

Koch, Christian; Müller, Cornelius

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Tax Amnesties and the Insurance Effect: An Experimental Study

Christian Koch* and Cornelius Müller†

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Abstract

Many countries grant exemption from legal prosecution under certain conditions, allowing for voluntary disclosures regarding tax evasion. Empirically, tax amnesties appear most successful when accompanied by an increase in enforcement efforts because they help evaders to adjust to the new circumstances. Time-limited amnesties are often repeated or in some countries even permanent. Anticipated tax amnesties can, however, serve as an *insurance* against a rise in the detection probability, potentially leading to less and not more tax compliance. We test the relevance of this *insurance effect* in an experimental tax game and disentangle it from an effect on *tax morale*, i.e., non-pecuniary motives to comply. We find that the former effect indeed decreases overall tax compliance by about 7-10 percent, showing its relevance for the design of tax policies. A high predictability of amnesties may work to their detriment.

JEL codes: H24, H26, C91

Keywords: tax compliance, tax amnesty, lab experiment, social norms

* *Corresponding author:* Koch. University of Vienna, Oskar-Morgenstern-Platz 1, 1090 Vienna, Austria, E-mail: chris.koch@univie.ac.at

†Müller. Daimler Truck AG, Fasanenweg 10, 70771 Leinfelden-Echterdingen, Germany, E-mail: cornelius.mueller@daimlertruck.com

1 Introduction

Tax amnesties are a popular instrument for governments around the world that aim to increase both short- and medium-term tax revenue. For instance, in the US, more than 110 amnesty programs have taken place since 1980. Despite many different features, all of these amnesties had in common that they did not forgive the tax owed but waived penalties and criminal prosecution (Mikesell and Ross, 2012).

Notwithstanding the frequent use of tax amnesties by tax authorities, it has been argued in the literature that amnesties do not have a direct effect on tax compliance because they do not per se alter the cost-benefit analysis of tax evasion (Alm and Beck 1991; Macho-Stadler et al. 1999). From a rational perspective, those citizens who decided to evade taxes in the past by weighting its pros and cons should not voluntarily disclose their underreported income, because a tax amnesty does not change the parameters of their decisions *ceteris paribus*. Potentially for this reason, Alm et al. (1990) made the observation that tax amnesties are most successful when they are accompanied by increased enforcement efforts, ‘inspiring’ a reevaluation of the decision to evade.

When enforcement efforts, however, fluctuate, the frequent use of tax amnesties may be problematic. Although many tax amnesties only provide a limited-time offer to voluntarily disclose underreported income, a lot of amnesties are repeated several times, as many of those programs in the US stated above. Moreover, Baer and Le Borgne (2008) note that countries such as Denmark, Germany, Mexico, Netherlands, Norway and Sweden have even implemented *permanent* tax amnesties that do not include any kind of time limit. Especially in these cases – but also with repeated amnesties – tax payers might anticipate them. *Anticipated* tax amnesties, however, change the incentives for tax evasion prior to the implementation of the amnesty. This type of tax amnesty could work as an ‘insurance’ against an increase in enforcement efforts, leading to more and not less tax evasion over a longer time horizon.

In this study, we experimentally test whether this *insurance effect* actually affects people’s behavior, whether it is strong enough to lower tax compliance, and whether it makes tax amnesties less effective even when an increasing audit rate *ceteris paribus* provides an incentive to voluntarily disclose evaded taxes. The key advantage of the laboratory setting is that our experimental variations will allow us to clearly identify the behavioral relevance of the insurance effect, i.e., we will be able to disentangle this *incentive* effect from other potential explanations for lower compliance. Repeated tax amnesties may also have an effect on ‘tax morale’, i.e., *non-pecuniary* motivations to comply. From the occurrence of these amnesties, people may learn about the social norm of compliance, e.g., that others do not comply as frequently as they have previously thought. Of course, disentangling these effects is almost impossible in the field. Understanding the extent to which the incentive rationale – compared to norms – shapes tax compliance is important

for designing tax policies, as further discussed below.

We implement a standard tax declaration game following Alm et al. (2009): participants earn and declare an income and face a certain audit probability as well as a fine for evaded taxes in case they are caught. In this game, we precisely observe subjects' voluntary disclosures and the exact amount of their tax evasion. Three crucial features of our game are jointly responsible for the novelty of our approach. First, in parts of our experimental sessions, we implement a permanent tax amnesty. Hence, subjects can anticipate that there is the possibility of a voluntary disclosure in the future. Second, the audit rate in the experiment fluctuates such that tax amnesties can, in principle, be used as an insurance device against an increase in the audit rate. Third, we carefully vary the information condition of subjects so that using the amnesty as an insurance is only possible in some of our treatments. This 'indirect' manipulation has the core advantage that it allows for two crucial features of real-world tax compliance choices: repetition and learning. Unanticipated tax amnesties – needed as a control in a more direct manipulation – can, of course, not be easily repeated.

In the *currInfo* treatment, the current audit rate is announced and hence subjects might disclose evaded income in case of a high audit rate. In the *noInfo* treatment, subjects know that the audit rate fluctuates but they do not know the period-specific audit rate. Hence, they cannot use the tax amnesty as an insurance device. Finally, in the *pastInfo* treatment, subjects receive a noisy signal about the current audit rate. These treatments allow us to analyze whether an anticipated tax amnesty lowers compliance because of the *insurance effect* but they also enable us to investigate whether amnesties have indirect effects, i.e., influence the *social norm* of tax compliance in the experiment.

We find that under optimal information conditions (*currInfo*), the permanent tax amnesty lowers compliance by around 7-10%, whereas there is no effect in the absence of information (*noInfo* treatment). With noisy information (*pastInfo*), the effect lies in between. Hence, results suggest that the *insurance effect* is behaviorally relevant. Indeed, subjects make rationally use of the tax amnesty: they voluntarily disclose evaded income mainly after jumps in the audit rate. While we do not observe a general effect on tax morale, the timing of introducing the amnesty in different parts of the experimental session matters. Offering the amnesty already at the very beginning has a negative impact on compliance. Finally, we can neither establish a positive nor a negative significant effect of the tax amnesty on overall government or tax revenue. On the one hand, an increase in the audit rate provides an incentive to voluntarily disclose evaded income, increasing revenue. On the other hand, anticipating that the amnesty insures against such a rise induces some to comply less and we find heterogeneous behavior supporting this notion.

Our results suggest that a high predictability of tax amnesties makes them less effective because tax payers understand the economic rationale of insurance they provide. This effect seems to work completely independent of any indirect impact on social norms and

tax morale. It questions, in particular, the idea of *permanent* amnesties. Our research is relevant for the observation of recent increased pressure on tax havens (e.g. OECD’s harmful tax practices initiative, Elsayyad and Konrad 2012). Johannesen (2014) analyzes the effect of the so-called European Saving Directive – for which tax amnesties provide an instrument to allow for repatriation of undisclosed money – and finds substantial increases in EU-owned bank deposits in unaffected tax havens such as Macao and Panama. Similarly, Menkhoff and Miethe (2019) find that tax evaders adapt to established information exchange treaties by using new disguises to hide their true income. Such behavior is consistent with our observation that (some) tax payers are sophisticated enough to follow the underlying economic rationale involved in *anticipated* tax amnesties. Switching to other tax havens even when one expects increasing pressure on those places in the future is optimal when anticipating that one will still be able to voluntarily disclose one’s income, i.e., one is insured through an amnesty.

Related literature: Our paper contributes to the theoretical and empirical literature on tax compliance and tax amnesties (see Baer and Le Borgne 2008). The theoretical analysis begins with the seminal economics-of-crime model of tax compliance by Allingham and Sandmo (1972) which has been extended to discuss amnesties. A crucial insight from this literature is that tax amnesties might not change the tax evasion decision at all (Alm et al. 1990 and Stella 1991), unless other tax parameters, e.g. the audit rate, change (Macho-Stadler et al. 1999). In contrast, Andreoni (1991) has argued that tax amnesties might help to smooth consumption shocks and Malik and Schwab (1991) consider an adaptive utility framework in which uncertainty arises because subjects do not a priori know how much dis-utility they will derive from tax evasion. Similarly, Bayer et al. (2015) consider tax cheaters that become increasingly scared of a detection when the potential audit approaches. Such (behavioral) patterns can explain why people voluntarily disclose evaded taxes even when tax enforcement measures do not change. Another insight from this literature is that not all individuals have the same willingness to evade (Kleven et al. 2011) and this may relate to the moral cost of tax evasion, as modeled by Langenmayr (2017). Similar to our setting, the latter explains voluntary disclosures as a rational reaction to a varying detection probability taking tax payer heterogeneity into account. Both Bayer et al. (2015) and Langenmayr (2017) find empirical support for their prediction that (frequent) tax amnesties are associated with lower compliance in the field.

Relative to this literature, we see the distinguishing features of our study to be that we directly test the behavioral relevance of an incentive-driven insurance effect of tax amnesties. Utilizing the advantage of the lab, we are able to disentangle this effect from any impact amnesties may have on non-pecuniary motivations to comply, i.e., their impact on tax morale. As distinguishing these different mechanisms is almost impossible in the field, we complement the previous literature.

Only a few tax compliance experiments deal with tax amnesties and yield inconclusive

results.¹ Alm et al. (1990) find that the effect of a tax amnesty seems to depend on whether the amnesty is accompanied with stricter enforcement. Torgler and Schaltegger (2005) only find a positive effect of a tax amnesty on compliance in case subjects vote about the amnesty before, whereas Rechberger et al. (2010) find that compliance tends to increase after an amnesty (without voting or stricter enforcement). Finally, Canavire-Bacarreza et al. (2021) vary the duration of tax amnesties and find that increasing the length of an amnesty cannot outperform a one-time amnesty. Importantly, the focus of these studies is different from ours as they implement one-time (or at least non-permanent) amnesties (and audit rate sometimes does not fluctuate). The key element of our inquiry is to rigorously analyze whether subjects follow the *insurance rationale* of an anticipated amnesty in an environment in which enforcement fluctuates and in which repetition – an inherent feature of a permanent amnesty – allows subjects to learn, as common in the field. In this setting, we propose a novel variation of the information conditions to distinguish whether the amnesty can be used as insurance or not.

2 Theoretical background

Building on the basic model of tax evasion by Allingham and Sandmo (1972), we show in this section how anticipated tax amnesties can lower the level of tax compliance because they can be used as an insurance device against increasing audit rates. We follow Langenmayr (2017) in explaining different degrees of tax compliance with and without an amnesty by introducing a heterogeneous moral cost of tax compliance. Relative to her work, we tailor our model to suit our experiment.² The analysis presented in this section is nonetheless more simplified than our experimental game to highlight how the crucial underlying mechanisms play out in a very transparent environment.

We consider a tax game with two periods. Individuals gain a pre-tax income I for which they have to make a tax declaration in $t = 1$. In $t = 2$, they can make use of a tax amnesty and voluntarily disclose any evaded income. Individuals decide about tax evasion and a voluntary disclosure by maximizing their expected utility.³ We assume risk neutral individuals with linear utility for which it is never optimal to declare only a share of their income. Once income is declared, a tax rate of τ is applied. If it is not declared, individuals are at risk of being audited both at $t = 1$ and $t = 2$. This somewhat more complex feature reflects our experimental design. While the audit probability p_L is

¹For a recent survey of lab and field experiments on general tax compliance issues see Mascagni (2017). Recent lab contributions include e.g. Kessler et al. (2019) and Kamm et al. (2021).

²We abstract e.g. from any administrative costs of an amnesty that play a crucial role in her work and only focus on tax payers, not the strategic response of tax authorities. For another more sophisticated model than ours, see Bayer et al. (2015).

³While assuming risk-neutrality has the benefit of being in line with the idea that participants in tax experiments usually either fully comply or evade, some participants may, of course, be risk averse. While a model encompassing risk aversion is technically more complicated, the fundamental effects are, of course, very similar (see Langenmayr 2017 for such a model).

low in $t = 1$, it is uncertain in $t = 2$. With probability γ it is high p_H , with $p_H > p_L$. It remains low with probability $1 - \gamma$. This structure may e.g. mirror that there is a certain probability that offshore accounts become known due information leaks. It also provides a reason why some rational taxpayers make use of an amnesty. After learning what probability realized, individuals have the opportunity to declare their outstanding tax debt in $t = 2$. In case taxes are evaded, not disclosed voluntarily and an audit takes place, the evaded amount τI as well as an additional fine f have to be paid.

It is well-known that individuals have a different willingness to pay their taxes. Following Langenmayr (2017), we model this via a moral cost of tax compliance, $\alpha_i \in [0, A]$, that is heterogeneous among individuals. As will be seen below, those with very a low moral cost will always evade in equilibrium and those with a very high cost will always comply. Those in between may use the tax amnesty to disclose evaded taxes to adjust to the new deterrence level. Some of these people may only choose to evade taxes because the amnesty serves as an *insurance device* against the rise in the detection probability.

No Amnesty (NA): As a benchmark, consider the case without an amnesty. Individuals have to base their evasion (or compliance) decision on its benefits and costs, with the expected cost primarily determined by the expected detection probability, over the two periods of the game. With p_L individuals are already detected in $t = 1$.⁴ If this is not the case, they can still be detected in $t = 2$, with probability $\gamma p_H + (1 - \gamma)p_L$. Overall, the probability of being detected at least once is $\tilde{p} = p_L + (1 - p_L)(\gamma p_H + (1 - \gamma)p_L)$. Hence, the expected payoffs are:

$$EU_{NA}(\text{Comply}) = I - \tau I$$

$$EU_{NA}(\text{Evade}) = I - \tilde{p}(1 + f)\tau I - \alpha_i$$

Comparing these two expected utilities, we see that only individuals with a sufficiently low moral cost $\alpha_i < \alpha^{NA}$ will evade taxes (see Figure 1(a)):

$$\alpha^{NA} = \tau I(1 - \tilde{p}(1 + f)).$$

Evading is more attractive the higher the gain from it (τI) and less attractive the higher the expected cost from it ($\tilde{p}(1 + f)\tau I$). Here, we assume that tax evasion is attractive at least to some, as common in the literature: $\tilde{p}(1 + f) < 1$.

Amnesty: Voluntary disclosure implies that individuals report all income on the evaded taxes. As in the experiment, we abstract from any administrative costs or fines associated with a voluntary disclosure, implying that individuals get the same utility from disclosing or not in $t = 2$ in case they did not evade any taxes in $t = 1$.

$$EU_A(\text{Comply, Disclose}) = I - \tau I = EU_A(\text{Comply, Not Disclose})$$

⁴If individuals are already detected in $t = 1$, it effectively does not matter whether they are detected or not in $t = 2$. They already had to pay back evaded taxes and the fine in $t = 1$.

Of course, voluntary disclosure only makes a difference for individuals who have evaded taxes and can meaningfully disclose any tax debt. Following the structure of the setting, these latter individuals may have already been detected in $t = 1$ with probability p_L . With p_L , they earn $\pi_A^1 = I - (1 + f)\tau I - \alpha_i$. With $1 - p_L$, they have not been audited and their expected payoff depends on whether they choose to disclose in $t = 2$ or not:

$$\pi_A^2(\text{Evade, Disclose}) = I - \tau I$$

$$\pi_A^2(\text{Evade, NotDisclose}) = I - p_j(1 + f)\tau I - \alpha_i$$

where $p_j \in \{p_L, p_H\}$ is the detection probability drawn by nature in $t = 2$. Notably, we assume that $\tilde{p} < p_H$, i.e., the increase in the detection probability is substantial enough so that (some) individuals may reevaluate their evasion decision once a jump in the audit probability occurs. In addition, we assume that disclosing also rescinds the moral cost. Optimal behavior in this situation can be deduced via backward induction. Given that individuals have evaded taxes in first place but have not been audited in $t = 1$, how will individuals make their decision to disclose? With the help of an amnesty, individuals can condition their behavior on the realization of the audit probability. Whether an individual discloses or not, depends on the moral cost α_i . Disclosure is optimal for moral costs $\alpha_i \geq \alpha_j^A$ with $\alpha_j^A \in [\alpha_H^A, \alpha_L^A]$, with

$$\alpha_j^A = \tau I(1 - p_j(1 + f)).$$

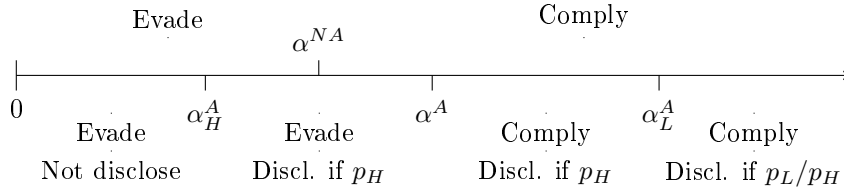
Put differently and as illustrated in Figure 1(b), those with $\alpha_i \geq \alpha_H^A$ will only disclose in case the audit probability is high, p_H , and those with $\alpha_i \geq \alpha_L^A$ will disclose even in case the audit probability is low. In other words, the cutoff is lower – more people use the amnesty – in case the probability is higher.

Given these disclosure decisions, what is the optimal decision in $t = 1$? If someone is not even disclosing when the detection probability is high p_H – i.e., moral costs are low $\alpha_i \in [0, \alpha_H^A]$ – it is optimal to evade in the first place, as $\tilde{p} < p_H$. If someone is even disclosing when the detection probability is low p_L – i.e., moral costs are high $\alpha_i \in [\alpha_L^A, A]$ – it optimal to comply in the first place, as $p_L < \tilde{p}$. For intermediate moral costs $\alpha_i \in [\alpha_H^A, \alpha_L^A]$, individuals disclose whenever the audit probability is high (p_H) but do not disclose when it is low (p_L). To decide whether it is optimal to comply or evade in $t = 1$, individuals compare the overall expected utility of both options taking into account that there is a chance of already being detected in $t = 1$:

$$EU_A(\text{Comply, Not Disclose/Disclose}) = I - \tau I \tag{1}$$

$$\begin{aligned} EU_A(\text{Evade, Disclose if } p_H) &= p_L \pi_A^1 + (1 - p_L) \pi_A^2 \\ &= p_L(I - (1 + f)\tau I - \alpha_i) + (1 - p_L)[\gamma(I - \tau I) + (1 - \gamma)(I - p_L(1 + f)\tau I - \alpha_i)]. \end{aligned} \tag{2}$$

(a) No Amnesty:



(b) Amnesty:

Figure 1: Equilibrium behavior and moral costs

In other words, when disclosing if p_H , the overall risk of being audited is naturally lower than when an amnesty – and hence disclosure – is not available, $p_L + (1 - p_L)(1 - \gamma)p_L < \bar{p}$. Similarly, the moral cost α_i does not apply when disclosing, i.e., it only applies with $1 - (1 - p_L)\gamma$ probability. Overall, these considerations imply that individuals with sufficiently low moral costs, $\alpha_i < \alpha^A$, will evade taxes:

$$\alpha^A = \tau I(1 - \bar{p}(1 + f)),$$

with $\bar{p} = \frac{p_L + (1 - p_L)(1 - \gamma)p_L}{1 - (1 - p_L)\gamma}$.⁵ As $p_L < \bar{p} < \tilde{p}$, it follows that $\alpha^{NA} < \alpha^A < \alpha_L^A$.⁶ In other words, some people with intermediate moral costs – with $\alpha_i \in [\alpha^A, \alpha_L^A]$ – decide to comply in $t = 1$, as they take into account that one cannot only be detected in $t = 2$ but also in $t = 1$. Of course, their potential disclosure would be immaterial. For others however – those with $\alpha_i \in [\alpha_H^A, \alpha^A]$ – the amnesty serves as an insurance device by allowing a disclosure conditional on the audit rate: they evade in $t = 1$ and disclose when the audit probability is high in $t = 2$. Notably, some of these *insurer* – with $\alpha_i \in [\alpha^{NA}, \alpha^A]$ – comply without an amnesty but are lured into evading by the amnesty and, then, only disclose if p_H . In other words, this type will reduce overall government or tax revenue (i.e., the sum of tax payments, penalties and amnesty payments).

Overall, these considerations imply that anticipated tax amnesties can serve as an insurance device against rising deterrence levels: tax compliance should decrease in the presence of such amnesties. In the extreme case that those being lured to evade by the amnesty are very frequent, it could actually lead to less overall tax revenue. In other words, the *insurance effect* may imply that a tax amnesty is even strictly counter-productive. Of course, those *insurer* – with $\alpha_i \in [\alpha_H^A, \alpha^{NA}]$ – that evade taxes with and without an amnesty but sometimes (if p_H) disclose in the presence of an amnesty may help to avoid that outcome, as they are predicted to increase tax revenue.

⁵Equating (1) and (2) gives: $\tau I[1 - (1 - p_L)\gamma - \{p_L + (1 - p_L)(1 - \gamma)p_L\}(1 + f)] = [p_L + (1 - p_L)(1 - \gamma)]\alpha_i \leftrightarrow \tau I[1 - (1 - p_L)\gamma - \{p_L + (1 - p_L)(1 - \gamma)p_L\}(1 + f)] = [1 - (1 - p_L)\gamma]\alpha_i \leftrightarrow \alpha_i = \tau I[1 - \frac{p_L + (1 - p_L)(1 - \gamma)p_L}{1 - (1 - p_L)\gamma}(1 + f)]$. Here, we use that $p_L + (1 - p_L)(1 - \gamma)p_L = 1 - (1 - p_L)\gamma$

⁶First: $p_L < \frac{p_L + (1 - p_L)(1 - \gamma)p_L}{1 - (1 - p_L)\gamma} \leftrightarrow p_L^2 < p_L$, as $1 - (1 - p_L)\gamma = p_L + (1 - p_L)(1 - \gamma)$. Second: $\bar{p} < \tilde{p} \rightarrow p_L + (1 - p_L)(1 - \gamma)p_L < p_L + (1 - p_L)(1 - \gamma)p_L + (1 - p_L)\gamma p_H - (1 - p_L)\gamma(\bar{p}) \rightarrow 0 < (1 - p_L)\gamma p_H - (1 - p_L)\gamma(\bar{p})$. The latter is true as we assume $\tilde{p} < p_H$.

Tax morale and compliance norms: Our treatments will vary the information conditions, allowing subjects to use the amnesty as an insurance device in some treatments but not others. The latter treatments will help to identify the effect of an amnesty on “tax morale” or the social norm of compliance in our experiment. Following Luttmer and Singhal (2014), we define tax morale to include non-pecuniary motivations for voluntary compliance that are outside the framework of expected utility maximization. A priori, the effect of a permanent tax amnesty on tax morale or compliance norms is unclear. On the one hand, tax amnesties could reduce compliance because tax evasion might become regarded as a trivial offense or people infer from them that fellow citizens seem to comply less than they expected. On the other hand, amnesties could increase compliance because getting caught might become less socially acceptable as it reveals that the tax evader wanted to evade until the point of getting caught and she cannot persuade others (and herself) that she wanted to become legal but there was no way. While the model presented before does not formally incorporate the impact of an amnesty on compliance norms or tax morale, a short-cut to think about them would be that the moral cost α_i could be directly influenced – positively or negatively – by whether there is an amnesty or not. After explaining the experimental design in the next section, I will come back to these considerations by formulating hypotheses in section 4.

3 Experimental design

To increase its ecological validity and to make it easier to understand for participants, our experiment implements a somewhat less stylized, richer design than the one described in the previous section. Nonetheless, the qualitative effects – in particular the insurance rationale – are the same. Subjects play our tax game twice for 30 periods each, once with and once without a tax amnesty. Using our basic tax game, we implement three treatments that vary the information subjects receive with respect to the audit rate and, thus, vary to what extent the tax amnesty can be used as an insurance device.

Tax game: Both for our settings with and without an amnesty, subjects learn their period income at the beginning of each period. This income fluctuates over time (as in Alm et al. 1992 and Alm et al. 1990) to avoid that subjects get bored. More precisely, the income is randomly drawn from the values 60, 70, 80, 90 and 100 which are all equally likely.⁷ Without a permanent tax amnesty, subjects are then asked to fill in their tax declaration, i.e., state their income of the current period which is then taxed at the rate of $\tau = 25\%$. Afterwards, the computer randomly determines whether an individual is

⁷In principle, a tax amnesty could also be used as an insurance against income shocks, as suggested by Andreoni (1991). We do not expect this explanation to play a role in our setting because we pay subjects all periods and we actually do not find any evidence that voluntary disclosure are used especially by subjects with bad luck regarding their income. Moreover, the *noInfo* treatment serves as a control for this kind explanation.

audited. Audited individuals have to pay taxes on the declared income and pay the evaded taxes of the *current* and the *previous three periods*. On top of that, there is a fine of 100% of the evaded taxes ($f = 1$). Without an audit, individuals only pay taxes on the declared income. At the end of each period, subjects are informed about whether or not they have been selected for an audit and see a period summary. The flow of the experimental game is illustrated in Figure C.1 and C.2 in online appendix C.

With a permanent tax amnesty, subjects can choose to voluntarily disclose tax debts after learning their period income. If they do so, they have to pay taxes on their current income plus the evaded taxes of the previous three periods but no penalty. In case of a future audit, these initially evaded taxes will be treated as if they were regularly paid. Introducing potential penalties not only for the last period but more previous periods has the following advantage. First, if there is uncertainty for more than one future period, it might be more easily understood by subjects that a tax amnesty could function as an insurance device. Second, this situation more closely reflects tax compliance in the field.

The audit rate fluctuates between $p_L = 2.5\%$ and $p_H = 25\%$. More precisely, starting with a low audit rate, the probability of an upward jump at the beginning of the next period is $\gamma = 15\%$. If the audit rate goes up, it remains there for 1 to 5 periods (each equally likely) and then falls back to the low level. This more complicated process was implemented to better analyze whether subjects use voluntary disclosure at the jump of the audit rate (and to facilitate our *pastInfo* treatment explained below). Of course, tax amnesties can still work as an insurance device. Given our parameters and starting with a low audit rate, the probability of being detected in the current or in the future three periods is about 23.5% (\tilde{p}).⁸ While risk-neutral individuals would, thus, fully evade, those with sufficiently high moral costs may comply. In the presence of an amnesty, subjects can ensure themselves against a rise in the audit probability. When individuals evade in the current period and the audit probability rises in the next one, it is even optimal for all risk-neutral subjects to use the voluntary disclosure because the overall detection probability increases to slightly above 50%.⁹ Of course, individuals constantly evading taxes will face an overall lower detection probability for their entire tax debt as some of it was evaded more than one period before.

Treatments: We manipulate the information subjects receive regarding the audit rate. In all treatments, subjects know that the audit rate fluctuates between a low and

⁸When a subject evades taxes in $t = 1$ in case of p_L , she will be detected with a probability of 0.025 in $t = 1$. In $t = 2$, the detection probability is $0.85 \cdot 0.025 + 0.15 \cdot 0.25 = 0.059$. In $t = 3$, the detection probability is $0.85(0.85 \cdot 0.025 + 0.15 \cdot 0.25) + 0.15(0.8 \cdot 0.25 + 0.2 \cdot 0.025) = 0.081$. In $t = 4$, the detection probability is $0.85(0.85(0.85 \cdot 0.025 + 0.15 \cdot 0.25) + 0.15(0.8 \cdot 0.25 + 0.2 \cdot 0.025)) + 0.15(0.8(0.75 \cdot 0.25 + 0.25 \cdot 0.025) + 0.2(0.85 \cdot 0.025 + 0.15 \cdot 0.25)) = 0.094$. Hence, overall, the probability of being detected is: $1 - (0.975 \cdot 0.94125 \cdot 0.9193125 \cdot 0.906403125) = 0.2353$.

⁹A subject evades taxes in $t = 1$. In $t = 2$, the audit probability rises to 0.25. Hence, in $t = 3$, the detection probability is $0.8 \cdot 0.25 + 0.2 \cdot 0.025 = 0.205$. In $t = 4$, the detection probability is $0.8(0.75 \cdot 0.25 + 0.25 \cdot 0.025) + 0.2(0.85 \cdot 0.025 + 0.15 \cdot 0.25) = 0.167$. Hence, the overall probability at $t = 2$ that evaded taxes will be detected is $1 - (0.75 \cdot 0.795 \cdot 0.83325) = 0.503$.

a high value. In the *currInfo* treatment, subjects are informed about the value of the audit rate in the current period. This information structure fully allows them to use the tax amnesty as an insurance instrument. In the *pastInfo* treatment, subjects get a noisy signal about the current audit rate or more precisely, they are informed about the audit rate of the previous period. This information structure partially allows subject to use the tax amnesty as an insurance instrument. In the *noInfo* treatment, subjects have no information regarding the actual audit rate, impeding any use of the amnesty as an insurance device. Individuals do neither know the current (or past period) audit rate nor the general level of the low and the high audit rate, only that the rate fluctuates between the two levels.

In principle, it would have been enough to only withhold the current (or past period) audit rate from subjects to make the tax amnesty useless as an insurance device. But we wanted to discourage subjects from using the *law of small numbers* (Kahneman and Tversky 1971) to incorrectly infer when to use the amnesty: following such a reasoning, subjects may expect to observe a jump to a high audit rate roughly every sixth period (15 percent). From not been audited e.g. in the last five periods, these subjects may infer that the audit rate had been low and that a jump of the audit rate is imminent. Not providing subjects with specific probabilities should make it very clear that the tax amnesty can not be used as an insurance instrument and discourage subjects from thinking in the way described above.¹⁰ Following Dai et al. (2015), such ambiguity might increase tax compliance. Nonetheless, the level of tax compliance will be different between treatments in any case because knowing the current audit probability has an impact on the compliance level even without a tax amnesty. We will control for this effect with the help of a regression analysis in our results section.

For all treatments, we use an AB/BA-design to control for order effects. The experiment consisted of 60 periods which were predetermined but unknown to the subjects. In one half (period 1-30 or period 31-60), subjects faced a permanent tax amnesty while in the other half, the game was a standard tax compliance game. Within treatment, we can, thus, test how a tax amnesty affects tax compliance and distinguish whether the amnesty is introduced in the first or last part of the experiment. The between-treatment comparisons allows us to identify whether any observed effect on compliance is really due to an insurance effect or whether an influence on social norms explains this effect. Importantly, a pure between-subject design could not determine whether it makes a difference to start with a tax amnesty or not and would hence potentially lead to false conclusions. Because tax amnesties are usually introduced to an ongoing tax system in the field, it seems important also to look at the case in which the tax amnesty is introduced in the second part of the experiment.

¹⁰Results show however that elements of this reasoning might even be present when specific probabilities are unknown.

There is no consent about what to do with the tax revenue in the experimental literature. Some studies implement a public good structure (e.g. Gerxhani and Schram 2006), in others the money goes back to the experimenter (e.g. Alm et al. 2009), and in a few studies the money is donated e.g. to the red cross (Doerrenberg and Duncan 2014). In our experiment, all tax revenue and penalties were transferred to the *Bundeskasse*, flowing into the German federal budget. Hence, as in the field, the tax revenue goes to the government budget. To ensure credibility, subjects were provided with a copy of the proof of payment by email after the experiment. Arguably, however, the choice of what to do with the tax revenue is less important in our experiment. We are not interested in the overall level of tax compliance but whether subjects actually make use of tax amnesties as instrument of insurance, understanding the underlying economic rationale.

Procedures: 120 subjects participated in the experiment which took place at the mLab at Mannheim University. Due to little and conflicting prior evidence, power calculations were somewhat limited but suggested that 40 subjects per treatment would be sufficient as – due to our implementation of the tax game – each subject represents one statistically independent observation. For each treatment, 20 subjects faced the permanent tax amnesty in the first half and 20 subjects faced it in the second half. After the experiment, measures of risk aversion were collected, as in Holt and Laury (2002). The entire experiment was programmed with zTree (Fischbacher, 2007) and recruitment was done with ORSEE (Greiner, 2004). The 120 subjects split up into 12 laboratory sessions with 8-12 participants each. The average duration was 75 minutes and the participants earned 11.85 Euro on average.

4 Hypotheses

Our treatments vary the amount of information subjects have when declaring their taxes. We expect the following ranking between treatments in the *level* of tax compliance, C , (with and without a tax amnesty): $C_{noInfo} > C_{pastInfo} > C_{currInfo}$. In *noInfo*, subjects face an ambiguous audit rate, potentially increasing their compliance level (Dai et al. 2015). In the other two treatments, more information potentially leads to less compliance because subjects can condition their decision on this information. Additionally, the low audit rate is more likely than the high audit rate, intensifying the effect.

For the main purpose of our study, the level of tax compliance is however unimportant.¹¹ The question is whether subjects use the tax amnesty as an insurance device against a rise in the detection probability, decreasing tax compliance. Of course, this incentive effect should only happen if the information condition allows for using said

¹¹If anything, observing higher compliance rates in *noInfo/pastInfo* would provide even more scope for an amnesty to lower compliance in these treatments than in *currInfo*, biasing against observing our central prediction.

amnesty as insurance, as in *currInfo*. While this *insurance effect* is eliminated in *noInfo*, this treatment serves as a control for a potential impact of an amnesty on non-pecuniary motives to pay taxes ('compliance norms'). As argued in section 2, this impact might go in either direction, therefore:

Open Question 1: In *noInfo*, is there an effect of a permanent tax amnesty on tax compliance and if so, in which direction?

Relative to this baseline, we expect the *insurance effect* to work better the more information subjects have. In *pastInfo*, subjects only receive information about the previous period's audit rate. Therefore, the tax amnesty is not a perfect insurance against a jump in the audit rate and tax compliance should fall less than in *currInfo*.

Hypothesis 1a: In *currInfo*, a permanent tax amnesty lowers tax compliance, at least compared to *noinfo*.

Hypothesis 1b: In *pastInfo*, if there is a negative effect of an amnesty – compared to *noinfo* – its magnitude is smaller than in the *currInfo* treatment.

An additional way to examine the theoretical considerations is to analyze whether and under which conditions subjects decide in favor of a voluntary disclosure. Following the reasoning above, we expect the following patterns in the data:

Hypothesis 2a: In *currInfo*, there is a positive number of voluntary disclosures and they are triggered by an upward jump of the audit rate.

Hypothesis 2b: In *pastInfo*, there are less disclosures than in *currInfo* and they are triggered by an upward jump of the previous period's audit rate.

Moreover, in *noInfo*, we do not expect any voluntary disclosures because of the *insurance effect*. Nonetheless, some disclosures might still be observed either due to an experimenter demand effect or because subjects follow a *law-of-small-number* reasoning:

Hypothesis 2c: In *noInfo*, there are less voluntary disclosures than in *pastInfo* and *currInfo*.

Lastly, an empirically interesting question is whether and how a permanent tax amnesty influences total tax revenue, i.e., tax payments plus penalties and amnesty payments. As argued in section 2, p_H provides an incentive to voluntarily disclose evaded taxes, implying potentially higher revenue (at least in *currInfo* and *pastInfo*). The *insurance effect* might, however, undermine such an outcome. In the extreme case, revenue could fall due to an amnesty.

Open Question 2: Does an anticipated tax amnesty have an effect on total government revenue? Is a tax amnesty less effective due to those who use it as an insurance device?

5 Experimental results

In this section we will present the experimental results. First, we will give an overview and analyze how compliance changes when a tax amnesty is introduced (Sec. 5.1). Afterwards, we will look at voluntary disclosures and how they vary across treatments (Sec. 5.2). Finally, the impact of a tax amnesty on tax revenue is investigated (Sec. 5.3).

5.1 Overview and tax compliance

Table 1 provides summary statistics by treatments (for other variables see also Table A.2 in the appendix). It shows that average tax compliance decreases the less information about the actual audit rate is provided, as conjectured in section 4. Here, the *compliance rate* is defined as declared income over true income. Nonetheless, the amount of evasion remains non-negligible in all treatments. On average, at least 1/3 of subjects have a positive tax debt, i.e., evaded at least some amount in the last three periods in every treatment (*Sub. with tax debt*). The main question, whether and in which information settings an anticipated tax amnesty lowers compliance cannot be clearly answered based on this table due to differences in the audit rate that is a random process. Note, however, that both for *pastInfo* and *currInfo*, the mean compliance rate is slightly lower with an anticipated amnesty even though the mean audit rates are slightly higher for this case.¹² For *noInfo*, the compliance rate is even slightly higher with an amnesty. In other words, the table's data shows a tendency in the direction of Hypotheses 1.

To formally analyze the data, Table 2 provides a regression analysis controlling for the audit rate, for the information conditions of different treatments, and for order effects. Following Alm et al. (2009), we use generalized least squares (GLS) random effects estimations that make use of the panel structure of our data (Spec. 1-3). Spec. 4/5 provide first controls (that are further extended in online appendix B). Following Gerxhani and Schram (2006), Spec. 4 reports linear probability estimations in which the dependent variable is no longer the *compliance rate* but *full compliance*, which is 0 for a *compliance rate* < 1 and 1 for a *compliance rate* ≥ 1 . As usual for tax experiments, we observe a very high proportion of full evasion and full compliance (see Figure A.1). Spec. 5 provides a fixed effects regression.

In all regressions, we include the following set of explanatory variables. First, tax compliance is expected to (negatively) depend on the period *Income*, a subject's *Wealth* (accumulated earnings of the individual), and a subject's *Tax debt* (accumulated evaded income over the last three periods). Second, compliance will also depend on the audit rate (*High audit rate*) as well as on the past experience of audits and voluntary disclosures

¹²Wilcoxon signed rank tests (that do not control for the audit rate) show a (marginally) significant difference between the amnesty and the no-amnesty case for *currInfo* (compliance rate: $p = 0.078$, Sub. with tax debt: $p = 0.100$), whereas the difference for the other treatments is insignificant.

Table 1: Summary statistics by treatment

Treatment	Total (60 periods)	With amnesty (30 periods)	Without amnesty (30 periods)
currInfo	Subjects = 40	Subjects = 40	Subjects = 40
	Mean compliance rate = 0.545	Mean compliance rate = 0.530	Mean compliance rate = 0.560
	Sub. with tax debt = 0.360	Sub. with tax debt = 0.353	Sub. with tax debt = 0.368
	Mean audit rate = 0.094	Mean audit rate = 0.101	Mean audit rate = 0.087
	Mean perceived audit rate = 0.103 (median =0.1)	Voluntary disclosures = 65	
pastInfo	Subjects=40	Subjects=40	Subjects=40
	Mean compliance rate = 0.702	Mean compliance rate = 0.686	Mean compliance rate = 0.718
	Sub. with tax debt = 0.408	Sub. with tax debt = 0.416	Sub. with tax debt = 0.400
	Mean audit rate = 0.095	Mean audit rate = 0.096	Mean audit rate = 0.094
	Mean perceived audit rate = 0.109 (median =0.1)	Voluntary disclosures = 36	
noInfo	Subjects=40	Subjects=40	Subjects=40
	Mean compliance rate = 0.765	Mean compliance rate = 0.769	Mean compliance rate = 0.761
	Sub. with tax debt = 0.539	Sub. with tax debt = 0.551	Sub. with tax debt = 0.527
	Mean audit rate = 0.088	Mean audit rate = 0.084	Mean audit rate = 0.092
	Mean perceived audit rate = 0.126 (median =0.1)	Voluntary disclosures = 45	

(*Lag audit, Lag vol. disclosure*). For these variables, later specifications add treatment specific dummies. Our main variable of interest is *Amnesty*, which is 1 if the respective period falls into the half of the experiment in which the permanent tax amnesty takes place. Later specifications add the corresponding dummies *Amnesty*currInfo*, *Amnesty*pastInfo*, and *Amnesty first half* which is a dummy variable indicating whether the permanent tax amnesty took place in the first half of the experiment or not. Finally, some specifications also add controls for learning over periods as well as subject characteristics (age, gender, risk aversion, experience with preparing a tax declaration in real life).

Income, *Wealth* and *Tax Debt*¹³ are negatively correlated with compliance, in line with previous studies (e.g. Alm et al. 2009). As expected, a *High audit rate* leads to more compliance, in particular when the current audit rate is known (*currInfo*). The experience of an audit or a voluntary disclosure in the past period leads to a decrease of the current compliance rate, somewhat independent from the treatment. Of course, both incidents lead to a tax debt of zero, potentially motivating some subjects to evade more taxes in the current period. Concerning subjects characteristics we, reassuringly, find that those participants with real-world experience of preparing a tax return do not behave differently than those lacking this experience, hinting at some external validity of our approach. As found in previous studies, higher risk aversion increases tax compliance and males have a tendency to evade more taxes.

Regarding the primary variables of interest, Spec. 1 shows a negative effect of the

¹³Note, that not including *Wealth* (due to the correlation with periods) or including subject's accumulated earnings only of the last three periods as a substitute leads to very similar results.

Table 2: Estimation of *Compliance rate* / *Full compliance*

	Spec. 1 GLS Comp. rate	Spec. 2 GLS Comp. rate	Spec. 3 GLS Comp. rate	Spec. 4 Lin. Prob. Full comp.	Spec. 5 OLS-FE Comp. rate
Income	-0.0023*** (0.0005)	-0.0024*** (0.0005)	-0.0024*** (0.0005)	-0.0037*** (0.0005)	-0.0026*** (0.0005)
Wealth	-0.0000** (0.0000)	-0.0000*** (0.0000)	-0.0000*** (0.0000)	-0.0000 (0.0000)	-0.0000 (0.0000)
Tax Debt	-0.0099*** (0.0006)	-0.0097*** (0.0006)	-0.0089*** (0.0006)	-0.0016** (0.0007)	-0.0015** (0.0007)
Amnesty	-0.0144* (0.0084)	0.0052 (0.0101)	0.0056 (0.0105)	0.0285 (0.0203)	0.0124 (0.0166)
Amnesty * currInfo		-0.0450** (0.0196)	-0.0468** (0.0202)	-0.0838*** (0.0361)	-0.0614** (0.0287)
Amnesty * pastInfo		-0.0318* (0.0175)	-0.0334* (0.0184)	-0.0465 (0.0348)	-0.0439 (0.0283)
Amnesty first half		-0.0287 (0.0235)	-0.0458* (0.0237)	-0.1113** (0.0526)	
High audit rate	0.1308*** (0.0215)	-0.0352** (0.0167)	-0.0380** (0.0165)	0.0078 (0.0161)	-0.0443*** (0.0167)
High audit rate * currInfo		0.3797*** (0.0414)	0.3812*** (0.0409)	0.3968*** (0.0489)	0.4001*** (0.0433)
High audit rate * pastInfo		0.1162*** (0.0305)	0.1182*** (0.0303)	0.1019*** (0.0305)	0.1373*** (0.0306)
Lag audit	-0.2413*** (0.0271)	-0.2474*** (0.0563)	-0.2317*** (0.0550)	-0.1361*** (0.0510)	-0.1612*** (0.0473)
Lag audit * currInfo		-0.0646 (0.0672)	-0.0621 (0.0658)	0.0156 (0.0609)	0.0362 (0.0564)
Lag audit * pastInfo		0.0851 (0.0697)	0.0779 (0.0680)	0.1202* (0.0644)	0.1172** (0.0563)
Lag vol. discl.	-0.1799*** (0.0581)	-0.1996** (0.0884)	-0.1957** (0.0870)	-0.1889** (0.0753)	-0.1749** (0.0804)
Lag vol. discl. * currInfo		-0.1278 (0.1227)	-0.0950 (0.1193)	0.1019 (0.1036)	0.0960 (0.0997)
Lag vol. discl * pastInfo		0.1595 (0.1338)	0.1762 (0.1301)	0.2076** (0.1032)	0.1856* (0.1057)
currInfo	-0.1095*** (0.0297)	-0.2099*** (0.0341)	-0.2278*** (0.0346)	-0.2818*** (0.0744)	
pastInfo	-0.0368 (0.0277)	-0.0379 (0.0307)	-0.0458 (0.0324)	-0.0555 (0.0791)	
Constant	1.0840*** (0.0388)	1.1316*** (0.0385)	0.9569*** (0.0802)	0.5998*** (0.1721)	0.9180*** (0.0380)
Control Periods	No	Yes	Yes	Yes	Yes
Control Sub. Characteristics	No	No	Yes	Yes	-
N	7080	7080	7080	7080	7080
Subjects	120	120	120	120	120
R ² overall (FE within)	.2658	.3001	.3158	.1548	.1177

The dependent variable is the compliance rate (declared income over true income) of full compliance per subject and period. Cluster-robust standard errors (subject level) are provided in parentheses. Subject characteristics include age, sex, risk aversion and experience with preparing a tax declaration in real life. ***, ** and * indicate significance at the 1%, 5% and 10% level.

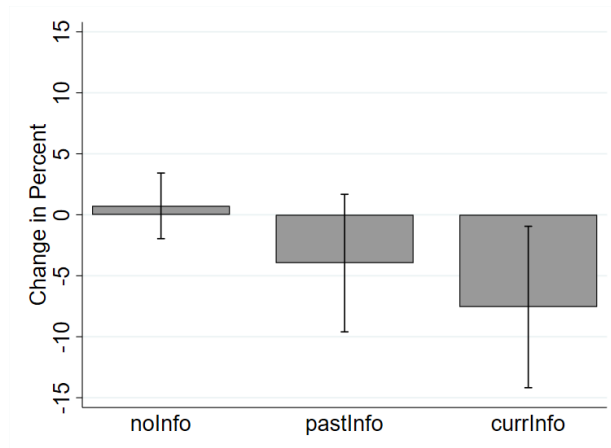


Figure 2: Estimated percentage change in compliance over treatments relative to mean compliance (main specification)

amnesty on compliance. Spec. 2-5 reveal that this effect is driven by *currInfo* – and to an extent – by *pastInfo*. In other words, while we do not observe a significant effect of an amnesty on compliance when the information condition prohibits that the amnesty can be used as an insurance device, its effect is stronger the better the information condition allows using it as an insurance device. Notably, whenever we find a significant effect of the amnesty relative to latent *noInfo* treatment, the overall effect in that treatment is also significant: we do not only find significant evidence of an *insurance effect* in *currInfo* – an partly in *pastInfo* – but also that this effect significantly lowers compliance in that treatment (and not only relative to *noInfo*). In *currInfo*, the tax amnesty decreases compliance by 4.0-5.5 percentage points ($Amnesty + Amnesty * currInfo$). In relation to the mean compliance rate of 54.5% in this treatment, this corresponds to a decrease in compliance of about 7-10%. Figure 2 shows this estimated change in compliance in percentage for all treatments, based on our main specification (Spec. 3).

Our design also allows for the analysis of order effects. Introducing the tax amnesty at the beginning of the experiment has a significant negative effect ($Amnesty\ first\ half$).¹⁴ Apparently, deliberating about the compliance decision in the presence of an amnesty has a negative effect consistent with the idea that it might affect the social norm of compliance. Once subjects have thought about evading taxes or not without an amnesty, it seems not to change these deliberations.¹⁵ Finally, online appendix B looks at heterogeneity

¹⁴Additionally, splitting this dummy up for the two parts of the experiment (with and without the amnesty - not shown in Table 2) shows that this negative effect is persistent. Average compliance rates are also lower in the second part of the experiment if the tax amnesty has been introduced at the beginning. Table B.2 additionally shows that the social norms effect seems to be driven by behavior in the *noInfo* and the *currInfo* treatment.

¹⁵This observation seems to be in line with a finding of Alm et al. (1990): When their subjects know at the beginning of the experiment that there will be a tax amnesty, average compliance is lower than when they are unaware of this fact. Alm et al. (1990) call their finding an *anticipation effect*. It is important to note, that we observe two different kinds of anticipation effects in our experiment. One corresponding to whether the amnesty was introduced in the first period reducing tax morale and another one corresponding to whether subjects follow the incentives of the amnesty, using it as insurance against

Table 3: Voluntary disclosures by treatment

Treatment	Audit rate properties (amnesty periods)	Voluntary disclosures
currInfo	Mean audit rate = 0.101	Total = 65
	Number of upward jumps = 143	At upward jump = 46
		During high audit rate = 57
		During low audit rate = 8 (= time-inconsistent)
		Mean tax debt = 37.0
pastInfo	Mean audit rate = 0.096	Total = 36
	Number of upward jumps = 131	At upward jump (t-1) = 21
		During high audit rate (t-1) = 27
		During low audit rate (t-1) = 9 (= time-inconsistent)
		Mean tax debt = 31.2
noInfo	Mean audit rate = 0.084	Total = 45
	Number of upward jumps = 119	(= time-inconsistent)
		Mean tax debt = 16.0

in behavior and provides some evidence that the amnesty’s negative effect in *currInfo* appears to be driven by those participants ‘at the margin’ of complying or evading, as suggested by our theoretical considerations. When the information is right, in particular these individuals appear to be lured into evading by the permanent amnesty.

Result 1: *While a tax amnesty does not per se reduce compliance, under optimal information, subjects reduce tax compliance by about 7-10% in line with an insurance effect, supporting Hypothesis 1a/b. In addition, introducing an amnesty from the start seems to weaken overall tax compliance.*

5.2 Voluntary disclosure

Next, we will evaluate Hypotheses 2a-c. Table 3 shows descriptive evidence about voluntary disclosures. In *currInfo*, 65 voluntary disclosures among 40 subjects are observed, and 46 of them take place directly after an upward jump in the audit rate, as suggested by Hypothesis 2a. Moreover, in *pastInfo*, 36 voluntary disclosures are observed, among which 21 occurred if there was an upward jump in the previous period. Surprisingly, there is also a large number of voluntary disclosures in *noInfo*: 45, even more than in *pastInfo*.¹⁶

Nonetheless, a regression analysis is required to control for differences in the audit rate process. Table 4 provides poisson regressions for which the dependent variable is the number of voluntary disclosures by subject, reducing the number of observations to one per subject. All regressions control for subjects’ characteristics. Spec. 1 uses *Mean auditrate*,

a rise in the audit rate.

¹⁶Notably, all voluntary disclosures considered here are material in the sense that they involve strictly positive back tax payments.

Table 4: Estimation - *Number of vol. disclosures*

	Spec. 1	Spec. 2	Spec. 3
	Poisson	Poisson	Poisson
pastInfo	-0.5284* (0.2883)	-0.5162* (0.2883)	-0.4778* (0.2853)
noInfo	-0.2038 (0.3284)	-0.2065 (0.3237)	-0.2403 (0.3330)
Mean auditrates	9.5099* (4.9734)		
Log mean auditrates		0.8601* (0.5144)	
Log jumps			0.6319** (0.2908)
Amnesty first half	0.0614 (0.2648)	0.0605 (0.2628)	0.0009 (0.2604)
Constant	0.0868 (0.9112)	3.0340* (1.7720)	0.3171 (0.8910)
Control Sub. characteristics	Yes	Yes	Yes
N	120	120	119
pseudo R^2	.0705	.0697	.0755
Log-Likelihood	-199.2	-199.4	-197.1

The dependent variable are the number of voluntary disclosures per subject. Robust standard errors are provided in parentheses. Subject characteristics include age, sex, risk aversion and experience with preparing a tax declaration in real life. ***,** and * indicate significance at the 1%, 5% and 10% level.

Spec. 2 *Log mean auditrates*, Spec. 3 *Log jumps* as an explanatory variable. When moving from *currInfo* to *pastInfo*, the number of voluntary disclosures decreases by 38 to 41 percent, ceteris paribus.¹⁷ From *currInfo* to *noInfo*, disclosures decreases by 19 to 21 percent, although this difference is not significant.

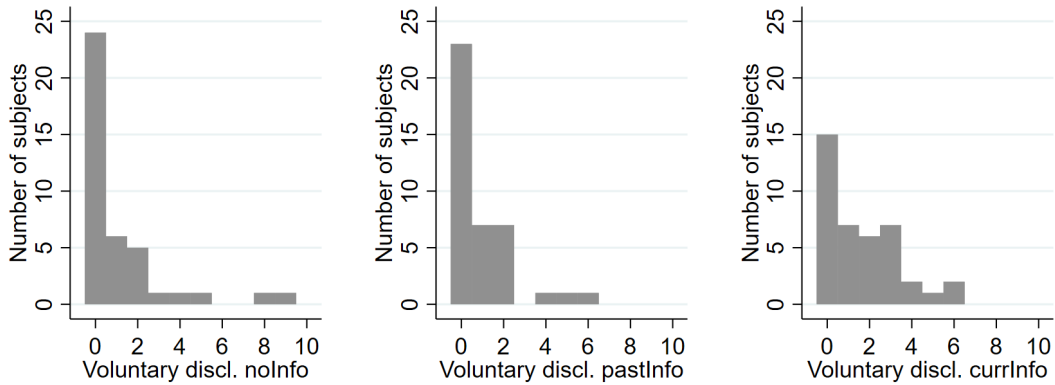


Figure 3: Number of voluntary disclosures per subject

Figure 3 shows how the numbers of voluntary disclosures are distributed across treatments and helps to explain our findings. In *noInfo* and *pastInfo*, 16 or 17 subjects (out of 40) decide to voluntarily disclose at least once. There are, however, two outliers in *noInfo* who exhibit 8 and 9 voluntary disclosures, partially explaining the high number of disclosures in this treatment. In *currInfo*, 25 out of 40 people decide for a voluntary

¹⁷ $\exp(-0.4778) = 0.620$; $\exp(-0.5284) = 0.590$.

Table 5: Estimation results for *Voluntary disclosure*

	Spec. 1 Lin. prob.	Spec. 2 Probit	Spec. 3 Logit
Income	-0.0001 (0.0002)	0.0009 (0.0037)	0.0004 (0.0073)
Wealth	-0.0001* (0.0000)	-0.0014* (0.0008)	-0.0026* (0.0016)
Tax debt	0.0017*** (0.0003)	0.0244*** (0.0038)	0.0491*** (0.0081)
High audit rate	-0.0077 (0.0114)	-0.1492 (0.2496)	-0.3735 (0.5404)
High audit rate * currInfo	0.0545*** (0.0168)	1.1131** (0.4507)	2.4712** (1.0505)
High audit rate * pastInfo	0.0185 (0.0212)	0.3272 (0.4205)	0.5432 (0.9399)
High audit rate-Jump	0.0214 (0.0190)	0.3169 (0.2783)	0.7066 (0.5511)
High audit rate-Jump * currInfo	0.2298*** (0.0618)	0.9514** (0.3712)	1.6695** (0.7425)
High audit rate-Jump * pastInfo	-0.0367 (0.0313)	-0.5471 (0.6834)	-0.9762 (1.5432)
High audit rate-Lag jump	-0.0118 (0.0142)	-0.1843 (0.3509)	-0.4892 (0.7101)
High audit rate-Lag jump * currInfo	0.0392 (0.0261)	0.5979 (0.4828)	1.4082 (1.0010)
High audit rate-Lag jump * pastInfo	0.1499*** (0.0377)	1.5423*** (0.4839)	3.3200*** (1.0436)
Amnesty period	0.0041 (0.0028)	0.0621 (0.0478)	0.1195 (0.0949)
Amnesty period * currInfo	0.0025*** (0.0009)	0.0373** (0.0166)	0.0629* (0.0337)
Amnesty period * pastInfo	0.0009 (0.0007)	0.0085 (0.0135)	0.0225 (0.0295)
currInfo	-0.0886*** (0.0235)	-1.5653*** (0.5240)	-3.1083*** (1.1933)
pastInfo	-0.0414** (0.0209)	-0.7255* (0.3846)	-1.5712* (0.8718)
Amnesty first half	-0.1614* (0.0850)	-2.5727* (1.4839)	-4.9043* (2.9717)
Constant	0.2128** (0.0952)	0.7601 (1.7030)	1.6310 (3.4544)
Control period	Yes	Yes	Yes
Control Sub. characteristics	Yes	Yes	Yes
N	3480	3480	3480
Subjects	120	120	120
R ² overall	.1256		
Log-Likelihood		-413.4	-413.4

The dependent variable is Voluntary disclosure (0 or 1) per subject and period. Cluster-robust standard errors (subject level) are provided in parentheses. Subject characteristics include age, sex, risk aversion and experience with preparing a tax declaration in real life. ***, ** and * indicate significance at the 1%, 5% and 10% level.

disclosure, significantly more than in the other two treatments (Wilcoxon rank sum, for both comparisons: $p < 0.05$). Despite the surprising number of disclosures in *noInfo*, the use of the tax amnesty seems to be structurally different between *pastInfo/currInfo* and the former treatment. Table 3 shows that the average back tax per voluntary disclosure is roughly twice as large in *pastInfo/currInfo* compared to *noInfo* (Wilcoxon rank sum, for both comparisons $p < 0.01$), suggesting that the tax amnesty is used more ‘rationally’ in the former two treatments.

This conjecture is also supported by the regressions of Table 5 that analyze when individuals choose to voluntarily disclose their evaded income. Linear probability, probit and logit estimations are utilized, where the dependent variable is 1 or 0, i.e., whether or not a subject decides for a voluntary disclosure in a given period. Besides some familiar regressors from Table 2, *High audit rate – Jump* and *High audit rate – Lag jump* as well as corresponding treatment interactions are included in the regressions to make apparent under which conditions subjects choose to disclose evaded income. These variables are only 1 when the audit rate is high and this rate has been low in the last (or next-to-last) period. As expected, a high audit rate does not per se lead to voluntary disclosures (expect for *currInfo*). Large effects are however observed for a jump of the current audit rate in *currInfo* and the for a lagged upward jump in *pastInfo*. Unsurprisingly, this type of rational behavior is not – or cannot be – observed in *noInfo*, the latent variable. Additional analysis in the online appendix B provides evidence that this behavior is again largely driven by those ‘at the margin’ of complying or evading.

Result 2: *In support of Hypothesis 2a-b, voluntary disclosures are triggered by an upward jump of the current or the previous-period audit rate in currInfo and pastInfo. There are less disclosures in pastInfo than in currInfo, but surprisingly many in noInfo, contradicting Hypothesis 2c.*

As discussed in the introduction, several explanations (e.g. consumption smoothing, realization of interest gains) may exist why people voluntarily disclose tax debts even though the audit rate has not changed or subjects have at least no knowledge of it. Nonetheless, the question is how relevant they are in the lab. Potentially, subjects simply follow a mis-perception as suggested by the *law of small numbers*. Subjects who evade income for several periods in *noInfo* and are not detected might infer that the audit rate was low in these period. They may conjecture that a jump in the audit rate is more likely when the audit rate has been low for a long even so they do not know the exact probabilities, implying that they make use of a voluntary disclosure. As indicated before, these disclosures are, however, structurally different from the ones in *currInfo/pastInfo* as they e.g. imply less back taxes.

Table 6: Estimation results for *Revenue share (with and without penalties)*

	Spec. 1 currInfo with penalties	Spec. 2 currInfo without pen.	Spec. 3 pastInfo with penalties	Spec. 4 pastInfo without pen.	Spec. 5 noInfo with penalties	Spec. 6 noInfo without pen.
Average auditrate	0.7643*** (0.2752)	0.5067*** (0.1873)	0.5284*** (0.1610)	0.2101 (0.1861)	0.2574* (0.1549)	-0.2299 (0.1503)
Amnesty	0.0120 (0.0103)	0.0103 (0.0078)	-0.0020 (0.0100)	0.0028 (0.0080)	0.0075 (0.0061)	0.0079 (0.0059)
Amnesty first half	-0.0090 (0.0107)	-0.0176 (0.0195)	0.0102 (0.0110)	0.0011 (0.0203)	-0.0082 (0.0090)	-0.0203 (0.0152)
Constant	0.1292*** (0.0274)	0.1045*** (0.0195)	0.1684*** (0.0168)	0.1592*** (0.0254)	0.2094*** (0.0162)	0.2218*** (0.0144)
N	80	80	80	80	80	80
Subjects	40	40	40	40	40	40
R^2 overall	.2048	.1107	.1053	.0088	.0776	.0377

The dependent variable is the revenue share (total taxes, penalties and amnesty payments over total income) per subject, separately calculated for periods with and without permanent tax amnesty. Cluster-robust standard errors (subject level) are provided in parentheses. ***,** and * indicate significance at the 1%, 5% and 10% level.

5.3 Overall tax revenue

Finally, we will address Open Question 2, i.e., investigate the effect of the permanent tax amnesty on total tax or government revenue. Our main focus will be on the *revenue share* which depicts the sum of tax payments, penalties and amnesty payments over total income. We use a relative measure as income is a random process.¹⁸ Table A.1 provides this share alongside with some other shares: the *compliance share* e.g. measures total taxes over income. Although the table's shares reflect some of the results described up to now it is misleading to the extent that it does not control for the audit rate.¹⁹

The regressions presented in Table 6 provides such an analysis. Here, the dependent variable is the revenue share, calculated only twice for every subject, for each half of the experiment. As this sharply reduces the number of observations, we provide simple treatment-specific estimations which regress the dependent variable on the mean audit rate per subject and half, the amnesty dummy (which is 1 for the half with the permanent tax amnesty and 0 else), a dummy whether the amnesty was introduced first, and a constant.²⁰ In addition, as the randomness of the penalty process might cover subtle effects, we include specifications with and without penalties. We cannot establish a significant effect of the amnesty in any treatment. For the *currInfo* and the *noInfo* treatment the coefficient is positive whereas it is mixed for the *pastInf* treatment. Of course, as conjectured in

¹⁸Alternatively, one could divide tax/amnesty payments and penalties by the amount of tax payments due if everyone complied fully, which is 25% of total income. In other words, for this measure, our coefficients simply have to be multiplied by four.

¹⁹For the *currInfo* treatment, the fine share e.g. is higher with an amnesty than without although many subject use the amnesty to avoid being audited, reflecting that the average audit rate was higher with an amnesty.

²⁰Due to the limited N in the estimations, we include only very few regressors but specifications that also include demographic variables or that fixed effects instead of random effects estimations lead to very similar results and are not presented here. In addition, Table B.4 in the online appendix replicates the basic results gained so far regarding the compliance rate with similar specifications. We observe a negative effect of the amnesty in *currInfo* and *pastInfo* (but not in *noInfo*). This effect is however only significant in *currInfo*.

section 2, two different forces might be at play: some individuals evading taxes with and without an amnesty use voluntary disclosures as mechanism to adopt to a higher audit rate, increasing tax revenue. Others may only be tempted to evade taxes due to the amnesty but only sometimes disclose their tax debt, reducing tax revenue. Online appendix B provides some – albeit weak – evidence of such heterogeneous effects.

***Result 3:** We cannot establish a significant increase in the revenue share due to the tax amnesty in any of our treatments. This is in line with mixed effects of an amnesty on different people for which we find some evidence.*

6 Conclusion

In this study, we analyze the effectiveness of permanent or anticipated tax amnesties. We focus on the case that amnesties are accompanied by increased enforcement efforts. In such an environment, tax amnesties may serve as an insurance device against a rise in the detection probability, providing incentives to comply less. Our contribution is to analyze the behavioral relevance of these incentive structures in the lab where we can control for other mechanisms, in particular, effects on non-pecuniary motives to comply, i.e., compliance norms. Our treatments vary the information subjects receive about the fluctuating audit probability. While the amnesty can be used as an insurance device under optimal information, this is not possible under no information. We find that this variation has a causal impact on how the amnesty is used. Under optimal information, compliance decreases significantly – both economically and statistically – providing clear evidence of the behavioral relevance of the *insurance effect*. While fiscal needs make it difficult for tax authorities to restrain themselves from using amnesties, a high predictability of tax amnesties potentially comes at a high price.

Reassuringly, we find no difference in behavior between participants with real-world experience of filling in tax returns and those without them, providing some support for the external validity of our key result that (some) tax payers understand the incentives a tax amnesty provides. In the field, tax evasion regarding capital income often comes along with an accumulated stock of capital which is hidden in a foreign country. Our experiment, however, does not reflect that the detection probability in (or for) different countries can be heterogeneous. As indicated in the introduction, if pressure on tax heavens does not lead to a uniform increase in the detection probability across countries, transfers from one tax heaven with a high detection probability to another one with a low probability can be attractive. Of course, not all citizens may think or foresee the possibility/necessity of such transfers between tax heavens. The risk is that at least some tax payers may only be lured into evading by the anticipation of an amnesty. Once their tax heaven becomes ‘unsafe’, they may discover that they do not have to repatriate their money but can move

it elsewhere. Only when high-tax countries succeed in eliminating all tax heavens – a daunting task – repatriation finally takes place. In this sense, our experiment may even underestimate the negative consequences of anticipated amnesties.

Regarding future research, it is possible to implement a more direct comparison of an anticipated and an unanticipated tax amnesty. Because non-anticipated amnesties can be implemented only once, such a design would naturally have to do without repetition. It would also be possible to design an intermediate treatment between perfectly anticipated and unanticipated tax amnesties. Of course, our results on total government revenue also warrant further investigation, e.g., with respect to alternative tax rates, fines, audit probabilities or the inclusion of administrative costs.

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A Appendix: Additional figures and tables

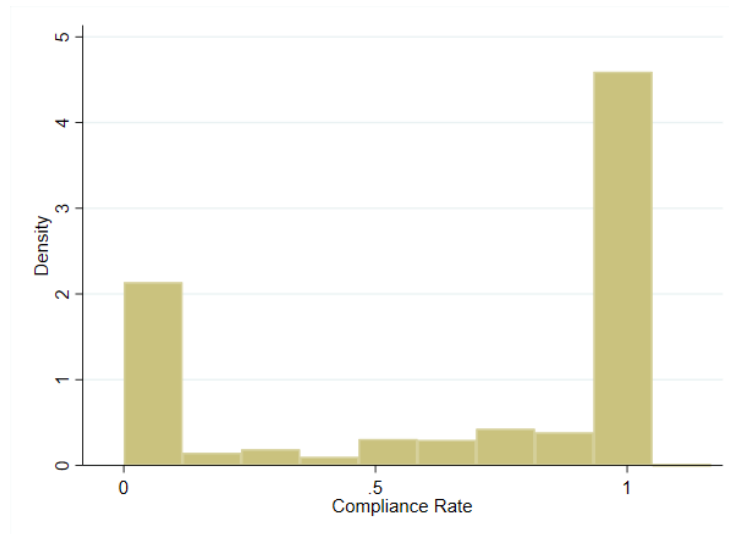


Figure A.1: Histogram of *Compliance rate*

Table A.1: Shares per treatment

Treatment		Total (60 periods)	With amnesty (30 periods)	Without amnesty (30 periods)
currInfo	Revenue share	0.203	0.214	0.191
	Compliance share	0.136	0.132	0.140
	Vol. disclosure share	0.013	0.025	--
	Fine share	0.054	0.057	0.051
pastInfo	Revenue share	0.223	0.222	0.223
	Compliance share	0.175	0.171	0.180
	Vol. disclosure share	0.006	0.012	--
	Fine share	0.042	0.040	0.044
noInfo	Revenue share	0.232	0.234	0.229
	Compliance share	0.191	0.193	0.190
	Vol. disclosure share	0.004	0.008	--
	Fine share	0.037	0.034	0.039

Table A.2: Summary statistics

Variable	Definition	Mean	Standard deviation
Compliance rate	Declared income over true income	0.6765	0.4209
Full compliance	Dummy variable equal to 1 if $Compliance\ rate \geq 1$	0.5271	0.4993
Voluntary disclosure	Dummy variable equal to 1 for subjects who decided for a voluntary disclosure in the current period	0.0325	0.1773
Income	Income at the beginning of the period, ranging from 60 to 100	80.0806	14.1154
High audit rate	Dummy variable equal to 1 if the audit rate is at the high level in the current period	0.3013	0.4588
Lag high audit rate	Dummy variable equal to 1 if the audit rate was at the high level in the previous period	0.3006	0.4585
Amnesty	Dummy variable equal to 1 for periods during the permanent tax amnesty	0.5000	0.5000
Amnesty first half	Dummy variable equal to 1 if the permanent tax amnesty takes place in the first half of the experiment	0.5000	0.5000
Age	Age of the subject	22.1083	4.5073
Male	Dummy variable equal to 1 if the subject is male	0.4583	0.4983
Prepared own tax return	Dummy variable equal to 1 for subjects who prepared already their own tax return in real life	0.2833	0.4506
Risk aversion	Measure of risk aversion, ranging from 0 to 10 and collected following Holt and Laury (2002)	5.7917	1.7838
Lag vol. disclosure	Dummy variable equal to 1 for subjects who decided for a voluntary disclosure in the previous period	0.0326	0.1777
Lag audit	Dummy variable equal to 1 for subjects who have been audited in the previous period	0.0888	0.2845
Tax debt	Evaded taxes of the previous three periods	15.6065	19.6490
Period	Period of the experiment (1-60)	30.5000	17.3193
Amnesty period	Period of the permanent tax amnesty	15.5000	8.6560
currInfo	Dummy variable equal to 1 for subjects in the currInfo treatment	0.3333	0.4714
pastInfo	Dummy variable equal to 1 for subjects in the pastInfo treatment	0.3333	0.4714
noInfo	Dummy variable equal to 1 for subjects in the noInfo treatment	0.3333	0.4714

For all variables, N=7200 (120 subjects * 60 periods)

B Appendix: Heterogeneity and robustness

In this section, we present some additional analyses, examining heterogeneity in behavior and, more generally, providing robustness checks. Tables B.1 and B.2 relate to *tax compliance*. Spec. 1 and 2 of Table B.1 analyze heterogeneity in behavior. In half our sessions the tax amnesty was introduced after subjects had experienced the situation without the amnesty. We use those sessions to provide a glimpse into the heterogeneous responses our participants show towards the amnesty. Our theoretical considerations suggest that some people may always evade and some may always comply. As usual for tax experiments, we primarily observe a lot of compliance (potentially due to the fact that the overall detection probability over several periods can be fairly high). While 11 (out of 120) subjects fully comply in all 60 periods, no one fully evades all the time. Similarly, almost a quarter of our participants (27) has an average compliance rate of at least 90%, while only 3 people have an average compliance rate of at most 10%. Of course, many people evade sometimes but consistent evasion – as suggested by our theoretical framework – almost does not happen. Bearing these numbers in mind, we conjectured that subjects with an overall low compliance level were those most ‘at the margin’ between complying and evading, i.e., those most susceptible for the insurance effect. Using only those sessions where the amnesty was introduced late, we implement a 1/3 (low compliance) vs. 2/3 (high compliance) split of subjects based on their compliance behavior in the first 30 periods when the amnesty had not been introduced.²¹ Providing 1/4 vs. 3/4 or 2/5 vs. 3/5 splits would, however, lead to very similar results.

Spec. 1 and 2 implement the main specification of Table 2 (Spec. 3) splitting the data as described above. They show that the negative effect of the amnesty in *currInfo* seems to be driven by those that previously showed a low pre-amnesty contribution. Consistent with expectations from our theoretical considerations, it appears that those individuals at the margin of complying or evading drive results. Spec. 3-6 of the same table provide further robustness checks using again all available data. In particular tobit, probit and logit specifications are implemented. These specification again support the finding of a negative effect of the amnesty on compliance in *currInfo* while there is no effect for *noInfo* and – if anything – a weak effect for *pastInfo*. Finally, Table B.2 confirms previous findings by running our main specification (Spec. 3 of Table 2) with three treatment dependent regressions.

Table B.3 relates to the determinants of *voluntary disclosures* and looks at hetero-

²¹To implement this split, we look at behavior when the audit rate is *low* in *currInfo* and *pastInfo*, as compliance is fairly homogeneous and substantial with a *high* audit rate, in particular in *currInfo*. In *noInfo*, we look at behavior under all audit rates as subjects cannot distinguish these rates. Looking at the low audit rate for all treatments would however lead to very similar results.

generosity in behavior by replicating the main linear probability specification. It replicates the results of the main part with the following twist: As seen before for compliance, the effect of the amnesty seems to be driven by those being at the margin of paying or not paying taxes, i.e., those with low pre-amnesty compliance.

Table B.4, B.5 and B.6 relate to our analysis of tax revenue. Table B.4 however simply replicates previous results regarding compliance using the *compliance share* measure with the specifications used to analyze revenue. Table B.5 looks at the heterogeneity previously analyzed for compliance and voluntary disclosure, focussing on the most interesting *currInfo* treatment. While the analysis is clearly under-powered, it reveals some indication that contravening forces are at play. Those with low pre-amnesty compliance – being at the margin of complying – tend to lower revenue (although not significantly though) while those with high pre-amnesty compliance tend to increase revenue.

As the previous analysis is clearly underpowered, Table B.6 proposes an additional analysis of heterogeneity, making use again of all available data and not only those sessions in which the amnesty was introduced late. For the case of the *currInfo* treatment, we try to analyze the underlying forces in more detail. For this purpose, we have to identify those that use the tax amnesty as an insurance. One approach would be to just classify those who reduce their tax compliance in case an amnesty is introduced. This approach may, however, be misleading to the extent that compliance rates are noisy. We therefore propose to classify those subjects whose compliance rates decreases by more than 7.5 percentage points as *Lured insurer* to abstract from this noise.²² Table B.6 provides similar regressions than Table 6 but focuses on *currInfo*. Spec. 1 and 3 only introduce a *Lured insurer* dummy and lead to very similar results than before. Spec. 2 and 4 additionally introduce the interaction between *Lured insurer* and *Amnesty*. For the revenue share without penalties, the amnesty dummy becomes significantly positive, reflecting that some subjects seem to use the tax amnesty as a transition device that helps them to adjust to the high audit rate. At the same time the interaction dummy is significantly negative, reflecting that those who seem to use the tax amnesty as an insurance device – and reduce their compliance due to it – provide relatively less payments. Spec. 3 seems to confirm that both effects cancel out. Spec. 1/2 – including penalties – shows that coefficients have the same sign for the revenue share (than those without penalties) but are much smaller and by far not significant. Hence, the investigated effect appears relatively subtle.

²²Using this classification has the nice feature that 14 subjects decrease their compliance rate below 7.5 percentage points whereas only 5 subjects increase their compliance rate above 7.5 percentage points. Hence, we observe a strong asymmetry between positive and negative deviations, which we would also expect due to the *insurance effect*. Using e.g. 2.5, 5.0 or 10.0 percentage points would not lead to such an asymmetry. Nonetheless, using 2.5, 5.0 or 10.0 percentage point as a threshold for insurer leads to similar regression results as presented below. Importantly such an asymmetry is neither observed for *pastInfo* nor *noInfo*. Hence, we do not provide the additional analysis for these treatment.

Table B.1: Estimation results for *Compliance rate/ Full compliance*

	Spec. 1 GLS Pre-amnesty Low comp.	Spec. 2 GLS Pre-amnesty High comp.	Spec. 3 Tobit(0) Comp. rate	Spec. 4 Tobit(0/1) Comp. rate	Spec. 5 Probit Full comp.	Spec. 6 Logit Full comp.
Income	-0.0037*** (0.0012)	-0.0030*** (0.0007)	-0.0031*** (0.0005)	-0.0107*** (0.0022)	-0.0155*** (0.0021)	-0.0276*** (0.0037)
Wealth	-0.0000 (0.0000)	-0.0000 (0.0000)	-0.0000 (0.0000)	-0.0000 (0.0000)	-0.0000 (0.0000)	-0.0001 (0.0001)
Tax Debt	-0.0029*** (0.0010)	-0.0043*** (0.0016)	-0.0028*** (0.0009)	-0.0054** (0.0022)	-0.0046* (0.0027)	-0.0070 (0.0047)
Amnesty	0.0762 (0.0929)	-0.0112 (0.0520)	0.0111 (0.0165)	0.0695 (0.0560)	0.1206 (0.0830)	0.2025 (0.1452)
Amnesty * currInfo	-0.2185** (0.1061)	0.0392 (0.0811)	-0.0787** (0.0357)	-0.2764** (0.1087)	-0.3746** (0.1513)	-0.6510** (0.2646)
Amnesty * pastInfo	-0.0400 (0.1464)	-0.0603 (0.0720)	-0.0510 (0.0314)	-0.1692* (0.0902)	-0.1752 (0.1451)	-0.3029 (0.2526)
Amnesty first half			-0.1008* (0.0553)	-0.2467 (0.1721)	-0.4101* (0.2296)	-0.7373* (0.4161)
High audit rate	-0.0994*** (0.0221)	-0.0025 (0.0115)	-0.0550** (0.0222)	-0.1137* (0.0637)	0.0391 (0.0688)	0.0634 (0.1179)
High audit rate * currInfo	0.7415*** (0.0556)	0.2570*** (0.0552)	0.5219*** (0.0674)	1.5022*** (0.2321)	1.5212*** (0.2065)	2.6951*** (0.3868)
High audit rate * pastInfo	0.3933*** (0.0788)	0.0564* (0.0305)	0.1719*** (0.0453)	0.4712*** (0.1282)	0.4110*** (0.1195)	0.7346*** (0.2076)
Lag audit	-0.3077** (0.1251)	-0.1391** (0.0628)	-0.2052*** (0.0596)	-0.6021*** (0.1601)	-0.5251*** (0.1911)	-0.9276*** (0.3346)
Lag audit * currInfo	0.1341 (0.1458)	-0.0902 (0.0822)	0.0248 (0.0675)	0.1263 (0.2176)	0.0981 (0.2323)	0.1243 (0.4025)
Lag audit * pastInfo	0.2488 (0.1699)	0.1397* (0.0755)	0.1447** (0.0734)	0.4536** (0.1897)	0.4588* (0.2484)	0.8186* (0.4309)
Lag vol. discl.	0.0182 (0.1224)	-0.2567** (0.1290)	-0.2039* (0.1089)	-0.6593** (0.2616)	-0.7081*** (0.2394)	-1.2463*** (0.4105)
Lag vol. discl. * currInfo	-0.1767 (0.1942)	0.0445 (0.1758)	0.0774 (0.1210)	0.3430 (0.3469)	0.3568 (0.3639)	0.6334 (0.6195)
Lag vol. discl. * pastInfo	0.2413 (0.1508)	0.2789* (0.1645)	0.2189 (0.1381)	0.6894* (0.3838)	0.7069** (0.3565)	1.3063** (0.6114)
Period	-0.0009 (0.0030)	0.0005 (0.0021)	-0.0004 (0.0009)	0.0015 (0.0030)	0.0039 (0.0036)	0.0067 (0.0064)
Period * currInfo	0.0015 (0.0030)	-0.0005 (0.0022)	0.0004 (0.0010)	-0.0003 (0.0036)	0.0008 (0.0042)	0.0023 (0.0074)
Period * pastInfo	-0.0039 (0.0038)	0.0001 (0.0024)	-0.0023* (0.0012)	-0.0070* (0.0038)	-0.0045 (0.0047)	-0.0070 (0.0083)
currInfo	-0.5040*** (0.0856)	-0.1866*** (0.0446)	-0.4397*** (0.0753)	-1.1767*** (0.2675)	-1.0545*** (0.3002)	-1.9161*** (0.5502)
pastInfo	-0.3067*** (0.0860)	-0.0034 (0.0402)	-0.0728 (0.0664)	-0.1009 (0.2680)	-0.0678 (0.3191)	-0.1185 (0.5760)
Constant	1.2222*** (0.2350)	1.0209*** (0.1194)	0.7667*** (0.1605)	1.2442* (0.6499)	0.4459 (0.7977)	0.7832 (1.4444)
Control periods	Yes	Yes	Yes	Yes	Yes	Yes
Control Sub. characteristics	Yes	Yes	Yes	Yes	Yes	Yes
N	1239	2301	7080	7080	7080	7080
Subjects	21	39	120	120	120	120
R ² overall	.3677	.1736				
Log-Likelihood			-4053.8	-5158.7	-3152.8	-3147.9

The dependent variable is the compliance rate (declared income over true income) or full compliance per subject and period. Cluster-robust standard errors (subject level, for Tobit estimations obtained via bootstrapping with 100 replications) are provided in parentheses. Tobit(0) treats 0 as censored data while Tobit(0/1) treats 0 and 1 as censored. Subject characteristics include age, sex, risk aversion and experience with preparing a tax declaration in real life. ***, ** and * indicate significance at the 1%, 5% and 10% level.

Table B.2: Estimation results for *Compliance rate*

	Spec. 1 GLS Spec. 4	Spec. 2 GLS Spec. 5	Spec. 3 GLS
	noInfo	pastInfo	currInfo
Income	-0.0030*** (0.0008)	-0.0021** (0.0009)	-0.0026*** (0.0007)
Wealth	-0.0007*** (0.0002)	-0.0008*** (0.0002)	-0.0006*** (0.0002)
Tax Debt	-0.0057*** (0.0014)	-0.0078*** (0.0009)	-0.0059*** (0.0010)
Amnesty	0.0049 (0.0135)	-0.0268* (0.0147)	-0.0495** (0.0193)
Amnesty first half	-0.0728** (0.0323)	-0.0389 (0.0410)	-0.0873* (0.0511)
High audit rate	-0.0348** (0.0170)	0.0854*** (0.0244)	0.3582*** (0.0395)
Lag audit	-0.2195*** (0.0567)	-0.1582*** (0.0365)	-0.2465*** (0.0353)
Lag vol. disclosure	-0.1883** (0.0823)	-0.0135 (0.0930)	-0.2089** (0.0854)
Constant	1.0002*** (0.0888)	0.8205*** (0.1337)	0.5478*** (0.1643)
Control periods	Yes	Yes	Yes
Control Sub. characteristics	Yes	Yes	Yes
N	2360	2360	2360
Subjects	40	40	40
R^2 overall	.2056	.3276	.3607

The dependent variable is the compliance rate (declared income over true income) per subject and period. Cluster-robust standard errors (subject level) are provided in parentheses. Subject characteristics include age, sex, risk aversion and experience with preparing a tax declaration in real life. ***,** and * indicate significance at the 1%, 5% and 10% level.

Table B.3: Estimation results for *Voluntary Disclosure*

	Spec. 1	Spec. 2
	Lin. prob. Pre-amnesty Low comp.	Lin prob. Pre-amnesty High comp.
Income	0.0005 (0.0006)	-0.0001 (0.0004)
Wealth	0.0001 (0.0001)	-0.0001 (0.0001)
Tax debt	0.0014*** (0.0005)	0.0019*** (0.0006)
High audit rate	-0.0397 (0.0311)	0.0030 (0.0133)
High audit rate * currInfo	0.0935* (0.0486)	0.0382 (0.0243)
High audit rate * pastInfo	0.0823 (0.0528)	0.0214 (0.0356)
High a rate-Jump	-0.0031 (0.0104)	0.0502 (0.0338)
High a rate-Jump * currInfo	0.4740** (0.1861)	0.0566 (0.0679)
High a rate-Jump * pastInfo	-0.0567 (0.0372)	-0.0880** (0.0444)
High a rate-Lag jump	-0.0077 (0.0080)	-0.0151 (0.0207)
High a rate-Lag jump * currInfo	0.0034 (0.0277)	0.0691 (0.0537)
High a rate-Lag jump * pastInfo	0.4267*** (0.1313)	0.0913** (0.0427)
Amnesty period	-0.0050 (0.0073)	0.0037 (0.0042)
Amnesty period * currInfo	0.0041** (0.0021)	0.0025** (0.0012)
Amnesty period * pastInfo	0.0013 (0.0020)	-0.0012 (0.0013)
currInf	-0.1829*** (0.0562)	-0.0913*** (0.0307)
pastInf	-0.1146** (0.0522)	-0.0107 (0.0327)
Constant	-0.0880 (0.2851)	0.1692 (0.1131)
Control periods	Yes	Yes
Control Demo	Yes	Yes
N	609	1131
Subjects	21	39
R^2 overall	.3760	.0725

The dependent variable is Voluntary disclosure (0 or 1) per subject and period. Cluster-robust standard errors (subject level) are provided in parentheses. Subject characteristics include age, sex, risk aversion and experience with preparing a tax declaration in real life. ***, ** and * indicate significance at the 1%, 5% and 10% level.

Table B.4: Estimation results for *Compliance share*

	Spec. 1	Spec. 2	Spec. 3
	currInfo	pastInfo	noInfo
Average auditrate	0.2602* (0.1551)	0.1278 (0.1778)	-0.2162 (0.1425)
Amnesty	-0.0112* (0.0061)	-0.0087 (0.0074)	0.0004 (0.0058)
Amnesty first half	-0.0182 (0.0217)	0.0046 (0.0215)	-0.0181 (0.0155)
Constant	0.1263*** (0.0181)	0.1653*** (0.0241)	0.2195*** (0.0144)
N	80	80	80
Subjects	40	40	40
R^2 overall	.0604	.0098	.0209

The dependent variable is the compliance share (tax compliance over total income) per subject, separately calculated for periods with and without permanent tax amnesty. Cluster-robust standard errors (subject level) are provided in parentheses. ***,** and * indicate significance at the 1%, 5% and 10% level.

Table B.5: Estimation results for *Revenue share (with and without penalty)*

	Spec. 1	Spec. 2	Spec. 3	Spec. 4
	currInfo	currInfo	currInfo	currInfo
	Pre-amnesty	Pre-amnesty	Pre-amnesty	Pre-amnesty
	Low comp.	Low comp.	High comp.	High comp.
	with penalties	without penalties	with penalties	without penalties
Average auditrate	1.2081*** (0.2854)	1.0324* (0.5417)	0.2345 (0.1739)	0.2154 (0.2335)
Amnesty	-0.0089 (0.0248)	-0.0011 (0.0211)	0.0014 (0.0105)	0.0102 (0.0132)
Constant	0.0711** (0.0331)	0.0136 (0.0371)	0.2028*** (0.0212)	0.1596*** (0.0252)
N	14	14	26	26
Subjects	7	7	13	13
R^2 overall	.3055	.2671	.0515	.0530

The dependent variable is the revenue share (total taxes, penalties and amnesty payments over total income) or the revenue share without penalties per subject, separately calculated for periods with and without permanent tax amnesty. Cluster-robust standard errors (subject level) are provided in parentheses. ***,** and * indicate significance at the 1%, 5% and 10% level.

Table B.6: Estimation results for *Revenue share (with and without penalty)*

	Spec. 1 currInfo with penalties	Spec. 2 currInfo with penalties	Spec. 3 currInfo without penalties	Spec. 4 currInfo without penalties
Average auditrates	0.7734*** (0.2855)	0.7714*** (0.2818)	0.5077*** (0.1909)	0.4233** (0.1741)
Amnesty	0.0119 (0.0104)	0.0134 (0.0109)	0.0103 (0.0078)	0.0262*** (0.0083)
Amnesty first half	-0.0090 (0.0108)	-0.0090 (0.0108)	-0.0176 (0.0196)	-0.0179 (0.0199)
Lured Insurer	0.0054 (0.0106)	0.0076 (0.0170)	0.0051 (0.0187)	0.0255 (0.0216)
Lured Insurer x Amnesty		-0.0044 (0.0223)		-0.0418*** (0.0142)
Constant	0.1265*** (0.0306)	0.1259*** (0.0310)	0.1026*** (0.0226)	0.1030*** (0.0210)
N	80	80	80	80
Subjects	40	40	40	40
R^2 overall	.2074	.2078	.1125	.1264

The dependent variable is the revenue share (total taxes, penalties and amnesty payments over total income) or the revenue share without penalties per subject, separately calculated for periods with and without permanent tax amnesty. Cluster-robust standard errors (subject level) are provided in parentheses. ***,** and * indicate significance at the 1%, 5% and 10% level.

C Design and instructions

C.1 Design illustration

Figures C.1 and C.2 illustrate the flow of the experimental design. While the former figure refers to the situation in which there is no amnesty, the latter depicts the situation with an amnesty.

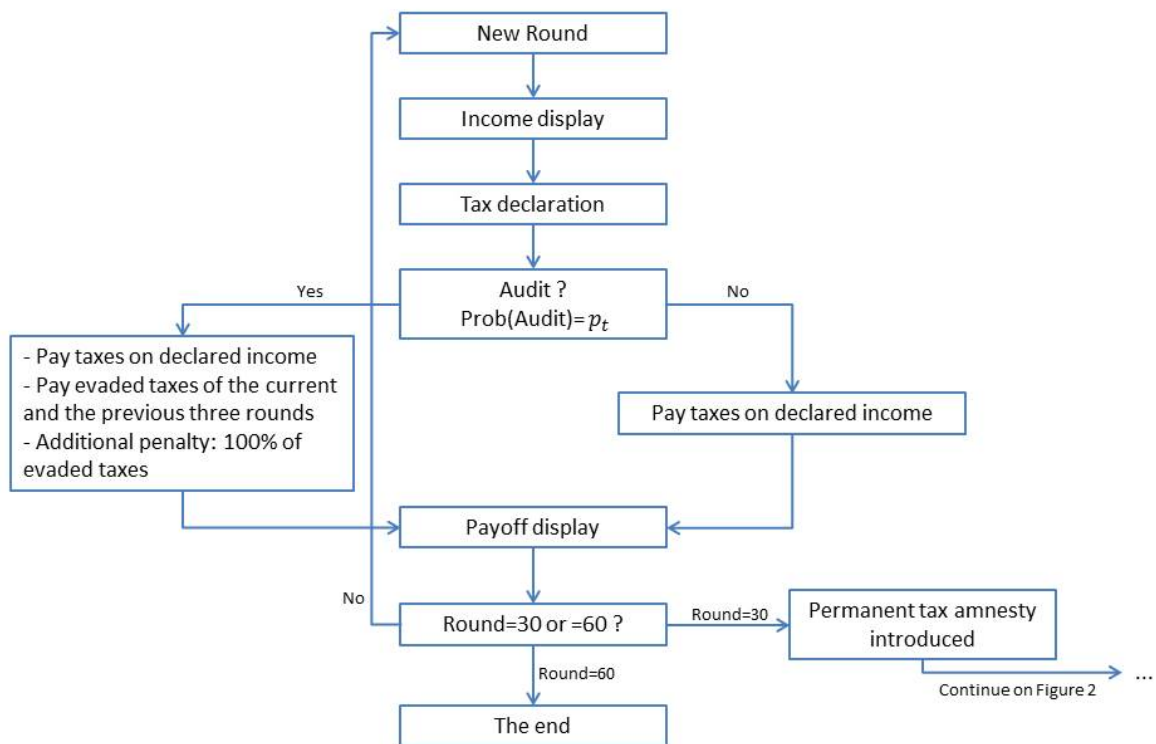


Figure C.1: Experimental design: periods without permanent tax amnesty

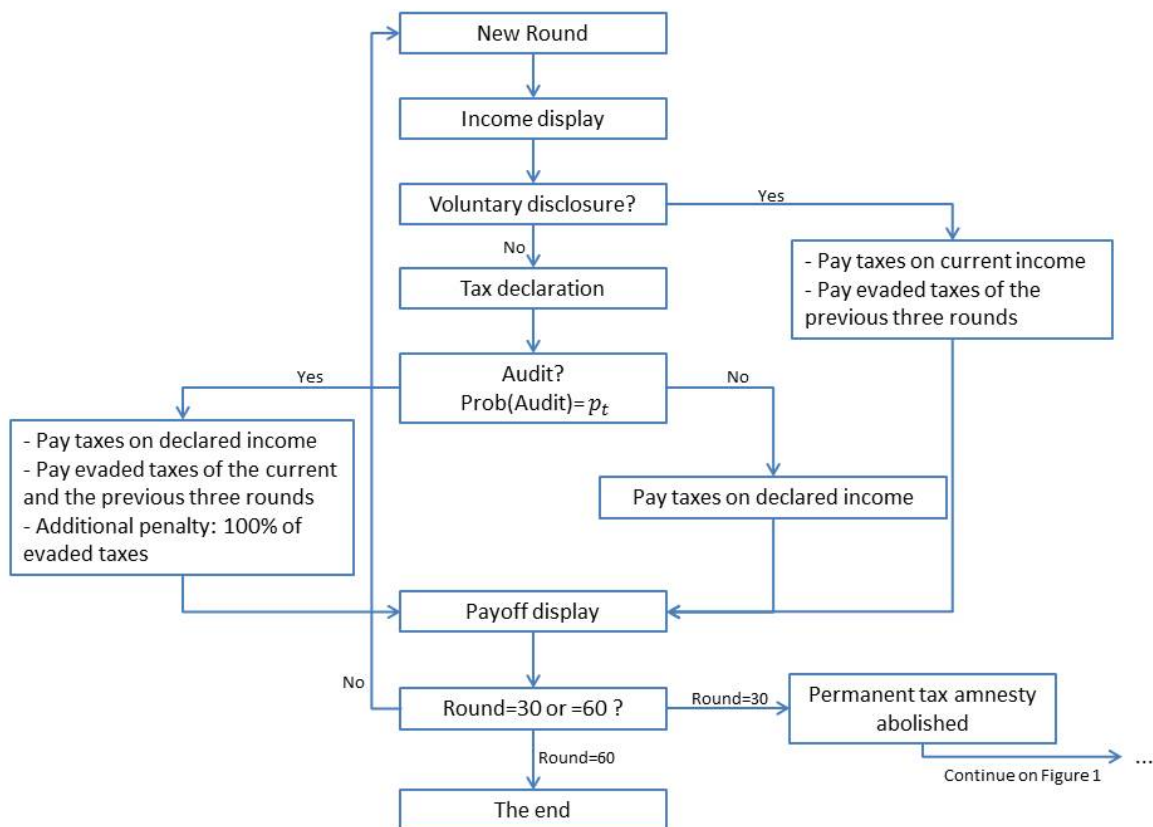


Figure C.2: Experimental design: periods with permanent tax amnesty

C.2 Instructions

The original instructions are written in German. In the following, you will find an English translation. The parts which are specific to the treatments are surrounded by square brackets, i.e. [currInfo: ...], [pastInfo: ...], [noInfo: ...].

Instructions for sessions with the permanent tax amnesty during the second half of the experiment:

Welcome to this experiment! Please read the following instructions carefully. Your final payoff depends among others on how well you have understood these instructions. You can ask questions at any time, just raise your hand. But please do not talk anymore to the other participants of the experiment.

Overview

You participate in a study about "taxes". Each of you has the role of a taxpayer who receives an income in each round and has to pay taxes on it. In each round, there is a probability of being selected for a tax audit. In this case, your tax declarations of the current round and the three previous rounds are investigated. If you have evaded taxes in these rounds, you have to pay the evaded taxes plus a penalty.

Concrete procedure

The experiment consists of several rounds. After some rounds, there can be changes. In this case, you are provided with additional instructions. The number of rounds that will be examined in case of a tax audit and the penalty fee for discovered evaded taxes will, however, not change at all. The number of rounds of the experiment is unknown to you, the duration of the experiment is, however, at most 1.5 hours. Before each round the computer will randomly assign an income of 60, 70, 80, 90 or 100 points to you. Each income is equally likely. Each round proceeds as follows:

Phase 1: (Tax Declaration)

Your income of the current round is displayed. You will also see the form of your tax declaration, in which you are asked for your income. You can enter values greater than or equal to 0. Your stated income is then taxed at a tax rate of 25%. (If you state more than your actual income, you pay more taxes than required. However, you cannot redeem evaded taxes from other rounds in this way.) In the following, you see the screen which is displayed during Phase 1. On the top right, you will see the time that you have for your decision. When the time has expired and you have not decided yet (i.e. you have not submitted your tax declaration in time by clicking OK), you will be immediately selected for a tax audit and your current income is considered as completely evaded. Otherwise, tax audits are purely random, i.e. the tax audits are independent of your decisions as well as of the outcome of past tax audits. The probability of a tax audit is greater than 0% and less than 100 %. A probability of X % means that, on average, X from 100 tax returns are selected for a tax audit.

[currInfo: The probability of a tax audit in the current round is displayed.]

[pastInfo: The probability of a tax audit in the current round is unknown to you, however, the probability which existed in the previous round is displayed.]

[noInfo: The probability of a tax audit in the current round is unknown to you.]

The probability of a tax audit can vary over the rounds between two values. Starting from the default value (= value in round 1), the probability of a jump at the transition to the next round is 15%, i.e. on average in 15 of 100 cases in which the probability of a tax audit is at the default value, there is a jump to a higher value for the next round. This probability of a jump to the higher value is independent of how many rounds the default value was already in force. If there is a jump to the higher value, the probability of a tax audit remains for a length of 1-5 rounds (each equally probable, so on average 3 rounds) on the higher value, before it falls back to the default value.

Phase 2a: (Tax Audit)

You are informed whether you have been selected by the computer for a tax audit. If so, you have to pay the evaded taxes of the current round and the three previous rounds. In addition, a penalty will be charged. This is the sum of evaded taxes. So, in the event of a tax audit you have to pay twice the evaded taxes of the current and the three previous rounds.

Phase 2b: (Result)

A summary of the current round is displayed, with the total income at the end. This is composed as follows:

- Total income = income at the beginning of the round
- taxes of the current round according to the tax declaration
- If applicable, subsequent payment of evaded taxes from the current round
- If applicable, subsequent payment of evaded taxes from past rounds
- If applicable, penalty fees

Note:

During the experiment, the following additional information are available:

- Your evaded taxes of the three previous rounds

[currInfo: - The probability of a tax audit in the current round]

[pastInfo: - The probability of a tax audit which existed in the previous round]

Payments

At the end of the experiment you will be paid individually and confidentially. Your payoff is the sum of the total incomes from the individual rounds (see Phase 2b). The points are converted as follows: 3.5 points = 1 eurocent. The taxes and penalty fees, if applicable, paid by you are also converted into euros and are transferred after the experiment to the account of Bundeskasse and flow into the federal budget of the Federal

Republic of Germany. At the end of the experiment, you have the option to enter your email address to receive a copy of the deposit slip.

Examples

Situation 1: Income in the current round is 100 points. In the tax declaration, 100 is stated as income. No tax evasion in the three previous rounds. Tax audit occurs.

Taxes of the current round according to the tax declaration = $100 * 0.25 = 25$

Subsequent payment of evaded taxes from the current round = 0

Subsequent payment of evaded taxes from past rounds = 0

Penalty fees = 0

Total income = $100 - 25 - 0 - 0 - 0 = 75$

Situation 2: Income in the current round is 100 points. In the tax declaration, 50 is stated as income. In the three previous rounds evaded taxes are 30 in total. Tax audit does not occur.

Taxes of the current round according to the tax declaration = $50 * 0.25 = 12.5$

Subsequent payment of evaded taxes from the current round = 0

Subsequent payment of evaded taxes from past rounds = 0

Penalty fees = 0

Total income = $100 - 12.5 - 0 - 0 - 0 = 87.5$

Situation 3: Income in the current round is 100 points. In the tax declaration, 60 is stated as income. In the previous round, evaded taxes are 20, in the round before 10 and in the round before 5. Tax audit occurs.

Taxes of the current round according to the tax declaration = $60 * 0.25 = 15$

Subsequent payment of evaded taxes from the current round = $40 * 0.25 = 10$

Subsequent payment of evaded taxes from past rounds = $20 + 10 + 5 = 35$

Penalty fees = $10 + 35 = 45$

Total income = $100 - 15 - 10 - 35 - 45 = -5$

Further procedure

If you have read the instructions, please work on the further situations in the following. Of course, you can always ask questions. When all participants have answered these correctly, the actual experiment starts.

Please edit!

Situation 1: Income in the current round is 80 points. In the tax declaration, 80 is stated as income. No tax evasion in the three previous rounds. Tax audit occurs.

Taxes of the current round according to the tax declaration = ...

Subsequent payment of evaded taxes from the current round = ...

Subsequent payment of evaded taxes from past rounds = ...

Penalty fees =...

Total income =...

Situation 2: Income in the current round is 80 points. In the tax declaration, 40 is stated as income. In the three previous rounds evaded taxes are 30 in total. Tax audit does not occur.

Taxes of the current round according to the tax declaration =...

Subsequent payment of evaded taxes from the current round =...

Subsequent payment of evaded taxes from past rounds =...

Penalty fees =...

Total income =...

Situation 3: Income in the current round is 80 points. In the tax declaration, 0 is stated as income. In the previous round, evaded taxes are 5, in the round before 10 and in the round before 5. Tax audit occurs.

Taxes of the current round according to the tax declaration =...

Subsequent payment of evaded taxes from the current round =...

Subsequent payment of evaded taxes from past rounds =...

Penalty fees =...

Total income =...

Instructions II [distributed after round 30]

For the further course of the experiment there is a change in the instructions: From now on there is in every round the option of a voluntary disclosure, i.e. to admit tax evasion. This results in an additional phase at the beginning of each round:

Phase 0: (Voluntary disclosure)

Your income of the current round is displayed. You have the possibility to opt for a voluntary disclosure, i.e. to admit tax evasion (this option exists regardless of whether you actually have evaded taxes or not). If you have evaded taxes in the previous rounds and opt for the voluntary disclosure, your income of the current round is fully taxed and you have to pay the evaded taxes of the three previous rounds - but no penalty - and you arrive directly at phase 2b. In the event of a future tax audit, the redeemed taxes are treated as if they had been correctly paid, i.e. you will not receive punishment for these. If you do not opt for a voluntary disclosure, you get to phase 1. Below you can see the screen that is displayed during Phase 0. If you want to choose the voluntary disclosure, you must check the box (click into the white box) and then click OK. The time that you have for your decision will be shown. When the time has expired and you have not yet decided - i.e. you have not yet clicked OK - you also get to phase 1.

Everything else remains as described in the previous instructions.

Example

Situation 4: Income in the current round is 100 points. Voluntary disclosure is chosen. In the previous round, evaded taxes are 20, in the round before 10 and in the round before 5. (Note: corresponds to situation 3 of the examples so far, only with voluntary disclosure instead of further tax evasion)

Taxes of the current round according to the voluntary disclosure = $100 * 0.25 = 25$

Subsequent payment of evaded taxes from the current round = 0

Subsequent payment of evaded taxes from past rounds = $20+10+5= 35$

Penalty fees = 0

Total income = $100 - 25 - 0 - 35 - 0 = 40$

Please edit!

Situation 4: Income in the current round is 80 points. Voluntary disclosure is chosen. In the previous round, evaded taxes are 5, in the round before 10 and in the round before 5. (Note: corresponds to situation 3 of the examples so far, only with voluntary disclosure instead of further tax evasion)

Taxes of the current round according to the voluntary disclosure = ...

Subsequent payment of evaded taxes from the current round = ...

Subsequent payment of evaded taxes from past rounds = ...

Penalty fees = ...

Total income = ...

C.3 Screenshots

currInfo:

The screenshot shows a dialog box titled 'currInfo' with a yellow border. At the top, there are two fields: 'Runde' with the value '2' and 'Verbleibende Zeit [sec]:' with the value '30'. The main content area displays the following text: 'Ihr Einkommen 90', 'Die Wahrscheinlichkeit einer Steuerprüfung in dieser Runde beträgt 2.5 %', and 'Ihre hinterzogenen (und noch nicht getilgten) Steuern der drei vorherigen Runden: 0.00'. Below this, there is a checkbox with the text: 'Ich gebe zu, Steuern hinterzogen zu haben und will jetzt die Möglichkeit einer strafbefreienden Selbstanzeige nutzen (Folge: jetziges Einkommen wird komplett versteuert und hinterzogene Steuern der drei vorherigen Runden werden nachgezahlt)'. An 'OK' button is located at the bottom right.

Figure C.3: currInfo: Voluntary disclosure (Phase 0)

The screenshot shows a dialog box titled 'currInfo' with a yellow border. At the top, there are two fields: 'Runde' with the value '2' and 'Verbleibende Zeit [sec]:' with the value '30'. The main content area displays the following text: 'Ihr Einkommen 90', 'Die Wahrscheinlichkeit einer Steuerprüfung in dieser Runde beträgt 2.5 %', and 'Ihre hinterzogenen (und noch nicht getilgten) Steuern der drei vorherigen Runden: 0.00'. Below this, there is a text input field with the text: 'Hiermit erkläre ich, folgendes Einkommen erhalten zu haben:'. An 'OK' button is located at the bottom right.

Figure C.4: currInfo: Tax declaration (Phase 1)

pastInfo:

The screenshot shows a software interface for 'pastInfo: Voluntary disclosure (Phase 0)'. At the top, there are two boxes: 'Runde' with the value '2' and 'Verbleibende Zeit [sec]: 30'. The main area contains the following text: 'Ihr Einkommen 90', 'Die Wahrscheinlichkeit einer Steuerprüfung in der vorherigen Runde betrug X %', and 'Ihre hinterzogenen (und noch nicht getilgten) Steuern der drei vorherigen Runden: 0.00'. Below this is a checkbox with the text: 'Ich gebe zu, Steuern hinterzogen zu haben und will jetzt die Möglichkeit einer strafbefreienden Selbstanzeige nutzen (Folge: jetziges Einkommen wird komplett versteuert und hinterzogene Steuern der drei vorherigen Runden werden nachgezahlt)'. An 'OK' button is located at the bottom right.

Figure C.5: pastInfo: Voluntary disclosure (Phase 0)

The screenshot shows a software interface for 'pastInfo: Tax declaration (Phase 1)'. At the top, there are two boxes: 'Runde' with the value '2' and 'Verbleibende Zeit [sec]: 30'. The main area contains the following text: 'Ihr Einkommen 90', 'Die Wahrscheinlichkeit einer Steuerprüfung in der vorherigen Runde betrug X %', and 'Ihre hinterzogenen (und noch nicht getilgten) Steuern der drei vorherigen Runden: 0.00'. Below this is a text input field with the label 'Hiermit erkläre ich, folgendes Einkommen erhalten zu haben:' and a blue input box containing the number '1'. An 'OK' button is located at the bottom right.

Figure C.6: pastInfo: Tax declaration (Phase 1)

noInfo:

The screenshot shows a software window with a yellow border. At the top left, a box labeled 'Runde' contains the number '2'. At the top right, a box labeled 'Verbleibende Zeit [sec]:' contains the number '30'. In the center, the text 'Ihr Einkommen 90' is displayed. Below this, it says 'Ihre hinterzogenen (und noch nicht getilgten) Steuern der drei vorherigen Runden: 0.00'. A checkbox is present with the text 'Ich gebe zu, Steuern hinterzogen zu haben und will jetzt die Möglichkeit einer strafbefreienden Selbstanzeige nutzen (Folge: jetziges Einkommen wird komplett versteuert und hinterzogene Steuern der drei vorherigen Runden werden nachgezahlt)'. An 'OK' button is located at the bottom right.

Figure C.7: noInfo: Voluntary disclosure (Phase 0)

The screenshot shows a software window with a yellow border. At the top left, a box labeled 'Runde' contains the number '2'. At the top right, a box labeled 'Verbleibende Zeit [sec]:' contains the number '30'. In the center, the text 'Ihr Einkommen 90' is displayed. Below this, it says 'Ihre hinterzogenen (und noch nicht getilgten) Steuern der drei vorherigen Runden: 0.00'. A text box contains the text 'Hiermit erkläre ich, folgendes Einkommen erhalten zu haben:' followed by a blue rectangular input field. An 'OK' button is located at the bottom right.

Figure C.8: noInfo: Tax declaration (Phase 1)