How Do Tax Evasion Opportunities Affect Prices?*

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Abstract

We investigate the causal link between tax evasion opportunities and market prices using two controlled experiments where buyers and sellers trade a fictitious good in competitive markets and a per-unit tax is imposed on sellers. In the first experiment, sellers in one experimental group are provided the opportunity to evade the tax whereas sellers in the control group are not. We find that markets with evasion opportunities have lower equilibrium prices. The rationale for this effect is that firms with evasion opportunities face a lower effective tax burden compared to firms without evasion opportunities. We hold the effective tax burden constant in the second experiment and find that markets with evasion opportunities tend to trade at higher prices than markets without evasion opportunities. Our findings have implications for tax incidence. In particular, we find that sellers with an evasion opportunity shift a larger share of their *effective* tax rate onto buyers compared to sellers in the control group.

Keywords: Tax Evasion, Tax Avoidance, Price Effects, Tax Incidence, Firm Behavior, Experiment

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1 Introduction

A number of academic studies document the prevalence of tax evasion among firms and self-employed individuals (e.g., Slemrod 2007; Kleven et al. 2011; Mikesell 2014; Fox et al. 2014; De Simone et al. 2020).[1] Because tax evasion reduces revenues and thus negatively impacts budgets, many governments have been investing resources into evasion-reducing policies. For example, the EU council intends to reinforce the capacity of EU member states to fight against e-commerce VAT fraud by launching a Central Electronic System of Payment information (CESOP). CESOP will keep records of cross-border payments within the EU and between the EU and Non-EU countries beginning in 2024. The idea is that this system will allow tax authorities to monitor and enforce the VAT on cross-border business-to-consumer supplies of goods and services (EU Commission 2023). Similarly, state governments in the US pushed for changes to the definition of nexus in an attempt to reduce non-compliance with the *use tax*.[2] While such evasion curbing policies are expected to increase tax revenues, they could also affect market prices and thus change the distribution of tax burdens with important implications for the equity profile of the affected tax systems.

We pursue two objectives related to the impact of tax evasion on market prices. The first objective is to test whether prices are different in markets where firms have the option to evade (sales) taxes relative to markets in which firms cannot evade. Firms that evade taxes face a lower effective tax burden compared to firms that do not evade taxes. Any observed differences in prices between markets with and without evasion opportunities may therefore be driven by the lower effective tax burden. Consequently, the second objective of our paper is to test if evasion opportunities affect market prices when the effective tax burden is held constant. Specifically, we compare market prices across markets that face the same effective tax burden, but where firms in some markets arrive at the lower effective tax burden through tax evasion, whereas firms in other markets face an exogenous reduction in the tax burden. This second objective sheds light on the question of whether an evasion-induced reduction in the tax burden has a differential effect than an exogenous tax burden reduction.

We address these research objectives using data generated in two controlled laboratory experiments where participants trade fictitious goods in a competitive double

[1]There are estimations about the extent of tax evasion in transaction taxes (which are the focus of our study). Sales tax gap estimates range from 2 to 41% for the VAT in the EU and 1 to 19.5% for the retail sales tax in the US (Mikesell 2014). Additionally, it is widely acknowledged that 'use tax' evasion by both businesses and individuals is much higher than retail sales tax evasion; e.g., GAO (2000) assume non-compliance rates of 20 to 50% among businesses and 95 to 100% among individuals in a study of the potential revenue losses of e-commerce.

[2]Consumers in the United States are required to pay 'use tax' in lieu of the general retail sales tax if the seller is not required – by law – to register as a tax collector in the consumers' state.

auction market, which is known to generate very plausible equilibria that align with theoretical expectations (e.g., Smith 1962; Dufwenberg et al. 2005; Grosser and Reuben 2013). Experimental participants in both experiments are randomly assigned roles as sellers or buyers and a per-unit sales tax is imposed on all sellers. In the first experiment, which addresses our first research objective, sellers in the evasion group make a tax reporting decision and are therefore able to underreport the number of units sold, whereas sellers in the control group have their correct tax liability deducted automatically.

The empirical results show that access to evasion affects market prices: the equilibrium price in the evasion group is statistically and economically lower than in the control group. One rationale for this finding is that sellers with an evasion option can reduce their effective tax rate relative to those without evasion. This allows firms with evasion opportunities to offer their goods at lower prices. At the market level, evasion-induced reductions in effective tax rates imply that the tax causes the industry supply curve to shift up by a smaller amount relative to situations without access to evasion.

Building on the result of the first experiment, we conduct a second experiment to study the price effects of evasion opportunities in situations with constant effective tax burden. This experiment, which addresses our second research objective, features a condition that is analogous to the control group in the first experiment, except that the effective tax burden is exogenously lowered to match the expected effective tax burden observed in the evasion group of the first experiment (through an automatic tax credit). Benchmarking the price in this tax credit group against the price in the evasion group of the first experiment identifies the price effect of evasion opportunities across situations with constant tax burden. We find that the market price in the evasion group is higher than the market price in the tax credit group. Although this difference is not always precisely measured, it indicates that access to evasion yields smaller price reductions than equivalent exogenous reductions in tax burdens. A potential explanation is that sellers in the evasion treatment adjust prices to account for the intrinsic and perceived extrinsic costs associated with an evasion-induced reduction in the tax rate.

Overall, our finding suggests that holding effective tax rates constant, tax evasion might actually increase market prices, implying that tax evasion imposes a double burden on society: foregone tax revenues and higher market prices.

We leverage the estimated price effects documented above to examine the economic incidence of the sales tax in the presence of tax evasion. Our findings are as follows: First, the share of the *nominal* tax rate borne by buyers is lower in the presence of evasion. Second, we find that sellers with an evasion opportunity shift a larger share of their *effective* tax rate onto buyers compared to sellers in the control group.[3]

[3] We refer to the tax rate that is legally due as the *nominal* tax rate. Reflecting that some taxpayers evade part of their legal tax liability and thereby reduce their tax burden, the *effective* tax rate refers to actual tax payment as a share of true taxable income, accounting for fines.

Our paper relates to literature that explores the effect of taxes on prices (outside the evasion context) using archival data. Consistent with the findings of our first experiment, the literature finds that taxes affect prices (Benzarti et al. 2020; Jacob et al. 2022; Dedola et al. 2022; Baker et al. 2023). Complementing this literature, we view it as our paper's main contribution and novelty to show that evasion-induced reductions in the tax burden also reduce prices. To this end, we further add to the literature studying tax evasion (Slemrod 2019). Unlike most of the tax evasion literature, we investigate the implications of tax evasion for an outcome variable rather than investigating the determinants of tax evasion.[4] We are aware of two papers studying tax incidence in the presence of tax evasion: Alm and Sennoga (2010) use a CGE model and Kopczuk et al. (2016) use archival data to study incidence across situations in which diesel taxes are imposed at different production stages. Since we randomize access to evasion and measure non-compliance, we offer cleaner causal identification than these two studies – though at the cost of lower external validity.

Although our particular set-up studies tax evasion, rather than avoidance, the general mechanism behind our results potentially also applies to avoidance. Similar to tax evasion, tax avoidance possibilities and the resulting reduction in the tax burden might give firms scope to sell their goods at lower prices. To this end, we relate to a stream of papers that examines the consequences of tax avoidance – though not for prices (see Jacob 2022 for an overview).

To the extent that our findings are generalizable to 'real-world' settings (Appendix D discusses external validity), our findings have implications for the effects of policies aimed at reducing tax evasion and avoidance. First, policies that reduce evasion opportunities will lead to higher market prices, *all else equal*. Second, policymakers can reduce evasion opportunities *and* lower prices by combining their evasion-curbing policies with changes in tax policy that leave effective tax burdens unchanged. This is an important finding, because it highlights how tax policy can be utilized to improve consumer welfare. Our results further imply that the price effects that result from exogenous changes in tax burdens cannot be used to quantify the likely price effects of policies targeting tax evasion.

Our results are potentially relevant in countries such as the United States where the Supreme Court's ruling in *South Dakota v Wayfair* has changed the way out-of-state merchants are treated with respect to retail sales tax collection. In particular, a number of states have updated their sales tax laws to follow South Dakota's lead in requiring out-of-state firms to serve as tax collectors, thus changing the tax evasion opportunities that previously existed with the *Use-Tax*. There have also been a push to restrict the sale of "zappers", which are used to evade sales taxes among firms. Our findings suggest that such measures are likely to result in higher prices as affected sellers fully adjust to

[4]Kotakorpi et al. (2021) study the effect of different tax-reporting institutions on pricing decisions.

the retail sales tax. This price effect of reducing evasion opportunities could be mitigated if the evasion-curbing policies were accompanied by reductions in the statutory tax rate.

2 Experiment 1: How do Evasion Opportunities Affect Prices?

We run two sets of experiments. In the first experiment, we study the price effect of having access to evasion opportunities. Because this effect may run through the evasion-induced reduction of the tax burden, the second experiment studies the effect of evasion opportunities in situations in which the reduction in effective tax rate is held constant. In this section, we describe the design and results of the first experiment and leave the description of the second experiment to Section 3.[5]

2.1 Experimental Design

Overview. The experimental design reflects a standard competitive experimental double auction market as pioneered by Smith (1962). The auction and the parameters are based on Grosser and Reuben (2013). In each round of the double auction market, 5 buyers and 5 sellers trade two units of a homogeneous and fictitous good. Sellers are assigned costs for each unit and buyers are assigned values. The roles of sellers and buyers as well as the costs and values are exogenous and randomly assigned to participants. We impose a per-unit tax on sellers – which we refer to as the *nominal* tax rate – to this set-up and give sellers in an *evasion* group the opportunity to evade the tax, whereas sellers in the *control* group pay the per-unit tax automatically (as with exact withholding). We employ a between-subjects design where each participant is either in the control or evasion opportunity group. The instructions provided to participants were identical for the control and evasion group except for information on the reporting decision and net income of sellers; see Appendix G.

Organization. We ran a total of 16 experimental sessions, where each lasted about 100 minutes (including review of instructions and payment). We conducted eight control and eight evasion sessions with a total of 160 subjects.[6] The experiments were programmed utilizing *z-tree* (Fischbacher 2007). Appendix Section C provides summary statistics on

[5]The second experiment is best described as an additional treatment arm of the first experiment. The general setting of the two experiments is very similar and the second experiment builds on the design and results of the first experiment, which is why we will only highlight the differences to the first experiment when we describe the second experiment in Section 3.

[6]We ran eight sessions (four evasion/four control) in 2013 and eight sessions (four evasion/four control) in 2021. The experimental outcomes are similar between the 2013 and the 2021 sessions; see Appendix Table A.1.

demographic characteristics of the participants. Experimental Currency Units (ECU) were used as the currency during the experiment. After the experiment, ECU were converted to Euro with an exchange of 30 ECU = 1 EUR and subjects were paid the sum of all net incomes (see below) in Euro.[7] It was public information that all tax revenue generated in the experiment is donated to the German Red Cross.

Description of a Session. Each session includes one market that is either a control or evasion market. Each market has five buyers and five sellers who each have two units of a fictitious good to trade. All ten subjects in one session/market first trade in three practice rounds and then 27 payoff relevant rounds.

Trade in the Double Auction. Subjects are given demand and supply schedules for a fictitious good at the beginning of the session (Ruffle 2005; Cox et al. 2018; Grosser and Reuben 2013). The demand schedule for buyers assigns a value to each of two items and the supply schedule for sellers assigns a cost to each of two items. The cost/value of the units vary across items and subjects as illustrated in Appendix Table F.1. This allows us to induce demand and supply curves for each market, which are depicted in Appendix Figure F.1. The schedules are chosen such that demand and supply elasticities are equal in equilibrium. The demand and supply schedules remain fixed across periods in a given session, and they do not differ between control and treatment markets.

Subjects trade the good in a double auction market that is opened for two minutes in each period. During this time, each seller can post an "ask" that is lower than the current ask on the market, but higher than the cost of the item to the seller. That is, sellers cannot trade an item below its cost. As in the literature, sellers must sell their cheaper unit before they sell their more expensive unit. Similarly, each buyer can post a "bid" that is higher than the current bid on the market, but lower than the value of the item to the buyer. Therefore, buyers cannot buy an item at a price that exceeds its value. Buyers must also buy their most valued item before their least valued item. The lowest standing ask and the highest standing bid are displayed on the computer screen of all ten market participants (Appendix Figure F.2 depicts a screenshot.)

An item is traded if a seller accepts the standing buyer bid or a buyer accepts the standing seller ask. Subjects are not required to trade a minimum amount of items and items that are not traded yield neither costs nor profits. Traders are not allowed to communicate with each other. This trading procedure is identical for the treatment and control groups.

[7] In addition, subjects received a show-up fee, which was 2.50 EUR in the 2013 sessions and 6.00 EUR in the 2021 sessions.

Calculation of Income. In the **control group**, gross income in each period consists of the sum of the profit on each unit traded. Sellers' gross profit on each unit is equal to the difference between the selling price and cost, while buyers' profit on each unit is the difference between value and price paid. All subjects are told that sellers have to pay a per-unit tax for each unit sold, that the tax rate is fixed across all periods at $\tau = 10$ ECU per-unit and that the tax is collected at the end of every third trading period. This yields 27 trading periods and 9 tax collections (we motivate this design feature in Appendix F). We define total gross profit in each trading period i ($i = 1, 2, ..., 26, 27$) as

$$\Pi_i^s = P_{i1}d_1 + P_{i2}d_2 - C_1 d_1 - C_2 d_2, \tag{1}$$

for sellers, and

$$\Pi_i^b = V_1 d_1 + V_2 d_2 - P_{i1}d_1 - P_{i2}d_2, \tag{2}$$

for buyers. Superscripts s and b indicate seller and buyer, respectively, $d_j = 1$ if good j is traded and 0 otherwise, P_{ij} is the price of good j in period i, C_j is the cost of good j and V_j is the value of good j.

Because taxes are collected at the end of every third trading period, a seller's net income for each tax collection period k ($k = 3, 6, 9, 12, 15, 18, 21, 24, 27$) is equal to:

$$\pi_k^s = \Pi_k^s + \Pi_{k-1}^s + \Pi_{k-2}^s - \tau U, \tag{3}$$

where U is the total number of units sold in the last three rounds and $\tau = 10$ is the nominal per-unit tax rate. Because buyers do not pay a tax, their net income for each tax collection period may be written as:

$$\pi_k^b = \Pi_k^b + \Pi_{k-1}^b + \Pi_{k-2}^b \tag{4}$$

Both buyers and sellers are shown their gross income after every trading period and their net income after every tax collection period. Subjects' final payoff is the sum of their net incomes from the nine tax collection periods.

Since buyers do not pay the tax, the calculation of gross and net income for buyers in the evasion group is identical to that of the control condition: see equations (2) and (4).

Sellers, on the other hand, make a tax reporting decision at the end of every third round. Sellers in the **treatment group** can report any number between 0 and the true amount sold in the previous three trading periods, and the reported amount is taxed at $\tau = 10$ ECU per unit. Sellers face an exogenous audit probability of $\gamma = 0.1$ (10%). Additionally, sellers who underreport sales and are audited must pay back the evaded taxes plus a fine equivalent to the evaded taxes. The tax rate, audit probability, and fine rate are fixed across periods and sessions, and all subjects – buyers and sellers – in the treatment group receive this information at the beginning of the experiment.

Therefore, unlike sellers in the control group who must pay taxes on each unit sold, sellers in the evasion condition are able to evade the sales tax by underreporting sales. Sellers' gross income in any trading period i is the same as in equation (1), but their net income in each tax collection period is rewritten as:

$$\pi_k^s = \begin{cases} \Pi_k^s + \Pi_{k-1}^s + \Pi_{k-2}^s - \tau R & \text{if not audited,} \\ \Pi_k^s + \Pi_{k-1}^s + \Pi_{k-2}^s - \tau U - \tau(U - R) & \text{if audited,} \end{cases} \tag{5}$$

where R is the reported number of units sold, U is the number of units actually sold over the last three rounds, and $\tau = 10$ is the nominal per-unit tax rate. Subjects' final payoff is the sum of their net incomes from the nine tax collection periods.

Market Equilibrium without Evasion. The demand and supply schedules (displayed in Appendix Figure F.1 and Table F.1) can be used to determine the competitive equilibrium price (and quantity) with and without the per-unit tax. Theoretically, we expect the market to clear with 7 units traded at any price in the range 48 ECU to 52 ECU in the case without taxes. We obtain a range of prices in equilibrium because the demand schedule is stepwise linear (Ruffle 2005; Cox et al. 2018; Grosser and Reuben 2013). Grosser and Reuben (2013) conducted an experiment using the same demand and supply schedule as we do and find that the "no tax" equilibrium is equal to that predicted by the theory, leading to the assertion that our "no tax" equilibrium is in line with theoretical expectations.

A per-unit tax on sellers increases the cost of each unit by 10 ECU and thus shifts the supply curve to the left, as shown in Figure F.1. In the absence of tax evasion opportunities, this theoretically produces a new equilibrium quantity of 6 units, which is supported by an equilibrium price in the range of 53 ECU to 57 ECU. Because the linearized form of the demand and supply schedules have equal elasticity in equilibrium, the incidence of the tax should theoretically be shared equally between buyers and sellers; buyers pay an extra 5 ECU and sellers receive 5 ECU less (after paying the tax), relative to the case without a tax.

The question we seek to answer is whether this equilibrium outcome is affected by the presence of tax evasion opportunities among sellers.

Definition of Prices. The experiment produced one price for each unit sold in a given market-period, which allows us to create three measures of market price. One measure is the price at which each item is sold, denoted P. We also calculate the mean and median price in a given market-period and denote them $\overline{P}$ and P_{50}, respectively.

2.2 Empirical Strategy

Non-parametric Analysis. Because the period-specific prices are not independent across the 27 periods within a given market, we implement our non-parametric ranksum-test analyses using the average price for each market; that is, we use the average of $\overline{P}$ by market. This implies that our non-parametric analysis is based on 16 independent observations; eight in the evasion and eight in the control conditions.[8]

Regressions. We regress our measure of price, $\overline{P}$, on a treatment dummy:

$$\overline{P}_{i,m} = \beta_0 + \delta T_m + \epsilon_{i,m}, \tag{6}$$

where $\overline{P}_{i,m}$ is the mean price of the good in period i (with $i = 1, ..., 27$) of market m (with $m = 1, ..., 16$).T_m is a dummy indicating if market m is in the evasion or control group. Our coefficient of interest is δ, which represents the difference in market price between the two conditions. We set up our data as a panel with 27 periods per market and account for the dependence of prices across periods within a market by clustering standard errors on the market level (which gives 16 clusters).We include period fixed effects (included for robustness reasons, although they only imply an intercept shift and should not matter for the treatment effect) and/or pre-determined demographic variables (age, gender, native language, field of study Economics indicator, each measured as the average on the market level) in some specifications (Appendix C provides details).

2.3 Results

Compliance Rate in Evasion Group. We find that 37 out of 40 sellers in the evasion-opportunity condition evade some positive amount of sales at least once, and 26 of the 40 sellers pursue a strategy of full evasion in the equilibrium reporting periods. The mean compliance rate, defined as reported sales divided by actual sales, is approximately 16% among all sellers in the evasion group and 72% among those who report non-zero sales. Real world evidence suggests that our compliance rates are not unreasonable. For example, the compliance rate for the 'use' tax in the United States is estimated to be between 0 to 5% among individuals (GAO 2000).

Price Effects. Figure 1 reports the mean market price by period for the markets with and without evasion opportunity. It shows that the price in the evasion group is lower

[8]While the number of independent observations, 16, appears to be low, it is not unprecedented to use such few observations in empirical analysis; see for example Grosser and Reuben (2013) who apply nonparametric tests based on four independent market-level observations and have sufficient statistical power. We use the Harris and Hardin (2013) method, which adjusts the p-values to the low number of observations to implement "exact" ranksum tests.

than in the control group. We also see that the mean market price varies in both groups in the first 10 to 14 trading periods. This is consistent with the existing literature, which generally finds that double auction markets take approximately 8 to 10 rounds to converge (Ruffle 2005). For this reason, and as is common in the literature, our results focus on data from trading periods 15 to 27 (however, we report results for the full sample as well).

The mean market price in both groups stabilized after round 14 at **54.56 ECU** in the control group and **51.33 ECU** in the evasion group, implying that the mean market price in the evasion group is 3.23 ECU lower than in the control group.These differences in prices between the groups are statistically significant from zero; the exact ranksum test (two-sided) gives a p-value of 0.001 for differences in mean prices.

The corresponding regression results are presented in Table 1. The estimated treatment effect of -3.23 ECU reported in model 1 of Panel B is statistically different from zero at the 1% level.This main estimate of the treatment effect remains statistically significant after correcting for the small number of clusters using the wild-bootstrap-t procedure described in Cameron et al. (2008); see Appendix Table A.4. The estimate becomes smaller (-2.70), but remains statistically significant as we add control variables (models 3 and 4). Appendix Table A.6 shows that the finding of evasion markets trading at lower markets than control markets is robust to the definition of price (this table considers the price at which each item is sold in Panel A and the median price in each period in Panel B). Further evidence is provided in Appendix Figure A.2, which shows that the mean market price in the control group is not drawn from the same distribution as that in the treatment group (Kolmogorov-Smirnov-test; p-value: 0.000).

Overall, we find that markets with evasion opportunities are characterized by lower prices than markets where evasion is not an option.

3 Experiment 2: Price Effect of Evasion Opportunities Across Markets with Same Tax Burden

Our previous finding supports the rationale that tax evasion lowers the effective tax rate facing sellers, which then allows sellers to trade at lower prices (see Appendix Section E). However, a reduction in tax burden might yield lower prices even in the absence of tax evasion, raising the following question: suppose firms in two markets experience similar reductions in effective tax burden, but firms in one market evade taxes to arrive at this lower tax burden whereas firms in the other market experience an exogenous reduction – would the price effect be the same across these two markets?

To shed light on this question, we run a second experiment in which we exogenously reduce the tax burden (through a tax credit) of sellers to the effective tax burden that we observe in the evasion group of the first experiment. Comparing this *tax credit* group and experiment 1's evasion group then allows us to explore a situation where all sellers have the same effective tax burden, but some sellers evade taxes to arrive at this tax burden, whereas other sellers face an exogenously determined lower tax burden.

3.1 Experimental Design

Overview. The tax credit sessions are identical to the control sessions of the first experiment except that the effective tax rate (ETR) is exogenously lowered to 2.50 ECU, which is the same as the ETR in the evasion group of the first experiment.[9] As in experiment 1, the nominal tax rate is 10 ECU, but sellers are told that they will receive an automatic tax credit of 7.5 ECU for every unit they sell. Therefore, sellers in the tax credit group face an ETR that is identical to that of sellers in the evasion group. Importantly, while the effective tax burden is the same across sellers in the evasion and tax credit groups, sellers in the tax credit group do not have to take any actions to arrive at this lower ETR.

Organization and Sample. We ran eight sessions – lasting approximately 100 minutes each – of this tax credit group for a total of eight markets and 80 subjects. Three of these eight sessions were conducted in 2015 and five in 2021. There were 10 subjects (five buyers and five sellers) in each session, and the average pay-off was 24 EUR.[10] Summary statistics are provided in Appendix Section C.

We exclude the 2021 evasion treatments from all subsequent analyses, because the ETR in the 2013 evasion group was an input in the 2015 tax credit group, and we want the 2015 and 2021 tax credit groups to be comparable. The only way to make both sets of tax credit sessions comparable is to use the same ETR in both. Consequently, the analyses that follow benchmark all eight sessions of the tax credit experiment (i.e., sessions from 2015 and 2021) against the four initial 2013 sessions of the evasion opportunity sessions (all of which have the same effective tax burden). The equilibrium price in the 2021 evasion groups is very similar to the price in the 2013 evasion groups; see Table A.1, making us confident to bundle 2013, 2015 and 2021 sessions of the control and tax credit groups in the subsequent analyses.

3.2 Results

The definition of prices and empirical strategy are analogous to the first experiment. We provide several pieces of evidence to support the claim that access to evasion affects

[9]Equation 7 in Appendix E shows how we calculate the ETR, which is based on the compliance rate of 7% of the initial 2013 evasion conditions. While the ETR in these initial evasion-group sessions is actually 2.56 ECU, we opted for 2.50 ECU because it is easier for subjects to mentally calculate.

[10]Show-up fee was 2.50/6.00 EUR in the 2015/2021 sessions.

market prices differently than a comparable exogenous reduction in tax burdens. First, Figure 2 shows that the mean market price in the tax credit condition is lower than in the evasion condition in each period. This is reflected in the mean prices over rounds 15-27 in these two groups: 51.66 ECU in the evasion group and 49.98 ECU in the tax credit group (see Table 2).This difference of 1.68 ECU is economically meaningful, as it is more than one-half of the ETR and larger than one standard deviation of mean price in the evasion group, and statistically significant (in non-parametric tests with 12 independent observations; see Table A.3).

Second, Table 3 reports regression results in which we benchmark the initial evasion groups against the tax credit groups, with the mean price in each period as dependent variable (as in Equation 6). All four models of Panel B (periods 15-27) show that the clearing price in the tax evasion markets is higher than in tax credit markets, despite identical ETRs.This difference is statistically significant in specifications 1 and 2. We see two statistical differences in demographics across the relevant experimental groups (Table C.1, *p-value(c)* in Appendix C). The inclusion of pre-defined control variables (specifications 3 and 4) reduces the treatment coefficient from 1.68 to 1.04, which remains economically meaningful at approximately 41.50% of the ETR, but the coefficient is not statistically significant.

Third, the cumulative price distributions in Figure A.1 also show that prices are always higher in the evasion group than in the tax credit group (Kolmogorov-Smirnov test p-value: 0.000). Finally, an additional indicator that prices are different across the tax credit and evasion groups is that the number of units sold is lower in the evasion group than in the tax credit group, despite constant tax burdens (Appendix Section B and Table B.1). This difference is statistically significant in all four specifications of Table B.1 (i.e., including those with control variables).

In summary, although the regression coefficients are not always statistically significant, the above analyses consistently suggest that markets with evasion opportunities trade at higher prices than markets with an equivalent, yet exogenously determined, tax burden.

4 Implications for Tax Incidence

Estimation of Economic Incidence. We estimate economic incidence of the nominal tax rate and the ETR on buyers (all incidence findings summarized in Table 2). The incidence of the nominal tax rate is the share of 10 ECU that sellers shift to buyers in the form of higher prices. The economic incidence of the ETR is the share of the ETR that is shifted onto buyers. To determine the incidence of the tax, we first have to determine the incidence of the tax in the control group, which requires knowing the market equilibrium in the absence of the tax. Although we did not run a "no tax" treatment, we are able to derive this "no tax" equilibrium by relying on theoretical predictions and the empirical evidence of Grosser and Reuben (2013). As outlined in Section 2.1, we theoretically expect the no-tax market to produce an equilibrium with 7 units at a price in the range 48 ECU to 52 ECU. This prediction is supported by empirical evidence in Grosser and Reuben (2013) who find a mean market price of 49.04 ECU (sd: 1.3) and 7.03 (sd: 0.36) units in the "no tax" equilibrium. We use their "no tax" result as a benchmark in the following discussion of economic incidence since our demand and supply schedules are identical to theirs.

Nominal Tax Rate. The equilibrium price in the control group (with tax but no evasion opportunity) is 54.56 ECU, which is approximately 5.52 ECU above the "no tax" equilibrium of 49.04 ECU. This suggests that the incidence of the nominal tax burden of 10 ECU in the control condition is shared equally between buyers and sellers.

The mean market clearing price in the evasion group is 51.66 ECU. Considering the nominal tax rate of 10 ECU per unit and the no-tax benchmark of 49.04 ECU, this implies that buyers in the evasion group pay 26% (= (51.66 − 49.04)/10) of the *nominal* tax burden, compared to ≈50% in the control group. In other words, access to evasion reduces the economic incidence of the nominal tax on buyers by about 29 percentage points (= 55.20% − 26.20%). This effect on incidence appears small when compared to the market price. However, the relevant comparison is the share of the nominal tax burden that buyers paid in the control group. Since buyers paid approximately 5 ECU of the nominal tax of 10 ECU in the control group, the largest expected effect of evasion is a reduction of 5 ECU. Using this baseline, a price reduction of 2.90 ECU is large.

ETR. We also investigate whether access to evasion changes the incidence of the ETR. Because the ETR is the same as the nominal tax rate in the control group, we already know that the ETR is approximately shared equally between buyers and sellers in the control group without. The price in the evasion group is 51.66 ECU, which suggests that sellers shift the full expected ETR onto buyers; buyers bear 2.62 ECU (= 51.66 − 49.04) even though the ETR is 2.56 ECU.[11] As a result, about 100% (= (51.66 − 49.04)/2.56) of a seller's expected ETR is shifted onto buyers.

What about incidence in the tax credit group? Notice that consumers in the tax credit group pay 0.94 ECU (= 49.98 − 49.04) of the nominal tax rate, implying that sellers in this group shifted 38% of their effective tax burden onto buyers. This shifting of the ETR is considerably lower than the full shifting of the ETR that we observe in the

[11] We would expect the price in the evasion group to increase by approximately 1.28 ECU (= 2.56/2) relative to the "no tax" equilibrium of 49.04 ECU if the incidence was shared 50-50; that is to 50.32 ECU. The observed price of 51.66 ECU is statistically larger than 50.32; one-sided test p-value: < 0.001.

evasion treatments – despite the fact that the ETR is the same.

5 Conclusion

While we show that tax evasion opportunities affect prices and that this has implications for the distributional effects of policies curbing evasion opportunities, we acknowledge that it is not clear that the magnitude of the effects is the same across all types of taxes and that it may depend on the tax parameters (e.g.,, the audit probability or fine rate). Conditional on the ease with which taxes can be evaded, it is also possible that the evasion mechanism matters. For example, Tran and Nguyen (2014) show that Vietnamese firms evade VAT by artificially increasing their sales and material costs, which is facilitated by colluding with other producers in the supply chain. The presence of collusion as a means of evasion suggests lower competitive pressure, which may lead to different incidence outcomes under a VAT compared to retail sales taxes where collusion among firms is not necessary for evasion. Given recent calls for the adoption of VAT in countries without a VAT system (such as the USA), this potential difference is worth investigating in future research.

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Tables and Figures (in order of appearance in manuscript)

Figure 1: Mean Price with and without Evasion Opportunity

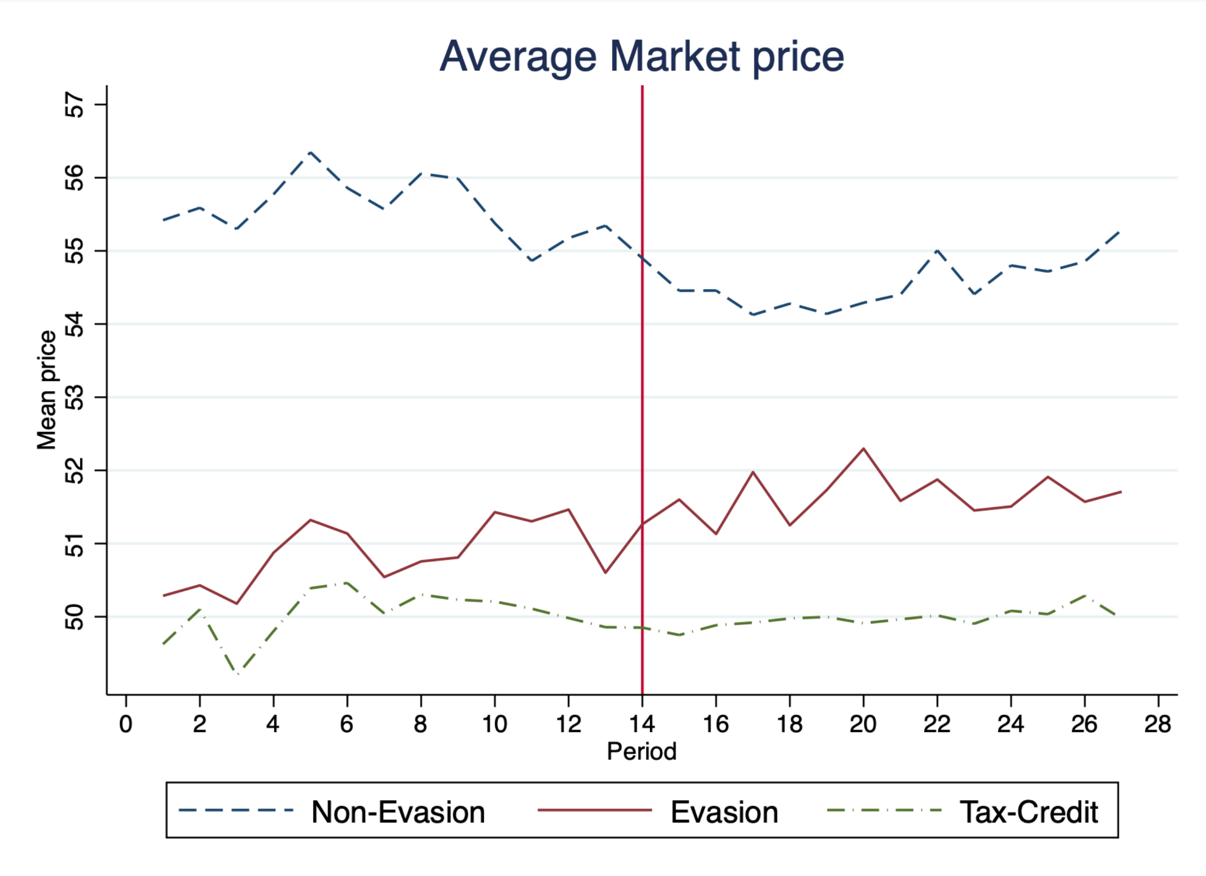
Notes: Reported is the mean price $\overline{P}$ in each period separately for the evasion and control groups in the first experiment. The sample includes eight independent markets in the control group and eight independent markets in the evasion group.

Table 1: Impact of Evasion Opportunity on Mean Price

	Model 1	Model 2	Model 3	Model 4
	Panel A: All Periods			
Evasion	-3.882***	-3.882***	-3.528***	-3.528***
	(0.766)	(0.790)	(0.866)	(0.893)
Constant	55.066***	55.231***	58.760***	58.925***
	(0.521)	(0.844)	(6.619)	(7.401)
Obs.	432	432	432	432
	Panel B: Periods>14			
Evasion	-3.228***	-3.228***	-2.701***	-2.701***
	(0.829)	(0.854)	(0.801)	(0.826)
Constant	54.556***	54.606***	63.267***	63.317***
	(0.507)	(0.484)	(7.790)	(8.105)
Obs.	208	208	208	208
Clusters	16	16	16	16
Period FE	No	Yes	No	Yes
Controls	No	No	Yes	Yes

Notes: Results from the first experiment. Robust standard errors adjusted for clustering at the market level are in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%. Estimates are based on equation (6) with the dependent variable defined as the mean price in a given market period. Regression results for the effect of the *evasion* group (where sellers have evasion opportunity; experiment 1) relative to the control group (where sellers do not have an evasion option; experiment 1). Panel A uses periods 1 to 27 and panel B uses periods 15 to 27. The sample includes eight independent markets in the control group and eight independent markets in the evasion group (for a total of 16 markets/clusters). Period FE is period fixed effects. Control variables include the average age, share of males, share of native German speakers, and share of subjects whose field of study is economics. These averages and shares are calculated at the market-level.

Figure 2: Mean Price with and without Evasion Opportunity and Constant Tax Burden



Notes: Reported is the mean price $\overline{P}$ in each period for the control group, evasion group (both from first experiment), and tax-credit group (no evasion opportunity, but with tax credit; second experiment). The sample includes eight independent markets in the control group, four independent markets in the evasion group, and eight independent markets in the tax credit group.

Table 2: Overview of Results and Economic Incidence

	Equilibrium		Incidence (%)	
	Price	Units	Nominal Tax	Effective Tax
No - Tax	49.04	7.03	–	–
Control	54.56	5.89	55.20	55.20
Evasion	51.66	6.46	26.20	102.34
Tax Credit	49.98	6.80	–	37.60

Notes: The results in *No Tax* row are from Grosser and Reuben (2013) who use identical supply and demand schedules in an experimental double auction without taxes. *Control* and *Evasion* refer to the experimental conditions without and with evasion opportunity from the first experiment. *Tax Credit* refers to the second experiment's condition without evasion opportunity and a tax credit of 7.5 ECU. The sample includes eight independent markets in the control condition, four independent markets in the evasion condition and eight independent markets in the tax-credit condition. Reported are the mean prices (in Experimental Currency Units) and number of units traded. "Incidence Nominal Tax" is the percent of the nominal tax rate (10 ECU) that is shifted onto buyers. "Incidence Effective Tax" is the percent of the effective tax rate (10 ECU in Control, 2.56 ECU in Evasion, 2.5 ECU in Tax Credit) that is shifted onto buyers.

Table 3: Impact of Evasion Opportunity on Mean Price across Markets with Constant Tax Burden

	Model 1	Model 2	Model 3	Model 4
	Panel A: All Periods			
Evasion	1.264	1.264	0.302	0.302
	(0.854)	(0.891)	(0.781)	(0.815)
Constant	49.995***	49.424***	74.124***	73.553***
	(0.512)	(0.970)	(13.394)	(14.367)
Obs.	324	324	324	324
	Panel B: Period>14			
Evasion	1.684**	1.684*	1.037	1.037
	(0.760)	(0.791)	(0.726)	(0.757)
Constant	49.977***	49.990***	78.913***	78.926***
	(0.501)	(0.496)	(10.282)	(10.651)
Obs.	156	156	156	156
Clusters	12	12	12	12
Period FE	No	Yes	No	Yes
Controls	No	No	Yes	Yes

Notes: Robust standard errors adjusted for clustering at the market level are in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%. Regression results for the effect of the *evasion* group (where sellers have evasion opportunity; experiment 1) relative to the tax credit group (where sellers face the same effective tax rate as in the evasion group, but do not have to evade to arrive there; experiment 2). To have identical effective tax rates in the two groups of interest, the regressions include the four initial evasion group sessions (see Section 3 for further explanation). The sample includes four independent markets in the evasion group and eight independent markets in the tax credit group (for a total of 12 markets/clusters). Estimates are based on equation (6) with the dependent variable defined as mean price in a given market period. Panel A uses periods 1 to 27, Panel B uses periods 15 to 27. Period FE is period fixed effects. Control variables include the average age, share of males, share of native German speakers, and share of subjects whose field of study is economics. These averages and shares are calculated at the market-level.

(Online) Appendix

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A Additional Results

Table A.1: Comparison of ‘Old’ and ‘New’ Data: Prices and Quantities by Experimental Condition

	Mean Price			Median Price			Units Sold		
	Control	Evasion	Tax Credit	Control	Evasion	Tax Credit	Control	Evasion	Tax Credit
Old Data	54.36	51.66	50.09	53.78	51.69	50.26	5.92	6.46	6.87
	(1.208)	(1.243)	(1.795)	(0.555)	(1.265)	(1.712)	(0.479)	(0.503)	(0.339)
Obs.	52	52	39	52	52	39	52	52	39
New Data	54.75	51.00	49.91	54.58	51.03	49.86	5.87	6.00	6.75
	(1.922)	(2.309)	(1.088)	(1.946)	(2.233)	(1.220)	(0.715)	(0.443)	(0.434)
Obs.	52	52	65	52	52	65	52	52	65

Notes: This table compares experimental outcomes across experimental data collected in the years 2013 and 2015 (‘old’ data) vs experimental data collected in 2021 (‘new’ data). In 2013, we collected data for four control and four evasion markets. In 2015, we collected data for three tax credit markets. In 2021, we collected data for four control markets, four evasion markets and five tax credit markets. The 2013 and 2015 sessions were conducted at the University of Cologne. The 2021 sessions were conducted at University of Hamburg. Reported is the mean of $\overline{P}$, P_{50}, and the number of units sold – see definitions in Section 2.2) – separately for the ‘old’ and ‘new’ data. All calculations restricted to periods 15-27. Both the new and old data have 52 session-periods for control and evasion. There are 39 session-periods in the old tax credit data and 65 session-periods in the new tax credit data. *Evasion* indicates participants with an evasion opportunity, *Control* indicates subjects without evasion opportunity, and *Tax Credit* indicates participants without evasion opportunity but with tax credit. Standard deviations in parentheses.

Table A.2: Prices and Quantities by Experimental Group (experiment 1)

	Average Price		Median Price		Units Sold	
	Control	Evasion	Control	Evasion	Control	Evasion
			Panel A: All Periods			
Mean	55.07	51.18	54.67	51.17	6.01	6.34
	(1.52)	(1.64)	(1.36)	(1.78)	(0.34)	(0.34)
			Panel B: Period>14			
Mean	54.56	51.33	54.18	51.36	5.89	6.23
	(1.48)	(1.91)	(1.37)	(1.90)	(0.46)	(0.37)
N	8	8	8	8	8	8
PValue		0.001		0.001		0.098

Notes: Reported are the mean and median price in a given market period as well as *Units Sold*, which is the market-level mean of units sold in a given market period, by experimental condition of the first experiment (see definitions in Section 2.2). Standard deviations in parentheses. *Evasion* indicates markets with an evasion opportunity and *Control* indicates markets without evasion opportunity. All numbers and statistics are based on 16 independent market-level observations (8 control, 8 evasion). Panel A uses all completed contracts from periods 1 to 27 and Panel B uses all completed contracts in periods 15 to 27. P-value is for the exact Wilcoxon ranksum test based on 16 independent market-level observations; null hypothesis is that there is no difference between evasion and control group.

Table A.3: Prices and Quantities by Experimental Group (experiment 2)

	Average Price		Median Price		Units Sold	
	Evasion	Tax Credit	Evasion	Tax Credit	Evasion	Tax Credit
			Panel A: All Periods			
Mean	51.26	49.99	51.27	49.87	6.52	6.78
	(1.51)	(1.48)	(1.82)	(1.42)	(0.27)	(0.20)
			Panel B: Period>14			
Mean	51.66	49.98	51.69	50.01	6.46	6.80
	(1.26)	(1.45)	(1.33)	(1.48)	(0.31)	(0.16)
N	4	8	4	8	4	8
PValue		0.016		0.008		0.028

Notes: Reported are the mean and median price in a given market period as well as *Units Sold*, which is the market-level mean of units sold in a given market period, by experimental condition of the second experiment (see definitions in Section 2.2). Standard deviations in parentheses. *Evasion* indicates markets with an evasion opportunity and *Tax Credit* indicates markets without evasion opportunity, but a tax credit. The effective tax burden is comparable across these two groups. All numbers and statistics are based on 12 independent market-level observations (8 tax credit, 4 evasion). Panel A uses all completed contracts from periods 1 to 27 and Panel B uses all completed contracts in periods 15 to 27. P-value is for the exact Wilcoxon ranksum test based on 12 independent market-level observations; null hypothesis is that there is no difference between evasion and control group.

Table A.4: Impact of Evasion Opportunity on Mean Price and Units Sold: Adjustment for Small Number of Clusters (experiment 1)

	Model 1	Model 2	Model 3	Model 4
Average Price	-3.228***	-3.228***	-2.701**	-2.701**
	(1.031)	(1.030)	(1.090)	(1.089)
Units Sold	0.337	0.337	0.309	0.309
	(0.220)	(0.220)	(0.242)	(0.242)
Obs.	208	208	208	208
Clusters	16	16	16	16
Period FE	No	Yes	No	Yes
Controls	No	No	Yes	Yes

Notes: Effect of evasion opportunity on outcome variables mean price in a given market period and units sold in a market period. Standard errors in parentheses are adjusted for clustering at the market level and corrected for the small number of clusters using the wild-bootstrap-t procedure described in Cameron et al. (2008). Results from Experiment 1. Regression results for the effect of the *evasion* group (where sellers have evasion opportunity; experiment 1) relative to the control group (without evasion opportunity; experiment 1). The sample includes eight independent markets in the control group and eight independent markets in the evasion group (for a total of 16 markets/clusters). The correction is implemented using Stata code provided by Judson Caskey and is available here: `https://sites.google.com/site/judsoncaskey/data`. * significant at 10%; ** significant at 5%; *** significant at 1%. All columns use completed contracts from periods 15 to 27. Number of observations is 208 (=16 markets $\times$ 13 periods). Control variables include the average age, share of males, share of native German speakers, and share of subjects whose field of study is economics. These averages and shares are calculated at the market-level.

Table A.5: Impact of Evasion Opportunity on Mean Price and Units Sold: Adjustment for Small Number of Clusters (experiment 2)

	Model 1	Model 2	Model 3	Model 4
Average Price	1.684**	1.684**	1.037	1.037
	(0.796)	(0.796)	(1.093)	(1.092)
Units Sold	-0.337*	-0.337*	-0.355**	-0.355**
	(0.202)	(0.202)	(0.175)	(0.175)
Obs.	156	156	156	156
Clusters	12	12	12	12
Period FE	No	Yes	No	Yes
Controls	No	No	Yes	Yes

Notes: Effect of evasion opportunity on outcome variables mean price in a given market period and units sold in a given market period. Standard errors in parentheses are adjusted for clustering at the market level and corrected for the small number of clusters using the wild-bootstrap-t procedure described in Cameron et al. (2008). Results from Experiment 2. Regression results for the effect of the *evasion* group (where sellers have evasion opportunity; experiment 1) relative to the tax credit group (where sellers face the same effective tax rate as in the evasion group, but do not have to evade to arrive there; experiment 2). To have identical effective tax rates in the two groups of interest, the regressions only include the four initial evasion group sessions (see Section 3 for further explanation). The sample includes eight independent markets in the tax credit group and four independent markets in the evasion group (for a total of 12 markets/clusters). The correction is implemented using Stata code provided by Judson Caskey and is available here: `https://sites.google.com/site/judsoncaskey/data`. * significant at 10%; ** significant at 5%; *** significant at 1%. All columns use completed contracts from periods 15 to 27. Number of observations is 156 (=12 markets $\times$ 13 periods. Control variables include the average age, share of males, share of native German speakers, and share of subjects whose field of study is economics. These averages and shares are calculated at the market-level.

Table A.6: Impact of Evasion Opportunity on Ask Price and Median Price (experiment 1)

	Model 1	Model 2	Model 3	Model 4
	Panel A: Ask Price			
Evasion	-3.180***	-3.178***	-2.611***	-2.609***
	(0.830)	(0.835)	(0.806)	(0.809)
Constant	54.473***	54.524***	63.879***	63.947***
	(0.511)	(0.460)	(7.566)	(7.668)
Obs.	1261	1261	1261	1261
	Panel B: Median Price			
Evasion	-2.817***	-2.817***	-2.073**	-2.073**
	(0.802)	(0.827)	(0.738)	(0.761)
Constant	54.178***	54.252***	64.898***	64.972***
	(0.469)	(0.429)	(6.407)	(6.669)
Obs.	208	208	208	208
Clusters	16	16	16	16
Period FE	No	Yes	No	Yes
Controls	No	No	Yes	Yes

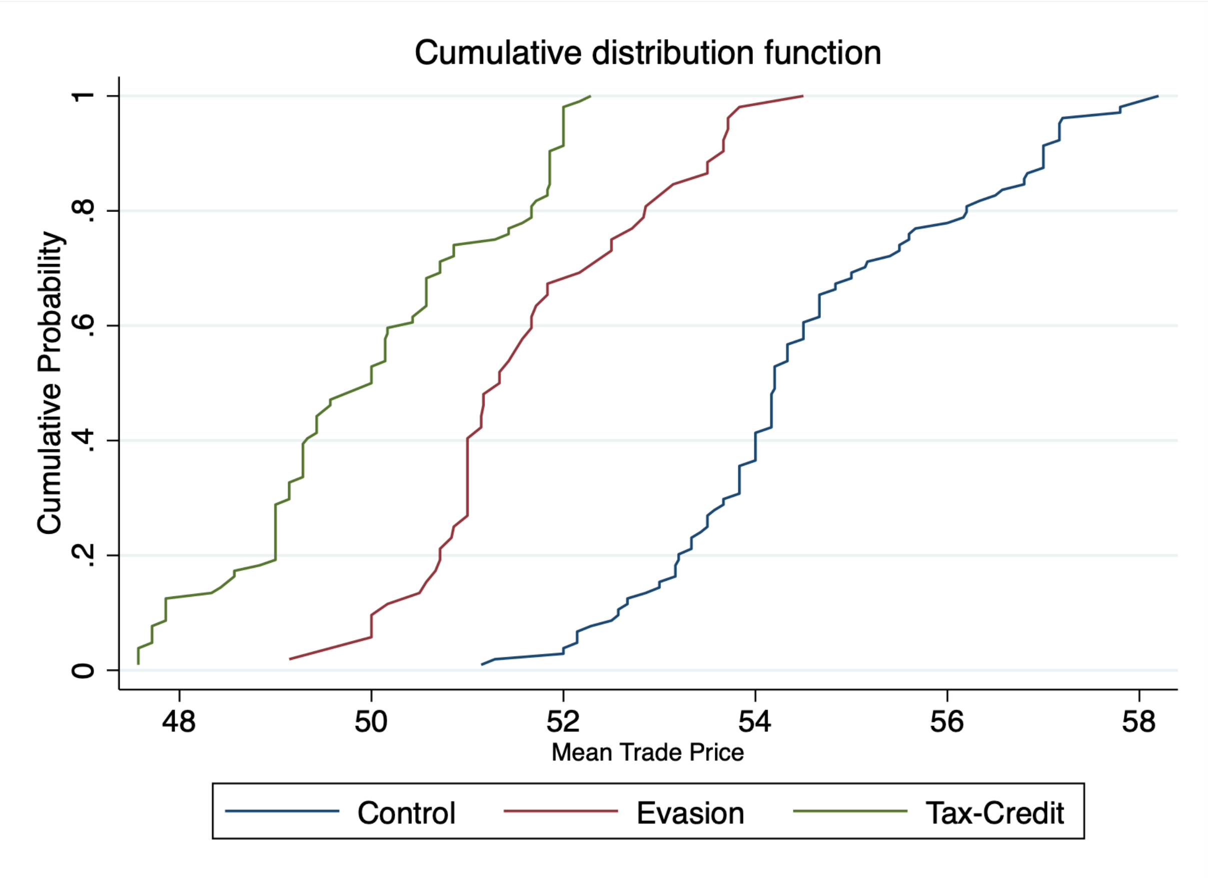
Notes: Robust standard errors adjusted for clustering at the market level are in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%. Estimates are based on equation (6) with the dependent variable defined as market price for each good sold in Panel A and median price in a given market period in Panel B. Results from Experiment 1. Regression results for the effect of the *evasion* group (where sellers have evasion opportunity; experiment 1) relative to the control group (without evasion opportunity; experiment 1). All panels use completed contracts from periods 15 to 27. The sample includes eight independent markets in the control group and eight independent markets in the evasion group (for a total of 16 markets/clusters). Period FE is period fixed effects. Control variables include the average age, share of males, share of native German speakers, and share of subjects whose field of study is economics. These averages and shares are calculated at the market-level.

Table A.7: Impact of Evasion Opportunity on Ask Price and Median Price (experiment 2)

	Model 1	Model 2	Model 3	Model 4
	Panel A: Ask Price			
Evasion	1.676**	1.677*	1.006	1.006
	(0.758)	(0.762)	(0.716)	(0.720)
Constant	49.958***	49.732***	78.941***	78.735***
	(0.499)	(0.518)	(9.967)	(10.087)
Obs.	1043	1043	1043	1043
	Panel B: Median Price			
Evasion	1.683*	1.683*	0.829	0.829
	(0.791)	(0.824)	(0.749)	(0.780)
Constant	50.010***	49.814***	79.068***	78.873***
	(0.511)	(0.554)	(9.489)	(9.991)
Obs.	156	156	156	156
Clusters	12	12	12	12
Period FE	No	Yes	No	Yes
Controls	No	No	Yes	Yes

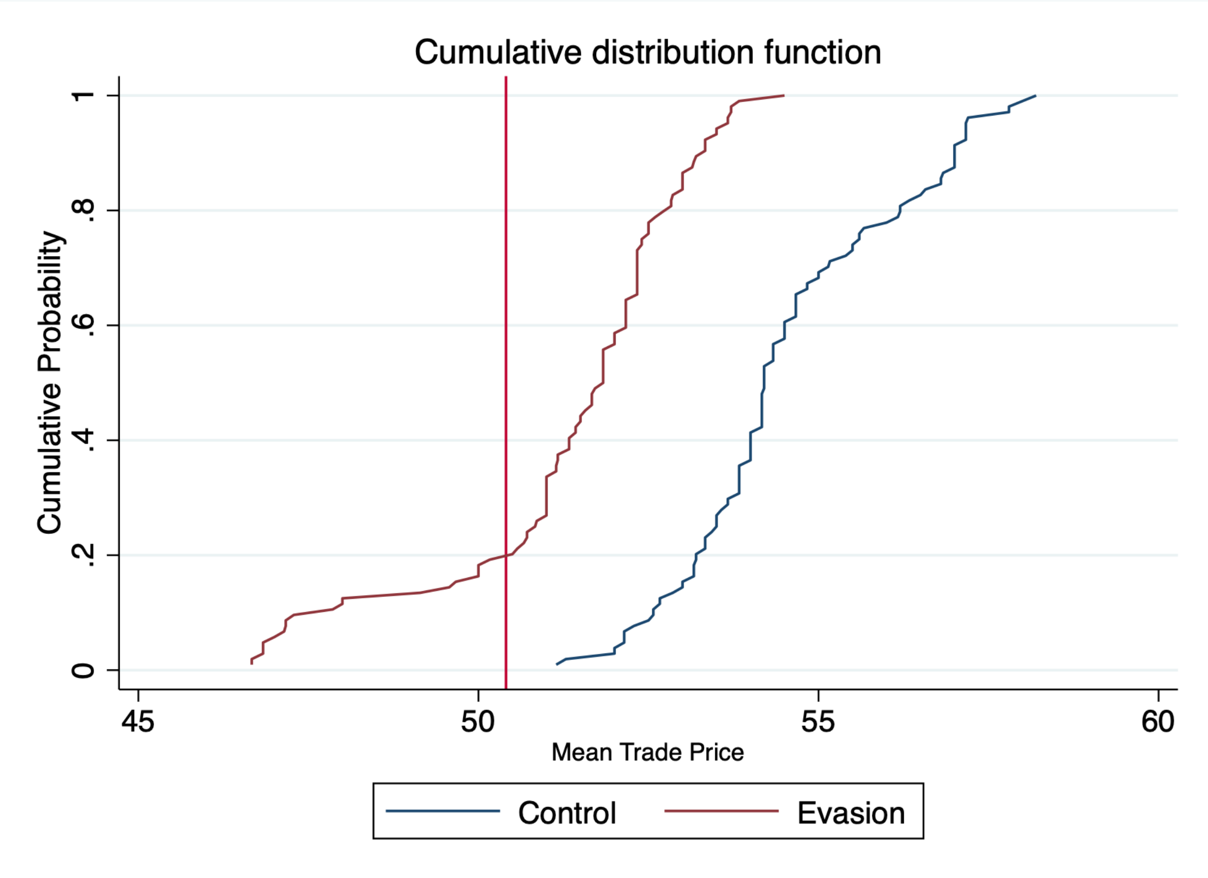
Notes: Robust standard errors adjusted for clustering at the market level are in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%. Estimates are based on equation (6) with the dependent variable defined as market price for each good sold in Panel A and median price in a given market period in Panel B. Results from Experiment 2. Regression results for the effect of the *evasion* group (where sellers have evasion opportunity; experiment 1) relative to the tax credit group (where sellers face the same effective tax rate as in the evasion group, but do not have to evade to arrive there; experiment 2). To have identical effective tax rates in the two groups of interest, the regressions only include the four initial evasion group sessions (see Section 3 for further explanation). All panels use completed contracts from periods 15 to 27. The sample includes eight independent markets in the tax credit group and four independent markets in the evasion group (for a total of 12 markets/clusters). Period FE is period fixed effects. Control variables include the average age, share of males, share of native German speakers, and share of subjects whose field of study is economics. These averages and shares are calculated at the market-level.

Figure A.1: Cumulative Distribution of Mean Price (experiments 1 and 2)



Notes: Reported is the cumulative distribution of mean market price $\overline{P}$ for the evasion condition, control condition, and tax credit condition (no evasion opportunity, but with tax credit). Distributions are based on data from market periods 15 to 27. The sample includes eight independent markets in the control condition, four independent markets in the evasion condition, and eight independent markets in the tax-credit condition. Two-sample Kolmogorov-Smirnov test for equality of distribution functions reports a maximum difference in distributions between the evasion and tax-credit group of 0.53 with pvalue of 0.000. This implies that the null hypothesis that the distributions are equal is rejected.

Figure A.2: Cumulative Distribution of Mean Price (experiment 1)



Notes: Reported is the cumulative distribution of average market price $\overline{P}$ for the evasion and control condition in the first experiment. Distributions are based on data from market periods 15 to 27. The sample includes eight independent markets in the control condition and eight independent markets in the evasion condition. Two-sample Kolmogorov-Smirnov test for equality of distribution functions reports a maximum difference in distributions of 0.72 with pvalue of 0.000. This implies that the null hypothesis that the distributions are equal is rejected.

B Treatment Effects on Units Sold and After-Tax Income

Aside from studying the effect of evasion opportunities on prices, our experimental setting also allows us to study quantity effects. Combining the price and quantity effects then also sheds light on the evasion-opportunity effects on after-tax income of buyers and sellers. In the following, we first report the quantity effects in the first and second experiment, and then investigate effects on after-tax income.

Quantity Effects in the First Experiment. Using the same strategy that we use to study price effects, we report the evasion-opportunity effect on units sold. In particular, the non-parametric analysis is based on the mean number of units sold at the market level (with 16 markets), while the regression analysis is based on the number of units sold in a market-period with standard errors clustered at the market-level (16 clusters).

The results in Appendix Table A.2 show that the mean number of units sold per period in the control group is **5.89**, whereas the evasion group sold an average of **6.23** units per period. We thus find a difference of 0.34 units. This difference between units sold in the evasion and control group is borderline statistically significant (based on non-parametric test with 16 independent observations). The trend of units sold over all periods in the two groups is displayed in Figure B.1 below.) The difference in units sold between the two conditions is even more obvious when we look at the total number of units sold by each group. Again, restricting attention to trading periods 15 to 27 (after the market clears), we find that the evasion condition sold a total of 648 units while the control condition only sold 613 units. Corresponding numbers for periods 1 to 27 are 1370 and 1299 in the evasion and control condition, respectively.

Considering regression results in Panel A of Table B.1 below, we find a stable treatment coefficient between 0.34 and 0.31 (depending on specification) which is not statistically significant. However, we do find a statistically significant effect (at the 10% level) as we consider all periods in our sample (not reported). Overall, we thus find that evasion markets tend to trade more units than control markets, but the effects are imprecisely measured.

Quantity Effects in the Second Experiment. As before, we exlude the new evasion sessions when we consider the effects of the tax-credit treatment, implying that we focus on 8 tax-credit markets and four evasion markets. The results in Appendix Table A.3 show that the mean number of units sold per period in the tax credit is **6.80**, whereas the evasion group sold an average of **6.46** units per period. We thus find a difference of 0.34 units. This difference between units sold in the evasion and tax credit group is statistically significant (based on non-parametric test with 12 independent observations). Panel B of

Table B.1 below compares the evasion opportunity group of the first experiment with the tax credit group of the second experiment in a regression approach. The Table shows that the coefficient of interest is negative and statistically significant in all four regression specifications.

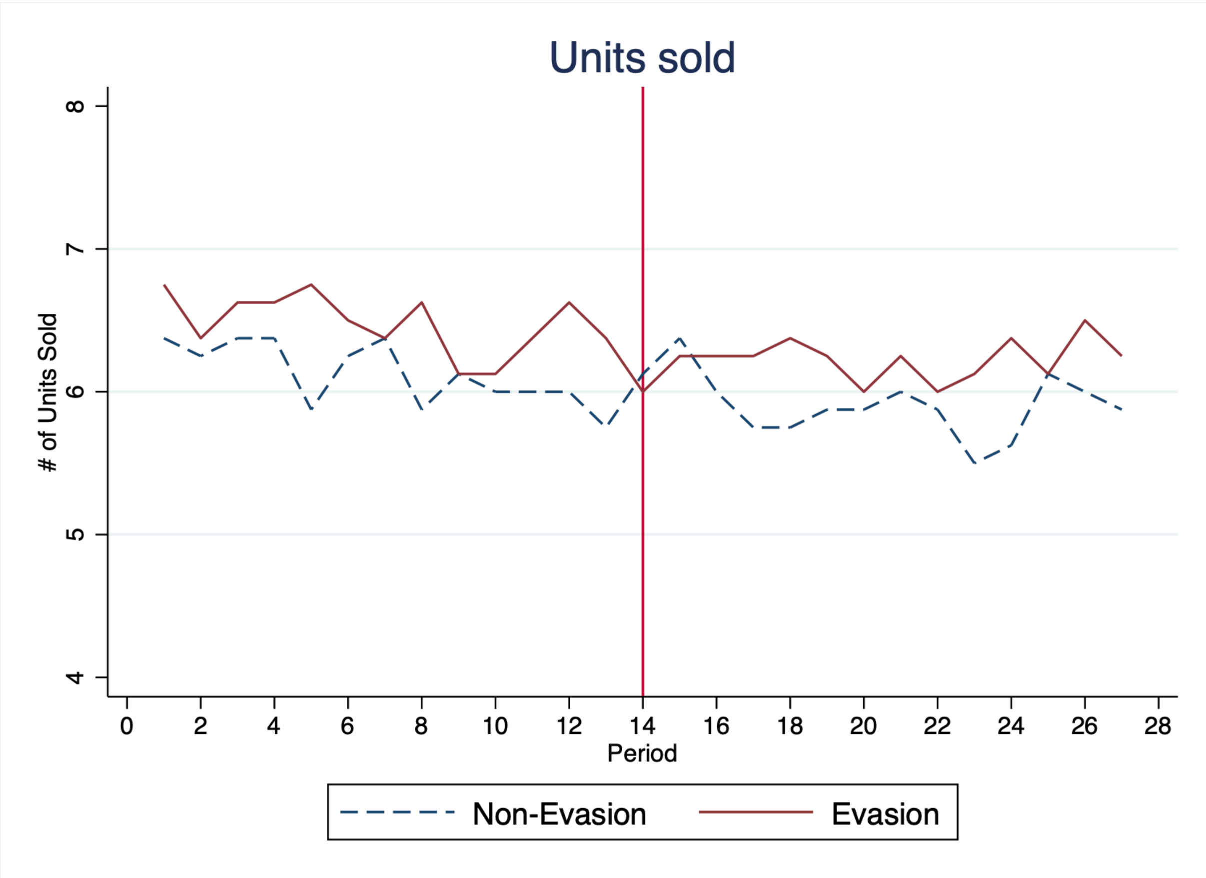
Consistent with the price effects that we find, these findings indicate less units sold in the evasion opportunity group relative to the tax credit group. This supports the notion that markets with evasion opportunity have different trading outcomes than tax credit markets despite equal effective tax burden.

Table B.1: Impact of Evasion Opportunity on Units Sold

	Model 1	Model 2	Model 3	Model 4
	Panel A: Tax Burden not Constant			
Evasion	0.337	0.337	0.309	0.309
	(0.203)	(0.209)	(0.195)	(0.201)
Constant	5.894***	6.144***	5.339***	5.589***
	(0.157)	(0.160)	(1.669)	(1.780)
Obs.	208	208	208	208
Clusters	16	16	16	16
	Panel B: Tax Burden Constant			
Evasion	-0.337*	-0.337*	-0.355**	-0.355**
	(0.153)	(0.160)	(0.119)	(0.124)
Constant	6.798***	6.612***	4.907**	4.721**
	(0.057)	(0.138)	(1.880)	(1.909)
Obs.	156	156	156	156
Clusters	12	12	12	12
Period FE	No	Yes	No	Yes
Controls	No	No	Yes	Yes

Notes: Robust standard errors adjusted for clustering at the market level are in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%. Panel A: Regression results for the effect of the *evasion* group (where sellers have evasion opportunity; experiment 1) relative to the control group (where sellers do not have an evasion option; experiment 1). The sample includes eight independent markets in the control group and eight independent markets in the evasion group (for a total of 16 markets/clusters). Panel B: Regression results for the effect of the *evasion* group (where sellers have evasion opportunity; experiment 1) relative to the tax credit group (where sellers face the same effective tax rate as in the evasion condition, but do not have to evade to arrive there; experiment 2). To have identical effective tax rates in the two groups of interest, Panel B regressions only include the four initial evasion group sessions (see Section 3 for further explanation). The sample in Panel B thus includes four independent markets in the evasion group and eight independent markets in the tax credit group (for a total of 12 markets/clusters). Estimates in both Panels are based on equation (6) with the dependent variable defined as the number of units sold in a given market period. Results use completed contracts from periods 15 to 27. Period FE is period fixed effects. Control variables include the average age, share of males, share of native German speakers, and share of subjects whose field of study is economics. These averages and shares are calculated at the market-level.

Figure B.1: Units Sold by Period and Treatment (Experiment 1)



Notes: Reported is the number of units sold in each period for the treatment (with evasion opportunity) and control (without evasion opportunity) groups; i.e., first experiment. The sample includes eight independent markets in the control group and eight independent markets in the evasion group.

Effects on After-tax Income. Experiment 1: Because markets with access to evasion trade at lower prices and higher quantity, the presence of tax evasion should lead to an increase in buyers' net income relative to buyers in the control group. Additionally, sellers' net income might also increase despite the lower price, because they only report a fraction of their true sales. Our findings are consistent with these predictions. In the absence of tax evasion (i.e., in the control group), average net income of buyers in equilibrium periods 15-27 is 229 ECU compared to sellers' net income of 190 ECU. The introduction of tax evasion opportunities increases buyers' average net income to 279 ECU and sellers' average net income to 260 ECU. These represent increases of 50 ECU and 70 ECU for buyers and sellers, respectively.

These effects are consistent with the observed price changes. Buyers' net incomes increase, because they pay 2.7 ECU less per unit in the evasion condition. Although sellers in the evasion condition receive 2.7 ECU less per unit, their effective tax rate falls by a larger margin (approximately 7.5 ECU) due to their evasion opportunity. As a result, both buyers and sellers experience an increase in net income, but sellers receive a much larger increase.

Experiment 2: In the second experiment, the average net incomes of both sellers and buyers increase in the tax credit group relative to the control group; the increase amounts to 79 ECU for buyers and 66 ECU for sellers, both relative to the control group without evasion opportunities. That is, for buyers the positive effect of the tax credit is larger than the positive effect of the evasion opportunity. This is consistent with the observation that the equilibrium price in the tax credit group is lower than in the evasion group. In contrast, because sellers in the tax credit treatment face the same tax rate as in the evasion treatment, but receive a lower price, the positive effect of the tax credit on net incomes of sellers is lower than the positive effect of the evasion opportunity.

C Summary Statistics

After the respective experimental session, subjects reported their age, gender, native language, level of tax morale, risk preference, and field of study. Tax morale is determined using a question very similar to one used in the World Values Survey (Inglehart nd): "How justified do you think it is to evade taxes if there was an opportunity to do so?". Subjects could reply on a 10-point scale ranging from '0 Always Justified' to '10 Not At All justified'. We generate a dummy variable indicating high tax morale that has value 1 if a subject reported that it is never justified to evade taxes.[12] Risk aversion is measured with a question that gives subjects the choice between a certain payment and a gamble whose expected payoff is the same as the certain payment.[13] Each of these variables is summarized in Table C.1. Non-parametric Wilcoxon rank-sum tests for differences in distributions between groups shed light on whether randomization was successful.

We find one statistically significant difference between the evasion group and the control group: the share of male participants, as indicated by *p-value(a)*. Comparing the tax credit group and the 'old' evasion group that we use in Table 3, we see two statistical differences, as reported in *p-value(c)*: the share of male participants and the share of participants majoring in Economics. We include control variables (incl. gender, age, econ major) in some treatment-effect regressions to rule out that our treatment effects are driven by observable characteristics.

[12]The WVS question reads: "Please tell me for the following statement whether you think it can always be justified, never be justified, or something in between: 'Cheating on taxes if you have the chance'." This is the most frequently used question to measure tax morale in observational studies (e.g., Alm and Torgler 2006 and Halla 2012). The original German question in our questionnaire reads: 'Fuer wie in Ordnung halten Sie es, Steuern zu hinterziehen, wenn sich die Moeglichkeit dazu ergibt?'

[13]A subjects is classified risk neutral if indifferent between the options, risk averse if prefers the certain payment and risk seeking if prefers the gamble. The original German question in our questionnaire reads: 'Bitte stellen Sie sich die folgenden Situationen vor: Situation A: Sie erhalten eine Auszahlung von EUR 50. Situation B: Es wird eine Muenze geworfen. Sie erhalten EUR 100, wenn Kopf erscheint. Sie erhalten EUR 0, wenn Zahl erscheint. Welche Situation wuerden Sie bevorzugen? a) Ich wuerde Situation A bevorzugen, b) Ich wuerde Situation B bevorzugen, c) Ich bin indifferent zwischen den beiden Situationen.

Table C.1: Summary Statistics of Demographic Variables

	Gender	Age	German	Tax Morale	Econ major	Risk Aversion
			Panel A: Control Group			
Mean	0.38	25.59	0.66	0.28	0.33	0.80
	(0.487)	(6.856)	(0.476)	(0.449)	(0.471)	(0.403)
# of Subjects	80	80	77	80	80	80
			Panel B: Evasion Group			
Mean	0.53	27.16	0.63	0.23	0.39	0.75
	(0.503)	(10.32)	(0.485)	(0.420)	(0.490)	(0.436)
# of Subjects	80	80	79	80	80	80
P-value(a)	0.057	0.263	0.701	0.467	0.411	0.450
			Panel C: Tax Credit Group			
Mean	0.40	26.23	0.54	0.17	0.34	0.78
	(0.493)	(4.097)	(0.502)	(0.382)	(0.476)	(0.420)
# of Subjects	80	80	76	80	80	80
P-value(b)	0.746	0.050	0.122	0.131	0.867	0.700
P-value(c)	0.039	0.251	0.534	0.514	0.049	0.373

Notes: Reported are the mean characteristics of all three experimental conditions; i.e., experiments 1 and 2 (with standard deviation in parentheses). Gender is a dummy that is equal to 1 if male, German is a dummy that is equal to 1 if native language is German, Econ is a dummy that is equal to 1 if major field of study is economics or business administration, tax morale is a dummy that is equal to 1 for subjects who believe cheating on taxes can never be justified, Risk is a dummy that is equal to 1 if risk averse (see the text in Appendix C for detailed explanations about the questionnaire questions and their definition. Note that tax morale and the risk variable is measured after the experiment, and is thus not predetermined). Four subjects did not report his/her language. Reported p-value are for the Wilcoxon rank-sum test; null hypothesis is that there is no difference in the characteristics between two groups. P-value (a) compares control group and evasion group. P-value (b) compares control group and tax credit group. P-value (c) compares the tax credit group to the 'old' evasion group that we use in Table 3.

D Motivation to use Lab Experiment and Discussion of External Validity

Our decision to use a laboratory experiment is based on the fact that causal identification requires random variation in access to evasion across otherwise similar markets. This is difficult to achieve using archival data since it is always an endogenous choice of firms to operate in markets where evasion is an option. Additionally, it would be very difficult to find an archival data set that includes information about both the evasion opportunity of the firm and the prices at which this firm sells its goods to buyers. We employ an experimental double auction similar to Grosser and Reuben (2013). These kinds of double auction markets have been used extensively in the experimental literature to study the incidence of taxes; e.g., (Kachelmeier et al. 1994; Borck et al. 2002; Ruffle 2005; Riedl 2010). The tax evasion component of our experiment also builds on established work from experimental research (e.g., Ruffle 2005, Fortin et al. 2007, Doerrenberg and Duncan 2014, Balafoutas et al. 2015, Blaufus et al. 2016, Austin et al. 2020). Our experimental design thus combines established design features from the experimental literature on double auctions, tax incidence, and tax evasion.

To which extent can our results be generalized to the 'real world'? As with almost all economic laboratory experiments, there remains doubt about the external validity of our results.[14] One general concern is that the setting in the lab is abstract and artificial. However, the literature shows that laboratory double auctions, which we use in our experiment, generate very plausible equilibria (e.g., Smith 1962; Holt 1995; Dufwenberg et al. 2005; Grosser and Reuben 2013). This suggests that our experimental setting is appropriate to study prices and quantities as outcome variables. In addition, although subjects trade in fictitious goods, they receive actual money pay-offs and thus face incentives similar to buyers and sellers in actual markets. Furthermore, the question of tax incidence (without tax evasion) has been widely studied in the laboratory setting (e.g., Riedl and Tyran 2005; Ruffle 2005; Cox et al. 2018; Grosser and Reuben 2013) and shown to lead to results that reflect theoretical predictions very well.

In order to make the tax evasion decision as realistic as possible we used actual tax terminology and announced to the participants that all tax revenue would be donated to the German Red Cross, a non-ideological charity organization that is usually perceived as reliable and transparent. That is, we made clear to participants that the revenue from the laboratory tax does not simply flow back to our research budget. This design choice thus contributes to mimicking the real-world situation where tax revenues are spent for a purpose and are not just wasted.[15]

[14]The generalizability of lab experiments is discussed by Falk and Heckman (2009). We restate some of their arguments here and translate them to our specific context.

[15]Tax morale research (Torgler 2007) finds that taxpayers are more likely to comply with tax laws if

Additionally, although evasion may occur among buyers as well, the real-world problem seems to be more relevant among sellers; sellers are usually responsible for remitting sales taxes to the government. In this sense, our laboratory setting mimics the operation of most transaction taxes in the real world in that we also have a set-up in which sellers remit the tax.[16] We acknowledge that prices and quantities on real-world markets, such as the retail commerce market, are not determined in a competitive double auction setting with full information of all actors. However, many real-world markets are considerably close to competitive markets and are characterized by a situation where both sellers and buyers have full information about prices (especially now that prices are very transparent online and easy to compare) and where these prices are determined in the interplay between supply and demand.

A further concern of generalizability relates to the costs of evasion in our empirical design. While our audit rate of 10% seems low, there is evidence of "real-world" tax systems with significantly lower audit rates. For example, a news article revealed that the tax agency in the state of Mississippi "audited just 2 percent of businesses operating in the state [in fiscal year 2012]."[17] While this does not necessarily imply that each firm faced an audit rate of 2%, it does suggest that our audit rate of 10% is not unreasonable. One might also be concerned that our design uses an exogenously determined audit whereas audit probabilities tend to be endogenous in the real world. But here too, we wish to note that exogenous audits are not uncommon. For example, tax gap estimates in the US are based on data from random audits. More importantly, we argue that the qualitative result we observe with random audit should carry through with endogenous audits. In particular, the main point is that market equilibrium is affected by access to evasion and this has implications for tax incidence and the distribution of tax burdens. It's possible that the magnitude of these effect might differ between exogenous and endogenous audit regimes. But the main point remains; we should expect different market outcomes when evasion is at play.

they believe that the tax revenue is spent transparently. Eckel and Grossman (1996) show that dictators share more in dictator games if the recipient is the American Red Cross. Overall, we donated EUR 714 to the Red Cross (including all treatments).

[16] The political purpose of transaction taxes such as VAT usually is that buyers pay the tax while sellers remit it. However, just as in our experiment, the actual economic burden of the tax in the real world is eventually determined in the interplay between demand and supply of buyers and sellers.

[17] The article is online here: `https://www.washingtontimes.com/news/2014/nov/10/sales-tax-dodging-on-the-rise-in-mississippi/`.

E Conceptual Framework: Potential Effects of Evasion Opportunity on Prices

This section provides a conceptual framework of the relationship between evasion opportunities and market prices. Note that we deliberately derive predictions for the situation where the evasion opportunity effect may run through a reduction of the tax burden. The case with constant tax burdens is discussed in Section 3. There are generally two opposing theoretical predictions for the effect of evasion opportunities on prices. We describe the rationale behind both predictions in the following.

E.1 Evasion Opportunity Affects Prices

For simplicity, let's assume that demand and supply curves are linear. Figure E.1 illustrates the effect of taxes on price for the cases with and without evasion. First, consider Panel A, a situation where evasion is not possible. As in the standard textbook case, the supply curve shifts up by the full amount of the nominal tax rate. This results in a new market equilibrium (p_c, q_c), where subscript c indicates the control group in our experiment where evasion is not possible (more on this below).

Sellers with an opportunity to evade taxes can hide a fraction of their sales. A seller who underreports sales and is not audited faces an effective tax rate that is lower than the nominal tax rate faced by sellers without an evasion opportunity. As illustrated in Panel B of Figure E.1, this lower effective tax rate then implies that the market supply curve in the presence of evasion opportunities shifts up by less than the nominal tax rate. This results in a new market equilibrium at (p_t, q_t), where subscript t indicates the experimental group with evasion opportunity (more on this below).

This intuition leads to a qualitative prediction: the equilibrium price in markets with evasion opportunities will be lower than in markets where evasion is not an option; i.e., $(p_t < p_c)$.[18]

The quantitative difference between the equilibrium prices in the evasion and non-evasion markets is determined by the magnitude of the shift in the evasion market's market supply curve. This shift is positively related to the effective tax rate faced by sellers in the evasion groups.[19] To illustrate things further, consider the following simple sales tax setting (which is applicable to our experimental design; see below). Sellers have to pay a nominal per-unit (excise) tax τ for each unit they sell, but are provided a tax

[18] Accordingly, the number of units sold will be higher in the evasion markets than in markets without evasion; i.e., $(q_t > q_c)$.

[19] Note that sales taxes (which we study in our experiment; see below) and pure profits based income taxes are likely to have very different effects on prices and quantities. In fact, a change in tax rate will not affect the equilibrium price in the case of profit-based income taxes because the price that maximizes profits X will be the same as the price that maximizes $(1 - \tau_{profits})X$.

reporting decision. The tax reporting decision is audited with an exogenous probability γ, and because all audits lead to the full discovery of actual sales, a fine rate of two must be paid if an evader is audited. This implies that seller i has to pay an (expected) effective tax rate of:

$$t_i^e = \frac{\tau(r_i + 2\gamma(s_i - r_i))}{s_i}, \tag{7}$$

where s_i denotes the number of units a seller actually sells and r_i is the number of units she reports.[20] This simple equation shows that the effective tax rate is increasing in the nominal tax rate and decreasing in evasion (for $\gamma \leq 0.5$).[21] Therefore, an increase in evasion implies a smaller shift in the market supply curve. While it is plausible to expect that the evasion rate will be larger than zero, it is difficult to predict the exact level of evasion ex-ante, and it is therefore not possible to make any predictions regarding the quantitative effects of the treatment on prices.[22]

E.2 Evasion Opportunity Does *Not* Affect Prices

There are at least two potential reasons why the opportunity to evade taxes may not affect market prices.

Separability. First, one finding in the theoretical literature is that firms treat their evasion and pricing decisions as separable; sellers first set a price at which to sell, and then later make their evasion decision (Yaniv 1995; Bayer and Cowell 2009). Intuitively, the separability result is analogous to other types of uncertainty models; for example, investment models in which the decision over how much to invest in total is separable from the decision on how much to invest in individual assets (Cass and Stiglitz 1970; Okawa and van Wincoop 2012). In this case, the opportunity to evade has no bearing on market prices. The separability result thus implies that the equilibrium price that arises in a market *with* evasion opportunities is the same as in a market *without* evasion opportunities (i.e., $p_t = p_c$).

[20] The seller's tax liability (including any fines) is (τr_i) with probability $(1-\gamma)$, and $(\tau s_i + \tau(s_i - r_i))$ with probability γ. Therefore, the expected effective tax rate can be written as $t_i^e = \frac{(1-\gamma)\tau r_i + \gamma(\tau s_i + \tau(s_i - r_i))}{s_i}$, which is equivalent to equation (7). Note that this effective tax rate reduces to the nominal tax rate τ for sellers who either do not evade or do not have an option to evade.

[21] Because the fine rate is two, evasion is only profitable for $\gamma \leq 0.5$. In this case, the effective tax rate is decreasing in evasion. Evasion is not profitable when $\gamma > 0.5$. In this case, the effective tax rate is increasing in evasion.

[22] It is difficult to predict the exact level of evasion, because, as we know from the tax evasion literature, the decision to evade is complex and depends on several factors including the nominal tax rate, deterrence parameters, the (biased) perception of audit probabilities, the degree of risk aversion, and the intrinsic motivation to pay taxes.

Choice Bracketing. Second, recent literature has elaborated that many taxpayers misperceive taxes. Based on a large review of the literature, Blaufus et al. (2022) show that taxpayers frequently use simplifying heuristics and that they are often rationally inattentive in the context of tax-paying behavior. Bundling the empirical results on tax misperceptions, Blaufus et al. (2022) develop a *Behavioral Taxpayer Response Model* according to which taxpayer behavior depends, among other factors, on tax information such as tax salience, tax complexity, tax framing, and tax timing. One type of such "tax information" that is potentially relevant in the context of our paper relates to choice bracketing behavior: in a sequence of interrelated decisions, subjects often make each decision in isolation, although the decision problem should be solved simultaneously. Such choice bracketing behavior then reduces the cognitive effort of the decision problem. Choice bracketing has been shown to be an important determinant of behavior across many contexts (Read et al. 1999; Read et al. 2006). Recent work by Blaufus et al. (2023) shows that choice bracketing matters in the context of taxation: In a situation where taxpayers first make a production decision and taxation of production-based profit is deferred to a later point of time, taxpayers do not fully take taxes into account in their initial production decision.

Such choice bracketing is potentially relevant in the context of our empirical design (see below for design details). Although the tax in our experimental design seems simple at first glance, sellers in our experiment trade and make pricing decisions in every trading round, and they make evasion decisions every third round. This creates a time gap between the pricing decision and the evasion decision. Furthermore, sellers operate in a fast-paced market where they are required to track bids, asks, number of items sold, tax liability, and time remaining in a trading period. It is not inconceivable that sellers operating in this kind of environment would make the initial pricing decision in isolation of the subsequent evasion decision to minimize mental-decision-making costs.[23]

Of course, there are also reasons to believe that choice bracketing might not arise in the specific context of our experiment. First, participants can potentially learn how the pricing and evasion decisions relate to each other over the 27 trading periods and 9 evasion decisions in our experiment. Also notice that our main results are based on data from periods 15-27, which implies that subjects have 14 trading periods and 4 evasion decisions to learn the connection between pricing and evasion. If subjects are actively learning about the connection between evasion and pricing then we would expect to observe less bracketing behavior (Blaufus et al. 2022).

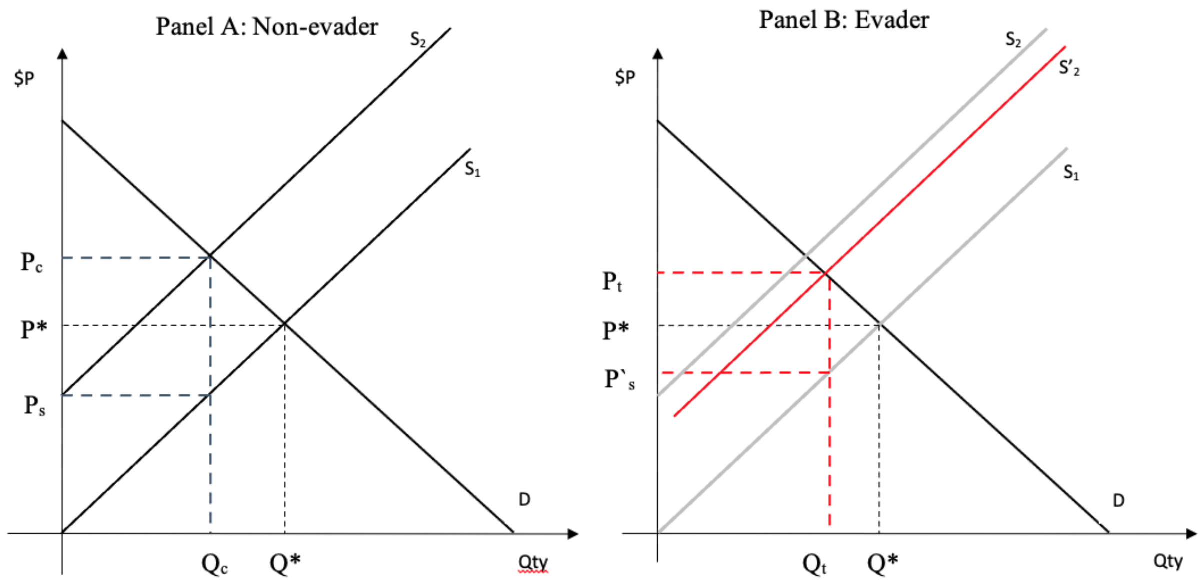
Second, why would choice bracketing apply to tax evasion and not the tax itself; i.e., should choice bracketing affect pricing in the control group too? While it is possible

[23] Note that, in contrast to the above separability result, the choice bracketing rationale also holds for risk averse taxpayers.

that choice bracketing leads subjects to view the pricing decision separately from the tax payment, the data do not suggest that subjects acted in this way. In particular, the observed equilibrium price in the condition without evasion opportunity reflects an equal split of the tax between buyers and sellers as predicted by theory (see Section 4 below). That sellers fully account for the tax in their pricing decisions makes sense since all that is required is adding 10 ECU to the cost of each item. Accounting for tax evasion is a more difficult task. The subject must account for the effective tax rate – rather than the nominal tax rate –, which is only known in expectation since the subject must consider audit probability and penalty.

Separability of decisions and choice bracketing thus both imply that pricing decisions of sellers are made independently of subsequent evasion decisions. It is worth noting that our experiment is not designed to distinguish between choice bracketing and separability. The goal of this section is simply to illustrate potential relationships between access to evasion and pricing behavior of sellers.

Figure E.1: Economic Incidence of Tax on Seller



Notes: The imposition of a per-unit tax would ordinarily cause the supply curve to shift to the left and the market equilibrium to move from point (P^*, Q^*) to (P_c, Q_c) as illustrated in Panel A. Because sellers are able to evade the tax, the supply curve shifts by a smaller amount causing the equilibrium to move from (P^*, Q^*) to (P_t, Q_t) as illustrated in Panel B, where $P_t < P_c$.

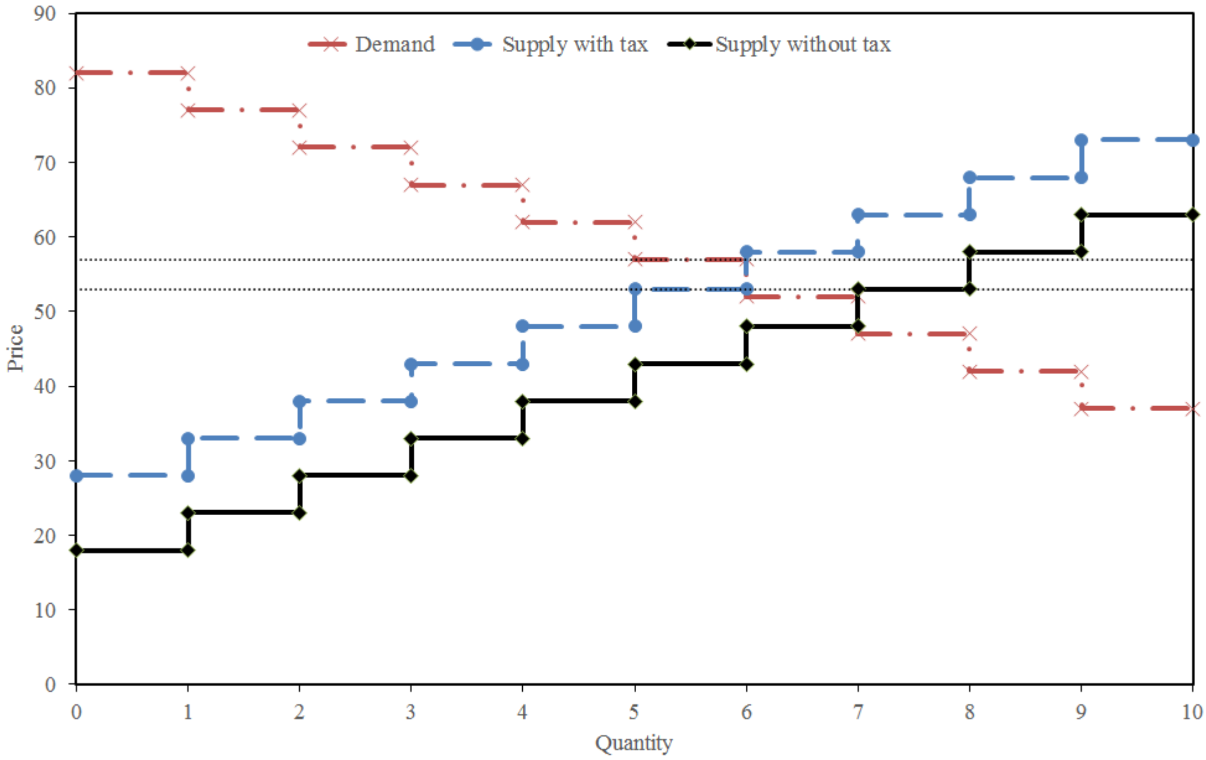
F Additional Information on Experimental Design

Table F.1: Demand and Supply Schedules

Buyer			Seller		
Subject	Value 1	Value 2	Subject	Cost 1	Cost 2
1	82	52	1	18	48
2	77	72	2	23	28
3	67	37	3	33	63
4	62	42	4	38	58
5	57	47	5	43	53

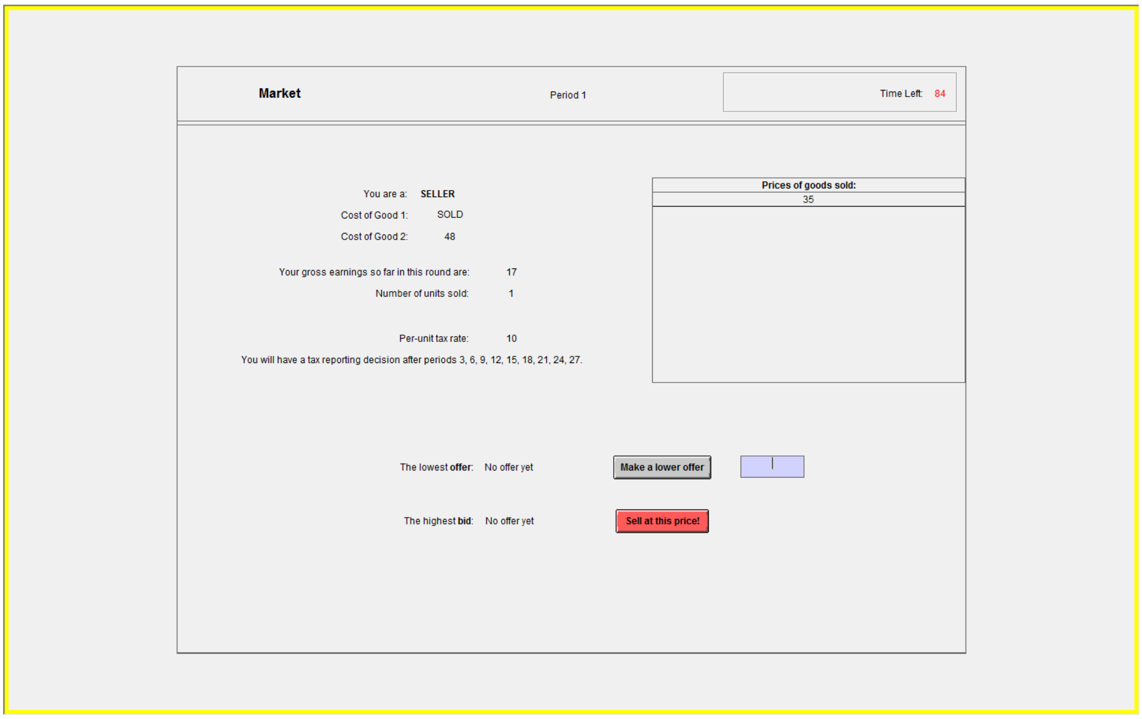
Notes: Reported are demand and supply schedules in the experimental double auction.

Figure F.1: Experimental Supply and Demand Schedule



Note: The figure is adapted from Grosser and Reuben (2013, page 42, Figure 1). It shows the demand schedule for buyers and the supply schedule for sellers with and without the per unit tax. The predicted equilibrium occurs where the curves intersect: quantity $q = 7$ and price p between 48 and 52 without tax and quantity $q = 6$ and price p between 53 and 57 with the ECU 10 per unit tax.

Figure F.2: Screenshot of the Market Place



Note: Screenshot of the lab experimental double-auction market place. The screen displays the market place for a seller in the treatment group with evasion opportunity. The seller has sold her first unit at a price of 35. The cost for the first unit was 18, yielding a current gross-income of 17. Her second unit with cost 48 is not traded at this point. The screen shown is translated to English, the original experiment was conducted in German. The market place is based on Grosser and Reuben (2013).

Motivation for tax collection after every third round. One advantage of allowing subjects to report after every third trading period is that it increases the probability that every subject has a positive amount to report and must therefore explicitly decide if they wish to underreport sales for tax purposes. Another advantage is that it yields 9 reporting decisions. This is advantageous because it means that subjects can learn the implications of tax evasion for their profits and update their beliefs about the probability of being caught. As a result, we can be assured that the market equilibrium in the evasion treatment reflects the impact of tax evasion on the behavior of market participants. Although reporting every period would maximize the number of reporting decisions, we opted against this option because excess supply in the market implies that some subjects will sell zero units in a given trading period, which trivializes the reporting decision. Another option is to have subjects make a single reporting decision at the end of the experiment. While this approach maximizes the chance that everyone has a positive amount to report, having a single reporting decision would not allow subjects to learn or update their beliefs. We opted for every third round as a reasonable compromise between these two extremes. Although subjects in the control group do not make a reporting decision, we collect taxes and report their net profits at the end of every third period to ensure comparability with the treatment group.

G Instructions

The following pages contain the translated instructions. The instructions for all groups were identical except for slight variations. In the following, we display the instructions for the control condition and indicate the differences between conditions in brackets. The original German versions of the instructions are available from the authors upon request.

Instructions

Welcome and thank you for participating in our experiment. From now on until the end of the experiment, please refrain from communicating with other participants. If you do not abide by this rule, we will have to exclude you from the experiment.
We kindly ask you to read the instructions thoroughly. If you have any questions after reading the instructions or during the experiment, please raise your hand and one of the instructors will come to you and answer your question in person. Your payment and your decisions throughout the experiment will be treated confidentially.
You can earn money in this experiment. How much you earn depends on your decisions and the decisions of other participants. During the experiment, your payments will be calculated in a virtual currency: Experimental Currency Units (ECU). **30 ECU correspond to 1 Euro**. After the experiment, your pay-off will be converted to Euro and given to you in cash. Additionally, you will receive a show-up fee of 2.50 Euro.

The Experiment

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

Overview
[***Control Condition:***
The experiment consists of 3 practice rounds and 27 paying rounds. At the beginning of each round, all buyers and sellers 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 in the market place and your earnings depend on your decisions and the decisions

of the other participants. Each unit sold will be subject to a per **unit tax of 10 ECU** for sellers. The tax rate is the same for all sellers and is due at the end of every third round. Details on the market place will be explained further below. All tax revenues paid by you and all other participants will be donated to the German Red Cross.
]

[***Condition with Evasion Opportunity:***
The experiment consists of 3 practice rounds and 27 paying rounds. At the beginning of each round, all buyers and sellers 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 in the market place and your earnings depend on your decisions and the decisions of the other participants. Each unit sold will be subject to a per **unit tax of 10 ECU** for sellers. The tax rate is the same for all sellers and is due at the end of every third round. At the end of every third round, sellers are asked to report the number of units that they sold in the previous three market rounds. There is a 10% chance that the reported decision will be checked for accuracy. Details on the market place will be explained further below. All tax revenues paid by you and all other participants will be donated to the German Red Cross.
]

[***Condition with Tax Credit:***
The experiment consists of 3 practice rounds and 27 paying rounds. At the beginning of each round, all buyers and sellers 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 in the market place and your earnings depend on your decisions and the decisions of the other participants. Each unit sold will be subject to a per **unit tax of 10 ECU** for sellers. Sellers additionally receive a **tax credit** of 7.50 ECU for each unit sold. The tax rate is the same for all sellers and is due at the end of every third round. Details on the market place will be explained further below. All tax revenues paid by you and all other participants will be donated to the German Red Cross.
]

The Market Place

Basics

The market place is opened for two minutes at the beginning of each round. All buyers and sellers trade a fictitious good. In each market period, each **seller can sell two units** of the fictitious good and each **buyer can buy two units** of the good.

Units, costs, and values

If you are a seller, you will be given the **costs** for two units of a fictitious good at the beginning of the experiment. These units shall be denoted "Unit 1" and "Unit 2", where Unit 1 costs less than Unit 2. The cost of these units to you is the same in all rounds. However, the cost of each seller's units will differ from the cost of other sellers' units. Each seller only knows her own costs.

If you are a buyer, you will be given the **values** for two units of a fictitious good at the beginning of the experiment. These units shall be denoted "Unit 1" and "Unit 2" where Unit 1 values more than Unit 2. The value of these units to you is the same in all rounds. However, the value of each buyer's units will differ from the value of other buyers' units. Each buyer only knows her own values.

Asks, Bids, and Transactions

Sellers can make "asks" and Buyers can make "bids" during the trading period. All asks and bids are visible to everyone through the screen that appears during the two minutes of trading. This screen will also state your type (Seller or Buyer), the time left in the trading period and the costs or values that you were assigned for each Unit. Each Seller can first sell Unit 1 and afterward Unit 2. Accordingly, Buyers can first buy Unit 1 and then Unit 2.

Sellers cannot sell goods at prices lower than the assigned cost for the respective Unit.
Buyers cannot buy at prices that exceed their assigned value for the respective Unit.

Sellers can make asks at any time during the trading period but each ask has to be lower than the current lowest ask on the market. Similarly, Buyers can always propose bids as long as they are larger than the current largest bid on the market.

To realize a **transaction**, Sellers can either accept a bid or buyers can accept an ask. The transaction price for the unit will then be equal to the accepted ask or bid.

(Gross) Earnings in the Market Place

Units that are not traded do not yield any earnings. Gross earnings for each Unit are as follows:

For Sellers:

Gross Earnings from selling Unit 1 = transaction price of Unit 1 - cost of Unit 1
Gross Earnings from selling Unit 2 = transaction price of Unit 2 - cost of Unit 2

For Buyers:
Gross Earnings from buying Unit 1 = value of Unit 1 - transaction price of Unit 1
Gross Earnings from buying Unit 2 = value of Unit 2 - transaction price of Unit 2

Screenshots from trading market

Sellers:

Here Screenshot Sellers

Sellers can accept a current bid by pressing "Sell at this Price". To make a new ask, Sellers have to enter their ask price into the field to the right of the "Make a smaller ask" button and press the button to submit the ask.

Buyers:

Here Screenshot Buyers

Buyers can accept the current ask by pressing "Buy at this Price". To make a new bid, Buyers have to enter their bid into the field to the right of the "Make a smaller bid" and press the button to submit the bid.

[*Added in the* ***condition with evasion opportunity****:*

The Reporting Decision for Sellers

After three consecutive trading periods, you will be shown the number of units traded over the three previous trading rounds and the respective gross earnings on those units. For each unit traded in the three previous periods, a per-unit tax of **10 ECU is due for sellers**.

Sellers will then be asked to report the number of units sold in the previous three rounds for tax purposes. The reported amount may be between zero and the total number of units that were actually sold over the previous three rounds. After the reporting decision is submitted by pressing the "OK" button, the computer will determine if it is checked whether the reported number equals the actual number of units sold over the last three periods. The computer makes this call by randomly selecting an integer number between 1 and 10. The reporting decision will **only** be checked if the computer selects the number 1. Therefore, there is a random chance of 10% that the reporting decision will be checked.
]

[*Net income information in the* ***control condition:***

Calculation of Net Income for Sellers

After three consecutive trading periods, the screen shows how many units of the fictitious unit you have traded over the previous three rounds and the resulting gross income from the previous three periods. For each unit traded in the three previous periods, a per-unit tax of **10 ECU is due for sellers**

Therefore, a seller's payment – the net income – , consists of her sum of all gross earnings from the three previous rounds (henceforth denoted "sum gross income") minus the tax payment. The tax payment is the number of units sold over the previous three periods multiplied by the tax rate of 10 ECU. Hence:

Net Income = sum gross income - (number of units sold in previous 3 rounds * per-unit tax rate)
]

[*Net income information in the* ***condition with evasion opportunity:***

Calculation of Net Income for Sellers

Sellers will be informed of the outcome of the random draw, and will be faced with one of the following two scenarios:

1. Computer selects a number between 2 and 10 (2, 3, 4, 5, 6, 7, 8, 9 or 10):
The reporting decision will *not* be checked. A seller's earnings after taxes – the net income –, in this case, consists of the sum of all her gross earnings from the three previous periods (henceforth denoted "sum gross income") minus the tax payment. The tax payment is the **reported** number of units sold multiplied by the tax rate of 10 ECU. Hence:

Net income = sum gross income - (reported number of units sold * per unit tax rate)

2. Computer selects number 1:
The reporting decision *will* be checked. A seller's earnings after taxes – the net income –, in this case, consist of sum of all her gross earnings from the three previous periods (henceforth denoted "sum gross income") minus the tax payment. The tax payment is based on the number of units **actually** sold over the last three periods. If the number of units was **not** reported correctly, a seller will additionally have to pay a penalty that is equal to the amount of **tax liability that was not paid**. Hence:

Net income = sum gross income - (actual number of units sold * per unit tax rate) - (number of units *not reported* * per unit tax rate)
]

[*Net income information in the* ***condition with tax credit:***

Calculation of Net Income for Sellers

After three consecutive trading periods, the screen shows how many units of the fictitious unit you have traded over the previous three rounds and the resulting gross income from the previous three periods. For each unit traded in the three previous periods, a per-unit tax of **10 ECU is due for sellers**. In addition, sellers receive a tax credit of **7.5 ECU** for each unit sold.

Therefore, a seller's payment – the net income – , consists of her sum of all gross earnings from the three previous rounds (henceforth denoted "sum gross income") minus the tax payment. The tax payment consists of the per-unit tax of 10 ECU per unit sold minus the tax credit of 7.5 ECU per unit sold. Hence:

Tax payment
= (number of units sold * per-unit tax rate) - (number of units sold * tax credit)
= number of units sold * (10 - 7.5)

Net income then is:

Net Income
= sum gross income - tax payment
= sum gross income - (number of units sold * (10 - 7.5))
]

Payment

The first 3 rounds serve as practice rounds, in which you cannot earn money. The subsequent 27 rounds are paying rounds.

Buyers do not pay taxes so that gross earnings equal net earnings. A buyer's payoff hence equals the sum of gross earnings from all 27 trading periods.

Sellers receive a payoff that consists of the sum of all net incomes, each of which is earned after every third paying round (i.e., after paying rounds 3, 6, 9, 12, 15, 18, 21, 24, 27.)

You will be paid the payoff in cash at the end of the experiment. Additionally, each participant receives a show-up fee of 2.50 Euro. If the sum of all gross or net incomes is negative or zero, you will be paid the show-up fee; that is, you cannot make losses and will earn a minimum amount of 2.50 Euro.

Final Remarks

After the completion of all 30 rounds – 3 practice round plus 27 paying rounds – the experiment is finished. You will be asked to complete a short questionnaire at the end of the experiment while we prepare the payments. All information collected through this questionnaire, just like all data gathered during the experiment, are anonymous and exclusively used for scientific purposes. After you have completed the questionnaire, please remain seated at your booth until we call you to come up front to pick up your payment.