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Inequitable wages and tax evasion

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ABSTRACT

In a two-stage lab experiment, we examine whether wage inequity has a greater impact on tax compliance and on the beliefs about peers' compliance levels among the wronged when it results from intentional human choice versus a random mechanism. Subjects are organized into groups of six. In the first stage, we perform a wage inequity manipulation by assigning equitable or inequitable wages to subjects as remuneration for a real-effort task. In the second stage, subjects are prompted to report their incomes, of which a certain percent is deducted but not redistributed between them. Then, subjects state their incentivized beliefs about the mean of the declared-to-true income ratio among their group members. We find that tax compliance and beliefs are eroded when wage inequity stems from intentional human choice but not when it is due to randomness. Consequently, it is not inequity per se that reduces tax compliance and corrupts beliefs, but rather when inequity is due to a human choice. Our results demonstrate that incidental unfairness in the form of intentional wage inequity adversely affects tax compliance and beliefs about peers' compliance levels. In conclusion, intentional wage inequity can be harmful for society. Preregistered at aspredicted.com #36099.

1. Introduction

The idea that ethical preferences are susceptible to personal experiences of unfairness was introduced by Elster (1989).[1] Specifically, if individuals feel that they have incurred injustice caused by an intentional human action, they may be inclined to exploit situations or institutions where relaxing one's ethics pays off. In fact, when a disadvantageous outcome is due to intentional human actions, it is perceived as unfair and stings more than when the same outcome is due to chance. The wronged are then hastened to settle the score with the wrongdoer (Blount, 1995; Loewenstein, Thompson, & Bazerman, 1989; Rabin, 1993; Fehr & Gächter, 2000). Even when there is no scope for reciprocity (so that the loss repair is at someone's else expense), the wronged may engage in unethical behaviors if these improve their financial conditions. They are, for example, more likely to steal (Greenberg, 1993; John, Loewenstein, & Rick, 2014), lie (Houser, Vetter, & Winter, 2012), and cheat (Galeotti, Kline, & Orsini, 2017; Birkelund & Cherry, 2020). These findings confirm Elster (1989) in that ethical preferences are susceptible to situational factors. In particular, a prior episode of being wronged may erode subsequent behaviors where ethics and financial gains are at odds.[2]

Just as ethical behaviors are shaped by equity concerns, so is tax compliance — which is to a significant extent shaped by ethics as captured in the concept of "tax morale" (e.g., Luttmer & Singhal, 2014; Alm, McClelland, & Schulze, 1992) — also influenced by the experience of unfairness. The general finding is that tax compliance decreases when taxpayers perceive that the tax regime puts them at a disadvantage (e.g., Bazart & Bonein, 2014; Spicer & Becker, 1980; Barth, Cappelen, & Ognedal, 2013). In the existing tax research on the relationship between equity concerns and tax compliance, the perceived disadvantage is attributed to the tax system, and hence reacting with decreased tax compliance conflates the motives of negative reciprocity and loss repair. It is, however, left unaddressed whether an incidental experience of a disadvantageous monetary outcome influences tax compliance in a systematic way and — as with distributive outcomes — if it matters whether the disadvantage is due to a random event or an intentional

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[1] In this paper, we follow Konow (2001), and use the terms (un)fairness, (in)justice, and (in)equity interchangeably.

[2] Even more strikingly, Grosch & Rau (2020) demonstrate that unfair payment schemes unleash antisocial behaviors among the wronged, even when these behaviors do not change their financial outcomes and are without scope for retribution.

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human action. Filling this gap, we believe, makes at least two contributions. First, results could enrich our understanding of how incidental distributive outcomes shape tax compliance. Second, it helps us to assess the potential societal consequences of the increased frequency of inequitable practices such as wage inequity, which mostly impact low-skilled and vulnerable workers (Sainato, 2018; Kasperkevic, 2015).

In order to examine the relationship between being wronged and tax compliance, we eliminate the motive of negative reciprocity when asking whether tax compliance is shaped by an incidental, prior experience of a disadvantageous monetary outcome. We present a one-shot income-reporting game where subjects are grouped and their main tasks are to report their wage earned on a trivial real-effort task. They also state their beliefs about the mean reported-to-true income wage ratio in their groups. We then manipulated two dimensions. In the first, *Wage* treatment arms, we manipulated whether the to-be-reported income for the trivial real-effort task is lower than the entitlement (i.e., *Inequitable wage treatments*) or as much as the entitlement (i.e., *Equitable wage treatments*). In the second, *Mode* treatment arms, we manipulated whether the wage manipulation is done by a random process (i.e., *Nature treatments*) or by an intentional human choice (i.e., *Intent treatments*). With the resulting four exogenous variations (i.e, crossing the two factors, both with two levels), we are able to test whether tax compliance is impacted differently when the inequitable wage is due to a human action versus a random event. Additionally, we examine whether being wronged is associated with eroded beliefs about the prevailing compliance levels among peers. To get a proxy of subjects' beliefs about the prevailing compliance level among the similarly treated peers, we elicited subjects' beliefs about the mean compliance rate in their groups in an incentive compatible fashion.

We find that the negative effect of receiving an inequitable wage on compliance level is stronger when the wage is due to human intentions than when it is due to chance. With this result, we wish to underscore that experiencing wage inequity could be followed by decreased tax compliance, suggesting that tax evasion could be seen as a compensation-seeking device. Second, we demonstrate that beliefs about the prevailing compliance levels of one's peers are also lowered following inequitable treatment. In particular, beliefs are the lowest among those receiving the inequitable wage due to human intentions. We also find that these eroded beliefs alone explain one quarter of the decreased compliance observed in the intentional wage inequity treatment, whereas the remaining three quarters is the direct effect of this treatment manipulation. Importantly, we do not find that receiving the low wage per se affects compliance level or beliefs.

We believe that our results contribute to the understanding of the potentially far-reaching consequences of unfair wage policies, both for the individual and for society as a whole. Consequently, public policy may increase its focus on enforcement of equitable wage practices, regulation of wage policies, and even perhaps providing special support and legal counseling for those at risk of inequitable wage practices. This could include increasing support to workers' unions and creating channels where the wronged workers could get support in standing up for themselves. Additionally, consistent with Traxler (2010), beliefs do matter. Public policy, in fact, could strategically manage changes in beliefs about the prevailing compliance levels in a direction that supports the formation of high compliance norms, and could also set examples of highly compliant taxpayers.

The remainder of this paper is organized as follows. In Section 2, we review the related literature. In Section 3, we motivate our experimental design choices. Section 4 describes the experiment in detail including the experimental design and predictions, while Section 5 presents the results. In Section 6, we discuss results, identify potential limitations, and draw conclusions.

2. Related literature

First, we summarize the relevant literature on the emergence and the consequences of disadvantageous distributive outcomes. In order to clearly motivate our design choices of the wage inequity manipulation in the experiment, we put a special focus on the adaptation of early social psychological equity theory to behavioral economics. Then, we review the existing literature on equity concerns and tax compliance. We also point out the absence of research investigating the potential influence of a prior experience of an unfair wage allocation on a tax compliance choice. Finally, in order to motivate our design choices on measuring tax compliance and beliefs about the peers' compliance, we look into the components of a tax compliance choice such as risk preference and beliefs or perceptions about the peers' compliance level.

2.1. Inequitable outcomes in distributive situations

The conjecture that people experience a loss when failing to reach some income target — be it an expectation, a promise, or any kind of reference point — was recognized by Daniel Kahneman and Amos Tversky in their seminal paper from 1979 (Kahneman & Tversky, 1979). An income target is often a wage that was set as a remuneration for a given level of input. According to equity theory from early social psychologists (e.g., Homans, 1958; 1961), in distributive situations such as a wage allocation scheme, people expect that outputs (i.e., wages) are proportional to inputs (i.e., work effort) and hence they set their expectation upon getting a certain output. It is also proposed that when this proportionality rule is violated, people feel that they received an inequitable outcome, which is then followed by behaviors aiming to restore equity or simply to compensate for the loss.

The accountability principle is an adaptation of the proportionality rule to behavioral economics (Konow, 2000). It prescribes that, when there are no reasons for an asymmetric allocation of outputs (for instance, if one party created more input than the other), then the fair distribution should be symmetric or equal. Any deviation from a symmetric distribution creates the feeling of loss, as the outcome falls short of what was expected. The same loss is, however, perceived differently when it is caused by intentional human actions as opposed to a random event. In the former case, it is perceived as unfair, and the wronged take actions to even the score with their wrongdoer (e.g., Blount, 1995; Greenberg, 1993; 1990), whereas this perception and this behavior are absent in the latter case. These divergent perceptions and consequences of the same disadvantageous outcome suggest that, in distributive situations, it is not only about *what* is the outcome but is also about *how* it was created that matters (Loewenstein et al., 1989). An intentionally imposed violation of proportionality not only creates the feeling of loss (as the income target was not reached) but also violates the expectation of adherence to a fairness norm. Even in the absence of reciprocity, there may be a disadvantageous spill-over into ethical behavior, as demonstrated by Houser et al. (2012) and Grosch & Rau (2020). Moreover, these reactions are not infrequently backed up with self-serving interpretations of what constitutes ethical behavior (e.g., Dana, Loewenstein, & Weber, 2012).

A clever demonstration of how otherwise identical distributive outcomes arising from a random draw versus human intentional actions can differently influence subsequent behaviors — even when there is no scope for reciprocity — was a two-stage lab experiment by Houser et al. (2012). In stage one, participants were receivers in a dictator game. In stage two, they privately tossed a coin and reported its outcome, which then determined their earnings. Those who received little or nothing in the dictator game reported having been treated unfairly and lied more about having gotten a favorable coin toss — a clear example of how the same outcome via different mechanisms can lead to a drastic difference in subsequent behavior.

In real life, failing to reach an income target could be caused by random and uncontrollable events such as slumps in tourism, illness, or weather. These events are neither seen as unfair nor followed by sabotage. In fact, when certain sectors are devastated by an "act of God",

some form of central help is publicly expected. At the same time, it is not uncommon to fall short of an income target or an entitlement solely due to human intentions. We regularly witness media discourse and whistle-blowing on a wide range of unfair practices of thriving corporations (e.g., Leskin, 2018; Occhiogrosso, 2019; Thomas, 2018). Such practices include refusing to pay workers a fair wage (Thomas, 2018), not paying overtime for them (Partington), hiring them illegally (Greenhouse, 2005), or exposing them to workplace hazards (Yarrow, 2011). Academic research also identifies the detrimental consequences of wage inequity that emerge under starvation wages and hurt individual health (Woolf, Johnson, & Geiger, 2006), educational prospects (Condron, 2011), interpersonal relationships (Dezső & Loewenstein, 2012), as well as the financial conditions of the underpaid through payday loans (Stegman, 2007).

2.2. Equity concerns in a tax compliance choice

The prominence of fairness concerns in tax compliance choices has been identified in theoretical works (e.g., Bordignon, 1993; Schnellenbach, 2010) and examined systematically in empirical tax research (e.g., Barth et al., 2013; Bazart & Bonein, 2014; Hofmann, Hoelzl, & Kirchler, 2008; Spicer & Becker, 1980). For instance, in a field study on Norwegian taxpayers, Barth et al. (2013) report increased tax evasion when taxpayers perceive that the tax system treats them unfairly with respect to monetary outcomes. Additionally, they find that the wronged taxpayers self-servingly distort their beliefs when stating that tax evasion is justified in their circumstances.

In a laboratory experiment, Spicer & Becker (1980) demonstrate that subjects are more likely to evade taxes when they perceive the prevailing tax regime as treating them disadvantageously compared to others. In a similar vein, Kirchler, Hoelz, & Wahl (2008) emphasize taxpayers' procedural fairness concerns (Kirchler, Hoelzl, & Wahl, 2008). They report increased tax evasion after taxpayers experienced an unfair interaction with tax authorities. Furthermore, Fortin, Lacroix, & Villeval (2007) show that taxpayers are sensitive to horizontal inequity, such that they increase evasion when they learn that others with the same income levels as theirs are assigned a lower tax rate.

There are studies addressing the relationship between equity concerns and tax compliance when high earners are subject to a higher tax rate than low earners (such as in a progressive tax system) but can exit this tax regime and pay taxes elsewhere. The question here is whether, in spite of the fact that the absence of high earners will reduce the to-be-redistributed funds, low earners would still prefer a progressive system. On the one hand, Casal, Grimm, & Schächtele (2019) report that, even under such a conflict of interest, low income taxpayers do not see the regressive tax system as fair and justified, and they prefer a progressive system. In other words, equity perceptions enter into the assessment of a tax system in a complex way: even under the threat of receiving a lower share from a redistribution, the low income taxpayers believe that higher earners should be assigned higher tax rates. On the other hand, Engelmann, Janeba, Mechtenberg, & Wehrhöfer, 2020 report results of an online survey experiment where subjects had to vote for a tax regime. They find that it is mostly political ideology that is linked to higher versus lower tax rates. Specifically, left-leaning respondents are in favor of higher taxes for those with higher income, whereas the right-leaning ones favor lower taxes in this case. Overall, however, the majority gravitated towards a medium tax rate.

These findings highlight the existence and implications of distributive concerns in tax compliance choices. There is, however, a specific feature of this tax research that limits its applicability in a broader context. Namely, there is a reciprocal relationship between taxpayers and the tax system where the system is perceived to be imposing unfairness (i.e., distributive or procedural) on the taxpayers.[3] Within this exchange, decreased compliance can be driven by negative reciprocity as well as by loss repair. Therefore, the observed decrease in compliance conflates negative reciprocity and compensation-seeking. Consequently, there is no telling whether taxpayers would adhere to evasion as a compensation-seeking device in situations where, prior to a compliance choice, they experience some form of distributive injustice.

Although the negative spill-over of being wronged in a distributive situation on subsequent unethical behaviors is well-documented (among others, see, e.g., Greenberg, 1993; John et al., 2014; Houser et al., 2012; Galeotti et al., 2017; Birkelund & Cherry, 2020; Grosch & Rau, 2020), to the best of our knowledge, no studies have hitherto examined whether tax compliance is reduced due to an incidental distributional unfairness preceding a tax compliance choice. Our intuition is that, in situations when a taxpayer receives the short end of the stick in the form of wage inequity, s/he would decrease tax compliance to make up some losses. Building on the literature documenting the differential impact of random versus intentional distributional outcomes, we test whether this distinction carries over to a tax compliance choice. In particular, we ask whether, when the disadvantageous distributive outcome preceding a tax compliance choice is due to a human intentional choice, we would observe a lower compliance than when the same outcome is due to randomness.

2.3. The components of tax compliance

From a standard viewpoint, a tax compliance decision is a choice under risk in which the decision-maker maximizes the evasion gamble (Allingham & Sandmo, 1972). Viewing the taxpayer as an expected utility maximizer implies that risk preference is crucial in a compliance choice. To this end, some research finds a positive association between risk aversion and tax compliance (for instance, Coricelli, Joffily, Montmarquette, & Villeval (2010), Bruner, D'Attoma, & Steinmo (2017), and Bernasconi & Bernhofer (2020)), whereas others report no evidence for this association (e.g., Dulleck et al., 2016).

Beyond risk preference, however, growing empirical evidence suggests that there are other factors at play, a notion that was also acknowledged by Allingham & Sandmo (1972). Certainly, in real life as well as in tax experiments, we observe stunningly high levels of compliance, which requires identifying the factors influencing the compliance choice and expanding the standard model (see, e.g., Alm (2019) for a recent and comprehensive review).

There seems to be ample room for multilayered, nonpecuniary motivations to pay taxes, described under the umbrella term of "tax morale" (Luttmer & Singhal, 2014). One component of "tax morale" is called *conditional compliance*. This captures taxpayers' motivation to adjust compliance to the perceived or believed prevailing compliance levels of peers (Luttmer & Singhal, 2014; Frey & Torgler, 2007; Traxler, 2010; Wenzel, 2005). These norms capture what others typically do in this situation, and are thus classified as *descriptive norms* in social psychology (e.g., Cialdini, 2011), *empirical expectations* in research at the intersection of philosophy and economics (e.g., Muldoon, Lisciandra, Bicchieri, Hartmann, & Sprenger, 2014; Bicchieri & Xiao, 2009), or *peer effects* in behavioral economics (e.g., Gächter, Gerhards, & Nosenzo, 2017). In a tax compliance choice model that includes conditional compliance, there is a penalty increasing in the deviation from what is perceived or

[3] One should notice that this research is agnostic about the causes of an unfair tax system or taxpayers' attributions of the causes. This approach makes sense as there is an implicit assumption that a tax system is designed through a careful and iterative process by the relevant bodies and experts. Hence, differential attributions of disadvantageous aspects of a tax system may not have scientific interest in empirical tax research unless they are directly comparing perceptions of different regimes and keep the exogenous variations of the regime types and the process through which they were created orthogonal.

believed to be the prevailing compliance of the peers (e.g., Traxler, 2010).

Experimental and field studies confirm the existence of conditional compliance. In some studies, the relevant others whose believed or perceived compliance level is important are a reference group that may consist of anyone with whom a taxpayer shares some traits or history (Bobek, Hageman, & Kelliher, 2013). In other studies, subjects play a multi-shot income-reporting game and, depending on their treatment assignment, receive feedback on their group's compliance level from the previous round (Alm, Bloomquist, & McKee, 2017; Fortin, Lacroix, & Villeval, 2007). The overarching finding of this work is that compliance increases when peers are observed to comply at a high versus a low level. However, when directly addressing the heterogeneous effects of the others' compliance levels, Lefebvre and coauthors (2015) document an asymmetry. There is no increase in tax compliance when others with a high compliance are featured but, in contrast, subjects are ready to decrease their own compliance when the others have a low compliance (Lefebvre, Pestieau, Riedl, & Villeval, 2015).

These laboratory findings are corroborated by two large-scale field studies of Hallsworth, List, Metcalfe, & Vlaev (2017) that cleverly used norm-nudging. The authors demonstrated that providing information about relevant others' compliance levels where the perceived proximity of the reference groups was exogenously varied is associated with increased compliance in the form of timely payment of the taxes due. In fact, the closer taxpayers felt to those whose compliance levels were disclosed to them, the more likely they were to pay their tax dues in a timely manner.

3. Experimental framework

Combining research on the consequences of unfair distributional outcomes with evidence supporting the role of equity concerns in tax compliance, we ask whether experiencing wage inequity adversely impacts tax compliance and the beliefs about peers' compliance levels. In order to craft an experimental design to address these questions, we merge some of the experimental methods used in studying the effects of distributive outcomes with methods measuring tax compliance in empirical tax research as well as risk preferences in the financial domain. Hence, in what follows, we lay out the reasoning behind each of our design choices and motivate them with the relevant literature.

3.1. Establishing the wage inequity manipulation

Our wage inequity manipulation capitalizes on research on equity theory and its adaptation to behavioral economics. A common way to create a wage inequity manipulation is to have subjects complete a real-effort task, tell them their entitlement for a given input (i.e., correctly completed tasks), and then pay them below the entitlement with no justifiable reason. The remuneration, which is exogenously set, becomes the entitlement and hence the reference point, whereas paying them below this amount creates the feeling of loss. In order to create the divergent causes of meeting the entitled wage (as in the *Equitable wages treatments*) or not (as in the *Inequitable wages treatments*), we need a treatment arm where a random event determines the realized wage (as in the *Nature treatments arms*), and another where the realized wage is determined by an intentional human choice (as in the *Intent treatment arms*).

Along these lines, in our experiment, we create unmet wage entitlements by adapting the methodology from Konow (2001), Dezső & Loewenstein (2019), and Dezső, Loewenstein, Steinhart, Neszveda, & Szászi (2015). Konow (2001), for instance, reported that even neutral observers find wage differences unfair when workers' performances are identical and there are no justifiable reasons for unequal wages. Similarly, across multiple experiments, Dezső & Loewenstein (2019) and Dezső et al. (2015) created asymmetric wage allocations between workers for identical performances, with no justifiable reason for any earnings differences. They found that the lower (i.e., inequitably) paid worker finds his/her earnings unfair and claims compensation in a subsequent bargaining with the worker who received the higher wage at his/her expense.

The reason why we have chosen a real-effort task to generate income rather than a simple endowment for subjects was twofold. First, with a real-effort task we can get subjects to create a countable, piece-rate input (i.e., number of correctly labeled images), and we can thus set the entitlement (i.e., output) exogeneously. This allows us to exogenously vary whether one gets the entitlement for the required input or gets less. Second, from empirical research on distributive preferences, we learn that choosing to have subjects generate their income rather than endowing them leads them to feel more entitled to their earnings, makes rational behavior more legitimate, and increases the external validity of the results (Cherry, Frykblom, & Shogren, 2002; Oliver, 2021).

The reasons why we have chosen the trivial image-labeling task are because passing this task requires neither specific knowledge nor special spelling skills from the subjects, and also because this task has been previously validated and used in other lab studies as a real-effort task (see, e.g., Dezső & Loewenstein (2019)). With these task features, special personal skills or characteristics that could systematically influence the task performance or enjoyment or could anyhow interact with tax compliance preferences are not issues. By using an all-or-nothing setup (i.e., 5 or more correctly completed images are paid the same amount, whereas fewer yield zero earnings), we ensure that subjects understand that exceeding the 5 correct images threshold does not correspond to extra earnings. Therefore, subjects would neither form expectations of higher wages for exceeding the minimum requirements, nor would they believe that there is any ranking based on effort provision.

Another design choice was that information about the wage entitlements, actual wages, and the method via which the realized wages were selected are disclosed *prior* to starting the image-labeling task. Consequently, when subjects start the image-labeling task, they are all aware of their entitlements and the actual wages they would receive for successful task completion (i.e., whether their entitlements are met or not). Although this design choice may weaken the inequity manipulation (and in real life, wage inequity is more likely to emerge after the work is completed), we intended to eliminate any potential confounding effects of surprise, resentment, rage, relief, or happiness. Nevertheless, in real life, the former three emotions may significantly shape behaviors after receiving an inequitable wage (e.g., Giacalone & Greenberg, 1997).

3.2. Income-reporting task

In almost all setups in experimental tax research, subjects are prompted to report their privately earned incomes, a certain proportion of which is deducted and unpaid to them (i.e., kept as a tax). Subjects also face some nonzero probability of being checked for whether their income reports are truthful, and, if not, they pay a fine proportional to the evaded amount. Apart from these basic features, there are three important design choices we make.

The first choice is whether subjects make a single compliance choice (i.e., one-shot game) or multiple choices (i.e., multi-shot game). Although, the multi-shot approach is the most commonly used, we used the one-shot game approach to ensure that the effect of the experimental manipulations administered before the compliance choice would not dissipate over multiple rounds.

The second choice is to determine what happens with the pooled taxes. In the so called "tax game", they are redistributed among group members, thus involving a strategic complexity in the compliance choice (e.g., Alm, Jackson, & McKee, 1992). By contrast, in one version of the so called "income-reporting task", the pooled taxes are simply kept by the experimenter, while in another version they are redistributed elsewhere (not among subjects whose taxes are pooled). As there is no redistribution among subjects, these versions eliminate strategic complexity from the choice. In a recent meta-analysis, Alm &

Table 1
Lotteries in the EG task adapted to Hungarian Forints (HUF).

Gamble	Low payoff	Likelihood	High payoff	Likelihood	Expected return	Standard deviation	Implied CRRA range
Gamble 1	2000 HUF	50%	2000 HUF	50%	2000 HUF	0	$3.74 < r$
Gamble 2	1700 HUF	50%	2700 HUF	50%	2200 HUF	500	$1.25 < r < 3.74$
Gamble 3	1400 HUF	50%	3400 HUF	50%	2400 HUF	1000	$0.77 < r < 1.25$
Gamble 4	1100 HUF	50%	4100 HUF	50%	2600 HUF	1500	$0.55 < r < 0.77$
Gamble 5	800 HUF	50%	4800 HUF	50%	2800 HUF	2000	$0 < r < 0.55$
Gamble 6	100 HUF	50%	5500 HUF	50%	2800 HUF	2700	$R < 0$

Note: CRRA is the constant relative risk aversion.

Table 2
Summary of the experimental steps.

Step	What happened
Stage 1	
1.	Pooling subjects into groups of six and randomly assigning them to the Nature or Intent treatments.
2.	Describing the experimental steps and information on the expected experimental earnings.
3.	Asking demographic questions.
4.	Eliciting risk preferences via EG task.
5.	Assigning types: one Type B and five Type As per group.
6.	Creating the wage manipulation.
7.	Administering four-item short survey.
8.	Conducting image-labeling task.
9.	Providing information about successful task completion, reminding about the actual wage, and telling how it was selected.
Stage 2	
10.	Providing income-reporting task instructions and details.
11.	Reporting income.
12.	Estimating mean group compliance.
13.	Learning about experimental earnings (total and itemized).
14.	Completing exit survey.

Malézieux, 2020 report that using a research fund is representative of a public good, and in general compliance is higher than when it is absent. In our experiment, we follow established procedures from Fortin et al. (2007) and Coricelli et al. (2010), employing an income-reporting game where the pooled taxes are paid into a scientific research fund, which information was public knowledge.[4]

The third choice is whether to use a tax frame with explicit references to taxes or a neutral frame with no such references. Although there is an ongoing discussion in the literature as to whether a loaded or a neutral context is the proper design choice, the consensus is that one should carefully look into the trade-off (Alekseev, Charness, & Gneezy, 2017). When studying complex cognitive processes, context could indeed facilitate task-understanding and performance, as is the case, for instance, with the Wason card task (Wason & Shapiro, 1971). Context could also have a significant impact when, beyond the pay-off structure, the social aspects of a behavior are essential to mimicking the natural occurrences. For example, cooperation is higher when a prisoner's dilemma game is framed as a "Community" versus a "Wall-street" game (Liberman, Samuels, & Ross, 2004). Also, when subjects are labeled as "partners" rather than "opponents" in a trust game, trusting and trustworthiness increases (Burnham, McCabe, & Smith, 2000).

To the best of our knowledge, there is no resolution on the issue of loaded versus neutral framing in tax experiments (e.g., Alm & Malézieux, 2020; Alm, McClelland, & Schulze, 1992). One consideration is, however, the finding that framing could interact with the experimental manipulations at hand (Durham, Manly, & Ritsema, 2014). Taking these into account, in order to mitigate the chance of subjects' home-grown compliance preferences entering into the study and confounding our results, we use a neutral experimental language without any explicit references to taxes. As we have no reason to prefer a loaded context and we also aim to avoid any interaction between the tax frame and the experimental manipulations, we adhere to the mainstream practice of experimental economics à la Smith (1976) and employ a non-loaded context.

3.3. Measuring risk preference

Given that a tax compliance choice is a decision under risk, following traditions of empirical tax research, we elicit subjects' risk preferences. To this end, we administer the six-gamble version of the Eckel and Grossman method (Dave, Eckel, Johnson, & Rojas, 2010; Eckel & Grossman, 2002, 2008), henceforth the EG task, adapted to Hungarian Forints (HUF).[5] The EG task is a simple method to elicit risk preferences in the financial domain, wherein subjects are prompted to select one from a list of gambles that they would like to have played (Charness, Gneezy, & Imas, 2013; Dave, Eckel, Johnson, & Rojas, 2010).

As shown in Table 1, each of the six gambles include a 50% chance of receiving either a high or a low payoff. In gambles 1 to 5, the expected returns increase linearly with risk. The expected return of gamble 6 is the same as that of gamble 5, but has a higher standard deviation. The gambles are arranged so that more risk-averse subjects would choose lower gamble numbers, while risk-neutral and risk-seeking subjects would choose higher gamble numbers, with risk-seekers typically opting for gamble 6. Using the six-gamble version allows us to differentiate between risk-neutral and risk-seeking subjects.

4. The experiment

The approximately 20-minute long experiment was programmed using oTree (Chen, Schonger, & Wickens, 2016), and was conducted at the lab facilities of Corvinus University of Budapest, Hungary. The experiment unfolded in two stages. In stage one, we established the experimental manipulations by imposing equitable or inequitable wages as remuneration for completing a real-effort task where the realized wage was determined by either a random process or an intentional human choice. In stage two, we elicited the two behaviors of interest: the tax compliance behavior (henceforth, compliance) in an income-reporting task, and subjects' incentivized beliefs (henceforth, beliefs) about the prevailing mean compliance level among their peers.

Subjects were truthfully informed that their identity was anonymous throughout the whole experiment — not only among experimental subjects but also in the eyes of the experimental staff. There were no exclusion criteria for participation, as long as the subjects were adults (i.e., at least 18 years old). Subjects received a 500 HUF show-up fee, while any additional earnings depended on their luck and choices, of which fact they were truthfully informed. In each experimental session,

[4] We acknowledge that some subjects may still perceive their contributions as public good payments even if the public good would only very remotely benefit them. Nevertheless, we have no reason to assume these perceptions would systematically vary between conditions so as to confound our results.

[5] At the time of the experiment, 1 EUR = 334 HUF.

subjects were pooled into groups of six. An average session included three groups, allowing us to randomize subjects into the different treatments.

4.1. Experimental procedure

To facilitate the comprehension of the experimental procedure, Table 2 summarizes the experimental steps.[6]

At the beginning of the experiment, subjects were pooled into groups of six. Then, each group was randomly assigned to the *Intent* or *Nature* treatment arms. Next, they read the instructions summarizing the forthcoming experimental steps, and they were provided information about the expected earnings, highlighting that these earnings depended on their choices and their luck.

Next, everyone filled out a basic demographic survey and then completed the EG task, implemented using the oTree app of Holzmeister (2017), adapted to HUF. To avoid any wealth effects, the earnings from this task were only disclosed at the end of the experiment.

Then, in each group, subjects learned that one of their group members was randomly assigned to be *Type B*, while the other five were *Type A*. They were assured that this assignment was anonymous (i.e., nobody, including the experimenters, knew who was Type A or B). The fact and method (i.e., random mechanism) of this role assignment and the information that there are five Type A subjects and one Type B subject in each group were public knowledge.

Subjects then learned that they were all to complete the same image-labeling task, entailing the labeling of 10 simple images. The completion criterion was correctly labeling any five images. Those meeting this criterion were entitled to a wage of 3000 HUF, while the others were entitled to a zero wage (0 HUF). Furthermore, those having more than five correct would not be entitled to higher than 3000 HUF. They were also informed that at this point their *actual* wage (which may or may not coincide with their entitlements) would be determined via the realization of one of the following two options:

1. Each Type A and B subject earns 3,000 HUF for completing the image-labeling task.
2. Each Type A subject earns 1,500 HUF, while the Type B subject earns 10,500 HUF for completing the image-labeling task.

In the *Nature* treatment arms, they all learned that a random mechanism was now selecting between the two options, where each option was equally likely to be selected. In the *Intent* treatment, Type A subjects learned that the Type B subject was now selecting the actual wage from the two options, while the Type B subject was indeed prompted to make his/her selection.[7]

Next, in the *Nature* treatments everyone learned which alternative was realized and, in the *Intent* treatments, which alternative was selected by the Type B subject.

In the next step, everyone responded to a four-item mid-survey. The first two items served as attention checks, testing whether subjects correctly remembered their actual wages, how they were determined, and how much their entitlements were. The second two questions asked them to rate their satisfaction with and, following Houser et al. (2012), their perceived fairness of their actual wages. Regardless of subjects' responses, they all proceeded to the image-labeling task.

Next, all subjects completed the image-labeling task. After submitting their work, they received feedback on how many images they had labeled correctly, whether they had fulfilled the completion criteria of labeling at least five images correctly, the number of Type A subjects in their group fulfilling the completion criteria, and — only for Type A subjects — whether the Type B subject successfully completed the task. Moreover, they were reminded of their actual wage and how it was selected, and they were also told the actual wage of the Type B subject in their group.

In stage two (see the lower panel of Table 2), we prompted both subject types to report their earnings from the image-labeling task, with both types knowing that everyone was being prompted to report their incomes. Subjects were told that 25% of their reported income would be deducted and deposited in a scientific research fund. They were also told that they were all facing a 15% chance of having their income reports checked for whether the true amount was reported. If they were found to have under-reported, 50% of the unreported amount was deducted from their final earnings. They were assured that the fact of being checked on their reports — as well as the outcome of this checkup — would remain their private knowledge.

Next, we elicited subjects' beliefs about their peers' mean compliance. We asked subjects to state their beliefs about the percent of true income declared in their groups, expressed as a number between 0% and 100% on a slider, but only among Type A subjects and excluding themselves. They were informed that, if their estimates fell within a 10% range of the true mean, they would receive 500 HUF at the end of the experiment.[8]

In the next step, subjects learned their experimental earnings (total and itemized). Then, they completed a four-item exit survey asking about: (1) how ethical an unbiased judge who knows their entire history would rate their income reporting behaviors; (2) how much they agree with the statement that "it is fair to under-report if someone received less than the originally assigned entitlement"; (3) how much they anchored their income reports on their beliefs about their other group members' reporting behaviors; and (4) how fair they found their experimental earnings.

The experiment concluded by paying subjects in cash.

4.2. Experimental design and predictions

The experiment employed two factors, each with two levels. The *Mode* factor describes whether the actual wage for completing the image-labeling task was determined by a random process, as in the *Nature* treatment arms, or by the intentional action of Type B subject, as in the *Intent* treatment arms. The *Wage* factor describes whether the actually received wage was the entitlement, as in the *Equity* treatment arms, or lower, as in the *Inequity* treatment arms. Crossing the *Mode* and *Wage* factors, we obtain a 2 X 2 factorial design with the following four treatments: *Nature–Equity, Nature–Inequity, Intent–Equity*, and *Intent–Inequity*.

The role of Type B subjects was purely instrumental, and hence we are uninterested in their behaviors. Consequently, our four predictions only pertain to the behaviors of Type A subjects. These predictions are as

[6] See Appendix A for the complete experimental materials in the original language (with screenshots) and their English translations.

[7] Note that at this point Type B was unaware of the facts that the assigned wage would be subject to an income-reporting task and also of what would then happen with the so-pooled money. This design choice allowed us to eliminate both Type B subject's strategic considerations and also the impact of his/her preferences for income reporting and attitudes towards scientific research.

[8] Type B subjects also estimated the mean declared and true income ratio for all five Type A subjects in their group. Both types knew that the estimation task was conducted under identical incentives for all subjects. Building on Gächter & Renner (2010), we assumed that rewarding precision would be an incentive-compatible way to elicit beliefs about the mean compliance in the one's group. Along these lines, estimates within a given range were monetarily rewarded and no reward was given to estimates outside of this range. Admittedly, there is an ongoing discussion about whether beliefs should be elicited before, after, or simultaneously with the focal behavior to ensure that beliefs and behaviors remain intact (i.e., they do not systematically influence each other). Nonetheless, d'Adda, Drouvelis, & Nosenzo (2016) demonstrated that, under proper incentives, beliefs and behaviors are uncontaminated and correspond to truthful preferences regardless of the elicitation order.

Table 3
Descriptive summary of the sample and the key experimental behaviors.

	Nature		Intent	
	Equity	Inequity	Equity	Inequity
Number of conducted sessions	13	10	9	14
N (everyone including Types A and B subjects)	78	60	54	84
N (all Type A subjects)	65	50	45	70
N Final (only attentive Type A subjects)	63	47	44	68
Total experimental earnings Mean (SD)	5434.52 (1310.07)	4222.06 (1242.11)	5302.84 (1614.81)	4476.94 (1484.37)
Risk survey earnings Mean (SD)	2366.67 (1347.64)	2370.21 (1112.95)	2225.00 (1503.81)	2651.47 (1382.24)
Only experimental earnings Mean (SD)	3067.86 (388.84)	1851.85 (277.47)	3077.84 (335.38)	1825.47 (317.73)
Gamble choice Mean (SD)	3.46 (1.12)	2.96 (1.12)	3.57 (1.37)	3.28 (1.33)
Beliefs Mean (SD), [95% CI]	0.62 (0.30) [0.54, 0.69]	0.65 (0.26) [0.58, 0.73]	0.65 (0.26) [0.57, 0.73]	0.53 (0.28) [0.46, 0.60]
Compliance Mean (SD), [95% CI]	0.74 (0.38) [0.65, 0.84]	0.85 (0.32) [0.75, 0.94]	0.71 (0.38) [0.60, 0.83]	0.44 (0.43) [0.38, 0.55]

Notes: SD stands for standard deviation. All earnings are expressed in Hungarian Forints (HUF). The "Total experimental earnings" and the "Only experimental earnings" include the 500 HUF show-up fee.

Table 4
Summary of OLS regressions (with robust standard errors) of satisfaction with and perceived fairness of the image-labeling task earnings on experimental factors and their interaction.

	Satisfaction	Perceived fairness
Intercept	0.619 (0.113) ***	0.921 (0.113) ***
Intent	0.290 (0.176) $^{+}$	0.148 (0.177)
Inequity	–0.981 (0.173) ***	–0.984 (0.173) ***
Intent X Inequity	–0.605 (0.245) **	–0.584 (0.245) *
LR χ^2	96.16 ***	98.41 ***
Df	3	3
N	222	222

Notes: Standard errors are in parentheses.
$^{***}p \leq 0.001$, ** $p \leq 0.01$, * $p \leq 0.05$, $+p \leq 0.10$

follows:

1. Beliefs would be lowest in the *Intent–Inequity* treatment.
2. Compliance would be lowest in the *Intent–Inequity* treatment.
3. In all four treatments, there would be a significant correlation between beliefs and compliance.
4. The observed decrease in compliance in the *Intent–Inequity* treatment would be, to a significant degree, mediated by the decrease in beliefs.

5. Results

We enrolled 276 subjects into 46 experimental sessions, of which 23 sessions were assigned to the *Nature* and 23 to the *Intent* treatments. We present a summary of the key descriptive results in Table 3. As all subjects completed the image-labeling task successfully, we do not have to exclude anyone for failing to complete the task. Specifically, in all four treatments everyone correctly labeled at least 8 out the 10 images. The overall mean (SD) performance is 97.88% (4.20), which does not differ between treatments, $F(3, 218) = 0.22$, $p = .88$. In other words, despite the fact that subjects had different incentives for the effort provision in the four treatments, their performance does not differ across treatments.

From the first row of the top panel of Table 3, we learn that sessions were unequally distributed between the four treatments. This is because the realization of the *Wage* factor depended on a random device in the *Nature* treatment, but on Type B subjects' choices in the *Intent* treatments. The second row presents how the total sample is distributed between treatments for both types. The third row shows how many Type A subjects we have in each treatment. Row four presents the final sample of 222 Type A subjects who passed the attention checks included in the mid-survey where we tested subjects' understandings of the experimental task. The proportion of subjects failing on the attention checks does not differ across treatment groups, χ^2 (3) = 1.27, $p = .74$. Recall that in the preregistration we planned to exclude from the analysis (but not from completing the experiment) those subjects who fail to pass these comprehension and attention check questions.

The first row of the middle panel of Table 3 presents the mean total experimental earnings of the final sample of Type A subjects. We find that these earnings differ across the four treatments, $F(3, 218) = 9.73$, $p \leq 0.001$. In the second row, we show that mean risk survey earnings do not differ across treatments, $F(1, 218) = 1.03$, $p = 0.38$. In the last row, we present mean final experimental earnings without the risk earnings. As one can see, the only experimental earnings still differ significantly between treatments, $F(1, 218) = 250.93$, $p \leq 0.001$. Consequently, differences in experimental earnings are solely due to the *Wage* treatment manipulations.

The first row in the bottom-most panel of Table 3 presents the mean gamble choices on the EG task. Following the procedures of Eckel & Grossman (2002), Eckel & Grossman (2008), or Dave et al. (2010), for each subject we take his/her gamble choice of the six offered gambles and compare the means with one-way ANOVA. The marginal mean (SD) is 3.32 (1.25) indicating a risk averse sample, and we do not find treatment differences on the conventional *alpha* level of 0.05, $F(3, 218) = 2.23$, $p = 0.086$. The next two rows present descriptive beliefs and compliance results in the four treatments, which we discuss later in this section.

As for demographics, neither mean age nor gender distribution differ between the four treatment groups. Across all treatments, the mean (SD) age is 21.23 (2.03) years, and 56.3% of subjects are male.[9]

5.1. Manipulation checks

Recall that, after we implemented the experimental manipulations (i.e., assigned subjects to one of the four treatments), subjects responded to a short survey. Beyond checking subjects' comprehension and the

[9] When computing mean age, we excluded two subjects as they indicated 1900 for their birth year. Additionally, although we had a three-level gender variable, no subjects indicated "other". Detailed Types A and B subjects' demographics, along with Type B subjects' beliefs and compliance levels are presented in Table B1 of Appendix B.

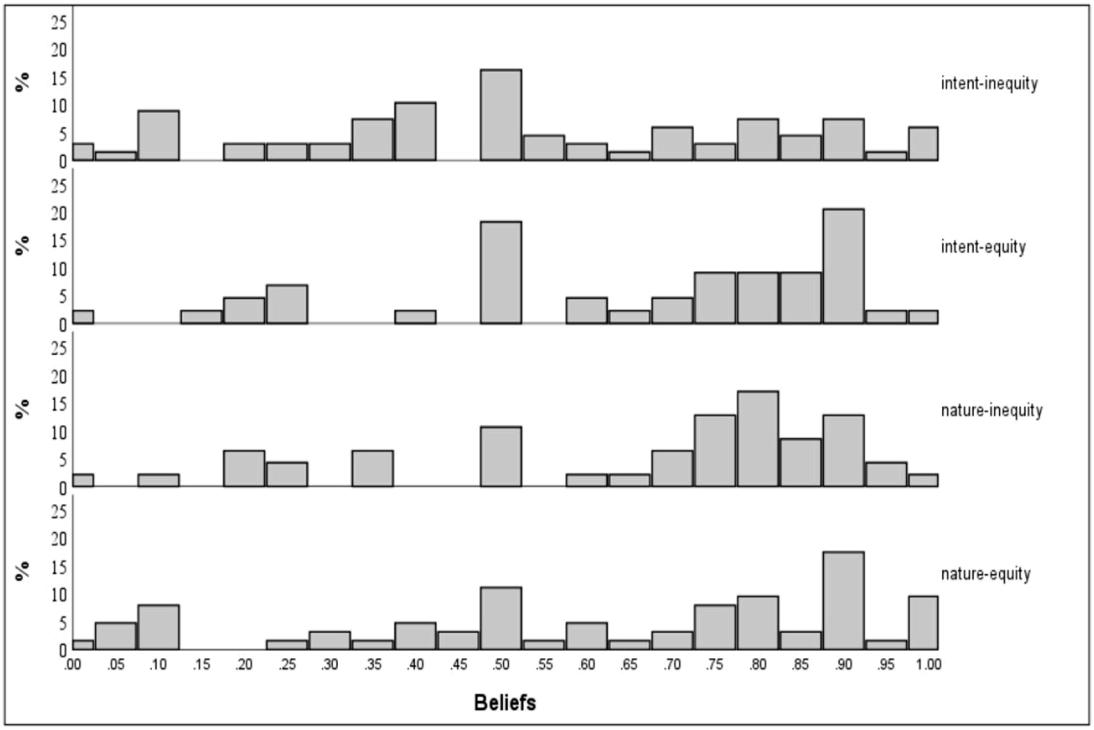

Fig. 1. The distribution of beliefs in the four treatments.

Table 5
Summary of OLS (with robust standard errors) regression results of beliefs on experimental factors and their interaction.

	Beliefs
Intercept	0.618 (0.038) ***
Intent	0.032 (0.054)
Inequity	0.036 (0.053)
Intent X Inequity	–0.157 (0.074) *
LR χ^2	7.74 *
Df	3
N	222

Notes: Standard errors are in parentheses. ***$p \leq 0.001$, *$p \leq 0.05$

correctness of their memories of what had happened (i.e., attention check), following the procedure of Houser et al. (2012), we also inquired about their satisfaction and perceived fairness regarding their actual wage on the image-labeling task.

We expected that, in the event of a successful manipulation, the effect of the *Inequity* manipulation would be stronger in the *Intent* than in the *Nature* treatment. By regressing (OLS with robust standard errors) the satisfaction and perceived fairness variables on the experimental factors and their interaction (summarized in Table 4), we find the expected *Intent X Inequity* interactions in both models. To specifically address whether satisfaction and perceived fairness are the lowest in the *Intent – Inequity* treatment, we conduct a planned contrast for each variable. Here we find that the mean satisfaction and perceived fairness are the lowest in the *Intent–Inequity* treatment, $t(218) = 8.06$, $p \leq 0.001$ and $t(218) = 8.61$, $p \leq 0.001$, respectively. Note that these results are robust after controlling for age, gender, and income level; see Table B2 in Appendix B.

5.2. Testing predictions

We have two key experimental variables: beliefs, which were entered as a percent, ranging between 0 and 100; and compliance, which is the proportion of the true income that was declared. We re-scale both variables to [0,1].

5.2.1. Beliefs about peers' mean compliance levels

In Figure 1 we present the distributions of the beliefs, and, in the second row of the bottom-most panel of Table 3, we present mean beliefs in the four treatments. We find that mean beliefs are significantly different in the *Intent–Inequity* than in the *Intent–Equity* treatment, Mann-Whitney $U = 1199.00$, $p = 0.046$, and also than in the *Nature–Inequity* treatment, Mann-Whitney $U = 1199.50$, $p = .048$ (both p-values are Bonferroni-adjusted to account for the two comparisons).

We regress (OLS with robust standard errors) beliefs on the experimental factors and their interaction, and summarize results in Table 5.[10] Here we find a significant *Intent X Inequity* interaction, indicating that the effect of inequity differs between the *Intent* and *Nature* treatment arms.

To specifically address our first prediction, we conduct a planned contrast. Here we find that the mean beliefs are the lowest in the *Intent – Inequity* treatment, $t(218) = 2.58$, $p = 0.010$.

Result 1: Beliefs about similarly treated peers' compliance level are lowest in the *Intent–Inequity* treatment.

5.2.2. Compliance level

In Fig. 2 we present the distributions of compliance in the four treatments, and, in the last row of Table 3, we present the mean compliance in the four treatments. We find that mean compliance is significantly different in *Intent – Inequity* than in the *Intent – Equity*, Mann-Whitney $U = 1033.00$, $p = 0.008$, and also than in the *Nature – Inequity*, Mann-Whitney $U = 834.00$, $p \leq 0.001$ (both p-values are Bonferroni-adjusted to account for the two comparisons).

To model compliance level, we conduct a series of Tobit regressions

[10] Note that we preregistered Tobit regression to model beliefs, but OLS with robust SE's is an equally adequate choice. For completeness and in adhering to the planned analysis, Tobit regression results are summarized in Table B4 Appendix B, and yield similar results.

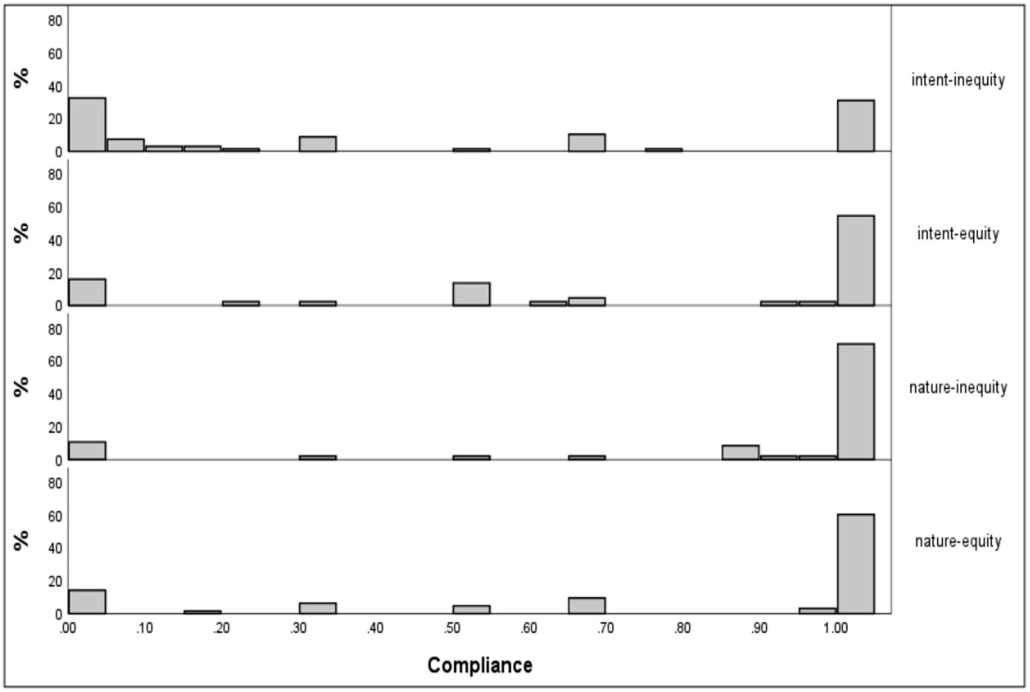

Fig. 2. The distribution of compliance in the four treatments.

Table 6
Summary of Tobit regressions results of compliance with different model specifications.

	Model I	Model II	Model III
Intercept	1.223 (0.149) ***	–0.099 (0.166)	0.665 (0.29) **
Intent	–0.139 (0.224)	–0.193 (0.176)	–0.242 (0.171)
Inequity	0.318 (0.233)	0.205 (0.184)	0.037 (0.176)
Intent X Inequity	–0.938 (0.318) **	–0.540 (0.249) *	–0.407 (0.237) +
Beliefs		2.081 (0.221) ***	1.953 (0.211) ***
Male			–0.354 (0.120) **
Risky choice			–0.127 (0.048) **
Log-Likelihood	–209.46	–167.34	–157.51
LL χ^2	27.58 ***	111.82 ***	131.47 ***
Df	3	4	6
N	222	222	222

Notes: Standard errors are in parentheses. ***$p \leq 0.001$, ** $p \leq 0.01$, * $p \leq 0.05$, +$p \leq 0.10$

under different specifications and summarize the results in Table 6. From the significant *Intent X Inequity* interaction term in Model I, we learn that the negative effect of inequity on compliance is stronger when it is due to human intentions than when it is due to randomness. In Model II, we add beliefs and find that they are positively associated with compliance, while we still obtain a significant *Intent X Inequity* interaction.

In Model III, we add gender (coded as a Male dummy) and risky choice. Here we see that, although we still obtain a marginally significant *Intent X Inequity* interaction and a positive association between beliefs and compliance, compliance is lower among males than females, and a higher gamble choice is associated with a decrease in compliance.

To directly address our second prediction, we perform non-parametric bootstrap sampling to obtain 95% confidence intervals (CIs) of the means in each treatment. These CIs are $[0.64, 0.83]$ in the *Nature – Equity*, $[0.73, 0.92]$ in the *Nature – Inequity*, $[0.59, 0.81]$ in the *Intent – Equity*, and $[0.34, 0.55]$ in the *Intent –Inequity* treatments. Indeed, compliance level is significantly lower in the *Intent – Inequity* treatment than in the other treatments, the rest of whose intervals all overlap.

Result 2: The compliance level is lowest in the *Intent – Inequity* treatment.

Result 3: There is a significant positive association between beliefs and the compliance level.

Consulting Fig. 2, in each treatment we can identity three distinct compliance types. We find *zero compliers* whose compliance level is 0, *full compliers* with compliance level of 1, and a majority of *partial compliers* in between. The distribution of these types differs across treatments, $\chi^2(6) = 21.92$, $p \leq 0.001$. We observe that the proportion of full compliers is lower while the proportion of zero and partial compliers is increased in the *Intent – Inequity* treatment compared to the other two treatments. Ordinal logistic regression of compliance types on

Table 7
Summary of ZOIBR of compliance.

	μ	σ	ν	τ
Intercept	–1.102 (0.594) +	0.191 (0.118)	–1.541 (0.965)	–0.311 (0.749)
Intent	0.198 (0.431)		0.643 (0.708)	–0.536 (0.522)
Inequity	0.218 (0.412)		0.103 (0.851)	0.086 (0.514)
Intent X Inequity	–1.177 (0.551) *		0.168 (1.023)	–0.781 (0.718)
Beliefs	3.162 (0.588) ***		–2.621 (0.894) ***	4.094 (0.746) ***
Male	–0.475 (0.292)		0.762 (0.505)	–0.695 (0.369) +
Risky choice	–0.082 (0.126)		0.231 (0.185)	–0.295 (0.156) +

Notes: Standard errors are in parentheses. Coefficients are on the logit scale for the μ and σ equations, and on the log scale for the ν and τ equations.
***$p \leq 0.001$, * $p \leq 0.05$, +$p \leq 0.10$

Table 8
Means and standard deviations (SD) of the exit survey responses in the four treatment groups.

	Nature		Intent	
	Equity	Inequity	Equity	Inequity
Judge	0.29 (1.21)	0.26 (1.28)	0.34 (1.26)	0.13 (1.01)
Ethical to under-report	–0.73 (0.94)	–0.66 (1.03)	–0.64 (0.99)	–0.40 (0.98)
Anchoring behavior on beliefs	2.40 (0.66)	2.26 (0.74)	2.34 (0.71)	2.13 (0.71)
Fairness of earnings	0.84 (0.87)	–0.06 (1.00)	1.09 (0.88)	–0.50 (1.13)

Notes: Response scales: "Judge": -2 - Very unethical, -1 - Unethical, 0 - Neither unethical, nor ethical, 1 - Ethical, 2 - Very ethical. "Ethical to under-report": -2 - Very much disagree, -1 - Disagree, 0 - Neither agree, nor disagree, 1 - Agree, 2 - Very much agree. "Anchoring behavior on beliefs": 1 - Not at all, 2 - Somewhat, 3 - Very much. "Fairness of earnings": -2 - Very unfair, -1 - Unfair, 0 - Neither unfair, nor fair, 1 - Fair, 2 - Very fair.

experimental factors ($LR\chi^2(6) = 21.98$, $p \leq 0.001$ of overall model statistics) shows that only the *Intent X Inequity* interaction is significant ($\beta = -1.34$, $p = 0.015$), implying that the odds of seeing a higher versus lower compliance type is the lowest in the *Intent – Inequity* treatment.

To exploit this heterogeneity of the three compliance types, we conduct a series of zero-one-inflated beta regressions, or ZOIBR (Ospina & Ferrari, 2010, 2012) under the usual model specifications of compliance. With ZOIBR we can directly examine how our treatment manipulations, beliefs, and moderators (i.e., risk preference and gender) influence the prevalence of the three compliance types, as well as the mean compliance level for partial compliers.

The results of the ZOIBR regressions are summarized in Table 7, where we have modeled expected compliance conditional on experimental variables and covariates (i.e., risky choice and gender) as a three-component model with three components and governed by four parameters. The first component, a continuous portion covering partial compliance (i.e., in (0,1)), is modeled as a beta distribution with mean μ and scale σ. Second, a discrete portion accounts for zero compliers, parameterized with the odds $\nu = P(\text{zero compliance})/P(\text{partial compliance})$. Third, another discrete portion accounts for full compliers, parameterized with the odds $\tau = P(\text{full compliance})/P(\text{partial compliance})$.

Each of the four regression equations follows the same specification. Focusing on the mean of the partial compliance component (i.e., μ), we see that increasing beliefs pushes the mean among partial compliers higher, while *Inequity* has no effect in the absence of *Intent*. However, when *Intent* is present, the presence of *Inequity* reduces compliance among partial compliers (i.e., *Intent X Inequity* interaction). We also learn that the odds of zero versus partial compliance (i.e., ν) decrease as beliefs increase, while no other factor is associated with these odds. Finally, the odds of full versus partial compliance (i.e., τ) increase with beliefs, whereas being male and more risk averse decrease these odds, although these latter two effects fall short of significance at the conventional level of $\alpha = 0.05$. Note that the experimental manipulations only impacted the compliance level of partial compliers but not the odds of becoming one of the corner types (i.e., zero or full compliers) over partial compliance. These odds were only associated with beliefs about the peers' compliance levels.

5.2.3. The relationship between beliefs and compliance

To determine whether and to what extent the effects of treatment manipulations on compliance are mediated by their effects on beliefs, we conduct a causal mediation analysis using the parametric version of the counterfactual-based method of Imai, Keele, & Tingley (2010a) and Imai, Keele, & Yamamoto (2010b). We first give an intuitive outline of the method, and we then present its results.

The idea is first to use the fitted beliefs model to estimate the beliefs that each subject would have had in alternative treatments. Then, we use these counterfactual beliefs and the compliance model to estimate counterfactual compliance behavior that would have arisen if, for instance, the treatment were changed from *Nature – Inequity* to *Intent – Inequity* but beliefs were held constant at those that would have arisen in *Nature – Inequity*; or, conversely, how compliance would change if the beliefs had been formed in *Intent – Inequity* versus *Nature – Inequity*, but the treatment was held constant at *Nature – Inequity*.

The results show an average change of $\delta = -0.075$ in compliance from *Nature – Inequity* to *Intent – Inequity* due to the indirect effect of beliefs. This accounts for an estimated $\nu = 24.64\%$ of the total (direct + indirect) effect on mean compliance when changing from *Nature – Inequity* to *Intent – Inequity* treatment.

Result 4: Approximately one quarter of the decrease in compliance observed in the *Intent – Inequity* treatment compared to the *Nature – Inequity* treatment is due to a decrease in beliefs, while the other three quarters is due to the direct effect of treatment manipulations.

As an exploration, to test whether the strength of the beliefs and compliance associations systematically differ across treatments, we present correlation coefficients and their corresponding CIs (adjusted for the multiple comparisons). The Spearman ρ's and their corresponding CIs are 0.56, [0.30, 0.75] in the *Nature – Equity*, 0.48 [0.14, 0.72] in the *Nature – Inequity*, 0.74 [0.50, 0.88] in the *Intent – Equity* and 0.53 [0.26, 0.72] in the *Intent – Inequity* treatments. We conclude that we find no differences in the strength of correlations between the four treatments.

5.2.4. Exit survey responses

Finally, in Table 8we summarize the exit survey responses. They should be interpreted with caution, because subjects might have aimed to respond in a way that is consistent with their experimental behaviors rather than providing true responses.

In the "Judge" row, we present the mean values for the question "How would an unbiased judge who knows how much money you earned on the image-labeling task and how your wage was determined judge your income reporting behavior?" We find no treatment differences, $W(3,111) = 0.37$, $p = 0.77$.[11] Similarly, in the second row, we observe no treatment differences in subjects' agreement on "It is ethical to report less than the true income if A earned less on the image-labeling task than his/her entitlement", $W(3,113) = 1.43$, $p = 0.24$. In the third row, we present the mean responses to the question "To what extent did you anchor your income-reporting behavior on your beliefs about how much your peers report?", and again we find no differences in treatment means, $F(3,218) = 0.84$, $p = 0.17$.

Unsurprisingly, we find a treatment difference when it comes to evaluating the fairness of earnings; see the last row of Table 8. A linear regression (overall model $LR\chi^2(3) = 82.74$, $p \leq 0.001$) of the perceived fairness of the earnings variable on the two experimental factors and their interaction shows that the presence of *Inequity* decreases the mean reported fairness by an average of 0.91(B = 0.91, 95%CI [-1.26, -0.55], $p \leq 0.001$). From the significant *Intent X Inequity* interaction, we see that the effect of *Inequity* is more extreme in the presence of *Intent*, as the fairness ratings are on average lower by 0.69 (B = - 0.69, 95%CI [-1.20, -0.17], $p \leq 0.01$) in the *Intent – Inequity* treatment. In other words, consistent with our expectations, subjects rated their earnings as least fair in the main experimental treatment.

[11] When necessary, we report the Welch-statistics to account for unequal variances.

6. Discussion

We manipulated the causes for receiving a wage below the entitlement. We tested whether tax compliance and beliefs about the similarly treated peers' compliance level are lower when the cause for the lower wage is an intentional human action than when it is due to randomness. Our key findings are that, when the disadvantageous wage arose due to a random process, neither increased tax evasion nor corrupted beliefs about peers' compliance levels resulted. By contrast, when the same wage was attributable to a deliberate human choice in which the decision-maker benefited at the expense of the wronged, tax compliance decreased and beliefs eroded.

These divergent effects of the different causes of a disadvantageous wage suggest that it is not inequity *per se,* nor is it the corresponding failure to reach an expected income level, that deteriorate compliance. Instead, what matters is whether inequity stems from an intentional human choice. This pattern eliminates the alternative explanation that tax evasion is driven by failure to reach a target wage, and it corroborates Houser et al. (2012) in documenting that experiencing intentionally imposed unfairness corrupts ethical preferences. Our findings also confirm the warning of Elster (1989) regarding the spillover of incidental, unfair treatments into behaviors that at least partly rely on ethics.

One practical implication of these results is that resources allocated towards setting and enforcing equitable wage policies might pay off for the entire society, as they could mitigate the likelihood of compensation-seeking via cheating some aspects of the social system. Moreover, they underscore the importance of institutions that support workers in attaining fair wages and fair practices in general and maintaining them over time.

An interesting general feature of our results is the high compliance levels in all but one treatment. In fact, only in the *Intent – Inequity* treatment did we find a mean compliance level (of 44%), which is more commensurate with the usual mean of 50% reported in tax experiments (see, e.g., Alm, 2019). The observed high levels in the other three treatments (ranging between 70% and 85%) are commensurate with the levels reported by Pántya, Kovács, Kogler, & Kirchler (2016) on a similar Hungarian population. Nonetheless, despite these high compliance levels, we still observed a drastic decrease in compliance in the *Intent – Inequity* treatment, consistent with our main prediction.

Another interesting side result (similar to what is reported by Dezső & Loewenstein (2019)) is that levels of effort provision did not differ across treatments. This suggests that knowing that one will receive a disadvantageous wage before engaging in the effort provision did not cause an effort decrease on the real-effort task. In other words, subjects did not decrease their efforts in response to the exogenous variations of the incentives. This could be because in our setup there was no reciprocal relationship between Type A and Type B subjects. Consequently, decreasing effort provision as a result of receiving the intentional wage inequity could have risked failing to complete the image-labeling task. In this event, subjects would have not earned even the low wage — which event would not influence Type B subjects' monetary outcomes. This is a key difference from principal-agent setups, where decreased effort from the agent upon some kind of perceived unfairness imposed by the principal could be seen as negative reciprocity with monetary consequences for both.

Additionally, and similar to existing research on tax compliance (Lefebvre, Pestieau, Riedl, & Villeval, 2015) and also on unethical behaviors (e.g., Houser et al., 2012), we documented three compliance types. To explore this heterogeneity, we conducted ZOIBR, which we believe is a novelty as, to the best of our knowledge, no prior tax research explored this heterogeneity with tools including ZOIBR. This procedure enabled us to examine how the three compliance types are affected by the experimental manipulations, and also how gender and risk preferences affect the three types separately. To this end, we have findings that may provide hints in reconciling some contradictory results about the effects of risk preference and gender on tax compliance.

Using ZOIBR, we found that the experimental manipulations only affected the compliance levels of partial compliers. Among this type, receiving a disadvantageous wage decreased compliance when it was due to an intentional human choice but not when due to chance. At the same time, the compliance level of partial compliers was not affected by gender or risk preferences. However, the odds of becoming a zero complier versus a partial complier, or a full complier versus a partial complier, were directly unaffected by the experimental manipulations (only through beliefs).

With respect to the association between risk preferences and tax compliance, we have several results. First, when ignoring the compliance type heterogeneity and using the conventional Tobit regressions, we find a positive association between risk aversion and tax compliance. These findings are consistent with, for instance, Coricelli et al. (2010), Bruner et al. (2017), and Bernasconi & Bernhofer (2020) but are in contrast to Dulleck et al. (2016). Second, results of the ZOIBR reveal an interesting heterogeneity about this relationship. In particular, risk preference is only associated with the odds of becoming a full versus partial complier such that increased risk aversion is associated with increased odds. At the same time, neither the compliance level for partial compliance, nor the odds of becoming a zero versus a full complier, were found to be associated to risk preferences. These diverse effects of risk preference may explain the existing inconsistencies on this matter documented in the literature. They suggest that risk preference only matters in choosing whether to comply fully or not, whereas the actual level of "not complying" is independent from the taste for risk. To closely inspect the risk and compliance types associations, a meta-analysis that pools many studies could give us important guidance.

We also observed a similar advantage of ZOIBR when examining the relationship between gender and tax compliance. Some tax research reports lower tax compliance among males than females (e.g., Dulleck et al., 2016), whereas some do not find this association (e.g., Hofmann, Voracek, Bock, & Kirchler, 2017). We find that, when ignoring the compliance type heterogeneity, we see lower compliance among males than females. However, when looking into the heterogeneity, we find that gender is only associated with becoming a full versus partial complier, such that males have lower odds than females. Again, perhaps, the inconsistent results reported in the literature may be due to the divergent association between each compliance type and gender.

With respect to the effect of the treatment manipulations on beliefs, we find that beliefs about the peers' compliance levels are the lowest after being subjected to an intentionally imposed wage inequity, and the strength of the association between beliefs and compliance did not differ between treatments. This latter finding suggests that the beliefs-compliance relationship was not different in either treatment. At the same time, the observed eroded beliefs explained one quarter of the decreased compliance observed among those who were subject to the intentional wage inequity manipulation. We argue that these results are worrisome for two reasons.

First, it is disconcerting how swiftly individuals adjust their beliefs about the prevailing compliance norms after being wronged once. Perhaps, when individuals observe another person violating the implicit norm of fairness to their detriment (when imposing the unfair wage on them), they infer that this behavior liberates everyone from the expectation of behaving ethically — almost as if there was a cross-norm inhibition in place (Keizer, Lindenberg, & Steg, 2008). Second, the intimate relationship between beliefs and compliance uncovered in the mediation analysis indicates that a strong beliefs-compliance association instantly emerges even in the laboratory (stripped of context), and people readily adjust their behaviors to these instantaneously formed, corrupted beliefs. We suspect that this mechanism can easily give fertile ground for a downward spiral, eventually making inequitable wage practices costly for the entire society.

Perhaps, capitalizing on research on conditional compliance, there may be a silver lining here. Policy could advantageously steer tax compliance through sophisticated management of beliefs. For instance,

when beliefs are corrupted among members of certain communities, in order to mitigate the potential risk of conditional compliance, they can set high paying members as examples with the hope that their compliance levels become salient enough to influence beliefs of the other members.

6.1. Limitations

There are some reasons for caution in interpreting our results. A common concern is whether laboratory experiments possess the necessary characteristics for external validity, given (among other features) their use of student subjects. However, there is evidence that the behavior of student subjects in laboratory experiments (including in tax compliance experiments) is not significantly different from that of non-student subjects, either in the laboratory or in similarly structured real-world settings (Alm, Bloomquist, & McKee, 2015; Dai, Galeotti, & Villeval, 2018).

Moreover, we adhered to a somewhat unusual experimental design by employing a one-shot income reporting game versus the multi-shot game more common in laboratory studies on tax compliance. We believe that in a multi-shot game the wage inequity manipulation could have potentially dissipated. Additionally, by using a one-shot income reporting game, we eliminated the effect of dynamic learning, which could have systematically interacted both with compliance choices and belief formation.

Furthermore, as we had no econometric model to estimate beliefs, we cannot claim a causal relationship between beliefs and compliance. We found that being subject to wage inequity causes the formation of eroded beliefs about the prevailing compliance levels of the peers and, based on the results of the counterfactual-mediation analysis, that one quarter of the decreased compliance is driven by the decreased beliefs in the main treatment. One should also bear in mind that, due to the lack of the redistribution for subjects in our experiment, the strategic element of forming corrupted beliefs that could justify low compliance (and free-riding) was absent in our design. In other words, the beliefs we elicited were measures about subjects' views about how their peers behave. Going into a more detailed investigation of how norms form, how they enter into the utility function, and estimating their weights would exceed the ambitions and scope of the current research. Instead, we solely focused on how beliefs about peers' compliance level change as a result of experiencing disadvantageous distribute outcomes and to what extent these beliefs mediate the changes in compliance levels.

Finally, one may claim that wage inequity (i.e., having gotten a wage less than the entitlement) is not separable from wage inequality (i.e., Type A made less than Type B). As we have laid out in the sections detailing research on distributive outcomes and their perceptions, once inequity arises then inequality necessarily follows, as the money that is not given to the person who is entitled to the money is instead given to somebody else. This feature not only mimics real life situations (i.e., when there are beneficiaries and victims of wage inequity) but it also ensures that efficiency concerns do not confound measured behaviors in empirical research. Consequently, the effect of inequity is not separable exogenously from inequality without introducing confounds such as, for instance, concern for efficiency, or without losing experimental control by allowing beliefs to be formed about where the money has gone, or without employing deception by not having a real Type B subject in the *Intent* treatment arms.

6.2. Concluding remarks

Preferences for honest tax payments — although unarguably influenced by risk preferences — are malleable, and depend on factors independent of the objective and perceived characteristics of a particular tax regime. In fact, tax compliance can be directly shaped by the wage allocation norms imposed on taxpayers, and, beyond that, they can be indirectly affected by beliefs about the peers' compliance levels, which are also susceptible to wage allocation norms. These arguments suggest that wage policy choices and their enforcement matter not only for individual wealth outcomes, but that they also have significant welfare implications and ultimately shape the institutions that a society creates. Consequently, the effects of inequitable wages that we identified in this experiment are both important and concerning. They thus call for measures promoting and enforcing equitable payments. Enforcing equitable payments, especially for those in vulnerable positions, should pay off not only at the individual level but also for the entire society.

Declaration of Competing Interest

None.

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Appendix A. Experimental Material

See supplementary material for the complete experimental material in the original language (Hungarian) and their English translations.

Appendix B. Additional Results

As one can see from Table B1, Type As and Type Bs do not differ with respect to their key demographics, indicating that the Type randomization was successful.

In Table B2 , we present results of the manipulation checks after controlling for key demographics. As one can see from this table, results are robust after controlling for age, gender, and income level. Additionally, males are less satisfied with their earnings than females, a year increase in age is associated with lower satisfaction, and perceived fairness and an increase in income level is associated with increase in reported satisfaction.

In Table B3 we present Type B subjects' mean compliance levels and beliefs.

In Table B4 , we present Tobit regressions of beliefs. We find a marginally significant *Intent X Inequity* interaction when modeling beliefs with Tobit regression. By attentive sessions we mean only to include those sessions where all subjects passed the attention check. As one can see from Table B5, when only including these sessions, we still get our main result, the *Intent X Inequity* interaction.

Table B1
Detailed sample demographics.

	Type A	Type B	Statistics
Age in years Mean (SD)	21.23 (2.03)	21.74 (3.38)	$F(1, 264) = 0.175$, n.s.
Male N (%)	125 (56.3%)	25 (54.3%)	χ^2 (1) = 0.059, n.s.
Income level Mean (SD)	2.77 (0.71)	2.63 (0.68)	$F(1, 266) = 1.58$, n.s.
Education level Median	1	1	Median Test = 0.522, n.s.
N	222	46	NA

Notes: SD stands for standard deviation. Recall that two Type A subjects ages are not included as they indicated 1900 to be their birth years. "Income level" codes are: 1 - Bottom 25%, 2 - Second 25%, 3 - Third 25%, 4 - Top 25% of the Hungarian population. "Education level" codes are: 1- High school grad, 2 - College/BA/BSc, 3 - University/MA/MSc.

Table B2
Summary of OLS regressions (with robust standard errors) of satisfaction with and perceived fairness of the image-labeling task earnings on experimental factors and their interaction including key demographics.

	Satisfaction	Perceived fairness
Intercept	0.486 (0.330)	0.928 (0.330) **
Intent	0.230 (0.165)	0.095 (0.162)
Inequity	–1.003 (0.179) ***	–1.012 (0.159) ***
Intent X Inequity	–0.544 (0.238) **	–0.527 (0.245) *
Gender = male	–0.233 (0.117) *	–0.167 (0.120)
Age in years	–0.010 (0.005) *	–0.010 (0.002) ***
Income level	0.187 (0.092) *	0.118 (0.103)
LR χ^2	108.18 ***	105.06 ***
Df	6	6
N	222	222

Notes: Standard errors are in parentheses.
***$p \leq 0.001$, ** $p \leq 0.01$, * $p \leq 0.05$

Table B3
Type B subjects' gamble choices, compliance levels and beliefs.

	Gamble choice	Compliance	Beliefs	N
Nature-Equity	3.92 (1.38)	0.41 (0.47) [0.13, 0.70]	0.53 (0.32) [0.33, 0.72]	13
Nature-Inequity	3.70 (0.95)	0.90 (0.27) [0.70, 1.09]	0.60 (0.29) [0.39, 0.82]	10
Intent-Equity	2.89 (1.05)	0.84 (0.30) [0.61, 1.08]	0.65 (0.16) [0.52, 0.77]	9
Intent-Inequity	3.07 (1.38)	0.73 (0.42) [049, 0.97]	0.63 (0.22) [0.50, 0.76]	14

Notes: The table presents means, SDs in parenthesis and 95% CIs in squared brackets.

Table B4
Summary of Tobit regression results of beliefs on experimental factors and their interaction.

	Beliefs
Intercept	0.629 (0.038) ***
Intent	0.017 (0.059)
Inequity	0.025 (0.057)
Intent X Inequity	–0.140 (0.082) +
Log-Likelihood	–61.46
LL χ^2	6.51 +
Df	3
N	222

Notes: Standard errors are in parentheses. ***$p \leq 0.001$, +$p \leq 0.10$.

Table B5
Summary of OLS regression results of beliefs on experimental factors and their interaction on the restricted sample of subjects in attentive sessions.

	Beliefs
Intercept	0.615 (0.041) ***
Intent	0.042 (0.056)
Inequity	0.053 (0.059)
Intent X Inequity	–0.182 (0.080) *
LR χ^2	7.79 *
Df	3
N	190

Notes: Standard errors are in parentheses.
***$p \leq 0.001$, *$p \leq 0.05$.

Supplementary material

Data associated with the experiment can found at https://github.com/leberkase/InequitableWagesTaxEvasion. Supplementary material associated with this article can be found, in the online version, at 10.1016/j.socec.2021.101811

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