Who Stands up to Persuade?
Voluntary Influencers in Public Support for Pigouvian Taxation

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We examine how voters choose to influence others' attitudes toward policy, focusing on the context of Pigouvian taxation. Data from a controlled laboratory experiment show that individuals are generally reluctant to stand up and persuade others. Among those who do, both tax supporters and objectors are equally likely to volunteer—and equally persuasive. As a result, overall negative attitudes toward Pigouvian taxes persist. Moreover, it is the strength of individuals' initial views, rather than an informational advantage, that increases the likelihood of self-nomination as first voters, regardless of the direction of those views. These findings help explain the enduring lack of public support for welfare-enhancing tax policies and suggest avenues for addressing it.

Keywords*:* market experiment, Pigouvian taxation, negative externality, peer influence, vote
JEL codes*:* D04, D62, D72, H23

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1. Introduction

There is broad consensus among economists that a well-designed carbon tax is the most efficient instrument for mitigating climate change (Rausch & Reilly, 2015). In practice, however, the adoption of carbon taxes has been slow, largely due to fragile public support, which undermines their political viability. One key reason for this low support is the global public good nature of climate change mitigation: its benefits are shared worldwide, regardless of who bears the costs. Moreover, climate action generates temporal externalities, as the benefits accrue primarily to future generations. For example, Australia's carbon tax, introduced in 2012, was repealed just two years later (Crowley, 2017). While its failure stemmed partly from political infighting, it ultimately reflected widespread public resistance—or indifference—toward environmental taxation. Similar patterns have emerged elsewhere: carbon tax proposals were rejected in referenda in Switzerland and Washington State (Umit & Schaffer, 2020), and France's proposed fuel tax increase was abandoned following strong opposition from the 'yellow vest' movement (Douenne & Fabre, 2022). To better understand the disconnect between theoretical support for Pigouvian taxation and actual public attitudes, we examine the willingness of supporters and objectors to stand up and influence others' opinions in a controlled market experiment.

Individuals' choices and opinions are often shaped by peer influence (Epple & Romano, 2011; Herbst & Mas, 2015; Moussaïd et al., 2013). With the widespread diffusion of social media, peer effects have become even more prominent in shaping public attitudes toward policy (Afridi et al., 2023; Fujiwara et al., 2024). In real-world settings, individuals typically decide independently whether to express their views publicly, and the willingness to do so varies considerably. For example, while some social media users frequently share opinions or circulate political content, many others remain largely passive. This endogeneity in the decision to "stand up" and influence others raises important questions about how peer effects shape

public support for policy. First, how willing are people to stand up to influence others, and is this decision correlated with an informational advantage about the workings of the proposed policy? Second, how do supporters and objectors differ in their willingness to stand up to influence others? Third, is the influence of supporters different from that of objectors?

We conducted a market experiment to explore these questions. The design builds on prior research on resistance to Pigouvian taxation and the role of peer influence (Huang & Xiao, 2021; Kallbekken et al., 2011; Tiezzi & Xiao, 2016). In the experiment, participants earn money by purchasing units of a hypothetical consumption good, with each purchased unit generating an external cost for all buyers in their market. Importantly, this external cost is deducted from each buyer's future payoff, which is collected from the experimenter one week later. The delay in imposing the cost is designed to reflect the temporal nature of climate change: the consequences of consumption or production activities, such as emissions, often materialize only after a significant time lag. For instance, consumers benefit from gasoline use immediately, but the resulting air pollution and climate impacts accumulate over time. Accordingly, Pigouvian taxes that aim to reduce present consumption often yield benefits only in the long run.

In our experiment, buyers trade in this market for 10 periods. At the beginning of the 11th period, they vote on whether to impose a tax on each unit purchased, with the outcome applying to the subsequent five periods. Unbeknownst to them, they are asked to vote again in the 16th period for the final five periods. Tiezzi and Xiao (2016, henceforth T&X) show that despite tax adoption being the dominant strategy—and in the group's collective interest—the intertemporal structure of costs and benefits increases the perceived complexity of the policy, resulting in reduced public support for carbon pricing. Huang and Xiao (2021) further demonstrate that support for Pigouvian taxation increases significantly when tax supporters are exogenously assigned to explain their views to other voters.

We compare buyers' behavior in this baseline condition (the No First Voter treatment), which replicates the Delay treatment in T&X, with two new treatments. The first examines individuals' spontaneous decisions to influence others. At the start of the first ballot, buyers independently decide whether they wish to act as the first voter. It is ex ante unclear whether individuals have intrinsic motivation to influence others, and whether tax supporters differ from objectors in their willingness to stand up. One might expect that supporters, who can emphasize the long-term benefits of the tax, would be more inclined to act as influencers and more persuasive than objectors. If this were true, voluntary influence could still increase support for the tax. However, these predictions are not supported in our experimental data.

We first find that more than half of the buyers are not willing to be the first voter. Second, the willingness to vote first is similar among objectors and supporters. Our data suggest that the decision to be the first voter correlates with the strength of one's initial view, irrespective of what that view is or whether one thinks s/he has superior information than others. Furthermore, despite their negative (and incorrect) views, objectors are as persuasive as supporters. As a result, the overall tax support rate in this treatment is not significantly higher than in the baseline.

In the second treatment, we explore whether the reluctance to act as first voters—particularly among supporters—is due to a lack of confidence stemming from insufficient information. To test this, we provide additional information showing how the tax would positively affect individual earnings. This information consists of two hypothetical examples comparing the earnings of identical individuals with and without the tax. Prior research (Tiezzi & Xiao, 2016, p. 128) has shown that such information can increase tax support. Yet in the present context, this intervention has little effect on individuals' willingness to be first voters. This finding suggests that reluctance to speak up is not primarily driven by informational disadvantage.

These results suggest that although peers can exert a significant impact on Pigouvian tax attitudes, it is important to identify the types of individuals who are willing to influence others. The benefits or harms of peer influence can be limited if people holding opposing views are equally willing (or unwilling) to influence others and equally persuasive. A key policy implication is that new incentive mechanisms may be needed to encourage informed individuals—those with a better understanding of the policy—to actively share their views. Public support for welfare-enhancing policies, such as Pigouvian taxes, could benefit from institutional designs that promote such proactive engagement. We return to this point in the concluding section.

Most closely related to this paper are the works of Tiezzi and Xiao (2016) and Huang and Xiao (2021). Tiezzi and Xiao (2016) introduce a lab experiment with an intertemporal setup and show that the delayed benefits of Pigouvian taxation can significantly reduce support for taxation. Their data suggest that the complexity associated with the intertemporal incentive structure may contribute to this negative delay effect. Huang and Xiao (2021) examine peer effects by exogenously assigning a tax supporter as the first voter. They find that the support rate for Pigouvian taxation increases when a tax supporter votes first and sends a message to the group. However, little is known about the endogenous decision to be the first voter—particularly regarding differences between tax supporters and objectors in their willingness to influence others. The present paper fills this gap.

Our study contributes to three strands of literature. First, we contribute to the growing experimental literature on the determinants of public support for climate policies, as reviewed by Drews and van den Bergh (2016), and more recently examined in an international context by Dechezleprêtre et al. (2025). Recent studies highlight the importance of policy design (Carattini et al., 2017; Klenert et al., 2018), fairness and justice considerations (Maestre-Andrés et al., 2022), political trust and efficacy (Stadelmann-Steffen & Dermont, 2020), and policy

framing (Beiser-McGrath & Bernauer, 2019) in shaping public acceptance. In contrast, we investigate the role of peer influence on public support for Pigouvian taxation within the context of intertemporal decision-making. We show that, although tax supporters can positively influence public support, low support levels may persist when individuals independently decide whether to express their views. This persistence arises because tax objectors are equally likely to speak out and exert influence, thereby sustaining low overall support. Second, our paper contributes to the experimental literature that investigates the determinants of "standing up" to lead the group (Bruttel & Fischbacher, 2013; Drouvelis & Nosenzo, 2013; Gächter et al., 2012; Gächter & Renner, 2018; Güth et al., 2007; Komai et al., 2011; Potters et al., 2007). This research has shown that information advantages and efficiency concerns often drive the decision to lead. In contrast, we find neither factor drives the decision to stand up as first voters. Instead, we observe that those holding stronger views are more likely to volunteer as first voters than those with weaker views, irrespective of what those views are. One possible explanation for this discrepancy is that the intertemporal decision-making structure in our experiment increases the decision complexity and leads individuals to predominantly rely on the strength of their beliefs.

Finally, our paper also speaks to research on how individuals react to new information provided by peers and the impact of such information on attitudes in the context of complex decisions (Bursztyn et al., 2014; Murphy & Shleifer, 2004; Pickup et al., 2022; Robbett et al., 2023). Murphy and Schleifer (2004) present a network model of persuasion that can help organize empirical regularities on how people form political beliefs. In particular, the model sheds light on why people are often persuaded by peers who they interact with and why persuasion is more effective when voters' awareness of specific issues is lower.[1] Bursztyn et

[1] This regularity may be particularly binding when complex policy measures involving intertemporal flows of costs and benefits, such as Pigouvian taxes, are at stake. For example, Nordhaus & Rivers (2023), using a survey distributed to more than 2000 respondents from YouGov, find that most of the U.S. population is little informed about major economic questions and policies. The low level of knowledge is often associated with ideological,

al. (2014) show that social learning (i.e., learning from the choice of others) has sizable effects on complex financial decisions. These effects are greatest when influencers are sophisticated (i.e., competent on the issue at stake) compared to followers. Robbett et al. (2023) find that social information provided by peers can have a strong impact on political beliefs when a reliable source of outside information is absent. However, this peer effect is much diminished when outside information becomes available. Consistently, we find that when the understanding of the working of the taxation is limited, tax objectors can be as influential as tax supporters despite the tax policy being in everyone's self-interest. On the other hand, the impact of first voters is smaller when followers have more information.

2. Experimental Design

2.1. Research questions and design overview

We pose three research questions:

1) Are people willing to stand up as first voters to influence others, and is this decision correlated with information advantage about the functioning of the tax?
2) Do tax supporters and objectors differ in their willingness to stand up as first voters?
3) Does the influence of first voters differ between tax supporters and objectors?

We address these questions using a laboratory experiment with three treatments. Our baseline treatment (*No First Voter*) is a replication of the Delay treatment in T&X, where buyers in the market vote independently on the introduction of a tax on consumption of goods that impose negative externality. The external costs will only be deducted from each buyer's future payoff to be collected one week later. In our setup, it is easy to calculate that, at market equilibrium without taxation, all buyers purchase three units at a price of 40 points in all treatments. The

political, and religious views challenging the economic consensus. Similarly, Anderson et al. (2023) find that ideology is the main driver of support for a state carbon tax in a referendum.

socially optimal outcome, though, can be reached if each buyer purchases only two units (see Appendix B for details). In the *Self-Nomination* treatment, buyers independently decide whether they want to be the first voter. The first voter can explain to other buyers why they voted "yes" or "no." In the *Self-Nomination Asymmetric Information* treatment, additional information about how the tax would affect their earnings is available to some, but not all, buyers.

2.2 Treatments

No First Voter treatment (Baseline)

In this treatment, four participants formed a group.[2] Each played the role of a buyer in a market. Buyers earned money by purchasing units of a hypothetical consumption good from an automated seller in their market. Each buyer could purchase up to three units and was informed of the resale value of each unit (160, 110, and 70 points, respectively) before the auction started. During the auction, each buyer submitted a bid for each of the three units, with the bid for a unit capped at the resale value of that unit (see the example in Appendix A). All the bids collected in each market were ranked from high to low. The automated sellers had a per-unit production cost of 40 points; buyers were not informed of this production cost. Sellers would accept all bids greater than or equal to the per-unit production cost, so the lowest possible market price at which all units can be sold is 40 points. All units were sold at the market price which corresponds to the lowest accepted bid. Each buyer's gross income for each purchased unit was the difference between the unit's resale value and the market price. Units that were not successfully purchased yielded zero income.

[2] The choice of 4 buyers is based on our calibrations of the model's parameters. The parameters are chosen to ensure participants have a reasonable amount of earnings to be collected one week after the experiment. See also T&X (pages 121-122) for the discussions of design details of the baseline treatment.

Each traded unit generated an external cost (i.e., a negative externality) of 60 points to all buyers regardless of whether they purchased a unit. That is, each buyer incurred an external cost of 60/4=15 points for each traded unit.[3] An important feature of the experiment is that the external cost (after being converted to dollars at the rate of 200 points = $1) was not deducted from the buyers' present payoff, but from their future payoff (an endowment of $18 or 3600 points) to be received one week later. At the minimum market price of 40 points, sellers covered their production costs. It is straightforward to see that the marginal payoff or marginal benefit of each additional consumption is positive. Thus, each buyer will trade all three units in each period. So, at the market equilibrium, 12 units were traded in each market in each period at a unit price of 40 points. In this case, in each period a buyer earns (160 – 40) + (110 – 40) + (70 – 40) = 220 points today but incurs a cost of 15 × 12=180 points one week later. Thus, each buyer's total earnings over the 20 periods are 4400 points (and will not receive any of the 3600 points to be collected one week later due to the externality).

The socially optimal outcome (which coincides with maximum profit) is, however, for each buyer to purchase only two units. If each buyer in each market purchased two units in each of the 20 periods, he/she earned (160 – 40) + (110 – 40) = 190 points per period and the total present payoff is 190 × 20 = 3800 points. The payoff to be received one week later is now 3600 – 15 × 8 × 20 = 1200 points. In this case, each buyer's total earnings are $3800+\gamma 1200$, where γ is the weekly discount factor. As shown in T&X (page 122), as long as $\gamma > 0.5$, buyers are better off by trading 2 units instead of 3. Since a one-week discount factor $\gamma \leq 0.5$ implies

[3] Technically, the total *external* cost imposed on other group members is 45. In addition, we assume that each buyer's consumption behavior also imposes a cost on herself, similar to a scenario in which an economic agent is both a polluter and a victim of pollution. While this self-inflicted cost is not an *external* cost, we avoid labeling it as an *internal* cost to keep the exposition simple. Setting the Pigouvian tax equal to 45 instead of 60 would not alter any of the quantitative predictions of our setup.

a weekly discount rate $r \geq 100\%$, the individual discount rate must be extremely high for buyers in our setup to find it convenient to purchase 3 units.[4]

The market trading was first repeated for two practice periods and 10 paying periods. At the end of each period, buyers received feedback on the market price, market quantity, their bids, and per-capita external cost. They also saw their per-period earnings and accumulated earnings for both the day of the experiment and one week later.

A voting opportunity for a Pigouvian tax was introduced at the beginning of the 11th period. The per-unit tax was equal to the per-unit external cost (60 points). The tax was revenue neutral in that an equal share of the total tax revenues collected in a market was returned to each buyer at the end of each period.[5] With the tax, the minimum market price now becomes 40 + 60 = 100 points and purchasing the third unit becomes unprofitable. Thus, as long as γ >0.5, the tax can achieve the socially optimal outcome mentioned above, where each buyer purchases two units. Thus, unless subjects exhibit extremely low γ, time discounting alone cannot explain voting against the tax. T&X show that complexity and bounded rationality can lead to the decision to vote against the tax.

Before the auction in the 11th period, all buyers simultaneously voted "yes" or "no" to the introduction of the tax. They could not cast neutral votes or abstain from voting. If at least two buyers in a market voted "yes," then the tax would be implemented.[6] After the ballot was completed, buyers were informed about whether the tax was accepted or rejected. They were

[4] We find an average weekly discount rate r=0.057 corresponding to a weekly discount factor $\gamma = 0.94$.
[5] As explained in Tiezzi and Xiao (2016), the revenue-neutral tax ensures that the distaste of the tax cannot be attributed to issues such as earmarking of the fiscal revenues or uncertainty regarding the future use of the revenues.
[6] This voting rule maximizes the probability of implementing the tax and yet avoids the adoption of taxation being determined by an individual buyer. Thus, in our experiment, the institutional barrier is unlikely to be the reason for the objection or to send any normative signal regarding the voting position. This allows us to focus on the behavioral mechanisms underlying the negative attitudes toward Pigouvian taxation.

not told who voted "yes" or "no," nor how many votes each option received. The tax regime was then effective for the next five periods (11th to 15th).[7]

In addition to the voting decision, we also asked buyers—before voting—to state their position toward the tax on a seven-point scale, from Strong Yes to Strong No. This elicitation was necessary to identify tax supporters and objectors, as explained below. In the 16th period, all buyers were prompted to vote again for the tax (which they did not learn in advance).[8] The second ballot allows us to study whether the experience of trading with the tax in place affects voting attitudes. The voting outcome of the second ballot was then effective for the final five periods (16th to 20th). If the tax was rejected, the market environment remained the same as in the first 10 periods.

Self-Nomination treatment

In this treatment, we examine how willing buyers are to stand up as first voters in general (research question 1), whether tax supporters and objectors differ in their willingness to stand up as first voters (research question 2) and whether the influence of first voters differs between tax supporters and objectors (research question 3). The only difference between this treatment and the baseline No First Voter treatment is the voting procedure. In each market, before voting and after every buyer had stated his/her position toward the tax, all buyers had the opportunity to indicate their willingness to act as a first voter. They were told that one of these voluntary first voters would be randomly chosen to be the actual first voter who would vote first and send a message to the other buyers in the same market. After this self-nomination

[7] As in the first set of instructions, in this second set of instructions we did not mention how many periods subjects would face. Subjects were told that the tax, if passed, would be implemented in the subsequent trading periods. We did not tell subjects the exact number of periods, primarily to minimize any unintended end-of-game effect.
[8] The purpose of having two voting rounds is to allow for the possibility of studying the impact of experiencing the tax (see Appendix F). This design choice follows previous papers (see, e.g., Kallbekken et al., 2010, 2011; Markussen et al., 2014).

decision, the first voter was selected and buyers were informed of this outcome.[9] Subsequently, the first voter voted and wrote a message to the other three buyers in the market, who then voted simultaneously. In the second ballot, the same first voter voted and wrote another message. Both the vote and message would be sent to all other buyers before they voted.

Self-Nomination Asymmetric Information treatment

This treatment differs from the Self-Nomination treatment in one aspect: the voting instructions additionally stated that "*additional information about how the tax would affect your earnings is available. Some but not all buyers will receive this additional information. Whether one buyer will receive the information or not is randomly determined by the computer and will be shown on your screen before proceeding to the voting*." This treatment sheds light on the role of information advantage in the decision to stand up (research question 1).

In each market, three out of four buyers (participants did not learn this ratio) were randomly selected to see the following message that explicitly calculated the intertemporal tradeoff of the tax: "*You are randomly selected to receive the additional information about how the tax would affect your earnings. To help decide whether to vote for the tax, it might be useful to compare the two examples in the instructions. The two examples show that with the tax, buyer 4 will earn 125 points today and lose 60 points next week. Without the tax, buyer 4 will earn 180 points today and lose 150 points next week. That is, compared to the case with no tax, with the tax buyer 4 will earn 55 points less today, but will earn 90 points more next week.*" The other buyer saw the following message on his/her screen: "*You will NOT receive the additional information about how the tax would affect your earnings*." This message was

[9] If no buyer was willing to be the first voter, then one of them was randomly chosen to be first voter. There were two such markets in our experiment. We exclude these two markets when we analyze the first voter's impact on the other three buyers. But we use the full sample when analyzing the self-nomination decisions.

shown on participants' screen after they had stated their initial position toward the tax, but before their self-nomination and voting decisions.[10]

The reason we used this (italicized) message is that T&X previously showed that receiving such information led more buyers to support the tax, indicating the instrumental value of this information. Thus, if lack of information advantage contributed to the reluctance to be first voters, we should expect to see more buyers choose to vote first in this treatment.

2.3. Procedure

At the beginning of each session, participants were randomly assigned to a market. They remained in the same market throughout the experiment. Instructions for the practice periods and the first 10 paying periods were distributed to participants in paper form and read aloud by the experimenter. Participants were not informed of the tax and the voting opportunity until the end of the 10th period when the second set of instructions was distributed and again read aloud by the experimenter. After the first ballot, the experiment continued for another five periods. Then, participants were prompted to vote in the second ballot, and the experiment continued for the final five periods. To avoid end-of-game effects, we did not tell participants how many periods remained during the experiment. To ensure that participants understood the instructions, they were also asked to complete comprehension questions after reading each set of instructions.[11] At the end of the 20 periods, we administered a survey including demographic questions and a simplified procedure (Coller and Williams, 1999) to elicit the one-week

[10] The design choice to provide additional information to three out of four buyers in each market was motivated by the objective of maximizing observations on how information influences self-nomination decisions. Simultaneously, we aimed to avoid creating scenarios in which individuals perceive no need to influence others. In other words, not all participants were informed of the additional information to prevent the perception that influencing others is unnecessary. Similarly, we refrained from disclosing the exact ratio of individuals receiving the additional information, so as not to discourage participants from standing up.

[11] We went to great lengths to ensure that participants understood the instructions. In addition to the comprehension questions, participants were asked to review a PowerPoint file that demonstrated screenshots of the main screens they would see as the experiment progressed.

discount rate of the subjects. Participants' identities were kept anonymous throughout the experiment (see Appendix A for all the instructions).

To implement the intertemporal payment scheme, participants were informed, in the first set of instructions, that their earnings from the experiment would have two components: the earnings they received that day and the earnings they would receive one week later (an $18 endowment, minus any external costs incurred in the experiment). To receive this amount, participants needed to return to the same lab one week after the experiment. They would not need to perform any additional tasks to collect the money. We followed the same procedure as T&X to minimize any credibility concerns about money collection.[12] Figure 1 summarizes the timeline of the experiment and Table 1 outlines the treatment differences in the voting stage.

The experiment was conducted at the Monash Laboratory for Experimental Economics (MonLEE) with university students (46% females). 60 participants (15 markets) participated in the No First Voter treatment, 112 participants (28 markets) in the Self-Nomination treatment, and 96 participants (24 markets) in the Self-Nomination Asymmetric Information treatment. Each computerized session was programmed in z-Tree (Fischbacher, 2007) and included 12, 16, or 20 participants. Earnings were expressed in experimental points and converted to Australian Dollars at the rate of 200 points per dollar. A typical session lasted two hours, with average earnings of $28, including a $5 show-up fee.

[12] Specifically, on the day of the experiment, each subject received a "payment certificate" stating the amount of money, date, time, and location for money collection. The certificate listed the contact details of the experimenter and was signed by the experimenter. If the specified collection time and day did not work, subjects could reschedule a different pick up time and day (but no sooner than the default date) or have someone else pick up the money on their behalf. Subjects received a reminder email on the day before the scheduled collection day. Where subjects did not show up, the experimenter contacted them again to reschedule a time.

Fig. 1. Timeline of the experiment

First day	Period 1~10	In each period: Each buyer could trade up to three units of the good.
	At the beginning of period 11	After comprehension questions: Buyers stated their positions toward the tax. First ballot (without knowing about the second ballot) ↓ **Voting procedure differs by treatment. See Table 1.** ↓ Buyers were informed of whether the tax would be implemented in the subsequent periods.
	Period 11~15	In each period: Buyers traded on the market with or without a tax, depending on the voting outcome.
	At the beginning of period 16	Second ballot Same as the first ballot (except that buyers did not state tax positions again).
	Period 16~20	In each period: Buyers traded on the market with or without a tax, depending on the voting outcome.
One week later		Participants picked up additional earnings: $18 minus the total external costs.

Table 1. Voting procedure in each treatment

Treatment	Voting Procedure
No First Voter	Buyers voted Yes or No on the tax
Self-Nomination	Buyers indicated their willingness to be the first voter. A first voter was then randomly selected among these would-be first voters. The first voter, either tax supporter or objector, voted first and sent a free-form message to other buyers who would then vote.
Self-Nomination Asymmetric Information	Three out of four buyers saw additional information about the tax. Buyers indicated their willingness to be the first voter. A first voter was then randomly selected among these would-be first voters. The first voter, either tax supporter or objector, voted first and sent a free-form message to other buyers who would then vote.

3. Results

Our focus is the decision to vote first (i.e., self-nomination). First, we compare the supporters' and objectors' decisions to vote first. Next, we report treatment differences in voting outcomes. The results of trading behaviour are detailed in Appendix B.

3.1. Decision to vote first

Table 2 presents the absolute number of self-nominations in each self-nomination treatment for each type of buyer according to their initial attitude. We classify those buyers who answered, "Slight No," "Moderate No" or "Strong No" in the initial position question as tax objectors, and those who answered "Slight Yes," "Moderate Yes" or "Strong Yes" as tax supporters. First, on average, about 39% of (44 out of 112) buyers self-nominated to be first voters in the Self-Nomination treatment, suggesting a general reluctance to be first movers. Providing additional information did not significantly reduce this reluctance. In the Self-Nomination Asymmetric Information treatment, among those who received the additional information, 47% (43 out of 96) self-nominated. The frequency among those who did not receive the information was 38% 38% (9 out of 24). The difference, however, is not significantly different between participants who received the information and those who did not ($p = 0.412$).[13] Hence, to answer our first research question:

Result 1: *In general, buyers are reluctant to be first voters, and this reluctance is not due to a lack of information advantage about the workings of the tax policy.*

[13] The reported p value is from an OLS regression analysis where the dependent variable is the self-nomination decision and the independent variable is whether the participant received the additional information. Standard errors are clustered at the market level.

Table 2. Frequency of self-nominations by treatment

	Self-Nomination	Self-Nomination Asymmetric Information	
Total number of buyers	112	96	
		Received information?	
		Yes	No
Initial tax supporters	44	51	11
# of self-nominations	19	24	5
Indifferent types	20	4	2
# of self-nominations	6	1	1
Initial tax objectors	48	17	11
# of self-nominations	19	9	3
Total number of self-nominations	44	43	

The decision to self-nominate seemed to be independent of whether buyers held a positive or negative view about the tax or whether some buyers hold an informational advantage. In the Self-Nomination treatment, we found 40% of objectors (19 out of 48) and 43% of supporters (19 out of 44) self-nominated ($p = 0.730$).[14] A similar pattern was observed in the Self-Nomination Asymmetric Information treatment: 43% of objectors (12 out of 28) and 47% (29 out of 62) of supporters self-nominated ($p = 0.733$).

Figure 2 provides more details on the correlation between the strength of initial views and self-nomination decisions. Given that the self-nomination decisions were qualitatively similar between the two treatments, we pooled the data from the two treatments together to obtain a sufficient number of observations in each cell.[15] Although both supporters and

[14] The reported p value comparing self-nomination decisions between initial self-identified supporters and objectors is from an OLS regression analysis where the dependent variable is the self-nomination decision and the independent variables are initial positions (both initial self-identified supporters and objectors). Standard errors are clustered at the market level.

[15] To justify the data pooling, in Appendix C, we conduct the analyses separately for the two self-nomination treatments (Figure C1 and Table C3), showing a qualitatively similar pattern of self-nomination decisions, although the statistical evidence is weaker in the Self-Nomination treatment.

objectors were equally likely to volunteer, those who held stronger views (i.e., stated Strong-Yes/No or Moderate-Yes/No) were more likely to self-nominate as first voters than those with weaker views (i.e., Slight-Yes/No).

Fig. 2. Fraction of buyers who self-nominated as first voters by initial attitude (pooled data from the two Self-Nomination treatments)

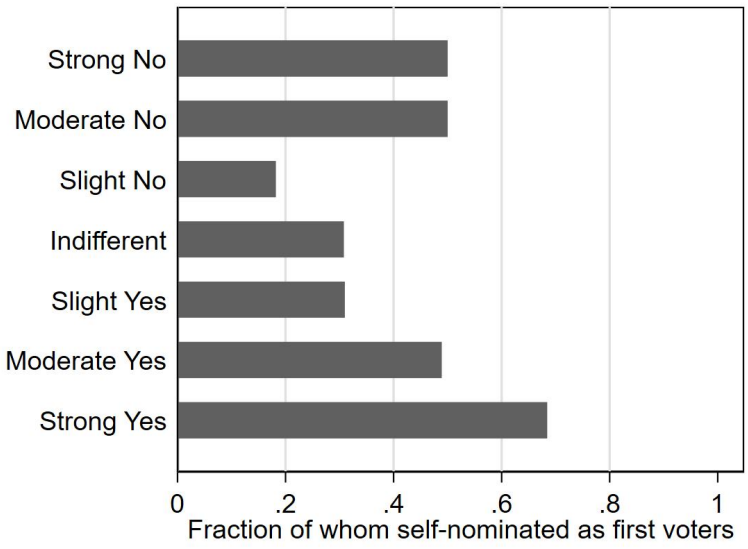

Table 3 reports a linear regression analysis explaining the decision to be the first voter. The estimates confirmed that Strong No buyers were significantly more likely to self-nominate than Slight No buyers (p = 0.005, F-test) by around 32 percentage points. Similarly, Strong Yes buyers were significantly more likely to self-nominate than Slight Yes buyers (p = 0.002, F-test).[16] In Appendix G, we report the results of robustness checks in which we add age, gender, political orientation and individual discount rates as control variables. The strength of the initial views held by the subjects remains the most important determinant of the probability of standing up as first-voters. The only exception is that females were significantly less likely to self-nominate. In sum, to answer our second research question:

[16] For more straightforward pairwise treatment comparisons, Appendix Table C4 reports the same regression analysis but using different initial attitude as the omitted variable. Moderate No buyers were significantly more likely to self-nominate than Slight No buyers (p = 0.045). Moderate Yes buyers also appeared more likely to self-nominate than Slight Yes buyers, but the difference is not significant (p = 0.104).

Table 3. Linear probability regression of the decision to self-nominate

	Coefficient	Standard error
Strong No (obs.=34)	0.318***	0.108
Moderate No (obs.=20)	0.318**	0.154
Indifferent (obs.=26)	0.126	0.134
Slight Yes (obs.=42)	0.128	0.110
Moderate Yes (obs.=45)	0.307***	0.110
Strong Yes (obs.=19)	0.502***	0.126
Constant	0.182**	0.080
# of obs.	208	

*Standard errors are clustered at the market level. The model uses Slight No (obs.=22) as the omitted category. * p < 0.10; ** p < 0.05; *** p < 0.01.*

Result 2: *The self-nomination decision did not differ between tax supporters and tax objectors, and was strongly correlated with the strength of one's initial view. However, this decision is not affected by the receipt of additional information about the workings of the tax policy, as the proportions of supporters and objectors who self-nominated are similar across both treatments. Thus, it is the intensity of views—rather than an informational advantage—that drives self-nomination, regardless of whether those views are supportive or oppositional.*

3.2 Voting decisions

Huang and Xiao (2021) report that the tax support rate is significantly higher when tax supporters vote first. Our data here allow us to further examine voting outcomes when first voters are self-determined. We start with the comparisons between No First Voter treatment and Self-Nomination treatment to learn the influence of self-elected first voters without any

information intervention. Then, we report the voting decisions in the Self-Nomination Asymmetric Information treatment when buyers are exposed to additional information in addition to the first voter's decision. Lastly, we report results from the content analysis of first voters' messages.

Effect of self-nominated first movers on voting decisions

Figure 3 shows the overall fraction of yes votes in the two ballots by treatment. In the Self-Nomination treatment, we found that only 39% of buyers voted "yes" to the tax in the first ballot, which is not significantly different from 45% in the baseline No First Voter treatment ($p = 0.614$).[17] In the second ballot, both treatments have 43% support rates. We find this result can be explained by the two opposite effects—the significant positive impact of tax supporters and the negative impact of tax objectors—cancel each other out. In markets where the first voters voted "yes," in the first ballot, 78% of all buyers voted "yes," which is significantly higher than the support rate in the baseline treatment (78% vs. 45%, p =0.004).[18] In the second ballot, 65% voted "yes," again significantly higher than 43% in the baseline treatment, (p = 0.039).[19] Interestingly, tax objectors also appeared to be highly influential on other buyers. In the first ballot, in groups where the first voters voted "no," only 16% of buyers voted "yes" (significantly different from 45% in the baseline treatment, p = 0.006). The support rate remained low in the second ballot at 30% (but not significantly different from 43% in the baseline treatment, p = 0.137).

[17] The reported p value is from an OLS regression analysis where the dependent variable is the voting decision and the independent variables are treatment dummies (all three treatments). We then use F-test to compare voting decisions between treatments. Standard errors are clustered at the market level.

[18] The reported p values comparing voting decisions between the two treatments are from an OLS regression analysis where the dependent variable is the voting decision and the independent variables are treatment dummies (No First Voter and Self-Nomination treatments). Only groups where the first voters voted "yes" are included. Standard errors are clustered at the market level. In all the comparisons with the No First Voter treatment, we include all the markets in this treatment.

[19] Two out of the 10 first voters who voted "yes" the first time changed to "no" in the second ballot. For the remaining eight markets where first voters continued to vote "yes," the proportion of yes votes remained high at 75%.

Fig. 3. Fraction of yes votes by treatment and ballot

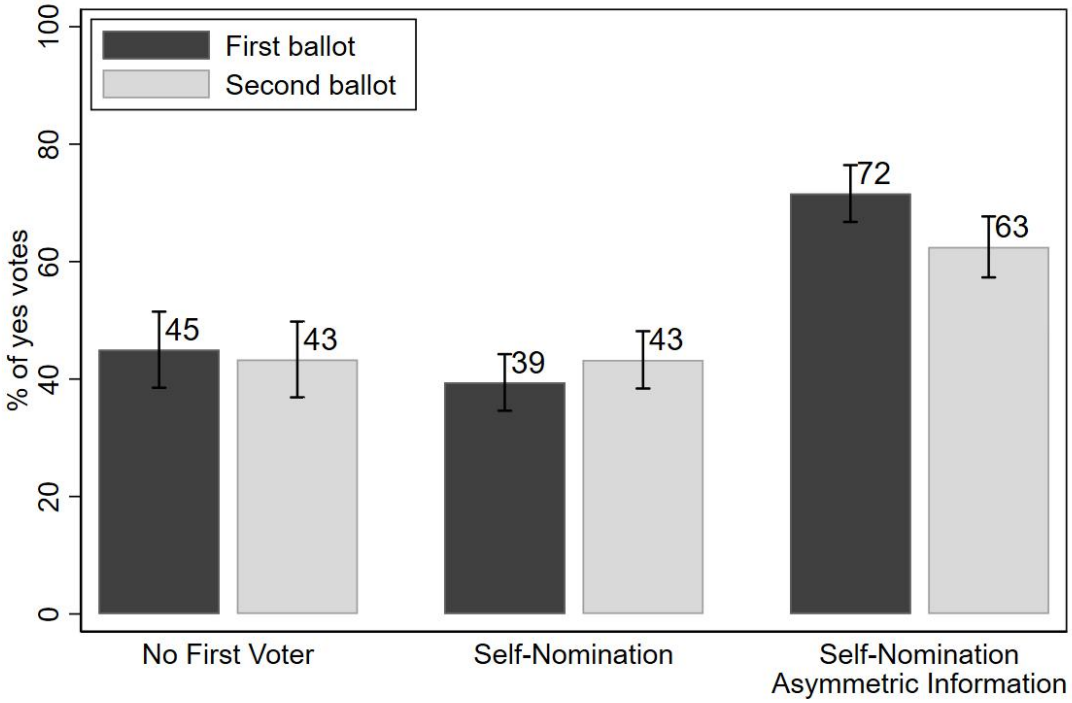

Table C1 in Appendix C presents more details on subjects' initial attitudes (before seeing the first voter's vote) and actual votes in the first ballot. Tax supporters are highly influential as first voters. 8 out of 14 followers who were initially against the tax and 3 out of 6 of those who were initially indifferent voted "yes." By contrast, only 11% of initial tax objectors and indifferent types voted "yes" in the baseline treatment. All followers (10 out of 10) who stated a positive view voted "yes" (compared to a ratio of 23 out of 23 in the baseline treatment). On the other hand, when the first voters voted "no," only 6 out of 17 initial supporters and 3 out of 12 indifferent types voted "yes" after seeing the first voter's vote. Followers who initially stated a negative view mostly voted "no" (only 1 out of 19 voted in favor of the tax). Hence, to answer our third research question:

Result 3: *Self-nominated first voters, whether supporters or objectors, can significantly influence buyers to change their views and both types of first voters tend to be equally persuasive.*

Effect of additional information on voting decisions

Fig 3 also shows that the support rate in the Self-Nomination Asymmetric Information treatment is significantly higher than the Self-Nomination treatment in both ballots. (First ballot: 72% vs. 39%, p=0.013; Second ballot: 63% vs. 43%, p=0.027). This information effect on support rate is interesting given the finding we reported above that the information did not change people's willingness to vote first. Our further analysis suggests two potential mechanisms underlying the information effect on the tax support rate. We acknowledge the limited power of the analysis due to the small sample size. Future research would be valuable to systematically investigate how information interacts with peer influence.

One mechanism is the effect on buyers' initial views. The ratio of those who indicated a slight 'Yes' or stronger positive views before seeing the first voter's decision is 70% (37 out of 53) among those who received the additional information, compared to 32% (6 out of 19) among those who did not.

The other mechanism is that information makes the initial supporters less likely to be influenced by the tax objectors. Table C2 in Appendix C presents data on subjects' initial attitudes and actual votes in the first ballot in the Self-Nomination Asymmetric Information treatment. When the first voter voted "no," 63% (10 out of 16) of initial supporters still voted "yes." This result stands in contrast to the Self-Nomination treatment, where only 35% (6 out of 17) of initial supporters voted "yes" (see the last column of Table C1). These findings suggest that the first voters can exert greater influences in the absence of additional information on the policy issue at stake, which aligns with the finding of Potters et al. (2007) that informed followers are less inclined to imitate their leader's decision. Appendix F expands on this by analyzing how the followers' voting decisions are influenced by the first voters. Additionally,

we show that both the presence of additional information and prior experience with tax tend to diminish the first voters' influence in the second ballot.

Content analysis of first voters' messages

To gain additional insights into these results, we conducted a content analysis of the messages sent by the first voters in the two Self-Nomination treatments. As some messages do not seem to provide any (meaningful) explanations while others do, we can test whether the impact of the first voters who provided explanations for their votes in their messages is stronger than that of those who did not. In addition, we consider another possibility in view of previous research that suggests social influence is stronger when individuals view their peers be confident in their opinions or decisions (Moussaïd et al., 2013). It is possible that the content of the messages reveals the first voter's level of confidence in their vote. We examine whether first voters whose messages revealed they were confident in their decisions exert stronger peer effects than those whose messages did not reveal such confidence. The message coding instructions and the results from the content analysis of messages are reported in Appendix D and E, respectively.[20] For the main analysis, we pooled the data across both Self-Nomination treatments to gain sufficient numbers of observations.

[20] We recruited 24 evaluators from the MonLEE student subject pool to analyze the messages, separately for those when first voters voted "yes" and when first voters voted "no." They were asked to classify each message under two coding systems. First, they classified each message as either "Explained why" (i.e. explained why introducing taxes is to everyone's best interest); "Statement Only" (i.e. only made a statement that introducing taxes is to everyone's best interest but did not explain why); "Other Reasons" (i.e. provided some reasons not related to profit maximization); or "No Reasons." Second, they classified each message based on whether the message revealed the first voter's confidence in his/her vote. This included three categories: "Confident," "Not Confident," or "No Information." We classify a message to a specific category if it is the most popular choice of all evaluators (all messages have a unique most popular choice). For the main analysis, we combine the categories "Statement Only," "Other Reasons," and "No Reasons" for the first coding system and the categories "Not Confident" and "No Information" for the second coding system to gain sufficient observations. For the resulting categories, the interrater agreement rate (Cohen's Kappa) is 0.55 for coding whether first voters provided reasons and 0.36 for coding whether first voters' messages revealed any confidence in their choices. The low agreement rate for the coding of confidence may also indicate that the messages do not reveal much about confidence level.

Contrary to our expectations, we found that initial tax objectors or indifferent types were not significantly more likely to vote "yes" when tax-supporting first voters provided reasons for their decisions (10 out of 18) than when they did not (11 out of 21) (see columns 1-3 in Table E1, $p = 0.564$, Fisher exact test). Whether tax-supporting first voters' messages revealed they were confident in their decisions also did not appear to affect first voters' impact on initial tax objectors or indifferent types (see columns 1-3 in Table E2, $p = 0.599$). Similarly, when first voters opposed the tax, whether their messages provided explanations or revealed they were confident did not significantly increase their impact on followers. For example, 13 out of 21 followers who initially supported or were indifferent to the tax followed the first voters by voting "no" when their messages provided reasons why they voted "no." The ratio is 13 out of 22 when the messages did not provide explanations. These findings suggest that first voters' influences mainly arise from their votes rather than their messages.

4. Discussion and Conclusion

In naturally occurring environments, not only the tax supporters, but also the objectors have the chance to influence the public. We find that tax objectors are as willing to stand up to influence others as tax supporters and that they are equally influential. As a result, we observe no positive peer effect when market participants spontaneously decide whether to share their views with others. These results suggest that those who decide to stand up to influence others are not those with a better understanding of the tax implications, although, in our setup, implementing the tax is not only the efficient solution, but it is also in each buyer's own interest.

Our findings contribute to the understanding of the gap between real public attitudes and the scientific support for Pigouvian taxation, which may be attributed to the possibility that citizens are simultaneously exposed to the views expressed by supporters and objectors. Scientific research is often not the only source of information available to citizens. Especially

with the spread of social media, the public currently has many opportunities to express its views and criticisms of published scientific research (Anderson, 2017; Yeo et al., 2017). For example, comment sections of online newspapers provide an outlet for climate skepticism (Jaspal et al., 2013; Koteyko et al., 2013). Some of this skepticism may come from the belief that the authorities' and scientists' interests are different from those of ordinary people (Kahan et al., 2012). Individuals may also have a higher trust in their non-expert peers who share the same interests, therefore being more willing to follow peer opinions. As a result, scientific research may not have an immediate impact on public attitudes.

It is worth highlighting that we do not observe a strong tendency to self-nominate to be the first voter. The lack of interest in being the first voter does not change when additional information on how the tax policy works becomes available. Instead, more polarized views about the tax policy seem to drive the decision to stand up as first voters. This might be due to the complex intertemporal decision environment which leads individuals to rely on the strength of their beliefs rather than searching for a better understanding of how the policy works.

One policy implication is that there is room to promote public support by designing mechanisms that motivate those individuals who have a good understanding of the policy at stake to play a more proactive role in shaping public attitudes toward welfare-enhancing policies. Further research would be valuable in testing different mechanisms that promote "good" peers to take the persuader's role and effectively influence public attitudes.

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