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Do you not like Pigou, or do you not understand him? Tax aversion and revenue recycling in the lab

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Abstract
Tax-aversion reduces the likelihood that price rationing can be a politically viable tool for environmental protection. We examine the case of the classic Pigouvian tax to control a negative externality, and consider how recycling the revenues, labeling of the tax and information about its purpose affects the support for taxation. We test the support for taxation within a single-price market experiment, in which purchases by some buyers impose external costs on others. Observing behavior consistent with tax-aversion, we also find that recycling the revenues to more narrowly targeted groups seems to increase support for taxation. In the absence of narrow revenue recycling, labeling a Pigouvian instrument as a 'tax' may significantly lower the likelihood of voter support.

Keywords
Pigouvian tax; Experiments; Tax aversion; Behavioral economics

1. Introduction

Over the last two decades, environmental policies have increasingly shifted from command-and-control regulation to incentive-based policies such as tradable permit schemes and Pigouvian taxes (i.e., taxes levied on market activities to internalize the cost of negative externalities). However, despite successes, such as Title IV of the 1990 Clean Air Amendments in the U.S. or user fee systems in Europe, programs that use market mechanisms and that are recommended by mainstream economists still face considerable skepticism from both the public and politicians alike. The lack of public support can be an impediment to achieving environmental targets through Pigouvian taxation, as illustrated by the current prospects of enacting a carbon permit system in the U.S.

Opinion polls, however, indicate strong support for environmental policies in general. Sixty-five percent of U.S. citizens think their county should reduce its greenhouse gas emissions regardless of what other countries do, and 69% of them think they should undertake a medium- or large-scale effort to reduce global warming [1]. This general support for climate policy diverges strongly, however, when it comes to support for specific policies. While 87% support funding more research into renewable energy, and 83% support providing tax rebates for efficient cars and solar panels, only 35% support increasing taxes on gasoline by 25 cents per gallon.[1] The public appears to support the intention of environmental policies, while they oppose increased taxation as a means to implement these policies. Given the fragile support for efficiency-enhancing policies, the questions of whether and how support can be increased is important. We conduct laboratory experiments to test several policy options that may influence the support for Pigouvian taxation.

1.1. Literature review

There is a large literature examining public support for taxation. When it comes to Pigouvian taxes, there has been a particular focus on transport-related taxes, and the work has been done primarily through large-scale surveys and focus group interviews. This body of research has identified many factors that seem to be important determinants of people's attitudes towards taxes. We will focus on the factors that are most relevant to policy, in particular factors relating to how the tax is designed and communicated.

The question of how tax revenues should be redistributed has been shown to be crucial for the acceptability of tax schemes (e.g., [3]). The allocation of revenues matters both for the perceived fairness of the instrument [4], [5] and [6], and more generally because of distributional concerns [7] and [8]. While there are strong theoretical arguments in favor of adding tax revenues to the general government budget [9], more targeted revenue recycling schemes have been proposed both because they can increase support, but also because of fairness considerations. Pigouvian taxes are often regressive, and one manner in which governments deal with this undesirable effect is to make lump-sum transfers to low-income families (e.g., the carbon tax in British Columbia).

One reason why the use of revenues is so crucial for public support might be a general failure to recognize that the tax will produce environmental benefits, independently of how the revenues are used. People might not see the difference between a Pigouvian tax—whose primary objective is to reduce activities that have negative externalities, and a Ramsey tax—whose primary objective is to raise revenue. To be more specific, people might not understand that a Pigouvian tax provides incentives to reduce the externality and thereby increase welfare. They believe instead that the tax only results in redistribution. This hypothesis is supported by Dresner et al. [7], who found that "what seemed to underlie the thinking among both focus groups and some businesses was a view of taxes solely as a means of raising revenue, rather than in terms of their incentive effects."

Another reason why earmarking is popular might be that without earmarking taxpayers have no clear idea of what the money is spent on, and they might believe it is spent "wastefully or even fraudulently, or that a substantial part of it goes for services of which they disapprove" [10]. Trust seems to be a key issue. Summarizing several country studies on impediments to environmental tax reform, Clinch et al. [11] concluded that "lack of trust in government was considered a key impediment in all countries." This mistrust of government could lead to a type of tax aversion, as found by McCaffery and Baron [12]: "For some people, and for some kinds of programs, the label 'tax' is enough to arouse a negative reaction.

There is an abundance of evidence in the economics and psychology literature that framing matters in many choice situations. One of the most famous examples is the finding that cooperation in a Prisoner's Dilemma game is significantly higher when the game is labeled "The Community Game" than when it is labeled "The Wall Street Game" [14]. Relating the issue of framing to taxation, McCaffery and Baron [12] tested the importance of labels in a survey on taxation. Their main finding was that "calling something a 'tax' affects people's attitude toward it, and their attitude is predictable from properties of the service and the subject." Such effects of framing have been found in many studies since. Relevant for public policy questions, Robinson et al. [15] observed that people perceive a certain number of deaths described generically as less bad than the same number presented with a context. Framing has also been found to matter for real-world votes. Bütler and Maréchal [16] found in a natural field experiment that the wording of an otherwise virtually identical question on a Swiss referendum on the decrease in the legal age of retirement led to significantly different approval rates.

Framing effects are also reported in voting experiments. Sausgruber and Tyran [17] found evidence that voters in experimental referenda suffer from fiscal illusion: they prefer indirect taxation over direct taxation even when the overall effects are the same. Kallbekken et al. [18] conducted an experiment that was intended to compare environmental tax schemes under different revenue redistribution mechanisms. They reported no significant differences in popularity between the different schemes due to a surprisingly large share of votes against taxation—almost half the subjects voted against tax schemes that would have been to their own material benefit (and that would have increased overall welfare), a result which can be seen as a type of tax aversion.

The findings, however, did highlight the importance of fairness/distributional considerations for the support for Pigouvian taxes.

1.2. Research questions

The use of experiments to examine policies for externalities started with Plott [19], who compared the efficiency of different policy tools to address externalities. There are very few studies, however, that use experiments to explore the issue of support for Pigouvian taxation. This is surprising given that experiments offer the ability to vary one or a few factors of particular interest, while controlling for other factors, which is particularly relevant for Pigouvian taxes. It is difficult to provide a good estimate of, for instance, the marginal damage of tailpipe emissions from cars in a city center, and very rarely are proposed tax rates based on such estimates. In the lab the researcher can define payoff functions, know and implement the optimal tax, and thus allow the study to focus on the Pigouvian tax as an instrument per se rather than deal with uncertain and unknown variables.

The brief literature review showed that both the design and communication (framing) of a tax can influence public support. When it comes to the design of Pigouvian taxes, it seems clear that the use of revenues is a crucial issue. An important reason why narrow recycling of the revenues (i.e., earmarking) can increase public support significantly, despite opposite recommendations by most economists, could be that people do not understand that a Pigouvian tax gives incentives to reduce the externality and instead view it as equivalent to a Ramsey tax. This is the motivation for our first research question:

Research question 1: Does the opposition to Pigouvian taxes result from a lack of understanding about the workings and effects of the instrument?

We examine this research question by testing whether opposition is influenced by providing an explanation of how the incentive structure works. In particular, the explanations, which included a numerical example, pointed out that the tax would result in less additional costs in the market and that the sum of earnings in the market would be higher with the tax than without. The instructions also pointed out that this did not mean that each participant would necessarily be better off individually; rather, the amount earned by each individual would depend on whether or not a tax scheme was imposed, his or her type, and the way that the revenues would be redistributed under the tax scheme. In addition to the observed behavior in the experiment, we also ask questions in the post-experimental survey, which help us address this research question.

This approach, adding specific explanations to the instructions, resembles the “cheap talk” approach used in experiments on stated preferences. For example, Cummings and Taylor [20] found that explicit warnings about cognitive biases increase consistency in subjects' valuations. Aadland and Caplan [21], however, observed that a cheap-talk script with neutral language does not mitigate the bias.

The effect of cheap-talk instructions is also unclear in more strategic environments. For example, Brosig et al. [22] found that video instructions in a public goods game that pointed out why it is socially optimal to cooperate had a small but significant positive effect on cooperation rates in such a game. Oxoby and Spraggon [23], however, do not observe such an effect from written instructions in a similar public good situation.

Our second research question focuses on the role of framing a Pigouvian tax. The literature indicates that people both mistrust the actual purpose of a Pigouvian tax (revenue raising versus influencing behavior) and how the revenues are spent, and that using the label "tax" might be sufficient to raise a negative reaction.

Research question 2: Is the general aversion to taxation so strong that support is significantly reduced by simply labeling a Pigouvian instrument a "tax"?

We examine this question by comparing voting behavior when the Pigouvian instrument is referred to as a tax and a fee. Using the typology in Levin et al. [24], the difference between the labels "tax" and "fee" can be considered to have a *valence framing effect*—critical information is cast in a positive or negative way. Levin et al. distinguished between three different frame types: risky choice, which refers to how a set of options with different risk levels is framed; goal, which refers to how a consequence or implied goal of a behavior is framed; and attribute, when object/event attributes or characteristics are framed. They survey 37 studies with attribute frame, the most relevant frame for us, and find *valence-consistent shifts* were reported in 35 of them—i.e., positive framing of attributes leads to more favorable evaluations than negative framing.

One way to reduce the opposition to taxation is to narrowly recycle (earmark) the revenues. This can be done in many different ways. What is perceived as a fair earmarking scheme may depend on whether people perceive the externality (e.g., pollution) as an infringement, or whether it is the tax that they perceive as an infringement. Coase [26] suggested that in the presence of an externality, the optimal outcome can be achieved independently of the distribution of property rights, e.g., whether the polluter holds a right to pollute, or whether the victim holds a right to a pristine environment. Examples of both can be found in earmarking schemes. The revenues from the French tax on aviation noise is used to soundproof nearby houses, and the revenues from the Japanese SO_x tax are used to compensate for health damages caused by air pollution. On the other hand, revenues from the Swedish NO_X-tax on energy production are returned almost fully to the producers themselves, and grandfathering permits in cap-and-trade programs could also be considered as compensation for polluters. More targeted redistribution schemes can result in a more equal distribution of net benefits, since such redistributions often reduce either the losses of remaining pollution or the losses through the tax burden. This is the motivation for our final research question:

Research question 3: Does recycling the revenues from a Pigouvian tax in a narrowly targeted way, as opposed to adding the tax revenues to the general public budget, increase support for taxation?

We examine this research question by comparing, *ceteris paribus*, voter preferences across three tax schemes that differ with respect to how the revenues are redistributed. One scheme is representative of general redistribution, while the two other schemes are representative of targeted redistribution (i.e., earmarking). While experimental economists (and psychologists) have examined several features related to taxation, experimental investigations into revenue recycling schemes are rare. Experiments that combined taxation with voting decisions dealt either with voting on the tax itself, or with the use of tax revenues given that a tax will be implemented. For example, tax revenues are not returned at all in a market-with-externalities-and-taxes experiment in Tyran and Sausgruber [28] (but some subjects receive a flat payment in order to avoid negative gains), and in Sausgruber and Tyran [17] revenues from a sales tax (without externalities) are returned lump-sum. In the experiment by Durante and Putterman [29] subjects choose the level of redistributive taxation to be applied to the initial distribution of endowments. The design closest to ours in terms of tax revenue recycling is Ackert et al. [30], where the subjects vote for one of two revenue-neutral tax alternatives. The tax in their experiment is, however, not designed to address any externalities.

2. Experimental design

We develop an experimental design that addresses the three research questions. Four elements of the design are described below: the basic structure of the experimental setting (the market), the details of the treatments, the questionnaire, the experimental procedure and, finally, the null hypotheses arising from the experimental design.

2.1. The market

Our experiment consists of a market for a fictitious good in which some buyers impose external costs on others through their purchases. After initial trading periods without taxation, buyers participate in four votes, in which they face binomial choices between tax schemes with different rules for how to distribute the collected tax revenues (including a *no tax* scheme).

Since we are not interested in the market as such, but in the voting behavior, we employ a variation of a uniform-price, multi-unit auction, which is predicted to – and indeed does – converge quickly towards the equilibrium (see [17], [28] and [35]).

Each experimental market consists of five (human) buyers and one (automated) seller. In each period each buyer can buy a maximum of four units. The buyers are informed about their resale values (which are 75, 60, 45, and 30 tokens, respectively, for the four units), and also that the seller's marginal cost will remain constant throughout the experiment. The seller's marginal cost is 20 tokens per unit, and therefore the equilibrium unit price in the market is also 20 tokens.

The buyers indicate their willingness-to-pay (WTP) in the first stage of each period. This is the WTP for each unit (instead of being asked for a unique WTP for each of the four units), and then the seller 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 four), while all buyers with a WTP

below the market price (and therefore below marginal costs) are excluded from making any purchases in this period.

While a Pigouvian tax results in more efficient outcomes overall, the gains (and losses) from such a tax are distributed unequally and depend on whether an economic agent is a “polluter,” a “victim” from pollution, both, or neither. To mirror this real-world categorization and to be able to implement schemes for the recycling of tax revenues, we design a market with five buyers of four different types—two of type 1 and one each of types 2, 3 and 4. Type 1 represents both a polluter and a victim of pollution, and type 3 represents a polluter only. Type 2 represents a victim only, and type 4 is a bystander (neither victim nor polluter).[7] (See Fig. 2.1.) Thus whenever polluters (types 1 and 3) purchase one unit of the good, victims (types 1 and 2) will each incur an external cost set at 6 tokens.[8] The optimal Pigouvian tax rate is therefore 18 tokens (6 tokens damage each for one buyer of type 2 and two of type 1).

In the equilibrium, without taxation, all buyers purchase four units at a price of 20. Since purchases by the three polluters (two buyers of type 1 and one of type 3) result in external costs for others, twelve of the 20 units bought in equilibrium result in external costs. The socially optimal outcome, given that the market price is below 27, occurs when the three buyers whose purchases cause external costs (i.e., the polluters) purchase only three units each.[9] This outcome results in a social improvement of 24 tokens over the equilibrium outcome since a reduction of three “polluting” units

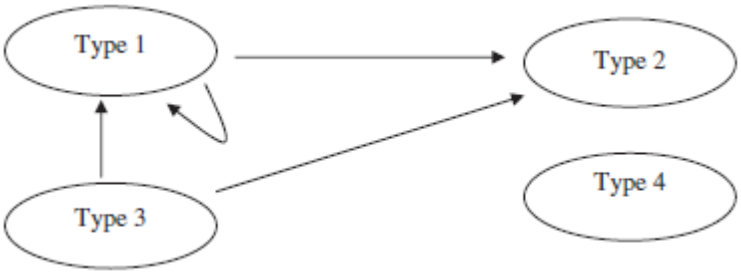

Fig. 2.1. The types of subject and their interactions through externalities.

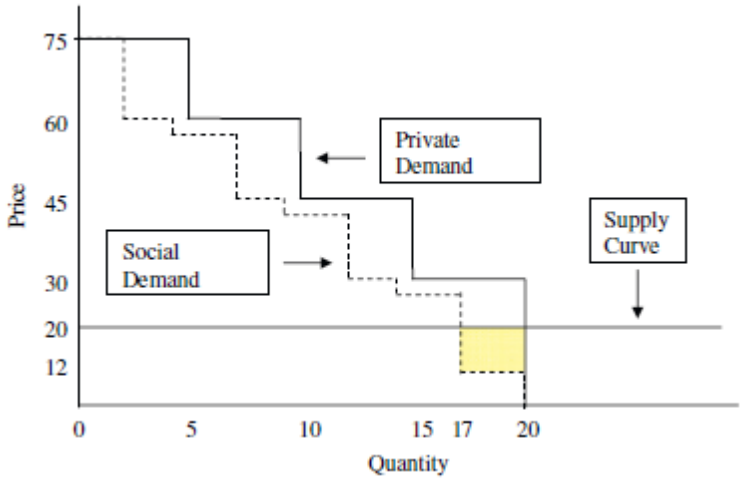

Fig. 2.2. Private and social demand curves.

Table 2.1. Between-subject treatments in the experiment.

		Explanation of the incentive effect	
		Explanation in instructions	No explanation
Word used to describe the instrument	Tax	*Explain + tax*	*Don't + tax*
	Fee	*Explain + fee*	*Don't + fee*

purchased saves 18 tokens per purchase for a total of 54, but also diminishes the consumer surplus by 30 tokens (3×(30−20)). The shaded area in Fig. 2.2 represents the efficiency improvement of the social optimum over the market equilibrium.

One way to induce socially optimal behavior by the polluters is to impose a Pigouvian tax equal to the external cost of 18 on polluting units only. Buyers of types 2 and 4, the non-polluters, do not have to pay the tax. With such a tax, the new equilibrium would still yield a market price of 20 as can be seen in Fig. 2.2. The equilibrium quantity, however, is now 17—the three polluters take the tax into account and buy a total of only nine units instead of twelve. Because they still purchase nine units, the tax generates revenue of 162 tokens, which has to be redistributed to the buyers to make the entire group of five buyers better off on the aggregate in the social optimum compared to the market equilibrium. This feature of only small consumption changes following the tax implementation is employed in order to mimic real-world behavior where such consumption changes are also relatively small.

In the first ten periods of the experiment, no tax is levied on any of the subjects, and the pure market game is played. Subjects play the role of each type several times during these ten periods, but starting from period 11 they remain the same type for the rest of the experiment. The subjects are informed about this. There are two reasons for having the subjects switch types during the first ten periods: The primary reason is that the subjects become aware of how the incentives faced by the different types differ. The secondary reason is that payoffs are evened out in the first ten periods.

2.2. Treatments and payoffs

The experiment had two between-subject treatment variables and one within-subject treatment variable. Table 2.1 provides the 2×2 between-subjects design, which varies two elements of the basic framework: explanation; *explain* and *don't explain*, and labeling; *fee* and *tax*. In the *explain* treatment, subjects received, in addition to the regular instructions

Table 2.2. Tax schemes.

Scheme	Description
No tax	No tax implemented
Tax scheme *General*	Pigouvian tax implemented with revenues redistributed to all subjects in equal lump-sum payments.
Tax scheme *Polluter*	Pigouvian tax implemented with revenues redistributed only to types 1 and 3—the polluters.
Tax scheme *Victim*	Pigouvian tax implemented with revenues redistributed only to types 1 and 2—the victims from the pollution.

Table 2.3. Equilibrium payoffs by type under the four tax schemes.

Tax scheme	*Type 1*	*Type 2*	*Type 3*	*Type 4*	*Total (group) payoff*[a]	*Gini Coefficient*
No tax	58.0	58.0	130.0	130.0	434.0	0.199
General	44.4	108.4	98.4	162.4	458.0	0.262
Polluters	66.0	76.0	120.0	130.0	458.0	0.159
Victim	66.0	130.0	66.0	130.0	458.0	0.166

[a] There are two subjects of type 1 in each group.

that all subjects received, explanations of how the instrument works. [10] In the *don't explain* treatment, subjects did not receive any explanations beyond the regular instructions. In the *tax* treatments, the instrument was referred to as a tax and, in the *fee* sessions, the instrument was termed a fee. The explanation and labeling treatments do not affect payoffs.

The within-treatment variable, tax scheme, has four alternatives—*no tax*, *general*, *victim* and *polluter*—and is summarized in Table 2.2. As described, the *no tax* scheme leaves the market unchanged. Under the *general* scheme, revenues are distributed lump-sum, while the *polluter* and *victim* schemes are representative of targeted revenue recycling schemes with the revenues directed to the polluters or victims. The three redistribution schemes are equally efficient (and more efficient than the *no tax* case), but the two targeted schemes (*victim* and *polluter*) result in a more equitable payoff distribution as compared to *no tax* and the *general* scheme. The treatment variable tax scheme is therefore used to explore whether a more equal outcome can increase support for taxation.

Payoffs under the four tax schemes vary significantly across subject types, which allow us to examine the importance of the size of the incentives in explaining support for taxation and considerations of equity. Table 2.3 displays the monetary payoffs for all five buyers under the different tax schemes, given that the market settles at the respective equilibria (price of 20 and quantity of 20 in scheme *N*; price of 20 and quantity of 17 in each of the three tax schemes). On payoff sizes, the table reiterates that all three tax schemes yield a higher total payoff for the entire group (by about 5%), but each tax scheme does has winners and losers as compared to the *no tax* scheme. On payoff equity, the Gini coefficient in the last column of the table shows that the overall distribution is quite different across the four schemes; using revenues to compensate victims or polluters leads to smaller Gini coefficients, which means the distributions are more equitable, while a lump-sum redistribution of revenues results in a more unequal distribution of final equilibrium payoffs.

There are votes on taxation in periods 11, 15, 19 and 23. Subjects vote on the three tax schemes and the *no tax* scheme. Because the subjects have had no experience with the effects of a tax scheme by period 10, the main stage of the experiment is preceded by a vote between the two tax schemes with targeted revenue redistribution: *polluter* vs. *victim*. By having to vote between two tax schemes, subjects will gain some experience with the effects of implementing a tax scheme in periods 11–14, and this will reduce the information (or experience) asymmetry between taxation and *no tax*. The primary focus of the analysis is on the subsequent referenda that pair each tax scheme with the *no tax*

option—vote 1: *no tax* vs. *general*; vote 2: *no tax* vs. *polluter*; and vote 3: *no tax* vs. *victim*. Since no order effects were observed in a similar design used by Kallbekken et al. [18], we decided to keep the order of votes constant across all sessions.

2.3. Questionnaire

To help us understand and analyze the observed voting behavior, we carry out an ex post questionnaire. The questionnaire is incentive-compatible in that subjects get paid for correct answers. The questionnaire was implemented after trading and voting to avoid the risk that the questions we asked would produce any bias in the voting decisions. We ask twelve questions in total, three relating to each of the four votes. For each vote we ask:

(1) Under which tax scheme, *X* or *Y*, will the types that impose additional costs on others purchase the least number of units?

(2) Under which tax scheme, *X* or *Y*, will you personally receive the greater payoff, provided that the other participants behave rationally?

(3) Under which tax scheme, *X* or *Y*, will the sum of payoffs for all five participants be greater?

For all questions, three alternative answers were provided: “Tax scheme *X*”, “Tax scheme *Y*” or “The tax schemes make no difference.” Note that the correct answers vary depending on the assigned type of the respondent and the schemes that are being voted on.

2.4. Experimental procedure

The experiment was conducted at the University of Innsbruck (Austria) with a total of 160 students as participants. There were a total of eight sessions with two sessions per treatment and four independent markets per session (32 independent markets for the entire experiment). All sessions were programmed and conducted using the software *z*-tree [36]. Students who had previously participated in similar experiments were not invited to participate. To minimize the risk of any experimenter bias, the same person read the instructions and led the experiment in all the sessions. Each session lasted about 100 min, including reading the instructions and the questionnaire at the end of the session. Subjects earned an average of €28 ($36 at the time of the experiment), including a show-up fee of €6 and €0.50 for each correct answer in the questionnaire.

2.5. Hypotheses

The experimental design yields three primary null hypotheses that correspond to the three research questions presented in Section 1.2. To investigate whether opposition to Pigouvian taxes arises from a lack of understanding about the instrument, we test the first null hypothesis (H_1): vote counts are equal across the *explain* and *don't explain* treatments. To address whether any aversion to taxation is exacerbated by referring to the instrument as a tax, as opposed to a fee, we have the second null hypothesis (H_2): vote counts are equal across the *tax* and *fee* treatments. And to examine whether support for a Pigouvian instrument is greater when the revenues are more narrowly

recycled (i.e., earmarked), we have the third null hypothesis (H_3): vote counts are equal, *ceteris paribus*, across the *general*, *victim* and *polluter* redistribution schemes.

3. Results: market activity and tax aversion

As expected, there was a relatively fast convergence to the equilibrium price in all treatments and all groups. The average price is around 30 tokens in the first period (range 20–50), and falls to 21.5 tokens by period 10 (the last period before the voting begins). The median is 20 from period eight onwards. Fig. 3.1 displays the average and median prices for all 32 groups across all periods.

Subjects acted quite rationally in their purchase decisions. The optimal number of units they should purchase (to maximize own payoff) depends on the price and the tax scheme. Overall 75% of all choices are consistent with self-interest. If we account for the fact that it is not always possible to buy more units than optimal (as subjects cannot buy more than 4 units under any circumstances), we find that subjects buy too many units 17% of the time when it is possible to do so, and too few 19% of the time. On aggregate it therefore seems as if the deviations from the optimal number are random.

The results suggest a significant degree of tax aversion; a large number of votes were cast against taxation in cases where taxation would have maximized both personal and group payoff. Of the 480 individual votes involving one of the tax schemes and the *no tax* alternative, 416 votes were cast when one alternative generates a higher equilibrium payoff for a subject than the other. Table 3.1 shows that in the 256 cases in which it would have materially benefited subjects to vote in favor of a tax scheme, 37% opted for *no tax.* In the 160 cases where *no tax* would have been individually rational, only 16% voted in favor of a tax scheme. Subjects are more than twice as likely to vote against taxation when it is in their material self-interest (tax-averse “mistakes”), than they are to vote in favor of taxation when it is not in their self-interest (tax-loving “mistakes”). The difference

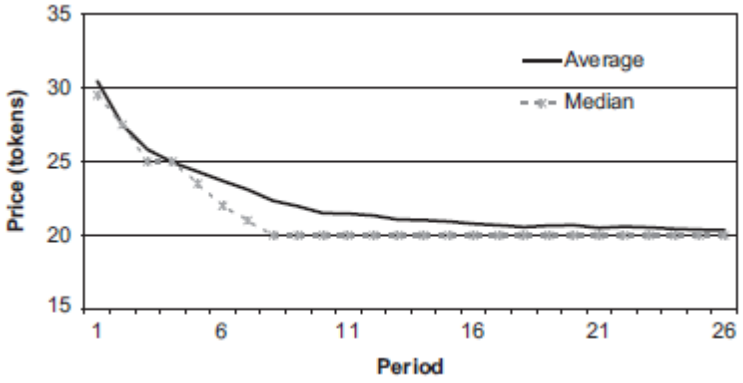

Fig. 3.1. Average and median unit price by period.

Table 3.1. Share of consistent votes by treatment.

	Yes votes against self-interest	No votes against self-interest	*N*
Pooled	25(16%)	95(37%)	416
General	12(13%)	16(25%)	160
Polluter	11(34%)	47(49%)	128
Victim	2(6%)	32(33%)	128

Note: The table excludes 64 votes cast by subjects who are indifferent from a material point of view as there is no clear definition of "consistent" in these cases.

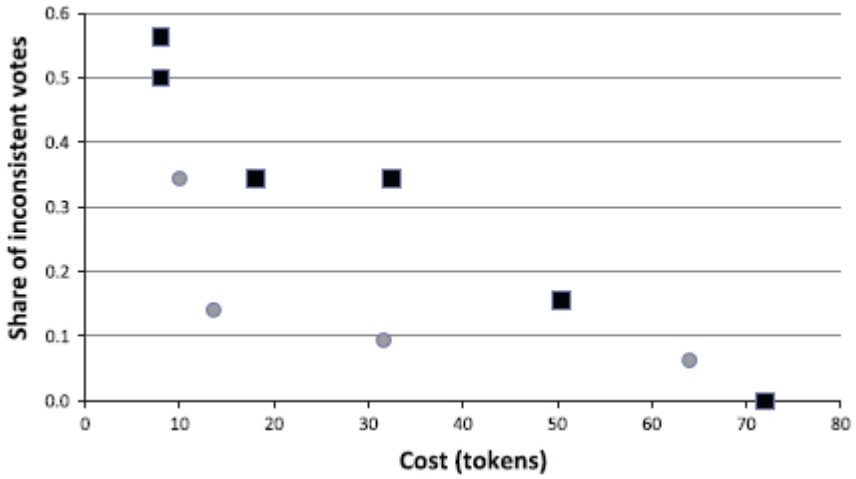

Fig. 3.2. Scatter plot of cost of voting inconsistent with material self-interest against share of subjects who do so. Black squares: votes cast against taxation by subjects with material incentives to vote in favor of taxation. Gray circles: votes cast for taxation by subjects with material incentives to vote against taxation.

between these two behaviors is statistically significant at the 0.1% level (chi-squared test). This indicates that some type of tax aversion might be an exception to the standard payoff-maximizing behavioral model.

Fig. 3.2 illustrates the relationship between the cost of a mistake (the absolute payoff difference between the relevant tax scheme and *no tax*) and the share of mistakes. The black squares represent tax-averse mistakes and the gray circles correspond to tax-loving mistakes. First, the scatter plot indicates that votes follow the law of demand—the higher the cost of voting against the material self-interest, the fewer voters do so. Second, it shows the evidence of tax aversion persists across different costs of voting inconsistently—at any given cost, the share of inconsistent votes is greater when it entails voting against, rather than for, the tax.

4. Results: voting behavior and treatment effects

Turning to our primary hypotheses, we first review the vote counts by treatment reported in Table 4.1. A comparison of voter support across the two between-subject treatment variables reveals little variation—44% and 46% for *don't explain* and *explain*; 41% and 48% *tax* and *fee*. Considering the individual treatments, support for the Pigouvian instrument ranges

Table 4.1. Votes in favor of taxation (total and percentage) by treatment.

	Tax	Fee	Total
Don't	49(41%)	56(47%)	105(44%)
Explain	50(42%)	60(50%)	110(46%)
Total	99(41%)	116(48%)	215(45%)

Table 4.2. Logit estimates for voting models.

	Pooled 1	Pooled 2	*General* Scheme	*Polluter* Scheme	*Victim* Scheme
Constant	−0.95 (0.004)	−0.51 (0.054)	0.08 (0.852)	−0.49 (0.151)	−0.59 (0.150)
Tax	−0.33 (0.368)	−0.33 (0.368)	−2.28 (0.001)	0.52 (0.258)	−0.16 (0.777)
Explain	0.23 (0.520)	0.23 (0.520)	−0.57 (0.360)	0.31 (0.496)	0.63 (0.266)
Tax × Explain	−0.33 (0.370)	−0.33 (0.370)	−2.06 (0.003)	0.10 (0.819)	0.47 (0.403)
Cost	0.06 (0.000)	0.06 (0.000)	0.06 (0.000)	0.04 (0.039)	0.05 (0.000)
Polluter Tax Scheme	0.70 (0.013)	–	–	–	–
Victim Tax Scheme	0.70 (0.023)	–	–	–	–
Gini coefficient	–	−7.02 (0.008)	–	–	–
Log(*L*)	−251.04	−251.07	−62.31	−107.86	−73.54
N	480	480	160	160	160

Note: The dependent variable is individual vote (yes = 1; otherwise 0) and *p*-values reported in parentheses.

from 41% in the *tax & don't explain* treatment to the 50% in the *fee & explain* treatment. Pair-wise chi-square tests fail to reject the null hypothesis that voter support is equal between any two treatments for any of the three tax schemes.

However, a review of the vote counts by tax schemes indicates a need for further analysis. As reported in Table 3.1, the frequency of votes inconsistent with material self-interest ("mistakes") is higher in the two more narrowly targeted revenue redistribution schemes. In the *general* vote, the total frequency of mistakes is 18% (28 out of 160), compared to 45% (58 out of 128) and 27% (34 out of 128) in the *polluter* and the *victim* votes. Further, 79 of the 92 mistakes in the *polluter* and *victim* votes are tax-averse mistakes, of which, type 1 participants (who have smaller stakes than any other non-indifferent type) committed 68 of them.

To account for these and other factors, we undertake a conditional analysis of individual voting behavior. Table 4.2 presents the results from five conditional logit voting models in which the dependent variable equals one or zero to signify a vote in favor of or against the tax. The five models include two pooled models that utilize all voting decisions between a tax scheme and *no tax* and three models that stratify the pooled data by tax scheme. Treatment effects are estimated in all models, with the *fee & don't explain* treatment defined as the omitted baseline. In the pooled models we employ a random effects specification to take advantage of the panel nature of the data to control for individual heterogeneity among subjects. [16] In the first pooled model we use a set of dummy variables to indicate whether a referendum presents the *no tax* option against the *victim* or *polluter* tax scheme (*general* scheme omitted), while in the second pooled model we use the Gini coefficients from Table 2.3 to account for the unequal payoffs distribution among the different schemes. Note that we cannot include both since they are of course highly correlated.

Before discussing our main interests, we confirm that the cost of voting against taxation (difference in payoff between a tax scheme and *no tax*) is positively related to voter support in each model; i.e., consistent with the law of demand. This fundamental relationship offers some confidence in our experimental data and subsequent results, and it is also consistent with results in Ackert et al. [30] who noted that subjects in their experiment voted less for payoff-equalizing taxes with an increase in their personal costs of doing so.

Estimated coefficients for the pooled models correspond to the aggregate results. In nearly all cases, estimates fail to reject the null hypothesis that voting behavior is equal across treatments, indicating that voting behavior is not significantly influenced by labeling the Pigouvian instrument a *tax*, providing an explanation of the instrument, or a combination of both. This result is independent from whether we consider the first or second pooled model. [18] However, results from the tax scheme models reveal that any aversion to the word 'tax' may be related to whether revenues are

Table 5.1. Correct answers by question type and treatment with significance (Mann–Whitney 2-tailed test).

Treatment	Incentive	Self-interest	Efficiency
Don't (%)	63	63	23
Explain (%)	61	61	51
Prob. (MW2tail)	0.652	0.770	0.000***
Fee (%)	57	63	34
Tax (%)	68	60	40
Prob. (MW2tail)	0.043*	0.508	0.380

earmarked. While insignificant in the *polluter* and *victim* models, estimated coefficients for the *tax* treatment are negative and highly significant in the *general* model. Note that this negative effect from the t-word arises with and without an explanation of the instrument (p=0.003 and 0.001). We therefore can reject the null that using the tax label does not affect voting behavior, but only when the revenues from the instrument are redistributed in a lump-sum to all participants. Results from the pooled model further show that the redistribution scheme matters. Estimates indicate that targeted redistributions, either with the *polluter* or *victim* schemes, have a significant positive effect on support, relative to the lump-sum alternative (*general*). Similarly, as indicated by the estimated coefficient on Gini, the decreased inequality related to targeted redistribution has a significant positive effect on support.

We summarize the results from Table 4.2 and our research questions. First, estimates across all models indicate that providing an explanation to participants had no significant effect on voting behavior, which suggests that any opposition to Pigouvian instruments is not due to a lack of understanding. Second, estimates find that using the "tax" label, as oppose to "fee," significantly lowers support for the lump-sum scheme (*general*), but not for the targeted redistribution schemes (*victim* and *polluter*). Thus, in the absence of earmarking, labeling a Pigouvian instrument as a "tax" may lower voter support, or conversely, targeting tax revenues may increase support for a Pigouvian instrument. And third, estimates find that targeted redistribution to *victims* or *polluters*

significantly increases voter support for Pigouvian instruments, relative to the lump-sum redistribution (*general*).

5. Results: insights from the questionnaire

A review of the responses to the questionnaire may offer some additional insights. Overall, the share of correct answers (49%) is surprisingly small, though we observe some potential patterns that may be informative. Of primary interest to us is whether and how the between-subject treatments influence the answers to the questions. We label the questions *incentive* (will types 1 and 3 reduce their purchases?), *self-interest* (under which scheme will your payoff be higher?) and *efficiency* (under which scheme will total payoff be higher?). We consider only the questions involving votes between one of the three tax schemes and *no tax*. Table 5.1 shows the share of correct answers by treatment. The differences are significant in two cases:

The number of correct answers to the incentive questions is significantly greater in the *tax* treatments than in the *fee* treatments ($p<0.05$). This was not a result we expected, and we do not offer an explanation for this result. For the efficiency questions, the number of correct answers is significantly greater in the *explain* treatment than in the *don't explain* treatment. In the *don't explain* treatment, only 23% give the correct answer, and among those who answer incorrectly, 45% believe the group is better off with *No tax*. In the *explain* treatment the share of correct answers more than doubles to 51%, and the share who thinks the group is better off with *No tax* drops to 29%. The difference in the share of correct answers is significant at the 0.1% level (Mann–Whitney 2-tailed test). That the group as a whole will always be better off with a Pigouvian tax than with *no tax* is surprisingly poorly understood, and explanations in the *explain* treatment have a very strong effect on raising the share of correct answers.

We do not find that more subjects understand the incentive effect in the *explain* treatment (the difference is not significant). The additional instructions in the *explain* treatment do point out both effects, but focus more on efficiency. This could help explain why we find a significant effect for the *efficiency* questions but not for the *incentive* questions.

Finally, an interesting finding is that the subjects who understand the incentive effect (i.e., answered the question correctly) are no more likely to understand which scheme is more efficient than the subjects who do not understand the incentive effect: In the *don't explain* treatment, the share of subjects who answer the *efficiency* questions correctly does not differ between those who answer the *incentive* questions correctly and those who do not. In the *explain* treatment, of those who answer the *incentive* questions correctly 58% also answer the *efficiency* questions correctly, while this share is 51% for those who do not answer the *incentive* questions correctly. Thus, it seems that what is obvious to an economist—if the activity causing the externality is reduced, overall welfare will increase—is far from obvious to our subjects, even when an explanation is provided.

6. Discussion

People often vote against tax schemes that serve their own material self-interest and increase social welfare, and we report such tax-averse voting behavior in this study. Such tax aversion raises important behavioral questions on the design and framing of Pigouvian instruments, which matter a great deal for the feasibility of enacting these efficient and welfare improving policies. Herein we investigated three research questions that provide new insights to the challenge of implementing Pigouvian instruments.

First, we offer evidence that opposition to Pigouvian instruments in our experiment is not because voters do not understand the workings and effects of the instrument. The level of support (i.e., vote counts) was not significantly affected by providing an explanation of the efficiency effects of a Pigouvian instrument, whereas the explanation did significantly increase the percentage of subjects who correctly indicated the group as a whole is better off with a Pigouvian tax (23–51%). Not only did explaining the merits of a Pigouvian instrument have no affect on voter support, the increased understanding from the explanation (as indicated by survey responses) did not translate to greater support. Evidence therefore indicates the opposition to Pigouvian taxes does not result from a lack of understanding about the workings and effects of the instrument. We note that, while subjects in the lab, at least theoretically, can calculate the welfare gains of the tax, this task is considerably more opaque in the real world. As such, explaining how Pigouvian taxes work may prove more influential in alleviating real world public opposition.

Second, we provide evidence that framing the Pigouvian instrument as a tax can significantly decrease support for a Pigouvian instrument, but the effect depends on the design of the instrument. Specifically, the “tax” label, as opposed to “fee”, lowers support for instruments that redistribute revenues in a lump-sum, but has no effect on instruments that targets (i.e., earmarks) the revenues. The political motivations behind the labeling of government charges are nothing new, but it is noteworthy to find that earmarking might mitigate the baggage associated with the t-word. Considering public uncertainty surrounding the workings and purposes of taxes, targeting revenues in a way that corresponds to the externality may provide assurances that the instrument is a Pigouvian tax; not a Ramsey tax.

Third, we find that support for a Pigouvian instrument is significantly greater when the revenues are targeted to narrowly defined groups, as compared to when they are redistributed in a lump-sum. Further, in our setting, earmarking revenues lowers within group inequality; thereby raising the possibility that distributional consequences (i.e., inequality aversion) may underlie the relative support for earmarking. This conjecture is consistent with reports from the experimental and behavioral economics literature that subjects often prefer more equal payoff distributions (see, for example, [8] and [45]). Pigouvian tax aversion, opposition to taxes that clearly enhance individual and social welfare, contradicts presumptions of material self-interest and raises behavioral questions that matter to the potential of efficient policies. We find no evidence that providing information which improves understanding of the Pigouvian instrument

reduces tax aversion. We also explore elements of policy design and framing and find that earmarking and labeling can be significant determinants of support.

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