The Impact Of Trial Runs On The Acceptability Of Environmental Taxes: Experimental Evidence

By: **Todd L. Cherry**, Steffen Kallbekken, & Stephan Kroll

Abstract

This paper examines the political difficulty of enacting welfare-enhancing environmental taxes. Using referenda in a market experiment with externalities, we investigate the effect of trial periods on the acceptability of two theoretically equivalent Pigouvian tax schemes. While implementing either tax is in subjects' material self-interest, we find significant levels of opposition to both schemes, though the level differs considerably. Results show that trial runs can overcome initial tax aversion, which is robust across schemes, but a trial with one scheme does not affect the acceptability of the other. Trial periods also mitigate initial biases in preferences of alternative tax schemes.

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1. Introduction

A significant challenge facing effective environmental policy is the political difficulty of implementing a Pigouvian tax – a tax intended to increase welfare by incentivising agents to internalise the social costs of an externality. Recent history provides many examples of failed attempts to enact a Pigouvian tax (or something similar), including the rejection of a Btu (British thermal unit) tax in the U.S. (Erlandson, 1994), the failure of the 2005 referendum to impose an Edinburgh road user charge (Gaunt et al., 2007), the rejection of the 1993 proposal to increase the value added tax on domestic energy in the U.K. (Dresner et al., 2006a), and the rejection of three proposals in 2000 to tax fossil energy in Switzerland (Thalmann, 2004).

This challenge, of course, is not new. Starting with Buchanan and Tullock (1975), the large literature on rent-seeking behaviour and special interest groups provides numerous arguments that can explain why proposals for Pigouvian taxes are defeated in the political process, and why the design is often suboptimal when such taxes are enacted.[1] Most of this literature is focused on the role that businesses and NGOs play in policy formulation. These are, however, not the only actors that can influence policy. The public's opinion also matters. This is obvious in some cases, such as when a tax proposal must receive a majority of the votes in a public referendum to be implemented, but in many other cases, the link between public acceptability and political feasibility is less direct. While policy can be advanced in the absence of strong support, elected policymakers often cannot politically afford to enact highly unpopular policies (List and Sturm, 2006). Indeed, in the case of the proposed Btu tax in the U.S., public officials rejected the policy in response to strong public opposition (Erlandson, 1994). And as Gaunt et al. (2007) point out, "commentators now acknowledge that the greatest impediment to implementation [of the Edinburgh road user charge] is public [. . .] acceptability."

Given that public opposition is a key barrier to enacting environmental taxes, what can be done to overcome this opposition? The successful implementation of a congestion charge in Stockholm may provide some insights to this question. Prior to a public referendum on the congestion charge, there was a trial run that enabled people to experience the workings and impacts of the policy. The experience seemed to boost acceptability, increasing public support in the polls by 18 percentage points. The increase in support was sufficient for the tax to win a majority in the public referendum, a win not predicted in the polls before the trial began (Winslott-Hiselius et al., 2009; Schuitema et al., 2010). Considering the concurrent political and procedural influences, such as lobbying activities, voting rules, and public relations, the impact of the trial run remains an open question.[2] Indeed, Hong Kong conducted a two-year pilot of an electronic road pricing system, after which local government boards voted down permanent implementation because of continued public opposition (Hau, 1990).

The prospect of trial runs raises two questions. First, when individuals form opinions about environmental taxes, can opposition arise from cognitive constraints and biases instead of material self-interest? Findings from the behavioural sciences indicate that people often fail to arrive at optimal decisions because of limits to rationality, and some of these behavioural elements may underlie the negative perceptions of Pigouvian taxes (e.g., heuristics, status quo bias, etc.). Second, can such behavioural influences be mitigated if people experience a trial period of the workings and impacts of the policy instrument? Considering the role of public perceptions in determining the acceptability of environmental taxes, it is important to investigate the potential of mechanisms, such as trials, that overcome opposition to the welfare-enhancing policies. Herein, we experimentally examine whether a trial run with a Pigouvian tax can lessen behavioural aversions and opposition to the policy. Our results indicate that it can, thereby increasing the acceptability of the tax. The finding is robust across different Pigouvian tax schemes.

[1] See for instance the Journal of Public Policy special issue on interest group influence (Dür and De Bièvre, 2007).

[2] A key procedural element was the decision to only allow Stockholm residents to vote in the referendum, which excluded affected commuters that lived in surrounding areas. Feeling disenfranchised, many surrounding areas held their own non-binding referendum to express their preferences, which were generally in opposition to the congestion charge.

2. Background

Previous work provides some background on the behavioural elements that impede acceptability and obstruct the implementation of efficient taxes. Perceptions of taxes play a central role in determining acceptability, which can be shaped by influences beyond material self-interest. Research shows that people tend to have an inherent preference for the current state of affairs (i.e., status quo bias), which can translate into opposition to efficient political reforms (Fernandez and Rodrik, 1991). Perceptions about the distributional fairness of a tax (i.e., inequality aversion) can also influence attitudes towards a proposed environmental tax (Dresner et al., 2006b; Eriksson et al., 2006). Kallbekken et al. (2010) provides experimental evidence that distributional concerns significantly affected voting decisions over alternative tax proposals.

Many studies have explored fiscal illusion, which contends that the institutional manner in which citizens must pay for government affects taxpayer perceptions of the price of government (Wagner, 1976; Heyndels and Smolders, 1995; Rosen, 1976). Sausgruber and Tyran (2005) find that people underestimate the tax burden of an indirect tax, whereas this is not the case for a direct tax, and that this illusion distorts the decisions in a (laboratory) referendum and leads to excessive redistribution. Similarly, people also can underestimate the actual effectiveness of Pigouvian taxes (Heres et al., 2013) because "there seems to be a negative bias in the opinion of respondents on the effectiveness of measures which aim at influencing their behaviour (especially price measures)" (Rienstra et al., 1999). For instance, Dresner et al. (2006b) report that both the general public and business view "taxes solely as a means of raising revenue, rather than in terms of their incentive effects." And, in case of the failure of the Edinburgh road user charge, Gaunt et al. (2007) note that "the public was largely unconvinced that the scheme would have achieved its dual objectives of reducing congestion and improving public transport."

As the workings of Pigouvian taxes are not well understood, it seems many people perceive them simply as fiscal taxes. The term "environmental tax" therefore may seem justified to the public only if the revenues are earmarked for environmental measures. Indeed, an environmental earmark provides assurance that an environmental tax will direct revenue to improve environmental quality, which may explain, to some extent, the well-documented preference for earmarking (Bös, 2000; Brett and Keen, 2000; Kallbekken and Sælen, 2011; Schuitema and Steg, 2008; Steg et al., 2006).

The Stockholm experience suggests that a trial run might refine perceptions and mitigate biases, and the literature provides some evidence to support this conjecture. A number of field and laboratory experiments have reported that individual decisions improved with different forms of experience, such as receiving within-treatment feedback of decisions over repeated trials (e.g., Cherry et al., 2003; Sausgruber and Tyran, 2005) and using subjects with prior field or laboratory experience related to the decision (e.g., List, 2003; Hussam et al., 2008). The evidence, however, is not conclusive (e.g., Kluger and Wyatt, 2004; Chen et al., 2007). A trial run, in our setting, is unique because it offers a one-shot experience with the workings of a Pigouvian tax before a binding referenda. The referenda then offer choices involving no tax and two alternative, but theoretically equivalent, Pigouvian taxes – a full and a threshold tax. While a typical full tax scheme imposes the tax on every unit, the threshold tax levies the tax only on units beyond a specific level. Given the threshold is set at the efficient level of activity, the Pigouvian threshold tax yields an efficient outcome while separating the incentive and income effects, which provides the incentives to reduce the externality to the optimal level while also providing less reason for political opposition (Pezzey, 2003, 2006).[3,4] By considering both the full and

[3] Though the threshold tax has so far mostly been a theoretical construct, there are several tax schemes that resemble such a threshold tax. For instance, the Norwegian motor vehicle registration tax where vehicle emissions below 105 g CO_2/km are not taxed, whereas emissions above this threshold are taxed (progressively) (Ministry of Finance, 2013). Also, the Swedish nitrogen oxide charge on energy production works like a threshold tax: it levies a flat rate on all emissions, then returns all the revenue to producers (less minor administrative costs) based on relative production levels or emissions intensities; therefore, emissions in excess of the average result in additional costs, emissions below the average result in a transfer (Millock et al., 2004). More generally, this example points to the similarities of a threshold tax and a cap-and-trade scheme with free permit allocation.

[4] Alternatively, instead of raising less revenue when reaching a certain abatement goal, a threshold tax (or free permits) can also be used to pursue a more stringent abatement goal with a fixed revenue goal (Pezzey and Jotzo, 2013).

threshold tax schemes, we can examine the preferences of alternative Pigouvian taxes while more fully investigating the influence of trial runs on the acceptability of the instruments.

3. Experimental design and hypotheses

3.1. Experimental design

3.1.1. The market

To investigate the potential influence of a trial period on tax acceptability, we construct an experimental market with externalities, which can be internalised with an efficient Pigouvian tax. The market is structured as a uniform-price, multi-unit auction, which is predicted to converge quickly towards the equilibrium (see Smith et al., 1982, or Tyran and Sausgruber, 2005). The market consists of five buyers and one (automated) seller exchanging a fictitious good with real value expressed in an experimental currency denominated in "tokens". Ten tokens equalled one Danish Krone or, at the time of the experiment, about €0.13 and US$0.19). The buyers impose external costs on each other through their purchases. In a market period, each buyer can buy a maximum of six units. The buyers are informed about their resale values (which are 76, 50, 36, 24, 20 and 6 tokens for the six units, respectively), and also that the seller's marginal cost is between 8 and 14 tokens – and that it will remain constant throughout the experiment. The resale values are not common knowledge. The seller's actual marginal cost is 10 tokens per unit, and therefore the equilibrium unit price in the market is also 10 tokens.

At the beginning of a market period, the buyers indicate their willingness-to-pay (WTP), which is the WTP for each unit (instead of being asked for a unique WTP for each of the six units). The seller then sets the uniform price equal to the lowest WTP above the seller's marginal cost. All buyers with a WTP above the uniform price can then purchase as many units of the good as they wish (maximum of six), while all buyers with a WTP below the market price (and therefore below marginal costs) are excluded from making any purchases in this period.[5]

Whenever a buyer purchases a unit of the good, she imposes an external cost of five tokens on each of the four other buyers in their group. The marginal damage from each unit purchased is therefore 20 tokens. The market equilibrium, without any tax, has all buyers purchasing five units at a price of 10. The socially optimal outcome, given the market price is below 20, is for each buyer to purchase three units. The socially optimal outcome, which can be achieved with an efficient Pigouvian tax, represents an improvement of 80 tokens over the market equilibrium, which translates to a 29% efficiency gain.[6] The shaded area in Fig. 1 represents the efficiency improvement of the social optimum over the market equilibrium.

3.1.2. The tax schemes

We consider two Pigouvian tax schemes: the typical *full tax* and the less common *threshold tax*. Both the full and threshold taxes are efficient with a tax rate equal to the external cost of 20 tokens per unit. The new equilibrium would still yield a market price of 10 as can be seen in Fig. 1 since the buyers pay the tax to the "government" and therefore the market price does not reflect the tax rate. The equilibrium quantity, however, declines from 25 to 15 units, with individual buyers reducing the number of units purchased from 5 to 3 units. With either tax, the market equilibrium is shifted to equal the socially optimal outcome.

The difference between the full tax and the threshold tax is that the full tax imposes the tax on all purchased units, whereas the threshold tax levies the tax only on purchases beyond the first three units

[5] This set-up is a slight change from the regular uniform-price, multi-unit auction in that a buyer is not asked about his or her WTP for each unit. The consequence is that buyers (particularly during the early periods of the experiment) might not purchase a single unit even though they had a WTP for higher valued units above the marginal cost. This difference, however, becomes unimportant once the market approaches equilibrium, while the design facilitates easier subject understanding of the mechanism.

[6] Purchasing ten fewer units reduces external costs by 20 tokens per purchase for a total of 200, but that gain is partly counteracted by a loss in consumer surplus of 120 tokens ($5 \times (10+14)$).

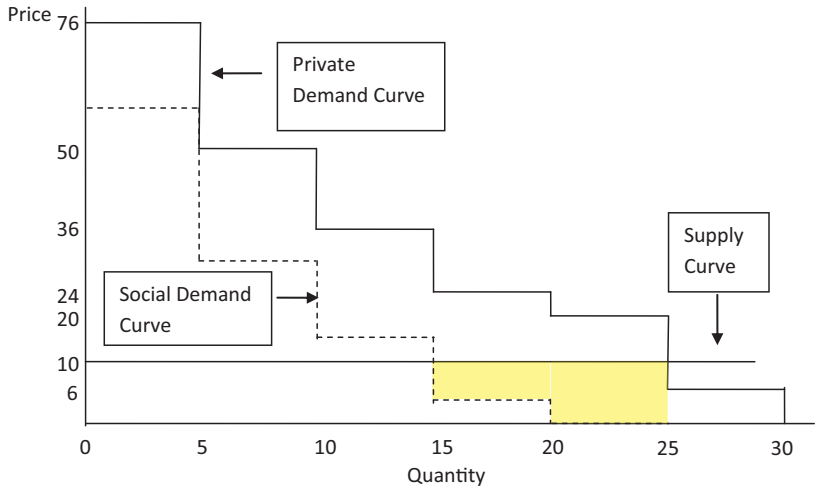

Fig. 1. Supply, demand and efficiency gains.

Table 1
Timeline for the experiment.

	Stage 1: No votes			Stage 2: Votes plus 3 periods after each vote		
	Periods 1–5	Periods 6–10	Periods 11–15	Vote 1	Vote 2	Vote 3
Full + trial	No tax	Full tax	No tax	Full vs. no tax	Thres vs. no tax	Full vs. thres
Full + no trial	No tax	No tax	–	Full vs. no tax	Thres vs. no tax	Full vs. thres
Threshold + trial	No tax	Thres tax	No tax	Thres vs. no tax	Full vs. no tax	Full vs. thres
Threshold + no trial	No tax	No tax	–	Thres vs. no tax	Full vs. no tax	Full vs. thres

(the socially optimal level). At equilibrium the two tax schemes produce identical outcomes.[7] Total individual profits under the full and threshold tax are 72 and 132 tokens, respectively, but whereas the threshold tax generates no revenues, the full tax generates 300 tokens of revenue that are redistributed in equal shares to the buyers. Net payoffs, including external costs of 60 tokens that each subject suffers in equilibrium, are therefore equal across the full and threshold tax schemes, at 72 tokens, which are 16 tokens greater than the no-tax market equilibrium. Therefore, presuming material self-interest, people should prefer a tax over no tax because a tax, full or threshold, yields higher payoffs; and they should be indifferent between a full and threshold tax.

3.1.3. Experimental framework

The experiment consists of two stages and follows a 2×2 design that varies two treatment variables: trial run (trial and no-trial) and tax scheme (full and threshold). Using Table 1, we first review stage one. In both the trial and no-trial treatments, subjects participate in two sets of five market periods without any tax. In the trial treatments, subjects participate in an additional set of five market periods that impose one of the two efficient tax schemes. This trial run occurs after the first set of no-tax market periods but before the second set. The tax treatment, full or threshold, determines which tax is exogenously imposed in stage one of the trial treatments.

In stage two, subjects vote in three referenda that determine the tax policy for the subsequent three market periods. The full and threshold tax treatments, in addition to determining the tax imposed as

[7] The two tax schemes yield identical payoffs at equilibrium, and for any combination of purchases within the group between the social optimum and the market equilibrium. Payoffs are not identical if any subject purchases two units or fewer – because there is no tax on the first three units.

a trial in stage one, dictate the options in the first and second referendum in stage two. In the full (threshold) tax treatment, the first referendum is between a full (threshold) tax and no tax, and the second referendum is between a threshold (full) tax and no tax. The third referendum is the same in all treatments and presents a choice between the two tax schemes (full vs. threshold).[8]

The design provides four treatments: *full tax with no trial, full tax with trial*, *threshold tax with no trial*, and *threshold tax with trial*. We are interested in how the trial treatments in stage one affect voting behaviour over the alternative tax schemes presented in stage two. The experimental design facilitates a clean investigation of questions regarding the ability of a trial run to overcome individual resistance to a tax policy that is materially beneficial. As Falk and Heckmann (2009) point out, experimental methods are well-suited for such inquiries of individual decision-making because the lab offers control over key elements that are often fixed or unobservable in the field. In our case, the lab offers clarity that is not available in the field by varying tax schemes while maintaining control over alternative payoffs and induced values – e.g., the incentive structure's impact on payoffs is unambiguous, the use of tax revenues is unambiguous, and the decision-makers and the decision process are unambiguous.

3.1.4. Procedures

The experiment was conducted in the summer of 2009 at the University of Copenhagen with a total of 170 student participants that were recruited from the general student population. We conducted nine sessions; two or three sessions per treatment. Each session consisted of two to five repeated markets per session with three referenda. Subjects remained in the same group of buyers throughout the market session (partner design). This resulted in 34 independent market sessions with 102 referenda and 510 individual votes. Due to "no-shows" we had an uneven number of subjects across the four treatments: 40 subjects (8 markets) in *full tax with no trial*, 45 subjects (9 markets) in *full tax with trial*, 50 subjects (10 markets) in *threshold tax with no trial*, and 35 subjects (7 markets) in *threshold tax with trial*. Students who had previously participated in similar market experiments were not invited to participate. The students were required to answer questions (and were later provided with the correct answers) to make sure they understood the market before the experiment began. Each session lasted about 105 min, including reading the instructions and the questionnaire at the end of the session. Subject earnings accumulated across the market session with an average payout of DKK 207 (about US$39). The experiment was programmed and conducted with the software z-Tree (Fischbacher, 2007). At the end of each period, subjects received information about the market price, the number of units they bought (and the gain from each of these units), the size of the tax payments they made, their share of the tax revenues, the additional costs caused by others' purchases and their total costs in the given period and accumulated over all periods.

3.2. Hypotheses

Presuming rational material self-interest, subjects should cast a vote in favour of either efficient tax (full or threshold) over a no-tax alternative. However, evidence from voting behaviour indicates this may not be the case – people sometimes vote against a tax even if it is in their material self-interest to vote in favour of it (Kallbekken et al., 2011). This is what we define as *tax aversion* for the purpose of this paper: to vote against a tax scheme that would increase both own and group payoff. There are no theoretical models that can fully explain tax aversion, but it is nonetheless an empirical phenomenon. By using identical payoff functions for all participants, we avoid the potential for distributional concerns to confound our results (see Kallbekken et al., 2010).

The research question is whether a trial run with an efficient Pigouvian tax can mitigate people's tax aversion, which gives the first null hypothesis: $\mathbf{H_0}^{\mathbf{1}}$: a trial with a full (threshold) tax has no significant effect on its acceptability.

Incorporating two alternative efficient tax schemes enables us to explore the extent and robustness of any effect from a trial by testing whether a trial with one efficient tax influences acceptability of a

[8] Observed behaviour suggests the experimental design mitigated potential order-effects, though the issue is considered in the conditional analyses.

Table 2
Mean profit, price and number of units bought per period by stage and implemented scheme.

	Stage 1				Stage 2		
	No tax		Trials				
	First 5	Last 5[a]	Full	Threshold	No tax	Full	Threshold
Mean profit	43.83	50.52	61.60	63.28	51.13	64.91	66.24
Mean price	11.84	10.92	11.18	10.97	10.64	10.79	10.69
Mean quantity	4.51	4.66	2.97	3.17	4.77	3.06	3.07

[a] The last five periods are periods 6–10 for the no-trial treatments and periods 11–15 for the trial treatments.

different efficient tax. Specifically, does a trial run with a full (threshold) tax affect the acceptability of a threshold (full) tax? This provides the second null hypothesis: $\mathbf{H_O}^{\mathbf{2}}$: a trial with one efficient tax does not affect the acceptability of another efficient tax.

Another layer of inquiry is possible due to the referendum between the full and threshold tax. On material grounds, subjects should be indifferent between the full and threshold tax. However, the threshold tax may be more acceptable because the scheme separates the incentive and income effects; providing the same incentives at the margin, while transferring less money from the polluter to the government (Pezzey, 2003). If so, we suspect that any effect from a trial period with an efficient tax (full or threshold) will mitigate any initial bias for the threshold tax; thereby increasing the acceptability of the full tax relative to the threshold tax. Correspondingly, we present the third null hypothesis: $\mathbf{H_O}^{\mathbf{3}}$: a trial with an efficient tax does not affect the relative support of full and threshold taxes.

4. Results

As the interest in Pigouvian taxes is to improve efficiency, we begin by reviewing the actual efficiency achieved in our treatments before we proceed to analysing voting behaviour. Table 2 reports the mean payoffs, market prices and units purchased by treatment in stages 1 and 2. First, the separate instances of the no-tax setting reveals that the market outcomes improved in each subsequent instance – average payoffs increased, and both market prices and the number of units purchased approached equilibrium predictions. Next, as theory would predict, average payoffs are consistently higher with either tax than without a tax. The absolute difference in average payoffs between no tax and either tax scheme is somewhat smaller than the predicted difference (11–15 vs. 16 tokens).[9]

The average price is consistently around 11. This does not affect incentives (in terms of which scheme to vote for or how many units to purchase), but it does reduce payoffs compared to theoretical equilibrium payoffs. While the incentives are theoretically identical across the schemes, payoffs are slightly higher with the threshold tax than with the full tax in both stages 1 and 2. This outcome arises because subjects purchase the social optimum of three units more frequently under the threshold tax scheme (75% and 82% of the time) than under the full tax scheme (65% and 67% of the time), which may be due to the threshold, set equal to the social optimum, acting as a focal point.

Table 3 shows aggregate voting behaviour by vote and treatment. The table reports the percentages of votes in favour of the tax in referenda 1 and 2, when the choice was between a tax (full or threshold) and no tax.

The numbers indicate significant levels of tax aversion – opposition to a tax when it is materially beneficial – but the numbers also indicate that a trial run diminishes this aversion. From the first row, 58% of subjects voted in favour of the full tax after a trial with the full tax, as compared to 40% when they had no trial.[10] This result is more pronounced for the threshold tax. From the third row, the difference in the percentage of votes in favour of a threshold tax between those with and without

[9] Calculated within each stage, and excluding the first five periods.

[10] Coincidentally, 18 percentage points is also the increase in support for the Stockholm congestion fee from before to after the trial period (Winslott-Hiselius et al., 2009).

Table 3
Votes in favour of the tax when the alternative is no tax (votes 1 and 2).

Treatment/vote	No trial	Trial
Full tax treatments: votes for full tax (Vote 1)	40% (16/40)	58% (26/45)
Full tax treatments: votes for threshold tax (Vote 2)	60% (24/40)	78% (35/45)
Threshold tax treatments: votes for threshold tax (Vote 1)	42% (21/50)	69% (24/35)
Threshold tax treatments: votes for full tax (Vote 2)	50% (25/50)	51% (18/35)

Table 4
Voting outcomes in the full vs. threshold referendum (vote 3).

Treatment	Votes for Full Tax	Votes for Threshold Tax
Full tax + no trial	22% (9/40)	78% (31/40)
Full tax + trial	31% (14/45)	69% (31/45)
Threshold tax + no trial	14% (7/50)	86% (43/50)
Threshold tax + trial	31% (11/35)	69% (24/35)
Overall	24% (41/170)	76% (129/170)

a threshold tax trial is 69% and 42%. Aggregate numbers therefore suggest that a trial run with a tax scheme increases its acceptability.

We consider the reach of a trial by contemplating whether a full tax trial influences the acceptability of a threshold tax, and vice versa. The aggregate numbers in Table 3 offer mixed signals. Support for a threshold tax is greater when voters had a full tax trial relative to when they had no trial with either tax (78% vs. 60%), but support for a full tax was not significantly influenced by having a threshold tax trial (51% vs. 50%).

To further explore the influence of trials, we consider whether a trial run with a tax causes an increase in the support for the full tax relative to the threshold tax, presuming an initial bias towards the threshold tax. The aggregate numbers do suggest a strong preference for the threshold tax despite the fact that the full and threshold tax are materially equivalent at equilibrium, and further, the numbers imply that a trial period increases the relative support for the full tax. Specifically, as Table 4 reports, 24% of voters across all treatments supported the full tax over the threshold tax, but stratifying by trial and no-trial reveals that support for the full tax was greater when subjects had a trial run with a full or threshold tax (31% vs. 18%). Aggregate numbers suggest that trials mitigate initial biases against the full tax and increase relative support for the full tax.

We now turn to a conditional analysis of individual voting behaviour to further develop our initial impressions. To examine the probability of voting in favour of a proposed tax, we estimate the following linear probability model:

$$Y_{it} = \alpha + \psi Trial_i + \delta TaxVote_{it} + \gamma Trial_i \times TaxVote_{it} + \phi_t + u_i + \varepsilon_{it},$$

where Y_{it} is a limited dependent variable that indicates whether the ith subject voted in favour of the proposed tax in period t (=1 if yes; =0 otherwise); $Trial_i$ is a vector of indicator variables that signifies the trial setting of the ith subject – full tax trial and threshold tax trial (=1 if yes; =0 otherwise; no trial is the omitted baseline); $TaxVote_{it}$ is a binary variable that indicates the referendum for subject i in period t was a choice between a threshold tax and a full tax; $Trial_i \times TaxVote_{it}$ is a vector of interaction terms that capture the vote-specific effect of the trial runs; ϕ_t is a set of $T-1$ dummy variables that capture potential nonlinear period effects; α is the estimated intercept, u_i are random effects that control for unobservable individual characteristics (e.g., risk aversion), and ε_{it} is the well-behaved error term.[11] To ensure any treatment effect of the trails is not confounded with the potential influence of prior experience with tax schemes that were implemented via referenda, the model includes a control

[11] The between-treatment design requires that individual effects are conditioned using a random effects specification, which LM tests confirm are significant ($p = 0.003$). Period-specific effects are jointly significant ($p = 0.059$). Clustering at the group level yields robust standard errors.

Table 5
Panel model estimates of voting models.

	Full tax	Threshold tax
Constant	0.390***	0.420***
	(0.079)	(0.054)
Trial w/full tax	0.188*	0.122
	(0.104)	(0.090)
Trial w/threshold tax	−0.096	0.266***
	(0.094)	(0.098)
Threshold vs. full vote	−0.215**	0.449***
	(0.092)	(0.079)
Trial w/full tax and threshold vs. full vote	−0.056	−0.232**
	(0.092)	(0.112)
Trial w/threshold tax and threshold vs. full vote	0.231**	−0.395***
	(0.112)	(0.143)
Tax in previous referenda	0.197**	0.333***
	(0.096)	(0.093)
Tax in previous referenda and threshold vs. full	−0.192*	−0.404***
	(0.109)	(0.115)
χ^2	90.44	62.21
(p value)	(0.000)	(0.000)
# of clusters	34	34
N	340	340

Note. Dependent variables are a binary variable equal to 1 for a yes vote and 0 for a no vote; estimates control for individual-specific and period-specific effects and cluster at the group level; robust standard errors are reported in parentheses unless otherwise noted; and *, ** and *** indicate significance at the 10, 5 and 1 percent levels.

variable that signifies whether the subject had experience with a tax from a previous referendum and an interaction of this variable with the threshold vs. full referendum to allow a vote-specific effect.[12]

Two linear probability panel models are estimated, a full-tax model and a threshold-tax model.[13] In the full-tax model, the proposed tax is the full tax and the model estimates the decision to vote in favour of the full tax as a function of the treatments (no trial, full tax-trial, and threshold-tax trial), the alternative to the full tax (no tax or threshold tax), and interaction terms that disentangles any referendum-specific treatment effects for the full vs. threshold vote. The threshold-tax model works equivalently. The proposed tax is the threshold tax and estimates condition the decision to vote in favour of the threshold tax on the treatments, the alternative and an interaction to capture any differential treatment effects across referenda type. To estimate the models, we first stratify the data by vote type: (a) full-tax vs. no-tax, (b) threshold-tax vs. no-tax and (c) threshold-tax vs. full-tax. Estimation of the full tax model only uses data from referenda that present the full tax as an option; (a) and (c). Likewise for the threshold tax model – (b) and (c). Note the data from the full-tax vs. threshold-tax referendum (type c) is present in both models, but the vote is coded to reflect the model's orientation.[14]

Table 5 reports the estimated coefficients from the linear probability models. The conditional estimates reject the first null hypothesis and corroborate the aggregate findings that a trial run with an efficient tax can improve the acceptability of that tax. From the full-tax model, estimates suggest that a trial with a full tax significantly increases the likelihood of voting in favour of the full tax; increasing it by 18.8 percentage points ($p = 0.07$). The result is stronger in the threshold model with estimates indicating that a trial with a threshold tax significantly increases the likelihood of voting in favour of the threshold tax by 26.6 percentage points ($p < 0.01$). However, it appears the influence of trial runs

[12] Note the *potential* for influence only exists in the second and third referenda, not the first.
[13] We prefer the linear probability model but results were robust to the alternative logit specification.
[14] Support for proposed tax equals 1 (0 otherwise); therefore, a vote for a full tax in the full vs. threshold referenda is coded as 1 in the full-tax model and coded as 0 in the threshold-tax model.

is not without limits. Estimates show that a trial with a threshold tax has no significant effect on the likelihood of supporting a full tax ($p = 0.18$) and a trail with a full tax does not influence the support of a threshold tax ($p = 0.31$). Therefore, trial runs of an efficient tax may mitigate tax aversion and bolster support for that same tax scheme, but this effect does not appear to extend to different tax schemes. This suggests that trial runs improve the understanding of a specific Pigouvian tax scheme rather than a general understanding of Pigouvian taxes.

Turning to the third hypothesis, we examine the impact of a trial run on the relative preference of the two efficient tax schemes. We first point out that the estimated coefficients for the threshold vs. full variable in each model reveal a strong preference for the threshold tax. The likelihood of supporting a full tax decreases about 21.5 percentage points when the alternative is a threshold tax instead of no tax, and the likelihood of supporting the threshold tax increases by about 44.9 percentage points when the alternative is a full tax as opposed to no tax. Considering that subjects should be generally indifferent between the full and threshold taxes, we examine whether trials can shift the preference for the threshold tax closer to indifference. Estimates provide some support for this conjecture. In the threshold tax models, estimated coefficients on the interaction terms reveal that a trial run with a full or threshold tax shifts support from a threshold tax to a full tax ($p = 0.04$ for full trial and $p < 0.01$ for threshold trial). Results from the full tax model offer corresponding evidence that trial runs with a threshold tax, but not full tax, influence relative preferences of the two tax schemes ($p = 0.07$). Thus, estimates reject the third null hypothesis that trial runs do not affect the relative support of the alternative taxes.[15]

5. Discussion

Our results reveal three main findings. First, as observed in the case of the congestion charge in Stockholm, a trial run with an efficient tax can significantly increase the acceptability of the tax. This result arises whether the tax is a full tax or a threshold tax. Second, the influence of a trial appears limited because a trial period with one efficient tax does not influence the acceptability of another efficient tax. Third, the threshold tax was overwhelmingly preferred to the full tax despite being materially equivalent, but a trial with an efficient tax seemed to mitigate this initial bias, increasing the relative support of the full tax.

The positive result of a trial run indicates that tax aversion is related to a bias or misperception that can be overcome. In this experiment the opposition to a Pigouvian tax is overcome with a trial period, whereas providing information alone did not increase support for efficient taxes in a previous experiment (Kallbekken et al., 2011, used a cheap-talk design to test the effect of information and of labelling). Of course, in the world outside the lab there are other factors that will interact with this result, such as the immediacy of the behavioural effects from the policy, availability and cost of alternatives, and the ease of changing behaviour. However, people often find it easier than expected to change behaviour and find this new behaviour more favourable than expected, as illustrated by experiences with the "most popular tax in Europe," the Irish plastic bags levy (Convery et al., 2007), and the Stockholm experience itself (Schuitema et al., 2010).

Additional research could provide further insights on the potential of trials to mitigate tax aversion. If tax aversion is mitigated because people initially underestimate the environmental benefits of the tax, and a trial run corrects this misperception, then the possibility of using trials as a means to boost support might be limited to cases where the tax will produce immediate, homogeneous and observable benefits for the involved parties. If, however, tax aversion is mitigated because people, for instance, find it easier than expected to change behaviour, then the use of a trial period as a policy tool has wider applicability. As Schuitema et al. (2010) point out in the final sentence of their paper, "making sure that people have the opportunity to gain positive expectations, and when possible, positive experiences with road pricing is one important factor for securing public support."

[15] Note that the final two explanatory variables are included to separate out the potential influence of prior experience with taxes due to the referenda; thereby isolating the effect from the trial run.

Another potential caveat is that people might not trust that a trial is merely a trial (Milton Friedman famously argued that nothing is as permanent as a temporary government programme). If people fear that the trial will be made permanent independently of the results and subsequent deliberations, a trial might face as strong opposition as a permanent implementation of the tax. This potential problem can be overcome with credible assurances that the trial is temporary (e.g., if implementation requires achieving a majority in a referendum).

The preference for the threshold tax over the standard (full) Pigouvian tax indicates that the revenues are crucial for support, as revenue collected is the only difference between the two taxes. This implies that distrust/risk aversion/loss aversion is possibly part of the reason for lack of support for (full) Pigouvian taxes. The popular but less common (and less researched) threshold tax may provide an interesting and politically relevant alternative to the more conventional full tax.[16] Under certain assumptions (see Pezzey, 2003, 2006), a threshold tax provides the same outcome as a standard tax except for the smaller revenues it raises. Threshold taxes have not been widely considered, but they are a compelling alternative because the tax separates, in part, the incentive and income effects (Pezzey and Jotzo, 2013); therefore providing a framework that reduces the externality to the optimal level while also providing less reason for political opposition.

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[16] While Pigouvian threshold taxes are uncommon, income taxes quite often have thresholds (for distributional reasons).

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