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ABSTRACT

We study the effects of different tax reporting mechanisms in experimental double auction markets in the laboratory. The sales tax is paid by the seller, and we compare market outcomes in a no-tax condition to cases where (i) tax evasion is impossible, (ii) taxes can be evaded but there is an exogenous (low) audit probability, or (iii) there is double-reporting by both the buyer and the seller, and the seller's audit probability is endogenously increased if her tax report is inconsistent with the buyer's report. The latter case mimics the use of so called third-party reporting in tax enforcement. We find that third-party reporting effectively deters evasion, and deterrence also has real effects on market outcomes: market clearing prices, quantities and overall efficiency return to the levels observed when tax evasion was impossible. When reporting is costly to buyers, they report significantly less trades. Tax compliance by sellers however remains at a relatively high level, even though payoffs would be maximized for both parties if no trades were reported. This suggests that the mere possibility of the existence of third party information may be a fairly effective deterrent on tax evasion, and tax administrators might consider making their information sources more widely publicized.

JEL Classification: H21, H22, H26, D40, D44, D91

Keywords: Tax Evasion, Tax Incidence, Third-Party Reporting, Double Auction, Experiment

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1 Introduction

Effective tax administration and enforcement can be seen as prerequisites for an extensive and well-functioning welfare state (e.g. Kleven (2014)). It is important to understand how the mechanism used to elicit tax reports affects compliance as well as market outcomes and tax incidence. The so called tax systems approach to the analysis of taxation (Slemrod and Gillitzer (2014)), as well as literature on behavioral public finance more generally, emphasize that the reactions of economic agents to taxation may depend not only on tax rates and tax bases, but also on other design features of the tax system.

In the current paper, we examine the impact of different tax reporting mechanisms in experimental double auction markets. The tax is paid by the seller, and we compare market outcomes in a no-tax condition to cases where (i) tax evasion is impossible, (ii) taxes can be evaded but there is an exogenous (low) audit probability, or (iii) there is double-reporting by both the buyer and the seller, and the seller's audit probability is endogenously increased if her tax report is inconsistent with buyers' reports. The latter case mimics a situation where so called third-party reporting is used in tax enforcement. Out of internal and external validity concerns, we use terminology which is framed to taxation setting and, moreover, the tax payments collected in the experimental markets are not kept by the expeirmenter but rather transferred to the governmental tax authorities in our design.

We aim to contribute to the literature on tax evasion in the following ways. First, we analyze the effects of tax evasion and enforcement on market outcomes, namely market prices and quantities traded. Much of the field evidence (e.g. Kleven et al. (2011) and numerous subsequent field experiments on tax enforcement) focus to a large extent on reporting responses, and in general real and reporting responses are difficult to disentangle from one another. In a laboratory setting, the different margins at which individuals respond to enforcement can be identified.

Second, while the importance of third-party information as a determinant of tax compliance has been acknowledged in earlier literature (e.g. Slemrod (2007), Kleven et al. (2011)), literature utilizing randomized variation in third-party information is scarce.[1] Some recent studies have randomized the salience of third-party information in the field (e.g. Harju et al. (2017)). We implement exogenous variation in the actual availability of third-party information in the lab.

Third, the third party's (here the buyer) incentives to report have not been studied extensively. In most cases, the existence (or the lack) of third-party information has been taken as given. Given that it may be in the interest of both the buyer and the seller not to report a given transaction, it is not clear a priori that buyers should have incentives to report truthfully. In particular, buyers and sellers may be able to (implicitly) collude and under-report. We explicitly examine the buyer's reporting decision, which affects the availability of third-party information to the tax authority.

There is an extensive literature on laboratory experiments analyzing tax evasion; see e.g. Malézieux (2018) for a recent review. In this literature, two papers are particularly close to ours. First, Abraham et al. (2017) analyze collusive tax evasion by individuals in the lab. They do not analyze the effect of double reporting (compared to other reporting mechanisms) on tax evasion per se, but rather focus on how social norms affect joint tax evasion. (In related field studies, Bjørneby et al. (2018) implement a randomized audit study to provide evidence of joint tax evasion by workers and firms, while Paulus (2015) documents the same phenomenon using survey and register data.) Second, Doerrenberg and Duncan (2017) have shown that providing sellers an opportunity to evade sales taxes implies that markets clear with higher quantities and lower prices than when evasion is not possible. Kopczuk et al. (2016) provide complementary field evidence on the relationship between evasion possibilities and tax incidence. In this study, we further ask how buyer reporting affects market clearing prices, quantities and tax compliance. We have two competing hypotheses: 1) Buyers are highly compliant. Sellers learn this and shift a larger share of the effective tax rate onto buyers through increased prices. 2) Buyers understand that not reporting trades allows sellers to evade taxes, and lowers the effective tax rate; sellers react by setting lower prices.

In line with theory, we find that the introduction of the tax increases market prices and decreases quantities traded. As expected, when tax evasion is possible, many sellers evade some of

[1] In Kleven et al. (2011), variation in third-party reporting comes from certain types of income being subject to third-party reporting, while others (notably self-employment income) are not. In studying firm responses to an audit experiment, Pomeranz (2015) compares those line-items in the VAT declaration of firms that are covered by the paper trail (transactions between two firms) to line items that are not (sales to final consumers). Naritomi (2018) compares retail transactions (where the extent of third-party information increased due to a campaign that incentivized consumers to send in their receipts to the authorities) and wholesale transactions (not affected by the campaign). Finally, using a laboratory experiment, Alm et al. (2009) study individual income tax compliance in a setting where subjects differ in terms of the fraction of their income that is subject to third-party information reported to the tax authority. They find that subjects with relatively more third-party reported income have a significantly higher tax compliance rate than subjects with relatively less third-party reported income.

their taxes due, and this leads to lower market prices and increased quantities traded. When the reporting institution makes use of complimentary third-party information provided by buyers, tax compliance is very high, implying that market prices increase and quantities traded decrease. To further analyze buyers' incentives to provide third party information, we examine the effect of reporting costs. When reporting is costly to buyers, not reporting any trades is the dominant strategy for buyers, and both parties not reporting is the only equilibrium in the reporting game (for payoff-maximizing agents). Accordingly, when reporting is costly to buyers, they report significantly less trades. However, somewhat surprisingly, tax compliance by sellers remains at a high level. The mere possibility of the existence of third party information may thus be a fairly effective deterrent on tax evasion, and tax administrators might consider making their information sources more widely publicized.

2 Experimental Design

2.1 The Double Auction

We conduct a 25-period standard continuous laboratory double auction market (Smith, 1962), with 5 sellers and 5 buyers trading units of a homogenous good.[2] In a market each seller can sell up to 4 units and each buyer can buy up to 4 units of the good. Each unit k of seller i has a different per unit production cost c_{ik} and each unit k of buyer i has a different per unit reservation value v_{ik}. Cost and value schedules are randomly assigned to sellers and buyers, respectively, and vary across traders (Table 5 and Figure 1 depicting the supply and demand curves).[3] The roles and cost/value schedules do not change during the experimental session and are private information.

The market opens for trading for 100 seconds in each period. Each buyer and seller can trade her units one at a time, starting with the first unit ($k = 1$), then the second ($k = 2$), and so forth. Traders may post offers to sell or buy at any time while the market is open. Each seller i may post an integer price $p_{ik} \in \{c_{ik}, ..., P^S - 1\}$ where P^S denotes the current standing ask (or 300 if there are no posted asks so far). Each buyer may post an integer price $p_{ik} \in \{P^B + 1, ..., v_{ik}\}$ where P^B is the current standing bid (or 0 if there are no posted bids so far). All traders observe the current standing bid and ask (if there are any). A transaction takes place when a seller accepts a standing bid or a buyer accepts a standing ask. A seller can accept a current standing bid as long

[2] In addition, to help subjects get familiar with the environment and the interface, we ran three unpaid practice periods before the payoff relevant periods.

[3] Costs and values are first randomized into sets of four costs and four values. These sets of four (see Table 5) are then randomly assigned to traders at the beginning of each experimental session.

as $P^B \geq c_{ik}$. A buyer can accept a current standing ask as long as $P^S \leq v_{ik}$. When a transaction occurs, current standing prices are removed and the process of posting bids and asks begins again until the market closes. All accepted prices are displayed on traders' screens for the entire duration of the trading phase.[4] Communication is not allowed at any point.

Seller i's gross profit for selling her k^{th} unit, π^S_{ik}, is defined as $\pi^S_{ik} \equiv p_{ik} - c_{ik}$, where p_{ik} is the price the seller receives from selling the unit. Buyer i's gross profit from buying her k^{th} unit, π^B_{ik}, is defined as $\pi^B_{ik} \equiv v_{ik} - p_{ik}$, where p_{ik} is the price the buyer pays for the unit. Units that are not traded do not yield profits or losses. A trader's market income from one period is the sum of the gross profits over all 4 units minus possible taxes and/or fines paid, and her final payoff consists of the sum of her market incomes over all 25 periods.

2.2 Treatments

We have five treatments that differ in whether a per-unit sales tax is imposed on the sellers, and how the tax is collected.[5] In the NO TAX treatment, there is no sales tax. A trader's market income is the sum of gross profits from each traded unit: $\Pi^S_i \equiv \sum_{k=0}^4 (d_{ik}p_{ik} - d_{ik}c_{ik})$ for sellers and $\Pi^B_i \equiv \sum_{k=0}^4 (d_{ik}v_{ik} - d_{ik}p_{ik})$ for buyers, where $d_{ik} = 1$ if the seller or buyer traded her k^{th} unit and 0 otherwise. In the AUTOMATIC treatment, a per-unit sales tax $\tau = 40$ ECU is imposed on the sellers. The tax is automatically collected, making tax evasion impossible. Hence a seller's market income is given by $\Pi^S_i \equiv \sum_{k=0}^4 (d_{ik}p_{ik} - d_{ik}c_{ik}) - \tau s_i$, where s_i denotes the number of units the seller sold in the market.

In the SELLER ONLY treatment, sellers are asked to file a tax report stating how many units they sold in the current period. A seller can report $r_i \in \{0, ..., s_i\}$, where r_i denotes the number of units seller i reports as sold. The sales tax is collected for each unit *reported* as sold, unless an audit is conducted, in which case the tax is collected for each unit actually sold. In addition, an audit implies the seller is fined $f = 40$ ECU for each sold unit the seller failed to report. The probability of an audit is exogenously fixed 10%, and it is independent across sellers. Now a seller's market income from trading in the current period is given by

$$\pi^S_i = \begin{cases} \sum_{k=0}^4 (d_{ik}p_{ik} - d_{ik}c_{ik}) - \tau r_i, & \text{if seller is not audited} \\ \sum_{k=0}^4 (d_{ik}p_{ik} - d_{ik}c_{ik}) - \tau s_i - f(s_i - r_i), & \text{if seller is audited.} \end{cases}$$

[4]See Figure 9 Appendix B for an example of a seller's trading screen.

[5]The experiment was framed, i.e. we explicitly used the words "buyer", "seller" and "tax". See Appendix C for English translations of instructions for treatment SELLER + BUYER. The full set of instructions is available from the authors.

The SELLER + BUYER treatment is otherwise as the SELLER ONLY treatment but with an endogenously determined audit probability. More specifically, for each unit they bought in the current period, buyers are asked to, costlessly, decide whether to report or not report that unit. The probability of an audit for a seller is 10%, unless the seller reports less sold units than the buyers who bought from her, in which case the audit probability is 80%.[6]

Finally, the SELLER + BUYERC treatment is otherwise as the SELLER + BUYER treatment but reporting is now made costly to the buyers. More specifically, if a buyer reports a positive number of units, she incurs a fixed reporting cost of 10 ECU.

2.3 Procedures

We conducted 27 sessions with 10 subjects in the PCRC laboratory of the University of Turku. In each session, we implemented one treatment condition. A total of 270 subjects, predominantly students at the University of Turku, participated in the experiment.[7] Participants were solicited through an online database using ORSEE (Greiner, 2015), and the experiment was run using the experiment software z-Tree (Fischbacher, 2007). After the experiment, subjects filled out a short questionnaire on background characteristics, level of tax morale, trust and risk preferences.[8] Sessions lasted up to 110 minutes, and participants earned, on average, 10.00 EUR for the experiment, including a 5 EUR showup fee. All tax revenue collected in the experiment was donated to the Finnish State Treasury, and this was common knowledge among the participants. Before conducting any sessions, we submitted a pre-analysis plan at the Open Science Framework (link).

[6]One may wonder why the audit probability is not 100% when a seller reports less units sold than the buyers who bought from her. We choose an audit probability below 100% to reflect the fact that the audit may fail to detect the full extent of evasion. Another way to incorporate this feature into the design would have been to set the audit probability to 100% in case of a mismatch in reports, but then have the audit detect the evasion with probability below 100%. We choose the former approach as it is easier for the subjects to understand.

[7]There were 60 participants in each of the treatments NO TAX, AUTOMATIC, SELLER ONLY and SELLER + BUYER. There were 30 participants in the treatment SELLER + BUYERC.

[8]Tax morale is elicited by asking subjects to choose whether "cheating on taxes if you have a chance" is "always justified, something in between or never justified". This question is very similar to the one used in the World Values Survey (www.worldvaluessurvey.org) and is a frequently used measure of tax morale in survey studies. Risk preferences are elicited by directly asking subjects to assess their willingness to take risks on a scale from 1 to 10. The complete post-experimental questionnaire is included in the pre-analysis plan submitted at the Open Science Framework.

3 Predictions

3.1 Market Outcomes

For treatments NO TAX and AUTOMATIC, standard economic theory offers precise quantitative predictions. More specifically, the cost and valuation schedules in our design were chosen so that supply equals demand at 17 units in the absence of the sales tax. This corresponds to a market clearing price between 158 ECU and 162 ECU. The inverse supply and demand functions are displayed in Figure 1.[9] Furthermore, imposing a 40 ECU per-unit sales tax on sellers implies a 40 ECU upward shift in the supply curve. Therefore, with the tax, markets clear with 13 units traded and a price between 178 ECU and 182 ECU: The distortion caused by taxes leads to higher prices and lower demand.

In the SELLER ONLY treatment, sellers can avoid (some of) the tax by choosing not to report (some of) the units sold. Hence, the opportunity to evade taxes enables sellers to lower their *effective* unit tax, implying that the supply curve shifts up by less than 40 ECU. Depending on the level of compliance, we thus expect the market price (quantity) in the SELLER ONLY treatment to be lower (higher) than in the AUTOMATIC treatment. As the effective tax is lower than when taxes are not fully enforced, the distortion in market prices and quantities is expected to be smaller. However, if some of the sellers choose not to evade fully, the market price (quantity) in SELLER ONLY treatment is expected to be above (below) the market price (quantity) in the NO TAX treatment.

In the SELLER + BUYER treatment, buyers can provide third-party information by costlessly reporting the trades they make. This information is then automatically matched with the reported units by a given seller. In case a seller reports less transactions than the buyers she traded with, the seller has an 80% chance of being audited. The combination of third-party information and deterrence should have a disciplining effect on evasion, provided that buyers report traded units sufficiently truthfully.[10] Furthermore, since even without a mismatch in reports, a seller faces the 10% baseline chance of an audit, and since the treatment assignment is random, the compliance rate in the SELLER ONLY treatment should provide a lower bound for the compliance rate – ditto the effective tax rate – in the SELLER + BUYER treatment. We thus expect the market outcomes in the SELLER + BUYER treatment to lie between those observed in the SELLER ONLY and in the AUTOMATIC treatments. The exact outcome will depend on the extent to which buyers and sellers are able to tacitly collude not to report truthfully; this issue is discussed in the next subsection.

[9] Due to the discrete nature of the market, the supply and demand functions are step-functions. This implies that the equilibrium prediction for the price is an interval.

[10] We discuss this matter more in Section 3.2.

As in the SELLER + BUYER treatment, buyers in the SELLER + BUYERC treatment are asked to decide for each of the units they bought in the current round whether they want to report that unit. However, buyers have to bear a fixed cost of 10 ECU if they choose to report more than zero units.[11] This makes reporting zero units optimal for selfish and money-maximizing buyers. We thus expect that buyers report a lower fraction of traded units compared to the SELLER + BUYER treatment. This implies that sellers face a lower risk of being audited and fined if they decide to evade taxes. Therefore, qualitatively speaking, we expect prices to be lower and quantities to be higher in the SELLER + BUYERC treatment than in the SELLER + BUYER treatment.

3.2 Reporting Behavior

By not reporting a sold unit, the seller avoids having to pay the tax of 40 ECU. At the same time, she faces a risk of being audited and having to pay the tax and a 40 ECU fine. In the SELLER ONLY treatment, the probability of an audit is exogenously fixed 10%. Given our (low) audit and penalty rates, the standard deterrence-model by Allingham and Sandmo (1972) predicts substantial non-compliance. A risk neutral and money-maximizing seller optimally reports zero units sold as the benefit from evasion, 40 ECU, exceeds the expected cost $0.1 \cdot (40 + 40) = 8$ ECU.

In the SELLER + BUYER treatment, the effective audit probability faced by the sellers depends on the reporting behavior of the buyers. For a risk neutral seller, reporting is optimal if and only if the (expected) audit probability is above 50%. This implies that reporting a positive number of units sold in the SELLER + BUYER treatment might be optimal, depending on how truthfully buyers report trades. Kleven et al. (2011) take into account the endogeneity of the audit probability in a situation where third-party information is available on a subset of tax items, incorporate this feature into a theoretical model and derive the prediction that reporting will be truthful on third-party reported income items. In that analysis, the existence of third-party information on certain items is taken as given, and the audit probability is common knowledge. In our context, uncertainty about whether buyers report truthfully leads to uncertainty about the existence of third-party information, and to ambiguity about the audit probability. Seller reporting behaviour will thus depend on their expecations about the audit probability. Since buyer reporting is costless, rational and money-maximizing buyers are indifferent between reporting and not reporting the units they bought. However, if buyers realize that their reporting behavior has a direct impact on sellers' eva-

[11] Kotakorpi and Laamanen (2017) argue, in the context of a study analyzing the effect of prefilled income tax returns on compliance, that the fixed cost of filing appears to be a key determinant of the reporting decision.

sion incentives, and thus an indirect impact on prices, selfish buyers would not report their trades.[12] On the other hand, it is possible that buyers have non-pecuniary incentives to report truthfully.[13] If sellers correctly anticipate truthful reporting by buyers, or if they learn that in the course of the experiment, they should become more compliant. We thus expect the seller compliance rate to be between that observed in the SELLER ONLY treatment and 100%.

All in all, it would be in the joint interest of (money-maximizing) buyers and sellers not to report any trades, but the seller has no dominant strategy as her optimal action depends on that of the buyers. The outcome of the game depends on how the sellers react to this strategic uncertainty, and whether sellers and buyers are able to tacitly collude on a non-reporting outcome.[14]

The situation is slightly different in the SELLER + BUYERC treatment. Since buyers have to bear a cost of 10 ECU if they report a positive number of units, selfish and money-maximizing buyers are no longer indifferent between reporting and not reporting. This disincentive to report should decrease the fraction of units reported by the buyers, on average, compared to the SELLER + BUYER treatment. This expected decrease in buyer reporting translates into a lower risk of an audit for a seller evading taxes. On the other hand, due to random assignment to treatments, potential intrinsic costs of misreporting should be distributed in the same way among sellers in the SELLER + BUYERC treatment and in the SELLER ONLY treatment. We therefore expect the seller reporting rate in the SELLER + BUYERC treatment to be between those observed in the SELLER ONLY and in the SELLER + BUYER treatments.

3.3 Tax Incidence

Following Doerrenberg and Duncan (2017), we estimate the economic incidence of the per-unit sales tax in two ways: i) the incidence of the nominal per-unit tax, and ii) the incidence of the (expected) effective per-unit tax. The incidence of the nominal tax refers to the mean share of the 40 ECU sales tax that is shifted onto buyers. This is defined as the difference between the market clearing price in the NO TAX condition and the market clearing prices in AUTOMATIC, SELLER ONLY and SELLER + BUYER conditions, respectively. As implied by the predicted market clearing price in AUTOMATIC (cf. Subsection 3.1), we expect the tax burden to be shared equally among

[12] For laboratory experiments on collusive tax evasion, see Abraham et al. (2017) and Balafoutas et al. (2015). For a field experiment, see Bjørneby et al. (2018).

[13] There may, for example, exist a social norm that dictates truthful tax reporting (e.g. Dwenger et al. (2016)), or the buyers may have a cost to lying in general (e.g. Abeler et al. (2014)).

[14] The potential collusion is only tacit as we do not allow communication between buyers and sellers. In real life such communication would of course be possible. However, any agreement to jointly evade taxes is of course cheap talk as any such deal would not be binding / enforceable in court.

buyers and sellers. This is due to the fact that in our setting, the (linearized) demand and supply schedules have the same elasticity in equilibrium. In SELLER ONLY and SELLER + BUYER, access to evasion is likely to allow sellers to lower their effective unit tax. We therefore expect the share of the nominal tax borne by buyers in these conditions to be less than or equal to what we observe in AUTOMATIC.

The incidence of (expected) effective per-unit tax refers to the share of the (expected) effective tax that is shifted onto buyers. In AUTOMATIC, the expected effective tax equals the nominal tax, whereas in SELLER ONLY, in SELLER + BUYER and in SELLER + BUYERC it depends on the expected audit probabilities (which in turn depend on the buyer's reporting decision in the latter two treatments). As Doerrenberg and Duncan (2017) argue, risky evasion opportunities could imply that sellers seek compensation for the risk related to evasion and shift more of the effective tax burden on buyers in the treatments with tax evasion. Further, given the strategic uncertainty inherent in the treatments involving double reporting, it is not clear a priori whether the outcome regarding tax incidence (for any given effective tax) should be similar in the double reporting treatments as in the SELLER ONLY treatment.

4 Results

4.1 Market Outcomes

Table 1 provides a descriptive overview of the data. Figure 2 (Figure 4) displays the mean (median) market price by period in each treatment, while Figure 6 shows the mean quantity traded by period in each treatment. To account for potential learning by the subjects in early periods, alongside results for the full sample, we also show results for periods 11-25 only. Almost all of our results are robust to using this truncated sample instead, and where the conclusions differ, we give results for the truncated sample in a footnote. Our analysis is based on both nonparametric and parametric methods. More specifically, we conduct nonparametric tests based on ranks using market level means (medians) over all relevant periods as units of observation.[15] Furthermore, we run pooled OLS panel regressions with mean price, median price and quantity traded in a period as dependent variables, and treatment dummies as independent variables (see Table 7 in Appendix A). The data is at market-period level (e.g. quantity traded at market i in period t). Since observations across periods within a given market are not independent, we cluster standard errors at the market level.

[15] We thus obtain 6 independent observations per treatment, apart from the SELLER + BUYERC treatment in which there are 3 independent observations.

Table 1: General descriptive statistics

Treatment	Price			Units sold	Compliance rate	Taxes collected
	Mean	Median	SD	Mean		Mean
Panel A: Periods 1-25						
NO TAX (PREDICTED)	[158,162]			17		
AUTOMATIC (PREDICTED)	[178,182]			13		
NO TAX	157.9	160	13.3	17.4		-
AUTOMATIC	176.3	179	13.1	13.3	100	13,260
SELLER ONLY	168.7	170	11	15.2	30	5,320
SELLER + BUYER	174	176	12.9	13.4	83	12,610
SELLER + BUYERC	164.7	170	16.2	14.2	68	11,410
Panel B: Periods 11-25						
NO TAX	159	160	9.5	17.4		
AUTOMATIC	177.7	180	9.1	12.8	100	
SELLER ONLY	168.9	170	8.3	15.1	29	
SELLER + BUYER	177.6	179	7.8	13.1	88	
SELLER + BUYERC	169.4	170	11.1	14	72	

Notes: (i) Mean (median) price indicates the mean (median) price over all trades across the relevant periods.
(ii) Mean units sold indicates the mean number of units sold per period.
(iii) Compliance rate is defined as the total number of trades reported by the sellers divided by the total number of trades over the relevant periods.

The parameters in our experiment imply that, absent the sales tax, the market is expected to clear with a market price between 158 ECU and 162 ECU and with 17 units traded. Our results for the NO TAX treatment are largely consistent with these predictions. In particular, the mean (median) price is not statistically significantly different from 160 (two-sided Wilcoxon signed rank tests (WSR), $p > 0.3$ for both the mean and the median). This result is further confirmed by two pooled OLS regressions in columns 2 and 4 in Table 7 in Appendix A (Wald tests for the linear hypothesis that the intercept equals 160 yields $p = 0.27$ for mean and 0.24 for median). However, the mean quantity per period is slightly higher than the predicted quantity of 17 (a two-sided WSR, $p = 0.03$).[16] Moreover, a pooled OLS regression in the fifth column of Table 7 in Appendix A confirms this observation (a Wald test, $p < 0.01$).

When a 40 ECU per-unit tax is imposed on sellers, standard theory predicts that the supply curve shifts up by 40 ECU, which in our setting implies an equilibrium with 13 units traded and a market price between 178 ECU and 182 ECU. Our results are again in line with the theoretical predictions: the mean (median) price is not statistically significantly different from 180 ECU (two-sided WSR, $p = 0.06$ for the mean, and $p > 0.2$ for the median).[17] Turning to parametric tests, models 1 and 3 in Table 7 reveal that the mean and the median price are statistically significantly below 180 ECU ($p < 0.05$ for both Wald tests). However, neither the mean nor the median price are

[16] Using data from periods 11-25 only, a two-sided WSR gives a p-value 0.06.
[17] Using periods 11-25 only, the p-values are 0.16 for the mean, and 0.38 for the median.

statistically significantly different from 178 ECU (Wald tests, $p = 0.19$ for the mean, and $p = 0.48$ for the median). Furthermore, the mean quantity per period is not statistically significantly different from 13 (a two-sided WSR, $p > 0.1$; a Wald test $p = 0.078$). The implication is that prices are higher and quantities lower in the AUTOMATIC treatment than in the NO TAX treatment (two-sided Wilcoxon rank-sum tests (WRS), $p < 0.01$ for all three tests).

In sum, our results on market outcomes in the NO TAX treatment, as well as in the AUTOMATIC treatment where a unit tax is levied on the sellers and no evasion is possible, correspond to the theoretical predictions. The outcomes in these two treatments provide useful benchmarks against which the outcomes of the other treatments can be compared.

Our qualitative predictions for the SELLER ONLY treatment are also confirmed by the data: market prices (quantities) are significantly lower (higher) in the SELLER ONLY treatment than in the AUTOMATIC treatment (two-sided WRS, $p < 0.01$ for mean and median price, and for mean quantity).[18] However, prices (quantities) still remain above (below) their no-tax equivalents (two-sided WRS, $p < 0.01$ for the mean price, $p < 0.05$ for the median price, and $p < 0.01$ for the mean quantity). This happens because sellers in the SELLER ONLY treatment are surprisingly compliant (see the discussion in Section 4.2).

On the other hand, market prices and quantities in the SELLER + BUYER treatment are very close to those in the AUTOMATIC treatment (two-sided WRS, $p > 0.3$ for all three tests). This is confirmed by the pooled OLS models 1, 3 and 5 (Wald tests for the equivalence of coefficients, $p > 0.05$ for all three tests).

To summarize the key results from the SELLER ONLY treatment, the market clearing price when there is a tax that can be evaded lies between the prices in the cases when there is no tax, and the case when there is a tax that is automatically collected. Correspondingly, the quantity traded lies between the outcomes in these two treatments. The differences from both of the polar cases (for both the price and the quantity) are statistically significant. Costless third-party reporting (in the and SELLER + BUYER treatment) is an effective deterrence mechanism, and the market clearing price and quantity return to the levels observed when evasion is not possible. We return to these results in the subsection on reporting behavior, as well as the Discussion section.

Finally, when reporting is costly to the buyers, the picture emerging is less clear. First, both mean and median prices are lower in the SELLER + BUYERC treatment than in the SELLER + BUYER treatment and in the AUTOMATIC treatment (two-sided WRS, $p < 0.05$ for all four tests), but not significantly different than in the NO TAX treatment and in the SELLER ONLY treatment (two-sided WRS, $p > 0.05$ for all four tests). Second, the mean quantity traded per period in

[18] We thus qualitatively confirm the corresponding result in Doerrenberg and Duncan (2017).

the SELLER + BUYERC treatment is statistically indistinguishable from the mean quantity in the AUTOMATIC treatment (a two-sided WRS, $p = 0.095$), from the mean quantity in the SELLER ONLY treatment (a two-sided WRS, $p = 0.18$), and from the mean quantity in the SELLER + BUYER treatment (a two-sided WRS, $p = 0.55$). By contrast, the mean quantity traded per period in the SELLER + BUYERC treatment is significantly lower than the mean quantity traded per period in the NO TAX treatment (a two-sided WRS, $p = 0.024$).[19]

4.2 Reporting Behavior

Column 6 of Table 1 shows compliance rates by treatment, defined as the ratio of the number of units sellers reported to the total number of trades. Figure 8 displays the evolution of the compliance rate by treatment. Four main observations stand out.

First, tax compliance crucially depends on the details of the reporting mechanism. A Kruskal-Wallis test confirms that compliance rates are statistically significantly different between treatments ($p < 0.01$). Furthermore, three Wilcoxon rank-sum tests indicate that this difference is due to reporting rates being significantly different in all pairwise comparisons ($p < 0.05$ for all three tests). Second, the compliance rate of 30% in the SELLER ONLY treatment is surprisingly high, given that reporting zero units sold is optimal for a selfish and risk neutral seller.[20] Third, the compliance rate is very high, over 80%, when costless third-party reporting is introduced, indicating that the availability of third-party information effectively deters tax evasion. Fourth, the compliance rate decreases yet stays at a relatively high level, 68%, even after reporting is made costly to the buyers.

The second observation immediately begs the question of why sellers are so compliant in the SELLER ONLY treatment. With our parametrization, risk aversion alone cannot explain the high compliance by the sellers. Using a constant relative risk aversion (CRRA) utility function $u(x) = x^{1-\rho}/(1-\rho)$ and assuming that the subjects are 'narrow bracketing' with respect to wealth, we can calculate what the relative risk aversion parameter ρ would have to be in order for truthful reporting to be optimal for sellers. As subjects earn a show-up fee of 5 EUR, which translates to 2500 ECU, the lower bound for a seller's wealth when making the reporting decision is 2500 ECU. For this wealth, not reporting is optimal for all plausible values of ρ (i.e. for values below 20).

To investigate the determinants of the reporting decision more closely, we examine reporting behavior of sellers at the individual level. Panel A in Table 2 depicts the relative frequencies of

[19]Restricting the sample to periods 11–25, mean quantities per period in SELLER + BUYERC and in AUTOMATIC are statistically different (a two-sided WRS, $p = 0.036$).

[20]For comparison, Doerrenberg and Duncan (2017) observe a compliance rate of about 7% in a closely related treatment.

different types of reports by treatment, using reports at individual-period level as units of observation.[21] The first row in Panel A reveals that roughly 51% of all reports are optimal (i.e. reported zero sold units), while close to 19% are truthful in treatment SELLER ONLY. In the SELLER + BUYER treatment, the relative frequency of optimal reports was 43.5%, a statistically significant decrease compared to the SELLER ONLY treatment (χ^2-test, $p < 0.01$). By contrast, a statistically significantly higher share of all reports (about 72%) are truthful in the SELLER + BUYER treatment compared to the SELLER ONLY treatment (χ^2-test, $p < 0.01$). When reporting is costly to the buyers, both the share of optimal and the share of truthful reports drop compared to the case where reporting is costless (to 27.2%. and to 51.7%, respectively; χ^2-tests, $p < 0.01$). Apparently, sellers correctly expect buyers to report less often in the SELLER + BUYERC treatment, and therefore react by being less truthful. At the same time, the strategic environment becomes more complicated, making it more difficult to figure out what the optimal report would be, which explains the low rate of optimal reports.

In Panel B in Table 2 we turn our attention to how consistent sellers are in their reporting behavior. We categorize sellers according to whether their reports are consistently optimal, consistently truthful or show no consistent pattern across all 25 rounds. That is, we employ a somewhat demanding definition of consistency, requiring the same reporting choice across all rounds. In the SELLER ONLY treatment, 30% of sellers consistently report optimally (i.e. 0 units), while the majority of sellers are not consistent in this sense in their reporting behavior. The same is true in the SELLER + BUYER and in the SELLER + BUYERC treatments: almost everyone in these two treatments show no pattern in their reporting behavior.

We emphasize, however, that we cannot disentangle the various motives that might underlie this observed inconsistent reporting behavior. For example, it might be the case that sellers simply have incorrect expectations regarding the extent to which buyers report their trades, and that those

[21] In the SELLER ONLY treatment, for a risk neutral and selfish seller, an optimal report is one with zero units reported, regardless of the actual number of units sold. In the SELLER + BUYER and the SELLER + BUYERC treatments, the optimal report depends on the number of units actually sold. More specifically, we first calculate the reporting rate of buyers as the ratio of the total number of units buyers reported to the total number of units traded. We then assume that each buyer reports each unit bought independently with a probability equal to the reporting rate. In other words, we assume that from the perspective of a seller, the number of units buyers report having bought from her is binomial distributed with the success (reporting) probability equal to the buyer reporting rate (81.1% in the SELLER + BUYER treatment, and 40.1% in the SELLER + BUYERC treatment). For a given number of sold units, we can thus calculate the expected return for different reports. These calculations reveal that in the SELLER + BUYER treatment, truthful reporting is optimal for a seller who sold one or two units, while reporting two units is optimal for a seller who sold three units, and reporting three units is optimal for a seller who sold four units. In the SELLER + BUYERC treatment, it is optimal to report one unit less than the number of sales when the number of sold units is one, two or three. With four units sold, reporting two units is optimal. We exclude individual-periods with zero units sold, as sellers are then constrained to report zero units.

who are classified as showing no pattern in their reporting behavior actually optimize with respect to their incorrect beliefs. By contrast, truthful reporting does not involve complicated calculations, and thus someone who prefers to be truthful in all circumstances should easily be able to do so. Despite this fact, we observe very few sellers who are consistently reporting their trades truthfully: only one seller in SELLER + BUYER and two sellers in SELLER + BUYERC reported their trades truthfully in all 25 periods. We thus conclude that unconditional *intrinsic* motives for full tax compliance are not very prominent among our subjects.

Table 2: Relative frequency of reporting by type

Panel A: Individual-period level				
	Optimal	Optimal and truthful	Suboptimal and truthful	Suboptimal and non-truthful
SELLER ONLY	50.7%	-	18.8%	30.5%
SELLER + BUYER	10.5%	33.0%	39.1%	17.4%
SELLER + BUYERC	27.2%	-	51.7%	21.1%
Panel B: Individual level				
	Always optimal	Always optimal and truthful	Always truthful	No pattern
SELLER ONLY	30%	-	0%	70%
SELLER + BUYER	0%	0%	3.3%	96.7%
SELLER + BUYERC	0%	-	13.3%	86.7%

Notes: (i) Optimality is defined in terms of self-interest and risk neutrality. In SELLER ONLY reporting 0 units is optimal. In SELLER + BUYER and in SELLER + BUYERC optimality depends on the number of units sold.
(ii) Panel A indicates the relative frequencies of different report types, treating reports at individual-period level as units of observation.
(iii) Panel B depicts the relative frequencies of different types of reporting profiles, treating individuals as units of observation.

Note, however, that there is a caveat to results obtained using data from Table 2: observations, on which Table 2 are based, are not independent of each other. This violates the assumptions underlying the χ^2-test. Therefore, we also investigate reporting behavior by estimating several panel regression models, in which we cluster standard errors at the market level, allowing observations within a given market to be correlated. More specifically, Table 8 shows results from various pooled panel regressions with individual-period reporting rate as the dependent variable, and individual characteristics as explanatory variables. Model 1 uses dummies for the SELLER + BUYER treatment and the SELLER + BUYERC treatment as the only independent variables. The model confirms that sellers are more compliant in the SELLER + BUYER treatment and in the SELLER + BUYERC treatment than in the SELLER ONLY treatment. Furthermore, sellers are also more compliant in the SELLER + BUYER treatment than in the SELLER + BUYERC treatment (a Wald test for equivalence of coefficients, $p = 0.017$). In Model 2, we add period fixed effects, and the results are largely unchanged. Models 3, 4 and 5 examine the association between the reporting rate and tax morale, risk attitude and generalized trust, respectively. Each of the associations has the expected sign, but none of them are statistically significant.

In Table 9 we examine the impact of various past events on the seller reporting rate. More specifically, in Model 6, in addition to treatment dummies, we add a dummy indicating whether the seller was fined in the previous period. The treatment effects are unchanged, and there is a small, weakly significant negative association between the reporting rate and being fined in the previous period. Model 7 in column three shows that just being audited – irrespective of whether the audit leads to a fine or not – is not associated with reporting. Models 8 and 9 in columns four and five, respectively, examine the relationship between historical profits and the reporting rate. The two models show that both previous period profits and cumulative profits are negatively correlated with the seller reporting rate in the SELLER ONLY treatment. The reason may be that a high previous period (cumulative) profit captures a low previous period reporting rate, which in turn is likely to be positively associated with a low reporting rate in the current period. However, in the SELLER + BUYER treatment and in the SELLER + BUYERC treatment previous period (cumulative) profits do not significantly affect the reporting rate in current period, presumably because a higher risk of an audit drives behavior in these two treatments (Models 8 and 9). Model 10 confirms that reporting behavior is strongly correlated between successive periods, and that this correlation is especially strong in the SELLER ONLY treatment.

Turning then to reporting behavior of buyers, we find that buyers report 81.1% of traded units in the SELLER + BUYER treatment. This share drops to 40.1% when reporting is made costly in the SELLER + BUYERC treatment (a two-sided Wilcoxon rank-sum test, $p = 0.024$). We further examine the determinants of buyer reporting by estimating various pooled panel regressions. The results are shown in Table 10. In all models, the dependent variable is reporting rate, defined to be the ratio between units a buyer reports over the number of trades she makes in a given period. Model 11 in the second column in Table 10 confirms that buyer reporting is indeed much more prevalent in the SELLER + BUYER treatment than in the SELLER + BUYERC treatment. Since it seems conceivable that high tax morale is related to one's reporting behavior even when one is not liable to pay the tax, we estimate a model including a measure of tax morale and its interaction with the dummy indicating SELLER + BUYERC treatment status (column three in Table 10). To do this, we introduce a binary variable that takes value 1 if the subject reported cheating on taxes never being acceptable. Interestingly, the coefficient on the binary tax morale variable is not statistically significantly different from zero, nor is the coefficient on the interaction. However, sum of the two coefficients is weakly significant (a Wald test, $p = 0.086$). This finding suggests that while tax morale does not matter for buyer reporting when reporting is costless, buyers with higher tax morale report weakly greater fraction of their trades than buyers with lower tax morale when reporting is costly.

4.3 Tax Incidence

The tax incidence results are displayed in Table 3. Column 1 shows the nominal incidence, defined as the mean share of the 40 ECU per-unit tax that is shifted onto buyers. The mean market clearing price in AUTOMATIC is 18.4 ECU higher than the mean price in NO TAX. This implies that buyers bear 46% of the tax burden, which is consistent with our prediction that the burden is shared equally among sellers and buyers. In SELLER ONLY buyers bear 27% of the nominal tax burden. The fraction increases to 40.25% in the SELLER + BUYER treatment, but drops again to 17% in the SELLER + BUYERC treatment.

Results on incidence of the expected effective per-unit tax are shown in the second column of Table 3. In AUTOMATIC, the expected effective per-unit tax faced by a seller is equal to the nominal per-unit tax, and hence the incidence is the same as for the nominal per-unit tax (46%). By contrast, in SELLER ONLY, in SELLER + BUYER and in SELLER + BUYERC tax evasion is possible, and therefore the expected effective tax per unit may differ from the nominal per-unit tax if the seller underreports units sold. More specifically, in the SELLER ONLY treatment, the expected tax liability of a seller who reports r_i units after having sold s_i units is given by

$$\gamma(\tau s_i + f(s_i - r_i)) + (1 - \gamma)\tau r_i,$$

in which γ denotes the audit probability, τ is the nominal per-unit tax and f is the fine. The expected effective per-unit tax for the seller is then obtained by dividing the expected tax liability by the number of units sold by the seller:

$$\tau^e = [\gamma(\tau s_i + f(s_i - r_i)) + (1 - \gamma)\tau r_i)]/s_i.$$

With our parametrization (i.e. $\gamma = 0.1, \tau = 40$, and $f = 40$), the expected effective per-unit tax becomes $\tau^e = 8 + 32(r_i/s_i)$. It is easy to see that τ^e is decreasing in the extent of underreporting. Given the observed mean compliance rate of 29.8% in the SELLER ONLY treatment, the estimated expected effective tax is 17.54 ECU, out of which buyers bear 10.8 ECU. Hence the share of the expected effective tax borne by buyers is 62%.[22] Compared to the AUTOMATIC treatment, sellers thus shift a higher share of the effective tax onto buyers in the SELLER ONLY treatment. We believe that the sellers keep the majority of the evasion rent to themselves in order to get compensation for the risks caused by evasion.

[22] In a similar set-up to our SELLER ONLY treatment, Doerrenberg and Duncan (2017) find that sellers with evasion opportunities shift the expected effective taxes fully onto buyers.

Table 3: Incidence of nominal and expected effective tax

	Incidence of nominal tax	Incidence of effective tax
AUTOMATIC	46%	46%
SELLER ONLY	27%	62%
SELLER + BUYER	40%	
SELLER + BUYERC	17%	

Notes: (i) Incidence of nominal tax is the mean share of the 40 ECU per-unit tax that is shifted onto buyers.
(ii) Incidence of effective tax is the mean share of the expected effective per-unit tax that is shifted onto buyers.

4.4 Efficiency

Table 4 shows a comparison of relative efficiencies across treatments. To calculate relative efficiencies, we first define the efficiency of a treatment as the total sum of earnings, collected taxes and fines (when applicable) in the treatment. By contrast, in the SELLER + BUYERC treatment, earnings are displayed net of the buyer reporting costs, which captures the fact that these costs reflect real efficiency-decreasing costs (e.g. time spent on reporting) and not just transfers. To get the relative efficiency of a treatment, we divide the efficiency in that treatment by the maximum efficiency observed across treatments. The comparison shows that efficiency is highest in the NO TAX treatment, and that the efficiency loss varies between 2.5% and 6.4% of the maximum efficiency.

Table 4: Earnings and Relative Efficiency

	Earnings	Taxes collected	Fines	Relative efficiency
NO TAX	35,302	-	-	100%
AUTOMATIC	19,790	13,260	-	93.6%
SELLER ONLY	28,311	5,320	793	97.5%
SELLER + BUYER	19,128	12,613	1,433	94.0%
SELLER + BUYERC	19,855	11,413	1,800	93.7%

Notes: (i) Earnings, taxes collected and fines are averages per session and expressed in ECUs.

5 Discussion

Our experiment provides strong evidence that the reporting institution matters for tax compliance. The (positive) correlation across different income tax categories between third-party reporting and compliance has been noted for example by Slemrod (2007). Kleven et al. (2011) found a similar correlation and presented a theory building on the Allingham and Sandmo (1972) model to explain the finding. The importance of third-party information for deterrence has also been highlighted in natural experimental settings e.g. by Pomeranz (2015) and Naritomi (2018). We provide evidence from a controlled randomized laboratory experiment that complements these field studies, and lends further support to their findings.

With a laboratory experiment we are however able to analyze a number of interesting further questions that are harder to study with field data. While the above studies together argue that third-party information has an effect on income tax reporting, exactly how does third-party information affect behavior, and why? The different types of effects of stricter enforcement, as well as the mechanisms behind the effectiveness of third-party information, are difficult to disentangle in a field setting.

First, the real effects (i.e. effects on market outcomes) of tax enforcement are of particular interest, and are in general difficult to disentangle with field data. Given the uncertainty inherent in a tax evasion game, and the further strategic element introduced by double reporting, the market outcomes and tax incidence with an automatically collected tax, and a strictly enforced tax with an evasion opportunity, are not necessarily identical. However, we do find that market efficiency (measured by the sum of income + taxes collected + fines) is at similar levels in the two cases in our experiment.

Second, a novel feature of our analysis is the focus on buyer reporting. Discussions of third-party reporting typically take the existence (or the lack) of the required information as given – but for such information to exist in the first place, the third party must have incentives to provide it. In our experiment, if buyers take into account the indirect effect of tax evasion on prices, they should not supply the tax authority with information that enables stricter tax enforcement. Nevertheless, we find that introducing double reporting by buyers has a very strong disciplining effect on evasion. Perhaps somewhat surprisingly, buyers and sellers are thus unable to (tacitly) collude on an outcome with a lower level of reporting, even though it would be in their joint (monetary) interest. Making reporting more costly potentially facilitates such collusion, and indeed, buyers are less likely to report when reporting comes with a cost. However, compliance of sellers remains at a surprisingly high level.

Hence taxpayer awareness of the mere *possibility* of the existence of third-party information appears a fairly effective deterrent on tax evasion. Tax administrators have traditionally been quite secretive about the type of information used in tax enforcement. Our findings point to a tentative policy implication, namely that it may be a good idea to make known at least the *types* of third-party information available to (if not the exact information held by) the tax authority. (Notifying taxpayers of the exact information held by the tax authority may in some cases backfire, a result found by Slemrod et al. (2017) and Carrillo et al. (2017) in a natural experiment setting. Okat (2016) shows in a theory model that it may be optimal not to use all available information in tax enforcement, in the sense that randomness in audit rules may be optimal to prevent learning by evaders; see Alm et al. (1992) for a related argument plus caveats.)[23] Naturally, providing information should be made as cheap as possible to the third party. Costly information provision has two downsides: the reporting cost constitutes an efficiency loss, and may facilitate collusion between the buyer and seller to jointly evade taxes.

While laboratory experiments offer a relatively cost-effective and feasible way of examining questions that are not easily amenable to studies in the naturally occurring world, a question remains concerning the extent to which the lessons learned in the laboratory using student subjects can be generalized to the "real world". This problem concerning the external validity of laboratory experiments has received a lot of attention recently (see, e.g., Levitt and List (2007); Falk and Heckman (2009); Cappelen et al. (2015); Snowberg and Yariv (2018)). Much of this discussion revolves around two questions: Do student subjects' behavior differ from the behavior of non-students? Is a laboratory environment too abstract and artificial to induce behavior comparable to a naturally occuring environment? We briefly address these concerns from the point of view of the present experiment.[24]

Many studies compare the behavior of student subjects and non-student subjects in various experimental tasks. These studies have found that university students are less generous than representative samples in Switzerland (Falk et al., 2013), in Norway (Cappelen et al., 2015), and in the U.S. (Snowberg and Yariv, 2018).[25] Moreover, Snowberg and Yariv (2018) find that correlations between behaviors across various experimental tasks are largely similar between students and a representative sample of the U.S. population. A related observation concerning the similarity of observed treatment effects is made by Alm et al. (2015) when comparing students to non-students in tax compliance experiments. These studies indicate that the compliance rates in the present experiment might underestimate the compliance rates of non-students. More importantly, since we

[23] A few previous lab experiments have discussed the impact of ambiguity – not having precise knowledge of objective audit probabilities – on tax evasion, and according to a recent review of tax experiments those papers found mixed results (Malézieux, 2018).

[24] Alm and Jacobson (2007) provide an extensive overview of literature using laboratory experiments in public economics.

[25] Choo et al. (2016) compare students to non-students in a tax compliance experiment. They find that students are more self-interested than non-students, although this finding may be confounded by differences in the experimental design between these two groups of subjects.

are not primarily interested in the *levels* of the variables measured in the experiment but rather in the *changes* in their levels from one treatment to another (i.e. in treatment effects), the observations that correlations between various behaviors are largely similar between students and non-students is reassuring.

A potentially more important concern is that a laboratory may be too artificial an environment to study such a loaded concept as tax evasion. We offer two remarks on this issue. First, Alm et al. (2015) compared the distribution of the compliance rate in a laboratory tax compliance experiment to the distribution of the compliance rate of real taxpayers in random audits conducted under the National Research Program of the U.S. Internal Revenue Service. When the audit probabilities in the experiment were similar to those occuring in the field, they observed strikingly similar mean compliance rates and compliance rate distributions in the experiment and in the field.[26]

Second, we believe that laboratory experiments on a particular research question should be seen as providing complementary evidence alongside other approaches, as discussed above. Furthermore, the (mean) prices and quantities we observe in the NO TAX and the AUTOMATIC treatments are very close to those predicted by theory. Moreover, to reinforce the correspondence between the lab environment and the naturally occurring world, we frame the experiment as a market exchange with real reporting decisions by using words such as "buyer", "seller", "tax", "fine" and "audit". Finally, we actually remit the collected taxes to the Finnish State Treasury, and make sure that the subjects know this from the start. Taken together, these arguments suggest that the qualitative impact on prices and quantities observed in the SELLER ONLY, the SELLER + BUYER and the SELLER + BUYERC treatments might well generalize to the real world.

[26] Snowberg and Yariv (2018) provide evidence that student subjects' behavior in experimental tasks does not depend on whether the tasks are completed in the laboratory or outside the laboratory.

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Appendix A: Tables

Table 5: Demand and supply schedules

	Buyer				Seller			
Subject	Value 1	Value 2	Value 3	Value 4	Cost 1	Cost 2	Cost 3	Cost 4
1	232	207	182	177	88	113	138	143
2	212	202	192	152	108	118	128	168
3	227	222	167	157	93	98	153	163
4	242	197	172	147	78	123	148	173
5	237	217	187	162	83	103	133	158

Notes: Costs and values were randomized into sets of four. The sets of four costs/values were randomly assigned to traders at the beginning of each experimental session.

Table 6: Summary statistics of demographic variables

	Gender	Age	Finnish	Tax morale	Risk attitude	Generalized trust
NO TAX						
Mean	0.73	29.17	1	0.28	4.73	6.07
St. Dev.	-	9.46	-	-	2.09	2.31
Number of Subjects	60	60	60	60	60	60
AUTOMATIC						
Mean	0.75	26.77	0.93	0.62	5.02	6.33
St. Dev.	-	7.01	-	-	1.99	2.10
N. of Subjects	60	60	60	60	60	60
SELLER ONLY						
Mean	0.75	28.02	1	0.62	5.37	5.85
St. Dev.	-	8.09	-	-	2.45	2.44
N. of Subjects	60	60	60	60	60	60
SELLER + BUYER						
Mean	0.62	27.57	0.97	0.57	5.18	6.87
St. Dev.	-	5.58	-	-	2.24	2.17
N. of Subjects	60	60	60	60	60	60
SELLER + BUYERC						
Mean	0.70	29.27	0.90	0.50	5.30	7.10
St. Dev.	-	8.19	-	-	2.10	2.01
N. of Subjects	30	30	30	30	30	30
P-value	0.46	0.50	0.035	0.001	0.17	0.19

Notes: Reported are the mean characteristics of the four treatment groups. Gender is an indicator that is equal to 1 if the subject is female, Finnish is an indicator that is equal to 1 if the subject's native language is Finnish. Tax morale is an indicator that is equal to 1 if the subject reported that cheating on taxes is never acceptable. Risk attitude is the subject's reported willingness to take risks (1 ="not at all willing" to 10 ="very willing"), and generalized trust is the subject's reported propensity to trust other people (1 = "one can never be too careful with other people" to 10 = "most people can be trusted"). P-values are for χ^2 test, apart from Age for which the p-value is for Kruskal-Wallis test. For each test, the null hypothesis is that there are no differences between the five treatment groups.

Table 7: Pooled Panel OLS Estimates

	Mean Price	Mean Price	Median Price	Median Price	Quantity	Quantity
CONSTANT	157.88***	154.83***	157.72***	154.10***	17.39***	17.44***
	(1.93)	(3.18)	(1.95)	(3.02)	(0.12)	(0.16)
AUTOMATIC	18.61***	18.12***	19.45***	19.38***	-4.13***	-3.13***
	(2.24)	(4.56)	(2.27)	(4.02)	(0.19)	(0.38)
SELLER ONLY	11.02***	13.76***	11.03***	13.94***	-2.17***	-1.83***
	(2.69)	(3.93)	(2.59)	(3.75)	(0.40)	(0.24)
SELLER + BUYER	16.36***	11.14**	16.11***	11.27***	-3.99***	-3.41***
	(2.54)	(4.61)	(2.44)	(4.22)	(0.33)	(0.37)
SELLER + BUYERC	7.17**	-0.40	7.80***	2.63***	-3.21***	-2.85***
	(2.78)	(4.61)	(2.44)	(4.22)	(0.33)	(0.37)
PERIOD		0.23**		0.28***		-0.0038
		(0.11)		(0.098)		(0.012)
AUTOMATIC * PERIOD		0.038		0.0056		-0.077***
		(0.23)		(0.20)		(0.022)
SELLER ONLY * PERIOD		-0.21		-0.22		-0.026
		(0.18)		(0.18)		(0.030)
SELLER + BUYER * PERIOD		0.40**		0.37**		-0.044***
		(0.18)		(0.16)		(0.019)
SELLER + BUYERC * PERIOD		0.58**		0.40**		-0.028
		(0.18)		(0.16)		(0.019)
R^2	0.53	0.64	0.54	0.64	0.67	0.70
Observations	675	675	675	675	675	675
Clusters	27	27	27	27	27	27

Notes: (i) Dependent variable mean (median) price is the mean (median) price in a given market period. Quantity is the number of units sold in a given market period. The data consists of all completed transactions from periods 1 to 25. The constant term captures the estimate for the NO TAX treatment. (ii) Robust standard errors clustered at the market level are in parentheses (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$).

Table 8: Effect of treatment on reporting rate

	Model 1	Model 2	Model 3	Model 4	Model 5
CONSTANT	0.32***	0.26***	0.33***	0.47	0.40**
	(0.031)	(0.040)	(0.045)	(0.26)	(0.13)
SELLER + BUYER	0.51***	0.51***	0.51***	0.43	0.36**
	(0.042)	(0.042)	(0.063)	(0.26)	(0.14)
SELLER + BUYERC	0.39***	0.39***	0.36***	0.45	0.22
	(0.054)	(0.054)	(0.049)	(0.28)	(0.18)
TAX MORALE (BIN.)			-0.010		
			(0.12)		
SELLER + BUYER * TAX MORALE (BIN.)			0.0057		
			(0.14)		
SELLER + BUYERC * TAX MORALE (BIN.)			0.11		
			(0.18)		
RISK ATTITUDE				-0.026	
				(0.040)	
SELLER + BUYER * RISK ATT.				0.013	
				(0.042)	
SELLER + BUYERC * RISK ATT.				-0.015	
				(0.045)	
GENERALIZED TRUST					-0.014
					0.021
SELLER + BUYER * GEN. TRUST					0.025
					(0.022)
SELLER + BUYERC * GEN. TRUST					0.028
					(0.030)
PERIOD FIXED EFFECTS	No	Yes	No	No	No
R^2	0.31	0.31	0.31	0.33	0.32
Observations	1874	1874	1874	1874	1874

Notes: (i) Robust standard errors clustered at the market level are in parentheses (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). (ii) Dependent variable Reporting rate is the ratio of the number of units reported to the total number of units sold of a seller in a given market period. Used are all periods from 1 to 25. One individual-period observation is excluded due to zero units sold. The constant term captures the estimate for the SELLER ONLY treatment. Tax morale (bin.) is a binary variable that takes value 1 if the seller reports cheating on taxes never being acceptable.

Table 9: Effect of past events on reporting rate

	Model 6	Model 7	Model 8	Model 9	Model 10
CONSTANT	0.33***	0.32***	0.59***	0.56***	0.094***
	(0.035)	(0.034)	(0.049)	(0.033)	(0.013)
SELLER + BUYER	0.53***	0.53***	0.21***	0.21***	0.48***
	(0.040)	(0.040)	(0.063)	(0.047)	(0.053)
SELLER + BUYERC	0.40***	0.40***	0.12*	0.15**	0.28**
	(0.061)	(0.059)	(0.062)	(0.052)	(0.079)
FINED IN T-1	-0.0012*				
	(0.00051)				
SELLER + BUYER * FINED IN T-1	-0.00059				
	(0.00069)				
SELLER + BUYERC * FINED IN T-1	-0.00017				
	(0.00094)				
AUDITED IN T-1		-0.0035			
		(0.051)			
SELLER + BUYER * AUDITED IN T-1		-0.065			
		(0.058)			
SELLER + BUYERC * AUDITED IN T-1		-0.050			
		(0.066)			
PROFIT IN T-1			-0.0023***		
			(0.00032)		
SELLER + BUYER * PROFIT IN T-1			0.0031***		
			(0.00044)		
SELLER + BUYERC * PROFIT IN T-1			0.0024***		
			(0.00044)		
CUMULATIVE PROFIT IN T-1				-0.00017***	
				(0.000016)	
SELLER + BUYER * CUMULATIVE PROFIT IN T-1				0.00029***	
				(0.000029)	
SELLER + BUYERC * CUMULATIVE PROFIT IN T-1				0.00018*	
				(0.000070)	
REPORTING RATE IN T-1					0.70***
					(0.047)
SELLER + BUYER * REPORTING RATE IN T-1					-0.38***
					(0.063)
SELLER + BUYERC * REPORTING RATE IN T-1					-0.22
					(0.14)
PERIOD FIXED EFFECTS	No	No	No	No	No
R^2	0.33	0,32	0.39	0.41	0.54
Observations	1799	1799	1799	1799	1799

Notes: (i) Robust standard errors clustered at the market level are in parentheses (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). (ii) Dependent variable Reporting rate is the ratio of the number of units reported to the total number of units sold of a seller in a given market period. Used are all periods from 1 to 25. One individual-period observation is excluded due to zero units sold. The constant term captures the estimate for the SELLER ONLY treatment.

Table 10: Effect of treatment on buyer reporting rate

	Model 11	Model 12	Model 13	Model 14
CONSTANT	0.81***	0.79***	0.81***	0.48***
	(0.035)	(0.048)	(0.12)	(0.082)
SELLER + BUYERC	-0.41***	-0.58**	-0.58*	0.50
	(0.059)	(0.094)	(0.26)	(0.26)
TAX MORALE (BIN.)		0.031		
		(0.075)		
SELLER + BUYERC*TAX MORALE (BIN.)		0.22		
		(0.16)		
RISK ATTITUDE			0.00065	
			(0.017)	
SELLER + BUYERC*RISK ATT.			0.030	
			(0.041)	
GENERALIZED TRUST				0.048**
				(0.016)
SELLER + BUYERC*GEN. TRUST				-0.12**
				(0.029)
PERIOD FIXED EFFECTS	No	No	No	No
R^2	0.19	0.21	0.20	0.26
Observations	1122	1122	1122	1122

Notes: (i) Robust standard errors clustered at the market level are in parentheses ($^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$). (ii) Dependent variable Reporting rate is the ratio of the number of units reported to the total number of units bought by a seller in a given market period. Used are all periods from 1 to 25. Three individual-period observations are excluded due to zero units bought. The constant term captures the estimate for the SELLER + BUYER treatment. Tax morale (bin.) is a binary variable that takes value 1 if the seller reports cheating on taxes never being acceptable.

Appendix B: Figures

Figure 1: Supply and demand

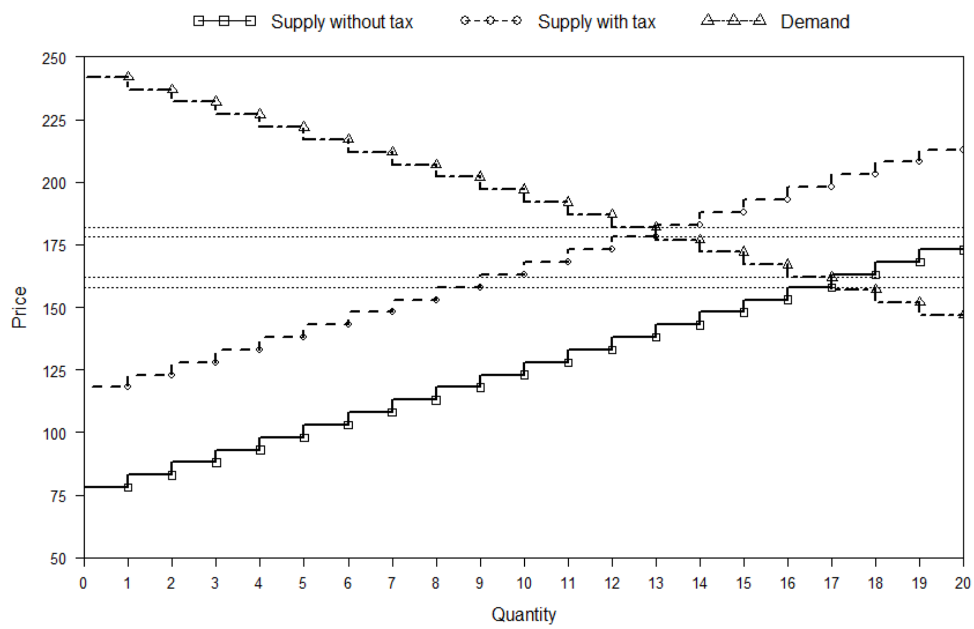


Figure 2: Mean Price by Treatment

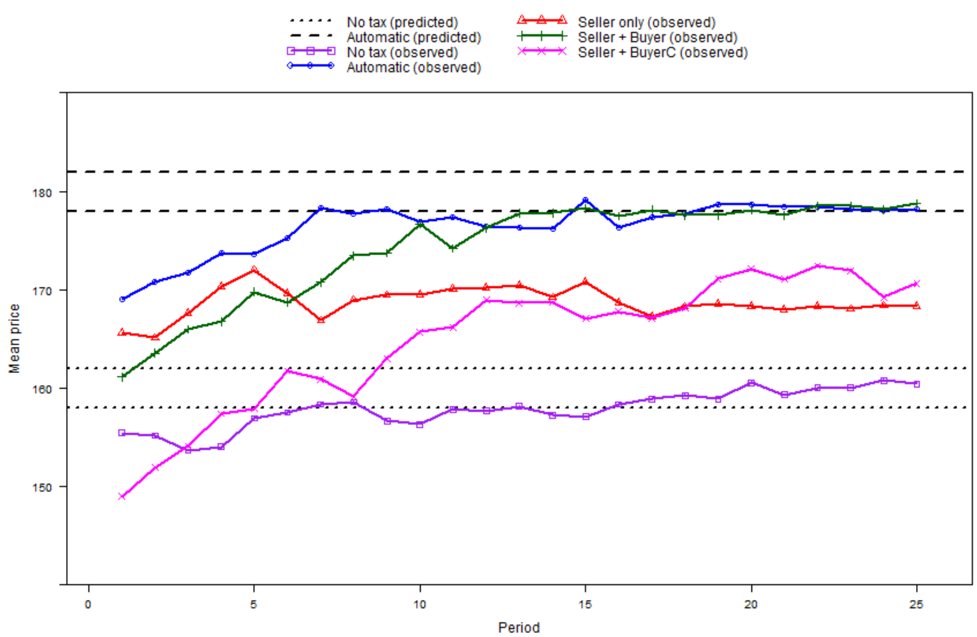


Figure 3: Mean Price by Treatment and Market

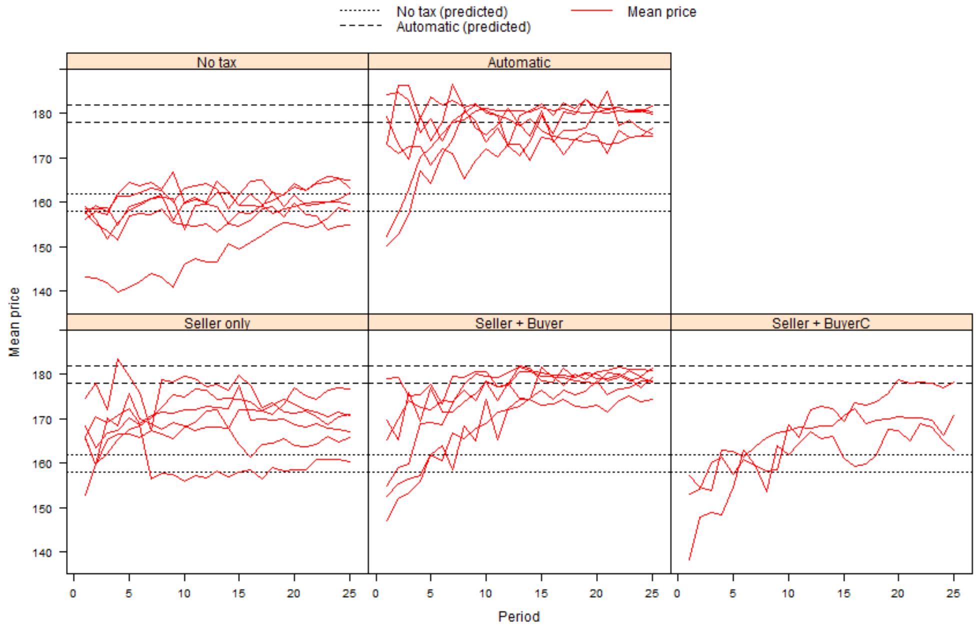


Figure 4: Median Price by Treatment

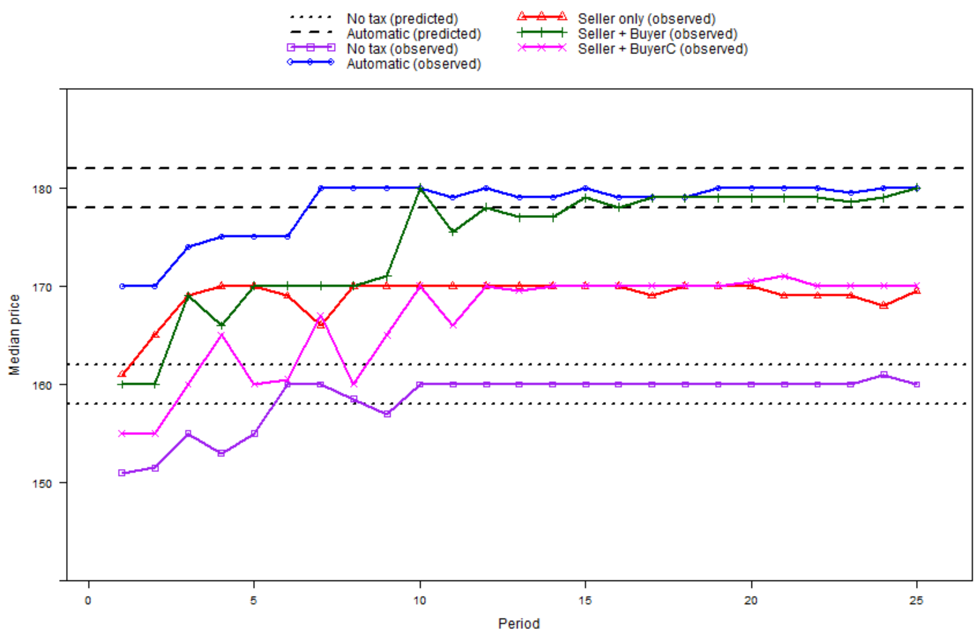


Figure 5: Median Price by Treatment and Market

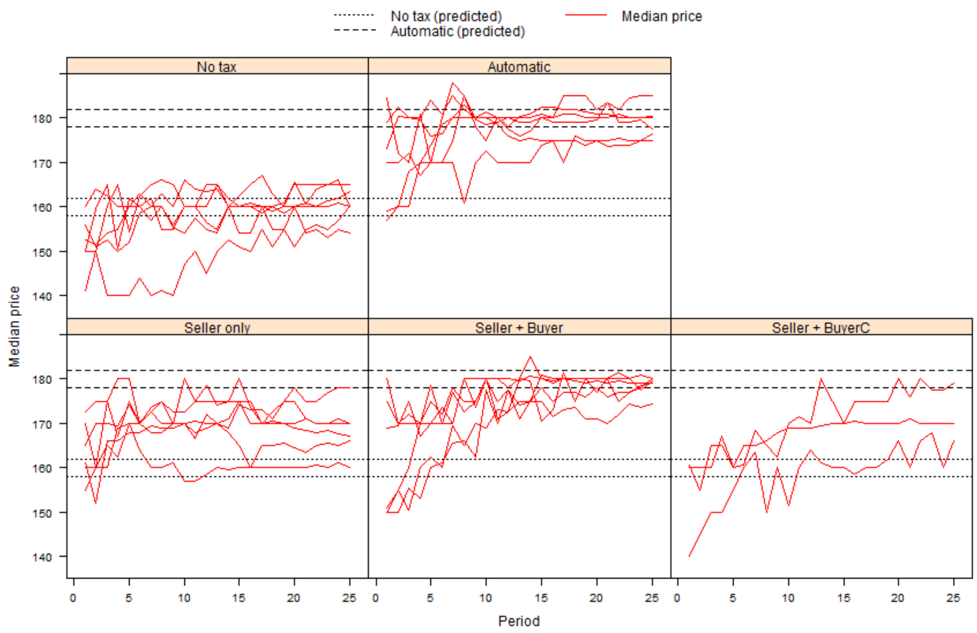


Figure 6: Mean Quantity by Treatment

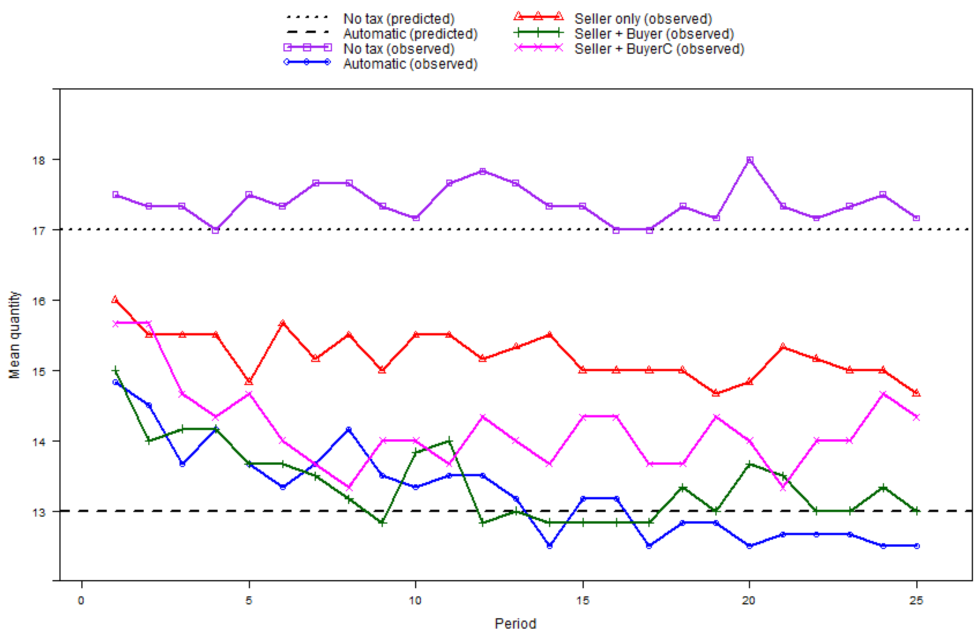


Figure 7: Quantity by Treatment and Market

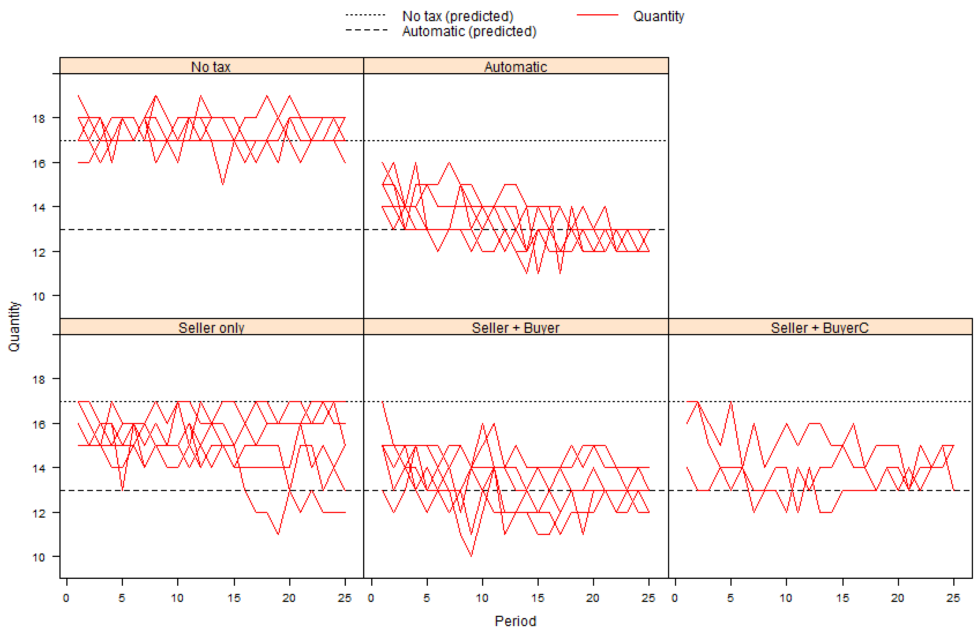


Figure 8: Reporting Rate by Treatment

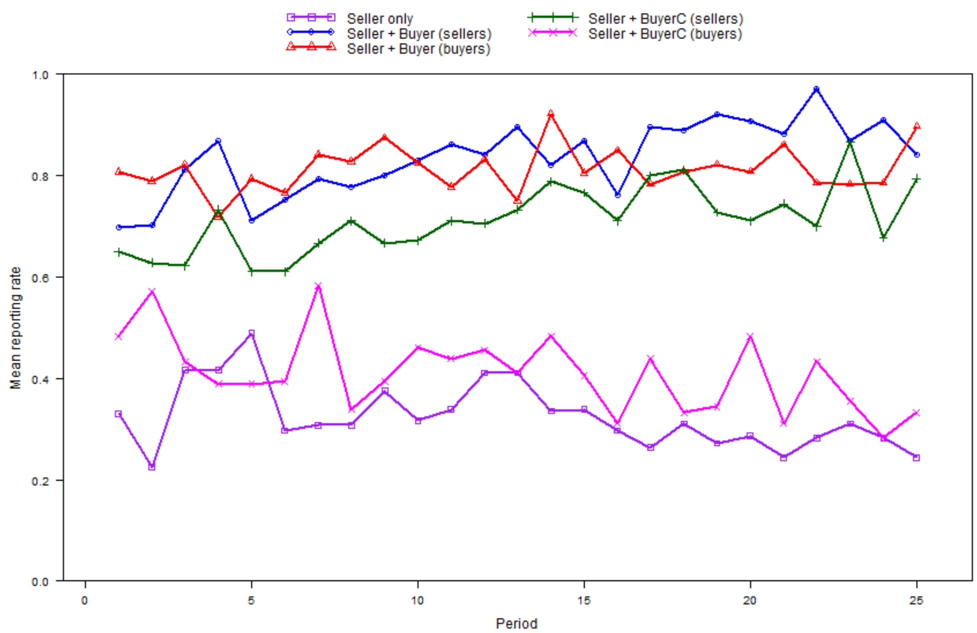
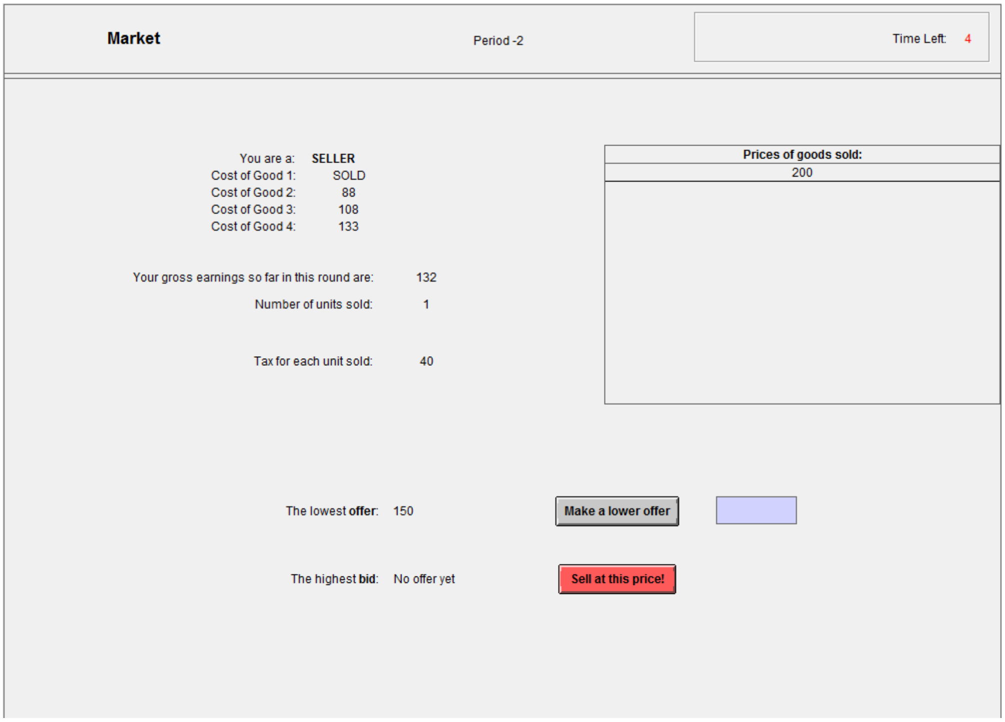


Figure 9: Market Trading Screen



Appendix C: Instructions

Translated instructions for the SELLER + BUYER *treatment*

General instructions

This session is part of an experiment on decision making. If you follow the instructions and make good decisions, you can earn money. Your earnings will be paid to you privately in cash. How much you earn depends on your own decisions and the decisions of other participants.
There are 10 persons in this room taking part in this experimental session. You are not allowed to communicate with others during the experiment. We kindly ask that you read these instructions very carefully. If you have questions, please raise your hand and the experimenter will come to you and answer your questions. Your decisions throughout the experiment, and your earnings will be treated confidentially.
You can earn money in this experiment. Your earnings depend on your and other participants' decisions and possibly chance. During the experiment, your earnings are calculated in an experimental currency called ECU ("Experimental Currency Unit"). At the end of the experiment your earnings will be converted to EURO so that 500 ECU = 1 EUR, and paid to you in private along with a 5 EUR participation fee.

The experiment

Roles

At the beginning of the experiment, the computer will randomly assign 5 participants to the role of "seller" and 5 participants to the role of "buyer". Therefore, you will either be a buyer or a seller. Your role as buyer or seller will remain the same throughout the experiment. You will only know your own role, and not the roles of others.

Overview

The experiment consists of 3 practice periods and 25 actual periods. Only the 25 actual periods affect your earnings. At the beginning of a decision period there is a market phase, during which sellers and buyers trade a fictitious good in a market place. As a buyer, you can buy units of the fictitious good, and as a seller, you can sell units.

You can earn ECU by trading in the market place, and your earnings depend on your, and other participants' decisions. Sellers are liable to pay a 40 ECU unit tax on each unit they sell. The tax is the same for all sellers and is due after each market phase. All "tax revenue" collected in the experiment is donated to the Finnish State.

The market

Basics

The market place opens for trading for 100 seconds at the beginning of each period. In the market traders trade a fictitious good. Each seller can sell up to 4 units, and each buyer can buy up to 4 units of the fictitious good. Trade is conducted through a trading screen.

Goods, costs and values

If you are a seller, you will be randomly assigned the production costs ("costs" from now on) for 4 units of the fictitious good at the beginning of the experiment. These units are denoted "Good 1", "Good 2", "Good 3" and "Good 4". The cost of Good 1 is lower than the cost of Good 2, the cost of Good 2 is lower than the cost of Good 3 and the cost of Good 3 is lower than the cost of Good 4. These costs will remain the same to you throughout the experiment. The costs of each seller differ from the costs of other sellers' goods. Each seller only knows his own costs.

If you are a buyer, you will be randomly assigned the values for 4 units of a fictitious good at the beginning of the experiment. These goods are denoted "Good 1", "Good 2", "Good 3" and "Good 4". The value of Good 1 is higher than the value of Good 2, the value of Good 2 is higher than the value of Good 3 and the value of Good 3 is higher than the value of Good 4. These values will remain the same to you throughout the experiment. The values of each buyer differ from the values of other buyers' goods. Each buyer only knows his own values.

Asks, bids, and trading

Sellers can make offers to sell and buyers can make offers to buy during the market phase. The lowest standing offer to sell and the highest standing offer to buy are visible to everyone on their trading screen. The screen also states whether you are a seller or a buyer, how much time is left in the trading phase and the costs or values that you were assigned for each of your 4 goods.

Each seller first has to sell Good 1 (the good with the lowest cost), then Good 2, then Good 3 and finally Good 4. Accordingly, each buyer first has to buy Good 1 (the good with the highest value), then Good 2, then Good 3 and finally Good 4.

Sellers cannot sell goods at a price that is lower than the cost for the respective good. Buyers cannot buy units at a price that exceeds the value for the respective good.

Sellers can post offers to sell any time during the market phase but each offer to sell has to be lower than the current lowest offer to sell on the market. Accordingly, buyers can post offers to buy any time but each offer to buy has to be lower than the current highest bid on the market.

A transaction takes place when either a seller accepts an offer to buy or a buyer accepts an offer to sell. The transaction price for the good then equals the accepted offer to sell or buy.

See Image 1: Example of a seller's trading screen, and Image 2: Example of a buyer's trading screen.

Here screenshot: Image 1. Example of a seller's trading screen

The upper bar of the trading screen displays the current period and how much time is left for trading. Seller's costs, gross profits, number of goods sold in the current period and a reminder of the per-unit tax are displayed on the left in the middle section. Note that the costs and tax in this example are not the same as those in this experiment. The trading prices are displayed on the right in the order in which the goods have been traded. The lower part of the screen shows the current standing offer to sell and current standing offer to buy. The seller can accept an offer to buy by pressing the "Sell at this price" button. To post a lower offer to sell, the seller has to write the offer in the empty field next to the "Make a lower offer" button and press the button.

Here screenshot: Image 2. Example of a buyer's trading screen

The upper bar of the trading screen displays the current period and how much time is left for trading. Buyer's value, gross profits and the number of goods bought in the current period are displayed on the left in the middle section. Note that the values in this example are not the same as those in this experiment. The trading prices are displayed on the right in the order in which the goods have been traded. The lower part of the screen shows the current standing offer to sell and current standing offer to buy. The buyer can accept an offer to sell by pressing the "Buy at this price" button. To post a higher offer to buy, the buyer has to write the offer in the empty field next to the "Make a higher offer" button and press the button.

Gross earnings from trading

Goods that are not bought or sold do not yield profits or losses. Gross profit from each traded good is the following:

Sellers

Gross profit from selling Good = Trading price of Good 1 - Cost of Good 1
Gross profit from selling Good 2 = Trading price of Good 2 - Cost of Good 2
Gross profit from selling Good 3 = Trading price of Good 3 - Cost of Good 3
Gross profit from selling Good 4 = Trading price of Good 4 - Cost of Good 4

Buyers

Gross profit from buying Good 1 = Value of Good 1 - Trading price of Good 1
Gross profit from buying Good 2 = Value of Good 2 - Trading price of Good 2
Gross profit from buying Good 3 = Value of Good 3 -Trading price of Good 3
Gross profit from buying Good 4 = Value of Good 4 - Trading price of Good 4

Gross earnings from trading equal the sum of gross profits.

Reporting of trades

After the trading phase each seller and buyer makes a decision concerning the reporting of the goods he traded in the current period.

Seller's reporting decision

Sellers are liable to pay a per-unit tax (40 ECU) for each good they trade, and the sum of taxes payable is determined by the number of trades a seller *reports* unless the report is checked for accuracy (see "The effect of reports" below). A seller can report any number between zero and the number of goods he traded in the current period. The reporting decision is sent by pressing the "OK" button.

Buyer's reporting decision

A buyer makes a reporting decision concerning the goods he bought in the current period. A buyer reports by ticking the box next to the good he bought. The reporting decision is sent by pressing the "OK" button.

The effect of reports

Whether a seller's reported number of trades equals the number of goods he actually sold in the current period can be checked. The probability of a seller's report being checked is determined as follows:

- In the basic case the seller's report is checked for accuracy with a probability of **10%**.
- In addition, the seller's and his trading partners' (buyers who bought from him) reports are cross-checked. If there is a mismatch between the reports so that the number of goods the seller reported as sold is lower than the number of goods bought from the seller reported by his trading partners, the probability that the seller's report is checked for accuracy is **80%**. If, instead, the number of goods reported by the seller is larger than the number reported by his trading partners, the probability of the check for accuracy is not affected.

The probability of a seller's report being checked is not affected by the seller's possible previous checks nor whether other sellers' reports are checked in the current period.

Example: Seller A sold all his 4 goods, but reports selling 1 good.

a If at most one of Seller A's trading partners reports having bought a good sold by him, the probability that Seller A's report is checked for accuracy is 10% (one in ten).

b If two or more of Seller A's trading partners report having bought goods sold by him, the probability that Seller A's report is checked for accuracy is 80% (eight in ten).

<u>Calculation of net earnings</u>

Sellers' net earnings

After the reporting phase the screen displays how many goods you sold and your gross profits. Your **net earnings** depend on the taxes you pay and possible fines. After the reporting phase, one of the following takes place:

1 **The seller's report is not checked for accuracy:** In this case the seller's profit after taxes, i.e. net earnings, equals the sum of gross profits earned in the current period minus taxes. Taxes payable equal the number of goods reported by the seller times the 40 ECU tax:
*Net earnings = sum of gross profits - (reported number of goods sold * 40 ECU tax)*

2 **The seller's report is checked for accuracy:** In this case the seller's profit after taxes, i.e. net earnings, equals the sum of gross profits earned in the current period minus taxes and possible fines. Taxes payable equal the number of goods actually sold by the seller times the 40 ECU tax. If the number of goods reported by the seller is smaller than the number of goods he actually sold, the seller has to pay a fine that equals the per unit tax (40 ECU) for each good he did not report in addition to the missing taxes:
*Net earnings = sum of gross profits - (actual number of goods sold * 40 ECU tax) - (number of goods not reported * 40 ECU tax)*

Buyers' net earnings

After the reporting phase the screen displays how many goods you bought and your gross profits. Buyers do not pay taxes, so the net earnings of a buyer equal the sum of gross profits:
Net earnings = sum of gross profits

Example 1: Seller's earnings

Seller A sold 2 goods. The cost of Good 1 is 112 ECU and the trading price 200 ECU, and the cost of Good 2 is 140 ECU and the trading price 171 ECU. The net earnings of Seller A:

i If Seller A reports both trades: 200 - 112 + 171 - 140 - 2*40 = **39 ECU**

ii If Seller A reports 0 trades and the report is not checked for accuracy: 200 - 112 + 171 - 140 = **119 ECU**

iii If Seller A reports 0 trades and the report is checked for accuracy: 200 - 112 + 171 - 140 - 2*40 - 2*40 = **-41 ECU**

Example 2: Buyer's earnings

Buyer B buys 3 goods. The value of Good 1 is 213 ECU and trading price 180 ECU, the value of Good 2 is 118 and trading price 100, and the value of Good 3 is 110 and trading price 105 ECU.

- Buyer B's net earnings: 213 - 180 + 118 - 100 + 110 - 105 = **56 ECU**

Payoffs

The first 3 periods are practice periods during which you cannot earn money. The 25 periods after the practice periods are payoff relevant, and your total earnings from the experiment consist of your net earnings from these periods and a 5 EUR participation fee.

If the sum of your net earnings is negative, you will be paid the participation fee, so you cannot make losses in this experiment and you will earn at least 5 EUR. Your total earnings will be paid to you in cash after the experiment.

Final note

The experiment ends after 28 periods. After this, we kindly ask you to fill out a short questionnaire while we prepare the payments. All information gathered in the questionnaire, as well as other data gathered in the experiment, will be handled confidentially and used solely for scientific research. After you have completed the questionnaire we ask that you stay seated until we invite you to collect your payment.

The **Aboa Centre for Economics (ACE)** is a joint initiative of the economics departments of the Turku School of Economics at the University of Turku and the School of Business and Economics at Åbo Akademi University. ACE was founded in 1998. The aim of the Centre is to coordinate research and education related to economics.

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