Tax Incidence in the Presence of Tax Evasion*

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Abstract

This paper studies the economic incidence of sales taxes in the presence of tax evasion opportunities. We design a laboratory experiment in which buyers and sellers trade a fictitious good in double auction markets. A per-unit tax is imposed on sellers, and sellers in the treatment group are provided the opportunity to evade the tax whereas sellers in the control group are not. We find that the market equilibrium price in the treatment group is lower than in the control group. This difference is economically and statistically significant, and implies that sellers with access to evasion shift a smaller share of the statutory tax burden onto buyers relative to sellers without tax evasion opportunities. Interestingly, we find that sellers with evasion opportunities shift the full amount of their effective tax rate onto buyers. Additional experimental treatments show that the full shifting of the effective tax burden is due to the evasion opportunity itself rather than the evasion-induced lower effective tax rate.

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1 Introduction

Motivation and research question. The standard textbook theory of taxation predicts that the economic incidence of a tax depends solely on the relative elasticity of demand and supply. This prediction is often deemed inconsistent with reality, and several other factors have been identified as influencing the incidence of taxes: e.g., salience (Chetty et al. 2009), remittance policy (Slemrod 2008), and market structure (Riedl 2010). Another factor likely to affect the incidence of taxes is the prevalence of tax evasion opportunities (Martinez-Vazquez 1996). Intuitively, access to tax evasion allows taxpayers to lower their tax burden by underreporting their tax base. As a result, the "real" behavioral responses that determine tax incidence are likely to differ between taxpayers who can evade and those who cannot. Understanding this possible source of deviation between observed and standard theoretical economic incidence is important given the prevalence of tax evasion in both developed and developing countries (Slemrod 2007; Schneider et al. 2010; Kleven et al. 2011). However, extensions to the standard model to account for the presence of tax evasion yield mixed theoretical predictions (Marrelli 1984; Cremer and Gahvari 1993; Yaniv 1995; Lee 1998; Bayer and Cowell 2009).

Although the impact of tax evasion on incidence has intuitive appeal and is policy relevant, there is very little empirical analysis of whether tax evasion affects the incidence of taxes. The objective of this paper, then, is to contribute empirical evidence on the effect of tax evasion on tax incidence. We are interested in knowing if, and how, tax evasion opportunities affect the distribution of the statutory tax rate as well as the effective tax rate between buyers and sellers. As a starting point for this analysis, we ask the following specific research question: are equilibrium prices different in markets where evasion is an option relative to markets without evasion opportunities?

Empirical approach. Data for the empirical analysis are generated in a between-subject-design laboratory experiment where subjects trade fictitious goods in a competitive double auction market. Subjects are randomly assigned roles as sellers or buyers in treatment and control groups, and a per-unit tax is imposed on all sellers. Sellers in the treatment group make a tax-reporting decision and are therefore able to under-report the number of units sold, whereas sellers in the control group have their correct tax liability deducted automatically. Evasion costs, including audit probability and fine rate, are exogenous. Because the only difference between the treatment and control group is access to evasion, we attribute any price differences between the two groups to the evasion opportunity. The impact of evasion on tax incidence is then determined by comparing the price differences to the statutory and effective tax rates.

Our decision to use a lab experiment is based on the fact that causal identification requires random variation in access to evasion across otherwise similar markets. This is obviously difficult to achieve using observational data since access to tax evasion is most likely one of the dimensions of a market that determines whether buyers and sellers select to participate in that market. Relying on the controlled environment of the laboratory means that we are able to avoid much of these econometric problems and thus produce clean identification of the treatment effect. Although our setting is artificial, the experimental laboratory has been used extensively to study the economic incidence of taxes. In fact, various studies have found that the theoretical results of tax incidence – without evasion – hold in competitive experimental markets such as a double auction (Kachelmeier et al. 1994; Borck et al. 2002; Ruffle 2005). We therefore introduce tax evasion to an environment that has been shown to provide credible results in the context of tax incidence.[1]

Results. The empirical results show that the equilibrium price in the treatment group with tax evasion is statistically and economically lower than in the control group. Accordingly, the number of units traded is higher in the case with evasion. By under-reporting sales, sellers reduce their effective tax burden, which allows them to sell at lower prices. We use these price effects to determine the impact of evasion on the incidence of the tax and report two important results. First, the share of the *statutory* tax rate borne by buyers is approximately 50 percent lower in the presence of evasion, which suggests that access to tax evasion changes the incidence of the statutory tax rate. Second, we find that sellers with an evasion opportunity shift their full *effective* tax rate onto buyers. We suspect this result is either due to the evasion opportunity itself, or the evasion-induced lowering of the effective tax rate. To disentangle these two effects, we ran additional treatments where the effective tax rate is exogenously lowered to the effective rate observed in the evasion treatments. The results from these additional treatments suggest that the full shifting in the evasion treatment is due to the evasion opportunity itself rather than the evasion-induced lowering of the effective tax rate. One interpretation of this finding is that sellers desire to be compensated for the risk associated with evasion.

The empirical results on statutory tax incidence are consistent with the predictions that we derive to rationalize the experiment. We predict lower prices and higher quantities in markets with evasion opportunities. The simple reason for this prediction is that sellers with an evasion option are able to reduce their effective tax rate relative to those without evasion. As a result, the tax causes the industry supply curve to shift up by less in the case with evasion. In our specific context, a per-unit tax on sellers who can evade taxes reduces the share of the statutory tax burden that is passed on to buyers.

Contribution to the literature. Addressing the research question posed in this paper makes three important contributions to the academic literature. First, several studies have attempted to identify the incidence of taxes using observational data.[2] To

[1]We employ an experimental double auction similar to Grosser and Reuben (2013). Riedl (2010) provides an overview of experimental tax incidence research.

[2]For example, Alm et al. (2009) and Marion and Muehlegger (2011) find that the incidence of the

overcome the challenges of identifying causal effects using observational data, several studies explore the question of economic incidence in a laboratory setting. For example, Kachelmeier et al. (1994), Quirmbach et al. (1996), Borck et al. (2002), and Ruffle (2005) find that the theoretical predictions of tax incidence hold true in a competitive laboratory market with full information.[3] We add to this strand of the literature by introducing tax evasion to a standard competitive experimental double-auction market, and show that this changes the incidence of the tax. This finding is important because it suggests that tax equivalence, which is the focus of the existing laboratory tax incidence literature, is unlikely to hold in the real world where buyers and sellers have different access to evasion.

Two studies more closely related to ours in that they estimate economic incidence in the presence of tax evasion are Alm and Sennoga (2010) and Kopczuk et al. (2015). The latter provides empirical evidence that the stage of production at which the tax on diesel is collected in the US affects the economic incidence of the tax. Although they suggest that this difference is driven by variation in access to evasion across production stages, reliance on observational data makes it difficult to cleanly identify whether this effect is fully due to variation in compliance behavior. Alm and Sennoga (2010) use a computable general equilibrium (CGE) model to simulate the economic incidence of tax evasion for a "typical" developing country. They find that the benefits of evasion generally do not stay with the evader if there is free entry, which suggests that evasion changes the incidence of taxes. Since we rely on the controlled environment of the lab, our empirical approach provides precise control over the market institutions, which allows us to randomize access to evasion and measure non-compliance accurately. As a result, we are able to offer cleaner identification of the impact of tax evasion on the economic incidence of the tax than these two studies. Nonetheless, we view our work as complementary to these papers. The illusive nature of tax evasion implies that consistent results across multiple techniques is required if we are to draw firm conclusions about causes and consequences of tax evasion. We argue that our results, combined with Kopczuk et al. (2015) and Alm and Sennoga (2010), provide evidence that the standard textbook model of tax incidence does not hold up in the presence of tax evasion opportunities.

Second, our paper adds to the general tax evasion literature. Naturally, obtaining credible causal evidence in the context of tax evasion is very difficult using observational studies (Slemrod and Weber 2012). A broad strand of literature has therefore employed lab experiments to study evasion (e.g., Fortin et al. 2007; Alm et al. 2009; Balafoutas

fuel tax in the US is fully shifted to final consumers and related to supply and demand conditions, Saez et al. (2012) find that tax equivalence does not hold in the context of the Greek payroll tax, and Fuest et al. (2013) find that the burden of local business taxes in Germany partly falls on employees via lower wages. Other examples include Evans et al. (1999), Gruber and Koszegi (2004), and Rothstein (2010).

[3]Kerschbamer and Kirchsteiger (2000) and Riedl and Tyran (2005) find that the laws of tax incidence do not translate to non-competitive experimental markets.

et al. 2015).[4] However, unlike most of the tax evasion literature, we focus on the implications of tax evasion (e.g., Andreoni et al. 1998; Doerrenberg and Duncan 2014) rather than on explaining tax evasion (e.g., Alm 2012). In particular, we show that real responses to taxes are small in part because of income shifting responses such as tax evasion. Additionally, our results support the general notion that economic outcomes such as prices are affected by tax evasion behavior (e.g., Andreoni et al. 1998). Third, our paper joins a growing literature showing that institutions matter for the effects of taxes. For example, Slemrod and Gillitzer (2013) put forward the "tax-systems" approach and argue that tax analysis has to consider all aspects of taxation, particularly aspects of administration, compliance, and remittance. Our paper supports this view in that it shows that taxes have different effects when the institution in place does not close all opportunities for non-compliance.

Relevance for policy. Our findings also have important policy implications related to the distribution of tax burden, the effectiveness of tax policy as a tool for influencing behavior, and the likely effect of recent anti-evasion policies. First, understanding the impact of tax evasion on the economic incidence of taxes is important for the correct evaluation of the distributional effect of tax policies aimed at reducing tax evasion. The results we present here further suggest that accounting for tax evasion in incidence studies may lead to a re-evaluation of the progressivity/regressivity of various taxes. For example, a sales tax where the benefits of evasion stay with sellers is likely to be more regressive than one where the benefits are shifted to buyers, especially if the evading sellers sell mostly to lower income individuals.

Second, taxes aimed at influencing real behavior are likely to be less effective if the market participants responsible for remitting the tax have access to tax evasion opportunities. Because the effective tax rate is lower among evaders, "real" behavioral responses to the tax are dampened, which limits the ability of the tax to achieve desired behavioral outcomes. More generally, to the extent that tax evasion cannot be fully eliminated, our findings suggest that it might be optimal to apply higher tax rates to goods sold in markets with evasion opportunities (e.g., Cremer and Gahvari 1993). Not only could this be more efficient, but it might also achieve the desired adjustments in behavior.

Third, the relevance and importance of our findings is especially obvious when one considers the prevalence of tax evasion across the world (Slemrod 2007; Schneider et al. 2010; Kleven et al. 2011). Transaction taxes, which we focus on in our study, are of particular interest in this context. For example, sales tax gap estimates range from 2 percent to 41 percent for the value added tax in the European Union and 1 percent to 19.5 percent for the retail sales tax in the United States (see Mikesell 2014 for a review of

[4] Andreoni et al. (1998) and Torgler (2002) provide surveys on tax compliance in experiments.

sales tax evasion estimates). Additionally, it is generally accepted 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 percent among businesses and 95 to 100 percent among individuals in a study of the potential revenue losses of e-commerce.[5]

Therefore, our results are relevant in countries such as the United States where, for example, a number of states have adopted, or are in the process of adopting, legislation aimed at (a) restricting the sale of "zappers", which are used to evade sales taxes, and (b) requiring online traders to register as sales tax collectors. Our findings suggest that such measures are likely to result in higher prices as affected sellers fully adjust to the retail sales tax. While we focus on sales taxes here, the findings also suggest that other anti-tax evasion initiatives, such as the Foreign Account Tax Compliance Act (FATCA), are likely to affect the level of economic activity as affected parties respond to the reduced evasion opportunities.

Structure. The remainder of the paper is structured as follows. We describe the experimental design in section 2, the theoretical predictions in section 3 and the results in section 4. Our findings are discussed in section 5 and section 6 concludes.

2 Experimental Design

2.1 Overview

The experimental design reflects a standard competitive experimental double auction market as pioneered by Smith (1962).[6] The auction and the parameters in our experiment 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 fictious 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 the lab participants. We impose a per-unit tax on sellers to this set-up and give sellers in the treatment 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 treatment group. Further details on the experimental design are provided in the next subsections.

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

[6] Double auction markets mimic a perfectly competitive market. Dufwenberg et al. (2005), for example, rely on an experimental double auction to study financial markets. Holt (1995) provides an overview.

2.2 Organization

The experiment was conducted in the Cologne Laboratory for Economic Research (CLER), University of Cologne, Germany. A large random sample of all subjects in the laboratory's subject pool of approximately 4000 persons was invited via email – using the recruitment software *ORSEE* (Greiner 2004) – to participate in the experiment. Participants signed up on a first-come-first-serve basis. Neither the content of the experiment nor the expected payoff was stated in the invitation email. The experiment was programmed utilizing *z-tree* software (Fischbacher 2007). We ran eight sessions over two regular school days in November and December 2013.[7] Each session consisted of either a control or treatment group market and lasted about 100 minutes (including review of instructions and payment of participants).

We conduct four control and four treatment sessions for a total of 80 subjects.[8] Experimental Currency Units (ECU) are used as the currency during the experiment. After the experiment, ECU are converted to Euro with an exchange of 30 ECU = 1 EUR and subjects are paid the sum of all net incomes (see below) in Euro. It was public information that all tax revenue generated in the experiment would be donated to the German Red Cross.

At the beginning of each session, subjects are randomly assigned to computer boothes by drawing an ID number out of a bingo bag upon entering the lab. The computer then randomly assigns each subject to role as buyer or seller, as well as her costs or values which stay constant during the experiment. Subjects are given a hard copy of the instructions when they enter the lab and are allowed as much time as needed to familiarize themselves with the procedure of the experiment. They are also allowed to ask any clarifying questions. The instructions are identical for the control and treatment group except for information on the reporting decision and net income of sellers. These differences in the instructions are highlighted in appendix section C.

2.3 Description of a session

Each session includes 1 market that is either a control or treatment group market. Each market has five buyers and five sellers who each have 2 units of a fictitious good to trade. Sellers and buyers are randomly assigned costs and values for both of their units; the roles as buyer or seller and the assigned values and costs are exogenously determined and

[7]We ran additional experimental treatments in July 2015. This section provides details for the first set of experiments, the details regarding the additional treatments are in section 5.2. There are two regular semesters at the tertiary level in Germany; winter semester lasting from October to March and summer semester between April and July. Therefore, the experiment was implemented during the regular semester.

[8]See section 4.2.1 for summary statistics on demographic characteristics of the participants.

stay constant for the entire experiment. All ten subjects in one session/market first trade in 3 practice rounds and then 27 payoff relevant rounds.

Trade in the Double Auction. As is common in experimental markets, subjects are given demand and supply schedules for a fictitious good at the beginning of the session (Ruffle 2005; Cox et al. 2012; 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 Table 1. This allows us to induce demand and supply curves for each market, which are depicted in Figure 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. In other words, sellers cannot trade an item below its cost. Additionally, 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.[9]

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, 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.

Income: 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 (buyers and sellers) 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. In other words, subjects complete three rounds of trading then tax is collected from sellers, then three more rounds of trading then another tax collection and so on.

[9] Figure 9 in the appendix depicts a screenshot of the experimental market place for a seller in the treatment group with evasion opportunity.

This yields 27 trading periods and 9 tax collections; we discuss this design feature below. We define total gross profit in each trading period i ($i = 1, 2, 3, ..., 25, 26, 27$) as

$$\Pi_i^s = P_{i1}d_1 + P_{i2}d_2 - C_1d_1 - C_2d_2, \tag{1}$$

for sellers and

$$\Pi_i^b = V_1d_1 + V_2d_2 - P_{i1}d_1 - P_{i2}d_2, \tag{2}$$

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 statutory 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.

Income: Treatment Group. Since buyers do not pay the tax, the calculation of gross and net income for buyers in the treatment group is identical to that of the control group: see equations (2) and (4). Sellers, on the other hand, make a tax reporting decision at the end of every third round. In other words, subjects complete three rounds of trading then sellers make a reporting decision; then three more rounds of trading then another reporting decision and so on.

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 under-report 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 behaviour 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.[10]

Sellers 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 percent) and pay a fine, which is equal to twice the evaded taxes if they underreport sales and are audited. 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 treatment group 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 statutory per-unit tax rate. Subjects' final payoff is the sum of their net incomes from the nine tax collection periods.

2.4 Market Equilibrium without Evasion

The demand and supply schedules described in Table 1 and displayed in Figure 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. 2012; Grosser and Reuben 2013).[11]

[10] 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.

[11] 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. Therefore, although we do not implement the "no-tax" treatment here, we expect 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 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 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).

The question we seek to answer is whether this equilibrium outcome is affected by the presence of tax evasion opportunities among sellers. The next section provides a theoretical discussion for why tax evasion may or may not affect the incidence of the tax.

3 Conceptual Framework

This section describes the relationship between evasion opportunities, market prices and quantities, and the incidence of taxes in the context of our experiment.

Prices and quantities. For simplicity, let's assume that demand and supply curves are linear, and that the evasion decision is made jointly with the decision to sell. Using these assumptions, Figure 2 illustrates the effect of tax evasion on price and quantity for the cases with and without evasion. First, consider panel A, which represents the control group where evasion is not possible. As in the standard textbook case, the supply curve shifts up by the full amount of the statutory rate. This results in a new market equilibrium (p_c, q_c); where subscript c indicates control group.

Sellers in the treatment group have the opportunity to evade taxes by hiding a fraction of their sales. A seller who underreports sales and is not audited faces an effective tax rate that is lower than the statutory tax rate faced by sellers in the control group. Given the deterrence parameters in our experiment – audit probability of 10% and a fine equal to twice the evaded taxes – , we expect that a large fraction of sellers will evade and thus face this lower effective tax rate.[12] As illustrated in panel B of Figure 2, this then implies that the market supply curve in the presence of evasion opportunities shifts up by less than the statutory tax rate. This results in a new market equilibrium at (p_t, q_t); subscript t indicates treatment group.

This intuition leads to a qualitative prediction: the equilibrium price in the treatment group with evasion opportunities will be lower than in the control group where evasion is not an option; i.e., $(p_t < p_c)$. Accordingly, the number of units sold will be higher in the treatment group than in the control group; i.e., $(q_t > q_c)$.

[12] This expectation of positive tax evasion is supported by evidence from the field (e.g., Kleven et al. 2011) and the lab (e.g., Alm 2012).

The quantitative difference between the equilibrium prices and quantities in the control and treatment group is determined by the magnitude of the shift in the treatment group's market supply curve. This shift is positively related to the effective tax rate faced by sellers in the treatment group. Note that sellers have to pay the statutory per-unit (excise) tax τ for each unit they sell, but are provided a tax 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 equal to twice the evaded taxes must be paid if audited. This implies that seller i has to pay an (expected) effective tax rate of:

$$t_i^e = \frac{\tau(r_i + 2\gamma(s_i - r_i))}{s_i}, \qquad (6)$$

where s_i denotes the number of units a seller actually sells and r_i is the number of units she reports.[13] This simple equation shows that the effective tax rate is increasing in the statutory tax rate and decreasing in evasion. 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 and quantities.[14]

An alternative qualitative prediction arises if we assume that sellers treat their evasion and selling decisions as separable; i.e., sellers first set a price at which to sell, and then later make their evasion decision (Yaniv 1996; Yaniv 1995). In this case, the opportunity to evade has no bearing on the market price and hence the incidence of the tax is unaffected by the presence of tax evasion among sellers. Though an interesting alternative prediction, we find it plausible that sellers - who know they will be able to underreport sales - take their tax evasion opportunities into account when setting prices. In fact, the separability assumption is far from generally accepted in the literature (Bayer and Cowell 2009; Lee 1998; Marrelli 1984; Virmani 1989).

Incidence. We differentiate between two concepts of tax incidence: (i) incidence of the statutory tax rate and (ii) incidence of the effective tax rate. The former describes the share of the statutory tax rate that is shifted on to consumers. In the context of the experiment, statutory incidence thus refers to the share of 10 ECU, the statutory per-unit tax rate, that sellers shift to buyers. Expressed differently, this is the difference between

[13] 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 (6). Note that this effective tax rate reduces to the statutory tax rate τ for sellers who either do not evade or do not have an option to evade.

[14] 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 statutory tax rate, deterrence parameters, the (biased) perception of audit probabilities, the degree of risk aversion, and the intrinsic motivation to pay taxes.

the equilibrium price in a no-tax scenario and the equilibrium price that we observe in our experiment. Considering the above rationale regarding prices and quantities, we expect this statutory incidence to be larger in the control than in the treatment group.

The incidence of the effective tax rate describes the share of the effective tax rate that is shifted onto buyers. Recall from equation (6) that the effective tax rate is equal to the statutory tax rate in the control group ($r_i = s_i$), and lower than the statutory tax rate in the treatment group ($r_i < s_i$). Since the supply and demand elasticities are equal in equilibrium in our experiment, we derive from textbook theory that the tax rate in the control group is shared equally between sellers and buyers. That is, the incidence of the statutory rate, and hence the effective tax rate, is predicted to be 50% in the control group.

Though the textbook theory would also predict a 50-50 split of the effective tax rate in the treatment group, the presence of risky evasion opportunities may imply that the incidence of the effective tax rate is different than 50% in the presence of evasion opportunities. This deviation from the theoretically expected 50%-result may be due to one of two reasons. First, because evasion is risky, it is possible that sellers shift more than their effective tax burden onto buyers as a means of receiving compensation for the evasion risk. Second, the evasion opportunity decreases the effective tax rate and sellers might perceive it to be easier to shift a lower tax rate onto buyers. Both mechanisms imply that the incidence of the effective tax rate is higher in the treatment group than in the control group. While our main experimental design, as described before, allows us to study the incidence of the statutory and effective tax rates in the control and treatment groups, it is not suitable to disentangle these two potential channels. We present additional treatments in section 5.2 to be able to make this distinction.

4 Empirical Strategy and Results

Recall that we are interested in identifying the impact of tax evasion opportunities on the economic incidence of a sales tax. We describe the empirical strategy used to identify this treatment effect in section 4.1 and our findings in section 4.2.

4.1 Empirical Strategy

Definition of prices. Given the discussion in section 3, we are particularly interested in knowing whether the market clearing price in the treatment group is different from that in the control group. Therefore, the first step in our empirical strategy is to define the market price. The experiment produced one price for each unit sold in a given market-period, which allows us to create three measures of market price. The first measure is simply the price at which each item is sold, which we denote P. We also calculate the mean and median price in a given market-period and denote them $\overline{P}$ and P_{50}, respectively. Therefore, our data set has one observation per market-period when price is measured by $\overline{P}$ or P_{50} and n observations per market-period when market price is measured by P, where n is the number of units sold in that market-period.

Non-parametric analysis. Due to random assignment to groups and markets, any (non-parametric) difference in these prices between the treatment and control groups is taken as evidence of the presence of treatment effects. Because the period-specific prices are not independent across the 27 periods within a given market, we implement our non-parametric analysis (ranksum tests; see footnote 15) using the average price for each market; that is, we use the average of P by market. This implies that our non-parametric analysis is based on eight independent observations; four in the treatment and four in the control groups.[15]

Regressions. We also test for treatment effects parametrically by regressing each measure of price, separately, on a treatment dummy. The baseline model for $\overline{P}$ is specified as follows:

$$\overline{P}_{i,m} = \beta_0 + \delta T_m + \epsilon_{i,m}, \tag{7}$$

where $\overline{P}_{i,m}$ is the mean price of the good in period i (with $i = 1, ..., 27$) of market m (with $m = 1, ..., 8$). T_m is a dummy for the treatment state, which is equal to one if treatment group and zero if control group. $\epsilon_{i,m}$ is a standard error term. Our coefficient of interest is δ, which represents the difference in market price between the two groups. More precisely, δ indicates the causal effect of evasion opportunity on the equilibrium market price. This causal interpretation follows from the fact that the groups are identical except for access to evasion and random assignment of participants to the two groups. We set up our data as a panel with 27 periods per market and run pooled ordinary least squares (OLS) regressions. To account for the dependence of prices across periods within a market, we cluster standard errors on the market level.[16] Because the treatment status of each market and hence the participants in that market is always the same, the treatment

[15] While the number of independent observations, eight, 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 Stata routine provided by Harris and Hardin (2013), which adjusts the p-values to the low number of observations, to implement "exact" ranksum tests (these are based on Wilcoxon 1945 and Mann and Whitney 1947).

[16] Note that estimators that allow for censoring, such as Tobit models, are unnecessary since the market price is not censored. Although the market price could be no lower than 18 and no higher the 82, the distribution of market prices suggest that these prices were never binding; the lowest market price is 30 and the highest is 63.

effect is identified using a between-market design.[17] We include period fixed effects in some specifications.

4.2 Results

4.2.1 Summary Statistics

After the experiment, subjects reported their age, gender, native language, level of tax morale and field of study. Tax morale is determined using a question very similar to one used in the World Values Survey (Inglehart nd).[18] Each of these variables is summarized in Table 2. Casual observation of the data shows that randomization into the treatment states worked well. This is confirmed by non-parametric Wilcoxon rank-sum tests for differences in distributions between groups; we do not observe any statistically significant differences in gender, age, share of participants whose native language is German, tax morale or field of study across the two groups. While we do not explicitly measure other attitudinal variables such as social norms or preferences, randomization implies that these omitted variables are also balanced across groups and therefore do not have any effect on our results. Among all participants, approximately 51% were male, 77% indicated German to be their native language, and the average age was 26 years. Approximately 24% of subjects stated that cheating on taxes can never be justified and 48% indicated that economics is their major field of study.

Table 2 also reports the compliance rate in the treatment group. We find that every subject evaded some positive amount of sales at least once and 33 of the 40 subjects in the treatment group fully pursued the profit maximizing rational strategy of full evasion in every reporting period. As a result the mean compliance rate is approximately 7% among all sellers in treatment group and 61% among those who report non-zero sales.[19]

[17] Notice that this also implies that it is not possible to estimate the treatment effect in the presence of market fixed effects. Each individual is randomly assigned to a market and everyone in the market has the same treatment status. Therefore, the treatment status of a market is the same as the treatment status of the individuals trading in that market.

[18] "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).

[19] This level of evasion is at the high end of evasion estimates in the experimental tax evasion literature (e.g., Fortin et al. 2007; Alm et al. 2009; Alm et al. 2010; Coricelli et al. 2010). However, these studies focus on income taxes and are therefore not directly comparable to our results. We do not know of any sales tax experiments in the tax evasion literature. Evidence from the real world suggest that our compliance rates are not unreasonable. For example, the compliance rate in our experiment is comparable to the compliance rate for the 'use' tax in the United States; 0 to 5 percent among individuals (GAO 2000).

4.2.2 Price

Non-parametric results. The non-parametric results presented in Figures 3 and 4 and Table 3 show clearly that the price in the treatment group is lower than in the control group. Figure 3 reports the mean market price by period for the treatment and control groups. The data show that the mean market price varied a lot 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).

Although price in both groups converged in roughly the same number of periods, the evolution of prices is different. Price increased steadily to equilibrium in the treatment group, and behave erratically in the control group. For this reason, and as is common in the literature, our primary results are based on data from trading periods 14 to 27 (we provide results for the full sample for illustrative purposes). The mean market price in both groups stabilized after round 14: at approximately **54.36 ECU** in the control group and **51.65 ECU** in the treatment group (see panel B of Table 3). This implies that the mean market price in the treatment group is 2.71 ECU lower than in the control group.[20] As shown in Figure 4 and the second column of Table 3, median prices are also lower in the treatment group than in the control group; the median price is **51.27 ECU** in the treatment group and **54.07 ECU** in the control group, resulting in a treatment effect of 2.80 ECU.

These differences in prices between the groups are statistically significant from zero; the exact ranksum tests (two-sided) give p-values of 0.029 for differences in median prices, and 0.057 for differences in average prices.[21] In other words, we find that markets with access to tax evasion trade at significantly lower prices than markets without access to tax evasion. The experimental results are thus consistent with our qualitative prediction that the market price will be lower in the treatment than in the control group.[22]

Regression results. We extend the analysis above by estimating equation (7) for the mean market price as the dependent variable. The estimated treatment effect of -2.70 ECU reported in model 1 of Panel B of Table 4 is statistically different from zero at the

[20]Note that the estimated treatment effect is larger for the full sample (panel A). Because this sample includes data before the market price converges, we prefer the estimate in panel B.

[21]Note that 0.029 is the lowest possible p-value for the exact ranksum test with 8 independent observations.

[22]Further evidence that tax evasion affects the market price is provided in Figures 7 and 8, which report the cumulative distribution of mean and median market prices, respectively, for the treatment and control groups. Both figures show clearly that the price in the control group is not drawn from the same distribution as that in the treatment group. This conclusion is supported by the Kolmogorov-Smirnov test for equality of distribution functions; in both cases we reject the null that the distributions are equal. This result also holds when we use the individual ask prices (P) instead of mean or median prices; results available upon request.

1 percent level.[23] This estimate remains significant at the 5 percent level even after correcting for the small number of clusters using the wild-bootstrap-t procedure described in Cameron et al. (2008); see Table 9 in appendix.[24] Additionally, the estimate is robust to the inclusion of period fixed effects (model 2), demographic covariates (model 3), both period fixed effects and demographic covariates (model 4), and the definition of price (Table 5). Estimating equation (7) with the median market price, P_{50}, as our dependent variable yields treatment effects of -1.60 ECU to -2.10 ECU that are statistically different from zero at the 1% level (see Panel A of Table 5). Although these estimates are approximately 0.70 to 1.00 ECU smaller than that reported in Panel B of Table 4, they remain economically meaningful.[25] These results confirm our earlier non-parametric findings that the market price in the treatment group is significantly lower than in the control group.

4.2.3 Units sold

We identify the treatment effect on units sold using the same strategy as above. In particular, the non-parametric analysis is based on the mean number of units sold at the market level, while the regression analysis is based on the number of units sold in a market-period with standard errors clustered at the market-level.

Non-parametric results. The predictions in section 3 suggest that treatment markets will clear at a lower price and higher quantity than the control-group markets. We have already demonstrated that the market clearing price is lower in the treatment group. This section shows that the treatment group also sold more units than the control group. The results in Table 3 show that the mean number of units sold per period in the control group is **5.96**. On the other hand, the treatment group sold an average of **6.49** units per period. The difference between units sold in the treatment and control group is statistically significant with the lowest possible p-value of 0.029 (exact two-sided ranksum test based on eight independent observations). In other words, the estimated treatment effect of 0.5 units is statistically different from zero. The difference in sales between the two groups 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 treatment group sold a total of 336 units while the control group

[23]Panel A of Table 4 reports the results for the full sample. These results are reported for illustrative purposes only since the market does not clear until around period 14.

[24]The correction is implemented using Stata code provided by Judson Caskey and is available here: `https://sites.google.com/site/judsoncaskey/data`.

[25]We also estimate the model with the ask price for each unit sold as the dependent variable and report the results in Panel B of Table 5. The estimated treatment effect in this case is -2.66 ECU to -2.72 ECU, which is almost identical to that for the mean market price as reported in Panel B of Table 4.

only sold 308 units. Corresponding numbers for periods 1 to 27 are 704 and 647 in the treatment and control group, respectively. The experimental results hence confirm our prediction that markets with access to evasion trade more units than markets without evasion opportunities.

Regression results. These results are supported by results from a regression analysis that are reported in Table 6. Focussing on Panel B, which reports results for periods 15 to 27, we find a statistically significant treatment effect of 0.6 units; relative to the control group, the treatment group sold approximately 0.6 more units per period.

5 Discussion

The results presented in section 4.2 show that markets with sellers who have the opportunity to evade taxes trade more units and do so at lower prices than markets where tax evasion is not possible. Section 5.1 explains the incidence results in the context of the theoretical model. Section 5.2 describes additional treatments that shed more light on our results. The external validity of our findings is discussed in section 5.3.

5.1 Incidence

The treatment effect identified above is consistent with the predictions in section 3. According to the predictions, tax evasion lowers the effective tax rate facing sellers, thus allowing them to trade at lower prices in a competitive market. As a result, the final tax burden shifted to buyers is lower than it would otherwise be in the absence of tax evasion. This is exactly what we find; we observe a mean compliance rate of 7% among all sellers, which implies an average effective tax rate of approximately 2.56 ECU among all sellers (see equation 6 to see how we calculate the effective tax rate). Sellers facing these lower effective tax rates trade at lower prices.

So how does this response among sellers affect the incidence of the tax? In order to answer this question, 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 estimate this "no-tax" equilibrium by relying on evidence from Grosser and Reuben (2013) who run a "no-tax" treatment using a comparable double auction market with the same demand and supply schedule as we do.[26] In line with the theoretical expectation, they find a mean market price of 49

[26]The experimental design in Grosser and Reuben (2013) differs from ours in that they use a within subject design where each subject trades in a market with and without the tax. We are aware that within subject and between subject designs may yield different results (Charness et al. 2012). However, we argue that their "no-tax" estimate is a reasonable baseline to use in our incidence analysis, especially

ECU (standard deviation: 1.3) and 7 units in the "no-tax" equilibrium. On the other hand, the market in our control group (with tax but no evasion opportunity) cleared with a mean price of 54.36 ECU (sd: 1.15) and 5.96 units, which is well within the equilibrium predicted by the theory: 53 ECU to 57 ECU with 6 units traded. Using this "no-tax" result as a benchmark, in the following we discuss the incidence of the statutory tax rate (10 ECU in both groups) and the effective tax rate (10 ECU in control group, and 2.56 ECU in the treatment group due to underreporting).

5.1.1 Incidence of statutory tax rate

The equilibrium price in the control group is approximately 5 ECU above the "no-tax" equilibrium of 49 ECU. This suggests that the incidence of the statutory tax burden in the control group is shared equally between buyers and sellers since the tax rate is 10 ECU per unit. Again, this is consistent with the theoretical framework; since the demand and supply schedules have equal price elasticity in equilibrium, the burden is expected to be shared equally between buyers and sellers.

The next step is to determine the extent to which access to evasion affected the incidence of the statutory tax. The mean market clearing price in the treatment group (with tax and evasion opportunity) is 51.65 ECU (sd: 1.26). Considering the statutory tax rate of 10 ECU per unit and the no-tax benchmark of 49 ECU, this implies that buyers in the treatment group pay 26.5% ($= (51.65 - 49)/10$) of the *statutory* tax burden, compared to the 50% in the case without evasion. In other words, access to evasion reduced the statutory tax burden on buyers by about 23 percentage points. This treatment effect on incidence appears small when compared to the market price. However, we argue that the relevant comparison is the share of the statutory tax burden that the buyers paid in the control group. Since buyers paid 5 ECU in the control group, the largest expected effect of evasion is a reduction of 5 ECU. Therefore, using this baseline, a treatment effect of 2.71 ECU is very large.

5.1.2 Incidence of effective tax rate

Finally, we wish to know whether access to evasion changed the incidence of the effective tax rate. Because the effective tax rate is the same as the statutory tax rate in the control group, we already know that the effective tax rate is shared equally between buyers and sellers in the control group. How does this incidence result change in the presence of tax evasion? Recall that the expected effective tax rate from equation (6) is estimated to be 2.56 ECU. If sellers with evasion opportunity continued to share the effective tax burden 50-50, we would expect the price in the treatment group to increase by approximately

since they randomized the order of tax and "no-tax" treatments. Additionally, their result is in line with the theoretical prediction which is further support for using their result as a baseline result.

1.28 ECU (= 2.56/2) relative to the "no-tax" equilibrium of 49 ECU; that is to 50.28. However, this is not what we observe. The price in the treatment group is 51.65 ECU, which suggests that sellers shift the full expected effective tax rate onto buyers; buyers bear 2.65 ECU (= 51.65 − 49) even though the effective tax rate is 2.56 ECU. As a result, about 103.5% (= (51.65 − 49)/2.56) of a seller's expected effective tax rate is shifted onto buyers.

5.2 Additional Treatments

This result raises an interesting question: why do we observe full shifting of the effective tax rate in the evasion treatment whereas we observe the theoretically expected 50-50 shifting in the control group? We suspect this is due to one of two reasons. First, this could be due the fact that the effective tax rate is lower in the treatment group. The lower effective tax rate in the evasion treatment might make it easier to shift more of the tax burden onto buyers. Second, this might be due to the evasion opportunity. Sellers might attempt to shift enough of their tax burden onto buyers because they desire to be compensated for the risk associated with evasion. We ran three additional sessions in order to separate this pure evasion effect from the effect of the lower effective tax rate. Below we describe the design and results from these additional treatments.

5.2.1 Design

The additional sessions are identical to the previous control sessions except that the effective tax is exogenously lowered to 2.5 ECU, which is the same as the effective tax rate in the evasion treatment.[27] As in the previous treatments, the statutory tax rate is set at 10 ECU, but sellers are told that they will receive a credit of 7.5 ECU for every unit they sell. Sellers do not make a reporting decision. Instead, all tax calculations including the tax credit adjustment are done automatically. Therefore, sellers in the additional treatments face an effective tax rate that is lower than their statutory tax rate. More importantly, there are no risks associated with this lowered effective tax rate. Although the effective tax rate is the same as in the evasion treatments, sellers in those treatments had to take on audit risk in order to arrive at this lower effective tax rate.

Operationally, the only difference between the additional treatments and the control group is the inclusion of the tax credit; everything else is the same. The differences in the instructions that subjects read at the beginning of the experiment are highlighted in appendix section C. We ran three sessions that lasted approximately 100 minutes each in July 2015 at the University of Cologne. The sessions were conducted in the same lab

[27]The effective tax rate in the evasion treatment is actually 2.56 ECU. However, we opted for 2.5 ECU because it is easier for subjects to mentally calculate while making their sales and purchasing decisions.

as before, but none of the subjects had participated in the previous sessions. There were 10 subjects (five buyers and five sellers) in each session, and the average pay-off was $22.

5.2.2 Results

The results from these additional treatments are reported in Figure 6 and Table 8. We find that the average equilibrium price in the additional treatments is **50.09 ECU** (sd: 2.16), which is lower than the price in both the evasion and control groups.[28] Though the equilibrium price in the additional treatments is more than $1.50 lower than in the evasion treatments, we cannot reject the null that the price difference between these two treatments is zero. Still, this price difference is economically meaningful. Notice that consumers in the deduction treatment pay 1.09 ECU out of the statutory tax rate, while those in the evasion treatment pay 2.63 ECU and those in the control group pay 5.35 ECU. This implies that sellers in the additional treatments shifted 42.4% $(= (50.09 - 49)/2.5)$ of their effective tax burden onto buyers. Importantly, this is considerably lower than the full shifting that we observe in the evasion treatments – despite the fact that the effective tax rate is the same. This provides suggestive evidence that the evasion opportunity itself, rather than the lower effective tax rate, is the main driver of the full shifting that we observe in the evasion treatments.

5.3 External Validity

As with all economic laboratory experiments, there remains doubt about the external validity of our results.[29] One major concern is that the setting in the lab is abstract and artificial. However, the literature shows that laboratory double auctions generate very plausible equilibria (e.g., Smith 1962; Holt 1995; Dufwenberg et al. 2005; Grosser and Reuben 2013.). 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. 2012; 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.[30] Additionally, although evasion may occur among buyers as

[28] As before, our empirical analysis is based on data from periods 15 to 27.

[29] See Levitt and List (2007) for a critical discussion of the generalizability of lab experiments. Falk and Heckman (2009) offer a defense of most concerns, some of which are also discussed here.

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

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. Importantly, while our audit rate of 10% seems low, there is evidence of "real-world" tax systems with significantly lower audit rates. For example, a recent 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]."[31] 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.

Our results are also relevant for the current wave of "Amazon laws" being passed or considered by US states in response to widespread use-tax evasion. A key feature of these laws is that they shift tax remittance obligations from buyers to sellers, which reduces the evasion opportunities usually enjoyed by buyers. Use-tax evasion by consumers in business-to-consumer (B2C) transactions is governed by similar parameters to our laboratory setting: low audit probability and no need to incur evasion costs beyond those established in the audit mechanism. As a result, compliance with the B2C use tax is comparable to what we find in our experiment. Therefore, our results speak to the potential impact that these laws might have on tax evasion and hence the the incidence of retail sales taxes in the US; lower evasion, higher prices, and hence greater tax burden on buyers and sellers.[32]

6 Conclusion

We use data generated in an economic laboratory experiment to identify the effect of tax evasion among sellers on the economic incidence of a per-unit tax. We find strong evidence that access to evasion opportunities affect the incidence of a per-unit tax. In particular, sellers who are able to evade a per-unit tax trade at lower prices and sell more units. In fact, relative to the baseline case where buyers face 50% of the statutory tax burden, buyers in the treatment group only face approximately 26% of the statutory tax burden. Although buyers pay lower prices than they otherwise would, we find that sellers fully shift the expected effective tax onto buyers. Additional treatments show that prices are different between markets with and without evasion opportunity even if the effective tax burden is the same. In other words, endogenous evasion-induced changes in the effective

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 332 to the Red Cross (including the additional treatments).

[31] The article was published on the website of WTVA news: `http://www.wtva.com/mostpopular/story/Sales-tax-dodging-on-the-rise-in-Mississippi/dg14bG-Prk60APNSt96RHQ.cspx`.

[32] The implementation of 'Amazon laws' present an opportunity to study the incidence effects of evasion in the real world. However, to the best of our knowledge, the data required for empirical analysis are currently unavailable.

tax burden have different price effects than exogenous changes in the effective tax burden of equal magnitude. This finding suggests that the full shifting of the effective tax burden observed in the evasion treatment is due to the evasion opportunity itself rather than the evasion-induced lower effective tax rate. One possible explanation for this finding is that evaders desire to be compensated for the risk of evasion and therefore trade at higher prices.

Our findings have welfare implications. In particular, access to tax evasion may lower the excess burden of taxation because sellers in markets with evasion opportunities are able to escape the statutory tax and hence trade at lower prices. Of course, this conclusion is sensitive to issues related to the costs of evasion, the use of tax revenue and the imposition of a revenue constraint. First, the result of higher welfare breaks down if evasion requires any real resource costs. Second, the conclusion is based on the assumption that the welfare effects of privately consumed goods is at least as big as publicly provided goods. Finally, this welfare implication only holds in the absence of a revenue constraint. This is due to the fact that tax evasion leads to revenue losses, which then imply higher tax rates in the presence of a revenue constraint.

Our results suggest that access to evasion reduces the effectiveness of taxes that are implemented with the specific intent of changing the activity level of market participants. Furthermore, because evasion reduces the amount of the tax that is shifted onto buyers, our findings also suggest that the regressivity of sales taxes depends on how the benefits of access to evasion varies along the income distribution. The results also imply that policy makers do not necessarily have an easy choice when deciding whether to pursue evasion reducing strategies or to exploit the potential efficiency gains of evasion. For example, Cremer and Gahvari (1993) show that the optimal Ramsey rule in the presence of tax evasion calls for higher tax rates on the good with the tax evasion opportunity. The argument is that evasion lowers the real behavioral response and thus lowers excess burden; this is confirmed by our results. However, given that governments often face revenue requirements along with the fact that tax evasion may require real resource costs, a strategy that seeks to minimize tax evasion opportunities might be more optimal. This is especially important in cases where the policy objective is to influence real behavior. Evasion reducing strategies may also make sense on revenue grounds. Although tax revenues represent a simple transfer from an economic welfare perspective, revenues are used to produce public goods/service that are likely to be underproduced or not produced at all as tax revenues decline.

Additionally, while we show that tax evasion opportunities affect tax incidence, it is not clear that the magnitude and effect is the same across all types of taxes. Conditional on the ease with which taxes can be evaded, it is also possible that the mechanism of evasion 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 the USA, we argue that this potential difference is worth investigating in future research. More generally, it would be interesting to know if and how evasion mechanisms in different tax systems affect the incidence of taxes.

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Tables and Figures

Tables

Table 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.

Table 2: Summary statistics of Demographic Variables

	Gender	Age	German	Tax Morale	Econ	Compliance
	Control Group (Non-Evaders)					
Mean	0.43	24.90	0.72	0.25	0.43	–
St. Dev.	0.50	6.87	0.46	0.44	0.50	–
N. of Subjects	40	40	39	40	40	–
	Treatment Group (Evaders)					
Mean	0.60	26.93	0.83	0.23	0.53	0.07
St. Dev.	0.50	12.25	0.38	0.42	0.51	0.23
N. of Subjects	40	40	40	40	40	40
P-value	0.12	0.23	0.26	0.79	0.37	–

Notes: Reported are the mean characteristics of treatment and control groups. 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, tax morale is a dummy that is equal to 1 for subjects who believe cheating on taxes can never be justified and Econ is a dummy that is equal to 1 if field of study is economics. One subject in the control group did not report his/her language. P-value is for the Wilcoxon rank-sum test; null hypothesis is that there is no difference in the characteristics between the two groups.

Table 3: Prices and Quantities by Treatment Group

	Price			Units sold	
Group	Mean	Median	Std. Dev.	Mean	Std. Dev.
			Panel A: Full Sample		
Non-evader	54.99	54.86	1.57	6.04	0.14
Evader	51.24	50.87	1.52	6.55	0.26
			Panel B: Period>14		
Non-evader	54.36	54.07	1.15	5.96	0.19
Evader	51.65	51.27	1.26	6.49	0.30
P-value	0.057	0.029	–	0.029	–

Notes: Reported is the market-level mean and market-level median of P, where P is the price at which each unit is sold in a given market period (see definition in the first paragraph of section 4.1). Units sold is the market-level mean of units sold in a given market period. All numbers and statistics are based on eight independent market-level observations. 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 eight independent market-level observations; null hypothesis is that there is no difference between the two groups.

Table 4: Impact of treatment on mean market price

	Model 1	Model 2	Model 3	Model 4
	Panel A: Full Sample			
Treat	-3.750***	-3.750***	-4.300***	-4.300***
	(1.009)	(1.077)	(0.347)	(0.371)
Constant	55.008***	54.181***	48.868***	48.040***
	(0.727)	(1.247)	(2.632)	(3.407)
R2	0.499	0.517	0.737	0.754
N	216	216	216	216
	Panel B: Period>14			
Treat	-2.701***	-2.701***	-2.651***	-2.651***
	(0.795)	(0.847)	(0.075)	(0.081)
Constant	54.362***	54.297***	49.508***	49.443***
	(0.539)	(0.516)	(0.572)	(0.750)
R2	0.553	0.563	0.884	0.894
N	104	104	104	104
Control variables	No	No	Yes	Yes
Period FE	No	Yes	No	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 (7) with the dependent variable defined as mean market price in a given market period. Panel A uses all completed contracts from periods 1 to 27, panel B uses all completed contracts in periods 15 to 27. Period FE is period fixed effects.

Table 5: Impact of treatment on median and ask market price

	Model 1	Model 2	Model 3	Model 4
	Panel A: Median Ask Price (P_{50})			
Treat	-2.087***	-2.087***	-1.589***	-1.589***
	(0.625)	(0.665)	(0.218)	(0.233)
Constant	53.779***	53.918***	60.175***	60.314***
	(0.089)	(0.222)	(1.655)	(1.809)
R2	0.538	0.563	0.853	0.878
N	104	104	104	104
	Panel B: Ask Price (P)			
Treat	-2.720***	-2.721***	-2.662***	-2.660***
	(0.798)	(0.808)	(0.065)	(0.069)
Constant	54.354***	54.255***	49.500***	49.481***
	(0.543)	(0.486)	(0.491)	(0.593)
R2	0.173	0.176	0.276	0.279
N	644	644	644	644
Control variables	No	No	Yes	Yes
Period FE	No	Yes	No	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 (7) with the dependent variable defined as median market price in a given market period in panel A; and the market price for each good in each market period in Panel B. All panels use completed contracts from periods 15 to 27. Period FE is period fixed effects.

Table 6: Impact of treatment on units sold

	Model 1	Model 2	Model 3	Model 4
		Panel A: Full Sample		
Treat	0.336***	0.334***	0.320***	0.324***
	(0.064)	(0.068)	(0.027)	(0.035)
Constant	6.088***	6.525***	6.701***	7.186***
	(0.059)	(0.144)	(0.406)	(0.277)
R2	0.090	0.292	0.100	0.301
N	1,006	1,006	1,006	1,006
		Panel B: Period>14		
Treat	0.402***	0.403***	0.598***	0.594***
	(0.125)	(0.125)	(0.051)	(0.056)
Constant	5.939***	6.177***	7.891***	8.102***
	(0.118)	(0.323)	(0.756)	(0.878)
R2	0.148	0.262	0.191	0.303
N	476	476	476	476
Control variables	No	No	Yes	Yes
Period FE	No	Yes	No	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 (7) with the dependent variable defined as the number of units sold in a given market period. Panel A uses all completed contracts from periods 1 to 27, panel B uses all completed contracts in periods 15 to 27. "Period FE" is period fixed effects.

Table 7: Overview of Results and Incidence of Statutory Tax Rate

Condition	Price	Units	Incidence Statutory Tax
No-Tax	49	7	–
Control	54.36	5.96	53.6%
Treatment	51.65	6.50	26.3%
Treat Effect	-2.71	0.54	-27.2

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 "Treatment" refer to the groups without and with evasion opportunity, respectively. Reported are the mean prices and number of units traded. "Incidence" is the share of the statutory tax rate (10 ECU) that is shifted onto buyers. "Treat Effect" indicates the non-parametric treatment effect defined as the difference between treatment and control group. All numbers expressed in Experimental Currency Units.

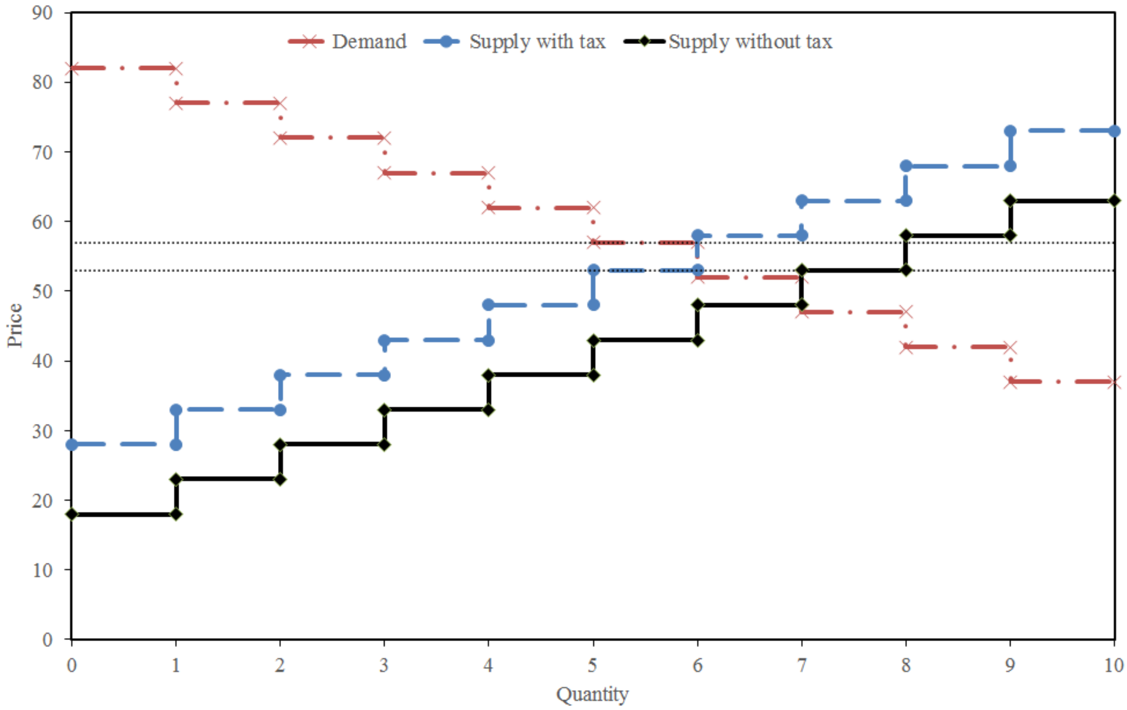
Table 8: Additional Treatments and Incidence of Effective Tax Rate

Condition	Price	Units	Incidence Effective Tax
No-Tax	49	7	–
Control	54.36	5.96	53.6%
Treatment	51.65	6.50	103.5%
Tax Credit	50.09	6.89	43.6%

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 "Treatment" refer to the groups without and with evasion opportunity, respectively. "Tax Credit" refers to the additional treatments without evasion opportunity and a tax credit of 7.5 ECU. Reported are the mean prices and number of units traded. "Incidence" is the share of the effective tax rate (10 ECU in Control, 2.56 ECU in Treatment, 2.5 ECU in Tax Credit) that is shifted onto buyers. All numbers expressed in Experimental Currency Units.

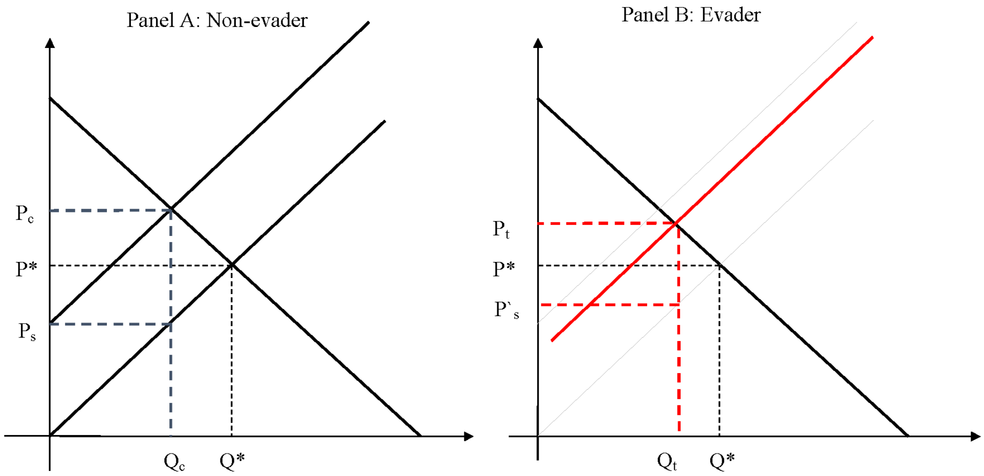
Figures

Figure 1: Supply and Demand Schedule



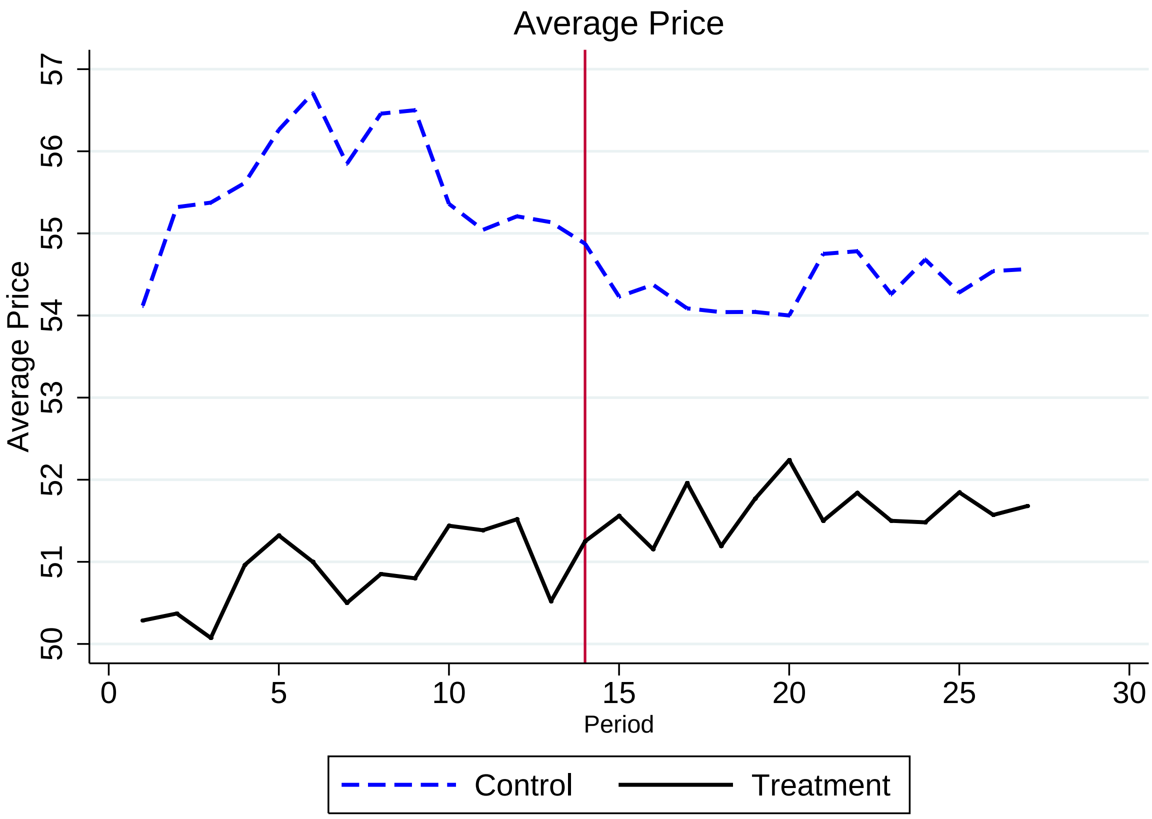
Note: The figure is adapted from Grosser and Reuben (2013, page 42, Figure 1). It shows the demand schedule for buyers and the supply schedule for sellers with and without the per unit tax. The predicted equilibrium occurs where the curves intersect: quantity $q = 7$ and price p between 48 and 52 without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

Figure 2: Economic incidence of tax on seller



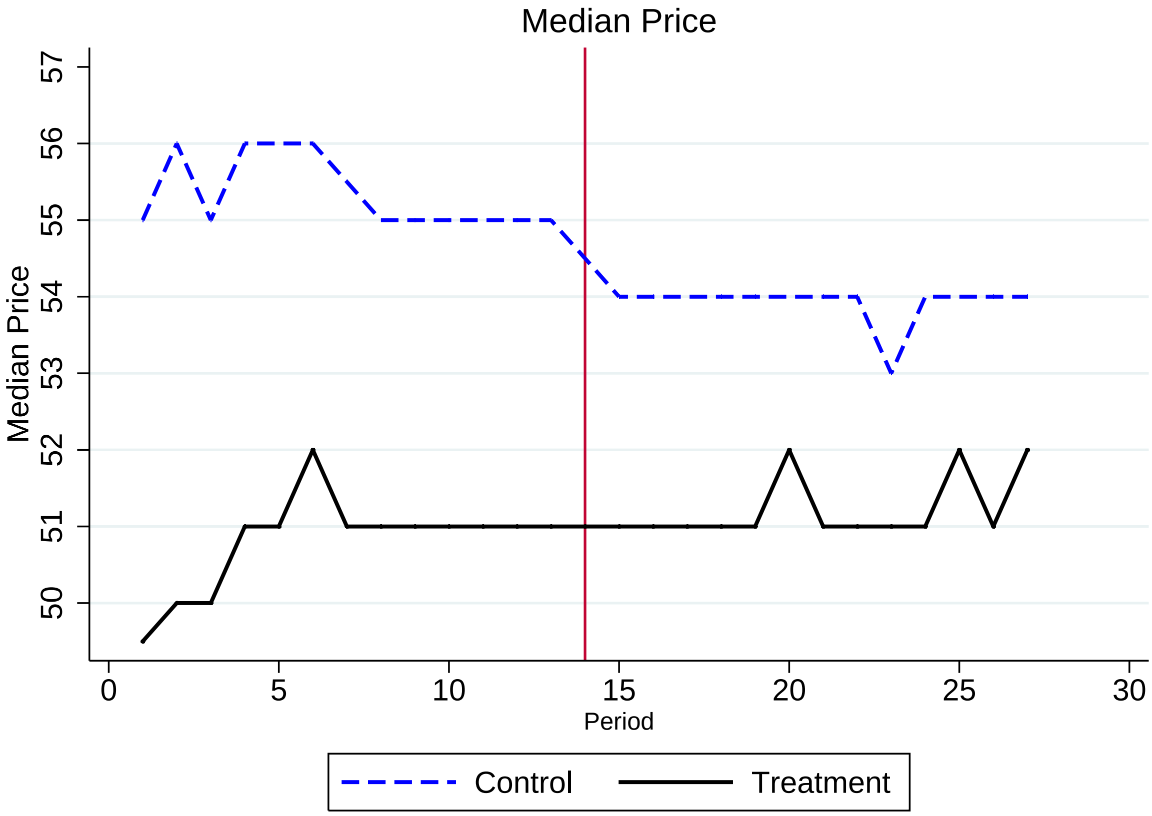
Notes: The imposition of a per-unit tax would ordinarily cause the supply curve to shift to the left and the market equilibrium to move from point (P^*, Q^*) to (P_c, Q_1) as illustrated in panel A. Because sellers are able to evade the tax, the supply curve shifts by a smaller amount causing the equilibrium to move from (P^*, Q^*) to $(P_c^{'}, Q_1^{'})$ as illustrated in panel B, where $P_c^{'} < P_c$.

Figure 3: Average market price by period and treatment



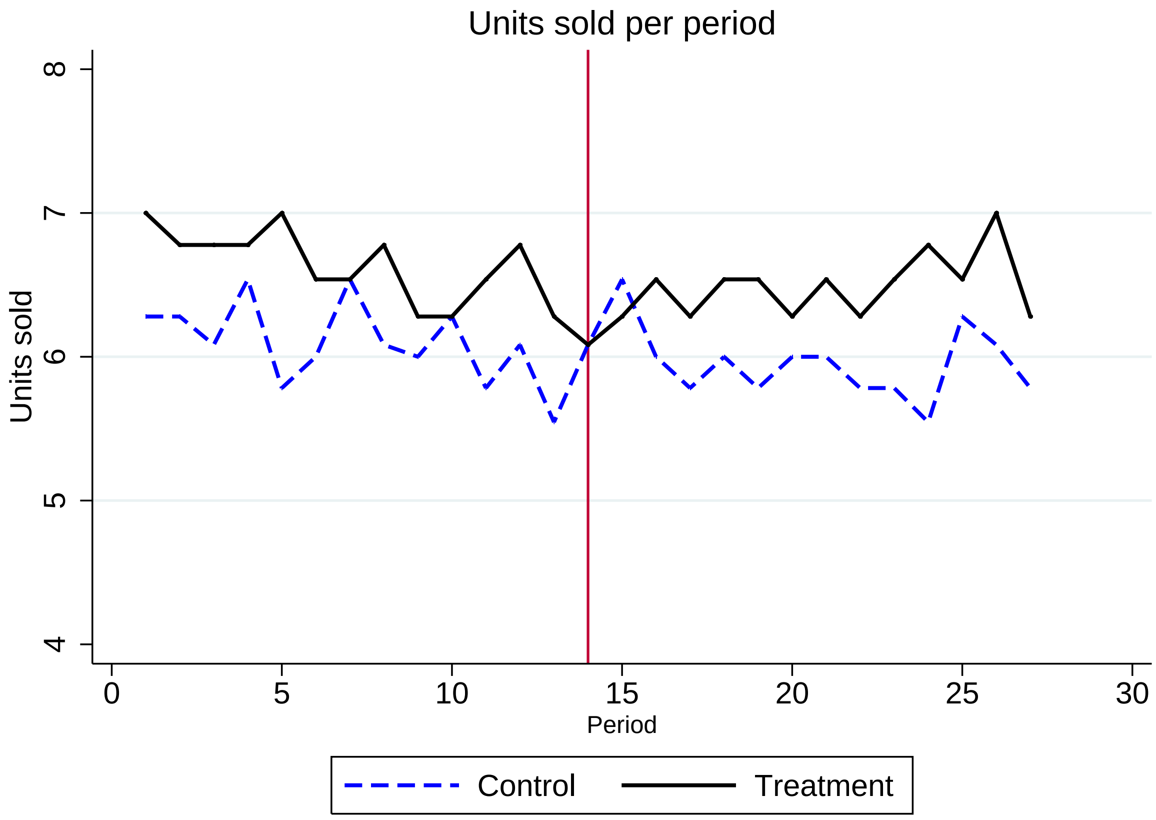
Notes: Reported is the average market price $\overline{P}$ in each period for the treatment and control groups. The vertical line indicates period 14; empirical results are based on market periods 15 to 27.

Figure 4: Median market price by period and treatment



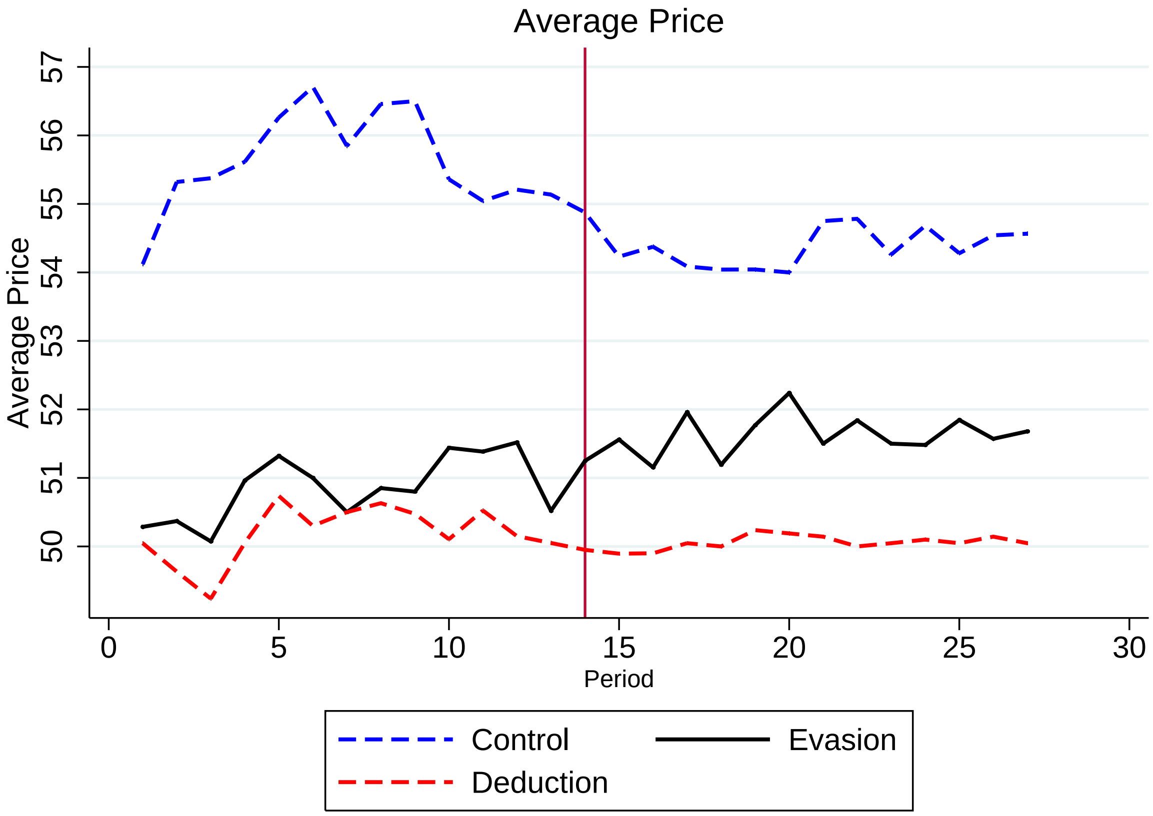
Notes: Reported is the median market price P_{50} in each period for the treatment and control groups.The vertical line indicates period 14; empirical results are based on market periods 15 to 27.

Figure 5: Units sold by period and treatment



Notes: Reported is the number of units sold in each period for the treatment and control groups.The vertical line indicates period 14; empirical results are based on market periods 15 to 27.

Figure 6: Additional treatments: Average market price by period and treatment



Notes: Reported is the average market price $\overline{P}$ in each period for the treatment group, control group and the additonal treatments. The vertical line indicates period 14; empirical results are based on market periods 15 to 27.

Appendices

A Tables

Table 9: Impact of treatment on market price and sales: adjustment for small number of clusters

	Price			Sales
	Mean	Median	Ask	
Treat	-2.701**	-2.087***	-3.077**	0.538**
	(1.123)	(0.743)	(1.398)	(0.232)
Constant	54.362***	53.779***	54.769***	5.923***
	(0.000)	(0.000)	(0.000)	(0.000)
R-Squared	0.553	0.538	0.162	0.234

Notes: 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). 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, and number of observations is 104.

Table 10: Impact of treatment on market price

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
	Ask Price		Mean Ask Price		Median Ask Price	
Treat	-2.662***	-2.660***	-2.651***	-2.651***	-1.589***	-1.589***
	(0.065)	(0.069)	(0.075)	(0.081)	(0.218)	(0.233)
Age	-0.367***	-0.370***	-0.368***	-0.368***	-0.641***	-0.641***
	(0.017)	(0.017)	(0.021)	(0.022)	(0.059)	(0.064)
Gender	-21.352***	-21.389***	-21.435***	-21.435***	-17.990***	-17.990***
	(0.177)	(0.174)	(0.219)	(0.234)	(0.633)	(0.676)
German	29.607***	29.642***	29.663***	29.663***	22.833***	22.833***
	(0.347)	(0.329)	(0.410)	(0.438)	(1.186)	(1.267)
Tax Morale	-1.274***	-1.258***	-1.245***	-1.245***	-0.921	-0.921
	(0.219)	(0.222)	(0.254)	(0.271)	(0.735)	(0.786)
Economics	5.126***	5.141***	5.156***	5.156***	2.562***	2.562***
	(0.153)	(0.162)	(0.183)	(0.195)	(0.529)	(0.565)
Constant	49.500***	49.481***	49.508***	49.443***	60.175***	60.314***
	(0.491)	(0.593)	(0.572)	(0.750)	(1.655)	(1.809)
R2	0.276	0.279	0.884	0.894	0.853	0.878
N	644	644	104	104	104	104
Period FE	No	Yes	No	Yes	No	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 (7) with the dependent variable defined as the market price for each good in each market period in Models 1 and 2; mean market price in a given market period in Models 3 and 4; and median market price in a given market period in Models 5 and 6. All panels use completed contracts from periods 15 to 27. Period FE is period fixed effects. 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, tax morale is a dummy that is equal to 1 for subjects who believe cheating on taxes can never be justified and Field of study is a dummy that is equal to 1 if field of study is economics.

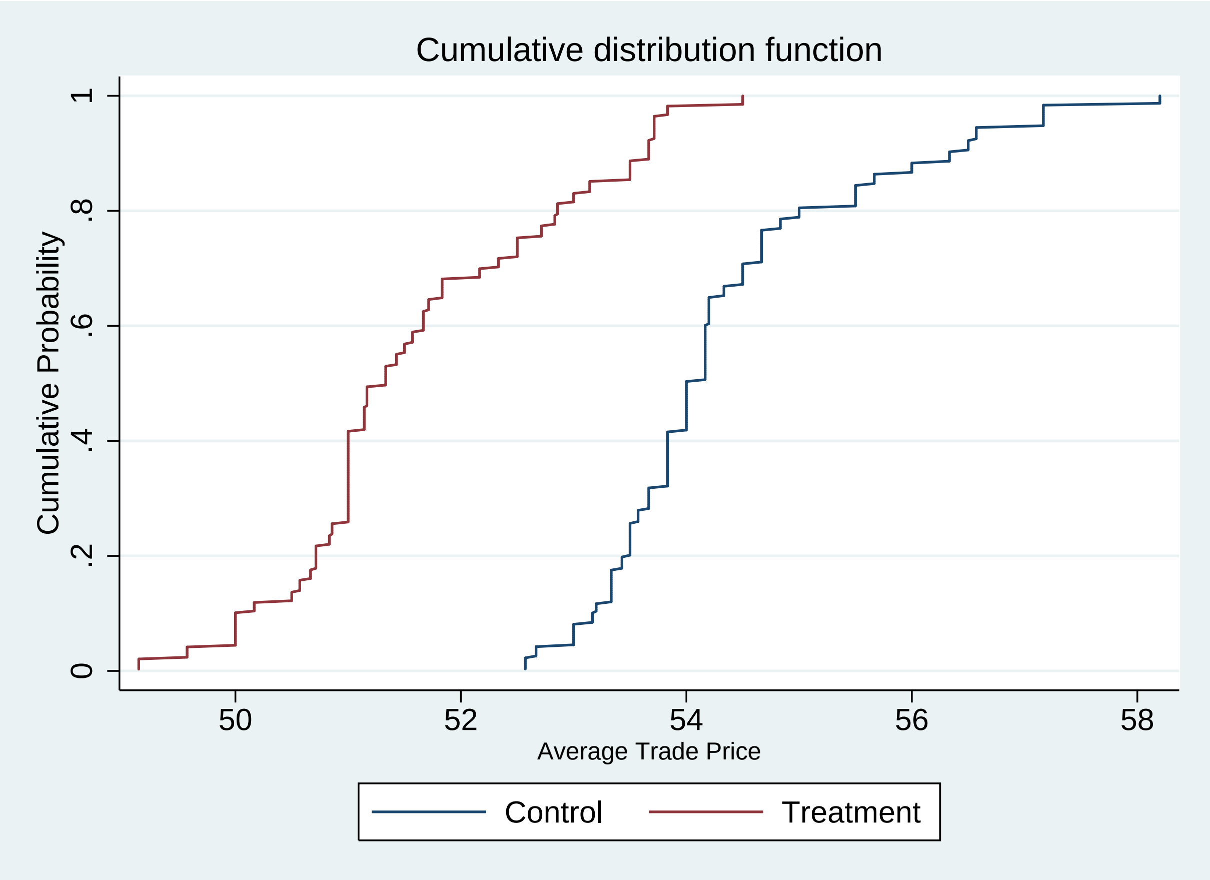
Table 11: Impact of treatment on units sold

	Model 1	Model 2	Model 3	Model 4
Treat	0.539***	0.540***	0.385***	0.383***
	(0.171)	(0.173)	(0.131)	(0.131)
Age			-0.017	-0.017
			(0.035)	(0.035)
Gender			2.349***	2.343***
			(0.353)	(0.363)
German			-2.000***	-1.973***
			(0.691)	(0.691)
Tax Morale			0.495	0.479
			(0.436)	(0.448)
Economics			-0.351	-0.349
			(0.305)	(0.305)
Constant	5.961***	6.147***	6.832***	7.005***
	(0.088)	(0.231)	(0.978)	(1.064)
R2	0.235	0.315	0.352	0.433
N	644	644	644	644
Control variables	No	No	Yes	Yes
Period FE	No	Yes	No	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 (7) with the dependent variable defined as the number of units sold in a given market period. Estimation is based on all completed contracts in periods 15 to 27. Period FE is period fixed effects. 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, tax morale is a dummy that is equal to 1 for subjects who believe cheating on taxes can never be justified and Field of study is a dummy that is equal to 1 if field of study is economics.

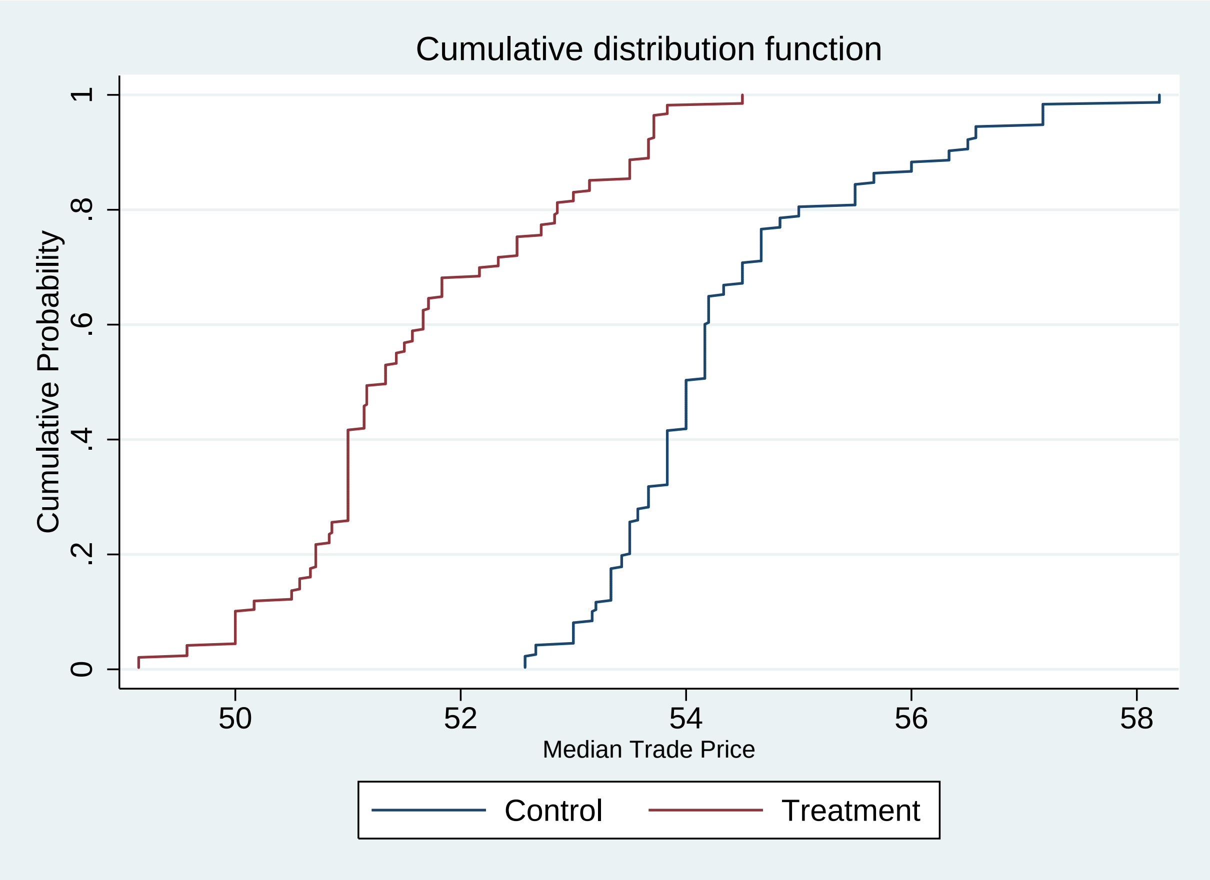
B Figures

Figure 7: Cumulative distribution of average market price by treatment



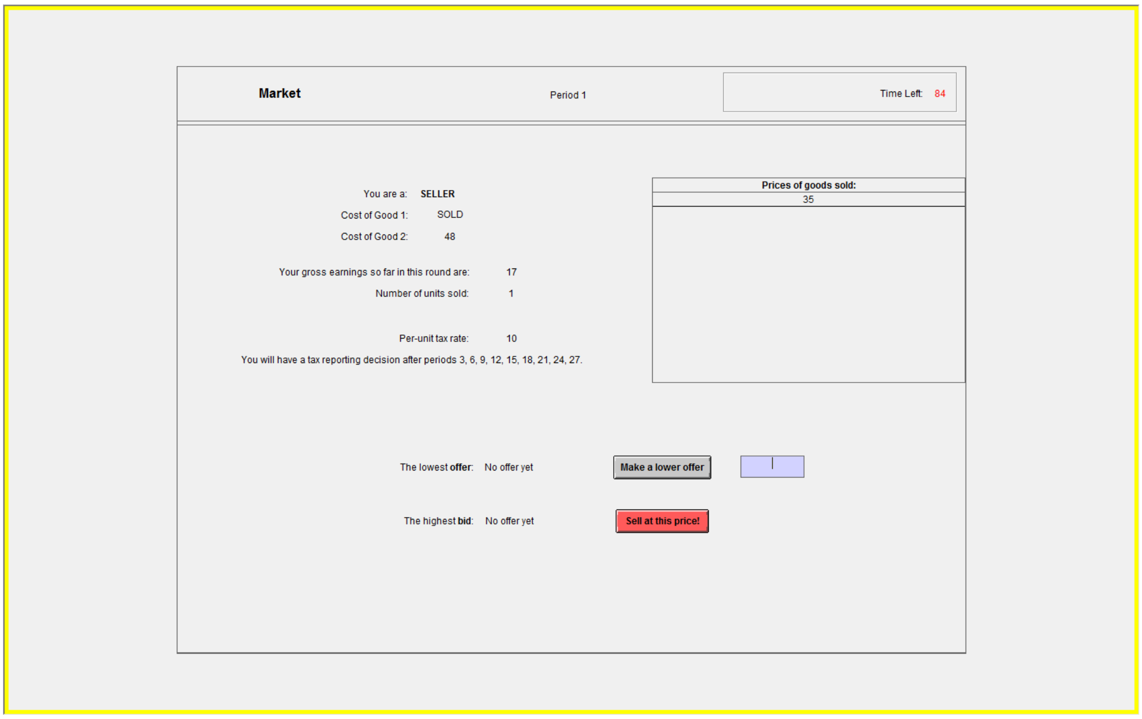
Notes: Reported is the cumulative distribution of average market price $\overline{P}$ for the treatment and control groups. Distributions are based on data from market periods 15 to 27. Two-sample Kolmogorov-Smirnov test for equality of distribution functions reports a maximum difference in distributions of 0.770 with pvalue of 0.000. This implies that the null hypothesis that the distributions are equal is rejected.

Figure 8: Cumulative distribution of median market price by treatment



Notes: Reported is the cumulative distribution of median market price P_{50} for the treatment and control groups. Distributions are based on data from market periods 15 to 27. Two-sample Kolmogorov-Smirnov test for equality of distribution functions reports a maximum difference in distributions of 0.751 with pvalue of 0.000. This implies that the null hypothesis that the distributions are equal is rejected.

Figure 9: 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).

C 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 group and indicate the differences between groups 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 Group:***
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.
]

[***Treatment Group 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.
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[***Additional Treatment 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.
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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

<u>Screenshots from trading market</u>

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* ***treatment group with evasion opportunity****:*

<u>The Reporting Decision for Sellers</u>
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.
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[*Net income information in the* ***control group:***

<u>**Calculation of Net Income for Sellers**</u>

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:

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

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[*Net income information in the* ***treatment group with evasion opportunity:***

<u>**Calculation of Net Income for Sellers**</u>

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:

<u>Net income</u> = 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)
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[*Net income information in the **additional treatments 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))
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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.