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Representative Policy-Makers?

A Behavioral Experiment with French Politicians^{*}

Roberto Brunetti[†] Matthieu Pourieux[‡]

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Abstract

This study leverages an online behavioral experiment to analyze *political representation*—whether politicians’ decisions align with citizens’ preferences over the same issue—and *behavioral representation*—whether politicians’ decisions align with citizens’ decisions within the same decision environment. We recruited 760 local politicians and 655 non-politicians in France to participate as policy-makers in a taxation-redistribution game. In the game, two policy-makers compete to choose a flat tax rate for a group of citizens, who are selected from the French general population and state their preferred tax rate. We exogenously manipulate (*i*) the information provided to policy-makers about citizens’ preferred tax rates and (*ii*) the degree of competition between policy-makers. Finally, we measure policy-makers’ beliefs regarding both citizens’ preferences and their competitor’s choice. We observe that policy-makers positively react to the information, but they often deviate from it, which can be mostly explained by their beliefs about both citizens’ preferences and their competitor’s choices. Varying the degree of political competition has no impact on these results. Finally, we find that politicians believe citizens want lower tax rates and are more confident in their beliefs than non-politicians. Once beliefs are accounted for, we observe little differences between the two groups within the game. Our findings suggest that policy-makers act as pro-social agents who implement citizens’ preferences based on their beliefs.

Keywords: Representation, Politicians’ Behavior, Online Experiment, Taxation-Redistribution

JEL codes: D31, P19, H24, H79, C90

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1 Introduction

Does politicians' behavior align with citizens' preferences? This issue is crucial in representative democracies and has consequently fueled a large number of studies. In a well-functioning representation system, politicians are incentivized by democratic institutions to implement policies supported by the majority of citizens. Such pattern has been largely documented, in particular in studies focusing on changes in policy outputs in reaction to changes in public opinion (see [Beyer and Hänni, 2018](#), for a review). However, as political enforcement is not guaranteed and institutions often fail, elected officials retain substantial discretionary power over policies, which they can use to reach their personal objectives. Many studies indeed highlight that politicians' behaviors deviate from citizens' preferences ([Gerber and Lewis, 2004](#); [Stadelmann et al., 2013](#); [Tausanovitch and Warshaw, 2014](#), among others). Relatedly, an increasing number of studies testify that politicians' personal characteristics matter to policy-making (see, *e.g.*, [Kuliomina, 2021](#), and references therein). In other words, politicians may not only be vote maximizers, but also utility maximizers ([Persson and Tabellini, 2002](#)). Overall, the debate about the extent of politicians' deviations from their constituents' preferences appears thus far from being settled.

One critical issue when seeking to advance this debate is that providing a causal analysis is particularly challenging due to omitted variables and reverse causality issues ([Wlezien and Soroka, 2016](#)). In addition, existing studies provide limited control over politicians' decision environment, which prevents a thorough analysis of politicians' decision-making process and underlying behavioral motives. To complement our understanding of democratic representation, this paper adopts a different approach from existing empirical studies based on an online economic experiment with 760 French local politicians and 655 French non-politicians. First, we design exogenous variations in the decision environment to gauge the importance of citizens' preferences in policy-makers' decisions. In other words, we test for *political representation*, that is, the alignment between politicians' decisions and citizens' preferences over the same issue. In particular, we observe policy-makers' behavior when confronted with an informational shock about citizens' preferences compared to a no-information benchmark while assessing their beliefs about such information. In addition, we analyze how their behavior change upon variations in the incentives to align with this information, by varying the level of political competition. Second, we compare decisions made by our politician sample with decisions made by a non-politician sample in the same decision environment. In other words, we test for *behavioral representation*, that is, the alignment between citizens' decisions and their representatives' decisions within the same decision environment. Assessing whether politicians' behavioral characteristics resemble that of the general population is crucial because they are most likely to have an incidence on policy-making.

There are several advantages to running an online economic experiment when investigating democratic representation. First, it is possible to tailor the decision environment to the researcher's needs. In particular, an experiment simplifies complex real-life multi-dimensional policy spaces into a uni-dimensional space. Likewise, it abstracts from the many political economy constraints (*e.g.*, lobbying, party affiliation, rent-seeking, etc.), which may confound the findings of observational studies. Second, experiments enable the measurement of variables that are otherwise unobservable. Importantly, politicians' beliefs

about citizens' preferences constitute one driver of politicians' decisions that have been identified early in the theoretical literature (*e.g.*, Kingdon, 1967; Calvert, 1985; Coughlin, 1992) but that remains poorly scrutinized in empirical works (Pereira, 2021; Liaqat, 2023). With an experiment, we can control the amount of information provided to participants and directly elicit their beliefs about such information straightforwardly.¹ Third, it simplifies the identification of the underlying decision mechanisms. By randomizing participants into treatments, we can disentangle possible explanations for observed behavior. In particular, when politicians are observed to abide by citizens' preferences in observational studies, it is often unclear whether this stems from political competition or politicians' pro-social motivations. The design of an experiment allows us to separate these explanations by manipulating the degree of political competition. Fourth, the decision environment in an economic experiment is highly replicable, so that it is possible to compare decisions from different groups of participants (*e.g.*, politicians and non-politicians). Fifth, there are monetary consequences for participants, mitigating the hypothetical bias that might be a specific concern in elite survey studies.

We recruited politicians and non-politicians to participate as policy-makers in an online taxation-redistribution experiment (Durante et al., 2014). Pairs of policy-makers compete to choose a tax rate which applies to the initial earning distribution of a 9-citizen group selected from the general French population. One part of the total tax proceeds is lost, while the remainder is equally redistributed among the group. This setting reproduces the equality-efficiency trade-off (Okun, 1975) in a taxation-redistribution framework (Meltzer and Richard, 1981). Finally, citizens are asked their taxation-redistribution preferences, an information which may be provided to policy-makers.

To answer our research question on *political representation*, we designed four experimental conditions generated from the combination of two treatments. The first treatment varies the information participants have about the median of citizens' preferred tax rate (henceforth: "median rate"). In one condition participants remain uninformed whereas in another they decide when given the information about each of the possible median rates (strategy method, Seltен, 1965). In all cases, we also elicit participants' beliefs about the actual median rate. This treatment allows us to compare a situation where policy-makers may decide based on the information about citizens' preferences with a situation where they rely only on their beliefs about such preferences and personal views about redistribution, as well as to evaluate the within-subject variation in redistribution decisions as a function of the provided information.

The second treatment varies the competition level between pairs of participants. In the *Competition* treatment, winning depends on which participant chooses the tax rate closest to the median rate. In the *Dictator* treatment, the winner is selected at random. In both cases, the winner obtains a monetary gain and the tax rate he chooses is applied to the 9-citizen group. Consequently, participants compete to obtain a payoff in both treatments, but this payoff is linked to the citizens' preferences only in the *Competition* treatment. Theoretical models in political economy assume that political candidates' self-interest (electoral concerns) steer them toward citizens' preferences (Mueller, 2003). Conversely, in the

¹Notably, the type of citizens' preference aggregation considered by politicians has been debated. For instance, it is unclear whether politicians focus on citizens' mean or median preferences (Mueller, 2003). Since we control the information participants receive, we can explicitly target a specific preference aggregation type, reducing interpretation issues.

absence of proper democratic institutions, the same self-interest may drive them away from citizens' preferences. Yet, the convergence toward citizens' preferences may alternatively originate from an intrinsic concern about the preferences of the people for whom the policy-maker decides. This second explanation is consistent with the conceptualization of politicians as socially-motivated agents whose mission is to fulfill the interests of the people (Miller and Stokes, 1963; Besley, 2005; Besley and Ghatak, 2018). By comparing a situation where there are no stakes for the policy-maker (*Dictator* treatment) to a situation where it is in the policy-maker's personal interest to respond to the information (*Competition* treatment), we are able to disentangle both explanations absent any confounding factors.

To answer our research question on *behavioral representation*, we compare politicians and non-politicians within the same experimental conditions, while controlling for a large set of individual characteristics. Investigating politicians' *behavioral representation* serves several purposes. First, it speaks to the generalizability of existing findings about individual-level behaviors to politician samples. Indeed, existing studies rely predominantly on convenience or representative non-politician samples. To what extent we may leverage insights from such studies to improve our understanding of policy decisions crucially hinges on the comparability of underlying populations. Second, it provides a way to better understand the mechanisms at play in *political representation*. By putting non-politicians in politicians' shoes, we are able to assess whether the response to treatments is general or specific, which bears very different implications. For instance, observing that politicians and non-politicians behave very differently in the same decision environment may hint that some characteristics of the political environment are important drivers of observed behavior (e.g., political selection, habits formation, political training, etc.). Third, given that our design involves some singular features (e.g., the treatments) and a very specific sub-group of the population (politicians), recruiting participants from the general population provides a benchmark allowing us to position our findings in the literature.

With respect to political representation, we highlight three main results. First, the preferences of citizens matter to both politicians and non-politicians. Indeed, observed tax choices are influenced by citizens' preferences, no matter whether the information about the median rate is provided or not. In the *Information* treatment, participants' choices are influenced by the information we provide: The Pearson's correlation coefficient between the chosen rate and the information level is 0.34. In the *No Information* treatment, participants' decisions are mainly correlated with their beliefs about the median rate.

Second, in the *Information* treatment, we observe substantial deviations from the information about the median rate. Participants' tax choices deviate from this information by 24.54 p.p. on average. This deviation is highest when the information is about a tax rate at the extremes, that is, a low tax rate (around 0%) and a high tax rate (around 100%). The deviation is minimum when the provided information is about a tax rate around 50%. As a consequence, even in a very stylized environment, we observe that policy-makers deviate from citizens' preferences. Furthermore, we find that participants' beliefs about the median rate and about the competitor's tax choice, which are not always aligned with the information provided, matter for observed deviations. This highlights the importance of participants' priors in explaining redistributive decisions.

Third, varying the degree of political competition bears little impact on the results. We observe that

participants in the *Dictator* treatment deviate more from the (expected) median rate than in the *Competition* treatment. Yet, this relationship is only marginally statistically significant in the *No Information* treatment and not statistically significant in the *Information* treatment. This suggests that in all cases participants hold an intrinsic preference for granting people what they prefer.

With respect to behavioral representation, we observe that politicians and non-politicians hold different views about citizens' preferences. Across all treatments, politicians believe that the median rate is lower than what non-politicians believe and they are more confident about this belief. This difference holds even when controlling for several socio-demographics and relates to tax choices: In the *No Information* treatment, politicians choose a lower tax rate than non-politicians (difference in means of 4.68 p.p.) but this behavioral difference is entirely captured by the difference in beliefs about the median rate. Similarly, in the *Information* treatment, where the importance of beliefs is less pronounced in the decision-making process, the behavioral difference between politicians and non-politicians is not statistically significant. Further, we do not observe that politicians significantly react in a different way to our treatments as compared to non-politicians. As a consequence, we find no evidence of a behavioral difference between politicians and non-politicians, once beliefs are accounted for.

Related literature This study complements different strands of literature. Primarily, it connects to the voluminous empirical literature on political representation, which yields mixed results regarding the alignment of policy decisions on citizens' preferences. This literature can be divided into two branches: representation in policy positions and representation in policy outputs ([Wlezien and Soroka, 2016](#)). Because we do not scrutinize actual policies, our study relates to the former. More specifically, our study relates to the literature on dyadic representation, that is, the relationship between individual constituencies (here a group of citizens) and individual representatives (here competing policy-makers). A daunting task in this literature is finding a good measure of what citizens want to compare with what politicians do while controlling for numerous confounding factors.

One initial strategy, especially in political science, relies on survey data to compare the public opinion with politicians' opinions on the same issues (see, *e.g.*, [Miller and Stokes, 1963](#); [Ågren et al., 2007](#)). More recently, a growing number of studies brought together surveys of citizens and surveys of politicians to study information provision in the spirit of field experiments (see below): they randomly expose or not politicians to survey-collected citizens' preferences and survey them about their future self-declared policy behavior. One main advantage of surveys is that any useful piece of information at the individual level can be obtained (*e.g.*, motivations, beliefs, etc.). Examples include [Sevenans \(2021\)](#), [Soontjens and Sevenans \(2022\)](#), [Chu and Recchia \(2022\)](#). However, such studies rely on stated preferences measurements (opinions) whereas our study involves revealed preferences (behavior) for both politicians and citizens. In addition to dealing with some methodological issues (such as hypothetical bias), this allows us to analyze the impact on politicians' behavior of a change in the incentive structure, which is at the core of political economy models where electoral concerns drive politicians' responsiveness to citizens' demands.

A second strategy relies on observed voting behavior to estimate each politician's ideal point on the

same uni-dimensional ideological scale (such as the Americans of Democratic Action scale). Studies then correlate politicians' ideology scores with proxies of citizens' preferences (*e.g.*, [Levitt, 1996](#); [Anscombe et al., 2001](#); [Clinton, 2006](#)), or directly compare such scores with citizens' ideal points obtained from extensive data on voting behavior ([Gerber and Lewis, 2004](#); [Bafumi and Herron, 2010](#); [Tausanovitch and Warshaw, 2013, 2014](#)). Comparing politicians' and citizens' ideal points provides the key advantage of identical scaling, hence avoiding the policy space's high-dimensional issue. Moreover, this strategy directly fits the underlying spatial voting models ([Black, 1948](#); [Downs, 1957](#)) and does not hinge on reduced-form results to formulate working hypotheses regarding policy alignment. However, ideal points require technical assumptions (such as that the policy spaces that are constructed for both citizens and politicians are sufficiently close) and may lack a natural interpretation (*i.e.*, it is difficult to translate an ideological score into a vote on a specific issue). Similar to the ideal points approach, our experimental design allows us to directly assess whether politicians deviate from citizens' preferences on a single issue and adequately quantify the deviations. However, decisions in the experiment have a straightforward interpretation in terms of taxation-redistribution

A third strategy uses referenda to obtain a snapshot of both citizens' preferences and politicians' behavior. Although some examples exist in other countries (*e.g.*, [Brunner et al., 2013](#)), the majority of studies benefit from the specific Swiss institutional setting, where citizens can demand a popular referendum on political decisions, allowing them to vote on the same laws with identical wording as politicians ([Stadelmann et al., 2012](#); [Portmann et al., 2012](#); [Stadelmann et al., 2013, 2014](#); [Portmann and Stadelmann, 2017](#)). These studies generally conclude that political representation is partial, which contradicts other studies using different empirical approaches that overall find higher levels of political representation. Yet, that all these results have been obtained using a specific setting calls for caution for external validity. Besides, that politicians and citizens (dis)agree on policy issues is not in itself proof of the (mis)alignment of politicians' behavior on citizens' preferences. For instance, it may reflect that politicians have biased beliefs about what citizens want, not that they lack the motivation to implement it. In our experiment, we provide direct evidence on the relationship between citizens' preferences and politicians' behavior in a context-free taxation-redistribution setting, while accounting for politicians' beliefs and controlling the incentive structure.

A fourth and more recent strategy resorts to field experiments with random assignment to study the causal impact of information provision about survey-collected policy preferences of citizens on politicians' daily activity. One seminal contribution is [Butler and Nickerson \(2011\)](#), which analyses how governors in New Mexico respond to new information about citizens' preferences about a policy issue for which the public opinion is very uncertain. They observe that politicians treated with the information vote more in line with it than untreated politicians. [Liaqat \(2023\)](#) uses the same method with local politicians in Pakistan while scrutinizing a broader range of policy issues. He observes that Pakistani politicians hold inaccurate beliefs about what citizens want and change their policy recommendations to party leaders accordingly once informed about it. Field experiments bring two major contributions to the literature: causal interpretation of findings and external validity. However, they are not well-suited to provide a refined understanding of the mechanism at play because the control about the experimental conditions

and the data at hand remain limited. On the contrary, our online economic experiment provides full control about the decision environment. This flexibility allows to expose participants to a strictly-defined no-information benchmark (that typically does not occur in naturally-occurring settings) and assess whether their behavior changes when they are not incentivized to consider the information (as opposed to political competition). In addition, we can fully evacuate any other confounding factors (such as political economy constraints). Consequently, it provides the opportunity to improve our understanding of politicians' decision-making process and underlying motivations. Moreover, our decision variable (tax rate) implies direct monetary consequences for participants, whereas the incentive structure in field experiments is less clear.

This paper also contributes to the literature relying on large-scale surveys and economic experiments to study the behavioral characteristics of politicians, such as their economic preferences, personality traits or cognitive biases (Fatas et al., 2007; Dietrich et al., 2012; LeVeck et al., 2014; Enemark et al., 2016; Sheffer et al., 2018; Heß et al., 2018; Sheffer and Loewen, 2019; Janezic and Gallego, 2020). Despite the recent rise in such studies, behavioral experiments involving real-world policy-makers remain scarce (Kertzer and Renshon, 2022). By comparing redistribution choices between a sample of politicians and a representative sample of non-politicians in an identical choice environment, this paper documents the specificity of politicians' characteristics in comparison to the general public (*behavioral representation*). As such, it adds to the literature on descriptive political selection, which has documented consistent differences between citizens and politicians, focusing mainly on socio-demographic characteristics (see, for instance, Gulzar, 2021).² To the best of our knowledge, no existing study compares the individual preferences over redistribution of politicians and non-politicians, with the exception of Helfer et al. (2024). The latter observe that Swiss MPs are less supportive of redistribution and perceive inequalities to be fairer than a representative sample from the general population. In addition to differences in the sample composition of politicians (French and local in our study), there are two major methodological differences with our work: the authors collect self-declared general opinions using a four-point scale whereas we rely on a revealed preferences approach using an incentivized experiment. Moreover, we do not only evaluate to what extent politicians and non-politicians differ in their preferences over redistribution: we also assess whether politicians follow non-politicians views about redistribution (*political representation*).

Further, our results also inform the literature on the determinants of redistribution. Such an area of inquiry has attracted renewed scientific interest due to the surge in inequalities in the last decades (Chancel et al., 2022) while redistribution has declined in several countries (Causa and Hermansen, 2020). The coexistence of both phenomena lies in contradiction with the influential Meltzer and Richard (1981) model, which predicts an increase of both demand for and supply of redistribution in the face of increased inequalities. As a consequence, many studies have focused on the drivers of citizens' demand for redistribution, using large-scale questionnaires as well as economic experiments (for reviews on existing studies, see Alesina and Giuliano, 2011; Mengel and Weidenholzer, 2022). Yet, such line of inquiry implicitly

²Our paper is also distantly related to the literature on political selection in general, which focuses on how and why a fraction of the population is selected into political office. Recent reviews of such literature can be found in Dal Bó and Finan (2018) or Gulzar (2021). In this paper, we do not seek to model a political selection process to investigate the mechanisms behind observed behavioral differences between politicians and non-politicians.

avoids analyzing the mapping from people's preferences to implemented policies, hence policy-makers' responsiveness to citizens' demands. Given that the policy views of the majority might be simply ignored by policy-makers (Bartels, 2008), understanding to what extent policy-makers are responsive to people's preferences for redistribution can shed light on the increasing inequalities/staggering redistribution conundrum. In this paper we look at the supply side of redistribution by recruiting actual politicians as experimental subjects and assessing their reaction to citizens' demands.

The remainder of the paper is organized as follows. Section 2 introduces the experimental design and presents our participant pools. Section 3 exposes our empirical strategy and our main hypotheses. Section 4 details our results. In Section 5, we report on our study's limitations and run a battery of robustness checks. Concluding remarks follow in section 6.

2 Experimental design

The experiment sequence is divided into four parts, as illustrated in Figure 1. After reading the preliminary instructions, participants engaged in a risk aversion task using a standard multiple price list (Harrison and Rutström, 2008).³ In this task, participants chose between a fixed payment and a lottery five times, with no feedback on their gains before the end of the experiment and random selection of the task for payment. The second part of the experiment consisted of a taxation/redistribution task, where participants selected a flat tax rate to be applied to a 9-citizen group. These two tasks were incentivized and participants were explained that 1 out of 50 participants would be selected to effectively receive their total payoff (sum of gains from risk aversion task and taxation redistribution task) at the end of the experiment.⁴ In the third part, participants performed a non-incentivized belief elicitation task. The order of the taxation/redistribution task and the belief elicitation task was randomized to control for order effects. Finally, participants responded to a socio-demographic questionnaire.⁵ An English-translated version of the instructions is presented in appendix A.

2.1 Taxation-redistribution task

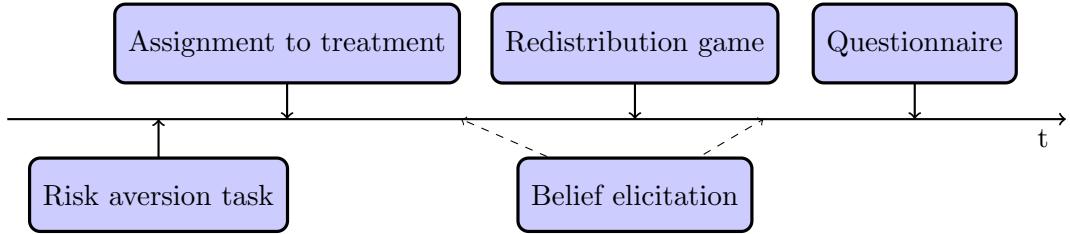
Structure of the task In the main task, there are two types of participants: policy-makers and citizens. Our sample of French politicians only participated as policy-makers. Non-politicians participated first as policy-makers and second as citizens, with the opportunity to earn a payoff for either role, chosen randomly. Crucially, they were told that they would play as citizens only after they submitted their decisions as policy-makers (including the belief elicitation task) and before the questionnaire, so that the decisions made by politicians and non-politicians as policy-makers are readily comparable.

³We control for risk preferences in our analysis because the redistribution task entails uncertainty regarding whether or not the participant earns a payoff.

⁴That is, we implement a Between Random Incentive System (BRIS), which is common in survey experiments. In their literature review, Charness et al. (2016) document that paying a subset of participants generally does not imply a substantial dilution of the incentives to answer truthfully.

⁵The questionnaire included questions about gender, age, department of residence, education level, field of study, socio-professional category, income and political preferences. Politicians also answered additional questions about their institutional characteristics, such as their current office, the number of former mandates and the number of inhabitants of their municipality.

Figure 1: Sequence of the experiment



Note: The dashed lines indicate the randomization in the order of the taxation-redistribution/belief elicitation tasks.

At the beginning of the task, policy-makers are anonymously paired (*stranger matching*) and are informed that no information regarding their competitor's identity of choices will be communicated to them. Each pair is matched with a 9-citizen group and informed that the citizens are drawn from a sample from the general French population. They are also informed that each citizen is randomly assigned one of nine possible initial endowments, and that the distribution of these endowments reflects the income distribution in France in 2017 (see table A.3.1 in the appendix). Each policy-maker then chooses a flat tax rate between 0% and 100% in increments of 1 p.p. to apply on the citizen group. Which competing policy-makers' tax rate is applied depends on treatment (see below). Once the tax rate is applied and tax receipts are collected, a fixed fraction of the total tax receipts is lost (*dead-weight loss*) while the remainder is equally redistributed among the nine citizens.⁶ In order to ease comprehension, a graphical representation of the mechanism is displayed to policy-makers (figure A.3.1 in the appendix). Moreover, they can verify the citizens' payoff associated with each tax rate both in the initial instructions and before each decision (figure A.3.2 in the appendix).

After participating as policy-makers, non-politicians participate as citizens. They are informed that they will take part in a group of 9 (anonymous) citizens, which is matched with two (anonymous) competing policy-makers, and that this group is different from the 9-citizen group for which they made decisions as policy-makers. In other words, their decisions as policy-makers cannot matter for themselves as citizens. Each citizen in the group is then informed about her initial endowment and is asked to express her preference for the tax rate, with options ranging from 0% to 100% in increments of 10 p.p. The computer then calculates the median preference within each group (the "median rate"), which may serve to calculate citizens' final payoff (*i.e.*, to determine the winner of the competition between policy-makers). Assuming citizens care only about maximizing their material payoff, the game's payoff structure yields a lower bound of 0 and an upper bound of 20% for the equilibrium median rate. The

⁶Formally, a citizen j 's payoff is defined as:

$$\pi_j = y_j(1 - \tau) + \tau(1 - e) \frac{1}{9} \sum_i^9 y_i$$

where y_j is the citizen's initial endowment, τ is the tax rate, 9 is the number of citizens in the group and e is the efficiency loss parameter. The efficiency loss parameter $e = 6.8\%$ has been computed so that the monetary gain of the poorest citizen would have been equal to the total efficiency loss, that is $|\partial \pi_i(y_L)/\partial \tau| = |\partial \sum_i^9 \pi_i/\partial \tau|$. Since the redistributed sum consists of a lump-sum transfer to all citizens, it is equivalent to a *publicly provided private good*.

fifth citizen is indeed pivotal and is indifferent between the rates in the [0-20] interval (see Table A.3.2 in the appendix). However, assuming citizens also care about other citizens' payoffs (*e.g.*, they hold distributional preferences) renders the equilibrium uncertain. Such a possibility is all the more likely as the changes in the fifth citizen's payoff are very limited (1 centime at most).

Treatments Each policy-maker chooses in one out of four experimental conditions, resulting from the combination of two between-subjects treatments.⁷

One treatment varies the information set policy-makers receive before choosing the tax rate. In the *No Information* treatment, policy-makers do not receive any information about the median rate. Consequently, participants in the *No Information* treatment choose one tax rate only. In the *Information* treatment, policy-makers decide while having the information about the median rate. We employ the strategy method (Selten, 1965), so that participants in the *Information* treatment choose a tax rate 11 times, one per each possible median rate between 0% and 100% in increment of 10 p.p. The use of the Strategy Method is not only logically-convenient in the design of an online interactive experiment, but it also allows to expose all subjects to the same information, thus assessing how tax choices change with the level of the median rate within subjects. In other words, we do not evaluate how policy-makers respond to the true median rate. Rather, we evaluate how they respond to a range of possible median rates, while accounting for their beliefs about the true median rate (see next sub-section). More specifically, participants in all treatments are informed about citizens' being asked to select a rate and about how the median rate is computed. In the *No Information* treatment, they are further told that such information will remain concealed.⁸ In the *Information* treatment, the exact median rate remains concealed but participants are asked to choose a tax rate for each possible median rate.

The other treatment involves a variation of political competition. In the *Competition* treatment, the chosen tax rate that is the closest to the median rate is implemented.⁹ In the *Dictator* treatment, the tax rate proposed by one of the two participants is randomly selected with equal probabilities. Therefore, the difference is that in the *Dictator* treatment the tax decision does not influence the probability that the tax rate is applied nor the possible payoff of the participant.¹⁰ In both cases, the participant whose decision is implemented earns an additional payoff of 33 Survey Currency Units (SCUs), corresponding to 16.5 euros given the exchange rate of 1 SCU = 0.5 euros.¹¹ On the contrary, the participant whose decision is not implemented does not earn anything in this task.

⁷For the sake of simplicity, non-politicians do not switch treatment when going from policy-maker to citizen: the rules of the game remain the same across the two roles.

⁸This feature was implemented to focus on the effect of information *per se*, independently from participants' knowledge that the information exists (which could be heterogeneous across participants). Moreover, in daily politics, politicians are aware that their actions are evaluated against citizens' demands, although these demands may not be fully available to them.

⁹Participants in the *Competition* treatment were also notified that draws would be resolved by uniform-random selection.

¹⁰This setting relates to the spectator condition that is frequently scrutinized in the literature on redistribution (see *e.g.*, Durante et al., 2014; Almås et al., 2020).

¹¹Given the average duration of the survey, this sum corresponds to a payment of about 40 euros per hour.

2.2 Belief elicitation task

We elicit policy-makers' beliefs about the median rate and about the tax choice of the competitor with whom they are matched. To ease comprehension of this task, we exploit a token allocation task discussed in [Delavande et al. \(2011\)](#) (see Figure A.4.2 for an example). Participants allocate 10 tokens to different events, with each token representing a probability of 10%. To elicit beliefs about the median rate, participants allocate the 10 tokens among the 11 possible median rates. To elicit beliefs about the competitor's tax choice, participants are asked to allocate the 10 tokens to 11 intervals representing the potential range of choices (since policy-makers choose tax rates in 1 p.p. increments, while citizens do so in 10 p.p. increments).¹² In the *No information* treatment, we elicit one belief about the competitor's choice because policy-makers make only one choice. By contrast, in the *Information* treatment, we elicit 11 beliefs about the competitor's choice because policy-makers make 11 choices (strategy method).

This belief elicitation task has the advantage of providing participants with an intuitive notion of probabilities and, by giving a fixed number of tokens, ensures that the probabilities add up to one ([Delavande et al., 2011](#)). Most importantly, we can retrieve the subjective probability distribution of their beliefs and compute its mean, median, mode and standard deviation. In particular, the standard deviation provides a measure of participants' confidence in their beliefs.

Importantly, we chose to rely on introspection, that is, asking people to report their beliefs truthfully, rather than on incentives to elicit beliefs. Indeed, whether and how to incentivize belief-elicitation tasks remain a hotly-debated topic in the experimental economics literature. As noted by [Charness et al. \(2021\)](#), complex mechanisms are often advised because of their adequate theoretical properties, but their practical implementation may be so cognitively demanding that they actually do not outperform simple introspection. Moreover, incentivizing comes with a series of issues that may bias the elicitation while not being easily solved (*e.g.*, hedging, that is, participants coordinating their beliefs and actions when both are paid in order to maximize expected payoffs). Considering that our main target population (politicians) is inexperienced with economic experiments, possibly reluctant to getting paid and more time-constrained than the remainder of the population, we considered incentivizing the belief-elicitation task might be too risky in terms of both response rate and data quality.

2.3 Experimental Settings

At the beginning of the experiment, participants were informed of the expected average duration of the experiment (25 minutes), that anonymization was strictly guaranteed and that 50 cents would be donated to a charity of their choice after full completion of the survey.¹³ Moreover, they were informed that participating would give them the opportunity to earn an additional payoff whose amount would

¹²The intervals are [0%,5%], [6%,15%], [16%,25%], [26%,35%], [36%,45%], [46%,54%], [55%,64%], [65%,74%], [75%,84%], [85%,94%], [95%,100%]. We then assign a value ranging from 0 to 100 in increments of 10 to each interval and treat this transformed variable the same way as the belief about the median rate. Notably, we chose to narrow the range of the two extreme intervals because they correspond to extreme views on redistribution, hence likely candidates for the competitor's tax choice: no redistribution (pure efficiency orientation) and full redistribution (pure equality orientation).

¹³The list of charities included the “*Institut Pasteur*”, a non-profit private organization supporting the study of biology, diseases and vaccines, the “*Restos du Cœur*”, a charity that provides food to people in need, and “*Médecins sans Frontières*”, an organization providing medical support to people in conflict zones.

depend on the decisions made during the experiment. This payoff could be either kept or donated to a charity, a choice that they made before knowing whether they were selected for payment.¹⁴ Participants knew from the beginning of the experiment that they would make this choice once the experiment is over. However, the charities list was presented only at the end of the experiment to avoid motivation-induced disparities across treatments.

The experiment was conducted on Limesurvey from the 2nd of November 2020 to the 30th of December 2020. Overall, 1528 people participated in the experiment, of whom 801 were French local politicians and 727 were French non-politicians. On average it took 24 minutes for participants to complete the experiment. Participants who chose to be paid (14.61% of politicians and 84.89% of non-politicians) and who were randomly picked by the computer received a bank transfer at the end of the survey period (beginning of January). The donations to charity have been managed in the same period by a survey institute named *Panelabs*, which also handled the recruitment of non-politicians. In our analysis we exclude 71 participants who completed the experiment in less than 4 minutes for short treatments (*No Information*) and less than 9 minutes for long treatments (*Information*). We also exclude one politician from Martinique¹⁵ and 18 participants who declared using a phone to login even though they were instructed not to at the beginning of the experiment.¹⁶ We also remove 4 politicians that did not answer to the question about their current office. Finally, we remove duplicates based on IP addresses (10 participants). The final sample includes 760 politicians and 655 non-politicians. Figure 2 depicts the geographical distribution of the response rate for politicians and non-politicians.

While the sample of non-politicians was reached through a survey institute (*Panelabs*), we contacted politicians by sending an official invitation by mail to both associations of local politician (*Associations d'élus*) and to the town halls of all French municipalities.¹⁷ The invitation was accompanied by a formal demand to forward it to politicians. Any local politician could participate in the experiment. We targeted local instead of upper-tiers politicians for several reasons. First, a long-standing tradition considers local governments as the “training grounds for democracy” (John Stuart Mill). Indeed, the proximity between politicians and citizens at the municipal level presumably improves democratic representation through better information, incentives and monitoring of politicians (Mansuri et al., 2023). At the same time, holding a local office in France often constitutes the first step toward a political career, so that the targeted group corresponds to the first layer of the political selection process. Consequently, focusing on local politicians appears a valuable starting point to improve our general understanding of politicians’ behavior

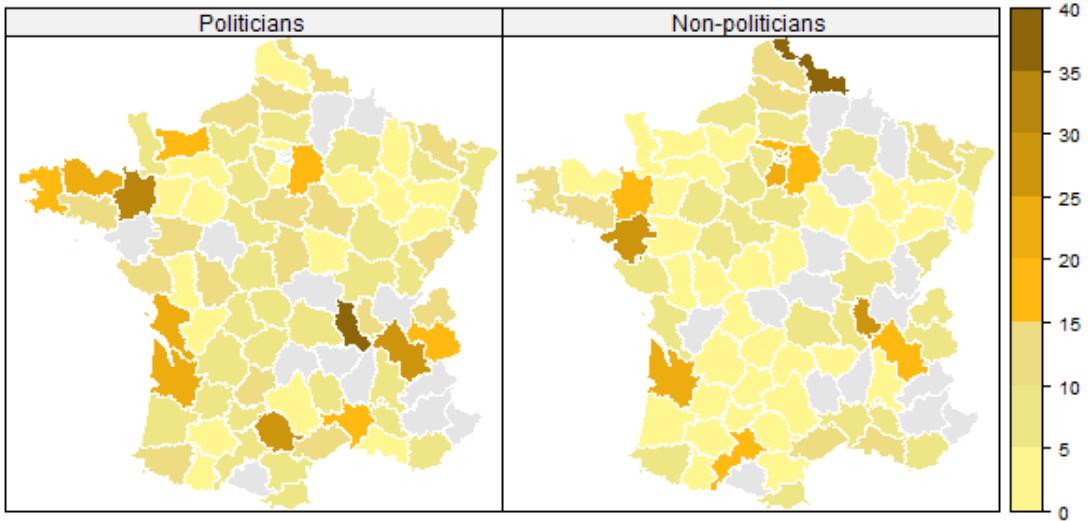
¹⁴Paying politicians has been recognized as a possible issue in empirical studies (Kertzer and Renshon, 2022). Indeed, they may be reluctant to obtain a private benefit in direct relationship with their public position. In order to maintain a high level of incentives while reducing the risk of attrition and/or adverse publicity, we offered the possibility to forego one’s payoff to the benefit of a charity as an alternative to the standard individual incentivization scheme. Donations to charity as an incentivization scheme for public officials has already been tested by Butler and Pereira (2018): They find that American state legislators perform equally in a quiz when they receive a personal gain or when their gains are given to a charity, and that both incentivization schemes improve performance as opposed to a no incentives benchmark.

¹⁵Our survey was addressed to only people from mainland France (*France métropolitaine*). However, some of the politician associations who agreed upon communicating the link to the experiment had some members abroad, which explains the presence of this Martinican politician.

¹⁶Running the experiment on a phone caused display issues in pre-experimental tests, therefore we decided to rule out such an option.

¹⁷The details on the recruitment of our politicians sample can be found in appendix A.7.

Figure 2: Number of participants per department



Note: In the grey shaded departments no answers were given.

in representative democracies. Second, local politicians are generally socio-demographically closer to non-politicians than upper-tiers politicians (Gulzar, 2021), which facilitates cross-groups comparisons. Third, existing surveys and economic experiments focusing on politicians’ behavior often merge office holders from different administrative layers or adopt a loose definition of “political elite”, which may result in noisy measures and cause interpretation issues (Kertzer and Renshon, 2022). Contrary to such practices, this paper focuses on a well-defined group of politicians. Fourth, the underlying population from which to sample is larger and presumably less time-constrained than the population from higher administrative layers, which reduces the risk of non-responses. Fifth, increasingly due to the decentralization movement, local politicians have many competencies (such as urban planning, property taxation, schooling, housing, etc.) that have a substantial impact on citizens’ daily well-being.

Of all invitations that were sent and accessed, 13% were completed in the politicians sample and 64% in the non-politician sample. Such a difference is not surprising considering that our politician sample was constructed from scratch whereas our non-politician sample was selected from *Panelabs*’s database, so that part of the selection is already accounted for in the latter group. For the sake of comparison, Vis and Stolwijk (2021) compile 342 large, multi-wave survey samples of political elites and observe an average response rate of about 23% for online-only surveys. Further, they observe important variations in response rates and identify elite level (*e.g.*, subnational politicians participate more) as well as countries’ survey tradition (*e.g.*, France shows among the lowest response rates) as important drivers for such variations. As a consequence, the response rate we observe corresponds to a lower bound with respect to existing findings on politicians’ surveys, but does not fall out of range. Notably, it is likely that the secretaries who received our mail accessed the questionnaire to check its validity, which means that

13% is a very conservative lower bound for the actual completion rate. Indeed, Figure B.2.1 plots the cumulative distribution of dropouts and shows that most politicians' dropouts occur on the first page. Upon excluding those dropouts, the completion rate rises to 27%.

Table 1: Summary statistics

	Sample politicians	All local politicians	Sample non-politicians	General population
Age	51.91	52.16	47.98	50.77
<i>Gender</i>				
Female	0.38	0.42	0.61	0.52
Male	0.62	0.58	0.39	0.48
<i>Occupation</i>				
Craftmen, Retailers or entrepreneurs	0.08	0.07	0.04	0.04
Employees	0.16	0.14	0.27	0.16
Executive	0.33	0.20	0.10	0.10
Farmers	0.05	0.08	0.00	0.01
Inactive	0.04	0.04	0.10	0.14
Intermediate professions	0.10	0.17	0.15	0.15
Laborers	0.03	0.07	0.05	0.12
Retired	0.19	0.23	0.26	0.28
<i>Geographical area</i>				
North	0.13	0.24	0.35	0.33
East	0.15	0.22	0.09	0.13
West	0.35	0.26	0.26	0.25
South	0.36	0.28	0.29	0.30
<i>Education</i>				
High school diploma	0.36		0.48	0.68
Higher education	0.64		0.52	0.32
<i>Gross yearly personal income</i>				
Less than 20k	0.19		0.26	
Between 20k and 30k	0.27		0.28	
Between 30k and 40k	0.19		0.19	
More than 40k	0.29		0.20	
<i>Party Preferences</i>				
Left	0.35		0.28	
Center	0.15		0.16	
Right	0.16		0.23	
None	0.34		0.34	
<i>Ideology</i>	6.74		7.01	
<i>Risk aversion index</i>	7.84		9.62	
<i>Current office</i>				
Mayor	0.21	0.07		
Vice-mayor	0.23	0.23		
Local councillor	0.56	0.70		
<i>Municipality's population</i>				
Less than 1000	0.46	0.56		
Between 1000 and 3500	0.30	0.24		
More than 3500	0.24	0.19		

Note: For the correspondence between French regions and geographical areas, see table B.1.1. For the classification of party preferences, see table B.1.2 in the appendix. “Ideology” represents a numerical variable between 0 (extreme left) and 10 (extreme right) based on a likert scale question. The dataset on all French politicians in the 2020-2026 mandate is collected by the French Interior Ministry and available online. The data on the general population and municipalities’ population has been collected from the 2020 general census conducted by the French National Institute of Statistics and Economic Studies (INSEE).

Table 1 presents the summary statistics of the individual characteristics obtained from the end-of-experiment questionnaire for both politicians and non-politicians. Politicians are slightly older, more educated and richer than non-politicians. Moreover, the proportion of male politicians (62%) exceeds

that of female politicians (38%), while the reverse holds true for non-politicians (39% male and 61% female). All these differences have been documented in the literature on political selection, both in developed and developing countries ([Gulzar, 2021](#)). The sample of politicians shows fewer respondents from the northern regions of France and more from the western regions. In terms of political preferences, the non-politician sample leans more toward the right-wing compared to politicians, both in terms of party preferences and self-declared ideology on a 0-10 scale. Lastly, politicians display a higher propensity for risk-taking than non-politicians, and this difference is statistically significant (two-sided t-test: $p < 0.01$). This last finding aligns with [Heß et al. \(2018\)](#), who find that German political representatives are significantly more risk loving than the average citizen. A similar result is obtained by [Sheffer et al. \(2018\)](#) on American local politicians, although with weaker statistical support.

To further document issues of selection and attrition, we also relate our samples' characteristics to the characteristics of the underlying populations, making use of France's open public data policy. With respect to the whole population of French local politicians, we do not observe significant differences in terms of age and gender. Yet, our sample contains a higher proportion of politicians holding executive positions, mayors and intermunicipal councillors, and is skewed toward more populous municipalities. Concerning non-politicians, we observe that our sample is younger, more educated and has a greater proportion of women than the overall French population. To account for such differences with the two general populations, we will test the robustness of our main analysis to using post-stratification weights.

Table 2: Distribution of participants by treatment

(a) Politicians (N = 760)			(b) Non-Politicians (N = 655)		
	No Information	Information		No Information	Information
Competition	220 (28.95%)	150 (19.74%)	Competition	190 (29.01%)	153 (23.36%)
Dictator	255 (33.55%)	135 (17.76%)	Dictator	177 (27.02%)	135 (20.61%)

Table 2 shows the distribution of participants by treatment. Although there are no relevant differences in the number of participants between the *Dictator* and *Competition* treatments, a greater difference arises between the *Information* and the *No Information* treatments. This difference might stem from the fact that there are more choices to make in the *Information* treatment, extending the experiment duration. Indeed, on average, the *Information* treatment took 32 minutes to complete, while the *No Information* treatment took 20 minutes. Further, we observe in Figure B.2.1 that the difference in dropouts between treatments for both groups is relatively steady except for a peak when participants are either asked to select a tax rate or to state their beliefs, depending on the order of tasks (*i.e.*, on page 4). In both cases, the *No-information* treatment involves making 10 decisions less than the *Information* treatment. In other words, it is likely that the number of decisions deterred some participants from finishing the survey. Moreover, we observe a greater participation gap between the *No Information* and *Information* treatments among politicians than among non-politicians, as well as a higher proportion of politicians in the former than in the latter (Table 2). As a consequence, it is possible that the tighter time constraints

that politicians faced led to an increased attrition. However, in table B.3.1, we observe no significant differences among participants across treatments based on observables, which mitigates the risk that selection is an important issue in our data for cross-treatments comparisons. Moreover, we make use of the fact that the risk elicitation task occurred at the beginning of the experiment to check whether participants who dropped out eventually behaved differently in this task than participants who stayed. We find no statically significant difference in risk aversion in the politician and non-politician samples.

3 Empirical strategy

In this study, we are primarily interested in the effects of our treatments on participants' tax choices. Due to the differing number of observations per participant in the *Information* and *No Information* treatments, the two treatments are not directly comparable. Consequently, we consider them separately and gauge the importance of citizens' preferences on tax choices in each treatment by analyzing individual responses to the beliefs about the median rate (*No Information* treatment) or to variations in the median rate (*Information* treatment).

First, we analyze the tax choices in the *No Information* treatment while pooling the politician and non-politician samples. The first specification is:

$$\begin{aligned} \tau_i = & \beta_0 + \beta_1 Dictator_i + \beta_2 Politician_i + \beta_3 BeliefMedian_i + \beta_4 Dictator_i \times BeliefMedian_i \\ & + \gamma X_i + \theta Z_i + \epsilon_i, \end{aligned} \quad (1)$$

where τ_i is the tax rate chosen by participant i , $Dictator_i$ is a dummy indicating whether the participant played as dictator ($= 1$) or in competition ($= 0$), $Politician_i$ is a dummy equal to 1 if the participant is a politician, 0 otherwise, $BeliefMedian_i$ is the mean of the subjective probability distribution about the median rate. We also include the interaction between the belief about the median rate and the *Dictator* treatment to capture the impact of the lack of political competition on the influence of citizens' preferences. X_i is a vector of personal characteristics including age, gender, gross annual income, education level, region of residence, occupation and political ideology based on party preferences. Further, we control for a set of experiment-specific controls (Z_i), such as the mean of the subjective probability distribution about the competitor's tax choice,¹⁸ a risk aversion index¹⁹, an order-effect dummy with value 1 (respectively 0) if the belief-elicitation task occurred before (respectively after) the redistribution task, and the time spent completing the survey. Finally, β_0 is a constant and ϵ_i is the error term, which is assumed to be normally distributed.

Second, we analyze the tax choices when participants have information about the median rate. In

¹⁸ Additionally, we run the main regressions using the median and mode instead of the mean of the subjective probability distributions. We find that our results are not driven by our definition of the belief variables (Table B.10.2).

¹⁹ Following Durante et al. (2014), the risk aversion index is computed as the sum of questions $i \in \{1, 2, 3, 4, 5\}$ in the multiple price list task, where participants choose the sure outcome instead of the lottery. Consequently, the index ranges from 0 to 15, with 0 indicating a risk-lover participant who never chooses the sure outcome, and 15 being the highest risk-aversion score (1+2+3+4+5).

this case each participant makes 11 decisions. The specification is:

$$\tau_{it} = \beta_0 + \beta_1 Dictator_i + \beta_2 Politician_i + \beta_3 Info_{it} + \beta_4 Dictator_i \times Info_{it} + \gamma X_i + \theta Z_i + \epsilon_{it}, \quad (2)$$

where τ_{it} is the tax rate chosen by participant i when the information given is $Info = [0, 10, 20 \dots 100]$ for each choice $t = [0, 1 \dots, 10]$. The vector of experiment-specific controls Z_i also includes the beliefs about the median rate. In this case, we cluster standard errors at the individual level to account for serial correlation.

Based on models (1) and (2), we make some behavioral conjectures on the alignment between policy-makers' choices and citizens' preferences (political representation): First, we do not expect β_1 to be significantly different from null. Indeed, this coefficient represents the behavioral difference across treatments when the median rate is zero and there is no reason to expect that it should change when we increase the degree of political competition. Second, we expect β_3 to be positive, as it represents the effect of the median rate, both in the form of participants' beliefs (model (1)) and the information we provide (model (2)), in the competition treatment (when the dictator dummy is equal to zero). Indeed, the payoff-maximizing strategy is for competitors to converge toward citizens' preferences. Third, the value of β_4 is indicative of the relative weight of two competing explanations for the relationship between tax choices and citizens' preferences. On the one hand, abiding by the prevailing logic in theoretical models of political economy, we may expect β_4 to be negative. Indeed, such a framework implies that competitors implement citizens' (expected) preferences while dictators only follow their normative views about redistribution. In the extreme, competitors strictly abide by the median rate to maximize the probability of winning whereas dictators implement the same decision (possibly absent or full redistribution) no matter the median rate, that is, $\beta_4 = -1$. On the other hand, we may still observe a positive relationship between citizens' preferences and the chosen tax rates even in the absence of political competition due to the pro-social motivations of policy-makers. Indeed, granting citizens what they want may seem fair, even without extrinsic incentives. In the extreme, we may expect no difference between the *Dictator* and the *Competition* treatments, that is $\beta_4 = 0$.²⁰

Finally, the second objective of this study is to compare the behavior of politicians and non-politicians in a taxation-redistribution context (behavioral representation). Specifically, we are interested in whether the chosen rate is different between both groups (which is captured by the β_2 parameter). Considering the shortage of studies focusing on behavioral differences between politicians and the remainder of the population, no literature-based conjecture can be made. It is one objective of this study to provide the first evidence of such differences. Furthermore, we focus on whether politicians and non-politicians differ in their reaction to our treatments. Consequently, we will further run both models (1) and (2) for each sample separately. We expect politicians and non-politicians to have different behavioral responses to our treatments. In particular, we expect politicians to be more inclined to implement citizens' preferences in our experiment since part of their daily activities implies being attentive to citizens' demands. Such

²⁰We also note that distributional concerns (foremost a taste for either maximizing the social surplus or minimizing inequalities) are likely important drivers of observed behavior in our redistribution task. In our analysis, such concerns are captured either by some of the control variables contained in X_i (such as political preferences) or as residuals.

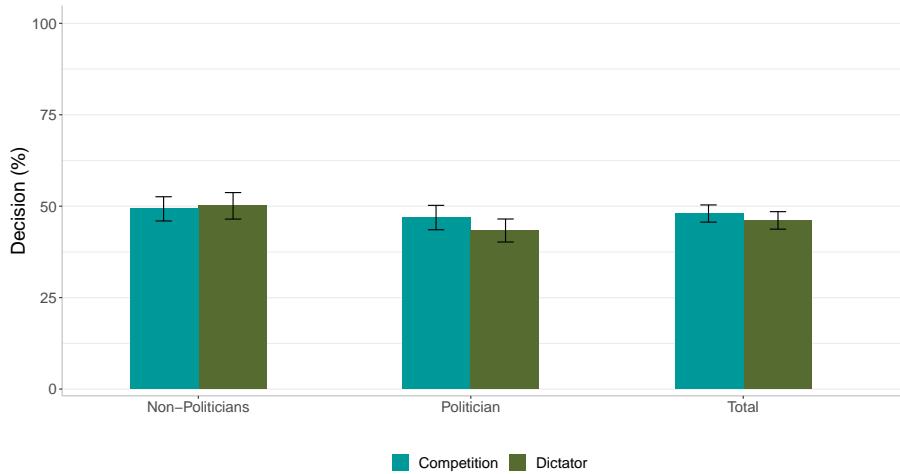
hypothesis translates into a higher β_3 for politicians than for non-politicians in both models: Politicians follow more what they think citizens want when not provided with the information about citizens' preferences, and they stick more to the information when provided. Furthermore, since they already know the rules of the political game because they already experienced at least one election, we expect politicians to be more reactive to the (expected) median rate when participating in a political competition, that is, a lower β_4 than for non-politicians.

4 Results

4.1 Descriptive analysis

Figure 3 reports the differences in the tax choices between the *Dictator* and the *Competition* treatments for politicians, non-politicians, and both groups pooled, in the *No Information* treatment. Three results can be highlighted: First, there is no difference between the two treatments for either politicians and non-politicians (in all cases, $p > 0.1$ in a two-sided t-test for differences in mean). Second, politicians choose a lower tax rate compared to non-politicians. The difference in mean between politicians' and non-politicians' choices is 2.38 p.p. in the *Competition* treatment and 6.74 p.p. in the *Dictator* treatment. However, the difference is statistically significant only in the latter case (two-sided t-test: $p < 0.01$).²¹ Third, in Figure B.4.1, we note that extreme strategies, which involve either maximizing efficiency (no redistribution) or minimizing inequalities (full redistribution), make up only a minority of observations (7.6%). Interestingly, there are approximately seven times as many policy-makers who implement a 100% tax rate compared to those who choose a 0% tax rate (56 compared to 8).

Figure 3: Tax choices - *No Information* treatment

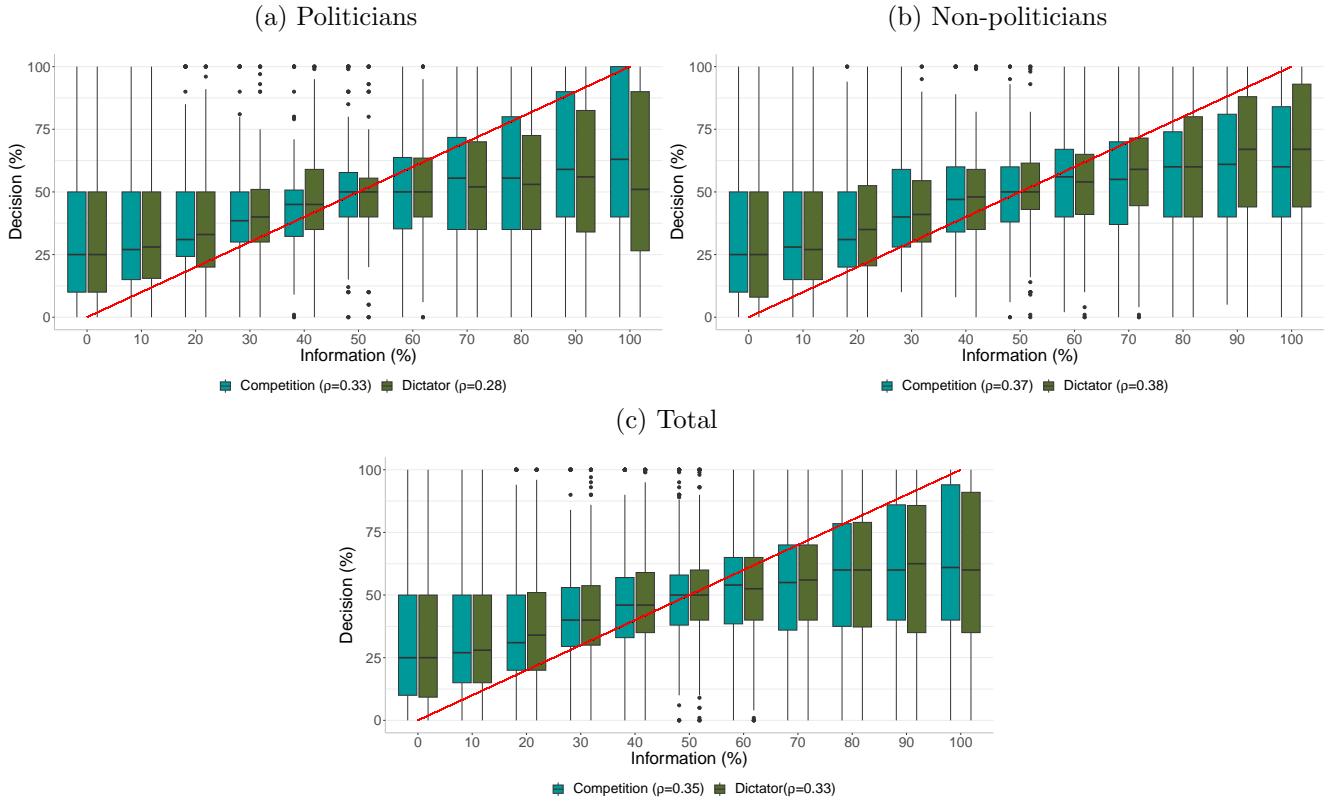


Notes: Mean and 95% confidence interval of the tax rate choice by group and by treatment.

Turning to the *Information* treatment, Figure 4 shows the correlation between the information about the median rate and the chosen tax rate. This correlation is positive for both politicians and non-

²¹We find similar results when we consider the entire distribution of tax choices, as in Figure B.4.1.

Figure 4: Tax choices - *Information* treatment



Note: The red line represents the hypothetical relationship between decision and information in case of perfect correlation. ρ is the Pearson correlation coefficient.

politicians and for both the *Dictator* and the *Competition* treatment. As a consequence, policy-makers do respond to the information. However, such correlation is hardly perfect both in the *Dictator* treatment ($\rho = 0.33$) and the *Competition* treatment ($\rho = 0.35$). Participants do deviate from the median rate, especially when the information represents an extreme view on taxation (tax rate close to 0% and to 100%). The average absolute difference between tax choice and the information is 24.54 p.p. Finally, politicians deviate more when they play as dictators compared to non-politicians: The Pearson's correlation coefficient between participants' tax choice and the information in the *Dictator* treatment is 0.28 for politicians and 0.38 for non-politicians.²²

To provide additional insights on the difference between our treatments and groups, we now focus on the elicited beliefs about the median rate and about the competitors' choice. Figure B.5.1 and Figure B.5.2 in the appendix illustrates these beliefs in the *No Information* treatment, considering the mean, the median and the mode of their subjective probability distribution. In all figures, beliefs exhibit a relatively normal distribution. We do not observe variation in beliefs between the *Dictator* and the *Competition* treatments for either politicians and non-politicians. However, we find strong evidence that politicians' beliefs are more skewed toward the right, implying that they believe both the median citizen and the competitor choose a lower tax rate than non-politicians. Further, we find that this difference is more

²²By contrast, in the *Competition* treatment the same correlation is 0.37 for non-politicians and 0.33 for politicians.

pronounced in the *Dictator* treatment than in the *Competition* treatment.

We observe similar patterns in the *Information* treatment (Figures B.5.3 and B.5.4),²³ with politicians exhibiting more right skewed beliefs than non-politicians for both the median rate and the competitor's choice. For the competitor's choice, this is particularly the case when the provided median rate is low (*i.e.*, the information we give using the strategy method), which is consistent with politicians expecting a lower median rate than non-politicians. In addition, we observe that the correlation between the information and the competitors' choice is much stronger than the correlation between the information and the tax choices for politicians (by about 11 p.p.), while non-politicians show a rather opposite pattern (by about 3 p.p.). In other words, politicians seem to believe that the competitor will align more strongly with the information than what they do, while it is much less the case for non-politicians, for whom the correlation lines are much more similar.

Finally, another difference involves the standard deviation of the beliefs about the median rate and the competitor: The average standard deviation of the belief about the median rate distribution is 15.17 (14.31) for non-politicians and 12.52 (12.28) for politicians in the *No Information* (*Information*) treatment, with the difference being statistically significant (two-sided t-test: $p < 0.01$ in the *No Information* treatment and $p = 0.0175$ in the *Information* treatment). Regarding the beliefs about the competitor, the standard deviation is 13.75 (12.75) for non-politicians and 11.77 (9.57) for politicians in the *No Information* (*Information*)²⁴ (two-sided t-test: $p < 0.01$ in the *No Information* and *Information* treatments). This suggests that politicians have greater confidence in their beliefs.

4.2 Regression analysis on tax choice

We now turn to the regression analysis presented in section 3. For exposition purposes, we first regress participants' tax choice on the *Dictator* treatment variable and on the *Politician* dummy, and we later add all the other variables. Given the censored nature of the dependent variable, we employ Tobit regressions censored at 0% and 100%.²⁵

Table 3 reports the results for the *No Information* treatment. In column (1), the coefficient on *Politician* is negative and significant, while the coefficient on *Dictator* is negative and not significant. The coefficient on *Politician* does not change when, in column (2), we add a wide set of individual-specific controls. However, the coefficient on the same variable is not significant when, in column (3), we also consider participants' beliefs about the median rate as explanatory variable, whose coefficient is positive and strongly significant. This reflects that being a politician is correlated with a different views on the median rate, which translates into a lower tax choice. In column (4), we also introduce the beliefs about the competitor's tax choice, which has a positive and statistically significant relationship with the

²³Specifically, while there is little difference in beliefs about the competitor's choice across treatments, we find that the distribution of policy-makers' beliefs about the median rate is more skewed toward the right in the *Competition* treatment. Given that under a pure self-interest assumption the voting equilibrium lies in the [0-20] interval, such a finding is consistent with policy-makers paying greater attention to citizens' behavior in the *Competition* than in the *Dictator* treatment when the information about the median rate is provided.

²⁴In the *Information* treatment, we consider the average of the standard deviations computed for each participant over the set of the 11 possible median rates.

²⁵In the appendix, we run the main regressions using OLS, and we obtain the same results.

Table 3: Tobit regressions - Tax choice - *No Information* treatment

	Dependent variable: Tax rate				
	(1)	(2)	(3)	(4)	(5)
Dictator	-1.628 (1.838)	-2.583 (1.828)	-0.767 (1.523)	-0.813 (1.490)	0.255 (4.411)
Politician	-4.584** (1.840)	-4.603** (2.147)	0.0226 (1.754)	0.740 (1.725)	0.719 (1.739)
Belief about Median			0.764*** (0.0503)	0.460*** (0.0744)	0.473*** (0.0840)
Belief about Competitor				0.381*** (0.0685)	0.380*** (0.0686)
Dictator \times Belief about Median					-0.0246 (0.100)
Constant	51.02*** (1.563)	54.86*** (7.707)	20.18*** (7.438)	15.50** (7.320)	14.89** (7.495)
Controls	X	✓	✓	✓	✓
Pseudo-R ²	0.000957	0.00663	0.0489	0.0546	0.0546
Observations	842	842	842	842	842

Notes: Controls include: age, gender, income, education level, occupation, region of residence, political ideology, a risk aversion index, a dummy for whether the belief elicitation task has been played before or after the redistribution game, and the time taken for completing the survey. Robust standard errors in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

tax choice.²⁶ Finally, in column (5), we introduce the interactions between the *Dictator* treatment and participants' belief about the median rate. Consistent with the insights of political economy models, the coefficient is negative, although low and not statistically significant.

Table 4 reports the results for the *Information* treatment. First, we find no significant differences between politicians and non-politicians and between the *Dictator* and *Competition* treatments. Including controls does not alter the results (columns 1 and 2). By contrast, the *Information* variable's coefficient is positive and strongly significant. Second, when in column (3) we introduce participants' beliefs about the median rate, the coefficient on *Information* does not change, whilst it decreases sharply when we include the beliefs about the competitor's choice in column (4). In all cases, the coefficient on *Belief about Median* is higher than the coefficient on *Information*, suggesting that participants put a higher weight on their personal beliefs than on the information they receive. Further, in column (5), the coefficient on the interaction *Dictator* \times *Information* shows the expected negative sign, but it is not statistically significant. Thus, we do not observe any difference in the impact of the median rate on the tax choice between the *Dictator* and the *Competition* treatment.²⁷

To summarize, we observe that policy-makers align with citizens' preferences: When no information is

²⁶The coefficient on *Belief about Median* decreases substantially. In Figure B.5.5 in the appendix, we depict the correlation between beliefs about the median rate and other participants' choices. The correlation is strong and positive, with a Pearson's correlation coefficient of 0.78, which is highly significant ($p < 0.01$).

²⁷In tables 3 and 4, standard errors are not corrected for multiple hypothesis testing. In appendix B.6, we find that applying different correction methods does not alter the significance of the coefficients.

Table 4: Tobit regressions - Tax choice - *Information* treatment

	Dependent variable: Tax rate				
	(1)	(2)	(3)	(4)	(5)
Dictator	-0.602 (1.783)	-0.602 (1.783)	-0.938 (1.605)	-1.313 (1.534)	-0.355 (2.068)
Politician	-0.833 (1.781)	-0.833 (1.781)	-0.280 (1.605)	0.397 (1.541)	0.397 (1.541)
Information	0.304*** (0.0189)	0.304*** (0.0189)	0.304*** (0.0189)	0.190*** (0.0204)	0.199*** (0.0261)
Belief about Median			0.477*** (0.0540)	0.294*** (0.0519)	0.294*** (0.0519)
Belief about Competitor				0.368*** (0.0348)	0.368*** (0.0348)
Dictator \times Information					-0.0192 (0.0342)
Constant	33.63*** (1.543)	33.63*** (1.543)	12.94*** (2.544)	9.094*** (2.520)	8.643*** (2.605)
Controls	X	✓	✓	✓	✓
Pseudo-R ²	0.0128	0.0128	0.0263	0.0376	0.0376
Observations	6303	6303	6303	6303	6303
Clusters	573	573	573	573	573

Notes: Controls include: age, gender, income, education level, occupation, region of residence, political ideology, a risk aversion index, a dummy for whether the belief elicitation task has been played before or after the redistribution game, and the time taken for completing the survey. Standard errors clustered at the individual level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

provided they rely on their beliefs about the median rate, while when the information is provided they take it into account (although imperfectly).²⁸ Such a finding is consistent with our behavioral conjecture on β_3 . Interestingly, we also observe little difference between the *Dictator* and the *Competition* treatments, with both β_1 and β_4 not significantly different from zero. Therefore, our results are compatible with a social-orientation interpretation, that is, policy-makers being intrinsically motivated to implement citizens' preferences, with little impact of political competition. Furthermore, we cannot reject the null of no difference between politicians and non-politicians once we account for differences in beliefs.²⁹

Finally, to position our paper within the literature on redistribution preferences, we briefly comment upon the effects of our control variables (as shown in Table B.7.1 in the appendix). First, we find that participants' ideology is highly correlated with the decision: Right-wing participants opt for a lower tax rate, that is, they implement less redistribution, which is consistent with the literature (see *e.g.*, [Alesina and Giuliano, 2011](#); [Mengel and Weidenholzer, 2022](#), and references therein). In the absence of a per-

²⁸We also test for $\beta_3 = 1$ in column (5) of Tables 4 and 5, that is, a perfect correlation between tax choice and the information, and find a significant difference ($p < 0.01$).

²⁹In Table B.5.1, we regress a politician dummy on the mean and standard deviation of participants' subjective probability distribution regarding the median rate and the competitor's choice, controlling for a wide range of personal characteristics. Notably, the distinction between politicians and non-politicians remains substantial and statistically significant.

sonal interest for lower taxes in the game, this result can be interpreted as a higher efficiency concern (*i.e.*, minimizing the efficiency loss to maximize the social surplus) among right-wing participants, as documented in the literature (see for instance the experiments with representative samples by [Fisman et al., 2017](#); [Müller and Renes, 2021](#)). Interestingly, the effect of ideology is more economically and statistically significant in the *No Information* treatment. Such a finding is compatible with the idea that policy-makers who do not know citizens' preferences will decide based on their moral values but will align with what citizens want when they are provided with the information. Second, in the *Information* treatment, participants with higher income levels tend to select a lower tax rate.³⁰ The negative relationship between income and support for redistribution is well-established in the literature ([Alesina and Giuliano, 2011](#); [Mengel and Weidenholzer, 2022](#)). However, we do not find a significant negative coefficient on income in the *No Information* treatment. Last, risk-averse participants show a preference for lower tax rates, but only in the *Information* treatment. This finding contrasts with the literature: Redistribution policies can indeed be conceptualized as an insurance device and, as such, be preferred by risk-averse individuals (see *e.g.*, [Durante et al., 2014](#)). However, in our design, the influence of risk-taking is uneasy to interpret because it likely also plays a role when participants form expectations about their ability to win the political competition.³¹ By contrast, we do not find that participants' gender, education, age and occupation correlate with the tax choice.

4.3 Politicians and non-politicians

In this section, we study the role of our variables of interest in the politician and non-politician samples taken separately.³² As shown in table 5, the results are qualitatively similar in the two samples. However, we observe a higher importance of the beliefs variables in the politician sample. For instance, the coefficient on the Belief about Median variable is 0.514 in column (1) (*No-Information* treatment) and 0.366 in column (3) (*Information* treatment) for politicians, whilst it is 0.390 in column (5) (*No-Information* treatment) and 0.266 in column (7) (*Information* treatment) for non-politicians. At the same time, the coefficient of the information about the median rate is lower for politicians. This suggests that politicians put less weight on the information provided and stick more to their personal beliefs. Politicians' higher confidence in their beliefs could explain such findings, but introducing the standard deviation of beliefs has virtually no effect on either the magnitude or the significance of the observed relationships (columns (2) and (4)). Furthermore, regarding our conjecture that political competition has a greater importance for politicians as compared to non-politicians, we find that the sign of the correlation between tax choices and either *Belief about Median* or *Information* is different between the two groups and lies in the expected direction (*i.e.*, politicians appear more reactive to citizens' preferences in a political competition).

To test for the statistical significance of the difference between politicians and non-politicians regarding

³⁰We also observe that higher-income participants believe that the citizens and the competitor selected a lower tax rate than lower-income participants (see table B.5.1).

³¹Additionally, as the risk-elicitation task systematically occurred before treatment, our measure of risk-taking also captures the effects of possible randomization failures, which further complicates the interpretation of the associated coefficient.

³²We run separate regressions to improve interpretability and comparability of the regression coefficients across samples. Alternatively, Table B.8.1 exposes the results from a full-sample regression model that includes interaction terms with the *Politician* dummy.

Table 5: Tobit regressions - Tax choice - Politicians and Non-politicians

	Politicians				Non-politicians			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dictator	2.055 (6.132)	1.934 (6.108)	0.757 (3.053)	1.112 (3.050)	-3.652 (6.492)	-4.361 (6.474)	0.691 (2.629)	0.667 (2.620)
Belief about Median	0.514*** (0.129)	0.507*** (0.126)	0.366*** (0.0846)	0.373*** (0.0842)	0.390*** (0.102)	0.386*** (0.101)	0.266*** (0.0639)	0.278*** (0.0643)
Dictator \times Belief about Median	-0.105 (0.152)	-0.0974 (0.154)			0.0842 (0.130)	0.0953 (0.129)		
Belief about Competitor	0.465*** (0.105)	0.461*** (0.105)	0.417*** (0.0508)	0.416*** (0.0509)	0.305*** (0.0775)	0.318*** (0.0774)	0.328*** (0.0463)	0.332*** (0.0459)
Sd Belief about Median		-0.180 (0.178)			-0.162 (0.111)		-0.262 (0.162)	-0.000278 (0.118)
Sd Belief about Competitor		0.341* (0.182)			0.124 (0.151)		0.136 (0.170)	-0.214** (0.100)
Information			0.159*** (0.0404)	0.161*** (0.0404)			0.230*** (0.0340)	0.224*** (0.0337)
Dictator \times Information			-0.0504 (0.0501)	-0.0507 (0.0502)			0.0153 (0.0462)	0.0139 (0.0460)
Constant	5.895 (8.513)	4.545 (8.694)	20.83* (11.38)	20.79* (11.05)	32.88*** (10.53)	36.48*** (12.11)	-21.06** (9.466)	-18.53** (9.438)
Treatment	No Info	No Info	Info	Info	No Info	No Info	Info	Info
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Pseudo R ²	0.0643	0.0655	0.0503	0.0507	0.0534	0.0543	0.0431	0.0438
Observations	475	475	3135	3135	367	367	3168	3168

Notes: Controls include: age, gender, income, education level, occupation, region of residence, political ideology, a risk aversion index, a dummy for whether the belief elicitation task has been played before or after the redistribution game, and the time taken for completing the survey. Robust standard errors in parentheses in column (1) and (3). Standard errors clustered at the individual level in parentheses in column (2) and (4). * significant at 10%, ** significant at 5%, *** significant at 1%.

the effects of beliefs, the level of information and the dictator treatment, we consider the pooled sample and include the interaction terms between the politician dummy and these variables in Table B.8.1. Although we observe effects of similar magnitude as in Table 5, we fail to reject the hypothesis of no difference between the two groups for all the effects. Therefore, we cannot draw a robust conclusion regarding the treatment differences between politicians and non-politicians.

4.4 Regression analysis on deviation from the median

In the previous section, we documented a strong and positive correlation between the chosen tax rate and the median rate, whether expressed as participants' beliefs or the information provided through the strategy method. However, this correlation is not perfect. Moreover, the *Competition* treatment adds strategic considerations that can naturally be expressed as deviations from the (expected) median rate. Intuitively, policy-makers condition their chosen rate on the competitors' choice: the closer the latter to the (expected) median rate, the closer the former has to be from the (expected) median rate in order to win the political competition. In other words, the likelihood to win the competition is both a decreasing function of the distance between the chosen rate and the (expected) median rate and an increasing function of the distance between the competitor's choice and the (expected) median rate. This

motivates a more comprehensive investigation into these deviations.

To do this, we create an additional dependent variable. In the *No Information* treatment, this new variable represents the absolute distance between the chosen tax rate and participants' beliefs regarding the median rate. In the *Information* case, it represents the absolute distance between the chosen tax rate and the information provided. We then run Tobit regressions with an upper censoring at 100%, that is, the maximum distance between the tax choice and the median rate.

Table 6 reports the results of this analysis. In the *No Information* treatment, a greater deviation of the competitor's expected behavior from the expected median rate is associated with a greater deviation of the policy-maker's chosen rate from the expected median rate. Similarly, in the *Information* treatment, a greater deviation of both the competitor's expected behavior and the expected median rate from the provided information is associated with a greater deviation of the chosen rate from the information. In other words, policy-makers deviate when the competitor is expected not to align with the information and when the expected median rate differs from the information. However, whereas we could have expected political competition to strengthen such relationships because it implies that policy-makers should pay more attention to both citizens' preferences (either as provided or in expectations) and the competitor's choice, we do not observe any of the interactions with the *Dictator* dummy to be significant. More specifically, we do observe some effect of the *Dictator* treatment, since participants in the *No Information* treatment deviate more on average from the expected median rate than in the *Competition* treatment when both beliefs are aligned. But we do not find that an expected deviation from the competitor shows a supplementary effect in the *Dictator* treatment, and we do not observe a significant effect of the *Dictator* dummy in the *Information* treatment (if anything the direction of the effect is even the opposite). Finally, we find that politicians deviate slightly more from the information than non-politicians in the *Information* treatment (significance at 10% level). Overall, our analysis on relative behavior (deviations) therefore leads to similar conclusions as our analysis on absolute behavior (tax choices).

4.5 Additional results

Heterogeneity analysis In section 4.2, we showed that participants' ideological orientation is the most significant control variable in our analysis. We therefore run the main regressions by dividing the sample into right-wing, centrist and left-wing participants to explore the heterogeneity of our variables effects with respect to political ideology (Table B.9.1). Two results can be stressed: First, in the *No Information* treatment, left-wing politicians choose a 9.5 p.p. higher tax rate than left-wing non-politicians (coefficient significant at the 5% level), while centrist and right-wing politicians choose a 6.7 p.p., respectively, 5.7 p.p., lower tax rate compared to non-politicians (coefficient significant at the 10% level). Consequently, the left/right political divide regarding redistribution choices appears more pronounced among politicians than among non-politicians. Second, right-wing participants exhibit a different pattern of behavior compared to centrist and left-wing participants. In particular, they do not respond significantly to the information about the median rate nor follow their beliefs about such rate when the information is provided. Also, when the information is not provided, they choose a lower tax rate in the *Dictator*

Table 6: Tobit regressions - Deviation from the Median

	Tax rate - Belief about median	Tax rate - Information
	(1)	(2)
Dictator	3.014** (1.370)	-2.071 (1.527)
Politician	-0.942 (1.214)	2.185* (1.163)
Belief about Median	-0.0102 (0.0385)	
Belief about Median - Belief about Competitor	0.381*** (0.0889)	-0.0526 (0.0449)
Belief about Median - Belief about Competitor × Dictator	-0.194 (0.127)	-0.0247 (0.0705)
Information		0.0155 (0.0168)
Info - Belief about Median		0.270*** (0.0445)
Info - Belief about Median × Dictator		0.101 (0.0669)
Info - Belief about Competitor		0.326*** (0.0389)
Info - Belief about Competitor × Dictator		-0.000569 (0.0594)
Constant	14.18*** (5.280)	3.360 (5.052)
Treatment	No Info	Info
Controls	✓	✓
Pseudo-R ²	0.0101	0.0380
Observations	842	6303

Notes: Controls include: age, gender, income, education level, occupation, region of residence, political ideology, a risk aversion index, a dummy for whether the belief elicitation task has been played before or after the redistribution game, and the time taken for completing the survey. Robust standard errors in parentheses in column (1). Standard errors clustered at the individual level in parentheses in column (2) * significant at 10%, ** significant at 5%, *** significant at 1%.

treatment (especially when they believe the median rate is low), and they follow more their beliefs about citizens' preferences in this treatment as compared to the *Competition* treatment. Although such findings are difficult to interpret, they seem to point toward an adverse reaction to our treatments among right-wing participants.

Politician sample In table B.8 in the appendix, we run the main regressions focusing on the politicians sample and introducing politicians-specific variables such as their current office (mayor, vice-mayor or councilor), the population of their municipality and their political experience (proxied by the number of former mandates). We find that none of these variables has a consistent and significant effect on politicians' choices throughout the table. However, we also observe that, in the *Information* treatment, mayors implement a higher tax rate than both vice-mayors and local councilors. Further, since beliefs are significantly correlated with participants' choices, we conduct a regression analysis in Table B.9.3 to

examine the relationship between politicians' beliefs and the same politician-specific variables. Our findings indicate that the current office and the number of former mandates do not correlate with politicians' beliefs. However, politicians elected in populous municipalities tend to believe that both the median rate and the competitor's tax choice are lower than politicians from less-populated municipalities.

Analyzing the effects of politicians' specific characteristics on observed behavior was not the primary objective of this study. However, we believe that future research on this particular point could provide interesting insights into political behavior, in line with the literature investigating how politicians' personal traits influence policy decisions (*e.g.* [Kuliomina, 2021](#)).

5 Robustness checks and limitations

We argued that there are many advantages to studying democratic representation with an economic experiment (improved control, qualitative data collection, financial incentives, reproducible context, etc.). Nonetheless, there are some limitations, both general to the experimental methodology and specific to our implementation. In this section, we lay the emphasis on some of the most prominent caveats and discuss them in relationship with some robustness checks that we could implement.

First, as emphasized in section 2, our samples of politicians and non-politicians differ along some dimensions from their respective general populations. We create post-stratification weights using the iterative proportional fitting (or raking) method ([Kolenikov, 2014](#)) to account for differences in gender, age, and geographical area of residence. This specification yields results similar to our main analyses (columns (1) and (2) in Table B.10.1).

Second, the effect of the *Competition* treatment may depend on the saliency of the monetary gains: If participants regard the amounts at stake as negligible, they may not be sufficiently motivated to consider political competition, hence a low treatment effect. Participants may indeed have chosen to be responsive to the (expected) median rate even when they are not incentivized to do so if they believe it is the socially acceptable response (*i.e.*, social desirability bias). Such an effect could also have been strengthened by the fact that the limited amount of information in the experiment may have signaled citizens' preferences as of particular importance (especially in the *Information* treatment because of the strategy method). To investigate this issue, we restrict our attention to the participants who choose to keep their payoff, which *a priori* entails a higher interest in the monetary gains (columns (3) and (4) in Table B.10.1). However, even in this case, the *Dictator* treatment does not have any effect on participants' decisions, and we find consistent results otherwise. Moreover, if a social desirability bias was a crucial confounder, we find it striking that the correlation between the tax rate and the information in the *Dictator* treatment is not even more important (or, conversely, that we still observe substantial deviations). Indeed, the number of participants that strictly stick to the median rate remains limited. Out of 573 participants in the *Information* treatment, only 14 (2.44%) participants always choose a tax rate equal to the information provided. Moreover, out of 6,303 tax choices made in the *Information* treatment, only 694 (11.01%) corresponded to the median rate information provided. When we exclude these observations in the regression analysis, our main results are unaltered (columns (5) and (6)).

Third, and more generally, the smallness of stakes may have laid participants to pay low attention to their answers, which could reduce the data quality. Such a concern may be even more significant for politicians who are used to manage important financial amounts. Besides, it is also possible that politicians did not truly believe that real amounts were at stakes, which could have decreased their attention to the task, especially as compared to a regular panelist sample used to receiving money (even though not necessarily in relation to their answers). Consequently, we re-run the main regressions by excluding those participants who are not consistent in the risk aversion task (7.92%), that is, participants who choose the lottery when the expected gain is low but switch to the fixed payment when the expected gain of the lottery increases, and those participants who took less than 10 minutes to complete the survey (24.31%). The results remain robust even with these more stringent sample selection criteria (columns (7) to (10) in Table B.10.1). Moreover, we do not observe that politicians are less consistent and more hasty than non-politicians: if anything, we observe the opposite.³³

Fourth, participants in our experiment do not react to the exact information about the median rate. Rather, they are given the opportunity to decide for all possible values of the median rate through the strategy method. In other words, policy-makers are asked to act “as if”. If politicians do value giving citizens what they truly want, such a feature may create a tension between the provided information and their priors about the true median rate, which could explain the importance of beliefs despite the presence of information. Additionally, the feature may also create a reliability issue with respect to the provided information: a really high or really low median rate may be dismissed by policy-makers because they believe it is unrealistic. Indeed, Figure 4 shows that decisions are more heterogeneous and deviations are higher when the information provided is at the extremes (median rate close to 0% and 100%). In addition, policy-makers could be skeptical on whether their competitor will choose such an extreme median rate. In order to corroborate this hypothesis, we run our main regression on the chosen rate for each median rate separately in Table B.10.3. We find that our results remain consistent throughout, although the coefficient in *Belief about Competitor* is strongest at the extremes and lowest at the mid-point. We also find that the constant term in the regression consistently increases with the information level. As a consequence, we do not find policy-makers’ behavior to be particularly more erratic for some information levels, while it seems to be consistent with a belief updating mechanism by which policy-makers trade-off their priors and the information they receive about it.

That being said, we cannot rule out that some of our results may be explained by some features of the experimental design. Although we took steps to mitigate them (e.g: emphasizing anonymity, stressing the importance of honest answers, controlling for order effects) and despite the above robustness checks, it is indeed possible that priming and experimenter-demand effects may play a role. In particular, the importance of participants’ beliefs that we observe may partly be explained by a mixture of these effects: The fact that we elicit (non-incentivized) beliefs may have led participants to rely more on them (for instance, because they thought it was the purpose of the experiment) or to gauge them against their

³³Only 49 politicians exhibited inconsistency in the risk aversion task, as opposed to 63 non-politicians (Chi-squared: $p = 0.028$). Politicians, on average, took 7.9 minutes more to complete the survey in the *No Information* treatment and 14.5 minutes more in the *Information* treatment (in both cases, t-test: $p < 0.01$).

decisions and vice-versa (for instance, to appear consistent throughout). Yet, we also note that there is no obvious reason why these effects should not apply equally to all treatments and groups, so that we do not expect them to affect the entirety of our results.

Finally, a word of caution should be cast regarding the generalizability of our results and their extrapolation to other contexts. Indeed, our findings were obtained from a very specific decision environment that purposely abstract from many contextual elements of actual policy decisions. In particular, we cannot directly compare our treatment varying the degree of political competition, which has little impact on the results, with the policies designed to improve political accountability in the field: Such policies may work precisely because of the political economy constraints from which this study abstracts. Furthermore, we should stress that our design does not *stricto sensu* implement an electoral competition, meaning that there is no vote happening. As a consequence, we do not strictly test the median citizen theorem according to which political candidate converge toward the median preference in the electorate because they are vote maximizers. Rather, we enforce the competition by tying the personal incentives to the median rate. Our results should be interpreted as informative about politicians' individual-level behavior in general in a revealed preference context, and especially how they react to both varying information about the preference of the people for whom the decide and varying political competition, as compared to non-politicians.

6 Conclusion

This paper adopts a complementary stance to the existing empirical literature on democratic representation by analyzing the behavior of 760 local French politicians in an anonymized and incentivized behavioral experiment. By abstracting from the many political economy constraints that compound the interpretation of actual policy decisions, we can directly observe how politicians react to citizens' preferences in a taxation-redistribution context. That is, we study *political representation*. Further, by exogeneously manipulating the information set and the degree of political competition, as well as measuring beliefs about citizens' preferences, we strengthen our understanding of the mechanisms at play. Additionally, by recruiting a sample of 655 French non-politicians, this study investigates *behavioral representation*, that is, whether politicians' decisions differ from non-politicians' decisions on the same issue.

Our findings highlight that policy-makers consider the preferences of the people for whom they decide. Specifically, when provided with the information about the median rate, they select a tax rate accordingly. Such a finding relates to [Butler and Nickerson \(2011\)](#) and [Liaqat \(2023\)](#)'s result that informing politicians about citizens' preferences in various domains lead to policy changes in the field. However, the correlation between the chosen rate and the information is not perfect, and we observe substantial deviations from the median rate. In other words, even in a highly stylized decision environment where the information is rendered particularly salient, policy-makers do not fully converge toward citizens' preferences. Importantly, such deviations appear mainly related to decisions-makers' beliefs about such preferences. In particular, the deviations from the information are significantly influenced by parti-

pants' priors about the median rate. Similarly, in the absence of information, such beliefs are highly correlated with the decision.

We also observe that modifying the degree of political competition bears no impact on the results: Competitors select tax rates similar to those chosen by dictators. This finding appears at odds with traditional political economy models, which often attribute the alignment between policy proposals and citizens' preferences to the personal interests of political candidates driven by electoral incentives. Combined with the fact that policy-makers in our experiment take into account citizens' preferences, this result suggests that they have an intrinsic preference for doing so. Alternatively, it is possible that increasing the degree of political competition results in the substitution of intrinsic with extrinsic motivations (motivational crowding-out) so that the incentives to implement citizens' preferences are unchanged across treatments. In either case, this result is important insofar as the literature has largely focused on improving politicians' accountability to mitigate government failures (see *e.g.*, [Besley and Burgess, 2002](#); [Olken, 2007](#); [Ferraz and Finan, 2011](#); [Fujiwara, 2015](#)), but far less on motivations-based interventions ([Mansuri et al., 2023](#)). More generally, and despite the increasing interest in behavioral insights to improve our knowledge of political behaviors and in the making of policies ([DellaVigna, 2009](#); [Wilson, 2011](#); [Chetty, 2015](#); [Schnellenbach and Schubert, 2015](#)), there are only few studies that consider politicians' through the lens of behavioral economics (such as biased, non-purely self-interested agents with distorted beliefs). We believe that our experiment testifies on the need for future research to fill this gap, while emphasizing that running large-scale behavioral experiments with such a specific public may prove challenging but can be ultimately achieved.

Regarding the comparison between politicians and non-politicians, we observe that, once differences in beliefs are taken into account, no behavioral differences are detected. This finding aligns with a recent meta-analysis that emphasizes that public-elite gaps in policy-making are likely much less pronounced than what is commonly believed by both the general public and political scientists ([Kertzer and Renshon, 2022](#)). As such, it represents an encouraging signal with respect to the generalizability of the empirical, and foremost experimental, results from non-politician studies on policy-related issues, as for instance the extensive literature on preferences for redistribution. Considering that running surveys or experiments with politicians is complicated and require a non-negligible amount of resources ([Vis and Stolwijk, 2021](#); [Kertzer and Renshon, 2022](#)), such a result is valuable.

However, we also observe that politicians in our experiment have different beliefs about citizens' preferences than non-politicians. Such a finding echoes recent research that showed that politicians and, in general, the "elite", have biased beliefs about citizens' preferences ([Pereira, 2021](#); [Liaqat, 2023](#)). In particular, our politician sample believe that the 9-citizen group demanded a lower tax rate on average than non-politicians, that translates into lower implemented redistribution. One candidate explanation is advanced by [Pereira \(2021\)](#), who argues that politicians' beliefs can be distorted by their higher exposure to conservative subconstituencies demanding more tax cuts. Such beliefs about citizens' general attitude toward taxation-redistribution may thus carry to the experiment. Relatedly, he also points out that politicians may be subject to social projection, that is, the projection of their own preferences on citizens. Considering that politicians are wealthier on average than non-politicians, they may personally prefer

lower tax rates and project this preference on citizens. Such an interpretation would lay the emphasis on the fact that politicians' personal characteristics may matter for enacted policies not only through their personal interest but also as a source of beliefs distortion regarding citizens' demands.³⁴ Yet another possible explanation for the difference in beliefs between politicians and non-politicians in our experiment may relate to the structure of the redistribution game. Considering that, under a strict selfishness assumption for citizens, the equilibrium median rate ranges from 0 to 20%, the observed difference in beliefs between politicians and non-politicians may be explained by either politicians being better at predicting the issue of the game (that is consistent with the higher cognitive abilities that has been observed among politicians in the literature, see *e.g.*, [Dal Bó et al., 2017](#)), or by politicians holding stronger beliefs that citizens are self-interested. In addition, we also find evidence in our experiment that politicians are more confident in their beliefs about citizens' preferences. Such a finding therefore strengthens the importance of taking politicians' beliefs into account in our understanding of policy-making and in the design of the incentives and institutions that structure political activities. From a research agenda perspective, improving our understanding of the drivers of belief formation among policy-makers and their impact on daily policies thus appears to be a fruitful line of inquiry.

Obviously, our design do not exhaust all the questions that could be addressed in a similar decision environment. For instance, an extension to our design could involve making politicians part of the income distribution in the experiment and having the chosen tax rate affect their payoff. Although present in our experiment, participants' self-interest is rather crude and does not involve the trade-off between material personal interest and other motives, such as fairness principles. Such an inquiry could be particularly interesting given that the main behavioral difference with non-politicians that we observe is the higher proportion of politicians giving up their payoff to the benefit of a charity (85.39% of politicians and only 15.11% of non-politicians). Consequently, there may be a difference in the sensitivity to financial interest between the two groups that could be further investigated. Moreover, future research could adopt a comparative perspective by recruiting different samples of politicians and analyze their behavior on similar issues. For instance, one could gather a sample of upper-tiers politicians and study whether they are more or less responsive to citizens' preferences as compared to local politicians, or whether they hold different beliefs about citizens' preferences. Relatedly, comparing politicians from different countries (including less democratic ones) using similar behavioral experiments would likely provide a finer description of the behavioral characteristics of leaders throughout the world. Both attempts could provide interesting insights regarding the impact of political careers and the political selection process on policy decisions.

³⁴However, when we control for individual wealth-related characteristics, such as personal income and education, as well as ideological preferences, the difference in beliefs between politicians and non-politicians persists. Consequently, social projection likely does not entirely explain our findings.

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Appendices

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A Experiment details

This section contains a French-to-English translation of the experimental instructions. We highlight changes from one treatment to another by bold characters at the beginning of a chunk of text. For instance, the mention [**Competition + No Information treatment**] indicates that the next chunk of text exposes the instructions for the combination of *Competition* and *No Information* treatments.

A.1 Introduction

This survey is part of a scientific research project aimed at gaining a better understanding of decision-making motives in certain economic contexts. It was developed by a team from the CREM laboratory (Centre de Recherche en Economie et en Management, CNRS, Université de Rennes I and Université de Caen Normandie).

The survey takes around 25 minutes to complete. If you complete the survey, a 50-centime donation will be made to a charity of your choice from a list presented to you at the end of the survey.

Please note: this survey is not compatible with the use of a cell phone. To complete the survey, you must use a standard-sized computer or tablet, otherwise some questions will be cut in half and it will not be possible to process the results.

The survey is divided into three parts. In the first 2 parts, you will make a series of decisions in the form of questions. Each of these two parts will begin with instructions to help you understand how to make your decisions. Then you will make your decisions. Finally, you will move on to the last part, which consists of a short questionnaire that closes the survey. Unless otherwise stated, the decisions you make in each section will generate a reward for you and possibly for other survey participants. This reward will be expressed in Survey Currency Units (SCUs). At the end of the survey, the rewards you earn in each section will add up to a total reward. When all the data has been collected, the computer will draw 1 out of 50 participants. All participants drawn will receive the total reward generated by their participation in the survey. Similarly, any rewards generated by the decisions of these randomly selected participants for other participants will be paid out. The conversion rate of SCUs into euros is as follows: 1 SCU = 0.5 euros. By participating, you have the opportunity to receive a reward in euros, the amount of which depends on your decisions and possibly on the decisions of other participants. All rewards will be paid out shortly after the end of data collection, but no later than December 30, 2020.

At the end of the survey, you will be asked to choose between two options regarding the reward you will receive if the computer selects you: you can choose to keep the reward or donate it in full to a charity of your choice from a list.

There is no right or wrong decision: You are free to decide as you wish. All we ask is that you read the instructions carefully and answer as truthfully as possible: Your reward at the end of the experiment, and that of other participants, depends on it!

In accordance with the French Data Protection Act (Loi Informatique et Libertés), all data collected is strictly anonymous and confidential. In particular, you will never know the identity or reward of other participants, and they will never know your identity or reward.

You can start the survey by clicking “Next”. Starting the survey indicates that you wish to volunteer to take part. You may stop responding at any time. However, for the purposes of the study, we will only be able to use fully completed questionnaires. What is more, if you don’t complete the survey, the above-mentioned 50 cents will not be donated to charity, and you forfeit any reward you may have received.

Thank you for your valuable participation!

A.2 Risk aversion task

In this task, you will make 5 decisions. Each decision consists in choosing one out of two options:

1. 100% chance of receiving 7 SCUs.
2. 50% chance of receiving 0 SCUs and 50% chance of receiving another amount of SCUs.

To determine your gratification for this part at the end of the survey, the computer will draw one of 5 decisions. If for this decision you chose option 1, you will receive 7 SCUs. If you chose option 2, the computer will draw randomly the amount that you will obtain: either 0 SCUs or the amount shown for this decision.

You can now make the 5 decisions below.

1. Please indicate if you wish to receive 7 SCUs with a probability of 100% or if you wish to have 50% chances to receive 0 and 50% to receive 12 SCUs.
2. Please indicate if you wish to receive 7 SCUs with a probability of 100% or if you wish to have 50% chances to receive 0 and 50% to receive 14 SCUs.
3. Please indicate if you wish to receive 7 SCUs with a probability of 100% or if you wish to have 50% chances to receive 0 and 50% to receive 16 SCUs.
4. Please indicate if you wish to receive 7 SCUs with a probability of 100% or if you wish to have 50% chances to receive 0 and 50% to receive 18 SCUs.
5. Please indicate if you wish to receive 7 SCUs with a probability of 100% or if you wish to have 50% chances to receive 0 and 50% to receive 20 SCUs.

A.3 Taxation-Redistribution task

During this game, you will make decisions about different people. You will never get to know the identity of these people. Similarly, they will never know your identity.

You will make decisions about a group of 9 people. These people have been selected within a sample of the French population by Panelabs, the leading provider of quantitative data for French researchers. These people are real and can be any resident of Metropolitan France. At the end of the survey and according to the decisions you will take, these 9 people will receive a certain reward expressed in euros. This reward will be paid to them by bank transfer at the end of the survey.

At the beginning of this game, a total of 473 SCUs will be distributed among these 9 people (identified by letters from A to I). Each of these individuals will therefore receive an initial amount in SCUs. The 9 initial amounts have been calculated to reflect the distribution of annual pre-tax income of French households (INSEE 2017 data).

Thus, the first decile indicates that 10% of French households earned less than 7,310 euros per year in 2017. The equivalent of this amount in the survey is 17 SCUs. The fifth decile indicates that half (50%) of French households earned less than 21,120 euros per year in 2017 and the other half of French households earned more than 21,120 euros per year in 2017. The equivalent of this sum in the survey is 49 SCUs. The ninth decile indicates that 90% of French people earned less than 42,370 euros per year in 2017. The equivalent of this sum in the survey is 100 SCUs.

The table below shows all the income deciles of French households and the possible initial amounts in SCUs.

Table A.3.1: French income distribution and correspondent distribution of initial endowments

Household income decile (euros)	7310	12,050	15,480	18,360	21,120	24,100	27,720	32,810	42,370
Initial possible endowments (SCU)	17	28	37	43	49	57	65	77	100

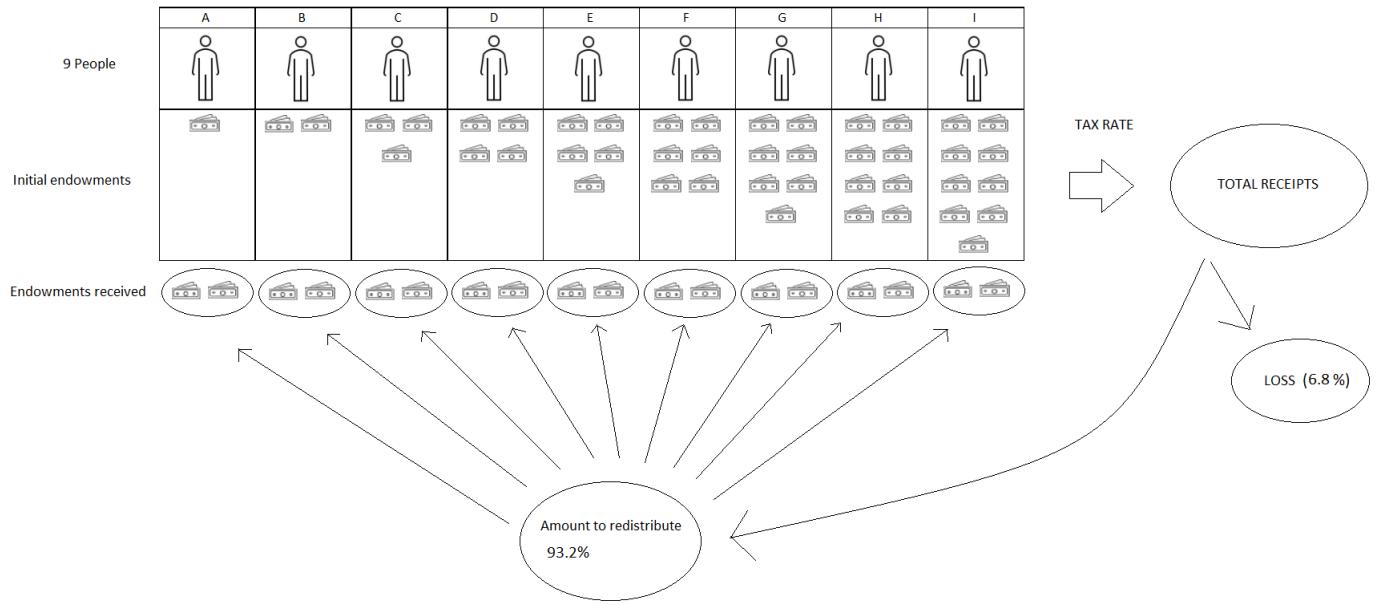
Each initial amount will be allocated to one person at random. Thus, one person will receive 17 SCUs, another will receive 28 SCUs, a third will receive 37 SCUs, etc.

You will be able to choose a tax rate expressed in percentages. The rate can range from 0% (no tax) to 100% (maximum tax).

- The rate chosen will be applied to the initial amount for each of the 9 people. A portion of each initial amount will be deducted. Each person will keep the amount not deducted;
- The amounts deducted will add up to a total amount;
- Then, a part of this total amount will be removed and will not be given to anyone. This part is fixed and amounts to 6.8% of the total amount;
- The rest of the total amount (93.2%) will then be divided equally among the 9 people.

Here is a schematic representation of the decision:

Figure A.3.1: Schema for comprehension of main task



The table below shows the amounts obtained by each of the 9 individuals for several possible rates. The first line corresponds to the initial situation. This situation is therefore identical to the final situation with a rate of 0% (second line). The final amounts obtained for different rates are presented in the following rows. The last column of the table shows the sum of the amounts. Thus, for a rate of 10% this corresponds to $20.2 + 30.1 + 38.2 + 43.6 + 49 + 56.2 + 63.4 + 74.2 + 94.9 = 469.8$ SCUs.

Figure A.3.2: Payoff table

	A	B	C	D	E	F	G	H	I	Total
Montant initial	17	28	37	43	49	57	65	77	100	473
Taux = 0%	17	28	37	43	49	57	65	77	100	473
Taux=10%	20.2	30.1	38.2	43.6	49	56.2	63.4	74.2	94.9	469.8
Taux=20%	23.4	32.2	39.4	44.2	49	55.4	61.8	71.4	89.8	466.6
Taux=30%	26.59	34.29	40.59	44.79	48.99	54.59	60.19	68.59	84.69	463.31
Taux=40%	29.79	36.39	41.79	45.39	48.99	53.79	58.59	65.79	79.59	460.11
Taux=50%	32.99	38.49	42.99	45.99	48.99	52.99	56.99	62.99	74.49	456.91
Taux=60%	36.19	40.59	44.19	46.59	48.99	52.19	55.39	60.19	69.39	453.71
Taux=70%	39.39	42.69	45.39	47.19	48.99	51.39	53.79	57.39	64.29	450.51
Taux=80%	42.59	44.79	46.59	47.79	48.99	50.59	52.19	54.59	59.19	447.31
Taux=90%	45.78	46.88	47.78	48.38	48.98	49.78	50.58	51.78	54.08	444.02
Taux=100%	48.98	48.98	48.98	48.98	48.98	48.98	48.98	48.98	48.98	440.82

To select a rate, position a cursor along a straight line from 0% to 100%. When you first position the cursor, a table will appear. This table will show the initial amount (first line) and the final amount (second line) for each of the 9 people. The totals for each line will be shown in the last column of the table.

Here's an example of a slider. You can now try out different configurations.

Please note: you don't have to decide anything right now; this is just a trial run to familiarize yourself with the decision process.

Figure A.3.3: Slider



In addition, each of the 9 people was asked what rate they would like to see implemented. Each of the 9 people then positioned a slider to reflect this wish. Unlike the slider shown above, the slider presented to them ranged in 10 p.p. increments. Each person had the choice of 0%, 10%, 20%, etc. up to 100%.

The computer then calculated the “median wish”, *i.e.* the rate at which half the people wanted a higher or equal rate, and the other half wanted a lower or equal rate.

[*Dictator + No Information* treatment] You will not be informed of this median wish.

Finally, you will be randomly grouped with another participant in this experiment. This participant will also make the same type of decisions as you for the same 9 people. This participant is not one of these 9 people. At the end of the game, the computer will randomly select one of the two participants (you or the other participant). It will then apply the drawn participant's decision and award him 33 SCUs. The participant who is not drawn will receive 0 SCUs and his decision will not be applied.

Example: if you are drawn, you will receive 33 SCUs, the other participant will receive 0 SCUs and the computer will apply the decision you have made. Each of the 9 people will therefore receive the final amount according to your decision. Conversely, if the other participant is drawn at random, you will receive 0 SCUs, the other participant will receive 33 SCUs and the computer will apply the decision the other participant has made. Each of the 9 people will therefore receive the final amount according to the other participant's decision.

[*Competition + No Information* treatment] You will not be informed of this median wish.

Finally, you will be randomly grouped with another participant in this experiment. This participant will also make the same type of decisions as you for the same 9 people. This participant is not one of these 9 people. At the end of this part, the computer will look at the median wish of the 9 people. Then, it will apply the decision of the participant who chose the rate closest to this median wish, and award 33 SCUs to this participant. The other participant will receive 0 SCUs and his decision will not be applied.

Example: if the rate you have chosen is closer to the median wish than the rate chosen by the other participant, you will receive 33 SCUs, the other participant will receive 0 SCUs and the computer will

apply the decision you have made. Each of the 9 people will therefore receive the final amount according to your decision. Conversely, if the rate chosen by the other participant is closer to the median wish than the rate you have chosen, you will receive 0 SCUs, the other participant will receive 33 SCUs and the computer will apply the decision the other participant has made. Each of the 9 people will therefore receive the final amount according to the other participant's decision.

Note: if the rate you choose and the rate chosen by the other participant are identical, the computer will draw one of the two participants (you or the other participant) at random. It will then apply the drawn participant's decision and award him or her 33 SCUs. The participant who is not drawn will receive 0 SCUs and his decision will not be applied.

[Dictator + Information treatment] You will not be informed of this median wish. However, you will be asked to enter a rate for the 11 possible median wishes, from 0% to 100%. For each possible wish, you will have to position a cursor to choose a rate to apply in this situation. There will therefore be 11 decisions, each with a heading like "If the median wish is ...%, please choose a rate".

Finally, you will be randomly grouped with another participant in this experiment. This participant will also make the same type of decisions as you. At the end of this part, the computer will draw one of the two participants (you or the other participant) at random. Then, it will look at the median wish of the 9 people. For this median wish, it will apply the decision of the drawn participant and award him 33 SCUs. The participant who is not drawn will receive 0 SCUs and none of his decisions will be applied.

Example: Let us imagine that the median wish of the 9 people is 50%. If you are drawn, you will receive 33 SCUs, the other participant will receive 0 SCUs and the computer will apply the decision you made under the heading "If the median wish is 50%". Each of the 9 people will therefore receive the final amount according to your decision. Conversely, if the other participant is drawn at random, you will receive 0 SCUs, the other participant will receive 33 SCUs and the computer will apply the decision the other participant made under the heading "If the median wish is 50%". Each of the 9 people will therefore receive the final amount according to the other participant's decision.

[Competition + Information treatment] You will not be informed of this median wish. However, you will be asked to enter a rate for the 11 possible median wishes, from 0% to 100%. For each possible wish, you will have to position a cursor to choose a rate to apply in this situation. There will therefore be 11 decisions, each with a heading like "If the median wish is ...%, please choose a rate".

Finally, you will be randomly grouped with another participant in this experiment. This participant will also make the same type of decisions as you for the same 9 people. This participant is not one of these 9 people. At the end of this part, the computer will look at the median wish of the 9 people. Then, it will apply the decision of the participant who chose the rate closest to this median wish, and award 33 SCUs to this participant. The other participant will receive 0 SCUs and his decision will not be applied.

Example: if the rate you have chosen is closer to the median wish than the rate chosen by the other participant, you will receive 33 SCUs, the other participant will receive 0 SCUs and the computer will apply the decision you have made. Each of the 9 people will therefore receive the final amount according

to your decision. Conversely, if the rate chosen by the other participant is closer to the median wish than the rate you have chosen, you will receive 0 SCUs, the other participant will receive 33 SCUs and the computer will apply the decision the other participant has made. Each of the 9 people will therefore receive the final amount according to the other participant's decision.

Note: if the rate you choose and the rate chosen by the other participant are identical, the computer will draw one of the two participants (you or the other participant) at random. It will then apply the drawn participant's decision and award him or her 33 SCUs. The participant who is not drawn will receive 0 SCUs and his decision will not be applied.

A.4 Belief elicitation task

[Before the redistribution task] Before making your decisions, you will now be asked to assess the likelihood of a series of events.

[After the redistribution task] You will now be asked to assess the likelihood of a series of events.

You will receive 10 tokens. You will have to distribute all 10 tokens between the different possible events. Each token represents 1 chance in 10 (a probability of 10%). If you place a lot of tokens on an event, it means you think it is very likely that this event is true. If, on the other hand, you place few tokens on an event, it means you think it is unlikely that this event is true. Finally, if you place half your tokens on an event, it means that you think there's a 1-in-2 chance of that event being true.

Dividing up these tokens will not result in a reward. The aim is simply to indicate what you think. We do, however, ask you to be as precise as possible when placing your tokens.

[Before the redistribution task] In Part II, you will be deciding on a rate that will affect the amount obtained by a group of 9 people.

[After the redistribution task] In Part II, you decided on a rate affecting the amount obtained by a group of 9 people.

Each of the 9 people has been asked individually what rate they would like to see implemented. Each of the 9 people has therefore positioned a slider similar to yours to reflect this wish. Unlike your slider, the slider presented to them went from 10 to 10, so each person had the choice between 0%, 10%, 20%, etc. up to 100%. The computer then calculated the “median wish”, *i.e.*, the rate such that half the people wanted a higher or equal rate and the other half wanted a lower or equal rate.

The events you are now going to evaluate as more or less probable concern this “median wish” and will be of the form: “According to you, the median wish is ...%”.

Example: if you place all your 10 tokens on the event “According to you, the median wish is 50%”, this means that you are certain that the median wish is 50%. On the other hand, if you place none of your 10 tokens on the event “According to you, the median wish is 50%”, this means that you are certain that the median wish is not 50%. Finally, if you place 5 tokens on the event “According to you, the median wish is 50%”, it means you think there's as much chance of the median wish being 50% as there is of it not being 50%.

[Before the redistribution task - *No Information* treatment] During Part II, you will also be grouped with another (anonymous) participant who will make the same type of decisions as you. This participant will choose a rate from 0 to 100%.

Figure A.4.1: Example of belief elicitation task - Median citizen

	0%	10%	20%	30%	40%	50%	60%	70%	80%	90%	100%
Selon vous, le souhait médian est de:	<input type="text"/>										
Jetons	<input type="text"/>										

! Restant = 10

[Before the redistribution task - *Information treatment*] During Part II, you will also be grouped with another (anonymous) participant who will make the same type of decisions as you. This participant will choose several rates from 0% to 100%, one for each of the 11 possible median wishes.

[After the redistribution task - *No Information treatment*] In Part II, you were also grouped with another (anonymous) participant who made the same type of decision as you. This participant chose a rate from 0 to 100%.

[After the redistribution task - *Information treatment*] In Part II, you were also grouped with another (anonymous) participant who made the same type of decision as you. This participant chose several rates from 0% to 100%, one for each of the 11 possible median wishes.

The events whose probability you are now going to evaluate concern this participant's decisions. Each event is of the following form: "The other participant's rate is between ...% and ...%". For example, if you place all your 10 tokens on the event "The other participant's rate is between 46% and 54%", this means you think it is certain that the other participant will choose a rate between 46% and 54%. On the contrary, if you place none of your 10 tokens on the event "The other participant's rate is between 46% and 54%", this means you think it is certain that the other participant will not choose a rate between 46% and 54%. Finally, if you place 5 tokens on the event "The other participant's rate is between 46% and 54%", this means that you think there is as much chance that the other participant will choose a rate between 46% and 54% as there is that he will choose a rate lower than 45% or higher than 55%.

[*No Information treatment*] Now divide your 10 tokens between the various possible events.

[Before the redistribution task - *Information treatment*] You will distribute a total of 10 chips 11 times. The other participant will make 11 decisions, one for each possible median wish. You are therefore asked to assess the likelihood of the same events occurring in the other participant's decisions for each of the possible median wishes. So, for example, you will have to determine how many tokens you will place on the event "In your opinion, the other participant's rate is between 46% and 54%" if the median wish is 0%, if it is 10%, if it is 20%, and so on. Note: for each median wish, you will have 10 tokens to divide between the different events. Each time, you must allocate all 10 tokens.

[After the redistribution task - *Information treatment*] You will distribute a total of 10 chips 11 times. The other participant made 11 decisions, one for each possible median wish. You are therefore asked to assess the likelihood of the same events occurring in the other participant's decisions for each of the possible median wishes. So, for example, you will have to determine how many tokens you will place on the event "In your opinion, the other participant's rate is between 46% and 54%" if the median wish is 0%, if it is 10%, if it is 20%, and so on. Note: for each median wish, you will have 10 tokens to divide between the different events. Each time, you must allocate all 10 tokens.

Now divide your 10 tokens between the various possible events.

Figure A.4.2: Example of belief elicitation task - Competitor

Selon vous, le taux de l'autre participant se situe:											
Restant = 10											
	Entre 0% et 5%	Entre 6% et 15%	Entre 16% et 25%	Entre 26% et 35%	Entre 36% et 45%	Entre 46% et 54%	Entre 55% et 64%	Entre 65% et 74%	Entre 75% et 84%	Entre 85% et 94%	Entre 95% et 100%
Jetons	
	

A.5 Voting decision [only non-politicians]

To complete Part II, you will now take the position of one of the 9 people. You will be grouped with 8 other people from the French population taking part in this survey. The computer will assign each person an initial amount. Then, you will choose the rate you would like to see implemented. The other 8 people will do the same. The computer will then calculate the "median wish", *i.e.*, the rate at which half the people (including you) would like to see a higher or equal rate, and the other half (including you) would like to see a lower or equal rate.

Finally, 2 other participants in this survey will be selected to decide for your group of 9 people. These 2 participants will not be informed of the median wish.

[**Dictator treatment**] The computer will select at random which of the two participants' decisions will be implemented.

[**Competition treatment**] The computer will apply the decision of the participant who chose the rate closest to the median wish.

Please note: these 2 participants are not part of the 9 people. Consequently, the decision you made earlier in Part II cannot count for your group of 9 (and therefore for you). On the other hand, it can count for another group of 9 people. The same applies to each of the participants in the survey: the decision taken by each of them does not apply to their group of 9 people, but to another group of 9 people.

To calculate your reward at the end of the experiment for this part II, the computer will conduct a random drawing. Either you will be one of the 2 participants deciding for a group of 9 people, or you will be part of a group of 9 people. In the first case, your previous decision can count toward your reward and that of the 9 people involved. In the second case, two other participants will decide for your group of 9, and your reward will be determined by their decisions.

Your initial amount is 37 SCUs. You are therefore person C in the table above.

Please choose the rate you would like to see applied (the amounts that will appear in the table correspond to what would happen if the rate applied were equal to your wish): *Slider between 0% and 100%, similar to Figure A.3.3.*

A.6 Exit survey

Answer options are in *italic*, separated by a semicolon.

- You answered this survey *At your home; At work; On public transportation; In a café, library or other public space; Other*
- Did other people help you to answer this survey? *Yes; No*
- Did you answer on *Your laptop; A tablet; Other*
- Are you *A man; A woman; Other*
- What is your year of birth?
- Which département do you live in?
- What is your highest level of education? *CAP/BEP; High school degree; Higher education degree; No qualification; Other*
- [If **High school diploma** is chosen in the previous question] What High school degree was it? *General, Technological; Professional.* If higher education degree: Was it a: *1-year college or equivalent; 2-year college or equivalent; 3-year college or equivalent; 1-year Master degree or equivalent; 2-year master or equivalent; Ph.D or equivalent; Other*
- In which field did you mainly study? *Art; Biology/Geology; Chemistry; Law; Geography; History; Informatics; Engineering; Languages; Literature; Mathematics; Medicine; Physics; Economics; Political science; Sociology; Other*
- To which socio-professional category do you belong? *Farmers; Craftsmen, shopkeepers and company managers; Executives and higher intellectual professions; Intermediate professions; Employees; Manual workers; Retired; Other people not in employment; Do not wish to answer*
- In which gross annual income bracket do you fall? *Less than 10k; Between 10k and 20k; Between 20k and 30k; Between 30k and 40k; Between 40k and 50k; Between 50k and 60k; Between 60k and 70k; Between 70k and 80k; Between 80k and 90k; Between 90k and 100k; More than 100k*

- Without thinking only of the elections, please indicate which political party you feel closest to, or least distant from: *Debout la France; Europe Écologie - Les Verts (EELV); La France Insoumise (LFI); La République En Marche (LREM); Le MoDem; Les Républicains (LR); Lutte Ouvrière (LO) ou Nouveau Parti Anticapitaliste (NPA); Nouvelle Donne (ND); Parti Communiste (PC); Parti Socialiste (PS); Rassemblement National (RN); Union des Démocrates et des Indépendants (UDI); Aucun parti; Other*
- In politics, people talk about the left and the right. On a scale of 0 to 10, where would you rank yourself? *0-10; Does not wish answer*
- And on the same 0 to 10 scale, how would you rank the following parties? La France Insoumise (LFI), Parti Socialiste (PS), La République En Marche (LREM), Les Républicains (LR), Rassemblement National (RN) *0-10; Does not wish answer*
- Once all the data has been collected, 1 in 50 participants will be selected by the computer to receive the reward generated during the survey. If you are selected, please indicate whether you would like to keep the reward for yourself or donate it in full to charity: *Keep the reward; Donate the reward to a charity*
- [If *Donate the reward to a charity* is chosen in the previous question]** Please select the association to which you would like to donate your gratification. This association will also receive an additional 50 cents as a thank you for your participation in this survey. *Institut Pasteur; Restos du Coeur; Médecins sans Frontières*
- [If *Keep the reward* is chosen in the previous question]** Please choose the association that will receive 50 cents to thank you for your participation in this survey.

For politicians only:

- The population of your municipality is: *Less than 1,000 inhabitants; Between 1,000 and 3,500 inhabitants; More than 3,500 inhabitants*
- At the local level, this mandate is your: *First; Second; Third; Fourth; Fifth; Sixth; Seventh or more; Not concerned*
- You are: *Mayor; Vice-Mayor; Town councilor*
- In addition, for the period 2020-2026, are you: *Intermunicipal councilor; Department's councilor; Region councilor; Member of Parliament (Deputy or Senator)*
- Previously, have you been: *Mayor; Vice-Mayor; Town councilor; Intermunicipal councilor; Department's councilor; Region's councilor; Member of Parliament (Deputy or Senator)*
- In total how many mandates did you have as Mayor, Deputy Mayor etc. (depending on the answers to previous questions)

A.7 Details on politicians recruitment

First, we sent official invitations by mail to French associations of local politicians. Our primary target was the Association des Maires de France (AMF), which is the most prominent French national association of mayors. The AMF is subdivided into several sections, also referred to as department associations.³⁵ To gain the consent of an AMF section for sharing a link to our experiment, we employed a two-stage procedure. Initially, we contacted each section via phone to introduce ourselves and obtain the contact email of its president or president's secretary. Subsequently, we reached out to the section's president through email, outlining the project's purpose without revealing the exact experiment details, and requested consent.

Following the follow-up approach recommended by Vis and Stolwijk (2021) for elite studies, we regularly repeated either of these steps for two months to minimize attrition. Of the 89 AMF sections (out of 93) that responded positively to the initial call, 51 replied to our email, and 32 ultimately granted consent. Thereafter, the experiment was disseminated in various forms depending on the section, such as through mailing lists, newsletters, etc. Some sections even directly provided us with email addresses of city halls or politicians for our use. To ensure a more comprehensive representation, we also contacted 9 other national associations, of which 4 eventually granted permission. The list of the national associations we contacted is detailed in Table A.7.1 while the map A.7.1 presents the AMF department sections that participated in the invitation-forward.

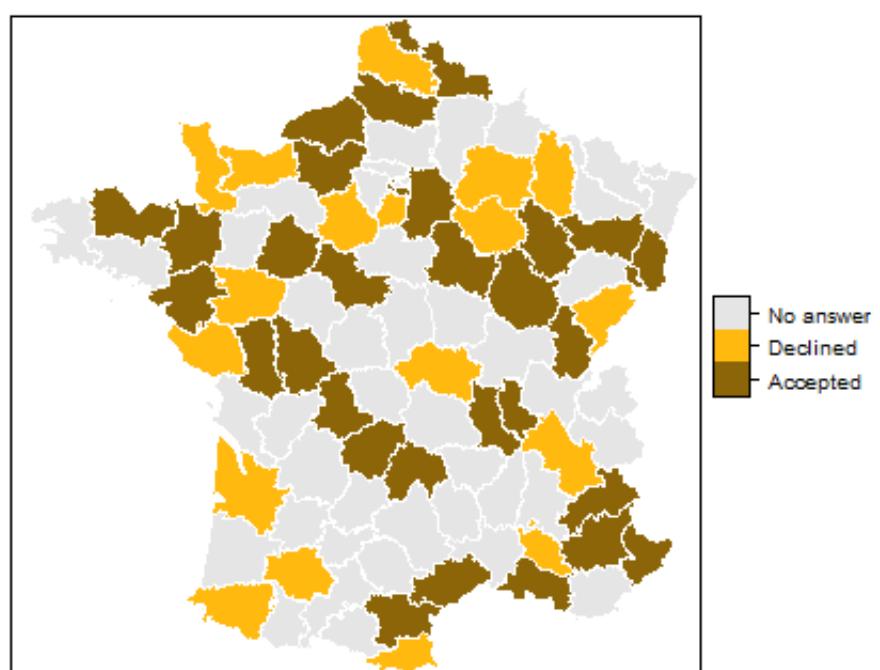
Second, we sent mails with the invitations directly to French town halls. The mails of all municipalities were collected by web-scraping using Python from a public service directory (<https://lannuaire.service-public.fr/navigation/mairie>).

Table A.7.1: List of national associations of local politicians

Association	Accepted
Association des Maires Ruraux de France	Yes
Villes de France	No
Fédération Nationale des Communes Forestières	No
Association Nationale des Elus du Littoral	Yes
Association Nationale des Elus des Territoires Touristiques	Yes
Association Nationale des Elus de la Montagne	No
Association des Petites Villes de France	Yes
Intercommunalités de France	No
France Urbaine	No

³⁵The list of all department associations can be found on <https://www.amf.asso.fr/m/page/ad.php>.

Figure A.7.1: Responses from AMF department sections



B Additional figures and tables

B.1 Geographical areas and ideological classification of political parties

Table B.1.1: Geographical areas and French regions

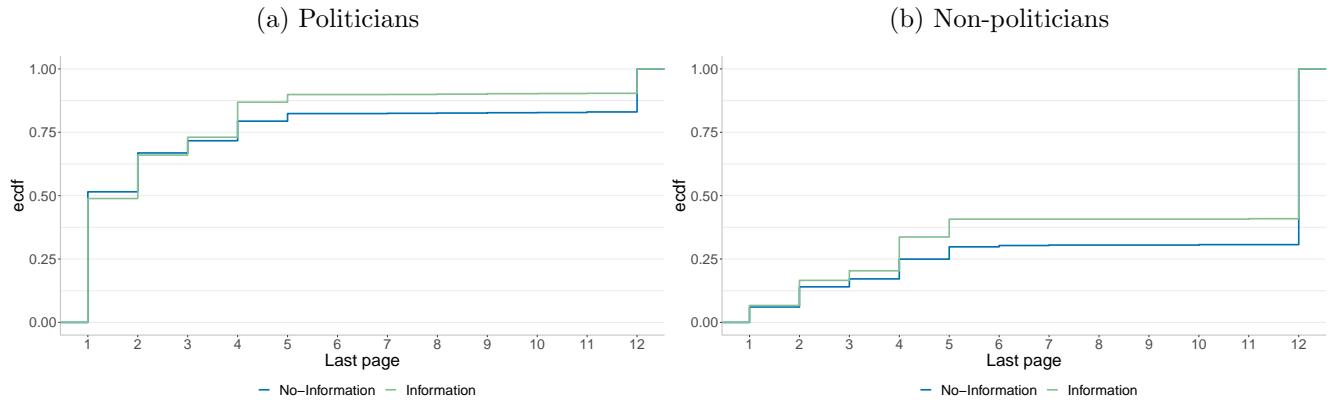
Geographical Area	Regions
North	<i>Hauts-de-France, Île-de-France, Normandie</i>
East	<i>Grand Est, Bourgogne-Franche-Comté</i>
West	<i>Bretagne, Centre-Val de Loire, Nouvelle-Aquitaine, Pays de la Loire</i>
South	<i>Provence-Alpes-Côte d'Azur, Occitanie, Auvergne-Rhône-Alpes</i>

Table B.1.2: Political parties on the left, center and right

Ideology	Political party
Left	<i>Parti Communiste, La France Insoumise, Lutte Ouvrière/Nouveau Parti Anticapitaliste, Parti Socialiste, Europe-Ecologie les Verts, Nouvelle Donne, Générations Parti Radical de Gauche/Mouvement Radical Social-Libéral, Place Publique</i>
Center	<i>La République en Marche, Mouvement démocrate</i>
Right	<i>Les Républicains, Union des démocrates et indépendants, Rassemblement National, Action Française, Debout la France</i>

B.2 Attrition analysis

Figure B.2.1: Cumulative distribution - Last page accessed



Notes: Page 4 and 5 correspond to the tax choices or the belief elicitation task, depending on the randomization of the tasks order.

B.3 Summary statistics

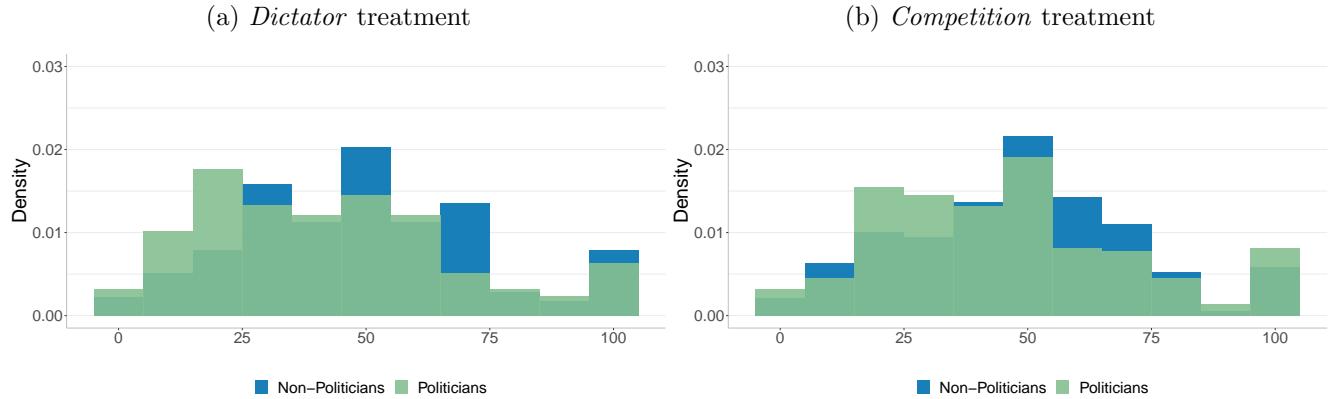
Table B.3.1: Summary statistics by treatment

	No Information		Information	
	Competition	Dictator	Competition	Dictator
<i>Politician</i>	0.54	0.59	0.50	0.50
<i>Age</i>	50.27	50.10	49.90	50.02
<i>Gender</i>				
Male	0.51	0.53	0.51	0.49
Female	0.49	0.47	0.49	0.51
<i>Education</i>				
High school diploma	0.41	0.42	0.42	0.43
Higher education	0.59	0.58	0.58	0.57
<i>Occupation</i>				
Farmers	0.01	0.05	0.04	0.01
Craftmen, Retailers and entrepreneurs	0.06	0.08	0.07	0.05
Inactive	0.06	0.07	0.06	0.08
Executives	0.25	0.21	0.21	0.22
Employees	0.22	0.19	0.22	0.21
Laborers	0.04	0.04	0.03	0.04
Retired	0.20	0.23	0.23	0.24
Intermediate professions	0.14	0.12	0.12	0.14
<i>Gross yearly personal income</i>				
Less than 20k	0.21	0.25	0.21	0.19
Between 20k and 30k	0.30	0.26	0.28	0.26
Between 30k and 40k	0.19	0.17	0.18	0.23
More than 40k	0.23	0.25	0.26	0.25
<i>Geographical area</i>				
North	0.26	0.19	0.26	0.26
West	0.27	0.34	0.32	0.32
East	0.13	0.13	0.11	0.12
South	0.34	0.34	0.31	0.31
<i>Party Preferences</i>				
Left	0.30	0.33	0.33	0.29
Center	0.17	0.15	0.16	0.14
Right	0.20	0.19	0.16	0.22
None	0.33	0.32	0.35	0.36
<i>Ideology</i>	6.67	6.36	6.81	7.04
<i>Risk aversion index</i>	8.12	9.17	8.73	8.60

Notes: For the correspondence between French regions and geographical areas, see table B.1.1. For the classification of party preferences, see table B.1.2 in the appendix. “Ideology” represents a numerical variable between 0 (extreme left) and 10 (extreme right) based on a likert scale question.

B.4 Distribution decisions

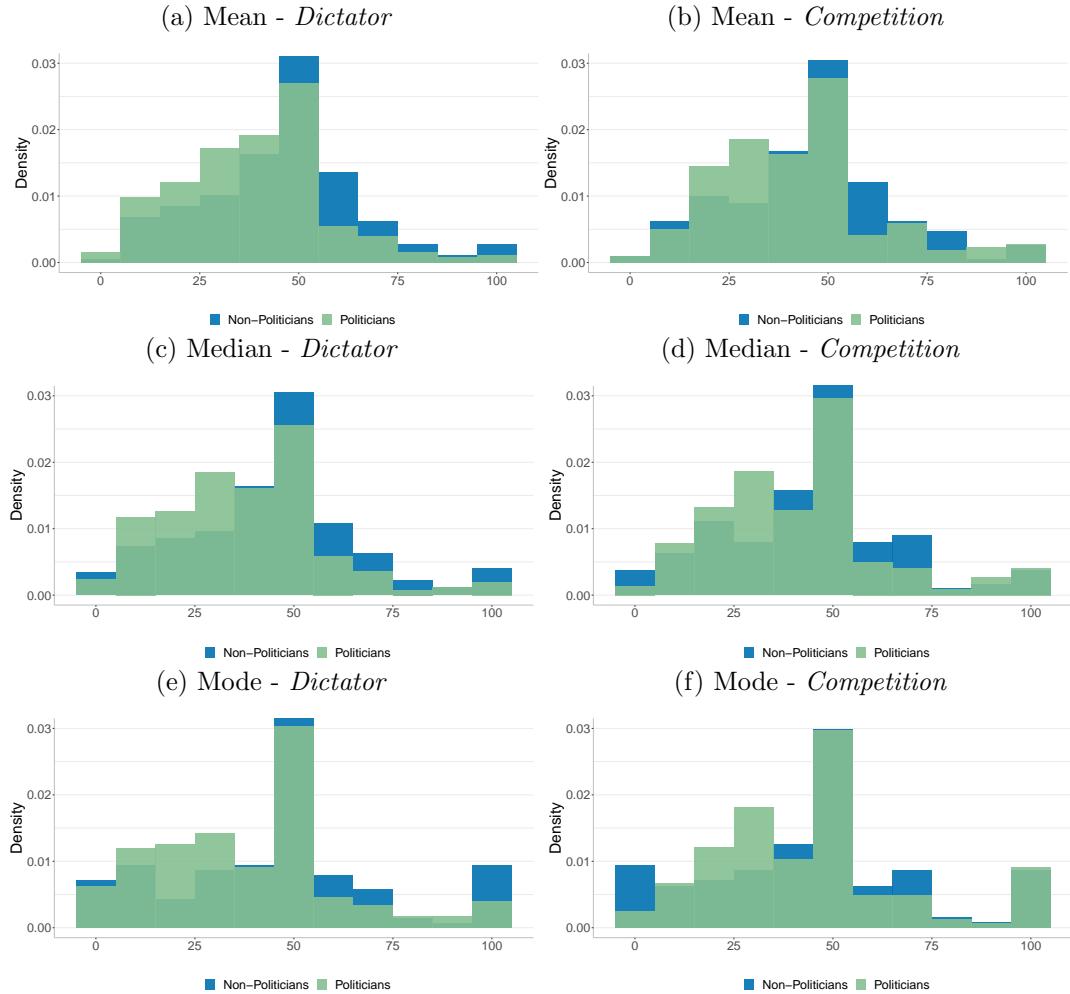
Figure B.4.1: Tax choice distribution - *No Information* treatment



Note: The Kolmogorov-Smirnov test for differences in distributions between politicians' and non-politicians' tax choices reports a $p < 0.01$ in the *Dictator* treatment and a $p = 0.2108$ in the *Competition* treatment. The same test for differences in distributions between the *Dictator* and *Competition* treatments reports a $p = 0.3098$ for politicians and $p = 0.9474$ for non-politicians.

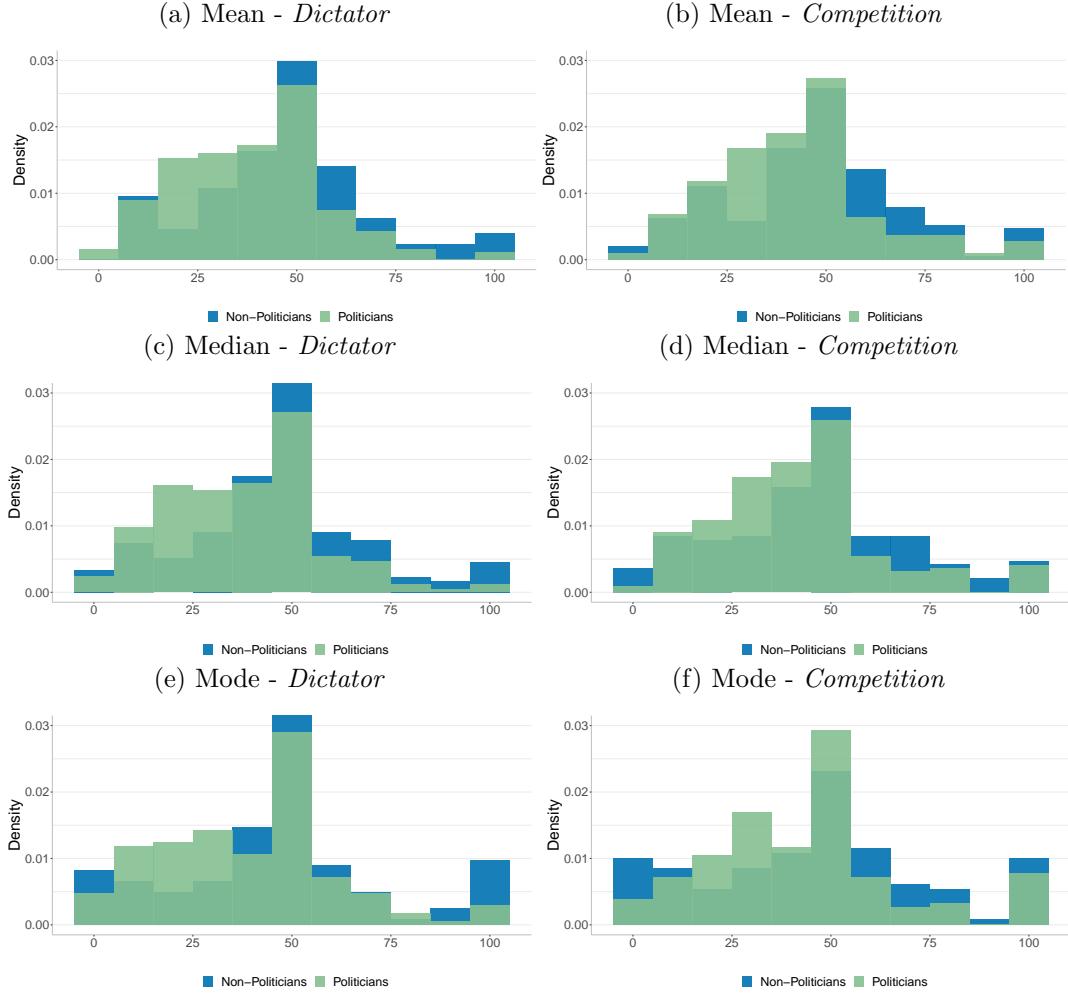
B.5 Beliefs

Figure B.5.1: Beliefs about the median rate - *No Information* treatment



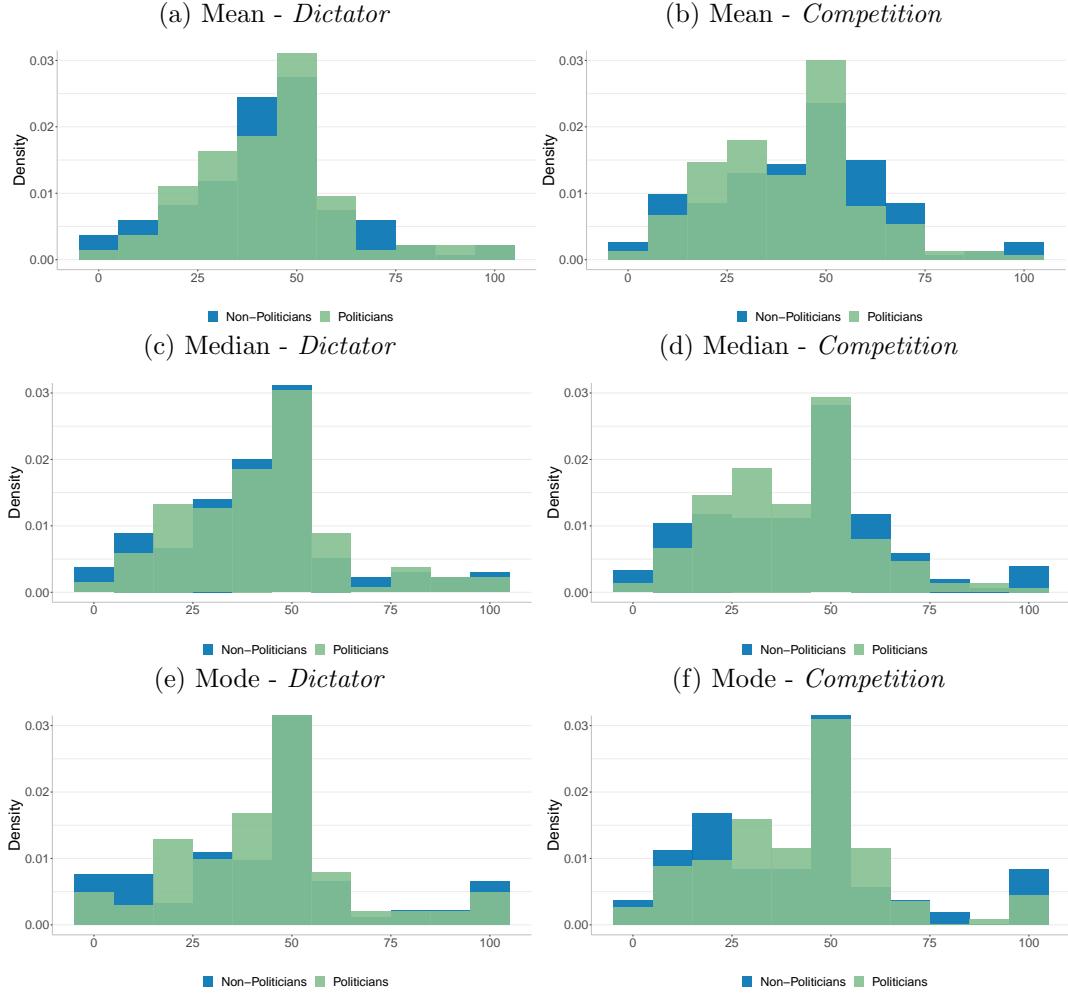
Note: This graph reports the distribution of the mean ((a) and (b)), the median ((c) and (d)), and the mode ((e) and (f)) of participants' subjective probability distribution about the median rate in the *No Information* treatment. The Kolmogorov-Smirnov test for differences in distributions of politicians' and non-politicians' beliefs reports a $p < 0.01$ in figures (a), (b), (c). For (d), the same test reports a $p = 0.07728$, for (e) a $p = 0.04505$, for (f) a $p = 0.7619$. In all cases, the difference in distributions between the *Dictator* and the *Competition* treatment is not significant ($p > 0.1$).

Figure B.5.2: Beliefs about the competitor - *No Information* treatment



Note: This graph reports the distribution of the mean ((a) and (b)), the median ((c) and (d)), and the mode ((e) and (f)) of participants' subjective probability distribution about the competitor's choice in the *No Information* treatment. The Kolmogorov-Smirnov test for differences in distributions between politicians' and non-politicians' beliefs reports a $p < 0.01$ in case (a), (b), (c). For (d), the same test reports a $p = 0.02994$, for (e) a $p = 0.045$, for (f) a $p = 0.18$. In all cases, the difference in distributions between the *Dictator* and the *Competition* treatment is not significant ($p > 0.1$).

Figure B.5.3: Beliefs about the median rate - *Information* treatment



Note: This graph reports the distribution of the mean ((a) and (b)), the median ((c) and (d)), and the mode ((e) and (f)) of participants' subjective probability distribution about the median rate in the *Information* treatment. The Kolmogorov-Smirnov test for differences in distributions between politicians' and non-politicians' beliefs reports a $p < 0.01$ in case (a), (b), (d), (e) and (f). For (c), the same test reports a $p = 0.0369$. In all cases, the difference in distributions between the *Dictator* and the *Competition* treatment is significant ($p < 0.01$).

Figure B.5.4: Beliefs about the competitor - *Information* treatment

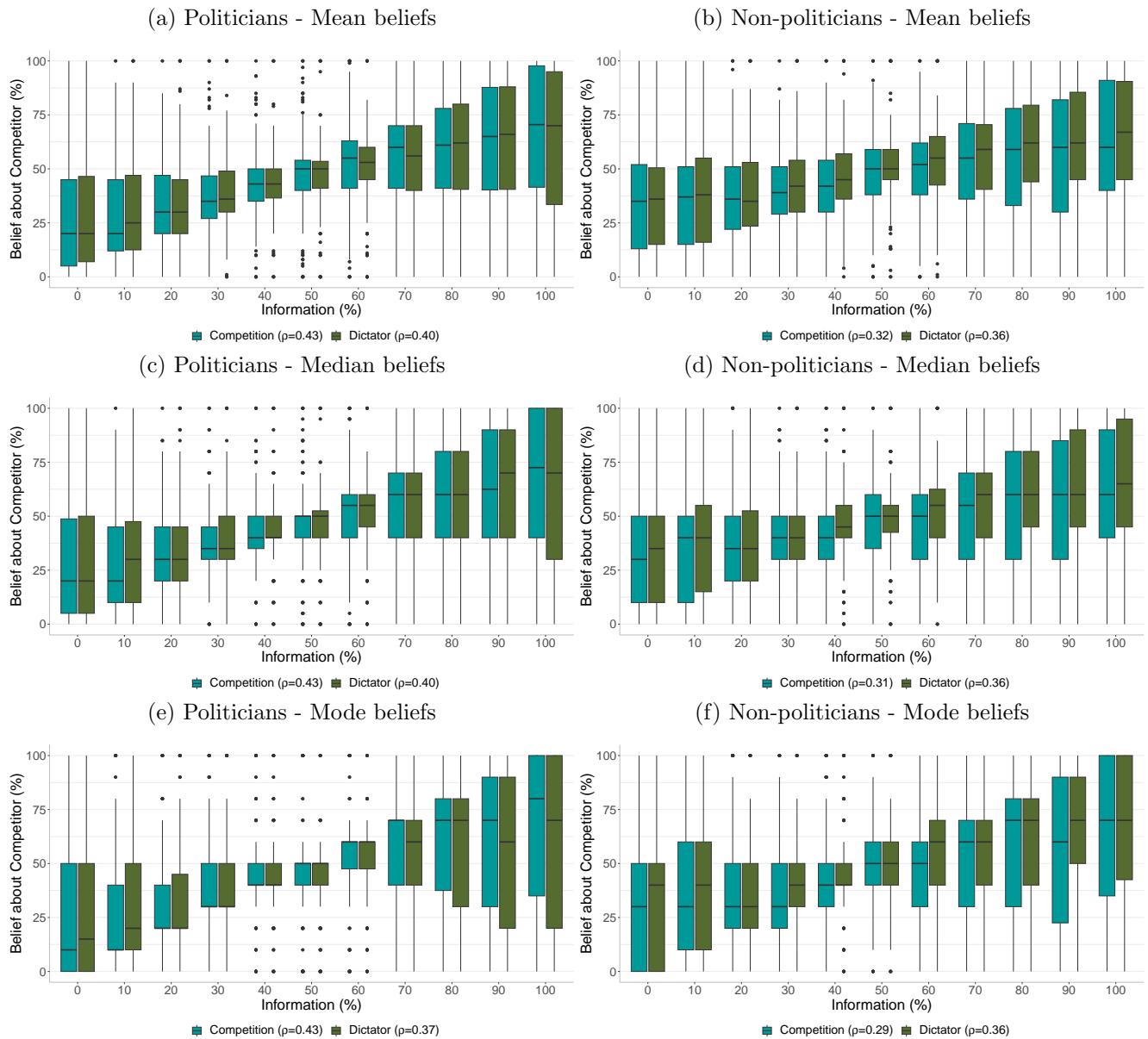
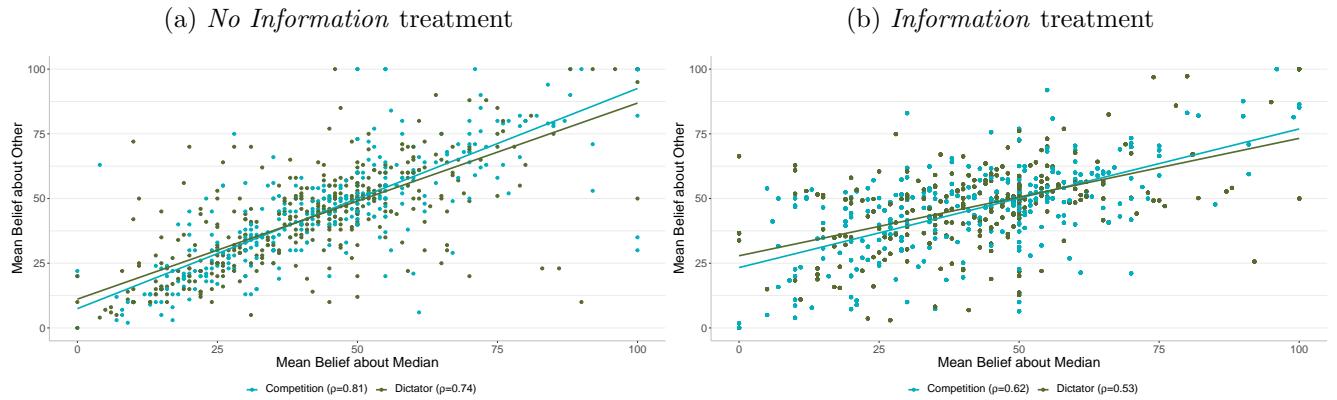


Table B.5.1: OLS regression - Beliefs

	Belief about Median	Sd Belief about Median	Belief about Competitor	Sd Belief about Competitor
	(1)	(2)	(3)	(4)
Politician	-5.552*** (1.538)	-2.330*** (0.770)	-7.290*** (1.578)	-1.780** (0.740)
Dictator	-0.877 (1.055)	0.363 (0.537)	-0.449 (1.013)	-0.354 (0.474)
Treat:Info	-2.324 (1.633)	-0.974 (0.824)	0.0699 (1.548)	-2.126*** (0.699)
Politician × Treat:Info	3.683* (2.171)	0.756 (1.104)	4.281** (2.036)	-0.263 (0.921)
Age	0.0376 (0.0527)	-0.0611** (0.0259)	0.0582 (0.0516)	-0.0395* (0.0223)
<i>Income - Base: <20k</i>				
Between 20k and 30k	-2.725* (1.652)	-0.921 (0.818)	-3.112** (1.562)	-1.263* (0.714)
Between 30k and 40k	-4.213** (1.790)	0.101 (0.900)	-2.026 (1.703)	-1.016 (0.799)
More than 40k	-4.650** (1.804)	-0.0400 (0.882)	-4.010** (1.728)	-1.028 (0.793)
Constant	44.46*** (4.507)	18.59*** (2.406)	43.42*** (4.547)	17.19*** (2.143)
Other controls	✓	✓	✓	✓
R ²	0.0317	0.0364	0.0435	0.0477
Observations	1415	1415	1415	1415

Notes: In the *Information* treatment, we gathered participants' beliefs about the competitor 11 times. Here, we calculate the mean of these 11 values for participants. To economize on space, only statistically significant coefficients are shown. Other controls include gender, education level, occupation, ideology, region fixed effects, the risk aversion index and the random order dummy. Robust standard errors in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Figure B.5.5: Correlation between beliefs (median rate v. competitor's chosen rate)



Note: In the *Information* treatment, we gathered participants' beliefs about the competitor's tax choice 11 times. Here, we calculate the mean of these 11 values for participants in the *Information* treatment to study the correlation with the beliefs about the median rate.

B.6 Multiple hypothesis testing

Table B.6.1: Multiple hypothesis testing - *No Information* treatment

	p-value	Sharpened q-value	Holm	Bonferroni
Belief about Competitor	4.32e-08	.001	4.32e-08	2.16e-07
Belief about Median	1.93e-08	.001	5.78e-08	9.64e-08
Dictator	.8798049	1	1	1
Dictator X Belief about Median	.7342243	1	1	1
Politician	.5689499	1	1	1

Notes: This table displays the p-values associated to three multiple hypothesis testing corrections from the coefficients of the regression results displayed in column (5) of table 3.

Table B.6.2: Multiple hypothesis testing - *Information* treatment

	p-value	Sharpened q-value	Holm	Bonferroni
Belief about Competitor	1.10e-26	.001	2.20e-26	6.59e-26
Belief about Median	9.51e-09	.001	2.85e-08	5.70e-08
Dictator	.8732815	.997	1	1
Dictator X Information	.5811377	.773	.5811377	1
Information	5.00e-14	.001	2.00e-13	3.00e-13
Politician	.9981379	.997	1	1

Notes: This table displays the p-values associated to three multiple hypothesis testing corrections from the coefficients of the regression results displayed in column (5) of table 4.

B.7 Control variables in the main regressions

Table B.7.1: Tobit regressions - Control variables

	Dependent variable: Tax rate	
	(1)	(2)
<i>Ideology - Base: Left</i>		
Center	-3.538*	-2.836
	(2.110)	(2.146)
Right	-7.686***	-4.275*
	(2.098)	(2.382)
None	-5.253***	-2.986
	(1.983)	(1.880)
<i>Income - Base: <20k</i>		
Between 20k and 30k	1.241	-0.959
	(2.170)	(2.265)
Between 30k and 40k	1.584	-6.333***
	(2.461)	(2.446)
More than 40k	3.698	-9.444***
	(2.410)	(2.644)
Risk aversion	0.102	-0.246*
	(0.119)	(0.133)
Constant	14.89**	16.63*
	(7.495)	(9.322)
Treatment	No Info	Info
Other controls	✓	✓
Pseudo R ²	0.0546	0.0428
Observations	842	6303

Notes: This table displays the coefficients on the control variables of the regression in the last column of Table 3 and Table 4. To economize on space, only statistically significant coefficients are shown. Other controls include age, gender, education level, occupation and region fixed effects and the random order dummy. Robust standard errors in parentheses in column (1). Standard errors clustered at the individual level in parentheses in column (2). * significant at 10%, ** significant at 5%, *** significant at 1%.

B.8 Analysis with the politicians sample

Table B.8.1: Tobit regressions - Politicians vs non-politicians

	(1)	(2)
Dictator	-2.341 (6.579)	-0.123 (2.668)
Politician × Dictator	5.632 (8.887)	0.591 (4.065)
Belief about Median	0.390*** (0.102)	0.241*** (0.0653)
Belief about Competitor	0.319*** (0.0795)	0.336*** (0.0477)
Dictator × Belief about Median	0.0542 (0.132)	
Politician × Dictator × Belief about Median	-0.165 (0.199)	
Politician × Belief about Median	0.158 (0.164)	0.109 (0.108)
Politician × Belief about Competitor	0.132 (0.130)	0.0736 (0.0690)
Information	0.228*** (0.0342)	
Dictator × Information	0.0154 (0.0463)	
Politician × Dictator × Information	-0.0659 (0.0681)	
Politician × Information	-0.0665 (0.0525)	
Constant	21.21** (8.233)	17.65* (9.429)
Treatment	No Info	Info
Controls	✓	✓
Pseudo R ²	0.0557	0.0436
Observations	842	6303

Notes: Controls include: age, gender, income, education level, occupation, region of residence, political ideology, a risk aversion index and a dummy for whether the belief elicitation task has been played before or after the redistribution game. Robust standard errors in parentheses in column (1). Standard errors clustered at the individual level in parentheses in column (2). * significant at 10%, ** significant at 5%, *** significant at 1%.

B.9 Heterogeneity analysis

Table B.9.1: Heterogeneity analysis - Tax choice - Ideology

	Leftwing		Centrist		Rightwing	
	(1)	(2)	(3)	(4)	(5)	(6)
Dictator	3.473 (7.142)	1.164 (4.247)	-3.706 (9.120)	-4.712 (4.788)	-21.60*** (7.896)	0.367 (4.646)
Politician	9.791*** (3.681)	1.194 (3.143)	-6.187 (3.799)	-0.693 (4.424)	-5.707* (3.301)	-0.797 (5.456)
Belief about Median	0.257* (0.156)	0.274*** (0.0821)	0.755*** (0.195)	0.227* (0.117)	0.285* (0.165)	0.139 (0.134)
Dictator × Belief about Median	-0.162 (0.146)		-0.00879 (0.215)		0.564*** (0.175)	
Belief about Competitor	0.746*** (0.133)	0.392*** (0.0584)	0.0517 (0.149)	0.313*** (0.0687)	0.348*** (0.132)	0.461*** (0.0786)
Information		0.214*** (0.0500)		0.158** (0.0646)		0.0763 (0.0569)
Dictator × Information		-0.0259 (0.0653)		0.0555 (0.0866)		0.0274 (0.0725)
Constant	17.51 (14.73)	43.58** (17.42)	16.38 (13.69)	29.98** (12.91)	7.166 (10.34)	-23.93 (15.29)
Treatment	No Info	Info	No Info	Info	No Info	Info
Controls	✓	✓	✓	✓	✓	✓
Pseudo R ²	0.0770	0.0523	0.0862	0.0572	0.0930	0.0528
Observations	266	1958	134	935	166	1177

Notes: Controls include: age, gender, income, education level, occupation, region of residence, a risk aversion index, a dummy for whether the belief elicitation task has been played before or after the redistribution game, and the time taken for completing the survey. Robust standard errors in parentheses in column (1) and (3). Standard errors clustered at the individual level in parentheses in column (2) and (4). * significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.9.2: Tobit regressions - Politicians sample

	(1)	(2)
Dictator	1.247 (6.132)	0.708 (3.083)
Belief about Median	0.498*** (0.128)	0.371*** (0.0824)
Dictator \times Belief about Median	-0.0953 (0.151)	
Belief about Competitor	0.476*** (0.106)	0.415*** (0.0502)
<i>Current Office</i> (Baseline: Councillor)		
Mayor	-3.440 (3.168)	5.921** (3.016)
Vice-mayor	-0.268 (2.451)	4.609 (3.524)
<i>Municipality Pop.</i> (Baseline: Less than 1000)		
Between 1000 and 3500	4.405* (2.434)	-2.583 (2.766)
More than 3500	0.732 (2.509)	-2.175 (3.040)
<i>Former Mandates</i> (Baseline: 0 mandates)		
1 mandate	-1.142 (2.404)	0.543 (2.962)
More than 1 mandate	-1.080 (2.874)	-3.287 (3.589)
Information		0.160*** (0.0403)
Dictator \times Information		-0.0504 (0.0501)
Constant	6.480 (8.587)	21.12* (11.38)
Treatment	No Info	Info
Controls	✓	✓
Pseudo R ²	0.0657	0.0516
Observations	475	3135

Notes: Controls include: age, gender, income, education level, occupation, region of residence, political ideology, a risk aversion index and a dummy for whether the belief elicitation task has been played before or after the redistribution game. Robust standard errors in parentheses in column (1). Standard errors clustered at the individual level in parentheses in column (2). * significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.9.3: OLS regression - Beliefs - Politicians sample

	Belief about Median	Sd Belief about Median	Belief about Competitor	Sd Belief about Competitor
	(1)	(2)	(3)	(4)
Dictator	-1.487 (1.452)	1.526** (0.729)	-2.128 (1.359)	-0.0358 (0.649)
Treat:Info	1.142 (1.506)	0.226 (0.734)	4.147*** (1.380)	-2.107*** (0.613)
<i>Current Office</i> (Baseline: Councillor)				
Vice-mayor	4.244* (2.332)	0.734 (1.165)	1.818 (2.211)	0.493 (1.003)
Local councillor	2.774 (2.013)	-0.156 (1.035)	0.948 (1.954)	0.513 (0.926)
<i>Former Mandates</i> (Baseline: 0 mandates)				
1 mandate	1.065 (1.763)	0.867 (0.864)	1.969 (1.701)	0.574 (0.761)
More than 1 mandate	0.403 (2.019)	-0.342 (1.014)	-1.648 (1.876)	0.932 (0.968)
<i>Municipality Pop.</i> (Baseline: Less than 1000)				
Between 1000 and 3500	0.778 (1.722)	-1.663** (0.844)	0.0779 (1.630)	-0.926 (0.738)
More than 3500	-4.333** (1.856)	-0.369 (0.982)	-3.744** (1.803)	-0.275 (0.867)
Age	0.0631 (0.0768)	-0.0650* (0.0359)	-0.00584 (0.0723)	-0.0255 (0.0329)
<i>Income</i> (Baseline: <20k)				
Between 20k and 30k	-1.891 (2.238)	-0.132 (1.154)	-2.447 (2.032)	-1.384 (1.036)
Between 30k and 40k	-5.970** (2.460)	-0.865 (1.225)	-2.673 (2.276)	-1.858* (1.103)
More than 40k	-4.427* (2.518)	-1.294 (1.205)	-3.321 (2.277)	-1.772 (1.122)
Constant	35.87*** (5.646)	14.29*** (3.094)	39.63*** (5.689)	12.94*** (2.610)
Other controls	✓	✓	✓	✓
R ²	0.0567	0.0489	0.0657	0.0625
Observations	760	760	760	760

Notes: In the *Information* treatment, we gathered participants' beliefs about the competitor 11 times. Here, we calculate the mean of these 11 values for participants. To economize on space, only statistically significant coefficients are shown. Other controls include gender, education level, occupation, ideology, region fixed effects, the risk aversion index and the random order dummy. Robust standard errors in parentheses in columns (1), (2), (3), (4), (5), (7). * significant at 10%, ** significant at 5%, *** significant at 1%.

B.10 Robustness checks

Table B.10.1: Tobit regression - Robustness checks

	Post-stratification weights		No-charity		Tax ≠ Info		Inconsistent		Too fast		OLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dictator	-4.272 (6.701)	1.140 (2.662)	0.984 (6.768)	-1.630 (2.830)	0.799 (2.002)	0.645 (2.068)	-0.509 (4.630)	-0.0587 (2.141)	1.724 (4.810)	0.392 (2.316)	0.0629 (4.030)	0.574 (1.875)
Politician	-0.394 (1.956)	1.170 (2.057)	3.499 (2.836)	4.213 (2.618)	0.0222 (1.778)	-0.793 (1.829)	0.922 (1.826)	-0.606 (1.850)	2.280 (2.145)	0.148 (2.129)	0.402 (1.642)	-0.366 (1.581)
Belief about Median	0.348*** (0.0998)	0.279*** (0.0629)	0.514*** (0.102)	0.293** (0.0719)	0.301*** (0.0524)	0.318*** (0.0532)	0.446*** (0.0902)	0.287*** (0.0521)	0.529*** (0.106)	0.316*** (0.0618)	0.443*** (0.0786)	0.257*** (0.0431)
Dictator × Belief about Median	0.110 (0.135)		-0.00690 (0.135)				-0.00247 (0.107)		-0.0830 (0.116)		-0.0172 (0.0882)	
Belief about Competitor	0.321*** (0.0782)	0.326*** (0.0456)	0.235*** (0.0746)	0.311*** (0.0470)	0.358*** (0.0351)	0.315*** (0.0357)	0.432*** (0.0751)	0.365*** (0.0365)	0.453*** (0.0899)	0.398*** (0.0392)	0.343*** (0.0649)	0.340*** (0.0313)
Information		0.223*** (0.0328)		0.225*** (0.0333)	0.189*** (0.0260)	0.132*** (0.0252)		0.203*** (0.0283)		0.207*** (0.0319)		0.191*** (0.0242)
Dictator × Information		0.0112 (0.0458)		0.0202 (0.0477)	-0.0260 (0.0337)	-0.0240 (0.0334)		-0.0199 (0.0363)		-0.0495 (0.0395)		-0.0184 (0.0318)
Constant	32.43*** (10.08)	-19.16* (9.780)	11.86 (10.06)	-13.60 (9.529)	17.05* (9.363)	20.65** (9.721)	11.46 (7.956)	21.46** (10.37)	2.383 (8.181)	23.26** (9.996)	16.06** (7.219)	18.68** (8.224)
Treatment Controls	No Info ✓	Info ✓	No Info ✓	Info ✓	Info ✓	Info ✓	No Info ✓	Info ✓	No Info ✓	Info ✓	No Info ✓	Info ✓
Pseudo R ²	0.0538	0.0439	0.0544	0.0449	0.0411	0.0337	0.0569	0.0428	0.0648	0.0446		
R ²											0.379	0.321
Observations	842	6303	379	3168	6149	5609	772	5841	640	4741	842	6303

Notes: Controls include: age, gender, income, education level, occupation, region of residence, political ideology, a risk aversion index, a dummy for whether the belief elicitation task has been played before or after the redistribution game, and the time taken for completing the survey. In columns (1) and (2) we apply post-stratification weights accounting for differences in gender, age, and region between our sample of politicians and non-politicians and the sample of all French politicians and the general population. In columns (3) and (4) we exclude participants who chose to keep the gratification instead of donating it to charity. In columns (5) and (6) we exclude participants who were not consistent in the risk aversion task. In columns (7) and (8) we exclude participants who took less than 10 minutes to complete the survey. In column (9) and (10) we run the main regressions by OLS. In column (11) we exclude participants who chose always a tax rate equal to the information. In column (12) we exclude all the observations where the chosen tax rate is equal to the information provided. Robust standard errors in parentheses in column (1), (3), (5), (7), and (9). Standard errors clustered at the individual level in parentheses in column (2), (4), (6), (8), (10), (11) and (12). * significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.10.2: Tobit regressions with median and mode of subjective probability distributions

	Median beliefs		Mode of beliefs	
	(1)	(2)	(3)	(4)
Politician	0.458 (1.755)	-0.301 (1.760)	0.199 (2.156)	-1.398 (2.540)
Dictator	0.176 (4.129)	0.436 (2.015)	-2.547 (4.704)	1.941 (2.759)
Belief about Median	0.441*** (0.0759)	0.293*** (0.0473)		
Belief about Competitor	0.362*** (0.0626)	0.345*** (0.0328)		
Dictator \times Belief about Median	-0.0255 (0.0939)			
Information		0.204*** (0.0261)		0.214*** (0.0357)
Dictator \times Information		-0.0193 (0.0342)		-0.0453 (0.0468)
Belief about Median			0.328*** (0.103)	0.323*** (0.0552)
Belief about Competitor			0.386*** (0.0837)	0.330*** (0.0392)
Dictator \times Belief about Median			-0.0579 (0.107)	
Constant	17.60** (7.593)	17.10* (9.279)	40.19*** (10.05)	8.821 (8.903)
Treatment	No Info	Info	No Info	Info
Controls	✓	✓	✓	✓
Pseudo R ²	0.0526	0.0423	0.0659	0.0474
Observations	842	6303	475	3449

Notes: When using the mode, we drop the observations for which the subjective probability distribution is multi-modal. This leads to a loss of 373 observations in the *No Information* treatment and 2,880 observations in the *Information* treatment. Controls include: age, gender, income, education level, occupation, region of residence, political ideology, a risk aversion index and a dummy for whether the belief elicitation task has been played before or after the redistribution game. Robust standard errors in parentheses in column (1) and (3). Standard errors clustered at the individual level in parentheses in column (2) and (4). * significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.10.3: Tobit regression for each information

	Dependent variable: Tax rate										
	0%	10%	20%	30%	40%	50%	60%	70%	80%	90%	100%
Dictator	-0.173 (2.377)	0.240 (1.936)	0.833 (1.727)	-0.966 (1.645)	-1.167 (1.598)	0.613 (1.508)	-0.768 (1.653)	0.0911 (1.829)	-1.262 (2.018)	-1.829 (2.308)	-2.036 (3.044)
Politician	3.393 (2.838)	3.317 (2.328)	2.744 (2.105)	0.130 (1.989)	-0.926 (1.950)	-0.695 (1.906)	-2.126 (1.987)	0.212 (2.283)	-1.901 (2.481)	-1.849 (2.791)	-2.976 (3.573)
Belief about Median	0.297*** (0.0893)	0.315*** (0.0714)	0.299*** (0.0625)	0.319*** (0.0587)	0.357*** (0.0584)	0.294*** (0.0555)	0.262*** (0.0565)	0.291*** (0.0584)	0.288*** (0.0645)	0.255*** (0.0666)	0.307*** (0.0907)
Belief about Competitor	0.478*** (0.0641)	0.343*** (0.0584)	0.323*** (0.0597)	0.308*** (0.0627)	0.229*** (0.0653)	0.283*** (0.0573)	0.356*** (0.0568)	0.333*** (0.0497)	0.343*** (0.0468)	0.398*** (0.0449)	0.502*** (0.0531)
Constant	2.620 (14.13)	8.341 (11.27)	9.924 (10.31)	13.63 (9.904)	20.33** (10.03)	26.01*** (9.944)	36.60*** (9.595)	34.38*** (10.05)	46.74*** (9.846)	48.34*** (11.05)	52.29*** (14.74)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Pseudo-R ²	0.0387	0.0326	0.0355	0.0362	0.0330	0.0369	0.0370	0.0333	0.0334	0.0311	0.0374
Observations	573	573	573	573	573	573	573	573	573	573	573

Notes: Controls include: age, gender, income, education level, occupation, region of residence, political ideology, a risk aversion index and a dummy for whether the belief elicitation task has been played before or after the redistribution game. Robust standard errors in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.