Journal of Economic Behavior and Organization 187 (2021) 192–204

Contents lists available at ScienceDirect

Journal of Economic Behavior and Organization

journal homepage: www.elsevier.com/locate/jebo

Peer effects in public support for Pigouvian taxation ☆

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ARTICLE INFO

Article history:
Received 27 May 2020
Revised 22 March 2021
Accepted 15 April 2021

JEL codes:
D03
D62
D72
H23

Keywords:
Market experiment
Pigouvian taxation
Negative externality
Peer effects
Vote

ABSTRACT

Despite the efficacy of Pigouvian taxes, governments often find them surprisingly controversial to implement. Evidence suggests the reason may be their complexity, which stems from the delay of externality. This paper studies whether communication among peers can promote public support for Pigouvian taxation. Using a market experiment with time-delayed negative externalities, we find that support for Pigouvian taxation increases when tax supporters can explain their position to other voters. We show that the peer effects cannot be explained by simple imitation or compliance, but are more likely to be driven by social learning. Our findings provide converging evidence for the role of complexity in the lack of support for Pigouvian taxes. These results point to the importance of giving voice and visibility to members of the general public who support efficient but complex tax policies.

1. Introduction

Implementing policy requires public support. Nevertheless, even theoretically optimal policies may lack sufficient public support. Take, for example, Pigouvian taxation, a market-based environmental policy aimed at resolving market inefficiencies created by negative externalities. Theoretically, a well-designed Pigouvian tax, e.g., a carbon tax, could be one of the most efficient means of reducing large-scale pollution problems (Rausch and Reilly, 2015). However, in practice, Pigouvian taxation often sees only fragile public support, and therefore can be difficult to implement.[1] One reason might be its complexity: the benefit of the tax may not always be transparent to the general public, as it requires understanding not only one's *own* behavioral responses to the policy but also the responses of others (Tiezzi and Xiao, 2016). The incongruity between negative public attitudes and positive theoretical recommendation raises an important question: how can we improve public understanding of socially optimal but complex policies, thereby changing negative attitudes towards those policies? In this

☆ We gratefully acknowledge the Australian Research Council Discovery Project (DP160102743) for funding this research.

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[1] For example, the Australian carbon tax, which came into effect only in 2012, was repealed in 2014 (Crowley, 2017). While its failure reflects political infighting among Australian politicians, it is ultimately the consequence of opposition to, or at least insensitivity toward, environmental taxes among the general public. Similarly, recent opinion polls conducted at Yale University (Leiserowitz et al., 2010) report that only 35% of United States citizens support increasing taxes on gasoline.

https://doi.org/10.1016/j.jebo.2021.04.019
0167-2681/

paper, we examine how communications from peers who support a complex tax policy can increase support for a policy and thus the likelihood of its being implemented.

Previous research suggests that individuals' choices and opinions are often influenced by their peers (Epple and Romano, 2011; Moussaïd et al., 2013; Herbst and Mas, 2015). Policy-related issues, such as curbing climate change, affect all of society. As a result, they frequently arise in conversations, and even more so when the consequences of regulatory interventions are not straightforward. Thus, peers may play an essential role in shaping people's views about policies. With the growing use of social media in political persuasion, peer effects can penetrate across a vast number of individuals, with their opinions shaped by laypeople advocating their views (Bond et al., 2012; Porter and Hellsten, 2014; Anderson, 2017). Nevertheless, we still know little about how peer effects impact public opinions on complex policies. We fill this gap by examining whether communication by peers who support Pigouvian taxes can influence broader social attitudes toward Pigouvian taxation.

While experts nearly unanimously acknowledge that carbon taxes can reduce large-scale pollution problems, there remains continued low support for such taxes. This contrast may indicate that information provided by policy supporters cannot effectively change negative attitudes. However, in practice, people may be exposed to, and influenced by, large amounts of information (e.g., from tax objectors). Further complicating this issue, people may exhibit bias in to whom they choose to listen (e.g., Guilbeault et al., 2018; Over and McCall, 2018). A laboratory environment allows greater control over all those factors.

Our experiment is built on previous research on attitudes toward Pigouvian taxation (Kallbekken et al., 2011; Tiezzi and Xiao, 2016). In our experiment, each participant can earn money by purchasing units of a hypothetical consumption good. Sellers in the market are automata. Therefore, the market is essentially a uniform price multi-unit auction with a reserve price. Each purchased unit of good generates an external cost (a negative externality) to all buyers in their market. The external cost is deducted from each buyer's future payoff, to be collected from the experimenter one week later. The reason we introduce an intertemporal delay of the negative externality is that previous studies (Tiezzi and Xiao, 2016) suggest that the intertemporal structure of the costs and benefits of the tax (i.e., paying the tax now for the future environmental benefit) may increase the perceived complexity of the tax policy and contribute to public resistance.

Buyers first trade in the market for ten periods. In the 11th period, they vote on introducing a tax, which equals the external cost of the consumption on the purchased units. The voting outcome is applied to the next five periods. Buyers do not know that in the 16th period, they will be asked to vote again on the tax for the remaining five periods. Notably, adopting the tax is designed to be the socially optimal and profit-maximizing strategy for each buyer. We compare buyers' voting behavior in this baseline with three treatments in which one of the self-identified tax supporters (via an elicitation of preferences for or against the tax before the vote) is randomly selected to be the first voter whose vote will be made public. The three first voter treatments vary on whether the first voter can send a message to other buyers before they vote and on message formats.

In the First Voter With Message treatment, the first voter can write a message that explains his/her decision. While there is generally mixed evidence of peer effects (Herbst and Mas, 2015), previous research on social learning and effects of advice shows that suggestions from an advisor can lead to more optimal decisions in complex games (Schotter and Sopher, 2003; Celen et al., 2010; Cooper and Kagel, 2016; Ding and Schotter 2017). Thus, we hypothesize that if tax objectors' initial negative views about a tax stem from the tax's complexity, having supporters provide information about the benefits of the tax can help reverse these opposing views.

We note that, in principle, any peer effects in the First Voter With Message treatment could be driven by conformity alone, i.e., the information provided in the message may not matter (Zajonc 1965; Schotter 2003; Bougheas et al., 2013; Charness et al., 2013; Bursztyn et al., 2014). For example, buyers may follow the first mover because there is a social utility of pure imitation. Similarly, they may do so simply to comply with the first mover's solicitation. Therefore, we conduct another two treatments to shed light on the importance of the message in the first-mover effect. In the First Voter No Message treatment, the first voter can only show his/her vote, with no message. This treatment can be used to measure the pure imitation effect. In the First Voter Fixed Message treatment, the first voter can show his/her own vote and then send a fixed format message recommending a vote of "yes" or "no" to the other buyers. This treatment helps to determine the effect of solicitation compliance (Levy et al., 2011; Feltovich and Grossman, 2015; Brandts et al., 2016; Kessler, 2017).

We find that, in the First Voter With Message treatment, half of the initial tax objectors and those who were indifferent to the tax changed their vote to "yes" after observing the first voter's "yes" vote and message. As a result, the tax support rate was significantly higher than in the baseline. The high support rate persisted in the second ballot. By contrast, the tax support rates in the other two first voter treatments were not significantly higher than that in the baseline. These results suggest that the peer effect observed in the First Voter With Message treatment is neither simple imitation nor compliance to the solicitation. Instead, people seem to learn from peers' messages and then change their vote. These findings are important, as they provide further evidence that complexity can be a significant obstacle to public support for Pigouvian taxation. Mechanisms that reduce complexity can effectively promote public support.

Our study contributes to the relatively new research agenda on how people form opinions about policies, such as income taxes and environmental policies (Gideon, 2017; Rees-Jones and Taubinsky, 2020; Stantcheva, 2020). It is important to differentiate whether negative attitudes stem from misconception and imperfect knowledge about the economic outcomes of taxation (e.g., tax aversion) or whether they are due to individual preferences (e.g., time preferences). While the former could be corrected by providing better information, the latter "may need to (perhaps even should) be taken as given and respected by policy makers" (Stantcheva, 2020). Messages from tax supporters can help tax objectors understand the working mechanisms of taxation, but they are unlikely to be persuasive if resistance stems from time preferences (e.g., the delay in seeing the tax benefit). Thus, our findings of the peer effect and the unique role of the explanatory messages highlight the importance of resolving complexity in promoting positive attitudes towards taxation.

Our paper further contributes to the extensive literature on peer effects, which have been examined in many contexts, such as saving and investment decisions, worker productivity, and education. To the best of our knowledge, no experimental studies have been conducted to examine the peer effect on the attitudes of public policies. Our controlled laboratory experiment allows us to test the peer effect in a new policy-relevant context, while also identifying the underlying mechanism. As discussed in Section 2, both imitation and social learning have been argued to contribute to the peer effect, with its presence varying on the decision context. We find that the peer effect is mainly driven by social learning, a result which may be due to our complex decision-making environment. By contrast, in previous studies decision-making environments have typically been much simpler and thus learning plays a much less significant role in decisions.

The remainder of the paper is organized as follows. Section 2 reviews related literature. Section 3 outlines the experimental design. Section 4 presents the experimental results. Section 5 concludes with discussions on policy implications and directions of future research.

2. Related literature

Our paper mainly connects to two lines of literature. One is the research on the public attitudes toward environmental taxation. The other is the peer effects on decision making.

There is an emerging literature using market experiments to understand the determinants of the acceptability of environmental taxes. Early discussions stress the role of (dis)trust of the government (Rivlin, 1989; Dresner et al., 2006). More recently, experimental studies focus on the role of cognitive limits and psychological biases, such as tax framing related to Tax Liability Side Equivalence (Sausgruber and Tyran, 2005), labeling as fee or tax (Kallbekken et al., 2011), aversion to being taxed (Kallbekken et al., 2010; Blumkin et al., 2012; Cherry et al., 2012), cultures and worldviews (Cherry et al., 2017), experiences with taxes (Cherry et al., 2014), and attitudes toward the moral responsibility of cleaning up one's own mess rather than relying on government intervention policies (Jakob et al., 2017).

Most closely related to this paper, Tiezzi and Xiao (2016), henceforth *T&X*) show that the delayed benefits of Pigouvian taxation can significantly reduce support for the taxation. Their data suggest that complexity associated with the intertemporal incentive structure might contribute to the negative delay effect. Our findings on the peer effect highlights how resolving complexity can help shape positive attitudes toward the tax policy.

Peer effects have been studied extensively in various behavioral domains, such as education (Epple and Romano, 2011; Sacerdote, 2011), risky behavior (Cooper and Rege, 2011; Dijk et al., 2014; Fafchamps et al., 2015), household recycling (Brekke et al., 2010; Czajkowski et al., 2017), voter turnout in elections (Fafchamps and Vicente, 2013; Nickerson, 2008) and tax compliance (Fortin et al., 2007; Alm et al., 2017; Hallsworth et al., 2017). Herbst and Mas (2015) conducted a meta-study of peer effects in worker productivity. They found a large variance in effect sizes across different studies. Other works that produced mixed results on peer effects include (Sacerdote, 2001; Lyle, 2007; Guryan et al., 2009; Lieber and Skimmyhorn, 2018). These imply that messages from tax supporters need not change the attitude of objectors in this new context, particularly if the resistance stems from individual preferences such as time preferences.

We extend the literature on peer effects to the context of public support for Pigouvian taxation. The controlled laboratory experiment allows us to identify the mechanism underlying the peer effect. Previous studies have shown that peer effects can be attributed to either imitation or social learning, depending on decision-making environments (Cooper and Rege, 2011; Bursztyn et al., 2014). We show that for peers to change individuals' initial attitudes toward taxation, they must provide explanations for their choices. An implication is that the peer effect in our context is mostly driven by social learning. This result supports the hypothesis that complexity is the cause of the negative attitudes towards taxation. It is also consistent with the finding reported in Bursztyn et al. (2014) that social learning effects are greatest when the first (second) investor is financially sophisticated (financially unsophisticated). In our context, we would assume that the most sophisticated individuals are more likely to correctly understand the working mechanism behind the taxation, and thus be more likely to support it.

Our experiment also relates to studies that compare the results of free-form peer communication with communication restricted to sending a pre-determined set of messages or simple suggestions in other strategic settings. In general, free-form communication is more effective as compared to restricted one, as it allows subjects to make more believable promises (Charness and Dufwenberg 2010), promote more efficient and flexible contracting (Brandts et al., 2016), and issue more credible threats to punish cheating (Cooper and Kühn, 2014). In line with these studies, in our (largely non-strategic) context, free-form communication helps first voters be more persuasive than they would be under limited or no communication.

Finally, our paper is related to experimental studies on leadership. While previous studies have focused mostly on how (exogenously or endogenously selected) leaders' advice or action can reduce followers' egocentric behavior and promote cooperation (Potters et al., 2005; Gächter et al., 2010; Levy et al., 2011), we extend the literature to investigate the scenario where complexity, rather than self-interest, stops people from supporting tax policies that could improve both individual and social welfare.

3. Experimental design and hypotheses

3.1. Design overview

Our baseline treatment (*No First Voter*) is built on the market experiment previously designed to study resistance to Pigouvian taxation (Kallbekken et al., 2011; Tiezzi and Xiao, 2016). Buyers in a market vote simultaneously and independently on whether to introduce a tax aimed at internalizing the externality of the purchasing behavior and achieving the socially optimal outcome. Given that we are interested in the pathway to promoting overall support, we introduce three new treatments where a tax supporter has an opportunity to influence others. We design the three treatments to address our research questions: are buyers more likely to support the tax after seeing the decision and argument provided by a tax supporter? What are the key contributing factors to the peer effects? In particular, we explore the role of explanations in the peer effects.

3.2. Treatments

No first voter treatment (Baseline)

In this treatment, four participants formed a group. 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, respectively) before the auction started. When an auction started, 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. The automated sellers had a per-unit production cost of 40. Buyers were not informed of the production cost. Sellers would accept all bids greater than or equal to the per-unit production cost. All accepted units were sold at the market price (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.

Each traded unit of goods generated an external cost of 60 (i.e., negative externality) to all buyers regardless of whether they purchased a unit. That is, each buyer incurred an external cost of $60/4 = 15$ for each traded unit. An important feature of the experiment is that the externality was fulfilled one week after consumption. Specifically, the external cost (after being converted to dollars at the rate of $200 = \$1$) was not deducted from the buyers' present payoff, but from their future payoff (an endowment of \$18), to be received one week later.

This market was 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 externality cost. They also saw their per-period earnings and accumulated earnings for both the day of the experiment and one week later. The profit maximizing strategy was to purchase all three units. However, the socially optimal outcome was for each buyer to purchase only two units.

As in *T&X*, a Pigouvian tax and a voting opportunity was introduced at the beginning of the 11th period. The per-unit tax was equal to the per-unit external cost (60). 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.[2]

The voting mechanism worked as follows. 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. After the ballot was completed, buyers were informed about whether the tax was accepted or rejected. They were 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).[3] Different than T&X, in addition to the voting decision, we also asked buyers—before voting—to state their positions toward the tax on a seven-point scale, from Strong Yes to Strong No. This elicitation was necessary to identify tax supporters and the impact of the first voter in the First Voter treatments, as explained below. In the 16th period, all buyers were prompted to vote again for the tax (something they had not been told about in advance). The voting result from the second ballot was then effective for the next five periods (16th to 20th). If the tax was rejected, the market environment remained exactly the same as in the first 10 periods.

First voter with message treatment

The only difference between this treatment and the baseline No First Voter treatment was the voting procedure. In each market, before voting and after every buyer had stated his/her position toward the tax on a seven-point scale. The computer randomly selected one participant who had stated a positive position (i.e., Slight, Moderate or Strong Yes) to be the first voter. The instructions did not explicitly explain the basis on which the first voter was chosen. The only information provided to the subjects was: "When voting on the tax, one member in your group will vote first and write a message to the other three members explaining why he/she voted Yes or No to the introduction of the tax." If no buyers in a market

[2] 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.

[3] 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-game effect.

Table 1
Voting procedure in each treatment.

Treatment	Voting Procedure
No First Voter	Buyers voted Yes or No on the tax
First Voter With Message	A tax supporter (who stated positive positions to the tax) voted first and sent a free-form message to other buyers who would then vote
First Voter Fixed Message	A tax supporter (who stated positive positions to the tax) voted first and sent either "Let's vote Yes" or "Let's vote No" to other buyers who would then vote
First Voter No Message	A tax supporter (who stated positive positions to the tax) voted first; other buyers voted after seeing the choice of the first voter

stated positive positions toward the tax, then one was randomly selected to be the first voter. We exclude these cases in the data analysis, as they are not the focus of our research question.

The first voter voted first and could also write a message to explain his/her voting decision to the other three buyers in the same market. Both the first voter's vote and his/her message were displayed to the other three buyers, who then simultaneously voted on the tax. In the 16th period, the same first voter was prompted to again vote first on the tax and write a message. All other buyers saw the first voter's vote and message before casting their own votes. Buyers were not asked to state their position toward the tax again.

First voter fixed message treatment

This treatment was identical to the First Voter With Message treatment, except that, instead of writing a free form message to explain the decision, the first voter could only choose to send either "Let's vote Yes" or "Let's vote No." This was common knowledge.

First voter no message treatment

In this treatment, the first voter could not send any messages, but was simply asked to publicize his/her vote in each ballot. All the other details were identical to the First Voter With Message treatment.

3.3. 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. 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.[4] At the end of the 20 periods, we administered a survey including demographic questions. Participants' identities were kept anonymous throughout the experiment (see Appendix A for all the instructions).

To implement the intertemporal payment scheme, in the first set of instructions, participants were informed 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.[5] Fig. 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, 136 participants (34 markets) in the First Voter With Message treatment, 88 participants (22 markets) in the First Voter Fixed Message treatment, and 124 participants (31 markets) in the First Voter No Message 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.

[4] 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.

[5] 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.

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.

Fig. 1. Timeline of the experiment.

3.4. Hypotheses

In the baseline No First Mover treatment, it is easy to find out that the profit-maximizing strategy under the tax was to purchase two units. This choice also coincided with the socially optimal level. Without the tax, the profit-maximizing strategy was to purchase three units, exceeding the socially optimal level. *T&X* provide the theoretical analysis and show that buyers seeking to maximize their profits should support the tax. In brief, comparing the payoff with and without the tax, each buyer could earn more with the tax as long as the time discounting rate was not unreasonably high.[6] However, *T&X* found that a significant number of buyers voted against the tax. We expect to replicate this result in our baseline.

T&X provided further experimental evidence suggesting that the increased complexity due to the intertemporal delay of the externality can lead to narrow bracketing. Specifically, unlike individual intertemporal choice settings, where one's future payoff is determined by his/her current choices, a market with stock externalities is a complex intertemporal social environment where one's future payoff is affected by both one's own current choice and other buyers' purchasing behavior. As a result, it is much more difficult to identify the optimal choice.

We hypothesize that, in the First Mover With Message treatment, information provided by tax supporters regarding why one should vote for the tax can help resolve the complexity and reverse the initial negative view of tax objectors. Previous research on social learning has shown that subjects often follow peers' or advisors' recommendations and copy their actions (often to the advantage of their welfare) in diverse situations. Examples include coordinating actions (Schotter and Sopher, 2003), learning to complete a complex task (Celen et al., 2010), and strategizing in school-matching mechanisms (Ding and Schotter, 2017). For example, Cooper and Kagel (2016) found that in a complex signaling game, receiving an advisor's suggestion (by sending free-form messages) for optimal decisions helped an advisee to play more strategically. The reason was that the advisor had strong incentives to help the advisee (which is also the case in our context) and the advisee learned directly from useful advice.[7] Thus, our main hypothesis is:

H1: *The proportion of Yes votes is higher in the First Voter With Message treatment than the No First voter treatment.*

We note, however, that there are two alternative mechanisms that can also lead to a higher support rate in the First Voter With Message treatment.[8]

[6] In particular, voting against the tax means the buyer has a one-week discount rate greater than 100%. Note that a one-week discount rate of 1% already implies an annual discount rate of 67.76%. In the experiment conducted in T&X, most subjects displayed a one-week discount rate in the range 1–20%, far lower than 100%.

[7] Schrah et al. (2006) showed that task complexity could drive individuals' desire to seek advice from experts. Their result hints at the possible connection between the complexity of taxes and the influence of peers who give advice.

[8] Another possible reason that buyers follow the first voter in our experiment setting is that the introduction of the tax requires only two "yes" votes. Thus, purely strategic considerations rather than attitude changes may lead a buyer to vote "yes." For example, a buyer who is not sure how to vote or

First, previous research on peer effects shows that people tend to imitate their peers (Epple and Romano, 2011; Moussaïd et al., 2013; Herbst and Mas, 2015). Merely revealing one's attitude might be sufficient to swing observers' votes (Davis et al., 1988). If buyers simply follow the first voter's vote, we would expect the *First Voter No Message* treatment to achieve a higher support rate than the baseline. In other words, the difference in the support rate between the *First Voter No Message* and the *No First Voter* treatments reveals to what extent H1 is driven by pure imitation effect.

H2 (imitation effect): The proportion of Yes votes is higher in the First Voter No Message treatment than the No First voter treatment.

Second, it is possible that the first voter's message could influence buyers even if it failed to provide any explanations for the first voter's decision or any instrumental information that could help buyers make decisions. In particular, studies have shown that in a public goods game, even a simple non-binding solicitation from a team member about how much to contribute can promote higher contribution to the public goods (Levy et al., 2011; Feltovich and Grossman, 2015; Brandts et al., 2016; Kessler, 2017). If solicitation alone is effective in our context, we would expect the proportion of buyers who vote "yes" to be higher when the first voter can send the message, "Let's vote Yes" (First Voter Fixed Message treatment). Thus, the comparison between the *First Voter Fix Message* and the *No First Voter* treatments informs to what extent H1 is driven by pure solicitation effect.

H3 (solicitation effect): The proportion of Yes votes is higher in the First Voter Fixed Message treatment than the No First voter treatment.

4. Results

Our main goal is to examine how tax supporters can impact other participants' voting decisions. Therefore, for the three treatments with first voters, we included in the analysis only cases where our design successfully selected tax supporters to be first voters (i.e., we excluded those markets where all four buyers reported a negative attitude in the three first voter treatments). For comparability, we also excluded two such markets in the baseline No First Voter treatment in the analysis of this section.[9] This resulted in 13, 22, 19 and 22 markets for our main data analysis in the No First Voter, the First Voter With Message, the First Voter Fixed message, and the First Voter No message treatments, respectively.[10]

We first present results comparing the No First Voter and First Voter With Message treatments to test H1. Then, we turn to the First Voter Fixed Message and First Voter No Message treatments to test the imitation and solicitation effects (H2 and H3). Throughout, we focus on voting decisions. We present detailed results on trading behavior in Appendix B. As reported there, the data confirm that adopting the tax is essential for social efficiency (see Figure B1). During the first 10 periods, the trading behavior was similar across treatments, and the market quantity was consistently at a highly socially inefficient level. From Period 11 onward, however, for groups that adopted the tax, there was a fast convergence to the socially optimal quantity of eight units. In contrast, for groups that did not adopt the tax, there was instead a fast convergence to the market equilibrium quantity of 12 units. Therefore, there was almost no group that could successfully coordinate on efficient outcomes without the tax.

4.1. The influence of first voters (testing H1)

Fig. 2 shows the proportion of buyers voting "yes" to the tax in the first and second ballots. Supporting H1, in the first ballot, the support rate in the No First Voter treatment was significantly lower than in the First Voter With Message treatment (52% vs. 70%, Fisher exact test, $p = 0.031$).[11] When we grouped the buyers based on their initial view, we found that the increase in the support rate originated naturally from buyers who initially stated a negative or indifferent position toward the tax: 11 out of 31 initial objectors and 11 out of 13 indifferent types voted "yes" after seeing the first voter's vote and message in the First Voter With Message treatment. In contrast, none of the 21 objectors, and only four out of

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does not think his/her vote would matter might vote randomly in the baseline. But, if this buyer sees the first "yes" vote, he/she might vote "yes" because his/her vote would be pivotal. As we reported later, our data do not support this possibility.

[9] Including these two markets in the analysis leads to an average tax support rate of 45% in the No First Voter treatment, resulting in an overestimate of the treatment effect compared to the First Voter With Message treatment. At the same time, it overestimates the difference to the other two First Voter treatments.

[10] In addition to the cases where all four buyers reported a negative attitude, there were six, one and seven markets, respectively, in each of the first voter treatments where the first voters initially stated a positive attitude but actually voted "no," and thus first voters were not tax supporters. As a result, we cannot include these markets in the analysis of peer effects of tax supporters. One concern is that excluding these markets might bias the samples in a certain way (e.g., oversample the markets with relatively higher rates of initial supporter in the three first mover treatments). We examine the initial attitude data to check this possibility. We find that the attitude distribution in each of the first voter treatments does not change significantly after excluding these markets. Thus, it is unlikely that excluding these markets would bias our estimation of the peer effects.

[11] We also conducted an OLS regression analysis where the dependent variable is the voting decision and the independent variables are treatment dummies (No First Voter treatment and three First Voter treatments). We then used an F-test to compare voting decisions between treatments. Standard errors are clustered at the group level. The details are reported in Table C3 in Appendix C, where we also conducted probit regressions and controlled for some individual characteristics for robustness. The results are similar across different specifications and consistent with results from the Fisher exact test.

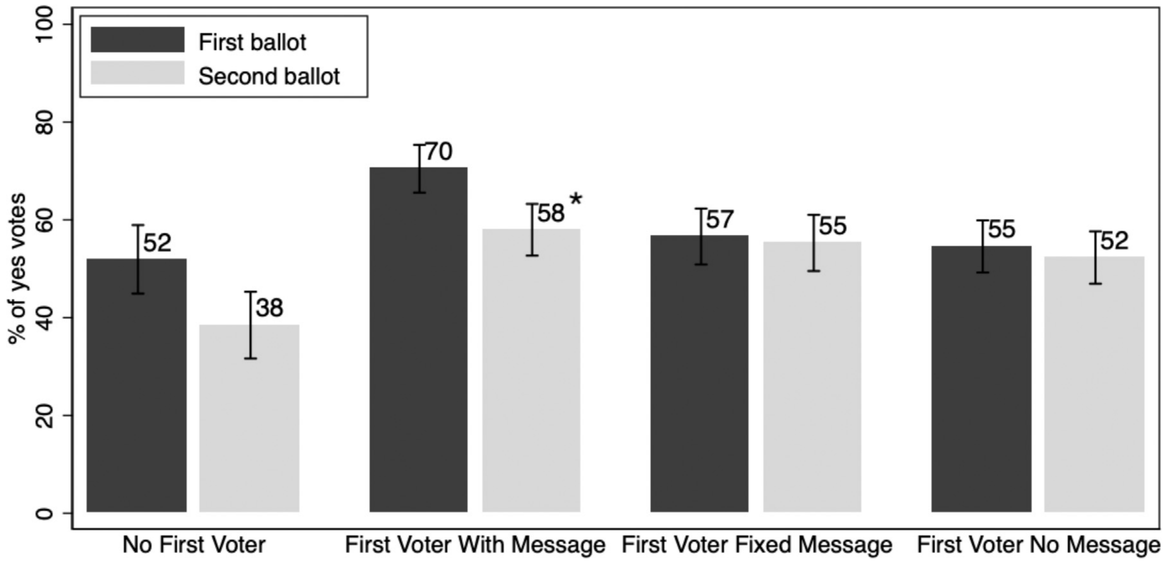

Fig. 2. Percent of "yes" votes among all buyers in the first and second ballots.
* The support rate is 67% when excluding the six markets where the first voter switched to vote "no" in the second ballot.

eight indifferent ones voted "yes" in the No First Voter treatment (Table C1 in Appendix C provides details of the number of "yes" votes in the first ballot given each category of initial views). Combining the initial objectors and indifferent types, their likelihood of voting yes in the First Voter With Message treatment was significantly higher than in the No First Voter treatment (50% vs. 14%, Fisher exact test, $p = 0.002$).

The support rate dropped by 14% and 12% in the second ballot of the No First Voter and First Voter With Message treatments, respectively.[12] As a result, the treatment difference was similar and remained significant (38% vs. 58%, Fisher exact test, $p = 0.036$). A further examination of the data reveals that this drop in the First Voter With Message treatment does not suggest that the tax supporter's influence decayed over time. Rather, six out of the 22 first voters changed their minds and voted "no" in the second ballot.[13] The content of the messages from those who switched to "no" in the second ballot suggests that most of them did not really know why one should support the tax (Table C2 in Appendix C lists all the messages in the two ballots). For example, one simply said "get higher sale" in the first ballot and "just trying another way round" in the second. Another said "I duno lol" in both messages. For the remaining 16 markets where first voters continued to vote "yes" the second time, the support rate remained high at 67%, significantly different from the 38% in the No First Voter treatment (Fisher exact test, $p = 0.003$).

Does tax experience after the first ballot affect voting in the second ballot? We observe a drop in the tax support rate in the second ballot in both treatments, suggesting that experiencing tax may have an adverse effect on voting for the tax. However, if the first voter's influence is persistent, then experiencing tax would have a less adverse effect in the First Voter With Message treatment. To investigate, we conduct a regression analysis where the dependent variable is whether a buyer votes yes in the second ballot. The independent variable is whether the tax is implemented in the buyer's market during periods 11 to 15. We allow different coefficients for each treatment.

Table 2 reports the OLS estimates, showing that in the No First Voter treatment, tax experience significantly decreased tax support in the second ballot, regardless of whether subjects voted for or against the tax in the first ballot. On the contrary, in the First Voter With Message treatment, tax experience insignificantly increased the support among subjects who voted "no" in the first ballot (β_1 vs.β_2, F-test, $p = 0.002$). Among those who voted "yes" in the first ballot, tax experience decreases the support, but to a lesser extent than that in the No First Voter treatment (β_1 vs.β_2, F-test, $p = 0.084$). Overall, these results point to the difficulty of improving tax support, as subjects who have experienced the tax saw their immediate payoff decrease and became more likely to vote against the tax next time. However, the first voter's influence helps mitigate this adverse effect of tax experience.

[12] The No First Voter treatment was a replication of the Delay treatment in *T&X*. They found that the tax support tax was 29% in the first ballot and 33% in the second ballot. Both were much lower than those reported in this study, presumably due to different subject pools and locations. *T&X* was conducted in a public university in the US, while the current study was conducted in a public university in Australia. It is worth noting that in another of our working papers (Huang et al., 2020), we did replicate the delay effect (i.e., support rate is generally lower when the tax benefit is delayed as compared to when it is not delayed) even though subjects in the Australian institution were more likely to support the tax in both the Delay and the No Delay condition.

[13] Tiezzi and Xiao (2016) also observed some supporters switched to vote "no" in the second ballot when the tax benefit was delayed. In our No First Voter treatment, 11 out of 21 supporters switched to vote "no" in the second ballot and four out of 25 objectors switched to vote "yes." These switches resulted in a lower overall support rate around 38%.

Table 2
Impact of tax experience on the tax support in the second ballot (OLS).

	Dependent variable: vote yes in the second ballot	
	First vote no	First vote yes
β_1: Tax Experience in No First Voter	−0.271***(0.062)	−0.399***(0.126)
β_2: Tax Experience in First Voter With Message	.077 (0.123)	−0.203* (0.104)
β_3: Tax Experience in First Voter Fixed Message	.229 (0.147)	−0.217* (0.121)
β_4: Tax Experience in First Voter No Message	.158 (0.119)	−0.261** (0.124)
Constant	.271*** (0.062)	.875*** (0.084)
N	124	180

Note: standard errors are clustered at the group level.
* $p < 0.1$;
** $p < 0.05$;
*** $p < 0.01$.

The higher tax support rate also translates to a lower market quantity of goods purchased, and consequently, higher welfare in the First Voter With Message treatment. Figure B2 in Appendix B shows the total market quantity per group over periods for each treatment. For the first 10 periods without the tax, the market quantity is consistently 12 units. For the last 10 periods, while the market quantity in the No First Voter treatment drops to 9.5 units, it drops to 7.8 units in the First Voter With Message treatment, i.e., very close to the socially efficient quantity of eight units (the unit of observations is the market quantity per group averaged across periods, ranksum test, $p = 0.002$).[14] Finally, we check whether the tax increases the average total profit, which sums up the current payoff and the payoff received next week. In the First Voter With Message treatment, subjects in markets with the tax earn, on average, about 246 points per period (close to the theoretically optimal level of 250). In contrast, those in markets without the tax earn about 216 per period (also close to the theoretically optimal level of 220). The treatment difference in the profit at the aggregate level is relatively smaller (First Voter With Message: 242 vs. No First Voter: 233, rank-sum test, $p = 0.013$). The reason for the relatively smaller difference is that while all but one groups adopted the tax in the First Voter With Message treatment, about half of the groups also adopted the tax in the No First Voter treatment.

Result 1: The proportions of Yes votes in both ballots are significantly higher in the First Voter With Message treatment than the No First voter treatment, leading to more adoption of the tax and higher social welfare.

4.2. The (lack of) evidence of imitation and solicitation effects (testing H2 and H3)

The improvement in the tax support rate did not occur in the other two first voter treatments where the first voters did not provide an explanation for their decision, thus rejecting both H2 and H3. In the First Voter Fixed Message treatment, all 19 first voters who voted "yes" sent the message "Let's vote yes." As shown in Fig. 2, only 57% of buyers voted "yes" to the tax in the first ballot after seeing the first voter's vote and the message. The rate was not significantly different from the 52% in the baseline (Fisher exact test, $p = 0.718$). It was also marginally significantly lower than the support rate of 70% in the First Voter With Message treatment (Fisher exact test, $p = 0.074$). In addition, and in contrast to the First Voter With Message treatment, only seven out of 32 initial tax objectors and three out of six indifferent types voted "yes" to the tax. Combining the initial objectors and indifferent types, their likelihood of voting "yes" in the First Voter With Message treatment was significantly higher than in the First Voter Fixed Message treatment (50% vs. 26%, Fisher exact test, $p = 0.041$). However, compared to the No First Voter treatment, these followers were not significantly more likely to vote "yes" in the First Voter Fixed Message treatment (14% vs. 26%, Fisher exact test, $p = 0.242$). The support rate remained relatively low at 55% in the second ballot.[15]

[14] We also test treatment differences by looking separately at the five periods after the first ballot and the last five periods after the second ballot. During periods 11–15, the average market quantity in the No First Voter treatment is 9.2 units, which is significantly higher than the 7.4 units in the First Voter With Message treatment (ranksum test, $p = 0.017$). Similarly, during periods 16–20, the average market quantity in the No First Voter treatment is 9.8 units, which is significantly higher than the 8.3 units in the First Voter With Message treatment (ranksum test, $p = 0.032$).

[15] Six out of 19 and six out of 22 first voters voted "no" in the second ballot of the First Voter Fixed Message and First Voter No Message treatments, respectively. Nevertheless, the overall support rate didn't drop as much, suggesting again that first voters had little impact on followers' decisions.

We observe a similar low support rate in the First Voter No Message treatment. As shown in Fig. 2, 55% of buyers voted "yes" to the tax in the first ballot. The support rate was not significantly different from the baseline (55% vs. 52%, Fisher exact test, $p = 0.861$) but was significantly lower than in the First Voter With Message treatment (55% vs. 70%, Fisher exact test, $p = 0.043$). Only five out of 35 initial tax objectors and five out of 10 indifferent types voted "yes" to the tax. Combining the initial objectors and indifferent types, their likelihood of voting "yes" in the First Voter With Message treatment was significantly higher than in the First Voter No Message treatment (50% vs. 22%, Fisher exact test, $p = 0.008$). However, compared to the No First Voter treatment, they were not significantly more likely to vote "yes" in the First Voter No Message treatment (14% vs. 22%, Fisher exact test, $p = 0.545$). The support rate remained low at 52% in the second ballot. In these two first voter treatments, tax experience appears to have a similar effect on voting in the second ballot as the First Voter With Message treatment (see Table 2).

The low tax support rates in these two treatments are partially reflected in market quantity and welfare. Figure B1 in Appendix B shows that in the first 10 periods, the market quantity is consistently 12 units in these two treatments. It then drops to 8.5 units in the last 10 periods. Despite the overall 55% support rate, about 80% of the groups adopted the tax since it only needed two "yes" votes out of four to pass. The market quantities in both treatments are significantly lower than in the No First Voter treatment (First Voter Fixed Message vs. No First Voter: $p = 0.050$; First Voter No Message vs. No First Voter: $p = 0.033$, rank-sum test) and significantly higher than in the First Voter With Message treatment (First Voter Fixed Message vs. First Voter With Message: $p = 0.047$ and First Voter No Message vs. First Voter With Message: $p = 0.019$, rank-sum test). The average total profit is directionally consistent but not statistically significant in all of these treatment comparisons (First Voter Fixed Message: 239; First Voter No Message: 237).

Result 2: The proportions of Yes votes in both ballots in the First Voter Fixed Message and First Voter No Message treatments do not significantly differ from that in the No First voter treatment. For the first ballot, the proportions of Yes votes in these two treatments are significantly lower than that in the First Voter With Message treatment.

4.3. Discussion and content analysis of the messages

The differences between the three first voter treatments and the baseline support H1, but not H2 or H3. These results suggest that for the first voter to influence other buyers, it was important that they provide an explanation for their voting decision. Buyers did not seem to simply follow the first voter's vote.[16] Nor did they recklessly comply with the first voter's solicitation to vote "yes." This indicates that the peer effect cannot be attributed to any social utility of casting the same vote as one's peers (Bursztyn et al., 2014).[17] Instead, the peer effect is more likely to be attributed to observational learning (Banerjee, 1992; Bikhchandani et al., 1992): buyers infer from the first voter's behavior *and* explanations that voting "yes" is in their best interest.

In our setting, buyers make decisions in a complex intertemporal social environment where their future payoffs are affected by both their current choice and other buyers' purchasing behavior. The complexity may introduce uncertainty about the consequences of adopting the tax and the nature of the intertemporal tradeoff involved in deciding to support the tax. First voters' explanations can provide information to buyers about what decision is in their best interest and help resolve the uncertainty. In this regard, buyers do not view the first voter's vote or their simple suggestion as sufficiently informative.

To further test the importance of explanations in the first voter effect, we conducted a content analysis of the messages written by the first voters in the First Voter With Message treatment. Given that 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 those whose did not. Additionally, we consider another possibility in view of previous research that suggests social influence is stronger when individuals view their peers to 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 confidence level 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.

To evaluate these messages, we recruited 13 evaluators from the MonLEE student subject pool. We use Houser and Xiao's (2011) classification coordination game to incentivize coding. Evaluators were seated separately and worked independently. They first read the coding instructions explaining their task. They were also provided with a copy of the instructions of the First Voter With Message treatment and asked to complete a quiz to ensure that they understood the instructions. They were not given any information about the purpose of the study. Evaluators then received all first voters' messages in the first ballot. 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 ex-

[16] This result also contradicts the possibility that buyers follow the first voter for strategic reasons. For example, a buyer who was not sure how to vote or did not think their vote would matter might vote randomly in the baseline but be more likely to vote "yes" if they knew that the first voter had voted "yes" and their vote would be pivotal.

[17] One source of the social utility discussed in previous literature is that people may be concerned with their incomes relative to their peers and thus tend to make the same choice as peers, i.e. "keeping up with the Joneses" (e.g., Abel, 1990; Campbell and Cochrane, 1999). This is unlikely to occur in our setting, as the outcome of the ballot applies to all the buyers in the market.

plain 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."[18]

After they completed coding all messages in the first ballot, they received messages in the second ballot and were similarly instructed to classify each message according to the two coding systems.[19] Each evaluator received $15 for coding all messages. In addition, two messages were randomly selected at the end of the session. For each, if an evaluator's classification matched the most popular classification, he/she received an additional $5.

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). It is worth noting, however, that the sample size is limited in some cases. Therefore, the interpretation of the data should be taken with caution and only as supplementary information to what we have learned from the behavioral data reported above. Our findings from this exercise confirm the importance of explanations in the effect of first voters.

In cases where first voters' messages were classified as "Explained Why," nine out of 12 (75%) initial objectors or indifferent types voted "yes." In contrast, when the messages were classified as "Statement Only," "Other Reasons," or "No Reasons," only 13 out of 32 (41%) initial objectors or indifferent types voted "yes" (the difference is marginally statistically significant, $p = 0.088$, Fisher exact test). The overall tax support rate was 86% in cases of "Explained Why," but dropped to 63% when the messages belonged to other categories. This result is consistent with the importance of explanations in peer effects, as we observed from the treatment difference in the voting behavior reported above.[20]

On the other hand, we do not observe much evidence for the role of confidence in the peer effect. The percentage of initial objectors or indifferent types who voted "yes" is only slightly higher when first voters' messages revealed they were "Confident" in their decisions than when the messages revealed they were "Not Confident" or provided "No Information" on confidence. The difference is not significant (11 out of 19 (58%) vs. 11 out of 25 (44%), $p = 0.543$, Fisher exact test). (See more detailed analysis in Appendix E.)

Result 3: The influence of first voters can be attributed to the meaningful information but not to the level of confidence embedded in their messages.

5. Conclusion

We conducted a controlled laboratory experiment to understand whether and how peer communication can influence public support for Pigouvian taxation. Our data provide causal evidence that policy supporters can significantly influence on others' decisions to support the tax. Importantly, we show that buyers do not simply follow a tax supporter's vote or suggestion. Thus, to promote the support rate, it is crucial to provide explanations to help participants understand the consequences of taxation. These results provide converging evidence that one key obstacle in implementing Pigouvian taxation may be complexity (due to the intertemporal structure of the tax).

By disentangling the reasons underlying the peer effect, our study sheds light on whether introducing the peer effect can be welfare improving. If people change their vote simply to conform to other peers (probably due to peer pressure), the resulting higher supporting rate may not necessarily be welfare improving. On the other hand, if people change the votes only when an explanation is provided, this suggests that the initial negative attitude is more likely stemming from misconception and imperfect knowledge about the economic outcomes of taxation. In this case, it can be socially beneficial to correct the attitudes. Our findings thus provide a behavioral foundation for applying peer effect and communication to influence public attitudes.

The evidence that peer effect does matter suggests that a next step that can be beneficial is how to design mechanisms and platforms that give voice and visibility to members of the general public who support efficient but complex tax policies. In naturally occurring environments, some people (e.g., experts) may have already been playing an active role in sharing their opinions in public, via tweets or blogs, or publishing articles. Their influence, however, is often limited to their own networks. Moreover, people may also be exposed to messages from tax objectors.[21] To amplify the influence of supporters,

[18] The message coding instructions are reproduced in Appendix D. Subjects were also asked to code messages from the other two self-nomination treatments reported later in the paper. A session lasted about 1.5 hours. The coding results of these two treatments are reported in Appendix E. For all these messages, 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 main analysis. For the resulting categories, the interrater agreement rate (Cohen's Kappa) is 0.66 for coding whether first voters provided reasons and 0.25 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.

[19] We omitted those messages if the first voter voted "no" in the second ballot. Messages in the first ballot were presented alongside messages in the second ballot, but subjects did not need to code the former again.

[20] Tiezzi and Xiao (2016) reported another treatment, called Delay_transparent, in which the instructions included additional examples illustrating the intertemporal tradeoffs that buyers make when deciding whether to adopt the tax. They find the tax support rate was significantly higher than in the baseline Delay treatment (a similar treatment to the No First Voter treatment in this study). This is also consistent with the result from the content analysis that explanations are the key to higher tax support rate.

[21] In our follow up study (Huang and Xiao, 2020), we show that if people are left on their own to decide whether to advocate their opinions, both supporters and objectors are equally willing to be first voters and their influence over other subjects is also comparable. As a result, the overall support

particularly the experts, institutions can design platforms to promote salience and accessibility of their opinions.[22] For example, tax supporters can distribute in the community information brochures that explain the workings and consequences of the Pigouvian taxes. Individuals may also be subject to self-selection bias when deciding who to listen to. Future research would be valuable to investigate how to design communication mechanisms that mitigate the selection bias. For example, will we observe more selection bias when disseminating information online compared to distributing information door to door through local community?

In our experiment, we test the peer effect in a market with only four buyers. An interesting question is whether the effect remains significant when there are more buyers. On one hand, since peer effect mainly works through resolving the complexity of the tax, we speculate that the information value of the message should not vary on the number of buyers in the market. On the other hand, a larger group may introduce coordination issues. Future research that examines the peer effect when the group size is larger can be fruitful.

Our results on the peer effect may generalize to other public policies, especially complex ones. However, we should be cautious about any potential countervailing effects in some specific environments and should examine them case by case. For example, in our setup, all buyers experience exactly the same costs and benefits of environmental taxation. As a result, their self-interest is in line with the introduction of taxes. This simplification allows a clear-cut interpretation of the data. However, in reality, there tends to be an unequal distribution of costs and benefits of environmental taxation. Some people are bigger winners or losers than others. This heterogeneity may have particularly important implications on voting decisions. It may also weaken the peer effect due to unaligned interests. One interesting avenue for future research might be to incorporate this unequal cost/benefit distribution of environmental taxation into the current market experiment framework.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jebo.2021.04.019.

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rate remains similar to the baseline without the possibility of peer effect. These findings further highlight the importance of promoting communication between the tax supporters and the public.

[22] In our experiment, the tax supporters are untrained non-professional voters. The findings of the effectiveness of messages from these non-experts may suggest that experts may be even more influential. It would be interesting to conduct field studies to compare the influence of experts and non-professionals.

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