**Stage 1 registered report for replication of Experiment 1 and 2 of “Axe the tax:
Taxes are disliked more than equivalent costs” (Sussman & Olivola, 2011)**

Jerome Olsen¹, Christoph Kogler², Mark J. Brandt², Linda Dezső¹, and Erich Kirchler¹

¹University of Vienna, Faculty of Psychology, Department of Applied Psychology:

Work, Education and Economy, Universitaetsstrasse 7, 1010, Vienna, Austria

²Tilburg University, School of Social and Behavioral Sciences, Department of Social

Psychology, Warandelaan 2, 5037AB, Tilburg, The Netherlands

Corresponding author: Jerome Olsen, jerome.olsen@univie.ac.at, T +43 1 4277

47331.

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Changes based on stage 1 review comments that deviate from this report will be pre-registered separately and linked to the OSF project (osf.io/q8g7f).

Abstract

Sussman and Olivola (2011) reported that people in the US show a stronger preference to avoid tax-related costs than to avoid tax-unrelated monetary costs of the same size and coined the term Tax Aversion to describe the phenomenon. The original Experiment 1 and 2 results indicated that people are willing to incur increased timely costs to receive a discount when it refers to taxes (e.g., “axe-the-tax discount”) than when it just refers to a regular discount (e.g., “customer rewards”). Their paper has received considerable attention and is often cited in tax behavior research when referring to citizens’ general aversion to taxes. We propose close replications of Experiments 1 and 2 in two high-powered studies in the US ($N = 600$ and $N = 700$, respectively). Because the original study was conducted in a country that relies on a sales tax system, where consumption taxes are very salient, we additionally propose conducting both replication studies in the UK to test whether the effect also applies to a value added tax system (again $N = 600$ and $N = 700$, respectively). The replication studies will test whether Tax Aversion is a stable phenomenon in the US and whether the effect extends to a consumption tax system where payments do not represent an out-of-pocket cost.

Keywords: Tax Aversion, Replication, Tax Behavior, VAT, Sales Tax, Financial Choice

1. Introduction

Calling a financial charge on citizens by what it is – a tax – or using a mild alternative – e.g., community charge – can lead to different perceptions by the public. One common historical account is the case of the Community Charge in the UK during the third and last premiership of Margaret Thatcher. Prior to 1989, local governments collected so-called Domestic Rates, a tax based on the property value of citizens' houses. Thatcher's 1989 reform introduced a flat tax on adults which was called Community Charge. The opposition and the media called it a Poll Tax instead of Community Charge, which some have credited with part of the Community Charge's quick demise (for detailed discussions see Bramley, Grand, & Low, 1989; Smith, 1991; Winetrobe, 1992).

Accordingly, researchers have identified how framing information can affect perceptions and attitudes. For instance, experimental research revealed higher willingness to pay for an equal-sized environmental charge when it was labeled as *Carbon Offset* than when labeled as *Carbon Tax* (Hardisty, Johnson, & Weber, 2009), implying an aversion to taxes. McCaffery and Baron (2006) identify such inconsistent evaluations of tax policies as specific instances of a more general isolation or focusing effect, where citizens' decisions on complex matters are often based on the most salient or obvious aspect of a decision problem. This explains why logically relevant information is often ignored and evaluations are influenced by easily processed labels or metrics echoed in public discourse.

1.1 The Original Axe the Tax Study

Sussman and Olivola (2011) demonstrate a similar phenomenon they label Tax Aversion, which they define as a preference for avoiding tax-related costs over monetary costs of equal size (or even higher) that are not related to taxes. Their

study consists of a series of five independent experiments, investigating Tax Aversion in different contexts. In Experiments 1 and 2 they manipulated the framing of a purchase discount as either related to tax (e.g., “axe-the-tax”) or as a regular, tax-unrelated discount (e.g., “customer rewards”) between subjects. In Experiment 1 participants had to indicate whether they would be willing to accept a longer car drive to receive the discount, while in Experiment 2 individuals had to state their willingness to wait in minutes to receive the discount. Results of both experiments showed that people accept increased timely costs if they can receive a tax-related discount compared to a tax-unrelated discount. Experiment 2 additionally showed that consumers who are offered a tax-related discount tend to demand lower savings rates when they have to line up for 15 minutes. The remaining three experiments showed that individuals prefer tax-exempt bonds over equally profitable bonds that are subject to tax (Experiment 3), that tax aversion is higher among individuals who identify with antitax parties (Experiment 4), and that this last effect is mitigated if antitax sympathizers are instructed to reflect on positive uses of taxes (Experiment 5).

Results of the five experiments demonstrate that citizens’ tax-related attitudes influence decisions beyond the immediate context of tax behavior and extend to different decisions in daily life. Accordingly, the authors discuss their results not only in terms of implications for tax policy but also for marketers. One methodological strength of the reported experiments is that they may be less prone to social desirability as compared to studies that explicitly ask participants about their tax-related attitudes.

To evaluate the original study’s impact we ran a bibliometric search in three different databases (Google Scholar, Web of Knowledge, and Scopus) on April 18th 2018 and found 73 unique citations since the paper’s publication in 2011. More than

half of the citations have a clear focus on tax-related issues and the publication is most often cited to refer to citizens' general aversion to taxes. Given the attention the publication has received so far, we believe a close replication attempt of the phenomenon of Tax Aversion is important and thus propose independent replications of Experiments 1 and 2 which capture the focal effects of the paper. In doing so, we will follow the guidelines of the Replication Recipe proposed by Brandt et al. (2014).

1.2 Axe the Tax Mechanisms

The mechanism behind the observed preference for a tax-related over a tax-unrelated discount in Experiments 1 and 2 could be twofold. First, a general aversion to taxes could drive the effect, where just thinking of saving on taxes is sufficient. Second, in a sales tax system where taxes are added only at the check-out, consumption taxes constitute a salient out-of-pocket loss which could drive the effect (Chetty, Looney, & Kroft, 2009). That is, tax-related discounts imply that customers can actually avoid paying consumer tax (this is not the case) and so consumers are likely to compare the price plus sales tax (regular price) against the price without any additional tax (tax-related discount). We suspect that both explanations apply and that the latter reinforces a potential general aversion to taxes. This view is also in line with the fact that Sussman and Olivola (2011) find tax aversion for investment decisions – a different domain – in Experiment 3, where sales tax does not apply.

The idea that a focus on the regular price (including sales tax) compared to a price without any additional tax could drive this observed effect of Tax Aversion raises the question whether customers in a value added tax (VAT) system, where the tax is displayed as part of the price and should therefore be less salient (Bird, 2010), show the same preference for tax-related over tax-unrelated discount offers. Here individuals in both conditions are likely to compare the options based on the final

sales price instead of either thinking of saving tax or receiving a discount on the pretax price. To obtain first empirical evidence whether the observed effect is specific to sales tax or rather a consequence of general tax aversion and, furthermore, to expand the research question to consumption taxes in general, we propose extending the replication efforts to a country that uses VAT, namely the UK. We chose the UK because the materials require only small modifications (i.e., small language and cultural differences), and a replication attempt should therefore be fairly easy to implement.

In summary, we propose close replications of Experiment 1 and 2 of Sussman and Olivola (2011) in the US to test whether Tax Aversion, operationalized through purchase decision scenarios, is a stable phenomenon. Additionally, we plan to conduct both experiments in the UK to answer the question whether Tax Aversion is also observable in a different cultural setting with a tax system that relies on VAT.

2. Experiment 1

2.1 Method

See Table S1 of the supplementary material for the 36-question guide to the Replication Recipe (Brandt et al., 2014) that addresses details about the nature of the original effect and differences between the original and replication study.

2.1.1 Participants

2.1.1.1 Original Study

A total of 238 participants were recruited for the original study using three different strategies ($n = 131$ Amazon's Mechanical Turk (MTurk), $n = 65$ passersby in a US shopping mall, and $n = 42$ Princeton University undergraduates). After

excluding 47 individuals (non-US residents, already participated in same study, or visibly challenged), the final sample size was $N = 191$.

Individuals from the original study who were recruited through MTurk and the shopping mall received payment, while students received either course credits or payment for their participation. Respondents from the shopping mall and on campus filled in a survey package that also contained unrelated questionnaires. The responses between the three samples are reported not to have differed from each other significantly.

2.1.1.2 Power Analysis and Replication Study Sample

The effect of interest was the proportion of respondents preferring to travel 30 minutes to receive a price discount on a new television compared to paying a regular price, where the discount was either an 8% tax-related discount or a 9% tax-unrelated discount (between-subject). Participants in the tax-related condition were more often willing to accept the additional travel time for a discount than participants in the tax-unrelated discount condition (76% vs. 59%), $\chi^2(1, N = 191) = 5.83$, $p = .016$, $r_e = .18$, 95% CI [0.04, 0.31].

Running a power analysis for differences between these two independent proportions using G*Power (Faul, Erdfelder, Buchner, & Lang, 2009) yields a sample size of $N = 390$ for $r_e = .18$ with $\alpha = .05$ and $1 - \beta$ (power) = .95. This assumes that the effect estimate in the original paper is an accurate estimate of the true underlying effect size. Given the general risk of an overestimated effect, our power could then turn out to be too low.

To address this, two procedures have been proposed in the literature that adjust targeted effect sizes downward: safeguard power (Perugini, Gallucci, & Costantini, 2014) and the $2.5 \times N$ rule (Simonsohn, 2015). In the first case, the

original effect's lower-bound of the 60% confidence interval is used as targeted effect size. The original effect size was $r_{\phi} = .18$ with 60% CI [0.12, 0.24]. Using the lower-bound of $r_{\phi} = .12$ would require $N = 896$ for 95% power. In the second case, it is suggested to multiply the original sample size by a factor of 2.5, which would yield $N = 573$. As stated before, basing the decision on the originally reported effect size results in $N = 390$. We would favor the largest sample size but given the available resources we set the targeted sample size at $N = 600$ for a replication of Experiment 1 for both the US and UK sample, respectively. This provides 95% power for effects as small as $r_{\phi} = .15$ and still 80% power for effects as small as $r_{\phi} = .11$ in each country.

2.1.2 Materials

2.1.2.1 US Sample

The materials are fully available in the original publication. Furthermore, the original authors were contacted to ask whether there were further unreported procedural details of potential importance to the study design, which was not the case.

The study comprises a two-group between-subject design. Both groups read a scenario about a purchase decision for a new television. Participants have to decide between paying the regular price or accepting a higher time investment to receive a discount. The between-subject factor varies the type of discount (8% tax-related discount vs. 9% tax-unrelated discount). Participants read the following scenario, followed by making a decision for one of two stores that either offers the television at the regular price or with one of the two discounts.

“You want to buy a new television and have a particular model in mind.

Calling around, you find that only two stores, Bob's Electronics and

Tom's Electronics, carry that model. Bob's Electronics is located very close, about a 5-minute drive, but offers no discounts on the television set. Tom's Electronics is located farther away, about a 30-minute drive, but offers the television set [tax-free, which is equivalent to an 8% discount/with a 9% discount]. Where do you go to make your purchase?"

☐ Bob's Electronics

☐ Tom's Electronics

Participants are then asked about basic demographic information and their political ideology with one item with an answering scale ranging from *extremely liberal* to *extremely conservative*. The experiment concludes with four questions measuring tax-related attitudes taken from the Motivational Postures *Commitment* subscale (Braithwaite, 2003), which is defined as the moral obligation to pay taxes and to support the principles of taxation. All materials can be found in the supplementary material.

2.1.2.2 UK Sample

In contrast to varying sales tax rates in the US, the UK relies on a 20% VAT charge on most goods and services. To address this difference, we propose changing two aspects of the scenario used in the UK.

First, we include different percentage values for the offered discounts (20% tax-related discount vs. 21% tax-unrelated discount) that correspond to externally valid numbers of the UK VAT system. The wording in the scenario will not be changed, just the numbers.

Second, the change of percentage values increases the attractiveness of traveling 30 minutes to receive the discount. We actually don't expect this to

influence the relative difference in attractiveness between tax-related and tax-unrelated discount, but the effect could decrease due to ceiling effects of individuals choosing the discount over the regular price. In the original US results, with 8% tax-related and 9% tax-unrelated discounts, the ratios of individuals choosing the discount were 76% and 59%. To address the potential danger of ceiling effects, we will perform a pre-test asking UK participants how long they would be willing to travel to receive a 20% discount on a purchase. We will then adjust the timely costs in the UK scenario to the rounded median waiting time observed in the pre-test. If we must use a value that deviates from 30 minutes, it is crucial to only focus on the relative difference between conditions and not the absolute levels when comparing country samples.

Another smaller change concerns the item measuring political ideology. Here we will use a single item with an answering scale ranging from *extremely left-wing* to *extremely right-wing* (as opposed to *extremely liberal* vs. *extremely conservative*). All remaining aspects of the materials will correspond to the US version.

2.1.3 Procedure

Participants will be recruited using Prolific Academic which allows sampling individuals in the US and the UK. Total participation time should not last longer than 5 minutes. Individuals will be paid a remuneration of \$1.0.

2.2 Planned Analysis

We will use the same analysis as reported in the original paper. That is, the proportions of individuals choosing a higher time investment to receive a discount between the two conditions using a χ^2 -test. This test will be run separately for the US

and UK samples. Individuals who report not being US/UK-residents will be excluded from the analysis.

A successful replication is defined as a significant χ^2 -test in the US sample. Additionally, we will test whether the replication results are different from an effect size that is large enough to still have been detectable (i.e., with 33% power) with the sample size of the original study (Simonsohn, 2015) to assess whether our results are different in magnitude from the original study. We regard replication failure as informative if we fail to find a significant χ^2 -test combined with an effect estimate significantly smaller than an effect detectable with 33% power and $N = 191$; i.e., $r = .11$.

Furthermore, we will run a logistic regression with decision as dependent variable. Independent variables will be the factor discount type (0 = tax-unrelated, 1 = tax-related), country (0 = US, 1 = UK), and the interaction of the two variables. This model tests the overall effect of discount type while controlling for possible moderating effects of sample type.

See the attached R Markdown file for a detailed analysis plan including data preparation and the actual analysis with mock datasets.

3. Experiment 2

3.1 Method

See Table S2 of the supplementary material for the 36-question guide to the Replication Recipe (Brandt et al., 2014) that addresses details about the nature of the original effect and differences between the original and replication study.

3.1.1 Participants

3.1.1.1 Original Study

In total 401 participants were recruited for the original study using MTurk. After excluding 50 individuals (non-US residents, already participated in same study, under the age of 18, short completion time), the final sample was $N = 351$.

3.1.1.2 Power Analysis and Replication Study Sample

Experiment 2 reports on two effects from two scenario choices, again comparing individuals who were offered either a tax-related or tax-unrelated discount (between-subject). In the first scenario, individuals were asked how long they would be willing to wait in line to receive a 9% (“axe-the-tax”/“customer rewards”) discount by answering binary yes/no questions for 5-minute time intervals increasing from 5 to 60 minutes. The proportion of Yes responses was higher in the tax-related discount condition (.53 vs. .43), $U = 3.11$, $p = .002$. In the second scenario, participants had to indicate the discount size necessary for them to wait 15 minutes in line. Again, this was measured using binary yes/no choices for discounts ranging from 5% to 12%. The proportion of Yes responses did not differ significantly between the two conditions (.72 vs. .79), $U = 1.81$, $p = .070$, but was interpreted as marginally significant in the original article.

Extracting exact effect sizes was not possible, as only U -values of non-parametric U-tests without corresponding z -values were reported. We contacted the authors who shared the raw data. Effect sizes were $r = .15$, 95% CI [0.05, 0.25], and $r = .10$, 95% CI [-0.01, 0.20] respectively.

Both scenarios were embedded in a larger questionnaire in a random order and it was therefore unlikely for the two choices to be taken in direct succession. To avoid direct contrast effects of the two choices we decided to limit the replication attempt to the first scenario that yielded a larger effect.

Running a power analysis for a U-test with $\alpha = .05$ and $1-\beta$ (power) = .95 for the original effect ($r = .15$) yields a required sample sizes of $N = 594$. Applying the two downward adjustment rules, safeguard power (60% CI [0.21, 0.39]) and $2.5 \times N$ rule (original $N = 351$), required sample sizes are $N = 1238$ and $N = 878$, respectively. Given the available resources we set the targeted sample size at $N = 700$ for a replication of the first scenario of Experiment 2. This provides 95% power for effects as small as $r = .14$ and still 80% power for effects as small as $r = .11$ in each country.

3.1.2 Materials

3.1.2.1 US Sample

Materials are again fully available from the original publication. Furthermore, the original authors were contacted to ask whether there were further unreported procedural details important for to the study design, which was confirmed not to be the case.

The study comprises a two-group between-subject design. Both groups read a scenario about a purchase decision for a jacket. Participants are told about the possibility to receive a 9% discount if they wait in line in one of two stores. They have to indicate how long they would be willing to wait to receive the discount. The between-subject factor varies the type of discount (“axe-the-tax” vs. “customer rewards”). The exact scenario goes as follows:

“Imagine that you are walking through the mall looking for a particular jacket that you have seen advertised. You come across two closely located stores that carry it. The first store offers no discounts, but has no wait to purchase the coat. The second store is having a special

["axe-the-tax" sale, with the store selling all items tax-free, equivalent to a 9% discount/"customer rewards" sale, with the store selling all items at a 9% discount]. However, due to the popularity of the sale, there is a wait to purchase items there. How long would you wait in line to receive the discount?"

Following the scenario, individuals have to answer a series of 12 binary yes/no choice questions that ask if they would be willing to wait X minutes to receive the 9% "axe-the- tax"/"customer rewards" savings, with X increasing in 5-minute intervals from 5 to 60 minutes.

As in Experiment 1, participants are then asked about basic demographic information and their political ideology. The experiment concludes with four questions measuring tax-related attitudes taken from the Motivational Postures *Commitment* subscale (Braithwaite, 2003). All materials can be found in the supplement.

3.1.2.2 UK Sample

As in Experiment 1, materials will be adapted to suit the 20% VAT applied in the UK, meaning that the discount size in the scenario will be set to 20%. As there is no indicated waiting time in the scenario, no further changes to the content are deemed necessary.

One minor change addresses language differences. We will replace the word "mall" by "shopping centre" which is more appropriate for a UK sample. All remaining aspects of the materials will correspond to the US version.

3.1.3 Procedure

Participants will be recruited using Prolific Academic which allows sampling individuals in the US and the UK. Total participation time should not last longer than 5 minutes. Individuals will be paid a remuneration of \$1.0.

3.2 Planned Analysis

Again, the analysis will be conducted in line with the original paper. First, we will calculate an average score for each individual indicating their willingness to wait by calculating the percentage of Yes responses. Individuals will be excluded, if they make inconsistent choices here, e.g., willingness to wait 15 minutes but not 10 minutes to receive a discount. Second, we will compare the Yes proportions between the two discount groups (“axe-the-tax”/“customer rewards”) using a U-test for both samples separately. Individuals who report not being US/UK-residents will be excluded from the analysis.

A successful replication is defined as a significant U-test in the US sample. Again, we will also test whether the replication results are different from an effect size that is large enough to still have been detectable (i.e., with 33% power) with the sample size of the original study (Simonsohn, 2015). We regard replication failure as informative if we fail to find a significant U-test along with an effect estimate significantly smaller than an effect detectable with 33% power and $N = 351$; i.e., $r = .08$.

Furthermore, we will run a mixed effects logistic regression with decision as dependent variable (12 responses per individual). Independent variables will be the factor discount type (0 = tax-unrelated, 1 = tax-related), country (0 = US, 1 = UK), and the interaction of the two variables. This model will address the repeated measures structure of the data and tests the overall effect of discount type while controlling for possible moderating effects of sample type. Such a model also provides the

possibility to explore whether the effect of discount type increases with longer waiting times.

See the attached R Markdown file for a detailed analysis plan including data preparation and the actual analysis with mock datasets.

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