How Does Firm Tax Evasion Affect Prices?

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Abstract

We investigate the causal link between tax-evasion opportunities and market prices in a controlled experiment where buyers and sellers trade a fictitious good in competitive 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 markets with evasion opportunities are characterized by an equilibrium with lower price. Results from additional experimental treatments suggest that this lower price is due to the evasion opportunity itself rather than the lower effective tax rate. Our findings have implications for tax incidence. In particular, sellers with access to evasion shift a smaller share of the nominal tax rate onto buyers relative to sellers without tax evasion opportunities. Additionally, we find that sellers with evasion opportunities shift the full amount of their *effective* tax rate onto buyers.

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

1 Introduction

A number of academic studies have documented the prevalence of tax evasion among firms and self-employed individuals (e.g., Slemrod 2007; Kleven et al. 2011; Mikesell 2014; Fox et al. 2014; De Simone et al. 2020).[1] Because tax evasion reduces revenues and thus negatively impacts budgets, many governments have been investing resources into evasion-reducing policies. For example, citing revenue loses, state governments in the US pushed for changes to the definition of nexus in an attempt to reduce non-compliance with the *use-tax*.[2] There is also a global effort to adopt policies that would limit evasion and (aggressive) tax avoidance: e.g., framework on Base Erosion and Profit Shifting (BEPS) and the global minimum corporate income tax agreed to by the G-7 nations. While these policies are expected to increase tax revenues, they could also affect market prices and thus change the distribution of tax burdens with important implications for the equity profile of the affected tax systems. Because equity is such an important consideration when designing tax policy, it is important that we understand the price impacts of evasion.

The academic literature provides ambiguous theoretical predictions of the relationship between tax evasion and prices. One prediction is that the evasion-induced reduction in tax burden gives firms scope for offering their goods at lower prices, and thereby increases demand for their goods. As a result, the evasion opportunity would lead to lower prices and higher sold quantities. However, there are at least two conceptual reasons that give rise to an alternative prediction. First, Bayer and Cowell (2009) show that under certain conditions, (risk neutral) taxpayers make evasion decisions separately from other decisions. Second, it has been shown that taxpayers often behave in ways that are consistent with 'choice bracketing': in a sequence of interrelated decisions, people frequently

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

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

make each decision in isolation, although the decision problem should be solved simultaneously (Read et al. 1999; Blaufus et al. 2022). Both separability and choice bracketing imply that pricing decisions are made independently of subsequent evasion decisions and, as a result, evasion opportunities do not affect market prices.

The extent to which tax evasion opportunities affect market prices and quantities is therefore eventually an empirical question. However, empirical evidence on this question is very scarce. The objective of our paper is to contribute to filling this gap in the literature by asking whether firms shift evasion-induced reduction in costs to consumers via lower prices. Specifically, we estimate the causal effect of evasion opportunities on market prices and sold quantities. We focus on a situation where firms remit sales taxes and have an opportunity to evade these taxes. Our context-specific research question is: are prices different in markets where the evasion of sales taxes is an option relative to markets where sales taxes cannot be evaded? The results of our research then have implications for the question of whether efforts to curb these evasion opportunities will have a material impact on market prices (and also on the profits of firms and the distribution of tax burdens between buyers and sellers).

We address our research question using data generated in a controlled laboratory experiment[3] where participants trade fictitious goods in a competitive double auction market (Smith 1962, Dufwenberg et al. 2005). Experimental participants are randomly assigned roles as sellers or buyers in treatment and control groups, and a per-unit sales 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.

[3]Laboratory experiments are frequently used in taxation and accounting research; examples include: Anctil et al. (2004), Ruffle (2005), Maas et al. (2012), Grosser and Reuben (2013), Falsetta et al. (2013), Barron and Qu (2014), Elliot et al. (2015), Bobek et al. (2016), Blaufus et al. (2017), Brueggen et al. (2018), Hurley et al. (2019), Chen et al. (2020), and Gonzalez et al. (2020). We discuss the external validity of our laboratory experiment in Section 7.

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

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. The number of units traded is higher in the case with evasion (though this difference is not always precisely measured). These empirical results support the prediction that access to evasion affects prices and quantities. The simple rationale for this prediction is that sellers with an evasion option are able to reduce their effective tax rate relative to those without evasion. This allows firms with evasion opportunities to offer their goods at lower prices. On the market level, evasion-induced reductions in effective tax rates imply that the tax causes the industry supply curve to shift up by a smaller amount relative to situations without access to evasion.

Our findings suggest that price differences are driven by the lower effective tax rate in the evasion treatments. But do all reductions in effective tax rates have the same effect? Or, does it matter that the lower effective tax rate is the result of tax evasion? We explore this question in a 'tax-credit' treatment where sellers' tax burdens are lowered to the effective tax burden observed in the evasion treatment using an exogenously determined tax credit. Comparing the tax-credit treatment and the evasion treatment then allows us to explore a situation where all sellers have the same effective tax burden, but some sellers must evade taxes to arrive at this lower tax burden whereas other sellers do not have to evade taxes to arrive there.

We find that the market price in the tax-credit treatment is lower than the market price in our main evasion treatment. Although this difference is not always precisely measured, it is economically meaningful. This is an indication that the price effects that result from exogenous changes in tax burdens cannot be used to quantify the likely price effects of policies targeting tax evasion. An important question we do not address empirically is, why do equivalent exogenous and evasion-driven reductions in the effective tax burden not have the same effect on prices? One possible explanation is that sellers adjust prices to reflect the fact that tax evasion is a costly (audit risk and/or moral costs) way of lowering effective tax rates.

The price effects that we find are used to investigate the economic incidence of the nominal and effective tax rates.[4] We document the following incidence results. First, the share of the *nominal* tax rate borne by buyers is approximately 50 percent lower in the presence of evasion. Second, we find that sellers with an evasion opportunity shift their full *effective* tax rate onto buyers. The results from the tax-credit experiment suggest that the full shifting of the effective tax rate is due to the evasion-induced lowering of the effective tax rate.

1.1 Contribution to the Literature

Our paper contributes to different strands of literature. First, our paper adds to the literature studying tax evasion.[5] Unlike most of the tax-evasion literature, we investigate

[4]Throughout the paper, we refer to the tax rate that is legally due as the *nominal* tax rate. However, some taxpayers evade part of their legal tax liability, which effectively reduces the tax rate due. The *effective* tax rate then refers to actual tax payment as a share of true taxable income, accounting for fines. See Section 3 for a more comprehensive definition.

[5]Andreoni et al. (1998), Alm (2012), Slemrod (2017) and Slemrod (2019) provide general surveys on tax-compliance research. 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 randomized experiments to study evasion (see references above).

the implications of tax evasion for an outcome variable rather than investigating the determinants of tax evasion.[6] Our main contribution to the literature is the finding that price setting is affected by the opportunity to evade taxes. In addition, the distinction between differential effects of exogenous and evasion-induced reductions in the tax burden adds new insights to the literature.

One further difference to most of the evasion literature is that we focus on transaction taxes rather than income taxes.[7] Our paper further relates to the study by Hoopes et al. (2016) who show that online retail firms (e-tailers) have a competitive advantage over traditional (brick and mortar) retailers because of widespread use-tax evasion among consumers.[8] The paper does not study if e-tailers set different prices than traditional retailers. Additionally, our results suggest that enforcement efforts to curb tax evasion impact consumption prices and quantities, and thus have implications for stakeholders outside the firms subject to the enforcement efforts. In this respect, we provide evidence of a tax enforcement externality that has not been shown before, and thereby add to literature such as Guedhami and Pittman (2008) and Gallemore and Jacob (2020) who provide evidence of other types of tax enforcement externalities.

Second, tax evasion and avoidance activities have the common effect of allowing firms to reduce their tax liability through lowering (legally or illegally) their tax base. Although our particular set-up studies tax evasion, rather than avoidance, the general mechanism behind our results potentially also applies to avoidance.[9] Similar to tax

[6]In a related recent paper, Kotakorpi et al. (2021) study the effect of different tax-reporting institutions on pricing decisions.

[7]In an overview article on tax research, Dyreng and Maydew (2018) identify that there is little research on non-income-based taxes (such as sales taxes) in the literature. They consider this lack of research to be surprising in light of the importance and prevalence of these types of taxes around the world. Our focus on sales taxes and their effects on prices thus contributes to closing this gap in the literature. One notable exception is Fox et al. (2014) who study tax evasion in the context of corporate transactions and provide indications for the existence of tax evasion in the context of commodity flows and destination based taxes.

[8]In this context, we also relate to a recent paper by Arya and Mittendorf (2018) which models the decision of online retailers to establish a physical presence in light of the sales-tax implications of such a move.

[9]The close link between legal tax avoidance and illegal tax evasion is for example emphasized by Hanlon and Heitzman (2010) who highlight that the distinction between avoidance and evasion is often difficult. Just as with evasion, aggressive tax avoidance strategies among firms come with uncertain outcomes (Blouin 2014); for example reputation damages (Dyreng et al. 2016) and the possibility of having to make future tax payments based on current tax positions (e.g., because the tax authorities

evasion, tax avoidance possibilities might give firms scope to sell their goods at lower prices, relative to a counterfactual situation where tax avoidance is not possible and where the effective tax rate is thus higher. This is for example consistent with the theoretical framework of Dyreng et al. (2022) (summarized in Jacob 2022) according to which tax avoidance mutes the effects of (corporate) taxes. However, there is very little empirical evidence on such muting effects (as concluded by Jacob 2022), and our paper provides an indication that closing avoidance channels (for example through the OECD BEPS initiative) potentially has real consequences (lower prices, higher quantities) because the muting effects of avoidance are reduced.

We are aware of only one study that sheds light on the relationship between tax-avoidance opportunities and prices. Jacob et al. (2021) find that the pass-through of business taxes to gasoline consumption prices is larger among gas stations belonging to firm groups headquartered in countries with stricter anti-avoidance rules. Consistent with our paper, this finding points to the importance of avoidance opportunities for prices. We view our results as complementary to this paper. First, while the paper uses archival data with arguably higher external validity, we use a controlled laboratory setting which allows for clean identification of the causal effect of interest.[10] Second, an obvious difference is that Jacob et al. (2021) study tax avoidance whereas we study evasion. As discussed above, while there are conceptual similarities, evasion and avoidance remain distinct concepts and it is thus important to collect distinct evidence for both evasion and avoidance. Third, we are able to study quantity effects and to examine whether the effect is simply explained by a reduced effective tax rate.

Another related study from the tax-avoidance literature is Dyreng et al. (2022)

do not approve the tax-avoidance strategy). Note, however, that there remain important differences between illegal evasion and legal avoidance, and we fully acknowledge that it may well be that avoidance opportunities have a differential effect on prices than evasion opportunities. For example, Blaufus et al. (2016) find differences between tax evasion and tax avoidance in a laboratory setting. One particularly important difference is that tax evasion is unlikely to be part of the tax strategy of listed and larger private firms. To this extent, our specific evasion results are particularly informative for smaller, private firms. Yet, as we discuss, our findings potentially also have implications for firms relying on tax-avoidance strategies, rather than tax evasion.

[10]One potential threat to identification in the relevant empirical test in Jacob et al. (2021) is that differences in price-setting behavior across gas stations belonging to groups headquartered in different countries are partly driven by other headquarter-country related factors.

who study the effect of tax incidence on tax avoidance using archival data. They show empirically that firms that are not able to shift the burden of taxes to workers (because of relatively elastic labor supply of their high-skilled employees) are more engaged in tax avoidance than firms who face inelastic labor supply. In other words, the paper finds that tax incidence affects avoidance. We complement this paper in that we find a relationship between evasion (rather than avoidance) and incidence, though in a set-up where causality runs in the opposite direction and using a different empirical approach.[11] More generally, studying one potential consequence of tax avoidance, we relate to a stream of papers that also examines the consequences of tax avoidance – though not for prices and quantities (see Jacob 2022 for an overview).[12]

Third, we relate to further papers in the domain of tax incidence. We know of two studies that estimate tax incidence in the presence of tax evasion: Alm and Sennoga (2010) study a CGE model in this context and Kopczuk et al. (2016) use archival data to study incidence across situations in which diesel taxes are taxed at different production stages. Since we rely on the controlled environment of the lab, our empirical approach provides precise control over the market institutions and allows us to randomize access to evasion and measure non-compliance accurately. As a result, we are able to offer cleaner causal identification of the impact of tax evasion on the economic incidence of the tax than these two studies. 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.

[11] Evidence that the causality between avoidance/evasion and incidence can run in both directions is not a threat to causal identification in our empirical set-up because we have randomized evasion opportunities. Dyreng et al. (2022) circumvent the potential identification threat of reverse causality by exploiting exogenous variation in avoidance costs that comes from the 1997 Check-the-Box regulation. In particular, they study if firms facing elastic versus inelastic labor supply responded differently to this regulation. Obtaining similar exogenous variation in real world contexts appears more challenging for tax evasion than for tax avoidance, because of the inherent problem that evasion is not observable (Slemrod and Weber 2012) and, as a result, variation in evasion opportunities across markets/industries is not well known. This adds to our decision to rely on a laboratory experiment.

[12] For example, we relate to papers studying the consequences of tax avoidance in capital markets (Desai and Dharmapala 2009; Hanlon and Slemrod 2009; Wilson 2009; Kima et al. 2011; Hasan et al. 2014; Chow et al. 2016; Edwards et al. 2016; Goh et al. 2016), for capital structure (Heider and Ljungqvist 2015), for forced managerial turnover (Chyz and Gaertner 2017), and for the reputational costs of tax avoidance (Gallemore et al. 2014; Graham et al. 2013).

2 Experimental Design

2.1 Overview

The experimental design reflects a standard competitive experimental double auction market as pioneered by Smith (1962).[13] 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 fictitous good. Sellers are assigned costs for each unit and buyers are assigned values. The roles of sellers and buyers as well as the costs and values are exogenous and randomly assigned to the lab participants. We impose a per-unit tax on sellers – which we refer to as the *nominal* tax rate – to this set-up and give sellers in 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.

2.2 Organization

We ran a total of 16 experimental sessions, where 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 eight control and eight treatment sessions with a total of 160 subjects.[14] The experiments were programmed utilizing

[13] 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.

[14] We ran eight sessions (four treat/four control) in 2013 and eight sessions (four treat/four control) in 2021. The 2013 sessions were conducted in the Cologne Laboratory for Economic Research (CLER) at University of Cologne and the 2021 sessions were conducted in the WISO Experimental Lab at University of Hamburg. Both laboratories are well established and modern experimental laboratories; further information about the labs are online at `https://ockenfels.uni-koeln.de/en/experiments/` (Cologne) and `https://www.wiso.uni-hamburg.de/en/forschung/forschungslabor/experimentallabor.html` (Hamburg). Participants were invited and managed through the recruitment software *ORSEE* in Cologne (Greiner 2015) and *hroot* in Hamburg (Bock et al. 2014). Potential participants were not informed about the content of the experimental sessions upon the invitation to participate in the experiments. Reassuringly, the experimental outcomes are very similar between the 2013 sessions conducted in Cologne and the 2021 sessions conducted in Hamburg; see Table 4 which compares experimental outcomes for the 'old' and 'new' data.

z-tree software (Fischbacher 2007). Appendix Section B provides summary statistics on demographic characteristics of the participants. 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.[15] It was public information that all tax revenue generated in the experiment would be donated to the German Red Cross.[16]

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 F.

2.3 Description of a session

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

Trade in the Double Auction. 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. 2018; Grosser and Reuben 2013). The demand schedule for buyers assigns a value to each of two items and the supply schedule for sellers assigns a

[15] In addition, subjects received a show-up fee, which was 2.50 EUR in the 2013/2015 sessions and 6.00 EUR in the 2021 sessions.

[16] Note that we ran eight further experimental sessions in the context of a tax-credit treatment to shed light on the mechanisms behind the first set of experiments (which is discussed here). The details regarding this tax-credit treatment are in section 5.

cost to each of two items. The cost/value of the units vary across items and subjects as illustrated in Appendix Table 13. 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, as in the literature, sellers must sell their cheaper unit before they sell their more expensive unit. Similarly, each buyer can post a "bid" that is higher than the current bid on the market, but lower than the value of the item to the buyer. Therefore, buyers cannot buy an item at a price that exceeds its value. Buyers must also buy their most valued item before their least valued item. The lowest standing ask and the highest standing bid are displayed on the computer screen of all ten market participants.[17]

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

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

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

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. 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 nominal per-unit tax rate. Because buyers do not pay a tax, their net income for each tax collection period may be written as:

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

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

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 behavior of market participants. Although reporting every period would maximize the number of reporting decisions, we opted against this option because excess supply in the market implies that some subjects will sell zero units in a given trading period, which trivializes the reporting decision. Another option is to have subjects make a single reporting decision at the end of the experiment. While this approach maximizes the chance that everyone has a positive amount to report, having a single reporting decision would not allow subjects to learn or update their beliefs. We opted for every third round as a reasonable compromise between these two extremes.[18]

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

[18] 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.

income in each tax collection period is rewritten as:

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

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

2.4 Market Equilibrium without Evasion

The demand and supply schedules described and displayed in Figure 1 (and Appendix Table 13) can be used to determine the competitive equilibrium price and quantity with and without the per-unit tax. Theoretically, we expect the market to clear with 7 units traded at any price in the range 48 ECU to 52 ECU in the case without taxes. We obtain a range of prices in equilibrium because the demand schedule is stepwise linear (Ruffle 2005; Cox et al. 2018; Grosser and Reuben 2013).[19]

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 linearized form of the demand and supply schedules have equal elasticity in equilibrium, the incidence of the tax should theoretically be shared equally between buyers and sellers; buyers pay an extra 5 ECU and sellers receive 5 ECU less (after paying the tax), relative to the case without a tax.[20]

The question we seek to answer is whether this equilibrium outcome is affected by

[19]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.

[20]We are aware that the price elasticities are not properly defined in equilibrium given that the demand and supply schedules are only piece-wise linear. However, for ease of exposition, we assume the the schedules are linear in order to illustrate the likely economic incidence of the per-unit tax. Notice that the linearized form of the schedules have equal slopes and thus equal elasticities in equilibrium.

the presence of tax evasion opportunities among sellers.[21] The next section provides a theoretical discussion for why tax evasion may or may not affect prices (and quantities).

3 Conceptual Framework

This section provides a conceptual framework of the relationship between evasion opportunities and market prices (as well as quantities). There are generally two opposing theoretical predictions for the effect of evasion opportunities on prices. We describe the rationale behind both predictions in the following.

3.1 Evasion opportunity affects prices and quantities

For simplicity, let's assume that demand and supply curves are linear. 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 nominal tax 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 nominal 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.[22] As illustrated in panel B of Figure 2, this then implies that the market supply curve in the presence of evasion opportunities shifts

[21] Note that we study the introduction of tax evasion opportunities in a market where everyone knows that evasion is possible. Analogously, everyone (buyers and sellers) in the no-evasion control condition knows that evasion is not possible. That is, in both treatments buyers have information about whether tax evasion is possible or not. We argue that this distribution of information reflects the 'real world' because efforts to address non-compliance are generally publicly announced and debated. The information set of the buyers should not matter if the effect is driven largely by sellers seeking to maximize profits by underpricing their competitors.

[22] 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).

up by less than the nominal 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., $(q_t > q_c)$.

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.[23] Note that sellers have to pay the nominal 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:

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

where s_i denotes the number of units a seller actually sells and r_i is the number of units she reports.[24] This simple equation shows that the effective tax rate is increasing in the nominal tax rate and decreasing in evasion (for $\gamma \leq 0.5$).[25] 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

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

[24]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 nominal tax rate τ for sellers who either do not evade or do not have an option to evade.

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

quantitative effects of the treatment on prices and quantities.[26]

3.2 Evasion opportunity does *not* affect prices and quantities

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

Separability. First, one finding in the theoretical literature is that firms treat their evasion and pricing decision as separable; sellers first set a price at which to sell, and then later make their evasion decision (Bayer and Cowell 2009). This setting is generalized to other forms of competition (e.g., Lee 1998; Cowell 2003). Intuitively, the separability result is analogous to other types of uncertainty models; for example, investment models in which the decision over how much to invest in total is separable from the decision on how much to invest in individual assets. In this case, the opportunity to evade has no bearing on market prices and quantities, and the incidence of the tax is hence also unaffected by the presence of tax evasion among sellers. The separability result thus implies that the equilibrium price and quantity that arise in a market *with* evasion opportunities is the same as in a market *without* evasion opportunities (i.e., ($p_t = p_c$) and ($q_t = q_c$).

Bayer and Cowell (2009) describe that the separability result emerges when a risk-neutral firm faces concealment costs and a tax audit with an exogenous probability. The separability result breaks down in a set-up with endogenous audits, and prices are then affected by evasion opportunity (Marrelli 1984; Lee 1998; Bayer and Cowell 2009). However, we have an exogenous audit probability in our experimental set-up. The second assumption behind the separability result is that firms are risk neutral. From an ex-ante perspective, it is not clear whether the participants in our experimental setting are risk neutral or not. The conceptual idea of separability is therefore potentially relevant from an ex-ante perspective (i.e., before running the experiment) and an appropriate starting point to reflect the conceptual channels in our setting. However, we acknowledge that

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

the literature points towards the direction that participants in laboratory experiments are mostly not risk neutral (Noussair et al. 2013).

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

Such choice bracketing is potentially relevant in the context of our empirical design. Although the tax in our experimental design seems simple at first glance, sellers trade and make pricing decisions in every round, and they make evasion decisions every third round. This creates a time disconnect between the pricing decision and the evasion decision. Furthermore, sellers operate in a fast-paced market where they are required to track bids, asks, number of items sold, tax liability, and time remaining in a trading period. It is not inconceivable that sellers operating in this kind of environment would make the initial pricing decision in isolation of the subsequent evasion decision to minimize mental-decision-making costs.[27]

[27]Note that, in contrast to the above separability result, the choice bracketing rationale also holds for

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

Second, why would choice bracketing apply to tax evasion and not the tax itself; i.e., should choice bracketing affect pricing in the control group too? While it is possible that choice bracketing leads subjects to view the pricing decision separately from the tax payment, the data do not suggest that subjects acted in this way. In particular, the observed equilbrium price in the control group reflects an equal split of the tax between buyers and sellers as predicted by theory (see Section 6 below). That sellers fully account for the tax in their pricing decisions makes sense since all that is required is adding 10ECU to the cost of each item. Accounting for tax evasion is a lot more difficult. The subject must account for the effective tax rate – rather than the nominal tax rate –, which is only known in expectation since the subject must consider audit probability and penalty.

Separability of decisions and choice bracketing thus both imply that pricing decisions of sellers are made independently of subsequent evasion decisions. Note that the underlying mechanisms are different: in the first case, it is theoretically optimal to have separate decisions, whereas the second case assumes that taxpayers treat the decisions independently because they are rationally inattentive and thus require a heuristic. It is worth noting that our experiment is not designed to distinguish between choice bracketing and separability. The goal of this section is simply to demonstrate that the relationship between access to evasion and pricing behavior of sellers is ambiguous and thus requires empirical analysis.

risk averse taxpayers.

4 Empirical Strategy and Results

Recall that we are interested in identifying the impact of tax evasion opportunities on prices and sold quantities. We describe the empirical strategy used to identify these effects 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. One 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. For reasons of brevity and consistency with the quantity results (which are on the market-period level; see below), the discussion of the results focuses on the average price in a given market-period, $\overline{P}$.

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 28) 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 16 independent observations; eight in the treatment and eight in the control groups.[28] We also test for differences in distributions across the treatment

[28]While the number of independent observations, 16, appears to be low, it is not unprecedented to use such few observations in empirical analysis; see for example Grosser and Reuben (2013) who apply nonparametric tests based on four independent market-level observations and have sufficient statistical

conditions (using Kolmogorov-Smirnov test for equality of distribution functions).

Regressions. We estimate treatment effects in a regression framework by regressing our the measure of price ($\overline{P}$ in the baseline) on a treatment dummy:

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

where $\overline{P}_{i,m}$ is the mean price of the good in period i (with $i = 1, ..., 27$) of market m (with $m = 1, ..., 16$). T_m is a dummy for the treatment state, which is equal to one if market m is in the treatment group (with evasion opportunity) and zero otherwise. Our coefficient of interest is δ, which represents the difference in market price between 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 (which gives 16 clusters).[29] Because the treatment status of each market and hence the participants in that market is always the same, the treatment effect is identified using a between-market design. We include period fixed effects and/or pre-determined demographic variables (age, gender, native language, field of study, each measured as the average on the market level) in some specifications (see Appendix B for details reagrding the measurement and coding of control variables). Note that we do not include risk aversion and tax morale in the regressions because these variables are plausibly determined by the treatment. In further regressions, we also consider the median price, P_{50}, and the price at which each item is sold, P, as outcome variables (where, as explained above, the latter has a larger number of observations).

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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). We detect differences between treatment groups with significant precision, which suggests that the number of observations is sufficient in our study.

[29] 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.

4.2 Results

4.2.1 Compliance Rate in Treatment Group

Before presenting our main price and quantity results, we report the compliance rate that we observe in the treatment group with evasion opportunity. We find that 37 out of 40 sellers in the evasion treatment evaded some positive amount of sales at least once and 26 of the 40 sellers fully pursued a strategy of full evasion in the equilibrium reporting periods. As a result the mean compliance rate, defined as reported sales divided by actual sales, is approximately 16% among all sellers in the evasion treatment and 72% among those who report non-zero sales.[30]

4.2.2 Treatment Effect on Prices

Non-parametric results. Figure 3 reports the mean market price by period for the treatment and control groups. The Figure shows clearly that the price in the treatment group is lower than in the control group. We also see that the mean market price varied in both groups in the first 10 to 14 trading periods. This is consistent with the existing literature, which generally finds that double auction markets take approximately 8 to 10 rounds to converge (Ruffle 2005). For this reason, and as is common in the literature, our results focus on data from trading periods 15 to 27 (however, we report results for the full sample as well).

The mean market price in both groups stabilized after round 14 at **54.56 ECU** in the control group and **51.33 ECU** in the treatment group (see panel B of Table 1). This implies that the mean market price in the treatment group is 3.23 ECU lower than in the control group.[31] These differences in prices between the groups are statistically significant

[30]This level of evasion is rather 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 suggests 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).

[31]Note that the estimated treatment effect is even larger for the full sample (55.07 ECU vs 51.18 ECU; see panel A). As shown in Appendix Figure 7 and the second column of Table 1, median prices are also lower in the treatment group than in the control group; the median price is **51.36 ECU** in the treatment

from zero; the exact ranksum test (two-sided) gives a p-value of 0.001 for differences in mean prices (median prices are also significantly different from each other). In other words, we find that markets with access to tax evasion trade at significantly lower prices than markets without access to tax evasion.

Further evidence that tax evasion significantly affects the market price is provided in Figure 5, which reports the cumulative distribution of mean market prices for the treatment and control groups. The Figure shows 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; we reject the null that the distributions are equal (p-value: 0.000).[32]

Regression results. We extend the analysis above by estimating equation (7) for the mean price in each trading period; the corresponding results are presented in Table 2. The estimated treatment effect of -3.23 ECU reported in model 1 of Panel B of Table 2 is statistically different from zero at the 1 percent level.[33] This main estimate of the treatment effect remains statistically significant after correcting for the small number of clusters using the wild-bootstrap-t procedure described in Cameron et al. (2008); see Appendix Table 8.[34] The estimate is robust to the inclusion of period fixed effects (model 2). It becomes smaller (-2.70), but remains economically meaningful and statistically significant, as we add control variables (models 3 and 4). Appendix Table 9 shows that the finding of treatment markets trading at lower markets than control markets is robust to the definition of price (this Table considers the price at which each item is sold in Panel A and the median price in Panel B). Overall, we find robust evidence that markets with evasion opportunities are characterized by lower prices than markets where evasion is not an option.

group and **54.18 ECU** in the control group, resulting in a treatment effect of 2.82 ECU.

[32]This result also holds when we use the median price in each market-period (see Appendix Figure **??**) and individual ask prices (results available upon request).

[33]Panel A of Table 2 reports the results for the full sample, where the treatment effects are even larger than in periods 15-27.

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

4.2.3 Treatment Effect on Quantities

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 (with 16 markets), while the regression analysis is based on the number of units sold in a market-period with standard errors clustered at the market-level (16 clusters).

Non-parametric results. The results in Table 1 show that the mean number of units sold per period in the control group is **5.89**. On the other hand, the treatment group sold an average of **6.23** units per period. We thus find a treatment effect of 0.34 units. This difference between units sold in the treatment and control group is not statistically significant, though we see a boarderline p-value of 0.124 (the trend of units sold over all periods in the two groups is displayed in Appendix Figure 8.) 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 648 units while the control group only sold 613 units. Corresponding numbers for periods 1 to 27 are 1370 and 1299 in the treatment and control group, respectively.

Regression results. Considering regression results for periods 15-27 (Panel B of Table 3), we find a stable treatment coefficient between 0.34 and 0.31 (depending on specification) which is not statistically significant. However, we do find a statistically significant effect (at the 10% level) as we consider all periods in our sample (Panel A). Overall, we thus find indications that treatment markets trade more units than control markets, but the effects are somewhat imprecisely measured.

5 Evasion-Induced vs. Exogeneous Reduction of Tax Burden

The results presented in section 4.2 show clearly that markets with sellers who have the opportunity to evade taxes trade at lower prices than markets where tax evasion is not possible. The identified empirical effect speaks to the two opposing predictions that we presented in section 3. Our findings support the prediction that tax evasion opportunities do have an effect on prices (and also quantities). The rationale for this prediction is that tax evasion lowers the effective tax rate facing sellers, which then allows sellers to trade at lower prices in a competitive market. As a result, the industry supply curve shifts by less than in the case without access to evasion.

But a market with lower tax rates would also yield lower prices even in the absence of tax evasion. So, are the results discussed above driven by the lower effective tax burden in the treatment group or is there something special about evasion-induced reduction in the tax burden? We run a tax-credit treatment to disentangle these two mechanisms. The main idea of this tax-credit treatment is the following. We exogenously reduce the tax burden of sellers to the effective tax burden that we saw in our initial evasion treatments. Comparing the tax-credit treatment and the evasion treatment then allows us to explore a situation where all sellers have the same effective tax burden, but some sellers evade taxes to arrive at this tax burden (sellers in the evasion treatment) whereas other sellers face an exogenously determined lower tax burden (sellers in the tax-credit treatment).

5.1 Design and Organization

Design. The tax-credit sessions are identical to the previous control sessions except that the effective tax rate is exogenously lowered to 2.50 ECU, which is the same as the effective tax rate in the initial evasion treatment of the main experiment.[35] As in the

[35]See equation 6 to see how we calculate the effective tax rate, which is based on the compliance rate of 7% that we see in the initial evasion treatments. We use the effective tax rate that emerged in our initial 2013 experimental evasion-treatment sessions; see below for an explanation. The effective tax rate in these initial evasion-treatment sessions is actually 2.56 ECU. However, we opted for 2.50 ECU because it is easier for subjects to mentally calculate while making their sales and purchasing decisions.

previous treatments, the nominal tax rate is set at 10 ECU, but sellers are told that they will receive a tax 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 tax-credit treatment face an effective tax rate that is lower than their nominal tax rate. While the effective tax burden is the same across sellers in the evasion and tax-credit treatments, sellers in the tax-credit treatment do not have to take any actions to arrive at this lower effective tax rate while sellers in the evasion treatments had to take on audit risk and/or moral costs in order to arrive at this lower effective tax rate. The differences in the instructions that subjects read at the beginning of the experiment are highlighted in appendix section F.

Implementation and Sample. We ran eight sessions – that lasted approximately 100 minutes each – of this tax-credit treatment for a total of eight markets and 80 subjects. Three of these eight sessions were conducted in 2015 at the lab of the University of Cologne (where we ran the 2013 sessions of the main experiment) and five sessions in 2021 at the lab of the University of Hamburg (where we ran the 2021 sessions of the main experiment). None of the subjects in the tax-credit treatment had participated in the main experiment. There were 10 subjects (five buyers and five sellers) in each session, and the average pay-off was 24 EUR. Summary statistics for the tax-credit treatment are provided in Appendix Section B.

We exclude the 2021 evasion treatments from all subsequent analyses because the effective tax rate in the 2013 evasion treatment was an input in the 2015 tax-credit treatment and we want the 2015 and 2021 tax-credit treatments to be comparable.[36] The only way to make both sets of tax-credit sessions comparable is to use the same effective tax rate in both. Consequently, the analyses that follow benchmark all eight sessions of the tax-credit experiment (i.e, sessions from 2015 and 2021) against the four initial 2013 sessions of the evasion-treatment sessions.

The equilibrium price in the 2021 evasion treatments is very similar to the price

[36]The 2021 evasion treatment has a different effective tax rate than the 2013 evasion treatment.

in the 2013 evasion treatments: mean price of 51.66 ECU for 2013 sessions vs 51 ECU for 2021 sessions; see Table 4. We interpret this similarity to be very advantageous for at least two reasons. First, our experimental results are robust to the lab sample (Cologne vs Hamburg) and time period during which the experiments were run (2015 vs 2021). Second, we are confident to bundle 2013, 2015 and 2021 sessions of the control and tax-credit treatments in the subsequent analyses.

The similarity between the 2013/2015 results and 2021 results is confirmed as we consider the main experimental outcomes and compare them across the 2013/2015 and 2021 data. As shown in Table 4, the results are very similar for the 'old' and 'new' data.

5.2 Results

Figure 4 depicts the mean market price in all experimental groups over the course of all periods. The Figure shows that the mean market price in the tax-credit condition is lower than in the evasion condition in each period. This is reflected in the mean prices over rounds 15-27 in these two groups: 51.66 ECU in the evasion group and 49.98 ECU in the tax-credit group (see Table 7). This difference of 1.68 ECU is economically meaningful as it is more than one-half of the effective tax rate and larger than 1 standard deviation of mean price in the evasion group. Comparing the cumulative price distributions in the evasion group and the tax-credit group (Figure 6) supports the notion that the prices are different across the two groups. This is further supported by the Kolmogorov-Smirnov test for equality of distribution functions where we reject the null that the distributions are equal (p-value: 0.000).

Table 5 reports regression results in which we benchmark the initial evasion treatments against the tax-credit treatments, with the mean price in each period (Panel A) and the number of units sold in a period (Panel B) as dependent variables (regressions are specified as in Equation 7). The regression coefficients in all four models of Panel A show that the clearing price in the tax-credit markets is lower than in tax evasion markets despite identical effective tax rates. This difference is statistically significant in specification 1 (on the 5% level). The difference remains statistically significant as we add period fixed effects in specification 2. The inclusion of pre-defined control variables somewhat reduces the treatment coefficient and it becomes insignificant.

Panel B shows that the number of units sold (i.e., quantity effects) is different across the two groups. The coefficient of interest is negative (indicating less sales in the evasion treatment) and statistically significant in all four regression specifications. This supports the notion that markets with evasion opportunity have different trading outcomes than tax-credit markets (despite equal effective tax burden).

Overall, we make five observations. First, price levels are different in each market round. Second, the difference in mean prices is economically meaningful. Third, we observe stark difference in price distributions. Fourth, in the regressions, the price effect always has the same sign and is statistically significant in two out of four specifications. Fifth, the difference in number of units sold is statistically significant in all regression specifications. Together, these observations strongly suggest that evasion-induced reductions in tax burdens affect prices differently than exogenous reductions in tax burdens. One possible explanation for this difference is that sellers in the evasion group adjust prices to reflect the audit risk and/or moral costs associated with the evasion-induced reduction in the tax rate. Unfortunately, our data do not allow us to shed more light on the explanation for the difference between the evasion group and the tax-credit group.

6 Implications for Tax Incidence

This section discusses the incidence implications of our experimental findings. Because we discuss incidence both in the context of the main experiment and the tax-credit treatment, the subsequent analyses again exclude the most recent (2021) evasion-treatment sessions to ensure comparability across all experimental groups; see 5 for an explanation. That is, the following empirical results are based on eight control-group markets (run in 2013 and 2021), four evasion-treatment markets (run in 2013), and eight tax-credit-treatment markets (run in 2015 and 2021 with effective tax rate equal to that of the 2013 evasion-treatment markets).

6.1 Estimation of Economic Incidence

We estimate economic incidence of the nominal tax rate and the effective tax rate. The economic incidence of the nominal tax rate is the share of 10 ECU that sellers shift to buyers in the form of higher prices. The economic incidence of the effective tax rate is 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 nominal tax rate in the control group ($r_i = s_i$), and lower than the nominal tax rate in the treatment group ($r_i < s_i$).

Under the simplifying assumption that the supply and demand elasticities are equal in equilibrium (see footnote 20), we derive the standard prediction that the tax rate in the control group is shared equally between sellers and buyers. That is, the incidence of the nominal tax rate, and hence the effective tax rate, is predicted to be 50% in the control group. Though the standard model 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 generally be due to one of the following reasons (which we aim to distinguish in the tax-credit treatment). First, the evasion opportunity decreases the effective tax rate and sellers might perceive it to be easier to shift a lower tax rate onto buyers. Second, it is possible that sellers shift more than their effective tax burden onto buyers by adjusting prices to reflect the audit-risk and/or moral costs associated with evasion. These mechanisms imply that the incidence of the effective tax rate is higher in the treatment group than in the control group. The results from the tax-credit experiment (see Section 5) suggest that evasion costs are indeed relevant.

Our main results indicate that sellers in the evasion-treatment group face a lower tax burden and trade at lower prices. To determine the incidence of the tax in the context of these results, we first have to determine the incidence of the tax in the control group, which requires knowing the market equilibrium in the absence of the tax. Although we did not run a "no-tax" treatment, we are able to derive this "no-tax" equilibrium by relying on theoretical predictions and the empirical evidence of Grosser and Reuben

(2013). As outlined in section 2.4, we expect the no-tax market to produce an equilibrium with 7 units at a price in the range 48 ECU to 52 ECU. This prediction is supported by empirical evidence in Grosser and Reuben (2013); they find a mean market price of 49.04 ECU (standard deviation: 1.3) and 7.03 (sd: 0.36) units in the "no-tax" equilibrium. Using the "no-tax" result as a benchmark, in the following we discuss the economic incidence of the nominal 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).[37]

6.2 Nominal tax rate

Table 6 summarizes the economic incidence of the nominal tax rate in the presence of tax evasion. The equilibrium price in the control group (with tax but no evasion opportunity) is 54.56 ECU, which is approximately 5.42 ECU above the "no-tax" equilibrium of 49.04 ECU. This suggests that the incidence of the nominal tax burden in the control group is approximately shared equally between buyers and sellers since the nominal 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 economic incidence of the nominal tax. The mean market clearing price in the treatment

[37]Using the results from Grosser and Reuben (2013) as a baseline for our incidence analysis is supported by at least three reasons. First, we use the same double auction as they do. Most importantly, the following components are identical: the number of buyers and sellers in each market, length of a trading period, the demand and supply schedules, the number of homogeneous goods to be traded, and the visual appearance of the market place as coded using z-tree. Second, the price they observe in their no-tax treatment is well within the theoretically-predicted price range. Finally, there is very little order effects on trading prices in their within-subjects design. Grosser and Reuben (2013) implement a within-subject design where each subject trades in a market with a tax and a market without tax. The order of tax and no-tax treatments is randomized to control for order effects, and we rely on their no-tax results as a benchmark for our incidence analyses. The mean trading price is 48.37 ECU (sd: 0.99) among subjects who participated in the no-tax treatment before the tax treatment and 49.04 ECU (sd: 1.3) among all no-tax treatments. The small difference between 49.04 ECU and 48.37 ECU indicates that order effects are tiny. This suggests that it is reasonable to use the overall no-tax mean price as a benchmark for our incidence analysis. Note that subjects who played the no-tax treatment first were aware that a second part would follow, but they were not given the instructions until the first part of the experiment (i.e., trading without tax) was completed. This implies that behavior in the no-tax treatment among those who play no-tax first is not confounded by subsequent parts of the experiment. The results for subjects who played the no-tax scenario first are not published but were requested from the authors.

group (with tax and evasion opportunity) is 51.66 ECU. Considering the nominal tax rate of 10 ECU per unit and the no-tax benchmark of 49.04 ECU, this implies that buyers in the treatment group pay 26% (= (51.66 − 49.04)/10) of the *nominal* tax burden, compared to the ≈50% in the case without evasion. In other words, access to evasion reduces the economic incidence of the tax on buyers by about 29 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 nominal tax burden that the buyers paid in the control group. Since buyers paid approximately 5 ECU of the nominal tax of 10 ECU in the control group, the largest expected effect of evasion is a reduction of 5 ECU. Using this baseline, a treatment effect of 2.90 ECU is very large.

6.3 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 nominal tax rate in the control group, we already know that the effective tax rate is approximately 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 1.28 ECU (= 2.56/2) relative to the "no-tax" equilibrium of 49.04 ECU; that is to 50.32. However, this is not what we observe. The price in the treatment group is 51.66 ECU, which suggests that sellers shift the full expected effective tax rate onto buyers; buyers bear 2.62 ECU (= 51.66 − 49.04) even though the effective tax rate is 2.56 ECU. As a result, about 100% (= (51.66 − 49.04)/2.56) of a seller's expected effective tax rate is shifted onto buyers. These results on the economic incidence of the effective tax rate are summarized in the first three rows of Table 7.

Interestingly, the incidence of the effective tax rate implies that the evasion-induced discount offered by sellers is consistent with the parameters of the evasion gamble. In particular, sellers experienced a roughly 50% reduction in their share of the tax and passed on all of this reduction to the buyers. Therefore, although all of the effective tax rate was passed on the buyers, this reflects a discount (relative to the control group) that is approximately equal to the expected savings to the sellers.

What about the incidence implications in the context of our tax-credit treatment? Notice that consumers in the tax-credit treatment pay 0.94 ECU ($= 49.98 - 49.04$) of the nominal tax rate, while those in the evasion treatment pay 2.62 ECU and those in the control group pay 5.52 ECU. This implies that sellers in the tax-credit treatment shifted 37% of their effective tax burden onto buyers. Interestingly, this shifting of the effective tax rate is considerably lower than the full shifting of the effective tax rate that we observe in the evasion treatments – despite the fact that the effective tax rate is the same. This provides additional (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.

7 Conclusion

Summary. We use data generated in a controlled laboratory experiment to identify the effect of tax evasion among sellers on consumption prices and traded quantities. We find strong evidence that tax-evasion opportunities cause lower prices and higher numbers of traded units. Our findings are consistent with the simple rationale that the evasion opportunity allows firms to reduce their tax liability and therefore offer goods at lower prices. Our findings further show that sellers increase their after-tax profits through the evasion opportunity. This implies that the revenue gains of increasing the number of units sold combined with under-reporting the tax base compensates for the revenue loss of selling at lower prices in the treatments with access to evasion. Not surprisingly, buyers also have higher net incomes in the presence of tax evasion. Overall, the increase in after-tax incomes through the evasion opportunity is higher for sellers than for buyers (calculation details in Appendix Section D).

Our findings further reveal interesting results about the incidence of taxes with and without evasion possibilities. In particular, relative to the baseline case where buyers face ≈50% of the nominal tax burden, buyers in the treatment group only face approximately 26% of the nominal tax burden. Although buyers pay lower prices than they otherwise would, we find that sellers fully shift the expected effective tax onto buyers. An additional treatment 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 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 or moral costs of evasion and therefore trade at higher prices.

Implications. These results potentially have implications for the effects of recent policies aiming at the reduction of tax evasion and tax avoidance.[38] In particular, our findings suggest that such policies can increase prices and lower sold quantities.

Therefore, our results are relevant in countries such as the United States where the Supreme Court's ruling in *South Dakota v Wayfair* has changed the way out-of-state merchants are treated with respect to retail sales tax collection.[39] In particular, a number of states have updated their sales tax laws to follow South Dakota's lead in requiring out-of-state firms to serve as tax collectors, thus changing the tax evasion opportunities that previously existed with the *Use-Tax*. There have also been a push to restrict the sale of "zappers", which are used to evade sales taxes among firms. Our findings suggest that, *all else equal*, 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

[38]While we focus on tax evasion, note that our results also have implications for tax avoidance because the underlying rationale for our findings potentially also applies to avoidance: just as evasion, avoidance allows firms to reduce their tax base and therefore gives scope to trade at lower prices.

[39]We argue that our experimental findings carry some external validity; see Appendix C for a detailed discussion of external validity in the context of our experiment.

(FATCA), are likely to affect the level of economic activity as affected parties respond to the reduced evasion opportunities. The general rationale behind our results also applies to recent measures to reduce avoidance activities of firms including the Global minimum corporate tax, OECD Base Erosion and Profit Shifting (BEPS), and Country by Country Reporting, (CbCR). As with evasion, tax avoidance possibilities allow firms to reduce their tax liability and therefore potentially offer goods at lower prices. In this regard, our results suggest that anti-avoidance policies potentially increase prices and reduce traded quantities.

While we show that tax-evasion opportunities affect prices and quantities, we acknowledge that it is not clear that the magnitude of the effects is the same across all types of taxes and/or 100% comparable to cases with avoidance possibilities. Conditional on the ease with which taxes can be evaded, it is also possible that the evasion mechanism matters. For example, Tran and Nguyen (2014) show that Vietnamese firms evade VAT by artificially increasing their sales and material costs, which is facilitated by colluding with other producers in the supply chain. The presence of collusion as a means of evasion suggests lower competitive pressure, which may lead to different incidence outcomes under a VAT compared to retail sales taxes where collusion among firms is not necessary for evasion. Given recent calls for the adoption of VAT in 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 prices and sold quantities.

Avenues for Future Research. We identify several further avenues for future research that emerge from our findings. The (tax accounting) literature studies the relationship between risk/uncertainty and tax planning decisions (e.g. Kim et al. 2011; Rego and Wilson 2012; Guenther et al. 2017; Dyreng et al. 2019). Relating to this literature, one interesting question is if our results would be similar in an institutional set-up in which there is uncertainty about the audit process and in which the desire for risk compensation is thus complicated. Another valuable follow-up study could investigate if the effect of evasion opportunities on prices and quantities is conditional on buyers' knowledge of a seller's actual evasion behavior. Relating to the economics literature on the tax liability-side equivalence, it could also be interesting to explore if the price effects of our set up are similar in situations in which buyers (rather than sellers) are responsible for remitting the tax to the authority. Finally, it would be fruitful to check whether the treatment effects we estimate depend on the type of market structure and the degree of competition.

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Tables and Figures

Tables

Table 1: Prices and Quantities by Treatment Group

	Average Price		Median Price		Sales	
	Control	Treatment	Control	Treatment	Control	Treatment
			Panel A: All Periods			
Mean	55.07	51.18	54.67	51.17	6.01	6.34
	(2.087)	(1.826)	(2.079)	(1.973)	(0.598)	(0.565)
			Panel B: Period>14			
Mean	54.56	51.33	54.18	51.36	5.89	6.23
	(1.610)	(1.875)	(1.480)	(1.836)	(0.606)	(0.526)
P-value		0.001		0.002		0.124
N	8	8	8	8	8	8

Notes: Reported is the mean of $\overline{P}$ and P_{50} by treatment group (see definitions in the first paragraph of section 4.1). Standard deviations in parentheses. Units sold is the market-level mean of units sold in a given market period. *Treatment* indicates participants with an evasion opportunity and *Control* indicates subjects without evasion opportunity. All numbers and statistics are based on 16 independent market-level observations (8 control, 8 treatment). Panel A uses all completed contracts from periods 1 to 27 and panel B uses all completed contracts in periods 15 to 27. P-value is for the exact Wilcoxon ranksum test based on 16 independent market-level observations; null hypothesis is that there is no difference between treatment and control group.

Table 2: Impact of treatment on mean market price

	Model 1	Model 2	Model 3	Model 4
	Panel A: All Periods			
Treatment	-3.882***	-3.882***	-3.528***	-3.528***
	(0.766)	(0.790)	(0.866)	(0.893)
Constant	55.066***	55.231***	58.760***	58.925***
	(0.521)	(0.844)	(6.619)	(7.401)
r2	0.496	0.504	0.518	0.526
N	432	432	432	432
	Panel B: Periods>14			
Treatment	-3.228***	-3.228***	-2.701***	-2.701***
	(0.829)	(0.854)	(0.801)	(0.826)
Constant	54.556***	54.606***	63.267***	63.317***
	(0.507)	(0.484)	(7.790)	(8.105)
r2	0.463	0.466	0.525	0.529
N	208	208	208	208
Cluster	16	16	16	16
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 mean market price in a given market period. *Treatment* indicates participants with an evasion opportunity. Panel A uses periods 1 to 27, panel B uses periods 15 to 27. The sample includes eight independent markets in the control group and eight independent markets in the evasion group (for a total of 16 markets/clusters). Period FE is period fixed effects. Control variables include the average age, share of males, share of native German speakers, and share of subjects whose field of study is economics. These averages and shares are calculated at the market-level.

Table 3: Impact of treatment on units sold

	Model 1	Model 2	Model 3	Model 4
	Panel A: All Periods			
Treatment	0.329*	0.329*	0.304*	0.304*
	(0.165)	(0.170)	(0.162)	(0.167)
Constant	6.014***	6.398***	5.433***	5.817***
	(0.116)	(0.145)	(1.329)	(1.439)
r2	0.074	0.168	0.142	0.236
N	432	432	432	432
	Panel B: Periods>14			
Treatment	0.337	0.337	0.309	0.309
	(0.203)	(0.209)	(0.195)	(0.201)
Constant	5.894***	5.894***	5.339***	5.339***
	(0.157)	(0.196)	(1.669)	(1.698)
r2	0.082	0.128	0.180	0.227
N	208	208	208	208
Cluster	16	16	16	16
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. *Treatment* indicates participants with an evasion opportunity. Panel A uses all completed contracts from periods 1 to 27, panel B uses all completed contracts in periods 15 to 27. The sample includes eight independent markets in the control group and eight independent markets in the evasion group (for a total of 16 markets/clusters). “Period FE” is period fixed effects. Control variables include the average age, share of males, share of native German speakers, and share of subjects whose field of study is economics. These averages and shares are calculated at the market-level.

Table 4: Comparison of 'Old' and 'New' Data: Prices and Quantities by Treatment Group

Mean Price			Median Price			Sales		
Control	Treat	Tax Credit	Control	Treat	Tax Credit	Control	Treat	Tax Credit
Panel A: Data collected in 2013/2015								
54.36	51.66	50.09	53.78	51.69	50.26	5.92	6.46	6.87
(1.208)	(1.243)	(1.795)	(0.555)	(1.265)	(1.712)	(0.479)	(0.503)	(0.339)
Panel B: Data collected in 2021								
54.75	51.00	49.91	54.58	51.03	49.86	5.87	6.00	6.75
(1.922)	(2.309)	(1.088)	(1.946)	(2.233)	(1.220)	(0.715)	(0.443)	(0.434)

Notes: This table compares experimental outcomes across experimental data collected in the years 2013 and 2015 ('old' data) vs experimental data collected in 2021 ('new' data). In 2013, we collected data for four control and four evasion markets. In 2015, we collected data for three tax-credit markets. In 2021, we collected data for four control markets, four treatment markets and five tax-credit markets. The 2013 and 2015 sessions were conducted at the University of Cologne. The 2021 sessions were conducted at University of Hamburg. Reported is the mean of $\overline{P}$, P_{50}, and the number of units sold – see definition in the first paragraph of section 4.1) – separately for the 'old' and 'new' data. All calculations restricted to periods 15-27. Both the new and old data has 52 session-periods for control and treat. There are 39 session-periods in the old tax credit data and 65 session-periods in the new tax credit data. *Treat* indicates participants with an evasion opportunity, *Control* indicates subjects without evasion opportunity, and *Tax Credit* indicates participants without evasion opportunity but with tax credit. Standard deviations in parentheses.

Table 5: Impact of main evasion treatment relative to additional tax credit treatment

	Model 1	Model 2	Model 3	Model 4
	Panel A: Mean Price			
Evasion Treat	1.684**	1.684*	1.037	1.037
	(0.760)	(0.791)	(0.726)	(0.757)
Constant	49.977***	49.806***	78.913***	78.742***
	(0.501)	(0.549)	(10.282)	(10.792)
	Panel B: Total Sales			
Evasion Treat	-0.337*	-0.337*	-0.355**	-0.355**
	(0.153)	(0.160)	(0.119)	(0.124)
Constant	6.798***	6.612***	4.907**	4.721**
	(0.057)	(0.138)	(1.880)	(1.909)
N	156	156	156	156
Cluster	12	12	12	12
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%. Regression results for the effect of the *evasion treatment* (where sellers have evasion opportunity) relative to the tax-credit treatment (where sellers face the same effective tax rate as in the evasion treatment, but do not have to evade to arrive there). To have identical effective tax rates in the two groups of interest, the regressions only include the four initial evasion-treatment sessions (see Section 5 for further explanation). The sample thus includes four independent markets in the evasion group and eight independent markets in the tax-credit group (for a total of 12 markets/clusters). Estimates are based on equation (7) with the dependent variable defined as mean market price in a given market period in panel A, and number of units sold in a given market period in Panel B. All panels use completed contracts from periods 15 to 27. Period FE is period fixed effects. Control variables include the average age, share of males, share of native German speakers, and share of subjects whose field of study is economics. These averages and shares are calculated at the market-level.

Table 6: Overview of Results and Economic Incidence of Per-unit Tax

Condition	Price	Units	Incidence Nominal Tax
No-Tax	49.04	7.03	–
Control	54.56	5.89	55.16%
Treatment	51.66	6.46	26.21%
Treat Effect	-2.90	0.57	-28.95

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. The sample includes eight independent markets in the control group and four independent markets in the evasion group. Reported are the mean prices and number of units traded. "Incidence Nominal Tax" is the share of the nominal 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.

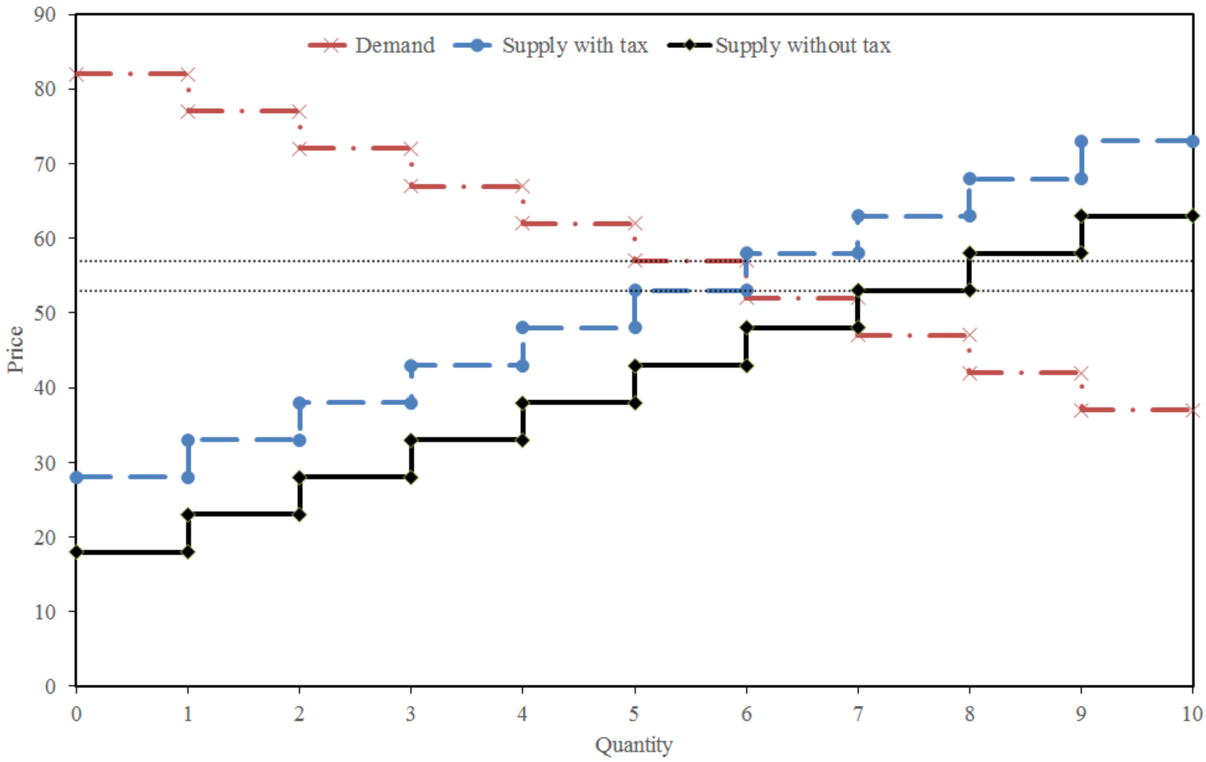
Table 7: Additional Tax Credit Treatment and Incidence of Effective Tax Rate

Condition	Price	Units	Incidence Effective Tax
No-Tax	49.04	7.03	–
Control	54.56	5.89	55.16%
Treatment	51.66	6.46	104.85%
Tax Credit	49.98	6.80	37.48%

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 treatment without evasion opportunity and a tax credit of 7.5 ECU. The sample includes eight independent markets in the control group, four independent markets in the evasion group and eight independent markets in the tax-credit group. Reported are the mean prices and number of units traded. "Incidence Effective Tax" 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.

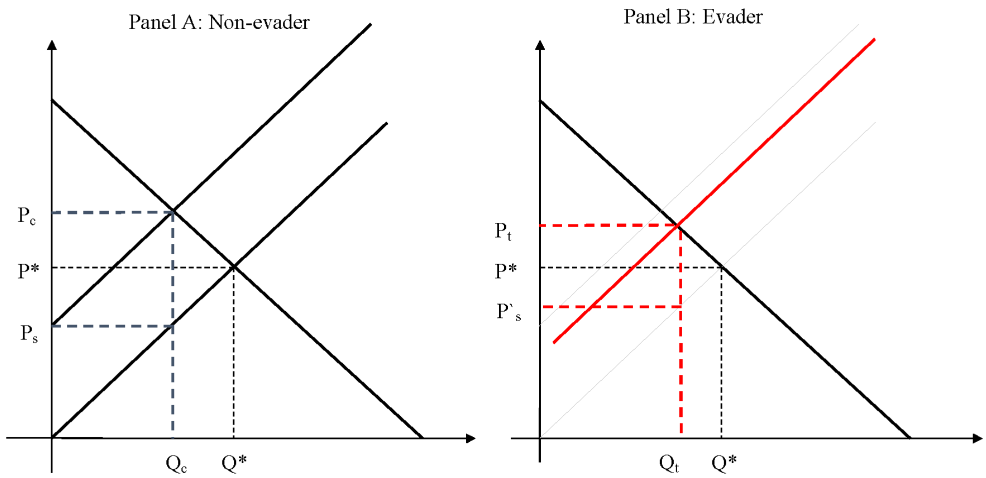
Figures

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 $P_c^{'} < P_c$.

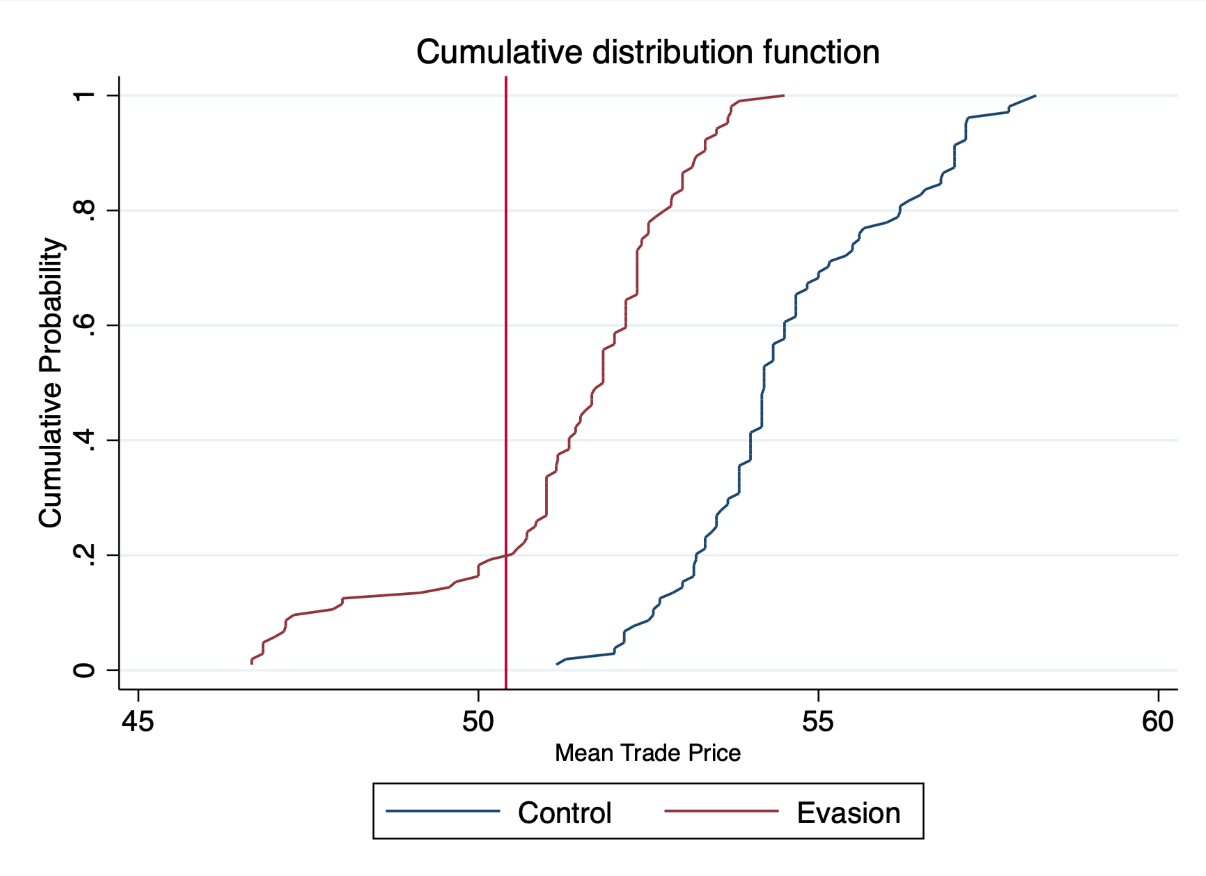
Figure 3: Mean market price by period and treatment

Notes: Reported is the mean market price $\overline{P}$ in each period separately for the treatment (with evasion opportunity) and control group (no evasion opportunity). The sample includes eight independent markets in the control group and eight independent markets in the evasion group.

Figure 4: Tax credit treatment: Mean market price by period and treatment

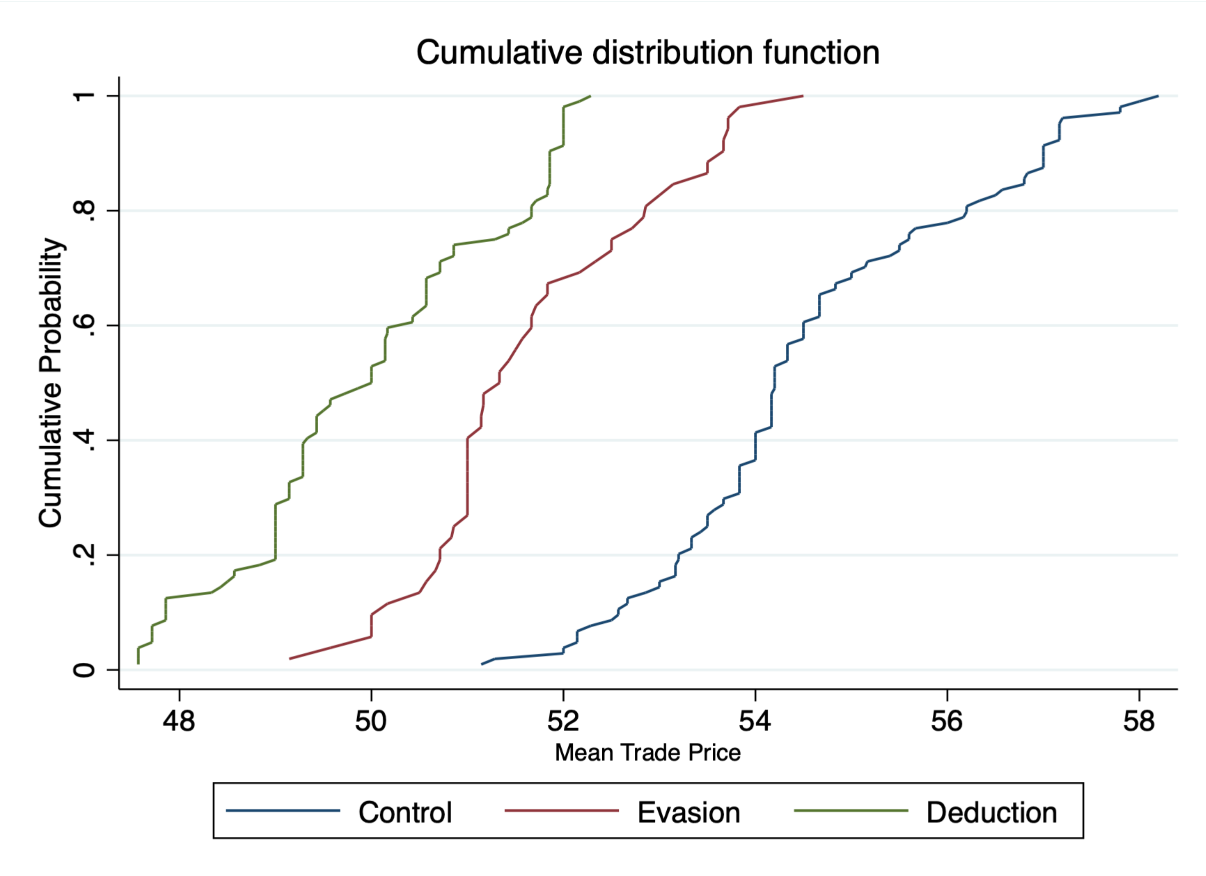
Notes: Reported is the average market price $\overline{P}$ in each period for the treatment group (with evasion opportunity), control group (no evasion opportunity), and the additional tax-credit group (no evasion opportunity, but with tax credit). The sample includes eight independent markets in the control group, four independent markets in the evasion group, and eight independent markets in the tax-credit group.

Figure 5: Cumulative distribution of mean market price by treatment



Notes: Reported is the cumulative distribution of average market price $\overline{P}$ for the treatment group (with evasion opportunity) and control group (no evasion opportunity). Distributions are based on data from market periods 15 to 27. The sample includes eight independent markets in the control group and eight independent markets in the evasion group. 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 6: Tax credit treatment: Cumulative distribution of mean market price by treatment



Notes: Reported is the cumulative distribution of average market price $\overline{P}$ for the treatment group (with evasion opportunity), control group (no evasion opportunity), and tax-credit group (no evasion opportunity, but with tax credit). Distributions are based on data from market periods 15 to 27. The sample includes eight independent markets in the control group, four independent markets in the evasion group, and eight independent markets in the tax-credit group. Two-sample Kolmogorov-Smirnov test for equality of distribution functions reports a maximum difference in distributions between the evasion and tax-credit group of 0.770 with pvalue of 0.000. This implies that the null hypothesis that the distributions are equal is rejected.

figuresection tablesection

Online Appendix

A Additional Results

Table 8: Impact of treatment on market price and sales: adjustment for small number of clusters

	Model 1	Model 2	Model 3	Model 4
Mean Price	-3.228***	-3.228***	-2.701**	-2.701**
	(1.031)	(1.030)	(1.090)	(1.089)
Units Sold	0.337	0.337	0.309	0.309
	(0.220)	(0.220)	(0.242)	(0.242)
N	208	208	208	208
Cluster	16	16	16	16
Control variables	No	No	Yes	Yes
Period FE	No	Yes	No	Yes

Notes: Treatment Effects for outcome variables mean price in a given market period and units sold in a market period. Standard errors in parentheses are adjusted for clustering at the market level and corrected for the small number of clusters using the wild-bootstrap-t procedure described in Cameron et al. (2008). *Treatment* indicates participants with evasion opportunity. The sample includes eight independent markets in the control group and eight independent markets in the evasion group (for a total of 16 markets/clusters). The correction is implemented using Stata code provided by Judson Caskey and is available here: `https://sites.google.com/site/judsoncaskey/data`. * significant at 10%; ** significant at 5%; *** significant at 1%. All columns use completed contracts from periods 15 to 27. Number of observations is 208 (=16 markets $\times$ 13 periods. Control variables include the average age, share of males, share of native German speakers, and share of subjects whose field of study is economics. These averages and shares are calculated at the market-level.

Table 9: Impact of treatment on ask price and median market price

	Model 1	Model 2	Model 3	Model 4
Panel A: Ask price of each sold item				
Treatment	-3.180***	-3.178***	-2.611***	-2.609***
	(0.830)	(0.835)	(0.806)	(0.809)
Constant	54.473***	54.524***	63.879***	63.947***
	(0.511)	(0.460)	(7.566)	(7.668)
R2	0.244	0.246	0.284	0.286
N	1261	1261	1261	1261
Panel B: Median Price				
Treatment	-2.817***	-2.817***	-2.073**	-2.073**
	(0.802)	(0.827)	(0.738)	(0.761)
Constant	54.178***	54.252***	64.898***	64.972***
	(0.469)	(0.429)	(6.407)	(6.669)
R2	0.419	0.423	0.548	0.552
N	208	208	208	208
Cluster	16	16	16	16
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 market price for each good sold in panel A and median price in a given market period in Panel B. *Treatment* indicates participants with an evasion opportunity. All panels use completed contracts from periods 15 to 27. The sample includes eight independent markets in the control group and eight independent markets in the evasion group (for a total of 16 markets/clusters). Period FE is period fixed effects. Control variables include the average age, share of males, share of native German speakers, and share of subjects whose field of study is economics. These averages and shares are calculated at the market-level.

Table 10: Impact of treatment on price – incl. control variables

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
	Ask Price		Mean Ask Price		Median Ask Price	
Treatment	-2.611***	-2.609***	-2.701***	-2.701***	-2.073**	-2.073**
	(0.806)	(0.809)	(0.801)	(0.826)	(0.738)	(0.761)
Age	-0.316	-0.316	-0.284	-0.284	-0.382	-0.382
	(0.267)	(0.268)	(0.276)	(0.284)	(0.237)	(0.245)
Gender	-2.820	-2.824	-2.752	-2.752	-3.158	-3.158
	(2.659)	(2.669)	(2.818)	(2.906)	(2.404)	(2.479)
German	-0.808	-0.813	-0.935	-0.935	0.458	0.458
	(3.545)	(3.563)	(3.683)	(3.798)	(3.270)	(3.372)
Study Field	0.779	0.780	0.642	0.642	-0.186	-0.186
	(1.989)	(1.999)	(2.057)	(2.121)	(1.877)	(1.935)
Constant	63.879***	63.947***	63.267***	63.317***	64.898***	64.972***
	(7.566)	(7.668)	(7.790)	(8.105)	(6.407)	(6.669)
N	1261	1261	208	208	208	208
Cluster	16	16	16	16	16	16
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. *Treatment* indicates participants with an evasion opportunity. All panels use completed contracts from periods 15 to 27. The sample includes eight independent markets in the control group and eight independent markets in the evasion group (for a total of 16 markets/clusters). Period FE is period fixed effects. Gender is the share of males, German is the share of native German speakers, Field of study is the share of subjects whose field of study is economics or business administration. These variables are shares are calculated at the market-level. Age is the average age in a market. See Appendix Section B for details regarding the measurement and coding of control variables.

Table 11: Impact of treatment on units sold – incl. control variables

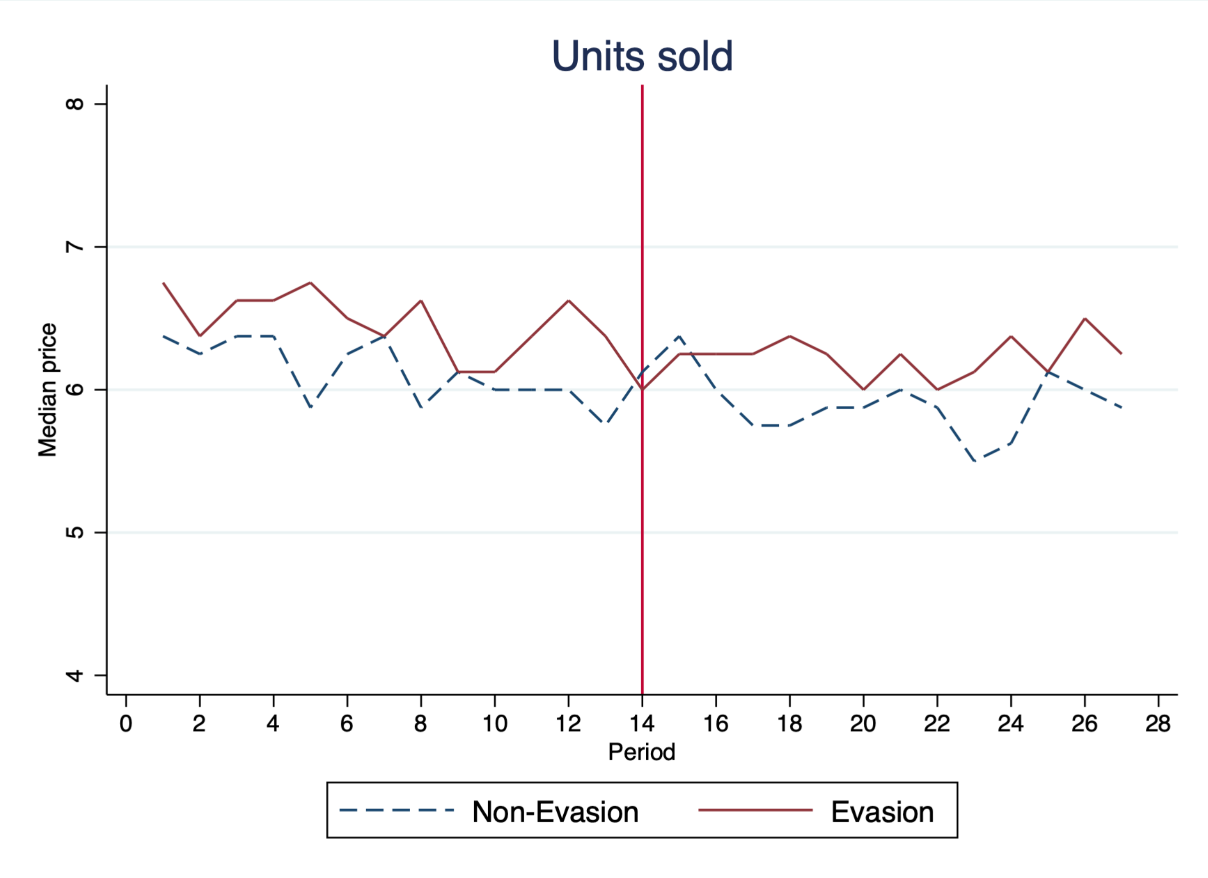
	Model 1	Model 2
Treatment	0.309	0.309
	(0.195)	(0.201)
Age	-0.024	-0.024
	(0.053)	(0.054)
Gender	0.197	0.197
	(0.566)	(0.584)
Native Speaker	1.259	1.259
	(1.160)	(1.196)
Study Field	0.834	0.834
	(0.512)	(0.527)
Constant	5.339***	5.589***
	(1.669)	(1.780)
N	208	208
Cluster	16	16
Period FE	No	Yes

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. *Treatment* indicates participants with an evasion opportunity. All panels use completed contracts from periods 15 to 27. The sample includes eight independent markets in the control group and eight independent markets in the evasion group (for a total of 16 markets/clusters). Period FE is period fixed effects. Gender is the share of males, German is the share of native German speakers, Field of study is the share of subjects whose field of study is economics or business administration. These variables are shares are calculated at the market-level. Age is the average age in a market. See Appendix Section B for details regarding the measurement and coding of control variables.

Figure 7: Median market price by period and treatment

Notes: Reported is the median market price P_{50} in each period for the treatment (with evasion opportunity) and control group (without evasion opportunity). The sample includes eight independent markets in the control group and eight independent markets in the evasion group.

Figure 8: Units sold by period and treatment



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

B Summary Statistics

After the experiment, subjects reported their age, gender, native language, level of tax morale, risk preference, and field of study. Tax morale is determined using a question very similar to one used in the World Values Survey (Inglehart nd): "How justified do you think it is to evade taxes if there was an opportunity to do so?". Subjects could reply on a 10-point scale ranging from '0 Always Justified' to '10 Not At All justified'. We generate a dummy variable indicating high tax morale that has value 1 if a subject reported that it is never justified to evade taxes.[40] Risk aversion is measured with a question that gives subjects the choice between a certain payment and a gamble whose expected payoff is the same as the certain payment.[41] Each of these variables is summarized in Table 12. Non-parametric Wilcoxon rank-sum tests for differences in distributions between groups shed light on whether randomization was successful. We do not observe any statistically significant differences in age, share of participants whose native language is German, tax morale, field of study and risk aversion across the two groups.

We find a few statistically significant differences between groups: in the share of male participants between the treatment and control group (38% vs 53%) and in the mean age between control and tax-credit group (25.59 vs 26.23 years). We include control variables (incl. gender and age) in some treatment-effect regressions to rule out that our treatment effects are driven by observable characteristics.

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

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

Table 12: Summary statistics of Demographic Variables

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

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

C Discussion of External Validity

To which extent can our results be generalized to the 'real world'? As with almost all economic laboratory experiments, there remains doubt about the external validity of our results.[42] One general concern is that the setting in the lab is abstract and artificial. However, the literature shows that laboratory double auctions, which we use in our experiment, generate very plausible equilibria (e.g., Smith 1962; Holt 1995; Dufwenberg et al. 2005; Grosser and Reuben 2013). This suggests that our experimental setting is appropriate to study prices and quantities as outcome variables. In addition, although subjects trade in fictitious goods, they receive actual money pay-offs and thus face incentives similar to buyers and sellers in actual markets. Furthermore, the question of tax incidence (without tax evasion) has been widely studied in the laboratory setting (e.g., Riedl and Tyran 2005; Ruffle 2005; Cox et al. 2018; Grosser and Reuben 2013) and shown to lead to results that reflect theoretical predictions very well.

In order to make the tax evasion decision as realistic as possible we used actual tax terminology and announced to the participants that all tax revenue would be donated to the German Red Cross, a non-ideological charity organization that is usually perceived as reliable and transparent. That is, we made clear to participants that the revenue from the laboratory tax does not simply flow back to our research budget. This design choice thus contributes to mimicking the real-world situation where tax revenues are spent for a purpose and are not just wasted.[43]

Additionally, although evasion may occur among buyers as well, the real-world problem seems to be more relevant among sellers; sellers are usually responsible for remitting sales taxes to the government. In this sense, our laboratory setting mimics the operation of most transaction taxes in the real world in that we also have a set-up in

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

[43]Tax morale research (Torgler 2007) finds that taxpayers are more likely to comply with tax laws if they believe that the tax revenue is spent transparently. Eckel and Grossman (1996) show that dictators share more in dictator games if the recipient is the American Red Cross. Overall, we donated EUR 714 to the Red Cross (including all treatments).

which sellers remit the tax.[44] We acknowledge that prices and quantities on real-world markets, such as the retail commerce market, are not determined in a competitive double auction setting with full information of all actors. However, many real-world markets are considerably close to competitive markets and are characterized by a situation where both sellers and buyers have full information about prices (especially now that prices are very transparent online and easy to compare) and where these prices are determined in the interplay between supply and demand.

A further concern of generalizability relates to the costs of evasion in our empirical design. While our audit rate of 10% seems low, there is evidence of "real-world" tax systems with significantly lower audit rates. For example, a news article revealed that the tax agency in the state of Mississippi "audited just 2 percent of businesses operating in the state [in fiscal year 2012]."[45] While this does not necessarily imply that each firm faced an audit rate of 2%, it does suggest that our audit rate of 10% is not unreasonable. One might also be concerned that our design uses an exogenously determined audit whereas audit probabilities tend to be endogenous in the real world. But here too, we wish to note that exogenous audits are not uncommon. For example, tax gap estimates in the US are based on data from random audits. More importantly, we argue that the qualitative result we observe with random audit should carry through with endogenous audits. In particular, the main point is that market equilibrium is affected by access to evasion and this has implications for tax incidence and the distribution of tax burdens. It's possible that the magnitude of these effect might differ between exogenous and endogenous audit regimes. But the main point remains; we should expect different market outcomes when evasion is at play.

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

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

D Treatment Effects on After-Tax Income

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

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

What about net-income effects in the additional tax-credit treatment? The average net incomes of both sellers and buyers increase in the additional treatment relative to the control group; the increase amounts to 79 ECU for buyers and 66 ECU for sellers, both relative to the control group without evasion opportunities. That is, for buyers the positive effect of the deduction is larger than the positive effect of the evasion opportunity. This is consistent with the observation that the equilibrium price in the tax-credit treatment is lower than in the evasion treatment. In contrast, because sellers in the tax-credit treatment face the same tax rate as in the evasion treatment, but receive a lower price, the positive effect of the deduction on net incomes of sellers is lower than the positive effect of the evasion opportunity.

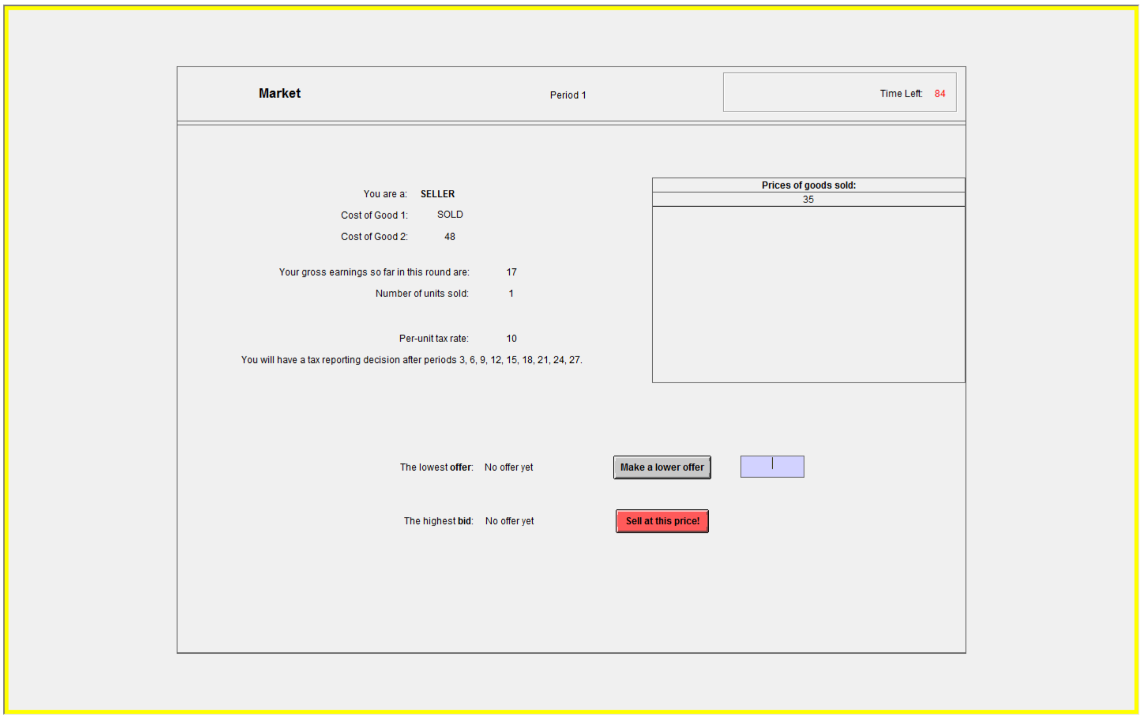
E Additional Information on Experimental Design

Table 13: Demand and Supply Schedules

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

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

Figure 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).

F 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.

<u>Overview</u>

[***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.

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[***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

Screenshots from trading market

Sellers:

Here Screenshot Sellers

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

Buyers:

Here Screenshot Buyers

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

[*Added in the* ***treatment group with evasion opportunity****:*

The Reporting Decision for Sellers

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

Sellers will then be asked to report the number of units sold in the previous three rounds for tax purposes. The reported amount may be between zero and the total number of units that were actually sold over the previous three rounds. After the reporting decision is submitted by pressing the "OK" button, the computer will determine if it is checked

whether the reported number equals the actual number of units sold over the last three periods. The computer makes this call by randomly selecting an integer number between 1 and 10. The reporting decision will **only** be checked if the computer selects the number 1. Therefore, there is a random chance of 10% that the reporting decision will be checked.
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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:

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

2. Computer selects number 1:

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

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

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[*Net income information in the* ***additional treatment 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.