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# Introduction

When intending to raise tax revenues, governments often need to make decisions that may be difficult: which groups in society should bear the lion's share of the tax burden? Who should benefit, and in what proportions, from the resulting governmental expenditures? Since these government expenditures typically involve redistributional elements to a large extent, that is – spending tax income for social security, public health, education, preservation of the environment, et cetera – the answer to these questions often implicitly reflect the amount of inequality a state is willing to accept.

Governments are, however, generally not well advised to simply increase taxes in line with what is often required to meet politically tempting promises. Reducing individuals' after-tax income or taxing general wealth risks substantial responses: Productive individuals may perceive a lower incentive to generate income and could therefore opt to work less and enjoy more leisure. They may also put a lot of effort in reducing their tax bill by different means, such as paying smart tax advisors, moving their money or even themselves to a different country or even engage in illegal tax evasion. As a consequence, the amount of what they pay and what is eventually available for redistribution sizably diminishes.

Public Economics describes this fundamental conflict in terms of the tradeoff between equity, collecting tax revenues to avail more funds for redistribution via government spending; and efficiency, exploiting the full economic potential to maximize overall income. The theory of optimal taxation considers both sides and aims to find the optimal balance between these two goals. For this, the field of normative Public Economics speaks to the equity side. It raises the question of which redistributive objectives a society wants to realize and how best to achieve them. In short, normative economics expresses the "taste" for redistribution in a social welfare function that defines the efficiency costs a society is willing to accept in order to arrive at a more equal distribution.

In principle, these social welfare functions are not given, but result from moral philosophical reasoning. Practically, however, most standard models follow a weighted objective between a "Benthamite" (Bentham, 1789) Utilitarian- or a Rawlsian (Rawls, 1971) rationale, simply describing the different levels of inequality aversion in a society. More recent theoretical developments propose alternative normative conceptualizations for these objective functions (e.g. Saez and Stantcheva, 2016) considering also other fairness or justice criteria. The field of positive economics, on the other hand, addresses and quantifies the efficiency costs of redistribution. Behavioral responses to taxation are empirically examined

to conceptually distinguish the different mechanisms affecting the tax base. In particular, these are distortions of economic behaviors, usually categorized along the dimensions of (negative) labor supply responses (Mirrlees, 1971; Sheshinski, 1972) and avoidance/evasion decisions (Allingham and Sandmo, 1972).

This dissertation can be structured along these two dimensions, and consists of three self-contained chapters that investigate underlying, more normative preferences and, from an efficiency perspective, revenue implications of tax evasion. With the use of controlled experimental settings, this thesis contributes to both normative as well as positive aspects of Public Economics. Chapter 1 and 2 speak more to the normative pillar and explore public preferences towards taxation of wealth. Specifically, **Chapter 1** explores *how* individuals prefer to tax wealth, asking, aside from misinformation and misperceptions that may play a role, are there any design-specific wealth tax preferences? Further exploring public preferences for wealth taxes, **Chapter 2** narrows in on misperceptions about wealth tax instruments. To which extent are misperceptions of fairly complex burden-implications of wealth tax instruments driving preferences for (or against) such taxes? **Chapter 3** addresses the positive pillar of Public Economics. It explores potentially positive tax revenue implications of tax evasion. Specifically, it asks: Does a tax system that allows for a certain extent of tax evasion yield efficiency gains beyond what a system with full enforcement would achieve?

### The Role of Personal Preferences and Perceptions for Wealth Taxation

The first part of this thesis addresses how personal preferences, (mis)perceptions and norms can inform optimal (tax) policies. As already mentioned above, standard models of optimal taxation express the "taste" for redistribution in social welfare functions (SWFs), which by and large follow either purely Utilitarian or Rawlsian principles. However, these SWFs are not "objective choices", but rather normative ones. Thus far, the field of normative economics based these choices on philosophical reasoning or aimed for Pareto efficiency.

A fairly new strand of research aims to consider public opinion. For this, they derive multiple dimensions of normative criteria from surveys, experiments and existing policies to inform SWFs that incorporate prevailing public attitudes. A natural question regarding this development is why even care about the public opinion? In their seminal work, Alesina and Angeletos (2005) make a compelling case of how hardwired underlying social values already implicitly shape economic policies. They found that the degree of redistribution in a society is highly correlated with the prevalent belief that wealth is mostly determined through luck or effort. This example illustrates how public opinion helps us to understand why current policies are enacted in the first place. Further, public opinion informs us about political feasibility, as future policies are highly dependent on public support. This is especially relevant where general public attitudes differ from those expected by elites. As an example Gaertner and Schokkaert (2012, p. 8) emphasize that "for a majority of citizens subjective utilities are not the exclusive nor even the most important criterion for

evaluating policies". Finally, and along similar lines, the conventional utilitarian approach understates the possibility that a tax system might not only be judged by its allocative achievements, but also by the procedures under which it achieves its goals. Therefore, Weinzierl (2014, p. 3) acknowledges that "incorporating key aspects of reality into the conventional model has been a hallmark of major contributions in optimal tax research [...] and often these efforts have improved the match between the theory's recommendations and real-world policy".

In this spirit, a number of works employ experimental methods to carve out normative criteria suggested by public opinion (e.g. Weinzierl, 2014; Kuziemko et al., 2015; Fisman et al., 2020; Stantcheva, 2020). These papers predominantly explore how public preferences are related to efficiency effects, fairness considerations, views about the government and misinformation. As a common finding, they conclude that efficiency concerns play a minor role in public preferences.

Chapter 1 and Chapter 2 feed into this research and explore if individuals yield tax-design specific preferences, and how profound misperceptions of tax concepts shape preferences for taxation - aspects which have not yet been considered in the literature. For this, my co-authors and I specifically examined the instrument of wealth taxation for two main reasons: First, it is at the heart of the prominent debate on growing wealth inequality (Alvaredo et al., 2013). Proponents argue that only such a substantial tax base qualifies to effectively address the problem of increased economic inequalities (Piketty, 2015b). At the same time, previous research focused either on the taxation of (capital) income or particularly the "hated" estate tax. Therefore, Chapter 1 and 2 aim to contribute to a more nuanced understanding of preferences towards this specific tax instrument. Second, with its particular purpose of taxing the entirety of all personal assets (as opposed to taxing income flows or consumption), it provides an opportunity to zero in on specific design features (i.e. periodicity), which would be less straightforward to examine for other types of more complex taxes.

Specifically, **Chapter 1** of my dissertation raises the question of whether the significant opposition towards the estate tax is applicable to other instruments of net wealth taxation – or if it is rather a reflection of disapproval with procedural aspects, i.e. how the tax is collected. In doing so, we test in particular the presence of framing effects, incidence concentration and the role of wealth characteristics within the different tax configurations. For this, we conducted a factorial vignette survey experiment in the US. Each respondent was randomized into one of four burden-equivalent wealth tax instruments that differed in timing (i.e. one-time wealth tax vs. yearly wealth tax) and framing. Subsequently, we asked each respondent to state their preferred overall lifetime tax burden given the respective tax instrument for a set of hypothetical individuals. Our findings yield several interesting insights. First, we find that the exceptional opposition towards the estate tax is not applicable to other instruments of wealth taxation and is only valid for certain subgroups. In general, our empirical findings provide preferred wealth tax rates

between 12.8 to 14.9% in effective lifetime tax burden. Second, in terms of periodicity, we find strong heterogenous treatment effects along partisan lines. Democrats clearly prefer concentrated over periodic wealth taxes. Republicans particularly reject the estate tax compared to all other wealth taxes - even the perfectly congruent one-time wealth tax. It is important to note that these preferences are expressed in terms of preferred tax burden in absolute terms.[1] We complemented our study with a direct within-subject comparison of tax instruments. Given a fixed (fairly moderate) level of wealth taxation, we asked whether participants prefer a single, concentrated wealth tax or a periodic wealth tax in small installments. Strikingly, in this choice, both Democrats and Republicans favored the periodic tax over a concentrated tax instrument. Against this background, we argue that the indicated (higher) absolute amounts of proposed wealth taxes in our main treatment apparently do not necessarily translate into a preference for one over the other. Analyzing open-ended textual answers on their motivation supports this suspicion: Both Democrats and Republicans perceive smaller payments as easier. Democrats furthermore like that a periodic wealth tax would generate an immediate and consistent revenue flow to the government. Finally, we uncover the influence of normative preferences for specific design features on the support for a wealth tax. Proposed effective tax rates of the estate tax and the one-time wealth tax show significant progressivity, whereas no progressivity can be observed for both periodic taxes. The presence of children has an especially significantly negative effect in one-off wealth taxes at the end of the lifetime. Chapter 1 deliberately intents to eliminate bounded rationality in comparing the lifetime tax burden consequences of periodic versus concentrated tax instruments.

**Chapter 2**, as a further exploration, aims to quantify the effect of exactly these misperceptions: The individual capacity to form personal preferences constitutes an essential element of the democratic process. At the same time, policies with far-reaching consequences often require profound expertise to craft and enact. Taxation is such an example. Due to its complex character, bounded rationality might induce biases, causing outcomes other than intended. This chapter quantifies shifts in stated preferences for wealth taxation caused by misperceived burden consequences of commonly politically discussed tax parameters: tax allowances and tax rates. For this, we conducted a randomized survey experiment in a 2 by 2 design. Our respondents were randomly selected to indicate both their preferred tax allowance and tax rate for either a yearly or a one-time wealth tax. Our treatment group was provided with easy-to-understand information on the resulting effective lifetime tax burden for the respective instrument. We find the preferred effective tax rate to drop significantly and substantially for a yearly wealth tax if our participants are fully informed, whereas we do not find this effect for the one-time wealth tax. Interestingly, even if misperceptions are resolved through our information treatment, our respondents still prefer the yearly wealth tax over a one-time wealth tax. The preferred effective tax burden of a yearly wealth tax is about 25 percentage points higher (40.0%

[1] We did this deliberately to cancel out differential cognitive loads for the one-time and periodic taxes.

vs. 15.2%). As we know from Chapter 1, proposing higher effective tax rates for a tax instrument does not necessarily translate into a preference for one over the other. Like in the previous study, we complemented our design with a within-subject question subsequently to the main part of our survey. In contrast to the previous part of the survey that focused on a single type of tax, our respondents in this setting face more the direct comparison: the one-time wealth tax of €X against the yearly wealth tax of an average of €X *per year*. Now, the direct comparison supports our main treatment finding: Respondents still clearly indicate a preference for a yearly wealth tax. Our respondents also briefly reasoned their choice in an open-ended survey question. The preference for a yearly wealth tax is mainly explained by a general preference for smaller payments compared to one concentrated payment, and also by continuous and predictable tax revenues for the government.

In conclusion, how do these two closely related chapters speak to each other? Prominently, we document a significant and positive preference for wealth taxation in both studies. However, the preferences for specific tax instruments seem to be diametrically inconsistent across the two studies at first sight: In Chapter 1, part of our participants seem to prefer concentrated taxes over periodic ones, while others are indifferent. In Chapter 2 the preference was clearly in favor for a periodic wealth tax. Nonetheless, it is striking how in both studies, the direct within-subject comparison of either a concentrated or a yearly wealth tax revealed a clear preference for the periodic tax. This is so even for Democrats, who in Chapter 1 favored the concentrated taxes over periodic taxes (in terms of their proposed absolute lifetime tax burden). We rationalize these findings along two leads. First, the interpretation of preferences for one tax over the other in terms of proposed lifetime tax burdens seems to be insufficient. It is more likely that preferences for a tax instrument are based on an interaction of both overall tax burden and perceived "bearability" of the tax payments. Second, the salience of the partitioned small payments of the yearly wealth tax was much higher in Chapter 2 than in Chapter 1, in which we asked for a single absolute amount of liability for the yearly wealth tax (in an attempt to eliminate differential cognitive burden). Taken together, we suspect that the preference for a number of smaller payments dominates other expressed advantages of a concentrated tax, i.e. the simpler enforcement, smaller evasion opportunities and the unrestricted control over one's assets during lifetime. Overall, it appears that Democrats are well aware of a lot of aspects that speak to a concentrated tax. Yet, if the smaller periodic payments are more salient, then the sentiment turns. While we are not able to fully derive these intuitions in a causal manner[2], we perceive this as an interesting path for future research.

Along these lines, two main limitations of these chapters need to be pointed out. First, as already mentioned above, the interpretation of preferences elicited either in terms of mere absolute amounts or in (small) percentages is likely to be biased. A more comprehensive approach should explore the interaction of misperceived tax implications and

[2]We also acknowledge the different samples of Chapter 1 (US) and Chapter 2 (German). However, we kept our design highly abstract and asked for same tax instruments.

practicability considerations. To our knowledge, this is not fully reflected in previous attempts of the literature to elicit preferences for different types of taxes, since the convention of *how* to ask for these preferences does not follow a standardized path thus far. Second, our experiment builds on the strong assumption of the comparability of a yearly and a one-time wealth tax. Indeed, differential beliefs about differently induced savings or consumption behavior and specifically, entitlement of future generations might be important in comparing these instruments. However, our research design attempts to address these concerns by purposely employing abstract wording and asking for a hypothetical individual with determined behavior, considering only a single generation. Nonetheless, we are not able to fully cancel out such non-captured differential fairness preferences.

Finally, we could draw a rather anecdotal conclusion on how to design a politically feasible wealth tax, if one must. First and foremost, a wealth tax should be periodically pointing out how small single payments would be in comparison to the high levels of wealth. Even better, an inequality averse politician would talk in terms of small percentages about such a tax rather than total amounts. Further, we can conclude from our first experiment that such a yearly wealth tax is less progressive (as a concentrated would have to be) and luckily gained assets are taxed higher than those obtained by effort. Finally, a periodic wealth tax does not need to consider the strong intergenerational transfer motive. The further the intergenerational transfer is away from the tax incidence, the less children have to be taken into account. In that sense, a yearly wealth tax might spare the government high tax allowances for family members. All in all, it seems that a politically feasible periodic tax could be designed in a simple and easy-to-understand way, which voters potentially further favor – in contrast to other, more complex tax instruments.

**Tax Evasion and Its Hidden Economic Benefits**

**Chapter 3** departs from the normative pillar of public finance and explores the welfare implications of behavioral responses to tax evasion. Resuscitating the both intriguing and counterintuitive conclusions by the theoretical work on "randomized taxation" by Weiss (1976) and Stiglitz (1982), we test the hypothesis that an optimal income tax might include incentives to evade taxes in order to increase overall tax revenues: Such incentives partially offset the undesirable distortions of taxes on labor supply by lowering effective tax rates. We implemented an original real effort experiment in an online labor market. Our findings show significant positive labor supply responses to the opportunity to evade. More importantly, the expected tax revenue significantly and substantially increases. Strikingly, this effect persists when comparing effective tax rates: Lowering effective tax rates through the opportunity to evade is more efficient than simply lowering statutory tax rates.

This finding hinges fundamentally on the mechanism of differential perceptions of tax evasion opportunities in contrast to a simple increase of the net wage rate. The deliberate decision to evade taxes is associated with costs due to risk. An explicit equivalent reduction in the statutory tax rate is, however, costless for the individual. Whereas the theoretical literature perfectly described the conditions for our results to occur (Weiss, 1976; Stiglitz,

1982), it nonetheless deemed the empirical relevance as highly unlikely (Hellwig, 2007). Through this lens, there might be more to this theoretical intuition than previously captured. For example, the problem can be read as a sub-question of the large literature on optimal decision-responses to risk (e.g. "precautionary saving"), "prudence" and higher-order risk aversion. This chapter leaves these underlying mechanisms remaining somewhat in a black box. However, our original insights show that the benefits of inducing after-tax uncertainty via incentives to evade should not be dismissed as a mere theoretical curiosity, but need to be explored further.

**Declaration of Co-Authorships.** All three Chapters of this dissertation are the result of collaborative efforts. In **Chapter 1**, I collaborated with Malte Chirvi. For this paper, the initial idea and the research design were carried out in proportional parts. I led in delivering the literature review, the implementation (coding of the experiment and data collection), the discussion and the textual sentiment analysis. This chapter is published as arqus-Discussion Paper No. 242. In **Chapter 2**, I collaborated with Malte Chirvi and Hans-Peter Huber. For this paper, I led in delivering the literature review and discussion. The initial idea, research design and data collection were executed in equal parts. This chapter is published in the "TRR 266 Accounting for Transparency Working Paper Series" (No. 54). In **Chapter 3**, I collaborated with Wladislaw Mill. For this paper, I delivered the initial idea. I led in conducting the literature review, discussion and implementation (coding of the experiment and data collection). The experimental design and the theoretical intuition were developed in equal parts.

# Chapter 1

# Preferences for Wealth Taxation – Design, Framing and the Role of Partisanship

Against the background of an increased concentration in private wealth (well documented by e.g. Alvaredo et al., 2013; Saez and Zucman, 2016), the discussion about the taxation of wealth is of growing prominence in both the academic as well the public policy sphere. Wealth taxes are substantially different to any form of income or capital income taxation as they do not tax income *flows* but rather target the entire *stock* of all financial and non-financial assets after the deduction of all debts.[1] According to its proponents, it is only such a substantial tax base (unlike income or consumption taxes) which qualifies to effectively address the presumed increasing wealth inequality.[2] Against this background, a recent piece by Saez and Zucman (2019) points out how a well enforced wealth tax would be an important component to restore overall progressivity of the US tax system, especially at the top of the US income and wealth distribution.

Aside if one deems these arguments as convincing from an economic point of view,[3] it has to be noticed how wealth inequality increasingly governs public debates and consistently put the taxation of wealth on the political agenda. Still, it is not given that an increased concern about inequality would necessarily translate into stronger political support for wealth taxation. Public discourse on redistribution may be dominated by highly politicized debates on specific tax instruments, especially to those currently in

[1] This type of tax instrument is often referred to as "net wealth tax", "net worth tax" or also "capital tax" in the literature. If not otherwise denoted, the term "wealth tax" refers to this definition in this paper.

[2] Prominently Piketty and Zucman (2014) argue that a fundamental gap between returns on capital and growth rates of economies is a central driver of wealth inequality: Their empirical analysis revealed how wealth distribution within each age group was substantially more unequal than the income distribution. Therefore, Piketty (2015a) proposes a progressive wealth tax to stabilize the level of wealth concentration.

[3] Indeed, the literature in within the classic optimal tax framework yields inconclusive results on the desirability of wealth taxation. The classic conclusion of Atkinson and Stiglitz (1976); Chamley (1986); Judd (1985) implying an optimal capital and wealth tax of zero. The recent framework of Saez and Stantcheva (2016, 2018) suggest a positive wealth tax.

place,[4] coupled with well researched strong misperceptions about (personal) social mobility.[5] Through this lens, it is not surprising that the current worldwide trend points towards repealing instruments of wealth taxation (Drometer et al., 2018),[6] contrary to predictions of standard political economic analysis (Meltzer and Richard, 1981).

Indeed, recent research finds public preferences for a positive, and even fairly high, taxation of wealth (Fisman et al., 2020; Kuziemko et al., 2015).[7] A remarkable amount of suggestive evidence supports this finding: prominent candidates for the 2020 Democratic presidential nomination enjoy popularity in their demand for a progressive wealth tax, a recent poll reports that 60% of respondents endorsed the idea of a 2% annual wealth tax on wealth above $50 million.[8] In face of the economic costs of the recent COVID-19 pandemic, a one-time wealth tax is discussed – an intervention that Donald Trump himself proposed back in 1999 to cut public debts.[9] In stark contrast, the wealth tax currently in place - the estate tax[10] - is one of the most controversial and emotionally discussed taxes, across the entire political spectrum (Krupnikov et al., 2006). Indeed, previous empirical studies document an exceptional unpopularity of this tax and attribute this mostly to misinformation (Slemrod, 2006; Krupnikov et al., 2006; Kuziemko et al., 2015). As a result, the estate tax is continuously contested and subject to substantive legal changes over the past decades: besides an intermediate repeal in 2010, top tax rates dropped from 55% in 2001 to 40% in 2020 whereas allowances increased from $675,000 to $11.58 million.

Given such a highly politicized debate, it remains an open question whether this opposition actually mirrors a general public reservation towards the taxation of wealth. It is in this spirit when the Forbes magazine asks *"Why Do People Hate Estate Taxes But Love Wealth Taxes?"*[11]

This peculiarity presents an interesting research opportunity as it raises a couple of questions for public economists: Is the opposition towards the estate tax originated in missing general support for wealth redistribution or is it rather a result of specific design features? What are those specific design features? The context of death? The taxation

[4] Papers show how the specific design of taxes, misinformation, and framing might outweigh general support for redistribution (e.g. Fisman et al., 2020; Kuziemko et al., 2015; Bartels, 2005).

[5] Piketty (1995) and Benabou and Ok (2001) establish the so-called "POUM" (prospect of upward mobility) hypothesis: individuals systematically overestimate their probability of upward mobility, so that they prefer less redistribution.

[6] Drometer et al. (2018) analyze 26 OECD countries and illustrate how seven OECD economies abolished periodic net wealth taxation over the past 15 years and only three countries still maintain such a tax: Switzerland, Norway and Spain. Estate taxes or corresponding inheritance taxes are still levied in two thirds of the analyzed OECD countries. However, also a large number of countries (Austria, Czech Republic, New Zealand, Norway, Portugal, and Sweden) recently abolished them.

[7] Fisman et al. (2020) reports preferred yearly wealth tax rates between 0.8% and 3.0% for wealth from saved incomes.

[8] Reuters/Ipsos poll see https://www.reuters.com/article/us-usa-election-inequality-poll/majority-of-americans-favor-wealth-tax-on-very-rich-reuters-ipsos-poll-idUSKBN1Z9141 (08.07.21).

[9] Specifically he proposed a "one-time net worth tax" of 14.25% on individuals and trusts worth $10 million or more. Notabene: a proposal much more severe than the tax plans of Elisabeth Warren. See https://edition.cnn.com/ALLPOLITICS/stories/1999/11/09/trump.rich/index.html (08.07.21).

[10] A transfer tax based on the overall value of wealth left by a decedent.

[11] See https://www.forbes.com/sites/taxnotes/2019/10/30/why-do-people-hate-estate-taxes-but-love-wealth-taxes/ (08.07.21).

of intra-family transfers? The concentrated incidence at only one point in life? The comprehensive character of the tax base (i.e. the type of assets affected)? Its (missing) progressivity? This paper aims to answer the questions raised - beyond the already well documented opposition due to misinformation. Therefore, we present, to our knowledge, the first investigation of how individuals' preferences for wealth taxation depend on the specific configuration of the wealth tax instrument.

For this, we conducted a factorial vignette survey experiment with over 3,200 respondents on Amazon's Mechanical Turk (MTurk). Each respondent was randomized into one of four wealth tax instruments: an estate tax, a one-time wealth tax, a decennial wealth tax or a yearly wealth tax. Each respondent was presented a series of hypothetical individuals that differed across four dimensions: level of wealth, type of assets, source of wealth and the number of children. Our subjects were then asked to state their preferred overall life-time tax burden in absolute terms (amount in USD) for each case presented in the assigned tax instrument. In doing so, our participants implicitly design an own tax system *T_instrument (level of wealth, source of wealth, type of assets, number of children)*. Comparing tax instruments - which are otherwise hardly comparable - constitutes an important property of our experimental design: Asking only for the preferred overall lifetime tax burden eliminates potential difficulties to translate periodic tax rates into concentrated tax rates (i.e. bounded rationality).[12] In that sense, the different tax instruments become equivalent - only differing in their name. Moreover, asking our subjects to construct an individual and personally preferred tax system immunizes our experimental design against misinformation and other biases towards existing tax instruments. Finally, we unambiguously state that this study assumes no behavioral response whatsoever to the final wealth of a person and the absence of other wealth taxes, the understanding of which we test in multiple comprehension control questions.

Based on this, we test our results against the following standard economic assumption: Given burden equivalence, tax preferences should not differ across wealth tax instruments. In particular, we investigate the following three hypotheses:

**Hypothesis 1.** *Preferences for an* **estate tax** *compared to an equivalent* **one-time wealth tax** *do not differ as they are perfectly congruent except for their names (i.e. framing effects).*

**Hypothesis 2.** *Preferences for* **one-time** *and* **periodic taxes** *do not differ as we equate their burden by design (i.e.concentration bias).*[13]

**Hypothesis 3.** *The characteristics of wealth (i.e. level of wealth, source of wealth, type of*

[12] For example: Given a positive wealth stock at the age of 30, an avg. growth rate of 3% and no allowance: A yearly wealth tax of 1% would translate into an equivalent estate tax of 33.1%. A yearly wealth tax of 2% would translate into an equivalent estate tax of 55.4%.

[13] Furthermore, our design cancels out any different wealth aggregating effects between the different tax instruments.

*wealth, number of children)*[14] *are not differently decisive for different tax instruments, only for the general level of wealth taxation (i.e. the effect of specific wealth characteristics).*

In taking a more comprehensive view on instruments for wealth taxation, this novel approach has another advantage: It strengthens the robustness of our findings by using different tax instruments as reference points for each other.

Our empirical findings nicely confirm general results of previous literature: Our respondents choose levels of wealth taxation varying from 12.8 to 14.9% in overall lifetime tax burden across tax instruments. Whereas these results may seem relatively high, Fisman et al. (2020) find preferred yearly wealth tax rates between 0.8% and 3.0% that translate into even higher effective tax rates of lifetime tax burden.[15] Further, our results yield proposed tax burdens being higher if assets are accumulated by luck instead of effort (Alesina and Angeletos, 2005). Individuals who are informed about the current legislation and wealth distribution propose significantly higher effective tax rates across all tax instruments (Kuziemko et al., 2015) and Republicans accept more inequality than Democrats (Capellen et al., 2019), thus preferring lower and less progressive tax rates. Moreover, it is especially older respondents and those with own children who strongly oppose particularly the estate tax (cf. bequest motives discussed by Cremer and Pestieau, 2006).

Regarding our first hypothesis, we find strong heterogeneous treatment effects along partisanship. Republicans' articulated preferences refer to the particular rejection of the estate tax: Proposed effective tax rates are significantly lower compared to all other wealth taxes. Especially remarkable is how this rejection does not hold for a perfectly congruent one-time wealth tax. This finding is particularly intriguing since it constitutes novel empirical evidence on hidden emotional charges, potentially triggered by political framing (Birney et al., 2006). As respective framing campaigns have been mainly been launched by the Republican Party and related think tanks, it is hardly surprising that results differ along the line of partisanship: Democrats unambiguously do not differentiate between the estate tax and an equivalent one-time wealth tax.

Regarding our second hypothesis, the differences along the lines of concentrated (i.e. estate and one-time wealth tax) versus periodic (i.e. yearly and decennial wealth tax) taxes, the distinction between partisan lines again reveals significant heterogeneous treatment effects: On the one hand, Democrats clearly prefer concentrated taxes (both the estate and the one-time wealth tax) over periodic wealth taxes *in proposed tax rates.* On the other hand, a clear majority of Democrats prefer a periodic wealth tax over a concentrated tax when these instruments are set in direct comparison (holding the level of taxation constant). Our textual analysis reveals how Democrats like to significantly tax accumulated wealth at the end of ones life and are rather careful with periodic payments to not restrict economic freedom. However, they also state reasons why they still prefer a

[14] As suggested by previous literature. Level of wealth: Kuziemko et al. (2015); Fisman et al. (2020), source of wealth: Alesina and Angeletos (2005); Weinzierl (2017); Almås et al. (2020), type of wealth: Boadway et al. (2010), number of children: Cremer and Pestieau (2006); Kopczuk (2013).

[15] With a time horizon of 30 years, a yearly rate of 0.8% respectively 3.0% would be equivalent to an effective tax rate of 21.4% respectively 59.9%.

periodic wealth tax over a concentrated tax (although not in tax rates): periodic installments are easier to handle as well as they immediately would generate a consistent stream of tax revenues to the government. Republicans are indifferent between the concentrated one-time wealth tax and both periodic taxes but reject the estate tax in proposed tax rates. In line with their preferences in proposed tax rates, they also prefer a yearly tax over a concentrated payment in direct comparison.

Regarding our third hypothesis that addresses the different influence of specific characteristics across tax instruments. We find significant differences between treatment groups with respect to the impact of the level of wealth and the taxpayer's number of children on proposed effective tax rates. In both cases, results clearly differ between concentrated tax treatments on the one hand and periodic tax treatments on the other hand: While no effect can be found in periodic taxes, preferences for higher taxes clearly and significantly increase with the value of assets and decrease with the number of children in the estate tax group and the one-time wealth tax group. These very similar results within the groups of concentrated and periodic taxes strengthen the robustness of our results.

This project first and foremost contributes to the growing literature on the *political feasibility* of redistributive policies (e.g. Bierbrauer et al., 2021), apart from the major literature on efficiency considerations.[16] Only a fairly young strand of theoretical literature bridges the gap between standard models of optimal (wealth) taxation and public preferences,[17] proposing to empirically elicit public attitudes towards redistribution in order to enrich standard models of optimal taxation that eventually translate into tangible tax designs.

In this realm we perceive our contributions as threefold: First, we shed a light on the opposition towards the estate tax beyond the well documented effect of misinformation. The seminal works by Slemrod (2006); Krupnikov et al. (2006) show how a majority of Americans vastly overestimate the share of taxable estates. The same can be found looking at public opinion polls (see Bowman et al., 2017). In a survey experiment, Kuziemko et al. (2015) explore how addressing misinformation on inequality, economic growth and its specific design more than doubled the support for increasing the estate tax.[18] Still, they admit that it remains an open question if addressing this misinformation fully explains the large treatment effect. Our findings propose a more fine-grained picture as they add the effect of political framing apart from mere misinformation to the story. Political and social scientists already mapped out meticulously how policy makers exploit the sensitive context of death through a sophisticated use of rhetoric to gather political majorities to repeal the estate tax (Bartels, 2006; Birney et al., 2006; Graetz and Shapiro, 2006). We,

[16] On the equity-efficiency take off see e.g. Diamond and Saez (2011); Straub and Werning (2020); Piketty and Saez (2013); Kopczuk (2013); on implementation considerations see e.g. Kopczuk (2013); Adam et al. (2011); Bastani and Waldenström (2018); on behavioral responses towards wealth taxation see e.g. Seim (2017); Brülhart et al. (2017); Jakobsen et al. (2018).

[17] See Weinzierl (2014); Saez and Stantcheva (2016, 2018).

[18] In line with former findings, only 12% of the participants answered correctly what share of the population is actually affected.

to our best knowledge, are the first to quantify the emotional load resulting from such framing strategies on the mere name "estate tax" in relation to an equivalent wealth tax instrument absent of this frame.

Second, along similar lines, we find that the exceptional opposition towards the estate tax is not applicable to other instruments of net wealth taxation. While preferences for wealth taxation are primarily discussed against the background of intergenerational wealth transfers (i.e. inheritance or estate taxation) research on preferences for other instruments of net wealth taxation remains limited. Slemrod (2006); Alesina et al. (2018); Bastani and Waldenström (2021) show the particularly strong opposition against the estate tax but do not clearly differentiate, whether this opposition is against wealth taxation in general or the estate tax (as an especially unpopular type of wealth taxation) in particular. Likewise, Fisman et al. (2020) experimentally find significant support for taxing wealth received from bequests on a yearly level. Even though they essentially test the support for a yearly wealth tax (on bequests), they generalize their results to the support for a fundamentally different estate tax. Our study provides a more differentiated view on the preferences for wealth taxation by taking several instruments into account, which serve as reference points to each other and thus provide more robust interpretations of our results.

Third, we uncover the influence of normative preferences for specific design features on the support for a wealth tax. Aside from the research on rather abstract normative concepts (e.g. Weinzierl, 2014), the research on preferences for specific tax design features remains sparse. Three recent papers are important to mention: Bastani and Waldenström (2018) are among the first ones who survey participants on different instruments of wealth taxation: property, inheritance and net-wealth taxation. While they also report a "puzzlingly" strong opposition to the inheritance tax, one of their main findings is that the design and structure of taxes is of prime importance. In case of inheritance taxation, respondents express significantly higher support when only "large" bequests are taxed. For the property tax, a simple name change already has a great positive effect on its popularity. Still, the underlying drivers for perceptions remain unclear and insights beyond the single tax instruments are not inferable due the lack of comparability (i.e. differences in tax levels and tax bases). They conclude that there are "some clues" about mechanisms behind the emotional load of taxing wealth. Along the same lines, Bastani and Waldenström (2021) show how support for an inheritance tax in Sweden increases by 30% in response to an information treatment. Interestingly, they include an additional design-specific dimension by asking about the respondents' support for either a low- or a high-exemption inheritance tax. With a considerably larger support for a high exemption tax they provide further evidence that design features might as well shape preferences. Most closely related to our paper, Fisman et al. (2020) reveal public preferences for jointly taxing income and wealth in an experimental approach. Respondents had to indicate their preferred total tax bill for each one of a series of hypothetical individuals that differed in the levels of income, wealth and sources of wealth. They find preferred wealth tax rates between 0.8% for wealth from saved incomes and 3.0% for wealth from inheritances (per

year). These findings are, however, restricted to a single period of joint taxation of income and wealth without exploring preferences regarding more specific wealth tax instruments. Thus, our paper is the first to elicit directly comparable preferences within the means of wealth taxation, whereas the preceding papers either focus on preferences for estate taxation or rather general preferences to tax wealth. In that sense, this project could be seen as a contribution to the calibration of a "realistic wealth tax system", which "involves a mixture of progressive taxes on inheritance, annual wealth holdings, and annual capital income flows" postulated by Piketty (2015a, p.454).

The rest of this paper is organized as follows. Sections 1.1, 1.2 and 1.3 will give a detailed description of our experimental design, the data and our empirical strategy. In Section 1.4 the results are presented followed by a brief discussion and concluding remarks.

## 1.1 Experimental Design

As outlined above, the main objectives that we had in mind when designing our study are twofold: First, we aim to investigate general preferences regarding the taxation of wealth. Second, we want to explore the specific design features that affect the support for different types of wealth taxes. It is important to mention that we focus on design aspects derived from attributes of taxes that have already been implemented (anywhere) and hence, can be seen as realistic options.

To our knowledge, both objectives have not been sufficiently addressed in the literature yet. Although some studies deal with related research issues in the field of wealth taxation, they are not addressing these fundamental questions. One exception is the recent study by Fisman et al. (2020): Their results can be seen as complementary to ours with respect to the first objective, i.e. the acceptance of wealth taxation in general. The major drawback of other related studies is the focus on one particular type of tax, often the estate tax (e.g. Slemrod, 2006; Birney et al., 2006). This holds true for Kuziemko et al. (2015) who emphasize the robustness of their results by comparing preferences regarding the estate tax and regarding other (not wealth tax related) redistributive measures. However, as the estate tax is described as one of the most controversial and emotionally discussed taxes (e.g. Krupnikov et al., 2006), it is not convincing to derive any statement on preferences regarding the taxation of wealth in general. To be more specific, one had to disentangle the two main sources that lead to observed preferences regarding estate taxes: Support for a wealth tax in general and the potentially deviating support for the estate tax. Furthermore, we do not consider non-tax-related measures as adequate reference points to interpret results regarding the estate tax.

To overcome these issues, we compare preferences regarding various (implementable) types of wealth taxes that differ with respect to fundamental design features: First, we consider the estate tax as well as a perfectly congruent tax, except for its name: a one-time wealth tax that is levied close to one's end of life. Second, besides taxes with a single concentrated payment, we look at another group of wealth taxes that has been

implemented in other countries[19] and is part of the current political debate: The group of periodic wealth taxes. In order to both analyze the effect of different levels of periodicity and to strengthen robustness in findings between concentrated and periodic taxes, we consider two different recurrent tax instruments: One tax that is levied every year (*yearly wealth tax*) and another tax that is levied every ten years (*decennial wealth tax*).

**Table 1.1:** Our Four Different Tax Instruments.

| **Concentrated Tax Payment** | **Periodic Tax Payment** |
|---|---|
| Estate Tax | Decennial Wealth Tax |
| One Time Wealth Tax | Yearly Wealth Tax |

Analyzing different types of wealth taxes enables us to a) disentangle preferences that rely on a specific tax design and for wealth taxation in general, b) reveal whether socio-economics affect preferences differently across tax types and, methodologically important, c) strengthen the interpretation of our results by using different tax instruments as reference points for each other. Furthermore, consistent results throughout similar wealth tax instruments serve as a robustness check and validate our results.

Despite the differences in their implementation, the formal comparability of concentrated and periodic wealth tax instruments is straightforward to demonstrate (see Appendix A.1). However, some issues remain:

- If comparing concentrated and periodic taxes with the same revenue, the amount of each yearly or a once-in-a-lifetime collection differs dramatically. Calculating the total tax burden based on (periodic) tax rates and (periodic) tax exemptions may not be straightforward for an average survey participant. Hence, asking for preferences regarding these parameters may lead to biased estimates of preferences towards different taxes.

- If growth is not fully exogenous, i.e. if the absolute growth in any period depends on the value of assets of the previous period, the burden of a periodic tax consists of two components: the levied tax and a restricted asset accumulation. Hence, the burden of a periodic tax may deviate from its revenue. People, including our survey participants, may therefore assess taxes differently.

- As discussed by e.g. Kopczuk (2013), taxpayers' savings or tax evasion behavior might depend on the design of the specific tax. If survey participants make assumptions about any tax-specific behavior, analyses comparing different tax instruments may be biased.

[19] Countries levying periodic (net) wealth taxes are e.g. Japan (only on real estate and business assets), Switzerland, Norway and Spain (Drometer et al., 2018). See Piketty and Saez (2013); Seim (2017); Bird (1991); Kopczuk (2013) for further discussions.

To avoid these issues, we take several precautions: First, we simply ask for the preferred total tax burden in absolute terms a hypothetical individual should pay in taxes. This reduces the complexity of an otherwise demanding tax computation. At the same time, we ensure that respondents remain aware of difference regarding the (time of) payments (see below). For our analyses we always translate proposed tax burdens into effective tax rates, i.e. the ratio of the proposed tax burden and the value of taxed assets. Second, we describe the wealth accumulating processes in our tasks to be fully exogenous and only specify the wealth of assets at the end one's life. We prefer this approach over an explicit note on endogenous and exogenous growth as it minimizes potential confusion. Third, we clarify some assumptions made at the beginning of our study. These include the absence of behavioral effects with respect to savings and tax evasion. Furthermore, respondents have to prove their comprehension of these assumptions based on control questions.

We use a between design to analyze preferences regarding the four different types of wealth taxes, i.e. every respondent is randomized into one of four groups containing one of the four types of wealth taxes. Subsequently, every respondent is faced with information and questions regarding only this one tax instrument.

The main part of our survey experiment are multiple successive questions on how much wealth taxes should be paid by hypothetical taxpayers given their financial situations. Again, all questions shown to the same respondent contain the same type of wealth taxes, whereas these types may differ between respondents. We are aware that differences in design features of a wealth tax beyond the name and periodicity exist. As these differences are related to characteristics of the taxpayer or assets subject to taxation, we enrich our setting with a vignette design. Using such a design allows us to vary the presented situation with respect to dimensions we expect to be decisive towards the preferences regarding wealth taxation:

- **Value of assets**: A general issue of taxation concerns the progressivity of taxes, i.e. who has to bear what share of the tax burden. This is strongly related to the question, of how people emphasize the redistributive character of a wealth tax. To focus on the taxation of high levels of wealth, we consider only assets worth $1m or more.

- **Type of assets**: Especially with respect to the estate tax debate, people are worried that such a tax might threaten companies and subsequently jobs.[20] Furthermore, the liquidity of assets might affect preferences. Therefore we want to reveal, whether people prefer to differentiate between different types of assets or want an identical fiscal treatment.

- **Source of assets**: As carried out in the literature (Alesina and Angeletos, 2005), the source of assets, especially differentiating between wealth accumulation based on

[20] See e.g. Birney et al. (2006). Bowman et al. (2017, p.62) cite a public opinion poll according to which a great share of those who want to "eliminate" the estate tax are afraid that it "might force the sale of small businesses and family farms".

"luck" and "effort", plays an important role in the context of wealth taxation. In their aim to contrast luck and effort, Fisman et al. (2020) operationalize luckily gained assets as "wealth, accumulated mostly from inheritance [...]". In this paper we want to empirically investigate this claim. Therefore, we add an additional purely "luck" related category.

- **Number of children**: The transfer of accumulated wealth to descendants through lifetime gifts or bequests plays a key role in both the political (Graetz and Shapiro, 2006) and the theoretical debate (Cremer and Pestieau, 2006): parents potentially obtain utility by e.g. protecting their dynasty, exchanging money with elderly care (by their children) or simple altruistic motives ("warm glow of giving").

Each of these dimensions consists of three different categories (see Table 1.2). In total, our vignette universe consists of $3^4 = 81$ vignette options. To avoid confounding of main and two-way interaction effects, the selection of vignettes shown to the respondents was not barely random but based on a randomized block confounded factorial design ($\mathsf{RBCF}{-}3^4$).[21] Generating such a design leads to nine sets that consist of nine vignettes each and are randomly assigned to our respondents.[22]

**Table 1.2:** Our Vignette Dimensions and Respective Categories.

| Dimension | Categories |
|---|---|
| Value of asset | \$1 million / \$10 million / \$30 million |
| Type of asset | Cash / Business shares / Real estate |
| Source of asset | Effort & hard work / Lottery & lucky investments / Inheritance |
| Number of children | None / One / Three |

Note: Overview of the different vignette dimensions and their respective categories.

As already mentioned, every respondent is confronted with only one of the four types of wealth taxes. As the ideas/concepts of the tax instruments may be unknown to the participants, we start our experiment with an explanation of the respective tax instrument. We keep these information lean and do not consider information on institutional or organizational matters. However, as asking for one number may be a little confusing in case of periodic taxes, we give additional interactive information below the answer field indicating the average periodic tax payment: *The Tax Authority charges an average tax payment of* **\$X** *each year/ten years.*

Following this introduction, our respondents are forwarded to the main part of our experiment: We present nine iterations of hypothetical taxpayers and asked for the preferred total amount of wealth taxes that should be paid. It is important to remember that only the vignette dimensions vary within the set of cases. The type of tax always remains the same and are only different between respondents. However, we made sure to standardize

[21] See Montgomery (2017), Chapter 9 and Su and Steiner (2018).

[22] Recent literature suggests nine vignettes being a reasonable number, see Sauer et al. (2011) or Auspurg and Hinz (2014).

the whole text independent of the type of tax to avoid any framing effects. The single questions take the form:

> Consider a person who starts building assets at the age of 30. By the age of 80, the end of his or her life, these assets are worth [asset value]. The assets mainly consist of [asset type] and were mostly accumulated by [source of asset]. The person has [number of children].
>
> If it were up to you, what amount should the person pay in [estate / yearly wealth / decennial wealth / one-time wealth] taxes [at the end of his or her life / over his or her entire life in total / at the age of 80]?

For entering their preferred tax burden, respondents could only type in round numbers with an automatically appearing comma as thousands separator. They were also free to switch between vignettes, go back and adjust their inputs within the set of the nine vignettes.

In a final step, we enriched our study by a within-subject comparison of tax instruments. Participants were asked to state their preference not only for the assigned tax instrument, but also in relation to another tax instrument – including a written motivation of their choice in an open-ended response format. Even though not being part of our main study, this provides first insights about the motives behind and beyond proposed tax rates.

## 1.2 Data

### 1.2.1 Data Collection

Our respondents were recruited through the crowdsourcing marketplace Amazon's Mechanical Turk (MTurk)[23] between November 26 and December 11, 2018. MTurk is an online worker platform, which allows requesters to post human intelligence tasks (HITs) that can be performed by workers who are registered at MTurk and are continuously rated by requesters. These tasks are typically relatively simple and short. Following common practice (e.g. Fisman et al., 2020) we decided for a neutral description when posting our HIT: "Please answer a series of short questions about your personal opinion on capital taxation." Guided by posts in worker forums and other recent studies we set the compensation for completing our survey to $2. Given the median processing time of 10.65 minutes,[24] the payment corresponds to a median hourly wage of $11.27, which can be seen as rather generously compared to other tasks. A share of $1.50 was paid as a bonus only if control questions had been answered correctly in order to incentivize attention during the study.

[23] Link to the survey: https://mpibonn.eu.qualtrics.com/jfe/form/SV_eyq4PeXKh3WxyvP. Screenshots can be found in Appendix A.3.

[24] Only about 5% off all respondents took less than half of the median time and only about 10% took more than twice this time.

The use of MTurk for academic and especially experimental purposes becomes increasingly prevalent with data being at least as reliable as data obtained via standard methods while requiring less money and time for their implementation (Horton et al., 2011; Berinsky et al., 2012). Nonetheless, a couple of well-known issues need to be accounted for in the research design. Most prominently indications for automated scripts ("bots") and the use of Virtual Private Servers (VPSs) by workers outside the US caused a recent decline in data quality (Kennedy et al., 2018). We went to great lengths to consider this concern: First, we implemented basic measures such as limiting the visibility of our survey to participants who signed up at MTurk with a US address and asking to confirm participants' US residency in the consent form. Next, participants had to pass a captcha-test that identifies non-human users on the first page. Moreover, we used a third-party web service, IP Hub, to ex-post identify all participants who used a VPS, VPN or proxy to potentially cover their location outside the US.[25] Furthermore, only workers with an approval rate of greater than or equal to 95% from previous tasks were allowed to participate in this study. To grant access also to the regular working population, we published this study only outside regular working hours. Finally, we prevented workers from participating in our study more than once: Respondents had to enter their unique worker ID on the first page before they were able to start the survey and only at completion received a password to submit to MTurk. We clearly stated that any violation would be penalized by rejecting the HIT which would result in a significant reputational loss for workers on MTurk. Our analysis shows that only a negligible number of workers indeed attempted to participate multiple times and those were excluded from our data analysis.

As part of the introduction we presented some notes on our assumptions:

> **Important:** In this study we assume that individuals' *behavior is not affected* by the existence of taxes. In particular, the estate tax will not affect economic activity, savings behavior, or lead to tax avoidance/evasion. Furthermore, *no other capital taxes are levied*.

Directly below these notes, we asked participants of our survey to evaluate three statements to incentivize re-reading the notes on our assumptions:

1. The existence and the amount of taxes does not affect economic activity and saving behavior.
2. The existence and the amount of taxes does not affect the level of tax avoidance and evasion.
3. The [...] tax AND other capital taxes are levied.

We took an especially conservative approach for our data analysis in monitoring who understood our assumptions instantly: respondents did not receive any feedback on the

[25] Kennedy et al. (2018) show how studies that depend on language comprehension are especially vulnerable to fraudulent IPs outside the U.S.

correctness of their answer and thus had no second guess. As shown in Table 1.3, a significant share of respondents was not able to give correct answers, although we used very similar wording and structure for the text and the subsequent questions. While the comprehension of the third question may affect the general level of proposed taxes, it does not play a role for the main findings of this study. However, assessing the absence of behavioral responses captured by questions 1 and 2 correctly is crucial for the interpretation of our results between tax instruments. To strengthen the robustness of our results, we show in an additional analysis that the estimates for the sub-sample of respondents who answered both questions correctly (54.78% of all respondents) point towards the same direction.

**Table 1.3:** Results of Control Questions.

| **Question** | **Share of correct answers** (in %) |
|---|---|
| 1 | 84.67 |
| 2 | 61.35 |
| 3 | 81.01 |

Note: Share of correctly answered control questions.

Respondents were only considered in our analysis if they met the following data quality requirements. First, respondents had to finish the whole survey. Second, we dropped those respondents, whose answers were inconsistent with respect to our principal question: the tax burden of wealthy individuals. This includes:

1. proposed tax burdens leading to tax rates higher than 100% in at least one of the nine indicated vignettes,

2. proposed tax burdens leading to tax rates higher than 0%, but lower than 1% in all of the nine vignettes and

3. tax burdens following some kind of "random walk" independent of the indicated wealth levels. We assume this if the absolute tax amount for any three vignettes containing the same wealth level was on average higher than the tax burden for the three vignettes containing a higher wealth level.

In all three cases, we assume responses to be insincere as proposed tax levels do not fit the respective wealth levels of the vignettes. Furthermore, we dropped some obvious cases of nonsense like tax burdens of "$1,234" followed by "$5,678". Our final sample contains 18,909 answers of 2,101 respondents (9 vignettes each; see Table 1.4).

**Table 1.4:** Number of Observations.

| Group | Pre-cleaning[26] | Low rates | High rates | Inconsistent | "Nonsense" | Post-cleaning |
|---|---|---|---|---|---|---|
| Estate tax | 792 | 114 | 23 | 103 | 1 | **593** |
| Yearly wealth tax | 782 | 181 | 40 | 128 | 11 | **481** |
| Decennial wealth | 771 | 182 | 37 | 147 | 8 | **469** |
| One-time wealth tax | 785 | 155 | 28 | 95 | 2 | **558** |
| **Total** | **3,130** | | | | | **2,101** |

Note: Number of observations pre- and post-cleaning.

There are some further inherent challenges in interpreting our survey results. First, we cannot fully parse genuine responses from insincere ones. However, we went to great lengths to do so in our data cleaning process and rigorously removed obviously inconsistent answers. Moreover, the analysis of the open-ended answers shows that our respondents indeed took their response quite serious. 99.7% of respondents provided a written answer with an average length of 27 words. We are aware of only one participant providing a copied answer from a website. To the best of our knowledge, answers were given in a genuine colloquial American English minimizing the chances of bots being involved. These open-ended answers furthermore suggest that the assumptions underlying our design (i.e. no behavioral effects and no other wealth taxes) indeed were taken into account by our participants: we found only 72 incidences (3.4% of responses) talking about behavioral responses[27] and no one stating word groups like "other capital taxes" or referring to the current "property tax". Second, preferences stated in survey experiments may deviate from "real-world" (voting) behavior.[28] Third, data gained by a survey experiment might not be representative with respect to the real (US) population. The latter point is linked to the descriptive statistics analyzed in the next chapter.

### 1.2.2 Descriptive Statistics

In Table 1.5 we show summary statistics of our analyzed respondents, separated by treatment groups to verify the randomization process. To evaluate the representativity of our sample, we compare the characteristics of all groups to data from the General Social Survey 2018[29] (GSS; except political preferences) and on votes from the US House of Representatives elections in 2018 (political preferences) to evaluate the representativeness of our sample.

**Bold values** indicate significant (on 5%-level) differences of characteristics compared to those of the US population. Respondents of our samples are younger, have less children,

[26]Respondents, who are US-residents and finished the survey. Based on our initial data we deleted 188 respondents uncovered as users with an IP-address from outside the US.

[27]Specifically, we checked for the words "evasion", "avoidance", "way around", "plan", "defraud" and "loophole". In any case, Capellen et al. (2019) and Fisman et al. (2020) also show that efficiency concerns are not decisive for redistributive preferences.

[28]See Hainmueller et al. (2015).

[29]The General Social Survey (GSS) is a project of the independent research organization NORC at the University of Chicago, with principal funding from the National Science Foundation. A survey "is based on approximately 2,500 face-to-face interviews with a nationally representative sample of English and Spanish speakers who reside in the US"; see https://hub.jhu.edu/2019/04/10/general-social-survey-stephen-morgan/ (26.07.2019).

are better educated and differ regarding ethnicity and political preferences. Furthermore, most treatment groups consist of less female and less married individuals. Obviously, some characteristics as age and marital status as well as children are correlated. Despite of these differences, the randomization process of our survey worked fairly well as only two significant differences between treatment groups occur: the share of respondents with children is greater in the estate group compared to the yearly-wealth group (no differences in the number of children) and we find a greater share of respondents who describe their ethnicity as "white" in the one-time wealth group compared to the estate group. As voting behavior and hence political views differ between different areas of the United States, we also want to check the representativity of our sample in this regard. A comparison of the geographical backgrounds of our survey participants and the US population shows very similar distributions among states (Figure 1.1). Hence, the geographical coverage of our survey worked well. To control for differences between groups, a covariate vector will consider personal characteristics in our regression analyses.

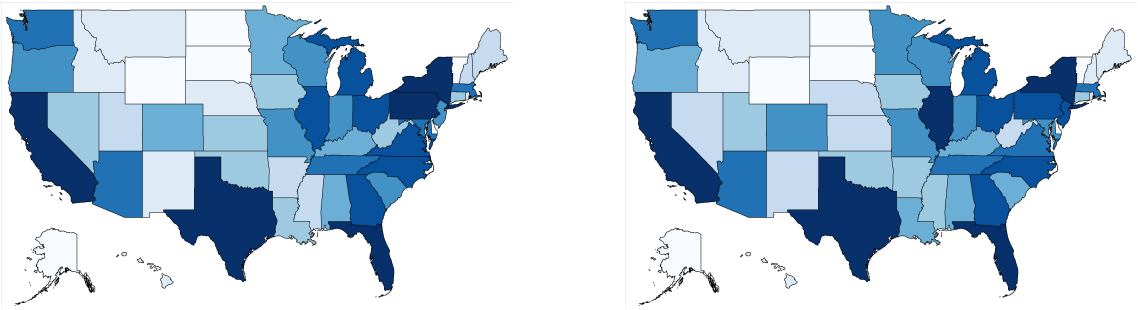
Deviations from the "real world" population are not a problem for the internal validity of our study. However, we need to be careful talking about external validity as the representativeness of our sample is limited and due to the reasons discussed at the end of the previous chapter. Despite this limitation, analyses, especially based on sample splits, may help understand the preferences regarding the taxation of wealth of different parts of the society and hence of the US population.

**Table 1.5:** Descriptive Statistics.

| | estate | yearly wealth | decennial wealth | one-time wealth | GSS 2018 |
|---|---|---|---|---|---|
| age | **36.5** | **36.5** | **36.3** | **36.8** | 46.6 |
| female | 0.51 | **0.49** | **0.46** | **0.48** | 0.53 |
| married | **0.43** | **0.43** | 0.45 | 0.45 | 0.49 |
| has children | **0.49** | **0.41** | **0.44** | **0.45** | 0.71 |
| # children | **1.02** | **0.90** | **0.97** | **0.92** | 1.85 |
| black | **0.08** | **0.07** | **0.09** | **0.07** | 0.15 |
| white | 0.74 | **0.78** | **0.77** | **0.79** | 0.72 |
| high school | **0.99** | **0.99** | **1.00** | **1.00** | 0.88 |
| bachelor | **0.56** | **0.56** | **0.57** | **0.58** | 0.31 |
| employed | 0.70 | 0.73 | 0.74 | 0.72 | 0.71 |
| republican | **0.28** | **0.29** | **0.26** | **0.29** | 0.44 |
| democrat | 0.51 | 0.54 | 0.52 | 0.54 | 0.53 |
| observations | 593 | 481 | 469 | 558 | 2,348 |

Note: Average values of our sample by treatment group compared the US population based on the GSS 2018.

**Figure 1.1:** Geographical Representation of our Sample.



Note: Number of our survey participants (left) and the real population (right) by state; the darker the color the higher the number of people from the respective state.

As it will be crucial for our analyses, significant differences in characteristics depending on partisanship should be briefly mentioned. Supporters of the Republicans are older and have more children compared to those supporting the Democrats. Furthermore, the share of female, employed and non-white respondents is smaller in the subgroup of Republicans, whereas the share of married respondents is higher. No differences can be found regarding the level of education. Regarding our comprehension questions, Republicans performed significantly worse. However, as differences in shares of correct answers are only about 5 percentage points in each case, the effect on response quality might be neglectable.

Comparing attitudes of supporters of both parties reveals further findings: First, those respondents who support the Republicans prefer significantly lower taxes on wealth throughout all tax instruments compared to Democrats. Second, we find remarkable and significant differences in answers to some additional questions we consider within our socioeconomic questionnaire: Democrats see inequality as a greater problem for society and show stronger support for general redistribution by the government. Furthermore, we ask whether our respondents consider either luck or effort as a more important factor for accumulating wealth. While Republicans see effort as the more important reason, Democrats state the opposite.

## 1.3 Empirical Strategy

To analyze the proposed tax burdens, they are always translated into effective tax rates, i.e. the ratio of the proposed tax burden and the value of assets being taxed. These effective tax rates represent the key variable throughout our analyses and serve as dependent variable in our regression analyses.

In a first step we always show differences in *average* effective tax rates between different tax instruments and vignette dimensions graphically. As described above, our randomly ordered vignette-assignment procedure ensures that each vignette category is equally often displayed to every respondent and in combination with every other category. Hence, all respondents are confronted with e.g. each of the three wealth levels ($1m, $10m, and $30m) *exactly three times*. Furthermore, each wealth level is displayed in combination with every

category of every other dimension *exactly once*. Therefore, all sets of vignettes and average proposed tax burdens are "balanced" with respect to the categories and combinations. The presented 95%-confidence intervals are calculated based on average effective tax rates per respondent.

In a second step, we estimate different regression equations to control for socioeconomic differences between treatment groups and exploit information of the whole set of answers of every respondent. We analyze effects between tax instruments and vignette effects. The identification of effects between taxes is given by the equation:

$$etr_{ij} = \alpha + \beta_1 treat + \beta_2 X_i + \varepsilon_{ij} + u_i$$

Vignette effect estimates based on the following equation:

$$etr_{ij} = \alpha + \beta_1 value_{ij} + \beta_2 source_{ij} + \beta_3 type_{ij} + \beta_4 children_{ij} + \beta_5 X_i + \varepsilon_{ij} + u_i$$

In both cases, $i$ indexes the respondent and $j$ the vignette, $etr_{ij}$ is the proposed effective tax rate[30] and $X_i$ is a covariate vector capturing the respondents' characteristics[31]. As we gather nine observations of proposed tax burdens for each respondent, we cannot assume these observations to be independent. To consider the structure of our data, our estimations include participant specific random effects.

## 1.4 Results

### 1.4.1 General Findings

We present our findings graphically and add regression tables to substantiate the results. Main analyses are based on the full sample, i.e. neglecting answers of the assumption control questions. However, as our analysis *between* tax instruments might be affected by the correct understanding of our "behavioral assumptions", we also account for respondents who wrongly answered this control question in separate regression analyses. All regression estimates presented in this chapter are based on the full set of covariates and random effects on respondent-level. However, estimates for covariates are only reported whenever they are of interest. As our regression analyses confirm most of the findings shown graphically, regression results are only briefly summarized.

Our general results of average chosen tax levels shed some light on our first research objective, the general preferences regarding the taxation of wealth. We find relatively high proposed tax level across all tax treatments. Interestingly, this even pertains to

[30] I.e. the quotient of the proposed tax burden and the indicated level of wealth.

[31] These include gender, age, ethnicity (dummies white, black), partisanship (dummies Republican, Democrat), education (ordinal), entrepreneurial activities within the family (dummy) and two wealth-related questions (dummies, whether the respondent has ever received a gift or inheritance greater than $10,000 in the past or expects to be affected by the estate tax in the future).

"lower" wealth levels that are not taxed under current estate tax law: Due to an estate tax exemption of $11.18m in 2018 assets worth $1m and $10m would lead to a tax of zero.

The proposed tax burdens result in effective tax rates varying from 12.8% (decennial wealth tax) and 12.9% (yearly wealth tax as well as estate tax) to 14.9% (one-time wealth tax). Whereas these results seem relatively high, Fisman et al. (2020) find preferred yearly wealth tax rates between 0.8% and 3.0% that translate into even higher effective tax rates of lifetime tax burden.[32]

Greater differences between tax instruments occur when comparing treatment groups with respect to the share of respondents who choose a tax amount of $0 throughout all vignettes. While only around 6% of respondents consistently reject taxes for the three "wealth tax" instruments, almost 14% do so for the estate tax.[33]

A broad discussion in the context of wealth taxation is about the link between misinformation and preferences. In our socioeconomic questionnaire we ask our participants for their best guess on what share of the population is affected by the (existing) estate tax. To answer the question on who is affected correctly, respondents not only need to know the current estate tax law, but also need to be aware of the current distribution of wealth. We therefore argue that respondents with answers closer to the true value are better informed. Our results confirm the existence of misperception: On average, our respondents assumed 31% of the population being affected by the estate tax. About one third of all respondents expected less than 10% of all Americans to be affected. The correct answer of "1%" was chosen by about 5% of our respondents.[34] Looking for differences between different subgroups gives some better insights about who has the better assessment of the actual estate tax. On average, these respondents support the Democrats, are older and better educated. Splitting the sample into "better informed" respondents (those who gave answers *not* higher than 10%, i.e. about one third of all respondents) and "uninformed" respondents (those who gave answers higher than 10%), we find significantly lower proposed effective tax rates for the latter group. However, in contrast to other studies in this field, our study is explicitly not about revealing preferences regarding an existing tax system: Respondents are rather asked to state how their preferred tax system should look like. Although, no causal link between misinformation and preferences can be concluded from our research design, some of these rather general findings may still contribute to the ongoing discussion.

In the following subsections, that orientate towards our three hypotheses, we want to explore how specific design features affect the support for different types of wealth taxes. One could try to deal with these issues based the average numbers presented

[32]With a time horizon of 30 years, a yearly rate of 0.8% respectively 3.0% would be equivalent to an effective tax rate of 21.4% respectively 59.9%.

[33]The exact numbers are: estate tax: 13.8%; yearly wealth tax: 5.8%; decennial wealth tax: 6.4%; one-time wealth tax: 6.6%. Only considering "non-refusers" yields effective tax rates of: estate tax: 15.7%; yearly wealth tax: 13.7%; decennial wealth tax: 13.7%; one-time wealth tax: 15.9%.

[34]As the choice "1%" is the closest possible choice to the correct value of about 0.1%, this can be seen as the "correct" answer. See https://www.taxpolicycenter.org/briefing-book/how-many-people-pay-estate-tax (26.07.2019).

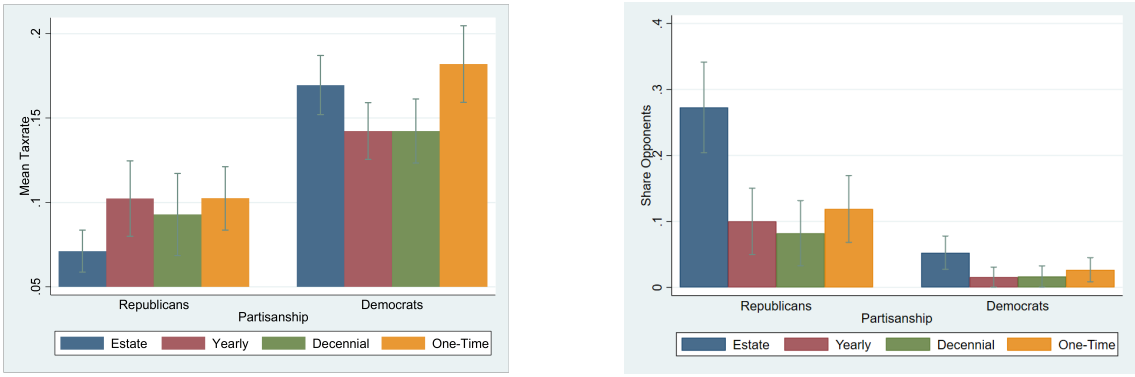
above. However, instead of reporting results based on average values of the full sample, we argue that more differentiated analyses based on sample splits provides better insights. The reasons are twofold: First, due to the fact that surveys based on MTurk do not deliver representative samples insights (for policy advice) would be limited. Second, treatment effects (differences between tax instruments as well as effects across subgroups) turn out to be clearly heterogeneous.

### 1.4.2 Hypothesis 1: Framing Effects

The estate tax and the one-time wealth tax are - except for their names - perfectly congruent. Furthermore and valid for all of our analyses, differences in the presentation and questions are kept on a minimal level to avoid any manipulation beyond rationality. Hence, preferences should not differ between both type of taxes if respondents were fully rational

Proposed effective tax rates are on average 2 percentage points lower in the estate tax group (12.9%) compared to the one-time wealth group (14.9%). Interestingly, the data reveal that this effect is mainly driven by differences in preferences of one subgroup: Supporters of the Republican Party (see Figure 1.2, left).[35] While average effective tax rates are not significantly different in the group of democratic voters (estate tax: 17.0%; one-time wealth tax: 18.2%), Republicans propose on average significantly lower tax rates for the estate tax (7.1%) compared to the one-time wealth tax (10.2%). This latter difference is strongly driven by those Republicans, who reject the estate tax entirely ("opponents"; see Figure 1.2, right): While the rejection rate is almost 30% in for the estate tax, it is only about 10% for the one-time wealth tax.

**Figure 1.2:** Proposed Tax Rates and Opponents by Partisanship.




Note: Average effective tax rates (left) and share of opponents (right) **by partisanship of the respondent and treatment group**; 95%-confidence intervals.

This result can be confirmed by our regression analysis results as shown in Table 1.6. Effective tax rates proposed by supporters of the Republicans are lowest in the estate tax group (between 2.1 and 3.3 percentage points lower than in other groups), whereas supporters of the Democrats propose higher tax rates in the concentrated tax instruments

[35] Interpreting confidence intervals, one has to keep in mind the different group sizes.

compared to both periodic taxes. Considering only those respondents who understood our assumptions correctly, results are similar in levels, though only partially significant. Of course, one has to keep in mind the much smaller group sizes of the restricted sample when interpreting significance levels.

**Table 1.6:** Between-Subject Results by Partisanship.

| | full set | | | behavioral question correctly | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Republicans | Democrats | other | Republicans | Democrats | other |
| base: one-time wealth | | | | | | |
| estate | -0.033*** | -0.006 | 0.014 | -0.023 | -0.005 | 0.018 |
| | (-2.59) | (-0.43) | (0.65) | (-1.36) | (-0.26) | (0.60) |
| yearly wealth | -0.005 | -0.037*** | 0.002 | -0.004 | -0.029 | -0.012 |
| | (-0.34) | (-2.69) | (0.09) | (-0.23) | (-1.60) | (-0.34) |
| decennial wealth | -0.012 | -0.038*** | 0.009 | -0.005 | -0.048*** | 0.008 |
| | (-0.88) | (-2.72) | (0.39) | (-0.29) | (-2.71) | (0.23) |
| Observations | 5283 | 9981 | 3636 | 2664 | 5688 | 1998 |

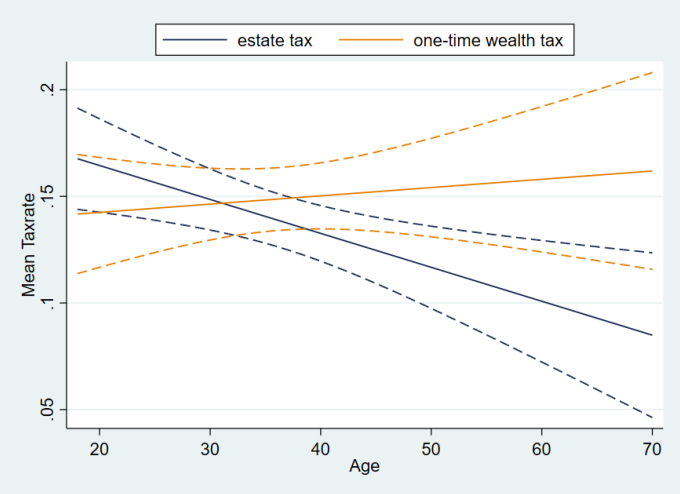
t statistics in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Random effects model; full set of controls; dependent variable: effective tax rate.

Although the sample split based on partisanship reveals the greatest differences, they can also be found along different lines. If looking at the correlation between the respondents' age and proposed effective tax rates (see Figure 1.3), it is slightly positive in the one-time wealth tax group and clearly negative in the estate tax group. This pattern leads to an effect that can also be observed in Figure 1.4: While younger respondents are indifferent between both taxes, the elderly clearly prefer the one-time wealth tax over the perfectly congruent estate tax. Finally, splitting the sample into respondents with and without children reveals that those respondents who have own children somehow oppose the estate tax (see Figure 1.5).

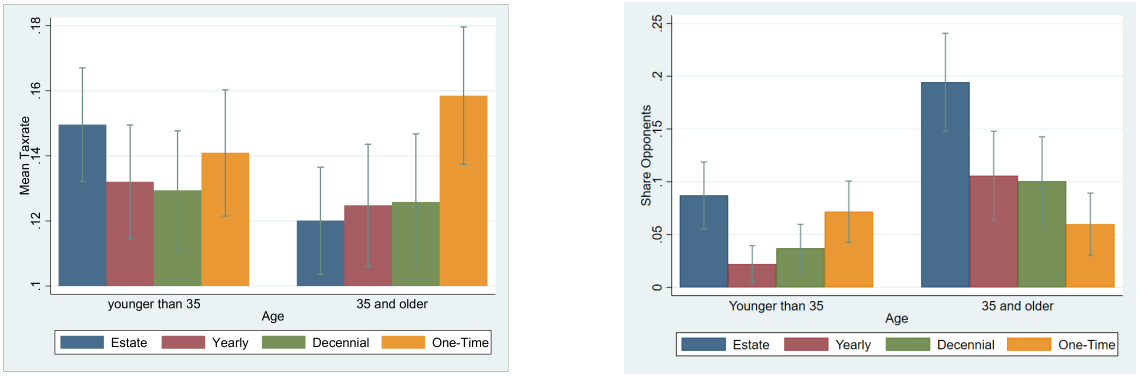
As mentioned in the Section "Descriptive Statistics", supporters of the Republicans are (on average) older and have (on average) a larger number children compared to Democrats. Hence, differences may be driven by both partisanship as well as age and the existence of children. Looking at respective subgroups, it turns out that each of these effects remains stable. First, Republicans, regardless of age and the existence of children, prefer the one-time wealth tax over the estate tax. Second, among Republicans and Democrats, older respondents and those with children propose lower effective tax rates in the estate tax group compared to those in the one-time wealth tax group. This latter finding is the main driver for slightly lower effective tax rates in the estate tax group if looking at all Democrats.

**Figure 1.3:** Age of the Respondent and Tax Rates.



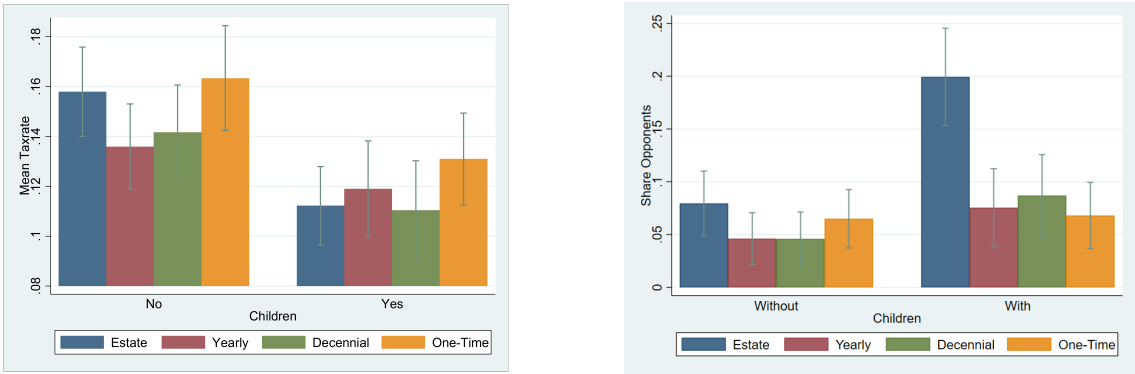

Note: Linear correlation between age of the respondent and proposed effetive tax rates; concentrated tax groups; 95%-confidence intervals.

**Figure 1.4:** Proposed Tax Rates and Opponents by Age.




Note: Average effective tax rates (left) and share of opponents (right) **by age of the respondent and treatment group**; 95%- confidence intervals.

**Figure 1.5:** Proposed Tax Rates and Opponents by Respondent's Children.




Note: Average effective tax rates (left) and share of opponents (right) **by children of the respondent and treatment group**; 95%- confidence intervals.

### 1.4.3 Hypothesis 2: Concentration Bias

Given our research design and the economic equivalence, preferences should also be identical for concentrated wealth taxes and both periodic wealth taxes, i.e. the yearly and the decennial wealth tax. Challenging our first hypothesis, we already found unexpected low effective tax rates in the estate tax group for some subgroups. Hence, we will focus on the comparison of the one-time wealth tax and both periodic taxes when analyzing the second hypothesis.

Proposed effective tax rates are on average 2 percentage points lower in both periodic wealth tax groups (12.8% and 12.9%) compared to the one-time wealth tax group (14.9%). This difference is mainly driven by differences in preferences of one subgroup: Supporters of the Democrat Party (see Figure 1.2). While Republicans seem to be indifferent between the one-time wealth tax and periodic wealth taxes, the effective tax rates proposed by Democrats are significantly and about 4 percentage points higher in the one-time wealth tax group (18.2%) compared to those in the periodic wealth tax groups (14.2%). Again, both results can be confirmed when looking at regression results (see Table 1.6). At the same time it becomes obvious that preferences regarding both periodic taxes do not differ in either subgroup. This confirms the robustness of the "periodic" treatments and our results with respect to our second hypothesis.

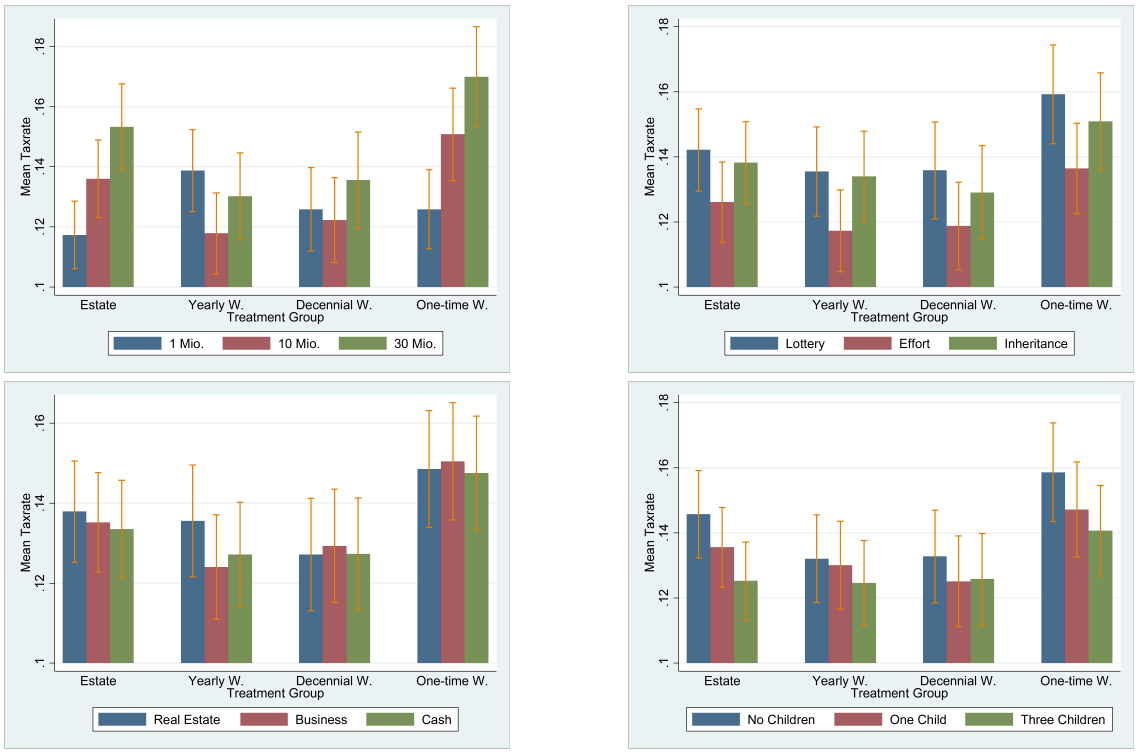
Importantly, one has to keep in mind that the concept of "preferences" is here only reflected in terms of proposed tax rates. In a second part of our study we also asked our participants in a direct within-subject comparison to decide between a concentrated and a periodic wealth tax, *keeping the level of taxation constant*. Here our results are somewhat different yet not conflicting - again pointing towards the decisiveness of tax concentration. See Section 1.5 for further results.

### 1.4.4 Hypothesis 3: The Effect of Specific Wealth Characteristics

Characteristics of the (hypothetical) taxpayers and their assets should not be differently decisive for our respondents between economically equivalent tax instruments. We expect proposed tax levels to be non-identical for different categories of the vignette dimensions, i.e. "value of assets", "source of assets", "type of assets" and "number of children". However, these differences should be identical across tax instruments, i.e. treatment groups.

As shown in Figure 1.6, hypothesis 3 can be confirmed for the dimensions "source of assets" as well as "type of assets". With respect to these dimensions, we find similar and robust results across all different treatments. In contrast, the results regarding the dimensions "value of assets" and "number of children" challenge hypothesis 3 as preferences differ between treatment groups. These differences are even more remarkable as they follow a clear pattern: Preferences are robust across both concentrated wealth tax groups as well as across both periodic wealth tax groups, but differ between concentrated and periodic taxes. Looking at the effect of the value of assets, effective tax rates of the estate tax and the one-time wealth tax show a significant progressivity, whereas no clear effect can be observed for both periodic taxes. Similarly, while proposed effective tax rates clearly decrease with the taxpayer's number of children[36] in both groups with concentrated tax instruments, there is no clear correlation in both periodic tax groups.

**Figure 1.6:** Proposed Tax Rates and Vignette Dimensions.




Note: Average effective tax rates; **by treatment group and vignette dimension**; 95%-confidence intervals.

[36] This decrease is significant when comparing proposed effective tax rates of taxpayers with no children and those, who have three children.

The regression analysis presented in Table 1.7 confirms the graphical analysis: Strictly and significantly increasing effective tax rates with respect to the value of assets and number of children can only be found for the estate tax and the one-time wealth tax. The effects of the type of assets are still small and taxes on inherited or "luckily gained" wealth are significantly higher compared to those on "earned" wealth. Both latter findings are robust across treatment groups.

**Table 1.7:** Vignette Results.

| | (1) estate tax group | (2) yearly wealth tax group | (3) decennial wealth tax group | (4) one-time wealth tax group |
|---|---|---|---|---|
| Vignette variables | | | | |
| Base: $1m | | | | |
| $10m | 0.019*** | -0.021*** | -0.004 | 0.025*** |
| | (5.90) | (-5.47) | (-1.07) | (7.76) |
| $35m | 0.036*** | -0.008** | 0.010*** | 0.044*** |
| | (11.36) | (-2.23) | (2.89) | (13.73) |
| Base: effort | | | | |
| lottery/lucky | 0.016*** | 0.018*** | 0.017*** | 0.023*** |
| | (5.07) | (4.77) | (5.08) | (7.11) |
| inheritance | 0.012*** | 0.017*** | 0.010*** | 0.014*** |
| | (3.84) | (4.39) | (3.06) | (4.51) |
| Base: cash | | | | |
| real estate | 0.004 | 0.008** | -0.000 | 0.001 |
| | (1.38) | (2.20) | (-0.05) | (0.32) |
| business shares | 0.002 | -0.003 | 0.002 | 0.003 |
| | (0.53) | (-0.82) | (0.60) | (0.89) |
| Base: no children | | | | |
| one child | -0.010*** | -0.002 | -0.008** | -0.011*** |
| | (-3.23) | (-0.53) | (-2.28) | (-3.57) |
| three children | -0.021*** | -0.007* | -0.007** | -0.018*** |
| | (-6.49) | (-1.95) | (-2.07) | (-5.59) |
| Control variables | | | | |
| female | -0.021* | -0.016 | -0.002 | -0.040*** |
| | (-1.72) | (-1.18) | (-0.13) | (-2.80) |
| age | -0.001 | -0.000 | 0.001 | 0.001 |
| | (-1.04) | (-0.73) | (0.94) | (0.96) |
| has children | -0.025* | -0.009 | -0.033** | -0.025* |
| | (-1.93) | (-0.60) | (-2.07) | (-1.66) |
| black | -0.019 | 0.008 | 0.027 | 0.016 |
| | (-0.78) | (0.26) | (0.90) | (0.48) |
| white | -0.014 | 0.021 | -0.002 | 0.032 |
| | (-0.89) | (1.14) | (-0.11) | (1.56) |
| republican | -0.060*** | -0.029 | -0.038* | -0.030 |
| | (-3.49) | (-1.45) | (-1.86) | (-1.42) |
| democrat | 0.032** | 0.012 | 0.001 | 0.058*** |
| | (2.05) | (0.65) | (0.05) | (3.01) |
| education | 0.006 | 0.009 | 0.022** | -0.000 |
| | (0.92) | (1.35) | (2.55) | (-0.05) |
| entrepr. in family | 0.022 | 0.002 | 0.005 | 0.016 |
| | (1.63) | (0.10) | (0.35) | (1.05) |
| expect estate tax | -0.020 | -0.013 | -0.021 | -0.014 |
| | (-1.58) | (-0.94) | (-1.38) | (-0.89) |
| inherited in past | 0.008 | 0.004 | 0.011 | 0.079*** |
| | (0.50) | (0.24) | (0.53) | (3.81) |
| Observations | 5337 | 4329 | 4221 | 5022 |

t statistics in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01

Note: Random effects model; full set of controls; dependent variable: effective tax rate; **by treatment group**.

Although not strictly related to our third hypothesis, the variation between different categories provides some interesting insights. First, proposed tax burdens are highest on assets if they are luckily accumulated (lottery and lucky investments) and lowest if they originate from savings of earned income. The significant difference between both kinds of assets clearly shows that our respondents value effort and somehow support a "punishment" of pure luck. Furthermore, average effective tax rates on inherited wealth are not significantly different to those on "luckily" gained assets. Hence, inherited wealth is perceived similar to rather luckily gained assets by the majority of our respondents. Second, tax levels are very similar independent of the type of assets. Hence, our respondents tend

to prefer a uniform taxation across assets and see no need for further differentiation.[37]

## 1.5 Add-On: Within-Comparison of Tax Instruments

The preceding analysis assessed the personal preferences towards the different tax instruments on the basis of the proposed level of taxation in the respective treatment group. However, such a notion of "preferences" may well be too narrow in order to draw conclusions for a preferred tax instrument: the presented proposed effective tax rates are always conditional on the assigned tax instrument due to the between-subject design of our study. We therefore added a second part to our study to elicit how preferences towards concentrated and periodic taxes are shaped *apart from the level of taxation*. Put differently, the "preference" for a specific tax instrument could also be dominated by its administrative implementation apart from the tax rates that are perceived fair for a different tax instrument. This is particularly interesting in light of political discussions as politicians can choose from a choice set of different tax instruments to meet revenue or redistributive targets.

For this, we added a simple within-subjects comparison to investigate the effects of the administrative implementation. After answering the vignette-based part about the assigned tax instrument, participants were presented one additional question where the initial allotted instrument was set in direct comparison to either a concentrated tax (i.e. the estate tax) or, depending on the randomization, a periodic tax (i.e. the yearly wealth tax). To be precise, respondents were asked if they would either prefer the already familiar tax instrument or the alternative instrument *holding the tax burden constant over the life course*: The wealth level was set to \$10 million and the effective tax rate fixed to 10%. All other vignette dimensions of our previous study were excluded.[38] Subsequently, they had to state their motivation for this choice in an open-ended response format question providing a deeper understanding of underlying motives and reasoning.

**Table 1.8:** Overview of the Within-Subject Comparisons.

| **Treatment group** | **Compared tax instrument** |
|---|---|
| Estate tax | Yearly wealth tax |
| Yearly wealth tax | Estate tax |
| Decennial wealth tax | Estate tax |
| One-time wealth tax | Yearly wealth tax |

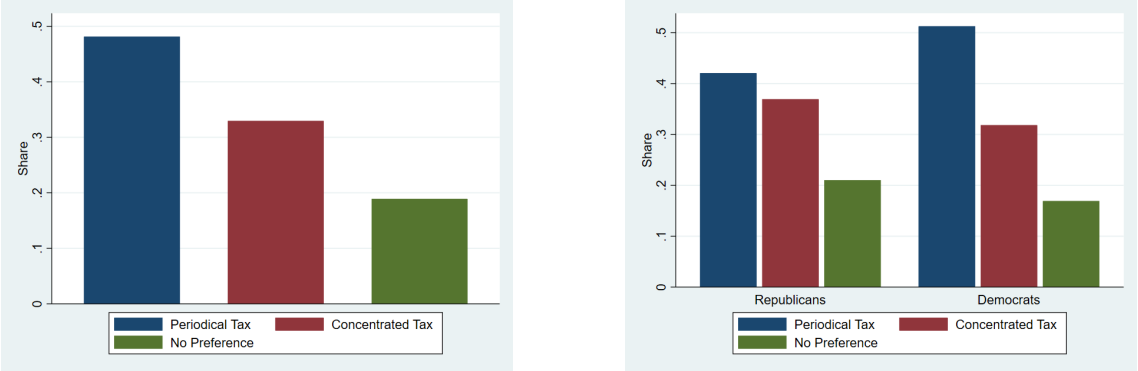
The results of our within-comparison suggest that drawing conclusions about preferences for tax instruments on the mere basis of proposed tax levels is indeed is too short-sighted. When our participants could freely chose the level of taxation we found

[37] Although we consider three very different types of assets, we do not capture preferences regarding more "emotionally charged" assets like one's childhood home or family jewelry.

[38] A screenshot can be found in the Appendix Fig. A5.

preferred average tax rates of about 13% across all tax instruments. Now, imposing a comparatively moderate lifetime tax burden of 10% we found that a periodic wealth tax is clearly and significantly preferred above a concentrated tax. Whereas the concentrated tax is preferred by only 33%, the periodic tax is picked by 48%. Comparing answers of Republicans and Democrats (see figure 1.7, right) this pattern is even more pronounced for Democrats: Interestingly Democrats favor the periodic tax over a concentrated tax relatively more than Republicans. These results, importantly, need to be interpreted separately from the preceding quantitative analysis of preferred tax rates. Since we do not hold the tax rate constant to that proposed by the participants in previous within comparison of the vignettes, this part of our study can not be set in direct comparison to our main study.

**Figure 1.7:** Within-Subject Choices Between Tax Instruments.




Note: Share of choices between tax instruments ("concentrated", "periodic" or "no preference"). Aggregated shares right, shares by party affiliation left.

At first glance, this finding may seem somewhat in contradiction to our findings of the significantly higher proposed tax rates for both concentrated tax instruments by Democrats. However, this is not the case if it points towards an interaction effect between the tax instrument and personally perceived fair tax rates: maybe Democrats prefer a periodic wealth tax instrument only if the yearly installments do not exceed a certain threshold. On the other hand, with an imposed tax rate well above what Republicans proposed in the first part of the study, they would favor a concentrated payment relatively more to the Democrats due to their relatively higher perceived yearly installment. Since our design does not allow to infer any causal inference on this point for multiple reasons,[39] this has to stay in the realm of speculation for this paper and offers room for further research. We will address these remarkable findings again in our discussion in Section 1.6.1.

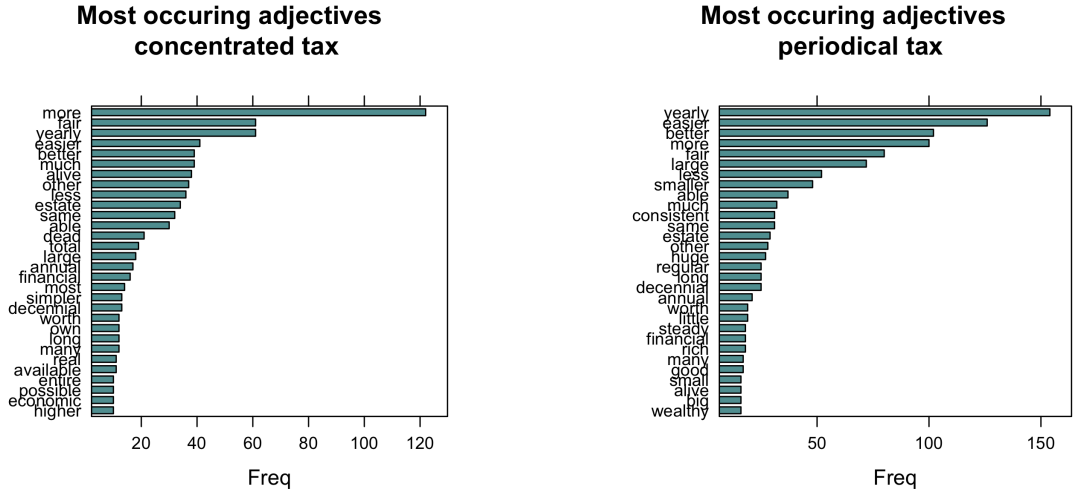
[39]This part of our study can not be set in direct comparison to our main study since we do not hold the named tax rate constant to the preceding within comparison of the vignettes. Second, we also find evidence for significant anchoring effects towards the initially assigned tax instrument. However, these effects are rather small and should not change the overall pattern of this comparison.

### 1.5.1 Textual Analysis

After our participants stated their preference between a concentrated and a periodic wealth tax (holding the lifetime tax burden constant), we asked them to briefly motivate their choice in an open-ended question format. Whereas the aforementioned analyses of this study provide insights on the specific patterns for different types of wealth taxation, we still lack profound understanding on the underlying motives. In our design we explicitly attempt to cancel out differences in preferences due to bounded rationality (i.e. the inability to translate a periodic into a concentrated tax) and efficiency concerns (i.e. tax base elasticity) in order to isolate the specific effects of framing and design features. So what is left on which basis' our participants form their preferences? The following textual analysis sheds some light on some broad patters of common reasoning.

With a response rate of 99.7% and a mean length of 27 words our respondents seem to take the task quite seriously. Respondents who reasoned their choice for a concentrated tax wrote on average around 10% longer texts than their periodic counterparts (mean of 29 vs. 26 words; p-value = 0.0094). Figures 1.8 and A12 show a first overview of the most frequent adjectives and nouns per choice group. Overall these patterns are fairly similar with adjectives like "fair", "easier" and "less" appearing prominently. However, there are also subtle differences pointing towards the importance of the textual context: For concentrated taxes words like "alive" and "dead" are distinct whereas it is "smaller", "huge", and "consistent" for periodic taxes. Interestingly, "government" is much more frequent in periodic taxes.

**Figure 1.8:** Most Used Adjectives, Concentrated Tax.




Note: Most frequently used adjectives in the open-ended answers when a concentrated tax (estate or one-time, left) or a periodic tax (yearly or decennial, right) was chosen.

In absence of context, such comparisons are of very limited insight. Therefore we created a dictionary with 5 different categories of prominent reasoning. To investigate the importance of these categories, simple relative frequencies in which these categories appear

are calculated for each decision. In particular these categories are: "political positive" and "political negative" in order to test for common political sentiment of proponents and opponents of a wealth tax; "behavior" in order to see if respondents actually show concerns about the tax base elasticiy; "transfer" in order to assess if the intergenerational transfer of wealth is decisive in specific tax instruments and finally "procedure" in order to see if it is merely administrative preferences that drive the decision. Figure 1.9 shows the share of each category for each choice type in decimals. Figure A13 furthermore distinguishes between specific tax instruments and party affiliation.

**Figure 1.9:** Most Occurring Motives for Tax Choices.

| | political_pos | political_neg | behavior | transfer | procedure |
|---|---|---|---|---|---|
| concentrated | 0.014 | 0.038 | 0.022 | 0.090 | 0.120 |
| periodical | 0.039 | 0.024 | 0.047 | 0.097 | 0.234 |
| no_type | 0.015 | 0.063 | 0.010 | 0.048 | 0.028 |

Note: Heatmap of the most occurring motives from green (low number of matches) to red (high number of matches) relative to the number of comments within this choice in decimals. Underlying dictionary: transfer: "children", "heir", "kids", "generation", "transfer", "family"; political_neg: "theft", "death tax", "unfair", "not fair", "double", "robbery", "steals", "twice", "evil", "waste", "punish", "repeatedly", "already"; political_pos: "inequality", "fair share", "redistribution", "fund"; behavior: "evasion", "avoidance", "way around", "plan", "defraud", "loophole"; procedure: "easier", "chunk", "not at once", "lump sum", "overwhelming", "simpler", "spread", "ongoing", "consistent".

Procedural considerations are by far the most important motives across all choices (with more than 23% of participants stating this in the periodic treatment), followed by intergenerational transfer considerations. Behavioral considerations are generally very low, and play at most for around 5% of participants choosing the periodic tax a decisive role. Whereas these categories are quite evenly distributed across tax instruments and party affiliations, the two political framing categories show strong differences: "political negative" is driven by Republicans who opted for the third option "Doesn't matter to me" expressing strong reservations against both types of wealth taxes. This class is strongly driven by the "double tax" argument.[40] On the contrary the "political positive" category is driven by Democrats opting for the yearly wealth or estate tax.[41]

In a final step, we manually skimmed through the specific cells of each category, which provides the most informative insights. Two very prominent patterns can be identified between the proponents of a concentrated tax and proponents of a periodic tax. Those who favor a concentrated tax at the end of the life stress how the individual is able to enjoy more freedom regarding his wealth during his lifetime (i.e. intertemporal choice of investments) and like the idea of only being confronted once without having to deal with it on a regular basis.[42] Generally, this group perceives it "easier" if only one payment has to be made. Moreover, they are generally worried that a periodic tax would be hard

[40] E.g. "An estate tax is blatant theft by the government. All of these earnings and accumulations of wealth have already been taxed when earned or aquired. To tax is again is a disgusting display of government overreach and is a disgrace."

[41] E.g. "Inherited wealth is one of the reasons we have such inequality in our society. The richest 1% mostly didn't work for their money - their money worked for them."

[42] E.g. "Get all the taxes paid for and done at once rather than over time."

to pay if funds are not sufficient or liquid. Proponents of a periodic tax (who are by far the majority, even among Democrats), on the other hand, argue that the tax burden is easier to be handled in small amounts. Further, they emphasize the advantage how such a tax would generate an immediate, consistent revenue stream to the government.[43] The second-most prominent motive "transfer" works in both directions: Proponents of the concentrated wealth taxes stress the importance of keeping dynastic wealth in check (mostly Democrats) or favor the idea that only the heirs carry the burden and not the decedent herself. Proponents of the periodic wealth, by contrast, explicitly want the earner and not the children to be burdened. These readings are further underlined by two additional, algorithmic driven text-mining methods, see Figures A14 and A15 in the Appendix.

## 1.6 Discussion

Thus far, misinformation was identified as one of the major drivers for the strong opposition towards estate taxation (Slemrod, 2006; Krupnikov et al., 2006; Kuziemko et al., 2015). The high level of misinformation is also revealed in our subsequent socio-economics questionnaire: our respondents assumed on average 31% of the population being affected by the estate tax. About one third of all respondents expected less than 10% of all Americans to be affected. The correct answer of "1%" was chosen by only about 5% of our respondents. The objective of our study is to reveal preferences for wealth taxation beyond this misinformation, i.e. normative preferences triggered through framing or due to the specific tax design. Therefore, this experiment explicitly does not take an existing tax system into account, in contrast to other studies in this field. Our respondents are rather asked to calibrate how their personally preferred tax system would look like. They proposed average tax rates of lifetime wealth-tax burdens between 12.8 and 14.9%. This finding is especially remarkable since it has to be kept in mind that these effective tax rates already include any tax exemptions: due to exemptions of $11.18m for single persons assets worth $1m and $10m would lead to a tax of zero under the US estate tax system (as of 2018). As an example, the average effective estate tax rate of 15% on assets worth $30m as proposed by our respondents translates into a statutory tax rate of about 24%, given this actual tax exemption. Despite these seemingly high proposals our study reflects similar wealth tax preferences as in Fisman et al. (2020). Moreover, in asking for the tax burden in total US-Dollars, we applied a rather conservative method. McCaffery and Baron (2006) show that responses in total amounts lead to lower progression and lower effective tax rates compared to responses given in percentages.

[43] E.g. "The government needs to have a steady stream of revenue. I don't think that would be achieved by a one time taxation of people as described."

### 1.6.1 Rationalizing Treatment Effects: Potential Motives

Related to our first hypotheses our results clearly indicate the existence of a strong framing effect for Republicans. They clearly prefer significantly higher tax rates in the one-time wealth tax over the estate tax (around 3.1 percentage points). To put it another way: Republicans reject the estate tax. Even more impressive is the fact that no further significant differences in preferences can be observed when Republicans are asked: they are indifferent between all other tax instruments. This opposition is mainly driven by a great share of Republican opponents consistently stating zero tax rates. Moreover, the framing effect is especially strong for older people and those with children. Democrats, on the other hand, do not distinguish in their preferences between the estate and a one-time wealth tax. Given the mere difference in name and not in design, we believe this finding being a result of the highly polarized political debate in the US. Graetz and Shapiro (2006) point out how a "growing think tank gap" of conservative actors being better funded prominently pushing a conservative tax cutting agenda. Bartels (2006) and Krupnikov et al. (2006) already reported strong differences in preferences towards the estate tax in 2006. With highly targeted campaigning and growing support for wealth taxation on the liberal side, framing might even be more prevalent and powerful.

Our findings regarding our second hypothesis, the comparison between concentrated and periodic taxes, is not as straight forward to rationalize. Whereas Democrats significantly indicate higher preferred tax rates for both concentrated taxes over both periodic tax instruments, Republicans only reject the estate tax and do not distinguish between other types of taxes. It is important to note, however, that their proposed tax rates (except their particularly low estate tax rate) are about 6 percentage points (about 40%) lower than those proposed by Democrats. While the preference for concentrated wealth taxes may be true in terms of proposed levels of taxation, the direct within-subject comparison complements the story with an interesting peculiarity: a clear majority of respondents, both Democrats and Republicans, favored the periodic tax over a concentrated tax instrument. This illustrates how the concept of preferences merely based on quantitative preferences does not reveal the full picture. Hence, it seems to be worthwhile to complement quantitative findings by a qualitative analysis. Democrats apparently prefer the periodic wealth tax over the concentrated taxes - if the level of a potential wealth tax is fixed and relatively low. This interpretation is supported by the lower proposed tax burdens in the periodic tax instruments, which against this background might be a result of concerns about the capacity to bear the periodic installments. Our textual analysis also matches this pattern. The majority of motives are of procedural nature: smaller payments are perceived to be easier. Interestingly, this motive is evenly distributed among Democrats and Republicans. Democrats furthermore like how a periodic wealth tax would generate an immediate and consistent revenue flow to the government.

Finally, also the findings regarding our third hypothesis feed into this interpretation. Across all tax instruments our participants want to tax luckily gained and inherited assets

higher than assets obtained through effort. Preferences for types of assets do not differ in any instrument. However, children and level of wealth trigger different preferences for concentrated than for periodic taxes: In concentrated taxes the existence of children significantly lowers the preferred amount whereas higher levels of wealth are taxed at a higher rate. For periodic taxes something like a flat tax around 12.5% (lifetime tax burden) is proposed. In light of our textual analysis the reason for missing progressivity might be the caution of taxing too heavily on a periodic basis. The strong effect of children on tax rates in concentrated instruments is also mirrored in our textual analysis. Here, the transfer motive was the second most prominent - especially for Republicans (see Figure A13). In fact, the proposed tax rates in the absence of children in the one-time tax is comparable with the proposed tax rate in presence of children in the periodic taxes.

Taken together, Democrats seem to prefer a periodic tax with more moderate tax levels. Nonetheless, if high levels of taxes can be imposed, they like to prefer high wealth taxes by the end of one's life in the style of an estate tax. Republicans, however, strongly oppose every instrument called estate tax but are more open to a one-time tax or periodic taxes, although on a much lower level.

The current theoretical literature predominantly does not reflect those tastes for a positive wealth tax. The classic conclusion of Atkinson and Stiglitz (1976),Chamley (1986) and Judd (1985) implies an optimal wealth tax of zero. This paper especially ties in with the more current work of Weinzierl (2014) and Saez and Stantcheva (2016, 2018) who bridge the gap between standard models of optimal (wealth) taxation and public preferences. Their framework allows to augment the welfare analysis of optimal taxation by a broad range of fairness principles and value judgments to determine a level of redistribution a society deems to be fair. Along these lines, our study yields a couple of insights: Apart from the classic optimal zero capital tax result (or even a positive subsidy) our respondents yet voice strong preferences for the taxation of wealth (in line with the findings of Fisman et al., 2020). These preferences are shaped by fairness principles and transfer motives similar to those proposed by Cremer and Pestieau (2006). However, our study also suggests a different type of motives beyond those provided in previous literature: The mere administrative implementation, i.e. the design configuration.[44] Such sentiment is yet hardly represented in familiar social welfare functions, however might provide substantiations for limited redistribution as a result of political discussions being strongly tied to existing policies (here the estate tax).

## 1.7 Conclusion

The understanding of preferences for redistributive policies gained more and more momentum in the theoretical literature and was subject to extensive empirical research over the past decade. Especially against the background of increased wealth inequality and

[44]To the best of our knowledge only Bastani and Waldenström (2018) discussed initial clues for such motives.

income-wealth ratios Piketty (1995) and Saez and Zucman (2016), the literature aimed to explore ambiguous empirical findings on the preferences regarding wealth taxation and conceptualize public opinions ruling the political debate. Yet, the current research frontier on preferences regarding wealth taxation centers around either rather abstract normative concepts (Weinzierl, 2014) or the emotionally loaded estate tax. Whereas Fisman et al. (2020) and Kuziemko et al. (2015) do find preferences for a positive wealth taxation, ample literature presents an exceptionally strong opposition towards the estate tax. Kuziemko et al. (2015) indicate that the fundamental opposition towards the estate tax might be driven by misinformation, however, they admit that it remains unclear if misinformation actually fully explains this phenomenon (just as Krupnikov et al., 2006). Bastani and Waldenström (2018) are among the first who survey participants on different instruments of wealth taxation: They conclude that there are "some clues" about mechanisms behind the emotional load of taxing wealth.

This study presents an experimental investigation of preferences regarding wealth taxation with tangible, real-word policy choices beyond estate taxation. In addition to the effect of various dimensions derived from the public debate, we aim to identify the role of tax-specific design features on preferences regarding wealth taxation and their interactions. In doing so, we are explicitly not interested in the underlying personal efficiency concerns or bounded rationality in comparison with these instruments.

We run a factorial vignette survey experiment with over 3,200 respondents on Amazon's Mechanical Turk (MTurk). Whereas our treatments capture other general channels that influence the preferences they crucially reflect design specific differences between the taxes. Our novel methodology enables us to disentangle the effect of general policy dimensions (i.e. value of assets, existence of children etc.) on the preferred level of taxation while identifying relative differences of these effects across the different tax instruments (between-subject). This comprehensive view on instruments of wealth taxation has another advantage: it strengthens the robustness of our findings by using different tax instruments as reference points for each other.

Our results connect and contribute to the existing literature in several ways. First, we are able to confirm major findings of previous literature: misinformed individuals propose a significantly lower level of wealth taxation across all tax instruments (Kuziemko et al., 2015); the source of wealth is decisive (Alesina and Angeletos, 2005) as assets accumulated by luck or inheritances are taxed significantly higher than savings from past salaries; the existence of children leads to a lower proposed tax burden (Cremer and Pestieau, 2006) and Republicans prefer much lower and less progressive tax rates than Democrats (Capellen et al., 2019). Second, we show how the specific design of tax instruments is indeed decisive for preferences towards wealth taxation, especially along the lines of concentrated (i.e. estate and one-time tax) versus periodic (i.e. yearly and decennial tax) taxes. While proposed effective tax rates of concentrated taxes show a significant progressivity, this pattern does not exist for periodic taxes. Third, these differences differ starkly along partisanship. Whereas Democrats clearly prefer concentrated taxes (both the estate and

the one-time wealth tax) over periodic wealth taxes in proposed tax rates, Republicans only reject the estate tax in particular. When imposing a moderate lifetime tax burden of 10%, a direct within-subjects comparison however reveals that both Democrats and Republicans prefer a periodic wealth tax over a concentrated wealth tax. Thus, we believe preferences probably are strongly connected to an interaction between the specific tax instrument and the tax rates. Since our within-comparison is not comparable to the previously proposed tax rates, we can not draw causal conclusions at this point. The investigation of preferences towards design features in direct interaction with tax rates offers room for future research. Finally, we present strong evidence for drivers of opposition towards the estate tax beyond the well documented misinformation: Republicans do not reject the perfectly congruent one-time wealth tax, for which they propose significantly higher tax rates than for the estate tax. This constitutes novel experimental clues for emotional charges, likely triggered by political framing (Birney et al., 2006). Remarkably, Democrats unambiguously do not differentiate between these tax instruments.

Connecting the dots of our paper, one interpretation might not be too far fetched: Democrats like to impose relatively high levels of taxes by the end of one's life. Nonetheless, they seem to prefer a periodic tax if the level of wealth taxes has to be rather low. Republicans, however, strongly oppose every instrument called estate tax but are more open to a one-time tax or periodic taxes, although on a much lower level. So the answer to the initially raised question of the Forbes Magazine *"Why Do People Hate Estate Taxes But Love Wealth Taxes?"* might be: Because they are perceived as more bearable, generate an immediate government revenue and do not explicitly affect intergenerational wealth transfers in the context of death.

# Chapter 2

# Biased Preferences for Wealth Taxation: The Case of Misperceived Tax Burden Consequences

The increased concentration of private wealth, frequently described in the economic literature (see e.g., Alvaredo et al., 2013; Piketty, 2015b; Saez and Zucman, 2016), has been an emotionally discussed topic for decades. Thinking about wealth taxation as a solution, it is important to consider that wealth taxes have been abolished over the recent decades (well documented by Drometer et al., 2018). At the same time, Saez and Zucman (2019) recently emphasized the need to restore overall progressivity of the US tax system, especially at the top of the US income and wealth distribution. To do so, they propose a well enforced wealth tax. Similarly, the OECD (2018) recommends tackling wealth inequality by means of a respective tax and reports a "renewed interest" in net wealth taxes in many countries. As an example, the introduction of a yearly wealth tax has been discussed during the 2019/2020 Democratic presidential nomination campaign in the US.[1] While not fully empirically analyzed yet, many expect the corona pandemic to have a reinforcing impact on wealth inequality (see e.g., Angelopoulos et al., 2021). Furthermore, public expenditures increased due to extended government assistance for private households and corporations to counteract negative effects of the crisis. Not surprisingly, both issues stimulated a debate on wealth taxation in many countries. Current discussions on the introduction of a wealth tax also revolve between a *one-time* and a *yearly* wealth tax. In 2020, the United Kingdom set up the Wealth Tax Commission to provide analysis for UK wealth tax proposals. In its final report, the commission came out in favor of a one-time wealth tax.[2] The same applies to Argentina, where senators already passed a one-off levy

[1] Scheuer and Slemrod (2021) provide a recent overview of wealth taxation, particularly with respect to the US proposals by Bernie Sanders and Elizabeth Warren compared to wealth taxation in Europe.

[2] For further information, see https://www.ukwealth.tax/ (08.07.21).

in December 2020.[3]

In light of its crucial function for societal cohesion, it is important to understand the specific dynamics of how and why these different developments in redistributive approaches evolve. In this paper we raise the question to what extent misperceived tax burden consequences of fundamental parameters of a tax system translate into distorted preferences for wealth taxation. Thus, it eventually offers another aspect to the picture on the **political feasibility** of redistributive tax policies: how to communicate tax policies in order to empower citizens to sovereignly shape informed individual political preferences. A point already raised by Fishkin (1997) who emphasizes how voter education can significantly change support for policies.

The self-determined capacity to articulate personal political preferences constitutes the foundation of the seminal median voter theorem explaining the demand for redistribution in a society (Romer, 1975; Meltzer and Richard, 1981). Recent research, however, shows how the explanatory power of these models is limited by - among others - **normative preferences** for redistribution (i.e. what a society deems to be "fair"), **efficiency concerns** or - and most importantly for this paper - **biased perceptions** (Romer, 2003; Georgiadis and Manning, 2012; Stantcheva, 2020). Different normative preferences and efficiency concerns have been studied extensively in the past years, with wealth taxation being the regularly chosen example. The literature thus far identified the following major determinants of public preference for redistribution: the source of wealth (i.e. luck vs. effort) (Alesina and Angeletos, 2005; Weinzierl, 2017; Almås et al., 2020; Fisman et al., 2020) as well as equality of opportunity and perceived social mobility (Alesina and Angeletos, 2005; Alesina et al., 2018). Interestingly, efficiency concerns seem to play a minor role for redistributive preferences - more decisive are different efficiency-equality trade-off preferences along partisan lines (Fisman et al., 2017; Capellen et al., 2019; Stantcheva, 2020).[4]

More connected to the undertaking of this paper are the limitations of the standard median voter theorem based on *misperceptions*. These are important to understand since they limit voter competence in making choices, which potentially would be different in the light of full and comprehensive information. Previous literature identifies biased perceptions mainly in the realm of *misinformation*. Piketty (2015b) and Benabou and Ok (2001) show how individuals systematically overestimate their probability of upward social mobility and thus prefer less redistribution. Bartels (2006), Birney et al. (2006) and Graetz and Shapiro (2006) demonstrate the effectiveness of sophisticated political framing and misinformation strategies to repeal the US estate tax. Indeed, many scholars demonstrate how preferences towards the (existing) estate tax strongly depend on the degree of factual

[3] See https://www.bbc.com/news/world-latin-america-55199058 (08.07.21).

[4] At least in studies with higher external validity with respect to a specific political context. More abstract lab studies provide more ambiguous results about efficiency concerns in redistributive preferences, which are highly dependent on the specific treatment calibration: Bolton and Ockenfels (2006) and Höchtl et al. (2012) also find efficiency concerns being neglectable; Messer et al. (2006) and Paetzel et al. (2014) find efficiency concerns being decisive over redistributive preferences.

knowledge about this tax (Krupnikov et al., 2006; Slemrod, 2006; Cruces et al., 2013). Kuziemko et al. (2015), Chirvi and Schneider (2020), Stantcheva (2020) and Bastani and Waldenström (2021) find preferences in favor for an estate tax to be significantly higher among those who are well informed about who and by what amount people are affected by the estate tax.

Whereas these papers assess the impact of biased perceptions mainly in the context of *misinformation*, our paper focuses on another subcategory of misperceptions: the narrowly defined question of how individuals fail to interpret the actual consequences of the parameters commonly used to characterize a tax system (i.e. *misconception*).

Specifically, we aim to answer the research question of how stated preferences on commonly discussed characteristics of a wealth tax system (i.e. tax rates and allowances) are biased by the misperception of the actual tax burden induced by computational deficiencies. We use the example of wealth taxation for the sake of its rather simple character. Since our research question targets the fundamental characteristics of a general wealth taxation, we refrain from examining very particular types of tax instruments, such as the inheritance tax, the estate tax or the property tax. These taxes have very specific characteristics such as the context of the levied tax (the decedent's death in case of the inheritance and estate tax) or the limited tax basis (real estate in case of the property tax). While the impact of such specific characteristics on perceptions and preferences is documented by related research (see e.g., Chirvi and Schneider, 2020; Kuziemko et al., 2015, for the estate tax), our focus is on a more general applicable pattern of misperception. However, our findings may be transferable on different types of taxes.

For this reason, we have chosen two different general wealth tax instruments: a yearly wealth tax and a one-time wealth tax. Since these types of taxes mostly differ only in the number and timing of tax levies, they can be generally compared on the basis of their effective tax burden - which we define as the cumulative tax burden over a person's lifetime.

We conducted a randomized survey experiment with over 1,200 respondents in Germany. These respondents were randomly assigned into one of the two wealth tax instrument groups. All respondents were presented with the same case of hypothetical persons differing only in their wealth. For these persons, they were asked to indicate their preferred tax allowance and tax rate with respect to the respective tax instrument. A randomly assigned part of both groups received information about the resulting tax burden consequences through an easy-to-understand interactive pie chart. Comparing informed to uninformed wealth tax groups, we are able to quantify the effect of misperceived tax burden consequences.

Our respondents indicate preferred levels of wealth taxation varying from 15.2% to 54.6% effective tax burdens across tax instruments. While we do not find misperceptions regarding a one-time wealth tax, preferences for a yearly wealth tax are strongly biased by misperceived tax burden consequences of the tax parameters. Respondents favor tax parameters for the yearly wealth tax that lead to effective tax rates of 54.6%, although

they actually only consider an effective tax burden of 40.0% to be "fair" if they have been informed about the tax burden consequences. Resolving computational deficiencies, we still find differences in informed preferences across the two wealth tax instruments. As indicated effective tax burdens of a yearly wealth tax are even for the informed respondents much higher than of a one-time wealth tax (40.0% vs. 15.2%), we conclude that our participants generally prefer a tax system with regular small installments over one with only one concentrated and thus larger amount payable. Since our experiment abstracts from many real-life aspects, our latter finding on informed preferences does not claim to fully explain political preferences.[5] This, however, does not affect the strong internal validity of our first finding on misperceived tax burdens.

We are not aware of any previous literature quantifying the effect of computational deficiencies on preferences in the context wealth taxation. In the context of income taxation, it is well documented that tax complexity leads to significant confusion and results in suboptimal (personal) behavioral responses (Chetty et al., 2013; Feldman et al., 2016). Blesse et al. (2019) show that a large majority of individuals indeed perceive taxes as overly complex and strongly support tax simplification. Based on the conclusion that misperception indeed exists for all sorts of taxes, Blaufus et al. (2020) develop a "behavioral taxpayer response model" to allow a more differentiated view on the reasons of misperceptions: Even if objective tax information such as tax rates and tax bases are available, taxes may be misperceived e.g. due to bounded rationality of individuals. This project aims to quantify this aspect of bounded rationality in terms of **computational deficiencies**.

Specifically, this study investigates computational deficiencies induced by the complex cumulative effect of a periodic wealth tax compared to a one-time wealth tax. To our knowledge, this effect is most closely discussed by the literature on the perceived reduced salience: Smaller but regular payments of service prices lead to higher payments than otherwise (Della Vigna and Malmendier, 2006; Iyengar et al., 2011). Such salience-decreasing mechanisms are also considered in the context of fiscal illusion: policy makers could exploit the underestimation of less salient taxes and thus collect taxes beyond what informed voters would allow for (Mill, 1848; Chetty et al., 2009; Finkelstein, 2009; Cabral and Hoxby, 2012). Based on this, we form the following first hypothesis:

**Hypothesis 1.** *Respondents will underestimate the tax burden of the yearly wealth tax due to its complex cumulative effect (computational deficiencies). This biased perception of the overall tax burden does not hold true for the one-time wealth tax. Hence, we expect the information treatment in the yearly wealth tax group having a significant tax burden decreasing effect, whereas the information treatment in the one-time wealth tax has no effect on the proposed tax burden.*

It is, however, less straightforward how preferences between periodic taxes and concentrated taxes are shaped once complexity- and salience imbalances are resolved. A rather

[5] Our research joins a set of papers which employ the method of tailored surveys with randomized treatments. In particular, similar abstractions from many real-life aspects are also applied in Weinzierl (2014), Weinzierl (2017) and Fisman et al. (2020).

small body of literature find smaller installments of tax payments as simply more *feasible* for the taxpayer. In the realm of public finance, Yaniv (1999) shows how a number of smaller, advanced tax payments increase tax compliance. Highfill et al. (1998) points out that uncertainty about the actual tax liability leads to suboptimal withholding responses resulting in interest free loans for the government (to avoid penalties). Thus, more frequent tax assessments would allow the taxpayer to better plan her liability and result in more optimal withholding. Kelly (2012) proposes to reform the property tax towards allowing multiple instalments (through automatic bank payment systems), reducing the size of the single tax bill (while maximizing convenience for the taxpayer).[6] For the case of wealth taxation, Chirvi and Schneider (2020) find first indications that individuals form *design-specific* preferences for wealth taxation beyond mere tax burden considerations. If asked about a preferred overall lifetime-tax burden, evidence for preferences between periodic or concentrated taxes is mixed along partisan lines in the US. However, the direct comparison given a fixed tax burden reveals a clear preference for periodic taxation. These initial insights from the literature leads to our second hypothesis:

**Hypothesis 2.** *Given the economic equivalence in tax bases, informed individuals in the yearly wealth tax group should not differ in their effective tax burden preferences from the one-time wealth tax group. However, previous literature from other fields yields indications that people tend to prefer smaller installments.*

Taken together, we conclude that perceptions of the tax burden consequences may be biased even if sufficient information on the effect of tax parameters is given. Taking recent discussions on wealth taxation as an example, people may support specific tax reform proposals, but are not able to determine the consequences if respective reforms would be implemented. This issue may affect polls as well as voting behavior and becomes even more severe if people have to decide between proposals regarding different (complex) types of taxes. As we will show in this study, wealth taxation serves as a good example in this regard.

The rest of the paper is organized as follows: Sections 2.1 shows a comprehensive comparison of both wealth tax instruments, Section 2.2 gives a detailed description of our experimental design, and Section 2.3 describes the data collection and data sample. In Section 2.4 results are presented, followed by a brief discussion and concluding remarks.

## 2.1 Comparability of Wealth Tax Instruments

The main objective of our study is to investigate general preferences regarding wealth taxation and how they are affected by computational issues. We are explicitly not interested in whether people like or dislike special forms of wealth taxation such as the property tax,

[6] We also acknowledge the vast literature on the underlying behavioral mechanisms leading up to these effects. For example hyperbolic discounting (Thaler, 1981), mental accounting (Thaler, 1999) or concentration bias (Kőszegi and Szeidl, 2013). However, the exploration of how these mechanisms directly translate into preferences for tax designs is beyond the scope of this paper.

the inheritance tax or the estate tax.[7] We aim to investigate more generally applicable underlying preferences regarding wealth taxes. This is an important point to make as preferences regarding types of wealth taxes may be affected by their special characteristics such as their incidence (e.g., the death in case of the estate/inheritance tax) or the precisely defined tax basis that does not include all sorts of assets (e.g., the property tax that only includes real estate).

For this reason, we deliberately chose two different instruments of a *general* wealth tax which is levied on individuals' net wealth including all types of assets: a yearly wealth tax and a one-time wealth tax.[8] As they differ only in the number and timing of the tax collection, they are generally comparable on the basis of their effective tax burden. As outlined in the introduction, both wealth tax instruments have already been implemented in different countries and are frequently considered in recent policy discussions.

In any case, the burden of a wealth tax depends on three parameters: the tax basis, the tax rate and the tax allowance. The general approach would be to calculate the tax burden as the product of tax basis minus tax allowance and tax rate. As most wealth taxes are levied on individuals' net wealth, the tax basis is usually calculated as the total value of assets minus liabilities. Furthermore, specific assets may be (partially) exempted from taxation. In our experiment, we present a taxable net value of assets to keep it as simple as possible.

As a yearly wealth tax is repeatedly levied in a person's life, we have to account for wealth accumulation over a person's lifetime. Looking at empirical data, wealth stocks are usually growing over time. In the absence of any tax this accumulation process can be described as a *savings plan* that determines the amount saved each year ($s_n$). In this paper we define such a *savings plan* as corresponding to a growth rate ($r$) in the absence of any wealth tax. In our experiment we explicitly present such a savings plan by stating the initial and final value of assets of a person.

$$s_n = r \cdot I_0 \cdot (1 + r)^{(n-1)} \tag{2.1}$$

Another related issue refers to the question, whether respondents might differentiate between nominal and real values of wealth. As a life-cycle extends over several decades, it is not straightforward to compare values at different points of time. However, as our scenarios are not linked to calendar years and are presented in a time-neutral way, we do not expect any misconception in this regard.

Based on the proposed tax parameters a one-time wealth tax and a yearly wealth tax can be compared by using measures such as future value (FV). In the absence of any taxes,

[7] Explicit normative preferences regarding different types of wealth taxation are explored by Chirvi and Schneider (2020).

[8] Chirvi and Schneider (2020) show for the US that respondents do not interpret the one-time wealth tax as an estate tax: Using a between design, especially supporters of the Republican party propose much lower effective tax burdens for the one-time wealth tax compared to the estate tax. At the same time, the design of the German inheritance tax is - compared to the design of the estate tax in the US - less similar to our one-time wealth tax.

the future value of an initial asset stock $I_0$ that grows for $N$ years (e.g., until the end of one's life) by accumulated savings can be easily calculated by:

$$FV_N = I_0 + \sum_{n=1}^{N} s_n \tag{2.2}$$

Regarding a one-time wealth tax with tax rate $\tau_o$ and tax allowance $\alpha_o$ that is levied once at the end of one's lifetime ($N$), the formula has to be modified:

$$FV_{N,o} = (I_0 + \sum_{n=1}^{N} s_n) \cdot (1 - \tau_o) + min\Big[(I_0 + \sum_{n=1}^{N} s_n), \alpha_o\Big] \cdot \tau_o \tag{2.3}$$

The wealth accumulated over $N$ years is reduced by the one-time wealth tax, which is levied on one's net wealth exceeding the tax allowance. Due to our instructions and the straightforward calculation we expect our respondents to have a rather accurate perception of the effective tax burden based on their choices of tax parameters.

Regarding a yearly wealth tax with a tax rate $\tau_y$ and a tax allowance $\alpha_y$, we need to consider that the tax payments reduce the net wealth at the end of every year $n$:

$$FV_{1,y} = I_0 + s_1 - max[I_0 + s_1 - \alpha_y, 0] \cdot \tau_y$$

$$FV_{2,y} = FV_{1,y} + s_2 - max[FV_{1,y} + s_2 - \alpha_y, 0] \cdot \tau_y$$

...

$$FV_{N,y} = FV_{N-1,y} + s_N - max[FV_{N-1,y} + s_N - \alpha_y, 0] \cdot \tau_y \tag{2.4}$$

The effective tax rate ($ETR$) of each tax instrument can then be calculated based on the $FV$ before and after the respective tax:

$$ETR = \frac{FV_N - FV_{N,tax}}{FV_N} \tag{2.5}$$

$$with \quad tax \in \{y, o\}$$

Comparing both tax instruments, even a rough estimation of the effective tax burden is much more complicated in case of a yearly wealth tax. This is not only because the yearly burden has to be calculated (based on both tax parameters and a changing tax basis) and subsequently aggregated, but each levied tax also affects the stock of wealth and hence, the tax base of the following period. We expect that this kind of tax instrument specific complexity leads to misperceived tax burden consequences in case of a yearly wealth tax.

Due to the fact that we are setting a fixed *savings plan* for wealth accumulation, this model might have some limitations that we want to address. Introducing a yearly wealth tax, two effects on the accumulated wealth stock have to be distinguished. First, the direct burden or "first round effect" of the tax. This burden is defined as the total sum

of wealth tax payments during one's lifetime (effective tax burden). Second, the indirect burden or "second round effect" of the tax due to behavioral response. As the yearly wealth tax is levied every year, it might affect the *savings plan*. Lower saving rates may be a consequence if the tax cannot be balanced by additional income or a reduction in consumption. Hence, the wealth accumulation process itself may be decelerated.

In this study, we will only consider the "first round effect" due to several reasons. First, to analyze the "second round effect" one has to make assumptions not only on the effect of a tax on interest rates and credit ratings. Even more arbitrary would be any assumption on elasticities of saving with respect to a wealth tax as they have not been empirically analyzed yet.

Further, we would need to make sure that our respondents follow these assumptions even in a setting with a brief hypothetical description on a taxpayer's situation and intuitively consider them when responding to our survey. Second, neglecting the "second round effect" does not counteract our research design. If the elasticity of saving is zero, our results represent the total effect. But even if this is not the case, we simply report the lower bound of the true wealth tax burden and, consequently, the treatment effect we aim to identify.

## 2.2 Experimental Design

In this study we aim to identify to what extent stated preferences towards wealth taxation are biased by bounded rationality. The main survey experiment of our study is based on a between design, whereas an additional question will also use a within design.

In order to determine the overall level of a wealth tax the calibration of a tax allowance and a tax rate are more important. Furthermore, it is noteworthy that wealth tax proposals usually specify general tax allowances as well as tax rates. Concrete tax amounts in exemplary cases are not illustrated. Hence, asking for these tax parameters appears to be the most appropriate tool to mirror the common political discourse on decisive dimensions for voting outcomes.

We ask our respondents to state tax parameters for three different scenarios of a hypothetical person with a fixed *savings plan* decoupled from taxation. As the yearly wealth tax is levied every year, it it is necessary to formulate clear assumptions of the lifetime accumulation of wealth. At the same time, we want to keep differences between groups as small as possible. Hence, we mention this wealth accumulation in all groups, i.e. also in case of the one-time wealth tax. To keep the presented scenarios realistic and account for periods of education with zero savings in early years, in our scenario wealth accumulation starts at the age of 30.[9] As an upper limit we chose the age of 80

[9]People under 30 have very low levels of wealth, on average only 4,000 € to 24,000 €, https://m.bpb.de/nachschlagen/zahlen-und-fakten/soziale-situation-in-deutschland/61778/vermoegen-in-west-und-ostdeutschland (08.07.21).

that represents a rounded value of life expectancy in Germany.[10] In order to capture a potentially desired progressivity as well as to analyze a broader range of wealth levels, we chose three different levels of wealth accumulations:

- initial wealth stock of 130,000 € to a final wealth stock of 350,000 €
- initial wealth stock of 1,100,000 € to a final wealth stock of 3,000,000 €
- initial wealth stock of 4,000,000 € to a final wealth stock of 10,800,000 €

Besides giving the opportunity to analyze preferences regarding progressiveness, this variation also reduces the risk of obtaining a larger share of responses with preferred tax exemptions above the wealth indicated in the survey.

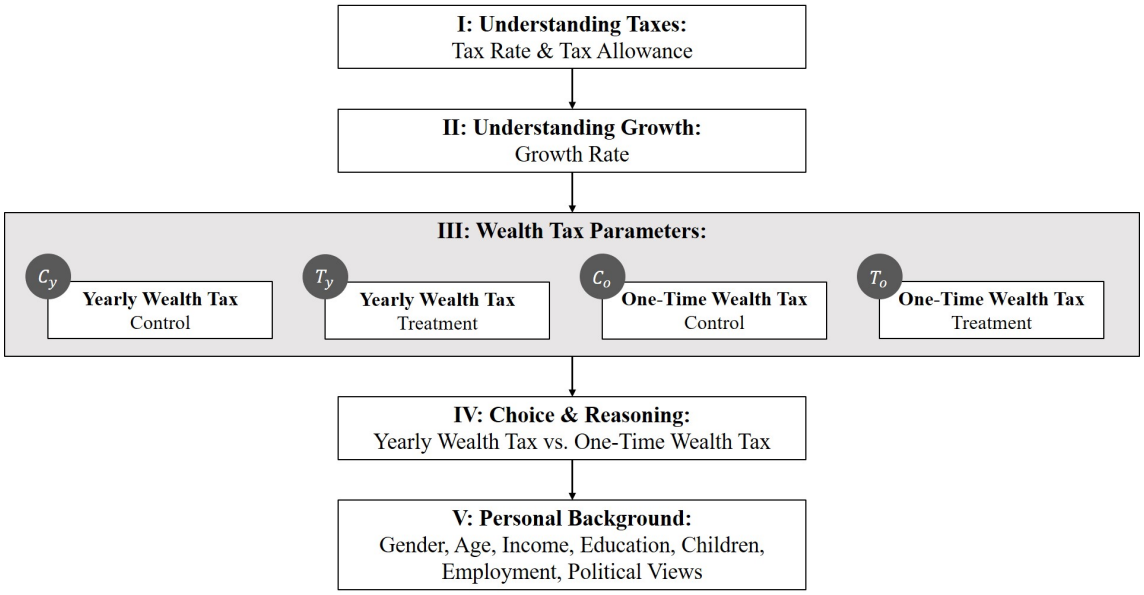
For some respondents, questions about taxation may be challenging as they are not familiar with this topic. Therefore, we provide information about central concepts, i.e. the calculation of both taxes as well as wealth accumulation. To make sure our respondents read this information and show the comprehension we asked some control questions afterwards. This information on "Understanding Taxes" and "Understanding Growth" forms the first and second part of our survey (see Figure 2.1). Our respondents generally show a very good understanding of the two concepts. Each control question was answered correctly by more than 90% of all respondents.[11]

Subsequently, our respondents are randomized in one of four groups: In the *yearly* wealth tax groups (control group: $\mathsf{C_y}$ and treatment group: $\mathsf{T_y}$), respondents have to state their preferred tax allowance and tax rates in case of a yearly wealth tax. In the *one-time* wealth tax groups (control group: $\mathsf{C_o}$ and treatment group: $\mathsf{T_o}$), the same has to be done in case of a one-time wealth tax. Before our respondents have to state their preferred tax parameters, we briefly give all groups some information on the respective tax instrument, i.e. that all assets are included in the tax basis and how it is levied.

[10] It represents a rounded average number for women and men with a completed age of 20-40 years, see Table 12621-0002 at https://www-genesis.destatis.de/genesis/online (08.07.21).

[11] The two questions about taxes are answered correctly by 95.8% and 94.6%, the question about growth is answered correctly by 92.6% of all respondents.

**Figure 2.1:** Survey Flow.




Note: This figure illustrates the survey flow. The survey contains of five parts. Part III is the main part where respondents were randomized in four different experimental groups. The full survey questionnaire is provided in the Appendix B.1.

Following this introductory information, our respondents reach the main part of the survey. We made sure to standardize this part and vary only the name of the respective tax. Putting the whole text on one single page, we first ask for the preferred tax allowance and then ask for the preferred tax rate in each of the three cases. The exact description of our scenarios as well as the wording of the question can be seen in Figure 2.2. We keep both as simple as possible due to two reasons: First, our respondents might be overloaded by a too complex (more realistic) scenario. Second, a (more) complex scenario is not necessary to identify the effect of computational issues on preferences.[12] However, the overall level of proposed wealth taxes might be biased due to this simplified design.

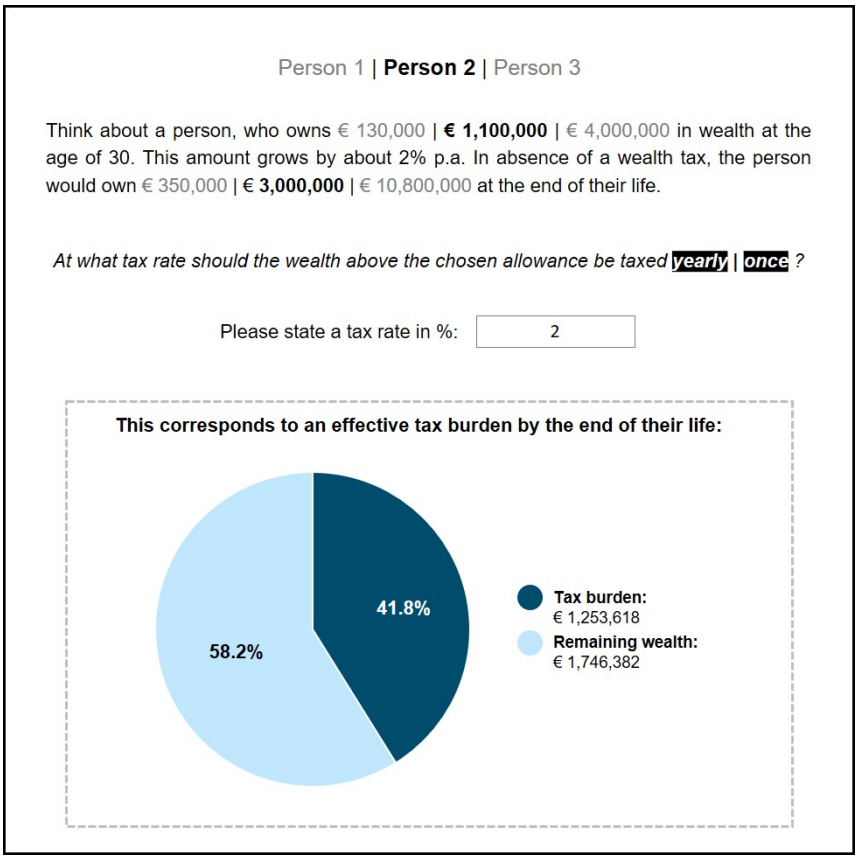
We expect bounded rationality to play an important role especially when people decide on tax parameters of the yearly wealth tax. To address this issue and quantify the bias induced by the complexity of this tax instrument, we randomly split both the yearly wealth tax as well as the one-time wealth tax group into our treatment group ($\mathsf{T_y}$ and $\mathsf{T_o}$) and control groups ($\mathsf{C_y}$ and $\mathsf{C_o}$). In our treatment groups we inform our respondents about the effective tax burden of their proposed tax parameters. In our control groups our respondents have to state their preferred tax rates without being informed on the effective tax burden. This enables us to reference treatment effects of both tax instruments against each other. Consequently, we end up with four random groups as presented in Figure 2.1.

[12] With our simplification we are well in line with other studies like Fisman et al. (2020) and Stantcheva (2020) who use similar levels of abstraction.

Our information treatment is designed to provide sufficient information about effective tax burden of the proposed tax parameters. Further, this information is presented in a way that our respondents fully understand as quickly as possible. In order to ensure the provision of sufficient information we chose a comprehensive presentation: As shown in Figure 2.2, we present each effective tax burden that results from the preferred tax parameters both visually and in textual form. The visual information treatment is based on a responsive pie chart that immediately shows the effective tax burden and allows respondents to readjust their first entered tax parameters. Hovering over the pie shares, the absolute and relative effective tax burden as well as the remaining wealth (wealth net of taxes) are again interactively highlighted. With this comprehensive depiction of the information we want to avoid any biases regarding metric or visual representations.

Based on this, we expect our treated respondents to be sufficiently informed about the effective tax burden of their chosen tax parameters. Hence, an analysis and comparison of results between $T_y$ (yearly wealth tax treatment group) and $T_o$ (one-time wealth tax treatment group) reveals the *informed* preferences regarding both tax instruments.

**Figure 2.2:** Main Survey Experiment.




Note: This figure describes the main experiment: Grey font indicates the different hypothetical persons (within-subject); black background font indicates the different wealth tax instrument (between-subject); area framed in dashed grey line shows the information treatment (between-subject).

As shown in Figure 2.1, we included another experimental element ("Choice & Reasoning") into our survey that takes place after the main part. We enriched our analysis on *informed* preferences by a within-subject comparison of both wealth tax instruments. Participants were asked to state their preference not only for their assigned tax instrument (yearly wealth tax or one-time wealth tax), but also in relation to the other tax instrument including a written reason of their choice in an open-ended question format.

Specifically, participants were shown the effective tax burden resulting from their tax parameter inputs to their assigned tax instrument in the previous part of the survey and it was pointed out very clearly that this tax burden equals the effective tax burden of the alternative tax instrument. To emphasize the different implementation of both tax instruments, the one-time wealth tax is presented as *one* amount to be paid, while the yearly wealth tax is presented as an average *yearly* amount to be paid.[13] Accordingly, participants could select one of the two wealth tax instruments or "no preference".

Even though not being part of our main study, this provides supportive evidence of how *informed* preferences differ across wealth tax instruments and first insights about the motives behind and beyond proposed tax rates.

## 2.3 Data

### 2.3.1 Data Collection

The survey experiment was conducted via the online access panel Respondi.[14] Respondi is a commercial survey company known primarily for market research or consumer product testing, but has recently been increasingly used for scientific research projects. Therefore one might well assume that these panelists are less familiar with this type of research and are therefore more likely to provide answers that are consistent with their opinion rather than any strategic behavior (an advantage also exploited by Stantcheva, 2020, p. 10).

Respondents were invited to participate in our survey by email. Respondi sent invitations to suitable panelists who lived in Germany, were at least 18 years old, and had varying monthly household net incomes. The invitation email only contained the duration of the survey. Only when clicking on the survey link, the participants received information about the research project. It was ensured that participants could only take part with their PC, laptop or tablet. Participation with a smartphone was technologically prevented. On our consent page, they were then also advised to answer to the best of their knowledge and were assured that participation was entirely voluntary.

The use of online crowdsourcing platforms such as Respondi or others (e.g., MTurk, clickworker, Prolific) for scientific purposes is becoming increasingly prevalent. The data are at least as reliable as those obtained using lab or field experiments, but require less money and time to collect (Horton et al., 2011). There are also potential drawbacks we

[13] For simplicity, the average *yearly* amounts were calculated as the *one-time* amount divided by 50.
[14] For further information, see https://www.Respondi.com/EN/ (08.07.21).

accounted for. First, panelists could participate in our survey even though they do not live in Germany. We used a third-party web service, IPHub, to screen out potential misuse of IP addresses.[15] Respondents using foreign IP addresses or virtual private networks (VPNs) were therefore screened out at the beginning of the survey. Second, panelists could participate multiple times. Respondi promised us to get unique respondents by paying the complete participation only once. We also wanted to ensure that we were getting high-quality data by checking respondents' attentiveness. Specifically, we asked respondents not to answer a question in the middle of our survey. If respondents did not pass this attention check, they were also screened out.

### 2.3.2 Data Sample

The survey was conducted between December 15, 2020 and January 5, 2021.[16] After sorting out inadmissible IPs or attention fails, we obtain an initial sample of N = 1,280 respondents.

Randomization into the four experimental groups worked well, although the sample is not perfectly balanced. This is due to the fact that the attention check took place after the allocation to the treatment groups. Table 1 shows that $\mathsf{T_o}$ (one-time wealth tax treatment group) is slightly over-sampled in both our initial sample and final sample.

As described in the previous chapter, we used some means to only obtain high-quality responses. This has paid off, as we are able to use almost the entire initial data set for the analysis. There are only a few responses that are excluded in the following step. These are firstly "Incomplete Tax Rates", i.e. these participants did not provide a tax rate for at least one of our three hypothetical persons, and secondly "Incoherent Tax Allowances", i.e. here the participants obviously did not give any serious answers, as tax allowances are somewhat arbitrary (e.g., 111 €). The few steps of data cleaning are shown in Table 2.1. In order to not arbitrarily delete further somehow inconsistent seeming responses we decided to intervene as little as possible.

**Table 2.1:** Data Cleaning.

| | **Yearly Wealth Tax** | | **One-Time Wealth Tax** | |
|---|---|---|---|---|
| | Control | Treatment | Control | Treatment |
| Initial Sample | 312 | 306 | 311 | 351 |
| Incomplete Tax Rates | 1 | 4 | 5 | 5 |
| Incoherent Tax Allowances | 5 | 2 | 4 | 5 |
| **Final Sample** | **306** | **300** | **302** | **341** |

Note: This tables shows data cleaning by group status. Respondents providing incomplete tax rates or incoherent allowances were screened out.

Responses that remain in the final sample are considered sincere. The median duration

[15] For further information, see https://iphub.info/ (08.07.21).

[16] Prior to the roll out of this survey, a pilot among business students was conducted at Humboldt University of Berlin in February 2019.

for completing our survey was 11.4 minutes.[17]

The final sample is not representative of the German population in all characteristics. Table 2.2 shows the summary statistics of our final sample for each group. We additionally include summary statistics of the German population as a reference. These data are based on the 2018 wave of the General Social Survey in Germany (ALLBUS) provided by GESIS-Leibniz-Institut für Sozialwissenschaften (2019).[18] Comparison of our data with ALLBUS shows that our final sample is representative in terms of age and employment, but less so in terms of gender, marital status, income, children, and education. On average, our participants have slightly higher incomes, are better educated, are less likely to be female and less likely to have children. Bold values in Table 2 indicate statistically significant differences from the German population ($p < 0.05$).

**Table 2.2:** Descriptive Statistics.

| | **Yearly Wealth Tax** | | **One-Time Wealth Tax** | | **ALLBUS** |
|---|---|---|---|---|---|
| | Control | Treatment | Control | Treatment | **2018** |
| Age | 52.1 | 51.4 | 51.9 | 52.0 | 51.2 |
| Female | **0.34** | **0.42** | **0.39** | **0.38** | 0.49 |
| Married | 0.52 | **0.48** | **0.50** | 0.53 | 0.58 |
| Personal Income | | | | | |
| 0 €-999 € | **0.11** | **0.11** | **0.14** | **0.15** | 0.24 |
| 1.000 €-1.999 € | 0.34 | 0.34 | 0.38 | 0.38 | 0.37 |
| 2.000 €-2.999 € | 0.25 | 0.23 | 0.25 | 0.21 | 0.23 |
| 3.000 €-3.999 € | **0.17** | **0.19** | **0.15** | **0.20** | 0.09 |
| 4.000 €-4.999 € | **0.08** | **0.08** | 0.05 | 0.04 | 0.03 |
| 5.000 € and more | 0.04 | 0.04 | 0.02 | **0.01** | 0.04 |
| Children | 0.65 | **0.58** | **0.60** | **0.63** | 0.69 |
| University | **0.39** | **0.37** | **0.38** | **0.36** | 0.20 |
| Employed | 0.59 | 0.67 | 0.60 | 0.61 | 0.62 |

Note: This table shows summary statistics in our four samples compared to the German General Social Survey (ALLBUS). The ALLBUS sample used here is the latest available wave of 2018. Survey weights at the personal level for Germany were used to determine the values. Statistically significant differences between our samples and the ALLBUS data are shown in bold ($p < 0.05$).

These few differences from the German population do not pose a problem for the internal validity of our experiment. However, we have to be cautious when making statements about the general attitude of Germans towards wealth taxation. Regarding internal validity, we need to check how well our randomization process worked. We verified this by running OLS regressions for each of the previous presented covariates in the form of $\mathsf{Group}_i = \alpha + \beta \cdot \mathsf{Covariate}_i + \epsilon_i$. The outcome variable $\mathsf{Group}_i$ is a dummy variable that indicates the group status $C_y$, $T_y$, $C_o$ and $T_o$ respectively. This procedure results in 48 regression coefficients (4 groups x 12 covariates) shown in Table B3 in the Appendix. Out of these 48 estimated coefficients, only one coefficient is significant at the 5% level and only four are significant at the 10% level. If we regress $\mathsf{Group}_i$ on all selected covariates, we get p-values for joint significance ranging from 0.12 to 0.89. Both results nicely show that

[17] The minimum duration for completing our survey was 3.2 minutes. Since participants could pause the survey and continue it at a later time, the maximum duration is not meaningful.

[18] It is a biennial representative survey in Germany which is comparable to the General Social Survey (GSS) in the United States.

covariates are not able to explain the experimental group status. Hence, we can conclude that the randomization process worked successfully.

## 2.4 Results

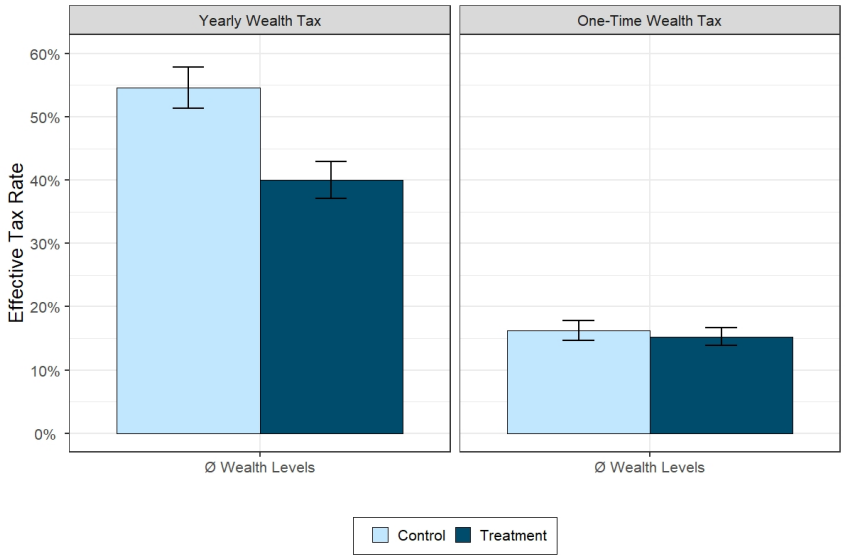
The results are presented along our two hypotheses. In the first part, we present the results on how bounded rationality (i.e. computational issues) affects stated preferences for wealth taxation. In the second part, we analyze how *informed* preferences differ across wealth tax instruments.

The following analysis is based on the effective tax rates resulting from the participants' tax parameter inputs in order to directly compare both wealth tax instruments. In Appendix B.2, we conduct further descriptive analysis of the tax allowances and tax rates separately. We find that participants seem to have a specific idea of what amount of wealth should generally be tax-exempted, which is independent from the specific wealth tax instrument. And even though participants can adjust both the tax allowance and the specific tax rate, they are more likely to calibrate their preferred wealth tax system by the means of the tax rate.

### 2.4.1 Analyzing the Effect of Bounded Rationality

Before getting into the details, Figure 2.3 provides an overview of our main results. The following graphs are based on proposed tax allowances and tax rates of all three scenarios. Hence, displayed effective tax burdens are the average values of the three different levels of wealth.

**Figure 2.3:** Baseline Results.




Note: Effective tax rates by experimental group with 95% confidence bars.

When comparing results in both untreated groups (bars colored in light blue), the effective tax burden within the *yearly* wealth tax group (about 55%) is on average much higher compared to the *one-time* wealth group (about 16%). This difference is large and highly significant. Nevertheless, we do not expect that these results express the truly preferred effective tax burden of respondents in the *yearly* wealth tax group. As described before, respondents intuitively propose their preferred tax allowance and tax rates, but may misperceive the resulting effective tax burden due to computational issues. We want to shed some light on this issue of bounded rationality by our information treatment.

For the treatment groups ($T_y$ and $T_o$) we presented some easy-to-understand information on the effective tax burden of the choices made. We expect these respondents to be well informed about the consequences of their proposed tax parameters. Hence, only informed (treated) respondents state their *informed* preferences if misperception is prevalent in the untreated group. The difference between untreated and treated respondents can be fully assigned to computational issues due to the complexity of the yearly wealth tax. Looking at effective tax burdens in both *yearly* wealth tax groups, we find a clear and significant effect of our information treatment.

While untreated respondents in the *yearly* wealth tax group intuitively propose, on average, an effective tax burden of about 55%, this number decreases to about 40% in the informed (treated) group (see Figure 2.3). Given the experimental design, this significant difference of almost 15 percentage points can be interpreted as the "computational-issue-effect" due to the complexity of the yearly wealth tax. Strictly speaking, our respondents seem to perceive an effective tax burden of 40% as "fair", but state tax parameters that lead to an effective tax rate of 55%, because they misperceive the effective tax burden of a yearly wealth tax.

Following similar reasoning, we expect no significant treatment effect in case of the one-time wealth tax. This is because computing the effective tax burden given a tax allowance and a tax rate is rather easy and, hence, an accurate perception of the effective tax burden should be straightforward given some calculation skills. Looking at effective tax rates in both one-time wealth tax groups shows that indeed no significant treatment effect can be identified. As outlined before, the non-existence of an effect in this setting also verifies that our information treatment does not create any distortions. Hence, this finding strengthens the validity of our results regarding the treatment effect in the yearly wealth tax groups.

The baseline results shown in Figure 2.3 are confirmed by regression analyses. We run linear regressions both without and with control variables (columns (1) and (2) in Table 2.3 respectively). However, as we expect our randomization process has worked well, results should not significantly differ.

**Table 2.3:** Treatment Effects.

| | **Effective Tax Rate** (1) | **Effective Tax Rate** (2) |
|---|---|---|
| Info Treatment | −0.010 | −0.002 |
| | (0.011) | (0.010) |
| Yearly Wealth Tax | 0.384*** | 0.394*** |
| | (0.018) | (0.017) |
| Yearly Wealth Tax x Info Treatment | −0.137*** | −0.146*** |
| | (0.025) | (0.024) |
| CONTROLS | NO | YES |
| N | 3.747 | 3.702 |

Note: This table presents linear regression coefficients with wealth levels fixed effects where the dependent variable is effective tax rate (reduced version of Table B4). Control variables are all observable characteristics including Age, Female, Inheritance Tax, Future Tax, Past Tax, Political Orientation, Redistribute, Inequality, Children, University Degree, Income, Employment and Control. Variables are defined in Table B1. Standard errors are clustered by respondents' IDs: $^{*}p < 0.1$, $^{**}p < 0.05$ and $^{***}p < 0.01$.

First, the dummy variable "Yearly Wealth Tax" shows that our untreated respondents propose significantly higher effective tax rates in the *yearly* wealth tax group (difference of about 38 percentage points (1) and 40 percentage points (2), respectively). Second, we can clearly find that the provided treatment has a large and statistically significant negative effect on the effective tax rate in the *yearly* wealth tax group ($T_y$), while it has no effect in the *one-time* wealth tax group ($T_o$). This is true for both specifications, i.e., even when all observable characteristics are included in the regression analysis. The treatment effect is very similar in terms of magnitude and statistical significance in both settings. Thus, the treatment effect is robust to the influence of the covariates. Third, differences in the treatment effect, caused by respondents' bounded rationality, cannot fully explain the difference between the different levels of wealth taxation. Although it reduces the effective tax burden by almost 15 percentage points, there remains a considerable difference in the effective tax burdens of almost 25 percentage points between the two wealth tax instruments. We interpret this difference in terms of existing differences in *informed* preferences between both tax instruments: People prefer much higher effective tax burdens in case of a yearly wealth tax compared to a one-time wealth (40.0% vs. 15.2%).

**Table 2.4:** Wealth Level Treatment Effects.

| | **Yearly Wealth Tax** (1) | **One-Time Wealth Tax** (2) |
|---|---|---|
| Info-Treatment x 350,000 € | $-0.107^{***}$ | 0.0003 |
| | (0.024) | (0.007) |
| Info-Treatment x 3,000,000 € | $-0.169^{***}$ | 0.002 |
| | (0.024) | (0.011) |
| Info-Treatment x 10,800,000 € | $-0.178^{***}$ | −0.010 |
| | (0.023) | (0.015) |
| CONTROLS | YES | YES |
| N | 1.797 | 1.905 |

Note: This table presents linear regression coefficients by wealth tax instrument where the dependent variable is effective tax rate (reduced version of Table B5). Control variables are all observable characteristics including Age, Female, Inheritance Tax, Future Tax, Past Tax, Political Orientation, Redistribute, Inequality, Children, University Degree, Income, Employment and Control. Variables are defined in Table B1. Standard errors are clustered by respondents' IDs: $^{*}p < 0.1$, $^{**}p < 0.05$ and $^{***}p < 0.01$.

In our survey design, we can also measure the treatment effect for different wealth levels. The treatment effect exists in the yearly wealth tax setting throughout all presented accumulated wealth levels (350,000 €; 3,000,000 €; 10,800,000 €) and is -10.7, -16.9 and -17.8 percentage points. In contrast, there is no statistically significant treatment effect for each of the accumulated wealth levels in the *one-time* wealth tax group (see Table 2.4). The smaller effect of the treatment for the lowest wealth level can be partly attributed to a higher proportion of rejection of a wealth tax for an individual with a net wealth of 350,000 €, because a rejection in the form of an exceeding tax allowance or a yearly tax rate of 0% is not affected by the treatment.[19]

Regarding potential heterogeneity of the treatment effect we examine whether the treatment effect differs across subgroups. Some groups of participants might respond less to additional information because they already understand the mechanism of a wealth tax. Furthermore, other groups of participants might completely ignore our information treatment. To test for potential heterogeneity, we extend the regression analysis for the yearly wealth tax groups by adding interaction terms in the form of Covariate × Info-Treatment in each case (see Table B6 in the Appendix). In general, we find little evidence of heterogeneity in our information treatment. However, *knowledge* about the existing wealth taxation in Germany (i.e. inheritance tax) seems to be a moderating factor. We asked our participants for their best guess on what share of the German population is affected by the existing inheritance tax. Reported shares below 10% indicate a good understanding of the current inheritance tax system since this number approximately represents the true share of the affected German population.[20]

On average, our respondents on average estimate 29.2% of the German population to be affected by the inheritance tax. Only 13.7% indicate an approximately accurate share of less than 10%. In comparison, only 5.9% of our respondents themselves state that they

[19] Rejection shares for an accumulated wealth stock of 350,000 € are about 30% in all four groups.

[20] It is difficult to determine this number accurately. Our determination of this number is outlined in Appendix B.4.1.

have already paid inheritance tax. According to our calculations, this figure is in the same order of magnitude as the actual proportion of people affected in Germany.

We find that respondents with better knowledge about inheritance taxation in Germany respond significantly less to the information treatment. That is, even without information treatment, they have less biased perceptions of the effects of their entered tax parameters (see Table B6 in the Appendix).

In sum, informing our respondents does have a strong effect in the *yearly* wealth tax group but does not significantly change the proposed effective tax burden in the *one-time* wealth tax group. We conclude that our information treatment succeeds to reveal misperceptions in the *yearly* wealth tax group.

### 2.4.2 Analyzing Informed Preferences

In Section 2.4.1, we show that a large share of the difference between the *untreated* yearly wealth tax group and the *untreated* one-time wealth group can be attributed to computational issues. We interpret the remaining difference after accounting for bounded rationality as existing differences in *informed* preferences for wealth taxation. According to our results, people propose much higher effective tax burdens in case of a yearly wealth tax compared to a one-time wealth tax (40.0% vs. 15.2%).

In this section, we briefly want to look at the remaining *informed* preferences for wealth taxation in treatment groups $\mathsf{T_y}$ and $\mathsf{T_o}$. We aim to analyze correlations between the specified tax rates and covariates. In order to identify which subgroups propose higher or lower tax burdens, we split the sample by the tax instrument and run linear regressions of effective tax rates on selected covariates.

**Table 2.5:** Regression Analysis of *Informed* Preferences by Wealth Tax Instrument.

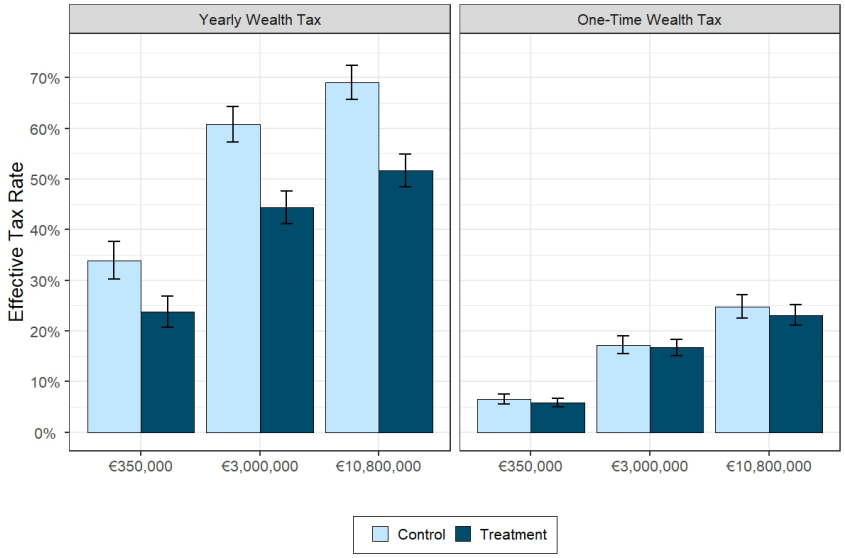
| | **Yearly Wealth Tax** (1) | **One-Time Wealth Tax** (2) |
|---|---|---|
| Constant: 350,000 € | 0.165 | −0.013 |
| | (0.171) | (0.075) |
| 3,000,000 € | 0.207*** | 0.109*** |
| | (0.012) | (0.006) |
| 10,800,000 € | 0.280*** | 0.173*** |
| | (0.014) | (0.009) |
| Age | 0.001 | −0.001 |
| | (0.001) | (0.001) |
| Female | 0.033 | −0.018 |
| | (0.031) | (0.013) |
| Inheritance Tax | 0.087 | 0.027 |
| | (0.074) | (0.039) |
| Future Tax | −0.033 | −0.023 |
| | (0.038) | (0.017) |
| Past Tax | 0.099* | 0.066* |
| | (0.056) | (0.038) |
| Political Orientation | 0.002 | −0.001 |
| | (0.015) | (0.008) |
| Redistribute | 0.024** | 0.029*** |
| | (0.011) | (0.005) |
| Inequality | 0.004 | −0.003 |
| | (0.013) | (0.006) |
| Children | 0.037 | 0.011 |
| | (0.036) | (0.017) |
| University | −0.00004 | 0.004 |
| | (0.032) | (0.016) |
| Income | −0.009 | −0.004 |
| | (0.012) | (0.007) |
| Employed | 0.024 | −0.012 |
| | (0.036) | (0.017) |
| Control | −0.152 | 0.009 |
| | (0.101) | (0.033) |
| N | 894 | 1,011 |

Note: This table presents linear regression coefficients where the dependent variable is effective tax rate. Variables are defined in Table B1. Standard errors are clustered by respondents' IDs: $^{*}p < 0.1$, $^{**}p < 0.05$ and $^{***}p < 0.01$.

First, we find people having preferences for a progressive wealth tax. In both groups,

effective tax rates clearly increase with wealth levels.[21] For example, respondents, on average, state 20.7 (10.9) percentage points higher effective tax rates for a hypothetical person with accumulated wealth of 3,000,000 € compared to 350,000 € in case of a yearly (one-time) wealth tax (see Table 2.5). The higher progressivity, however, is relativized if one takes into account the significantly lower level of taxation at an accumulated wealth of 350,000 € in case of a one-time wealth tax.

**Figure 2.4:** Preferences for Progressive Wealth Taxation.



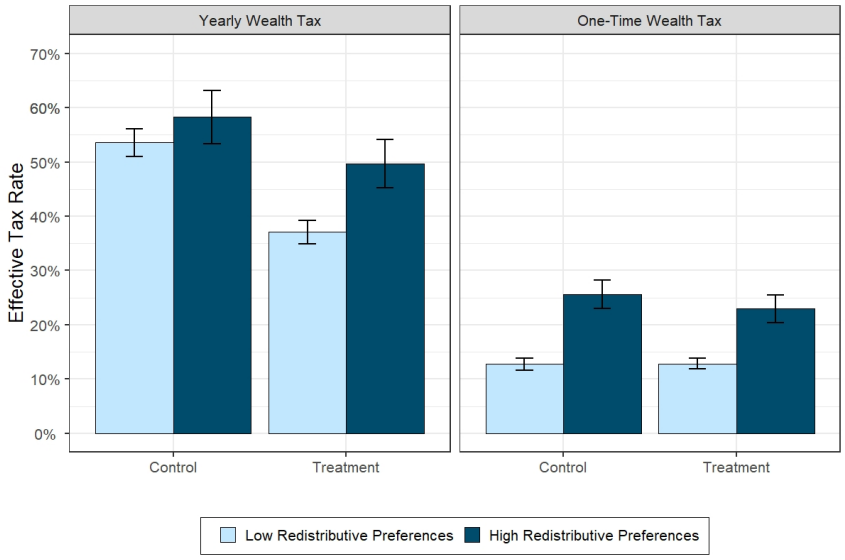

Note: Effective tax rates by wealth levels and groups with 95% confidence bars.

Second, effective tax burdens depend on one's own redistributive preferences. This effect is similar in both wealth tax groups (see Table 2.5). Redistribution preferences are thus very clearly reflected in the effective tax burdens.

To further emphasize the importance of information we can see how people might not be able to express their preferences in a yearly wealth tax system. Figure 2.5 illustrates the effective tax rates by redistributive preferences and wealth tax instrument.

[21] It should be noted that progressivity in terms of effective tax rates can also result from a constant tax rate for each wealth bracket. Thus, while we find strict progressivity in effective tax rates in more than 90% of cases, only about half of them are accompanied by strictly increasing tax rates.

**Figure 2.5:** Redistributive Preferences.




Note: Effective tax rates by redistributive preference (median split) and experimental groups with 95% confidence bars.

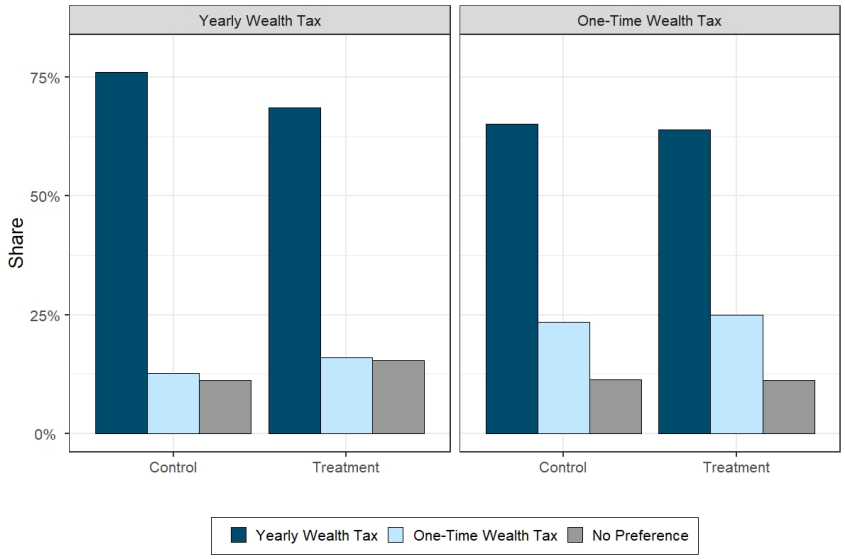
While redistributive preferences are clearly reflected both in the control and treatment group of the one-time wealth tax, the redistributive preferences in case of a yearly wealth tax are less distinctive in the control group than in the treatment group.[22] This highlights how tax misperceptions might shape the actual political outcome.

People proposing much higher effective tax rates in case of a yearly wealth tax compared to a one-time wealth tax does not necessarily mean that they prefer a yearly wealth tax over a one-time wealth tax. Nevertheless, we can support this interpretation by a within-subject question after the main part of our survey. In contrast to previous parts of the survey that focused on a single type of tax, our respondents face in this setting more options: the one-time wealth tax of X €, the yearly wealth tax of an average of X € per year, or no preference (see Section 2.2). The effective tax burdens of both wealth tax instruments are based on respondents' tax parameter inputs in the previous part of the survey. Further, they are informed that the effective tax burden is equal for both tax instruments and hence, at this stage our participants are provided with sufficient information about the tax burden consequences. This is supported by Figure 2.6 that shows no differences depending on being already informed in the first stage of the experiment.

Respondents clearly indicate a preference for a yearly wealth tax. Overall, 68.4% of respondents opt for the yearly wealth tax, 19.4% for the one-time wealth tax and 12.2% are indifferent. In Figure 2.6, we display the shares of the preferred tax system by groups.

[22] Respondents with low redistributive preferences in the control group even indicate higher effective tax rates than respondents with high redistributive preferences in the treatment group.

**Figure 2.6:** Within-Choice.




Note: Share of chosen tax instrument by experimental groups.

The vast majority of our participants opt for the yearly wealth tax in each experimental group. Thus, this reinforces our finding that higher preferred effective tax burdens for a yearly wealth tax is a direct expression of it being the preferred tax instrument. Running a logit regression to explain the choice for a yearly wealth tax instrument, we find that in addition to facing the yearly wealth tax in the previous part, higher redistributive preferences, higher income, and being employed increase the likelihood of choosing the yearly wealth tax, while respondents who describe themselves as conservative are less likely to choose the yearly wealth tax (see Table B7).[23]

The decision for or against a specific wealth tax instrument could also be influenced by personal interests. Since the age of the participants, their income and also their expectation to be affected by the inheritance tax do not play a role, we assume that these interests have only a minor influence on the answers regarding hypothetical persons.

We also implemented a very insightful textual analysis in order to better understand the specific reasons underpinning our respondents' choices. In an open-ended survey question they were asked to briefly motivate their choice betwenn tax instruments. Strikingly, the preference for a yearly wealth tax is mainly explained by (1) a general preference for smaller payments compared to one huge payment for a one-time wealth tax and by (2) continuous and predictable tax revenues for the state (see Appendix B.4.2 for the detailes textual analysis).

In sum, remaining differences in *informed* preferences across wealth tax instruments in terms of effective tax rates are confirmed when people are directly choosing the wealth tax instrument.

[23]Looking at the specific party affiliation, supporters of the conservative AfD and the liberal FDP in particular vote more often for the one-time wealth tax (see Figure B6).

## 2.5 Discussion & Conclusion

Comparing the effective tax burdens based on proposed tax parameters in case of a yearly wealth tax and a one-time wealth tax, we find a large and significant difference. Our research design enables us to explain part of this difference explicitly by bounded rationality: While the total difference between preferences for both taxes in the untreated groups amounts to about 39 percentage points (yearly wealth tax about 55%; one-time wealth about 16%), a share of about 15 percentage points of this difference can be assigned to computational issues. Hence, our respondents clearly underestimate the tax burden consequences of the yearly wealth tax. Informing respondents about these consequences does barely change the preferred effective tax rate in the one-time wealth tax, but decreases the effective tax burden in the yearly wealth tax by about 15 percentage points. Therefore, almost 40% of the difference can be explained by misperceived consequences of the yearly wealth taxes.

However, this effect may be biased due to three reasons: First, we neglect any "second round effects" of the yearly wealth tax, i.e. we only consider the direct tax burden and assume that our hypothetical persons in our scenarios do not change to their savings plans. Thus, our research design might underestimate the actual burden induced by the tax. As a result, our information treatment, which is calculated based on these assumptions, also underestimates our treatment effect. Second, respondents might be subject to a *round number bias* and are reluctant to enter sufficiently differentiated decimal places in their proposed tax rates. In consequence, they would be less likely to adjust their tax parameters sequentially until they get their exact preferred effective tax burden. This might bias responses in both directions. A further analysis in our Appendix B.2, however, clearly indicates that this issue can at least (partially) be resolved in the treatment group: a significant share of answers shifted from round numbers to numbers including decimals. Third, our design does not provide any additional incentives for respondents to react to our information treatment. Especially due to this latter point, we assume that we rather underestimate the treatment effect.

After resolving computational issues, a large and significant difference (more than 60% of the total difference) remains. Indeed, this trend is supported by our additional within-question: Respondents - independent of the experimental group - clearly prefer a yearly wealth tax over a one-time wealth tax. In line with findings of other disciplines (see e.g., Della Vigna and Malmendier, 2006; Iyengar et al., 2011), we reason that people accept higher total payments if they are split into (several) smaller ones. This finding also supports the findings of Blesse et al. (2019) who show that individuals are actually willing to accept complexity if normative preferences (here: the demand for smaller installments) preponderate.

Yet, it is important to emphasize how our experiment builds on the strong assumption of the comparability between a yearly and a one-time wealth tax. Whereas this does not affect the strong internal validity of our results with respect to the documentation

of the significant computational deficiencies (cf. our first hypothesis), the interpretation of the "informed preferences" strongly favoring a periodic wealth tax (cf. our second hypothesis) might as well be affected by differential beliefs on e.g. purpose of tax revenues or entitlement of the tax payer. Our research design attempts to address these concerns as far as possible: It purposely refrains from inducing beliefs on public spending with most abstract wording possible and considering only a single generation. While with this level of abstraction our study is well in line with a growing body of literature using such methods to elicit normative preferences for taxation,[24] we are, of course, not able to completely cancel out these concerns. Nonetheless, with this we believe to at least provide an important piece to the picture of preference formation towards wealth taxation.

Lastly, one might argue that uncertainty about legislative changes in the future might affect preferences regarding different types of wealth taxation. For example, a one-off tax that is levied at an age of 80, would not affect 30-year-old taxpayers in case of a repeal within 49 years. Obviously, the legislator could also increase or decrease the respective tax in this period of time. However, we don't perceive zhis issue being of concern for our results. By design, we put our respondents into the role of implementing their personally preferred tax policy for the proposed period of time. They choose their preferred parameters of a certain type of wealth tax rather than evaluate whether they propose a certain type of wealth tax under uncertainty.

Taken together, we argue that preferences for tax parameters of wealth taxation are driven by three main factors: First, preferences depend on how these parameters translate into the total (effective) tax burden. Second, preferences depend on whether the single tax payments are seen as reasonable or affordable for the taxpayer. Third, although it might be that responses for round numbers are especially strong, and thus biased in an experimental setting, we do not think of this as a mere experimental phenomenon. We rather claim that also "true" preferences for round numbers exist "in reality". Especially in case of the yearly wealth tax, the latter two factors might complement each other leading to effective tax burdens that are higher than the preferred tax burden of a one-time wealth tax.[25]

One example in our results serves as a nice illustration of these effects: The preferences of respondents who describe themselves as economically conservative/liberal. Although stating relatively low distributional preferences, their proposed tax parameters in the untreated group are only slightly lower compared to those, who support redistribution (see Figure 2.5). However, the treatment effect is much stronger among conservatives/liberals showing that being aware of the tax burden consequences, they sharply reduce their proposed tax rates. In other words, they support supposedly low wealth taxes although disagreeing on high wealth tax burdens. Parts of this finding can also be observed in polls about a yearly wealth tax in Germany: Even the majority of voters of the conservative

[24] See e.g., Weinzierl (2014), Weinzierl (2017) and Fisman et al. (2020).

[25] This can explain why Chirvi and Schneider (2020) come to an apparently conflicting finding as they focus on the first factor of preferences (total tax burden) and abstract from preferences towards tax parameters that are the center of tax reform discussions.

CDU/CSU (62%) and the liberal FDP (52%) support the introduction of a yearly wealth tax of 1%.[26]

The key conclusions that can be drawn from our results are twofold and tie in with our two hypotheses: First, computational issues exist in case of the yearly wealth tax, but can (at least partially) be resolved by the provision of information about tax burden consequences. These insights yield the unsurprising, yet fairly important implication of how a public discourse should be administered in face of complex reforms like taxation. Easily accessible but comprehensive information is key to be capable of forming qualified voting preferences. The risk of misleading or unintended policies is apparent - just as apparent as the potential exploitation of these misperceptions by asymmetrically better informed societal players like lobbyists and policy makers. Second, we find a prevailing preference for a yearly wealth tax after resolving misperceived effective tax burden consequences. Whereas we believe this preference corresponds to a somehow more intended, actually *informed* preference we can not fully factor in other potentially important behavioral biases. It is up to further research to explore how such biases (e.g. timely preferences as hyperbolic discounting or concentration bias; or round number bias) or specific heuristics translate into preferences towards tax instruments.

[26]In total, 72% support such a tax, while 23% oppose it, see https://www.tagesschau.de/inland/deutschlandtrend/deutschlandtrend-1897.html (08.07.21). In this sense, we also find that voters of the CDU/CSU differ surprisingly little from voters of the SPD in terms of proposed yearly wealth tax burdens (see Figure B5).

# Chapter 3

# The Bright Side of Tax Evasion

Responses to income taxation are usually categorized along two dimensions: labor supply responses and evasion/avoidance decisions. Previous literature shows that mainly the latter dimension is empirically relevant to assess revenue implications (Saez et al., 2012; Gruber and Saez, 2002). Thus, the predominantly proposed implication was not to adjust tax rates (as Mirrlees, 1971, would suggest) but rather to minimize evasion opportunities. Surprisingly, the interaction of these two dimensions turns out to be empirically understudied: if evasion opportunities imply positive labor supply responses, the elimination of those might not be desirable. This research gap seems even more surprising against the rather futile history of fighting tax evasion: Recent official estimates by the Internal Revenue Service (IRS) (2020) quantified the average annual gross tax gap[1] between 2011 and 2013 at $441 billion, which translates into a non-compliance rate of 16%. To put this into perspective: this is as much as half of the US deficit in 2012, or, the federal spending on health and education taken together (Office of Management and Budget, 2013). With 71%, this tax gap is primarily driven by underreporting in individual incomes and self-employed individuals (Internal Revenue Service (IRS), 2019).[2] Recent empirical research supports this picture: when the true tax base lacks observability (i.e. self-reported incomes and sectors that rely on cash), workers do exploit these information asymmetries with substantial tax evasion (Kleven et al., 2011; Slemrod, 2007). A behavior which is observable throughout the whole income distribution, from plumbers to lawyers (Alstadsæter et al., 2019).

Based on these observations, the issue of income tax evasion and avoidance has been extensively studied over the past decades from both a theoretical as well as an empirical angle (Slemrod, 2019). The predominant implication in this strand of literature is to increase tax compliance in order to raise revenues. Supporting the assumption of a rational selfish agent choosing her level of illegal behavior (Becker, 1968), it has been shown that increased fines and audit probabilities reduce tax evasion (Blackwell, 2010). Building upon the seminal framework of Allingham and Sandmo (1972), lab experiments mainly

[1] Estimated total true tax liability minus taxes paid voluntarily and timely.

[2] Individual income taxes account for the major part of this gap, excluding corporations, employment- as well as estate taxes.

focused on the relation of deterrence, audit probabilities and fines on compliance (Torgler, 2002). Other empirical studies focused on the effect of intrinsic motivation (Wahl et al., 2010) or morals and social norms (Fellner et al., 2013). The conventionally highlighted adverse consequences of tax evasion are the subsequent injustice in distribution and, more importantly for this project: the reduced tax revenues. As a result, the common perspective in this strand of literature postulates tax avoidance and evasion leading to inefficient outcomes and being morally unacceptable.[3] Accordingly, most of the empirical literature focused on increased enforcement.

However, an early strand of theoretical literature deems this perspective as too short-sighted, proposing a yet disregarded positive side to tax evasion. The seminal work by Weiss (1976) theoretically shows how an optimal income tax might include incentives to cheat in order to partially offset the undesirable distortions of taxes on labor supply. In other words, tax evasion can reduce the effective tax rate on labor income which in turn increases the incentive to work (i.e. the tax base). Most importantly, Weiss (1976) argues that allowing for tax evasion can have a welfare-increasing effect *beyond* what would be achieved by simply reducing the statutory tax rates equivalently. That is, the opportunity to cheat can reduce the excess burden of a tax system (i.e. by increasing labor supply), while tax revenues are being held constant or even increasing - representing a real Pareto improvement. Subsequent works discuss this mechanism under the concept of "randomized taxation" with the "desirability of cheating" as a special case. Specifically, Stiglitz (1982), Brito et al. (1995) and Hellwig (2007) discuss the necessary assumptions for tax evasion opportunities to be optimal. In essence, these are twofold. First, labor supply needs to respond to taxes. The more a decrease in taxes increases labor supply, the greater the revenue from decreasing effective tax rates through the opportunity to evade. Second, risk needs to increase labor supply. In our case risk about the after-tax income is being induced by the opportunity to evade. Specifically, they conclude that the induced uncertainty only increases tax revenues, if individual' preferences exhibit a property of increasing risk aversion.[4] Whether this condition holds is essentially an empirical question. Hence, previous literature remained ambiguous about the empirical relevance of this theoretical suggestion (Yitzhaki, 1987; Hellwig, 2007).

The inconclusive, exclusively theoretical discussion on the desirability of evasion opportunities calls for an empirical assessment. We want to bring this counterintuitive notion to an empirical test. The question of whether the opportunity to evade taxes can indeed increase overall tax revenue remains empirically unanswered thus far.

To the best of our knowledge we are the first to empirically test the direct link between the opportunity to cheat and overall tax revenues. Whereas Swenson (1988) empirically tests the relation between taxes, labor supply and tax revenues depicted by the Laffer curve, he does so without the opportunity of evasion. Most closely related to our paper,

[3] With the only reason why tax evasion might be acceptable being disproportionate costs of enforcement (see for example Keen and Slemrod, 2017).

[4] In other words, individual risk aversion needs to increase with income in order for evasion opportunities to be optimal.

Doerrenberg and Duncan (2014) investigate the effect tax evasion opportunities have on labor supply in laboratory experiments. Specifically, they assess the effect of evasion opportunities on labor supply with respect to changes in a flat tax. Their findings are fairly ambiguous: the direction of the treatment effect is contingent on the specific history of tax rates across rounds. Generally speaking, the labor supply effect is stronger for decreasing tax rates than for increasing ones. Our study differs from the latter two papers in two fundamental ways: First, both studies are implemented in true lab-settings. We argue that these lab setting are ill-suited to explore labor supply responses. Subjects participate with a pre-determined time frame. As a result, the opportunity costs of work become fairly small. Indeed, both papers document very small labor supply responses. Second, these papers solely focus on labor supply responses and do not elaborate on overall tax revenues. Even though (positive) labor supply responses represent an important aspect to the proposed theoretical mechanism, this mechanism also comprises a more comprehensive welfare perspective including e.g. the role of paid penalties and risk aversion. In terms of tax revenues, we are aware of a recent paper by Bergeron et al. (2021) who examines in a field experiment how to raise tax revenue in situations where tax evasion is inevitable (i.e. developing countries). They find the interaction between enforcement and tax rates to be decisive. Tax rates in developing countries are found to be too high with little compliance on the extensive margin due to liquidity constraints. Yet, the sequential interplay of increasing enforcement and tax rates will optimize the revenue maximizing tax rate. However, they remain unclear about the absolute level optimal of enforcement. Even though this paper focuses on overall tax revenues, they are not able to address labor supply responses, since they only consider the property tax with no possible response on the intensive margin (i.e. adjusted labor supply).

Taken together, empirical work exploring the effect of evasion opportunities either regarded mere labor supply responses without overall implying total tax revenues or did not consider the possibility of compliance responses on the intensive margin (i.e. focus on property taxation). Surprisingly, none of these works refers to the literature of randomized taxation. This project aims to fill this gap. It wants to challenge the notion that tax evasion unavoidably reduces the overall tax revenue and investigates if an opportunity to cheat in income taxation might actually increase overall tax revenues. It proposes an original attempt to cleanly identify how the opportunity to cheat affects the overall tax revenues.

For this, we ran an original real effort experiment in a real online labor market with nearly 1,000 participants. Importantly, in order to reveal honest labor supply responses, our participants individually decide upon the time they want to spend working on our real effort task for which they are paid on a piece-rate basis. Specifically, each of our subjects is asked to indicate her willingness to work for eight different payment scenarios: Within each flat-tax rate of 20%, 40%, 60% and 80%, they are faced with either a "low wage" or a "high wage". The order of the "high wage" and "low wage" payments as well the different tax rates is randomized. After stating the number of tables they are willing to work on

in each of these eight payment scenarios, respondents are required to actually work on only one randomly picked scenario and are paid out accordingly. While this setting is completely identical in our treatment and control group, the treatment group is given the opportunity to evade the tax: They decide upon their labor supply while simultaneously being able to invest (part of) their income into a lottery. If participants win, they avoid paying any tax on the invested amount; if they lose, they have to pay the tax on the invested amount plus an additional fine of 20% on the money invested. The subjects in the control group are only able to decide upon their labor supply, without any opportunity to avoid the tax.

This lab-in-the-field experimental design offers a couple of important advantages: First, we collect 8 different data points per participant on labor supply. This allows us to elicit individual reservation wages and labor supply elasticities while avoiding undesired effects of tediousness between the different scenarios. Moreover, the labor market characteristics allow us to elicit actual willingness to work: each participant is free to decide on the amount of time spent on the experiment herself. Respondents' participation in the experiment is only granted as long as the payment scheme is perceived as profitable. This constitutes a methodological advantage to related studies that implemented real effort tasks only in true lab-settings with fixed time frames (e.g. Doerrenberg and Duncan, 2014). In addition, such a gamified and artificial experiment allows us to cleanly identify the mechanism suggested by the theory. It deliberately abstracts from many real-life aspects, cancelling out confounding factors like framing, lying aversion, moral costs or social preferences (as discussed in e.g. Slemrod, 2007; Kirchler et al., 2008).

Our empirical findings are as follows: First, we find in general strong labor supply responses to changes in the net-of-tax rate. Under the most profitable condition (high wage and low tax) only 7% of all participants decided not to work, whereas under the worst condition (low wage and high tax) already 34% of all participants decided not to work. The average labor supply – aggregated over all tax levels and the two wage levels – in the Evasion-treatment increased substantially, on average by 37%, compared to the NoEvasion-treatment. Second, we find that a considerable amount is evaded - on average almost 40% of the income. Finally, and most important, we find that the opportunity to evade significantly and substantially increases the expected tax revenue, by more than 50% in the highest tax scenario. Whereas we document a classical Laffer curve with peak at a 60% tax in our control treatment, the substitution effect between work and leisure seems to be offset and tax revenues keep increasing with increasing tax rates (highest tax revenues at 80%) in our Evasion-treatment. This effect is strongest for the low-wage scenario. Strikingly, this effect still prevails when comparing effective tax rates: Lowering effective tax rates through the opportunity to evade is more efficient than simply lowering statutory tax rates.

Our empirical support of the mechanism suggested by Weiss (1976) speaks fundamentally to a variety of research areas. We view our contribution as being twofold. First, our results directly speak to the theoretical debate on the desirability of evasion opportuni-

ties (i.e. "randomized taxation"). Specifically, Hellwig (2007) posits that a stochastic tax scheme is only desirable in the case of increasing risk aversion, which he deems empirically unlikely. On the contrary, our experiment strongly suggests the empirical existence of this mechanism, yielding efficiency gains by inducing uncertainty. The exhibited positive labor supply responses are so strong that overall tax revenues actually increase – even if the defined penalty and detection probability should lead to a lower expected tax revenue. Put in terms of *certainty equivalents*: individuals are even willing to pay a price to enjoy the uncertainty induced by the opportunity to evade; despite the prevalent intuition of this mechanism to rather be a curiosum. Thus, our empirical findings might also suggest mechanisms, which are simply not yet sufficiently reflected in the previous theoretical debate. Specifically, the problem appears to be a sub-question of the large literature on optimal decision-responses to risk (e.g. "precautionary saving"; Kimball, 1990), "prudence" and higher-order risk aversion (e.g. Deck and Schlesinger, 2010; Noussair et al., 2014; Ebert and Wiesen, 2014). Even though our research design only partially reflects such specific traits discussed in this literature, our results suggest that the idea of inducing after-tax uncertainty via incentives to evade should not be dismissed as a mere theoretical curiosity but need to be explored deeper.

Second, and most substantial, our findings have fundamental implications for the welfare analysis of tax evasion. A few studies already highlighted potential positive effects of tax evasion, however, these are limited to rather static considerations. The standard literature on optimal administrative tax enforcement usually equates the marginal costs of increased tax enforcement (e.g an additional tax official) to the marginal revenue gains (for a comprehensive overview see: Slemrod, 2019). Nevertheless, these studies consider the tax basis as a datum and ignore its endogeneity. Two studies go beyond this mechanism: Keen and Slemrod (2017) argue to also factor in real labor supply responses of increased enforcement. However, they only consider the negative labor supply responses of increased enforcement to the extent an equivalent explicit tax rate increase would have. Slemrod and Traxler (2010) connect to the literature of Weiss (1976) and Hellwig (2007) acknowledging the possibility of labor supply responses beyond what a similar increase in tax rates would imply. As a result, their theoretical work already anticipates the possibility of the desirability of allowing for some extent of non-observability of the tax base, *even if enforcement would be costless.* Yet, this theoretical eventuality lacks empirical support. Our empirical results clearly confirm this hunch: tax evasion opportunities are still desirable, even if perfect enforcement would be costless. Furthermore, and in a more fundamental sense, our findings also speak to the closely connected literature on the excess burden of income taxation. The seminal work of Feldstein (1999) postulates that it is irrelevant whether the efficiency costs leading to the deadweight loss arise from decreased labor supply or tax evasion. Chetty (2009) objects to this, carving out how the deadweight loss from evasion and avoidance is smaller than that of decreased labor supply. Due to yet unconsidered transfers to others (e.g. charitable giving) and differently perceived costs of tax sheltering, he argues for a distinction between the elasticity of taxable income and

earned income elasticity. Our results add another dimension to the story: the *interaction* between the evasion opportunity and labor supply decision adds an efficiency enhancing effect *beyond* what would be achieved by a simple reallocation of resources. Therefore, the focus on the concept of taxable income elasticity might even further overemphasize the negative implications of tax evasion. Along similar lines, conventional tax gap measures might be overstated due to the tax-base-reducing behavioral effects triggered by perfect enforcement. In its consequence, conventional tax gap measures are potentially based on misleading counterfactuals.[5]

Finally, some of our papers' implications might be taken into account for policy discussions. The level of tolerated evasion will be a more and more conscious choice of governments in the rise of cashless economies. With the introduction of the E-Krona in Sweden and the E-Yuan in China, these countries are on the brink to become fully cashless.[6] These digital currencies are neither decentralized nor anonymous and thus yield complete observability of the tax base, making tax enforcement close to costless. The examined mechanism raises the question of how this affects the behavior of vendors, cleaners, plumbers, hairdressers, etc. Our results show how a government might be well-advised to not force its citizens to complete compliance. Notwithstanding, there are already cases in which governments tolerate a certain degree of evasion and avoidance behavior: fiscal competition. If statutory tax rates are set by a federal government whereas tax collection is implemented by the local authority, these authorities are able to determine their factual tax rate through their enforcement policy (e.g. through number of tax officials or granting deductions).[7] Previous works associate this behavior with the conventional notion of efficient tax administration. Our findings add another rationale to decrease the effective tax rate: tax base increasing effects and thus potentially higher revenues. Furthermore, our results imply a differential treatment of different income levels. This finding yields a policy recommendation for the most prevalent case: Due to limited resources, governments are just not capable of fully observing the tax base. Since the positive effect on labor supply is more pronounced for low-wage earners, governments should rather focus their resources on the enforcement of high-wage earners. Given this is mostly already common practice, our paper provides another economic perspective on this implicit rationale.

The remainder of the paper proceeds as follows: Section 3.1 provides a brief model with predictions, sections 3.2 and 3.3 will give a detailed description of our experimental design and the data. In Section 3.4 the results are presented followed by a brief discussion and concluding remarks in Section 3.5.

[5] Gemmell and Hasseldine (2014) already discuss such an effect - but not beyond what would be achieved by the enforcement-equivalent tax increase.

[6] In 2018, only about 10% of the transactions in Sweden remained in cash (see: https://www.nytimes.com/2018/11/21/business/sweden-cashless-society.html (08.07.21). A trend that has been massively accelerated during the Covid-19 pandemic.

[7] See Stö and Traxler (2005) for conceptual considerations; Cremer and Gahvari (2000), Bönke et al. (2017), Baretti et al. (2002) for empirical references.

## 3.1 Motivating Framework

In this section we present the theoretical model of Weiss (1976) who extends the seminal framework of Allingham and Sandmo (1972) by integrating the decision on reporting income with the decision on labor supply. This framework serves to illustrate how the determinants of labor supply and tax evasion eventually affect overall tax revenues. The subsequent experimental design aims to match this framework and operationalizes these determinants. Since our paper is not a direct test of this theory, we rather briefly introduce the intuition. For formal derivations please see Weiss (1976).

### 3.1.1 Model and Hypotheses

Each individual decides about her labor supply $L$ given a certain wage rate $\omega$. The resulting pre-tax income $c = \omega L$ is taxed by the government with a proportional tax rate $t$, which leads to an after-tax income of $\omega L(1-t)$. However, individuals also have the opportunity to underreport a share of their gross income $a$ ($a \in \mathbb{Q} | 0 < a < 1$). The government audits with a probability $(1-p)$. If the individual is audited, the regular tax on $a$ has to be paid as well as an additional fine $q$, which is proportional to the income understated $C = a\omega L$. Importantly, the decisions on labor supply and unrerreporting are taking place *jointly* in this framework. Thus taxable income constitutes an endogenous variable with the individual substituting across two margins: the decision to take risk or no risk as well as the decision between leisure and labor supply.[8] Leisure is denoted by $l$. For simplicity, there is no non-labor income, no redistribution and audits as well as avoidance decisions are costless (i.e. no social norms or frictions in underreporting income). All income $c$ is consumed. The expected utility function is continuous, twice differentiable and concave.

$$F(L, C) = E[U(c, l)] = pU[\omega L(1-t) + ta\omega L, -L] + (1-p)U[\omega L(1-t) - qa\omega L, -L] \tag{3.1}$$

With $c^*$ and $C^*$ satisfying the first-order conditions ($\frac{\delta F}{\delta L} = 0$ and $\frac{\delta F}{\delta C} = 0$), the expected tax revenues for the government are then depicted by

$$E(T) = c^* t - C^*[pt - (1-p)q] \tag{3.2}$$

With this, the audit probability can be set such that individuals are indifferent to evade:

[8] Cowell (1985) discusses a very similar framework with formal and informal labor markets. He shows how substituting along these two margins only yields ambiguous predictions since "all sorts of behavior could be consistent with rational expected utility maximization."

$$p^* = \frac{q}{t+q} \tag{3.3}$$

To examine the changes of $c^*$ and $C^*$ in response to enforcement changes, $\frac{\delta F}{\delta L} = 0$ and $\frac{\delta F}{\delta C} = 0$ are totally differentiated w.r.t. $p$ (Weiss, 1976, p. 1345). Evaluated at $p^*$, this implies:

$$\frac{\delta C^*}{\delta p} = -\frac{U_1}{U_{11}} \frac{t+q}{tq} \tag{3.4}$$

It follows that individuals evade inversely proportionally to their coefficient of absolute risk aversion if $p$ is raised.

In order to assess the effect of the opportunity to evade on labor supply Weiss (1976) examines the second derivative at $p^*$, $\frac{\delta^2 L^*}{\delta p^2}$ (Weiss, 1976, p. 1345). Based on this, he describes how a positive expectation of evasion now yields ambiguous predictions (depending on the concavity or convexity of the first-order condition). Since expected income for a given effort is raised, the marginal utility of income decreases, which in turn decreases labor supply. However, the absolute risk aversion now becomes decisive: If the absolute risk aversion increases with wealth, labor supply will indeed increase. Whereas if absolute risk aversion decreases with wealth, labor supply will as well decrease. Intuitively speaking, tax evasion increases labor supply if the individual anticipates paying a penalty (i.e. is risk averse) since the labor supply increasing effect of risk aversion outweighs the labor supply decreasing substitution effect (for formal derivations, see Weiss, 1976).

To illustrate the eventual tax revenue increasing effect, Weiss (1976) uses a utility function of the type

$$U(L, l) = \frac{1}{1-b}(L)^{1-b} + l \tag{3.5}$$

He presents that the condition for tax evasion having a revenue increasing effect is $t > \frac{2b}{b+1}$ (see Weiss, 1976, p. 1349), whereas $b$ is the Arrow-Pratt relative risk aversion.

This theoretical framework by Weiss (1976) provides us with three directly testable predictions:

**Hypothesis 1.** *With the incentive to cheat tax evasion increases.*

**Hypothesis 2.** *Labor supply will increase with the opportunities to evade taxes.*

Based on the mechanism proposed by Weiss (1976), the confirmation of the fist two hypotheses constitute the necessary conditions to test our third hypothesis, the centerpiece of our paper:

**Hypothesis 3.** *The opportunity to evade increases overall tax revenues.*

## 3.2 Experimental Design

The goal of the design of this experiment is to mirror the basic framework proposed by Weiss (1976) in order to assess its counterintuitive implication: with reduced enforcement, overall tax revenues might increase. This design aims to cleanly investigate this, thus far, exclusively theoretically discussed notion. For this, we exploit the advantage of a controlled, abstracted lab-in-the-field experiment, focusing on the mechanisms suggested in the theoretical debate. Specifically, we operationalize the determinants *labor supply* and *tax evasion* in a deliberately abstract setting: Participants were able to earn income for a real-effort task on a piece-rate basis. For this income taxes were due (i.e. as a "fee"). Whereas the control group had no choice but to pay the fee, the treatment group was able to invest (parts of) their income into a lottery to avoid paying those fees. Since our participants were able to freely decide upon the number of tasks they are willing to work on, we were able to determine the differential labor supply and resulting tax revenue between the two treatment groups.[9] The following paragraphs provide a more detailed description.

### 3.2.1 Experimental Environment: A Real (Online) Labor Market

The empirical literature on tax evasion faces the fundamental issue of missing data on different levels: Firstly, the evasion opportunities themselves are hardly quantifiable since these are often situated in highly complex legislative settings. Accordingly, changes of these opportunities are even more obscure. Further, data on criminal behavior is, by nature, very elusive. Moreover, even the legal part of determinants is not straightforward to measure: Overall tax revenues are subject to constant legal changes and, on top of that, prone to macroeconomic cycles. Similarly, information on tax enforcement itself is not publicly disclosed. Even though studies roughly estimate actual audit probabilities, these are highly dependent on the specific sub-group of income earners as well as the sophisticated enforcement strategies of governmental agencies. Besides that, individual perceptions of enforcement might highly diverge from real probabilities and are therefore even harder to measure. Finally, reversed causality poses a problem since the level of enforcement potentially is not exogenous: it responds to the level of overall tax revenues and vice versa. As a consequence, labor supply elasticities and the resulting revenue implications are difficult to estimate and cannot be interpreted in a causal manner. Thus, it is close to impossible to answer the question at hand based on observational data (Slemrod and Weber, 2012). Therefore, our research question calls for a tightly controlled experiment.[10] Such a truly randomized experiment generates reliable data on individual decisions on labor supply as a response to the opportunity to evade. The typical high "internal validity" allows us to isolate the specific mechanisms and derive causal statements on the revenue implica-

[9] For this experiment we obtained approval by the German Association for Experimental Economic Research e.V. (GFEW) in advance.

[10] Indeed, this method is widely employed in the literature on tax evasion(Alm and Malézieux, 2020).

tions of evasion incentives. Against the background of the rather theoretical motivation of our research question, we used a "neutral" frame and fully "gamified" our experiment (a common technique as an alternative to a "loaded" frame, see Alm and Malézieux, 2020). This way, we deliberately abstract from many real-life aspects, cancelling out confounding factors like framing, lying aversion, moral costs or social preferences (as discussed in e.g. Slemrod, 2007; Kirchler et al., 2008).

In particular, we exploited the advantages of a real (online) labor market for our experiment; Amazon's Mechanical Turk (MTurk). MTurk is an online platform, on which usually companies post relatively simple and quick tasks (these tasks are called "Human Intelligence Tasks", HITs). These tasks are mostly repetitive like transcribing data, classifying images, transcribing audio clips, etc. (Horton et al., 2011; Berinsky et al., 2012; Paolacci et al., 2010; Mason and Suri, 2012). Recently, social scientists established this platform as a frequent subject pool for conducting experiments.[11] Multiple studies have shown that the data obtained on MTurk is at least as reliable as data obtained via traditional methods.[12]

For the purposes of our study, MTurk presents a number of significant advantages compared to a standard lab setting. First, MTurk samples tend to be more representative of the US population than typical student samples: these samples are usually more diverse in age, ethnicity, education and geographical location (Difallah et al., 2018; Buhrmester et al., 2011; Berinsky et al., 2012; Paolacci et al., 2010). Second, peer effects can be excluded as participants have no way of meeting the other participants. Subjects' anonymity can be sufficiently ensured as only their anonymized MTurk-ID is collected.

Third and most strikingly, experiments embedded in online labor markets present a particularly useful environment for real-effort tasks, as subjects in online experiments face real opportunity costs. With each participant being free to decide on the amount of time spent on the experiment herself, we are able to elicit the actual willingness to work. Appearing to an appointment in a physical lab with a pre-determined duration yields "sunk costs" that would motivate participants to work below their reservation wage otherwise. Exploiting the open labor market characteristic of Amazon Mechanical Turk, respondents' participation in the experiment is only granted as long as the payment scheme is perceived as profitable.[13] This allows us to overcome a common problem of labor supply experiments in labs and elicit credible preferences in labor supply.[14]

Nonetheless, leaving the lab and recruiting from a more general population on the internet also bears a couple of risks. In particular, non-US based MTurkers using Virtual Private Servers (VPSs) or automated scripts ("bots") have appeared to cause a decline

[11] For example: Suri and Watts (2011); Peysakhovich et al. (2014); Rand et al. (2014); Mao et al. (2017); Jordan et al. (2017).

[12] See among others: Arechar et al. (2018); Horton et al. (2011); Berinsky et al. (2012).

[13] Our average hourly payment was calibrated very carefully to the common average payment level on MTurk to prevent anomalous labor supply responses, see Section C.2.1.

[14] Related studies that implemented real effort tasks in true lab-settings only documented small labor supply responses (e.g. Doerrenberg and Duncan, 2014).

in data quality.[15] We implemented a couple of measures and checks to reduce this risk as far as possible: As is common in practice, only US-based workers, verified through IP addresses in MTurk, with an average approval rate of 95%, and at least already 500 sucessfully completed tasks were allowed to take part in our experiment.[16] We further implemented basic measures such as limiting the visibility of our survey to participants who signed up at MTurk with a US address and asking to confirm participants' US residency in the consent form. As a "gate-keeper" and to double check the self-indicated location, we used a third-party web service, "IP Hub". This service identified participants attemping to participate in our experiment using a tool to mask their location outside the US (i.e. VPS, VPN or proxy). These participants were automatically excluded before they could enter our experiment and thus did not have to be paid. Next, participants had to pass a captcha-test that identifies non-human users on the first page. Subsequently, we designed an attention check which visually re-sampled a typical choice set between six different games, similar to the Eckel Grossmann task (Eckel and Grossman, 2002). Here, we wrote in a short text to just select the third game if they attentively read the instructions. All subjects failing this task were either not reading the instructions carefully or potentially bots. Failing this task led to the direct exclusion of the experiment. Further, to consider the possibility of participants using automated means to process our real effort task (e.g. "optical character recognition" software) we examined the average time needed for counting a single table per participant. Assuming that such a software would solve such a task in a fraction of a second, we do not find indication for such tools.[17] Finally, we prevented workers from participating in our study more than once: Respondents had to enter their unique worker ID on the first page before they were able to start the survey and only at completion received a password to submit to MTurk. We clearly stated that any violation would be penalized by rejecting the HIT which would result in a significant reputational loss for workers on MTurk.

### 3.2.2 Labor Supply

To operationalize one of our key determinants, labor supply, we utilized the "counting-zeros" real effort task, originally applied by Abeler et al. (2011): to count the numbers of zeros in a 10 x 15 digit table randomly filled with 150 zeros and ones. We included this task not only because it is very monotonous and tedious and therefore includes positive costs of effort, whereas intrinsic motivation is (largely) excluded. It is also artificial with clearly no value to the experimenter so that subjects would not anticipate higher payments or ratings by the experimenter with different levels of effort. Finally, it is mostly independent

[15] Recent studies (e.g. Kennedy et al., 2018; Stokel-Walker, 2018) have shown that the subject pool on MTurk potentially has issues with bots, non-US based workers with poor English skills or simply inattentive participants.

[16] Requesters can review the work done by MTurkers and decide to approve or reject the work. Approved work is paid as indicated in the contract and rejected work is not paid. Hence, higher approval rates of workers indicate a higher quality of work.

[17] Only two participants needed less than 10 seconds per table and nine participants less than 20 seconds (for more information see Figure C5).

from ability with no mathematical or motoric skills necessary (Abeler et al., 2011, p.473). For this task, our subjects were paid on a per-piece basis. Only if the right amount of zeros was entered, our subjects were able to proceed.

Based on these rules, the participants were asked to indicate the number of tables they are willing to work on. In detail, each participant had to make eight different work decisions: for each of the eight different payment schemes they had to indicate their personal willingness to work. In four "low wage" cases participants earn \$0.12 per table and in another four "high wage" cases \$0.25 per table. Within each of these two wage levels, participants have to pay either 20%, 40%, 60% or 80% of their gross income in fees to the experimenter. Both the order of these two payment blocks as well as the order of tax rates were randomized. After stating the number of tables they would be willing to work on in each of these eight scenarios, respondents were required to actually perform only one randomly picked payment scheme – a fact, which was made clear to the respondents before taking their decisions. This way, we are able to elicit honest labor supply responses for all potential tax rates in a truly *incentive compatible* way: Each decision is relevant. If a participant indicates her willingness to work on e.g. 60 tasks for a scenario and this scenario is then randomly picked, approximately one hour has to be spent working on the experiment. Since the payment was only processed if the participant reached the very end of the experiment (i.e. after solving all indicated tables), a non-fulfilled labor supply indication would inevitably result in a total payment of \$0.[18]

**Table 3.1:** Overview of the Eight Different Payment Schemes.

| Payment Block | Tax Rates in % | | | |
|---|---|---|---|---|
| Low: \$0.12 | 20 | 40 | 60 | 80 |
| High: \$0.25 | 20 | 40 | 60 | 80 |

Note: Each participant was faced with four scenarios of low payment and four scenarios of high payment per table. Both the order of these two payment blocks as well as the order of tax rates were randomized.

### 3.2.3 Treatment Variation: Tax Evasion

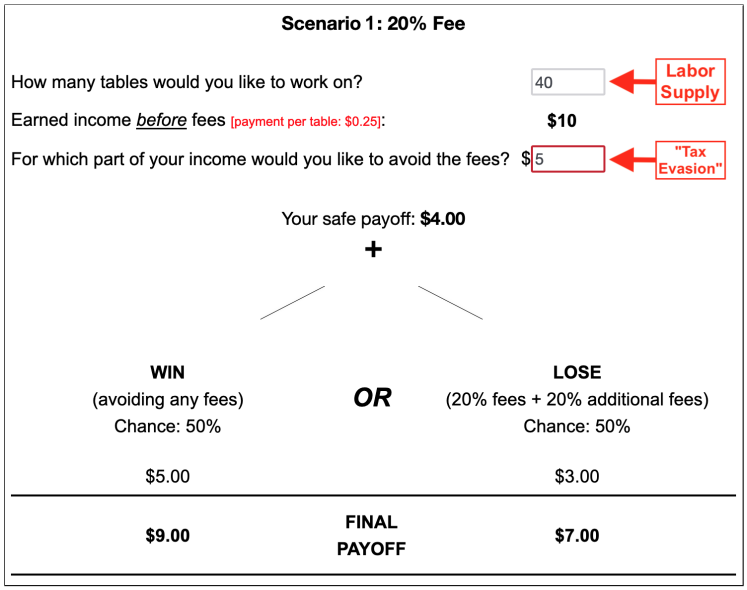
This aforementioned payment scheme was subject to our between-subject treatment variation. Specifically, our design comprises two main treatments in which the opportunity to evade is either given or not.

**The Evasion-treatment**: In the Evasion-treatment our participants jointly decide upon their labor supply and the amount evaded of their resulting before-tax income. In particular, participants are free to invest (parts of) their gross income into a lottery. The lottery is represented by a fair coin toss with a 50% chance to win, and a 50%

[18]Further, this payment scheme was structured against the background of the typical payment on MTurk. Specifically, incentives were structured such that there is sufficient room for labor supply responses: payments were better than typically in some situations; worse in others. For further details see Section C.2.1.

chance to lose. If participants win, they avoid paying any fee on the invested amount; if they lose, they have to pay the fee on the invested amount plus an additional fee of 20% on the income invested. These parameters were deliberately chosen: The 50% audit probability to maximize participants' comprehension Above all, this experiment aims to assess the theoretically suggested mechanism. Therefore we tried to prevent other confounding perceptual biases induced by e.g. small probabilities. The 20% penalty, on the other hand, provides us with a clear baseline scenario, in which risk-neutral participants are indifferent between investing into the lottery and not investing at all. Moreover, 20% is indeed a fairly realistic value.[19] To reduce the cognitive load of this decision to a minimum, we designed an interactive decision tree for this screen: The first input field asked for the number of tables the participant is willing to perform. Right below the according income before fees is calculated in real time. The second input field then asked for which part of this income the participant would like to avoid the fees. Based on this, the participant received immediate feedback on both potential payoffs in case of winning (i.e. paying no fees on the amount invested) or losing (i.e. paying the fees plus an additional fine on the amount invested) the lottery. Thereby, the participant is invited to play around with the two inputs while the potential payoffs are recalculated in real time on-screen.

**Figure 3.1:** Screenshot of the Evasion-Treatment.

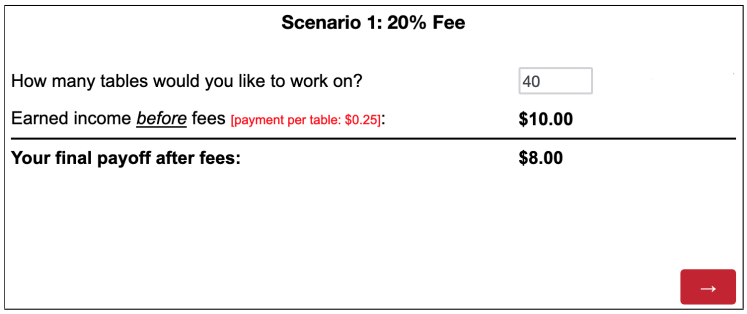



Note: Important: The red arrows and boxes were *not* part of the screen. Here, they only serve the purpose to illustrate how we operationalized "labor supply" and "tax evasion" in our experiment.

[19] E.g. in the US: the penalty amounts to 20% - 40% on the amount understated. See: https://www.law.cornell.edu/uscode/text/26/6662 (08.07.21)

**The NoEvasion-treatments**: In the NoEvasion-treatments,[20] subjects are only able to decide upon their labor supply, without any opportunity to avoid the fees. Based on the applicable fee, the participant receives immediate on-screen feedback about the final payoff after fees. Again, participants are invited to play around with their input (i.e. number of tables) with the final payoff calculated in real time on-screen.

**Figure 3.2:** Screenshot of the NoEvasion-Treatment




### 3.2.4 Key Dependent Variable: Expected Tax Revenue

We are able to calculate the expected tax revenue for each individual ($\mathbb{E}(R)_{i,t}$) by employing the following equation (Weiss, 1976):

$$\mathbb{E}(R)_{i,t} = h_i \cdot w \cdot t - e_i \cdot (0.5 \cdot t - 0.5 \cdot 0.2) \tag{3.6}$$

where $h$ indicates the labor supply (i.e. how many tables subject $i$ indicated to work), $w$ is the wage per table (i.e. \$0.12 or \$0.25), $t$ is the tax level (i.e. 20%, 40%, 60% or 80%) and $e_i$ is the income attempted to evade. With an audit probability of 50% and a penalty of 20%, the incentive to evade increases with increasing tax rates since the difference to the expected income increases. The predictions for risk-neutral, revenue maximizing participants in this setting are straight forward: While being indifferent in the 20% tax scenario, income maximizing individuals always invest all of their pre-tax income into the lottery. Or in other words; with every investment into the lottery, the government loses revenue in expectation. For the NoEvasion-treatments the term $e_i$ is, by definition, zero.

### 3.2.5 Procedure and Further Explanatory Variables

This section describes the chronological order of the actual experiment in more detail. The sequence of the specific experimental procedure is depicted in Figure C4 in the Appendix.

[20] We additionally include a **NoEvasion-Lottery**-treatment. Here, subjects decide upon their labor supply and the income evaded, which directly mirrors the decision tree in the NoEvasion-Lottery-treatment. However, tax evasion will with 100% certainty lead to punishment, which is unambiguously explained both in the instructions and on-screen (see screenshot in Appendix C3). This treatment was only designed to ensure the treatment effect not being driven by the longer instructions in the Evasion-treatment, the availability of a second choice or the reduced cognitive load in the NoEvasion-NoLottery-treatment. Our expectation of no systematic, significant differences to the NoEvasion-NoLottery-treatment is confirmed by the data (see Appendix C.4).

**Preceding Task Description.** Before starting our study, our participants were informed about the general structure of the experiment and their expected payoff with the following information: The experiment consists of two stages, the first lasting about 15 minutes comprising some basic demographic questions, a short "mini experiment" (i.e. a small game to elicit the risk aversion) and a decision on how long to work in the second stage. In the second stage they would simply have to perform the amount of work indicated in the first stage, which could be between 0 and 45 additional minutes. For the first stage the participants received an average compensation of $2.50, whereas the payment of the second stage was conditional on the amount of work they choose to perform.

**Demographics.** At the beginning of this first stage, our participants had to answer a short questionnaire on standard socio-economic background variables. These are: gender, age, education, employment status, household income, state, ethnicity, political orientation. Moreover we asked for the average hours per week they spend doing online tasks for money in order to assess if these tasks constitute their primary source of income.

**Risk Elicitation.** Subsequently, the risk preferences of the participants were elicited using a version of the Eckel and Grossman (2002) method suggested by Dave et al. (2010).[21,22] Participants were faced with a choice set of six different gambles with each gamble involving a 50% chance of winning an either high or low payment. Only the first gamble is a safe bet with a $1.65 payoff. The following choice options increase linearly in expected payoff but also in risk, with an expected payoff of $2.25 in the most risky scenario ($0 or $4.50). In their overview article Charness et al. (2013) describe this method as relatively easy to understand and thus producing less noisy estimates of risk preferences than other elicitation methods, especially when participants have low math abilities. A relevant advantage given the more diverse non-student sample of Mturk.

**Trail Stage.** Before proceeding to the actual work decisions, our participants were asked to count the zeros of two tables in order to familiarize themselves with the upcoming task. In this trial period we measured the individual time needed to proceed to the following table in order to give individual feedback on the average time required per table but also to be able to account for "ability" in our analysis.

**Work Decisions and Work Stage.** As described above, participants take 8 work decisions, of which only one randomly selected actually has to be performed. Applying this strategy method allows to elicit multiple informative data points per subject (i.e. decisions for eight different payment schemes), without confounding effects like tiredness due to working on all eight payment schemes. It is important to note that our instructions strongly emphasized how the payment will only be made when all indicated tables are counted correctly (i.e. only then the Mturk code which qualified the payment was displayed). To help gauge the individual time required, participants received individual

[21] Importantly, we counterbalanced if this risk elicitation decision had to be made before or after the work and evasion decisions took place in order to control for potential risk-hedging strategies between our participants. Our data indicated no order effect on the risk preferences nor any income effect (i.e. no effect of the order on the labor supply).

[22] In fact, for none of the 8 situations did the order significantly change the labor supply.

feedback on the estimated duration based on their average time per table in the preceding trial period.[23]

**Final Payoff.** Feedback on the final payoff is only provided at the very end of our experiment to cancel out wealth effects that may distort work supply decisions and risk aversion. The final payment consists of a \$0.50 fixed payment, the outcome of the risk-elicitation lottery (which amounts to an average compensation of \$2.50 together with the former) as well as the outcome of the work/evasion decision. Thus, the possible final payments range between \$0.50 and \$20.00 if the participants throughout picked the most risky decisions.

## 3.3 Data

### 3.3.1 Organization

Our study was implemented between March and April of 2019. During this period, the link to our study was called 1628 times by potential experimental subjects. 253 of those subjects used either a mobile device or a proxy server and were not allowed to take part in this study to ensure attentiveness and to exclude bots. Of the remaining 1375 subjects, 106 subjects tried to do the study several times (most of these subjects failed the attention check and tried to redo the experiment nevertheless). Further 61 subjects failed the attention check and were not allowed to continue. 163 subjects stopped the study before coming to the labor task and were hence dropped from the analysis as these subjects did not make all relevant choices.[24] Thus, overall we have 996 subjects across the our treatments, with 510 subjects in the Evasion-treatment and 486 in the NoEvasion-treatments.[25]

On average participants needed 38 minutes to finish our experiment, and earned a respective hourly wage of \$8.88.[26]

### 3.3.2 Demographics

The median age of our participants was 37 years (on average = 40.16), ranging from 18 to 87 years. Most of our subjects are in the age group between 30 and 44 (46%), followed by

[23] We set a limit to a max. of 60 tables. Based on a pilot study, the experiment then takes around 60 minutes in total.

[24] We further excluded all subjects who systematically invest into the lottery when it makes no sense (49 subjects), i.e., all those subjects who invested into the lottery for all tax levels and both wages when the detection was 100% (i.e., in the NoEvasion-Lottery-treatment). The best explanation for this behavior is confusion which is backed by 1) the fact that subjects who invest into the lottery in the NoEvasion-Lottery-treatment needed significantly ($t(64.1) = -4.3$, $p \leq 0.001$) and substantially more approaches to answer the control questions (subjects not investing into the lottery needed $M = 0.83$ ($SD = 1.05$) approaches while subjects investing into the lottery needed $M = 1.67$ ($SD = 1.28$) approaches) and 2) subjects indicated so in the open answer comment-space where participants explicitly said that they were confused by the option of investing in a sure loss-lottery. Excluding all subjects who invested instead resulted virtually in the same results. Since, as we expected, the two NoEvasion-treatments do not show any systematic differences from each other, we pool the two NoEvasion-treatments hereafter.

[25] NoEvasion-Lottery-treatment: 206 participants, NoEvasion-NoLottery-treatment: 280 participants.

[26] For further details see Section C.2.1.

the age group of 45 to 64 years (27%) and the age group of 18 to 29 (22%). Overall, our sample is slightly younger than the average US-American with a median age of 38.2 and with 16% of the population older than 65 years (compared to 5% in our sample).[27] In terms of gender, our sample is fairly balanced: 52% of our participants were female compared to 50.8% females in the US population. The ethnic composition is less representative: 80% of subjects are White compared to 61.3% Whites in the US population. Moreover, our participants indicated to have a higher education than the average US citizen. 60% of subjects implied to have at least a Bachelor's degree as the highest qualification compared to roughly 33% in the United States as a whole. Hence, our sample is younger, slightly more female, more white and better educated than the average US citizen. Even though our sample does not fully represent the typical American, our sample is substantially more diverse than the generic student sample typically used for taxation-experiments.

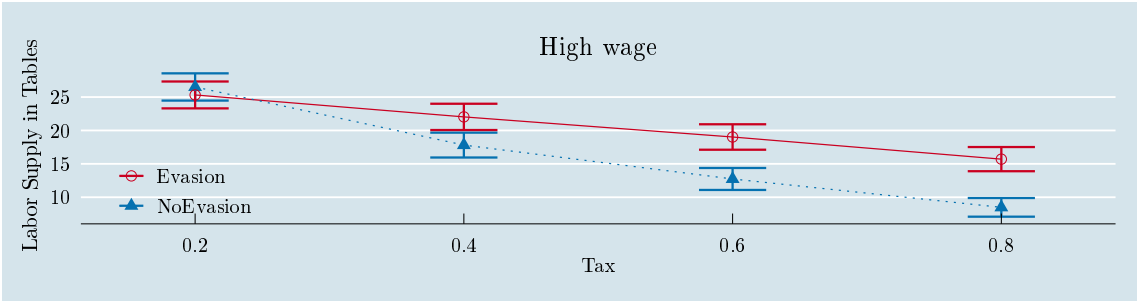
## 3.4 Results

The main goal of this study is to investigate whether the option to evade taxes can increase expected tax revenue. Before coming to the main part we will first have a look at the labor supply across treatments and a brief look at the tax evasion decisions.

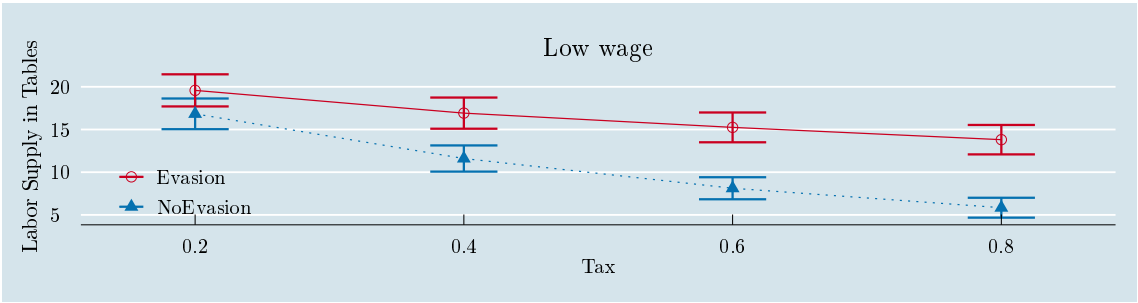
[27]For comparison estimates see the census aggregates: https://www.census.gov/quickfacts/fact/table/US/PST045216 (08.07.21) and https://www.census.gov/data/tables/2019/demo/educational-attainment/cps-detailed-tables.html (08.07.21).

### 3.4.1 Labor Supply

**Figure 3.3:** Labor Supply as a Function of Tax.




**(a)** Labor supply in the high-wage situation as a function of tax with 95% confidence intervals.




**(b)** Labor supply in the low-wage situation as a function of tax with 95% confidence intervals.

Note: Blue, dashed lines represent the NoEvasion-treatments, while red, solid lines represent the Evasion-treatment. The corresponding tunnels surrounding the respective dots represent the 95% confidence intervals.

Concerning the labor supply we see very clearly that incentives work. Under the most profitable condition (high wage and low tax) only 7% of all participants decided not to work. Under this condition participants on average were willing to work on M = 25.90 (SD = 23.00) tables.[28] However, under the worst condition (low wage and high tax) already 34% of all participants decided not to work.[29] Under this condition participants on average were willing to work only on M = 9.92 (SD = 17.32) tables. Participants on average also supplied significantly more work under high wages M = 18.50 (SD = 19.02) compared to low wages M = 13.56 (SD = 17.43), t(995)= 16.1, p $\leq$0.001.

The average labor supply – aggregated over all tax levels and the two wage levels – was the highest in the Evasion-treatment with M = 18.45 (SD = 19.33) tables. The average labor supply in both NoEvasion-treatments was M = 13.49 (SD = 15.16). This difference between the Evasion-treatment and the NoEvasion-treatments is highly significant, t(958.8)= -4.5, p $\leq$0.001. On average participants in the Evasion-treatment worked 37% more compared to the NoEvasion-treatments.

[28] An apparent discrepancy between the experiment and the theory is that we measure labor supply in number of tasks whereas the theory employs time of work. Our results show clearly that these measures are empirically equivalent (see Appendix C.2.2 for a detailed analysis).

[29] Which is significantly different across the three treatments (Evasion: 25%, NoEvasion-Lottery: 37%, NoEvasion-NoLottery: 48%) using a three-way proportion test (p $\leq$0.001).

Figure 3.3a depicts the labor supply in the two treatments for high wages and Figure 3.3b depicts the labor supply for low wages. Both graphs are rather similar and show a clear sensitivity towards the tax levels. Under the NoEvasion-treatments the labor supply is decreasing almost linearly with the tax level. Under the Evasion-treatment we can see a similar trend but we can also very clearly see that the labor supply is less sensitive to the tax levels.

To investigate this relationship further we use the following mixed effects model of labor supply to estimate the treatment effects:

$$\begin{aligned} LS_{i,t,w} =& \beta_0 + \beta_1 \cdot \mathbb{1}_{\text{Evasion}} + \beta_2 \cdot t + \beta_3 \cdot \mathbb{1}_{\text{Evasion}} \cdot t + \epsilon_i + \epsilon_{i,t,w} + C_M \\ C_1 =& 0 \\ C_2 =& C_1 + \beta_4 \cdot \text{Risk}_i + \beta_5 \cdot t \cdot \text{Risk}_i + \beta_6 \cdot \text{Risk}_i \cdot \mathbb{1}_{\text{Evasion}} + \beta_7 \cdot t \cdot \mathbb{1}_{\text{Evasion}} \cdot \text{Risk}_i \\ C_3 =& C_2 + \beta_X \cdot X \end{aligned} \tag{3.7}$$

where $LS_{i,t}$ represents the labor supply of subjects $i$ for tax $t$ under wage $w$ with $i \in \{1, \ldots, n\}$, $t \in \{.20, .40, .60, .80\}$, and $w \in \{\text{High}, \text{Low}\}$. $\mathbb{1}_{\text{Evasion}}$ denotes a dummy with value one if the participants are in the Evasion-treatment, i.e. participants can evade their income, and zero if the participants are in the NoEvasion-treatments. $t$ denotes the tax-level-effect, with $t \in \{.20, .40, .60, .80\}$. To account for the nested structure of the data we included $\epsilon_i$ as the random effects of the individual $i$. $\epsilon_{i,t,w}$ is the residuals. $\text{Risk}_i$ indicates the elicited risk preferences of subject $i$ with higher values indicating more risk-lovingness. $X$ is a vector of further control variables including age, gender, ethnicity, income, party affiliation, employment status, education, and hours spent on online work.

Table 3.2 reports the estimates of the split regression by wage. Under a 20% tax, the Evasion and the NoEvasion treatments do not differ, in line with our predictions. As subjects in the Evasion-treatment should be indifferent between evasion and no-evasion and hence, no treatment effect is expected. More interestingly, we can see that the labor supply is significantly and substantially less sensitive to tax-increases under the Evasion-treatment compared to the NoEvasion-treatments. Further, we see that risk has no influence under low wages but does have an influence under high wages. We can also see that all results are robust to the inclusion of controls.

**Table 3.2:** Linear Mixed-Effects Model of Labor Supply.

| | Labor Supply (in tables worked) | | | | | |
|---|---|---|---|---|---|---|
| | Low-Wage | | | High-Wage | | |
| Constant (20% Tax & NoEvasion) | 16.07*** (0.82) | 15.30*** (1.76) | 19.10*** (3.41) | 25.26*** (0.92) | 19.74*** (1.96) | 28.76*** (3.74) |
| Evasion | 3.17** (1.15) | 3.19 (2.52) | 2.93 (2.51) | 0.04 (1.28) | 2.38 (2.79) | 2.23 (2.78) |
| Tax (in %) | −0.18*** (0.01) | −0.20*** (0.02) | −0.20*** (0.02) | −0.30*** (0.01) | −0.24*** (0.02) | −0.24*** (0.02) |
| Tax x Evasion | 0.09*** (0.01) | 0.08** (0.03) | 0.08** (0.03) | 0.14*** (0.01) | 0.08* (0.03) | 0.08* (0.03) |
| Risk | | 0.23 (0.48) | 0.16 (0.48) | | 1.68** (0.53) | 1.54** (0.53) |
| Tax x Risk | | 0.01 (0.01) | 0.01 (0.01) | | −0.02** (0.01) | −0.02** (0.01) |
| Risk x Evasion | | −0.03 (0.65) | 0.07 (0.65) | | −0.80 (0.72) | −0.75 (0.72) |
| Tax x Risk x Evasion | | 0.002 (0.01) | 0.002 (0.01) | | 0.02* (0.01) | 0.02* (0.01) |
| Controls | × | × | ✓ | × | × | ✓ |
| Sbj specific effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 3,984 | 3,984 | 3,984 | 3,984 | 3,984 | 3,984 |
| Log Likelihood | −15,592.48 | −15,598.12 | −15,590.26 | −16,287.54 | −16,286.90 | −16,274.08 |
| Akaike Inf. Crit. | 31,196.95 | 31,216.24 | 31,218.52 | 32,587.09 | 32,593.79 | 32,586.17 |
| Bayesian Inf. Crit. | 31,234.69 | 31,279.14 | 31,338.03 | 32,624.83 | 32,656.69 | 32,705.68 |

*Notes:* $^{\cdot}p < 0.1; ^{*}p < 0.05; ^{**}p < 0.01; ^{***}p < 0.001;$

Note: Controls include age, gender, ethnicity, income, party affiliation, employment status, education, hours spent on online work, and the average time needed for solving the two sample tasks. Evasion denotes a dummy with value one if the participant was in the Evasion-treatment – participants have the opportunity to evade taxes and punishment will be met with a 50% probability – and zero otherwise. The omitted category is the NoEvasion-treatment. *Tax* denotes a one percentage point increase in the tax level. *Risk* denotes the elicited risk aversion with higher values indicating more risk-lovingness. Errors are clustered on the subject level, i.e., subject-specific effects do account for subject-specific heterogeneity.

Our results for the labor supply responses nicely tie in with other empirical findings and the theoretical predictions of the related literature. The respondents in our baseline treatment (i.e. the NoEvasion-treatments) exhibit strong negative labor supply responses towards increasing tax rates. Figures C6 and C8 depict the uncompensated labor supply and income elasticities in the NoEvasion-treatments, which range between 0.5 and 1.3 in the high wage scenario and are only slightly smaller in the low wage scenario (between 0.3 and 0.99). The empirical literature mirrors similar large elasticities of taxable income, however, mainly for individuals at the top-percentile of the income distribution (with elasticities ranging between 0.5 to 1.5, Chetty, 2009). In our setting these high elasticities seem very reasonable: the opportunity costs of the online labor market become very competitive for our high tax rates. Given the average time needed per table, the payment for the real effort task translates into an hourly payment of merely $1.21 for the very least favorable case (low payment, highest tax rate) - against a target payment of about $6/hr on Mturk (for a more detailed discussion see C.2.1).[30] Our results furthermore suit our predictions in terms of the previously discussed income and substitution effects (see Section 3.1.1). In the baseline (NoEvasion-treatments), the substitution effect clearly drives the labor supply responses across all tax rates: with each increase in the tax rate the labor supply significantly decreases, since the opportunity costs of either leisure or other tasks outside our experiment become relatively cheaper. The prominence of the substitution effect is also in line with Imbens et al. (2001) who estimated very small income effects, suggesting that the uncompensated elasticity can be approximated with the compensated elasticity of labor supply.

**Result 1 a** Participants very clearly respond to the tax-level increase: while under the

[30] See Berg (2015).

best condition 93% of participants decided to supply labor only 66% of participants decided to do so in the worst condition.

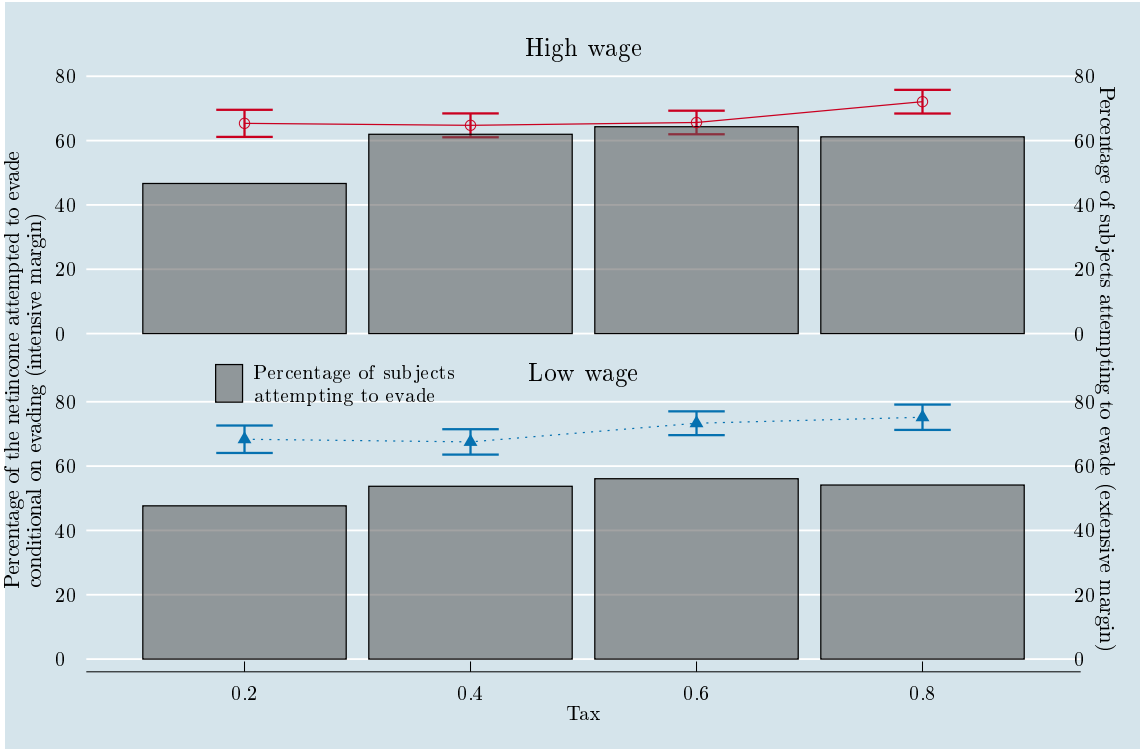
**Result 1 b** Further, subjects supplied significantly more work under high wages compared to low wages.

**Result 1 c** The labor supply decreased significantly under increasing taxes however, this decrease was significantly smaller in the Evasion-treatment.

**Result 1 d** On average participants in the Evasion-treatment worked 37% more compared to the NoEvasion-treatments.

### 3.4.2 Tax Evasion

**Figure 3.4:** Tax Evasion in the Evasion-treatment.




Note: Percentage of the net income attempted to evade conditional on evading as a function of tax with 95% confidence intervals in the Evasion-treatment. Blue, dashed lines represent the percentage of the net income attempted to evade in the low wage situation, while red, solid lines represent the percentage of the net income attempted to evade for the high wage situation all conditional on evading at all. The corresponding tunnels surrounding the respective dots represent the 95% confidence intervals.
The grey bars depict the percentage of subjects attempting to evade at the separate tax levels (i.e. the extensive margin).

Concerning tax evasion we find that most subjects (81%) decided to evade taxes in at least one setting in the Evasion-treatment. On average – aggregated over all tax-levels and the two wages situations – participants tried to evade M = 38.45 (SD = 32.76)% of their income. We can also see that the decision to evade was very similar for the two

wages (average evasion under low wage: M = 37.66 (SD = 36.48), average evasion under high wage: M = 39.23 (SD = 33.36), t(509)= 1.5, p≥0.05).[31]

Figure 3.4 shows the attempted evasion as a function of the tax. We can very clearly see that the more profitable it is to evade the more participants also evade. In particular, subjects tried to evade on average M = 31.53 (SD = 35.99)% under a 20% tax while under a 80% tax subjects tried to evade M = 42.40 (SD = 40.24)% of their income, t(509)= -6.3, p ≤0.001.

Looking at the extensive margin, i.e. the percentage of subjects deciding to evade, we see that under a 20% tax on average 57% of subjects tried to evade. Under a 80% tax on average 66% of subjects tried to evade.

Looking at the intensive margin, i.e. the percentage of the net income attempted to evade conditional on evading, we see that under a 20% tax on average M = 66.95 (SD = 31.41) is evaded by those who evade. Under a 80% tax on average M = 72.54 (SD = 30.89) is evaded by those who evade.

Table 3.3 shows a mixed effects model of the percentage of attempted evasion of participants' income as a function of the tax. Table 3.3 also accounts for the participants elicited risk-preferences and further accounts for several controls. Surprisingly, we can see that risk does not influence the decision to evade taxes. We also see that all results are robust to the inclusion of further controls.

**Table 3.3:** Linear Mixed-Effects Model of Tax Evasion.

| | Tax Evasion | | | | | |
|---|---|---|---|---|---|---|
| | Low-Wage | | | High-Wage | | |
| Constant | 33.28*** (1.79) | 27.56*** (4.00) | 31.16** (9.94) | 32.80*** (1.70) | 24.91*** (3.80) | 38.20*** (9.07) |
| Tax | 0.15*** (0.03) | 0.14* (0.06) | 0.14* (0.06) | 0.21*** (0.03) | 0.23*** (0.06) | 0.23*** (0.06) |
| Risk | | 1.59 (0.99) | 1.65 (1.00) | | 2.19* (0.94) | 2.31* (0.95) |
| Tax x Risk | | 0.003 (0.01) | 0.003 (0.01) | | −0.004 (0.02) | −0.004 (0.02) |
| Controls | × | × | ✓ | × | × | ✓ |
| Sbj specific effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 2,040 | 2,040 | 2,040 | 2,040 | 2,040 | 2,040 |
| Log Likelihood | −10,061.83 | −10,062.60 | −10,049.96 | −10,165.53 | −10,164.84 | −10,149.79 |
| Akaike Inf. Crit. | 20,131.67 | 20,137.20 | 20,129.92 | 20,339.06 | 20,341.67 | 20,329.58 |
| Bayesian Inf. Crit. | 20,154.15 | 20,170.93 | 20,214.23 | 20,361.54 | 20,375.39 | 20,413.89 |

*Notes:* $^{\cdot}p < 0.1; ^{*}p < 0.05; ^{**}p < 0.01; ^{***}p < 0.001;$

Note: Controls include age, gender, ethnicity, income, party affiliation, employment status, education, hours spent on online work, and the average time needed for solving the two sample tasks. The omitted category is the Evasion-treatment – participants have the opportunity to evade taxes and punishment will be meted with a 50% probability. *Tax* denotes a one percentage point increase in the tax level. *Risk* denotes the elicited risk aversion with higher values indicating more risk-lovingness. Errors are clustered on the subject level, i.e., subject-specific effects do account for subject-specific heterogeneity.

Our reported level of evasion in the Evasion-treatment is well in line with estimates in the evasion literature, which finds non-compliance rates in income taxation ranging from 30% to 78% (Fortin et al., 2007; Alm et al., 2009). In detail, we report an average non-compliance rate of 38% aggregated over all tax levels (intensive margin) and an extensive margin with between 57% and 66% of subjects attempting to evade. Figure C9 in the

[31] For a tax of 40 and 80% the evasion was significantly higher under the high wage compared to the low wage.

Appendix further examines on the individual decisions to evade: the vast majority of subjects who decided to evade did so throughout all 8 scenarios (around 30% in the extensive margin) and to a large extend (around 70% of their income in the intensive margin). Furthermore, Figure C10 shows that the nature of evasion decisions (w.r.t. extensive/intensive margin) does not differ across the low or high wage level.

**Result 2 a** Most subjects (81%) decided to evade taxes in at least one setting in the Evasion-treatment.

**Result 2 b** On average participants tried to evade 38.45% of their income.

**Result 2 c** With increasing tax rate the proportion of participants trying to evade increased.

### 3.4.3 Expected Tax Revenue

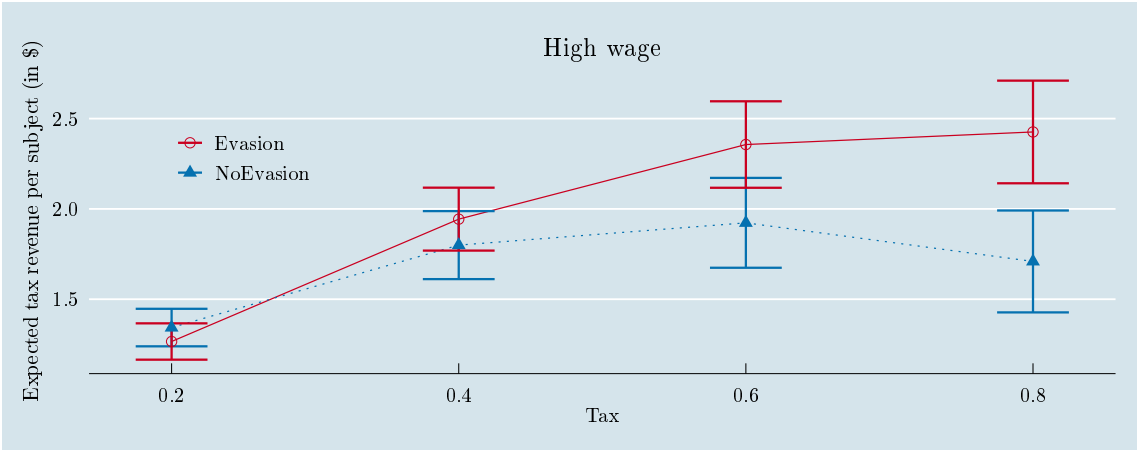
Now we consider the main dependent measure of this study: the expected tax revenue. We have seen that the labor supply is higher if evasion is possible, but we have also seen that participants use the option to evade to a substantial amount. Hence, the main question of this paper is: can the opportunity to evade still overall increase expected tax revenue?

To answer this question we first calculate the expected tax revenue for each individual ($\mathbb{E}(R)_{i,t}$) by using the following equation:

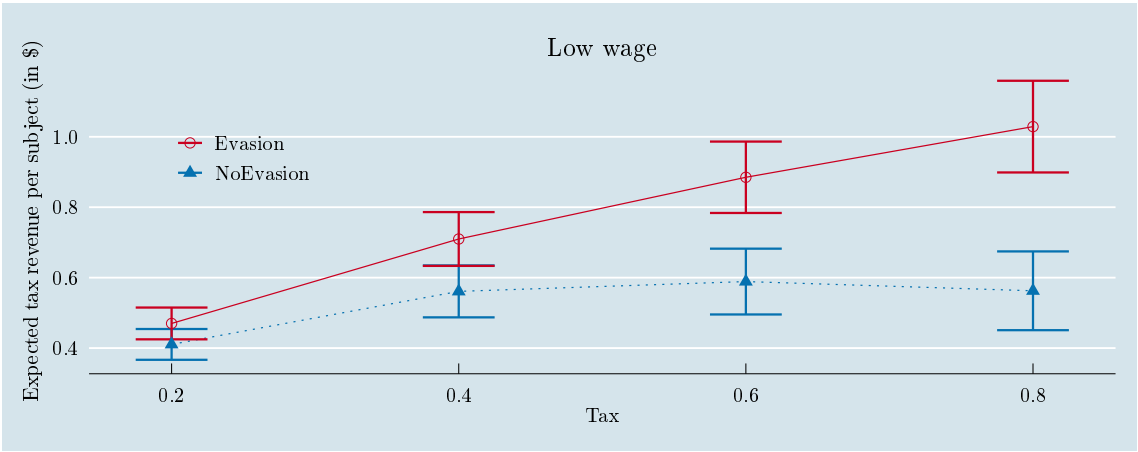
$$\mathbb{E}(R)_{i,t} = h_i \cdot w \cdot t - e_i \cdot (0.5 \cdot t - 0.5 \cdot 0.2) \tag{3.8}$$

where $h$ indicates the labor supplied (i.e. how many tables subject $i$ indicated to work), $w$ is the wage per table, $t$ is the tax level and $e_i$ is the income attempted to be evaded.

**Figure 3.5:** Expected Tax Revenue.




**(a)** Expected tax revenue in the high-wage situation as a function of the tax with 95% confidence intervals split by treatment.




**(b)** Expected tax revenue in the low-wage situation as a function of the tax with 95% confidence intervals split by treatment.

Note: Blue, dashed lines represent the NoEvasion-treatments, while red, solid lines represent the Evasion-treatment. The corresponding tunnels surrounding the respective dots represent the 95% confidence intervals.

First, we have a quick look on the two extreme scenarios: a tax rate of 20% and a tax rate of 80%. In Figure C11 we compare the tax-revenue under these two extreme scenarios. Under the nominal tax rate of 20% both treatments have an effective tax rate of 20% and do not differ in their labor supply which obviously translates into the expected tax revenue which is statistically identical between the two treatments (NoEvasion-treatment: M = 0.88 (SD = 0.77); Evasion-treatment: M = 0.87 (SD = 0.79), t(993.5)= 0.2, p≥0.05).[32] However, the situation is very different under a tax rate of 80%. In this situation participants in the Evasion-treatment evade almost M = 42.40 (SD = 40.24)% of their income while at the same time they increase their labor supply by 106% compared to the

[32]Under low wages the expected tax revenue are the following: NoEvasion-treatment: M = 0.41 (SD = 0.49); Evasion-treatment: M = 0.47 (SD = 0.52), t(993.9)= -1.9, p≥0.05). Under high wages the expected tax revenue are the following: NoEvasion-treatment:M = 1.34 (SD = 1.17); Evasion-treatment: M = 1.27 (SD = 1.16), t(990.8)= 1.0, p≥0.05).

NoEvasion-treatment. Overall this results in a statistically highly significant and substantially higher expected tax revenue. While under the NoEvasion-treatment the expected tax revenue on average was $M = 1.14$ ($SD = 2.11$) it was $M = 1.73$ ($SD = 2.29$) in the Evasion-treatment; a highly significant difference $t(993) = -4.2$, $p \leq 0.001$.[33]

Let us now focus on the expected tax rate in the NoEvasion-treatments under increasing tax levels. Figure 3.5a depicts the expected tax revenue under the NoEvasion-treatments and the Evasion-treatment as a function of the tax rate for high wages. Figure 3.5b depicts the same under low wages. We can see that the expected tax revenue in the NoEvasion-treatments nicely shows the features of the Laffer Curve – the expected tax revenue has an inverted U-shape. In our case this is, an increase from 20% to 40% leads to a higher tax revenue. A further increase in the tax rate to 60% does not change the tax revenue significantly anymore. An even further increase in the tax rate up to 80% leads then even to a decrease in the expected tax revenue due to reduced labor supply. This picture is evident under both wage-levels. For the Evasion-treatment the pictures look quite different: we very clearly see an increase in the expected tax revenue with increasing tax levels. Other than in the NoEvasion-treatments we do not see a decrease in the expected tax revenue for any increase in the tax levels (only under the high wage situation a shift from the 60% tax to the 80% tax seems to keep the expected tax revenue roughly constant).

To investigate this relationship further we use the following mixed effects model of expected tax revenue to estimate the treatment effects:

$$
\begin{aligned}
\mathbb{E}(R)_{i,t} =& \beta_0 + \beta_1 \cdot \mathbb{1}_{\text{Evasion}} + \beta_2 \cdot t + \beta_3 \cdot \mathbb{1}_{\text{Evasion}} \cdot t + \epsilon_i + \epsilon_{i,t,w} + C_M \qquad (3.9)\\
C_1 =& 0\\
C_2 =& C_1 + \beta_4 \cdot Risk_i + \beta_5 \cdot t \cdot Risk_i + \beta_6 \cdot Risk_i \cdot \mathbb{1}_{\text{Evasion}} + \beta_7 \cdot t \cdot \mathbb{1}_{\text{Evasion}} \cdot Risk_i\\
C_3 =& C_2 + \beta_X \cdot X
\end{aligned}
$$

$\mathbb{E}(R)_{i,t}$ represents the expected tax revenue from subjects $i$ for tax $t$ under wage $w$ with $i \in \{1, \ldots, n\}$, $t \in \{.20, .40, .60, .80\}$, and $w \in \{\text{High}, \text{Low}\}$. $\mathbb{1}_{\text{Evasion}}$ denotes a dummy with value one if the participants are in the Evasion-treatment, i.e. participants can evade their income, and zero if the participants are in the NoEvasion-treatments. $t$ denotes the tax-level-effect, with $t \in \{.20, .40, .60, .80\}$. To account for the nested structure of the data we included $\epsilon_i$ as the random effects of the individual $i$. $\epsilon_{i,t,w}$ is the residuals. $Risk_i$ indicates the elicited risk preferences of subject $i$ with higher values indicating more risk-lovingness. $X$ is a vector of further control variables including age, gender, ethnicity, income, party affiliation, employment status, education, and hours spent on online work.

[33] Under low wages the expected tax revenue are the following: NoEvasion-treatment:$M = 0.56$ ($SD = 1.25$); Evasion-treatment: $M = 1.03$ ($SD = 1.50$), $t(978.3) = -5.3$, $p \leq 0.001$). Under high wages the expected tax revenue are the following: NoEvasion-treatment:$M = 1.71$ ($SD = 3.16$); Evasion-treatment: $M = 2.43$ ($SD = 3.27$), $t(993.7) = -3.5$, $p \leq 0.001$).

Table 3.4 reports the estimates of the split regression by wage based on a linear regression with tax as a continuous variable. Table 3.5 reports these estimates with dummies for the respective tax levels. Under a 20% tax the Evasion and the NoEvasion treatments do not differ. With an increasing tax also the expected revenue increases in both treatments, while this is significantly more so in the Evasion-treatment. We can also see that all effects are mirrored under high wage. It is also interesting to see that the linear model predicts no effect of the tax on the expected tax revenue if we control for risk-preferences, which, however, is only true for the NoEvasion treatments. Further, we see that risk has no influence on the expected tax revenue. We can also see that all results are robust to the inclusion of controls.

Overall – aggregated over all tax levels and all wages – the expected tax revenue is significantly higher in the Evasion-treatment ($M = 1.30$ ($SD = 1.46$)) compared to the NoEvasion-treatments ($M = 1.01$ ($SD = 1.32$), $t(991.4) = -3.3$, $p = 0.001$). In fact, the expected tax revenue on average is 52% higher in the Evasion-treatment compared to the NoEvasion-treatments.[34] Thus, the answer to the main question of the paper – i.e. can the opportunity to evade overall increase the expected tax revenue – is: yes!

**Result 3 a** Under 20% tax the average expected tax revenue is indistinguishable between the Evasion-treatment and the NoEvasion-treatments.

**Result 3 b** The expected tax revenue resembles a classical Laffer curve with peak at 60% in our NoEvasion-treatments, i.e. the expected tax revenue has an inverted U-shape relation to the increasing tax level.

**Result 3 c** In the Evasion-treatment the expected tax revenue is significantly less sensitive to the increasing tax levels.

**Result 3 d** Most importantly: the expected tax revenue on average is higher under the Evasion-treatments compared to the NoEvasion-treatments.

**Result 3 e** For a tax of 80% the expected tax revenue is 52% higher in the Evasion-treatment compared to the NoEvasion-treatments.

[34] Under low wages the expected tax revenue on average is 83% higher and under high taxes the expected tax revenue is 42% higher.

**Table 3.4:** Linear Mixed-Effects Model of Expected Rax Revenue.

| | Expected Tax revenue per subject (in $) | | | | | |
|---|---|---|---|---|---|---|
| | Low-Wage | | | High-Wage | | |
| Constant | 0.46*** (0.04) | 0.45*** (0.09) | 0.60*** (0.18) | 1.51*** (0.10) | 1.19*** (0.22) | 1.96*** (0.42) |
| Evasion | 0.04 (0.06) | 0.04 (0.13) | 0.02 (0.13) | −0.10 (0.15) | 0.08 (0.32) | 0.06 (0.32) |
| Tax | 0.002*** (0.001) | −0.0004 (0.001) | −0.0004 (0.001) | 0.01*** (0.001) | 0.003 (0.003) | 0.003 (0.003) |
| Tax x Evasion | 0.01*** (0.001) | 0.01*** (0.002) | 0.01*** (0.002) | 0.01*** (0.002) | 0.02*** (0.004) | 0.02*** (0.004) |
| Risk | | 0.003 (0.03) | −0.0003 (0.03) | | 0.10 (0.06) | 0.09 (0.06) |
| Tax x Risk | | 0.001** (0.0003) | 0.001** (0.0003) | | 0.001 (0.001) | 0.001 (0.001) |
| Risk x Evasion | | 0.0001 (0.03) | 0.01 (0.03) | | −0.06 (0.08) | −0.05 (0.08) |
| Tax x Risk x Evasion | | −0.001 (0.0005) | −0.001 (0.0005) | | −0.001 (0.001) | −0.001 (0.001) |
| Controls | × | × | ✓ | × | × | ✓ |
| Sbj specific effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 3,984 | 3,984 | 3,984 | 3,984 | 3,984 | 3,984 |
| Log Likelihood | −4,560.18 | −4,575.31 | −4,594.17 | −8,162.31 | −8,175.30 | −8,185.78 |
| Akaike Inf. Crit. | 9,132.36 | 9,170.62 | 9,226.34 | 16,336.61 | 16,370.60 | 16,409.56 |
| Bayesian Inf. Crit. | 9,170.10 | 9,233.52 | 9,345.85 | 16,374.35 | 16,433.50 | 16,529.07 |

*Notes:* $^{\cdot}p < 0.1; ^{*}p < 0.05; ^{**}p < 0.01; ^{***}p < 0.001;$

Note: Controls include age, gender, ethnicity, income, party affiliation, employment status, education, hours spent on online work, and the average time needed for solving the two sample tasks.Evasion denotes a dummy with value one if the participant was in the Evasion-treatment – participants have the opportunity to evade taxes and punishment will be meted with a 50% probability – and zero otherwise. The omitted category is the NoEvasion-treatment. *Tax* denotes a one percentage point increase in the tax level. *Risk* denotes the elicited risk aversion with higher values indicating more risk-lovingness. Errors are clustered on the subject level, i.e., subject-specific effects do account for subject-specific heterogeneity.

**Table 3.5:** Linear Mixed-Effects Model of Expected Tax Revenue.

| | Expected Tax revenue per subject (in $) | | | | | |
|---|---|---|---|---|---|---|
| | Low-Wage | | | High-Wage | | |
| Constant (20% Tax) | 0.41*** (0.05) | 0.39*** (0.10) | 0.54** (0.18) | 1.34*** (0.11) | 1.05*** (0.24) | 1.81*** (0.43) |
| 40% Tax | 0.15*** (0.04) | 0.13 (0.08) | 0.13 (0.08) | 0.46*** (0.09) | 0.33· (0.20) | 0.33· (0.20) |
| 60% Tax | 0.18*** (0.04) | 0.07 (0.08) | 0.07 (0.08) | 0.58*** (0.09) | 0.48* (0.20) | 0.48* (0.20) |
| 80% Tax | 0.15*** (0.04) | −0.01 (0.08) | −0.01 (0.08) | 0.37*** (0.09) | 0.18 (0.20) | 0.18 (0.20) |
| Evasion | 0.06 (0.06) | 0.06 (0.14) | 0.04 (0.14) | −0.08 (0.15) | 0.06 (0.34) | 0.04 (0.34) |
| 40% Tax x Evasion | 0.09· (0.05) | 0.11 (0.11) | 0.11 (0.11) | 0.22· (0.13) | 0.36 (0.28) | 0.36 (0.28) |
| 60% Tax x Evasion | 0.24*** (0.05) | 0.32** (0.11) | 0.32** (0.11) | 0.51*** (0.13) | 0.61* (0.28) | 0.61* (0.28) |
| 80% Tax x Evasion | 0.41*** (0.05) | 0.49*** (0.11) | 0.49*** (0.11) | 0.79*** (0.13) | 0.92*** (0.28) | 0.92*** (0.28) |
| Risk | | 0.01 (0.03) | 0.003 (0.03) | | 0.09 (0.06) | 0.08 (0.06) |
| 40% Tax x Risk | | 0.01 (0.02) | 0.01 (0.02) | | 0.04 (0.05) | 0.04 (0.05) |
| 60% Tax x Risk | | 0.03 (0.02) | 0.03 (0.02) | | 0.03 (0.05) | 0.03 (0.05) |
| 80% Tax x Risk | | 0.05* (0.02) | 0.05* (0.02) | | 0.06 (0.05) | 0.06 (0.05) |
| Evasion x Risk | | −0.0003 (0.04) | 0.005 (0.04) | | −0.05 (0.09) | −0.04 (0.09) |
| 40% Tax x Evasion x Risk | | −0.01 (0.03) | −0.01 (0.03) | | −0.04 (0.07) | −0.04 (0.07) |
| 60% Tax x Evasion x Risk | | −0.03 (0.03) | −0.03 (0.03) | | −0.03 (0.07) | −0.03 (0.07) |
| 80% Tax x Evasion x Risk | | −0.03 (0.03) | −0.03 (0.03) | | −0.04 (0.07) | −0.04 (0.07) |
| Controls | × | × | ✓ | × | × | ✓ |
| Sbj specific effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 3,984 | 3,984 | 3,984 | 3,984 | 3,984 | 3,984 |
| Log Likelihood | −4,554.29 | −4,573.44 | −4,592.30 | −8,135.57 | −8,149.04 | −8,159.52 |
| Akaike Inf. Crit. | 9,128.59 | 9,182.89 | 9,238.61 | 16,291.15 | 16,334.09 | 16,373.04 |
| Bayesian Inf. Crit. | 9,191.49 | 9,296.11 | 9,408.44 | 16,354.05 | 16,447.31 | 16,542.87 |

*Notes:* $^{\cdot}p < 0.1; ^{*}p < 0.05; ^{**}p < 0.01; ^{***}p < 0.001;$

Note: Controls include age, gender, ethnicity, income, party affiliation, employment status, education, hours spent on online work, and the average time needed for solving the two sample tasks.Evasion denotes a dummy with value one if the participant was in the Evasion-treatment – participants have the opportunity to evade taxes and punishment will be met with a 50% probability – and zero otherwise. The omitted category is the NoEvasion-treatment. *Tax* denotes the respective tax level, i.e. 20, 40, 60 or 80%. *Risk* denotes the elicited risk aversion with higher values indicating more risk-lovingness. Errors are clustered on the subject level, i.e., subject-specific effects do account for subject-specific heterogeneity.

### 3.4.4 Further Mechanisms

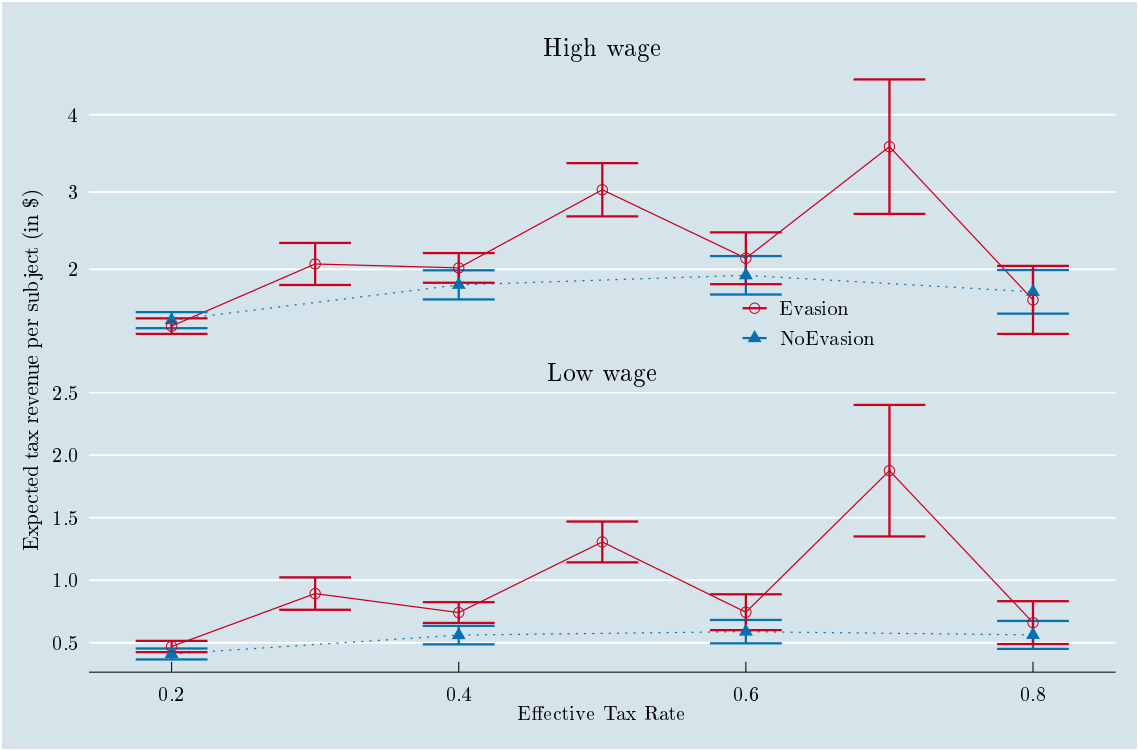
#### (Enforced) Statutory Tax Rates vs. (Equivalent) Effective Tax Rates

Most crucial for the interpretation of our results is the comparison of tax revenues in terms of effective tax rates. One could very well argue that the opportunity to evade is nothing but a factual decrease in effective tax rates (taking into account fines and audit probabilities). According to the standard framework by Mirrlees (1971), positive labor supply responses therefore come as no surprise. Importantly, the mechanism proposed by Weiss (1976) distinctively points out a revenue increasing effect of tax evasion *beyond* that would be achieved by an equivalent decrease in statutory tax rates.

To examine this effect, we first compare the statutory tax rates (i.e. 20%, 40%, 60%, 80%) with the average self-chosen effective tax rates of each respective level (resulting from the actual evasion decisions). To do so, we calculate for every subject their personal effective tax level (i.e. expected tax payment divided by the net income). For a statutory tax level of 20% the average effective tax rate is 20% – this is achieved by construction as the penalty is 20%. For a statutory tax rate of 40% the average effective tax rate is 36% while for a nominal tax rate of 60% and 80% the average effective tax rate is 50% and 64% respectively. These effective tax levels are virtually identical between the two wage levels (i.e. all effective tax levels are statistically indistinguishable between the two wage situations).

But how do these equivalent tax rates translate in total tax revenues? In Figure 3.6 we illustrate the expected tax revenue in terms of the effective tax rate in comparison to the equivalent statutory tax rates of the NoEvasion-treatments. For every effective tax rate above 20%, it is clearly visible how the expected tax revenue is also higher than its statutory counterpart in the NoEvasion-treatments. Individuals who evade at a given tax rate are compiled into bins of 10% - thus, the "spikes" are those who self-selected into tax evasion, whereas the data points of the Evasion-treatment congruent with the statutory tax rates are driven by the participants who did not evade (the amount of participants who fully evade in a given tax rate and thus would fall into the lower statutory rate is neglectable). Even further, comparing the respective higher effective rate to the lower statutory rate (e.g. 20% of the NoEvasion-treatments vs. 30% of the Evasion-treatment), tax revenues still persist to be higher. This result is especially remarkable against the background of our treatment calibration: By design, every investment into the lottery *lowered* the expected tax revenue. Nonetheless, the positive labor supply responses are so strong, they crowd-out the losses of tax evasion if the tax base (i.e. labor supply) would have been static - even beyond the revenue equivalent. The results very clearly show: no matter by how much the statutory tax rate is reduced, the tax revenue of a 60% tax with the opportunity to evade is never achieved.

**Figure 3.6:** Expected Tax Revenue as a Function of the Effective Tax Rate.



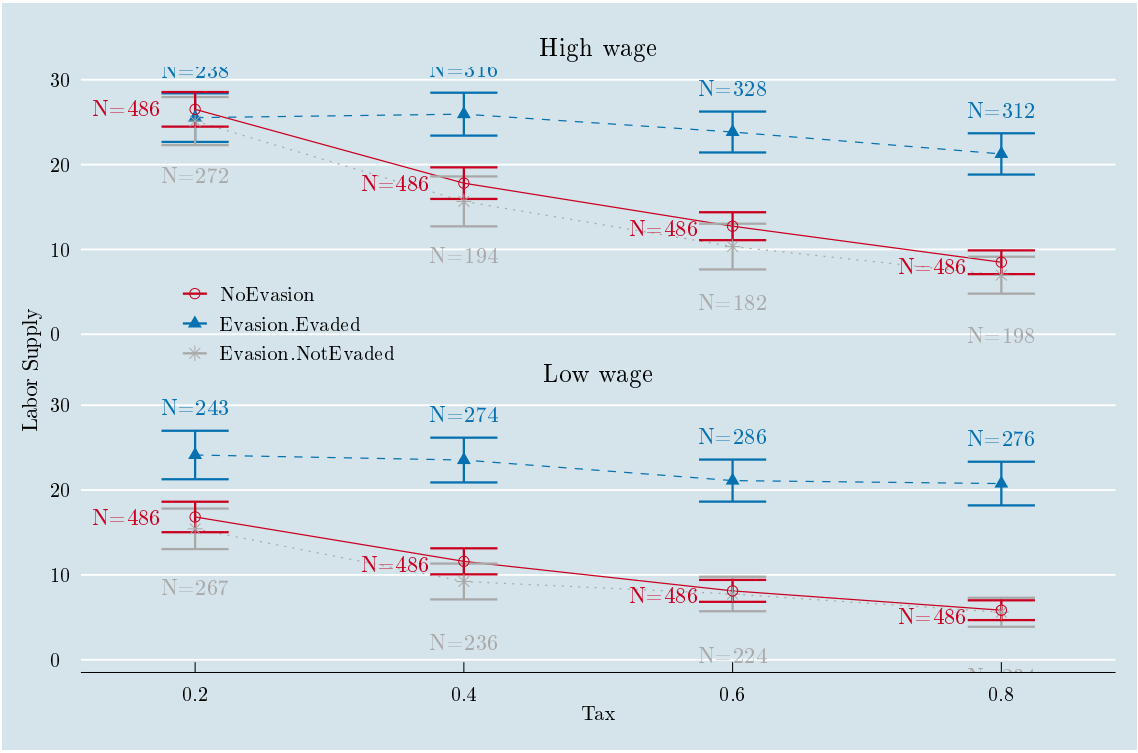

Note: Expected tax revenue as a function of the effective tax rate (exp. tax divided by the net income) with 95% confidence intervals split by treatment. Blue, dashed lines represent the NoEvasion-treatments, while red, solid lines represent the Evasion-treatment. The upper panel depicts the high wage situation while the panel at the bottom depicts the low wage situation. The corresponding tunnels surrounding the respective dots represent the 95% confidence intervals.

### Self-Selection into Evasion in the Evasion-treatment (ATE vs. ITT)

In order to understand the composition of the labor supply responses of our Evasion-treatment group it is important to account for the possibility of self-selecting out of evasion. Our participants in the Evasion-treatment were able to discretely pick any level of evasion between up to 100% of gross income – but also 0%. Therefore, our Evasion-treatment group also comprises individuals who deliberately did not evade. In turn, the average-treatment effect (ATE) constituted by the individuals who actually evaded in the Evasion-treatments potentially differs from the presented aggregated intention-to-treat (ITT) effect. To examine this difference, we compare the labor supply of those subjects in the Evasion-treatment who did not evade at a given tax-rate and those who evaded against the subjects who were not able to evade (NoEvasion- treatments). Figure 3.7 depicts for both wage-levels the labor supply as a function of the tax for these three situations. For each tax rate, about 40% of participants did not evade despite having the opportunity in the Evasion-treatment. This picture is largely driven by a subgroup of participants who actually never evaded throughout all of the different tax rates (ca. 22% of participants in the high wage and ca. 33% of participants in the low wage; see Figure C13). We clearly see that subjects who did not evade differ strongly from those who evaded. The deliberately

non-evading subjects demonstrate a substantially lower labor supply, indistinguishable to those who could not evade (across all tax levels). Reversely, the labor supply of those who did evade was substantially higher compared to those who could not or did not want to evade.

**Figure 3.7:** Labor Supply by Evasion Decision (Extensive Margin).




Note: Labor supply of subjects by treatement and extensive margin (i.e. if a subject decided to evade within each respective tax level). Grey, dotted lines represent the Evasion-treatment where subjects did not evade; red, solid lines represent the NoEvasion-treatments; blue, dashed lines represent the Evasion-treatment where subjects evaded. The corresponding tunnels surrounding the respective dots represent the 95% confidence intervals.

This analysis reveals how our treatment effect is driven by the subgroup of evaders. This yields the following interesting insight: it seems not to be the mere existence of the evasion opportunity driving the effect - but rather the actual evasion decision itself.

### The Role of Other Determinants

To further explore and understand the potential determinants of who self-selected into tax evasion and who drove the large labor supply responses, we ran a couple of further analysis on personality traits in Section C.3.4 of the Appendix. We do not find any systematic difference for ability[35] (see Figure C17) or other socio-economic traits like household income (see Figure C21), importance of online work (see Figure C20) or gender (see Figure C22) on the decision to evade and labor supply. Our risk measure (similar to Eckel and Grossman, 2002), on the other hand, reveals that participants who are less risk

[35] Proxied by the average time needed to solve the two trial tables.

averse above the median invest 4% - 10% more into the lottery (see Figure C14). However, this does not translate into higher labor supply and tax revenues.

## 3.5 Discussion & Conclusion

The findings of this project may challenge a long-standing assumption that tax evasion leads to a reduced overall tax revenue. With novel empirical insights, this project provides a more nuanced view on the effects of tax evasion on the overall tax revenue (and thus, part of the social costs of tax evasion). This project depicts, to our knowledge, the first empirical investigation of the direct relationship between the opportunity to evade and overall tax revenues. Building upon a thus far exclusively theoretical debate on potential welfare increasing effects of tax evasion initiated by Weiss (1976), we shed an emprirical light on the inter-relatedness of labor supply and evasion incentives - a mechanism which the theoretical debate deemed as empirically rather unlikely.

Given the near impossibility of cleanly exploring such a question with observational data, we exploit the advantages of a highly controlled experimental approach. Specifically, we implemented an original real effort experiment in a real (online) labor market. Our treatment group had to take a joint decision on labor supply and level of tax evasion, whereas our control group was only able to decide upon the preferred labor supply, without the opportunity to evade.

Our findings not only show significant positive labor supply responses to the opportunity to evade (increased labor supply by on average 37%). Also the expected tax revenue significantly and substantially increased up to more than 50%. Strikingly, this effect persists when comparing effective tax rates: Lowering effective tax rates through the opportunity to evade is more efficient than simply lowering statutory tax rates, which is valid throughout all statutory tax rates above 20%. The effect is entirely driven by the share of participants who actually evaded in the Evasion-treatment.

This empirical finding, which we perceive as the key contribution of this paper, hinges on a fundamental mechanism that should be further discussed: the differential perception of tax evasion opportunities in contrast to a simple increase of the net wage rate. In particular, the evasion decision is associated with risk and therefore perceived as more costly than an explicit decrease in tax rates, which, of course, is costless for the individual. Our findings unequivocally show that these tax reductions are indeed *not* perceived equivalent. Yet, our results are not able to uncover the specific reasons for this differential perception. With ambiguous theoretical predictions, the underlying channels remain somewhat in a black box. Whereas the theoretical literature perfectly described the conditions for our results to occur (Weiss, 1976; Stiglitz, 1982), it nonetheless deemed the empirical relevance as rather unlikely (Hellwig, 2007). In this respect, there may be more to this theoretical intuition than previously captured.[36] Within the expected utility framework, promising

[36] I.e: The previous debate already defined the property of increasing risk aversion as a necessary condition.

extensions might be to link the problem to the literature on the optimal response of decision variables to risk. Specifically, this is: precautionary savings theory (e.g. Kimball, 1990), prudence and higher-order risk aversion (e.g. Deck and Schlesinger, 2010; Noussair et al., 2014; Ebert and Wiesen, 2014). Along these lines, evasion yields a higher risk of a negative income shock. Prudent subjects may want to insure against the risk by increasing effort. These mechanisms might further interact with behavioral effects outside the expected utility framework: Loss aversion (Kahneman and Tversky, 1979) and reference dependent preferences (Kőszegi and Rabin, 2006). The regret of being detected in light of the outcome of not being detected may trigger additional incentives to work hard. Finally, the mere perception of probabilities might be biased (Tversky and Kahneman, 1974). The original empirical insights from our experiment serves as an informative starting point to provide a new conceptual contribution, which is, however, beyond the scope of our paper.

Our research design, as a standard principle of lab experiments, highly abstracts from many real life aspects. Since our research question is rooted in a theoretical debate, this abstraction depicts an advantage to less controlled settings with a high number of confounds. On the other hand, this offers room for potential critique in terms of external validity. Against this background, three main limitations need to be discussed: First, the choice of parameters concerning audit probability, penalty and tax rates. Especially our audit probability of 50% is most likely higher than in reality. However, the main objective of our study was to maximize internal validity. That is, to reduce the cognitive load in order to elicit unbiased preferences and construct a true baseline in which participants are indifferent (i.e. 20% tax with 50% audit probability). To facilitate full comprehension we therefore aimed to keep a fairly complex joint decision on labor supply and evasion as simple as possible. Moreover, actual audit probabilities are rather unknown to the public and, also, highly dependent on the income level as well as profession.[37] Finally, even if commonly known, the perception of these audit probabilities is likely to be biased itself (c.f. Tversky and Kahneman, 1974). Second, we deliberately refrained from employing any type of loaded framing. That includes no moral costs of lying, no social costs of lost tax revenues (i.e. no redistribution) and no administrative costs of evasion (i.e. effort to exploit loopholes). In short, we excluded all externalities of tax evasion. Admittedly, this constitutes a strong assumption, as it is well proven that individuals do not respond to sactions as mere profit-maximizers, and even less so in social settings (Engel, 2014). Nonetheless, we argue this being a necessity in order to identify the "core" mechanism at hand. Biases like inequality aversion or guilt aversion would introduce noise to our findings, which would have prevented to cleanly isolate the mechanism proposed by the theory. Moreover, even though the use of frames usually increases compliance (Alm and Malézieux, 2020), this effect is not necessarily given and could depend on the interaction with other determinants (Alm et al., 1992). Furthermore, even if externalities are introduced, the direction of their effect on tax revenues would not be straightforward: Individuals might adjust on the margin of tax evasion. However, they may also adjust on the margin of

[37] See https://www.irs.gov/about-irs/irs-audit-rates-significantly-increase-as-income-rises (08.07.21)

increased labor supply. Third, lab experiments are typically criticized for the use of small stakes. For our case, stakes were actually as close to reality as they can be: since our experiment was posted on the online labor market MTurk our participants are very likely to be part of "gig economy" workers who use this platform as a main source of income. With an average of 38 minutes, these workers faced real opportunity costs as they could have engaged in other income-generating tasks on this platform. If they lost the lottery, they could have worked for close to nothing; if they won the lottery, our payment was above average.[38] Also, our real effort task of counting zeros yields very little scope for intrinsic motivation. Thus, we do not expect our participants to participate for different than monetary reasons. Additionally, Abeler et al. (2019) find that the amount of lying in lying games is not correlated with the size of the stakes in these games. Finally, our estimated elasticities of taxable income match those found in observational data (Chetty, 2009). Overall, we acknowledge the abstract character of our research design and the potentially resulting limitations in external validity. However, it is deliberately kept very abstract and is reduced to the critical components of the research question. A future research agenda should explore the external validity of these findings – putting it to the test of different contexts and exploring its fundamental character from multiple angles.

Our quite straightforward confirmation of a surprisingly under-studied question constitutes a potentially fundamental contribution to the notion on welfare implications of tax evasion, i.e. the excess burden or dead-weight loss of taxation. A couple of sparse works already pointed towards possible welfare increasing effects of allowing some degree of tax evasion: Chetty (2009) argues that tax evasion is not simply lost resources to society. It rather translates into transfers to others like charitable giving and thus still yields welfare increasing effects. Taking such social benefits of sheltering into account, Keen and Slemrod (2017) discuss the optimal degree of tax enforcement against the background of costly enforcement. However, our argument is somewhat more involved: even in the absence of tax enforcement costs and social benefits of tax sheltering (like charitable giving or paid penalties), the interaction between the evasion opportunity and labor supply decision adds a revenue increase beyond what would be achieved by an equivalent tax rate decrease. Simply put, the excess burden of a given tax system encouraging tax evasion is smaller than that of a system discouraging it.[39]

Tax evasion has many negative consequences for society: increased unfair distribution of wealth, unequal treatment of people, undermining the rule of law. However, one of the main problems typically associated with tax evasion, i.e. reduced tax revenues, might not be as obvious as typically thought. In answering the last question, this research agenda speaks only indirectly to the moral question of tax evasion. It is beyond the scope of this project to answer the moral and philosophical questions arising if tax revenue could be

[38] See Appendix C.2.1 for further analyses.

[39] However, this might only be the case in a situation where tax rate is suboptimal and not an efficient one, as Yitzhaki (1987, p. 134) importantly points out.

increased by the opportunity to evade taxes.[40] Future research will need to tackle the pressing moral and social problems associated with such a mechanism.

Nonetheless, we propose a couple of policy implications suggested by our results: Given that governments cannot (for financial and/or moral reasons) deter all tax evasion, the question is how to most efficiently deploy governmental resources. Our findings suggest several recommendations. First, our effect is relatively largest for low-income workers. Thus, targeting high-income individuals is more beneficial. Second, tax enforcement is less disturbing in labor markets with low labor supply elasticities. Through this lens, already incomplete enforcement for self-employed should focus on respective sectors whereas strict third-party reporting should be relaxed where positive labor supply responses are to be expected. Against this background, our work thus also contributes to the literature on optimal tax administration.

[40] See Sandmo (1981) for an interesting discussion on the desirability of a purely utility maximizing taxpayer or a "Kantian" driven tax payer as well as the implication for horizontal equity.

# Appendices

# Appendix A

## A.1 Selection and Comparability of Tax Instruments

We consider several wealth taxes, which tax the *entirety of all assets*[1] one owns and which are *formally comparable*, but differ in their tax design. Fortunately, different instruments of wealth taxation already exist and thus can be used as a basis for our study. Besides the taxation of wealth transfers at the end of one's life, concepts of periodic (net worth[2]) wealth taxes have been discussed and implemented in other countries.[3]

Despite the differences in their implementation, the formal comparability of concentrated and periodic wealth tax instruments is straightforward to demonstrate. Concentrated and periodic tax payments can be compared by using measures such as the future value. In the absence of any taxes, the future value of an initial asset stock $I_0$ that grows for $n$ years (e.g. until one's death) by rate $r$ can be easily calculated by:

$$FV_{n,\,no\,tax} = I_0 \times (1+r)^n$$

Considering a wealth tax with tax rate $t_e$ and tax exemption $e_e$ that is levied once at the end of one's lifetime, e.g. an estate tax, the formula has to be modified:

$$FV_{n,\,e} = I_0 \times (1+r)^n \times (1-t_e) + min[I_0 \times (1+r)^n,\, e_e] \times t_e$$

The wealth accumulated over $n$ years is simply reduced by the estate tax, which is levied on assets exceeding the exemption.

To illustrate the mechanism of periodic wealth taxes, we look at a yearly wealth tax. Given a tax rate $t_y$ and a tax exemption $e_y$ we have to take into account that the tax reduces the asset stock at the end of every year,[4] i.e.:

$$FV_{1,\,y} = I_0 \times (1+r) - max[I_0 \times (1+r) - e_y,\, 0] \times t_y$$
$$FV_{2,\,y} = FV_{1,\,y} \times (1+r) - max[FV_{1,\,y} \times (1+r) - e_y,\, 0] \times t_y$$
$$\dots$$
$$FV_{n,\,y} = FV_{n-1,\,y} \times (1+r) - max[FV_{n-1,\,y} \times (1+r) - e_y,\, 0] \times t_y$$

If the asset stock never falls below the tax exempted value, we can transform this to:

$$FV_{n,\,y} = I_0 \times [(1+r) \times (1-t_y)]^n + e_y \times t_y \times \frac{1-[(1+r) \times (1-t_y)]^n}{1-[(1+r) \times (1-t_y)]}$$

[1] I.e., we do not consider taxes such as the property tax, which is only levied on some categories of assets.

[2] The frequently used term "net" just clarifies something common to all wealth taxes: Only the net wealth (assets after the deduction of liabilities) is subject to these taxes.

[3] Countries levying periodic (net) wealth taxes are e.g. Japan (only on real estate and business assets), Switzerland, Norway and Spain (Drometer et al., 2018). See Piketty and Saez (2013), Seim (2017), Bird (1991) and Kopczuk (2013) for further discussions.

[4] Of course this could also be any other date of every year.

The effective tax rate $\mathtt{etr}$ of any tax can always be calculated based on future values before and after the respective tax:

$$\mathtt{etr} = 1 - \frac{\mathsf{FV}_{with\,tax}}{\mathsf{FV}_{without\,tax}}$$

Two taxes leading to the same future values can be seen as formal equivalent as the burden of taxation is equal. Furthermore, based on future values or effective tax rates, wealth taxes can be easily compared quantitatively.

We describe the wealth accumulating processes in our tasks to be fully exogenous as we specify the source of assets as "win in the lottery and lucky investments", "received inheritance and gifts from family members" or "saved salaries from employment". Furthermore, we only specify the wealth of assets at the end of the life of a person to not trigger thoughts about any endogenous growth in general. We prefer this approach over an explicit note on endogenous and exogenous growth as it keeps notes on assumptions lean and minimizes potential confusion. Referring to the calculations above, exogenous growth in case of periodic taxes can be expressed by simply specifying $\mathtt{r}$: E.g., given an exogenous saving amount of \$$\mathsf{Z}$ p.a., we set $\mathtt{r} = \mathsf{Z} \times \frac{1}{\mathsf{FV}_{\mathtt{t}-1,\mathtt{y}}}$ in every period $\mathtt{t}$.

## A.2 Discussion of Differences Between Tax Instruments

A number of empirical works find indications for behavioral responses towards the taxation of intergenerational wealth: Estimated elasticities of the reported estate tax base with respect to the net-of-tax rate range from 0.1 to 0.2.[5] By contrast, the effect of recurrent wealth taxation on taxable wealth is estimated to have an elasticity between 0.1 and 0.85.[6] Nevertheless, it is important to bear in mind that these studies are highly sensitive to specific institutional settings and methodological approaches and are thus hardly comparable. An empirical claim as to which tax is more prone to these responses can therefore not be made. Here, we take a closer look at specific behavioral channels and how they might affect preferences towards the proposed tax instruments.[7] These are saving, consumption and wealth accumulation as well as different forms of tax avoidance (like mobility, reporting and timing).

Beyond the formal equivalence of tax instruments, economic behavior can depend on the tax design as the utility might differ due to certain time preferences for consumption. In case of differences between debit and credit interests or credit limitations, periodic taxes increase the price of consumption or reduce the consumption opportunities in early periods. However, as we a) only consider wealthy individuals who build up assets in the course of their lives, b) do not mention any consumption in our vignettes and c) always present an already completed wealth accumulating process, this should only play a minor role in the perception of our cases. One additional issue that may occur and is hard to control for is the scenario that individuals gain utility solely on the grounds of their wealth accumulating process, e.g. they enjoy their current account balance.

In addition to the differences in preferences due to the wide variety of assumptions with regard to lifetime consumption, tax-specific planning opportunities might play a role in the stated preferences. These comprise numerous channels of which mobility, (under)reporting and intertemporal shifting of the tax base are among the most prominent. While estimating tax base elasticities, the empirical literature remains unclear about the prominence of specific channels. Adam et al. (2011) argue in favor of taxing wealth during the entire course of one's life to prevent a long-time horizon that would enable the richest to plan tax avoidance. When large amounts of money are accumulated by the end of one's life, investments in tax avoidance become more attractive.[8] Moreover,

[5] See Kopczuk and Slemrod (2006); Holtz-Eakin and Marples (2001); Joulfaian (2006); Glogowsky (2016).

[6] See Brülhart et al. (2017); Seim (2017); Zoutman (2015).

[7] See Kopczuk (2013) for a more detailed discussion on the differences of the taxation of transfers (i.e. bequests) and the (net-)wealth.

[8] Kopczuk (2013) notes that estate tax planning might be driven by expected tax rates rather than the actual tax scheme.

current estate and inheritance tax schemes provide timing opportunities in the (partial) exemption of lifetime gifts. Even the timing of death seems to provide room for tax planning (Kopczuk and Slemrod, 2003). These aspects suggest a potential bias in preferences for the annual wealth tax. However, we address such efficiency concerns by communicating behavioral assumptions as described in Chapter 1.2.1. Although we are not able to fully cancel out this bias, we at least find similar results between the full sample and the sample restricted to those who understood our assumptions correctly.

An opposing bias could be induced by the potentially high administrative costs of periodic taxes. The assessment of taxes in general is time-consuming and associated with direct and indirect costs. In case of wealth taxes, an additional challenge is the valuation of assets. Especially for real estate and businesses, a proper valuation is anything but straightforward and thus potentially very costly for both taxpayers and the fiscal authorities.[9] This issue may be even more severe for highly diversified asset portfolios. It is obvious that costs of general assessments and valuations increase with the frequency of taxation periods.

Finally, one important difference between (periodic) wealth taxes and the estate tax is the (mis) perceived transfer tax related character of the latter: Whereas a recurrent wealth tax presumably only limits one's own consumption (at short sight), the estate tax likely affects two (or more) related parties, which potentially involves externalities. Cremer and Pestieau (2006) show how the optimal tax structure crucially depends on the underlying bequest motive. With a high proportion of accidental motives, the optimal tax should be higher. On the contrary, altruistic and strategic motives are more prone to distortive effects regarding economic behavior. This potentially induces not only a preference bias towards periodic wealth taxes, but also towards the one-time wealth tax, which does not imply a transfer due to its name.[10] We believe that our research design takes this into account: The existence of children was stressed in the (periodic) wealth tax treatments just as the non-existence of children was stressed in some vignettes of the estate tax.[11]

[9] See Hey et al. (2012, Part B, Chapter IV).

[10] Regarding the important comparison of the one-time wealth tax and the estate tax, one could argue that the name of the estate tax actually triggers some perception of a transfer payment. However, in both questions we asked about the tax at "the age of 80", which was described as the end of one's life in the vignette text.

[11] Of course, (periodic) wealth taxes also burden inter-vivos giving.

## A.3 Experimental Design (Screenshots)

**Figure A1:** Screenshot: Explanations and Control Questions (Estate Tax Group).

The following questions are about **your personal opinion** on how much hypothetical people should pay in taxes on their wealth.

In order to finance public spending, taxes are levied. For this, the government employs different types of taxes, e.g. on income, consumption, or assets. If the government aims to collect a certain tax revenue, every increase in one tax should decrease other taxes by the same amount.

This study is about an **estate tax**. An estate tax once taxes the entire assets, which the decedent owns at the end of his or her life. The taxed estate may include cash, account balances, real estate, and shares. Low asset values might be exempted.

***Important:*** *In this study we assume that individuals' behavior is not affected by the existence of taxes. In particular, the estate tax will not affect economic activity, savings behavior, or lead to tax avoidance/evasion. Furthermore, no other capital taxes are levied.*

**Control Question:**

Which of the following assumptions should be kept in mind when participating in this study? For this, multiple answers are allowed.

- The existence and the amount of taxes does not affect economic activity and saving behavior.
- The existence and the amount of taxes does not affect the level of tax avoidance and evasion.
- The estate tax AND other capital taxes are levied.

**Figure A2:** Screenshot: Explanations and Control Questions (Yearly Wealth Tax Group).

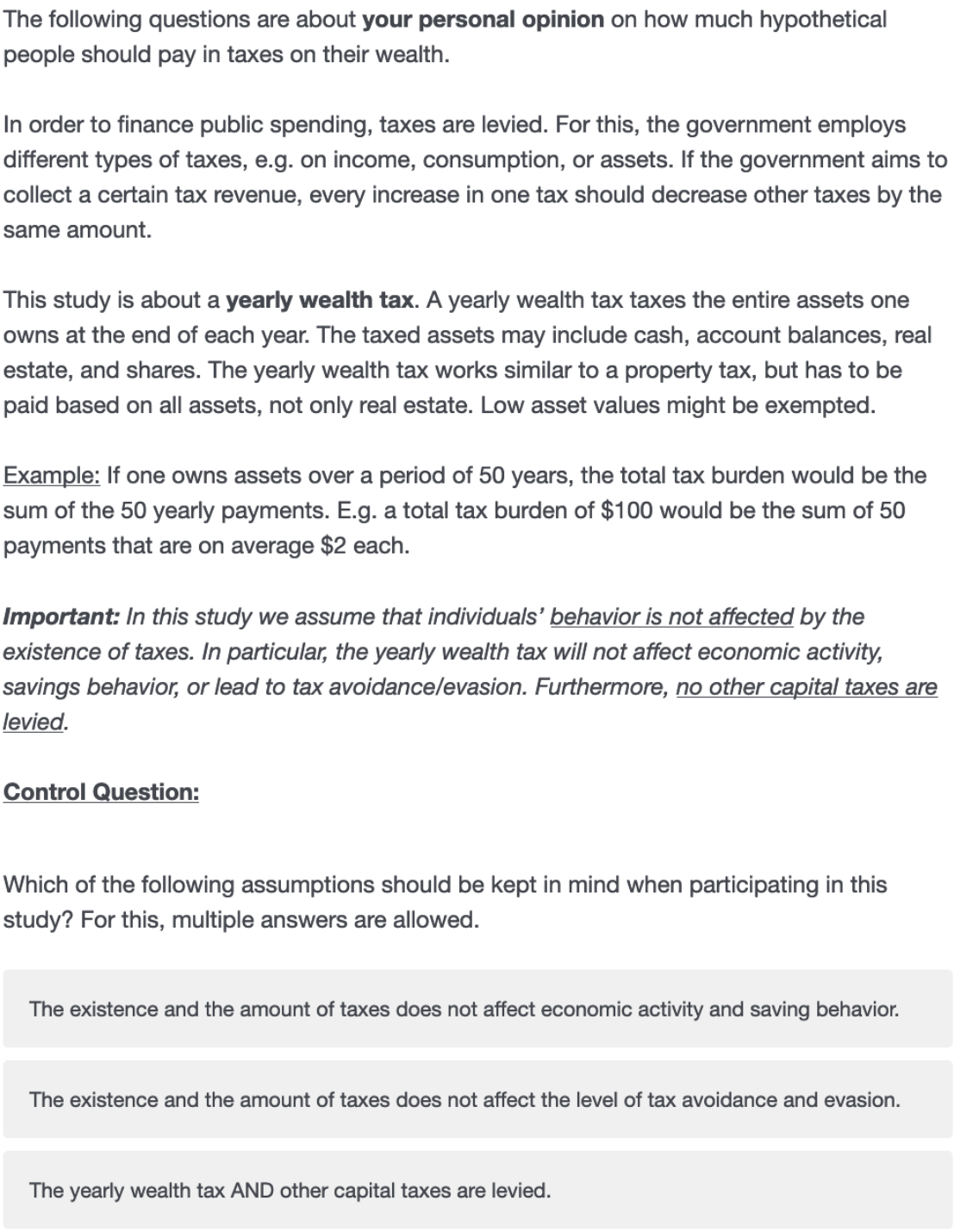


**Figure A3:** Example of a Single Vignette (Estate Tax Group).

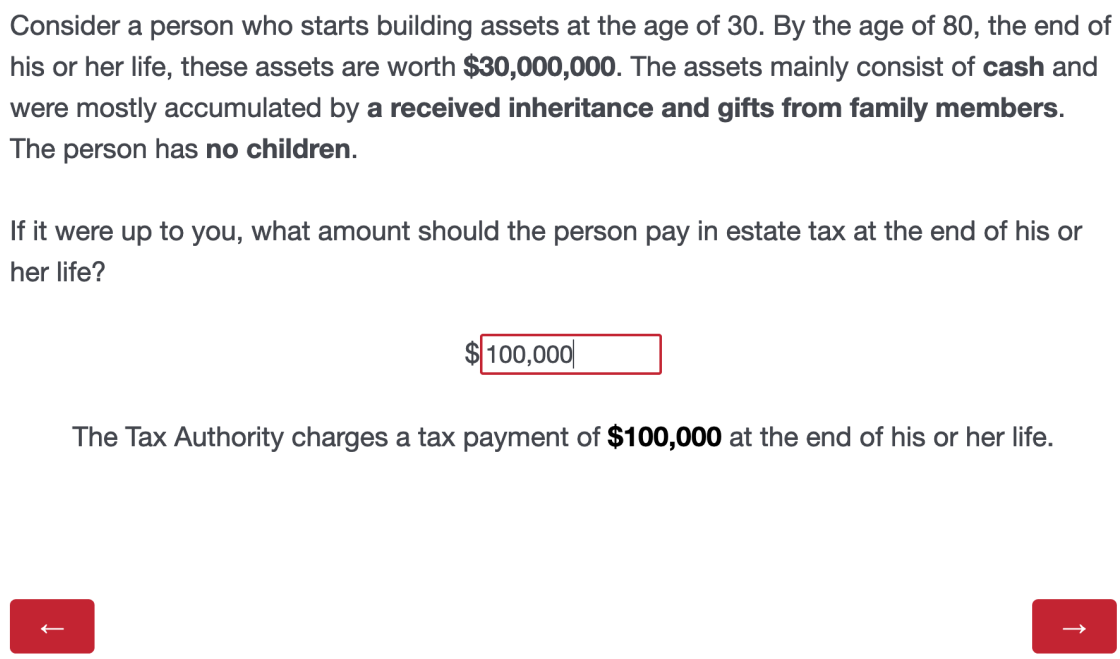



**Figure A4:** Example of a Single Vignette (Yearly Wealth Tax Group).

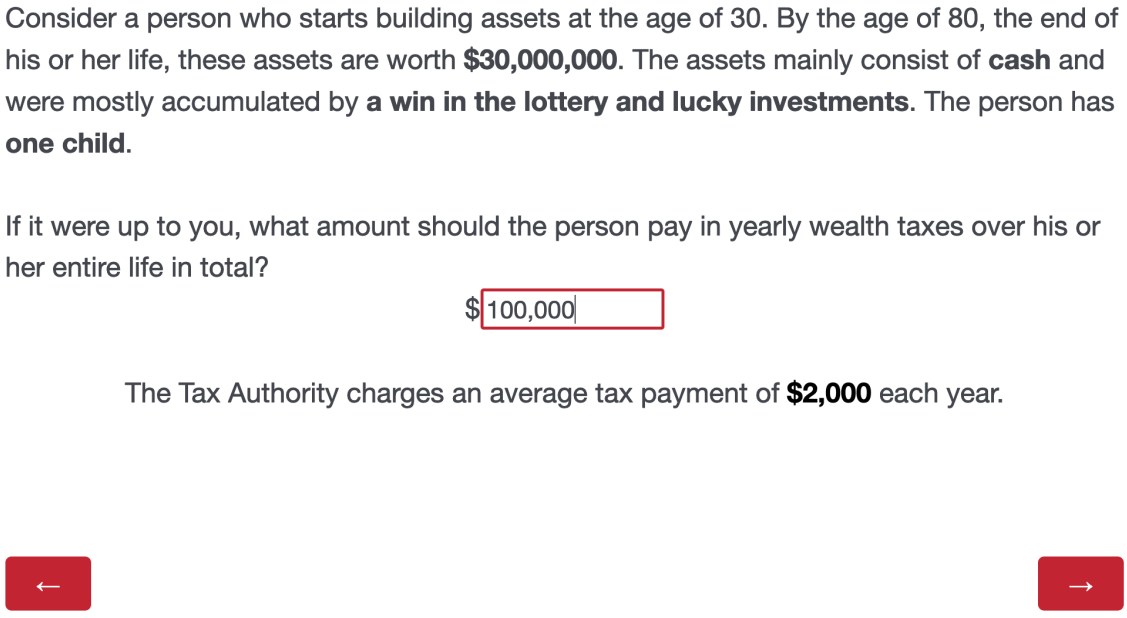
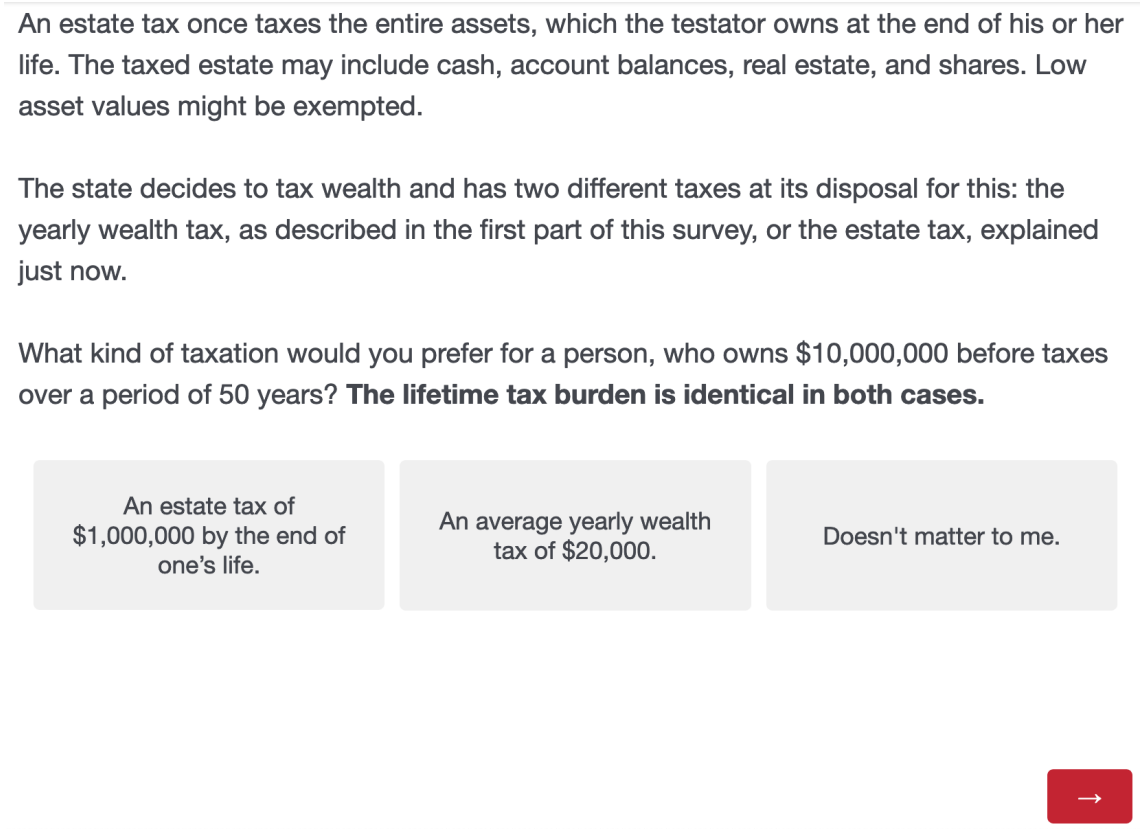


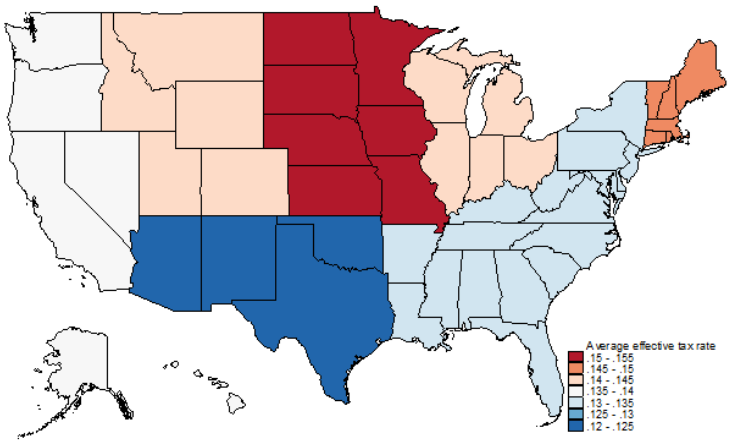
**Figure A5:** Within-Subject Comparison (Yearly Wealth Tax Group).



## A.4 Effective Tax Rates by Region

We analyzed effects of the existence of estate or inheritance taxes on state level[12] as well as the level of income and property taxes on state level on proposed effective tax rates. However, we find no significant correlation between actual taxes that are levied in the state of the respondent and her proposed tax burdens. Due to the very small number of observations for some states, we display aggregated values per region. Average proposed effective tax rates based on different regions of the United States are shown in Figure A6. Defining these, we follow the Bureau of Economic Analysis and split the country into eight different regions:[13] Far East, Rocky Mountains, Plains, Great Lakes, Mideast, New England, Southeast and Southwest. In general, respondents living in southern regions tend to propose lower taxes compared to those living in the north. The lowest average tax rates can be found in the Southwest, whereas the survey participants living in the Plains proposed the highest taxes.

**Figure A6:** Proposed Average Effective Tax Rates by Region.




[12] We consider the existence rather than the level of estate and inheritance taxes as tax levels are not comparable due to a huge variation in tax exemptions.

[13] See https://apps.bea.gov/regional/docs/regions.cfm (22.07.2019).

## A.5 Further Between-Subject Results

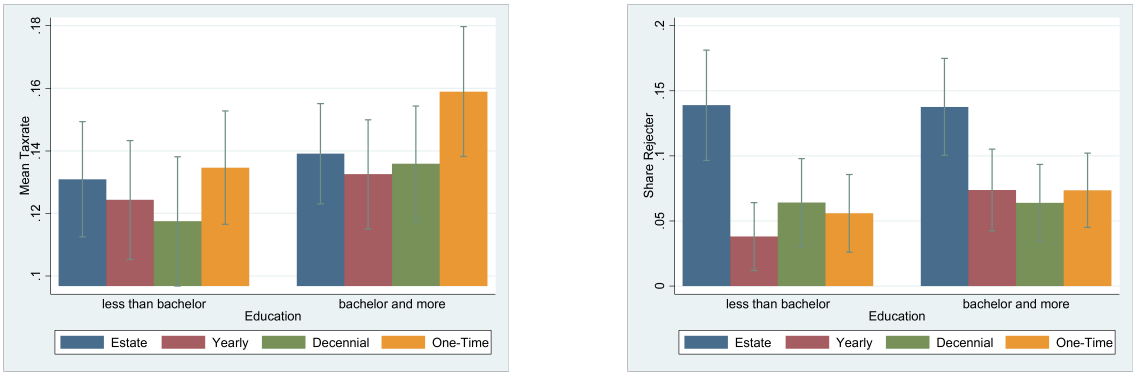
**Table A1:** Further Between-Subject Results.

| | full set | behavioral control question correctly |
|---|---|---|
| | (1) | (2) |
| Base: one-time wealth | | |
| estate | -0.010 | -0.005 |
| | (-1.13) | (-0.42) |
| yearly wealth | -0.020** | -0.020 |
| | (-2.13) | (-1.54) |
| decennial wealth | -0.021** | -0.025** |
| | (-2.24) | (-1.99) |
| Vignette variables | | |
| Base: $1m | | |
| $10m | 0.006*** | 0.014*** |
| | (3.72) | (6.38) |
| $35m | 0.022*** | 0.034*** |
| | (13.00) | (15.28) |
| Base: effort | | |
| lottery/lucky | 0.019*** | 0.021*** |
| | (10.93) | (9.47) |
| inheritance | 0.013*** | 0.015*** |
| | (7.89) | (6.67) |
| Base: cash | | |
| real estate | 0.003** | 0.004* |
| | (2.00) | (1.87) |
| business shares | 0.001 | -0.000 |
| | (0.57) | (-0.20) |
| Base: no children | | |
| one child | -0.008*** | -0.007*** |
| | (-4.77) | (-3.00) |
| three children | -0.014*** | -0.015*** |
| | (-8.14) | (-6.86) |
| Control variables | | |
| female | -0.020*** | -0.024*** |
| | (-2.94) | (-2.63) |
| age | -0.000 | -0.000 |
| | (-0.02) | (-1.09) |
| has children | -0.022*** | -0.015 |
| | (-3.08) | (-1.52) |
| share black | 0.006 | -0.047** |
| | (0.42) | (-2.28) |
| share white | 0.006 | -0.000 |
| | (0.61) | (-0.02) |
| republican | -0.041*** | -0.065*** |
| | (-4.11) | (-4.73) |
| democrat | 0.027*** | 0.014 |
| | (3.06) | (1.17) |
| education | 0.009** | 0.010* |
| | (2.31) | (1.91) |
| entrepr. in family | 0.013* | 0.016 |
| | (1.76) | (1.58) |
| expect estate tax | -0.017** | -0.026*** |
| | (-2.33) | (-2.63) |
| inherited in past | 0.025*** | 0.013 |
| | (2.61) | (0.96) |
| Observations | 18909 | 10359 |

t statistics in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Random effects model; full set of controls; dependent variable: effective tax rate.

**Figure A7:** Proposed Effective Tax Rates and Opponents by Education.




Note: Average effective tax rates (left) and share of opponents (right) **by education and treatment group**; 95%- confidence intervals.

**Table A2:** Between-Subject Results by Information.

| | full set | | behavioral control question correctly | |
|---|---|---|---|---|
| | <=10% affect. | >10% affect. | <=10% affect. | >10% affect. |
| Base: one-time wealth | | | | |
| estate | -0.022 | -0.008 | -0.011 | -0.008 |
| | (-1.05) | (-0.82) | (-0.40) | (-0.59) |
| yearly wealth | -0.054** | -0.011 | -0.050* | -0.010 |
| | (-2.47) | (-1.11) | (-1.81) | (-0.72) |
| decennial wealth | -0.020 | -0.022** | -0.030 | -0.027* |
| | (-0.89) | (-2.21) | (-1.09) | (-1.95) |
| Observations | 4833 | 14076 | 3024 | 7335 |

t statistics in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01

Note: Random effects model; full set of controls; dependent variable: effective tax rate; **by information**.

**Table A3:** Between-Subject Results by Age.

| | full set | | behavioral control question correctly | |
|---|---|---|---|---|
| | age < 35y | age >= 35y | age < 35y | age >= 35y |
| Base: one-time wealth | | | | |
| estate | 0.009 | -0.032** | 0.019 | -0.027 |
| | (0.73) | (-2.51) | (1.06) | (-1.64) |
| yearly wealth | -0.011 | -0.030** | 0.002 | -0.042** |
| | (-0.87) | (-2.20) | (0.13) | (-2.35) |
| decennial wealth | -0.017 | -0.028** | -0.021 | -0.029 |
| | (-1.34) | (-1.98) | (-1.16) | (-1.62) |
| Observations | 10440 | 8469 | 5499 | 4860 |

t statistics in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01

Note: Random effects model; full set of controls; dependent variable: effective tax rate; **by age**.

**Table A4:** Between-Subject Results by Respondent's Children.

| | full set | | behavioral control question correctly | |
|---|---|---|---|---|
| | no childr. | has childr. | no childr. | has childr. |
| Base: one-time wealth | | | | |
| estate | -0.003 | -0.016 | -0.006 | -0.001 |
| | (-0.27) | (-1.32) | (-0.38) | (-0.04) |
| yearly wealth | -0.025* | -0.012 | -0.033* | 0.004 |
| | (-1.86) | (-0.90) | (-1.84) | (0.24) |
| decennial wealth | -0.024* | -0.020 | -0.051*** | 0.009 |
| | (-1.78) | (-1.56) | (-2.83) | (0.53) |
| Observations | 10386 | 8523 | 5922 | 4437 |

Note: Random effects model; full set of controls; dependent variable: effective tax rate; **by children**.

**Table A5:** Between-Subject Results by Education.

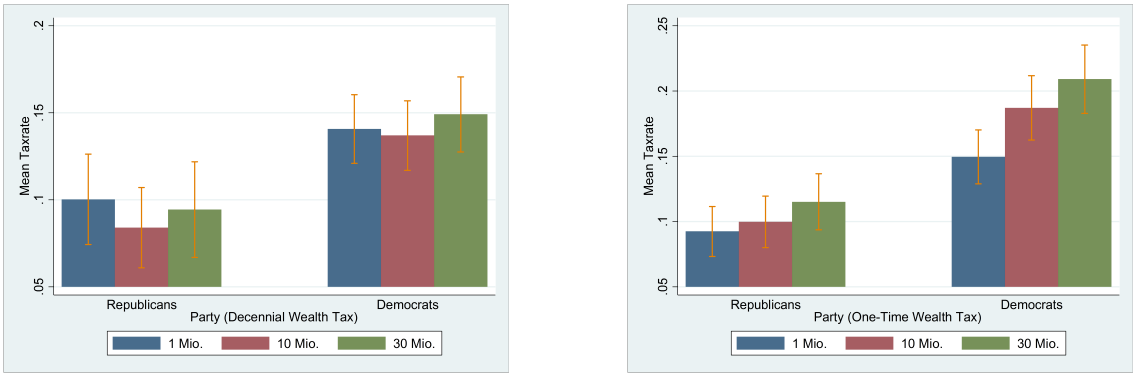
| | full set | | behavioral control question correctly | |
|---|---|---|---|---|
| | no bachelor | bachelor | no bachelor | bachelor |
| Base: one-time wealth | | | | |
| estate | -0.005 | -0.015 | -0.003 | -0.005 |
| | (-0.36) | (-1.24) | (-0.16) | (-0.29) |
| yearly wealth | -0.015 | -0.024* | -0.013 | -0.027 |
| | (-1.14) | (-1.87) | (-0.70) | (-1.52) |
| decennial wealth | -0.023* | -0.021 | -0.033* | -0.023 |
| | (-1.68) | (-1.58) | (-1.72) | (-1.31) |
| Observations | 8136 | 10773 | 4491 | 5868 |

t statistics in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01

Note: Random effects model; full set of controls; dependent variable: effective tax rate; **by education**.

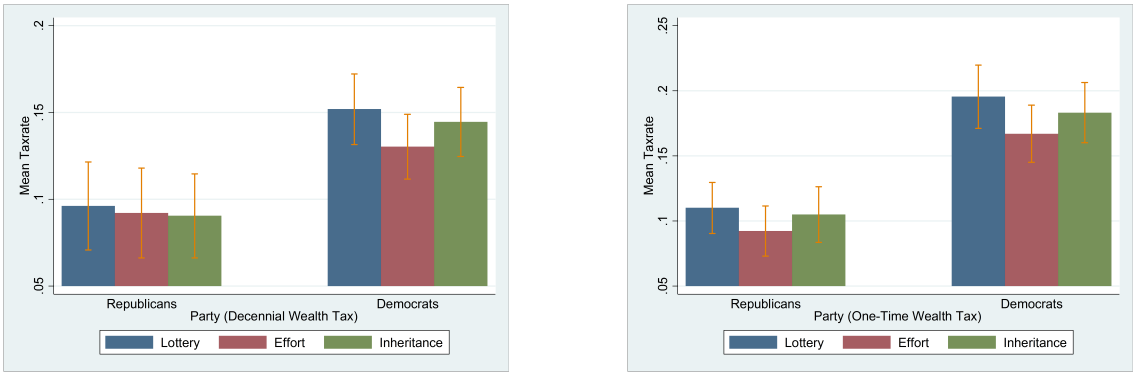
## A.6 Further Vignette Results

**Figure A8:** Proposed Tax Rates in the Decennial and One-Time Tax Groups by Partisanship and Dimension "Value of Assets".



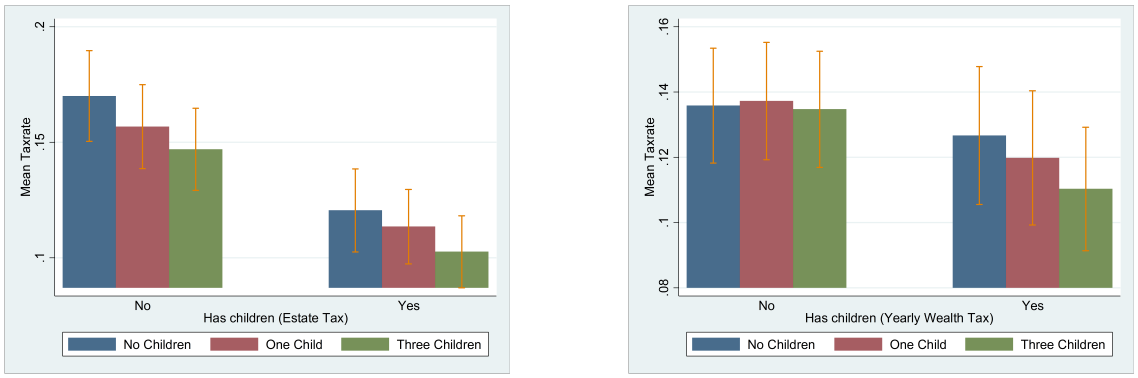

Note: Average effective tax rates in the decennial wealth tax group (left) and the one-time wealth tax group (right); **by partisanship of the respondent and vignette dimension "value of assets"**; 95%-confidence intervals.

**Figure A9:** Proposed Tax Rates in the Decennial and One-Time Tax Groups by Partisanship and Dimension "Source of Assets".



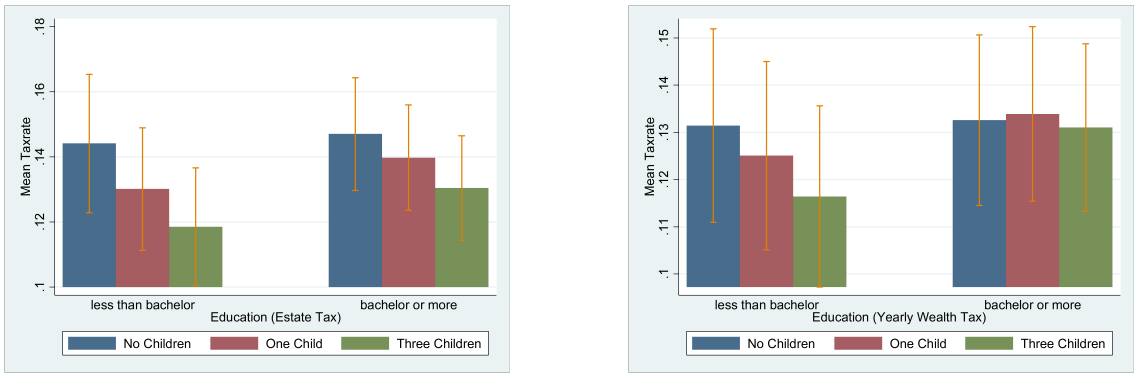

Note: Average effective tax rates in the decennial wealth tax group (left) and the one-time wealth tax group (right); **by partisanship of the respondent and vignette dimension "source of assets"**; 95%-confidence intervals.

**Figure A10:** Proposed Tax Rates in the Estate and Yearly Tax Groups by Respondent's Children and Dimension "Number of Children".




Note: Average effective tax rates in the estate tax group (left) and the yearly wealth tax group (right); **by children of the respondent and vignette dimension "number of children"**; 95%-confidence intervals.

**Figure A11:** Proposed Tax Rates in the Estate and Yearly Tax Groups by Education and Dimension "Number of Children".




Note: Average effective tax rates in the estate tax group (left) and the yearly wealth tax group (right); **by education of the respondent and vignette dimension "number of children"**; 95%-confidence intervals.

**Table A6:** Vignette Results by Partisanship

| | decennial wealth tax group | | one-time wealth tax group | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | Republicans | Democrats | Republicans | Democrats |
| Base: $1m | | | | |
| $10m | -0.016*** | -0.004 | 0.007 | 0.037*** |
| | (-2.95) | (-0.77) | (1.36) | (7.95) |
| $35m | -0.006 | 0.008* | 0.023*** | 0.060*** |
| | (-1.07) | (1.70) | (4.25) | (12.62) |
| Base: effort | | | | |
| lottery/lucky | 0.004 | 0.022*** | 0.018*** | 0.028*** |
| | (0.75) | (4.40) | (3.33) | (6.04) |
| inheritance | -0.002 | 0.014*** | 0.013** | 0.016*** |
| | (-0.29) | (2.92) | (2.40) | (3.44) |
| Base: cash | | | | |
| real estate | -0.001 | 0.001 | -0.008 | 0.003 |
| | (-0.19) | (0.12) | (-1.50) | (0.62) |
| business shares | 0.006 | 0.003 | -0.001 | 0.003 |
| | (1.08) | (0.59) | (-0.19) | (0.55) |
| Base: no children | | | | |
| one child | -0.005 | -0.011** | -0.009* | -0.013*** |
| | (-0.84) | (-2.26) | (-1.69) | (-2.81) |
| three children | -0.003 | -0.008* | -0.018*** | -0.017*** |
| | (-0.53) | (-1.65) | (-3.35) | (-3.66) |
| Observations | 1098 | 2196 | 1440 | 2709 |

t statistics in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01

Note: Random effects model; full set of controls; dependent variable: effective tax rate; **by partisanship**.

**Table A7:** Vignette Results, Additional Interaction Effects; by Treatment Group.

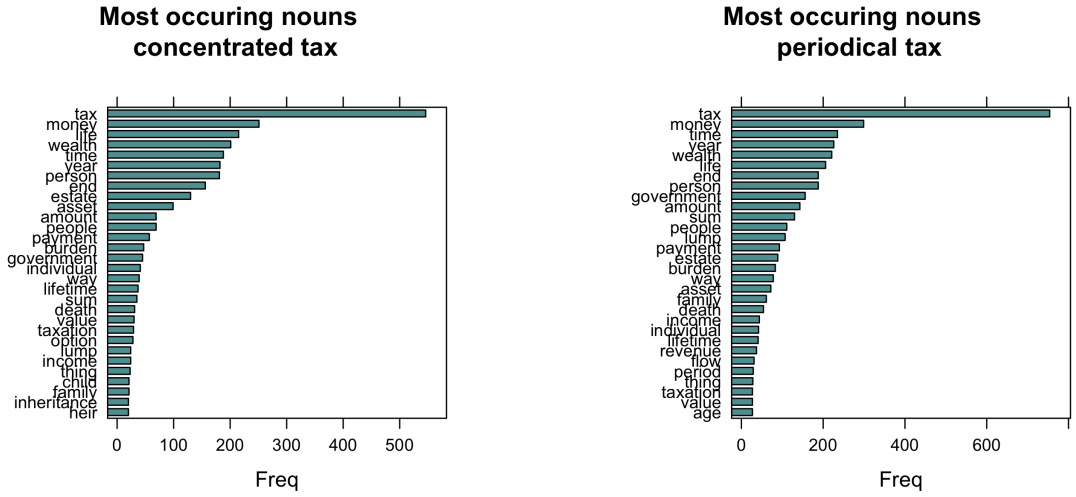
| | estate tax group | yearly wealth tax group | decennial wealth tax group | one-time wealth tax group |
|---|---|---|---|---|
| Base: $1m | | | | |
| $10m | 0.014 | 0.003 | -0.013 | 0.022** |
| | (1.65) | (0.26) | (-1.44) | (2.60) |
| $35m | 0.034*** | 0.011 | 0.014 | 0.042*** |
| | (4.11) | (1.07) | (1.61) | (4.95) |
| Base: effort | | | | |
| lottery/lucky | 0.015 | 0.024* | 0.013 | 0.025** |
| | (1.85) | (2.35) | (1.46) | (2.90) |
| inheritance | 0.004 | 0.019 | -0.007 | 0.017* |
| | (0.44) | (1.90) | (-0.79) | (2.03) |
| Base: cash | | | | |
| real estate | -0.003 | 0.035*** | 0.009 | -0.009 |
| | (-0.33) | (3.45) | (0.97) | (-1.10) |
| business shares | 0.009 | 0.012 | 0.012 | -0.007 |
| | (1.09) | (1.18) | (1.36) | (-0.78) |
| Base: no children | | | | |
| one child | -0.012 | 0.014 | -0.011 | -0.028*** |
| | (-1.40) | (1.42) | (-1.27) | (-3.32) |
| three children | -0.011 | 0.005 | 0.004 | -0.034*** |
| | (-1.27) | (0.51) | (0.46) | (-4.02) |
| 10 Mio. × lottery/lucky | 0.008 | -0.005 | 0.003 | 0.004 |
| | (1.03) | (-0.57) | (0.39) | (0.53) |
| 10 Mio. × inheritance | 0.008 | -0.008 | 0.024** | 0.002 |
| | (1.04) | (-0.86) | (2.93) | (0.27) |
| 35 Mio. × lottery/lucky | 0.008 | -0.004 | -0.005 | -0.007 |
| | (0.97) | (-0.44) | (-0.60) | (-0.88) |
| 35 Mio. × inheritance | 0.014 | -0.003 | 0.013 | -0.005 |
| | (1.82) | (-0.30) | (1.55) | (-0.69) |
| 10 Mio. × real estate | 0.005 | -0.027** | -0.005 | -0.004 |
| | (0.65) | (-2.86) | (-0.65) | (-0.48) |
| 10 Mio. × business shares | -0.000 | -0.013 | -0.004 | 0.001 |
| | (-0.06) | (-1.43) | (-0.51) | (0.10) |
| 35 Mio. × real estate | 0.001 | -0.025** | -0.002 | -0.002 |
| | (0.16) | (-2.69) | (-0.23) | (-0.30) |
| 35 Mio. × business shares | -0.006 | -0.012 | -0.003 | 0.002 |
| | (-0.71) | (-1.32) | (-0.34) | (0.24) |
| 10 Mio. × one child | -0.006 | -0.008 | 0.006 | 0.003 |
| | (-0.79) | (-0.85) | (0.77) | (0.39) |
| 10 Mio. × three children | 0.000 | -0.009 | 0.004 | 0.002 |
| | (0.05) | (-1.00) | (0.51) | (0.28) |
| 35 Mio. × one child | -0.007 | -0.011 | -0.005 | 0.007 |
| | (-0.96) | (-1.16) | (-0.60) | (0.94) |
| 35 Mio. × three children | -0.005 | -0.002 | -0.011 | 0.010 |
| | (-0.63) | (-0.25) | (-1.31) | (1.22) |
| lottery/lucky × real estate | -0.006 | -0.006 | -0.004 | -0.004 |
| | (-0.78) | (-0.63) | (-0.45) | (-0.52) |
| lottery/lucky × business shares | -0.005 | 0.008 | -0.009 | -0.012 |
| | (-0.67) | (0.87) | (-1.07) | (-1.52) |
| inheritance × real estate | 0.010 | 0.003 | 0.007 | 0.004 |
| | (1.23) | (0.37) | (0.90) | (0.48) |
| inheritance × business shares | -0.000 | -0.003 | 0.008 | -0.000 |
| | (-0.05) | (-0.32) | (1.03) | (-0.03) |
| lottery/lucky × one child | 0.006 | -0.001 | 0.019* | 0.013 |
| | (0.77) | (-0.10) | (2.34) | (1.70) |
| lottery/lucky × three children | -0.009 | -0.009 | 0.008 | -0.001 |
| | (-1.12) | (-0.93) | (0.94) | (-0.11) |
| inheritance × one child | 0.002 | 0.000 | 0.005 | -0.003 |
| | (0.23) | (0.03) | (0.67) | (-0.41) |
| inheritance × three children | -0.008 | 0.003 | -0.007 | -0.006 |
| | (-0.97) | (0.32) | (-0.80) | (-0.73) |
| real estate × one child | 0.013 | -0.016 | -0.010 | 0.012 |
| | (1.74) | (-1.76) | (-1.17) | (1.52) |
| real estate × three children | -0.002 | -0.009 | -0.013 | 0.025** |
| | (-0.22) | (-1.00) | (-1.54) | (3.21) |
| business shares × one child | -0.003 | -0.014 | -0.006 | 0.019* |
| | (-0.37) | (-1.48) | (-0.74) | (2.37) |
| business shares × three children | -0.007 | -0.011 | -0.015 | 0.019* |
| | (-0.95) | (-1.16) | (-1.82) | (2.40) |
| Observations | 5337 | 4329 | 4221 | 5022 |

t statistics in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Rrandom effects model; full set of controls; dependent variable: effective tax rate; additional interaction effects; by treatment group.

## A.7 Further Textual Analyses

**Figure A12:** Most Used Nouns When a Concentrated or a Periodic Tax Was Chosen.




Note: Most frequently used nouns in the open-ended answers when a concentrated tax (estate or one-time, right) or a periodic tax (yearly or decennial, left) was chosen.

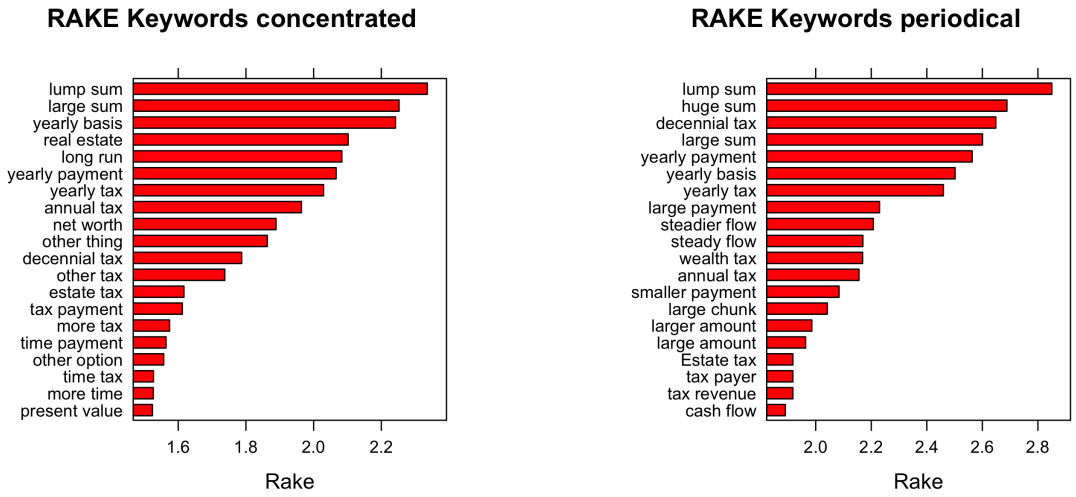
**Figure A13:** Heatmap of Most Occuring Motives.

| | political_pos | political_neg | behavior | transfer | procedure |
|---|---|---|---|---|---|
| estate | 0.018 | 0.049 | 0.022 | 0.099 | 0.101 |
| one_time | 0.005 | 0.005 | 0.022 | 0.066 | 0.175 |
| yearly | 0.043 | 0.028 | 0.048 | 0.083 | 0.234 |
| decennial | 0.023 | 0.009 | 0.042 | 0.150 | 0.234 |
| no_pref | 0.015 | 0.063 | 0.010 | 0.048 | 0.028 |

| | political_pos | political_neg | behavior | transfer | procedure |
|---|---|---|---|---|---|
| rep.estate | 0.013 | 0.083 | 0.013 | 0.096 | 0.127 |
| rep.one_time | 0.000 | 0.000 | 0.051 | 0.068 | 0.136 |
| rep.yearly | 0.010 | 0.041 | 0.052 | 0.129 | 0.247 |
| rep.decennial | 0.019 | 0.000 | 0.000 | 0.151 | 0.245 |
| rep.no_pref | 0.016 | 0.122 | 0.016 | 0.024 | 0.016 |
| dem.estate | 0.023 | 0.027 | 0.035 | 0.093 | 0.109 |
| dem.one_time | 0.011 | 0.011 | 0.000 | 0.074 | 0.191 |
| dem.yearly | 0.057 | 0.022 | 0.053 | 0.057 | 0.230 |
| dem.decennial | 0.018 | 0.000 | 0.036 | 0.126 | 0.243 |
| dem.no_pref | 0.011 | 0.011 | 0.005 | 0.059 | 0.037 |

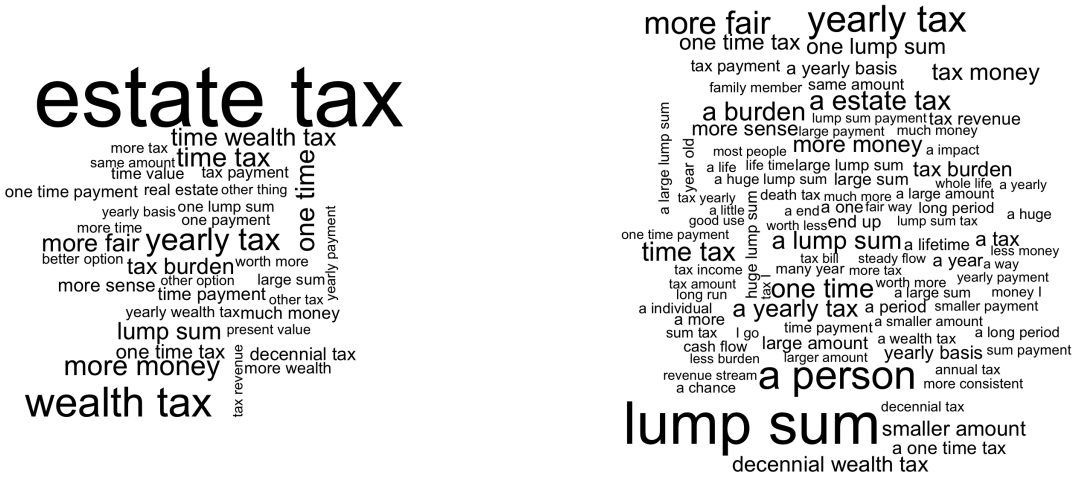
Note: Heatmap of the most occuring motives from green (low number of matches) to red (high number of matches). Underlying dictionary: transfer: "children", "heir", "kids", "generation", "transfer", "family"; political_neg: "theft", "death tax", "unfair", "not fair", "double", "robbery", "steals", "twice", "evil", "waste", "punish", "repeatedly", "already"; political_pos: "inequality", "fair share", "redistribution", "fund"; behavior: "evasion", "avoidance", "way around", "plan", "defraud", "loophole"; procedure: "easier", "chunk", "not at once", "lump sum", "overwhelming", "simpler", "spread", "ongoing", "consistent".

**Figure A14:** Keywords Identified by the RAKE Algorithm About the Within-Subject Choice.




Note: Keywords identified by the RAKE algorithm based on the open-ended answers when a concentrated tax (estate or one-time, left) or a periodic tax (yearly or decennial, right) was chosen. "RAKE" stands for "Rapid Automatic Keyword Extraction" and is a common keyword extraction method. Basically, it deploys a list of stopwords and phrase delimiters to remove those items from a text to identify the most relevant words or phrases. In a next step, the algorithm creates a matrix of word co-occurrences and calculates both the sum of the number of co-occurrences each word has with any other word as well as the number of times each word appears in the text. Finally, if keywords or keyphrases appear together in the same order multiple times, a score of these keyphrase is computed like the one for a single keyphrase.

**Figure A15:** Wordcloud on the Within-Subject Choice.




Note: Wordcloud compiled by the Textrank algorithm based on the open-ended answers when a concentrated tax (estate or one-time , left) or a periodic tax (yearly or decennial, right) was chosen. The "Textrank" algorithm is, just as the RAKE algorithm, another method to identify keywords and key phrases. Basically, the algorithm identifies how phrases are related to each other by exploring overlapping terminology and then setting up links between sentences. Keywords are identified based on a constructed network to explore how words are follwing each other. Both the resulting key phrases and keywords are finally ranked by their importancy.

# Appendix B

## B.1 Full Survey Questionnaire

The survey structure is displayed in Figure 2.1 in Section 2.2. The survey contains of five parts. Part III is the main part where respondents were randomized in four different groups. Each question is labeled with the corresponding variable name in parentheses. Interested readers can have a look at the German survey themselves at the following link: https://mpibonn.eu.qualtrics.com/jfe/form/SV_d13rpDRZiIzAhwx.

### Part I: Understanding Taxes

1. What is the income tax payable on an income of 50.000 €, an allowance of 20.000 € and a tax rate of 10%? [Control Taxes 1]
2. What is the income tax payable on an income of 10.000 €, an allowance of 20.000 € and a tax rate of 10%? [Control Taxes 2]

### Part II: Understanding Growth

3. What is the wealth of a person if they initially own 100 € and then save another 5%, i.e. the wealth increases by 5%? [Control Growth]

### Part III: Main Part

4. In your opinion, how much wealth should be exempt from the (yearly/one-time) wealth tax? [Tax Allowance]
5. At what tax rate should the wealth above the chosen allowance be taxed (yearly/once)? [Tax Rate Person 1]
6. At what tax rate should the wealth above the chosen allowance be taxed (yearly/once)? [Tax Rate Person 2]
7. At what tax rate should the wealth above the chosen allowance be taxed (yearly/once)? [Tax Rate Person 3]

### Part IV: Choice & Reasoning

8. What type of wealth taxation would you prefer for this person? The burden of the tax is identical in both cases. [Choice]
9. Please explain why you have chosen this type of wealth tax. [Reasoning]

**Part V: Personal Background**

10. To what extent do you think it is the government's job to redistribute wealth? Think on a scale from 1 to 7. 1 means that it should not be the government's job to redistribute wealth; 7 means it should definitely be the government's job. [Redistribute]
11. To what extent do you agree with the following statement? "Wealth inequality is a serious problem." [Inequality]
12. There are different opinions about the reasons for wealth and economic success. Some see luck and the support of others as the decisive factors, while others see personal commitment and hard work as the main factors. In your opinion, which prerequisite generally plays a greater role? [Luck Effort]
13. What do you suppose is the share of the population that is affected by the current inheritance tax? [Inheritance Tax]
14. Do you expect to be affected by inheritance tax yourself in the future (as bequeather or heir)? [Future Tax]
15. Please indicate your sex assigned at birth. [Female]
16. Please indicate your age. [Age]
17. Have you ever had to pay inheritance tax yourself? [Past Tax]
18. Do you or your close family own a business? [Entrepreneur]
19. How many children do you have? [Children]
20. What is your nationality? [Nationality]
21. What is your marital status? [Married]
22. What is your highest educational qualification? [University]
23. What is your current employment status? [Employed]
24. What is your current net monthly income (i.e. after taxes and duties)? [Income]
25. How would you most likely classify your political orientation? [Political Orientation]
26. Which party would you most closely align yourself with? [Party]

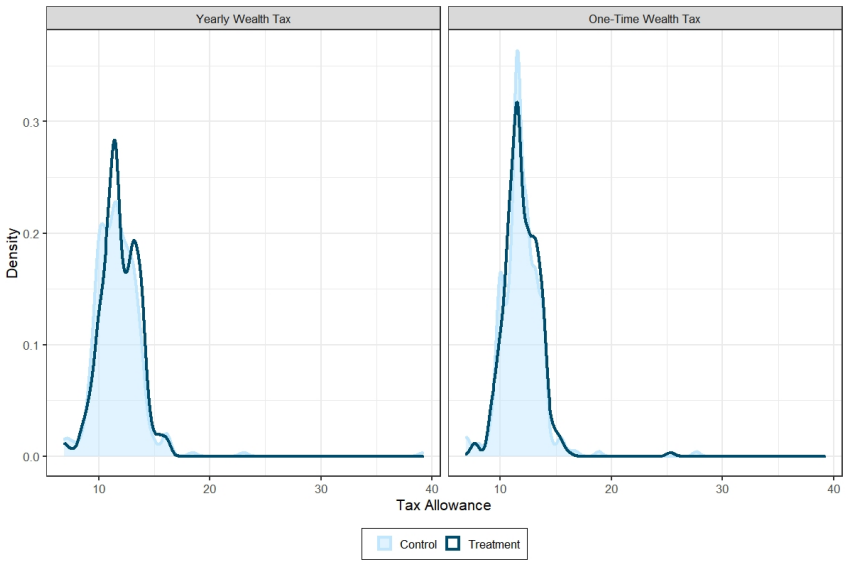
## B.2 Wealth Tax Parameters

While we focus on derived effective tax burdens in the main section, in this additional part we want to further analyze how respondents compose their preferred tax parameters. Respondents first propose a general tax allowance and then tax rates for three hypothetical individuals that differ only in their accumulated net wealth. All of these parameters are entered on the same page. When respondents are informed about the consequences of the tax burden, they are expected to adjust their initial thoughts on the tax parameters. Even though they can adjust both the tax allowance and the specific tax rate, people are more likely to adjust their specific tax rates than the tax allowance.

### Tax Allowances

In Figure B1 the distribution of tax allowances by experimental group is presented. Due to extreme outliers we used the natural logarithm to get a meaningful distribution.

**Figure B1:** Tax Allowances.



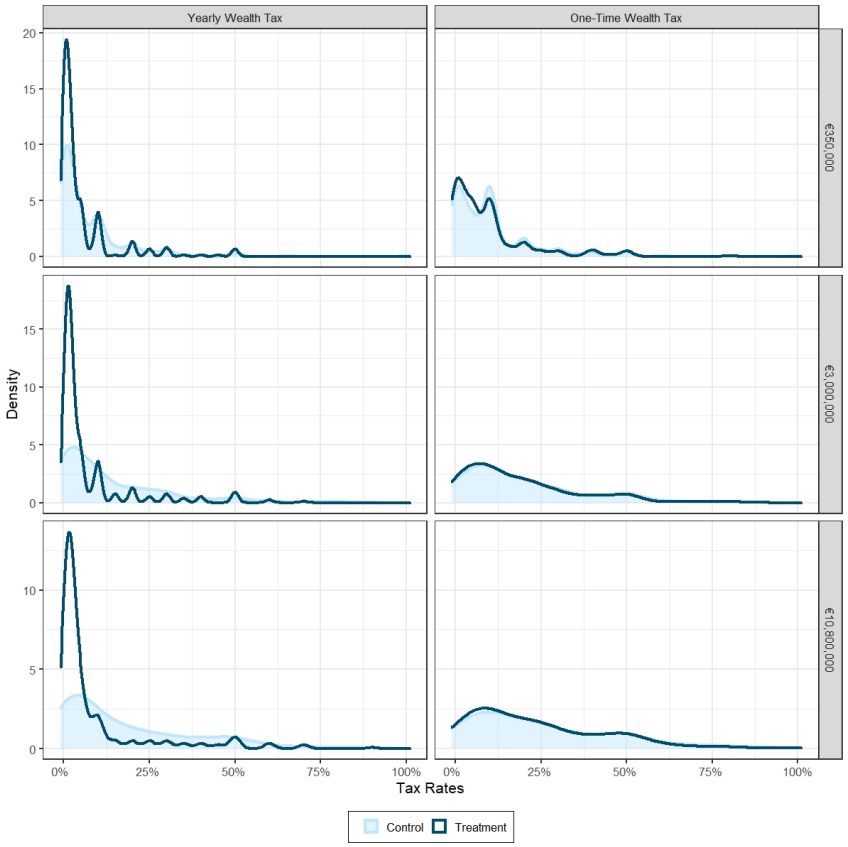

Note: This figure displays the distribution of the inserted tax allowance (natural logarithm) by experimental groups.

The tax allowance is hardly affected by neither the wealth tax instrument nor the information treatment. The median tax allowance is 100,000 € in each group. The mean value trimmed by 1% are all in a similar range: 463,347 € (control) / 411,075 € (treatment) in the *yearly* wealth tax group and 353,558 € (control) / 341,988 € (treatment) in the *one-time* wealth tax group. Only 0.5% of respondents want to have no tax allowance at all and about 0.6% of respondents reject any wealth tax with a proposed tax allowance exceeding the maximum accumulated wealth. The participants seem to have a clear idea of how much wealth should be exempt from wealth tax regardless of the wealth tax instrument. These preferred values are not significantly different between the four experimental groups (Welch One-Way ANOVA Test, p-Value = 0.66). This means, the information treatment does not affect the amount of the general tax allowance.

### Tax Rates

After entering a general tax allowance participants were asked to state their preferred tax rate for each of three hypothetical persons differing in their accumulated wealth (350,000 €, 3,000,000 €, and 10,800,000 €). In Figure B2 the distribution of tax rates by experimental group is presented for each wealth level.

**Figure B2:** Tax Rates.




Note: This figure displays the distribution of the inserted tax rates by different wealth levels and experimental groups.

While the distribution of tax rates in the *one-time* wealth tax group is not distinguishable between control and treatment group, the tax rates in the *yearly* wealth tax group are shifted to lower values in the treatment group. The information on the effective tax burden leads to strong tax rate adjustments only in the *yearly* wealth tax group. These tax rate adjustments translate into changes of effective tax rates as described in Section 2.4.1.

Unfortunately, we are not able to track how many times respondents entered and readjusted their tax rate. Of course, the participants will not do this infinitely until they get their exact preferred effective tax burden result visualized in the responsive pie chart. But as tax rates significantly change in *treated* yearly wealth tax group, we know they do readjust.

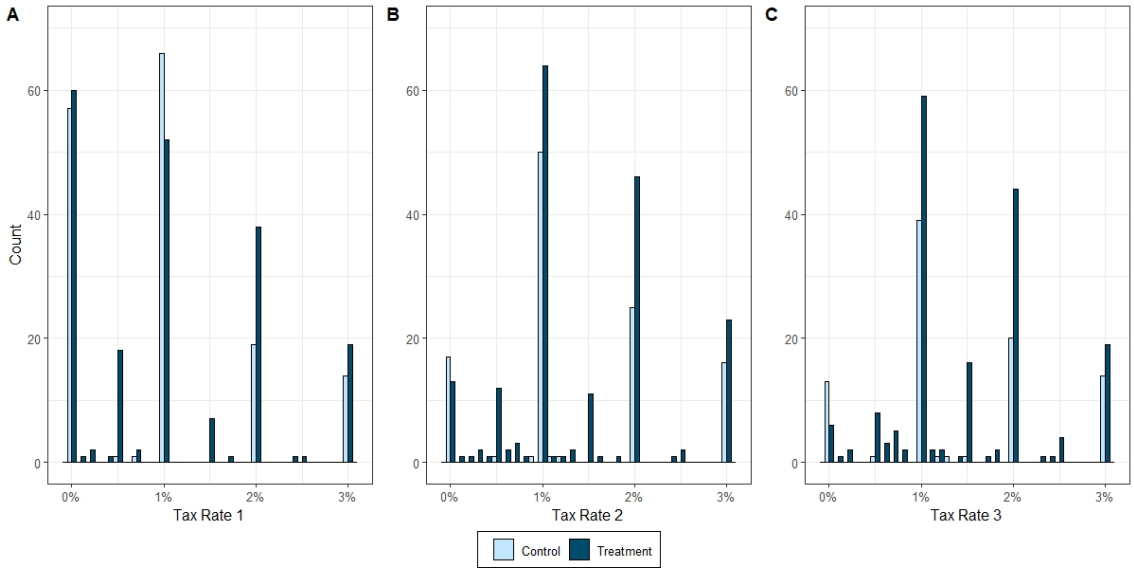
### Round Number Bias

Since all other common taxes (e.g., income tax or sales tax) use double-digit tax rates, people might be anchored at these salient round tax rates. For example, to obtain similar effective tax burden in both wealth tax instruments, respondents need to use an uncommon tax rate range from 0.5% to 2% in the *yearly* wealth tax group, while respondents can use common tax rate ranges

from 15% to 45% in the *one-time* wealth tax group.[1] The limited tax rate margin for a yearly wealth tax implies that people should also use decimals although probably having a round number bias.

In Figure B3, we illustrate the potential *round number bias* for the *yearly* wealth tax across our three hypothetical persons.

**Figure B3:** Tiny Tax Rates and Round Number Bias.




Note: This figure displays the absolute frequency of inserted tax rates within the range of 0% to 3% in the *yearly* wealth tax group.

These figures nicely show that the information treatment also causes a move away from rounded figures. While the control group almost exclusively used the round number 0%, 1%, 2% and 3%, the treatment group also used very specific decimals. Regarding the total tax rate distribution, 22% of the treated *yearly* wealth tax group used at least one tax rate with decimals, while this is only the case in 3.3% of the control group.

Since, on the one hand, the information treatment increases the probability of using decimal places and, on the other hand, the *round number bias* does not necessarily go in only one direction, we can argue that the *round number bias* has only little influence on our results.

[1] This applies if we disregard the tax allowance and assume the *savings plan* of our study.

## B.3 Additional Tables

**Table B1:** Variable Definitions.

| **Variable** | **Definition** |
|---|---|
| Effective Tax Rate | Calculation described in Section 2.2 |
| Age | entered age between 18 and 83 |
| Female | 1 if gender is female, otherwise 0 |
| Married | 1 if marital status is, otherwise 0 |
| Inheritance Tax | estimated share of population that is affected by inheritance tax (0,1) |
| Past Tax | 1 if already paid inheritance tax, otherwise 0 |
| Future Tax | 1 if expect to be affected by inheritance tax, otherwise 0 |
| Political Orientation | 7 point likert scale: left right political spectrum |
| Redistribute | 7 point likert scale: government should redistribute wealth |
| Inequality | 7 point likert scale: inequality is a serious problem |
| Children | 1 if one or more children, otherwise 0 |
| University | 1 if Bachelor, Master or PhD, otherwise 0 |
| Income | 6 increasing income brackets: |
| | 1: 0 €-999 €, 2: 1,000 €-1,999 €, 3: 2,000 €-2,999 € |
| | 4: 3,000 €-3,999 €, 5: 4,000 €-4,999 €, 6: 5,000 € and more |
| Employed | 1 if full-time, part-time or self employed, otherwise 0 |
| Control | arithmetic mean of Control Tax 1, Control Tax 2 and Control Growth (0,1) |

Note: This table shows definitions of our mainly used survey-based variables.

**Table B2:** Summary Statistics.

| **Statistic** | **N** | **Mean** | **SD** | **Min** | **.25** | **.75** | **Max** |
|---|---|---|---|---|---|---|---|
| Effective Tax Rate | 3,747 | 0.311 | 0.271 | 0.000 | 0.094 | 0.477 | 0.980 |
| Age | 1,249 | 51.864 | 13.915 | 18 | 41 | 63 | 83 |
| Female | 1,249 | 0.384 | 0.486 | 0.000 | 0.000 | 1.000 | 1.000 |
| Inheritance Tax | 1,248 | 0.292 | 0.207 | 0.010 | 0.138 | 0.400 | 1.000 |
| Future Tax | 1,249 | 0.264 | 0.441 | 0.000 | 0.000 | 1.000 | 1.000 |
| Past Tax | 1,247 | 0.059 | 0.235 | 0.000 | 0.000 | 0.000 | 1.000 |
| Political Orientation | 1,246 | 3.745 | 1.128 | 1.000 | 3.000 | 4.000 | 7.000 |
| Redistribute | 1,249 | 4.259 | 1.788 | 1.000 | 3.000 | 5.000 | 7.000 |
| Inequality | 1,249 | 5.254 | 1.670 | 1.000 | 4.000 | 7.000 | 7.000 |
| Children | 1,243 | 0.615 | 0.487 | 0.000 | 0.000 | 1.000 | 1.000 |
| University | 1,248 | 0.376 | 0.485 | 0.000 | 0.000 | 1.000 | 1.000 |
| Income | 1,247 | 2.768 | 1.233 | 1.000 | 2.000 | 4.000 | 6.000 |
| Employed | 1,248 | 0.616 | 0.487 | 0.000 | 0.000 | 1.000 | 1.000 |
| Control | 1,249 | 0.943 | 0.155 | 0.000 | 1.000 | 1.000 | 1.000 |

Note: This table presents summary statistics of all variables used in our regression analysis.

**Table B3:** Randomization Check.

| | **Yearly Wealth Tax** | | **One-Time Wealth Tax** | |
|---|---|---|---|---|
| | Control | Treatment | Control | Treatment |
| Age | 0.0003 | −0.0006 | 0.0001 | 0.0002 |
| | (0.0009) | (0.0009) | (0.0009) | (0.0009) |
| Female | −0.0418* | 0.0405 | 0.0040 | -0.0026 |
| | (0.0250) | (0.0249) | (0.0249) | (0.0259) |
| Married | 0.0134 | −0.0281 | −0.0089 | 0.0236 |
| | (0.0244) | (0.0242) | (0.0243) | (0.0252) |
| Personal Income | | | | |
| 0 €-999 € | −0.0328 | −0.0344 | 0.0267 | 0.0404 |
| | (0.0362) | (0.0360) | (0.0361) | (0.0376) |
| 1.000 €-1.999 € | −0.0244 | −0.0203 | 0.0210 | 0.0237 |
| | (0.0253) | (0.0251) | (0.0252) | (0.0263) |
| 2.000 €-2.999 € | 0.0205 | −0.0045 | 0.0236 | −0.0396 |
| | (0.0288) | (0.0286) | (0.0287) | (0.0298) |
| 3.000 €-3.999 € | −0.0097 | 0.0179 | −0.0503 | 0.0422 |
| | (0.0317) | (0.0315) | (0.0316) | (0.0329) |
| 4.000 €-4.999 € | 0.0859* | 0.0644 | −0.0584 | −0.0919* |
| | (0.0497) | (0.0494) | (0.0495) | (0.0515) |
| 5.000 € and more | 0.0717 | 0.1061 | −0.0434 | 0.1344* |
| | (0.0737) | (0.0732) | (0.0735) | (0.0764) |
| Children | 0.0324 | −0.0387 | −0.0128 | 0.0191 |
| | (0.0251) | (0.0249) | (0.0249) | (0.0260) |
| University | 0.0137 | −0.0012 | 0.0017 | −0.0142 |
| | (0.0252) | (0.0250) | (0.0251) | (0.0261) |
| Employed | −0.0324 | 0.0547** | −0.0185 | −0.0038 |
| | (0.0250) | (0.0248) | (0.0249) | (0.0260) |
| p-value | 0.3048 | 0.1169 | 0.8928 | 0.2925 |

Note: This table shows coefficients of a series of OLS regressions in the form of $\texttt{Group}_i = \alpha + \beta \cdot \texttt{Covariate}_i + \epsilon_i$, where $\texttt{Group}_i$ is a dummy indicating the group status and $\texttt{Covariate}_i$ is any of the listed variables. The p-value of F-statistic for joint significance of all listed Covariates explaining the group status is added at the bottom of the table. Standard errors in parentheses: $^{*}p < 0.1$, $^{**}p < 0.05$ and $^{***}p < 0.01$.

**Table B4:** Full Baseline Regression Results.

| | **Effective Tax Rate** (1) | **Effective Tax Rate** (2) |
|---|---|---|
| Info Treatment | −0.010 | −0.002 |
| | (0.011) | (0.010) |
| Yearly Wealth Tax | 0.384*** | 0.394*** |
| | (0.018) | (0.017) |
| Yearly Wealth Tax x Info Treatment | −0.137*** | −0.146*** |
| | (0.025) | (0.024) |
| Age | | 0.001 |
| | | (0.001) |
| Female | | 0.010 |
| | | (0.012) |
| Inheritance Tax | | 0.122*** |
| | | (0.030) |
| Future Tax | | −0.016 |
| | | (0.014) |
| Past Tax | | −0.016 |
| | | (0.029) |
| Political Orientation | | −0.002 |
| | | (0.006) |
| Redistribute | | 0.029*** |
| | | (0.004) |
| Inequality | | 0.006 |
| | | (0.005) |
| Children | | 0.002 |
| | | (0.014) |
| University | | 0.0001 |
| | | (0.013) |
| Income | | −0.006 |
| | | (0.005) |
| Control | | −0.030 |
| | | (0.036) |
| Employed | | −0.001 |
| | | (0.014) |
| N | 3.747 | 3.702 |

Note: This table presents linear regression coefficients with wealth levels fixed effects where the dependent variable is effective tax rate. Standard errors are clustered by respondents' IDs: $^{*}p < 0.1$, $^{**}p < 0.05$ and $^{***}p < 0.01$.

**Table B5:** Wealth Level Treatment Effects.

| | **Yearly Wealth Tax** (1) | **One-Time Wealth Tax** (2) |
|---|---|---|
| 350,000 € | 0.126 | −0.035 |
| | (0.117) | (0.054) |
| 3,000,000 € | 0.394*** | 0.073 |
| | (0.118) | (0.054) |
| 10,800,000 € | 0.477*** | 0.149*** |
| | (0.118) | (0.054) |
| Info-Treatment x 350,000 € | −0.107*** | 0.0003 |
| | (0.024) | (0.007) |
| Info-Treatment x 3,000,000 € | −0.169*** | 0.002 |
| | (0.024) | (0.011) |
| Info-Treatment x 10,800,000 € | −0.178*** | −0.010 |
| | (0.023) | (0.015) |
| Age | 0.001 | −0.0002 |
| | (0.001) | (0.0005) |
| Female | 0.048** | −0.029*** |
| | (0.023) | (0.010) |
| Inheritancetax | 0.215*** | 0.013 |
| | (0.054) | (0.028) |
| Future Tax | −0.009 | −0.017 |
| | (0.027) | (0.012) |
| Past Tax | −0.027 | 0.002 |
| | (0.050) | (0.023) |
| Political Orientation | −0.001 | −0.007 |
| | (0.010) | (0.006) |
| Redistribute | 0.032*** | 0.027*** |
| | (0.008) | (0.004) |
| Inequality | 0.007 | 0.003 |
| | (0.009) | (0.004) |
| Children | −0.001 | −0.002 |
| | (0.026) | (0.012) |
| University | −0.020 | 0.010 |
| | (0.023) | (0.011) |
| Income | −0.007 | −0.005 |
| | (0.009) | (0.005) |
| Employed | 0.004 | −0.006 |
| | (0.026) | (0.012) |
| Control | −0.074 | 0.027 |
| | (0.070) | (0.028) |
| N | 1,797 | 1,905 |

Note: This table presents linear regression analysis by wealth tax instrument. Dependent variable is effective tax rate. Standard errors are clustered by respondents' IDs: $^{*}p < 0.1$, $^{**}p < 0.05$ and $^{***}p < 0.01$.

**Table B6:** Heterogeneous Treatment Effects.

| | **Yearly Wealth Tax** (1) | **One-Time Wealth Tax** (2) |
|---|---|---|
| Info-Treatment | 0.062 | 0.083 |
| | (0.239) | (0.107) |
| Age | 0.001 | 0.0002 |
| | (0.001) | (0.001) |
| Female | 0.068** | −0.036** |
| | (0.033) | (0.014) |
| Inheritance Tax | 0.316*** | 0.001 |
| | (0.078) | (0.038) |
| Future Tax | 0.006 | −0.006 |
| | (0.039) | (0.017) |
| Past Tax | −0.173** | −0.047* |
| | (0.075) | (0.024) |
| Political Orientation | −0.003 | −0.011 |
| | (0.014) | (0.008) |
| Redistribute | 0.042*** | 0.027*** |
| | (0.012) | (0.006) |
| Inequality | 0.008 | 0.009* |
| | (0.012) | (0.005) |
| Children | −0.028 | −0.016 |
| | (0.037) | (0.017) |
| University | −0.031 | 0.016 |
| | (0.034) | (0.015) |
| Income | −0.006 | −0.004 |
| | (0.014) | (0.007) |
| Employed | −0.018 | −0.001 |
| | (0.037) | (0.017) |
| Control | −0.043 | 0.063 |
| | (0.092) | (0.046) |
| Age x Info-Treatment | −0.001 | −0.001 |
| | (0.002) | (0.001) |
| Female x Info-Treatment | −0.035 | 0.017 |
| | (0.045) | (0.019) |
| Inheritance Tax x Info-Treatment | −0.230** | 0.026 |
| | (0.108) | (0.054) |
| Future Tax x Info-Treatment | −0.039 | −0.017 |
| | (0.054) | (0.024) |
| Past Tax x Info-Treatment | 0.272*** | 0.113** |
| | (0.094) | (0.045) |
| Political Orientation x Info-Treatment | 0.006 | 0.011 |
| | (0.021) | (0.011) |
| Redistribute x Info-Treatment | −0.018 | 0.001 |
| | (0.016) | (0.008) |
| Inequality x Info-Treatment | −0.004 | −0.012 |
| | (0.018) | (0.008) |
| Children x Info-Treatment | 0.065 | 0.027 |
| | (0.052) | (0.024) |
| University x Info-Treatment | 0.031 | −0.011 |
| | (0.046) | (0.022) |
| Income x Info-Treatment | −0.003 | 0.001 |
| | (0.018) | (0.010) |
| Employed x Info-Treatment | 0.042 | −0.011 |
| | (0.051) | (0.024) |
| Control x Info-Treatment | −0.109 | −0.054 |
| | (0.137) | (0.057) |
| N | 1,797 | 1,905 |

Note: This table presents linear regression coefficients with wealth levels fixed effects where the dependent variable is effective tax rate. Standard errors are clustered by respondents' IDs: $^{*}p < 0.1$, $^{**}p < 0.05$ and $^{***}p < 0.01$.

**Table B7:** Logit Regression.

| | **Choice: Yearly Wealth Tax** |
|---|---|
| Info-Treatment | −0.159 |
| | (0.153) |
| Yearly Wealth Tax | 0.649*** |
| | (0.156) |
| Age | 0.010 |
| | (0.007) |
| Female | −0.097 |
| | (0.164) |
| Inheritance Tax | 0.128 |
| | (0.413) |
| Future Tax | −0.151 |
| | (0.189) |
| Past Tax | −0.291 |
| | (0.324) |
| Political Orientation | −0.209*** |
| | (0.076) |
| Redistribute | 0.127** |
| | (0.057) |
| Inequality | 0.078 |
| | (0.059) |
| Children | −0.225 |
| | (0.184) |
| University | −0.266 |
| | (0.165) |
| Income | 0.134* |
| | (0.072) |
| Employed | 0.521*** |
| | (0.184) |
| Control | 0.436 |
| | (0.480) |
| Constant | −0.355 |
| | (0.813) |
| N | 1,086 |

Note: This table presents logit regression coefficients where the dependent variable is a dummy indicating a yearly wealth tax chosen in the within-question. Standard errors in parentheses: $^{*}p < 0.1$, $^{**}p < 0.05$ and $^{***}p < 0.01$.

## B.4 Further Aspects

### B.4.1 Inheritance Taxation in Germany

This paper focuses on misperceived tax burden consequences in the context of the highly complex yearly wealth tax, but knowledge about inheritance taxation in Germany could also play a role. We asked our participants for their best guess on what share of the German population is affected by the existing inheritance tax. We argue that respondents giving percentages closer to the actual share of the German population are generally better informed about wealth distribution and wealth taxation in Germany.

However, it is not straightforward to calculate the precise share of those affected by the inheritance tax, even based on official data. The German official data on inheritance and gift taxation ("Erbschaft- und Schenkungsteuerstatistik") contain the number of taxable inheritances for each year. Also the number of deaths per year is known. But, in contrast to the estate tax, the inheritance tax is not levied at the level of the deceased person, but at the heir level. Hence, in case of 100 deaths and 100 taxable inheritances the share of affected can be between 100% (for a decedent-heir ratio of 1, i.e. every decedent passes his assets to only one heir) and 1% (for a decedent-ratio of 100; i.e. only one decedent passes his assets to 100 heirs). Furthermore, a tax on an inheritance is usually not determined in the same year of the decedents' death. This makes a direct assignment even more difficult. According to the "Statistisches Bundesamt" the number of deaths in Germany accounted for about 1 Mio. (939,520) in 2019.[2] At the same time, the number of taxable inheritances accounted for 122,905 in 2019.[3] Hence, even in the unrealistic case of only one heir per decedent, the share of those affected by the inheritance would be about 10% - 15%. Assuming three heirs per decedent already decreases this number to less than 5%.

Another way to estimate who is affected by the inheritance tax is to compare the basic tax allowances and the wealth distribution in Germany. While the minimum tax allowance only accounts for 20,000 €, each parent can transfer assets worth 400,000 € to each child without being taxed. Hence, inter-generational transfers of parents with 2 children are tax-free up to 1.6 Mio. €. Furthermore, additional tax exemptions apply if businesses or family homes are transferred. According to a recent study of the Bundesbank, only 10% of German households own assets worth more than 555,400 €. Even the top 5% own only about 850,000 €.[4] Therefore, we considered reported shares below 10% to be right.

### B.4.2 Textual Analysis of Respondents' Reasoning

We further asked our respondents to briefly reason their choice in an open-ended survey question. This helps us to understand the underlying motives for choosing one wealth tax instrument over the other. With a 99.2% response rate and an average length of 14 words, our respondents seem to take the task quite seriously. The mean length differs considerably among preferred taxes: preferences for a one-time wealth tax: 17.83 words, preferences for a yearly wealth tax: 12.78 words and no preference: 10.74 words.

To get a quick impression and structure of our answers, we use a Biterm Topic Model (BTM),

[2] https://de.statista.com/statistik/daten/studie/156902/umfrage/sterbefaelle-in-deutschland/.

[3] Only "unbeschränkt und festgesetzte Steuer": https://www.destatis.de/DE/Themen/Staat/Steuern/Weitere-Steuern/Tabellen/erbschaftsteuer-erbschaft-schenkungstatistik.html.

[4] https://www.bundesbank.de/resource/blob/794130/d523cb34074622e1b4cfa729f12a1276/mL/2019-04-vermoegensbefragung-data.pdf.

which is an unsupervised machine learning algorithm that learns topics by modelling word co-occurrence patterns. This is particularly useful for short text answers such as in open-ended survey questions.

To reduce low-quality answers, we (1) remove numeric and punctuation characters (2) convert letters into lower case and (3) remove answers with less than 3 words. By lemmatization of words we aim to reduce the inflectional forms of each word into a common base. This is very important for languages such as German that have so many different conjugations.

By setting the number of topics to 6, we find the following topics. The top 10 words of each topic are displayed in Table B8 and the word clusters are visualized in Figure B4.

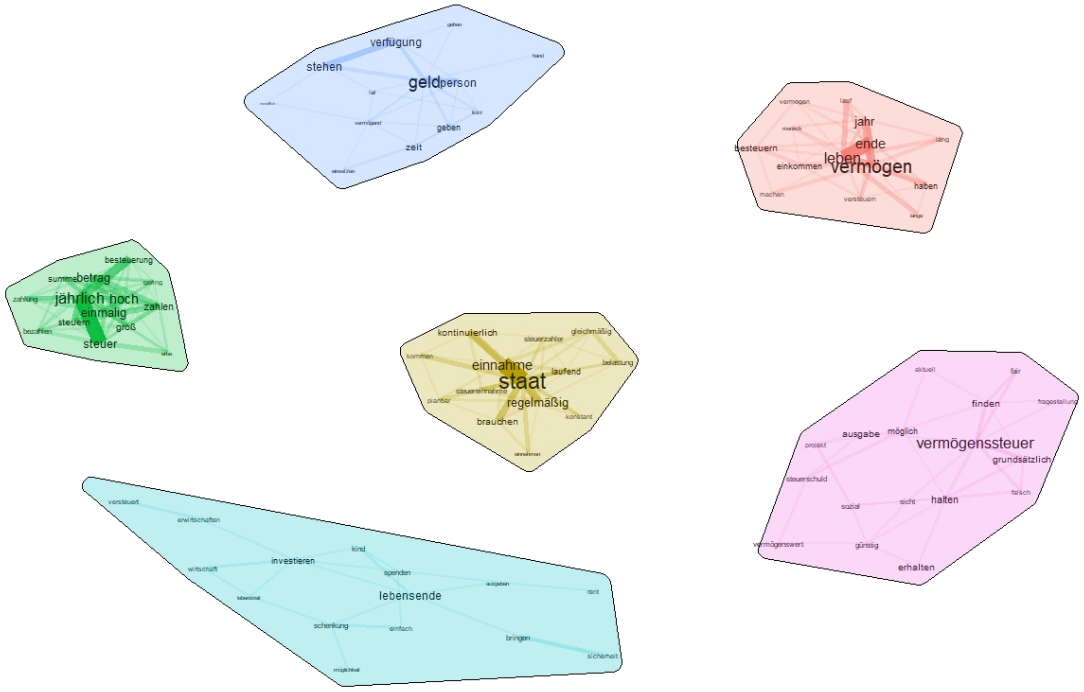
The most coherent topics revolve around the more manageable small payments ("Small Payments Easier" & "One-Time Payment Too Large") and predictable and regular tax revenues for the state ("Continuous Tax Revenue") in case of a yearly wealth tax. People who are in favor for the one-time wealth tax reason their choice that a yearly wealth tax would otherwise restrict their business opportunities ("Economic Freedom").

**Table B8:** Topics produced by BTM.

| **Label** | **Topic** | **Top 10 Words** |
|---|---|---|
| Small Payments Easier | 1 | wealth, life, end, year, money, tax, end-of-life, income, have, person |
| Continuous Tax Revenue | 2 | state, revenue, yearly, regular, tax, money, need, continuous, permanently, year |
| One-Time Payment Too Large | 3 | yearly, large, tax, amount, once, wealth, pay, tax, end-of-life, big |
| Possible Tax Avoidance | 4 | wealth, end-of-life, money, big, invest, general, donate, bring, children, donation |
| Economic Freedom | 5 | money, state, person, available, stand, year, yearly, time, give, large |
| Fair Taxation | 6 | wealth tax, have, find, expense, general, yearly, receive, possible, fair, cheap |

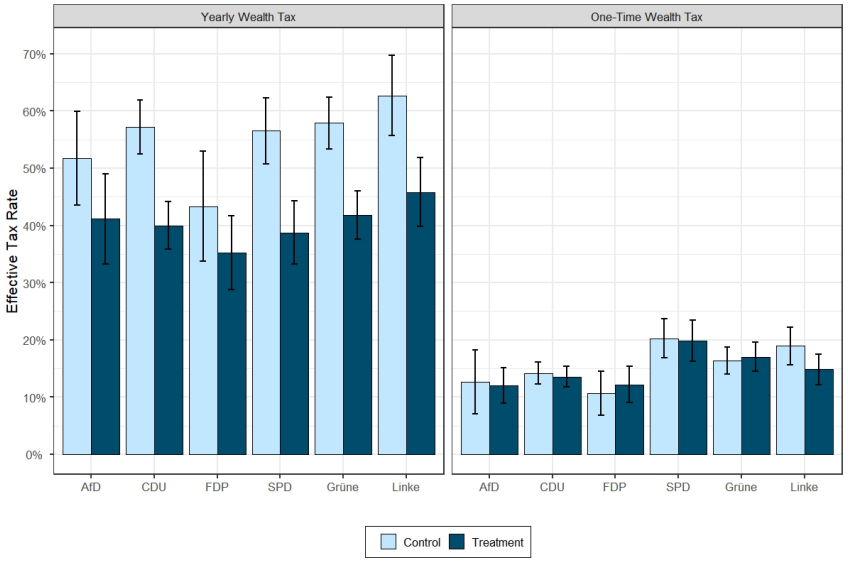
Note: This table presents produced topics by biterm model (BTM). We run a biterm model with 6 topics and one background topic to filter out common words.

**Figure B4:** BTM Topic Clusters.



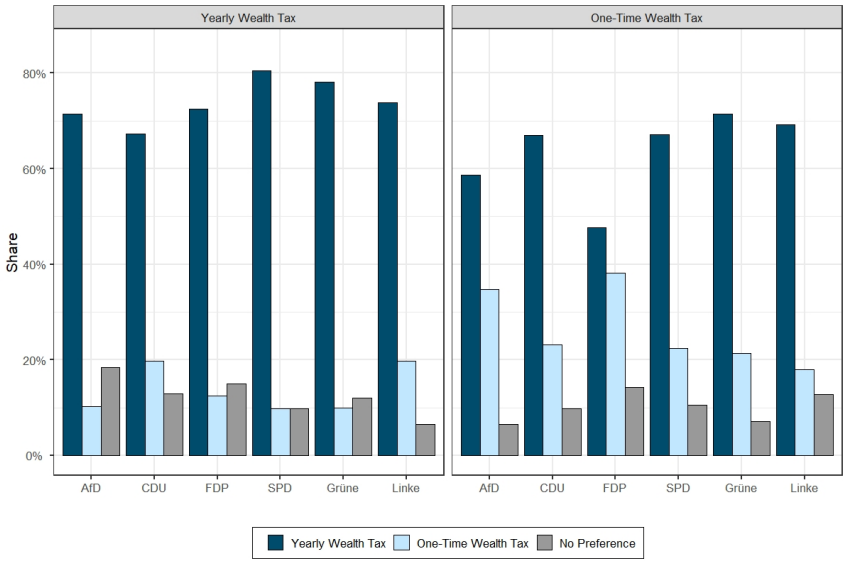

## B.4.3 Party Preferences

**Figure B5:** Baseline Results by Party.




Note: Effective tax rates by experimental group and party with 95% confidence bars.

**Figure B6:** Within Choice by Party.




Note: Share of chosen tax instrument by experimental groups and party.

# Appendix C

## C.1 Experimental Design

### C.1.1 Instructions Trial Stage

Below you see a 10x15 table with zeros and ones. We would like you to count the numbers of zeros. **Only if you entered the right amount, you will be able to proceed to the next page**. **Please count the numbers of zeros for two subsequent tables on the next pages.** This will familiarize you with the task in order to indicate how many of these tables you want to work on in the second part of this HIT. You will be paid per table in the second part of our study.

### C.1.2 Instructions Evasion-Treatment

Good job! It took you on average [**seconds needed in trial**] seconds per table.

Now, we would like to know how many of these tables you would like to work on in the second part of this study.
To do so, we want you to indicate your preferred number of tables for **eight different payment schemes. After taking your eight decisions, you will have to work on your decision for only one randomly picked payment scheme!**
These payment schemes differ in:

- The payment per table and
- A proportionate fee you have to pay to the requester - i.e., how much of your earned money per table you can keep.

**You can also avoid the payment of the fee.** To do so, you can participate in a **lottery**. The lottery is represented by a fair coin toss – you have a 50% chance to win, and you have a 50% chance to lose. On any amount **not invested** in the lottery you will need to pay the fee. For any amount **invested** in the lottery the following holds:

- **If you win:** you avoid paying any fee on the invested amount.
- **If you lose:** you will need to pay the fee on the invested amount plus an additional fee of 20% on the money invested.

Thus, you will need to make two decisions in this part of the study:

1. How many tables you would like to solve (which will result in a gross payment).
2. How much money of the gross payment you want to invest in a fair lottery (i.e on which part of your income you would like to attempt to avoid the fees).

You can freely decide on how many tables you would like to work on, with at most 60 tables. Thus, you can also decide not to work at all and correspondingly you would obtain only the payment for the mini-experiment and the $0.50 fixed payment.
As already mentioned you will make the two decisions under **8 different payment schemes**. The payment schemes differ in the **payment per table** and the **proportionate fee**.
**The payment per table** will be either $0.12 or $0.25 for every correctly solved table. Each of these two levels is shown in a block of 4 scenarios (e.g. Block I: $0.12 and Block II: $0.25). Thus, you can earn between additional $0 (if you decide to work on 0 tables) and maximally $15 (if you decide on 60 tables) gross.
The **proportionate fee** will be either 20%, 40%, 60% or 80%. Each of these four levels will be shown in both blocks (= eight scenarios). The proportionate fee indicates how much of your gross earnings you will be effectively paid by the end of the experiment.
For example: if the proportionate fee is 60% you will obtain only 40% of your gross income. So if you earned $5 gross, you would obtain only $5*40/100=$2 by the end of the experiment.
*Strategic Advice:*
*Given the odds of the lottery (50% / 50%) and the additional fee of 20% if the lottery is lost, it might not be profitable to invest in the lottery if the regular fee is low (20%). However, with higher regular fees (40%, 60% or 80%) investments into the lottery might be profitable.*
**At the end of the experiment only ONE of the 8 different payment schemes will be made payoff-relevant for you. You will be informed about which one is payoff-relevant before working on the tables.**

### C.1.3 Instructions NoEvasion-Lottery-Treatment

Good job! It took you on average [**seconds needed in trial**] seconds per table.

Now, we would like to know how many of these tables you would like to work on in the second part of this study.

To do so, we want you to indicate your preferred number of tables for **eight different payment schemes. After taking your eight decisions, you will have to work on your decision for only one randomly picked payment scheme!**

These payment schemes differ in:

- The **payment per table** and
- A proportionate **fee** you have to pay to the requester - i.e., how much of your earned money per table you can keep.

You can participate in a lottery to avoid the fees. **However, you have a 100% chance to lose**. On any amount **not invested** in the lottery you will need to pay the fee. For any amount **invested** in the lottery you will need to pay the fee on the invested amount plus an additional fee of 20% on the money invested.

Thus, you will need to make two decisions in this part of the study:

1. How many tables you would like to solve (which will result in a gross payment).
2. How much money of the gross payment you want to invest in **the lottery you will always lose**.

You can freely decide on how many tables you would like to work on, with at most 60 tables. Thus, you can also decide not to work at all and correspondingly you would obtain only the payment for the mini-experiment and the $0.50 fixed payment.

As already mentioned you will make the two decisions under **8 different payment schemes**. The payment schemes differ in the **payment per table** and the **proportionate fee**.

**The payment per table** will be either $0.12 or $0.25 for every correctly solved table. Each of these two levels is shown in a block of 4 scenarios (e.g. Block I: $0.12 and Block II: $0.25). Thus, you can earn between additional $0 (if you decide to work on 0 tables) and maximally $15 (if you decide on 60 tables) gross.

The **proportionate fee** will be either 20%, 40%, 60% or 80%. Each of these four levels will be shown in both blocks (= eight scenarios). The proportionate fee indicates how much of your gross earnings you will be effectively paid by the end of the experiment.

For example: if the proportionate fee is 60% you will obtain only 40% of your gross income. So if you earned $5 gross, you would obtain only $5*40/100=$2 by the end of the experiment.

*Strategic Advice:*

*Given the odds of the lottery (i.e. you will never win) and the additional fee of 20%, it is not profitable to invest into the lottery because you would definitely lose money.*

**At the end of the experiment only ONE of the 8 different payment schemes will be made payoff-relevant for you. You will be informed about which one is payoff-relevant**

**before working on the tables.**

### C.1.4 Instructions NoEvasion-NoLottery-Treatment

Good job! It took you on average [**seconds needed in trial**] seconds per table.

Now, we would like to know how many of these tables you would like to work on in the second part of this study.

To do so, we want you to indicate your preferred number of tables for **eight different payment schemes. After taking your eight decisions, you will have to work on your decision for only one randomly picked payment scheme!**

These payment schemes differ in:

- The payment per table and
- A proportionate fee you have to pay to the requester - i.e., how much of your earned money per table you can keep.

You can freely decide on how many tables you would like to work on, with at most 60 tables. Thus, you can also decide not to work at all and correspondingly you would obtain only the payment for the mini-experiment and the $0.50 fixed payment.

As already mentioned you will make the two decisions under **8 different payment schemes**. The payment schemes differ in the **payment per table** and the **proportionate fee**.

**The payment per table** will be either $0.12 or $0.25 for every correctly solved table. Each of these two levels is shown in a block of 4 scenarios (e.g. Block I: $0.12 and Block II: $0.25). Thus, you can earn between additional $0 (if you decide to work on 0 tables) and maximally $15 (if you decide on 60 tables) gross.

The **proportionate fee** will be either 20%, 40%, 60% or 80%. Each of these four levels will be shown in both blocks (= eight scenarios). The proportionate fee indicates how much of your gross earnings you will be effectively paid by the end of the experiment.

For example: if the proportionate fee is 60% you will obtain only 40% of your gross income. So if you earned $5 gross, you would obtain only $5*40/100=$2 by the end of the experiment.

**At the end of the experiment only ONE of the 8 different payment schemes will be made payoff-relevant for you. You will be informed about which one is payoff-relevant before working on the tables.**

### C.1.5 Screenshots

**Figure C1:** Screenshot of the Real Effort Task.

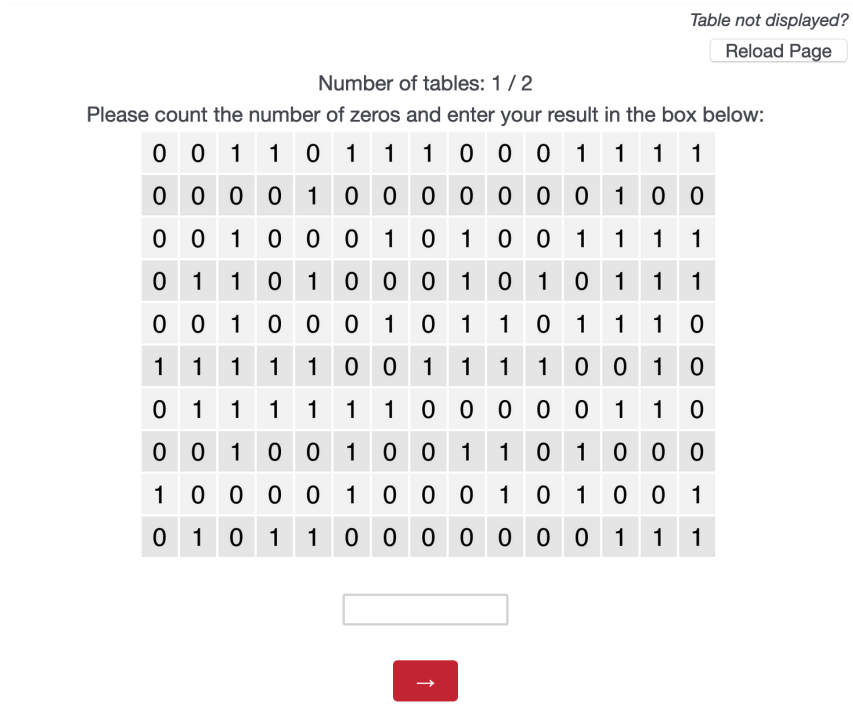



**Figure C2:** Screenshot of the Evasion-Treatment.

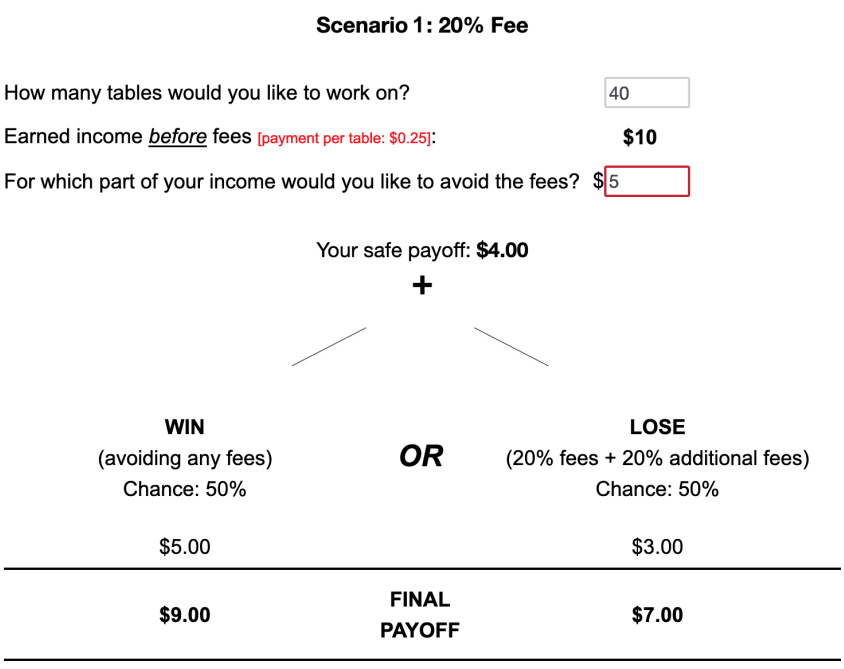


**Figure C3:** Screenshot of the NoEvasion-Lottery-Treatment.

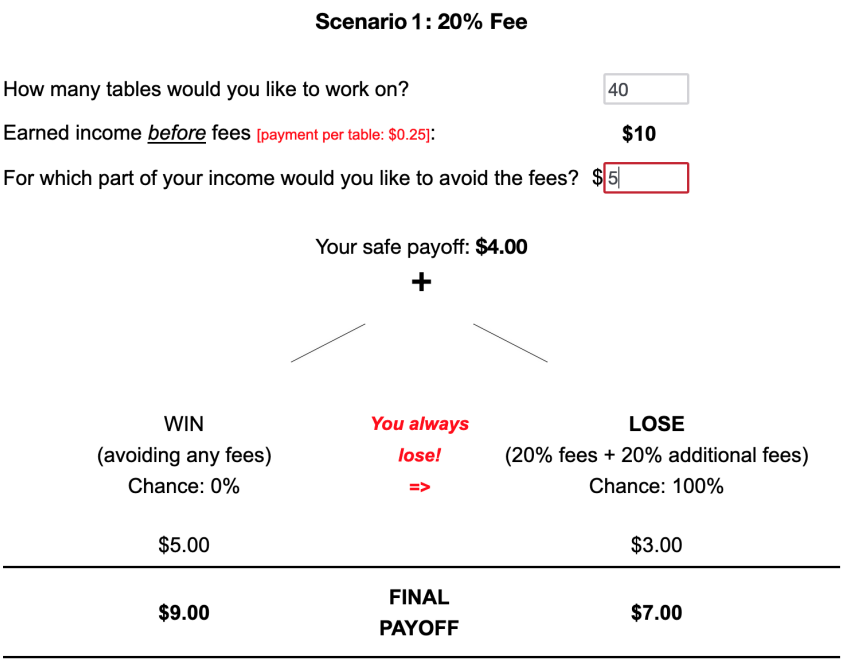
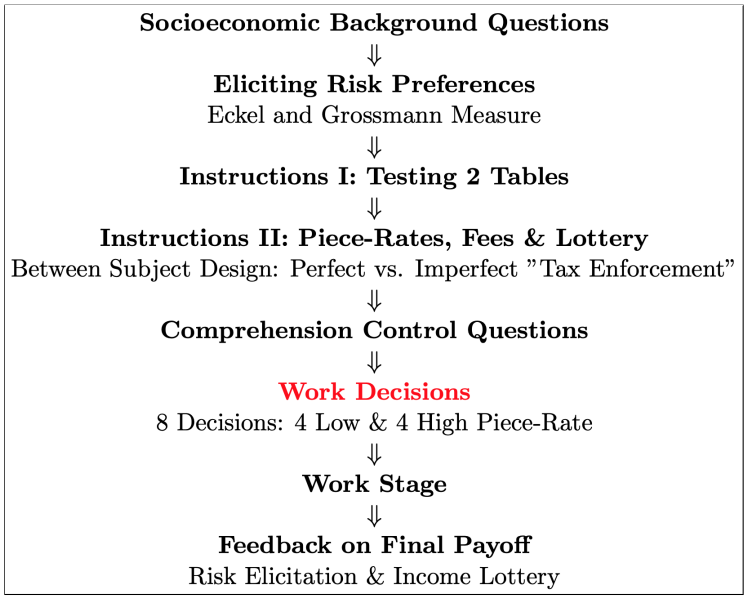



**Figure C4:** Sequence of the Experimental Procedure.




## C.2 Characteristics of Participants

Table C1 investigate whether subjects' characteristics are balanced across treatments. Table C1 shows and compares all characteristics of all subjects across treatments. It is straightforward that all characteristics are evenly balanced across treatments – no treatment differs in any characteristic from the other treatments. Thus, random assignment of subjects to the treatments worked nicely.

The only noticeable difference is how often subjects missed the control questions. In the treatment without any lottery subjects on average needed fewer approaches to answer the question, but it is noteworthy that this treatment (due to its reduced complexity) also had fewer control questions.

**Table C1:** Characteristics of Subjects in the Three Treatments.

| | Evasion | | NoEvasion-Lottery | | NoEvasion-NoLottery | | Any Sign. Differences? |
|---|---|---|---|---|---|---|---|
| | $\overline{M}$ | $CI_{95\%}$ | $\overline{M}$ | $CI_{95\%}$ | $\overline{M}$ | $CI_{95\%}$ | |
| Age | 39.68 | [38.60,40.76] | 40.34 | [38.54,42.14] | 40.46 | [38.95,41.97] | × |
| Female | 00.51 | [00.47,00.55] | 00.51 | [00.45,00.57] | 00.56 | [00.50,00.62] | × |
| HighDegree | 00.69 | [00.65,00.73] | 00.69 | [00.63,00.75] | 00.65 | [00.59,00.71] | × |
| Employed | 00.75 | [00.71,00.79] | 00.78 | [00.72,00.84] | 00.71 | [00.65,00.77] | × |
| HighIncome | 00.52 | [00.48,00.56] | 00.55 | [00.49,00.61] | 00.50 | [00.44,00.56] | × |
| Democrats | 00.57 | [00.53,00.61] | 00.51 | [00.45,00.57] | 00.49 | [00.43,00.55] | × |
| White | 00.80 | [00.76,00.84] | 00.81 | [00.75,00.87] | 00.79 | [00.75,00.83] | × |
| Risk | 05.26 | [05.04,05.48] | 04.76 | [04.43,05.09] | 04.90 | [04.63,05.17] | × |
| HoursOnlineWork | 17.31 | [15.88,18.74] | 16.14 | [14.47,17.81] | 17.36 | [15.95,18.77] | × |
| N | 510 | | 206 | | 280 | | |

Note: We use two-sample t-tests to compare all characteristics. In particular, we test every treatment against every other treatment. × denotes no significant differences between any treatments on the particular dimension. ✓ denotes a significant difference in at least one of the comparisons at a 5% level on the particular dimension. We use the Benjamini and Hochberg (1995) p-value adjustment. $\overline{M}$ denotes the mean of the respective characteristic. $CI_{95\%}$ denotes the 95% confidence interval. *Age* denotes a continuous variable on the age of the participants. *Female* denotes a dummy with value one if the participant is female. *HighDegree* denotes a dummy with value one if the participant has at least a College-degree. *Employed* denotes a dummy with value one if the participant indicated to be either full time or half-time employed. *HighIncome* denotes a dummy with value one if the participant lives in a household with at least a yearly income of 75k. *Democrats* denotes a dummy with value one if the participant indicated to vote for Democrats. *White* denotes a dummy with value one if the participant indicated to be Caucasian. *Risk* denotes a continuous variable on the risk preferences indicated in the -task. *HoursOnlineWork* denotes a continuous variable on the indicated hours the participant works online.

### C.2.1 Payment

The average time participants needed to finsih our experiment was 38 minutes. For this, they earned a respective hourly wage of $8.88. Taking into account the ability of subjects (i.e. how fast subjects were able to solve the two sample tasks) we can calculate the maximum amount of tables subjects would have been able to work on in an hour. For a given hour the average number of tables subjects could work on are 50.5 [25% quantile=32.7; 75% quantile= 64.52]. Thus, the maximum payoff per hour on average in case of low wage was $6.06 if no tax would have to be paid; the maximum payoff per hour under high wage was: $12.63. Under low wage and 20%, vs. 80% tax the maximum payoff per hour was accordingly $4.85 vs. $1.21. Under high wage and 20%, vs. 80% tax the maximum payoff per hour was accordingly $10.1 vs. $2.53.

Looking more specifically at low ability workers (subjects needing on average more time to finish the two sample tasks than the median subject) we see that they would obtain on average a maximum payoff per hour of $3.85 under the low wage situation and $8.02 under the high wage

situation if no taxes would need to be paid. Looking at high ability workers (subjects requiring on average less time to finish the two sample tasks than the median subject) we see that they would obtain on average a maximum payoff per hour of $8.27 under the low wage situation and $17.23 under the high wage situation if no taxes would need to be paid.

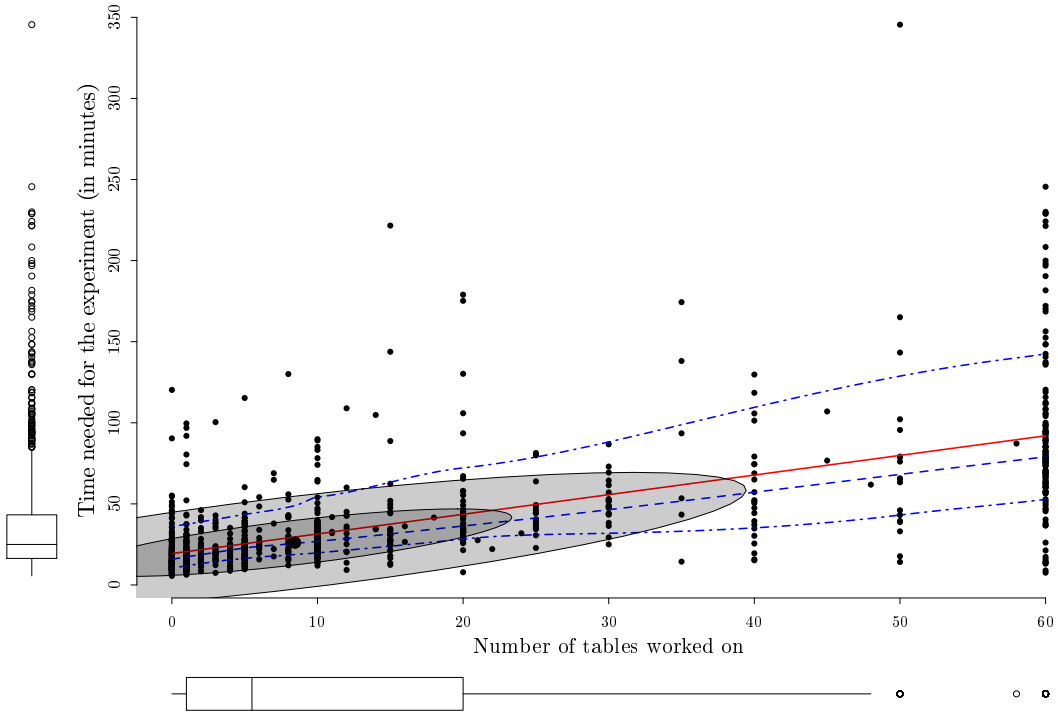
Comparing that to a target payment of about $6 per hour for typical US-based MTurkers[1] we can see that high ability workers would be willing to work up to a tax of 65% under the high wage situation and up to a tax of 27% under the low wage situation. Low ability workers under the high wage situation would be willing to work up to a tax of 25% and up to a tax of -56% under the low wage situation. Thus, the incentives are structured such that there is sufficient room for labor supply responses (i.e. payoffs are better than typically for Mturkers) but also such that not all situations are worthwhile.

### C.2.2 Number of Worked Tables and Time Spent

An apparent discrepancy between our experiment and the theory we are building on is that we measure labor supply in the number of tasks solved whereas the theory uses time of work. Here we show that these two measures are empirically equivalent in the sense that there is a clear strong positive relationship between the number of tasks solved and the time spend on them as seen in Figure C5. Running a OLS reveals a strong and highly significant association ($\beta$=72.64,t(996)=28.02, $p \leq 0.001$, cohen's d=0.89) between the number of tables to be solved and the time needed to finish the experiment. Specifically, we see that participants needed on average 72.64 seconds longer to finish the experiment for each additional table worked.

[1] See Berg (2015).

**Figure C5:** Tables Worked and Time Spent.

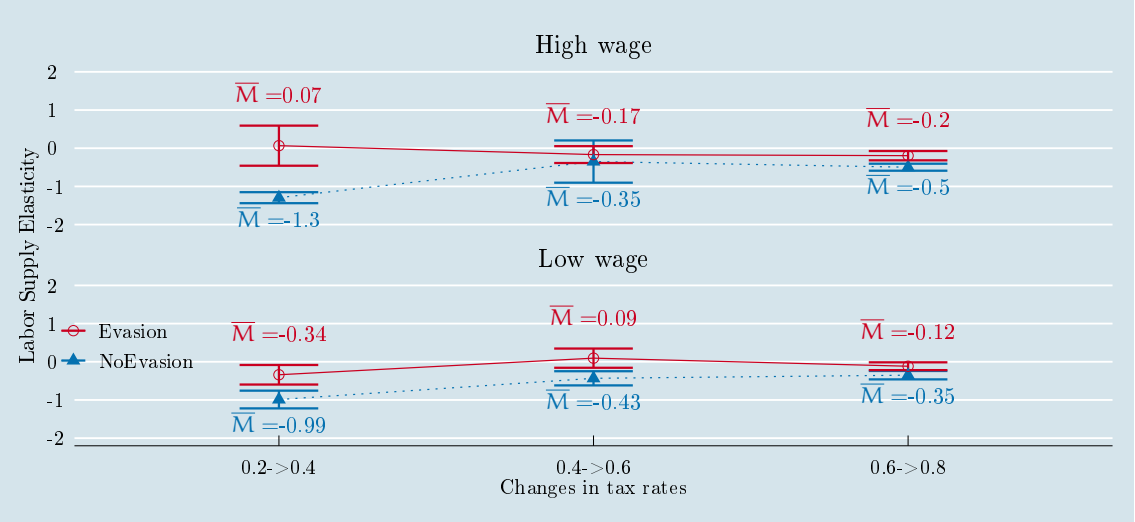



Note: The figure shows the association between the number of tables worked on (on situation is randomly realized for participants and the corresponding decision is the number of tables assigned to work on). The x-axis denotes the number of tables worked on by participants, while the y-axis denotes the time needed overall to finish the whole experiment (in minutes). For each of the two measure the marginal boxplots are shown next to the respective axis. The black dotes in the main part of the figure denote individual observations. The red line depicts the linear regression line, while the blue line denotes the loess smoothing estimation with the corresponding confidence intervals. The gray bubbles denote data-concentration ellipses.

## C.3 Additional Analyses

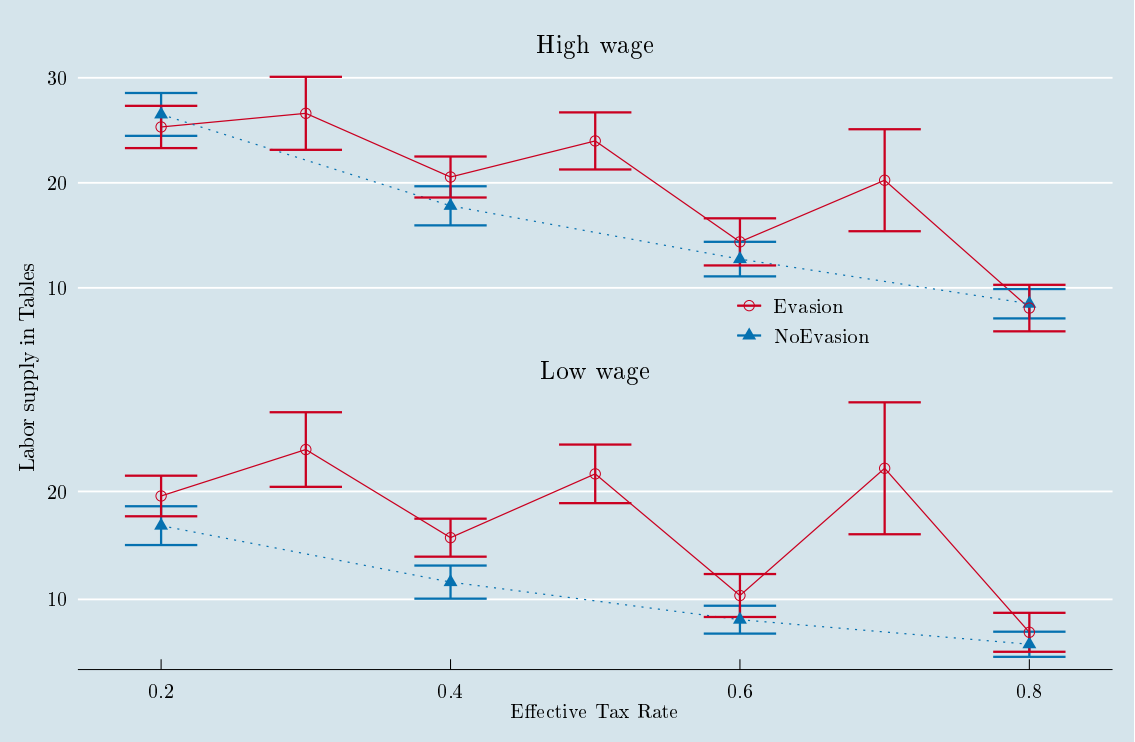
### C.3.1 Labor Supply

**Figure C6:** Elasticity of Labor Supply.




Note: Labor supply elasticity as a function of tax-change with 95% confidence intervals for the two treatments by wage level. Participants with a labor supply of 0 in a respective tax rate were excluded from the calculations. Blue, dashed lines represent the NoEvasion-treatment, while red, solid lines represent the Evasion-treatments. The corresponding tunnels surrounding the respective dots represent the 95% confidence intervals.

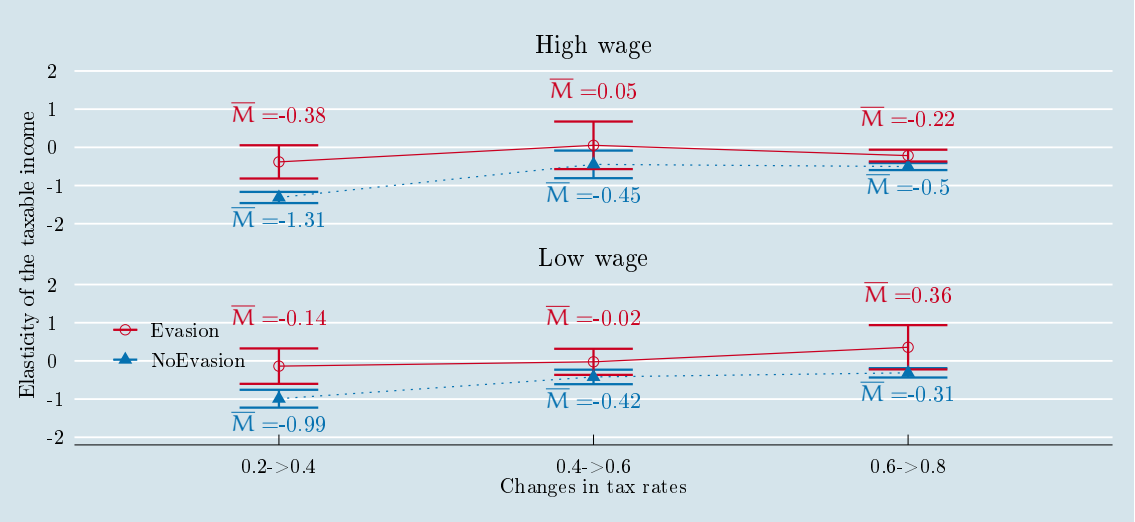
**Figure C7:** Labor Supply as a Function of the Effective Tax Rate.




Note: Labor supply as a function of the effective tax rate (exp. tax divided by the net income) with 95% confidence intervals split by treatment. Blue, dashed lines represent the NoEvasion-treatments, while red, solid lines represent the Evasion-treatment. The upper panel depicts the high wage situation while the panel at the bottom depicts the low wage situation. The corresponding tunnels surrounding the respective dots represent the 95% confidence intervals.

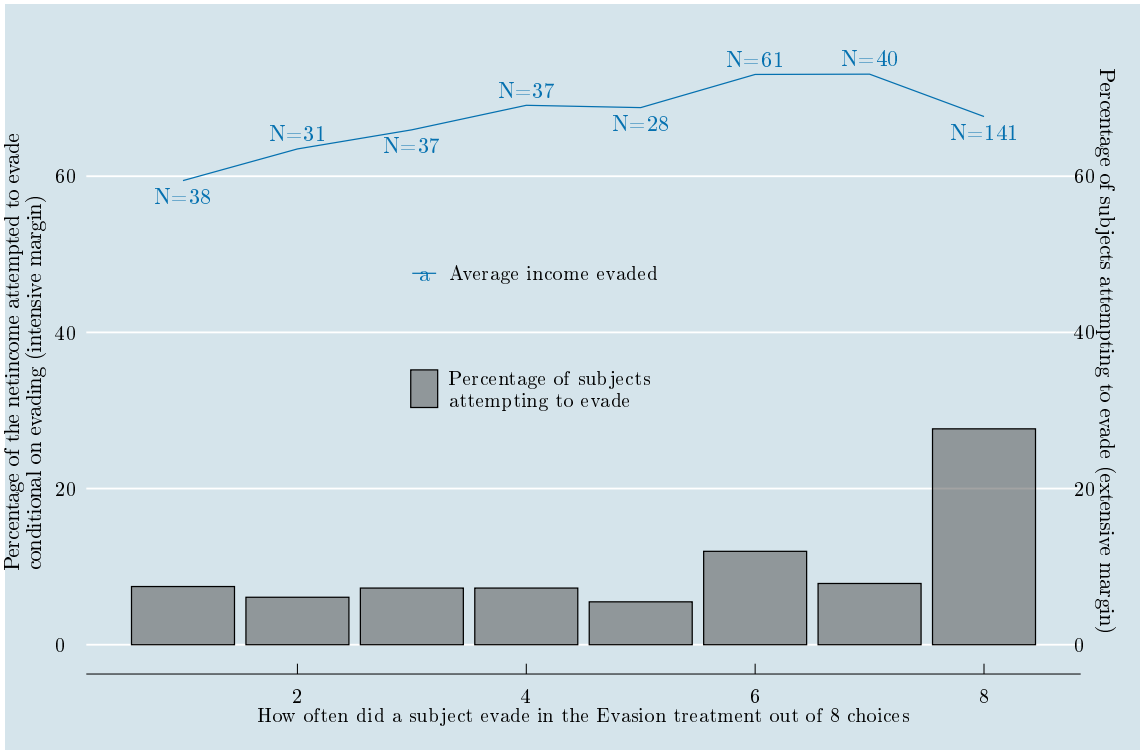
### C.3.2 Evasion Decision

**Figure C8:** Elasticity of Taxable Income.



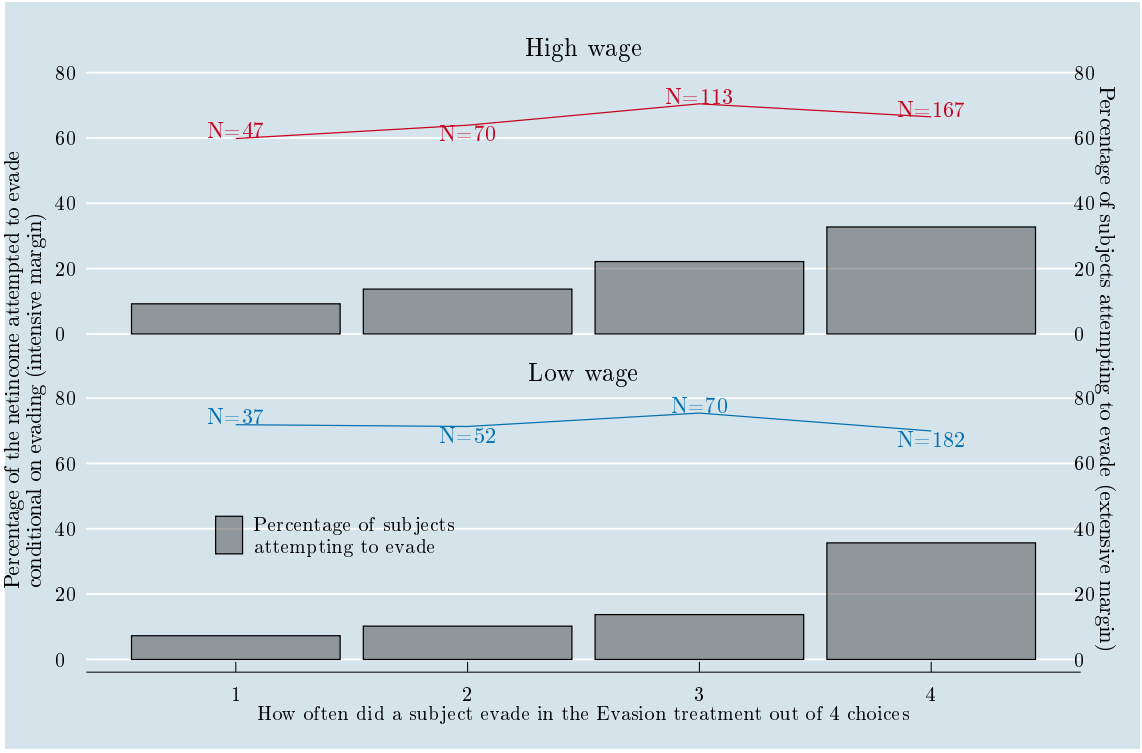

Note: Elasticity of the taxable income as a function of tax-change with 95% confidence intervals for the two treatments by wage level. Participants with a labor supply of 0 in a respective tax rate were excluded from the calculations. Blue, dotted lines represent the NoEvasion-treatment, while red, solid lines represent the Evasion-treatments. The corresponding tunnels surrounding the respective dots represent the 95% confidence intervals.

**Figure C9:** Individual Evasion Decisions; Extensive and Intensive Margin.




Note: Blue, solid lines represent the percentage of the net income attempted to evade conditional on evading at all. The corresponding numbers indiciate the number of people evading one, two, etc. times. The grey bars depict the percentage of subjects attempting to evade one, two, etc. times (i.e. the extensive margin).

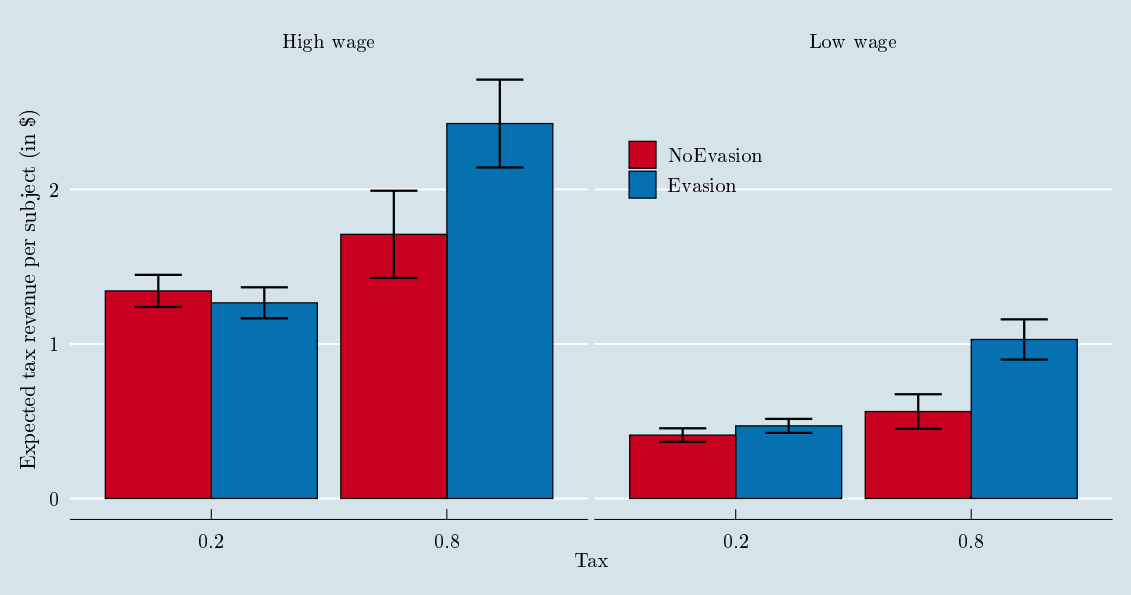
**Figure C10:** Individual Evasion Decisions by Wage Level.




Note: Blue/red, solid lines represent the percentage of the net income attempted to evade conditional on evading at all in the high/low wage situation. The corresponding numbers indicate the number of people evading one, two, etc. times.
The grey bars depict the percentage of subjects attempting to evade one, two, etc. times (i.e. the extensive margin).

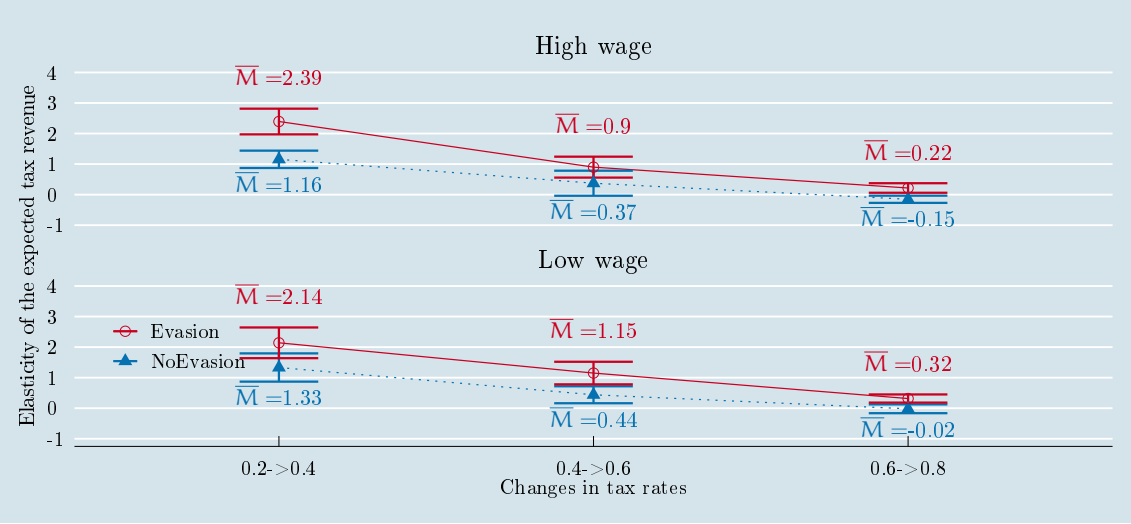
### C.3.3 Tax Revenues

**Figure C11:** Box-Plot of Expected Rax Revenues by Treatment.




Note: The left panel shows the high-wage situations while the right panel shows the low-wage situations. Blue bars depict the NoEvasion-treatments, while red bars depict the Evasion-treatment. The corresponding tunnels on top of the bars represent the 95% confidence intervals.

**Figure C12:** Elasticity of Tax Revenue.




Note: Elasticity of the tax revenue as a function of tax-change with 95% confidence intervals for the two treatments by wage level. Participants with a labor supply of 0 in a respective tax rate were excluded from the calculations. Blue, dotted lines represent the NoEvasion-treatment, while red, solid lines represent the Evasion-treatments. The corresponding tunnels surrounding the respective dots represent the 95% confidence intervals.

**Table C2:** Cubic Mixed-Effects Model of Expected Tax Revenue.

| | Expected Tax revenue per subject (in $) | | | | | |
|---|---|---|---|---|---|---|
| | Low-Wage | | | High-Wage | | |
| Constant | 0.53*** (0.04) | 0.44*** (0.09) | 0.58*** (0.17) | 1.69*** (0.10) | 1.29*** (0.20) | 2.06*** (0.41) |
| as.factor(Treatment2)Evasion | 0.24*** (0.06) | 0.29* (0.12) | 0.27* (0.12) | 0.30* (0.13) | 0.53· (0.29) | 0.51· (0.29) |
| poly(Tax2, 2, raw = FALSE)1 | 3.42*** (0.81) | −0.56 (1.74) | −0.56 (1.74) | 8.62*** (2.03) | 4.91 (4.35) | 4.91 (4.35) |
| poly(Tax2, 2, raw = FALSE)2 | −2.79*** (0.81) | −3.27· (1.74) | −3.27· (1.74) | −10.57*** (2.03) | −9.85* (4.35) | −9.85* (4.35) |
| as.factor(Treatment2)Evasion:poly(Tax2, 2, raw = FALSE)2 | 1.28 (1.14) | 0.95 (2.49) | 0.95 (2.49) | 0.98 (2.84) | −0.61 (6.22) | −0.61 (6.22) |
| as.factor(Treatment2)Evasion:poly(Tax2, 2, raw = FALSE)1 | 9.65*** (1.14) | 11.87*** (2.49) | 11.87*** (2.49) | 18.85*** (2.84) | 21.31*** (6.22) | 21.31*** (6.22) |
| as.factor(Treatment2)Evasion:Risk | | −0.02 (0.03) | −0.01 (0.03) | | −0.07 (0.08) | −0.07 (0.08) |
| Risk | | 0.03 (0.02) | 0.03 (0.02) | | 0.12* (0.05) | 0.11* (0.05) |
| poly(Tax2, 2, raw = FALSE)1:Risk | | 1.22** (0.47) | 1.22** (0.47) | | 1.13 (1.18) | 1.13 (1.18) |
| poly(Tax2, 2, raw = FALSE)2:Risk | | 0.15 (0.47) | 0.15 (0.47) | | −0.22 (1.18) | −0.22 (1.18) |
| as.factor(Treatment2)Evasion:poly(Tax2, 2, raw = FALSE)1:Risk | | −0.73 (0.64) | −0.73 (0.64) | | −0.79 (1.61) | −0.79 (1.61) |
| as.factor(Treatment2)Evasion:poly(Tax2, 2, raw = FALSE)2:Risk | | 0.08 (0.64) | 0.08 (0.64) | | 0.46 (1.61) | 0.46 (1.61) |
| Controls | × | × | ✓ | × | × | ✓ |
| Sbj specific effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 3,984 | 3,984 | 3,984 | 3,984 | 3,984 | 3,984 |
| Log Likelihood | −4,536.59 | −4,536.76 | −4,555.62 | −8,119.52 | −8,115.88 | −8,126.36 |
| Akaike Inf. Crit. | 9,089.18 | 9,101.52 | 9,157.24 | 16,255.04 | 16,259.76 | 16,298.72 |
| Bayesian Inf. Crit. | 9,139.50 | 9,189.58 | 9,301.91 | 16,305.36 | 16,347.83 | 16,443.39 |

*Notes:* ·p < 0.1;*p < 0.05;**p < 0.01;***p < 0.001;

Note: Here we use an orthogonal polynomial regression with degree two. Controls include age, gender, ethnicity, income, party affiliation, employment status, education, hours spent on online work, and the average time needed for solving the two sample tasks.Evasion denotes a dummy with value one if the participant was in the Evasion-treatment – participants have the opportunity to evade taxes and punishment will be meted with a 50% probability – and zero otherwise. The omitted category is the NoEvasion-treatment. *Tax* denotes a one percentage point increase in the tax level. *Risk* denotes the elicited risk aversion with higher values indicating more risk-lovingness. Errors are clustered on the subject level, i.e., subject-specific effects do account for subject-specific heterogeneity.

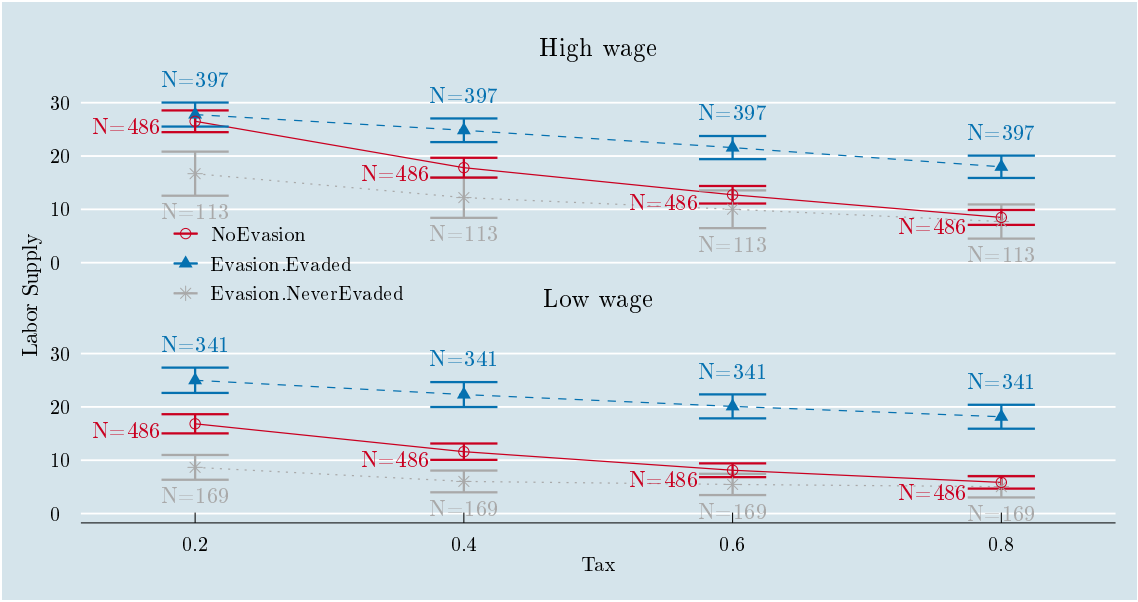
### C.3.4 Channels

In this section we investigate whether there is a differential effect of tax-evasion on labor supply.

#### Self-Selection

First, we compare the labor supply of those subjects in the Evasion-treatment who never evaded and those who evaded at least once against the subjects who were not able to evade (NoEvasion-treatments). Figure C13 depicts for both wage-levels the labor supply as a function the tax for these three situations. We clearly see that subjects who never evaded behave rather differently compared to those who evaded at least once. These subjects had a substantially lower labor supply even for a tax of 20%. It is also evident that the behavior of those who decided not to evade and those who could not evade converged at a tax level of 60% and it is also evident that the labor supply of those who did evade was substantially higher compared to those who could not evade.

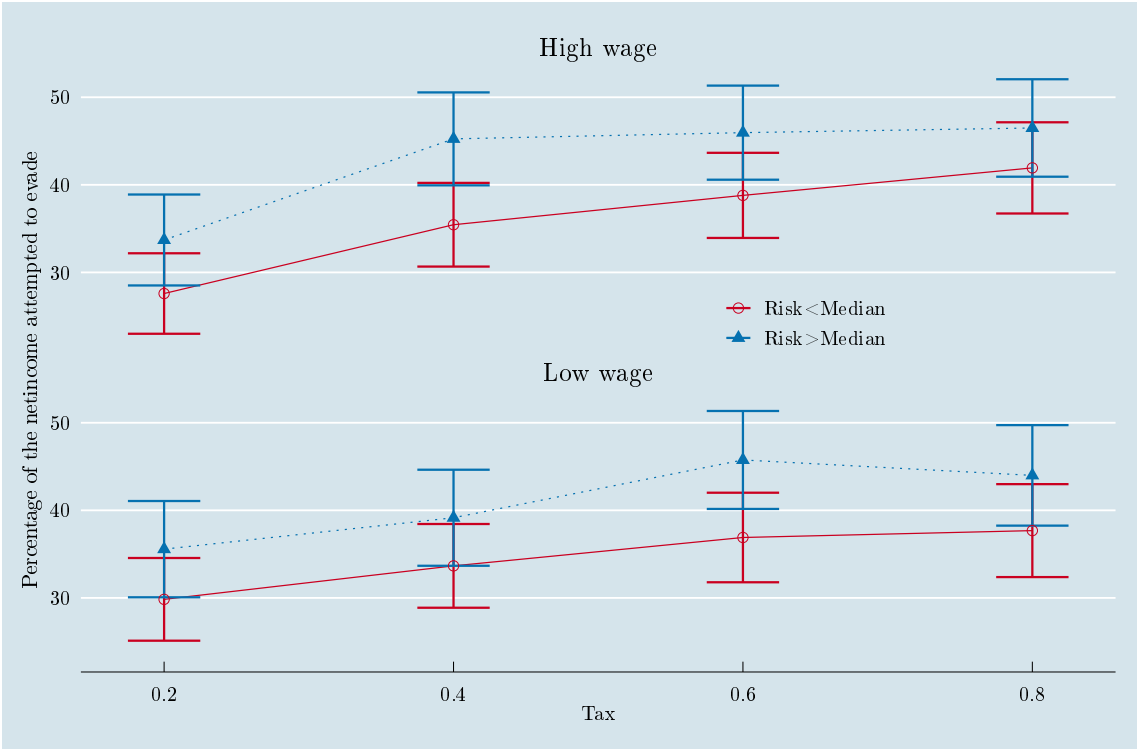
**Figure C13:** Labor Supply of Strict Non-Evaders.




Note: Grey, dotted lines represent the Evasion-treatment where subjects never evaded (i.e. across all tax levesls); red, solid lines represent the NoEvasion-treatments; blue, dashed lines represent the Evasion-treatment where subjects evaded for at least one tax level. The corresponding tunnels surrounding the respective dots represent the 95% confidence intervals.

### The Role of Personal Traits

**Figure C14:** Income Evaded by Risk Aversion (Median Split) and Wage Level.




Note: Blue, dashed lines represent the percentage of the net income attempted to evade by subjects with risk measures above the median , while red, solid lines represent the percentage of the net income attempted to evade by subjects with risk measures below (or equal to) the median. The upper panel depicts the high wage situation while the panel at the bottom depicts the low wage situation. The corresponding tunnels surrounding the respective dots represent the 95% confidence intervals.

**Figure C15:** Average Income Evaded by Risk Aversion (Median Split).

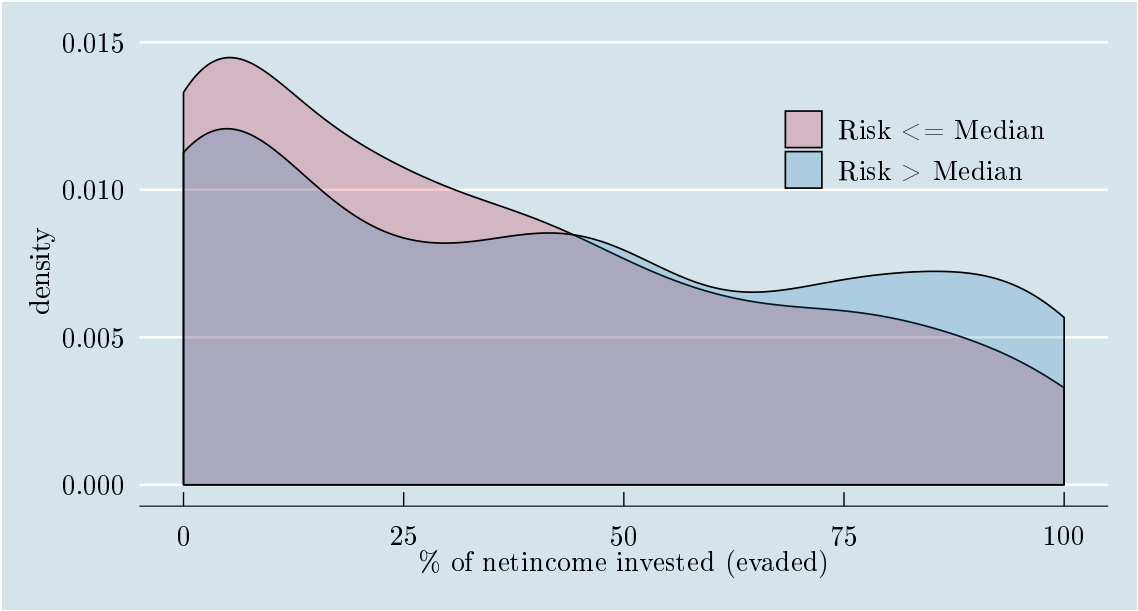
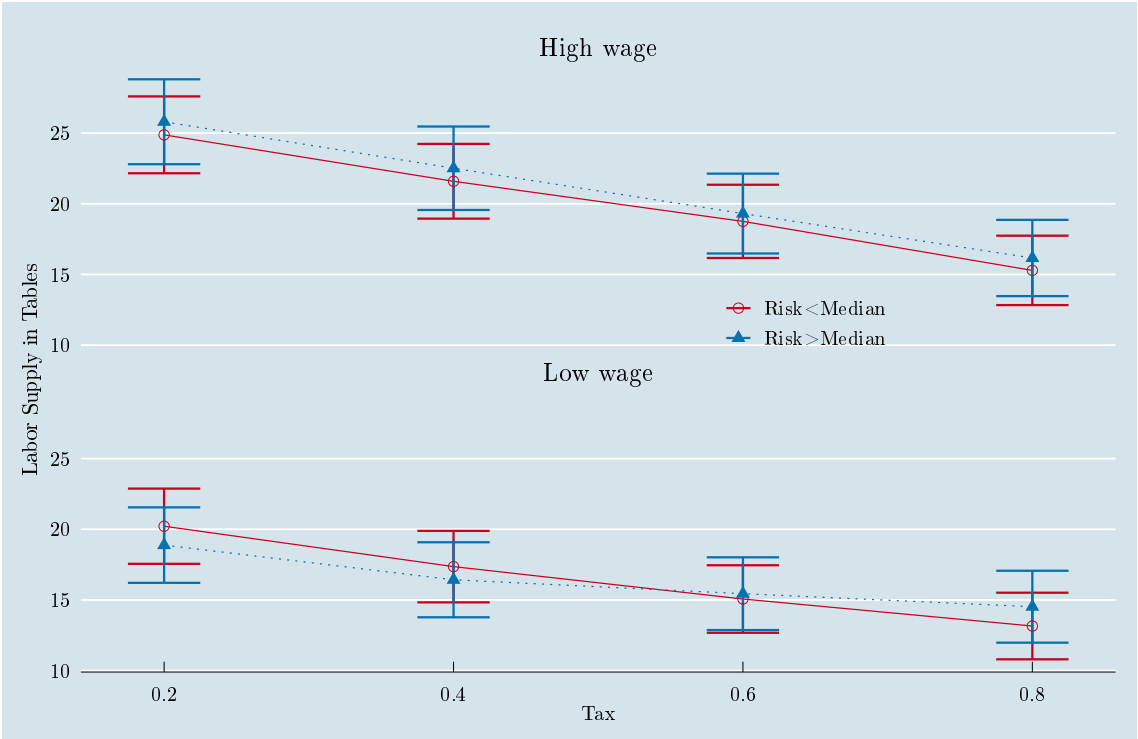



**Figure C16:** Labor Supply by Risk Aversion (Median Split) and Wage Level.




Note: Blue, dashed lines represent the labor supply by subjects with risk measures above the median , while red, solid lines represent the labor supply by subjects with risk measures below (or equal to) the median. The upper panel depicts the high wage situation while the panel at the bottom depicts the low wage situation. The corresponding tunnels surrounding the respective dots represent the 95% confidence intervals.

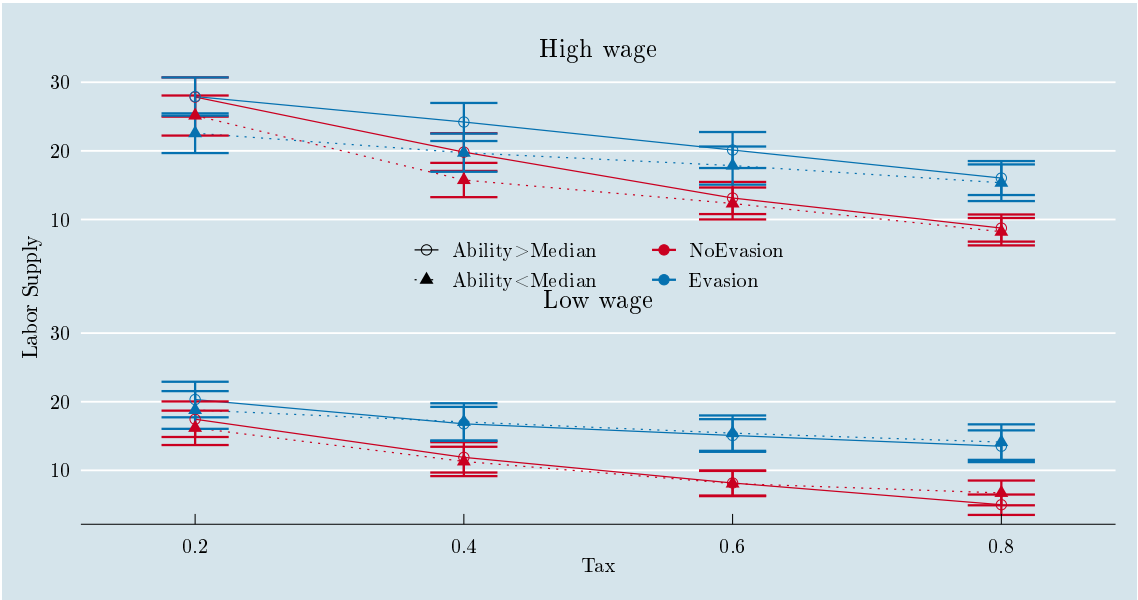
**Table C3:** Zero Inflated Normal Regression of Investment Behavior as a Function of Risk.

| | Extensive margin (i.e. Invest>0) | | | | | | | | Intensive margin (i.e. Invest\|Invest>0) | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Low wage | | | | High wage | | | | Low wage | | | | High wage | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) | (15) | (16) |
| Constant | −0.22 | 0.08 | −0.03 | −0.003 | −0.36· | 0.19 | 0.44* | 0.35· | 1.55*** | 1.42*** | 1.27*** | 1.09*** | 3.46*** | 2.07*** | 1.98*** | 1.90** |
| | (0.20) | (0.20) | (0.20) | (0.20) | (0.20) | (0.20) | (0.21) | (0.20) | (0.34) | (0.32) | (0.31) | (0.31) | (0.72) | (0.62) | (0.59) | (0.63) |
| Risk | 0.04 | 0.02 | 0.08 | 0.05 | 0.06 | 0.08· | 0.04 | 0.03 | 0.11 | 0.13· | 0.17* | 0.20** | 0.22 | 0.57*** | 0.51*** | 0.55*** |
| | (0.05) | (0.05) | (0.05) | (0.05) | (0.05) | (0.05) | (0.05) | (0.05) | (0.08) | (0.08) | (0.08) | (0.08) | (0.18) | (0.15) | (0.15) | (0.15) |
| Model | Log | Log | Log | Log | Log | Log | Log | Log | Norm | Norm | Norm | Norm | Norm | Norm | Norm | Norm |
| Tax | 20% | 40% | 60% | 80% | 20% | 40% | 60% | 80% | 20% | 40% | 60% | 80% | 20% | 40% | 60% | 80% |
| Observations | 510 | 510 | 510 | 510 | 510 | 510 | 510 | 510 | 244 | 274 | 286 | 276 | 238 | 316 | 328 | 312 |
| Adjusted $R^2$ | | | | | | | | | 0.003 | 0.01 | 0.01 | 0.02 | 0.002 | 0.04 | 0.03 | 0.04 |
| Log Likelihood | −352.73 | −352.00 | −348.54 | −351.32 | −351.53 | −337.38 | −331.99 | −340.49 | | | | | | | | |

*Notes:* ·p<0.1;*p<0.05;**p<0.01;***p<0.001;

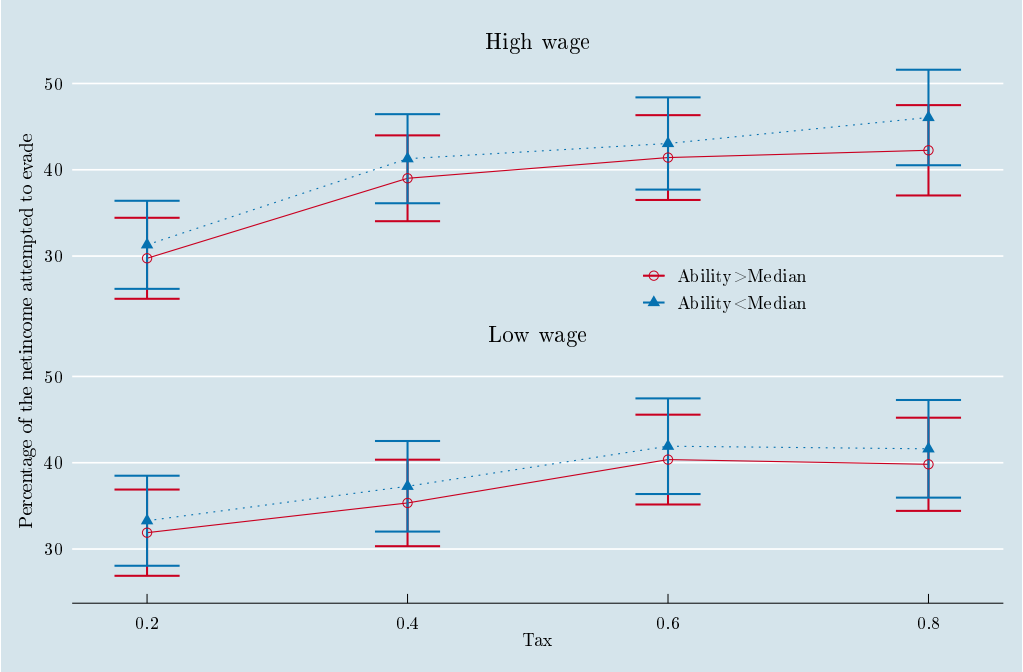
Next, we investigate whether there is a self-selection of unproductive subjects into behaving as tax-evaders. For this, we regress on the decision to ever evade taxes (i.e. whether a given subject invested in any instance into the lottery) by the average time needed to solve the two trial tables. We find that the average time needed for the two trials has no effect on the decision to evade ($\beta$=-0.00,t(509)=-0.05, $p \geq 0.05$, cohen's d=0.00).

**Figure C17:** Labor Supply by Ability.



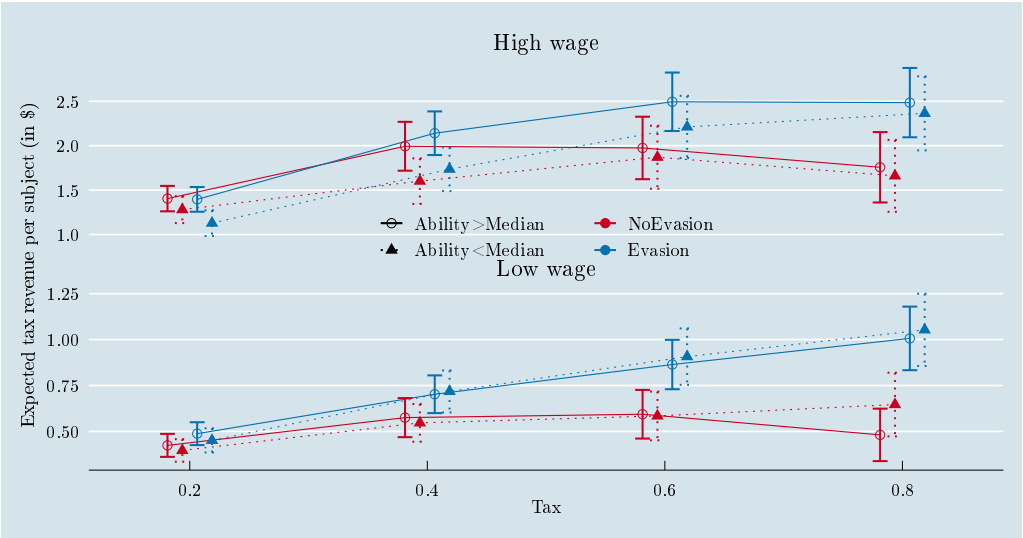

Note: Blue lines represent the Evasion-treatment while red lines represent the NoEvasion-treatments. Triangles connected by dashed lines represent the labor supply of subjects with a low ability of solving the tables (i.e. slower than the median subject), while dots connected with solid lines represent the labor supply of subjects with a high ability of solving the tables (i.e. faster than the median subject). The corresponding tunnels surrounding the respective dots represent the 95% confidence intervals. The upper panel depicts the high wage situation while the panel at the bottom depicts the low wage situation.

**Figure C18:** Evasion Decision by Ability.



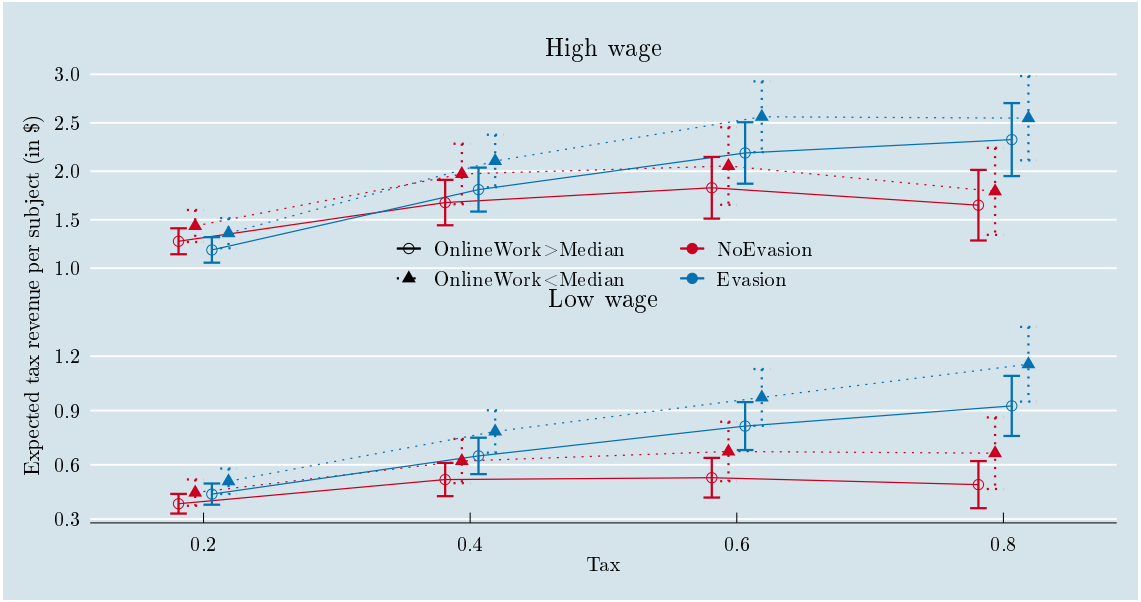

Note: Percentage of the net income attempted to evade as a function of tax in the Evasion-treatment for high and low ability subjects. Blue, dashed lines represent the percentage of the net income attempted to evade by subjects with a low ability of solving the tables (i.e. slower than the median subject), while red, solid lines represent the percentage of the net income attempted to evade by subjects with a high ability of solving the tables (i.e. faster than the median subject). The upper panel depicts the high wage situation while the panel at the bottom depicts the low wage situation. The corresponding tunnels surrounding the respective dots represent the 95% confidence intervals.

**Figure C19:** Expected Tax Revenue by Ability.



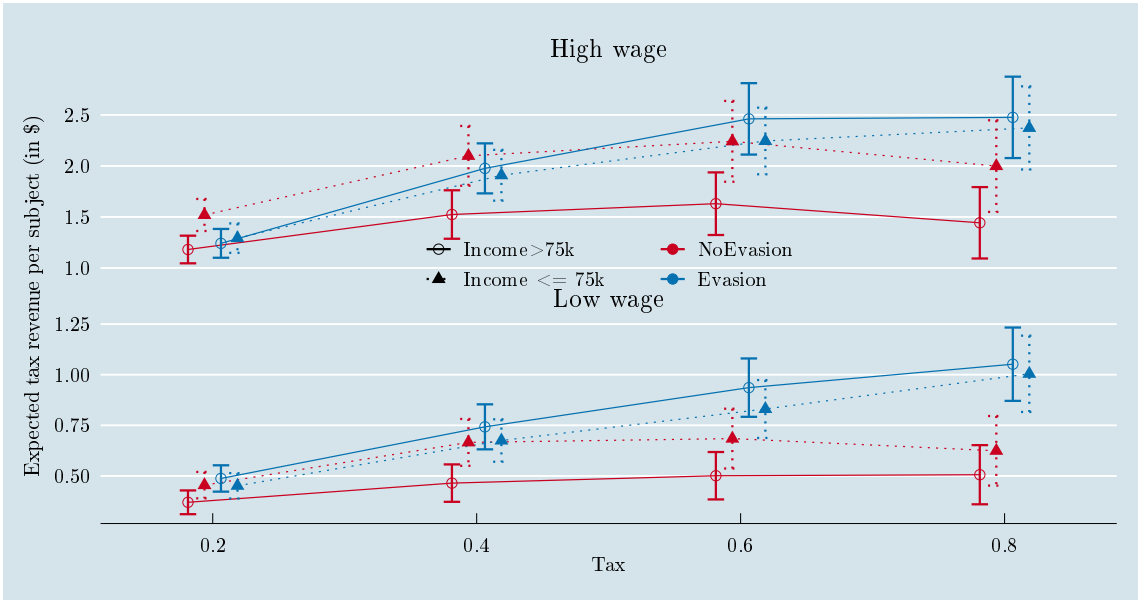

Note: Blue lines represent the Evasion-treatment while red lines represent the NoEvasion-treatments. Triangles connected by dashed lines represent the expected tax revenue of subjects with a low ability of solving the tables (i.e. slower than the median subject), while dots connected with solid lines represent the expected tax revenue of subjects with a high ability of solving the tables (i.e. faster than the median subject). The corresponding tunnels surrounding the respective dots represent the 95% confidence intervals. The upper panel depicts the high wage situation while the panel at the bottom depicts the low wage situation.

**Figure C20:** Expected Tax Revenue by Hours Spent on Online Work.



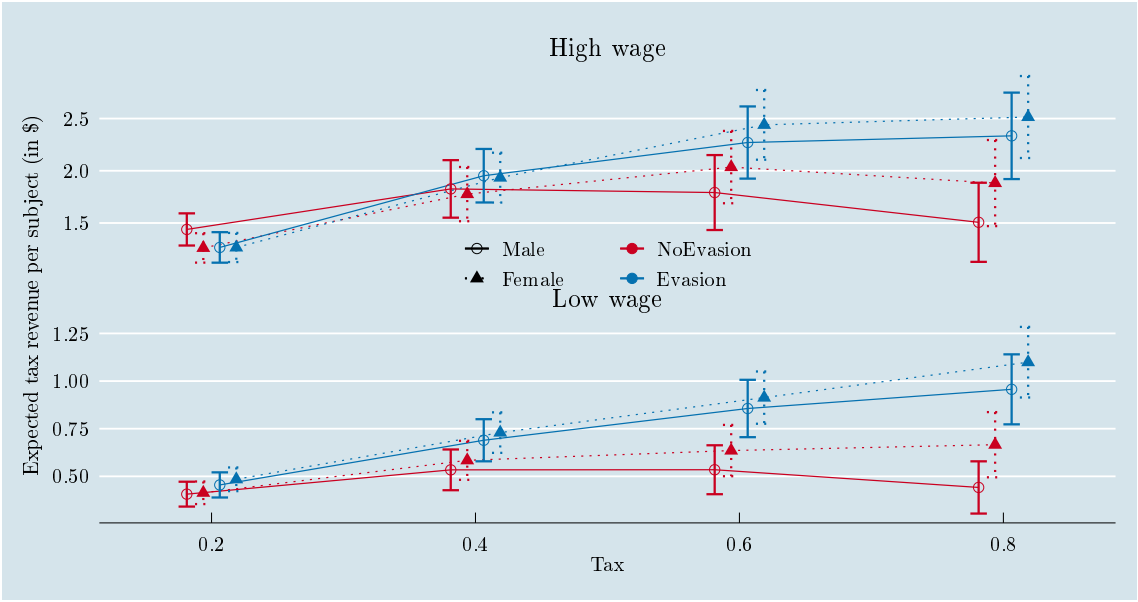

Note: Expected tax revenue as a function of tax and treatment by hours spent on OnlineWork. Blue lines represent the Evasion-treatment while red lines represent the NoEvasion-treatments. Triangles connected by dashed lines represent the expected tax revenue of subjects with a few hours of online works (i.e. fewer than the median subject), while dots connected with solid lines represent the expected tax revenue of subjects with a lot of hours of online works (i.e. higher than the median subject). The corresponding tunnels surrounding the respective dots represent the 95% confidence intervals. The upper panel depicts the high wage situation while the panel at the bottom depicts the low wage situation.

**Figure C21:** Expected Tax Revenue by Income.




Note: Expected tax revenue as a function of tax and treatment by income. Blue lines represent the Evasion-treatment while red lines represent the NoEvasion-treatments. Triangles connected by dashed lines represent the expected tax revenue of subjects with a lower income (i.e. annual income of less than 75k), while dots connected with solid lines represent the expected tax revenue of subjects with a higher income (i.e. annual income of more than 75k). The corresponding tunnels surrounding the respective dots represent the 95% confidence intervals. The upper panel depicts the high wage situation while the panel at the bottom depicts the low wage situation.

**Figure C22:** Expected Tax Revenue by Gender.



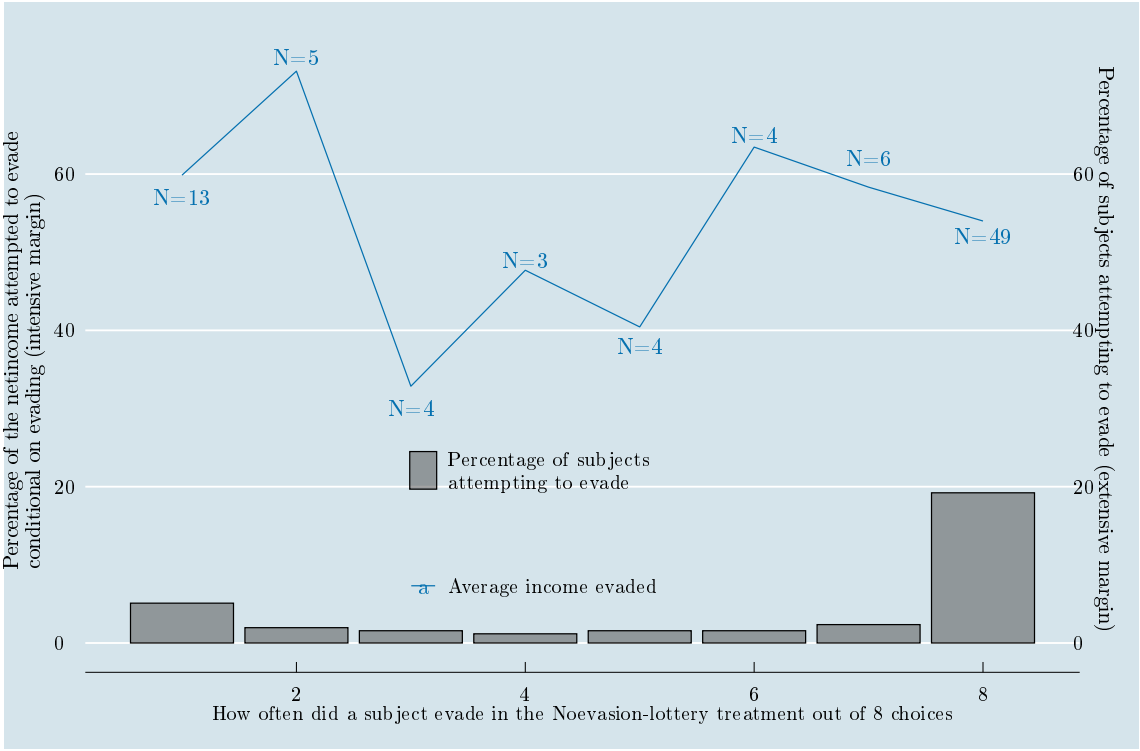

Note: Expected tax revenue as a function of tax and treatment by gender. Blue lines represent the Evasion-treatment while red lines represent the NoEvasion-treatments. Triangles connected by dashed lines represent the expected tax revenue of female subjects, while dots connected with solid lines represent the expected tax revenue of male subjects. The corresponding tunnels surrounding the respective dots represent the 95% confidence intervals. The upper panel depicts the high wage situation while the panel at the bottom depicts the low wage situation.

## C.4 Difference in the Two No-Evasion Treatments

The NoEvasion-Lottery-treatment was purely designed to ensure that the behavior is not driven by longer instructions in the Evasion-treatment, the availability of another option in the Evasion-treatment or the reduced cognitive load in the NoEvasion-treatment.[2] In fact, two NoEvasion-treatments do not differ significantly from each other: The average labor supply was $M = 14.25$ ($SD = 16.15$) in the NoEvasion-Lottery-treatment and $M = 12.94$ ($SD = 14.39$) in the NoEvasion-NoLottery-treatment, $t(411.3) = 0.9$, $p \geq 0.05$. They do not differ in any of the eight possible situations. All the results also go through if we just compare the Evasion-treatment to the main evasion treatment, i.e. NoEvasion-NoLottery.

[2] Abeler and Jäger (2015) show how the complexity in tax system might change the reaction function to changes in tax rates.

**Figure C23:** Individual Evasion Decisions in the NoEvasion-Lottery-Treatment (Extensive and Intensive Margin).




Note: Percentage of the net income attempted to evade conditional on evading as a function of how often a subject evaded. Blue, solid lines represent the percentage of the net income attempted to evade conditional on evading at all. The corresponding numbers indicate the number of people evading one, two, etc. times. The grey bars depict the percentage of subjects attempting to evade one, two, etc. times (i.e. the extensive margin).

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# Curriculum Vitae
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## University Education

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## Academic Work Experience

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