Time delay, complexity and support for taxation

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Abstract

People often experience the benefits of taxation over time. We design an intertemporal market experiment with negative externalities to examine how delaying the benefits of taxation affects support for taxes. We find that when negative externalities occur immediately, people learn to adopt Pigouvian taxes, which are aimed at reducing negative externalities and restoring market efficiency. By contrast, when negative externalities are delayed, people are less receptive to taxation. This effect persists over time. Our data reveal that the strong negative delay effect can be explained in large part by narrow bracketing and the increased perceived complexity of the environment, rather than by time discounting per-se. We argue and demonstrate that increasing the transparency of intertemporal tradeoffs can effectively promote support for taxation.

Introduction

Taxation, as an incentive-based instrument, is a core policy tool used to address negative externalities, such as pollution. Taxes can improve social welfare and regulate undesirable activities by increasing the price of the targeted undesirable activity. When the tax amount is equal to the external cost at the optimal level of the targeted activity, as in Pigouvian taxation, the social optimum can be restored if externalities are the only deviations from optimality. Even though standard welfare economics has shown that incentive-based instruments like taxation are ultimately beneficial, there are often obstacles to implementing taxation, due to low public support.

One example is climate change. In the United States, political opinion is now shifting to support taking action on climate change. There appears to be substantial agreement among U.S. economists spanning the political and academic spectrum (Hsu, 2009) that carbon taxes are the most efficient means of reducing large-scale pollution problems. The carbon tax approach would complement European action on carbon emissions through the European Union (EU) Emission Trading Scheme (ETS). Nonetheless, support for efficiency-enhancing policies is fragile. The lack of public support can impede fiscal interventions to change behavior and improve social welfare. Therefore, it is important to understand the reasons for public reactions to different tax proposals.

1 The lack of public support for taxes can also be an impediment to choosing the most cost-effective policy instrument. For example, Goulder and Schein (2013) noticed that quantity-controls like Cap-and-Trade systems are often preferred to taxes by policy makers to address the Climate Change problem, due to their higher political palatability. Cap-and-Trade systems, however, can be less cost-effective than taxes if the allowances are allocated for free.

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In this paper, we draw attention to the fact that many consumption or production activities produce negative externalities only over time. Stock externalities are a typical example of such time-delayed externalities. Consumers obtain the benefit of consuming gasoline immediately, but pollution only accumulates over time. When externalities are caused by a stock of pollution rather than a flow, and when stocks decay slowly, as with greenhouse gases, the problem is dynamic and current emissions can cause future environmental damage. Stock externalities imply that costs and benefits of policy measures, such as Pigouvian taxation, occur at different times. When people cannot readily see the benefits of certain taxes, they can be less likely to support them. This is particularly true for Pigouvian taxes.

Previous research on public support for taxes (Kallbekken et al., 2011; Cherry et al., 2012; Blaufus and Möhlmann, 2014; Cherry et al., 2014) has neglected this dynamic aspect of the problem, even though research on intertemporal choice has shown that people are less willing to take preventive costly actions now if the predicted losses occur only in the future (Frederick et al., 2002).

To understand the role of intertemporal trade-offs in public support for taxation, we design an intertemporal market experiment with consumption externalities, a variation of previous experiments on public support for taxes (Kallbekken et al., 2011; Sausgruber and Tyran, 2005). We manipulate the timing of the externality and introduce opportunities for the participants to vote on whether to introduce a tax on consumption. We first compare voting results when the external costs of consumption happen in the present (No Delay treatment) and when the external costs occur one week later (Delay treatment). In both treatments, participants first purchase units of a consumption good for ten periods, then vote on whether to introduce a tax on the purchased items in the following trading periods. The voting outcome is applied to the next five periods. Participants are then given another opportunity to vote on taxation for the last five periods.

In addition to testing the delay effect, studying tax attitudes in a controlled laboratory experiment sheds light on the mechanisms underlying the negative delay effect. Previous research on intertemporal choices suggests that time discounting may not be the only reason for the delay effect. In particular, we hypothesize that the intertemporal structure of the externalities largely increases the perceived complexity of decision-making in the market and leads to narrow bracketing, i.e. basing today's decisions mainly on today's payoffs with little regard for future payoffs (Read et al., 1999; Rabin and Weizsäcker, 2009). The reason is that in our Delay treatment, decision-makers’ current consumption choices affect each other’s current and future payoffs. This is in contrast to simple individual intertemporal decision-making, where one’s future payoffs are affected by her own decisions only.

Understanding whether complexity and narrow bracketing lead to the potential delay effect can have important policy implications, e.g., for designing institutions to help deal with complexity. To understand the role of complexity, we design the parameters in such a way that time discounting alone could not account for the lower support for taxation in the Delay treatment compared to the No Delay treatment, unless there were an extremely high one-week discount rate according to a simple exponential time discounting model. Interestingly, even in this setting, we observe a much lower support for taxation in the Delay treatment. Inspired by previous research on default effects (e.g., Thaler and Benartzi, 2004), we also test whether framing the tax as a default might mitigate the negative delay effect (Remove treatment). We find, however, that support remains low (and unchanged) in the Remove treatment.2 Supporting the hypothesis of complexity and narrow bracketing, data from the Delay_Transparency treatment show that we can eliminate the detrimental delay effect by providing participants explicit information about the intertemporal tradeoffs involved in the tax voting decision. We discuss the implications of our findings for the design of tax policies.

The remainder of the paper is organized as follows. Section 2 discusses the related literature. Section 3 describes the experiment design, the predicted outcomes, and procedures. Section 4 presents the results. Section 5 describes and reports results from the Delay_Transparency treatment. Section 6 concludes.

Related literature

Much research, both experimental and non-experimental, has focused on identifying the determinants of people’s attitudes toward taxes. A few non-experimental papers (Rivlin, 1989; Dresner et al., 2006) have stressed the role of trust toward the government collecting tax revenues. Experimental research has focused on other determinants, such as the perceived fairness of the instrument (Feher and Schmidt, 1999) and equity considerations (Durante and Putterman, 2014). In a study particularly relevant to our paper, Sausgruber and Tyran (2005) and Kallbekken et al. (2011) designed a market experiment to test the framing of taxation as a factor affecting public support. They found that the framing of taxation affects the perception of the tax burden. Likewise, they explored how earmarking tax revenues affects public support.

Other researchers have explored a phenomenon called tax aversion, in which people perceive the burden of tax payments to be greater than other differently-labeled but economically equivalent payments (Small et al., 2006; McCaffery and Baron, 2006; Kallbekken et al., 2010; Blaufus and Möhlmann, 2014). More recently, Blumkin et al. (2012) added experimental

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2 The absence of a default effect contrasts with previous findings on the power of default options in the take-up rate of policies such as organ donation or saving (Thaler and Benartzi, 2004; Johnson and Goldstein, 2003).

3 Researchers have also conducted experiments to study other related questions, such as tax compliance (Alm et al., 1999); the role of communication in the diffusion of policy innovations (Tyran and Sausgruber, 2005); and whether a voting rule with and without a punishment mechanism increases contributions to a public good (Kroll et al., 2007).
Evidence on “money illusion,” showing that individuals underestimate the burden associated with an indirect consumption tax (that erodes real purchasing power) relative to the corresponding burden associated with a direct wage tax.

These previous contributions on public support for taxation have neglected the role of time delayed externalities on support for taxation, i.e. the fact that taxes implying upfront costs against future benefits (reduced externalities) may receive less support. This is true even though a large intertemporal choice literature has stressed that people are less willing to take preventive costly actions now if the losses occur only in the future (Frederick et al., 2002).

The concept of time discounting offers one explanation as to why people may be less likely to accept taxes when the benefit occurs in the future: individuals value the current costs and benefits more than the future ones (Frederick et al., 2002). Nonetheless, time discounting may not be the only reason for any potential delay effect. Research on intertemporal individual choice has shown that higher cognitive loads make people more present-biased and more likely to succumb to visceral temptations (Shiv and Fedorikhin, 2002). Brown et al. (2009) found lower values of the present bias parameter $\beta$ in more complex environments compared to less complex ones. They also showed that both private learning (direct experience) and social learning (learning from the experience of others) reduce present bias (increase the value of $\beta$).

The relationship between complexity and myopic choices may be explained by narrow bracketing, i.e. individuals making separate sequential decisions in each time period rather than considering the consequences of their current choices for their future payoff (Read et al., 1999). Narrow bracketing is more likely to occur when the future consequences are too complicated to consider (Gabaix and Laibson, 2006). In view of this previous research, we investigate the role of complexity on support for taxes when the benefit of taxes is delayed and determined by one’s own and others’ choices.

**Experiment design**

To address our research question, we design an intertemporal market experiment with externalities and voting for the introduction of a Pigouvian tax. Our design is related to three strands of experimental research: laboratory markets predicted to quickly converge towards the equilibrium (Smith, 1962; Smith et al., 1982); laboratory markets to examine policies for externalities (Plott, 1983; Tyran and Sausgruber, 2005; Kallbekken et al., 2010, 2011); and experiments using voting to endogenously introduce tax institutions (Sausgruber and Tyran, 2011).

In our setting, subjects earn money by trading a hypothetical consumption good in the market. Each unit traded causes external costs that are equally split by all buyers in each market. Each treatment has two practice trading periods, during which subjects do not make money, and 20 paid trading periods. Participants are not told how many paid trading periods they will participate in during the experiment. The 20 paid trading periods are divided into three stages. The first stage is the first ten paid periods in which participants can trade units of the good in each period. Participants are not given any information about the following two stages. The second stage is the following five periods. At the beginning of this stage (Period 11), participants are asked to vote on whether to introduce a Pigouvian tax (called tax in the experiment) in the following trading periods. Participants are not given any information about the third stage. The third stage is the last five periods that proceed in the same way as the second stage. In particular, at the beginning of Period 16, participants are asked to vote for the same tax again. After the 20th trading period, we administer a survey to elicit the subjects’ one-week discount rate and to obtain feedback on the experiment.

To allow the manipulation of the timing of the externality and the benefit of taxation, we tell subjects at the beginning of the experiment that, in addition to the earnings from trading accumulated during the experiment, they will receive an additional $18 cash payment one week after the experiment. To receive this $18 endowment, participants must return to the same lab exactly one week after the end of the experiment, without having to perform any additional task. To minimize any credibility concerns on the part of subjects in the experiment, at the end of each session each subject is given a “payment certificate” signed by the experimenter and indicating the amount to be received, the date, time, and location for the collection of the payment, and the contact details of the experimenter, including office address, telephone and email address. In addition, the day before the scheduled pickup day, participants receive a reminder email. Fig. 1 provides the time line of our experiments.

**The market**

Given that our main interest is the difference in voting behavior between treatments, we design a simple uniform-price, multi-unit auction (a simplified version of Smith et al. (1982)) in which each market consists of four buyers and one
automated seller. The auction market we design has also been used by others to investigate people’s attitudes towards taxes (Sausgruber and Tyran, 2005, 2011; Kallbekken et al., 2011 and Cherry et al., 2014). The demand and supply parameters for all treatments are illustrated in Fig. 2.

The buyers are informed about the resale values of the three units they may purchase (160, 110, and 70, respectively) and told that the seller’s marginal cost will remain constant throughout the experiment. In each trading period \( k = 1, \ldots, 20 \), each buyer \( i = 1, \ldots, 4 \) reveals his/her willingness to buy (WTP) for each unit \( j = 1, \ldots, 3 \) by posting a bid \( b_{ijk} \in [0, \ldots, v_{ij}] \) where \( v_{ij} \) is the resale value of unit \( j \) for buyer \( i \). Sellers have a constant, per-unit production cost \( c \) of 40 points. For each unit \( j \), bids from all buyers in each market are ordered from high to low. Sellers will accept all the bids greater than or equal to \( c \) and sell the units at the market price \( p_{k} \), which equals the lowest accepted bid in trading period \( k \). As the market price is determined by the lowest accepted bid, the bids of each buyer affect market prices and buyers experience price fluctuations during the trading phase of the experiment. Each buyer \( i \)'s gross income earned on each unit \( j \) in each trading period \( k \) is

\[
\pi_{ijk} = v_{ij}/C_{0} p_{k} \times MEC_{k} \quad (1)
\]

where \( d_{ijk} = 1 \) if the \( j \)th unit is traded in period \( k \) and \( d_{ijk} = 0 \) otherwise.

As in Sausgruber and Tyran (2005), the following trading and information rules apply: (i) Buyers must place one bid at a time, starting with the first unit; (ii) buyers cannot resell what they have bought; (iii) each buyer knows only her own resale values and that the unknown seller’s marginal production cost remains constant throughout the experiment; and (iv) communication among participants is not allowed.

At the end of each trading period, each buyer can review information about the outcome of the trading period. The computer shows information about market price, quantity, the buyer’s bids, per-capita externalities as described below, and accumulated and per-period earnings. Each buyer can also review the history of results, i.e. the outcome, market price and quantity, units purchased, and payoff of past trading periods (see Appendix A.2 for samples of outcome screens).

Participants are told that trading causes external effects. In particular, a marginal external cost \( MEC = 60 \) points is caused by each unit traded so that the per-unit external cost per person is \( MEC_{i} = MEC/n = 15 \), where \( n = 4 \) is the total number of buyers in each market. The key feature of our experiment is that we manipulate the time at which the external cost is paid, either immediately on the day of trading, or one week later.

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- **Immediate externality (No Delay treatment)**
  
  In the No Delay treatment, the external cost of trading is deducted from the subjects’ earnings on the day of trading. With the immediate externality, each buyer \( i \)'s pretax payoff in each trading period \( k \) is

\[
\pi_{ik} = \sum_{j} d_{ijk} \pi_{ijk} - \sum_{j} \sum_{j} d_{ijk} MEC_{ik}
\]

where \( d_{ijk} = 1 \) if the \( j \)th unit is traded in period \( k \) and \( d_{ijk} = 0 \) otherwise.
Delayed externality (Delay treatment and Remove treatment)

In the Delay and Remove treatments, the external cost of trading is deducted from the $18 endowment subjects receive one week after the day of trading. This condition simulates an environment where activities that are currently beneficial, such as the consumption of gasoline, may cause negative external costs in the future, such as pollution.

Building on the theory of stock externalities (Farzin, 1996; Karp, 2005; Kolstad, 2010), trading today causes negative external costs in future periods and external costs accumulate over time. Considering only two time periods, today (trading day), \( t \), and one week later, \( t+1 \), we define the stock of external costs affecting subject \( i \) at time \( t+1 \) as

\[
s_{it+1} = \delta \left( \sum_i \sum_j d_{ij} MEC_{it} \right)
\]

where \( 0 \leq \delta \leq 1 \) is the persistence rate of the external costs generated at time \( t \). When \( \delta = 0 \), the external costs at time \( t \) are not carried forward to time \( t+1 \). When \( \delta = 1 \), the external costs at time \( t \) are entirely carried over to time \( t+1 \). Stated differently, \( \delta \) indicates what fraction of the additional costs from trading at time \( t \) will be borne at time \( t+1 \). In our experiment, \( \delta = 1 \).

The discount factor used to compare payoffs at time \( t \) and \( t+1 \) is \( \beta{\gamma} = \beta/(1+r) \), where \( r \) is the (weekly in our setting) discount rate and \( 0 < \beta \leq 1 \). The value \( \beta = 1 \) produces the standard model of constant (exponential) discounting, and if \( 0 < \beta < 1 \) there is quasi-hyperbolic discounting. Each subject \( i \)'s pretax payoff in each trading period \( k \) becomes

\[
\pi_{it} = \sum_j d_{ij} \pi_{ijt} - \beta \sum_{\tau=1}^{T-1} \gamma^\tau (s_{it+\tau})
\]

where \( \tau \) is the number of delays from the current time period \( t \) and \( T \) is the total number of time periods. When there are only two time periods, \( t \) and \( t+1 \), \( \tau = 1 \) and expression (3) becomes

\[
\pi_{it} = \sum_j d_{ij} \pi_{ijt} - \beta \gamma s_{it+1}
\]

In our setup, it is easy to calculate that, at market equilibrium, without taxation, all buyers purchase three units at a price of 40 (see \( D_0 \) in Fig. 2) in both the No Delay and the two Delay treatments. However, the socially optimal outcome can be reached if each buyer purchases only two units (see Appendix C for details).

Tax

In the Delay and No Delay treatments, at the beginning of Periods 11 and 16, subjects vote for the introduction of a revenue neutral Pigouvian tax. The tax rate is equal to the marginal external cost \( T = MEC = 60 \) on each unit traded, and an equal share of the total tax revenues collected in each market in each period is returned to each buyer. The revenue recycling mechanism is identical in the Delay and No Delay treatments. That is, even when the external costs are delayed, the tax is paid immediately and the fiscal revenues are divided equally among buyers and returned immediately. This ensures that any treatment differences can be attributed to the delayed externality, rather than to issues such as earmarking of the fiscal revenues or uncertainty regarding the future use of the revenues. As shown in Fig. 2, the tax shifts the demand curve downwards, from \( D_0 \) to \( D_1 \).

\[\text{Fig. 2. Induced Market Demand and Supply.}\]

\[\text{Since each trading period } k \text{ has the same set up, to ease notation we henceforth omit the } \text{“} k \text{” subscript from payoff functions.}\]
A profit-maximizing buyer \( i \) will support the tax as long as the profit under the tax regime \( (\pi_{\text{tax}}) \) is higher than without the tax \( (\pi_i) \). It is easy to calculate that condition \( \pi_{\text{tax}} \geq \pi_i \) holds in the No Delay treatment. In the Delay treatments condition \( \pi_{\text{tax}} \geq \pi_i \) holds if \( \beta \gamma \geq 0.5 \) or \( \gamma \geq 0.5/\beta \) (see Appendix C for details). This result suggests that, if time discounting alone is the reason for any delay effect on tax support, we will observe a different voting behavior in the Delay and No Delay treatments only when the individual discount rate is extremely high: if we assume exponential discounting \( (\beta = 1) \), voting against the tax means the buyer has a one-week discount factor \( \gamma < 0.5 \) (i.e., \( r > 100\%) \).\(^8\) On the other hand, the intertemporal environment in which our subjects make their voting decisions is much more complicated than a simple survey question where the tradeoff between a smaller immediate reward and a bigger delayed reward is explicit. As we mentioned in Section 2, due to bounded rationality, people often over-discount or overlook the future in computationally difficult intertemporal environments (Brown et al., 2009; Read et al., 1999; Gabaix and Laibson, 2006). In the delay conditions, the benefit of introducing the tax (the reduction of the external cost) is determined not only by one’s own trading behavior, but also the other three buyers trading behavior. Thus, the future benefit of the tax may be difficult to compute. As a result, buyers may decide whether to support the tax mainly considering today’s payoffs with little regard for payoffs next week. Therefore, even with a low discount rate, we still expect to observe a lower support for the tax in the Delay conditions if the decision environment is sufficiently complicated for buyers.

**Voting procedure**

Before reaching the ballot, subjects receive an additional set of instructions (see Appendix A.3) with examples explaining the tax mechanism and the consequences of the revenue-neutral taxation on their payoff. They are asked to answer a set of questions to make sure they understand the functioning of the tax. In the ballot, all participants simultaneously vote yes or no on the introduction of the tax; abstentions or neutral votes are not possible. Voting is anonymous. The tax is implemented in a market for the following trading periods if at least two participants in the market vote yes (Casari and Luini, 2009). Participants are not told about the second ballot when voting in the first ballot. Similar to previous studies (Markussen et al., 2014), each participant is informed about the voting outcome in her own market, but not about individual votes. That is, participants are not informed exactly how many voted for the tax.

These same voting rules apply to the Remove treatment, except that subjects are informed at the beginning of Periods 11 and 16 that the tax has been introduced, but they can vote no if they want to remove it (see Appendix A.3). We design the Remove treatment such that people still need to vote. That is, the voting decision screen of the Remove treatment is exactly the same as the Delay treatment. The only difference between the two is that, in the voting instructions, the tax is framed as a default. Thus, our design of the Remove treatment can be considered as a weak form of default setting. Yet, we still expect the default may increase support for taxation if it makes the tax a cognitively easier choice (Gigerenzer, 2008) or if it leads subjects to perceive imposing the tax as the norm or as an implicit recommendation from the policy maker (or from the experimenter in the experiment) (McKenzie et al., 2006).

**Questionnaire**

As we explain above, for time discounting alone to cause any delay effect on tax support in our setting, the individual discount rate would have to be extremely high. Although such a high discount rate does not seem to be possible, we nonetheless conduct a survey to examine whether that was the case in our experiment. We do this by eliciting the one-week discount rate of each subject.

We use a simplified version of the basic experimental design introduced by Coller and Williams (1999).\(^9\) Subjects are given a set of nine decisions. Each decision consists of choosing between $20 today and a larger amount in one week.

**General experimental procedures**

The experiment was conducted at the Pittsburgh Experimental Economics Laboratory (PEEL) with a total of 212 students as participants. Each subject could participate in only one of the three treatments: 76 subjects (19 markets) participated in the No Delay treatment; 72 (18 markets) in the Delay treatment; and 64 (16 markets) in the Remove treatment. At the beginning of each session, subjects were randomly assigned to markets and remained in the same market throughout the experiment. Subjects were not informed about the voting stage until they reached the end of trading Period 10 and were given a second set of instructions. To ensure subjects understood the instructions, each one had to finish a comprehension quiz before making any decisions. Each session included 16 or 20 subjects and lasted around 90 minutes. There were a total of 12 sessions with four sessions per treatment, for a total of 53 markets. Earnings were expressed in experimental points.

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\(^8\) Note that a one-week discount rate of 1% already implies an annual discount rate of 67.76%.

\(^9\) Casari and Luini show that peer punishment in public good games is carried out only when there is a coalition of two or more agents sharing the same norm.

\(^10\) There are different mechanisms to elicit the discount rate. We adopt simple ones because the main part of the experiment is already complicated and long.
and exchanged for cash at $1 per 200 points. Participants earned on average $28, including a show-up fee of $5. All sessions were programmed and conducted using the software Z-tree (Fischbacher, 2007).

To maintain the salience of the external costs symmetric across treatments, we showed each buyer the external costs incurred at the end of each trading period (see Appendix A.2 for computer screen samples).11 In naturally occurring environments, the impact of the external costs on welfare can be less salient when costs are observable only in the future. This can be another reason why people are less willing to accept taxation. We expect the delay effect on support for taxation to be even more significant in the real world when people cannot see as clearly how the externality will influence their future welfare.

Results

Trading activity

The average market quantity in the first 10 trading periods is around 11 in all treatments, and is not statistically significantly different across treatments (Mann-Whitney test, \( p > 0.50 \)).\(^{12} \) Fig. 3 reports the average market quantity over the 20 trading periods, by treatment. Consistent with previous studies (Sausgruber and Tyran, 2005; Tyran and Sausgruber, 2005; Kallbekken et al., 2011), there was a fast convergence to the market equilibrium quantity of 12 units during the first 10 trading periods, even though this leads to an inefficient outcome.

After the first ballot, the market quantity drops to the efficient level of eight units in the No Delay, because most groups (84%) adopt Pigouvian taxation. In the Delay and Remove, only 28% and 25% of the groups adopt the tax, respectively. As a result, the market quantity in these treatments remains relatively high (10 units), although it is lower than in the first 10 periods.

The average market price is not statistically significantly different across treatments (No Delay vs. Delay, \( p = 0.491 \); No Delay vs. Remove, \( p = 0.556 \)), but it is statistically significantly different from the equilibrium market price of 40 points (\( t \)-test, \( p < 0.01 \)). In trading periods 11–15 and 16–20, the average market price is not statistically significantly different across treatments.

Voting on taxation

Table 1 shows some descriptive statistics on the number of yes votes at both group and individual levels in the two ballots by treatment. In the first round, the support rate for taxation is significantly lower in the Delay and the Remove treatments than in the No Delay (29.15% Delay vs. 59.21% No Delay, \( p < 0.01 \); 26.55% Remove vs. 59.21%, No Delay, \( p < 0.01 \)). In the second round, the rate of group yes votes remains significantly lower in the Delay and Remove treatments than in the

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11 One concern raised by an anonymous reviewer is that the “additional costs per person” is shown in the central box of the outcome screen in the No Delay, but in the bottom right corner box in the Delay and Remove. As a result, we may have made the additional costs less salient in the delay conditions compared to the No Delay. To check whether this may be the reason for the treatment difference we find, we conducted another Delay treatment where we added the information “additional costs per person” in the central box of the outcome screen as in the No Delay. As in the original delay conditions, we still kept this information in the bottom right box of the screen to illustrate how the next week’s payoff is changed. That is, the additional costs per person are shown twice in the new Delay treatment. By doing so, we make the externality even more salient compared with the No Delay. Yet, we still observe a statistically significant lower support in the new Delay condition than the No Delay. This shows that the negative delay effect is not due to salience difference in the external costs. We report the details of this additional experiment and its results in Appendix B.

12 In all the non-parametric tests, we calculate the average at group level and treat each group as an independent observation. The non-parametric test we use is the Mann-Whitney test unless specified.
No Delay (33.33% vs. 78.95%, 32.81% vs. 78.95%, p < 0.01). These results show that the majority of people would support the tax if the external cost occurred immediately, but the delay of the external cost would significantly reduce tax support.13 Making taxation the default option does not seem to reduce the resistance to taxation when the benefit of taxation is delayed.

It is interesting that tax support increases in the No Delay (59.21% first ballot vs. 78.95% second ballot, p < 0.01) but not in the Delay and Remove (29.15% vs. 33.33%; 26.55% vs. 32.81%, p > 0.10). To provide more detailed information on how individuals’ voting behavior switches between the two ballots, Fig. 4 reports the distribution of all possible combinations of voting behavior in the two ballots by treatment. “YesNo” is the percent of subjects in each treatment who switched from voting yes the first time to voting no the second time; “NoYes” is the percent of subjects who switched from voting no the first time to voting yes the second time; “YesYes” is the percent of subjects who voted yes both times; and “NoNo” that of subjects who voted no both times.

Fig. 4 suggests that, compared with the two delay conditions, those who voted no the first time are more likely to switch to yes in the second ballot in the No Delay. On the other hand, those who voted yes the first time are more likely to continue to vote yes in the second ballot in the No Delay. To provide statistical evidence for these treatment differences, we calculate for each group in each treatment the probability of switching to yes in the second ballot as the ratio of the total number of “NoYes” to the total number of no votes in the first ballot. We find a statistically significant difference in the probability of switching from no to yes between the No Delay and Delay (63.7% vs. 21.6%, p < 0.01) and between the No Delay and Remove (63.7% vs. 19.3%, p < 0.01). We cannot reject the null hypothesis that subjects are equally likely to switch from no to yes in the Delay and Remove (p = 0.684).

Similarly, among those who voted yes the first time, we compare the probability of buyers to switch from yes to no. This probability is much higher in the Delay and Remove than in the No Delay treatment, although only the difference between Delay and No Delay is significant. (4.9% No Delay vs. 30.4% Delay, p = 0.039; 4.9% No Delay vs. 13.6% Remove, p = 0.274).

These results suggest that, compared with delayed benefits, when the benefits are immediate, people are not only more likely to initially vote for taxation, but also more likely to switch to support taxation if they did not at the beginning. Moreover, in the two delay conditions, even the minorities who initially supported taxation may shift to opposing the tax over time. One possible explanation is that experiencing the tax helps subjects to learn its beneficial effect, but only when the benefits are not delayed. Previous studies suggest that experiencing the tax can change tax attitudes (Tyran and Sausgruber, 2005). This literature, however, did not investigate the situation when the benefits are delayed. We conduct a regression analysis to explore the role of tax experience on individual voting decisions in our experiment.14 The dependent variable is whether the buyer voted yes in the second ballot. The explanatory variable is whether the tax is implemented in the buyer’s market between Periods 11 and 15 as a result of the first ballot.15 We allow different coefficients for each

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</tr>
<tr>
<td>No Delay First yes</td>
<td>2.368</td>
</tr>
<tr>
<td>Second yes</td>
<td>3.158</td>
</tr>
<tr>
<td>Delay First yes</td>
<td>1.166</td>
</tr>
<tr>
<td>Second yes</td>
<td>1.333</td>
</tr>
<tr>
<td>Remove First yes</td>
<td>1.062</td>
</tr>
<tr>
<td>Second yes</td>
<td>1.312</td>
</tr>
</tbody>
</table>

Note: the mean of group voting is the average vote (yes = 1; no = 0) in each group in each treatment. The mean of individual voting is the average vote (yes = 1; no = 0) across subjects in each treatment.

In all treatments, a few subjects did not pick up the second payment. The delay effect is robust when we include only those who picked up their second payment in the following week.

14 We report results from linear regressions where we cluster standard errors at group level to control for group heterogeneity, as this simplifies the interpretation of the coefficients. Probit regressions yield qualitatively the same results.

15 The regression models we analyze here potentially suffer from endogeneity problems given that tax experience to some extent is determined by a buyer’s own voting behavior. Although it is beyond the scope of this study, it would be valuable to conduct further studies to learn how delay interacts with the effect of tax experience on the tax attitude by exogenously manipulating tax experience (e.g., randomly assigned by a computer).
treatment. We conduct this regression analysis separately for those who voted yes the first time and those who voted no the first time. Results are shown in Table 2.

Regression (1) shows that among those who voted no the first time, tax experience has a positive effect on the probability of voting yes the second time but only in the No Delay treatment ($\beta_1$). On the other hand, regression (2) suggests that for those who voted yes the first time, tax experience actually has a significant negative effect in the two delay conditions ($\beta_2$ and $\beta_3$) and we do not observe such a negative effect in the No Delay ($\beta_1$ is positive but not significant). In both regressions, tax experience in No Delay is jointly significantly different from the other two treatments ($\beta_1$ is jointly significantly different from $\beta_2$ and $\beta_3$, F tests, $p < 0.05$). The negative tax experience effect in the two delay conditions can be explained by the fact that buyers in these two treatments see that current earnings are actually lower when the tax is introduced, although the amount of external costs deducted from next week’s earnings is smaller. If buyers focus on current earnings as we discuss in Section 4.3, they will switch to vote no in the second ballot.

These results provide insights to help understand how people’s attitudes towards taxation change over time, as observed in Fig. 4. First, when the benefit of taxation is delayed, people are less likely to experience the tax due to the lower initial support for taxation. Secondly, unlike the No Delay, experiencing the tax in a delay condition has no impact on those who voted no the first time, and it can have a negative impact on those who voted yes the first time.

Our finding of the positive tax experience effect in the No Delay is consistent with previous research showing that market experience eliminates market anomalies (Smith et al., 1988). It is also consistent with similar market experiments with externalities (Tyran and Sausgruber, 2005) showing that roughly two-thirds of subjects who experience the beneficial effects of a tax policy innovation vote to uphold the innovation in later periods. Interestingly, a new finding in our study is that when the benefit of taxation is delayed, experiencing taxation does not lead one to be more likely to support it, and might even have a negative impact on those who supported it initially.

Complexity, narrow bracketing and support for taxation

We have elaborated that for time discounting alone to predict any negative effect of delay on support for taxation in our setup, subjects should display a very high one-week discount rate ($r \geq 100\%$). We hypothesize, however, that even with a low discount rate, we will still observe the negative delay effect due to complexity and narrow bracketing. To shed light on the role of complexity, we first examine subjects’ one-week discount rate elicited in the survey (see Section 3.4). In total, 22 subjects (31\%) in the Delay and 11 (17\%) subjects in the Remove display a one-week discount rate of 1\%, which is the choice a rational exponential discounter is expected to select. Most subjects (66 out of 72 in the Delay and 53 out of 64 in the Remove) display a one-week discount rate in the range 1–20\% (on average, $r = 8.6\%$ in Delay and 9.5\% in Remove). Overall, the average one-week discount rate is far lower than 100\%: 10.4\% in the No Delay, 11\% in the Delay treatment and 12.6\% in the Remove.\(^{16}\) We also find the correlation coefficients between yes votes and discount rates are low and insignificant: first ballot: $0.019$, $p > 0.10$ (Delay); $-0.115$, $p > 0.10$ (Remove); second ballot: $-0.035$, $p > 0.10$ (Delay); $-0.141$, $p > 0.10$ (Remove). These results provide the first evidence that the low support for the tax in the two delay conditions does not seem to be explained by exponential discounting.\(^{17}\)

Next we examine whether the voting decisions are consistent with the hypothesis of complexity and narrow bracketing. Narrow bracketing means individuals only consider the consequences of their current decisions on their current payoff and fail to consider the consequences of their current choices on their future payoff (Read et al., 1999). In our experiment, narrow bracketing implies that buyers’ voting decisions are less likely to be affected by the historical level of the external

\(^{16}\) The discount rate pattern is consistent with similar studies measuring short-term discount rates using monetary rewards. For example, Reuben et al. (2010) found an average one-week discount rate of 5.46\% with 33\% of subjects switching at a one-week discount rate of 1\%.

\(^{17}\) We also checked whether the discount rate, $r$, played a role in the first voting decision in the regression analysis reported in Table 3 by adding $r$ as explanatory variable. We found again $r$ was not significant in any treatment. These regression results are reported in Appendix E.
costs in the two delay conditions compared to the No Delay. Thus, we compare the role of the average external costs during the first 10 periods in buyers’ voting decisions between the Delay and No Delay conditions. We conduct a regression analysis of the first voting decision. The dependent variable is whether the buyer voted yes in the first ballot (Fstyes). The explanatory variables are the average external costs per person in the first 10 periods and we allow different coefficients for each treatment (NDExtc10, DExtc10 and RExtc10). The regression results are shown in column (1) of Table 3. The coefficient of NDExtc10 is significantly lower than that for males (p < 0.02). In contrast, the coefficients of DExtc10 and RExtc10 are only marginally significant (p = 0.06). Both are significantly lower than the coefficient of NDExtc10 (F tests, p < 0.01). This suggests that, compared with the NoDelay treatment, the correlation between support for taxation and the level of externalities is significantly lower when the externalities only affect the future. This result is consistent with narrow bracketing.

Previous research suggests that narrow bracketing is more likely among females than males (Rabin and Weizsäcker, 2009). If narrow bracketing contributes to the delay effect, the gender differences in narrow bracketing would predict that females are less likely to consider the external cost and to vote for the tax than males when the externality is delayed. Our data are consistent with this prediction. While there is no gender difference in the supporting rate in the No Delay treatment, females seem to be less likely to support the tax in the Delay and Remove treatments (NoDelay: 57% vs. 61%, Delay: 51% vs. 57%, Remove: 19% vs. 33%, p = 0.07). Both are significantly lower than the coefficient of NDExtc10 (F tests, p < 0.01). This suggests that, compared with the NoDelay treatment, the correlation between support for taxation and the level of externalities is significantly lower when the externalities only affect the future. This result is consistent with narrow bracketing.

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We also analyze the participants’ answer to the survey questions to examine whether buyers indeed suggest complexity in the voting decisions. We performed content analysis (Krippendorff, 2004) on the messages written by the subjects at the end of the experiment as answers to a set of questions related to their voting behavior. Specifically, subjects were asked to answer the following questions: (1) “How did you decide to vote in favor or against the tax?”; (2) “Was your second vote different from your first vote during the experiment?” (answer: Yes/No); (3) “If Yes, why did you change your mind?” We recruited 12 evaluators from the same subject pool to participate in the content analysis of answers to questions (1) and (3). To reduce the amount of work for each evaluator so that he/she paid sufficient attention to the task in the one-hour session, for each treatment, we randomly divided all the messages into two sets, each of which were evaluated by two different evaluators. There were six sets of messages (two for each of the three treatments). Before receiving any messages, evaluators were given detailed written instructions to become acquainted with the rules of the experiment from which the messages were generated. Each evaluator was instructed to evaluate every message and was told she would earn $15 (including a $5 show-up fee) for doing so. Following Houser and Xiao (2011), evaluators also knew that at the end of the session, two messages would be randomly chosen to test whether the code matched that of the other evaluator who received the same set of messages. If they matched, the evaluators would receive another $2 for those two messages. (See Appendix D.1 for instructions for the content analysis.)

### Table 2
Voting behavior in the second ballot and previous tax experience.

<table>
<thead>
<tr>
<th>Dependent variable: Sndyesi, 1 if buyer i voted Yes in the second ballot</th>
<th>Coef. (s.e)</th>
<th>Coef. (s.e)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>(1)</strong> Includes only those who voted No in the first ballot</td>
<td><strong>(2)</strong> Includes only those who voted Yes in the first ballot</td>
<td></td>
</tr>
<tr>
<td>( \beta_1: ) NDtaxexperience</td>
<td>0.521***</td>
<td>0.072</td>
</tr>
<tr>
<td>(0.117)</td>
<td>(0.087)</td>
<td></td>
</tr>
<tr>
<td>( \beta_2: ) Dtaxexperience</td>
<td>0.196</td>
<td>-0.299*</td>
</tr>
<tr>
<td>(0.214)</td>
<td>(0.159)</td>
<td></td>
</tr>
<tr>
<td>( \beta_3: ) Rtaxexperience</td>
<td>-0.012</td>
<td>-0.282*</td>
</tr>
<tr>
<td>(0.169)</td>
<td>(0.145)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>0.179***</td>
<td>0.882***</td>
</tr>
<tr>
<td>(0.032)</td>
<td>(0.081)</td>
<td></td>
</tr>
<tr>
<td>nobs</td>
<td>129</td>
<td>83</td>
</tr>
</tbody>
</table>

Note: Both are linear regressions with clustered standard errors at group level. NDtaxexperience = 1 if a buyer i in the No Delay treatment traded with tax between period 11 and 15; =0, otherwise. Dtaxexperience = 1 if a buyer i in the Delay treatment traded with tax between period 11 and 15; =0, otherwise. Rtaxexperience = 1 if a buyer i in the Remove treatment traded with tax between period 11 and 15; =0, otherwise.

**Statistical significance at 5% level, respectively.**

*Statistical significance at 10% level, respectively.

***Statistical significance at 1% level, respectively.

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18 Similar to the regression analysis reported in Table 2, we report results from linear regressions with clustered standard errors at group level. Probit regressions produce qualitatively the same results.
The evaluators coded the messages from the first question into five categories and the messages from the second question into six categories (see Appendix D.1). The inter-rater reliability is satisfactory, with the Cohen’s $k = 0.70$ for each pair of messages set in each treatment.

In each treatment, compared with those who voted yes the first time, we find that more subjects who voted no the first time found it difficult to understand the functioning of the new institution. In particular, in the No Delay treatment, 30.4% of those who voted no and only 10.3% of those who voted yes the first time indicated difficulty ($Z$-test, $p = 0.04$).19 These percentages are 29.3% vs. 10.6% in the Delay ($Z$-test, $p = 0.11$) and 29.3% vs. 7.1% in the Remove treatment ($Z$-test, $p = 0.09$). In the No Delay treatment, 7 out of 62 messages are from buyers who voted no and indicated complexity as one reason for their voting decisions. This proportion is higher in the Delay and Remove treatments: 12 out of 60 in Delay and 12 out of 55 in Remove. However, the difference is only marginally significant ($p < 0.10$). The details are reported in Appendix D.2.

### Removing complexity to promote support for taxation

The data reported above suggest that complexity and narrow bracketing play an important role in the negative delay effect on support for taxation. This finding indicates that one simple way to promote support for taxation is to provide market participants with explicit information on the intertemporal consequences underlying taxation.20 We conducted a new treatment (Delay_Transparency) to test the effectiveness of this solution on promoting tax support. The success of this solution strengthens the evidence for the role of complexity and narrow bracketing.

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19 As we detailed in Appendix D.2, we compare only the messages that coders agree on in at least one category. It turns out no message has more than one category that is agreed on by both coders. The same is true for Delay and the Remove.

20 A recent paper (Hilgers and Wibral, 2014) shows that choice bracketing is malleable and that exogenous feedback on behavior from institutions can lead individuals to shift from narrow to broad bracketing, i.e. subjects who previously narrow bracketed can learn to bracket broadly after receiving useful information.

### Table 3

Voting behavior in the first ballot and external costs.

<table>
<thead>
<tr>
<th></th>
<th>(1) Coef. (s.e.)</th>
<th>(2) Coef. (s.e.)</th>
<th>(3) Coef. (s.e.)</th>
<th>(4) Coef. (s.e.)</th>
</tr>
</thead>
<tbody>
<tr>
<td>NDExtc10,</td>
<td>0.010***</td>
<td>0.008**</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.004)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>DExtc10,</td>
<td>0.008*</td>
<td>0.006</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.004)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>RExtc10,</td>
<td>0.008*</td>
<td>0.006</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.004)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>DTExtc10,</td>
<td>0.008**</td>
<td>0.008**</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.004)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>mNDExtc10,</td>
<td>0.010**</td>
<td>0.008**</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.004)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>fNDExtc10,</td>
<td>0.009**</td>
<td>0.008**</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.004)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>mDExtc10,</td>
<td>0.008**</td>
<td>0.007*</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.004)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>fDExtc10,</td>
<td>0.007*</td>
<td>0.005</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.004)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>mRExtc10,</td>
<td>0.008*</td>
<td>0.006</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.004)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>fRExtc10,</td>
<td>0.007*</td>
<td>0.005</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.004)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>mDTExtc10,</td>
<td>0.008** (0.004)</td>
<td>0.008** (0.004)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>fDTExtc10,</td>
<td>0.008**</td>
<td>0.008**</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.004)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>−1.106</td>
<td>−1.018</td>
<td>−0.786</td>
<td>−0.706</td>
</tr>
<tr>
<td></td>
<td>(0.730)</td>
<td>(0.693)</td>
<td>(0.645)</td>
<td>(0.618)</td>
</tr>
<tr>
<td># obs</td>
<td>212</td>
<td>212</td>
<td>272</td>
<td>272</td>
</tr>
</tbody>
</table>

Note: All are linear regressions with clustered standard errors at group level.

***Statistical significance at 1%, respectively.

**Statistical significance at 5%, respectively.

*Statistical significance at 10%, respectively.

---

19 As we detailed in Appendix D.2, we compare only the messages that coders agree on in at least one category. It turns out no message has more than one category that is agreed on by both coders. The same is true for Delay and the Remove.

20 A recent paper (Hilgers and Wibral, 2014) shows that choice bracketing is malleable and that exogenous feedback on behavior from institutions can lead individuals to shift from narrow to broad bracketing, i.e. subjects who previously narrow bracketed can learn to bracket broadly after receiving useful information.
The only difference between this new treatment and the Delay treatment is that in the voting instructions for the Delay treatment, we added the following sentences after the examples illustrating how the payoffs are calculated when the tax is accepted and when the tax is rejected:

“These 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”.

Thus, the voting instructions in the Delay_Transparency provide buyers with explicit information on the intertemporal tradeoffs they are making when deciding whether to vote in favor of the tax.

We implemented the treatment in the same way as the Delay.21 We recruited 60 participants (15 groups of 4 buyers) from the same subject pool. After 10 trading periods, buyers were asked to vote for introducing the tax. The voting outcome was implemented for the following five periods. A second ballot was introduced at the beginning of Period 16.

We find strong evidence that making intertemporal tradeoffs transparent significantly promotes support for taxation. In both the first and second ballots, the average number of yes votes per group in the Delay_Transparency is significantly higher than in the Delay. It is as high as in the No Delay (first ballot: 2.533 Delay_Transparency vs 1.166 Delay, p < 0.01; 2.533 Delay_Transparency vs 2.368 No Delay, p = 0.912; second ballot: 2.733 Delay_Transparency vs 1.333 Delay, p < 0.01; 2.733 Delay_Transparency vs 3.158 No Delay, p = 0.082).

We reported in Section 4 that, when the benefits of taxation are delayed, people are not only less likely to vote for taxation, but also less likely to switch to support taxation when they did not at the beginning. We are interested in whether removing complexity helps voters change their negative attitude over time in this new treatment. We calculate the values of “YesNo”, “NoYes”, “NoYes” and “YesYes” as the other three treatments reported in Fig. 4. Again, for each group we calculate the probability of switching yes in the second ballot as the ratio of the total number of “NoYes” to the total number of no votes in the first ballot. We find a statistically significant difference in the probability of switching from no to yes between the Delay_Transparency and Delay (41.0% vs. 21.5%, p = 0.05). We cannot reject the null hypothesis that subjects are equally likely to switch from no to yes in Delay_Transparency and No Delay (41.0% vs. 63.7%, p = 0.13).

Similarly, among those who voted yes the first time, we calculate for each group the probability of buyers who will switch from yes to no. This probability in the Delay_Transparency is in between the No Delay and Delay, although it is not significantly different from either of the two treatments (14.4% Delay_Transparency vs. 4.9% No Delay and 30.4% Delay, p > 0.10).

We are also interested in whether providing explicit information about the intertemporal tradeoffs leads people to be more likely to consider the externality when they decide how to vote. We thus include the data from Delay_Transparency (explanatory variable DTExtc10) in the regression in Table 3 (column 3). We find that the coefficient of DTExtc10 is significantly positive. The coefficient of DTExtc10 is not different from that of NDExtc10 (F test, p = 0.84) but significantly different from DExtc10 and RExtc10 (F test, p < 0.01). Interestingly, the gender differences we observed in the two delay conditions disappear in the Delay_Transparency. Females and males are equally likely to support the tax (64% vs. 63%, Z-test, p = 0.90). When separating females (f) and males (m) in the regression reported in Table 3 column (4), we find no gender difference in the coefficients of DTExtc10 (F test, p > 0.90). To summarize, data from the Delay_Transparency suggest that, when provided with clear information on the intertemporal tradeoffs, people not only become more likely to vote for taxation, but also more likely to switch to support taxation when they did not at the beginning. We also find evidence that resolving complexity prevents those who initially supported taxation from changing to vote against the tax over time. Moreover, with the help of information, subjects, especially females, seem to be more likely to take into account the future external costs when deciding whether to support the tax.

Conclusions

In this paper, we provide strong experimental evidence of a negative relationship between time delay of negative externalities and support for taxation. More specifically, we show that when the negative external effects of consumption are delayed, people are less willing to accept the introduction of Pigouvian taxes as incentives to change consumption behavior. The relationship between support for taxation and the temporal structure of the costs and benefits of taxation is robust even when we frame taxation as the default option.

We find that the majority of those who vote against a tax switch their view to supporting the tax after having experienced the tax institution. Such a switch does not occur, however, when there is a delay in the negative externality. This suggests that the introduction of trial runs to experience the working and impact of a new tax policy might boost

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21 The payment pickup rate in this treatment is about 92%, which is lower than the 97% pickup rate in the No Delay and higher than the 83% pickup rate in the Delay although neither of the differences is significant (p > 0.10). Yet, as reported below, we observe a significantly higher support rate in this new treatment than in the Delay treatment and no significant differences in voting behavior between the No Delay and Delay_Transparency. This result offers additional evidence that the delay effect discovered in this experiment cannot be attributed to the (insignificant) differences in the pickup rates.
acceptability when the benefits of the new policy are immediate, but not when they are delayed. It would be interesting to conduct further empirical studies to test whether trial runs, which have been shown to increase taxes’ acceptability both in the lab (Cherry et al., 2014) and in the real world (Schuitema et al., 2010), also work when the benefits of taxation are delayed.

One practical implication of our findings is that taxes aimed at reducing future externalities should be initially set at a rate lower than the optimal one implied by static analysis, and then increased over time to the target rate. Policy strategies framing the future external costs as more immediate and salient could also be useful in decreasing distaste for taxation. For example, recent advances in the neurosciences suggest that episodic tags presented during a delay discounting procedure reduce impulsive choice through an induction of episodic imagery and support the dynamic adjustments necessary to make choices that maximize future payoffs. In a set of intertemporal choice studies, Peters and Büchel (2010) found that increasing the tangibility of the future significantly reduced discount rates. They accomplished this by accompanying intertemporal choices with a reference to an event the subjects had planned for a future date.

Equally importantly, we demonstrate that exponential time discounting alone is not sufficient to explain the delay effect on the attitude towards tax. When asked directly to make an intertemporal choice, people do not display the high time discount rate required by theory to explain their voting decisions. Indeed, answers to the survey questions suggest that distaste for taxation is correlated with the perceived complexity of the environment. The intertemporal structure of the decision environment might increase complexity and lead people to narrow bracket, that is, to consider the consequences of their current choices only on their current payoffs, but not on the future ones. This is consistent with previous literature showing that environments in which current choices influence future constraints or utilities are computationally difficult and the resulting bounded rationality is an important explanation of seemingly irrational behaviors like under-saving or over-consumption (Brown et al., 2009).

We show that providing explicit information about the intertemporal tradeoffs implied by taxation almost completely eliminates the delay effect. While females are less likely to support the tax than males when the externalities are delayed, we observe no gender differences when participants are provided explicit tradeoff information. These findings offer important insights for policy makers. To improve support for Pigouvian taxation, we should consider how to provide credible and easy-to-understand information about the intertemporal tradeoffs under the tax. Such information may be particularly valuable for females. Manipulating the salience of the external costs could also play a role. In our experiment the salience of the negative externality was symmetric across treatments. In particular, buyers could see both immediate and delayed payoffs in the outcome screen at the end of each trading period. In naturally occurring environments, the delay effect may be even stronger if people cannot readily understand how any externality will influence their future welfare.

Acknowledgments

This research was supported by a Marie Curie International Outgoing Fellowship (PIOF_GA_298094) and Australian Research Council Discovery Project (DP160102743). We thank Leonardo Boncini, Todd Cherry, Daniel Houser, George Loewenstein, Luigi Luini, John Miller, Rupert Sausgruber, Matthias Weber, Alberto Zanardi and participants in the Behavioral and Experimental Economics Workshop, Florence 2013; the Economic Science Association World Meetings, Zurich 2013; the 54th Annual Meeting of the Italian Economic Association, Bologna 2013; the conference on Taxation, Social Norms and Compliance, Nuremberg 2014; Workshop on Behavioral Public Economics (Vienna) and in seminars at Carnegie Mellon University, Milano Bicocca University, the University of Siena, NYU, Nuffield College, University of East Anglia, University of Birmingham, City University of Hong Kong, Chinese University of Hong Kong, Monash University and Maastricht University for valuable suggestions. We gratefully acknowledge Rupert Sausgruber and Jean-Robert Tyran for sharing their Z-Tree code used in Sausgruber and Tyran (2005). We also thank David Hagmann for excellent research assistance.

Appendix A. Instructions

Instructions on the auction

(All the three treatments)

• General
Thank you for coming! You've earned $5 for participating, and the instructions explain how you can make decisions and earn more money which will be paid to you in cash.

This is an experiment in the economics of market decision making. In this experiment we are going to simulate a market in which each participant will be a buyer in a sequence of trading periods.

There should be no talking at any time during this experiment. If you have a question, please raise your hand, and an experimenter will assist you.
During the experiment your earnings will be calculated in experimental points. Experimental points will be converted in Dollars at the following exchange rate:

200 experimental points = 1$

At the end of today's experiment you will receive, in cash, the earnings you make today. In addition, you will receive a payment certificate to pick up your $5 participation bonus and an additional cash payment of $18 the same day next week.

For example, if today is Monday, you will receive the $5 participation bonus and the additional $18 cash payment next Monday. To pick up these amounts, you need to come back to the same lab between 3:30 and 4:00 pm the same day next week (if you cannot make it at this time please send an email to [experimenter's email address here] to schedule another time on the same day or you can send someone else to pick up your cash payment on the same day). You do not need to participate in any decision task next week to receive the additional $18 payment.

(Delay and Remove only)

However, as we describe below, you may lose some of this $18 depending on the decisions you and the other 3 buyers in your market make today. Therefore, the final amount of the additional cash payment you will pick up next week will depend on the decisions you and the other 3 buyers in your market make in today's experiment.

(All the three treatments)

In today's experiment, you will first participate in two practice trading periods followed by a number of paid trading periods. In the practice trading periods you do not earn money, but you should take these periods seriously since you will gain valuable experience for the paid trading periods.

Specific instructions to buyers

In this experiment each participant is a buyer. Each buyer is randomly assigned to a group of 4 buyers – a market – and remains in the same market with the same buyers throughout the experiment. What is happening in other markets is irrelevant for your own market and hence for your own earnings. During each trading period each buyer can buy units (up to 3 units) of a hypothetical consumption good from an automated (computerized) seller.

Resale value of a unit. At the beginning of each trading period, you will be given three separate resale values for each of the three units of the good you can purchase. These are your privately known resale values. You can think of the resale value of a unit as the potential earnings you can make out of that unit. Your resale values will remain the same in each period during the experiment.

Bid. As a buyer, you can submit a “bid” to buy a unit from the seller during a trading period. A “bid” is the amount you are willing to pay for that unit of the good. You must submit one “bid” for each of the three units. (If you do not want to purchase a unit, you may simply submit a bid “0.”) Your bids have to follow the following two rules: (1) “Trade at no loss”: your bid for each unit cannot be above your resale value for that unit; (2) Your bid for the third (second) unit cannot be above your bid for the second (first) unit.

How the market works

At the beginning of each trading period each buyer submits bids for each unit offered in the market. At the end of each trading period, all submitted bids are collected and ranked from high to low. If two or more bids are equal, ranks will be randomly assigned by the computer.

1. How the Market Price is determined

The automated seller has a production cost unknown to all buyers. The production cost does not change during the experiment. The seller never trades at a loss, therefore it will not accept bids below its production cost. The seller will accept, among all bids from all buyers in the market, the lowest bid above or equal to the production cost. This will be the per-unit Market Price. Bids that are below the production cost will be rejected and buyers who have submitted those bids won’t buy any units (i.e. buyers will neither pay for those units they placed a bid nor gain any resale value from those units).

The market price can be different in each period because it depends on the bids that are submitted in each period.

2. How the Market Quantity is determined

Buyers will purchase a unit when their bid is greater than or equal to the market price. The Market Quantity is the total number of units purchased by the 4 buyers in one market in one period at the market price.

Example. Suppose, in one market and in one trading period, the automated seller’s production cost is 70. And suppose the automated seller collects the following bids from the 4 buyers.

<table>
<thead>
<tr>
<th>Bid Unit 1</th>
<th>Buyer 1</th>
<th>Buyer 2</th>
<th>Buyer 3</th>
<th>Buyer 4</th>
</tr>
</thead>
<tbody>
<tr>
<td>80</td>
<td>0</td>
<td>90</td>
<td>94</td>
<td>85</td>
</tr>
<tr>
<td>85</td>
<td>80</td>
<td>90</td>
<td>40</td>
<td>85</td>
</tr>
<tr>
<td>135</td>
<td>135</td>
<td>140</td>
<td>145</td>
<td>140</td>
</tr>
</tbody>
</table>

The bids are ranked from high to low as follows: 145, 140, 135, 135, 94, 90, 85, 85, 80, 80, 40, 0. In this case, the Market Price is 80 (the lowest bid above or equal to the production cost of 70). All 10, and only the 10 units for which the bids were
equal to or above the market price of 80 will be purchased by the buyers who submitted the corresponding bids. These 10 sold units are bolded in the table. Each of these 10 units will be exchanged at 80. The market quantity in this case is 10. The number of sold units is determined by the number of submitted bids above or equal to the market price. Units for which the submitted bids are below the market price will not be sold.

**Please note:** The information on values and production costs of a unit is private. Buyers do not know the bids of other buyers, nor do they know the per-unit production cost for the seller.

(No Delay treatment)

3. **Additional Costs from Trading**

Each unit traded in the market (i.e. each unit sold) causes an additional cost of 60 points that will be equally split by the 4 buyers in the market. This means that each of the 4 buyers in the market has to pay an additional cost of $60/4 = 15$ points.

**Note** that you will bear a share of the additional costs even if you do not buy any units yourself.

Using the example above where the market quantity is 10 units, in this case, each buyer incurs an additional cost of $(60/4) * 10 = 150$ points $= $0.75.

4. **How your earnings today in each trading period are calculated**

Your Final earnings in one trading period = Gross earnings in the trading period – Additional Costs per person in the trading period, where

Gross earnings in one trading period = (Resale value – Market price) of each unit purchased

In the example above Buyer 4 buys two units. Her resale value for Unit 1 is 200, her resale value for Unit 2 is 140 and her resale value for Unit 3 is 100. The market price is 80. Her Gross earnings in this period are $= 200$ (resale value of Unit 1) + $140$ (resale value of Unit 2) $- 2 * 80$ (market price) $= 340 - 160 = 180$.

Since the market quantity is 10, the additional costs per person are $(60/4) * 10 = 150$. Her Final earnings in this period are $= 180$ (Gross earnings) – $150$ (Additional costs per person) $= 30$.

As you can see, in this case, even though Buyer 4’s resale value for Unit 3 is 100, which is higher than the market price 80, Buyer 4 did not purchase the unit because her bid for Unit 3 (40) is lower than the market price (80).

Your total Final earnings for today are the sum of your Final earnings in each trading period over all the paid trading periods.

5. **How your earnings next week are calculated**

Each participant will receive $18 next week. You do not need to participate in any decision task next week to receive the cash payment for the next week. You just need to pick it up in the lab between 3:30 and 4:00 pm on the same day next week.

(Delay and Remove treatments)

Additional costs from trading

Each unit traded in the market (i.e. each unit sold) causes an additional cost of 60 points that will be equally split by the 4 buyers in the market. This means that each of the 4 buyers in the market has to pay an additional cost of $60/4 = 15$ points.

**Note** that you will bear a share of the additional costs even if you do not buy any units yourself.

These additional costs will not affect your earnings today but will be deducted from the $18 cash payment you will receive next week.

Using the example above where the market quantity is 10 units, in this case, each buyer incurs an additional cost of $(60/4) * 10 = 150$ points $= $0.75. This $0.75 additional cost will be deducted from the $18 cash payment each buyer will receive next week.

How your earnings today in each trading period are calculated

Your Final earnings in one trading period = (Resale value – Market price) of each unit purchased

In the example above Buyer 4 buys two units. Her resale value for Unit 1 is 200, her resale value for Unit 2 is 140 and her resale value for Unit 3 is 100. The market price is 80. Her Final earnings in this period are $= 200$ (resale value of Unit 1) + $140$ (resale value of Unit 2) $- 2 * 80$ (market price) $= 340 - 160 = 180$.

As you can see, in this case, even though Buyer 4’s resale value for Unit 3 is 100, which is higher than the market price 80, Buyer 4 did not purchase the unit because her bid for Unit 3 (40) is lower than the market price (80).

Your total Final earnings for today are the sum of your Final earnings in each trading period over all the paid trading periods.

How your earnings next week are calculated

The additional costs imposed on each buyer in each period will be deducted from the $18 cash payment each buyer will receive next week.

In the example above, since the market quantity is 10 the additional costs per person are $(60/4) * 10 = 150$ points $= $0.75. Thus, the $18 cash payment to be received by Buyer 4 in the next week will be deducted by $0.75.
So, the final cash payment each buyer will receive next week = $18 – the sum of the Additional Cost per person in each period today.

Therefore, the final amount of the cash payment you will pick up next week will depend on the decisions you and the other 3 buyers in your market make today.

You do not need to participate in any decision task next week to receive the cash payment for the next week. You just need to pick it up in the lab between 3:30 and 4:00 pm on the same day next week.

Sample outcome screen

No delay treatment

<table>
<thead>
<tr>
<th>Period</th>
<th>Market Price</th>
<th>Market Quantity</th>
<th>Number of Units You purchased</th>
<th>Your Gross Earnings</th>
<th>Your Final Earnings</th>
</tr>
</thead>
<tbody>
<tr>
<td>Practice</td>
<td>40</td>
<td>12</td>
<td>3</td>
<td>220</td>
<td>40</td>
</tr>
<tr>
<td>Practice</td>
<td>40</td>
<td>12</td>
<td>3</td>
<td>220</td>
<td>40</td>
</tr>
<tr>
<td>1</td>
<td>40</td>
<td>12</td>
<td>3</td>
<td>220</td>
<td>40</td>
</tr>
</tbody>
</table>

Delay and remove treatments

<table>
<thead>
<tr>
<th>Your Balance Today as of now</th>
<th>Your Balance Next Week</th>
</tr>
</thead>
<tbody>
<tr>
<td>Previous Balance (1) 0</td>
<td>Balance 3500</td>
</tr>
<tr>
<td>Final Earnings of this Period (2) 40</td>
<td></td>
</tr>
</tbody>
</table>
Voting instructions

(No Delay and Delay treatments)

You and the other three participants in your market will now vote whether to introduce a tax of 60 points on each purchased unit of the good. If at least two out of four buyers in each market vote “Yes”, the tax is accepted and the following changes are implemented for the following trading periods: (1) a tax of 60 points will be deducted from your gross earnings for each unit you purchase; (2) at the end of each period, an equal share (one-fourth) of the total tax revenues collected from all units traded in your market will be returned to each buyer. All the other rules described in the instructions for the first 10 trading periods remain the same. In particular, seller’s production cost and each buyer’s resale values of each unit remain the same as the previous 10 periods.

Example

Suppose the tax of 60 points per unit is accepted as the outcome of the voting in your market. (Remove treatment)

A tax of 60 points on each purchased unit of the good is now introduced and the following changes are implemented for the following trading periods: (1) a tax of 60 points will be deducted from your final earnings for each unit you purchase; (2) at the end of each period, an equal share (one-fourth) of the total tax revenues collected from all units traded in your market will be returned to each buyer. All the other rules described in the instructions for the first 10 trading periods remain the same. In particular, seller’s production cost and each buyer’s resale values of each unit remain the same as the previous 10 periods.

Example

(All three treatments)

To illustrate how the tax would affect the outcome of the market and your earnings we use the same example from the instructions for the first 10 trading periods. In that example, when a buyer obtains one unit of the good, she will receive her resale value but now she will also have to pay the tax of 60 points. Consider Buyer 4. Buyer 4’s resale value for Unit 1 is 200, her resale value for Unit 2 is 140 and her resale value for Unit 3 is 100. Since Buyer 4 will also have to pay the tax of 60 points on each purchased unit, the maximum she could pay to the seller and still make a gain is \(\frac{200 - 60}{40} = 140\) for Unit 1, \(\frac{140 - 60}{40} = 100\) for Unit 2 and \(\frac{100 - 60}{40} = 100\) for Unit 3.
Consider again the example in which the seller collects the following bids from the 4 buyers. Let us assume each buyer bids 60 less than before for each unit due to the tax he/she has to pay for each purchased unit.

<table>
<thead>
<tr>
<th>Bid Unit</th>
<th>Buyer 1</th>
<th>Buyer 2</th>
<th>Buyer 3</th>
<th>Buyer 4</th>
</tr>
</thead>
<tbody>
<tr>
<td>Unit 1</td>
<td>(135 – 60) = 75</td>
<td>(135 – 60) = 75</td>
<td>(140 – 60) = 80</td>
<td>(145 – 60) = 85</td>
</tr>
<tr>
<td>Unit 2</td>
<td>(85 – 60) = 25</td>
<td>(90 – 60) = 30</td>
<td>(94 – 60) = 34</td>
<td>(85 – 60) = 25</td>
</tr>
<tr>
<td>Unit 3</td>
<td>(80 – 60) = 20</td>
<td>0</td>
<td>(80 – 60) = 20</td>
<td>0</td>
</tr>
</tbody>
</table>

The bids are ranked from high to low as follows: 85, 80, 75, 75, 34, 30, 25, 25, 20, 20, 0, 0. Again, suppose the automated seller’s production cost is 70. Thus, the Market price is 75 (that is, the lowest bid above or equals to 70). The Market quantity is 4. These 4 sold units are bolded in the table. Following the same rule as in the first 10 trading periods, each of these 4 units will be traded at the Market price 75.

(No Delay only)

To illustrate how a buyer’s earnings today are calculated, consider again the case of Buyer 4. Since the Market price is 75, Buyer 4 buys 1 unit. Since the Market quantity is now 4, the **Additional costs per person** are (60/4)*4 = 60. Buyer 4’s gross earnings in this period are = 200 (resale value of unit 1) – 60 (tax) – 75 (market price) = 65. Since 4 units are sold, the total tax revenues in this period are 4*60 = 240. One fourth of the total tax revenues, 240/4 = 60 points will be returned to buyer 4.

Buyer 4’s **Final earnings** in this period = 65 (Gross earnings) – 60 (Additional costs per person) + 60 (returned tax revenues) = 65.

Suppose the tax proposal is rejected. Trading will continue as before the vote and no changes will apply. Thus, in the above example, the seller will only accept bids above or equal to the production cost 70. The Market price is therefore 80. The Market quantity is 10. The additional costs per person are (60/4)*10 = 150 points. Buyer 4 buys two units. Her final earnings for that period are 30.

(Delay and Remove only)

To illustrate how a buyer’s earnings today are calculated, consider again the case of Buyer 4. Since the Market price is 75, Buyer 4 buys 1 unit. Since 4 units are sold, the total tax revenues in this period are 4*60 = 240. One fourth of the total tax revenues, 240/4 = 60 points will be returned to buyer 4.

Buyer 4’s **Final earnings** in this period are = 200 (resale value of unit 1) – 60 (tax) – 75 (market price) + 60 (returned tax revenues) = 125.

Since the Market quantity is now 4, in this period the **Additional costs per person** are (60/4)*4 = 60 points. These additional costs will not affect Buyer 4’s earnings today but will be deducted from the $18 cash payment Buyer 4 will receive next week.

So, the final **cash payment each buyer will receive next week = $18 – the sum of the Additional Cost per person in each period.**

(Delay only)

Suppose the tax proposal is rejected.

(No Delay only)

Before starting, you and the other three participants in your market can vote whether you are in favor of the introduction of the tax. The tax will be removed only if at least three out of four buyers in each market vote “No”. Suppose the tax of 60 points per unit is removed as the outcome of the voting in your market.

(Delay and Remove only)

Trading will continue as before the vote and no changes will apply. Thus, in the above example, the seller will only accept bids above or equal to the production cost 70. The Market price is therefore 80. The Market quantity is 10. Buyer 4 buys two units. Her final earnings for that period are 180.

Since the Market quantity is now 10, in this period the additional costs per person are (60/4)*10 = 150 points. Again, these additional costs will be deducted from the $18 cash payment Buyer 4 will receive next week.

(All three treatments)

All final earnings in the following periods will be calculated as illustrated above.

You will be informed about the outcome of the vote in your group on the screen before the trading continues. Nobody, however, will be informed about individual votes of other participants. In the ballot, all participants simultaneously vote Yes or No for the introduction of the tax. Abstentions or neutral votes are not possible. Voting is anonymous.

Before proceeding to the vote you will be asked to do an exercise to make sure you understand the instructions.

If you now have questions, please, raise your hand and wait until an experimenter will come by to answer your questions individually.

**Appendix B. Robustness check**

One anonymous reviewer objected that by not having the “additional costs per person” in the central box of the outcome screen in the Delay and Remove treatments, we may have made the additional costs less salient compared to the No Delay treatment. To check whether this could be the reason for the treatment difference we find, we conducted another Delay
treatment with 64 subjects where we made sure the "additional costs per person" was visible both in the central box of the outcome screen - as in the No Delay treatment – and also in the bottom right box of the screen so that the external costs in each period are equally salient across treatments. The No Delay outcome screen and the new Delay outcome screen are shown below.

No Delay outcome screen

New Delay outcome screen
By mentioning the additional costs per person twice – as we do in the No Delay outcome screen – we increase the salience of the externality in the new Delay treatment as compared to the No Delay. If we still find a significant difference between Delay and No Delay, we can conclude that the negative delay effect we find is not due to salience difference in the external costs.

Another concern that was raised pertains to the way we wrote the example when explaining how participants’ earnings would be calculated. Specifically, in the auction, instructions for subjects in the No Delay read: “Her **Final earnings** in this period = 180 (Gross earnings) – 150 (Additional costs per person) = 30.” But subjects in the Delay read: “Thus, the $18 cash payment to be received by Buyer 4 in the next week will be deducted by $0.75.” A less-than-fully-attentive subject could feel that the external cost was larger in the first case than the second, as 150 taken from 180 feels larger than 0.75 from 18. To avoid this potential “illlusion” contrast difference, in the new Delay treatment, we also edited the examples in the instructions. We made the edits in such a way that they would work against our hypothesis. In particular, when we explained how the externality would be deducted from the $18 payment, we no longer converted 150 points to $0.75 (see below 5. **How your earnings next week are calculated**). Therefore, if any previous result was due to lack of “attention,” subjects should at this point have realized that the external costs were very large: 150 (points) were deducted from 18 (dollars).

As in the previous instructions, in both the Delay and the No Delay conditions, we always converted the 150 points to $0.75 when we illustrated how the externality was calculated; thus, it was still clear to the subjects how much 150 points were worth in dollars (see below 3. **Additional Costs from Trading below**).

**Corresponding sections from the instructions are provided below. To help see the differences between the new instructions for the Delay treatment and the No Delay treatment, we marked the parts that are the same or different in the two treatments.** In both the new Delay treatment and the No Delay treatment, “150 points = $0.75” was only mentioned in “3. **Additional Costs from Trading.**” When we illustrated how the earnings were calculated, we always used 150 points in both treatments.

With all these changes that worked against our hypothesis, we still observed a statistically significantly lower support in the Delay condition than in the No Delay condition, both in the voting behavior in the first ballot (yes votes in the first ballot No Delay 59.21% vs new Delay 40.63%, MW test $p=0.026$) and in the second ballot (yes votes in the second ballot No Delay 78.95% vs new Delay 42.19%, MW test $p=0.0002$). In addition, as we observed in the previous Delay treatment, subjects did not learn to support the tax in the second ballot: the probability of switching from no the first time to yes the second time was significantly lower in the new Delay condition than in the No Delay condition: 4.69% vs 48.45% Mann-Whitney test, $p=0.001$. Thus, the new data suggests that the negative delay effect cannot be attributed to either the salience differences in the external costs or insufficient attention.

< Instructions >

**Additional costs from trading**

[No Delay and Delay treatments]

Each unit traded in the market (i.e. each unit sold) causes an additional cost of 60 points that will be equally split by the 4 buyers in the market. This means that each of the 4 buyers in the market has to pay an additional cost of 60/4 = 15 points. **Note that you will bear a share of the additional costs even if you do not buy any units yourself.**

Using the example above where the market quantity is 10 units, in this case, each buyer incurs an additional cost of \((60/4) \times 10 = 150 \text{ points} = 0.75\). **[Delay treatment only]**

These additional costs will not affect your earnings today but will be deducted from the payment you will receive next week.

**How your earnings today in each trading period are calculated**

[No Delay treatment]

Your Final earnings in one trading period = Gross earnings in the trading period – **Additional Costs** per person in the trading period, where

\[
\text{Gross earnings in the trading period} = (\text{Resale value} – \text{Market price}) \text{ of each unit purchased} \]

In the example above Buyer 4 buys two units. Her resale value for Unit 1 is 200, her resale value for Unit 2 is 140 and her resale value for Unit 3 is 100. The market price is 80. Her Gross earnings in this period = 200 (resale value of Unit 1) + 140 (resale value of Unit 2) – 2 \times 80 (market price) = 340 – 160 = 180

Since the market quantity is 10, the additional costs per person are \((60/4) \times 10 = 150\). Her **Final earnings** in this period = 180 (Gross earnings) – 150 (Additional costs per person) = 30. **[Delay treatment]**

Your Final earnings in one trading period = (Resale value – Market price) of each unit purchased.
In the example above Buyer 4 buys two units. Her resale value for Unit 1 is 200, her resale value for Unit 2 is 140 and her resale value for Unit 3 is 100. The market price is 80. Her Final earnings in this period = 200 (resale value of Unit 1) + 140 (resale value of Unit 2) − 2 * 80 (market price) = 340 − 160 = 180.

[No Delay and Delay treatments]
As you can see, in this case, even though Buyer 4’s resale value for Unit 3 is 100, which is higher than the market price 80, Buyer 4 did not purchase the unit because her bid for Unit 3 (40) is lower than the market price (80).

Your total Final earnings for today are the sum of your Final earnings in each trading period over all the paid trading periods.

How your earnings next week are calculated

[No Delay treatment]
Each participant will receive $18 next week. You do not need to participate in any decision task next week to receive the cash payment for the next week. You just need to pick it up in the lab on the same day next week.

[Delay treatment]
Each participant will receive $18 next week. However, the final amount of the cash payment you will pick up next week will depend on the decisions you and the other 3 buyers in your market make today.

In the example above, since the market quantity is 10, the additional costs per person are (60/4)*10 = 150 points. This additional cost will be deducted from Buyer 4’s cash payment for the next week.

So, the final payment each buyer will receive next week = $18—the Sum of the Additional Cost per person in each period today.

You do not need to participate in any decision task next week to receive the cash payment for the next week. You just need to pick it up in the lab on the same day next week.

Appendix C. Time discounting and voting for tax

No Delay treatment

In the No Delay treatment, the external cost of trading is deducted from the subjects’ earnings on the day of trading. With the immediate externality, each buyer’s pretax payoff in each trading period is

\[ \pi_i = \sum d_{ij} \pi_{ij} - \sum d_{ij} \text{MEC}_i \]  

where \( d_{ij} = 1 \) if the jth unit is traded in this period and \( d_{ij} = 0 \) otherwise. \( \pi_{ij} \) is measured as the difference between the resale value of the jth unit and the market price. In our setting, \( \text{MEC}_i = 15 \). As shown in Fig. 1, the marginal payoff or marginal benefit of each additional consumption unit is positive and decreasing. Thus, buyers have an incentive to trade all three units available in each period. Without tax and assuming buyers trade at market equilibrium, buyer i’s maximum payoff in each period is \( \pi^*_i = 40 \).

With taxation, an amount equal to the marginal external cost \( T = \text{MEC} = 60 \) is paid by each buyer on each unit traded and an equal share of the total tax revenues collected in each market in each period is returned to each buyer. The after-tax payoff becomes:

\[ \pi_{i,\text{tax}} = \sum d_{ij} \pi_{ij} - \sum d_{ij} \text{MEC}_i - \sum d_{ij} T + \frac{\sum_{j} \sum_{i} d_{ij} T}{n} \]

\[ = \sum d_{ij} \pi_{ij} - \sum d_{ij} T \]  

(C.1.b)

As shown in Fig. 1, the tax shifts the demand curve downwards, from \( D_0 \) to \( D_1 \). The marginal payoff is positive only for the first two units. As a consequence, the profit-maximizing strategy for each buyer is to purchase two units. In this case, assuming buyers trade at market equilibrium, buyer i’s maximum payoff in each period is \( \pi^*_{i,\text{tax}} = 70 \). So, subjects should vote for the tax if they are maximizing their payoff.

Delay and Remove treatments

When the externality of consumption at time \( t \) is delayed to time \( t + 1 \), each buyer’s pretax payoff in each trading period is:

\[ \pi_t = \sum d_{ij} \pi_{ij} - \beta \sum_{t=1}^{T-t} \gamma^r \sum_{i} \sum_{j} d_{ij} \text{MEC}_i \]

where \( \gamma = 1/(1+r) \) and \( r \) is the discount rate and \( 0 < \beta \leq 1 \). The value \( \beta = 1 \) produces the standard model of constant (exponential) discounting, and if \( 0 < \beta < 1 \) there is quasi-hyperbolic discounting. In the latter case, subjects at time \( t \) discount the payoff in \( t + 1 \) at a higher rate than the one used to discount, at time \( t + 1 \), the payoff at time \( t + 2 \).
As there are only two time periods with one week interval, we can rewrite (C.2.a):

\[ \pi_{it} = \sum_{j} d_{ijt} \pi_{ijt} - \beta \gamma \sum_{i} \sum_{j} d_{ijt} MEC_{i} \]  

(C.2.b)

Since \( 0 \leq \beta \gamma \leq 1 \), as in the No Delay treatment the marginal payoff or marginal benefit of each additional consumption unit is positive. Thus, buyers will trade all three units available in each period. Without tax and assuming buyers trade at market equilibrium, buyer \( i \)'s maximum payoff in each period is \( \pi_{it} = 220 - 180 \beta \gamma \).

With a tax \( T = MEC = 60 \) on each traded unit and returning the total tax revenues equally to each participant in the market, the after tax payoff is:

\[ \pi_{it, \text{tax}} = \frac{\sum_{j} d_{ijt} \pi_{ijt} - \beta \gamma \sum_{i} \sum_{j} d_{ijt} MEC_{i} - \sum_{j} d_{ijt} T + \sum_{j} \sum_{i} d_{ijt} T}{n} \]  

(C.2.c)

The marginal payoff is positive only for the first two units and thus each buyer will purchase two units when tax is imposed. Assuming buyers trade at market equilibrium, buyer \( i \)'s maximum payoff in each period is \( \pi_{it, \text{tax}}^* = 190 - 120 \beta \gamma \).

If subjects are profit maximizers they will vote for the tax when:

\[ \pi_{it, \text{tax}}^* \geq \pi_{it}^* \]  

(C.2.d)

Solving condition (C.2.d) for \( \beta \gamma \) when subjects trade at equilibrium under our parameter setup, we obtain \( \beta \gamma \geq 0.5 \). Thus, if a significant proportion of buyers satisfies \( \beta \gamma < 0.5 \), we should observe significantly less buyers voting for taxation in the two delay conditions compared to the No Delay treatment.

Appendix D. Content analysis of survey answers

Instructions

Thank you for coming! You’ve earned $5 for showing up on time, and the following instructions will explain your task in this session.

Your task

You will be given a list of messages. Your task is to evaluate whether each of the messages can be classified as expressing one of the following reasons: (you may assign more than one reasons to a message)

- I don’t like tax
- I want to make more money: today, next week, unclear
- Preference for Default
- Too difficult/confusing to understand the new rules
- Practice made me change my mind
- Other

The messages were written by participants in a market experiment. The experiment consisted of 3 stages and 20 periods in total.\(^{22}\)

- For the first 10 periods, participants decided whether and how many units of a hypothetical consumption good to purchase in a market. The first instructions’ set attached below explains how each participant made a decision in the first 10 periods.
- After 10 periods of trading, participants were asked to vote Yes or No for the introduction of a tax on the purchase of each unit of the good. If the majority voted for the introduction of the tax, participants would experience the tax for 5 trading periods. The second instructions’ set attached below explained to the participants how the tax would affect their earnings in each period.
- The participants were asked to vote a second time whether to introduce the tax at the beginning of period 16; this voting outcome was applied to the last 5 trading periods.

At the end of the experiment, each participant was asked to fill out a survey. The messages you will be asked to evaluate are answers to either the question: “How did you decide to vote in favor or against the tax?” or to the question “If your second vote was different from your first vote during the experiment, why did you change your mind?”

More instructions for coding each message: (you may assign more than one reasons to a message)

1) You should classify a message as “I don’t like tax”, if the message suggests a general dislike/disapproval/mistrust for tax.
2) You should classify a message as “I want to make more money”, if the message suggests that increasing the earnings is the motivation for the vote. For this category, you will need to further indicate whether the message writer wants to make more money today, next week, or whether the timing is unclear.

\(^{22}\) During the experiment, the subjects did not know the exact number of periods in each stage.
### Results

Tables D.2a and D.2b list the frequency of messages that are classified under each category. Table D.2a shows the distribution of classification of the messages for the first question: “How did you decide to vote in favor or against the tax?” Table D.2b shows the distribution of classification of the messages for the second question: “If your second vote was different from your first vote during the experiment, why did you change your mind?” In classifying each message, each coder could choose more than one category. For those messages classified under more than one category, when the two coders agreed on at least one of the categories, we considered that as an agreement and we picked the agreed upon one as the final classified category for the message. It turns out that for each message there is no more than one category agreed to by both coders. The listed classification outcomes in Tables D.2a and D.2b are only for the messages that coders reached an agreement upon. The last two columns in each table contain the total number of messages and the total number of messages with agreements in each treatment, respectively.

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23 In computing the Cohen’s inter-rater reliability statistic we merged the categories “Too difficult/confusing to understand the new rules” and “Other.” When coding a message in the “Other” category, coders were asked to add a comment. In most cases, the reason for coding a message under the “Other” category was that the message implied a lack of understanding of the question, so the two categories were merged. Also, since each rater could classify each message into more than one category, in cases in which more than two categories were indicated and there was at least one agreement, we considered the agreed upon category. Finally, five buyers did not answer question 1. We also exclude two messages: one wrote “N/A” and the other wrote “no”.

Appendix E

Table E1 below reports the regression results of the first voting decisions using the same specifications as reported in Table 3 but adding the one-week time discount rate \( r \). We allow different coefficients of \( r \) in different treatments. The regression results show that \( r \) is not significant in any treatment.

<table>
<thead>
<tr>
<th>Frequency (%)</th>
<th>Classification categories</th>
<th>Number of messages</th>
<th>Number of messages with agreement</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>I don’t like tax</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>I want to make more money</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Preference for Default</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Too difficult or confusing to understand the new rules/other</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Practice made me change my mind</td>
<td></td>
<td></td>
</tr>
<tr>
<td>“Yes” Voters</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No Delay</td>
<td>0(0%)</td>
<td>3</td>
<td>3</td>
</tr>
<tr>
<td>Delay</td>
<td>0(0%)</td>
<td>6</td>
<td>5</td>
</tr>
<tr>
<td>Remove</td>
<td>0(0%)</td>
<td>5</td>
<td>3</td>
</tr>
<tr>
<td>“No” Voters</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No Delay</td>
<td>0(0%)</td>
<td>18</td>
<td>13</td>
</tr>
<tr>
<td>Delay</td>
<td>0(0%)</td>
<td>10</td>
<td>10</td>
</tr>
<tr>
<td>Remove</td>
<td>0(0%)</td>
<td>11</td>
<td>9</td>
</tr>
</tbody>
</table>

Note: all are linear regression with clustered standard errors at group level.

Appendix E

Table E1 below reports the regression results of the first voting decisions using the same specifications as reported in Table 3 but adding the one-week time discount rate \( r \). We allow different coefficients of \( r \) in different treatments. The regression results show that \( r \) is not significant in any treatment.

References
