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Complexity and Learning Effects in Voluntary Climate Action: Evidence from a Field Experiment*

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Abstract

We test for the presence of complexity aversion in a context where individuals interact with complex regulatory frameworks. Exploiting rules of the EU Emission Trading System with counter-intuitive implications, we test how individuals act under different information sets. We find evidence of a clear and consistent “learning effect” across all information treatments, regardless of whether they are simple but counter-intuitive or intuitive but complex. There is no evidence of motivated beliefs. Our findings highlight the importance of communicating the choice-relevant implications of complex contexts to households and show that simple factual statements and detailed explanations are equally effective.

Keywords: climate action; cap-and-trade; complexity aversion; information provision; field experiment

JEL classification: C93, D83, Q54

1 Introduction

There is an implementation gap in climate policy. The set of instruments in place is not sufficient to achieve the global and national emission targets (Lecocq et al., 2022; Liu and Raftery, 2021). Voluntary climate action by households might help in closing this gap. However, knowing which actions are most effective in reducing emissions is challenging. On top of technical details and economic interdependence, complex regulatory frameworks further contribute to counter-intuitive chains of causes and effects.

In this paper we take the EU Emission Trading System (ETS) as a starting point. It has been shown that it creates choice situations faced by households where common sense is a poor guide. For example, trying to reduce ones carbon footprint can increase total emissions (Perino, 2015) and cancelling emission allowances immediately can be ineffective (Gerlagh and Heijmans, 2019). Moreover, nobody seems to have an incentive to educate consumers about these effects (Herweg and Schmidt, 2022).

We report results from a survey-based, revealed-preference experiment with a large sample of German households where they face a choice between an intuitive but ineffective and a counter-intuitive but effective option to reduce greenhouse gas (GHG) emissions. Using different informational treatments, we investigate how effective simple and complex messages are in increasing the effectiveness of voluntary climate action and whether such efforts in educating households have a detrimental effect on engagement.

Previous research indicates that humans display complexity aversion, that is, the preference for simple over complicated alternatives, all else equal. The complexity of a choice task has been shown to affect behavioral decision-making, often leading to inconsistencies with rational choice models and to objectively undesirable outcomes. That complexity aversion may not be limited to the choice tasks typically used in laboratory research

is suggested by “simplicity theory” (Puri, 2020; Fudenberg and Puri, 2021, 2022), which allows for different “complexity costs” associated with the alternatives in general choice tasks.

Our study is motivated by the observation that complexity aversion naturally interacts with information uptake (Goodman, 2014; Zilker et al., 2020), and that this may have important consequences for the effectiveness of climate action, as well as other instances where complex regulatory frameworks and decentralised decisions jointly determine aggregate outcomes. A prime example is the EU ETS, whose rules make it very difficult to predict whether additional individual, corporate or government efforts reduce carbon emissions and by how much – to such an extent that work-horses of climate policy, such as renewable support (Jarke and Perino, 2017), energy efficiency measures (Jarke-Neuert and Perino, 2020), and coal phase-outs (Rosendahl, 2019) and voluntary climate action by households (Perino, 2015) can – at least at the margin – actually *increase* total emissions.

The particular counter-intuitive effect we focus on in this study is due to the 2018 reform that enabled the so-called Market Stability Reserve (MSR) to permanently cancel allowances automatically (Perino, 2018). Before the reform, retirement of one European Union Allowance (EUA) from the market saved exactly one ton of carbon dioxide from being emitted. This equality made the EU ETS a *simple* and practical tool for individuals, NGOs and national governments to force abatement over and above public policy goals (Doda et al., 2021; Kohleausstiegsgesetz, 2020; Ministry of the Environment, 2016).

Since the 2018 reform, however, the one-to-one correspondence no longer applies: the *timing* of retirement matters for the size of the abatement effect. In particular, the immediate retirement of an emissions allowance is less effective than delayed retirement (Gerlagh and Heijmans, 2019). This fact conflicts with most people’s intuition that climate protection efforts should be intensified sooner, rather than later. Thus, without

detailed knowledge about the implications of the EU ETS – knowledge that even climate policy experts do not necessarily have – the less effective alternative of immediate retirement is likely selected over the more effective one of delayed retirement of an emissions allowance. Swaying people towards the more effective, but less intuitive alternative requires to *convincingly* inform them about the consequences implied by the regulatory framework of the EU ETS.

As such information is complex, the timing of voluntary retirement of EUAs is not only a convenient tool to study the impact of complexity and learning in a real world setting, but also a practical problem that climate-conscious consumers, companies and governments actually face. For example, there are NGOs that are specialized on retiring EUAs, as well as companies that offer such services.¹ Most notably, the German government has committed itself to cancel allowances as part of the coal phase-out, yet has not specified the timing (Kohleausstiegsgesetz, 2020).

To illustrate this issue, consider the real choice task that we conceived for the present study: subjects could either waive a monetary payment of €5 to retire an EUA immediately (option *A*), or waive the payment to retire the EUA in one year's time (option *B*), or keep the payment for themselves (option *C*). Given the design of the EU ETS at the time of conducting the experiment, option *A* reduces total emissions by 0.24 tons of carbon dioxide less than option *B* due to repercussions in the MSR. We hypothesize that the provision of this information and its explanation shifts choice probability mass from *A* to *B* ("learning effect").

But processing this information is cognitively costly, as it can be long and complex, depending on the specific treatment received. Applying findings by Sonsino et al. (2002)

¹For Tomorrow gGmbH and Compensators e.V. are NGOs that offer delayed cancellation of EUAs while CarbonKiller cancels them immediately. A company specialized on delayed cancellation of EUA is Cap2 GmbH.

to our context, information provision may lead people to stick to the less effective alternative *A* or switch to outside option *C*, either because they are paralyzed by cognitive load or to avoid the complexity of engaging with the information provided and choosing between *A* and *B* altogether. We call this the “complexity effect” of information provision. In principle, if the complexity effect is strong and the learning effect is weak, information provision could lead into a “complexity trap,” that is, choice performance could actually worsen, i.e. the probability of *B* being chosen is reduced.

In our study, participants are randomly assigned to one out of five experimental conditions. In all conditions subjects make a real decision about whether to retire an EUA at an opportunity cost of €5. In four conditions, subjects also decide on the timing of this retirement. The amount of (true) information supplied to subjects prior to their decision varies across conditions. The information describes the EU ETS and the MSR, and explains how (and in some treatments also why) the timing of retirement influences the actual amount of emissions saved. The level of detail of the description is taken as a measure of “complexity.” With this design, we estimate the effects of learning and complexity on choice behavior and find clear evidence of learning effects.

The remainder of the paper is structured as follows. Section 2 describes the context and basic design of data collection. In Section 3 we discuss a battery of pre-experimental survey measurements, which gives an idea about relevant prior preferences and beliefs in the sampling population at hand. The data is also used for deeper analysis in later sections. Sections 4 and 5 establish benchmarks for the effects of treatment by presenting results from two control conditions. The key results of the experiment are provided in Section 6, which are complemented in Section 7 by an analysis of treatment effect heterogeneity. Section 8 discusses the results in the context of the relevant literature and concludes with public policy implications.

2 Study Design

The data underlying our analysis is drawn from the German Socio-Ecological Panel (Klick et al., 2021) operated by RWI – Leibniz Institute for Economic Research. The data originate from the wave that was fielded in spring 2021 and was gathered by the survey institute *forsa*.

In total, 8,677 individuals participated in the 2021 survey. A random sub-sample of 4,444 individuals was drawn from the panel to participate in our study. Data were collected on-line. Participation was voluntary. The sample is a broad cross-section of the German population, but is not representative: participants tend to be older and better educated than the average of the German population (see Table A2 in the Appendix), partly because our sample is restricted to adults.

Participants were randomly assigned to one out of five experimental conditions. Within each condition, all subjects received exactly the same questionnaire. Across conditions, the questionnaire was purposefully varied slightly.² Thus, differences in response behavior across conditions can be causally attributed to the questionnaire variations.

All conditions involved a real decision on whether to retire a European Union Allowance (EUA) to prevent emission of carbon dioxide at an opportunity cost of €5. One out of four randomly drawn choices was actually implemented. This was explained to the participants in the introductory text. The first control condition $z = 1$ consisted of a dichotomous choice between keeping the endowment of €5 – to be paid out in the form of an Amazon voucher – or giving up the endowment by retiring an EUA. The timing of the retirement was not further specified (see Appendix B.3 for the full choice setting).

The second control condition $z = 2$ is identical to the first, except that it also involved

²The variation was limited to one out of the ten questions that were used for the present study. The remaining questionnaire over and above those ten questions, not used for this study, was identical for all participants.

the decision of whether the EUA should be retired immediately or one year after the experiment. In case no EUA was retired, the timing of the receipt of the voucher was identical to $z = 1$, i.e. shortly after the completion of the survey. Condition $z = 2$ provided no information on the implications of the timing of retirement (the full choice setting is provided in Appendix B.4).

Treatment conditions $z = 3$ and $z = 4$ are identical to the second control condition $z = 2$, except that participants were provided with explanations of the delayed EUA retirement option prior to their decision. Treatment condition $z = 3$ added a short sentence stating that immediate retirement reduces carbon dioxide emissions by 0.24 tons less than retirement one year after the experiment (see Appendix B.5 for the full choice setting). This explanation is very simple and most likely counter-intuitive, as it is generally believed that immediate action is more effective than delayed action (UNDP, 2021).

Treatment condition $z = 4$, by contrast, entails a detailed explanation of why the relative effectiveness of the two abatement options is as stated, with reference to the Market Stability Reserve (MSR) – an add-on to the EU ETS with the objective to provide stability and to improve the system’s resilience to shocks by adjusting the supply of allowances to be auctioned. The level of detail makes the explanation more intuitive, but increases its complexity (the text is provided in full length in Appendix B.6).

In treatment condition $z = 5$, the explanation has the same level of detail as in treatment condition $z = 4$. However, the delay was not one year but an unspecified period that is weakly larger than one year and maximizes the impact of the retirement (see Appendix B.7 for details). This reflects the offer made by several NGOs in the real world. All data and materials for the present study are published on the Open Science Framework (OSF) repository under DOI 10.17605/OSF.IO/5HUCM .

The experiment began immediately after the pre-experimental items introduced in

the next section, starting with a general introductory text that explained the basic task: the choice between keeping a €5 endowment in the form of an Amazon voucher or putting the money into climate protection by preventing the emission of carbon dioxide.

A second text then explained in simple but accurate terms the procedure of carbon abatement by retiring an EUA from the EU ETS. Those texts were shown to all participants, as the basic task was the same for all. Yet, no further details about how the retirement of an EUA translates into climate protection were given at this point. Such details were the locus of experimental manipulation across experimental conditions.

The first control condition $z = 1$ serves as a natural baseline for two reasons. First, it implements the task in the most basic form – the four other experimental conditions are variations. Second, it relates the present study seamlessly to previous research that used the same method of carbon abatement as a form of personal climate action (e. g. Goeschl and Perino, 2012; Löschel et al., 2013; Diederich and Goeschl, 2014; Bruns and Perino, 2021).

The sampling and assignment process is summarized in Table 1, listing the planned and actual number of individuals drawn for the study, broken down by experimental conditions. Non-uniform assignment frequencies result from purposeful design of assignment probabilities based on statistical power analysis. Since participation in the survey was voluntary, subjects could refuse to participate or drop out at any point of the survey. The last column lists the number of respondents, reflecting the effective sample for the analysis.

Table 1: Planned and Sampled Number of Individuals by Experimental Condition, and the respective Number of Completes.

| Condition | Planned | Sampled | Completed |
|-----------------------------|---------|---------|-----------|
| Control condition $z = 1$ | 400 | 513 | 491 |
| Control condition $z = 2$ | 600 | 704 | 665 |
| Treatment condition $z = 3$ | 1,000 | 1,073 | 996 |
| Treatment condition $z = 4$ | 1,000 | 1,076 | 989 |
| Treatment condition $z = 5$ | 1,000 | 1,078 | 998 |
| Total | 4,000 | 4,444 | 4,139 |

Notes: The last column lists the counts net of individuals who refused to participate or dropped out at some point of the survey.

3 Pre-Experimental Items

The questionnaire started with a battery of five pre-experimental items that were presented to all participants and served to measure preferences, beliefs, and knowledge with respect to climate change and mitigation policy. While the following descriptive results provide insights about relevant priors in the sample, this data is used for the heterogeneity analyses presented in Section 7.

3.1 Motivational Aspects

To measure motivational aspects, four items were designed, the first of which asks for the degree of agreement with the proposition “It is my moral duty to make an active contribution to climate protection.” Responses to all items were measured on a five-point ordinal scale and ranged from “fully disagree” to “fully agree” (Table 2). 82.0 % of the respondents agree or fully agree with this statement – see the row on “Moral duty”. This result is taken as evidence that a vast majority of participants feels intrinsically urged to make a personal contribution to climate action.

The second row of Table 2 lists the responses to the statement “Effective climate protection can only be done by public policy” (“Public policy”). Disagreement with this

Table 2: Responses to Four Items on Motivational Aspects.

| Item | fully disagree | rather disagree | neither nor | rather agree | fully agree | n/a |
|---------------|----------------|-----------------|-------------|--------------|-------------|------|
| Moral duty | 175 | 208 | 348 | 1,837 | 1,554 | 17 |
| Public policy | 347 | 887 | 785 | 1,500 | 595 | 25 |
| Urgency | 196 | 261 | 359 | 1,363 | 1,940 | 20 |
| Hype | 1,890 | 1,022 | 446 | 498 | 268 | 15 |
| Moral duty | 4.2% | 5.0% | 8.4% | 44.4% | 37.6% | 0.4% |
| Public policy | 8.4% | 21.4% | 19.0% | 36.2% | 14.4% | 0.6% |
| Urgency | 4.7% | 6.3% | 8.7% | 32.9% | 46.9% | 0.5% |
| Hype | 45.7% | 24.7% | 10.8% | 12.0% | 6.5% | 0.4% |

Notes: "Moral duty" stands for the proposition "It is my moral duty to make an active contribution to climate protection."; "Public policy" for "Effective climate protection can only be done by public policy."; "Urgency" for "The climate problem will not tolerate any delay. We must act now."; and "Hype" for "The climate issue is overrated.". 'n/a' stands for the non-response category "don't know/prefer not to say."

statement is considered to measure the belief that personal action can make a difference.

While there is some heterogeneity, the absolute majority of respondents believes that effective climate protection can only be achieved by public policy; in other words, personal action does not have a significant impact.

A temporal dimension is added with the statement: "The climate problem will not tolerate any delay. We must act now" ("Urgency"). This item is designed to measure the belief that timing matters in climate action, with a preference for immediate over delayed actions. The vast majority of about 80 % agrees or fully agrees with this statement (Table 2).

The fourth item serves to separate out the opposite end of the motivational spectrum, asking for the degree of agreement with the statement "The climate issue is overrated." Responses are shown in the fourth row of Table 2 ("Hype"). A majority of about 70 % disagrees or fully disagrees, but there is a significant minority of about 19 % of respondents who agree or fully agree. These individuals can be expected to be weakly or not at all motivated to make a personal contribution to climate action, nor to spend significant

cognitive effort to engage with climate policy instruments.³

While these four items refer to clearly distinct aspects of the attitude towards climate change and policy, there might be an overarching latent motivational factor that drives response behavior across the whole item battery. The results of a principal factor analysis, displayed in Table 3, show that this is indeed the case. The analysis retains a single factor, which we call “motivation to contribute”, or “motivation” for short. This term is intuitive, as “moral duty” and “urgency” load strongly positively, and “hype” strongly negatively on the factor. “Public policy” also loads positively, but rather weakly. Using the Bartlett scoring coefficients, we can construct a “motivation” score for each participant that is used for deeper analysis of behavior in the experiment below. The in-sample distribution of scores is bi-modal and highly skewed, with the absolute majority at the high end.

Table 3: Results of a Maximum Likelihood Principal Factor Analysis of Four Motivational Aspects.

| | Factor “Motivation” | | |
|-------------------|---------------------|------------|---------|
| | Loading | Uniqueness | Scoring |
| Moral duty | 0.7264 | 0.4724 | 0.1762 |
| Public policy | 0.3483 | 0.8787 | 0.0454 |
| Urgency | 0.9308 | 0.1337 | 0.7981 |
| Hype | −0.7052 | 0.5027 | −0.1608 |
| Eigenvalue | 2.0125 | | |
| # observations | 4,094 | | |
| log \mathcal{L} | −1.2982 | | |
| Schwarz’s BIC | 35.8655 | | |
| Akaike’s AIC | 10.5963 | | |

Notes: Non-response category “don’t know/prefer not to say” set to missing, which drops 45 out of 4,139 subjects. The columns “Loading”, “Uniqueness”, and “Scoring” list the factor loadings, unique variances, and Bartlett scoring coefficients for each of the four variables, respectively. A likelihood ratio test independent vs. saturated yields $\chi^2(6) = 5341.08$, rejecting the null at $p = 0.0000$. A likelihood ratio test 1 factor vs. saturated yields $\chi^2(2) = 2.59$, not rejecting the null at conventional significance levels ($p = 0.2733$).

³There is a limitation to this interpretation, however: there is an overlap of 160 participants (4 percent) that show strong motivation to make a personal contribution based on the “Moral duty” and “Public policy” items, but also agree or fully agree to the “Hype” statement.

3.2 Knowledge of the EU Emissions Trading System

The final item of the pre-experimental battery is different in that it is not about motivation but a self-assessment of the individual knowledge about the EU ETS as an established climate policy instrument – the most important in the participants’ jurisdiction. This item asks “Could you explain to a friend how the EU Emissions Trading System works?”, with response categories being “I could explain the system in detail”, “I could explain the system roughly”, “I could not explain the system”, and “I don’t know the system”. The majority of 60.1 % of the participants does not know or understand the ETS (Table 4). Hence, for the majority of participants, the choice task involves significant uncertainty. In other words, there is enough scope for learning, which is important for our experiment.

Table 4: Participants’ Knowledge of the EU ETS.

| ETS | could explain in detail | could roughly explain | could not explain | don’t know the system | n/a | Total |
|----------------|----------------------------|--------------------------|----------------------|--------------------------|------|--------|
| # Observations | 78 | 1,537 | 1,933 | 552 | 39 | 4,109 |
| Shares | 1.9% | 37.1% | 46.7% | 13.4% | 0.9% | 100.0% |

Note: The item asks “Could you explain to a friend how the EU Emissions Trading System works?”. “n/a” stands for the non-response category “don’t know/prefer not to say.”

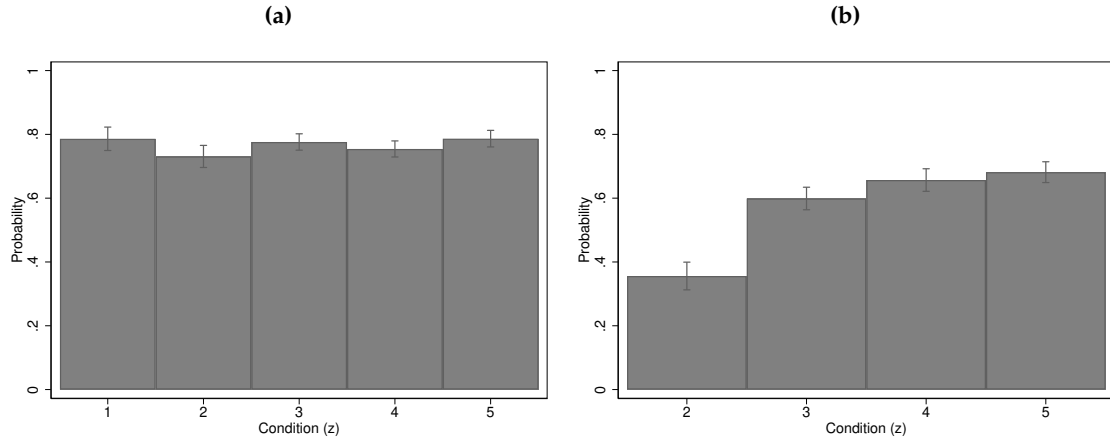
4 Willingness to Contribute

While our study is not designed to measure willingness to pay, from the literature (e. g. Goeschl and Perino, 2012; Löschel et al., 2013; Diederich and Goeschl, 2014; Bruns and Perino, 2021), we can expect a significant fraction of the sample to be willing to trade-in the €5 endowment for the retirement of an EUA.⁴

The opportunity cost of EUA retirement is deliberately set at the low end, much lower

⁴From the responses to the pre-experimental questionnaire discussed in the previous section, we can expect that at least one out of five subjects is willing to retire an allowance, as 22 percent showed an especially strong motivation to make a personal contribution to climate protection. Possibly up to four out of five respondents might retire an allowance, because 82 percent feel morally urged to take actions for climate protection. Of course, this prediction was not part of the pre-registered hypotheses, we note it here as a post-hoc check of internal consistency.

Figure 1: Willingness to Contribute (panel a) and Relative Effectiveness of Contribution (panel b) by experimental condition.



than the EUA market price at the time of study, as it is just an instrument to avoid pollution of the data by participants that do not care about the task. We wanted to have EUA retirement rates high in the control conditions to leave enough scope for treatment effects (which we expected to be negative). The descriptive results are consistent with our expectation: Of the 491 subjects in condition $z = 1$, 386 subjects, or about 79 percent, have chosen EUA retirement. Only about 14 percent (68 subjects) decided to keep the endowment and 7 percent (37 subjects) selected the non-response category “don’t know/prefer not to say.”

Defining $a = 1$ to indicate the choice of a EUA retirement, whereas $a = 0$ absorbs the two alternatives, the Amazon voucher and the no-choice alternative, we term the probability $\Pr(a = 1)$ “willingness to contribute” (WTC). The mean of a over the sample of participants assigned to the first control condition $z = 1$ is $0.7862 = 386/491$; this value is precisely equal to the conditional probability $\Pr(a = 1 | z = 1)$ obtained from a null probit model, the results of which are shown in the “Null”-column of Table 5. 0.7862 serves as the WTC benchmark for the other experimental conditions (see the left-hand panel of Figure 1).

The second control condition $z = 2$ is identical to the first with a single exception: the EUA retirement option is split into two, immediate retirement versus delayed retirement one year after the experiment. Deliberately, the distinction was not further explained to the participants, since the condition serves as a baseline against which the effect of the content of explanation can be evaluated. Extending the definition of $a = 1$ to indicate immediate or delayed EUA retirement, our auxiliary hypothesis reads that the WTC is unaffected by the split: $H_0^{aux} : \Pr(a = 1 | z = 1) = \Pr(a = 1 | z = 2)$.

If a subject of the first control condition believes that retirement is immediate, then adding the choice to retire later would only affect the WTC if that individual prefers later over early retirement, e.g. because she believes that retiring of allowances requires preparations or wants to grant industry some time to adapt – arguments brought forward in favor of late retirement in a cognitive pretest. In this case, WTC would increase. The same holds if a participant believes that retirement in the first control condition is delayed but she prefers early retirement. In all other cases, the second control condition merely adds a weakly dominated alternative. Since we conjectured that most participants believe retirement in the first control condition to be early and prefer it that way, we did not expect a difference in the WTC between the two control conditions.

As becomes evident from the “Partial”-column of Table 5, H_0^{aux} is rejected at the five percent significance level, although the difference of 5.5 percentage points is small. Actually, the WTC is significantly smaller, not higher, in the second control condition. Hence, the pattern observed is neither consistent with the weak axiom of revealed preferences, nor with the Asymmetric Dominance Effect. The latter effect has been found to increase the attractiveness of options that, in contrast to other options, strictly dominate an alternative (Huber et al., 1982).

Effectively, the second control condition $z = 2$ adds a second layer to the abatement

Table 5: Marginal Effects from Maximum Likelihood Probit Estimations of the Treatment Effects on the Willingness to Contribute (WTC).

| | Null Model | | Partial Model | | Full Model | |
|--------------------|------------|-------------|---------------|-------------|------------|-------------|
| $z = 1$ margin | 0.7862 | (0.0186)*** | 0.7862 | (0.0187)*** | 0.7862 | (0.0188)*** |
| $z = 2$ effect | | | -0.0553 | (0.0247)** | -0.0553 | (0.0258)** |
| Treatment effects: | | | | | | |
| $z = 3$ | | | | | -0.0100 | (0.0231) |
| $z = 4$ | | | | | -0.0319 | (0.0228) |
| $z = 5$ | | | | | 0.0004 | (0.0229) |
| # observations | 491 | | 1,156 | | 4,139 | |
| $\log \mathcal{L}$ | -254.84 | | -642.15 | | -2,240.68 | |
| Wald χ^2 | | | 4.89 | | 9.01 | |
| Wald p | | | 0.0270 | | 0.0608 | |
| Pseudo R^2 | 0.0000 | | 0.0037 | | 0.0021 | |

Notes: Predictive margins for $z = 1$ and effects of the respective discrete change of z relative to $z = 1$. In parentheses are the bootstrap standard errors derived from 1,000 replications. Stars indicate that a Wald test rejects the null that the respective margin is uniform or the respective effect is equal to zero at conventional significance levels (* at $p < 0.1$, ** at $p < 0.05$, and *** at $p < 0.01$).

choice relative to control condition $z = 1$: first, participants need to decide on whether or not to abate, and in case they choose abatement, they also need to decide on how to abate. There is experimental evidence that increasing the number of stages in decision-making tasks reduces the attractiveness of options (Sonsino et al., 2002) and that participants tend to avoid cognitive demand (Kool et al., 2010). This is particularly relevant if participants have not formed priors about the role of timing in retiring EUAs. Thus, a likely explanation for the drop in the WTC is that the presentation of the delay option without any information on why it is added or how it performs relative to immediate retirement made the decision too complex for some respondents. This is also supported by the fact that 10 percent selected the non-response category “don’t know/prefer not to say” in $z = 2$, as compared to only 8 percent in $z = 1$, although this difference is not statistically significant at conventional levels.

5 Relative Effectiveness of Contribution

Taking the second control condition $z = 2$ as a benchmark and defining $d = 1$ to indicate the choice of delayed EUA retirement, where the reference category $d = 0$ is immediate retirement, we term the probability $\Pr(d = 1)$ “relative effectiveness of contribution” (REC), since delayed EUA retirement yields a higher abatement effect than immediate retirement.

A natural hypothesis appears to be $\Pr(d = 1 \mid z = 2) < 0.5$, that is, the average subject is inclined towards immediate retirement. There is reason to expect that most subjects have the prior that it is better to protect the environment earlier (UNDP, 2021), implying a preference for the immediate versus the delayed EUA retirement option. The instructions of the second control condition provide no intuitive reason of why timing should matter other than explicitly allowing to make a choice about that dimension. Hence, there is no reason to adjust any prior-held belief about the relative effectiveness of early versus late abatement.

Observed behavior is consistent with hypothesis $\Pr(d = 1 \mid z = 2) < 0.5$: Of the 486 subjects in condition $z = 2$ who have chosen to retire, 64 percent (313 subjects) have chosen immediate EUA retirement and 36 percent (173 subjects) delayed retirement. The conditional probability $\Pr(d = 1 \mid z = 2) = 0.3560$, resulting from the null probit model estimation reported in the “Null”-column of Table 6, equals $173/486$, the mean of d over the sample of participants assigned to condition $z = 2$, and serves as a REC benchmark for the three treatment conditions to be discussed in the next section (see the right-hand panel of Figure 1).

Table 6: Marginal Effects from Maximum Likelihood Probit Estimations of the Treatment Effects on the Relative Effectiveness of Contribution (REC).

| | Null Model | | Full Model | | With Covariates | |
|--------------------|------------|-------------|------------|-------------|-----------------|-------------|
| $z = 2$ margin | 0.3560 | (0.0218)*** | 0.3560 | (0.0220)*** | 0.3517 | (0.0233)*** |
| Treatment effects: | | | | | | |
| $z = 3$ | | | 0.2430 | (0.0291)*** | 0.2575 | (0.0293)*** |
| $z = 4$ | | | 0.3009 | (0.0283)*** | 0.3025 | (0.0285)*** |
| $z = 5$ | | | 0.3256 | (0.0278)*** | 0.3239 | (0.0296)*** |
| Covariates: | No | | No | | Gender, income | |
| # observations | 486 | | 2,790 | | 2,478 | |
| $\log \mathcal{L}$ | -316.4 | | -1,807.9 | | -1,601.0 | |
| Wald χ^2 | | | 142.9 | | 144.3 | |
| Wald p | | | 0.0000 | | 0.0000 | |
| Pseudo R^2 | 0.0000 | | 0.0398 | | 0.0427 | |

Notes: Predictive margins for $z = 2$ and effects of the respective discrete change of z relative to $z = 2$. In parentheses are the bootstrap standard errors derived from 1,000 replications. Stars indicate that a Wald test rejects the null that the respective margin is uniform or the respective effect is equal to zero at conventional significance levels (* at $p < 0.1$, ** at $p < 0.05$, and *** at $p < 0.01$).

6 Causal Effects of Complex Information Provision

This section presents our key results. As our measure of “complexity”, we take the level of detail of the descriptions provided in the treatment conditions on the Market Stability Reserve (MSR) and its implications on how the timing of retirement influences the amount of greenhouse gas emissions saved. Those implications are very difficult to grasp even for experts (Gerlagh and Heijmans, 2019; Perino, 2018; Perino et al., 2022b).

With respect to this definition of complexity, treatment conditions $z = 3$ and $z = 4$ differ substantially: In condition $z = 3$, the assertion that immediate retirement is less effective than delayed retirement was added, but no further explanation of this assertion was given. In contrast, in treatment condition $z = 4$, a detailed explanation with reference to the functioning of the MSR was added to back the assertion. Although the explanation was given in the simplest possible terms, it is a long and demanding text (see Appendix B.6).

While containing the same explanation and, hence, the same level of detail as treatment $z = 4$, condition $z = 5$ only differs in that the delay is not exactly one year, but an unspecified period, weakly larger than one, that maximizes the impact of the retirement. Thereby, treatment $z = 5$ replicates what NGOs, such as “ForTomorrow” and “Compensators,” offer in the voluntary offsetting market.

6.1 Willingness to Contribute

Given the treatment conditions’ varying degrees in complexity, we hypothesize that complexity aversion could sway participants to the very simplest options: keeping the endowment or choosing the non-response category. Alternatively, the information provided might conflict with the moral belief that urgent action on climate change is a moral obligation, such that there may be an aversion to see this belief challenged or an urge to act against it (Bénabou and Tirole, 2011). If the dissonance or ambivalence induced is sufficiently strong, participants might be less willing to contribute at all in order to avoid having to make a choice that they are ambivalent about (Luce et al., 1997; Anderson, 2003; Pauer et al., 2022). These considerations lead us to the following complexity hypothesis:

Hypothesis 1 WTC is lower in the treatment conditions $z = 3, 4$ and $z = 5$ than in control condition $z = 1$: $\Pr(a = 1 | z = 1) > \Pr(a = 1 | z) \forall z = 3, 4, 5$.

Evidently, the probit treatment effect estimates, reported in the “Full”-column of Table 5 and illustrated in the left-hand panel of Figure 1, are inconsistent with Hypothesis 1: For all treatment conditions, the WTC is not different from that of control condition $z = 1$ in statistical terms. The results remain unchanged when controlling for those individual characteristics that were not perfectly balanced between the experimental groups (see Table A3 in the Appendix). However, with an alternative definition of the outcome variable that includes respondents who abandoned the survey during the decision task,

the WTC is statistically significantly lower in $z = 4$ than in control condition $z = 1$, by about 5 percentage points (Table A4).

6.2 Relative Effectiveness of Contributions

For those who are willing to contribute, the information provided in the treatment conditions is relevant, as it reveals a strict dominance in effectiveness at identical opportunity costs. Furthermore, the information is most likely surprising to the respondents, as the implications of the MSR, and even the mechanism's mere existence, are known only to an expert circle. The common belief among supporters of climate action is that early action is better than delaying it, at least at identical costs (UNDP, 2021). This belief becomes evident from the answers to our pre-experimental items, where about 80 percent agreed or fully agreed that climate change requires immediate action (see Table 2). Nonetheless, due to the information provided in the treatment conditions, we expect that at least some participants will update their beliefs and prefer delayed EUA retirement, leading to the following hypothesis on the "learning effect":

Hypothesis 2 The relative effectiveness of contribution (REC) is higher in the treatment conditions than in control condition $z = 2$:

$$\Pr(d = 1 | z = 2) < \Pr(d = 1 | z) \forall z = 3, 4, 5.$$

However, updating of beliefs over the relative effectiveness of immediate and delayed retirement can be biased, because "bad news" are discounted in certain contexts (Eil and Rao, 2011; Gershman, 2019; Kuzmanovic et al., 2018; Yao et al., 2021). It thus can be expected that not all contributors choose the delayed retirement option under treatment.

The probit treatment effect estimates shown in the "Full"-column of Table 6, and illustrated in the right-hand panel of Figure 1, are clearly consistent with Hypothesis 2:

REC is more than 24 percentage points higher in all treatment conditions relative to control condition $z = 2$. There is also a certain monotonicity of treatment effects, that is, the successively added complexity of the pro-delay information did not deter subjects from acting accordingly. However, the effect of adding more information is small, both in economic and statistical terms. Note that the treatment effects are qualitatively the same when we include covariates (column "With Covariates" in Table 6) and when the reference category ($d = 0$) also includes those who chose the Amazon voucher and the no-choice alternative (Table A5).

7 The Role of Motivated Reasoning and Need for Cognition

In all treatment conditions, the delayed retirement option is claimed to be more effective in abating carbon emissions than immediate retirement. This claim can be safely assumed to contradict the prior belief and moral inclination of the majority of participants (see the previous section). The explanations for this claim provided in treatment conditions $z = 4$ and $z = 5$ require some careful reading and abstract thinking, but those who engage with the explanation, that is, read and process it, are more likely to update their beliefs about the relative effectiveness of the two alternatives of immediate versus delayed retirement. Two aspects may moderate the update process, thereby giving rise to treatment effect heterogeneity: motivated reasoning and need for cognition.

7.1 The Role of Motivated Reasoning

Motivated beliefs are difficult to update, as they tend to be more inert to conflicting information than "rational" beliefs (Eil and Rao, 2011; Gershman, 2019; Kuzmanovic et al., 2018; Yao et al., 2021). Hence, we expect the treatment effects to differ between those who agree with the pre-experimental survey items "moral duty" and "urgency" and those

who disagree. The treatment effect estimates from extended probit estimations in which the four motivational items are included indicate, though, that this is only partly the case (Table 7). Generally, the treatment effects on WTC tend to be positive subject to disagreement with the “moral duty” statement, but negative subject to agreement, yet not all differences are statistically significant.

With respect to the “urgency” item, there is no evidence for heterogeneous treatment effects on WTC. Likewise, there is no heterogeneity in the treatment effects on REC with respect to the “moral duty” statement. However, the treatment effects are systematically bigger among those who agree with the “urgency” statement compared to those who disagree. This result contrasts with the motivated reasoning conjecture: Those who *ex ante* believe that climate action is urgent are more responsive to information that states that waiting increases effectiveness. Given this empirical evidence, the role of motivated reasoning seems limited.

We know from the factor analysis presented in Section 3 that the responses to the four motivational items are not independent: the analysis retained a single principal factor that can be interpreted as a general “motivation to contribute.” Using the Bartlett scoring coefficients to construct a “motivation” score for each participant, we can use that score in the regressions, instead of the individual items. While not reporting the estimates here because the “motivation” scores have no natural metric over and above their ordinal ordering, such that effect sizes are difficult to interpret, it turns out that all interaction effects between the treatment indicators and the score are statistically insignificant for WTC, but highly significant and clearly positive for REC: the treatment effect sizes are increasing with the “motivation” score. In short, treatment effects are heterogeneous for REC, but not for WTC.

One concern is that the results reflect an experimenter demand effect. The finding that

Table 7: Moderating Effects of the Motivational Pre-experimental Item Battery on the Willingness to Contribute (WTC) and Relative Effectiveness of Contribution (REC) by Means of Maximum Likelihood Probit Estimations.

| | Willingness to contribute (WTC) | | Relative effectiveness of contribution (REC) | |
|------------------------------------|---------------------------------|-----------------------|--|------------------------|
| | Moral Duty | Urgency | Moral Duty | Urgency |
| Reference margin | 0.6450 (0.0197)*** | 0.6292 (0.0217)*** | 0.5281 (0.0323)*** | 0.7085 (0.0304)*** |
| Effect of agreement | 0.1584 (0.0228)*** | 0.1863 (0.0248)*** | 0.0763 (0.0345)** | -0.1259 (0.0328)*** |
| Treatment effects at disagreement: | | | | |
| z = 2 | 0.0577 (0.0861) | 0.0275 (0.0830) | | |
| z = 3 | 0.1715 (0.0775)** | 0.1045 (0.0791) | 0.1851 (0.1044)* | 0.0623 (0.0991) |
| z = 4 | 0.1015 (0.0789) | 0.0460 (0.0810) | 0.2796 (0.1040)*** | 0.0440 (0.0975) |
| z = 5 | 0.1997 (0.0751)*** | -0.0261 (0.0802) | 0.2621 (0.0993)*** | 0.1587 (0.0930)* |
| Treatment effects at agreement: | | | | |
| z = 2 | -0.0448 (0.0274) | -0.0397 (0.0293) | | |
| z = 3 | -0.0307 (0.0265) | -0.0188 (0.0283) | 0.2582 (0.0296)*** | 0.2743 (0.0306)*** |
| z = 4 | -0.0284 (0.0259) | -0.0174 (0.0284) | 0.3107 (0.0285)*** | 0.3403 (0.0298)*** |
| z = 5 | -0.0088 (0.0256) | 0.0466 (0.0264)* | 0.3400 (0.0285)*** | 0.3553 (0.0297)*** |

Notes: Estimates derived from a maximum likelihood probit regression with four full factorial interactions of z and the pre-experimental item variables moral duty, urgency, public policy, and hype (in reduced coding, coding neither nor/disagreement/full disagreement as disagreement (= reference category) and agreement/full agreement as agreement). $n = 4,094$, $\log \mathcal{L} = -1,790.81$, Wald $\chi^2(24) = 778.76$, Wald $p = 0.0000$, pseudo $R^2 = 0.1859$ for WTC and $n = 2,774$, $\log \mathcal{L} = -1,757.19$, Wald $\chi^2(19) = 213.79$, Wald $p = 0.0000$, pseudo $R^2 = 0.0607$ for REC. In parentheses are the bootstrap standard errors derived from 1,000 replications. Stars indicate that a Wald test rejects the null that the respective margin is uniform or the respective effect is equal to zero at conventional significance levels (* at $p < 0.1$, ** at $p < 0.05$, and *** at $p < 0.01$). Reference margin is the relative effectiveness of contributions when having expressed a neutral attitude towards the respective pre-experimental item. "Agreement effect" is the average partial effect of respective discrete changes from the reference category to agreement to the respective pre-experimental item statement. The four rows below "At Disagreement" list the treatment effects of respective discrete changes of z relative to the reference category subject to disagreement to the respective pre-experimental item statement. Likewise for the four rows below "At Agreement."

those participants who believe in an urgent need for climate action are more responsive to being informed that waiting is more effective assuages this concern. Although at no point the instruction alluded to which action should be taken, the mere statement that one action is more effective than another can be interpreted as a recommendation. However, the pull of an experimenter demand effect over and above the informational value of the statement should be independent of any attitude towards the issue at hand. Hence, given the heterogeneity in beliefs for urgent action, those who think the need for climate action is less pressing should delay retirement more often. Yet, we find the opposite. Moreover, the association of information on the effectiveness of a particular course of action with a recommendation to (not) pursue it would in most real-world settings be even more salient than in our experiment.

7.2 The Role of Need for Cognition

Another possible source of treatment effect heterogeneity is need for cognition. Understanding the explanation requires some careful reading and abstract thinking. People generally tend to avoid cognitive demand (Kool et al., 2010), but individuals differ with respect to their need for cognition, i.e. their “tendency to engage in and enjoy effortful cognitive endeavors” (Cacioppo et al., 1984, p. 306). Cacioppo et al. (1983) show that need for cognition affects how individuals process persuasive messages. Moreover, need for cognition is likely to be positively correlated with cognitive ability. For our experiment, this means that those with higher cognitive skill (and hence lower marginal cost of engaging with the explanation) are more likely to read and process it.

We measured need for cognition post-experimentally by means of the standard Need for Cognition (NFC) test (Cacioppo and Petty, 1982; Bless et al., 1994).⁵ The test consists

⁵We emphasize that the following results should be taken with due caution with respect to causal interpretations, because the NFC test followed after, and may hence be influenced by the experiment.

of four statements to which the respondent can express applicability on a seven-point Likert scale (1 = does not apply at all, 7 = does fully apply): (i) "It is enough for me simply to know the answer without understanding the reasons for the answer of a problem," (ii) "I like my life to be full of tricky tasks to solve," (iii) "I would prefer more complicated problems to simple problems," , and (iv) "First and foremost, I think because I have to." From the four items we constructed a simple need for cognition score for each participant by summing up the response codes (appropriately inverting the scales for the first and last item). Thus, the minimum achievable score is 4 and the maximum is 28. The distribution of the score in the sample is unimodal (mode = 16) with a mean of 17.8 (s. d. 3.9) and a median of 18.

Classifying individuals with a score at or below the mid-point as having a low need for cognition and those with a score above the mid-point as having a high need for cognition, we can check for treatment effect heterogeneity in probit regressions with interaction effects between the treatment indicators and the need for cognition indicator. It turns out that the treatment effects are not significantly different for individuals with high and low need for cognition for both WTC and REC. Hence, there is no evidence of treatment effect heterogeneity with respect to need for cognition. There is, however, a highly significant independent effect of need for cognition on the WTC: on average, switching from the low to the high need for cognition class increases the probability of EUA retirement by seven percentage points.

8 Discussion and Conclusion

The effectiveness of individual efforts to achieve a goal, such as emissions reductions, is frequently mediated by the context. The EU ETS exemplifies a context in which outcomes often contradict widely-held beliefs. For example, recent research has shown that

announcing a future mandatory coal phase-out can, *ceteris paribus*, *increase* greenhouse gas emissions (Rosendahl, 2019; Perino et al., 2022a). Another counter-intuitive implication of the ETS relates to the timing of abatement efforts: Retiring emission allowances is a *more* effective abatement option when delayed by several years (Perino, 2018; Gerlagh and Heijmans, 2019).

Exploiting the fact that timing matters, we have empirically investigated how individuals respond to (a) having the choice about the timing of their voluntary abatement efforts in the form of retiring an emission allowance and to (b) being confronted with either no, simple but counter-intuitive, or complex but intuitive information about the effectiveness-ranking of options. To this end, we have conceived a field experiment with more than four thousand participants that was embedded in a survey conducted in Germany in 2021.

When comparing the simple, dichotomous choice setting where participants can contribute to the public good with a setting that explicitly offers the choice between immediate and delayed contributions – at identical personal costs –, we find a small but statistically significant drop in the willingness to contribute: the increase in complexity and ambivalence in the provided information about contribution options, which were not ranked in the instructions to the experiment, discouraged some participants from contributing. This result is consistent with earlier empirical work (Agnew and Szykman, 2005; Carvalho and Silverman, 2022; Iyengar and Lepper, 2000), where individuals who did contribute mostly chose the intuitive but strictly dominated option of immediate contribution.

Adding information did not systematically affect contributions overall, but substantially increased their effectiveness. This was, by and large, independent of whether the provided information consisted only of a counter-intuitive statement, in a sense that it

contradicted prior beliefs of most participants about the effectiveness ranking of the contribution options, or an intuitive but complex explanation for the effectiveness ranking. The key feature hence was the presence of information on the relative effectiveness of two otherwise identical options, not whether it was explained in intuitive terms.

We neither have found evidence that not providing an explanation, nor that providing a complex explanation, discouraged participants from both contributing and taking up the action-relevant part of the information. The uptake of information provided was most pronounced by individuals who most strongly believed in the opposite ranking. Hence, the evidence is consistent with a rational updating of beliefs, but not with motivated reasoning.

The evidence assembled in this study shows that it is important for the effectiveness of voluntary climate action and, more generally, decentralised efforts that decision makers are informed about the relative effectiveness of options. It appears less important, though, how this information is packaged. Unfortunately, communicating counter-intuitive and complex information is not an attractive task: "nobody wants to educate naïve utilitarians about the functioning of the emissions market" (Herweg and Schmidt, 2022, p.18).

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Appendix

A Tables

Table A1: Summary Statistics by Experimental Condition

| | z = 1 | z = 2 | z = 3 | z = 4 | z = 5 | χ^2 | p-value |
|-------------------------------|-------|-------|-------|-------|-------|----------|---------|
| Male | 0.552 | 0.582 | 0.580 | 0.555 | 0.525 | 8.057 | 0.0895 |
| Age | 56.00 | 56.22 | 55.44 | 54.80 | 56.01 | 3.932 | 0.4152 |
| Qual. for university entrance | 0.509 | 0.513 | 0.542 | 0.524 | 0.517 | 2.276 | 0.6852 |
| Employed | 0.528 | 0.499 | 0.517 | 0.534 | 0.524 | 2.065 | 0.7237 |
| Income < 1,200 Euro | 0.056 | 0.054 | 0.071 | 0.067 | 0.057 | 2.840 | 0.5850 |
| Income 1,200 - 2,700 Euro | 0.296 | 0.361 | 0.304 | 0.282 | 0.317 | 10.866 | 0.0281 |
| Income 2,700 - 4,200 Euro | 0.313 | 0.287 | 0.337 | 0.356 | 0.310 | 9.192 | 0.0565 |
| Income \geq 4,200 | 0.336 | 0.298 | 0.288 | 0.296 | 0.316 | 4.021 | 0.4032 |
| <i>Household size:</i> | | | | | | | |
| 1 person | 0.228 | 0.264 | 0.262 | 0.258 | 0.275 | 3.860 | 0.4253 |
| 2 persons | 0.475 | 0.498 | 0.464 | 0.473 | 0.452 | 3.603 | 0.4624 |
| 3 persons | 0.132 | 0.109 | 0.143 | 0.121 | 0.140 | 5.700 | 0.2227 |
| 4 and more persons | 0.165 | 0.130 | 0.132 | 0.148 | 0.134 | 4.585 | 0.3326 |

Note: The last two columns contain the χ^2 -statistics with ties and the p-values for a Kruskal-Wallis equality-of-populations rank test with 4 degrees of freedom.

Table A2: Comparison of the Sample with the German Population

| | Sample | Population |
|---------------------------------------|--------|------------|
| Male | 55.8% | 49.5% |
| Qualification for university entrance | 52.3% | 35.7% |
| Employed | 52.1% | 50.6% |
| High income | 30.4% | 35.5% |
| Age < 25 years | 0.2% | 24.2% |
| Age 25 - 64 years | 63.8% | 54.5% |
| Age ≥ 65 years | 34.0% | 21.3% |
| <i>Household size:</i> | | |
| 1 person | 26.0% | 40.9% |
| 2 persons | 47.0% | 34.1% |
| 3 persons | 13.0% | 12.0% |
| 4 and more persons | 14.0% | 13.1% |

Note: Data for the German population in 2021 is taken from Statistisches Bundesamt (2022). In that survey, the threshold for high income is €4,000, whereas we set it at €4,200.

Table A3: Marginal Effects from Maximum Likelihood Probit Estimations of the Treatment Effects on the Willingness to Contribute (WTC) With Covariates.

| | | Full |
|--------------------|----------------|-------------------------|
| $z = 1$ margin | 0.8056 | (0.0194) ^{***} |
| Treatment effects: | | |
| $z = 2$ | -0.0520 | (0.0262) ^{**} |
| $z = 3$ | -0.0068 | (0.0238) |
| $z = 4$ | -0.0369 | (0.0247) |
| $z = 5$ | -0.0142 | (0.0240) |
| Covariates | Gender, Income | |
| n | 3,609 | |
| $\log \mathcal{L}$ | -1,856.47 | |
| Wald χ^2 | 57.46 | |
| Wald p | 0.0000 | |
| Pseudo R^2 | 0.0158 | |

Notes: Predictive margins for $z = 1$ and effects of the respective discrete change of z relative to $z = 1$, derived from a maximum likelihood probit regression. In parentheses are the bootstrap standard errors derived from 1,000 replications. Stars indicate that a Wald test rejects the null that the respective margin is uniform or the respective effect is equal to zero at conventional significance levels (* at $p < 0.1$, ** at $p < 0.05$, and *** at $p < 0.01$).

Table A4: Marginal Effects from Maximum Likelihood Probit Estimations of the Treatment Effects on the Willingness to Contribute (WTC) when including Respondents who Dropped out during the Experiment.

| | Null Model | | Partial Model | | Full Model | |
|--------------------|------------|-------------|---------------|-------------|------------|-------------|
| $z = 1$ margin | 0.7751 | (0.0180)*** | 0.7751 | (0.0186)*** | 0.7751 | (0.0190)*** |
| $z = 2$ effect | | | -0.0625 | (0.0251)** | -0.0625 | (0.0253)** |
| Treatment effects: | | | | | | |
| $z = 3$ | | | | | -0.0290 | (0.0232) |
| $z = 4$ | | | | | -0.0543 | (0.0237)** |
| $z = 5$ | | | | | -0.0253 | (0.0237) |
| # of observations | 498 | | 1,180 | | 4,298 | |
| $\log \mathcal{L}$ | -265.5 | | -674.5 | | -2,463.4 | |
| Wald χ^2 | | | 6.0 | | 8.5 | |
| Wald p | | | 0.0142 | | 0.0759 | |
| Pseudo R^2 | 0.0000 | | 0.0043 | | 0.0017 | |

Notes: Predictive margins for $z = 1$ and effects of the respective discrete change of z relative to $z = 1$. In parentheses are the bootstrap standard errors derived from 1,000 replications. Stars indicate that a Wald test rejects the null that the respective margin is uniform or the respective effect is equal to zero at conventional significance levels (* at $p < 0.1$, ** at $p < 0.05$, and *** at $p < 0.01$).

Table A5: Marginal Effects from Maximum Likelihood Probit Estimations of the Treatment Effects on Relative Effective Contributions (REC) With and Without Subjects who Selected the Non-response Category.

| | Null Model | | Full Model | | Full excl. NR | |
|--------------------|------------|-------------|------------|-------------|---------------|-------------|
| $z = 2$ margin | 0.2602 | (0.0172)*** | 0.2602 | (0.0170)*** | 0.2879 | (0.0190)*** |
| Treatment effects: | | | | | | |
| $z = 3$ | | | 0.2047 | (0.0236)*** | 0.2347 | (0.0262)*** |
| $z = 4$ | | | 0.2353 | (0.0240)*** | 0.2602 | (0.0249)*** |
| $z = 5$ | | | 0.2759 | (0.0236)*** | 0.2874 | (0.0255)*** |
| # of observations | 665 | | 3,648 | | 3,311 | |
| $\log \mathcal{L}$ | -381.2 | | -2,443.7 | | -2,223.5 | |
| Wald χ^2 | | | 130.4 | | 127.3 | |
| Wald p | | | 0.0000 | | 0.0000 | |
| Pseudo R^2 | 0.0000 | | 0.0280 | | 0.0311 | |

Notes: Predictive margins for $z = 2$ and effects of the respective discrete change of z relative to $z = 2$. In parentheses are the bootstrap standard errors derived from 1,000 replications. Stars indicate that a Wald test rejects the null that the respective margin is uniform or the respective effect is equal to zero at conventional significance levels (* at $p < 0.1$, ** at $p < 0.05$, and *** at $p < 0.01$). In “Null” and “Full” the reference category contains immediate retirement, the voucher, and non-response. The “Full excl. NR” excludes subjects who selected the non-response category “don’t know/prefer not to say.”

B The Experiment in the Questionnaire

B.1 General Introduction for All Participants

In the following, you can claim a bonus of 5 Euros in the form of an Amazon voucher. However, you can also decide to forego this bonus in order to personally make an active contribution to climate protection by preventing the emission of one ton of carbon dioxide (CO₂). To put this into perspective, one metric ton is the average amount emitted by each German (through consumption, electricity use, heating and mobility) over the course of five weeks.

How exactly one can prevent the emission of this ton of CO₂ and what your decision situation looks like is explained on the following pages. You make your decision afterwards.

Regardless of whether you decide to receive the 5-Euro bonus in the form of an Amazon voucher or to make an active contribution to climate protection, we will only implement the decision of every fourth participant. To do this, we will randomly select 1,000 participants from the total of 4,000 participants after the survey has ended. If you are one of them, you will receive the 5-Euro-bonus or we will implement the active contribution to climate protection—depending on how you have decided.

B.2 Specific Introduction for All Participants

The European Union (EU) wants to quickly and significantly reduce emissions of climate-damaging greenhouse gases (e.g., carbon dioxide, CO₂) and achieve climate neutrality by 2050. In order to effectively control the emission of these pollutants, large power plants and industrial facilities must acquire and surrender a so-called emission allowance for each ton of greenhouse gas they emit. The emission allowance is then used up forever and cannot be used again. The amount of these emission allowances issued by the EU is strictly limited.

You are now given the opportunity to decide whether an emission allowance is irreversibly withdrawn from circulation. This is called “retiring”. A retired emission allowance is no longer available to power plants. The power plants can therefore emit one ton of CO₂ less. This effectively reduces overall emissions in the EU and makes an active contribution to climate protection.

A confirmation of retirement will be sent to you by e-mail after completion of the survey if you have decided to retire an emission allowance and if you are one of the participants whose decision will be implemented.

B.3 Choice Setting in Control Condition $z = 1$

You now can retire an emission allowance that entitles you to emit one ton of CO₂. This will not incur any costs for you. However, you will forgo your bonus of 5 Euros.

How do you decide?

- Climate protection: I waive my 5-Euro bonus and want an emission allowance to be retired on a binding basis.
- Consumption: I want to keep my 5-Euro bonus. No emission allowance will be retired.
- don't know/no answer

B.4 Choice Setting in the Second Control Condition $z = 2$

You now can retire an emission allowance that entitles to emit one ton of CO₂. **You can decide whether the retirement occurs either immediately or in one year. Even if you decide to retire in one year, this decision is binding and cannot be reversed later.**⁶ This will not incur any costs for you. However, you will forgo your bonus of 5 Euros.

How do you decide?

- Climate protection variant A: I waive my 5-Euro bonus and want an emission allowance to be retired immediately.
- Climate protection variant B: I waive my 5-Euro bonus and want an emission allowance to be retired in a year's time.
- Consumption: I want to keep my 5-Euro bonus. No emission allowance will be retired.
- don't know/no answer

B.5 Choice Setting in Treatment Condition $z = 3$

You now can retire an emission allowance that entitles to emit one ton of CO₂. You can decide whether the retirement occurs either immediately or in one year. Even if you decide that retirement occurs in one year, this decision is binding and cannot be reversed later. This will not incur any costs for you. However, you will forgo your bonus of 5 Euros.

⁶Note that this text was not highlighted in the survey questionnaire, but is highlighted here to indicate the difference between control conditions $z = 1$ and $z = 2$.

The timing of the retirement makes a difference. The number of emission allowances issued next year depends on the number of emission allowances retired this year. Therefore, retiring now (variant A) reduces total emissions by 0.24 tons of CO₂ less than retiring in one year (variant B). Retiring in one year (variant B) is the greater contribution to climate protection.⁷

How do you decide?

- Climate protection variant A: I waive my 5-Euro bonus and want an emission allowance to be retired immediately.
- Climate protection variant B: I waive my 5-Euro bonus and want an emission allowance to be retired in a year's time.
- Consumption: I want to keep my 5-Euro bonus. No emission allowance will be retired.
- don't know/no answer

B.6 Choice Setting in Treatment Condition $z = 4$

You now can retire an emission allowance that entitles you to emit one ton of CO₂. You can decide whether the retirement occurs either immediately or in one year. Even if you decide that retirement occurs in one year, this decision is binding and cannot be reversed later. This will not incur any costs for you. However, you will forgo your bonus of 5 Euros.

The timing of retirement makes a difference due to the Market Stability Reserve. It reduces the number of emission allowances available in the future based on the number of emission allowances that are not used at the end of a calendar year but are carried over by the power plant operators into the next year. If an emission allowance is now purchased and immediately retired, the number of emission allowances carried over at the end of a year is reduced.

This in turn means that more emission allowances are issued than would have been the case without the retirement. Although retirement deprives power plants of an emission allowance, this is partially (24% per year) offset by the Market Stability Reserve in the future. However, if a purchased emission allowance is not retired immediately, but is set aside and only retired later, then the emission allowance is still counted as part of the carryover at the end of a calendar year.

The allowance therefore continues to trigger a reduction in the number of allowances issued in the

⁷Note that this text was not highlighted in the survey questionnaire, but is highlighted here to indicate the difference between control condition $z = 2$ and treatment condition $z = 3$.

future via the Market Stability Reserve. Power plant operators are therefore not only deprived of the emission allowance purchased and later retired, but fewer new ones are made available.

In summary, this means: The timing of the retirement makes a difference. The number of emission allowances issued next year depends on the number of emission allowances retired this year. Therefore, retiring now (variant A) reduces total emissions by 0.24 tons of CO₂ less than retiring in one year (variant B). Retiring in one year (variant B) is the greater contribution to climate protection.⁸

How do you decide?

- Climate protection variant A: I waive my 5-Euro bonus and want an emission allowance to be retired immediately.
- Climate protection variant B: I waive my 5-Euro bonus and want an emission allowance to be retired in a year's time.
- Consumption: I want to keep my 5-Euro bonus. No emission allowance will be retired.
- don't know/no answer

B.7 Choice Setting in Treatment Condition $z = 5$

You can now retire an emission right that entitles you to emit one ton of CO₂. You can decide whether the retirement occurs either immediately or at a future date. Even if you decide that retirement occurs at a future date, this decision is binding and cannot be reversed later. This will not incur any costs for you. However, you will forgo your bonus of 5 Euros.

The timing of retirement makes a difference due to the Market Stability Reserve. It reduces the number of emission rights available in the future based on the number of emission rights that are not used at the end of a calendar year but are carried over by the power plant operators into the next year. If an emission right is now purchased and immediately retired, the number of emission rights carried over at the end of a year is reduced.

This in turn means that more emission rights are issued than would have been the case without the retirement. Although retirement deprives power plants of an emissions allowance, this is partially (24% per year until 2023, 12% per year thereafter) offset by the Market Stability Reserve in the future. **This is true for each year that the Market Stability Reserve is active. This is expected to be the case for several more**

⁸Note that this text was not highlighted in the survey questionnaire, but is highlighted here to indicate the difference between treatment condition $z = 3$ and $z = 4$.

years.⁹ However, if a purchased emission allowance is not retired immediately, but is set aside and only retired later, then the emission allowance is still counted as part of the carryover at the end of each calendar year.

The allowance therefore continues to trigger a reduction in the number of allowances issued in the future via the Market Stability Reserve. Power plant operators are therefore not only deprived of the emission allowance purchased and later retired, but fewer new ones are also made available each year.

In summary, this means: The timing of the retirement makes a difference. The number of emission allowances issued next year depends on the number of emission allowances retired this year. Therefore, retiring now (Option A) reduces total emissions by at least 0.24 tons of CO₂ less than retiring at a future date (Option B). However, it is likely that the difference in effectiveness between Variant A and Variant B is significantly higher. Retiring at a future date (variant B) is the greater contribution to climate protection.

How do you decide?

- Climate protection Option A: I waive my 5-euro bonus and want an emission right to be retired immediately.
- Climate protection option B: I waive my 5-euro bonus and want an emission allowance to be retired on a binding basis at an as yet undetermined future date when the climate impact of the retirement is greatest.
- Consumption: I want to keep my 5-euro bonus. No emission allowance will be retired.
- don't know/no answer

⁹Note that this text was not highlighted in the survey questionnaire, but is highlighted here to indicate the difference between treatment condition $z = 4$ and $z = 5$.