes of every subject are visible.

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[18] In the partial information treatment only the left side of the screen was shown. In the full information treatment the empty boxes on the outer right side of the screen were filled with subjects' pictures.

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Impressum:
Arbeitskreis Quantitative Steuerlehre, arqus, e.V.
Vorstand: Prof. Dr. Ralf Maiterth (Vorsitzender),
Prof. Dr. Kay Blaufus, Prof. Dr. Dr. Andreas Löffler
Sitz des Vereins: Berlin

Herausgeber: Kay Blaufus, Jochen Hundsdoerfer,
Martin Jacob, Dirk Kiesewetter, Rolf J. König,
Lutz Kruschwitz, Andreas Löffler, Ralf Maiterth,
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ISSN 1861-8944