A Self-Funding Reward Mechanism for Tax Compliance

Enrique Fatas[1], Daniele Nosenzo[2] , Martin Sefton[3] and Daniel John Zizzo[4]

Abstract: We compare in a laboratory experiment two audit-based tax compliance mechanisms that collect fines from those found non-compliant. The mechanisms differ in the way fines are redistributed to individuals who were either not audited or audited and found to be compliant. The first, as is the case in most extant tax systems, does not discriminate between the un-audited and those found compliant. The second targets the redistribution in favor of those found compliant. We find that targeting increases compliance when paying taxes generates a social return. We do not find any increase in compliance in a control treatment where individuals audited and found compliant receive symbolic rewards. We conclude that existing tax mechanisms have room for improvement by rewarding those audited and found compliant.

Keywords: tax evasion, rewards, audits.

JEL Classification Codes: C92, H26.

Acknowledgements: We thank the ESRC (NIBS Grant ES/K002201/1) for financial support, Axel Sonntag for research assistance, and participants to presentations at Brussels and Nottingham for useful feedback. The experiment was approved by the Economics Ethics Committee of a major British University. The usual disclaimer applies. Data available at OSF (Fatas, Enrique, Daniele Nosenzo, Martin Sefton, and Daniel J Zizzo. 2021. "A Self-Funding Reward Mechanism for Tax Compliance." OSF. osf.io/dpc64)

[1] Center for Social Norms and Behavioral Dynamics, University of Pennsylvania, and School of Business and Economics, Universidad ICESI.

[2] Department of Economics and Business Economics, Aarhus University.

[3] School of Economics, University of Nottingham.

[4] School of Economics, University of Queensland.

1. Introduction

A standard mechanism for encouraging tax compliance, and the mechanism used most widely by tax authorities, is an audit-based deterrence mechanism. Individuals are randomly audited and those found non-compliant are punished (e.g., fined). In this paper we deviate from the deterrence literature and report an experiment investigating whether the standard mechanism can be improved by incorporating positive rewards for those found compliant. Our experiment differs from previous studies on the use of rewards for promoting tax compliance in that we study a fully self-funded mechanism. Moreover, our experiment is designed to disentangle different behavioral explanations for compliance.

The rationale for rewards goes back to traditional elements in tax compliance: auditing and sanctioning. The use of audits allows authorities to place individuals in one of three categories, i) individuals who have been found to be non-compliant; ii) individuals who have been found to have complied, and iii) individuals who have not been audited. Current practice penalizes individuals in the first category, but does not distinguish between the second and third. In fact, individuals in the second category may be penalized if audits are costly and inconvenient for them. In this paper we explicitly consider the potentially positive effects of discriminating between the latter two categories, by introducing a mechanism that rewards individuals who are audited and found to be compliant. Note that our mechanism rewards honest taxpayers by using the information collected from the standard auditing process, so no additional information (and auditing cost) is needed.

Theoretically optimal audit schemes often prescribe rewards to those audited and found compliant (Border and Sobel, 1987; Mookherjee and Png, 1989), but these are rarely used in practice. One possible reason for the absence of rewarding systems is that a reward-based scheme that increases compliance will require additional rewards to be paid, and funding these rewards may be costly; this may be politically unpopular and constitutes a barrier to using such schemes.[1] Only recently, different tax agencies have implemented lotteries that may be interpreted as probabilistic rewards. Naritomi (2019) reports on the program carried out by the Sao Paolo state government. Consumers receive monetary rewards (lottery tickets) if they asked merchants for receipts. The paper reports a significant increase in the tax revenue in retail sectors (around 23%) over four years. Wan (2010) and Fabbri (2015) analyze similar schemes in China. Note that these lottery systems are designed to motivate consumers to get a receipt

[1] Another possibility is that rewards may crowd out intrinsic motivation to pay taxes, with an unclear net effect.

from the merchant, who is then induced to increase his tax compliance. Notably, rewards (i.e. lottery prizes) are not targeted at the taxpayer and are exogenously introduced into the system, so lotteries could fail to be self-funded if the positive impact on compliance is not enough to cover the cost of the rewards.

Our paper also contributes to the broad literature on mechanisms for the efficient contribution of public goods that combines rewards and sanctions in budget neutral ways (see Falkinger et al 2000, Sausgruber and Tyran, 2007, Fatas et al, 2010a, 2010b). As in this literature, we aim to improve compliance by applying a mechanism in a budget neutral way. Rather than focusing in public good provision, our paper extends the analysis towards tax compliance using self-funded rewards.

Previous laboratory experiments on tax compliance have mainly focused on mechanisms involving fines[2] (Andreoni et al., 1998; Alm, 2012; Alm, 2019; Mascagni, 2018 contain surveys; Choo et al., 2016 contains a discussion of the use of the experimental method for studying tax compliance; Kirchler and Wahl, 2010, provide a standardized survey inventory based on self-reports). The success of deterrence policies crucially relies on how taxpayers perceive the legitimacy and capability of tax authorities, and taxpayers' trust on the tax system. In Kirchler et al (2008)'s *slippery slope* framework the power of tax authorities and the trust of taxpayers in tax authorities robustly predict compliance (see Kogler et al, 2013). Batrancea et al (2019) support a robust and positive correlation between trust, power and compliance.[3]

We are aware of only a few experimental studies of tax compliance mechanisms incorporating monetary rewards: Alm et al. (1992a), Bazart and Pickhardt (2011), and Kastlunger et al. (2011) study reward-based mechanisms in the laboratory; Dwenger et al. (2016), Carrillo et al (2016), and Koessler et al. (2018), report the results of field experiments. In these designs, rewards are offered essentially as an additional, exogenous incentive to comply, either as a direct cash transfer or as the possibility to be entered into a lottery to win a cash prize.

[2] Some of the studies incorporate a public good element, which may be partially funded by fines. We specifically mean fines that are redistributed to taxpayers without discriminating between non-audited taxpayers and taxpayers found compliant.

[3] In their vast experimental study covering 44 countries in five continents, they also find that in high compliance environments, severe enforcement rules may decrease compliance. Kastlunger et al (2013) find a similar result in Italy: coercive power was found to be negatively correlated with trust, and enforced compliance positively correlated with tax evasion. Gobena and Van Dijke (2017) connect the success of coercion with identification with the nation among Ethiopian and US taxpayers.

Results are mixed: in some cases, rewards are found to increase compliance rates (as in the randomized field study run by Carrillo et al (2016) in Argentina, targeting property taxes) while other studies report no significant effect of the intervention (as in the field experiment run by Dwenger et al (2016) targeting a local church tax in Germany, as rewards increase compliance among honest participants, and decrease compliance among cheaters). This highlights, once more, the perils of relying on reward mechanisms that require additional resources to be injected into the system, since the (uncertain) benefits from increased compliance should be set against the (certain) cost of using rewards.

Differently from the theoretical and empirical mechanisms previously studied in the literature, the mechanism we study is a fully self-funded reward mechanism in the sense that rewards are directly funded from penalties. Specifically, in our mechanism the fines from non-compliant individuals are transferred to compliant individuals as far as this is possible. In the extreme case in which all audited individuals are non-compliant the fines are still added to the tax revenue, and distributed to non-audited taxpayers. In the other extreme, if all audited individuals are found compliant, no fines are collected, and no rewards are distributed, so no additional funds are required.[4]

To study the effectiveness of this mechanism we implement it in a *Targeted* treatment (so named since the resources collected from fines are targeted towards honest taxpayers) and compare this with an *Untargeted* treatment where we use a standard deterrence-based mechanism in which fines are incurred by those caught evading taxes and fine revenue is redistributed equally among non-audited individuals and individuals who are audited and found to be compliant. We use the Untargeted treatment as our baseline condition since in this condition honest reporting is not rewarded: the revenue raised by the tax authority (through tax reports and fines) is redistributed among taxpayers without discriminating between individuals who are found to be compliant and those who have not been audited (and who may be tax evaders).

The paper most closely related to ours is Fochmann and Kroll (2016), who also study the effectiveness of different mechanisms for redistributing tax revenue among taxpayers. In their experiment, subjects first choose a contribution to a public good and then report their income,

[4] Note that our mechanism always has consequences on the distribution of income, so some honest taxpayers benefit from rewards (if audited and found compliant) while some others, honest or dishonest, do not (if they are not audited). At the social level, there is never a need to feed additional resources into the system to fund the rewards mechanism.

which is taxed. They study the effectiveness of mechanisms where the revenue from taxation (including any fines paid by evaders) is redistributed only to the three highest (or lowest) public good contributors vis-à-vis a mechanism that redistributes tax revenue equally among taxpayers. They find that the equal distribution mechanism is most effective. A crucial difference between their mechanisms and ours is that in our experiment the criteria for redistribution are based on tax compliance decisions rather than contributions to a public good, and so our mechanism incentivizes honesty directly, rather than indirectly through the perceived fairness of the redistribution system. [5]

Our laboratory reward mechanism may be successful for a variety of reasons, quite apart from the intended incentive effects. One reason is that rewards may trigger an experimenter demand effect. Our participants could interpret the existence of rewards for honest reporting as an indication of desired behavior and comply because they simply want to conform with the desired behavior (see Zizzo, 2010, Karakostas and Zizzo, 2016, and Engel et al, 2020, for analysis of demand effects in the laboratory). Relatedly, participants could believe that the reward induces such a response from others and increase compliance because of social preferences that dictate complying when others are willing to do so. To study the role of these factors we also conduct a *Symbolic* treatment using a mechanism in which those audited and found compliant receive a negligible fixed reward. Thus, in this treatment the incentive effect of rewards is minimal, but the potential experimenter demand and beliefs effects may still be present.

We also systematically manipulate the amount dishonest taxpayers have to pay if caught (fines may be small, medium or high) and the social benefits participants may get from the compliant behavior of others (zero or positive). By exploring each combination of parameters we are able to identify whether compliance survives the elimination of social benefits, and control for other-regarding preferences, and whether compliance is driven primarily by deterrence (high fines). Moreover, we systematically control for the effect of rewards on beliefs by eliciting subjects' compliance expectations in an incentive compatible way.

Our results suggest that rewards significantly and substantially increase compliance only when taxes generate some social benefits. In particular, compliance is higher in our Targeted

[5] A related mechanism is also proposed by Duffy and Matros (2014) to increase voter turnout. They study, theoretically, a mechanism where the fines collected from non-participants are used to finance a lottery prize awarded to one voting participant. Croson et al. (2015) document how the redistribution of fines can effectively increase cooperation in different public good games when fines are imposed on low contributors.

treatment and this cannot solely be explained by errors or social preferences. We find instead evidence that the effect of the Targeted treatment is partially mediated by beliefs: the availability of targeted rewards makes subjects more optimistic about others' compliance, and this encourages their own compliance. Notably, we do not find differences between our Untargeted and Symbolic conditions.

The remainder of the paper is organized as follows. In the next section we describe our experimental design and procedures. We present our results in section 3, discuss them in section 4 and conclude in section 5.

2. Experimental design and procedures

2.1 Experimental design

Our experimental design involved three between-subjects' treatments, which we label *Untargeted*, *Targeted* and *Symbolic*, each involving groups of 9 subjects. All treatments were based on linear public good contribution games with tax audits. In every game subjects simultaneously chose whether to keep an endowment of £10 or whether to pay taxes that would generate a return of y for each group member ("comply" in what follows). In each group, three out of nine subjects were then randomly selected and audited. Audited subjects that were found non-compliant had to pay a fine f, whereas no fine was paid if an audited subject was found compliant. In all treatments, there were six games, each involving different combinations of public good return y (£0 or £2) and fines f (£0, £3 and £6). Table 1 shows the values of y and f in each of the six games.

Table 1: Experimental Games

Game	*y*	*f*
1	£0	£0
2	£0	£3
3	£0	£6
4	£2	£0
5	£2	£3
6	£2	£6

Our three treatments varied in how the fines collected from non-compliant subjects were redistributed across group members. In Targeted, the fines collected from those audited and found to be non-compliant were used as targeted rewards for audited subjects found to be compliant. In particular, label n_c and n_n the number of subjects who are audited and found

compliant and non-compliant, respectively. If a subject was audited and found compliant, he or she earned a reward equal to $(n_n \cdot f / n_c)$. If no audited subject was found compliant, the money collected from fines was evenly redistributed across all non-audited subjects.

In the Untargeted treatment, the money collected from those audited and found to be non-compliant was evenly redistributed across all other subjects. This mechanism does not distinguish between honest taxpayers and taxpayers who have not been audited (and who may or may not be honest). Thus, honest taxpayers are not rewarded in the Untargeted treatment, and we can use this treatment as a benchmark condition to gauge the effectiveness of our targeted mechanism, where the rewards are specifically directed to honest taxpayers, vis-à-vis a mechanism that does not reward honest reporting.[6]

The Symbolic treatment was introduced to disentangle between two potential effects of targeting rewards: the material incentive effect (i.e. the fact that rewards are concentrated in the hands of compliant subjects who were audited) and a potential psychological "demand" effect, whereby by targeting rewards to compliant participants the mechanism may implicitly signal that compliance is desirable. In the Symbolic treatment, we still use targeted rewards but reduce their monetary value to a negligible amount in order to eliminate the material incentive effect of targeting while preserving its potential psychological effect.[7] In particular, in the Symbolic treatment, the money collected from those audited and found non-compliant was evenly redistributed across all other subjects, as in the Untargeted treatment; in addition, as in Targeted, compliant subjects received a targeted reward. However, the Symbolic treatments used a fixed targeted reward of £0.05, the smallest money unit used in our experiment. The £0.05 reward has little monetary value, but may nevertheless carry a significant psychological value for subjects if they interpret it as a signal that compliance is desirable.

Assuming that subjects are risk neutral and care only about their own monetary payoffs, in all games and in all treatments, there is a unique Nash equilibrium in dominant strategies where

[6] In the Untargeted treatment, the revenue from fines is not redistributed to those individuals who are audited and found non-compliant. This is to maintain comparability with the Targeted treatment. Had we redistributed the money also to non-compliant individuals, we would have effectively lowered the cost of the fine relative to the Targeted treatment. An alternative would have been to design a treatment where fines collected are not redistributed to subjects – but this again would have lowered comparability with the Targeted treatment.

[7] Note that our design prevents us from studying the "bomb-crater-effect" (taxpayers complying less after being audited), associated by Kastlunger et al (2009) to a misperception of chance by taxpayers (affected by the gambler's fallacy). Mittone et al (2017) show how this result survives among sophisticated Bayesian players.

no subject complies.[8] However, there is ample evidence in the experimental literature that individuals often depart from such predictions. In the tax compliance experimental literature subjects pay taxes even though they maximize expected earnings by evading (e.g. Alm, et al., 1992b). Similarly, in the context of public goods games, many subjects contribute to the public good even when they have no material incentive to do so (see Ledyard, 1995 and Chaudhuri, 2011 for reviews of the literature). Voluntary payment of taxes or voluntary contributions to a public good could be due to a variety of reasons, for example errors, moral compliance norms, or social preferences. Based on this, it is reasonable to expect that, in some of our games, subjects may comply to some extent, even though the benchmark prediction is that they should not.

Our parameters were chosen to discriminate among alternative explanations for compliance and include some interesting special cases. When $y = 0$ there are no social returns from complying, and so this condition controls for motives based on social preferences. When in addition $f = 0$ the decision problem is essentially an individual decision problem in which a player's action has no effect on others and there are no incentives at all to comply. The degree to which subjects comply in this case can be seen as a control for any inbuilt bias for compliance due, for example, to confusion (Andreoni, 1995; Houser and Kurzban, 2002) or a pure social norm to comply (Karakostas and Zizzo, 2016; Kimbrough and Vostroknutov, 2016). We also used parameterizations where taxes generate social benefits ($y = 2$) and varying positive fines ($f = 3$, 6).

We expect any positive compliance rate to be higher with $y = 2$ than with $y = 0$, as not only subjects with a concern for others' payoffs may prefer to comply, but also the opportunity cost of complying decreases if there is a public good return. Similarly, compliance should be higher the higher the fine f, as increases in f raise the expected cost of non-compliance.

It is worth noting that the opportunity cost of compliance is almost the same in all treatments if $f = 0$ or if all other taxpayers are compliant (with a relatively minor difference in the *Symbolic* treatment, as honest taxpayers who are audited get the symbolic payment). However, if $f > 0$, for a given number of others who are non-compliant, the opportunity cost of complying becomes comparatively smaller in *Targeted* relative to *Untargeted* and *Symbolic*. Thus, with positive fines, and as long as the experimental manipulation of mechanisms does not alter subjects´ beliefs about the overall compliance level, we would expect higher compliance in the

[8] See Appendix A for details.

Targeted treatment. Because of that, we systematically control for beliefs in the second part of the experiment, as the next section explains.

2.2 Experimental procedures

The experiment was programmed in zTree (Fischbacher, 2007) and run at a British University. Subjects were invited from the university subject pool using ORSEE (Greiner, 2015).[9] In total 162 students participated in nine sessions (18 subjects per session), earning £12.71 for ninety minutes on average. No subject took part in more than one session. All instructions were provided in a neutral frame.[10] Every session consisted of two parts. Before starting with Part 1, participants were randomly selected into groups of nine and remained in the same group for the rest of the session, i.e. for Part 1 and 2.

Part 1

At the beginning of Part 1, subjects received instructions that were also read aloud by the experimenter, and that did not include details of Part 2. Part 1 consisted of six tasks, each corresponding to one of the six games shown in Table 1; the tasks were presented to subjects in randomized order with no feedback until the end of the experiment.[11] Subjects learned that their final earnings in each task would be the sum of their *decision* earnings and their *selection* earnings for that task. Both types of earnings were explained in detail and the subjects' understanding of how earnings were calculated was checked before making any decisions.

In each of the six tasks subjects saw a "decision earnings" payoff table similar to the one in Figure 1. Subjects were asked to either choose A (corresponding to non-compliance) or B (corresponding to compliance).

[9] The subject pool mainly contains university students.

[10] Although the framing was neutral (subjects chose between two options, A and B, with no reference to taxation, audits, fines or rewards), the decision task preserved the key features of tax compliance decisions. Subjects knew that by selecting "option B" they were generating a monetary return for other participants (modeling the public good provided through tax revenue), and that a payment would be subtracted from their final earnings if they were randomly "selected" by the computer and found to have selected "option A" (modeling the auditing and sanctioning of tax evaders). The experimental instructions are provided in the Supplementary Materials. For a recent experiment studying the role of framing for tax compliance, see Casal et al. (2016).

[11] We randomly determined 9 orderings of the six tasks prior to the first session of the experiment and, in each group, we assigned one order to a subject. Thus, the orderings are balanced across treatments and sessions.

Figure 1: Decision earnings of one out of six part 1 tasks

Decision earnings:

Overall number of group members choosing B:	0	1	2	3	4	5	6	7	8	9
Earnings of a group member who chooses A	10.00	12.00	14.00	16.00	18.00	20.00	22.00	24.00	26.00	-
Earnings of a group member who chooses B	-	2.00	4.00	6.00	8.00	10.00	12.00	14.00	16.00	18.00

Notes: Example with $y = 2$. Note that for y=0, earnings of a group member who chooses A are always 10 and earnings of a group member who chooses B are always 0.

Subjects were informed that, for each task, the computer would choose three out of nine members of a group at random.[12] Subjects were also carefully informed about the consequences of being selected (audited) and found non-compliant. If a selected subject chose A in that task, he/she would be fined for doing so and received negative selection earnings. If the selected subject chose B, he/she would either get zero or positive selection earnings, depending on the treatment and the decisions made by the other audited participants. The calculation of the selection earnings for all subjects varied across treatments as described in section 2.1.

Before making any decisions subjects were also provided with and trained to use an "earnings calculator" that could be used to calculate the final earnings for all subjects, for arbitrary inputs. Subjects were required to fill a short questionnaire controlling whether they understood the experimental set up of Part 1. All subjects had to answer all questions correctly before proceeding to the actual tasks of Part 1. After finishing all six tasks of Part 1 (choosing either A or B for six different payoff tables), subjects received additional instructions for Part 2.

Part 2

Part 2 had another six tasks. Each of the games presented in Part 1 was presented again in randomized order, but this time subjects were asked to provide an estimate of how many subjects in their group chose B in Part 1 for that given game. This belief elicitation was incentivized such that subjects whose stated beliefs exactly matched the actual number of B choices in their group received an additional £3 and those within the range of +/-1 received an additional £1.50.

[12] The realization of the random draw was disclosed to subjects only after they had made their choices in all six Part 1 tasks.

The experiment concluded with a question to measure risk attitude;[13] with the Social Desirability Scale (Stöber, 2001) 16 items questionnaire, that may be helpful to measure the individual sensitivity to comply out of a perceived social or experimental pressure to do so; and with standard demographic questions (see Hofmann et al., 2017 for the role of socio-demographic variables in tax compliance decisions).[14]

Payment followed. A random incentive lottery system was used by which only one of the tasks, randomly determined, was paid for real from each part. Subjects did not know which tasks were selected until the end of the experiment.

3. Results

The 162 subjects were equally distributed across treatments, resulting in 54 subjects per treatment. Table 2 shows that samples were well balanced across treatments with respect to age and gender, though with fewer economics major students in the Symbolic treatment and notable variability in risk attitudes across treatments;[15] this will be controlled for in the regression analysis in section 3.2.

Table 2: Sample characteristics

	Untargeted	*Targeted*	*Symbolic*
Age [years]	23.06 (4.88)	22.78 (4.73)	22.74 (4.32)
Gender: Male [%]	46.30 (49.94)	44.44 (49.77)	40.74 (49.21)
Economics [%]	20.37 (40.34)	20.37 (40.34)	9.26 (29.03)
Risk (amount invested) [GBP]	45.15 (96.92)	55.78 (142.16)	35.94 (74.10)

Notes: means, standard deviations in parentheses

[13] The question asked: "Suppose that in a lottery game the possibility to win £1,000 is 10%. How much would you pay at most to buy a lottery ticket?" and subjects responded by entering a whole amount in British Pounds. In the regressions of section 3 we will rescale subjects' responses by dividing each amount by 100.

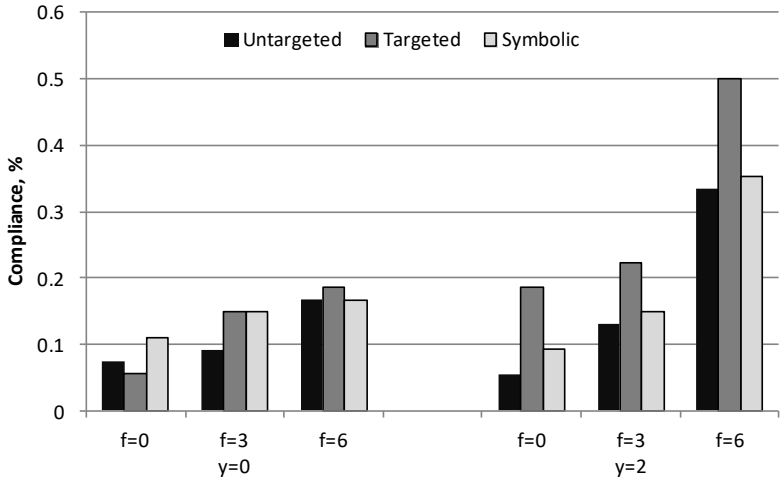
[14] Hofman et al (2017) is probably the most recent meta-study of survey based compliance studies and documents a large variation in the effects of demographic characteristics (age, sex, education and income), leading the authors to conclude that they have little impact on compliance. Kastlunger et al (2010) includes an excellent review of gender effects in tax compliance. In line with previous results, women are more compliant than men (and only men decrease compliance after being audited) in northern Italy (while Kirchler and Maciejovsky (2001) find the opposite result in nearby Austria).

[15] The treatment differences in age, the proportion of economics students, the proportion of male students, or amount invested in the risk task are not significant according to Kruskal-Wallis (for Age and Risk) and χ^2 tests (for Gender and Economics) (all p-values > 0.606). Non-parametric tests reported in the paper are at the level of individual subjects (using individual-level averages when needed to control for non-independence of observations by the same subjects). Note that subjects received no information about other subjects' decisions until the end of the experiment and so there are no cross-subject interdependencies between observations within a session.

3.1 Compliance Rates

Figure 2 shows average compliance rates across treatments for different combinations of *f* and *y*. In all treatments the observed compliance rates are significantly higher than the benchmark prediction of total non-compliance, but vary considerably across games. The broad patterns of compliance show that subjects respond to incentives in an intuitive way, which reflects our conjectures outlined in section 2.1. When $f = 0$ and $y = 0$, the compliance rate is about 8% across all treatments. As *f* and *y* increase, mean compliance rates broadly increase in all treatments, in line with our priors. Compliance rates tend to be higher when there is a public good return from compliance ($y = 2$) than when there is not ($y = 0$), and the effect is particularly marked in the Targeted treatment. [16] They also tend to be higher with high ($f = 6$) than with low ($f = 3$) or zero ($f = 0$) fines.

Figure 2: Percentage of compliant choices across treatments

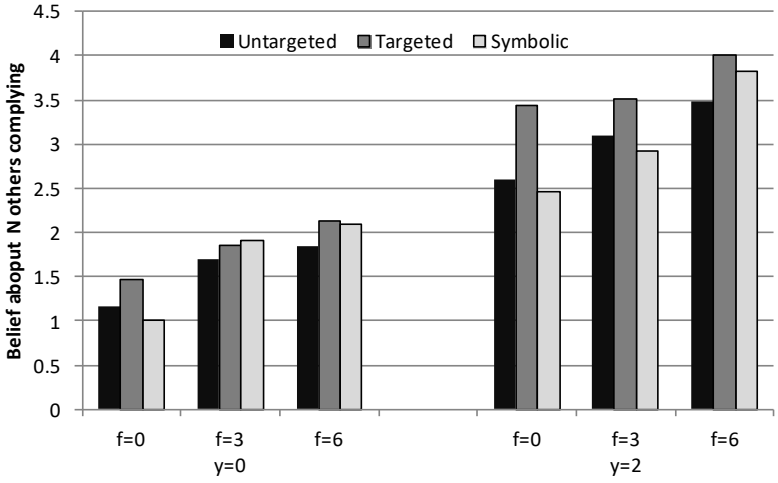


What makes rewards effective? By inspection, Figure 2 shows that treatment differences are more marked under $y = 2$ than under $y = 0$. This is preliminary evidence for the conclusion that targeted rewards are effective in raising tax compliance and that their effectiveness does not stem from its symbolic value. We will analyze the statistical significance of these effects in section 3.2, using regression analysis.

[16] Note with f=0 and y=2 compliance is higher in *Targeted* than in *Untargeted* , making it hard to fully reconcile the increase in compliance with revenue maximizing behavior, as there are no monetary incentives to comply in this game. However, Figure 2 also shows that the treatment effect goes in the same direction in 5 out of 6 possible combinations of f and y, in some cases with even a larger magnitude (e.g. when y=2 and f=6).

Before moving to the formal analysis, in Figure 3 we show subjects' beliefs of compliance in each of the six games of our three treatments. The height of each bar is the average across all subjects of the (incentivized) guess about how many other members of the group are compliant. The observed patterns in the beliefs of compliance mirror the observed patterns in actual compliance rates. Spearman correlations between choice to comply and belief about the number of other subjects complying are always positive for all six (*y, f*) combinations, and range between 0.152 (p-value = 0.054)[17] and 0.375 (p-value < 0.001).[18] Subjects expect others to comply less when $y = 0$ than when $y = 2$. Beliefs of compliance also increase in the level of the fine *f*.

Figure 3: Belief about number of other subjects complying



3.2 Regression analysis

Table 3 contains Logit regressions with standard errors clustered at the subject level to control for non-independence of observations. We estimate separate models for $y = 0$ and $y = 2$. The dependent variable is a binary variable indicating whether a subject complies or not. The Untargeted treatment is our control condition and therefore we use dummy variables for the Targeted and Symbolic treatments to verify effects relative to such baseline. Additional independent variables included control variables for whether subjects have an Economics major

[17] All p-values reported in the paper are two-sided, unless explicitly mentioned otherwise.

[18] The correlations are computed pooling across treatments for statistical power reasons. The largest correlation is for ($y = 2, f = 6$) and the smallest for ($y = 2, f = 3$). That said, subjects over-estimate the compliance rates of others; see Appendix B for more details. Appendix B also presents and discusses the finding that compliant subjects expect higher compliance rates from others than non-compliant subjects do, particularly in the Targeted and Symbolic treatments.

background (=1 if so), are male (=1 if so), their age, and their risk and social desirability scale scores as measured by the relevant questions (*Risk* and *SDS*, respectively). Models 2-3 and 5-6 also include a variable capturing the level of the fine *f*, and models 3 and 6 include the *Belief of compliance* (that is, the number of subjects believed complying).

Table 3: Regression analysis on the choice to comply

(a) $y = 0$			
Dep var: 1 if compliant choice	(1)	(2)	(3)
Targeted	-0.001	-0.001	-0.002
	(0.046)	(0.045)	(0.043)
Symbolic	-0.008	-0.008	-0.001
	(0.053)	(0.052)	(0.049)
f	--	0.014**	0.011*
	--	(0.005)	(0.005)
Belief of compliance	--	--	0.022**
	--	--	(0.006)
Economics	-0.033	-0.032	-0.034
	(0.041)	(0.040)	(0.034)
Male	-0.047	-0.045	-0.033
	(0.037)	(0.036)	(0.032)
Age	0.003	0.003	0.001
	(0.003)	(0.003)	(0.002)
Risk	-0.016	-0.016	-0.012
	(0.018)	(0.018)	(0.017)
SDS	0.017*	0.016*	0.011
	(0.008)	(0.008)	(0.007)
χ^2 *test Targeted = Symbolic, p-value*	0.882	0.883	0.956
N	486	486	486
(b) $y = 2$			
Dep var: 1 if compliant choice	(4)	(5)	(6)
Targeted	0.114*	0.116*	0.103
	(0.056)	(0.058)	(0.055)
Symbolic	-0.010	-0.010	0.003
	(0.056)	(0.055)	(0.053)
f	--	0.047**	0.042**
	--	(0.007)	(0.007)
Belief of compliance	--	--	0.037**
	--	--	(0.008)
Economics	-0.082	-0.080	-0.076
	(0.045)	(0.043)	(0.040)
Male	0.009	0.009	0.010
	(0.043)	(0.043)	(0.040)
Age	0.000	0.000	-0.000
	(0.007)	(0.007)	(0.006)
Risk	-0.032*	-0.032*	-0.026
	(0.016)	(0.015)	(0.014)
SDS	0.012	0.012	0.007
	(0.008)	(0.008)	(0.008)
χ^2 *test Targeted = Symbolic, p-value*	0.019	0.019	0.043
N	486	486	486

Notes: Marginal effects, standard errors in parentheses; ** and * stand for p-value < 0.01 and 0.05, respectively. Standard errors are clustered at the subject level to control for non-independence of observations that come from the same subjects.

In line with our previous discussion, the regressions in panel (a) of Table 3 show that treatments do not have an effect with $y = 0$. In the absence of a public good dimension, the way rewards are administered does not have any strong impact on willingness to comply. However, once we add the public good dimension - see panel (b) of Table 3 -, we observe differences in compliance rates depending on the mechanism used to administer rewards. When rewards are targeted to complying individuals, tax compliance increases with respect to both an untargeted mechanism and a mechanism that use targeted but purely symbolic rewards, and the effect is significant at the 5% level. The difference between Targeted and Untargeted becomes somewhat smaller and insignificant (p-value = 0.060) when the belief of compliance is taken into account (see model 6). This suggests that the effect of the Targeted treatment operates partly through changes in the perception of what others are doing. [19]

Among the control variables included in the regressions of Table 3, the fine *f* has a positive and significant impact on compliance, as already suggested by Figure 2. The impact of the fine is stronger for $y = 2$ than for $y = 0$, as shown by the marginal effects reported in Table 3. Also in line with our previous discussion, the likelihood of compliance is positively related to one's belief, both under $y = 0$ and $y = 2$, with the effect again being slightly larger in the latter case. We find some evidence that economists contribute less, in line with an established finding (e.g., Marwell and Ames, 1981; Carter and Irons, 1991), although again the effect does not reach statistical significance at the 5% level. We also find the intuitively appealing result that more risk loving subjects are less likely to comply, significantly so when y = 2 and belief of compliance is not controlled for. Finally, we find that, in the absence of a public good dimension, compliance rates are positively related to subjects' social desirability concerns: subjects who gave more socially-desirable responses in the questionnaire are also more likely to comply in the experiment. Interestingly, this effect vanishes once we add the public good dimension.

3.3 Earnings

Table 4 shows the realized payoffs in each of our six games across treatments. Unsurprisingly in the light of the success of our reward mechanism in eliciting greater compliance when there is a public good dimension, we find that that in the Targeted treatment on average subjects had

[19] We obtain similar results running non-parametric tests. Compliance in *Targeted* is significantly higher than in *Untargeted* (p-value=0.014) and *Symbolic* (p-value=0.049), running a rank-sum test with the average number of compliance decisions per individual as independent variable (for y=2). Similarly, the proportion of free-riders is lower in *Targeted* than in *Untargeted* (39% versus 59%, p-value=0.034) and *Symbolic* (39% versus 56%, but not significantly, p-value=0.083), using a χ^2 (again, for y=2).

greater earnings than in Untargeted or Symbolic, especially when $y = 2$. However, the differences are small. In fact, we do not find any significant differences in earnings between treatments, for either $y = 0$ or $y = 2$.[20]

Table 4: Realized earnings

		Untargeted	Targeted	Symbolic
$y = 0$	$f = 0$	9.26 (2.64)	9.44 (2.31)	8.89 (3.17)
	$f = 3$	9.07 (3.22)	8.51 (3.47)	8.52 (3.67)
	$f = 6$	8.33 (4.56)	8.15 (4.03)	8.33 (4.63)
$y = 2$	$f = 0$	10.44 (2.25)	11.48 (3.79)	10.74 (2.83)
	$f = 3$	11.04 (3.45)	11.78 (3.69)	11.19 (3.62)
	$f = 6$	12.67 (4.82)	14.00 (4.58)	12.82 (4.81)

Notes: means, standard deviations in parentheses.

4. Discussion

Our experiment compares two audit-based deterrence mechanisms that collect fines from those found non-compliant. The mechanisms differ in the way fines are redistributed to individuals who were either not audited or audited and found to be compliant. The first, as is the case in most extant tax systems, does not discriminate between the un-audited and those found compliant. The second targets the redistribution in favor of those found compliant.

We find that targeting increases compliance when paying taxes generates a social return. One possible explanation might be that the mere fact of rewarding compliance may signal its desirability and increase the rate of compliance. To control for this possibility we also conducted a treatment where those found compliant were given "symbolic" rewards, i.e. rewards of negligible material value. Here we found no increase in compliance relative to our untargeted treatment. Thus we conclude that it is not the mere assigning of rewards that improves compliance.

We also find that compliance increases with the size of the fine, and when the social return associated with compliance is positive. These findings are in line with previous findings in the literature and demonstrate that compliance rates shift predictably with changes in the material costs and benefits of evasion/compliance. However, material incentives are not the whole story. In all of the treatments and conditions we ran, an individual maximizes expected earnings by evading. Thus the positive compliance rates demonstrate that other factors are at play. One

[20] Using rank sum tests, for $y = 0$, p-value= 0.740, 0.885 and 0.880 for Targeted vs. Untargeted, Targeted vs. Symbolic, and Symbolic vs. Untargeted, respectively; for $y = 2$ p-value= 0.192, 0.192 and 0.880 for Targeted vs. Untargeted, Targeted vs. Symbolic, and Symbolic vs. Untargeted, respectively.

important finding demonstrating this is the strong association between the likelihood that a subject complies and that subject's beliefs about others' compliance: when subjects expect others to comply, they are more likely to do so themselves. Explaining this in terms of material incentives is all the more difficult since, from a purely selfish perspective, the incentive to evade is increased when others comply.[21]

However, the finding sits well with previous findings from survey data that find a positive relation between intrinsic motivation to pay taxes and beliefs about the tax compliance of others (e.g. Frey and Torgler, 2007). The observed positive relation could reflect an effect of beliefs on actions whereby people are more willing to comply when they believe others do so, akin to "conditional cooperation" in public good experiments where people are more willing to contribute when others do so (e.g., Keser and van Winden, 2000; Croson et al., 2005; Fischbacher and Gächter, 2010). Of course, there are other possibilities and further research is needed to identify channels and directions of causation.

5. Conclusions

The use of reward mechanisms to promote compliance is theoretically established but rarely applied. That rewards are rarely used in practice obviously limits the opportunity to gauge the effectiveness of reward-based audit mechanisms using field data. However, laboratory experiments do offer an opportunity to test-bed such mechanisms. Our experiment serves such a purpose. We find that targeting increases compliance when paying taxes generates a social return. The effect cannot be entirely explained by the symbolic value of targeted rewards, nor by the pure material incentives that are at play across the different mechanisms. Compliance is strongly supported by the belief that more other people also comply.

The clear policy message of our paper is that existing tax mechanisms have room for improvement by rewarding those audited and found compliant. This would not be a financial burden on the tax authorities as it would be paid by the fines on those audited and found non-compliant, and it could not just be replaced by employing symbolic rewards. One possible objection to the mechanism is that people would *want* to be audited; it is of course unavoidable that any tax collection technology is inefficient and distortions already exist in the current tax system, which is rife with tax avoidance; in practice, only those who pay their taxes would want

[21] Moreover note that targeted rewards increase compliance also when $f = 0$ (see Figure 2), which suggests that the effectiveness of the mechanism cannot be entirely explained by material incentives considerations.

to be audited under the proposed mechanism, and so this distortion would only work on the assumption that taxes are being paid. Arguably, as targeting increases compliance in our experiment only when paying taxes generates a social return, our mechanism would be less effective in tax systems perceived as inefficient or corrupt, as the social benefit of taxes would be blurred. While extremely efficient tax systems make the social benefits more visible (at the cost of reducing the incentives), systems perceived as inefficient generate more incentives (making the social benefits of taxes less salient). Both limitations (the salience of social benefits and the size of incentives) operate in different directions, and more research is needed in this area.

As in other experimental settings, the small group size could also be considered a limitation of our study. We view our experiment as a small-scale study showing that self-funded rewards may increase compliance at no extra budget cost. Implementing our mechanism at a much larger scale (e.g. a national income tax system) would certainly be a challenge, maybe because the incentives might not be large enough.[22] Nevertheless, two recent field studies, Naritomi (2019) and Fabbri et al (2019), show that rewards may significantly increase compliance in very different large scale settings (the state of Sao Paolo in Brazil and the city of Rimini in Italy, respectively).

Another possible objection to our reward mechanism is that, while the experimental mechanism has 100% effectiveness at detecting cheating, in practice tax audit technologies are prone to errors (some cheaters may survive audits and then be rewarded). It is unclear whether, in the presence of detection errors, the mechanism would be as effective as in our experimental setting, and this could also be an avenue for further research. Finally, our mechanism goes against the presumption of innocence by rewarding those that are audited and found complying, but not those who are not audited even if complying. Research on the fairness perceptions of this policy would be an obvious next step.[23] Yet, those who are not audited are currently advantaged relative to those who are audited because of avoiding the real hassle and costs (psychological and otherwise) that often come with being audited, and so the reward could be seen as a fair compensation that is currently lacking.

[22] Kleven et al. (2011) audited a random sample of approximately 20.000 taxpayers in Denmark, used a substantial share of the country total audit resources. The share of evaders was only around 10%.

[23] Kirchler et al (2003) is a natural reference, surveying small business owners and fiscal officers to find how perceptions of fairness differ and how tax evasion and tax avoidance are perceived as different phenomena.

References

Alm, J. 2012. "Measuring, Explaining, and Controlling Tax Evasion: Lessons from Theory, Experiments, and Field Studies." *International Tax and Public Finance* 19(1): 54–77.

Alm, J. 2019. "What Motivates Tax Compliance?" *Journal of Economic Surveys,* 33 (2), 353-388.

Alm, J., B.R. Jackson, and M. McKee. 1992a. "Deterrence and Beyond: Toward a Kinder, Gentler IRS." In *Why People Pay Taxes*, ed. J. Slemrod. Ann Arbor: University of Michigan Press, 311–29.

Alm, J., G.H. Mcclelland, and W.D. Schulze. 1992b. "Why Do People Pay Taxes." *Journal of Public Economics* 48(1): 21–38.

Andreoni, J. 1995. "Cooperation in Public-Goods Experiments - Kindness or Confusion?" *American Economic Review* 85(4): 891–904.

Andreoni, J., B. Erard, and J. Feinstein. 1998. "Tax Compliance." *Journal of Economic Literature* 36(2): 818–60.

Batrancea, L., Nichita, A., Olsen, J., Kogler, C., Kirchler, E., Hoelzl, E., Weiss, A., Torgler, B., Fooken, J., Fuller, J. and Schaffner, M., 2019. Trust and power as determinants of tax compliance across 44 nations. *Journal of Economic Psychology*, *74*, p.102191.

Bazart, C., and M. Pickhardt. 2011. "Fighting Income Tax Evasion with Positive Rewards." *Public Finance Review* 39(1): 124–49.

Border, K.C., and J. Sobel. 1987. "Samurai Accountant: A Theory of Auditing and Plunder." *The Review of Economic Studies* 54(4): 525–40.

Carrillo, P., E. Castro, and C. Scartascini. 2016. "Do Rewards Work to Maintain and Increase Tax Compliance? Evidence from the Randomization of Public Goods", Inter American Development Bank, mimeo.

Carter, J.R., and M.D. Irons. 1991. "Are Economists Different, and If So, Why?" *Journal of Economic Perspectives* 5(2): 171–77.

Casal, S., C. Kogler, L. Mittone, and E. Kirchler. 2016. "Tax Compliance Depends on Voice of Taxpayers." *Journal of Economic Psychology* 56: 141–50.

Chaudhuri, A. 2011. "Sustaining Cooperation in Laboratory Public Goods Experiments: A Selective Survey of the Literature." *Experimental Economics* 14(1): 47–83.

Choo, C.Y.L., M.A. Fonseca, and G.D. Myles. 2016. "Do Students Behave like Real Taxpayers in the Lab? Evidence from a Real Effort Tax Compliance Experiment." *Journal of Economic Behavior & Organization* 124: 102–14.

Croson, R., E. Fatás, and T. Neugebauer. 2005. "Reciprocity, Matching and Conditional Cooperation in Two Public Goods Games." *Economics Letters* 87: 95–101.

Croson, R., E. Fatas, T. Neugebauer, and A.J. Morales. 2015. "Excludability: A Laboratory Study on Forced Ranking in Team Production." *Journal of Economic Behavior & Organization* 124: 13–26.

Duffy, J., and A. Matros. 2014. "On the Use of Fines and Lottery Prizes to Increase Voter Turnout'." *Economics Bulletin* 34(2): 966–75.

Dwenger, N., H. Kleven, I. Rasul, and J. Rincke. 2016. "Extrinsic and Intrinsic Motivations for Tax Compliance: Evidence From a Field Experiment in Germany." *American Economic Journal: Economic Policy* 8(3): 203–32.

Engel, C., Mittone, L. and Morreale, A., 2020. Tax morale and fairness in conflict an experiment. *Journal of Economic Psychology*, *81*, 102314

Fabbri, M. 2015. "Shaping Tax Norms through Lotteries." *International Review of Law and Economics* 44: 8–15.

Fabbri, M., Nicola Barbieri, P., and Bigoni, M. 2019. "Ride Your Luck! A Field Experiment on Lottery-based Incentives for Compliance". *Management Science*, *65*(9), 4336-4348.

Falkinger, J., Fehr, E., Gächter, S., and Winter-Ember, R. 2000. "A simple mechanism for the efficient provision of public goods: Experimental evidence". *American Economic Review*, *90*(1), 247-264

Fatas, E., Morales, A. J., and Ubeda, P. 2010. "Blind Justice: An experimental analysis of random punishment in team production". *Journal of Economic Psychology*, *31*(3), 358-373

Fatas, E., Jimenez-Jimenez, F., and Morales, A. J. 2010. "Blind fines in cooperatives". *Applied Economic Perspectives and Policy*, *32*(4), 564-587.

Fischbacher, U. 2007. "Z-Tree: Zurich Toolbox for Ready-Made Economic Experiments." *Experimental Economics* 10(2): 171–78.

Fischbacher, U., and S. Gächter. 2010. "Social Preferences, Beliefs, and the Dynamics of Free Riding in Public Good Experiments." *American Economic Review* 100(1): 541–556.

Fochmann, M., and E.B. Kroll. 2016. "The Effects of Rewards on Tax Compliance Decisions." *Journal of Economic Psychology* 52: 38–55.

Frey, B.S., and B. Torgler. 2007. "Tax Morale and Conditional Cooperation." *Journal of Comparative Economics* 35(1): 136–59.

Gobena, L.B. and Van Dijke, M., 2017. Fear and caring: Procedural justice, trust, and collective identification as antecedents of voluntary tax compliance. *Journal of Economic Psychology*, *62*, pp.1-16

Greiner, B. 2015. "Subject Pool Recruitment Procedures: Organizing Experiments with ORSEE." *Journal of the Economic Science Association* 1(1): 114–25.

Hofmann, E., M. Voracek, C. Bock, and E. Kirchler. 2017. "Tax Compliance across Sociodemographic Categories: Meta-Analyses of Survey Studies in 111 Countries." *Journal of Economic Psychology* 62: 63–71.

Houser, D., and R. Kurzban. 2002. "Revisiting Kindness and Confusion in Public Goods Experiments." *American Economic Review* 92(4): 1062–69.

Karakostas, A., and D.J. Zizzo. 2016. "Compliance and the Power of Authority." *Journal of Economic Behavior and Organization* 124: 67–80.

Kastlunger, B., S. Muehlbacher, E. Kirchler, and L. Mittone. 2011. "What Goes Around Comes Around? Experimental Evidence of the Effect of Rewards on Tax Compliance." *Public Finance Review* 39(1): 150–67.

Kastlunger, B., Dressler, S.G., Kirchler, E., Mittone, L. and Voracek, M., 2010. Sex differences in tax compliance: Differentiating between demographic sex, gender-role orientation, and prenatal masculinization (2D: 4D). *Journal of economic psychology*, *31*(4), pp.542-552.

Kastlunger, B., Kirchler, E., Mittone, L. and Pitters, J., 2009. Sequences of audits, tax compliance, and taxpaying strategies. *Journal of Economic Psychology*, *30*(3), pp.405-418.

Kastlunger, B., Lozza, E., Kirchler, E. and Schabmann, A., 2013. Powerful authorities and trusting citizens: The Slippery Slope Framework and tax compliance in Italy. *Journal of Economic Psychology*, *34*, pp.36-45.

Keser, C., and F. van Winden. 2000. "Conditional Cooperation and Voluntary Contributions to Public Goods." *Scandinavian Journal of Economics* 102(1): 23–39.

Kimbrough, E.O., and A. Vostroknutov. 2016. "Norms Make Preferences Social." *Journal of the European Economic Association* 14: 608–38.

Kirchler, E. and Maciejovsky, B., 2001. Tax compliance within the context of gain and loss situations, expected and current asset position, and profession. *Journal of Economic Psychology*, *22*(2), pp.173-194.

Kirchler, E. and Wahl, I., 2010. Tax compliance inventory TAX-I: Designing an inventory for surveys of tax compliance. *Journal of Economic Psychology*, *31*(3), pp.331-346.
Kirchler, E., Hoelzl, E. and Wahl, I., 2008. Enforced versus voluntary tax compliance: The "slippery slope" framework. *Journal of Economic Psychology*, *29*(2), pp.210-225.
Kleven, H. J., Knudsen, M. B., Kreiner, C. T., Pedersen, S., & Saez, E. 2011. "Unwilling or unable to cheat? Evidence from a tax audit experiment in Denmark". *Econometrica*, *79*(3), 651-692.
Kirchler, E., Maciejovsky, B. and Schneider, F., 2003. Everyday representations of tax avoidance, tax evasion, and tax flight: Do legal differences matter?. *Journal of Economic Psychology*, *24*(4), pp.535-553.
Koessler, A.C., B. Torgler, L.P. Feld, and B.S. Frey. 2018. "Commitment to Pay Taxes: Results from Field and Laboratory Experiments." *European Economic Review*, 115, 78-98.
Kogler, C., Batrancea, L., Nichita, A., Pantya, J., Belianin, A. and Kirchler, E., 2013. Trust and power as determinants of tax compliance: Testing the assumptions of the slippery slope framework in Austria, Hungary, Romania and Russia. *Journal of Economic Psychology*, *34*, pp.169-180.
Ledyard, J.O. 1995. "Public Goods: A Survey of Experimental Research." In *The Handbook of Experimental Economics*, eds. A.E. Roth and J.H. Kagel. Princeton: Princeton University Press, 111–81.
Marwell, G., and R.E. Ames. 1981. "Economists Free Ride, Does Anyone Else? Experiments on the Provision of Public Goods, IV." *Journal of Public Economics* 15(3): 295–310.
Mascagni, G. 2018. "From the Lab to the Field: A Review of Tax Experiments." *Journal of Economic Surveys* 32(2): 273–301.
Mittone, L., Panebianco, F. and Santoro, A., 2017. The bomb-crater effect of tax audits: Beyond the misperception of chance. *Journal of Economic Psychology*, *61*, pp.225-243.
Mookherjee, D., and I. Png. 1989. "Optimal Auditing, Insurance, and Redistribution." *The Quarterly Journal of Economics* 104(2): 399–415.
Naritomi, J. 2019. "Consumers as tax auditors". *American Economic Review*, *109*(9), 3031-72
Sausgruber, R., and Tyran, J. R. 2007. "Pure redistribution and the provision of public goods". *Economics Letters*, *95*(3), 334-338
Stöber, J. 2001. "The Social Desirability Scale-17 (SDS-17): Convergent Validity, Discriminant Validity, and Relationship with Age." *European Journal of Psychological Assessment* 17(3): 222–32.
Wan, J. 2010. "The Incentive to Declare Taxes and Tax Revenue: The Lottery Receipt Experiment in China." *Review of Development Economics* 14(3): 611–24.
Zizzo, D.J. 2010. "Experimenter Demand Effects in Economic Experiments." *Experimental Economics* 13(1): 75–98.