

How Do Tax Evasion Opportunities Affect Prices?

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Abstract

We investigate the causal link between tax evasion opportunities and market prices using two controlled experiments where buyers and sellers trade a fictitious good in competitive markets and a per-unit tax is imposed on sellers. In the first experiment, sellers in one experimental group are provided the opportunity to evade the tax whereas sellers in the control group are not. We find that markets with evasion opportunities have lower equilibrium prices. The rationale for this effect is that firms with evasion opportunities face a lower effective tax burden compared to firms without evasion opportunities. We hold the effective tax burden constant in the second experiment and find that markets with evasion opportunities trade at higher prices than markets without evasion opportunities. 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 document the prevalence of tax evasion among firms and self-employed individuals (e.g., Slemrod 2007; Kleven et al. 2011; Mikesell 2014; Fox et al. 2014; De Simone et al. 2020).¹ Because tax evasion reduces revenues and thus negatively impacts budgets, many governments have been investing resources into evasion-reducing policies. For example, the EU council intends to reinforce the capacity of EU member states to fight against e-commerce VAT fraud by launching a Central Electronic System of Payment information (CESOP). CESOP will keep records of cross-border payments within the EU and between the EU and Non-EU countries beginning in 2024. The idea is that this system will allow tax authorities to monitor and enforce the VAT on cross-border business-to-consumer supplies of goods and services (EU Commission 2023). Similarly, state governments in the US pushed for changes to the definition of nexus in an attempt to reduce non-compliance with the *use tax*.² While such evasion curbing policies are expected to increase tax revenues, they could also affect market prices and thus change the distribution of tax burdens with important implications for the equity profile of the affected tax systems.

In this paper, we pursue two objectives related to the impact of tax evasion on market prices. The first objective is to test whether prices are different in markets where firms have the option to evade (sales) taxes relative to markets in which firms cannot evade. Firms that evade taxes face a lower effective tax burden compared to firms that do not evade taxes. Any observed differences in prices between markets with and without evasion opportunities may therefore be driven by the lower effective tax burden. Consequently, the second objective of our paper is to test if evasion opportunities affect market prices when the effective tax burden is held constant. Specifically, we compare

¹There 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.

²Consumers 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.

market prices across markets that face the same effective tax burden, but where firms in some markets arrive at the lower effective tax burden through tax evasion, whereas firms in other markets face an exogenous reduction in the tax burden. This second objective sheds light on the question of whether an evasion-induced reduction in the tax burden has a differential effect than an exogenous tax burden reduction.

We address these research objectives using data generated in two controlled laboratory experiments where participants trade fictitious goods in a competitive double auction market (Smith 1962, Dufwenberg et al. 2005). Experimental participants in both experiments are randomly assigned roles as sellers or buyers and a per-unit sales tax is imposed on all sellers. In the first experiment, which addresses our first research objective, sellers in the evasion group make a tax reporting decision and are therefore able to underreport the number of units sold, whereas sellers in the control group have their correct tax liability deducted automatically. Evasion costs, including audit probability and fine rate, are exogenous. Because the only difference between the evasion and control groups is access to evasion, we attribute any price differences between the two groups to the evasion opportunity.

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

Building on the result of the first experiment, we conduct a second experiment to study the price effects of evasion opportunities in situations with constant effective tax burden. This experiment, which addresses our second research objective, features a condition that is analogous to the control group in the first experiment, except that the effective tax burden is exogenously lowered to match the expected effective tax burden observed in the evasion group of the first experiment (through an automatic tax credit).

Benchmarking the price in this tax credit group against the price in the evasion group of the first experiment identifies the price effect of evasion opportunities across situations with constant tax burden. We find that the market price in the evasion group is higher than the market price in the tax credit group. Although this difference is not always precisely measured, it indicates that access to evasion yields smaller price reductions than equivalent exogenous reductions in tax burdens. This finding is important, because it suggests that holding effective tax rates constant, tax evasion actually increases market prices. The results further suggest that tax evasion imposes a double burden on society: foregone tax revenues as well as higher market prices.

An important question (which 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 way of lowering effective tax rates. For example, evasion potentially comes with moral costs and risk averse taxpayers might wish to be compensated for the risk involved in evasion.

We use the price effects that we find to investigate the economic incidence of the sales tax and document the following findings. 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.³

Contribution to the Literature. Our paper contributes to different strands of literature. First, our paper adds to the literature studying tax evasion.⁴ Unlike most of the tax evasion literature, we investigate the implications of tax evasion for an outcome variable rather than investigating the determinants of tax evasion.⁵ We contribute two

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

⁴Andreoni et al. (1998), Alm (2012), Slemrod (2017) and Slemrod (2019) provide general surveys on tax compliance research.

⁵In a related recent paper, Kotakorpi et al. (2021) study the effect of different tax-reporting institutions on pricing decisions.

findings to the evasion literature. First, access to tax evasion opportunities leads to lower market prices (presumably through a lower tax burden). Second, holding the effective tax burden fixed, markets with evasion opportunity actually trade at higher prices compared to markets without evasion (presumably because of risk and moral costs of evasion).

Our paper relates to 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. The paper does not study if e-tailers set different prices than traditional retailers. Additionally, our results suggest that enforcement efforts to reduce tax evasion opportunities impact consumption prices, 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, we relate to literature that explores the effect of taxes on prices (outside the evasion context) using archival data. Consistent with the findings of our first experiment, the literature finds that taxes affect prices (Benzarti et al. 2020; Jacob et al. 2022; Dedola et al. 2022; Baker et al. 2023). We complement this literature in that we show that evasion-induced reductions in the tax burden also reduce prices.

Third, 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.⁶ Similar to tax

⁶The 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 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.

evasion, tax avoidance possibilities and the resulting reduction in the tax burden might give firms scope to sell their goods at lower prices. 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 business 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 potentially has real consequences, because the muting effects of avoidance are reduced.

We are aware of one study that sheds light on the relationship between tax avoidance opportunities and prices. Jacob et al. (2022) 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.⁷ Second, an obvious difference is that Jacob et al. (2022) 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, using our second experiment, we examine whether the evasion opportunity effect is explained by a reduced effective tax burden.

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

⁷One potential threat to identification in the relevant empirical test in Jacob et al. (2022) 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.

where causality runs in the opposite direction and using a different empirical approach. More generally, we relate to a stream of papers that examines the consequences of tax avoidance – though not for prices (see Jacob 2022 for an overview).⁸

Fourth, we relate to 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) use a CGE model in this context and Kopczuk et al. (2016) use archival data to study incidence across situations in which diesel taxes are imposed at different production stages. Since we 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 opportunities on the economic incidence of the tax than these two studies.

2 Conceptual Framework: Potential Effects of Evasion Opportunity on Prices

This section provides a conceptual framework of the relationship between evasion opportunities and market prices. Note that we deliberately derive predictions for the situation where the evasion opportunity effect may run through a reduction of the tax burden. The case with constant tax burdens is discussed in Section 4. 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.

2.1 Evasion Opportunity Affects Prices

For simplicity, let's assume that demand and supply curves are linear. Figure 1 illustrates the effect of taxes on price for the cases with and without evasion. First, consider Panel

⁸For 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).

A, a situation where evasion is not possible. As in the standard textbook case, the supply curve shifts up by the full amount of the nominal tax rate. This results in a new market equilibrium (p_c, q_c) , where subscript c indicates the control group in our experiment where evasion is not possible (more on this below).

Sellers with an opportunity to evade taxes can hide a fraction of their sales. A seller who underreports sales and is not audited faces an effective tax rate that is lower than the nominal tax rate faced by sellers without an evasion opportunity. 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.⁹ As illustrated in Panel B of Figure 1, this then implies that the market supply curve in the presence of evasion opportunities shifts up by less than the nominal tax rate. This results in a new market equilibrium at (p_t, q_t) , where subscript t indicates the experimental group with evasion opportunity (more on this below).

This intuition leads to a qualitative prediction: the equilibrium price in markets with evasion opportunities will be lower than in markets where evasion is not an option; i.e., $(p_t < p_c)$.¹⁰

The quantitative difference between the equilibrium prices in the evasion and non-evasion markets is determined by the magnitude of the shift in the evasion market's market supply curve. This shift is positively related to the effective tax rate faced by sellers in the evasion groups.¹¹ 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:

⁹This 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).

¹⁰Accordingly, the number of units sold will be higher in the evasion markets than in markets without evasion; i.e., $(q_t > q_c)$.

¹¹Note 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 $(1 - \tau_{profits})X$.

$$t_i^e = \frac{\tau(r_i + 2\gamma(s_i - r_i))}{s_i}, \quad (1)$$

where s_i denotes the number of units a seller actually sells and r_i is the number of units she reports.¹² This simple equation shows that the effective tax rate is increasing in the nominal tax rate and decreasing in evasion (for $\gamma \leq 0.5$).¹³ Therefore, an increase in evasion implies a smaller shift in the market supply curve. While it is plausible to expect that the evasion rate will be larger than zero, it is difficult to predict the exact level of evasion ex-ante, and it is therefore not possible to make any predictions regarding the quantitative effects of the treatment on prices.¹⁴

2.2 Evasion Opportunity Does *Not* Affect Prices

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

Separability. First, one finding in the theoretical literature is that firms treat their evasion and pricing decisions as separable; sellers first set a price at which to sell, and then later make their evasion decision (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. The separability result thus implies that the equilibrium price

¹²The 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 (1). 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.

¹³Because 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.

¹⁴It 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.

that arises in a market *with* evasion opportunities is the same as in a market *without* evasion opportunities (i.e., $p_t = p_c$).

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 a starting point to reflect the conceptual channels in our setting. However, we acknowledge that 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. (2023) shows that choice bracketing matters in the context of taxation: In a situation

where taxpayers first make a production decision and taxation of production-based profit is deferred to a later point of time, taxpayers do not fully take taxes into account in their initial production decision.

Such choice bracketing is potentially relevant in the context of our empirical design (see below for design details). Although the tax in our experimental design seems simple at first glance, sellers in our experiment trade and make pricing decisions in every trading round, and they make evasion decisions every third round. This creates a time gap between the pricing decision and the evasion decision. Furthermore, sellers operate in a fast-paced market where they are required to track bids, asks, number of items sold, tax liability, and time remaining in a trading period. It is not inconceivable that sellers operating in this kind of environment would make the initial pricing decision in isolation of the subsequent evasion decision to minimize mental-decision-making costs.¹⁵

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

Second, why would choice bracketing apply to tax evasion and not the tax itself; i.e., should choice bracketing affect pricing in the control group too? While it is possible that choice bracketing leads subjects to view the pricing decision separately from the tax payment, the data do not suggest that subjects acted in this way. In particular, the observed equilibrium price in the condition without evasion opportunity reflects an equal split of the tax between buyers and sellers as predicted by theory (see Section 5 below). That sellers fully account for the tax in their pricing decisions makes sense since all that is required is adding 10 ECU to the cost of each item. Accounting for tax evasion is a

¹⁵Note that, in contrast to the above separability result, the choice bracketing rationale also holds for risk averse taxpayers.

more difficult task. The subject must account for the effective tax rate – rather than the nominal tax rate –, which is only known in expectation since the subject must consider audit probability and penalty.

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

3 Experiment 1: How do Evasion Opportunities Affect Prices?

As summarized in the Introduction, we run two sets of experiments. In the first experiment, we study the price effect of having access to evasion opportunities. Because this effect may run through the evasion-induced reduction of the tax burden, the second experiment studies the effect of evasion opportunities in situations in which the reduction in effective tax rate is held constant. In this section, we describe the design and results of the first experiment and leave the description of the second experiment to Section 4.¹⁶

3.1 Experimental Design

Overview. The experimental design reflects a standard competitive experimental double auction market as pioneered by Smith (1962).¹⁷ The auction and the parameters in our experiment are based on Grosser and Reuben (2013). In each round of the double

¹⁶The second experiment is best described as an additional treatment arm of the first experiment. However, we use the terms *Experiment 1* and *Experiment 2*, because the second experiment builds on the results of the first experiment and they were therefore not conducted simultaneously. Note that the general setting of the two experiments is very similar and the second experiment eventually builds on the design and results of the first experiment. As a result, many of the information provided in this section also apply to the second experiment and we will only highlight the differences to the first experiment when we describe the second experiment in Section 4.

¹⁷Double 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.

auction market, 5 buyers and 5 sellers trade two units of a homogeneous and fictitious 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 an *evasion* group the opportunity to evade the tax, whereas sellers in the *control* group pay the per-unit tax automatically (as with exact withholding). We employ a between-subjects design where each participant is either in the control or evasion opportunity group.

Organization. We ran a total of 16 experimental sessions, where each session consisted of either a control or evasion group market and lasted about 100 minutes (including review of instructions and payment of participants). We conducted eight control and eight evasion sessions with a total of 160 subjects.¹⁸ The experiments were programmed utilizing *z-tree* software (Fischbacher 2007). Appendix Section C provides summary statistics on demographic characteristics of the participants. Experimental Currency Units (ECU) were used as the currency during the experiment. After the experiment, ECU were converted to Euro with an exchange of $30 \text{ ECU} = 1 \text{ EUR}$ and subjects were paid the sum of all net incomes (see below) in Euro.¹⁹ It was public information that all tax revenue generated in the experiment would be donated to the German Red Cross.

At the beginning of each session, subjects were randomly assigned to computer booths by drawing an ID number out of a bingo bag upon entering the lab. The computer then randomly assigned each subject to role as buyer or seller, as well as her costs or

¹⁸We ran eight sessions (four evasion/four control) in 2013 and eight sessions (four evasion/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 <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 similar between the 2013 sessions conducted in Cologne and the 2021 sessions conducted in Hamburg; see Appendix Table A.1 which compares experimental outcomes for the ‘old’ and ‘new’ data.

¹⁹In addition, subjects received a show-up fee, which was 2.50 EUR in the 2013 sessions and 6.00 EUR in the 2021 sessions.

values which stay constant during the experiment. Subjects were given a hard copy of the instructions when they entered the lab and were allowed as much time as needed to familiarize themselves with the procedure of the experiment. They were also allowed to ask any clarifying questions. The instructions were identical for the control and evasion group except for information on the reporting decision and net income of sellers. These differences in the instructions are highlighted in Appendix Section F.

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

Trade in the Double Auction. 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 cost to each of two items. The cost/value of the units vary across items and subjects as illustrated in Appendix Table E.1. This allows us to induce demand and supply curves for each market, which are depicted in Figure 2. 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.²⁰

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.

Calculation of Income. In the control group, gross income in each period consists of the sum of the profit on each unit traded. Sellers' gross profit on each unit is equal to the difference between the selling price and cost, while buyers' profit on each unit is the difference between value and price paid. All subjects (buyers and sellers) are told that sellers have to pay a per-unit tax for each unit sold, that the tax rate is fixed across all periods at $\tau = 10$ ECU per-unit and that the tax is collected at the end of every third trading period. In other words, subjects complete three rounds of trading, then tax is collected from sellers, then three more rounds of trading, then another tax collection and so on. 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 ($i = 1, 2, 3, \dots, 25, 26, 27$) as

$$\Pi_i^s = P_{i1}d_1 + P_{i2}d_2 - C_1d_1 - C_2d_2, \quad (2)$$

for sellers, and

$$\Pi_i^b = V_1d_1 + V_2d_2 - P_{i1}d_1 - P_{i2}d_2, \quad (3)$$

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

²⁰Figure E.1 in the appendix depicts a screenshot of the experimental market place for a seller in the treatment group with evasion opportunity.

income for each tax collection period k ($k = 3, 6, 9, 12, 15, 18, 21, 24, 27$) is equal to:

$$\pi_k^s = \Pi_k^s + \Pi_{k-1}^s + \Pi_{k-2}^s - \tau U, \quad (4)$$

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:

$$\pi_k^b = \Pi_k^b + \Pi_{k-1}^b + \Pi_{k-2}^b \quad (5)$$

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

Since buyers do not pay the tax, the calculation of gross and net income for buyers in the evasion group is identical to that of the control condition: see equations (3) and (5). 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 underreport sales for tax purposes. Another advantage is that it yields 9 reporting decisions. This is advantageous because it means that subjects can learn the implications of tax evasion for their profits and update their beliefs about the probability of being caught. As a result, we can be assured that the market equilibrium in the evasion treatment reflects the impact of tax evasion on the behavior of market participants. Although reporting every period would maximize the number of reporting decisions, we opted against this option because excess supply in the market implies that some subjects will sell zero units in a given trading period, which trivializes the reporting decision. Another option is to have subjects make a single

reporting decision at the end of the experiment. While this approach maximizes the chance that everyone has a positive amount to report, having a single reporting decision would not allow subjects to learn or update their beliefs. We opted for every third round as a reasonable compromise between these two extremes.²¹

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%) 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 evasion condition are able to evade the sales tax by underreporting sales. Sellers' gross income in any trading period i is the same as in equation (2), but their net income in each tax collection period is rewritten as:

$$\pi_k^s = \begin{cases} \Pi_k^s + \Pi_{k-1}^s + \Pi_{k-2}^s - \tau R & \text{if not audited,} \\ \Pi_k^s + \Pi_{k-1}^s + \Pi_{k-2}^s - \tau U - \tau(U - R) & \text{if audited,} \end{cases} \quad (6)$$

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

Market Equilibrium without Evasion. The demand and supply schedules described and displayed in Figure 2 (and Appendix Table E.1) can be used to determine the competitive equilibrium price (and quantity) with and without the per-unit tax. Theoretically, we expect the market to clear with 7 units traded at any price in the range 48 ECU to 52 ECU in the case without taxes. We obtain a range of prices in equilibrium because the

²¹Although 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.

demand schedule is stepwise linear (Ruffle 2005; Cox et al. 2018; Grosser and Reuben 2013).²²

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 2. 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.²³

The question we seek to answer is whether this equilibrium outcome is affected by the presence of tax evasion opportunities among sellers.²⁴

Definition of Prices. We are particularly interested in knowing whether the market clearing price is different across experimental groups. 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 \bar{P} and P_{50} , respectively. Therefore, our data set has one observation per market-period when price is measured by \bar{P} or P_{50} , and n observations per market-period

²²Grosser 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.

²³We 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.

²⁴Note 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.

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 Appendix B), the discussion of the results focuses on the average price in a given market-period, \bar{P} .

3.2 Empirical Strategy

Non-parametric Analysis. Because the period-specific prices are not independent across the 27 periods within a given market, we implement our non-parametric analysis (ranksum tests; see Footnote 25) using the average price for each market; that is, we use the average of \bar{P} by market. This implies that our non-parametric analysis is based on 16 independent observations; eight in the evasion and eight in the control conditions.²⁵

Regressions. We estimate treatment effects in a regression framework by regressing our measure of price, \bar{P} , on a treatment dummy:

$$\bar{P}_{i,m} = \beta_0 + \delta T_m + \epsilon_{i,m}, \quad (7)$$

where $\bar{P}_{i,m}$ is the mean price of the good in period i (with $i = 1, \dots, 27$) of market m (with $m = 1, \dots, 16$).²⁶ T_m is a dummy indicating if market m is in the evasion group or in the control group without evasion opportunity. Our coefficient of interest is δ , which represents the difference in market price between the two conditions. We set up our data as a panel with 27 periods per market and run pooled OLS regressions. To account for the dependence of prices across periods within a market, we cluster standard errors on

²⁵While the number of independent observations, 16, appears to be low, it is not unprecedented to use such few observations in empirical analysis; see for example Grosser and Reuben (2013) who apply nonparametric tests based on four independent market-level observations and have sufficient statistical power. We use the 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 experimental conditions with significant precision, which suggests that the number of observations is sufficient in our study.

²⁶We also study the effect of evasion opportunities on the number of units sold. Because our focus is on the price results, the results for the quantity of units sold are presented in Appendix B.

the market level (which gives 16 clusters).²⁷ 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 (included for robustness reasons, although they only imply an intercept shift and should not matter for the treatment effect) and/or pre-determined demographic variables (age, gender, native language, field of study Economics indicator, each measured as the average on the market level) in some specifications (Appendix C provides details regarding the measurement and coding of the control variables).

3.3 Results

Compliance Rate in Evasion Group. Before presenting our main results, we report the compliance rate that we observe in the group with evasion opportunity. We find that 37 out of 40 sellers in this condition evade some positive amount of sales at least once, and 26 of the 40 sellers fully pursue 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 group and 72% among those who report non-zero sales.²⁸

Price Effects. Figure 3 reports the mean market price by period for the markets with and without evasion opportunity. The figure shows that the price in the evasion group is lower than in the control group. We also see that the mean market price varies in both groups in the first 10 to 14 trading periods. This is consistent with the existing literature, which generally finds that double auction markets take approximately 8 to 10 rounds to converge (Ruffle 2005). For this reason, and as is common in the literature,

²⁷Note 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.

²⁸This 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 for the ‘use’ tax in the United States is estimated to be between 0 to 5% among individuals (GAO 2000).

our results focus on data from trading periods 15 to 27 (however, we report results for the full sample as well).

The mean market price in both groups stabilized after round 14 at **54.56 ECU** in the control group and **51.33 ECU** in the evasion group (see Panel B of Appendix Table A.2). This implies that the mean market price in the evasion group is 3.23 ECU lower than in the control group.²⁹ These differences in prices between the groups are statistically significant from zero; the exact ranksum test (two-sided) gives a p-value of 0.001 for differences in mean prices (median prices are also significantly different from each other).

The corresponding regression results (based on equation (7) for the mean price in each trading period) are presented in Table 1. The estimated treatment effect of -3.23 ECU reported in model 1 of Panel B is statistically different from zero at the 1% level.³⁰ This main estimate of the treatment effect remains statistically significant after correcting for the small number of clusters using the wild-bootstrap-t procedure described in Cameron et al. (2008); see Appendix Table A.4.³¹ The estimate is robust to the inclusion of period fixed effects (model 2). It becomes smaller (-2.70), but remains statistically significant as we add control variables (models 3 and 4). Appendix Table A.6 shows that the finding of evasion markets trading at lower markets than control markets is robust to the definition of price (this table considers the price at which each item is sold in Panel A and the median price in each period in Panel B). Overall, we find that markets with evasion opportunities are characterized by lower prices than markets where evasion is not an option.³²

²⁹Note that this difference is larger for the full sample (55.07 ECU vs 51.18 ECU; see panel A of Appendix Table A.2). As shown in the second column of Appendix Table A.2, median prices are also lower in the treatment group than in the control group; the median price is **51.36 ECU** in the treatment group and **54.18 ECU** in the control group, resulting in a treatment effect of 2.82 ECU.

³⁰Panel A of Table 1 reports the results for the full sample, where the treatment effects are even larger than in periods 15-27.

³¹The correction is implemented using Stata code provided by Judson Caskey and is available <https://sites.google.com/site/judsoncaskey/data>.

³²Further evidence that tax evasion opportunities significantly affect the market price is provided in Appendix Figure A.1, which reports the cumulative distribution of mean market prices for the treatment and control groups. The figure shows 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:

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

The results presented in the previous section show that markets with sellers who have the opportunity to evade taxes trade at lower prices than markets where tax evasion is not possible. This empirical finding speaks to the two opposing predictions that we presented in Section 2. It supports the prediction that tax evasion opportunities do have an effect on prices. The rationale is that tax evasion lowers the effective tax rate facing sellers, which then allows sellers to trade at lower prices in a competitive market.

However, a reduction in tax burden would yield lower prices even in the absence of tax evasion. This raises the question of whether the evasion opportunity effect is driven solely by the evasion-induced reduction of the tax burden. Put differently, suppose firms in two markets experience similar reductions in effective tax burden, but firms in one market evade taxes to arrive at this lower tax burden whereas firms in the other market experience an exogenous reduction. Would the price effect be the same across these two markets?

In this section, we present the results of a second experiment that sheds light on this question. Since tax evasion naturally comes with reductions in the tax burden, the main idea of this *tax credit* experiment is the following. We exogenously reduce the tax burden of sellers to the effective tax burden that we observe in the evasion group of the first experiment. Comparing the tax credit group and the evasion group then allows us to explore a situation where all sellers have the same effective tax burden, but some sellers evade taxes to arrive at this tax burden, whereas other sellers face an exogenously determined lower tax burden. As noted in Footnote 16, the second experiment can be viewed as an additional experimental arm of the first experiment. However, we describe them as *Experiment 1* and *Experiment 2*, because the second experiment builds on the results of the first experiment, which means they were not conducted simultaneously.

0.000).

4.1 Experimental Design

Overview. The tax credit sessions are identical to the control sessions of the first experiment except that the effective tax rate is exogenously lowered to 2.50 ECU, which is the same as the effective tax rate in the evasion group of the first experiment.³³ As in the previous experiment, the nominal tax rate is set at 10 ECU, but sellers are told that they will receive an automatic tax credit of 7.5 ECU for every unit they sell. Therefore, sellers in the tax credit group face an effective tax rate that is lower than their nominal tax rate, but identical to that of sellers in the evasion group. Importantly, while the effective tax burden is the same across sellers in the evasion and tax credit groups, sellers in the tax credit group do not have to take any actions to arrive at this lower effective tax rate. This is different from the sellers in the evasion group who must 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.

Organization and Sample. We ran eight sessions – that lasted approximately 100 minutes each – of this tax credit group for a total of eight markets and 80 subjects. Three of these eight sessions were conducted in 2015 at the lab of the University of Cologne (where we ran the 2013 sessions of the first experiment) and five sessions in 2021 at the lab of the University of Hamburg (where we ran the 2021 sessions of the first experiment). None of the subjects in the tax credit group had participated in the first experiment. There were 10 subjects (five buyers and five sellers) in each session, and the average pay-off was 24 EUR.³⁴ Summary statistics for the tax credit condition are provided in Appendix Section C.

We exclude the 2021 evasion treatments from all subsequent analyses because the

³³See equation 1 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 conditions. We use the effective tax rate that emerged in our initial 2013 experimental evasion-condition sessions; see below for an explanation. The effective tax rate in these initial evasion group 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.

³⁴The show-up fee was 2.50 EUR in the 2015 sessions (as in the 2013 sessions) and 6.00 EUR in the 2021 sessions.

effective tax rate in the 2013 evasion group was an input in the 2015 tax credit group, and we want the 2015 and 2021 tax credit groups to be comparable.³⁵ 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 opportunity sessions (all of which have the same effective tax burden).

The equilibrium price in the 2021 evasion groups is very similar to the price in the 2013 evasion groups: mean price of 51.66 ECU for 2013 sessions vs 51.00 ECU for 2021 sessions; see Table A.1. We interpret this similarity to be advantageous for at least two reasons. First, the price 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 groups in the subsequent analyses. The similarity between the 2013/2015 results and 2021 results is generally confirmed as we consider the main experimental outcomes by experimental group and compare them across the 2013/2015 and 2021 data. As shown in Table A.1, the results are similar for the ‘old’ and ‘new’ data.

4.2 Results

The definition of prices and empirical strategy are analogous to the first experiment. Figure 4 depicts the mean market price in all three experimental groups (of Experiments 1 and 2) 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 2).³⁶ This difference of 1.68 ECU is economically meaningful, as it is more than one-half of the effective tax rate

³⁵The effective tax rate differs between the 2021 and 2013 evasion sessions because of the difference in compliance rates: 4% in the 2021 evasion sessions vs. 2.56% in the 2013 evasion sessions (both calculated using Equation 1).

³⁶Note that, for reasons of completeness, Table 2 also reports the number of units sold across experimental conditions. We focus on the price effects here and leave a discussion of the quantity results to Appendix B.

and larger than 1 standard deviation of mean price in the evasion group, and statistically significant (in non-parametric tests with 12 independent observations; see Table A.3).

Table 3 reports regression results in which we benchmark the initial evasion groups against the tax credit groups, with the mean price in each period as dependent variable (regressions are specified as in Equation 7). The regression coefficients in all four models of Panel B (periods 15-27) show that the clearing price in the tax evasion markets is higher than in tax credit markets, despite identical effective tax rates.³⁷ This difference is statistically significant in specifications 1 and 2 (on the 5% level in specification 1 without any covariates or fixed effects). The inclusion of pre-defined control variables somewhat reduces the treatment coefficient and it becomes insignificant.³⁸

Together, the analysis tends to indicate that evasion-induced reductions in tax burdens affect prices differently than exogenous reductions in tax burdens. Specifically, markets with evasion opportunity trade at higher prices than markets with an equivalent, yet exogenous, tax burden. Possible explanations for this difference are that sellers in the evasion treatment adjust prices to account for the intrinsic and perceived extrinsic costs associated with an evasion-induced reduction in the tax rate. 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.

5 Implications for Tax Incidence

This section discusses the incidence implications of our experimental findings. Because we discuss incidence both in the context of the first and second experiment, the subsequent analyses again exclude the most recent (2021) evasion sessions to ensure comparability across all experimental groups (see Section 4 above for an explanation).

³⁷Panel A presents the results for the case where we include all periods. The coefficients indicating the evasion group are also positive, yet somewhat smaller and less precisely measured.

³⁸Inference is robust to small-cluster adjustments; see Table A.5. Comparing the cumulative price distributions in the evasion group and the tax credit group (Appendix Figure A.2) 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).

Estimation of Economic Incidence. We estimate economic incidence of the nominal tax rate and the effective tax rate on buyers. 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. The results of the first experiment indicate that sellers in the evasion group face a lower tax burden and trade at lower prices than sellers in the control group. 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 3.1, 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. We use their “no tax” result as a benchmark in the following discussion of economic incidence.

Nominal Tax Rate. Table 2 summarizes the economic incidence of the nominal tax rate in the presence of tax evasion opportunities. 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 condition is shared equally between buyers and sellers since the nominal tax rate is 10 ECU per unit. This is consistent with the theoretical prediction; since the demand and supply schedules have equal price elasticity in equilibrium, the tax burden is expected to be shared equally between buyers and sellers.

The next step is to determine the extent to which access to evasion affects the economic incidence of the nominal tax. The mean market clearing price in the evasion group is 51.66 ECU. Considering the nominal tax rate of 10 ECU per unit and the no-tax benchmark of 49.04 ECU, this implies that buyers in the evasion group pay 26%

($= (51.66 - 49.04)/10$) of the *nominal* tax burden, compared to the $\approx 50\%$ in the control group. In other words, access to evasion reduces the economic incidence of the nominal tax on buyers by about 29 percentage points ($= 55.20\% - 26.20\%$). This 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 price reduction of 2.90 ECU is large.

Effective Tax Rate. In a next step, we wish to know whether access to evasion changes 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 without evasion. How does this incidence result change in the presence of tax evasion opportunities? The price in the evasion 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.³⁹ 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 last column of Table 2.

What about the incidence implications in the context of our tax credit group? Notice that consumers in the tax credit group pay 0.94 ECU ($= 49.98 - 49.04$) of the nominal tax rate, implying that sellers in this group shifted 38% of their effective tax burden onto buyers. 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.

³⁹We would expect the price in the evasion group to increase by approximately 1.28 ECU ($= 2.56/2$) relative to the “no tax” equilibrium of 49.04 ECU if the incidence was shared 50-50; that is to 50.32 ECU. The observed price of 51.66 ECU is statistically larger than 50.32; one-sided test has p-value < 0.001 .

6 Conclusion

We provide evidence that tax evasion opportunities affect market prices. First, markets with a tax evasion opportunity clear at lower prices than markets without evasion opportunity. This effect is likely driven by the evasion-induced reduction of the tax burden. Second, comparing markets with the same tax burden, we tend to find that evasion opportunities actually increase market prices (though this finding is not always statistically significant).

These results potentially have implications for the effects of policies aimed at reducing tax evasion. First, policies that reduce evasion opportunities will lead to higher market prices, *all else equal*. Second, and arguably more interestingly, policymakers can reduce evasion opportunities *and* lower prices by combining their evasion-curbing policies with changes in tax policy that leave effective tax burdens unchanged. This is an important finding, because it highlights how tax policy can be utilized to improve consumer welfare. Our results further imply that the price effects that result from exogenous changes in tax burdens cannot be used to quantify the likely price effects of policies targeting tax evasion.

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

While we focus on sales taxes in our paper, the findings also suggest that other anti-tax evasion initiatives, such as the Foreign Account Tax Compliance Act (FATCA) or the EU’s Central Electronic System of Payment information (CESOP), are likely to

affect prices as affected parties respond to the reduced evasion opportunities. On a more speculative basis, our results may also have some implications for policies that target (legal) tax *avoidance* opportunities. Tax avoidance opportunities, just as evasion opportunities, allow firms to reduce their tax burden and therefore give them scope to trade at lower prices. This would then imply that the underlying rationale behind our findings also applies to tax avoidance and that anti-avoidance policies potentially increase prices if they are not accompanied by tax rate reductions. However, we are cautious in extending our findings to tax avoidance because it is not clear that evasion and avoidance have comparable effects.⁴⁰

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

We identify several further avenues for future research that emerge from our findings. The 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 set of studies, one interesting question is if our results would be similar in an institutional set-up where there is uncertainty about the audit process and in which the desire for risk compensation is thus complicated. Another valu-

⁴⁰See Footnote 6 in the Introduction where we acknowledge and discuss the differences between evasion and avoidance in more detail. We provide a general discussion of the external validity of our experimental findings in Appendix D.

able follow-up study could investigate if the effect of evasion opportunities on prices 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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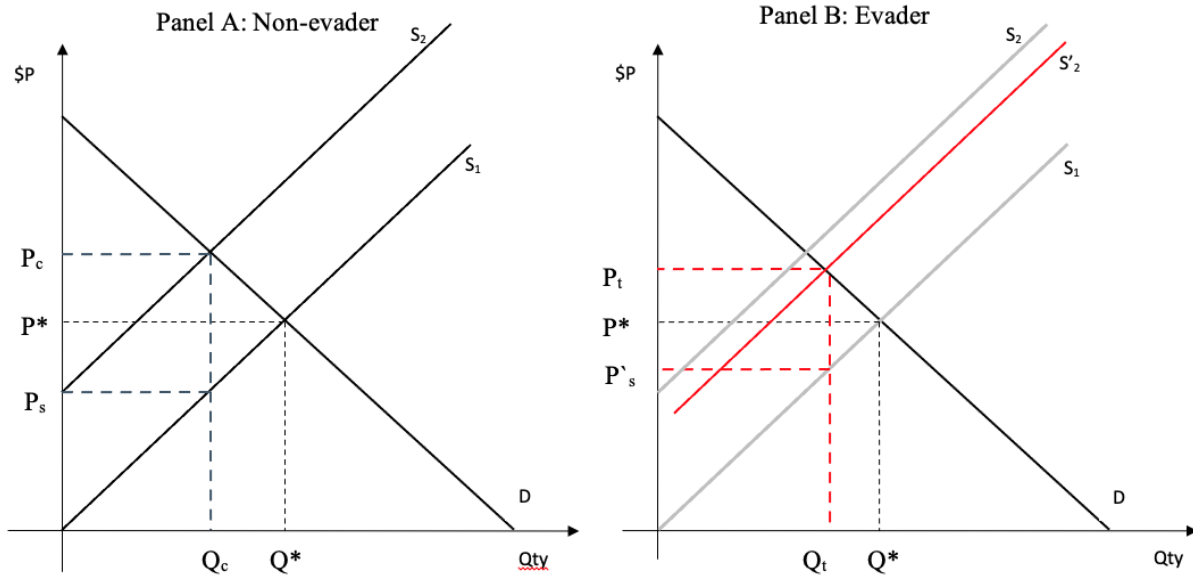
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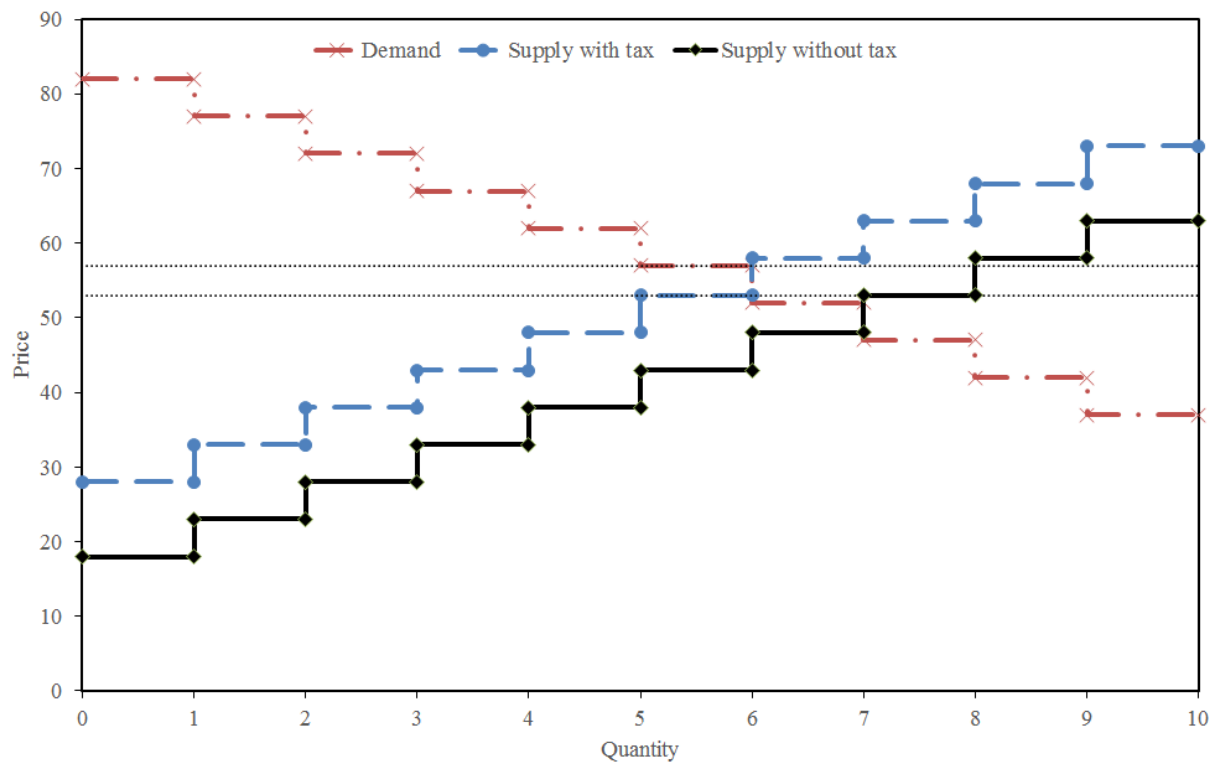
Tables and Figures (in order of appearance in manuscript)

Figure 1: Economic Incidence of Tax on Seller



Notes: The imposition of a per-unit tax would ordinarily cause the supply curve to shift to the left and the market equilibrium to move from point (P^*, Q^*) to (P_c, Q_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 2: Experimental Supply and Demand Schedule



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

Figure 3: Mean Price with and without Evasion Opportunity



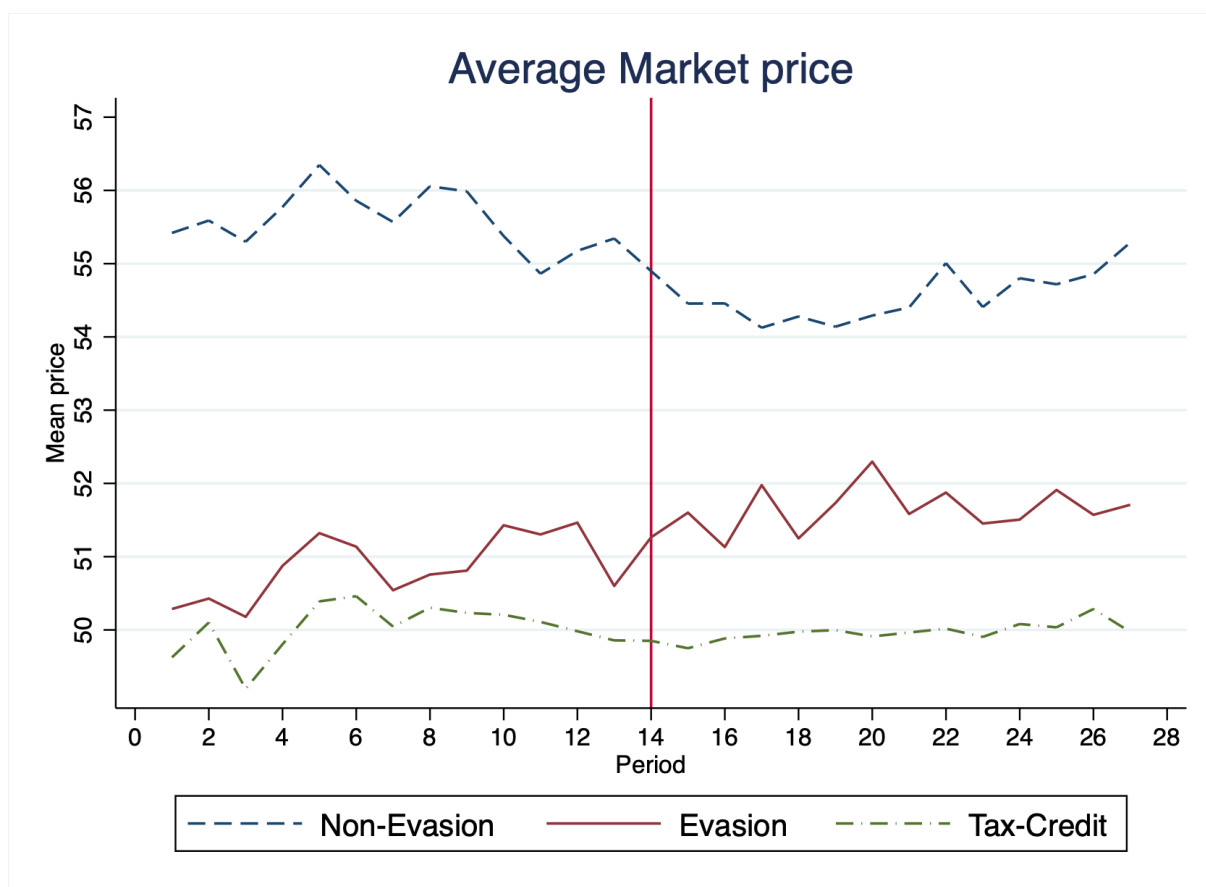
Notes: Reported is the mean price \bar{P} in each period separately for the evasion and control groups in the first experiment. The sample includes eight independent markets in the control group and eight independent markets in the evasion group.

Table 1: Impact of Evasion Opportunity on Mean Price

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

Notes: Results from the first experiment. Robust standard errors adjusted for clustering at the market level are in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%. Estimates are based on equation (7) with the dependent variable defined as the mean price in a given market period. Regression results for the effect of the *evasion* group (where sellers have evasion opportunity; experiment 1) relative to the control group (where sellers do not have an evasion option; experiment 1). Panel A uses periods 1 to 27 and panel B uses periods 15 to 27. The sample includes eight independent markets in the control group and eight independent markets in the evasion group (for a total of 16 markets/clusters). Period FE is period fixed effects. Control variables include the average age, share of males, share of native German speakers, and share of subjects whose field of study is economics. These averages and shares are calculated at the market-level.

Figure 4: Mean Price with and without Evasion Opportunity and Constant Tax Burden



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

Table 2: Overview of Results and Economic Incidence

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

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

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

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

Notes: Robust standard errors adjusted for clustering at the market level are in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%. Regression results for the effect of the *evasion* group (where sellers have evasion opportunity; experiment 1) relative to the tax credit group (where sellers face the same effective tax rate as in the evasion group, but do not have to evade to arrive there; experiment 2). To have identical effective tax rates in the two groups of interest, the regressions include the four initial evasion group sessions (see Section 4 for further explanation). The sample includes four independent markets in the evasion group and eight independent markets in the tax credit group (for a total of 12 markets/clusters). Estimates are based on equation (7) with the dependent variable defined as mean price in a given market period. Panel A uses periods 1 to 27, Panel B uses periods 15 to 27. Period FE is period fixed effects. Control variables include the average age, share of males, share of native German speakers, and share of subjects whose field of study is economics. These averages and shares are calculated at the market-level.

(Online) Appendix

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A Additional Results

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

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

Notes: This table compares experimental outcomes across experimental data collected in the years 2013 and 2015 (‘old’ data) vs experimental data collected in 2021 (‘new’ data). In 2013, we collected data for four control and four evasion markets. In 2015, we collected data for three tax credit markets. In 2021, we collected data for four control markets, four evasion markets and five tax credit markets. The 2013 and 2015 sessions were conducted at the University of Cologne. The 2021 sessions were conducted at University of Hamburg. Reported is the mean of \bar{P} , P_{50} , and the number of units sold – see definitions in Section 3.2) – separately for the ‘old’ and ‘new’ data. All calculations restricted to periods 15-27. Both the new and old data have 52 session-periods for control and evasion. There are 39 session-periods in the old tax credit data and 65 session-periods in the new tax credit data. *Evasion* indicates participants with an evasion opportunity, *Control* indicates subjects without evasion opportunity, and *Tax Credit* indicates participants without evasion opportunity but with tax credit. Standard deviations in parentheses.

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

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

Notes: Reported are the mean and median price in a given market period as well as *Units Sold*, which is the market-level mean of units sold in a given market period, by experimental condition of the first experiment (see definitions in Section 3.2). Standard deviations in parentheses. *Evasion* indicates markets with an evasion opportunity and *Control* indicates markets without evasion opportunity. All numbers and statistics are based on 16 independent market-level observations (8 control, 8 evasion). Panel A uses all completed contracts from periods 1 to 27 and Panel B uses all completed contracts in periods 15 to 27. P-value is for the exact Wilcoxon ranksum test based on 16 independent market-level observations; null hypothesis is that there is no difference between evasion and control group.

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

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

Notes: Reported are the mean and median price in a given market period as well as *Units Sold*, which is the market-level mean of units sold in a given market period, by experimental condition of the second experiment (see definitions in Section 3.2). Standard deviations in parentheses. *Evasion* indicates markets with an evasion opportunity and *Tax Credit* indicates markets without evasion opportunity, but a tax credit. The effective tax burden is comparable across these two groups. All numbers and statistics are based on 12 independent market-level observations (8 tax credit, 4 evasion). Panel A uses all completed contracts from periods 1 to 27 and Panel B uses all completed contracts in periods 15 to 27. P-value is for the exact Wilcoxon ranksum test based on 12 independent market-level observations; null hypothesis is that there is no difference between evasion and control group.

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

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

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

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

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

Notes: Effect of evasion opportunity on outcome variables mean price in a given market period and units sold in a given market period. Standard errors in parentheses are adjusted for clustering at the market level and corrected for the small number of clusters using the wild-bootstrap-t procedure described in Cameron et al. (2008). Results from Experiment 2. Regression results for the effect of the *evasion* group (where sellers have evasion opportunity; experiment 1) relative to the tax credit group (where sellers face the same effective tax rate as in the evasion group, but do not have to evade to arrive there; experiment 2). To have identical effective tax rates in the two groups of interest, the regressions only include the four initial evasion group sessions (see Section 4 for further explanation). The sample includes eight independent markets in the tax credit group and four independent markets in the evasion group (for a total of 12 markets/clusters). The correction is implemented using Stata code provided by Judson Caskey and is available here: <https://sites.google.com/site/judsoncaskey/data>. * significant at 10%; ** significant at 5%; *** significant at 1%. All columns use completed contracts from periods 15 to 27. Number of observations is 156 (=12 markets \times 13 periods). Control variables include the average age, share of males, share of native German speakers, and share of subjects whose field of study is economics. These averages and shares are calculated at the market-level.

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

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

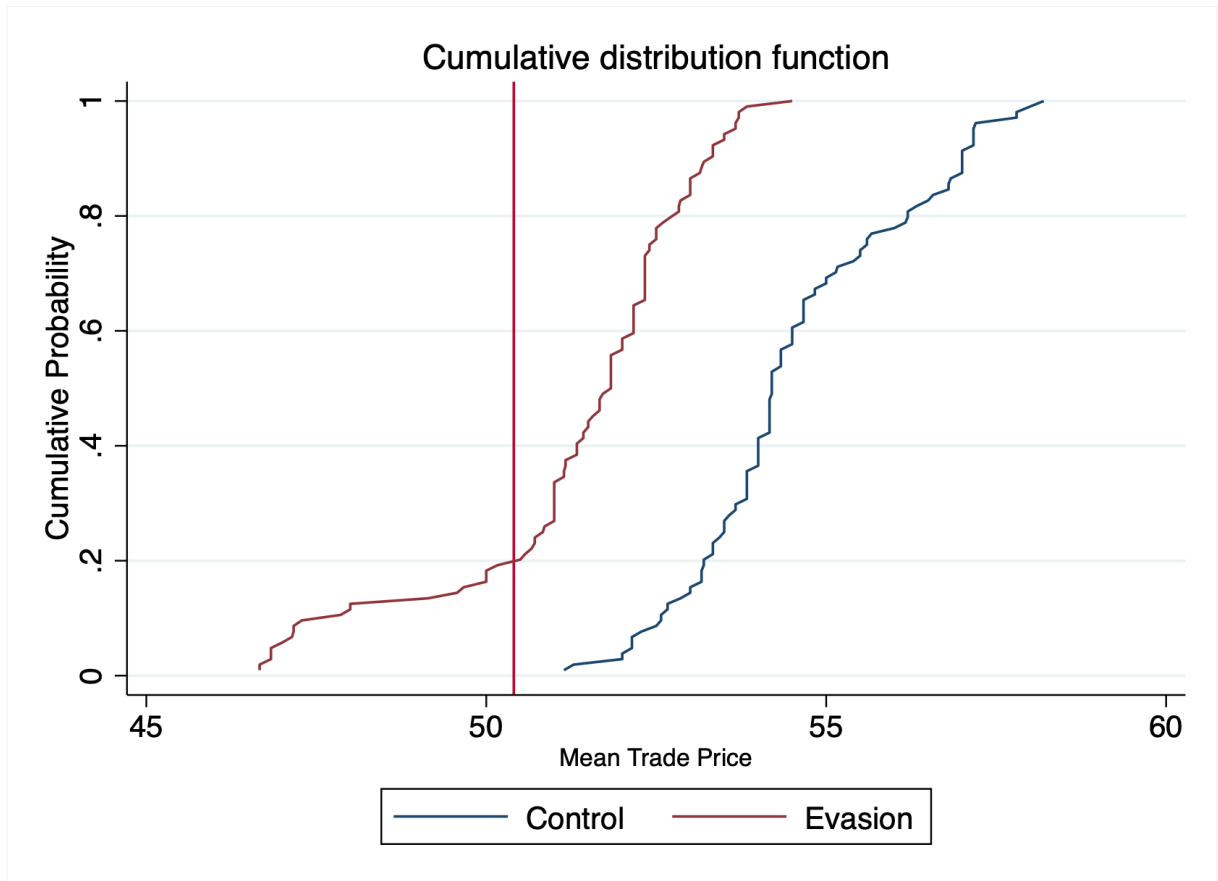
Notes: Robust standard errors adjusted for clustering at the market level are in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%. Estimates are based on equation (7) with the dependent variable defined as market price for each good sold in Panel A and median price in a given market period in Panel B. Results from Experiment 1. Regression results for the effect of the *evasion* group (where sellers have evasion opportunity; experiment 1) relative to the control group (without evasion opportunity; experiment 1). All panels use completed contracts from periods 15 to 27. The sample includes eight independent markets in the control group and eight independent markets in the evasion group (for a total of 16 markets/clusters). Period FE is period fixed effects. Control variables include the average age, share of males, share of native German speakers, and share of subjects whose field of study is economics. These averages and shares are calculated at the market-level.

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

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

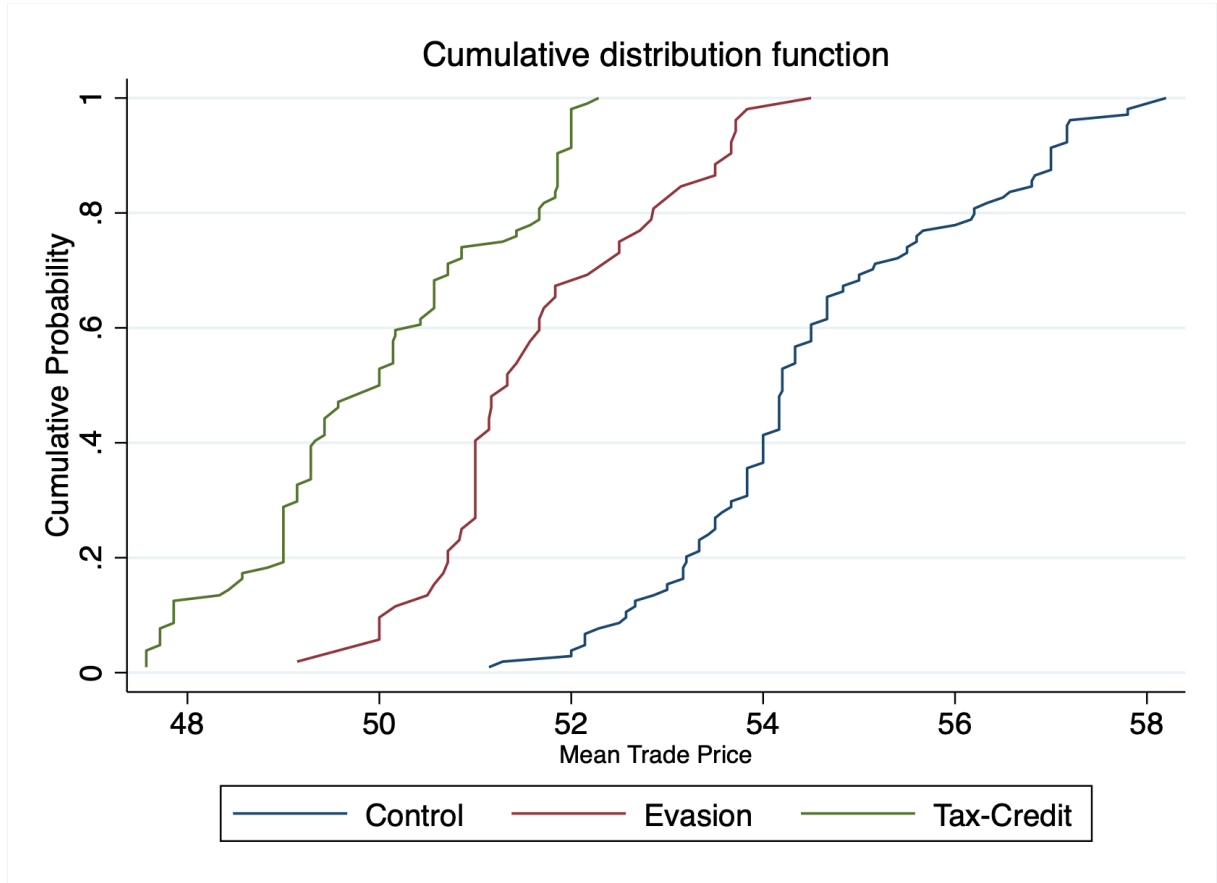
Notes: Robust standard errors adjusted for clustering at the market level are in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%. Estimates are based on equation (7) with the dependent variable defined as market price for each good sold in Panel A and median price in a given market period in Panel B. Results from Experiment 2. Regression results for the effect of the *evasion* group (where sellers have evasion opportunity; experiment 1) relative to the tax credit group (where sellers face the same effective tax rate as in the evasion group, but do not have to evade to arrive there; experiment 2). To have identical effective tax rates in the two groups of interest, the regressions only include the four initial evasion group sessions (see Section 4 for further explanation). All panels use completed contracts from periods 15 to 27. The sample includes eight independent markets in the tax credit group and four independent markets in the evasion group (for a total of 12 markets/clusters). Period FE is period fixed effects. Control variables include the average age, share of males, share of native German speakers, and share of subjects whose field of study is economics. These averages and shares are calculated at the market-level.

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



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

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



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

B Treatment Effects on Units Sold and After-Tax Income

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

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

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

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

imprecisely measured.

Quantity Effects in the Second Experiment. As before, we exclude the new evasion sessions when we consider the effects of the tax-credit treatment, implying that we focus on 8 tax-credit markets and four evasion markets. The results in Appendix Table A.3 show that the mean number of units sold per period in the tax credit is **6.80**, whereas the evasion group sold an average of **6.46** units per period. We thus find a difference of 0.34 units. This difference between units sold in the evasion and tax credit group is statistically significant (based on non-parametric test with 12 independent observations). Panel B of Table B.1 below compares the evasion opportunity group of the first experiment with the tax credit group of the second experiment in a regression approach. The Table shows that the coefficient of interest is negative and statistically significant in all four regression specifications.

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

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

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

Notes: Robust standard errors adjusted for clustering at the market level are in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%. Panel A: Regression results for the effect of the *evasion* group (where sellers have evasion opportunity; experiment 1) relative to the control group (where sellers do not have an evasion option; experiment 1). The sample includes eight independent markets in the control group and eight independent markets in the evasion group (for a total of 16 markets/clusters). Panel B: Regression results for the effect of the *evasion* group (where sellers have evasion opportunity; experiment 1) relative to the tax credit group (where sellers face the same effective tax rate as in the evasion condition, but do not have to evade to arrive there; experiment 2). To have identical effective tax rates in the two groups of interest, Panel B regressions only include the four initial evasion group sessions (see Section 4 for further explanation). The sample in Panel B thus includes four independent markets in the evasion group and eight independent markets in the tax credit group (for a total of 12 markets/clusters). Estimates in both Panels are based on equation (7) with the dependent variable defined as the number of units sold in a given market period. Results use completed contracts from periods 15 to 27. Period FE is period fixed effects. Control variables include the average age, share of males, share of native German speakers, and share of subjects whose field of study is economics. These averages and shares are calculated at the market-level.

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



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

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

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

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

C Summary Statistics

After the respective experimental session, subjects reported their age, gender, native language, level of tax morale, risk preference, and field of study. Tax morale is determined using a question very similar to one used in the World Values Survey (Inglehart nd): “How justified do you think it is to evade taxes if there was an opportunity to do so?”. Subjects could reply on a 10-point scale ranging from ‘0 Always Justified’ to ‘10 Not At All justified’. We generate a dummy variable indicating high tax morale that has value 1 if a subject reported that it is never justified to evade taxes.⁴¹ 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.⁴² Each of these variables is summarized in Table C.1. Non-parametric Wilcoxon rank-sum tests for differences in distributions between groups shed light on whether randomization was successful. We 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.

⁴¹The 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?’

⁴²A subjects is classified risk neutral if indifferent between the options, risk averse if prefers the certain payment and risk seeking if prefers the gamble. The original German question in our questionnaire reads: ‘Bitte stellen Sie sich die folgenden Situationen vor: Situation A: Sie erhalten eine Auszahlung von EUR 50. Situation B: Es wird eine Muenze geworfen. Sie erhalten EUR 100, wenn Kopf erscheint. Sie erhalten EUR 0, wenn Zahl erscheint. Welche Situation wuerden Sie bevorzugen? a) Ich wuerde Situation A bevorzugen, b) Ich wuerde Situation B bevorzugen, c) Ich bin indifferent zwischen den beiden Situationen.’

Table C.1: Summary Statistics of Demographic Variables

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

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

D Motivation to use Lab Experiment and Discussion of External Validity

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

To which extent can our results be generalized to the 'real world'? As with almost all economic laboratory experiments, there remains doubt about the external validity of our results.⁴³ 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

⁴³The 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.

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.⁴⁴

Additionally, although evasion may occur among buyers as well, the real-world problem seems to be more relevant among sellers; sellers are usually responsible for remitting sales taxes to the government. In this sense, our laboratory setting mimics the operation of most transaction taxes in the real world in that we also have a set-up in which sellers remit the tax.⁴⁵ 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].”⁴⁶ While this does not necessarily imply that each firm faced

⁴⁴Tax 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).

⁴⁵The 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.

⁴⁶The article is online here: <https://www.washingtontimes.com/news/2014/nov/10/sales-tax-dodging-on-the-rise-in-mississippi/>.

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.

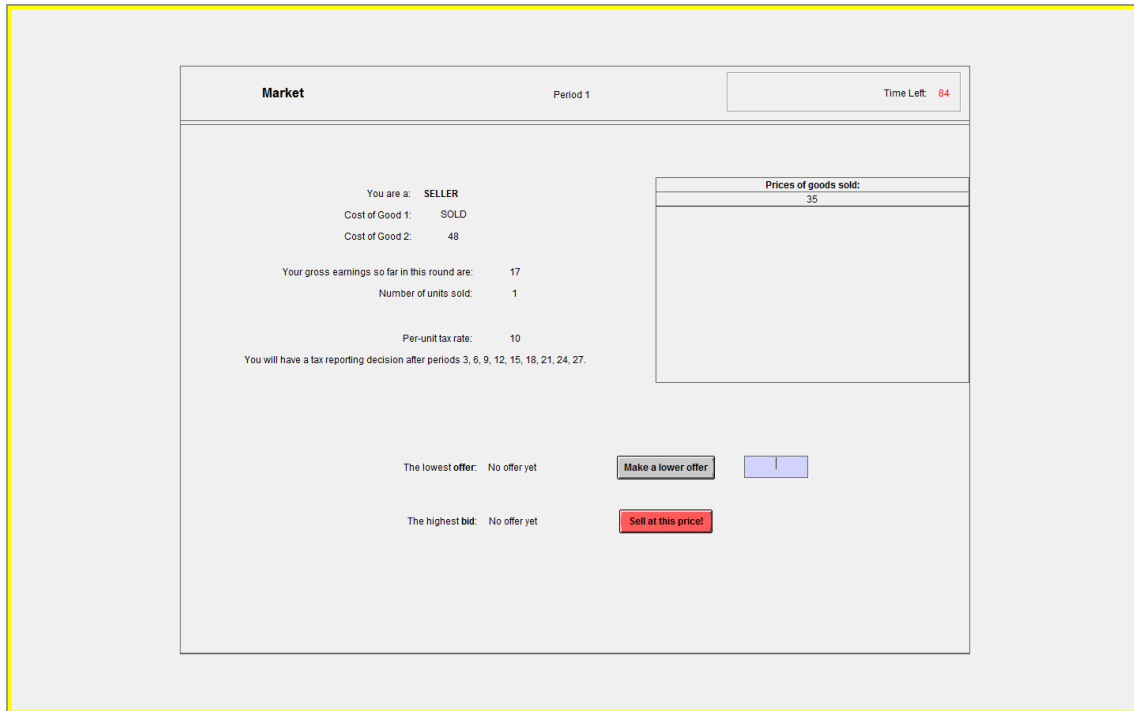
E Additional Information on Experimental Design

Table E.1: Demand and Supply Schedules

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

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

Figure E.1: 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 condition and indicate the differences between conditions in brackets. The original German versions of the instructions are available from the authors upon request.

Instructions

Welcome and thank you for participating in our experiment. From now on until the end of the experiment, please refrain from communicating with other participants. If you do not abide by this rule, we will have to exclude you from the experiment.

We kindly ask you to read the instructions thoroughly. If you have any questions after reading the instructions or during the experiment, please raise your hand and one of the instructors will come to you and answer your question in person. Your payment and your decisions throughout the experiment will be treated confidentially.

You can earn money in this experiment. How much you earn depends on your decisions and the decisions of other participants. During the experiment, your payments will be calculated in a virtual currency: Experimental Currency Units (ECU). **30 ECU correspond to 1 Euro**. After the experiment, your pay-off will be converted to Euro and given to you in cash. Additionally, you will receive a show-up fee of 2.50 Euro.

The Experiment

Roles

At the beginning of the experiment, the computer will randomly assign five participants to the role of **"sellers"** and five other participants to the role of **"buyers"**. Therefore, you will either be a buyer or a seller. Your role as seller or buyer will remain the same

throughout the experiment. You will only know your own role and not the roles of other participants.

Overview

[Control Condition:

The experiment consists of 3 practice rounds and 27 paying rounds. At the beginning of each round, all buyers and sellers trade a fictitious good in a **market place**. As a buyer, you can buy units of the fictitious good and as a seller you can sell units. You can earn ECU in the market place and your earnings depend on your decisions and the decisions of the other participants. Each unit sold will be subject to a per **unit tax of 10 ECU** for sellers. The tax rate is the same for all sellers and is due at the end of every third round. Details on the market place will be explained further below. All tax revenues paid by you and all other participants will be donated to the German Red Cross.

]

[Condition with Evasion Opportunity:

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

]

[Condition with Tax Credit:

The experiment consists of 3 practice rounds and 27 paying rounds. At the beginning of each round, all buyers and sellers trade a fictitious good in a **market place**. As a buyer, you can buy units of the fictitious good and as a seller you can sell units. You can earn ECU in the market place and your earnings depend on your decisions and the decisions of the other participants. Each unit sold will be subject to a per **unit tax of 10 ECU** for sellers. Sellers additionally receive a **tax credit** of 7.50 ECU for each unit sold. The tax rate is the same for all sellers and is due at the end of every third round. Details on the market place will be explained further below. All tax revenues paid by you and all other participants will be donated to the German Red Cross.

]

The Market Place

Basics

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

Units, costs, and values

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

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

buyers' units. Each buyer only knows her own values.

Asks, Bids, and Transactions

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

Sellers cannot sell goods at prices lower than the assigned cost for the respective Unit.

Buyers cannot buy at prices that exceed their assigned value for the respective Unit.

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

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

(Gross) Earnings in the Market Place

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

For Sellers:

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

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

For Buyers:

Gross Earnings from buying Unit 1 = value of Unit 1 - transaction price of Unit 1

Gross Earnings from buying Unit 2 = value of Unit 2 - transaction price of Unit 2

Screenshots from trading market

Sellers:

Here Screenshot Sellers

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

Buyers:

Here Screenshot Buyers

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

[Added in the *condition with evasion opportunity*:

The Reporting Decision for Sellers

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

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

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

]

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

Calculation of Net Income for Sellers

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

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

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

]

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

Calculation of Net Income for Sellers

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

1. Computer selects a number between 2 and 10 (2, 3, 4, 5, 6, 7, 8, 9 or 10):

The reporting decision will *not* be checked. A seller's earnings after taxes – the net income –, in this case, consists of the sum of all her gross earnings from the three previous periods (henceforth denoted "sum gross income") minus the tax payment. The tax payment is the **reported** number of units sold multiplied by the tax rate of 10 ECU. Hence:

$$\underline{\text{Net income}} = \text{sum gross income} - (\text{reported number of units sold} * \text{per unit tax rate})$$

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:

$$\underline{\text{Net income}} = \text{sum gross income} - (\text{actual number of units sold} * \text{per unit tax rate}) - (\text{number of units } \textit{not reported} * \text{per unit tax rate})$$

]

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

Calculation of Net Income for Sellers

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

Therefore, a seller's payment – the net income –, consists of her sum of all gross earnings from the three previous rounds (henceforth denoted "sum gross income") minus the tax

payment. The tax payment consists of the per-unit tax of 10 ECU per unit sold minus the tax credit of 7.5 ECU per unit sold. Hence:

Tax payment

$$\begin{aligned} &= (\text{number of units sold} * \text{per-unit tax rate}) - (\text{number of units sold} * \text{tax credit}) \\ &= \text{number of units sold} * (10 - 7.5) \end{aligned}$$

Net income then is:

Net Income

$$\begin{aligned} &= \text{sum gross income} - \text{tax payment} \\ &= \text{sum gross income} - (\text{number of units sold} * (10 - 7.5)) \\ &] \end{aligned}$$

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.