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The experimental tax and regulatory compliance literature has shown the effectiveness of competitive audit selection mechanisms (ASMs) based on declarations and a signal of the taxpayers' actual income. However, collecting information about actual income prior to audit selection is costly. In this article, we test the effectiveness of an endogenous ASM based solely on declared income. We show theoretically and in a laboratory experiment that this new endogenous ASM significantly increases compliance in comparison with an ASM where all taxpayers face audit with equal probability. However, a further consequence of conditioning solely on declared income is that poorer taxpayers are audited more frequently, reducing the effectiveness of this ASM in generating revenue and reducing inequality. We further compare the new mechanism with an ASM that also uses a noisy signal of actual income and show that it is a significant improvement over the other two ASMs in terms of compliance, revenue, and inequality. Our results suggest that ASMs that condition only on reported income can increase compliance but should be implemented with caution, and investing in acquiring information before audit selection can have substantial benefits.

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A Competitive Audit Selection Mechanism with Incomplete Information*

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December 17, 2019

Abstract

The experimental tax and regulatory compliance literature has shown the effectiveness of competitive audit selection mechanisms (ASMs) based on declarations and a signal of the taxpayers' actual income. However, collecting information about actual income prior to audit selection is costly. In this article, we test the effectiveness of an endogenous ASM based solely on declared income. We show theoretically and in a laboratory experiment that this new endogenous ASM significantly increases compliance in comparison with an ASM where all taxpayers face audit with equal probability. However, a further consequence of conditioning solely on declared income is that poorer taxpayers are audited more frequently, reducing the effectiveness of this ASM in generating revenue and reducing inequality. We further compare the new mechanism with an ASM that also uses a noisy signal of actual income and show that it is a significant improvement over the other two ASMs in terms of compliance, revenue, and inequality. Our results suggest that ASMs that condition only on reported income can increase compliance but should be implemented with caution, and investing in acquiring information before audit selection can have substantial benefits.

Keywords: Tax compliance, Endogenous audit, Heterogeneous income.

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1 Introduction

Audits are the primary means tax and regulatory agencies have at their disposal to increase compliance. A higher frequency of audits has been shown to increase compliance in laboratory (Alm, 2012; Alm et al., 1992) as well as in field settings (Slemrod et al., 2001; Kleven et al., 2011; Fellner et al., 2013; Meiselman, 2018; Dwenger et al., 2016). However, increasing the frequency of audits is costly. Competitive audit selection mechanisms are powerful tools with which to increase tax and regulatory compliance without increasing the number of audits (Gilpatric et al., 2011; Cason et al., 2016).

The available competitive audit selection mechanisms were proposed mostly in the context of environmental regulations, and are based on the assumption that the enforcement authority has noisy, but unbiased, information concerning the level of non-compliance before selecting who to audit. For instance, in the case of emissions regulations, the enforcement authority has available an unbiased estimate of each plant's actual emissions and observes the declared emissions. In the context of tax compliance, the tax authority may also make an initial rough appraisal of declarations, perhaps by comparing with other declarations or using knowledge of general economic conditions. By taking advantage of this information, the enforcement authority can rank the regulated agents according to their estimated levels of non-compliance and then select them for audits according to this rank. However, acquiring such estimates will inevitably be costly, and it is not clear that these costs outweigh the benefits.

This paper proposes a competitive audit selection mechanism which is based solely on *declared* output or income. We show, both theoretically and with a laboratory experiment, that this mechanism increases compliance of taxpayers with heterogeneous incomes in comparison with the baseline case where all taxpayers are audited with equal probability. In addition, we compare the performance of the new mechanism with one where the tax authority can makes use of a noisy signal of true income in order to evaluate the benefits of acquiring such a signal.

This paper addresses the problems created by the information requirements of the audit selection mechanisms thus far proposed in the literature: Gilpatric et al. (2011) assume that the auditors possess noisy, but unbiased, information about taxpayers' true incomes; Colson and Menapace (2012) assume the existence of informative output measures for a subgroup of firms; and Alm and McKee (2004) suppose that the tax office can divide the taxpayers into subgroups having the same income. This last supposition might not be possible because true incomes can be observed only when taxpayers are audited, which means that the tax authority might have the income data for only a few taxpayers in some sectors. The tax office may also face additional problems, even in sectors with a high number of audits. For example, the incomes of some taxpayers may vary significantly between years, or the incomes of individual taxpayers may depend on idiosyncratic factors, such as personal contacts or luck, which are not known to the tax authority. As a result, at least in some sectors, auditors do not have unbiased estimates of true incomes at the individual level readily to hand, and incomes will be heterogeneous in any group that the tax office is able to select. Given these two problems, the ranking of taxpayers reflects not only their undeclared incomes but also the heterogeneity of their actual incomes.

Competitive audit mechanisms based on declared, rather than undeclared, output were studied theoretically by Bayer and Cowell (2009) and Oestreich (2015, 2017). They show that these competitive audit mechanisms lead to higher tax compliance. However, output in these models is endogenously chosen by the taxpayers, and it is observed by other taxpayers (alternatively: there is a perfect information among taxpayers). Moreover, the solution is based on the symmetric Nash equilibrium concept, which means that output is homogeneous on the equilibrium path. Our theoretical predictions follow a model which is similar to the declaration stage in the model presented by Bayer and Cowell (2009): our model differs in that output is exogenous and heterogeneous by assumption. We examine the properties of competitive audit selection mechanisms in a situation in which the taxpayers have unobserved heterogeneous income or, more generally, when the subjects are heterogeneous in the variable they are supposed to report. The theoretical solution shows that the competitive audit selection mechanism, in which the audit probability depends only on the declared incomes of taxpayers, leads to higher declared incomes than random audit selections, where all taxpayers are selected with the same exogenously given probability.

Taking as given that acquiring an initial signal of true income is costly, and that such information increases compliance, it is natural to ask how much a tax authority should be willing to pay to obtain such a signal, assuming one is available. We therefore also consider a mechanism that takes advantage of an estimate of true income, which we indeed find to further increase compliance. To establish the maximum cost at which this pre-audit information should be sought, we need to calculate total revenue, which depends not only on tax receipts from declared income, but also on fines from audits. This points to a stark difference between the mechanisms: assuming equilibrium strategies, when audit probabilities depend only on declared incomes, poorer taxpayers are more likely to be audited than richer taxpayers, while the opposite is true when an estimate of income is incorporated. However, our theory shows that any change in fines due to auditing is outweighed by increased tax revenues, and predicts that both competitive audit selection mechanisms (ASMs) increase total revenue, and more so when information regarding true income is available.

Finally we also consider the theoretical impact of ASMs on inequality. Clearly inequality is reduced when a signal of income is used, because of greater compliance and the increasing relationship between income and probability of audit. However, when only declared income is used, the impact of greater compliance is muted by more frequent auditing of the poor. Again, despite this countervailing force, inequality is predicted to be reduced under both competitive ASMs, with a greater reduction when a signal about income is utilised.

These predictions are tested using an economic experiment. We propose a design in which all taxpayers receive income that is drawn from a uniform distribution. Their task is to choose the declared income which is then taxed at a fixed rate. They may be selected for audit with a particular audit probability. Subjects who are selected for audit and declare less than their income pay a penalty. The experiment has three treatments that differ in the way the audit probability is determined: an audit selection mechanism where all taxpayers are audited with the same exogenously given probability (*random*); a competitive audit selection mechanism based on declared income (*no-signal*); and a competitive audit selection mechanism which makes use of an estimate of true income (*signal*). In the treatments with a competitive audit selection, the subjects are divided into groups of five taxpayers. In *no-signal*, the audit probability decreases with the difference between each subject's declared income and the average declared income of the other four subjects in their group, whereas in *signal* it increases with the difference between the subject's undeclared income and the group average.

In line with the theoretical predictions we find that, in comparison to *random*, compliance is significantly improved on average by using the competitive audit selection mechanism based solely on declared income, and more so by additionally utilizing information about true income. We also find the predicted relationship between income and audit probability with poorer taxpayers audited more often in *no-signal* and less often in *signal*. So while revenue is increased and inequality decreased in *signal*, the positive impact in these respects in *no-signal* are small and statistically insignificant.

The rest of the paper is structured as follows. Section 2 describes the experimental design and hypotheses. Sections 3.1 and 3 present the data and the results of the experiment. Finally, section 4 concludes.

2 Experimental design

2.1 Treatments and predictions

In this section we describe the experimental task and treatments, and state our hypotheses. The more general theoretical framework on which the experiment is based, and the proofs underlying our hypotheses, are contained in Appendix A.

At the beginning of each round, each taxpayer i receives income I_i, which is drawn from a uniform distribution between 0 and 200 CZK.[1] The task of each taxpayer is to choose a declared income $R_i \in [0, I_i]$. This declared income is taxed at a rate $\tau = 0.6$. Taxpayers who are selected for audit and declare less than their income pay a penalty equal to their undeclared income $I_i - R_i$. Thus, a taxpayer receives $0.4R_i$ if audited, and $0.4R_i + (I_i - R_i) = I_i - 0.6R_i$ if not selected for audit.

The experiment consists of three treatments which differ in the way that audit probabilities are calculated. In the treatment with the random audit selection mechanism (*random*), the audit probability of each taxpayer i is $\pi_i = 0.4$. The audit probabilities for the remaining two treatments have a form relative difference contest success function (Beviá and Corchón, 2015). They resemble closely the generalized audit selection rule proposed by Gilpatric et al. (2011), and are chosen such that the expected number of audits is the same in all three treatments.

In the treatment with the competitive audit selection mechanism with no signal regarding taxable income (*no-signal*), taxpayers interact in groups of five. Each taxpayer's audit probability is independent of actual income, but is increasing in the difference between their declared income and the average declared income in their group:

$$\pi_i = 0.4 - 0.004 \left(R_i - \frac{\sum R_{-i}}{N-1} \right),$$

where R_{-i} stands for the incomes of the four remaining members of the group.

The treatment with the competitive audit selection mechanism where the tax authority has access to a noisy estimate of actual taxable income (*signal*) is similar to *no-signal*, with the only exception being that the audit selection mechanism is based on undeclared rather than declared income. In particular, the audit probability is

$$\pi_i = p + 0.004 \left(Z_i - \frac{\sum Z_{-i}}{N-1} \right),$$

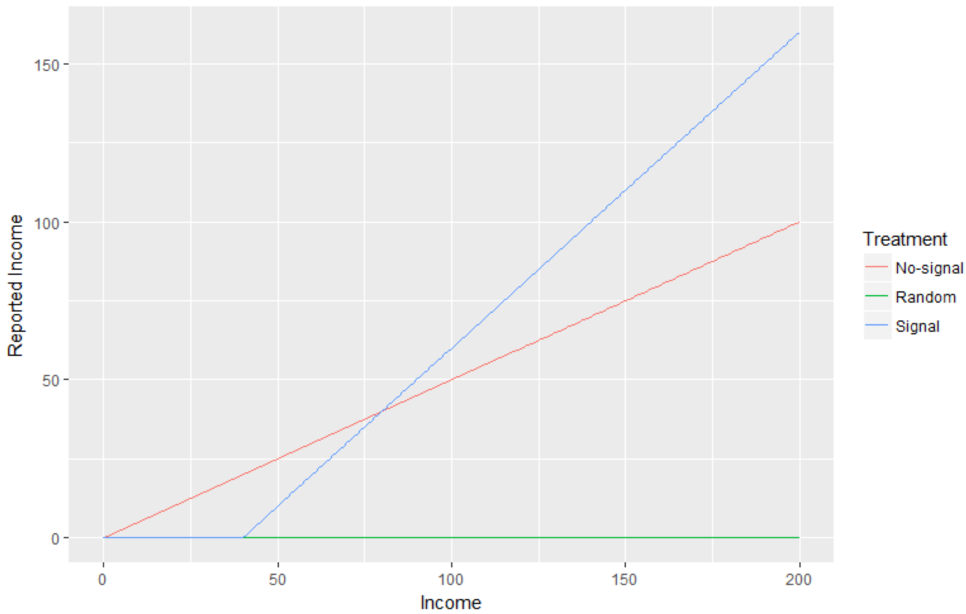
[1] This amount was equal to approximately 7 Euro at the time of the experiment, a little less than twice the average student wage.

where $Z_i = I_i - R_i$ is the undeclared income. This can be considered a reduced form for a situation where the tax authority has only an estimate of undeclared income, and always audits a fraction of those with the highest undeclared incomes, with the randomness of the selection function standing in for the noisiness of the signal. The reason we chose not to model the noisy signal explicitly is to keep the two competitive audit treatments (especially the experimental instructions) as close as possible, while still capturing the essential features of the two information environments.

Using the results from Appendix A, we can now derive equilibrium strategies for the parameters used in the experiment. declared income in *random* is zero. In *no-signal*, declared income should be one half of the actual income, $R_i = I_i/2$. In *signal*, declared income is zero when the actual income is below 40 and each additional income above 40 is fully declared, i.e. $R_i = \max\{0, I_i - 40\}$. The average declared incomes in the random, no-signal and signal treatments are 0, 50 and 128, respectively. This calculation gives us our first hypothesis:

Hypothesis 1: Average declared income in *signal* is higher than in *no-signal*. The average declared income in *no-signal* is higher than in *random*.

Figure 1: Equilibrium strategies based on experimental parameters.

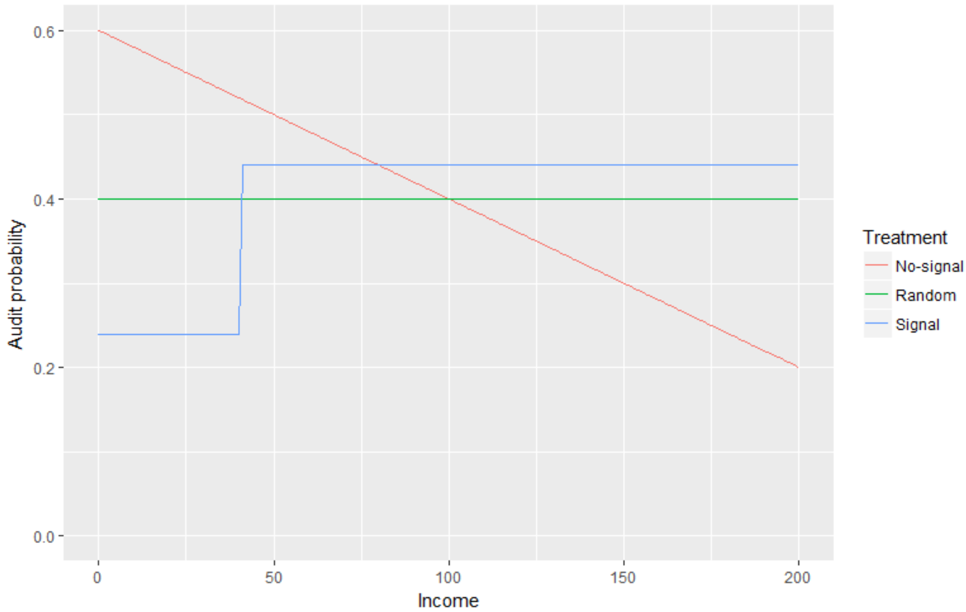


However, the comparison of the average declared incomes does not tell the whole story. Figure 1 shows how the theoretically predicted declared income depends on actual taxable income. It demonstrates that the effects on individuals of different audit selection mechanisms depend on the levels of taxpayers' actual incomes. Taxpayers with incomes lower than 80 CZK are expected to comply more fully in *no-signal*. On the other hand, taxpayers with higher income should declare more in *signal*. Following this prediction about declared income, we can formulate our second hypothesis:

Hypothesis 2: For low income levels, declared income will be higher in *no-signal* than *signal*. For medium- and high-income levels, declared income will be lower in *no-signal* than *signal*.

Figure 2 sheds further light on the difference between low- and high-income individuals. The figure depicts the relationship between equilibrium audit probability[2] and income level. In *no-signal*, the lack of information about each individual's income mean that high-income individuals are audited with lower probability. In *signal*, the audit selection mechanism is based on undeclared incomes, which results in constant audit probabilities. Only the taxpayers with incomes below 40 CZK are audited with lower probability, which reflects that they are constrained by the lowest possible declared income of zero. The relationship shown in figure 2 gives us our third hypothesis:

Figure 2: The figure shows the relationship between expected audit probability and income. The red line is for the case when the audit is based on undeclared income. The blue line is for the case when the audit is based on declared income.



Hypothesis 3: The probability of being audited is decreasing in income in *no-signal* and weakly increasing in *signal*.

Total revenue comes from two sources: tax declarations and fines from failed audits. Income from declarations is purely a function of declared income, so if Hypothesis 1 is

[2]Recall that the audit probability depends on the taxpayer's compliance behaviour.

correct, this will be highest in *signal*, and lowest in *random* on average. Income from fines on the other hand, depends not only on the proportion of income declared by individuals who are audited, but also on the probability with which each taxpayer is audited. According to Hypothesis 3, in *signal* richer people should be audited more frequently than in *random*, mitigating the reduction in audit revenue due to lower tax declarations, whereas in *no-signal*, poorer people are being audited, exacerbating the reduction in audit revenue. Nevertheless, in theory, the net effect of both competitive ASMs is to increase total revenue. Table 1 shows the total revenue by treatment, assuming the parameters implemented in our experiment. These are reflected in the following hypothesis:

Hypothesis 4: Total revenue is highest in *signal*, and lowest in *random*.

With respect to inequality in net income, there are again countervailing effects. In *signal*, inequality is reduced by both the steeper relationship between income and declared income, and the fact that richer people are audited more frequently. In contrast, in *no-signal*, the reduction in inequality relative to *random* due to higher declarations is counterbalanced by the fact that poorer people are more often audited. The predicted variances in net income given our experimental parameters are shown in Table 1,[3] and give rise to our final hypothesis:

Hypothesis 5: The variance in post-tax income is highest in *signal*, and lowest in *random*.

Table 1: Predicted revenue and inequality

	Signal	No-signal	Random
Expected revenue	53.17	46.63	40
Standard deviation of final payoff	24.1	35.1	942.5

2.2 Procedures

The experiment was conducted at MUEEL in Brno, Czech Republic, in 2017 and 2018. The subjects were mostly students and recruited through hroot (Bock et al., 2014). The experiment was programmed in zTree (Fischbacher, 2007). We used neutral instructions, i.e. the tax motivation of the game is not clear from the instructions (the instructions were in Czech language - an English translation is provided in Appendix B). We ran 11 sessions

[3]Note that the predicted variance in *random* is exceptionally high because equilibrium disclosed income is zero and so ex-post income is either zero or equal to ex-ante income, depending on whether a given taxpayer was audited or not.

using a between-subjects design. In particular, we ran three sessions for *random* for which each subject was considered to be one independent observation, four sessions for *no-signal* where groups of five were considered to be one independent observation and four sessions for *signal* where again each group of five was considered an independent observation. The total number of participants was 200, with no less than 15 participants in each session. The sessions consisted of 30 rounds with partner-matching, and lasted almost 90 minutes. The subjects received payments based on five randomly selected rounds. The mean payoff was 240 CZK (approx. 9 EUR, a little more than twice the average student wage).

At the beginning of each experimental session, an experimenter read the instructions aloud, with the subjects (taxpayers) following along with their copy. Subjects were asked to answer control questions in order to reinforce comprehension of the instructions before the experiment. To avoid the risk of anchoring, the questions did not include any particular numbers. All numerical inputs were entered by subjects themselves.

3 Results

3.1 Data

The dataset consists of observation from the 200 participants, each of them playing 30 periods, individually or as part of a group for 6,000 observations in total. We filter out the observations in which the income is $I = 0$ because, under those circumstances, the subjects had no opportunity to evade taxes. As it is standard in similar experimental literature founding their predictions on equilibrium models, we do not use data from the early rounds in the analysis to allow subjects to learn (e.g. Gilpatric et al., 2011). In particular, we use the data only from the last 15 rounds. As a robustness check, all results were estimated using all 30 periods. The results remain the same, or at least very similar, in terms of statistical as well as economic significance.

Table 2 displays the descriptive statistics for the selected variables for the three treatments. The table includes choice and outcome variables as well as socio-demographic variables. There are fewer subjects in *random* since, in that case, each subject is considered as an independent observation, whereas a group of five constitute an independent observation in the other two treatments. Although our sample is not balanced in terms of gender, this should not bias the results since we control for personal characteristics in the regressions.[4]

[4]As a further check, we ran regressions of declared income on a gender dummy for the combined data, and separately by treatment: the point estimate in the combined data indicated that females declared 1.5 less than males; in no case did we find a statistically significant relationship.

Approximately one half of the subjects were students of economics or business. Some of our subjects had previously participated in other economics experiments, but they had not participated in a similar tax compliance experiment.

Table 2: Descriptive statistics

	signal	*no-signal*	*random*
Subjects	80	75	45
Groups (Independent observations)	16	15	45
Income	97.6	101.2	100.7
Declared income	70.9	63.5	53.0
Expected total revenue	55.4	52.5	50.9
Standard deviation of final payoff	28.4	38.2	41.9
Female	0.38	0.49	0.6
Age	22.2	23.1	21.5
Students of economics or business	0.48	0.45	0.42

3.2 Declared income

As can be seen in Table 2, and in line with Hypothesis 1, average declared income was 70.9, 63.5, and 53.0 in the *signal*, *no-signal*, and *random* treatments, respectively. Column 1 in Table 3 shows the result of linear regressions of declared income on treatment dummies (*no-signal* is the baseline), which shows that the difference between *random* and both competitive ASMs are statistically significant ($p < 0.01$ for *signal*, and $p = 0.033$ for *no-signal*); the difference between the two competitive ASMs is weakly significant ($p = 0.089$).[5] As shown in the second column, controlling for income increases the average difference between *signal* and *no-signal* to almost 10, and raises the level of statistical significance ($p = 0.010$).

Hypothesis 2 relates to the different predicted relationships between income and declared income across the treatments. Given the non-linear nature of our prediction for *signal* (see Figure 1), we begin by estimating nonparametric kernel regressions of declared income on income separately for each of the three treatments. As can be seen in Figure 3, and as hypothesized, the slope of the estimated relationship is steeper for *signal* than *no-signal*, with declared incomes lower in the former treatment for low incomes, and higher for high incomes. Contrary to the theoretical prediction, declared income rises with income in *random*. To keep things parsimonious, and as our nonparametric regressions suggest that all three

[5] We cluster standard errors at the group level in all regressions related to individual behaviour to account for non-independence.

relationships are close to linear, we estimate a linear regression allowing for different slopes in the different treatments to assess the statistical significance of these observed differences. This is reported in column 3 of Table 3. All differences of slopes and intercepts between all three treatments are highly significant ($p < 0.01$), with the exception of the slopes of *random* an *no-signal*, which do not differ. Controlling for gender, age, and field of study does not alter any of the conclusions in this section (column 4).

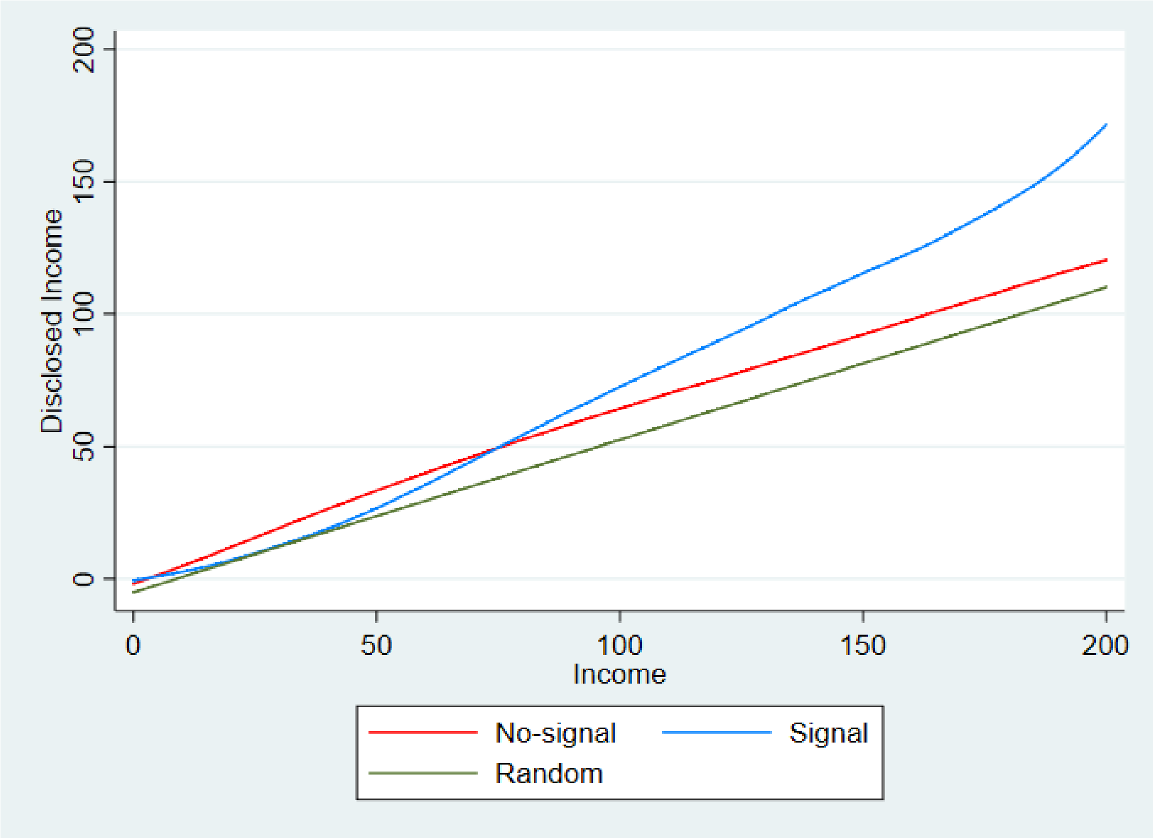
Table 3: Declared income

	(1)	(2)	(3)	(4)
signal	7.391*	9.880**	-15.65***	-16.36***
	(4.285)	(3.763)	(1.728)	(1.742)
random	-10.58**	-10.23**	-7.987**	-9.435***
	(4.860)	(4.581)	(3.268)	(3.308)
income		0.697***	0.598***	0.597***
		(0.0266)	(0.0294)	(0.0294)
signal#income			0.258***	0.260***
			(0.0379)	(0.0381)
random#income			-0.0228	-0.0231
			(0.0596)	(0.0591)
female				1.767
				(2.547)
age				-0.814*
				(0.487)
economics student				-1.110
				(2.311)
Constant	63.49***	-7.071**	2.918**	21.48*
	(2.906)	(3.015)	(1.429)	(11.74)
Observations	3,000	3,000	3,000	3,000
R^2	0.018	0.656	0.678	0.680

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Figure 3: Relationship between declared income and actual income (nonparametric kernel regression - optimal bandwidth selected using improved AIC).

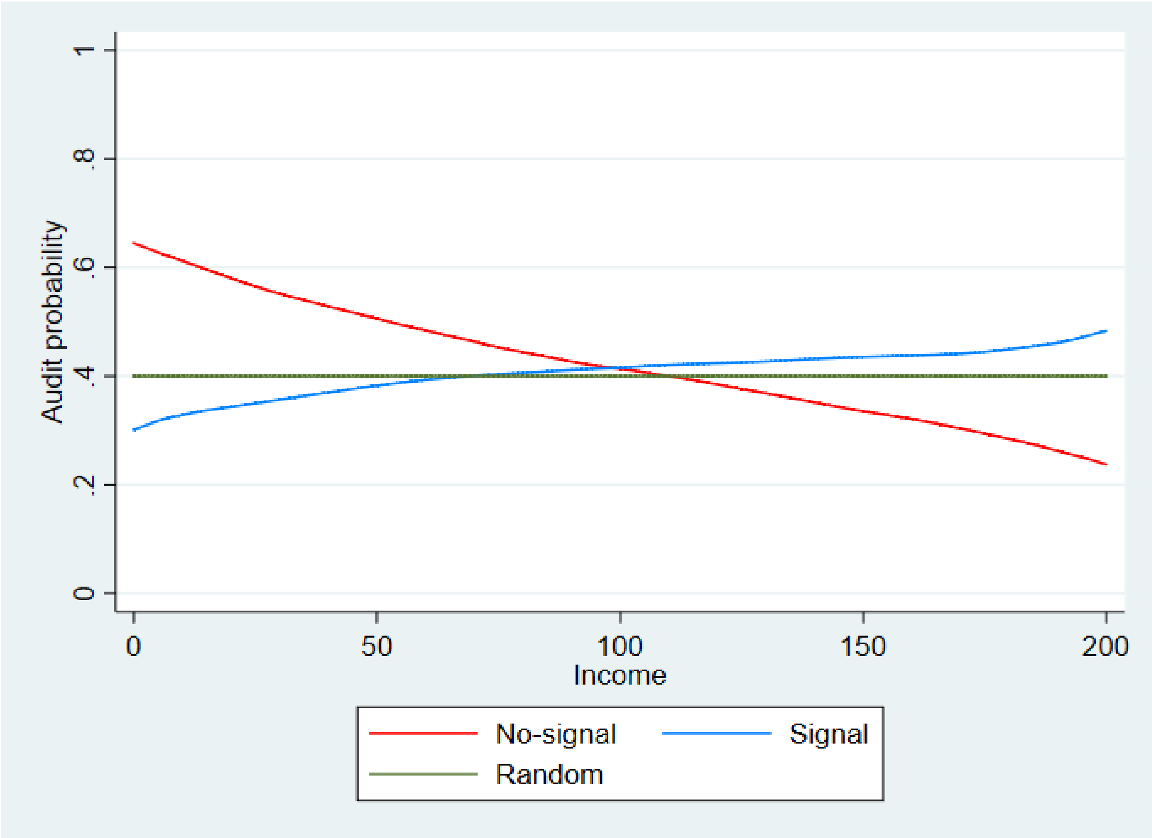


3.3 Audit probabilities

The third hypothesis is also confirmed by experimental data. Figure 4 displays the predicted values of nonparametric regressions of audit probability on income, estimated separately for the signal and no-signal treatments. We can see that the estimates correspond closely to the theoretically predicted relationship (Figure 2). A probit regression of audit probabilities on a treatment dummy, income, and the interaction, find that audit probabilities are negatively related to income in *no-signal* ($p < 0.01$), positively related to income in *signal* ($p = 0.019$), and that the difference in these relationships is statistically significant ($p < 0.01$), with all standard errors clustered at the group level.[6]

[6]Details available on request.

Figure 4: Relationship between audit probabilities on actual income (nonparametric kernel regression - optimal bandwidth selected using improved AIC).



3.4 Revenue

In this section we focus on expected total revenue, i.e. the expectation of revenue based on the audit probabilities implied by actual declarations, but before taxpayers were randomly selected for audit. This removes the noise associated with the randomness in audit selection, and allows for more precise estimation of treatment effects. As reported in Table 2, and in line with Hypothesis 4, average revenue was 55.4, 52.5, and 50.9 in the *signal*, *no-signal*, and *random* treatments, respectively. Table 4 displays the results of regressions on revenue, with the same specifications as for declared income.[7] Regressing only on treatment dummies shows that revenue is significantly higher in *signal* than the other two treatments, but *no-*

[7]For our regressions for revenue, we continue to use individual data rather than aggregating to the group level. This is necessary to allow us to control correctly for income, because the non-linearity in predicted treatment differences mean that for some realizations of income levels, the sign of the predicted average treatment difference between *signal* and *no-signal* can reverse if group income predictions are incorrectly based on average group income, rather than averaging after predictions are made at the individual level.

signal does statistically no better than *random*. Controlling for income only increases the magnitude and statistical significance of the advantage of the ASM in *signal*.

When allowing for interaction effects between the treatment and income (column 3), the results are very similar to those for declared income. Note, however, that unlike with declared income, the relationship between revenue and income in *random* is steeper than in *no-signal*, reflecting the decreased number of audits on richer taxpayers in the latter treatment. Again, controlling for demographics does not alter any of these conclusions (column 4).

3.5 Inequality

Our measure of inequality is the standard deviation of net income within a group and period. Subjects from *random* are randomly distributed to virtual groups of five to maintain comparability with the other two treatments. Average ex-post inequality was significantly less than the theoretical value of ex-ante income inequality, $\sqrt{\frac{200^2}{12}} \approx 57.7$: 28.4 in *signal* ($p < 0.01$), 38.2 in *no-signal* ($p = 0.015$), and 41.9 in *random* ($p = 0.024$). The regressions in Table 5 show that inequality is significantly lower in *signal* relative to the other two treatments, but inequality is not significantly different between *no-signal* and *random*. This result still holds when controlling for the standard deviation of gross income, and for demographics.

4 Discussion and Conclusion

Earlier competitive audit selection mechanisms have been based on the assumption that the enforcement authority has noisy, but unbiased, information about each individual's regulated output. This assumption may be very restrictive in some settings, perhaps especially so in the tax compliance setting.

In this paper, we propose a competitive audit selection mechanism that is based only on the declared incomes of taxpayers, and we examine its properties. Using experimental methods, we show that the proposed mechanism leads to higher tax compliance than a random mechanism in which all taxpayers are audited with the same baseline probability. In particular, we show that the mechanism works even if the incomes of the taxpayers in the reference group differ substantially. Furthermore, the mechanism is designed in such a way that the expected level of the audits is kept constant, which means that the additional cost of implementing the mechanism consist only of the administration of the more complex audit selection procedure. In sum, our paper suggests that the competitive audit selection mechanism might be an affordable and effective tool for reducing tax evasion even if the tax

Table 4: Expected total revenue

	(1)	(2)	(3)	(4)
signal	2.908**	4.805***	-10.35***	-10.17***
	(1.355)	(0.626)	(0.711)	(0.696)
random	-1.647	-1.378	-7.297***	-7.474***
	(1.839)	(0.940)	(0.737)	(0.744)
income		0.531***	0.457***	0.457***
		(0.0116)	(0.00884)	(0.00881)
signal#income			0.152***	0.152***
			(0.0102)	(0.0102)
random#income			0.0584***	0.0586***
			(0.0136)	(0.0136)
female				1.439***
				(0.484)
age				0.00346
				(0.0880)
economics student				-0.248
				(0.515)
Constant	52.51***	-1.279	6.283***	5.600***
	(1.040)	(1.163)	(0.445)	(2.091)
Observations	3,000	3,000	3,000	3,000
R^2	0.003	0.939	0.954	0.954

Standard errors clustered by group in parentheses

*** p<0.01, ** p<0.05, * p<0.1

office does not have information about the actual income of taxpayers and is not able to place taxpayers in reference groups of taxpayers with similar incomes.

However, the desirability of this mechanism is limited by the fact that low-income taxpayers in the reference group are audited more frequently. This may be difficult to justify

Table 5: Inequality (standard deviation of net income)

	(1)	(2)	(3)
treatment1	-10.52***	-11.16***	-11.67***
	(1.597)	(1.506)	(1.561)
treatment3	2.781	2.602	3.994
	(2.907)	(3.174)	(3.144)
sd_income		0.349***	0.343***
		(0.0312)	(0.0294)
female			-6.079*
			(3.606)
age			0.360
			(0.809)
econ_study			5.925
			(4.318)
Constant	37.53***	18.52***	10.86
	(1.443)	(2.456)	(18.33)
Observations	600	600	600
R^2	0.174	0.339	0.349

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

from a social justice point of view. Moreover, it has a countervailing effect on the increase in tax revenue and decrease in inequality engendered by greater compliance. In contrast, when a signal of true income is available, not only is compliance further increased, but more frequent auditing of the rich further reduces inequality. Thus, the value of such a signal may be greater than the resulting increase in revenue, if inequality reduction is an objective.

Finally, we would like to note that although we did not find significant improvements in total revenue and inequality when only declared income influenced audit probabilities, we feel this is secondary to the robust effect on individual behaviour. This considerable increase in compliance suggests that for larger samples, a statistically significant increase in revenue and reduction in equality would be found, although the issue of targetting the poor for audits would remain.

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A Theoretical framework

A.1 Model description

This section describes theoretical model that creates framework for the experiment. Taxpayer i receives an income I_i drawn form a distribution $F(I)$ with support $[\underline{I}, \overline{I}]$. The taxpayer chooses the declared income $R_i \in \langle 0, I_i \rangle$. declared income is taxed by rate τ, so the taxpayer pays a tax τR_i. The taxpayer i is audited with a probability $\pi_i(R_i, R_{-i})$. The formula for π_i depends on the audit selection mechanism used by the tax authority. If the taxpayer is chosen for audit, she pays a fine which depends on the undeclared income, $\phi(I_i - R_i)$ where ϕ is the fine rate and $\phi > \tau$.

We examine three different audit selection mechanisms. Under the random audit selection mechanism, the audit probability is the same for all taxpayers regardless of their declared income, $\pi_i = p$. When the competitive audit selection rule with limited information is applied, the audit probability depends on both the income declared by the taxpayer and the income declared by other taxpayers. In particular, the form of our audit selection rule resembles the generalized audit selection rule proposed by Gilpatric et al. (2011) closely. It has the following form:

$$\pi_i = p - \delta \left(R_i - \frac{\sum R_{-i}}{N-1} \right),$$

where N is the number of taxpayers in a group. This mechanism assigns higher audit probability to the taxpayers with relatively lower declared income. Parameter p defines the basic audit probability, and parameter δ defines the sensitivity of the audit selection rule to the declared income. The random audit selection mechanism is obviously a special case of the competitive audit selection mechanism for which $\delta = 0$.

The audit selection rule with complete information has the same functional form, but it uses undeclared income instead of declared income:

$$\pi_i = p + \delta \left(Z_i - \frac{\sum Z_{-i}}{N-1} \right),$$

where $Z_i = I_i - R_i$ is the undeclared income. The formula for this audit selection mechanism can be rewritten as

$$\pi_i = p - \delta \left(R_i - \frac{\sum R_{-i}}{N-1} \right) + \delta \left(I_i - \frac{\sum I_{-i}}{N-1} \right).$$

We can see that the difference between the two competitive audit selection mechanisms is the last term in the previous equation. This term shows that the audit selection mechanism with complete information accounts also for the differences in actual incomes.

The important aspect of the audit selection mechanism with limited information is that the audit probability depends only on the difference between the individual's declared income and the average declared income of other taxpayers. Therefore, this audit selection mechanism does not require information concerning each taxpayer's income or the income distribution in their peer group. On the other hand, it loses a substantial amount of information since the differences in the declared income may be caused not only by different compliance but also by differences in actual incomes. While the audit selection mechanism with complete information can differentiate between these effects, the mechanism with incomplete information cannot.

A.2 Equilibrium

Risk-neutral taxpayer i chooses the declared income R_i in order to maximize the expected wealth:

$$W = (1 - \pi(R_i, R_{-i}))(I_i - \tau R_i) + \pi(R_i, R_{-i})((1-\tau)R_i + (1-\phi)(I_i - R_i)).$$

Under the random audit selection mechanism, the first derivative of the expected wealth can be written as $p\phi - \tau$. Suppose that the tax rate τ is higher than $p\phi$. If this is the case, then the optimal choice of the risk-neutral taxpayer is to evade paying taxes on all of her income.

Now, we derive the solution for the competitive audit selection mechanism with limited information. Suppose that the other player's strategy is $R(I_i)$, where R is a non-decreasing function. The first order condition is given as follows:

$$\left(p - \delta R_i + \delta \int R(I_i)dF(I)\right)\phi - \tau + \delta\phi(I_i - R_i) = 0$$

We focus on the symmetric equilibria of the model. The solution of the first order condition is a linear function $R = a + bI$, where the declared income must be in the interval $[0, I]$. Depending on the values of the exogenous parameters, the taxpayer's optimal choice may be constrained by the upper bound I or the lower bound 0. This gives us two possible equilibrium strategies[8].

[8] These strategies do not form multiple equilibria. Only one of these strategies forms an equilibrium depending on model parameters.

The first equilibrium occurs when the tax rate is low compared to the audit probability and the fine. In this situation, the low-income taxpayers declare their whole income, as they face a high probability of being audited. The taxpayers with higher income face lower probabilities of being audited, and they optimally react by evading being taxed on some amount of their income. Formally, the equilibrium strategy has the following form:

$$R(I_i) = \begin{cases} I_i & \text{if } I_i < \hat{I} \\ a + bI_i & \text{if } I_i \geq \hat{I}, \end{cases} \tag{1}$$

where $a > 0$. The equilibrium values of the parameters a, b and $\hat{I}$ are given by the solutions of the following three conditions (the last condition is derived from the equation $a + b\hat{I} = \hat{I}$), respectively:

$$a = \frac{p\phi - \tau}{\delta\phi(1 + F(\hat{I}))} + \frac{F(\hat{I})}{1 + F(\hat{I})}E(I|I < \hat{I}) + \frac{1 - F(\hat{I})}{2(1 + F(\hat{I}))}E(I|I > \hat{I})$$

$$b = \frac{1}{2}$$

$$F(\hat{I})E(I|I < \hat{I}) + \frac{(1 - F(\hat{I}))}{2}E(I|I > \hat{I}) - \frac{1 + F(\hat{I})}{2}\hat{I} = \frac{\tau - p\phi}{\delta\phi}$$

The second equilibrium is applicable when the tax rate is relatively high compared to the audit probability and fine. The taxpayers with lower income evade paying taxes on all their income, as they risk losing a small amount of money if they are audited. On the other hand, taxpayers with high income have a lot of money at stake, so they will optimally declare some income. In formal terms, the equilibrium strategy has the following form:

$$R(I_i) = \begin{cases} 0 & \text{if } I_i < \hat{I} \\ a + bI_i & \text{if } I_i \geq \hat{I} \end{cases} \tag{2}$$

The equilibrium values of the parameters a, b and $\hat{I}$ are given by the solution of the following three conditions (the last condition is derived from the equation $a + b\hat{I} = 0$), respectively:

$$a = \frac{p\phi - \tau}{\delta\phi(1 + F(\hat{I}))} + \frac{1 - F(\hat{I})}{2(1 + F(\hat{I}))}E(I|I > \hat{I})$$

$$b = \frac{1}{2}$$

$$\frac{(1 - F(\hat{I}))}{2}E(I|I > \hat{I}) - \frac{1 + F(\hat{I})}{2}\hat{I} = \frac{\tau - p\phi}{\delta\phi}$$

Both equilibria under the competitive audit selection mechanism with limited information result in higher tax compliance compared to using random audits. This result forms the main hypothesis of our experiment.

Still, the lack of information about taxpayer's undeclared income affects the taxpayers' equilibrium strategies. Suppose that the audit selection rule is based on undeclared income. The best-response function is defined implicitly by the following first-order-condition in which $Z(I)$ denotes the equilibrium strategy of the other players:

$$\tau - \left(p + \delta Z_i - \delta \int Z(I)dF(I)\right)\phi + \delta\phi Z_i = 0$$

In the symmetric Nash equilibrium, the taxpayer's strategy is to evade taxes on some fixed amount c. If their income is lower than this amount, then they will evade paying taxes on all their income.

$$Z(I_i) = \begin{cases} I_i & \text{if } I_i < c \\ c & \text{if } I_i \geq c \end{cases} \tag{3}$$

The amount of evaded taxes c is determined by the following equation:

$$p + \delta F - \delta\phi(F(c)E(I|I < c)) + (1 - F(c)c) - \tau = 0.$$

We can observe one stark contrast between the equilibrium strategies under complete and incomplete information. The slope of the equilibrium strategy is lower when the audit selection mechanism does not have complete information. Therefore, higher-income taxpayers evade more taxes when estimates of individuals' incomes are missing.

B Instructions

In this section, we provide the instructions for all treatments.

B.1 Signal

Introduction

In this experiment, we study your decision-making as individuals as well as groups. Your earnings are based on your decisions. Therefore, we recommend you to read the following instructions carefully. You will be paid at the end of the experiment in cash and in private.

You will make your decisions without communication with the other participants in the experiment. If you have any questions, please raise your hand, and an experimenter will come to answer it in private.

Please do not communicate with other participants during the experiment, do not use your mobile phone or other devices than the computer you are seated at and pay attention to the experiment. In case of disobedience, you will be excluded from the experiment without any reward.

The course of the experiment

The experiment will take place in groups of five people: you and four other participants. Players in your group are in this room, but we don't tell you who belongs to your group. The group is randomized at the beginning of the experiment and does not change throughout the experiment.

The experiment consists of 30 identical rounds. At the beginning of each round, you receive an amount from the range of 0 to 200 CZK from us. Each integer amount can be selected with the same probability. You can imagine the amount selection as a draw from a hat that contains 201 balls with numbers from 0 to 200.

In the next step, you will be asked to declare a certain amount. This declared amount may be the same or lower than the amount you received. E.g., if you received 102 CZK, you can declare any integer number between 0 and 102 CZK. We will deduct 60% from the declared amount, so you can keep 40% of the declared amount. The amount of money that you received from us at the beginning of the round and did not declare will be called the undeclared amount. The fate of the undeclared amount depends on a chance. With a certain probability, an adverse event occurs and you lose the entire undeclared amount. Otherwise, you keep the undeclared amount.

The basic probability that an adverse event will occur and you lose the unreported amount is 40%. In addition, for every 10 CZK your reported amount is lower than the average of the amounts reported by other players in your group, the chance of an adverse event increases by 4 percentage points. Conversely, every10 CZK above the average of the other players in the group means a 4% reduction in the probability of an adverse event. For example, if you report an amount of 81 CZK higher than the average of other players in your group, the chance of an adverse event is $81 \times 0.4 = 32.4$ percentage points lower than the baseline, that is, $40 - 32.4 = 7.6\%$. Conversely, if you reported 35 CZK less, the probability would be $40 + (35 \times 0.4) = 54\%$. Whether an adverse event occurs will be drawn in each round again.

Payoffs

There are two possible scenarios in each round of this experiment:

1. An adverse event occurs. Your payoff for that round is $0.4 \times$ declared amount.
2. No adverse event occurs. Your payoff will be $0.4 \times$ declared amount + undeclared amount.

At the end of each round, you receive information about whether an adverse event occurred, and what your payoff was in the round. We will also inform you of the average amount reported by other players and of the probability of an adverse event. At the end of the experiment, you will receive your payoff in CZK from five randomly selected rounds of the experiment.

B.2 No signal

Introduction

In this experiment, we study your decision-making as individuals as well as groups. Your earnings are based on your decisions. Therefore, we recommend you to read the following instructions carefully. You will be paid at the end of the experiment in cash and in private. You will make your decisions without communication with the other participants in the experiment. If you have any question, please raise your hand, and an experimenter will come to answer it in private.

Please do not communicate with other participants during the experiment, do .not to use your mobile phone or other devices than the computer you are seated at and pay attention to the experiment. In case of disobedience, you will be excluded from the experiment without any reward.

The course of the experiment

The experiment will take place in groups of five people: you and four other participants. Players in your group are in this room, but we don't tell you who belongs to your group. The group is randomized at the beginning of the experiment, and do not change throughout the experiment.

The experiment consists of 30 identical rounds. At the beginning of each round, you receive an amount from the range of 0 to 200 CZK from us. Each 1 CZK may be selected with the same probability. You can imagine the amount selection as a draw from a hat that contains 201 balls with numbers from 0 to 200.

In the next step, you will be asked to report the amount. This *declared amount* may be the same or lower than the amount you received. E.g. if you receive a refund of 102 CZK, you can declare any integer number between 0 and 102 CZK. There will be always deducted 60% from the *declared amount* and you will keep 40% of the *declared amount.* The amount of money that you received from us at the beginning of the round and did not report is called the *undeclared amount.* The fate of the *undeclared amount* depends on a chance. With a certain probability of an adverse event occurs and you lose the entire *undeclared amount.* Otherwise, you keep the *undeclared amount.*

The basic probability that an adverse event will occur and the undeclared amount is lost is 40%. Besides, each 10 CZK by which your *declared amount* is lower than the average *declared amounts* of four other players in your group, the probability of adverse event occurrence increases by 4 percentage points. On the other hand, each 10 CZK above the average *declared amounts* of four other players in your group decreases the probability of adverse event occurrence increases by 4 percentage points.

For example, if your declared amount is by 81 CZK above the average *declared amounts* of four other players in your group, the probability of adverse event occurrence is by $81 \times 0.4 = 32.4$ percentage points lower than basic probability, that means 40–32.4 = 7.6%. On the other hand, if you declare amount by 35 CZK lower than the average declared amounts of four other players in your group, the probability will be $40 + (35 \times 0.4) = 54\%$. Whether the adverse event occurs is drawn in each round.

Payoffs

There are two possible scenarios in each round of this experiment:

1. An adverse event occurs. Your payoff for that round is 0.4 $\times$ declared amount.

2. No adverse event occurs. Your payoff will be 0.4 $\times$ declared amount + undeclared amount.

At the end of each round, you receive information about whether an adverse event occurred, and what your payoff was in the round. We will also inform you of the average amount reported by other players and of the probability of an adverse event. At the end of the experiment, you will receive your payoff in CZK from five randomly selected rounds of the experiment.

B.3 Random

Introduction

In this experiment, we study your decision-making as individuals as well as groups. Your earnings are based on your decisions. Therefore, we recommend you to read the following instructions carefully. You will be paid at the end of the experiment in cash and in private. You will make your decisions without communication with the other participants in the experiment. If you have any question, please raise your hand, and an experimenter will come to answer it in private.

Please do not communicate with other participants during the experiment, do .not to use your mobile phone or other devices than the computer you are seated at and pay attention to the experiment. In case of disobedience, you will be excluded from the experiment without any reward.

The course of the experiment

The experiment consists of 30 identical rounds. At the beginning of each round, you receive an amount from the range of 0 to 200 CZK from us. Each 1 CZK may be selected with the same probability. You can imagine the amount selection as a draw from a hat that contains 201 balls with numbers from 0 to 200.

In the next step, you will be asked to report the amount. This *declared amount* may be the same or lower than the amount you received. E.g. if you receive a refund of 102 CZK, you can declare any integer number between 0 and 102 CZK. There will be always deducted 60% from the declared amount and you will keep 40% of the *declared amount.* The amount of money that you received from us at the beginning of the round and did not report is called the *undeclared amount.* The fate of the *undeclared amount* depends on a chance. With a certain probability of an adverse event occurs and you lose the entire undeclared amount. Otherwise, you keep the *undeclared amount*.

The probability that an adverse event will occur and the *undeclared amount* is lost is 40%. Whether the adverse event occurs is drawn in each round.

Payoffs

There are two possible scenarios in each round of this experiment:

1. An adverse event occurs. Your payoff for that round is 0.4 $\times$ declared amount.

2. No adverse event occurs. Your payoff will be 0.4 $\times$ declared amount + undeclared amount.

At the end of each round, you receive information about whether an adverse event occurred, and what your payoff was in the round. We will also inform you of the average amount reported by other players and of the probability of an adverse event. At the end of the experiment, you will receive your payoff in CZK from five randomly selected rounds of the experiment.

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