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Bringing Tax Avoiders to Light: Moral Framing and Shaming in a Public Goods Experiment*

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Abstract

With a series of public goods games in a 2×2 -design, we analyze two channels that might moderate social dilemmas and increase cooperation without using pecuniary incentives: moral framing and shaming. Cooperation increases when non-contributing to a public good is framed as morally debatable and socially harmful tax avoidance. However, cooperation is only durable when free-riders are “shamed” by disclosing their misdemeanor. We find shaming effects to be strong enough to make appeals to morality redundant for participants’ decisions.

JEL codes: E62, H26, H30

Keywords: Shaming, framing, tax avoidance, public goods experiment

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1 Introduction

Over the past years, the media has repeatedly reported on large-scale tax avoidance schemes by firms and wealthy individuals, often assisted by wealth management firms and the professional classes. Some of the reported activities may constitute criminal tax evasion, fraud, or money laundering – but most seem perfectly legal. Most of the people and firms exposed by the media will likely never face prosecution or criminal charges of any kind. Still, the press and the wider public judge their escape from taxes as morally reprehensible, opportunistic and disreputable. The incriminated persons may not have violated any law, but their behavior is seen as debasing the spirit of the tax law for the sake of personal gains. The broad media coverage has not only brought that behavior to light but it overwhelmingly also issued a devastating moral verdict. For the politicians, sports stars, artists or business people who were pilloried for tax avoidance, the unprecedented and embarrassing publicity damaged their reputation – and the shaming might deter them and others in the future.

These recent developments exemplify two insights: first, socially cooperative behavior (here, contributing to government finance as the legislation seems to have willed) often cannot be fully formalized in laws, legislated and judicially enforced. Rather it requires a specific “morality” – a sense of virtue and decency, of duty and civic obligations – or a normatively “right” demeanor. Appeals to this morality may positively affect individuals’ pro-social behavior. Second, to reinforce or induce socially warranted behavior, informal reputational mechanisms – for example the naming and shaming of alleged tax dodgers – may play an important role. The experience or already the fear of being visibly identified to the general public or to one’s peers as non-cooperative and opportunistic can make individuals act more cooperatively.

Although often working together, moral loading and shaming are two distinct triggers for cooperative behavior. While a number of things are known about how these triggers work separately – moral framing through subtle nudges rather than by direct cues, and shaming through blatant exposure – their joint effects are still scarcely studied (see Section 2). Which one is stronger? Can they reinforce each

other, or does one behavioral trigger crowd-out the other?

In this paper, we report an experimental study that separates and interacts the two triggers and analyzes their differential impact. Our experiment consisted of a public goods game that was played with two different but equivalent (at least within standard economic logic) descriptions: first, in a neutral form as a voluntary contribution mechanism and, second, as a morally loaded tax avoidance game where not paying taxes is presented as a legally adequate but possibly socially questionable behavior. Either variant is played both with and without shaming, i.e., disclosing to the other players the names and pictures of individuals whose contributions fell short of what is socially warranted.

We, first, show that shaming is an effective reputational mechanism towards socially cooperative behavior: with disclosure of an individual's lack of cooperation, contributions are between 60 to 120 percent higher than in treatments without disclosure. Both for the neutral and for the morally loaded setting we find that when shaming looms, participants seek to avoid it. The differences become smaller in later rounds, but remain significant until the end of the experiment.

Second, moral loading also increases pro-social behavior significantly. However, its impact pales against the shaming effect – and it evaporates and becomes insignificant in later stages of the experiment.

In sum, (the threat) of disclosing unwarranted behavior is an effective strategy for reducing tax avoidance and, more generally, for promoting pro-social behavior. The communication of moral arguments also works but is less effective and, once a shaming mechanism is in place, has no noticeable effect.

The remainder of this paper is organized as follows: Section 2 reviews previous literature. Section 3 describes the experimental design in detail. Section 4 contains the experimental protocol and summary statistics of our sample of participants. Section 5 outlines our hypotheses. In Section 6 we present and interpret the main results of our experiments. Section 7 concludes. The Appendices contain some of the screen messages from the experiment (Appendix A), additional results (Appendix B), and the experimental instructions (Appendix C).

2 Related Literature

Our experiment examines, jointly and separately, two potential triggers of pro-social behavior: moral framing and public shaming. To our knowledge, only very scarce empirical or experimental evidence is available that compares these triggers in their effectiveness to deter legal but socially undesirable behavior such as tax avoidance. Still, many relevant aspects of our topic have found some attention in the literature.

Framing. A large number of experimental studies show that individuals are highly sensitive to framing, i.e., to different descriptions of equivalent situations. Public goods games are one case in point. For example, Andreoni (1995) variously framed strategically equivalent public goods game as a positive versus a negative externality; contributions in the negative frame were 45 percent lower than in the positive frame. Boehm and Theelen (2016) framed positive vs. negative outcomes in their public good game and find similar results. With a comparable experimental design, Sonnemans et al. (1998) contrasted an equivalent contribution mechanism framed as a “public good” vs. a “public bad”. A public good received significantly higher contributions than the bad. Ellingsen et al. (2012) played a Prisoners Dilemma game, alternatively labeled as a “Community Game” versus a “Stock Market Game”. Playing the former resulted in more cooperation, but not when the game was played sequentially. The authors conclude that framing works more as a coordination device than as a trigger to desiring pro-social actions. In a lab experiment on tax paying, Blaufus et al. (2016a) show that an economically equivalent decision was treated differently when it was framed as (admissible) tax avoidance versus (illegal) tax evasion. In the evasion scenario, tax minimization was less pronounced. The differences vanished once pecuniary consequences were introduced but could be restored with the use of “moral priming”.

Appeals to morality. The effectiveness of moral suasion, i.e., of appeals directed to the audience’s sense of what is right and proper, finds mixed support in the literature. While many studies, often relying on lab experiments, document that moral motivation matters when people make decisions (see, e.g., Fehr

et al., 2013), field evidence for tax compliance is often less compelling (Luttmer and Singhal, 2014). For example, Blumenthal et al. (2001) used a controlled field experiment in Minnesota (USA) to send letters with normative appeals to voluntarily report taxable income to a large group of taxpayers. Overall, the letters had little effect on reported income. Dal Bò and Dal Bò (2014) tested the effect of moral appeals with a public goods game and find a significant but only very short-lived positive effect on contributions. Only in combination with the option to punish did the effect prove to be persistent. Meiselman (2018), with a controlled field experiment among non-filers in Detroit, finds that mailings appealing to the taxpayers' civic pride did not increase returns. However, messages on penalty salience and compliance costs increased compliance. In a field experiment, Fellner et al. (2013) tested the effects of moral appeals on compliance with a mandatory but poorly enforced fee for public broadcasting in Austria. Moral appeals to pay the fee only had a positive effect in municipalities where evasion was common; the salience of a high risk of detection seems to work much better as a deterrent. By contrast, Bott et al. (2017) find that when tax authorities in Norway included a moral appeal in letters to potential tax avoiders, this on average doubled the self-reported foreign income. In a large-scale natural field experiment in the UK, Hallsworth et al. (2017) vary normative messages sent out to overdue taxpayers; they find that conveying descriptive norms (mentioning, e.g., that most taxpayers are compliant) have a significantly larger effect on increasing payment rates than injunctive norms (which appeal to social responsibility).

Moral or religious *priming* (i.e., stimuli meant to – often subconsciously – activate certain responses and memories) can be used to reinforce cooperative behavior. In the public goods games by Drouvelis et al. (2015), priming consisted of a word-search puzzle with words related to collective action, contributions in the treatment group were around 11 percentage points higher than in the control group. Maxwell et al. (1999), with a lab experiment, find that participants primed with the evaluation of (subjectively) fair price ranges were more cooperative in price negotiations.

Outlawing behavior by making it illegal arguably gives stronger behavioral cues, compared to simple appeals which might be perceived as cheap talk. The violation

of laws is sanctioned exogenously by fines or imprisonment, and endogenously punished via disapproval or ostracism by the society. In a public goods experiment, Tyran and Feld (2006) tested whether norm activation through laws also occurs when laws are non-deterrent. The study observes that exogenous mild sanctions did not increase cooperation, compared to treatments where no law was in place.¹ When sanctions were imposed endogenously, the public good supply increased considerably above baseline. Severe laws (high monetary sanctions) almost completely eradicated free-riding.

Disclosure of decisions in Public Goods Games. Abandoning anonymity in economic games has increasingly attracted attention recently. In a public goods game framed as charitable giving, Andreoni and Ragan (2004) used pictures to make donors and their contributions identifiable to fellow group members. Such visual identification led to significantly increased contributions to the public good. Interestingly, donations were highest when disclosure was a deliberate choice.

In a one-shot public goods game by Rege and Telle (2004), treated participants could observe each other during the experiment; only direct communication was forbidden. Revealing the players' identities increased cooperative behavior in the experiment significantly. Noussair and Tucker (2007) replicated the experiment by Rege and Telle (2004), but played the game over the course of 20 periods. Interestingly, in the long run pro-social behavior dropped below the level reached in the anonymously playing control group. In another public goods experiment, Bochet et al. (2006) dissolved anonymity by allowing face-to-face communication and chat-boxes. Participants who met and talked before the actual experiment started made significantly larger contributions than the anonymous control group. Savikhin Samek and Sheremeta (2014) displayed pictures plus names of participants who contributed less than the maximum possible amount to a public good. They observed significantly increased contributions resulting from this treatment. In field experiments, Ariely et al. (2009) or Ashraf et al. (2014) find that pro-social behavior increases considerably when individual effort is displayed publicly,

¹For a German church tax with historically zero audits and fines, Dwenger et al. (2016) show that deterrence reduces tax evasion only by a fairly modest extent. Most tax payments seem to be driven by duty-to-comply preferences.

relative to a control condition where effort remains private.

Disclosure and tax compliance. The effects of disclosure have also been studied in the context of tax evasion and avoidance. Blaufus et al. (2016b) and Casal and Mittone (2016) had public goods games played without confidentiality in a tax evasion setting; not declaring the full endowment (or income) was monetarily sanctioned if detected. Both studies find a positive effect of the picture treatments on tax-declared income. Casal and Mittone (2016) varied their experiment such that participants in one treatment got the option to pay for remaining anonymous. This option was frequently used and led to more abundant tax evasion. Fortin et al. (2007) and Coricelli et al. (2010) studied the effect of disclosure on tax evasion without the inclusion of a public good. In their experiment, tax payments went, as the experimenters described, to a pool funding future research and were thus lost to the participants. Fortin et al. (2007) disclosed only information on single tax payments, but displayed neither pictures nor names. This resulted in significantly lower tax payments compared to the scenario where compliance information remained entirely undisclosed. Coricelli et al. (2010) published pictures, but not the amount of underreported taxes from audited tax evaders. They find a significant deterring effect of public exposure. Additionally, Coricelli et al. (2010) find that cheating was accompanied by emotional arousal, especially under the threat of being publicly revealed.

In some Scandinavian countries, individual tax returns are publicly available. Bø et al. (2016) exploit a policy change in Norway in 2001, when information on tax returns became available on the internet. Following the policy change, reported taxable income increased by roughly three percent. Since the effect was largest in densely populated areas, Bø et al. (2016) conclude that it was driven mainly by the wish to avoid media attention or public shaming. Perez-Truglia and Troiano (2015) find evidence for reduced tax evasion in the US when tax delinquents were shamed by informing their neighbors. Japanese tax laws know a threshold for the duty to disclose income tax returns. Hasegawa et al. (2012) find that taxpayers close to the threshold systematically under-reported income so as to avoid public disclosure.

Interacting non-pecuniary triggers. Our experiment complements a small literature (albeit on distributive behavior) that disentangles and interacts different behavioral motivations for pro-sociality. DellaVigna et al. (2012) find that for individuals to make donations in a door-to-door fund-raising campaign, both moral motives – they care about a specific worthy cause – and social and (self-)image motives – they give in to social pressures to donate – are in fact at work. This is in line with what we find, too. Cappelen et al. (2017) study whether pro-social behavior, in the form of giving in a dictator game, is motivated by (intrinsic) moral or by (extrinsic) social motivation; the latter is manipulated by disclosing to recipients whether or not their money comes from a subject in a dictator experiment. The moral motivation is found to have a strong effect while the social motivation only matters when a moral motive justifying the behavior pre-exists. Hence, while social motivation is crowding-in with moral motivation, it becomes less relevant when there is no underlying moral argument for sharing. Our findings (derived in a tax compliance context) point into a different direction: social motivation may be effective, but “name and shame”-programs targeting tax avoidance would trump them.

3 Experimental Design

Our 2×2 experimental design consisted of a linear public goods game played in four variants. Each variant drew on an identical contribution mechanism and identical payoff functions: each participant was provided with an endowment of $E = 100$ Experimental Currency Units (ECU). Of these 100 ECU, the participants could invest between zero and 40 ECU into a productive public venture. The remaining ECU were going to a private account that did not yield any returns. We use the 60:40 split to simulate a (maximum) tax rate of 40 percent in our framed experiments (see below). Groups consisted of $N = 5$ members and were fixed in composition over the ten rounds of the experiment.

The experimental payoffs for player i were $\pi_i = E - x_i + g_i$, where $x_i \in [0, 40]$ denotes the contribution to the public venture by player i and g_i is player i ’s payoff from the public account. This return is given by $g_i = \gamma / N(x_i + X_{-i})$, where X_{-i}

is the sum of investments to the public good by the (four) other group members except player i , and γ is an efficiency factor (rate of return) of the public good. We chose $\gamma = 1.5$ in all treatments. The parameters described here resulted in the payoff matrix depicted in the instructions (see Figure C.4 in Appendix C).

The type of treatment depended on whether there was framing (moral loading), disclosure of low contributors, both, or none (baseline). This resulted in the 2×2 experimental design sketched in Table 1.

Table 1: EXPERIMENTAL DESIGN

	No disclosure	Disclosure
No framing	I	II
Framing	III	IV

Treatment I is a simple and neutral voluntary contribution game; it will be referred to as the “baseline”. Framing and disclosure treatments were implemented as follows:

Framing. In treatments III and IV, the public goods game was framed as a tax avoidance game with the moral loading that taxes are actually meant to be paid. As a lesson from the extant literature on the (in-)effectiveness of moral cues (see Section 2), we opted for a highly salient framing that still leaves full liberties to individuals what to do and how to judge. First, we altered the wording of the public goods game in the instructions. We replaced “endowment” by “taxable income” and “contribution” by “tax payments”. The neutral option to invest up to 40 ECU into a profitable public project was changed into a linear income tax of 40% that *should be* paid and would finance a public project. Individuals had the option to reduce their personal tax burden by choosing any lower tax rate than the 40% stipulated by the law. We explicitly mentioned that this possibility was legal tax avoidance (no evasion) and not accompanied by any monetary punishments.

To make participants consider the moral dimension of their choices, as a second element of framing we presented to participants in treatments III and IV a short text on screen, laid out as a newspaper commentary. Under the headline “Tax avoidance is legal, but can it ever be legitimate?”, the text briefly defined tax avoidance as a legal way to reduce one’s personal tax burden that might still not have been intended by government. Thus, we insinuated that tax avoiders would not act in the spirit of the law, even if they technically did not violate it. We questioned rhetorically whether one would act ethical when valuing public services and common goods highly but free-riding on these benefits. Towards the end, we reminded readers that tax avoidance is a personal decision; whether it was seen as socially justifiable was deliberately left to participants’ own judgment.² Figure A.2 in Appendix A shows the newspaper commentary in German as well as its English translation.

Disclosure. After investment decisions, information on the own payment, the sum of the investments in the group, and the individual payoff in the respective round were presented to participants in all four variants of the game. In treatments II and IV, this information was followed by the photographs and the actual contribution of those group members who contributed less than the possible 40 ECU (Treatment II) or paid less than 40% taxes on their income (Treatment IV). All group members were provided with the same information. Full contributions meant complete anonymity. That is, group members who did not free-ride were never mentioned. Figure A.1 in Appendix A shows the disclosure screen. If all group-members chose “40 ECU”, a note reporting this outcome was displayed on the screen.³

²Chaudhuri et al. (2016), in a trust game, also revealed the game’s inherent conflict to their participants. The authors find a positive effect on trust of this “goal framing” (p. 117).

³ The wording of the disclosure texts was slightly adjusted between neutral and framed treatments. In Treatment II, the message read “On the next page, those group members who contributed less than the maximum possible amount to the public good will be publicly disclosed”. It was modified in Treatment IV to: “On the next page, those group members who paid less taxes than scheduled will be publicly disclosed by their picture and their respective tax payment. Due to the reduction of the individual tax burden, tax revenues decrease to the detriment of the whole group.” The headline of the page disclosing the pictures in Treatment II read: “The following group members have contributed less than the maximum possible amount of 40 ECU”. For Treatment IV it was modified to: “The following group members have reduced their personal

4 Experimental Protocol

The experiment was conducted at the computerized laboratory (LLEW) at the Leibniz University Hannover in August and September 2017. Participants were recruited from the general student population with the software hroot (Bock et al., 2014). A total of 215 subjects (112 male, 102 female, one subject made no statement) participated in the experiment. Earnings averaged around 11 Euro in approximately one hour. Additionally, participants received a lump-sum show-up fee of 4 Euro. We conducted 16 sessions and attempted to have 15 participants (i.e. three groups of five) in each session of our four treatments. Since a few invited students failed to show up, we ran three treatments with 10 participants in one of the sessions and the Baseline with 10 participants in two of the four sessions. The experiment was programmed with z-Tree (Fischbacher, 2007).

Before the start of a session, one of the four treatments was randomly selected and then played by all participants in that session. Subjects were randomly seated and then matched (according to their seat number) to groups of five by the experimental software. The participants' photos were taken right before the instructions (see Appendix C) were handed out. To avoid differential expectational effects, we took photos also in the two treatments where decisions would not be disclosed. Participants gave written consent to shortly saving and potentially using their photo in the experiment (see Appendix C). If a subject would not sign the consent form, she was not permitted to participate but still received the show-up fee. This only happened in one case.

Before playing the experiment, participants had to answer a short computer-based comprehension test (see Appendix C). In the treatments framed as a tax avoidance game, the newspaper comment was presented to the subjects on-screen for two and a half minutes before the first decisions were made.

At the end of each round, subjects received information on their own contribution (tax payment), the total contributions in their respective group, and information on their own payoff in the current round. In treatments II and IV, this information was followed by a screen displaying the pictures of those group members who tax burden. This action is legal and not connected to any monetary sanctions.”

invested less to the public good than the maximum possible 40 ECU, and the actual contribution/tax payments made (see Figure A.1 in Appendix A).

After the last decision had been made in round 10, the payoff of one round was randomly selected and paid in cash to the participants. Before payouts, we asked participants to answer a short socioeconomic questionnaire. The subjects' socio-demographics are summarized in Tables 2 (for the answers to attitude questions, see Table 6 below).

Table 2: SUMMARY STATISTICS OF INDIVIDUAL CHARACTERISTICS

Variable	Mean	Median	Standard deviation
Female	48%		
Economics Major	21%		
Bachelor degree	35%		
Employed	36%		
Tax declaration	62%		
Age	24.18	24.00	4.62
Income	344.53	300.00	257.87
Semester	7.03	6.00	4.01

Notes: Total number of subjects is 215. "Economics Major" indicates whether a subject studies economics or management. "Bachelor degree" takes the value of 1 if it is the subject's highest educational degree. "Employed" indicates whether a participant holds a job besides studying. "Tax declaration" takes the value of 1 if the subject has at least once in life filed a tax declaration. "Income" is monthly disposable income after deducting all fixed expenses.

Table 2 shows that our sample is quite balanced between female and male participants.⁴ A relatively low share of 21% of participants was enrolled in an economics major program. 23% of the sample studied at the department of philosophy, 19% in an engineering-related major, and 12% were enrolled in a natural science program. With these numbers our sample represents quite a good cross-section of the

⁴Summary statistics separated by treatment can be found in Table B.1.

student population in Hannover. Due to the moderate share of economics students we are confident to limit the bias towards rational behavior and the familiarity with public goods games, commonly observed among economics students.

5 Hypotheses

For the baseline (Treatment I), where decisions are made without any moral guidelines or publicity sanctions in place, we expected the typical results for public good games: initially, subjects would contribute around one half of what they could invest but contributions would decline quickly.

For the pure framing treatment we expect the following

Hypothesis I (Framing Effect): *When framing non-cooperation as tax avoidance (Treatment III), contributions (or now, tax payments) will be significantly higher than in the baseline setting.*

Hypothesis I rests on the premise that the term “tax” – which connotates a legal obligation – together with the moralizing mock newspaper article triggers a sense of duty to cooperate in participants, at least over and above the neutral baseline setting.

For the disclosure treatments we predict

Hypothesis II (Shaming Effect): *When less-than-full compliance becomes publicly known, contributions to the public good/tax payments will be significantly higher than in the cases where decisions are not disclosed.*

Thus, we predict that (the threat of) disclosing pictures and information on individual’s failed cooperation (naming) to act as a social punishment: being called out – and implicitly described as anti-social (then, shaming) – reduces the temptation to reap personal gains through free-riding.

This mechanism should work both in the neutral (Treatment II) and in the framed (Treatment IV) setting. In the former case, free-riding means harming the group for personal gains. In the latter setting, tax avoidance additionally means visibly breaking the unwritten call to pay (all) taxes. Regardless of the scenario, we expect the differences between non-disclosing and shaming treatments to be sizable and long-lasting. Some participants might be ready to reveal themselves as free riders as the experiment goes on, but we do not figure contagion to be as widespread as to make the shaming effect disappear.

The 2×2 experimental design also allows for a simple difference-in-difference analysis, exploring whether the shaming effect works more strongly in the framed setting than in the neutral setting or, conversely, whether moral cues become more binding when they are come together with reputational sanctions. Echoing findings by Cappelen et al. (2017) this leads to

Hypothesis III (Reinforcement Effect): *Under the threat of shaming, moral loading works more effectively than without disclosure. Shaming effects are stronger in morally loaded than in neutral settings.*

6 Results

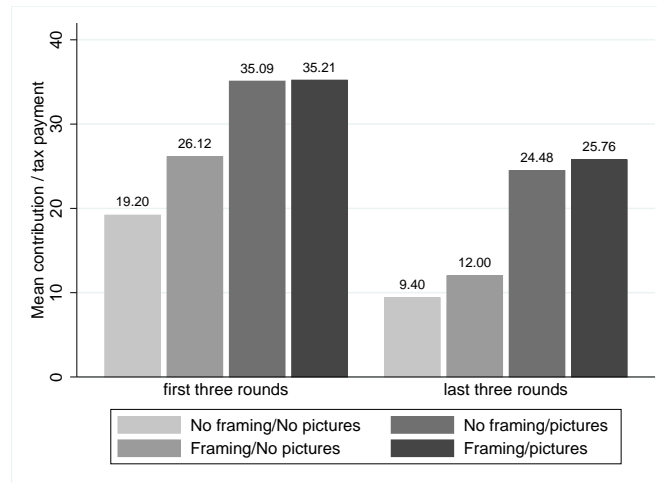
The analysis of the experiment focuses on two variables of interest: contributions to the public good (tax payments, respectively) and the share of participants who are fully compliant, i.e., who invest the maximum possible amount of 40 ECU (40% of the endowment) into the public good. While the latter variable may not be very informative in a pure VCM game without disclosure, it becomes meaningful for the treatments. First, in the tax frame with its implicit call for full compliance, the decision on how much taxes to pay could be predated by a binary decision whether to avoid or not. Second, as we chose to disclose pictures as soon as a person invests less than 40 ECU, full compliance is tantamount to keeping an untarnished reputation.

6.1 Descriptive analysis and non-parametric tests

Contributions. In the baseline, contributions to the public good on average amounted to 13.8 ECU over all rounds of the experiment. When pictures were disclosed in the neutral setting (Treatment II), average contributions rose by 126% over baseline to 31.3 ECU. When the game was instead framed as a tax compliance scenario (Treatment III), investments rose to 19.25 ECU, an increase of almost 40% over baseline. In the joint Treatment IV, average tax payments were 31.1 ECU – an increase by 62% relative to Treatment III, but no improvement over Treatment II. Hence, the framing did not make any difference for the shaming effect.

Figure 1 depicts the average contributions/tax payments in the first and in the last three rounds of the experiment, confirming the pattern just described for the entire game.

Figure 1: MEAN CONTRIBUTIONS/TAX PAYMENTS



In line with other public good games, participants contributed roughly half of what they could in the first three rounds of the baseline; their average investments dropped by around 10 ECU in the last three rounds, compared to the earlier stages.

In the tax-framed Treatment III participants paid 65% of the due taxes in the first three rounds. In the final rounds, tax payments decreased by 14 ECU, approaching

the numbers in the unframed baseline. Hence, the framing effect evaporates over time.

Full compliance. On average, 25% of participants in Treatment III fully paid their taxes. This more than doubles the 12% reached in the baseline, where no moral duty was implied. With disclosure of decisions, average full contributions/tax payments are again very similar with and without framing: 68% [69%] of participants invested 40 ECU into the public good in Treatment II [IV], which is a large increase over the baseline.

Figure 2: SHARE OF FULL CONTRIBUTORS

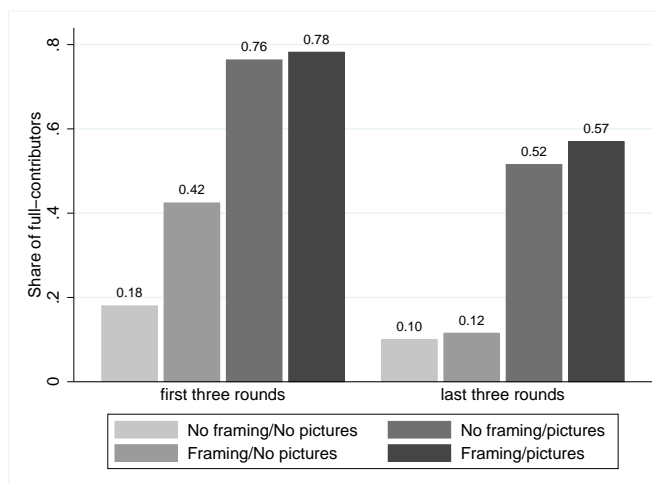
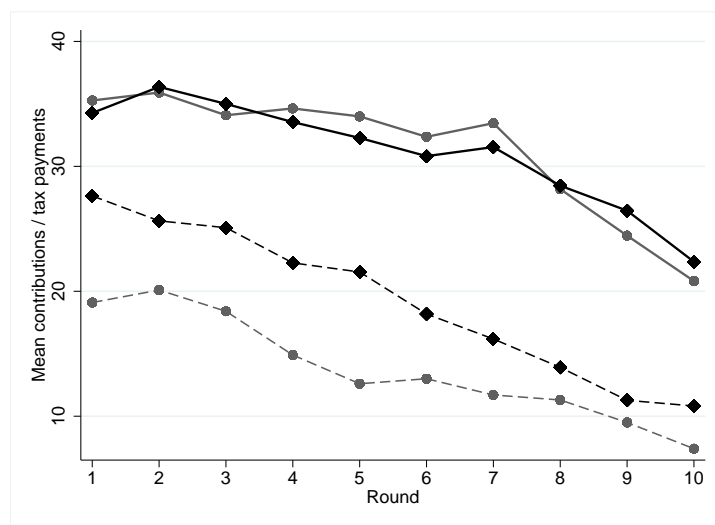


Figure 2 depicts average full compliance rates in the treatments' first and last three rounds. As in Figure 1, we find large differences between baseline and treatments, while full compliance in the latter is very similar also in the experiment's early and late stages.

Interestingly in Figure 2, 42% of the participants in Treatment III did initially not cut their tax payments, although this would not have met with any consequences. This is a strong increase over the 18% in the baseline. However, the differences disappear almost completely in the last three rounds.

Round effects. Figure 3 shows how average individual contributions (tax payments) developed over time in the four treatments. Overall, investments into the public good are noticeably higher in the framed than in the neutral treatments. In both settings without disclosure, contributions start to decline after round 3, but at a faster pace in the baseline. Only in the last three rounds, individual contributions (tax payments) become quite similar.

Figure 3: CONTRIBUTIONS/TAX PAYMENTS OVER TIME.



Dashed gray line: Baseline: No framing/No pictures. Solid gray line: Treatment II: No framing/Pictures. Dashed black line: Treatment III: Framing/No pictures. Solid black line: Treatment IV: Framing/Pictures.

Between framed and unframed games with disclosure (solid lines), almost no differences are visible. In the final three rounds, tax payments are slightly higher than voluntary contributions. Over the first seven rounds, payments were relatively stable at levels between 30 and 35 ECU. After round 7, contributions plummeted to slightly above 20 ECU in the last round.

The differences in average individual contributions between the treatments with and without disclosure are very large over all 10 rounds. For the games with disclosure, framing does not matter; in particular the differences between the frames are far smaller than between the “undisclosed” treatments.⁵

⁵Figure B.3 in Appendix B shows time-series graphs for the share of full contributors in the

Table 3: EFFECTS OF FRAMING AND DISCLOSURE ON CONTRIBUTIONS/TAXES PAID

Panel A: Shaming in neutral setting			
Rounds	Baseline ($N = 50$)	Treatment II ($N = 55$)	p -value
1-10	13.8 (14.09)	31.3 (14.34)	< 0.001
1-3	19.2 (14.19)	35.09 (10.76)	< 0.001
8-10	9.4 (13.20)	24.48 (17.58)	< 0.001
Panel B: Shaming in framed setting			
Rounds	Treatment III ($N = 55$)	Treatment IV ($N = 55$)	p -value
1-10	19.25 (16.32)	31.11 (15.09)	< 0.001
1-3	26.12 (15.66)	35.21 (11.10)	< 0.001
8-10	12.00 (14.52)	25.76 (17.94)	< 0.001
Panel C: Framing without disclosure			
Rounds	Baseline ($N = 50$)	Treatment III ($N = 55$)	p -value
1-10	13.8 (14.09)	19.25 (16.32)	< 0.001
1-3	19.2 (14.19)	26.2 (15.66)	< 0.001
8-10	9.4 (13.20)	12.00 (14.52)	0.205
Panel D: Framing with disclosure			
Rounds	Treatment II ($N = 55$)	Treatment IV ($N = 55$)	p -value
1-10	31.3 (14.34)	31.11 (15.09)	0.905
1-3	35.09 (10.76)	35.21 (11.10)	0.718
8-10	24.48 (17.58)	25.76 (17.94)	0.505

Notes: Mean contributions/tax payments are depicted for all four treatments. Measures are analyzed for all periods and the first as well as the last three periods. N denotes the number of subjects in the treatment. Standard errors are in parentheses. p -values present the significance level of differences between the respective experiments and are calculated using the two-sample Mann-Whitney U -test.

Non-parametric tests. Table 3 presents bivariate tests as a first approximation towards the statistical validity of our first two hypotheses. We employ non-parametric two-sample Mann-Whitney U -tests. Panels A and B of Table 3 show that baseline and treatment in the neutral and framed settings follow different distributions (p -values are smaller than 0.001 over all rounds as well as in the first and the last three rounds). Panel C of Table 3 shows evidence for a framing effect without disclosure: when investments are framed as tax payments, subjects contribute significantly more. The impact of framing becomes insignificant when isolating the last three rounds, however. With disclosure (Panel D), the framing effect vanishes.⁶

Reinforcement effect. We do not find support for our third hypothesis. The disclosure of information and pictures in the neutral setting and in the morally loaded tax avoidance setting give rise to very similar investments into the public good. Because cooperation in the tax setting is more widespread compared to the voluntary contribution game, the shaming effect in the neutral experiments is eventually significantly larger (p -value of 0.002, two-sample Kolmogorov-Smirnov test, see also Table B.4 in the Appendix).

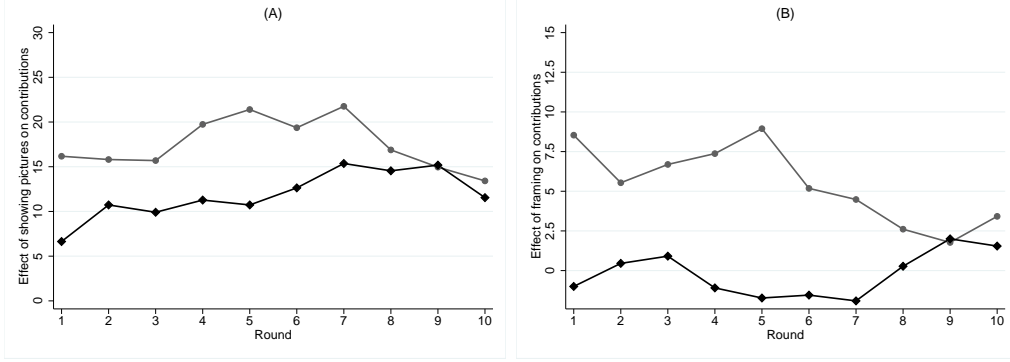
Figure 4 reports differences between experiments (mean values) for each round and visualizes why our third hypothesis cannot be supported.

Panel A depicts differences in contributions from comparing baseline and treatment in the neutral (gray line, circle markers) and the framed (black line, diamond markers) setting. The shaming effect proves to be significantly larger in the neutral context. The differences converge as time in the experiment passes, but remain significant ($p = 0.07$, two-sample Kolmogorov-Smirnov test) even in the last three rounds. With disclosure, differences in contributions between the neutral and framed setting are around zero. Without disclosure, framing plays a significantly larger role ($p < 0.001$, Panel B in Table B.4). Again, differences converge in the last rounds, but remain statistically significant ($p = 0.028$).

four treatments. The overall picture is very similar to Figure 3. In the last three rounds, however, the shares of full contributors in the games with private information almost coincide.

⁶Table B.3 in Appendix B presents non-parametric tests for the share of fully compliant players, with results very similar to those presented in Table 3.

Figure 4: DIFFERENCES RESULTING FROM THE DISCLOSURE OF PICTURES (PANEL A) AND FRAMING (PANEL B).



Gray line (with circle markers): Differences in contributions between baseline and treatment in the neutral setting (Panel A), differences resulting from framing without disclosure (Panel B). Black line (with diamond markers): Differences between baseline and treatment in the framed setting (Panel A), differences resulting from framing with disclosure (Panel B).

6.2 Multivariate analysis

Contributions/tax payments. Using contributions as the dependent variable, we ran random effects regressions with the subject ID as the cross-sectional variable and the round number as the time variable. Specifically, the model is:

$$\begin{aligned} Contribution_{it} = & \beta_0 + \beta_1 TreatmentII_i + \beta_2 TreatmentIII_i \\ & + \beta_3 TreatmentIV_i + \sum_{k=1}^l \beta_k Controls_i + \varepsilon_{it} + u_i \end{aligned} \quad (1)$$

In Equation (1), contributions (tax payments) of subject i in round t depend on the assignment to one of the four treatments. *Treatment II*, *III*, *IV* are dummy variables that take the value of 1 if subject i was assigned to one of the treatments. The baseline is the reference category. Our treatment dummies are complemented by control variables gathered from the questionnaire at the end of the experiment. In Equation (1), u_i is the subject-specific error term, ε_{it} is the corresponding equation error term.

Table 4: LINEAR RANDOM EFFECTS REGRESSIONS

	(1)	(2)	(3)	(4)	(5)	(6)
	Round 1-10	Round 1-10	Round 1-3	Round 1-3	Round 8-10	Round 8-10
Treatment III	5.455 (3.428)	6.109** (2.899)	6.921** (3.342)	7.835*** (2.726)	2.600 (3.318)	3.097 (2.787)
Treatment II	17.52*** (3.394)	15.01*** (3.060)	15.89*** (2.605)	12.97*** (2.281)	15.08*** (4.315)	12.34*** (4.212)
Treatment IV	17.31*** (3.444)	15.05*** (3.242)	16.01*** (2.431)	13.42*** (2.394)	16.36*** (4.406)	13.73*** (4.359)
Age		0.325 (0.215)		0.368*** (0.135)		0.251 (0.356)
Female		0.123 (1.708)		0.697 (1.568)		1.875 (2.392)
Employed		-2.685** (1.332)		-3.428** (1.625)		-2.531 (1.870)
Tax declaration		0.212 (1.396)		-0.202 (1.571)		-0.388 (1.656)
Econ. Major		-4.470** (2.042)		-1.671 (2.390)		-5.703** (2.289)
Bachelor		1.832 (1.474)		2.304* (1.387)		0.724 (2.063)
Semester		0.062 (0.207)		-0.013 (0.198)		0.131 (0.279)
Income		-0.004 (0.003)		-0.004 (0.003)		-0.002 (0.003)
Rule following		1.642*** (0.611)		1.324* (0.687)		1.969*** (0.763)
Publicity		0.605 (0.412)		1.118** (0.463)		-0.013 (0.440)
Sociality		0.119 (0.662)		0.452 (0.806)		0.914 (0.893)
Trust(gov.)		2.041*** (0.478)		2.322*** (0.570)		1.883*** (0.597)
Trust (others)		0.301 (1.085)		0.584 (1.096)		0.153 (1.312)
Political alignment		-0.225 (0.371)		0.022 (0.449)		-0.459 (0.452)
Round		-1.584*** (0.136)		-0.386 (0.444)		-2.600*** (0.580)
Constant	13.80*** (1.276)	5.430 (9.193)	19.20*** (1.631)	-1.945 (6.970)	9.400*** (1.775)	15.52 (14.07)
Observations	2150	2010	645	603	645	603
N	215	201	215	201	215	201
R^2 -within	0	0.169	0	0.002	0	0.006
R^2 -between	0.352	0.479	0.270	0.431	0.227	0.334
R^2	0.201	0.343	0.205	0.324	0.171	0.265
Wald-Tests						
II = III	p < 0.001	p = 0.003	p = 0.003	p = 0.026	p = 0.002	p = 0.019
III = IV	p < 0.001	p = 0.003	p = 0.002	p = 0.015	p = 0.001	p = 0.009
II = IV	p = 0.952	p = 0.991	p = 0.951	p = 0.805	p = 0.800	p = 0.782

Notes: The dependent variable is the contribution to the public good/tax payment, measured in discrete steps of 5, with a minimum of 0 and a maximum of 40. Reference is Treatment I (baseline). See Table 2 for a description of the socio-economic covariates. For self-reported attitudes and beliefs, all items except for “Political alignment” are coded on a scale from 0 (strong disagreement) to 5 (strong agreement). “Rule following” question: *In your opinion, how important is it to always follow given rules?* “Publicity”: assessment whether names and pictures of detected tax avoiders should be disclosed. “Sociality”: assessment of the statement *It is important to make a contribution to the community*. “Trust in government” asked whether one would be more willing to contribute to the community if the government could be trusted to act responsibly with the subject’s contribution. “Trust in others”: *You cannot be careful enough when dealing with other people - Others can be trusted*. Political alignment on a scale from 0 (left) to 10 (right). Standard errors, clustered on the group-level, are in parentheses. Wald-Tests are reported. * : $p < 0.1$; ** : $p < 0.05$; *** : $p < 0.01$.

Column (1) of Table 4 only takes into account the treatment dummies. The difference between baseline (the default) and Treatment II – i.e., the shaming effect in the non-framed games – is highly significant. The (threat of) disclosure increased contributions by 17.52 ECU over baseline.

From Table 4, we cannot directly deduce the shaming effect in the tax avoidance setting. Therefore, we report post-estimation Wald-Tests and show that shaming in the framed setting has a highly significant and positive effect on contributions (null hypothesis: $III = IV$). The size of the effect is 17.31–5.45. Displaying pictures of tax avoiders leads to tax payments 11.86 ECU higher than in the baseline.

Column (2) introduces further control variables. Only a few do significantly influence contributions. Being employed as well as studying economics leads to significantly less cooperative behavior while a stronger self-reported preference for rule-following and trust in institutions is connected to higher contributions.

Not surprisingly, contributions decrease the more rounds are played. The covariates reduce the magnitudes of the effects in Treatments II and IV to a small extent. However, the framing effect in the neutral context is now significant. Deciding about tax avoidance leads to payments that are around 6 ECU higher, compared to the unframed scenario.

The framing effect in the first three rounds (columns (3) and (4)) of the baseline proves to be larger and more robust. Because participants in the baseline contributed almost half of what they could, the difference to the other two treatments is smaller in the experiment’s early stages.

From column (4), older subjects contributed more, but the effect size is fairly small. Additionally, having a Bachelor’s degree and being in favor of shaming detected tax avoiders led to significantly higher contributions in the first three rounds. The study program does not matter for the experiment, and we also find no round effects.

Columns (5) and (6) analyze the last three rounds of the experiment. Here, the framing effect does not survive. The magnitude of the shaming effect remains very similar to its size in the first three rounds, however.

Full compliance. Our second dependent variable takes the value of 1 if a subject invested the maximum possible amount of 40 ECU into the public good and else zero. The variable’s binary character allows for a non-linear regression model in Eq. (1) and thus to estimate the increase in the likelihood of becoming “fully compliant”, conditional on being confronted with framing, shaming, or both. Because coefficients in nonlinear models cannot be meaningfully interpreted, we additionally report partial effects in Table 5.

Model (2) of Table 5 shows the baseline results for all 10 rounds. In the neutral setting, we find a significant framing effect. Deciding about tax avoidance increases the likelihood of full compliance (paying 40 ECU) by 16.8 percentage points, compared to a voluntary contribution.⁷ Wald-Tests show that a framing effect in the treatments II and IV does not exist ($p = 0.83$). Shaming has not only a large effect on contributions, but also on the likelihood of investing the maximum possible share of the endowment. In the neutral context, disclosing pictures and information on decisions increases the probability of full compliance by around 51 percentage points. As in Table 4, we cannot derive the shaming effect in the framed setting directly. A Wald-Test ($III = IV$) shows that the shaming effect is highly significant. The effect size is $0.53 - 0.168 = 0.362$. Thus, the likelihood of making a full contribution increases by 36 percentage points.

Adding controls (Column (4)), we see that *Rule following*, *Publicity*, and *Trust in the government* have a statistically significant but negligible effect on the probability of full compliance. In the last three rounds (columns (5) and (6)), we again see the framing effect disappear and the likelihood of full compliance decrease. Nevertheless, the shaming effect remains significant in both the neutral and the tax avoidance setting.

6.3 Interpretation

In two of our four treatments, free-riding on the public good was private information and not bound with any consequences. The only change was to present the

⁷Since both the dependent variable and the treatment dummies are binary, we get an increase in percentage points. For continuous variables, we measure an increase in %.

Table 5: PROBIT REGRESSIONS FOR FULL COMPLIANCE

	(1) All rounds	(2) All rounds	(3) All rounds	(4) All rounds	(5) Last 3	(6) Last 3
	<i>Coeff.</i>	<i>Marginal Effects</i>	<i>Coeff.</i>	<i>Marginal Effects</i>	<i>Coeff.</i>	<i>Marginal Effects</i>
Treatment III	0.904** (0.439)	0.168** (0.083)	1.096** (0.444)	0.183** (0.073)	0.478 (0.577)	0.061 (0.072)
Treatment II	2.728*** (0.510)	0.508*** (0.076)	2.642*** (0.551)	0.440*** (0.074)	2.671*** (0.820)	0.342*** (0.085)
Treatment IV	2.842*** (0.539)	0.530*** (0.077)	2.805*** (0.559)	0.467*** (0.076)	3.011*** (0.870)	0.385*** (0.088)
Age			0.054 (0.035)	0.009 (0.006)	0.059 (0.059)	0.008 (0.007)
Female			-0.102 (0.289)	-0.017 (0.048)	0.006 (0.417)	0.001 (0.053)
Semester			0.043 (0.036)	0.007 (0.006)	0.074 (0.053)	0.01 (0.007)
Econ. Major			-0.195 (0.311)	-0.032 (0.052)	-0.619 (0.485)	-0.079 (0.06)
Bachelor			0.294 (0.241)	0.049 (0.04)	0.063 (0.417)	0.008 (0.053)
Income			-0 (0)	-0 (0)	-0 (0)	-0 (0)
Employment			-0.174 (0.249)	-0.029 (0.04)	-0.350 (0.42)	-0.045 (0.051)
Tax declaration			-0.075 (0.247)	-0.013 (0.041)	0.076 (0.366)	0.01 (0.047)
Rule following			0.218** (0.092)	0.036** (0.016)	0.469*** (0.160)	0.060*** (0.019)
Publicity			0.147** (0.061)	0.024** (0.01)	0.071 (0.088)	0.009 (0.011)
Sociality			0.09 (0.119)	0.015 (0.020)	0.194 (0.191)	0.025 (0.025)
Trust (gov.)			0.298*** (0.073)	0.05*** (0.011)	0.330*** (0.115)	0.042*** (0.012)
Trust (others)			0.129 (0.164)	0.022 (0.027)	0.025 (0.239)	0.0032 (0.031)
Pol. alignment			-0.025 (0.065)	-0.004 (0.011)	-0.071 (0.094)	-0.009 (0.012)
Round			-0.186*** (0.023)	-0.031*** (0.004)	-0.412*** (0.119)	-0.053*** (0.015)
Constant	-1.948*** (0.356)		-4.891*** (1.252)		-3.870* (2.312)	
Observations	2, 150	2, 150	2, 010	2, 010	603	603
Wald-Tests						
II = III	p < 0.001		p < 0.001		p < 0.001	
III = IV	p < 0.001		p < 0.001		p < 0.001	
II = IV	p = 0.83		p = 0.769		p = 0.663	

Notes: The dependent variable takes a value of 1 if whenever a subject chose to invest 40 ECU (40 % of her endowment) to the public investment. Marginal effects are reported for each regression. See Table 2 for the description of the socio-economic covariates and Table 4 for the description of self-reported attitudes and beliefs. Standard errors, clustered on the group-level, are in parentheses. Wald-Tests are reported.

*** : $p < 0.01$; ** : $p < 0.05$; * : $p < 0.1$.

contribution mechanism in a different, morally loaded, context. In the tax avoidance frame, we find investments into the public good to be significantly higher, the share of full-contributors even twice as high. This difference could be attributed to the implicit rule of taxpaying as a duty. Although individually costly and probably not fancied, people have learned, and in many cases internalized, that taxes are to be paid. Without framing, contributions indicate to the pure attitude towards pro-social (cooperative) behavior, which is not only very volatile, but also less pronounced than in the case where contributions are presented as an institutionalized service to the community and as a moral duty.

The fabricated newspaper commentary that we presented to the participants might play a crucial role here. Contrary to studies which analyze the effect of moral appeals on tax compliance, we do neither explicitly invoke that, say, “the rule is to pay taxes” or “the right thing to do would be to pay taxes”, nor do we directly appeal to the subjects’ morality or their good consciousness. The text is first and foremost informative and should lead subjects to deliberate the moral aspects of tax avoidance and develop their own position on the issue, bearing the consequences – individual gains at the cost of decreasing tax revenue and a higher tax burden for the community – in mind.

In particular the first three rounds of our experiment show that many participants perceived tax avoidance as “morally wrong” and complied with their duty to pay taxes (a similar wording was used in the instructions). However, with no enforcement or sanctioning mechanism in place, simply framing a decision as legal but morally ambiguous does not stabilize persistently high levels of cooperation. Customization and the observed free-riding on behalf of other group members might erode the rule of taxpaying as a duty. In the last three rounds, no differentiation between tax payments and voluntary contributions can be made.

In our disclosure treatments we name tax avoiders (non-contributors) by publishing their pictures and information on their payments. The results demonstrate that social pressure cannot only significantly increase cooperative behavior but can also make it persistent; free-riding is eliminated to a large degree. In line with the literature (see Section 2), we attribute this effect to the anticipation of shaming (the feeling of being blamed by others), which individuals typically want to avoid. In

the framed context, we interpret shaming as arising from visibly and recognizably violating the imperative of tax compliance.

Interestingly, decisions in the neutral and the framed context became almost indistinguishable under the social pressure from disclosing pictures. We interpret this as follows: subjects in all experiments were aware of the social benefit of making high (or even full) contributions and of the material consequences resulting from free-riding. Without framing the voluntary contribution as a duty, however, there is no incentive to forego individual gains. With shaming as a non-monetary sanction, participants are, as in the tax-avoidance scenario, not willing to be revealed as cheap profiteers. In both settings, the subjects were ready to pay a very high price to prevent shaming.

An important reason why differences between baselines and treatments are persistent is that shaming is certain. When disclosure would be a matter of (small) probabilities, individuals might gamble and make individually rational decisions.

6.4 Extensions

Self-reported attitudes and beliefs. The regression analysis in Tables 4 and 5 shows that only few of our questions on attitudes and beliefs had a statistically significant influence on the decision of investing into a profitable public good. Moreover, for “rule following” and “trust in government”, the effects are quite small.

Table 6 reports mean values for our attitudinal questionnaire items and presents *t*-tests for statistical differences between the framing and shaming treatments. Since answers were collected after the experiments, results are likely to be influenced by experiences gained in the different treatments.⁸

These observations show that participants confronted with the disclosure of decisions attached (after the game!) a significantly higher value to always following given rules. However, preferences for rule-following were more pronounced when no implicit rule at all was introduced. “Trust in government” – which might be

⁸Overall summary statistics (i.e., not distinguished by treatment) for the items presented in Table 6 can be found in Table B.2 in Appendix B.

Table 6: SELF-REPORTED ATTITUDES AND BELIEFS BY EXPERIMENT (MEANS)

Variable					<i>t</i> -test, two-sided			
	Baseline	II	III	IV	I = II	III = IV	I = III	II = IV
Rule following	3.36	3.69	3.05	3.51	-4.82***	-6.86***	4.02***	3.11***
Publicity	1.74	2.64	2.00	3.15	-8.87***	-11.04***	-2.60***	-4.86***
Sociality	3.86	4.33	4.02	4.53	-8.65***	-8.82***	-2.36**	-4.57***
Trust in govt.	1.96	2.40	1.85	1.84	-5.54***	0.22	1.23	7.29***
Trust in others	4.34	4.40	4.36	4.58	-1.3	-4.46***	-0.49	-3.87***
Pol. alignment	4.08	3.67	4.25	3.51	3.07***	5.85***	-1.32	1.27

Notes: All items except for “Political alignment” are coded on a scale from 0 (strong disagreement) to 5 (strong agreement). “Rule following” question: *In your opinion, how important is it to always follow given rules?* “Publicity”: assessment whether names and pictures of detected tax avoiders should be disclosed. “Sociality”: assessment of the statement *It is important to make a contribution to the community.* “Trust in government” asked whether one would be more willing to contribute to the community if the government could be trusted to act responsibly with the subject’s contribution. “Trust in others”: *You cannot be careful enough when dealing with other people - Others can be trusted.* Political alignment on a scale from 0 (left) to 10 (right).

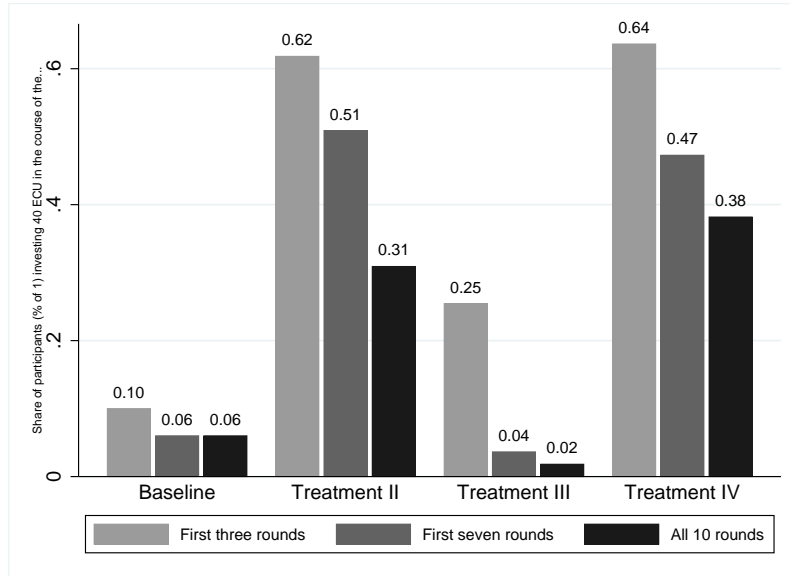
understood as a pro-attitude towards taxation and collective action – only matters when the government is either no part of the game (trust in government is higher when free-riders are named) or in comparison of the treatments (trust in government is higher in the neutral setting).

Participants stated a significantly more positive opinion on the disclosure of free-riders when they themselves were assigned to a treatment where this actually happened to non-cooperative subjects. Participants also had a more positive opinion on shaming when not contributing implied tax avoidance. The same conclusions apply to the item “Sociality” and “Trust in other people”.

Survival of full compliance. Figure 5 tracks the share of participants who continuously contributed (paid taxes) the maximum possible part of their endowment.

Unsurprisingly, the numbers for the baseline are very small. In Treatment III (framing, no disclosure) however, 25% of participants paid 40% taxes in each of the experiment’s first three rounds. For longer intervals, this value plummeted below the values of the baseline. This might be cautiously seen as a sign of contagion from repeatedly observing violations of the taxpaying-norm. In the neutrally framed setting with disclosure, 31% of the subjects always contributed

Figure 5: SHARE OF FULLY COMPLIANT PARTICIPANTS (IN %)



Baseline: No framing/No pictures, Treatment II: No framing/Pictures, Treatment III: Framing/No pictures, Treatment IV: Framing/Pictures.

the maximum possible amount; with framing, the share is 38%. However, we find a steady decline in full compliance over time: over the first three rounds, more than 60% of participants had invested 40% of their endowment. Still, the differences between non-disclosure and disclosure groups remain large.

7 Conclusions

Our experiment tests two mechanisms that, in principle, help to promote pro-social behavior when pecuniary incentives (rewards or fines) or legal enforcement are not available: moral framing and shaming.

We, first, show that framing subjects into a specific setting with an implicit moral duty to cooperate leads to increased investments into a public good: participants are less likely to free-ride when they decide in the context of an legally admissible yet socially reprehensible behavior. However, without any enforcement such a behavioral standard might quickly erode.

We, second, show that social control (fear of being shamed) can significantly reduce free-riding and perhaps even stabilize cooperation: the fear of being blamed for selfish and socially irresponsible behavior is great – and even great enough to make cues to moral issues superfluous to steer individual decisions.

With our experimental design we cleanly identify the shaming effect since, in contrast to research on illegal behavior, the effect of disclosure is not confounded with elements of deterrence or strategical concerns resulting from the (moderate) risk of getting caught. When shaming is certain, subjects are willing to pay a very high price to circumvent it.

Transferring our results to policy debates, the significant role of public exposure suggests that (the threat) of disclosing unwarranted behaviour is an effective strategy for reducing tax avoidance and, more generally, promoting pro-social behavior. The communication of moral arguments also works – but less effectively and, once a shaming mechanism is in place, without noticeable effect.

In our experiment, shaming leaves subjects better off in terms of monetary payoffs. This does not imply, however, that the high social pressure it obviously induces is welfare-increasing in general (also see DellaVigna et al., 2012, for a related point). Moreover, pillorying could be questionable outside the lab and would quickly collide with concerns about privacy and human rights, especially when it sets in at the slightest incidence of wrong-doing. Concerning real-world equivalents, however, the concept of (institutionalized) shaming has recently received heightened attention in the context of taxation⁹ and our results affirm the general (if transitory) efficacy of such measures. Whether such gains can outweigh the cost of the pillory obviously is a question that cannot be answered in a laboratory.

⁹For instance, publicly available tax returns led to a significant increase in tax-declared income in Norway (Bø et al., 2016). Authorities in Slovenia and Greece are or have been publishing names and addresses of (notorious) tax delinquents.

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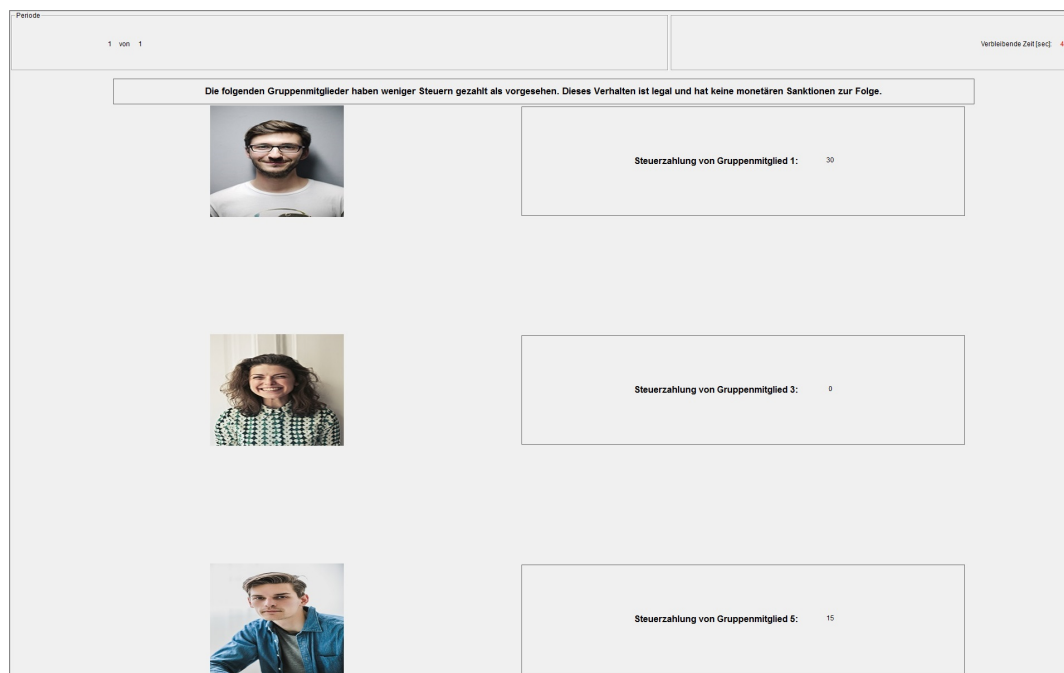
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Appendices

A Screen messages

Disclosure screen

Figure A.1: DISCLOSURE SCREEN WITH PICTURES AND PAYMENT INFORMATION



In this sample picture, group members 1, 3, and 5 avoid taxes. Their pictures, together with information about their tax payment are disclosed to all group members. Group members 2 and 4 did not avoid taxes and thus remain anonymous. The headline reads “The following group members have reduced their personal tax burden. Doing so is legal and not connected with monetary consequences”. In the non-framed experiment, the message read “The following group members contributed less than the maximum possible amount of 40 ECU.” The pictures do not show actual participants. The photos were retrieved from the website pexels.com, which provides free stock photos with a CC0 (Creative Commons Zero) license for private and commercial use.

Newspaper article

Figure A.2: NEWSPAPER COMMENTARY

Steuervermeidung ist legal, aber ist sie auch legitim?

Kommentar

In der jüngeren Vergangenheit haben Whistleblower Steuervermeidung im großen Stil aufgedeckt. Beteiligt sind nicht nur multinationale Konzerne, sondern auch tausende Privatpersonen, die über Briefkastenfirmen Einkommen oder Vermögenswerte an den heimischen Steuerbehörden vorbeilenken. Diese Praxis ist in der Regel legal, rechtliche Konsequenzen drohen nicht.

Steuervermeidung ist in erster Linie keine Frage von Unrecht im juristischen Sinn. Es geht eher um die Moral in einem Wirtschaftssystem, dessen Kapital längst keine Grenzen mehr kennt, dessen Steuern aber an Länder gekettet sind. Verhält sich ein Steuerbürger moralisch, der das Gemeinwesen prima findet, wenn es Universitäten, Straßen oder Polizisten finanziert, der es aber, wenn es das eigene Einkommen betrifft, als Gegenspieler begreift?

Steuervermeidung ist eine Gesetzesumgehung mit legalen Mitteln. Steuervermeider mögen zwar im

engeren Sinn gegen keine Steuergesetze verstoßen, sie handeln allerdings auch nicht im Geiste des Steuergesetzes. Vielmehr noch, Steuervermeider untergraben die Integrität des Steuersystems, denn oft war die von ihnen gewählte Steuergestaltung so vom Gesetzgeber nicht gewollt oder vorgesehen.

Jede und jeder Steuerpflichtige mit der Möglichkeit zur Steuervermeidung steht vor der persönlichen Entscheidung, ob sie oder er Steuervermeidung vor sich selbst und vor anderen rechtfertigen kann. Dies sollte allerdings unter Abwägung der gesellschaftlichen Konsequenzen geschehen: Steuervermeidung führt zu geringeren Steuereinnahmen und somit zu weniger Leistungen des Staates beziehungsweise zu einer ungleichen Verteilung der Lasten.

Nicht alles was legal ist, ist auch legitim. *tas*

As presented to participants on screen prior to Round 1.

English translation:

“Tax avoidance is legal, but can it be legitimate?”

In the recent past, whistle-blowers have uncovered large-scale tax avoidance. It not only involves large multinational corporations but also thousands of individual taxpayers who use shell companies to hide income or assets from national tax authorities. Generally, these tax practices are lawful; no legal consequences are looming.

Primarily, tax avoidance is not a matter of wrongdoing in any legal sense. It is more a matter of morality in an economic system where capital flows know no

boundaries while taxation is still chained to nation states. Does a taxpayer behave ethically when he cherishes public funding for universities, infrastructure, or the police but considers the state as an opponent as soon as funding affects his own purse?

Tax avoidance means bypassing the law with legal means. Tax avoiders might not violate tax laws directly – but they do not act in its spirit either. Moreover, tax avoiders undermine the very integrity of the tax system since the adopted tax strategy was neither wanted nor intended by the law-maker.

Every taxpayer with an opportunity to avoid taxes faces the personal choice whether he or she can make tax avoidance seem right - for himself/herself and for others. This, however, choice should be made with bearing the societal consequences in mind: tax avoidance leads to lesser government revenues and, thus, to fewer public services and a more unjust distribution of the tax burden.

Not everything that is legal is also legitimate.”

B Additional Results

Table B.1: SUMMARY STATISTICS OF SOCIOECONOMIC ITEMS BY TREATMENT

Variable	Baseline	Treatment II	Treatment III	Treatment IV
Female	44%	55%	44%	47%
Economics Major	24%	25%	20%	15%
Bachelor degree	40%	29%	35%	36%
Employed	38%	35%	44%	28%
Tax declaration	72%	56%	69%	53%
Age	24.29 [23] (3.07)	23.94 [24] (3.12)	23.96 [24] (2.57)	24.54 [24] (7.64)
Income	299.85 [300] (225.31)	341.64 [300] (248.03)	321.98 [300] (253.24)	409.72 [355] (285.91)
Semester	7.06 [6] (4.41)	6.51 [6] (3.7)	7.09 [7] (3.14)	7.45 [6] (4.64)

Notes: Number of subjects in Baseline is 50. Number of subjects in treatments II, III and IV is each 55. For description of items see Table 2. Median values are in brackets, standard deviations are in parentheses.

Table B.2: SUMMARY STATISTICS FOR SELF-REPORTED ATTITUDES AND BELIEFS

Variable	Mean	Median	Std. dev.
Rule following	3.40	4.00	1.13
Publicity	2.40	2.00	1.76
Sociality	4.19	4.00	0.95
Trust in government	2.01	2.00	1.35
Trust in others	4.42	5.00	0.79
Political alignment	3.87	4.00	2.15

Notes: All items except for *Political alignment* coded on a 0-5 scale, ranging from strong disagreement to strong agreement. *Rule following* question: “In your opinion, how important is it to always follow given rules?”. *Publicity* assesses whether detected tax avoiders should be publicly disclosed. *Sociality* assesses the statement “It is important to make a contribution to the community”. *Trust in government* asks whether participants would be more willing to contribute to the community if the government could be trusted to act responsibly with the contribution. *Trust in others* rates whether “[y]ou cannot be careful enough when dealing with other people – others can be trusted”. *Political alignment* is measured on a 0-10 scale from left to right.

Table B.3: EFFECTS ON THE SHARE OF FULL COMPLIANCE

Panel A: Effect of disclosure in the neutral setting (Treatment I, II)

Rounds	Baseline ($N = 50$)	Treatment II ($N = 55$)	p -value
1-10	12	68	< 0.001
1-3	18	76	< 0.001
8-10	10	52	< 0.001

Panel B: Effect of disclosure in the framed setting (Treatment III, IV)

Rounds	Treatment III ($N = 55$)	Treatment IV ($N = 55$)	p -value
1-10	25	69	< 0.001
1-3	42	78	< 0.001
8-10	12	57	< 0.001

Panel C: Framing effect without disclosure (Treatment I, III)

Rounds	Baseline ($N = 50$)	Treatment III ($N = 55$)	p -value
1-10	12	25	< 0.001
1-3	18	42	< 0.001
8-10	10	12	0.666

Panel D: Framing effect with disclosure (Treatment II, IV)

Rounds	Treatment II ($N = 55$)	Treatment IV ($N = 55$)	p -value
1-10	68	69	0.698
1-3	76	78	0.694
8-10	52	57	0.321

Notes: All values in percent. All measures are analyzed for all periods and the first as well as the last three periods. N denotes the number of subjects per treatment. p -values present the significance level of differences between the respective experiments in a two-sample Mann-Whitney U -test.

Table B.4: DIFFERENCES-IN-DIFFERENCES BETWEEN THE FOUR EXPERIMENTS

Panel A: Differences in the effects of disclosure on contributions: neutral versus framed

Rounds	Difference II–I	Difference IV–III	p -value
1-10	17.52	11.85	0.002
1-3	15.89	9.09	0.007
8-10	15.08	13.76	0.070

Panel B: Differences in the effects of framing on contributions: disclosure versus no disclosure

Rounds	Difference IV–II	Difference III–I	p -value
1-10	-0.21	5.45	< 0.001
1-3	0.12	6.92	0.007
8-10	1.27	2.6	0.028

Panel C: Differences in the effects of disclosure on the share (in %) of full compliance: neutral versus framed

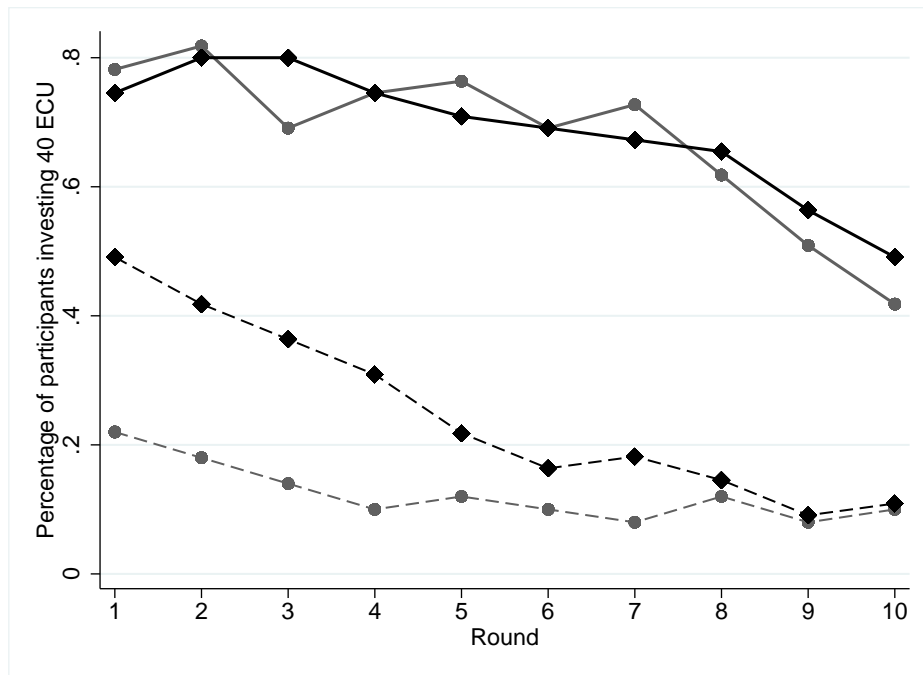
Rounds	Difference II–I	Difference IV–III	p -value
1-10	55.24	43.82	0.012
1-3	58.36	35.76	0.070
8-10	41.52	45.45	0.140

Panel D: Differences in the effects of disclosure on the share (in %) of full compliance: disclosure versus no disclosure

Rounds	Difference IV–II	Difference III–I	p -value
1-10	1.09	12.51	0.052
1-3	1.82	24.42	0.007
8-10	5.45	1.52	0.287

Notes: All measures are analyzed for all periods and the first as well as the last three periods. Average differences between experiments are calculated for each of the $N = 10$ rounds per experiment. p -values represent the significance level of the mean differences in differences, tested by two-sample Kolmogorov-Smirnov tests. Exact p -values are reported. A p -value less than 0.1 indicates that one group is significantly smaller than its counterpart.

Figure B.3: SHARE OF FULL CONTRIBUTORS



Notes: Dashed blue line: Experiment I: No framing/No pictures. Solid blue line: Experiment II: No framing/Pictures. Dashed red line: Experiment III: Framing/No pictures. Solid red line: Experiment IV: Framing/Pictures.

C Instructions

Instructions given to the participants in the experiments were originally in German. This Appendix presents their English translation. Distinct instructions were prepared for each experiment. In the following, the different versions are combined, with variations between experiments being marked by square brackets.

General Information [common to all treatments]

Thank you for participating in today's experiment. Upon completion of the experiment you will receive a participation fee of 4 euros. This fee is independent of the experiment's events. In the experiment you have the chance to earn additional money. The amount will depend on your decisions and on the decisions of your fellow players. The total payoff constitutes of your earnings in the experiment plus the participation-fee. Today's experiment consists of a total of 10 rounds.

Please observe the following:

- Please read the instructions thoroughly. If you do not understand certain aspects, do not hesitate to ask. However, do not ask your question audibly. Instead, please raise your hand.
- Seats are provided with a visual cover. Verbal communication with fellow participants is not permitted. You also must not leave your seat.
- Please turn off your mobile phone or other electronic devices and store them in your bag.
- The pencil on your desk can be used. On the instructions, you may make markings or take notes.
- The program with which the experiment is carried out must not be closed. Please do not open any other programs on the computer.
- Standby times might occur because participants proceed at different speeds.

- At the end of the experiment with a total of 10 rounds, one of the rounds will be randomly selected for your payout (in cash).
- You will receive your total earnings at the end of the experiment. Please return the instructions to the experimenter.

Proceedings

In the experiment you will be part of a group consisting of exactly 5 group members. The composition of the group is fixed over the course of the experiment. The 10 rounds are independent.

Initial endowment and decision [Treatment I, II] In each of the 10 rounds, you (and each member of your group) have an endowment of 100 ECU (Experimental Currency Unit) at your disposal. Of the 100 ECU, a maximum of 40 ECU can be invested in a profitable public venture. However, you can also decide to invest less or nothing at all. Depending on your decision, at least 60 ECU and maximum 100 ECU go to a private account which does not yield profits. The exchange rate from ECU to euro is 1:10, i.e., 10 ECU equals 1 euro.

Initial endowment and decision [Treatment III, IV] In each of the 10 rounds, you (and each member of your group) are endowed with a taxable income of 100 ECU (Experimental Currency Unit). The linear tax rate is 40%, that is, you have to pay 40 ECU taxes. Your tax payment and the taxes of your fellow group members will be invested in a profitable public venture.

However, you have the legal possibility to reduce your individual tax burden: in each round of the experiment, you decide how much tax you pay. This simultaneously determines the rate with which your endowment is taxed. If you do not use the possibility to reduce your tax burden, the tax rate remains at the scheduled 40%. Reducing the tax burden is legal and, thus, not connected to any monetary sanctions. Depending on your decision, at least 60 ECU and maximum 100 ECU go to a private account which does not yield profits. The exchange rate from ECU to euro is 1:10, i.e., 10 ECU equals 1 euro.

Earnings and payoffs

[Treatment I, II:] Revenues from the public venture depend on the contributions of all group members. The following applies: the more a group member contributes, the higher the returns for each group member.

[Treatment III, IV:] Revenues from the public venture depend on the tax payments of all group members. The following applies: the more taxes a group member pays, the higher the returns for each group member.

Payoffs in each round emerge as follows:

$$\text{Payoffs} = \text{Stock of private account} + \text{revenue from the public venture.}$$

Table 1 [here: Figure C.4] exemplifies some payoffs, conditional on your own contribution and the contributions of the other four group members.

Please note:

- [in Treatment I, II:] For simplicity, you cannot select an arbitrary contribution between 0 and 40 ECU. You choose from the series of values depicted on your screen.
- [in Treatment III, IV:] For simplicity, you cannot select an arbitrary tax payment between 0 and 40 ECU. You choose from the series of tax payments depicted on your screen.
- [in Treatment I, III:] Your decision about your contribution [tax payment, Treatment III] will not be disclosed. The group members will not learn how much you invested into the public venture.
- [in Treatment II, IV:] Your decision about your contribution [tax payment, Treatment IV] will be disclosed. The group members will learn how much you invested into the public venture.

Figure C.4: PAYOFF TABLE

		Own contribution/tax payment (in ECU)								
		0	5	10	15	20	25	30	35	40
Sum of contributions/tax payments of the other four group members (in ECU)	0	100,0	96,5	93,0	89,5	86,0	82,5	79,0	75,5	72,0
	5	101,5	98,0	94,5	91,0	87,5	84,0	80,5	77,0	73,5
	10	103,0	99,5	96,0	92,5	89,0	85,5	82,0	78,5	75,0
	15	104,5	101,0	97,5	94,0	90,5	87,0	83,5	80,0	76,5
	:	:	:	:	:	:	:	:	:	:
	30	109,0	105,5	102,0	98,5	95,0	91,5	88,0	84,5	81,0
	:	:	:	:	:	:	:	:	:	:
	45	113,5	110,0	106,5	103,0	99,5	96,0	92,5	89,0	85,5
	:	:	:	:	:	:	:	:	:	:
	60	118,0	114,5	111,0	107,5	104,0	100,5	97,0	93,5	90,0
	:	:	:	:	:	:	:	:	:	:
	75	122,5	119,0	115,5	112,0	108,5	105,0	101,5	98,0	94,5
	:	:	:	:	:	:	:	:	:	:
	90	127,0	123,5	120,0	116,5	113,0	109,5	106,0	102,5	99,0
	:	:	:	:	:	:	:	:	:	:
	105	131,5	128,0	124,5	121,0	117,5	114,0	110,5	107,0	103,5
	:	:	:	:	:	:	:	:	:	:
	120	136,0	132,5	129,0	125,5	122,0	118,5	115,0	111,5	108,0
	:	:	:	:	:	:	:	:	:	:
	135	140,5	137,0	133,5	130,0	126,5	123,0	119,5	116,0	112,5
	:	:	:	:	:	:	:	:	:	:
	150	145,0	141,5	138,0	134,5	131,0	127,5	124,0	120,5	117,0
	155	146,5	143,0	139,5	136,0	132,5	129,0	125,5	122,0	118,5
	160	148,0	144,5	141,0	137,5	134,0	130,5	127,0	123,5	120,0

Information at the end of any round

After each group member has decided about their own contribution [tax payment, Treatment III,IV], your individual contribution [tax payment], the total group contributions [tax revenue] and your personal payoff in the respective round will be displayed to you on your screen.

[Addendum for Treatment II:] Additionally, those group members who invested less than 40 ECU to the public venture will be disclosed at the end of each round. In this case, the picture and the contribution of the respective group members will be presented to all group members. Who contributes the full amount will remain anonymous.

[Addendum for Treatment IV:] Additionally, those group members who paid less than the scheduled 40 ECU taxes will be disclosed at the end of each round. In this case, the picture and the tax payment of the respective group members will be presented to all group members. Who did not reduce the individual tax burden will remain anonymous.

Final Information [common to all treatments]

After reading these instructions we ask you to answer some questions on your computer. Answering these questions only checks comprehension and is not relevant for payoffs. The experiment will start upon completion of the comprehension test.

After the experiment we ask you to answer a few questions. For this purpose a short questionnaire will start automatically. The questionnaire is not relevant for payoffs either. [Added in Treatments II, IV: The answers will not be disclosed.]

Declaration of consent

To allow the use of their photographs for the experiments, participants had to consent to the following declaration.

For today's experiment it is necessary to take your picture and to save it digitally for a short time. Dependent on the events in the experiment it might happen that your photography is presented to you and other participants on screen.

Upon completion of the experiment your photo will be deleted from the camera and the computer. Continued use outside the lab or circulation of your data is foreclosed.

With your signature you consent to the possible use of your photography in today's experiment.

In case you refuse the possible use of your picture you cannot participate in today's experiment. Yet you still receive the participation fee of 4 euros.

Comprehension test

Question A1. A group consists of exactly 5 group members (Yes/No).

Question A2. The composition of the group changes during the experiment (Yes/No).

Question A3. Your decisions remain anonymous (Yes/No).

Question A4. All group members receive the same return from the public good (Yes/No).

Question B. What is your income if you invest 20 ECU and the four other group members invest in sum 120 ECU into the public venture?

Question C. What is your income if you invest 30 ECU and the four other group members invest in sum 60 ECU into the public venture?