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Yellow Vests, Pessimistic Beliefs, and Carbon Tax Aversion[†]

By Thomas Douenne and Adrien Fabre*

Using a representative survey, we find that after the Yellow Vests movement, French people would largely reject a tax and dividend policy, i.e., a carbon tax whose revenues are redistributed uniformly to each adult. They overestimate their net monetary losses, wrongly think that the policy is regressive, and do not perceive it as environmentally effective. We show that changing people's beliefs can substantially increase support. Although significant, the effects of our informational treatments on beliefs are small. Indeed, the respondents that oppose the tax tend to discard positive information about it, which is consistent with distrust, uncertainty, or motivated reasoning. (*JEL* D83, H23, H31, Q54, Q58)

The French government initially committed to an ambitious trajectory for the price of carbon.[1] Initiated in 2014 at €7/ton of carbon dioxide (tCO_2), the French carbon tax reached €44.6/tCO_2 in 2018 and was supposed to continue growing to reach €86.2/tCO_2 by 2022. However, at the end of 2018, the same government that had accelerated the price trajectory decided to abandon it and froze the tax at its current level for an undetermined period of time. This turnaround in French climate policy is the direct consequence of the popular protest by the "Yellow Vests," which started in opposition to the carbon tax.[2] Among several factors, the negative impact of the tax on households' purchasing power has certainly

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[1] Specifically, the "Contribution Climat-Énergie" is a *sectoral* carbon tax specific to fossil fuels.

[2] Following a massive petition against rising gasoline prices in November 2018, hundreds of thousands of people started protesting. They wore recognizable fluorescent clothing, gathered at roundabouts and toll booths every day, and demonstrated in Paris each Saturday. The Yellow Vests expressed a general concern over their purchasing power and discontent with French elites and institutions.

been a key driver of public discontent. The increasing revenues from the carbon tax were mostly used to fund the budget rather than redistributed to households, raising concerns over the distributive effects of the policy. To counteract the negative impact of carbon taxation on households' purchasing power, economists have proposed a scheme known as "tax and dividend," i.e., a carbon tax whose revenue is redistributed uniformly to each adult. A total of 3,354 American economists recently supported this strategy in *The Wall Street Journal* "to maximize the fairness and political viability of a rising carbon tax." Therefore, an implicit assumption is that with a design that ensures that the properties of the tax are aligned with people's *preferences*, one should be able to generate support for it. However, is this truly sufficient? In this paper, we show that to understand the link between the properties of a policy and its support, one has to account for a critical ingredient: *beliefs*.

The objective of this paper is to understand how the beliefs regarding a policy form and then determine the attitudes toward it. Recent events undoubtedly make the French carbon tax an interesting case study. To explain French attitudes toward carbon taxation, we surveyed a representative sample of 3,002 French households. We focus on a carbon "tax and dividend" policy, i.e., an increase in the carbon tax by €50/tCO_2, the revenue of which is redistributed uniformly to each adult. Our reform allows one to clearly specify the distributive effects of the policy, in contrast to the one abandoned by the government. Only 10 percent of respondents approve of the reform, and 70 percent disapprove of it (the rest do not know or do not want to answer). We analyze the perceptions of three well-known determinants of the acceptance of a carbon tax: the impact on one's purchasing power, the progressivity of the scheme, and the scheme's environmental effectiveness. We compare the subjective beliefs regarding the impacts on one's purchasing power to the objective distribution computed using official household survey data. This comparison shows that people largely overestimate the incidence of the tax on their household. For instance, while 70 percent of households are expected to benefit from this reform, only 14 percent think that they would. Similarly, while the scheme proposed in our survey is progressive, a large majority of individuals perceive it as regressive. In addition, a majority of respondents do not believe that such a policy would reduce pollution and combat climate change. Using information reported on their energy equipment and usage, we are able to compute a respondent-specific estimate of the tax incidence on their purchasing power. This estimation enables us to examine the heterogeneity in what we call *biases* about the perceived tax incidence. We find that the people most opposed to the policy, and in particular those that support the Yellow Vests, are the most biased, i.e., the most inclined to overestimate their losses. Thus, one may wonder whether pessimistic beliefs lead to policy rejection or the causality runs in the opposite direction.

To disentangle the effect of initial beliefs on attitudes toward the policy from the reverse effect of attitudes on perceptions, we investigate the effect of providing new information to respondents through random treatments. Respondents randomly receive (or do not receive) a piece of information about the progressivity and/or effectiveness of the policy as well as customized information—derived from our respondent-specific estimation—on whether their household is expected to win or lose from the policy. We also specify that this latter information is correct in five

cases out of six, a probability that we carefully estimated out of sample. Our first observation is that our treatments generally fail to change pessimistic beliefs. For example, among those who would benefit from the reform despite pessimistically believing that they would lose, only 12 percent are convinced that they would gain when we disclose our estimation to them. Even worse, respondents revise their beliefs in an asymmetric way, giving more weight to new information when it shows that they would lose from the reform, i.e., when it provides them with arguments against the tax. We also find evidence consistent with motivated reasoning[3] in the formation of beliefs since those who already approved of the reform are more likely to correctly revise their beliefs, while those most opposed to it, such as supporters of the Yellow Vests, tend to discard new information unless it goes against the tax. Moreover, we find that this phenomenon is accentuated among highly educated people, suggesting that it stems from an adaptive advantage rather than a cognitive deficiency.

We use the randomly displayed information as instruments to estimate the causal effect of holding certain beliefs (measured as binary variables) on policy support. In the case of self-interest (taken as one's beliefs about winning or losing purchasing power from the policy), we supplement these treatments by testing the support for a different policy, a tax and *targeted* dividend; people become eligible for this dividend when their incomes are below a certain threshold, and we vary the threshold across respondents to create exogenous variations in eligibility. The method we use in this case is noteworthy since it creates random variation in the beliefs of winning around the eligibility thresholds and enables us to estimate the causal effect of this belief using a fuzzy regression discontinuity design (RDD). Our results indicate that convincing people of the actual incidence and effectiveness of the policy could lead to majority support. Indeed, we find that self-interest has a large effect on support for the policy: the belief that one does not lose from it increases the acceptance rate by more than 50 p.p. Similarly, believing that the tax is environmentally effective increases the approval rate of the reform by more than 40 p.p. We also provide noncausal evidence that believing in the progressivity of the scheme has a large effect on support. Overall, these results suggest that the rejection of carbon taxation does not typically result from clashing principles, such as a disinterest in the climate or a dislike of price instruments, but rather from overly pessimistic beliefs about the properties of the reform. Provided that people's opposition to the policy reinforces their pessimism—which our results suggest—their biases are reinforced such that new information might only further push their attitude in one direction.[4]

The contribution of this paper is twofold. First, it contributes to a recent literature that has emerged to understand the political economy of climate policies since this issue is becoming critical in the public debate. For a thorough review

[3] Motivated reasoning is the tendency to place more credence on arguments for conclusions we want to believe than on arguments for conclusions we do not want to believe (see Kunda 1990). This psychological mechanism can be a driver of the well-known "confirmation bias."

[4] The "campaign effect" documented by Anderson, Marinescu, and Shor (2019) (in the case of referenda in Washington State in the United States) is an example of how support for a carbon tax can decrease substantially after it enters the public debate. This may explain why the acceptance of an increase in the carbon tax plummeted with the Yellow Vests movement, down from a level of 48 percent (ADEME 2018) in the middle of the range of that in other countries (Brechin 2010). This effect confirms that the French carbon tax may be an insightful case study to understand what could happen in other countries when a controversial policy is publicly debated.

of this literature, we refer the reader to Carattini, Carvalho, and Fankhauser (2018) and suggest the more synthetic Klenert et al. (2018) as well as Millner and Ollivier (2016) for a review of the political obstacles to environmental policies. Stern, Dietz, and Kalof (1993) is an early work proposing and testing a model of attitudes on environmental quality intended to disentangle egoistic from altruistic motives on the one hand and beliefs from values on the other hand. Among all possible attitudes, they show that beliefs about consequences for self-interest are the only predictor of the willingness to pay Pigouvian taxes. Using a postelectoral survey in Switzerland, Thalmann (2004) also finds a correlation between carbon tax acceptance and self-interest, proxied by the number of cars owned. In surveys on British, Swedish, and Swiss respondents, Bristow et al. (2010); Brannlund and Persson (2012); and Carattini et al. (2017), respectively, document a higher approval rate when the reform addresses distributional issues. Baranzini and Carattini (2017) report that a majority of the people they interviewed in Geneva do not believe that the tax would be effective, which confirms what Dresner, Jackson, and Gilbert (2006) find with focus groups in the United Kingdom. Surveying Norwegian people, Kallbekken and Sælen (2011) show that self-interest matters for acceptance, but less than concerns for environmental effectiveness or distributional effects do. Using US data, Anderson, Marinescu, and Shor (2019) argue that ideology explains most of the support for carbon taxation and suggest that this effect would dominate that of self-interest.

In the present paper, we also study how acceptance depends on these three motives (i.e., self-interest, perceived environmental effectiveness, and progressivity). We contribute to the literature by providing robust evidence for causal effects where past studies essentially show correlations, often relying on proxies such as fuel consumption to proxy for self-interest (e.g., Thalmann 2004; Kallbekken and Sælen 2011; Anderson, Marinescu, and Shor 2019). In contrast, we assume neither that people are fully rational nor that they have perfect information. Thus, our methodology offers a novel examination of the political economy of climate policies since it allows one to disentangle erroneous *beliefs* from the pure effects of *preferences*.[5] The paper also quantifies biases regarding the costs of the carbon tax. To the best of our knowledge, this is the first study that compares subjective beliefs and objective data about the private costs that arise from carbon taxation. Given the intense public debate over the incidence of such a policy, identifying and measuring the discrepancy between actual impacts and their subjective perception is critical.

Beyond the case of carbon pricing, our paper contributes to the literature on the formation of political beliefs. Recent research has shown how beliefs on inequality and social mobility affect people's attitudes regarding distributive policies (e.g., Cruces, Perez-Truglia, and Tetaz 2013; Kuziemko et al. 2015; Alesina, Stantcheva, and Teso 2018). Our paper expands this literature by investigating the relationship between beliefs and attitudes on climate policies. Using a representative survey, our paper shows how beliefs toward a policy causally impact attitudes toward it. It also

[5] We take preferences over policies as the mapping from beliefs (on facts) to attitudes (on policies), i.e., how attitudes are determined as a function of beliefs. Conversely, motivated reasoning represents the feedback loop from attitudes to beliefs.

shows that people's attitudes toward a policy can be linked to how they process new information about it. Among other possible mechanisms, our results are consistent with theories of motivated reasoning (Kunda 1990; see Bénabou and Tirole 2016 for a recent review) that have thus far been mostly tested in a lab (e.g., Redlawsk 2002; Thaler 2019). In our context, motivated reasoning could be a manifestation of *tax aversion*, which we can define as a "gut" rejection of a tax (or taxation in general) that influences beliefs about the properties of a tax such as its effectiveness, fairness, or equivalence to a measure labeled differently. This would explain how popular protests against the carbon tax affected people's beliefs on related policies like the tax and dividend studied in this paper.

The remainder of the paper is organized as follows. In Section I, we describe our survey and other data sources. In Section II, we compare subjective perceptions to objective data and measure the bias regarding the impacts of carbon taxation. In Section III, we study the formation of beliefs and propose several mechanisms to rationalize people's pessimism. In Section IV, we estimate the effects of changing people's beliefs about tax incidence and effectiveness on acceptance. Section V concludes. Further results and methodological complements are reported in the online Appendix.

I. Context, Survey, and Data

A. *Context of the Study*

The Yellow Vests constitute a singular protest movement. Although they are overrepresented within the Far Left and Right, they are supported by a large fraction of the French from across the political spectrum.[6] Thousands of small-scale protests were organized autonomously on social networks, and the movement was remarkably independent from political parties and unions. Before the emergence of the movement, none of the major political parties campaigned against the carbon tax, and this policy did not trigger specific opposition until the increase in oil prices brought it to the forefront of the debate.[7] The opposition then quickly gained ground, notably through Facebook, where a petition against the tax and a call to protest on roundabouts were widely spread. These protests initially occurred every day and did not start decreasing until December 2018, when the government responded with a set of measures including supplements to low wages and modest pensions and the abandonment of the carbon tax increases initially scheduled. The fading movement came to an almost complete halt at the end of April 2019 when the government agreed to some of the demands for greater purchasing power and direct democracy (Boyer et al. 2020).

[6] Table H.1 in the online Appendix provides data on our respondents' attitudes toward the Yellow Vests depending on their sociodemographic characteristics and left-right leaning. This shows that the support for the movement is widespread. People at the center of the political spectrum are the least supportive, with 46 percent having a positive attitude toward the Yellow Vests versus 66 percent for the whole population.

[7] Fuel prices peaked in October 2018. The movement gained momentum at that time, leading to the first massive protest on November 17.

A simple interpretation of these protests could be that French people are far more concerned with their purchasing power than with climate change. However, our companion paper documents that a large majority of French people are aware of and concerned about climate change and are supportive of various climate policies, such as a tax on air travel, green investments, or stricter pollution norms (Douenne and Fabre 2020),[8] and our survey suggests that French people's willingness to pay for a carbon tax is similar to that of other countries (see the online Appendix). Instead, French people may simply not regard a carbon tax as the appropriate policy to address climate change. Thus, the present paper sheds light on people's beliefs about the carbon tax, how these beliefs form, and how they affect policy support.

B. *Our Survey*

Survey Data Collection.—We conduced the survey in February and March 2019, three months after the government decided to abandon the planned increase in the carbon tax. The 3,002 responses were collected through the survey company Bilendi. This company maintains a panel of French respondents whom they can email with survey links. The respondents received €3 if they fully completed the survey. The respondents who choose to respond are first channeled through screening questions that ensure that the final sample is representative in terms of six sociodemographic characteristics: gender, age (five brackets), education (four), socioprofessional category (eight), town size (five), and region (nine). We relaxed the quotas by 5 percent to 10 percent relative to the actual proportions in order to facilitate the sampling process. Table A.1 in the online Appendix shows that our sample is still extremely representative. Nonetheless, observations are weighted to correct for small differences between the sample and population frequencies (e.g., in education). The median time to complete the survey was 19 minutes. We ensured that all questions requiring some concentration were in the first half of the survey. We took several steps to ensure the best possible data quality. Our representative sample was obtained after excluding the inattentive and quickest respondents. We confirm in the online Appendix that this sampling restriction does not affect the main results.

The Survey.—All code and data are on github.[9] The full survey in French can be found online,[10] and we provide the translated questionnaire in the online Appendix. It contains several random branches and treatments that are independent of one another. Figure 1 shows the sequence of information or treatments (represented by ellipses) and questions (boxes). This section presents each part of the survey in turn.

Information Intervention on Environmental Issues: The survey opens with a brief presentation: 3 short sentences to welcome the participant, introduce ourselves as "two social science researchers," and explain that the survey will last 15 to 20 minutes. We

[8] The levels of awareness and concern are similar to those of other countries (Stokes, Wike, and Carle 2015). For instance, 72 percent know that climate change is anthropogenic, compared to 66 percent in the United States (Gallup 2019).

[9] github.com/thomasdouenne/yellow_vests_aej_ep.

[10] preferences-pol.fr/doc_q.php#_e.

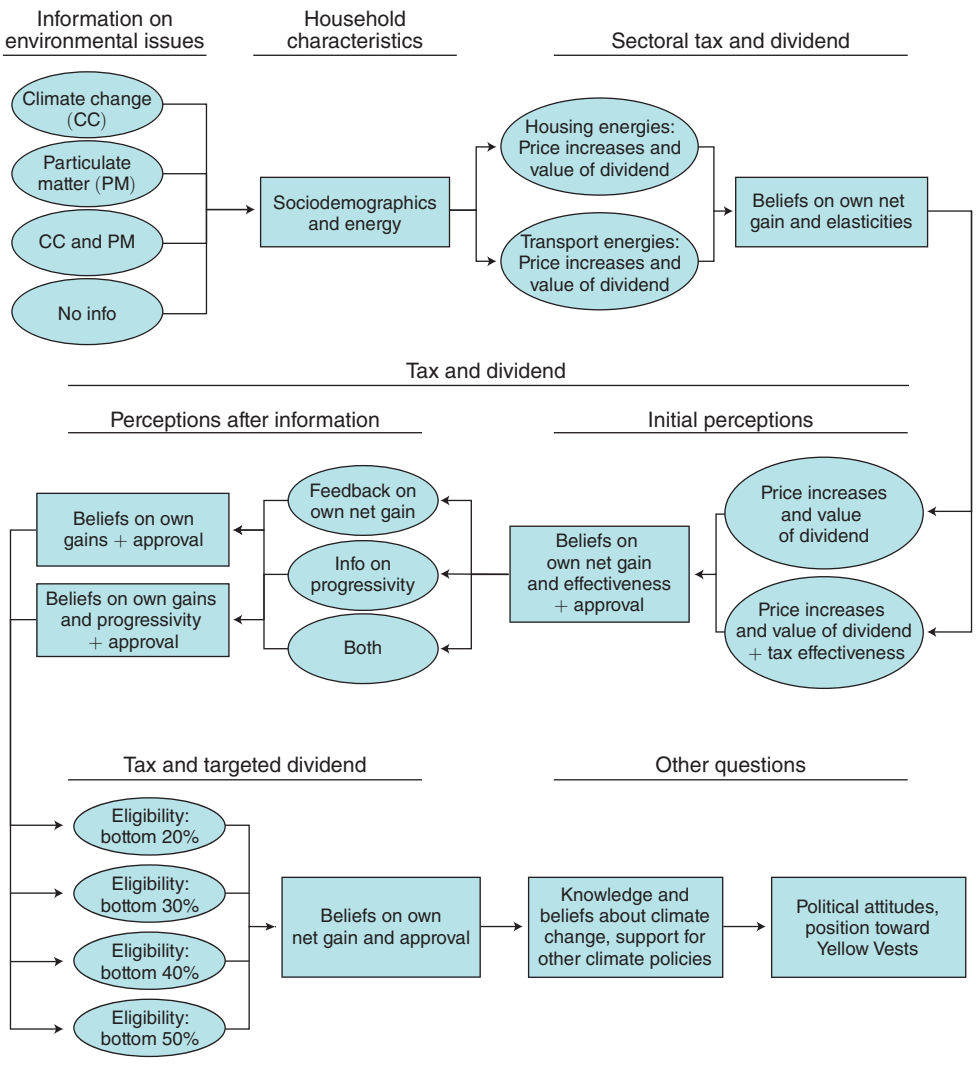

FIGURE 1. SEQUENCE OF INFORMATION OR TREATMENTS (ELLIPSES) AND QUESTIONS (BOXES)

Note: The sequence of informative treatments and of questions on beliefs and support for different tax and dividend policies is informative of how beliefs are revised in view of new information and allows us to estimate the causal effects of these beliefs on policy support.

then randomly display two blocks of information: one on climate change and another on particulate matter (i.e., air pollution). This informational treatment divides the sample into four groups who receive either one block of information, the other block of information, no block of information, or both blocks of information. The objective of these treatments is to see whether providing salient information on the consequences of climate change or air pollution affects respondents' answers later in the survey. The climate change information includes temperature trends for the long-run future, concerning facts on current and expected impacts, and a claim that keeping global warming below 2°C is technically feasible. The particulate

information consists of the estimated impact on French mortality (48,000 deaths per year) and life expectancy (reduced by 9 months on average in France) and the assertion that reducing fuel consumption would improve health. We save the time spent on each block and display links to scientific references to support the information.

Household Characteristics: In addition to the six quota strata, the sociodemographic characteristics include zip code, household structure, and the income of the respondent and of the household. A block on energy characteristics contains questions that allow us to estimate the impact of a carbon tax increase on housing expenditures (energy source and home size) and transport expenditures (number of vehicles, type(s) of fuel, distance traveled last year, and average fuel economy). The distributions of the answers are in line with official statistics, as shown in Table A.2 in the online Appendix.

Sectoral Tax and Dividend: We first randomly allocate the respondent to one of the two sectors to which the French carbon tax applies: housing *or* transport. We present a specific policy to them: a sectoral tax and dividend, i.e., an increase in housing *or* transport energy taxes that would finance a lump-sum transfer to all adults.[11] We detail the price increases that would follow and the value of the dividend they would receive: for the housing energy tax, +13 percent for gas and +15 percent for heating oil together with a yearly transfer of €50 per adult, and for the transport energy tax, +€0.11 per liter (L) of gasoline and +€0.13/L for diesel with a yearly transfer of €60 per adult. These figures are equivalent to an increase in the carbon price on these sources of energy by €50/tCO_2, but we do not mention the name "carbon tax" at this stage since we do not want people to think that it also falls on the other sector. The value of the dividends was obtained such that the policy is budget neutral and assuming typical price elasticities (see Section IC). We present the policy starting with "The government studies …" to capture the effect of distrust in the government that could arise in the actual political process.

Then, we ask the respondent whether their household would win, lose, or be unaffected by the reform in terms of purchasing power (*win/lose category* thereafter). Depending on their answer, we further ask respondents to estimate their expected gain (or loss) among five (or six) intervals. The interval thresholds are tailored to each respondent since they are computed in proportion to the number of consumption units (c.u.) of the household (as defined by Eurostat).[12] Similarly, households' gains and losses are always expressed per c.u. in the analysis. To obtain accurate answers, we did not incentivize the questions by using monetary rewards. Indeed, Sapienza and Zingales (2013) show that people think that economic experts are too optimistic regarding the carbon tax, and so incentivizing the answers could have led respondents to misreport their true beliefs and shift them toward what they think

[11] We chose to redistribute the money per adult instead of per c.u. to make the scheme more understandable. We limited the number of beneficiaries to two per household to better align this policy with current welfare benefits that depend on the number of c.u.s.

[12] For instance, for a single-member household (c.u. = 1), the intervals of the expected gain (in €/year) are (0, 10), (10, 20), etc., and for a childless couple (c.u. = 1.5), these intervals are (0, 15), (15, 30), etc.

the researchers expect. Finally, to see whether people think that the tax achieves its purpose of incentivizing people, we ask respondents to estimate their own elasticity and that of French people in general. To this end, we borrow the phrasing of Baranzini and Carattini (2017) and ask for the expected decrease in consumption (in five brackets) that would follow a 30 percent increase in the price of heating (or equivalently, an increase of €0.50/L in fuel prices).

Tax and Dividend.—

Initial Perceptions: Our main reform of interest is an increase of €50/tCO_2 in the French carbon tax, which concerns *both* housing and transport.[13] The revenues generated are again redistributed equally so that each adult receives a yearly lump-sum compensation of €110. We now explicitly present the reform as an increase in the carbon tax, although, as before, we do not give the implicit carbon price but rather the effect on energy prices (the same as before, but on both sectors) and the value of the dividend.[14] We randomly display a new informational treatment stating that "scientists agree that a carbon tax would be effective in reducing pollution," thereby splitting the sample in two. After describing the reform and providing (or not providing) this information, a first block of questions elicits the respondent's perceptions. We ask about their win/lose category and their subjective net gain in purchasing power in the same manner as for the sectoral tax with adapted intervals. We then ask them whether the reform would be effective in reducing pollution and combating climate change. Finally, we ask, "Would you approve of this reform?" and let the respondent choose between "Yes," "No" and "PNR (I don't know, I don't want to answer)."[15] In the following, we say that respondents *approve* of a reform if they respond "Yes" and *accept* the reform if they do *not* respond "No." Table I.1 in the online Appendix describes the rates of support for the tax and dividend policies at different stages of the survey.

Perceptions *after Information*: To assess how people form their beliefs and measure the importance of self-interest and fairness motives in the acceptance of the reform, we then provide some information on the effect of the reform. To half of the sample selected at random, we provide customized information explaining the following: "In five cases out of six, a household with your characteristics would [win/lose] through the reform. (The characteristics taken into account are: heating using [energy source] for an accommodation of [surface] m^2; [distance] km travelled with an average consumption of [fuel economy] L for 100 km.)" In Section IC, which details how we compute each respondent's net gain, we show that our prediction that a household wins or loses is correct in 83 percent of cases, which is our "five cases out of six." To a third of the sample selected at random, we explain that "this reform would increase the purchasing power of the poorest households and

[13] Electricity and industries are exempt from the French carbon tax because they are already covered by the EU Emissions Trading System.

[14] For the exact phrasing, see question 35 in the online Appendix.

[15] In English, "PNR" stands for "Prefer Not to Respond."

decrease that of the richest, who consume more energy." To the remaining sixth of the sample, we provide both the customized feedback on their net gain and the information on progressivity.[16]

Then, we again ask about the win/lose category (i.e., if the respondent's household would win, lose, or be unaffected by the reform) and for the approval of the reform. We also ask respondents about the perceived advantages and disadvantages of the policy, including the effect on the poorest households. To the latter half of the sample, immediately after the treatment on progressivity, we explicitly ask whether they think that the reform would benefit the poorest, since most respondents appeared not to believe our information.

Tax and Targeted Dividend: To disentangle the effect of self-interest from other motives for acceptance in Section IV, we then propose to respondents an alternative reform where only some people are eligible for the dividend. Specifically, we propose one of four alternative reforms where the payments, which are still equal among recipients, are targeted to adults whose income is below some threshold. The four possible thresholds correspond to the twentieth, thirtieth, fortieth, and fiftieth percentiles of the income distribution. They are computed using inflated deciles of individual income from the *Enquête sur les Revenus Fiscaux et Sociaux* (ERFS 2014) produced by Insee (the French national statistics bureau).[17] We randomly allocate respondents whose income lies between two thresholds to the reform defined by one of these thresholds. For example, a person at the twenty-fifth percentile of the income distribution has a 1-in-2 chance to face the reform targeted to the bottom 30 percent, where they are eligible for the dividend, and a 1-in-2 chance to face the reform targeted to the bottom 20 percent, where they are not. When the income is close to only one threshold (i.e., when its percentile in the distribution is below 20 or within $[50; 70]$), the allocated reform corresponds to that one. When the respondent's income is distant from all thresholds, i.e., when it is in the *top* 30 percent (above €2,220/month), the reform they face is determined by the income of the household's second adult. Finally, when both adults (or the only adult) in the household are in the top 30 percent, their reform is allocated randomly from the 4 variants. Table 1 details the income thresholds and dividends of the four variants and the proportion of respondents allocated to each of them, along with the proportion one would expect from the official data. The two sets of figures match almost perfectly, indicating that our sample is representative according to income.

We describe to each respondent the variant they face: the price increases, the income threshold, and the value of the dividend. We also specify how many persons in their household would be eligible for the payment. Finally, we ask respondents again for their anticipated win/lose category and their approval. The random variation in eligibility creates exogenous variation in the win/lose belief, which we use to estimate its causal effect on acceptance in a fuzzy RDD.

[16] In total, one-half of the respondents are treated with the information on progressivity. For the customized feedback, we choose to treat two-thirds of the sample to increase our statistical power on the identification strategy that exploits a subsample of those who received the feedback (see Section IVA).

[17] Income sources that are entitlements for the household rather than its members, such as certain welfare benefits, are divided equally among the two oldest adults in the household.

Table 1—Characteristic of the Targeted Reform by the Payment Target

Targeted percentiles	≤20	≤30	≤40	≤50
Income threshold (€/month)	780	1,140	1,430	1,670
Payment to recipients (€/year)	550	360	270	220
Proportion of respondents	0.356	0.152	0.163	0.329
Expected proportion of respondents	0.349	0.156	0.156	0.339

Note: This table reads as follows: when targeted people are those below the twentieth percentile (≤20), all adults with an income below €780/month receive a dividend of €550/year; 0.356 of our respondents are assigned to this policy (for which they may or may not be eligible depending on their income), compared to 0.349 if our survey were exactly representative of the true income distribution of the French population.

Other Questions: We do not detail the other questions of the survey, because we devote a companion paper to their analysis, Douenne and Fabre (2020). In these questions, we examine the opinions on environmental policies, including other ways to use the revenues from a carbon tax. We measure the knowledge and perceptions of climate change and ask specific questions on the influence of climate change on the choice to give birth and one's willingness to change one's lifestyle. We study the use of, availability of, and satisfaction with public transportation and active mobility. We also ask questions regarding political preferences, including attitude toward the Yellow Vests. Finally, we let the respondent express any comments in a text box.

C. *Official Household Surveys*

In addition to our survey, we use three official household surveys produced by Insee[18] for two purposes. First, we use the consumer and transport surveys to compute the objective distribution of the increases in fuel expenditures and the revenue from the tax (and, hence, the value of the dividend). Second, we use the housing survey to compute a respondent-specific estimate of their objective net gain.

Eliciting Objective Aggregates and Distributions.—We use the database constructed by Douenne (2020) that matches the consumer and transport surveys. It includes over 10,000 households, for whom it provides information on all their revenues and expenditures—including their energy bills—together with many sociodemographic characteristics. From this combined dataset, we are able to determine the increase in expenditures that households would face following changes in tax rates and compute the total tax revenue to be redistributed in a lump sum (see the online Appendix for more details). We thereby obtain the distribution of households' *objective* net gains in purchasing power implied by the policies proposed.

[18] The additional datasets are the consumer survey *Budget de Famille* (BdF 2011), the transport survey *Enquête Nationale Transports et Déplacements* (ENTD 2008) and the housing survey *Enquête Logement* (EL 2013). We use data from National Accounts to homogeneously inflate households' sectoral expenditures from each dataset we use to make them representative of the most recent trend and comparable across datasets. For more information about these surveys, see the online Appendix C.1.

Formally, the net gain γ_h of household h (the notations are explained in the online Appendix) can be expressed as follows:

$$\gamma_h = N_h^a \cdot D - \Delta E_h^{transport} - \Delta E_h^{housing}, \tag{1}$$

where D denotes the value of the dividend, N_h^a is the number of adults receiving it in this household, and ΔE is the increases in their energy expenditures. Our computations use the typical elasticities found in the literature on French households: -0.4 for transport and -0.2 for housing.[19] We assume that consumers bear 80 percent of the incidence of energy taxes.

Computing Households' Expected Net Gains.—

Simulating Expected Net Gains: To measure each respondent's bias and provide customized feedback on their win/lose category, we estimate their net gain by using equation (1). We directly compute the increase in transport fuel expenditures $\Delta E^{transport}$ from the information reported on their yearly distance traveled and the average fuel consumption of their vehicles. From the housing survey—which provides information on households' housing energy bills—we compute $\Delta E^{housing}$ and regress it on household characteristics. We then use the coefficients obtained to compute $\widehat{\Delta E}^{housing}$ for the households of our own survey respondents. The specification we chose is presented in the online Appendix and compared with alternative specifications and prediction methods.

Assessing Feedback's Accuracy: To assess the accuracy of our prediction, we test it out of sample on the consumer survey. Indeed, for households in this survey, we both obtain the true net gain γ and use our prediction to estimate $\hat{\gamma}$ and then assess the likelihood of correctly predicting their win/lose category. Because we made the prediction by using a different survey than the one on which we tested it, we avoided the risk of overfitting. For five households out of six, we correctly predict whether their purchasing power would increase or decrease due to the policy. We make this ratio symmetrical to balance the shares of overly optimistic and overly pessimistic feedback. Among the households predicted to win in the consumer survey, 83.4 percent were actual winners, while among those predicted to lose, 83.4 percent were actual losers (see Figure C.1 in the online Appendix). Assuming that the characteristics reported by our respondents are correct, there is no reason to believe that the probability of error is higher or lower when simulations are applied to our survey respondents.[20]

[19] These values correspond to the short-run uncompensated price elasticities estimated by Douenne (2020) and are in line with previous findings on French households (e.g., Clerc and Marcus 2009; Bureau 2011).

[20] In particular, a critical assumption is that people correctly reported their distance traveled and the average fuel economy of their vehicles so that the computation of $\Delta E^{transport}$ is correct. As shown in Table A.2 in the online Appendix, the values reported by respondents follow a distribution very similar to that found in the official statistics.

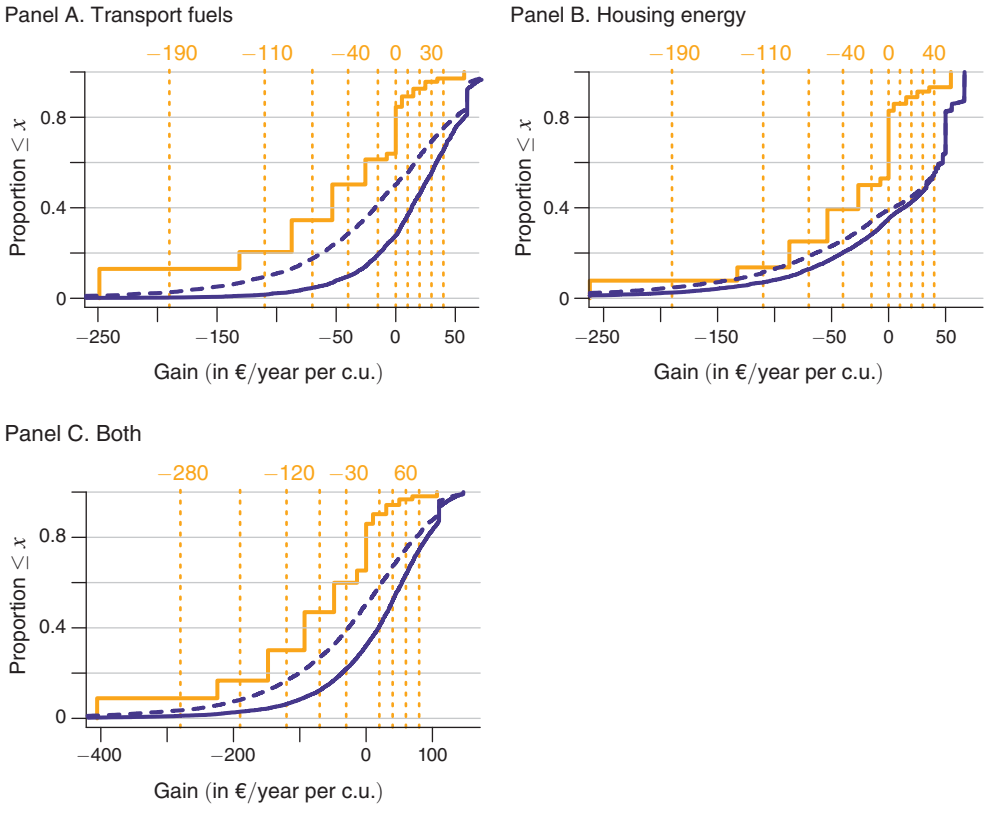

FIGURE 2. CUMULATIVE DENSITY FUNCTION OF OBJECTIVE (DARK BLUE) VERSUS SUBJECTIVE (ORANGE) NET GAINS FROM OUR TAX AND DIVIDEND

Notes: The dashed blue lines represent the distributions of the objective gains in the extreme case of totally inelastic expenditures. The vertical dotted orange lines show the limits of the intervals of the answers to the subjective gains.

II. Pessimistic Beliefs

A. *Self-Interest*

Overestimation of Policy Costs: The analysis of respondents' beliefs about the impact of both sectoral taxes and the carbon tax and dividend policy shows a clear overestimation of the incidence of these taxes on their household. Figure 2 compares the CDF of the objective net gains estimated using official data with the subjective beliefs from our survey for both sectoral taxes and the tax and dividend policy. It is evident from these figures that on average, respondents overestimate the cost of these policies, even in the extreme case of perfectly inelastic expenditures. This result holds both for sectoral carbon taxes on transport and housing energy and the carbon tax and dividend policy. The average objective net gains from the taxes on transport, housing, and both (i.e., the tax and dividend) are €18 per c.u., €6 per c.u., and €24 per c.u., respectively. Extrapolating from our survey, we instead find

average subjective net gains of −€61, −€43, and −€89, respectively.[21] For the tax and dividend, while 70 percent of households should benefit (in monetary terms) from it, only 14 percent think that they would (and 22 percent see themselves as unaffected).[22] The median gap of €116 between objective and subjective gains for this policy indicates a substantial bias toward loss from typical respondents. This bias is widespread, as we find that 89 percent of respondents underestimate their purchasing power gains relative to our household-specific estimation. (The full distribution of respondents' bias is provided in Figure C.2 in the online Appendix.) This proportion remains as high as 77 percent when assuming inelastic expenditures, which provides a lower bound on the share who underestimate their net utility gain.

Heterogeneity in Bias: To characterize profiles of individuals more likely to misperceive their gains, we regress what we call a *large bias* over many respondent characteristics. A large bias is defined as a gap between objectively estimated and subjective net gains greater than €110 per c.u. for our tax and dividend policy.[23] The results given in Table 2 show that having a large bias is largely idiosyncratic: when controlling for a large set of variables[24] (column 1), R^2 remains small (0.06). Nevertheless, we identify several variables having a significant effect on *large bias*, even when controlling the false discovery rate at 5 percent.[25] While we find that environmentalists are less likely to display a large bias and that the standard left-right political leaning has no significant effect, the degree of support for the Yellow Vests movement is positively associated with a large bias. This effect increases with the degree of adhesion, up to 20 p.p. for individuals who reported being part of the movement. Column 3 additionally includes one's attitude toward the policy when it is first presented as a covariate. We see that people who approve of the policy at the initial stage are 28 p.p. less likely to have a large bias than those who do not accept it and 10 p.p. less likely than those who do not know.

Overall, the biases are large and closely related to one's convictions. However, the direction(s) of the causality between beliefs and attitude toward the policy is not resolved at this stage. The results of Section III suggest that people form their beliefs differently depending on their political views and identity, while Section IV shows that perceived outcomes causally influence support.

[21] The subjective intervals are translated into numerical values, assuming that the distribution within each interval is the same as that in the Insee data. Within each bin, we simply take the actual average for the CDF. Among the several methods that we considered to assign numerical values, all realistic ones yield identical results, and we find that the policy costs are overestimated, even in the most conservative approach (taking the maximal bounds of intervals).

[22] For the transport and housing energy taxes, the objective proportions of winners are very similar at 74 percent and 67 percent, respectively, while the subjective shares are 16 percent and 17 percent, respectively (with 22 percent and 30 percent unaffected, respectively).

[23] Indeed, our estimation differs from the true objective gain by more than €110 in only 5 percent of cases. This definition ensures that for the 55 percent of respondents with a large bias, there is a clear gap between their subjective perceptions and their actual net gains. Other definitions yield very similar results.

[24] The control variables used throughout the paper are described in the online Appendix.

[25] To conduct the multiple testing procedure (following Benjamini and Hochberg 1995), instead of associating each dummy with a different null hypothesis, we used *F*-tests of joint nullity for the dummies of each categorical variable and for two additional triplets of variables: those related to household composition and those related to income.

Table 2—Determinants of Bias in Subjective Gains

	Large bias ($\lvert\hat{\gamma} - g\rvert > 110$)		
	OLS	logistic	OLS
Initial tax: PNR (I don't know)			−0.179
			(0.023)
Initial tax: Approves			−0.284
			(0.031)
Yellow Vests: PNR	0.039	0.035	0.024
	(0.036)	(0.035)	(0.036)
Yellow Vests: understands	0.081	0.062	0.041
	(0.025)	(0.024)	(0.025)
Yellow Vests: supports	0.108	0.103	0.051
	(0.026)	(0.025)	(0.026)
Yellow Vests: is part	0.202	0.193	0.147
	(0.048)	(0.040)	(0.047)
Environmentalist	−0.064	−0.061	−0.025
	(0.026)	(0.026)	(0.026)
Left-right: left	−0.066	−0.044	−0.045
	(0.063)	(0.065)	(0.061)
Left-right: center	−0.062	−0.048	−0.046
	(0.065)	(0.068)	(0.064)
Left-right: right	−0.024	−0.010	−0.026
	(0.064)	(0.066)	(0.063)
Left-right: extreme right	−0.076	−0.057	−0.088
	(0.066)	(0.069)	(0.065)
Left-right: indeterminate	−0.009	0.017	−0.007
	(0.061)	(0.063)	(0.060)
Controls: sociodemo, political leaning	✓	✓	✓
Observations	3,002	3,002	3,002
R^2	0.061		0.098

Notes: Standard errors are reported in parentheses. For the logit, the average marginal effects are reported and not the coefficients. The omitted variables are *Yellow Vests: opposes* and *Left-right: extreme left*. The list of controls can be found in the online Appendix. A large bias is defined as a difference between the subjective (g) and objectively estimated ($\hat{\gamma}$) net gain larger than €110/year per c.u.

B. *Environmental Effectiveness*

A well-established result in the literature on the acceptability of climate policies is the perceived ineffectiveness of Pigouvian instruments (e.g., Dresner et al. 2006; Kallbekken, Kroll, and Cherry 2011; Baranzini and Carattini 2017). In particular, people do not see carbon taxes as effective in combating climate change. Our findings confirm this result. Among the respondents who did not receive the information on environmental effectiveness, only 15 percent answered "Yes" when asked whether our tax and dividend would be effective in reducing pollution and fighting climate change, 68 percent answered "No," and 18 percent answered that they did not know.

An explanation sometimes encountered to explain perceptions of ineffectiveness is that most people believe that energy consumption is quite inelastic (Kallbekken and Sælen 2011; Carattini, Carvalho, and Fankhauser 2018). In our survey, we

randomly elicited respondents' perceived price elasticity of either housing or transport energy for French people. We find that higher perceived elasticities are associated with a higher likelihood to believe that the policy is effective, even when controlling for many respondents' characteristics. However, these effects are too modest to explain the perceived ineffectiveness (see the online Appendix). Indeed, among respondents who perceive the policy to be environmentally ineffective, almost half anticipate responses to price changes larger than those in the literature.[26]

A more plausible explanation for perceived ineffectiveness is that people do not believe that the policy would be sufficient to *substantially* affect pollution and climate change. Taking respondents' average anticipated elasticities for transport and housing energy (that are fairly accurate[26]), our tax and dividend policy should reduce French greenhouse gas (GHG) emissions by 5.7 Mt (metric tons) of carbon dioxide equivalents (CO_2e) each year. This reduction corresponds to 0.8 percent of French annual emissions and 0.01 percent of global emissions.[27] Thus, although respondents do anticipate responses to price incentives, our results suggest that they do not perceive a €50/tCO_2 national carbon tax to be a proportionate reaction to climate change.

C. *Progressivity*

It is often argued that a critical barrier to the acceptance of carbon taxation is its perceived distributional impact, in particular the higher burden imposed on lower income households (Bristow et al. 2010; Brannlund and Persson 2012; Gevrek and Uyduranoglu 2015). However, the literature has shown that redistributing the revenue of a carbon tax through uniform lump-sum transfers—i.e., a tax and dividend—can make the policy progressive (West and Williams 2004; Bento et al. 2009; Williams et al. 2015), including for France (Bureau 2011; Douenne 2020). Figure C.3 in the online Appendix displays the average net gain by income decile for our tax and dividend. This figure shows that lower income households would gain more than richer households in both relative and absolute terms. However, among respondents who did not receive the information on the progressivity, only 19 percent of respondents think the policy would benefit the poorest households, compared to 60 percent who declare that it would not and 20 percent who do not know.

III. Determinants of Beliefs

The previous section has shown that people's low acceptance of our tax and dividend correlates with pessimistic beliefs about the properties of the scheme. As knowledge about these properties has been shown to be decisive for acceptance (Carattini, Carvalho, and Fankhauser 2018), it is important to assess how people

[26] Overall, the average subjective elasticities are close to the estimates of the literature for transport (at −0.45) and somewhat overestimated for housing (−0.43). Among those who declared that the policy was not effective, 45 percent (resp. 43 percent) anticipated an aggregate elasticity at or below −0.5 for housing (respectively for transport), while elasticities obtained from the literature are approximately −0.2 for housing and −0.4 for transport.

[27] The computations are based on household carbon emissions simulated from official data. In 2014, French GHG consumption-based emissions were equal to 712 MtCO_2e (Baude et al. 2019). In 2017, global emissions were 53.5 gigatons of CO_2e (UNEP 2018).

Table 3—Share of Respondents with New Beliefs Aligned with Feedback

Feedback:	Aligned with feedback: $G^F = \hat{\Gamma}$	
	win ($\hat{\Gamma} = 1$) (75.8%)	lose ($\hat{\Gamma} = 0$) (24.2%)
Initial belief winner ($g^0 > 0$) (14.0%)	78.8% [73.2%;83.4%]	81.5% [65.0%;91.3%]
Initial belief unaffected ($g^0 = 0$) (21.7%)	21.6% [17.6%;26.2%]	44.9% [33.5%;56.8%]
Initial belief loser ($g^0 < 0$) (64.3%)	12.2% [10.3%;14.5%]	93.9% [90.9%;96.0%]
Initial belief affected ($g^0 \neq 0$) (78.3%)	26.1% [23.7%;28.7%]	92.9% [89.8%;95.1%]
All (100%)	25.1% [23.0%;27.3%]	85.7% [82.2%;88.7%]

Notes: The 95 percent confidence intervals for the binomial probabilities are given in brackets. The table reads as follows: among those who initially think that they would win ($g^0 > 0$) but are told that they are expected to lose ($\hat{\Gamma} = 0$), 81.5 percent agree that they would lose ($G^F = 0$). The feedback $\hat{\Gamma}$ is not a random draw but a deterministic outcome of the characteristics reported by the respondents in the survey. The subsample used is the 1,968 respondents who received feedback.

form their beliefs. In the following, we test respondents' reactions to information about their gains, environmental effectiveness, and progressivity. If overly pessimistic views simply reflected a lack of knowledge, we would expect them to revise their beliefs after they receive new information, which is what we refer to as "updating."

A. *Self-Interest*

Pessimism in the Revision of Beliefs.—Our respondent-specific estimation of net gains (see Section IC) enables us to tell respondents that given their characteristics, they have a five-in-six chance to "win" or "lose" from the policy. We can then examine how they update their beliefs about their win/lose category after receiving this information. The full transition matrices of people's beliefs are given in Tables D.2 and D.3 in the online Appendix. More concisely, Table 3 reports the share of respondents whose beliefs after being informed are aligned with our feedback with the corresponding 95 percent binomial confidence intervals. It shows a highly asymmetric response depending on the feedback received. On the one hand, for the 24 percent of individuals who receive "lose" feedback ($\hat{\Gamma} = 0$), the ex post belief is, on average, consistent with the fact that 83 percent of them do, in reality, lose from the tax and dividend policy, with 86 percent endorsing our "lose" feedback. If anything, these people would rather tend to *agree too much* with our noisy signal, especially when excluding people who initially consider themselves to be unaffected (i.e., focusing on $g^0 \neq 0$). On the other hand, the 76 percent who received "win" feedback ($\hat{\Gamma} = 1$) appear to be much more conservative in their revision since only 25 percent of them endorse the "win" feedback. Among the respondents who initially thought that they would lose in this group, a mere 12 percent switch their answer from "lose" to "win." This is in sharp contrast to the respondents who initially thought they would win and receive "lose" feedback, since 82 percent of

them endorse our prediction. Thus, pessimistic beliefs persist despite our treatment, while optimistic beliefs do not.

Table D.4 in the online Appendix conducts the same analysis for the 28 percent of respondents whose gain is very positive or very negative, i.e., above €110 per c.u. in absolute terms. For these respondents, our out-of-sample prediction of the win/lose category is correct in 99 percent of the cases, as seen in Figure C.1 in the online Appendix. The alignments with our feedback are similar between the whole sample and these respondents for whom we are certain to make a correct prediction. The similarity of alignments for different prediction accuracies rules out the possibility that a large fraction of respondents do not update their beliefs, because their private information would be *truly* more accurate than our prediction.

Causal Effect of Feedback on Beliefs.—To be relevant, our feedback on respondents' win/lose category had to be customized and as accurate as possible. Since our information intervention was not a randomized treatment, we have yet to identify the causal effect of receiving *win* versus *lose* feedback on respondents' beliefs. Despite this nonrandom assignment of the treatment, there is still a way to estimate the causal effect of the feedback on people's beliefs. The binary win/lose feedback is a variable $\hat{\Gamma}$ that jumps from 0 to 1 when our continuous estimation of respondents' net gains $\hat{\gamma}$ exceeds the zero threshold. The following equation enables us to estimate the threshold effect around zero net gain in an RDD and, thus, to obtain the effect of receiving *win* compared to *lose* feedback:

$$G_i^F = \alpha_0 + \alpha_1 \hat{\Gamma}_i + \alpha_{\gamma,1} \hat{\gamma}_i + \alpha_{\gamma,2} \hat{\gamma}_i{}^2 + \alpha_{\mathbf{C}} \mathbf{C}_i + \alpha_{\mathbf{I}} \mathbf{I}_i + \eta_i. \tag{2}$$

The dependent variable G^F represents respondents' belief about their gain after the feedback. It is equal to 0 if they believe they lose and to 1 otherwise. The term $\mathbf{C}$ is a set of respondents' characteristics, and $\mathbf{I}$ a set of variables describing their households' incomes. The two terms $\hat{\gamma}$ and $\hat{\gamma}^2$ allow for a quadratic specification for estimated net gains to properly isolate the effect of receiving positive rather than negative feedback. To better identify the threshold effect in the RDD, we focus on the subsample of respondents whose estimated net gain $\hat{\gamma}$ is close to 0 (less than €50 per annum in absolute value). Table 4 displays the results, including our main specification (column 1), the same estimation performed on the full sample (column 2), a modified version of column 1 where interaction terms have been added to study heterogeneous treatment effects (column 3), and a specification where the dependent variable corresponds to the belief to *win* instead of to *not lose* (column 4).

Everything else being equal, after receiving the feedback, respondents' predicted winners are 27 p.p. more likely to believe they do not lose than respondents' predicted losers (column 1). They are also 15 p.p. more likely to believe that they win (column 4). Interestingly, column 3 shows that the effect of the informational treatment is heterogeneous. From the two interaction terms, we see that among the respondents who initially accepted the tax and dividend policy (i.e., when it was first presented), the effect of the *win* feedback on the belief not to lose goes from 27 p.p. to 41 p.p., while for those supportive of the Yellow Vests—who declared themselves

Table 4—Effect of Feedback on Belief of Winning

	Believes does not lose			Believes wins
	(1)	(2)	(3)	(4)
Predicted winner ($\hat{\Gamma}$)	0.269	0.208	0.246	0.153
	(0.058)	(0.035)	(0.064)	(0.045)
Initial tax acceptance (A^0)	0.306	0.331	0.015	0.310
	(0.066)	(0.038)	(0.087)	(0.051)
Yellow Vests supporter			0.019	
			(0.058)	
$\hat{\Gamma} \times A^0$			0.166	
			(0.079)	
$\hat{\Gamma} \times$ Yellow Vests supporter			−0.151	
			(0.069)	
$\hat{\Gamma} \times G$			0.080	
			(0.079)	
Controls: incomes (piecewise continuous), estimated gains, sociodemo, other motives	✓	✓	✓	✓
Controls: initial win/lose category (G)			✓	
Subsample	$\lvert\hat{\gamma}\rvert < 50$		$\lvert\hat{\gamma}\rvert < 50$	$\lvert\hat{\gamma}\rvert < 50$
Observations	757	1,968	757	757
R^2	0.301	0.320	0.419	0.253

Notes: Standard errors are reported in parentheses. The list of controls can be found in the online Appendix.

to be part of or support the movement—it goes down to less than 10 p.p. Thus, the information provided to respondents is processed differently depending on their attitude toward the policy. While the treatment is rather effective at convincing those most favorable to the policy about its impact on their household, those most opposed to it do not appear to be receptive to this information.

Mechanisms.—There are several ways to rationalize pessimistic and heterogeneous beliefs against the tax and dividend. We propose the following four mechanisms: distrust, uncertainty, motivated reasoning, and intentional misreporting.

Distrust: The first mechanism is that respondents distrust what we present to them. They may perceive our information to be biased and think that we wrongly estimated their likelihood of winning and are overly optimistic.[28] As a result, they may discount our new information relative to their prior belief or assign relatively more weight to our information when it is pessimistic. This distrust may stem from an impression that experts understate the costs of a carbon tax or that the government will break its promise to pay the dividend. For instance, Sapienza and Zingales (2013) report that 51 percent of Americans are skeptical that their government would deliver on using the proceeds of a carbon tax to reduce other taxes (see also Dresner

[28] Another possibility is that respondents give too much value to their private information relative to the base rate information. That is, pessimistic winners might be overconfident in seeing themselves as specific, so they partly discard the new information, e.g., by thinking that they are part of the one-sixth for whom our prediction is erroneous, perhaps because they believe that they always lose from new policies more than others.

et al. 2006; Hsu, Walters, and Purgas 2008). A similar level of skepticism regarding the dividend could explain much of the pessimism about net gains.

Uncertainty: The second mechanism stems from people's uncertainty regarding their gain. That uncertainty would make them see their possible gain as a distribution (see Stiglitz 2019). Then, instead of reporting the average of this distribution, loss-averse people would conservatively estimate their gains. Also, the effect of uncertainty on updating is ambiguous. On the one hand, uncertain people could be more likely to rely on our base rate information; on the other hand, their subjective probability of losing could remain high despite our information.

Motivated Reasoning: The third mechanism to explain the observed asymmetry in belief revision is that some people have a strong skeptical attitude toward the carbon tax that affects the formation of their beliefs. They would engage in motivated reasoning, i.e., update their beliefs in a way that is consistent with their initial views (Druckman and McGrath 2019; Little 2019) rather than integrate information in a way that leads to accurate conclusions. Although motivated reasoning is linked to distrust since it also involves neglecting information, in the case of distrust, people discard information because they do not trust its source; for motivated reasoning, they dismiss the information when its content contradicts preexisting views. Motivated reasoning entails a deviation from Bayesian updating—contrary to the first two mechanisms—but it can still be *rationalized* as a psychological adaptation to preserve one's sense of identity (Kahan 2013). By analyzing the determinants of a correct update, the online Appendix shows that those who initially disapprove of the tax and dividend and those who are part of the Yellow Vests movement are significantly less likely to correctly update their win/lose belief after feedback (by 18 and 14 p.p., respectively), even controlling for their prior belief. This finding and the related one from Section IIIA are consistent with motivated reasoning since political views and identity are correlated with the way people form their beliefs (see the online Appendix for a discussion). However, we did not demonstrate the causality, and this correlation could also stem from higher distrust (or higher uncertainty) among these groups.

Intentional Misreporting: A fourth possibility is that some respondents intentionally report overly pessimistic beliefs compared to what they actually think. This could stem from a rejection of the tax and could follow from strategic thinking if they believe that their survey answers might influence policymakers. Such respondents could be aware that they would gain but still reject the tax for other motives, even more so if they are still uncertain about their gain. Their misreporting could also be due to a type of motivated reasoning that would not directly affect their beliefs but rather induce them to misreport what they think. This could help them justify their rejection of the policy, even more so in that it could be costly for their egos to admit that they were wrong to reject the policy.

B. *Environmental Effectiveness*

Table D.5 in the online Appendix reports the effect of displaying relevant information on the belief that our tax and dividend is environmentally effective. The effect of reporting a scientific consensus on environmental effectiveness (E) is positive and statistically significant, but its magnitude—approximately 5 p.p.—seems modest given that the question immediately follows the information intervention. The effects of information on climate change (CC) or particulates (PM) are smaller, and only CC is significant, which is understandable since we displayed the information at the very beginning of the survey and it does not mention any environmental policy. As suggested by Millner and Ollivier (2016), given the complexity of the mechanisms at play, drawing a causal link between the causes and consequences of environmental problems requires considerable cognitive effort, making it difficult to convince one of the effectiveness of policies that decentralize efforts to address pollution. Finally, we observe that our information interventions have no significant effect on beliefs about the causes and consequences of climate change. Overall, these treatments appear to be insufficient to change most people's minds about climate change and carbon tax effectiveness.

C. *Progressivity*

Table D.6 in the online Appendix reveals the absence of an effect of explaining that our tax and dividend is progressive on perceived progressivity. The correlation between the two is close to 0 (at -0.006) and even has an unexpected negative sign. Column 2 of the same table clarifies why our treatment does not change the overall share of people who think that the policy is regressive. Those who have a large bias in their perception of gains are in fact *more* prone—by 13 p.p.—to perceive *regressivity* once provided the information. This result may be a manifestation of the boomerang effect with people inclined to motivated reasoning, which has already been documented for Republican attitudes on climate change in the United States (Zhou 2016). Indeed, Hovland, Janis, and Kelley (1953) showed that when someone is pressured to make a certain choice, psychological reactance (theorized by Brehm 1966) can cause them to resist this pressure by adopting an opposite alternative. Although the effect on those without a large bias is not significant, providing them with information is associated with a lower perceived regressivity by 5 p.p. To conclude, without a deep explanation of the underlying mechanisms, the progressivity of the policy remains unintuitive for most people, and we cannot convince them easily.

IV. How Beliefs Determine Attitudes

Our results clearly indicate that at present, a carbon tax is unlikely to be accepted in France. However, we have also shown that people display overly pessimistic perceptions about the true effects of the policy. Most of them overestimate the negative impact on their purchasing power, think that the policy is regressive, and do not see it as environmentally effective. In this section, we examine to what extent the low

acceptance rate reflects intrinsic preferences or incorrect perceptions. The question we address is whether convincing people about the actual incidence of the policy and its effectiveness would be sufficient to generate public support.

A. *Self-Interest*

Identification Challenge: Among the three-quarters of the respondents expected to win from our tax and dividend, 62 percent both consider that they would not win and disapprove of the policy. We want to estimate to what extent knowing that they would win would lead them to approve of the reform. Because respondents thinking that they would win might differ in many respects from those thinking that they would not, we need a specific identification strategy to estimate the causal effect of the perception of winning on approval.

Main Identification Strategy: To identify the effect of self-interest on acceptance ceteris paribus, we exploit exogenous variations in gains and losses. To do so, we consider a tax and targeted dividend, where respondents are randomly assigned to a compensation scheme for which they are or are not eligible depending on their income (see Section IB). Formally, we denote $I_{i,1}$ as the income percentile of respondent i and $I_{i,2}$ as that of the second adult in their household if there is one. We define the eligibility of adult $j \in \{1;2\}$ as follows:[29]

$$T_{i,j} = \begin{cases} 0, & \text{if } I_{i,j} > t_i; \\ 1, & \text{otherwise,} \end{cases} \tag{3}$$

where $t_i \in \mathcal{T} = \{20;30;40;50\}$ is the eligibility threshold randomly allocated to household i (see Section IB). Since eligibility increases the likelihood of believing that one wins from the policy—without necessarily implying it—our method leads to a fuzzy RDD where eligibility corresponds to the intention to treat and the respondents who believe that they will win from the tax and targeted dividend correspond to the treated. Formally, we denote G_i^T as a dummy variable equal to 0 if respondent i thinks that they would lose from the tax and targeted dividend and to 1 otherwise. Similarly, A_i^T is a dummy variable equal to 0 if respondent i disapproves of this policy and to 1 otherwise. We can then write the model as a two-stage least squares (2SLS) with the following first-stage equation:

$$\begin{aligned} G_i^T = {} & \alpha_0 + \alpha_{T,1} T_{i,1} + \alpha_{T,2} T_{i,2} + \alpha_{T,3}\left(T_{i,1} \times T_{i,2}\right) \\ & + \sum_{k \in \mathcal{T}} \alpha_k \mathbf{1}_{t_i=k} + \alpha_S S_i + \alpha_{\mathbf{C}} \mathbf{C}_i + \alpha_{\mathbf{I}} \mathbf{I}_i + \eta_i, \end{aligned} \tag{4}$$

where $\mathbf{C}_i$ is a vector of respondent characteristics, S_i is a dummy variable equal to 1 when there is a single adult in the household, and $\mathbf{I}_i$ is a vector of income variables defined as $(I_{i,j}, (\min(I_{i,j} - k, 0))_{k=20,70})'_{j=1,2}$. Note, $\mathbf{I}_i$ allows for a continuous piecewise linear relationship in incomes with slope changes at the twentieth and

[29] As explained in Section IB, we explicitly limit the number of beneficiaries to two per household.

TABLE 5—FIRST-STAGE REGRESSIONS RESULTS FOR SELF-INTEREST

	Believes does not lose		
	Targeted dividend (G^T)		After feedback (G^F)
	(1)	(2)	(4)
Transfer to respondent (T_1)	0.199	0.224	
	(0.034)	(0.030)	
Transfer to spouse (T_2)	0.172	0.156	
	(0.042)	(0.039)	
$T_1 \times T_2$	−0.145	−0.158	
	(0.045)	(0.037)	
Predicted winner ($\hat{\Gamma}$)			0.269
			(0.058)
Initial policy acceptance (A^0)	0.123	0.154	0.306
	(0.041)	(0.033)	(0.066)
Controls: incomes (piecewise continuous), estimated gains, sociodemo, other motives	✓	✓	✓
Controls: policy assigned	✓	✓	
Subsample	[p10; p60]		$\lvert\hat{\gamma}\rvert < 50$
Effective *F*-statistic	15.6	23.8	21.3
Observations	1,969	3,002	757
R^2	0.221	0.196	0.301

Notes: In columns 1 and 2, we use the random eligibility for the dividend (conditional on income) as a source of the exogenous variation in the belief. In column 4, we use the discontinuity in the win/lose feedback when the net gain switches from negative to positive. The column numbers correspond to second-stage results, as given in Table 6.

seventieth percentiles. We also introduce fixed effects for the policy assigned $\mathbf{1}_{t_i=k}$ ($k \in \mathcal{T}$) to control for preferences regarding the specificities of the policy, i.e., the share of the population targeted by the policy and the value of the dividend. To obtain more precise estimates, we control for initial acceptance of our tax and dividend since this should explain much of the variation in the dependent variable. Finally, the second stage is written as follows:

$$A_i^T = \beta_0 + \beta_1 \hat{G}_i^T + \sum_{k \in \mathcal{T}} \beta_k \mathbf{1}_{t_i=k} + \beta_S S_i + \beta_{\mathbf{C}} \mathbf{C}_i + \beta_{\mathbf{I}} \mathbf{I}_i + \varepsilon_i, \tag{5}$$

where $\hat{G}_i^T$ denotes the fitted value of G_i^T from the first-stage regression. As seen from the first-stage results shown in Table 5 below, the eligibility of both respondents and households' second adults are positively correlated with beliefs about winning, and so both instruments are relevant. The exclusion restriction states that conditional on income, being eligible affects approval solely through beliefs on winning. The RDD procedure employed in the first stage ensures that this is the case. Conditional on income, eligibility is random, and by controlling for the specific policy assigned ($\mathbf{1}_{t_i=k}$), it should affect acceptance only through self-interest.

Alternative IV Identification: As an alternative identification strategy, we exploit a methodology similar to the main specification—i.e., a fuzzy RDD—but apply it to the customized feedback. Indeed, we use our estimation of respondents' net gains $\hat{\gamma}$ as the assignment variable and the binary win/lose feedback $\hat{\Gamma}$ as the intention to treat. Since our feedback $\hat{\Gamma}$ (which jumps from 0 to 1 at the threshold of zero net gain) is predictive of the belief about the win/lose category after feedback (see

Table 6—Effect of Self-Interest on Acceptance

	Acceptance ("Yes" or "Don't know" to policy support)			
	Targeted dividend (A^T)			
	IV: random target/ eligibility		OLS	After feedback (A^F) IV: discontinuity in feedback
	(1)	(2)	(3)	(4)
Believes does not lose (G)	0.534	0.476	0.438	0.644
	(0.132)	(0.106)	(0.014)	(0.170)
Initial policy acceptance (A^0)	0.356	0.354	0.361	0.420
	(0.041)	(0.034)	(0.026)	(0.074)
Controls: incomes (piecewise continuous), estimated gains, sociodemo, other motives	✓	✓	✓	✓
Controls: policy assigned	✓	✓	✓	
Subsample	[p10; p60]			$\lvert\hat{\gamma}\rvert < 50$
Effective F-statistic	15.6	23.8		21.3
Observations	1,969	3,002	3,002	757
R^2	0.320	0.308	0.472	0.541

Notes: Standard errors are reported in parentheses. The list of controls can be found in the online Appendix. The source of the exogenous variation in the belief used in the first-stage estimations for the targeted dividend is the random assignment of the income threshold, which determines eligibility for the dividend. The first stage for the nontargeted dividend instead exploits the discontinuity in the win/lose feedback when the net gain switches from negative to positive.

Section IIIA), G^F, we can determine the effect of this belief on acceptance after feedback, A^F. This alternative fuzzy RDD leads to the following 2SLS:

$$G_i^F = \alpha_0 + \alpha_1 \hat{\Gamma}_i + \alpha_{\gamma,1}\hat{\gamma}_i + \alpha_{\gamma,2}\hat{\gamma}_i{}^2 + \alpha_{\mathbf{C}}\mathbf{C}_i + \alpha_{\mathbf{I}}\mathbf{I}_i + \eta_i, \tag{6}$$

$$A_i^F = \beta_0 + \beta_1 \hat{G}_i^F + \beta_{\gamma,1}\hat{\gamma}_i + \beta_{\gamma,2}\hat{\gamma}_i{}^2 + \beta_{\mathbf{C}}\mathbf{C}_i + \beta_{\mathbf{I}}\mathbf{I}_i + \varepsilon_i, \tag{7}$$

where $\hat{G}_i^F$ denotes the fitted value of G_i^F from the first-stage regression. The first-stage equation thus corresponds to our main specification of the RDD estimated in Section IIIA (column 1 in Table 4). The identification assumption of this second IV states that conditional on estimated net gains ($\hat{\gamma}$)—which we control for with a quadratic specification—receiving a win feedback ($\hat{\Gamma} = 1$) affects approval solely through self-interest. As before, we restrict our analysis to respondents close enough to the threshold by retaining only those with net gains below €50 per annum in absolute value ($\lvert\hat{\gamma}\rvert < 50$).

Results: The first-stage regression results are given in Table 5. The effective F-statistics (Olea and Pflueger 2013) range from 15.6 to 21.3, indicating that both targeted transfers and feedback are strong instruments. Table 6 provides the second-stage results. Overall, the estimated effect of self-interest indicates that believing that one would not lose increases acceptance by more than 50 p.p. (53 p.p. in our main specification). Both IV strategies yield consistent results, although they apply to different policies since revenue recycling is not designed in the same manner, and they are estimated on different subsamples since compliers are not the same in these two specifications.

Alternative Specifications for Robustness: In our main specification (column 1 in Table 6), we exclude households where none of the adults have an income from the tenth to sixtieth percentiles to keep only those close enough to the thresholds. In an alternative specification (column 2), we replicate the same estimation using the full sample. In column 3, we also compare our results with a simple OLS regression on the full sample. Finally, we investigate alternative versions of the previous models in the online Appendix. We estimate the effect of winning instead of not losing and of approval instead of acceptance (online Appendix Table E.1). We estimate our main specification with the slope of incomes changing at additional thresholds (thirtieth, fortieth, fiftieth, or sixtieth percentile). Finally, we allow for heterogeneous effects along the income dimension (online Appendix Table E.2).

Overall, our main point estimate is robust to these alternative specifications. For example, compared to our main point estimate of 53 p.p., the full sample and simple OLS point estimates are close at, respectively, 48 p.p. and 44 p.p., and we find no significant heterogeneity in the effect along the income dimension.

B. *Environmental Effectiveness*

Main Identification Strategy: One of the strongest barriers to carbon tax implementation is a widespread perception of its environmental ineffectiveness. Our objective is therefore to assess to what extent learning about the environmental benefits of the tax could increase support. To identify this effect, we estimate a 2SLS model where the first stage uses random information to predict beliefs about environmental effectiveness, while the second stage regresses acceptance on the fitted exogenous variations in these beliefs. Because information on particulate matter (Z_{PM}) is poorly correlated with beliefs regarding effectiveness, we restrict the set of instruments to our informational treatments on the scientific consensus (Z_E) and climate change (Z_{CC}). Although these treatments do not have a very large effect on people's beliefs (as discussed in Section IIIB), these instruments are significantly related to our endogenous variable. Denoting $\dot{A}^0$ as the dummy for initial approval of the tax and dividend and $\dot{E}$ as the dummy for the belief that the policy is environmentally effective, we can write a 2SLS model as follows:

$$\dot{E}_i = \alpha_0 + \alpha_1 Z_{E,i} + \alpha_2 Z_{CC,i} + \alpha_{\mathbf{C}} \mathbf{C}_i + \eta_i, \tag{8}$$

$$\dot{A}_i^0 = \beta_0 + \beta_1 \hat{\dot{E}}_i + \beta_{\mathbf{C}} \mathbf{C}_i + \varepsilon_i, \tag{9}$$

where $\hat{\dot{E}}_i$ denotes the fitted value of $\dot{E}_i$ from the first-stage regression and $\mathbf{C}$ is a vector of characteristics. Acknowledging that our information intervention could affect acceptance motives other than effectiveness alone, we include other motives in our list of control variables to avoid potential bias.

Results: The first-stage regression results can be found in Table 7. To avoid the problem of weak instruments in our main specification, we adopt strict definitions for our variables (i.e., the answer *"Yes,"* denoted by a dot, to the belief in effectiveness and approval) since they yield a higher effective F-statistic: 11 instead of

Table 7—First-Stage Regressions Results for Environmental Effectiveness

	Environmental effectiveness	
	"Yes" (1; 3)	not "No" (A4)
Info on environmental effectiveness (Z_E)	0.059	0.062
	(0.014)	(0.017)
Info on climate change (Z_{CC})	0.028	0.030
	(0.013)	(0.017)
Controls: sociodemo, other motives, incomes, estimated gains	✓	✓
Effective F-statistic	11.2	6.0
Observations	3,002	3,002
R^2	0.123	0.121

Notes: Regarding the column names, A4 refers to columns with alternative second stages in online Appendix Table E.3. We use the information randomly displayed about climate change (Z_{CC}) and the effectiveness of carbon taxation (Z_E) as sources of exogenous variation in the belief. We chose the set of instruments that maximizes the effective F-statistics. The Sargan test does not reject the validity of our overidentification restrictions (p-value of 0.93).

Table 8—Effect of Believing in Environmental Effectiveness on Approval

	Initial tax and dividend		
	Approval ($\dot{A}^0$)		Acceptance (A^0)
	IV (1)	OLS (2)	IV (3)
Believes in effectiveness ($\dot{E}$)	0.416	0.374	0.505
	(0.168)	(0.013)	(0.242)
Instruments: info E.E. and C.C.	✓		✓
Controls: sociodemo, other motives, incomes, estimated gains	✓	✓	✓
Effective F-statistic	11.2		11.2
Observations	3,002	3,002	3,002
R^2	0.161	0.342	0.218

Notes: Standard errors are reported in parentheses. The list of controls can be found in the online Appendix. The dependent variable corresponds to either initial approval (answer "Yes" to support of the policy) or acceptance (answer not "No"). The first stage exploits the information randomly displayed about climate change (C.C.) and the effectiveness of carbon taxation (E.E.) as exogenous instruments.

6 for broad definitions (*not "No"*). Table 8 reports the results of the second stages. They all consistently indicate a strong positive and significant effect of beliefs about environmental effectiveness on support for the policy. All else equal, believing that the tax is effective increases the likelihood of approving it by 42 p.p. (column 1).

Alternative Specifications for Robustness Checks: In addition to the 2SLS (specification 1), we estimate an OLS (column 2) model with strict definitions for our variables. We also estimate other specifications with different definitions for our variables. The 2SLS in column 3 employs acceptance instead of approval as the dependent variable. In the online Appendix, we estimate a 2SLS with broad definitions only as well as two OLS regressions (acceptance regressed on *"Yes"* as well as

not "*No*" responses to environmental effectiveness). As a robustness check, we also estimate our main specification using a limited information maximum likelihood (LIML).

Overall, our main result is robust to these alternative specifications. The effect estimated with the OLS (column 2) is only slightly lower than the local average treatment effect estimated in our main specification—38 versus 42 p.p. Given the exogeneity of our instruments, the only concern of the relative weakness of our instruments is a potential bias toward the OLS, which—as suggested by the results of column 2—would entail estimates that are too conservative in our case. Finally, we obtain identical results when running a 2SLS or an LIML for our main specification (column 1). Regarding the broad definition of acceptance, the LIML estimate is broadly consistent with the 2SLS result (column 3), though it is somewhat higher: 64 p.p. versus 51 p.p. (column A2 of Table E.3 in the online Appendix).

C. *Progressivity*

Since informing respondents does not convince them that our tax and dividend policy is progressive (see Section IIIC), we cannot perform an IV estimation to identify the causal effect of understanding the progressivity on support for the policy. In our online Appendix, we estimate how one's belief in progressivity—interacted with other motives—correlates with acceptance using simple OLS and logit regressions. Controlling for many respondent characteristics and other motives for support, the effect of progressivity remains statistically significant and as high as 27 p.p. in our preferred specification. Of course, this result should be interpreted with caution since we can still suspect that the results are affected by unobserved confounders and reverse causality.

To conclude, these results show that convincing citizens of the true incidence and effectiveness of a tax and dividend could substantially increase support for such a policy. Extrapolating from the causal effects that we find for self-interest and environmental effectiveness, we estimate that if everyone could be convinced that the reform is environmentally effective, acceptance would reach 61 percent (and approval would reach 45 percent), and if everyone could learn their win/lose category, acceptance would reach 47 percent. Thus, if people could be convinced on more than one motive, the tax and dividend would presumably be supported by a large majority, although we cannot causally identify the interaction effect of several motives combined (see the online Appendix for noncausal estimates). Our results also qualify the findings of Anderson, Marinescu, and Shor (2019), who suggest that ideology better predicts carbon tax acceptance than self-interest does. By distinguishing beliefs from preferences, our results suggest that ideology plays an indirect role by shaping beliefs about one's self-interest and that beliefs directly affect acceptance.

V. Conclusion

In this paper, we study how beliefs about a policy form and then determine attitudes toward it. We investigate this question through the study of carbon taxation in

France during the Yellow Vests movement that started to protest fuel price increases. Our analysis is based on a new survey and official household survey data, enabling us to compare subjective beliefs with objective impacts on French households. We find that 70 percent disapprove of a carbon tax and dividend policy, which can be explained by pessimistic beliefs about its properties. Of our survey respondents, 89 percent overestimate its negative impact on their purchasing power, and most of them do not perceive it as environmentally effective or progressive. Pessimistic beliefs appear correlated with people's support for the scheme: the more they oppose the mechanism, the more pessimistic they are. The causality between beliefs and attitudes toward the policy could actually go in both directions. People more opposed to the tax are more (pessimistically) biased in their processing of *new* information with respect to it, suggesting that political views and identity shape beliefs about the impacts of taxation. We simultaneously find that beliefs causally determine acceptance and that if people could be convinced about the incidence and effectiveness of a tax and dividend, this policy would likely be accepted by a majority, given the large effects of these motives (approximately 50 p.p. each).

However, our treatments that provide accurate arguments in favor of the scheme mostly fail to convince people. The pessimism could be related to a strong distrust of the government, as documented, e.g., in Alesina, Stantcheva, and Teso (2018) and Foucault et al. (2019), echoing recent findings that the ambition of climate policies increases with the level of trust (Rafaty 2018). These results leave us with three main challenges. First, since it is unlikely that the issue of trust can be resolved in the short run, it seems necessary to find climate policies that would be accepted by a majority. We address this question in a companion paper (Douenne and Fabre 2020) in which we assess both knowledge and beliefs about climate change and the preferred policies of French people. Second, since trust in the government needs to be restored in the longer run, it is crucial to analyze what causes distrust and how it can be overcome. Third, it is important to assess to what extent the mechanisms of belief formation and their effects on political attitudes that we document can be generalized to other policies and other contexts. Although rejection of the tax may be lower in a different country, biases in perceptions and political polarization may occur everywhere. Thus, a lesson must be learned for policy design and implementation to avoid another carbon tax debacle *à la Française*.

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