Consumption Choices and Policy Preferences under Social Interdependence

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Matthias Sebastian Holzmann

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Name des Erstgutachters: Prof. Alexander M. Danzer, PhD

Name des Zweitgutachters: Prof. Bertil Tungodden, PhD

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Matthias Holzmann

Statement of Contributions

Chapter 1 "Excuse My Behavior: Social Norms and Moral Costs" is joint work with Alexander M. Danzer. The project is based on a shared research idea. The experimental design was developed jointly. Data collection and empirical analyses were carried out by the author, based on funding provided by Alexander M. Danzer. The author wrote the first draft of the chapter, and subsequent revisions were undertaken jointly.

Chapter 2 "Pivotality in Consumption and Support for Regulation: Evidence from a Survey Experiment on CO_2 Pricing" is a single-authored project.

Chapter 3 "It Matters Whom You Regulate: The Role of Regulation Targets and Decision Rights for Policy Preferences" is joint work with Alexander M. Danzer. The project is based on a shared research idea, and the experimental design was developed jointly. The conceptual framework was developed by the author. Data collection and empirical analyses were carried out by the author, based on funding provided by Alexander M. Danzer. The author wrote the first draft of the chapter, and subsequent revisions were undertaken jointly.

Summary of Chapters

Chapter 1 examines how excusability affects consumption choices that entail a trade-off between private benefits and negative externalities. In a survey experiment, participants make consumption decisions involving a trade-off between personal monetary gains and CO_2 emissions. We distinguish between two types of excuses: replacement excuses (RE), whereby one's own consumption can be rationalized by assuming that others would substitute any foregone consumption, and waste excuses (WE), whereby abstaining from consumption may allegedly result in wasted resources. We find that both RE and WE significantly increase selfish consumption choices, with WE generating the strongest effect. Moreover, the availability of excuses systematically shifts participants' beliefs, increasing perceived prevalence and social acceptability of selfish behavior, while simultaneously reducing the moral costs associated with such choices.

Chapter 2 studies how reduced pivotality affects support for CO_2 pricing. In many consumption contexts with externalities, individuals are not fully pivotal: their actions have limited impact on aggregate outcomes. Such reduced pivotality may shape key determinants of support for CO_2 pricing: consumption behavior and beliefs about others. In an experimental market, participants make purchasing decisions that generate CO_2 emissions. In the control condition, a single consumer determines the market outcome, whereas in the treatment two consumers share the market. This reduces perceived pivotality, as refraining from consumption can be replaced by another consumer. Participants then vote on implementing a CO_2 price under three revenue recycling schemes. The treatment lowers support for CO_2 pricing by five percentage points only when revenues are allocated to the general state budget. The treatment also increases aggregate externalities, consistent with realized replacement behavior rather than with beliefs about being replaced.

Chapter 3 investigates whether policy support depends on the regulation target. In a survey experiment, individuals act as choice architects (CA) and can fix an allocation choice of a monetary fund between private payoffs and a donation reducing CO_2 emissions. We randomize the regulation target into three groups: oneself, someone else, or a group including oneself and someone else. The results show that CAs are more willing to regulate when their own outcomes are affected. The willingness to regulate diminishes when others' outcomes are also involved and becomes even smaller when only others' outcomes are at stake. Moreover, valuations of decision autonomy and anticipated misalignment with one's own future preferences are more predictive of regulation choices than concerns about satisfying others' preferences. Our findings provide novel evidence that regulatory decisions reflect a trade-off between self-interest, paternalistic preferences, and concerns for decision autonomy.

Statement on the Use of AI

AI-based tools were used in the preparation of this dissertation for selected programming-related tasks, questionnaire refinement, formatting support, and language polishing. In particular, ChatGPT, Gemini, and Claude were used to assist with revising text passages, refining and formatting questionnaire materials, and supporting selected coding tasks. The underlying research ideas, study designs, analyses, interpretations, and conclusions originate from the author or, in the case of co-authored chapters, from the respective authors. After the use of these tools, the author carefully reviewed, revised, and refined all relevant materials and checked them for consistency, coherence, and accuracy. Full responsibility for the content of this dissertation rests with the author.

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Preface

A central concern of microeconomics is how individuals make consumption choices. This question becomes especially important when consumption decisions generate externalities, such as CO_2 emissions, that impose social costs on others by contributing to climate change. Recent estimates place the social cost of an additional ton of CO_2 at around $185, suggesting that emissions-intensive consumption can impose substantial damages beyond the individual decision-maker (Rennert et al., 2022). This raises two related questions. First, how do individuals make consumption choices when private benefits must be weighed against environmental harm? Reducing the emissions associated with individual consumption often requires either consuming fewer emissions-intensive goods or substituting toward less CO_2-intensive, but often more expensive, alternatives. Understanding the behavioral foundations of such decisions is therefore essential. Second, because these social costs are not fully reflected in market prices, environmentally harmful consumption creates a case for regulation. The feasibility of such regulation, however, depends not only on its economic rationale, but also on public support and its determinants (see, e.g., Kallbekken and Sælen, 2011; Woerner et al., 2024; Dechezleprêtre et al., 2025).

Economic theory often studies consumption choices as individual decisions. In many consumption contexts involving externalities, however, individuals may perceive their own choices as not fully pivotal, that is, as having only limited influence on aggregate outcomes and the associated externalities. They may argue that a product has already been produced and that the externality has therefore already occurred, that someone else would consume it otherwise, or that it would be wasted if left unpurchased. Such narratives can reduce perceived personal responsibility for environmentally harmful behavior. More generally, prior research shows that individuals exploit moral wiggle room and diffuse responsibility in order to behave in self-serving ways while maintaining a positive self-image (Dana et al., 2007; Bartling and Fischbacher, 2012; Schwardmann and van der Weele, 2019). Recent work further suggests that the belief that one's own consumption will be replaced by others can substantially erode moral restraint, although social norms may in some cases limit this effect (Bartling and Özdemir, 2023;

Ziegler et al., 2024). Thus, environmentally relevant consumption choices may be shaped not only by prices and preferences, but also by moral interpretation, perceived responsibility, and social norms.

Lower perceived responsibility for externalities associated with individual consumption may increase the willingness to choose options associated with higher externalities (Ziegler et al., 2024). Applied to consumption decisions that generate CO_2 emissions, this may aggravate the underlying market failure by weakening moral restraint. From a welfare-economic perspective, this strengthens the case for corrective policies such as CO_2 pricing. At the same time, it raises an important political-economy question: does reduced personal responsibility also affect support for such regulation? This is particularly relevant because CO_2 pricing often faces substantial public opposition (Dechezleprêtre et al., 2025). Perceived pivotality may shape key determinants of support for CO_2 pricing. These include personal financial consequences, perceived effectiveness, and distributional concerns (Kallbekken and Sælen, 2011). It may do so, for example, through its effects on beliefs about others' behavior and on individuals' own consumption choices.

Interdependence in consumption choices has a second implication beyond reduced pivotality. Because individuals consume in markets shared with others, environmental regulation typically affects not only the individual decision-maker, but also other market participants. Even beyond perceived responsibility for emissions, this raises a broader question that is also central in public debates about taxation and climate policy: whose choices are affected by regulation, and how does this shape support for it? This question is particularly relevant for policies such as CO_2 pricing, which can affect consumers differently by imposing private costs and, if effective, restricting CO_2-intensive consumption. For this reason, such policies are often perceived as hard interventions. Previous studies show that individuals tend to prefer soft interventions, such as nudges, over hard interventions, such as taxes or product bans (Bartling et al., 2023), and may more generally resist paternalistic policies (Ambuehl et al., 2021), even though a growing debate argues that soft policies may not be effective enough to induce substantial behavioral change (Chater and Loewenstein, 2023). Support for environmental regulation may therefore depend not only on perceived effectiveness and distributional consequences, but also on the regulation target—whether the policy constrains oneself, others, or both.

This thesis investigates these issues in three related steps. Chapter 1 studies how excuses or justifications that reduce perceived personal responsibility shape environmentally relevant consumption choices. It also examines whether such excuses operate through self-serving distortions in social norms and moral costs. Chapter 2 then focuses on support for regulating markets in which consumption choices generate negative externalities and individuals are not fully pivotal in determining aggregate outcomes. Chapter 3 broadens the perspective by study-

ing how support for environmental regulation depends on the regulation target. All three chapters are based on survey experiments designed to study environmental preferences in stylized settings in which individuals trade off monetary payoffs against CO_2 emissions via donations. All chapters use samples that are representative of the German population with respect to age, gender, education, and federal state of residence. Taken together, the results show that justificatory narratives for more CO_2-intensive products increase externality-generating behavior and shift reported social norms and moral costs in a self-serving direction. Reduced pivotality lowers support for CO_2 pricing while increasing aggregate consumption. Finally, policies that regulate only others receive the least support, support is higher when both oneself and others are regulated, and it is highest when only oneself is regulated.

Chapter 1 examines whether individuals become more willing to choose higher private payoffs at the cost of higher externalities when they are exposed to justificatory narratives that reduce perceived personal responsibility. This mirrors many real-world consumption choices, in which the more CO_2-intensive option is often cheaper, for example in the case of flights versus trains or fast fashion versus more sustainable brands. The chapter focuses on two forms of justification: the replacement excuse (RE), according to which one's own consumption would be replaced by others, and the waste excuse (WE), according to which forgone consumption would otherwise be discarded. These narratives may make environmentally harmful choices appear more acceptable without changing the underlying material trade-off. In addition to studying behavioral responses, the chapter examines whether excuse narratives affect perceived social norms and moral costs, which may be important channels through which they operate.

To examine this question, Chapter 1 implements a survey experiment in which participants make incentivized consumption choices in three domains—clothing, food, and transportation. In each domain, participants choose between a cheaper but more CO_2-intensive option that yields a private payoff and a more expensive but more sustainable alternative that leads to a donation reducing CO_2 emissions. Participants are randomly assigned to one of four groups: a control group, an information treatment (*Info*), a *RE* treatment, and a *WE* treatment. Because the excuse treatments may not only provide moral justification but also additional information about the consumption context, the design includes an information treatment that provides the same background information without justificatory framing. The chapter further elicits beliefs about others' choices, perceived social acceptance, and a proxy for moral costs based on a multiple price list (MPL) in order to examine potential channels.

The results show that excuse narratives increase the choice of more CO_2-intensive products, with the *WE* consistently producing the largest quantitative effect. By contrast, the *Info* treatment has only limited effects on consumption choices. The stronger effect of *WE* is also reflected in the channel measures: it more strongly shifts beliefs in a self-serving direction and

reduces moral costs than *RE*. By contrast, effects on social-acceptance ratings are more similar across the two excuse treatments. A follow-up experiment further shows that these effects can persist over time and are not systematically offset by correcting misperceptions about others' behavior. These findings show that narratives reducing perceived responsibility can increase externality-generating behavior and shift social norms and moral costs in a self-serving direction, even without changes in market structure.

This raises the question of whether reduced responsibility induced by market design, rather than by narratives, has similar effects on consumption behavior. It also raises a second question: does such reduced pivotality shape support for regulating markets in which individual choices are not fully decisive for aggregate outcomes? Chapter 2 therefore examines whether reduced pivotality affects support for a CO_2 price and aggregate consumption when individual choices are no longer fully decisive for market outcomes. In contrast to Chapter 1, where reduced responsibility is introduced through justificatory narratives, this chapter makes interdependence between consumption choices an actual feature of the decision environment. By doing so, it shifts the focus from moral justification in individual choice to support for corrective policy under interdependent consumption.

To study this question, Chapter 2 implements a survey experiment in which participants make consumption choices in a stylized market setting. In each decision, they can purchase up to two products. Buying a product yields a private payoff, but also generates CO_2 emissions, whereas refraining from consumption yields no payoff and no emissions. Participants make this decision twice: first without a CO_2 price and then with a CO_2 price that raises the product's cost in proportion to its emissions.

The treatment varies whether consumption takes place under full pivotality or under replacement logic (*RL*). Under *RL*, aggregate supply is held constant while the number of market participants increases to two. Participants act sequentially: a first mover (FM) can serve the whole market, but forgone consumption may be replaced by a second mover (SM). In this way, the design introduces reduced pivotality directly into the market environment. It also creates rivalry in consumption, since one participant's decision can restrict another participant's opportunity to consume. In addition, participants vote on the CO_2 price under three revenue schemes—state budget, redistribution among participants, and donation to environmental causes. Depending on the voting outcome, either the choice with or without the CO_2 price is implemented. As in Chapter 1, this chapter elicits beliefs about others' purchasing behavior and perceived social acceptance in order to examine potential mechanisms.

The central result of Chapter 2 is that *RL* lowers support for CO_2 pricing by about 5 percentage points when revenues are used for the state budget. By contrast, *RL* does not significantly reduce support when revenues are earmarked for redistribution among participants or for en-

vironmental projects. At the same time, *RL* increases aggregate consumption, indicating that reduced pivotality affects both individual behavior and policy support.

Beliefs and social norms help explain this pattern. In the state-budget condition, *RL* increases the perceived effectiveness of the CO_2 price, as measured by expected differences in aggregate consumption with and without the CO_2 price. At the same time, greater perceived effectiveness is associated with lower support for the CO_2 price. This pattern appears counterintuitive at first. One possible explanation is that individuals who perceive the CO_2 price as highly effective in reducing consumption may also see it as restricting others' ability to consume. This may make the policy appear less fair and is consistent with the consumption results. The increase in aggregate consumption appears to be driven less by FMs strategically exploiting *RL* than by actual replacement through SMs. Consequently, FMs seem reluctant to serve the whole market. In addition, social-acceptance ratings indicate that buying all available products is viewed as less appropriate in the *RL* market. These findings suggest that reduced pivotality shapes support through beliefs about effectiveness and fairness, while also affecting consumption behavior through actual replacement rather than strategic exploitation.

Chapter 2 provides suggestive evidence consistent with a fairness-based interpretation, namely that a highly effective CO_2 price may reduce support because it is perceived as excluding others from consumption. However, the chapter cannot fully resolve this mechanism. More broadly, these findings reinforce a question already raised by the interdependence of consumption choices in markets: how does support for regulation depend on who is constrained by a policy, and how? Since a CO_2 price affects not only oneself but also others, the effectiveness concerns discussed in Chapter 2 suggest that individuals care about the policy consequences for other market participants as well.

Taken together, these findings point to a broader determinant of policy support beyond responsibility for aggregate outcomes: whether regulation affects only oneself, only others, or both. Chapter 3 therefore studies this question and examines two mechanisms that may shape support for regulation. Regulation does not only affect outcomes; it also constrains decision rights. If individuals value both their own and others' autonomy (Freundt et al., 2023), support may depend on who is affected by a policy for this reason as well. This is especially relevant for hard interventions, because individuals may be reluctant to impose strong restrictions on others' choices (Bartling et al., 2023). The chapter therefore also examines whether the valuation of decision rights helps explain support for regulation. Moreover, supporting regulation often means implementing one's own preferred outcome for others. Insights from the social-preferences literature suggest that individuals care about others' outcomes (see, e.g., Fehr and Charness, 2025, for a review). The chapter therefore investigates whether anticipated misalignment between one's own preferences and those of others helps explain support for regulation.

To study this question, Chapter 3 implements a survey experiment in which participants act as choice architects (CAs) who decide whether to regulate a decision task carried out by a decision-maker (DM). The DM's task consists of allocating €2 between a private payoff and a donation that reduces CO_2 emissions. The experiment exploits the two-wave structure of the survey: each participant acts as a CA in the first wave and as a DM in the second survey wave. Before treatment assignment, the experiment elicits each participant's own preferred allocation, their valuation of autonomy for themselves and for others, and their beliefs about others' preferred allocation choices. Participants are then informed that they take on the role of a CA with respect to a DM facing the allocation task in the subsequent survey wave. The key experimental variation is the regulation target: the DM is either oneself (*Self* treatment), another person (*Other* treatment), or a group consisting of oneself and another person (*Group* treatment). Participants then decide whether to impose their previously stated preferred allocation on the respective DM(s) or to grant freedom of choice.

The central result of Chapter 3 is that CAs regulate themselves most often, followed by the *Group* treatment, and least often in the *Other* treatment. This pattern suggests that individuals take into account not only the consequences of regulation for themselves, but also those for others, and that they incorporate both when making regulation decisions. Pre-treatment measures of autonomy valuations and beliefs provide further insight into this pattern. Differences between a CA's own preferred allocation and their beliefs about others' preferred allocations do not systematically predict the regulation decision. By contrast, the valuation of one's own and others' autonomy predicts whether regulation is chosen. This suggests that reluctance to regulate arises less from uncertainty about others' preferences than from concern for their decision autonomy.

Taken together, the chapters show the following. Chapter 1 demonstrates that narratives reducing perceived personal responsibility increase externality-generating consumption and shift perceived social norms and moral costs in a self-serving direction. Chapter 2 shows that reduced pivotality lowers support for CO_2 pricing when revenues accrue to the state budget, while aggregate consumption increases; beliefs about policy effectiveness and fairness help explain this pattern. Chapter 3 shows that support for regulation varies systematically with the regulation target and is shaped more strongly by autonomy concerns than by anticipated misalignment with others' preferences: it is highest when only oneself is regulated, lower when both oneself and others are regulated, and lowest when only others are regulated.

The dissertation shows that environmentally relevant behavior is shaped by the interdependence of consumption choices and their collective consequences. Individual decisions contribute to aggregate externalities, while policies intended to regulate such behavior affect not only those who support them, but also other market participants. A recurring theme across

all three chapters is that beliefs about others matter, but in different ways depending on the setting. They may serve as self-serving justifications for environmentally harmful behavior, shape perceptions of the effectiveness and fairness of regulation, or become less important once individuals are given direct authority to impose constraints on others. More broadly, the dissertation highlights that perceived responsibility, social norms, and concern for others are central to understanding both environmentally relevant consumption and support for environmental policy.

These findings have several policy implications. First, a lower sense of responsibility for aggregate outcomes can have two adverse effects: it can increase externality-generating behavior while weakening support for corrective regulation. This suggests that policymakers should pay closer attention to how strongly individuals perceive their own choices as affecting aggregate outcomes across different markets, such as energy, transportation, food, or clothing. The results further suggest that revenue earmarking may be particularly important for maintaining support in settings in which individuals have little influence on aggregate outcomes. More generally, fairness concerns about excluding others from consumption may be especially relevant for policy support in such settings.

Second, price-based regulation may work differently across settings with different degrees of perceived responsibility. Corrective price incentives raise the cost of environmentally harmful choices and may thereby offset lower moral costs. However, perceived responsibility also shapes social norms, which are themselves important determinants of consumption behavior. If norms shift in a self-serving direction, price incentives alone may not be sufficient to induce behavioral change. Policymakers may therefore want to accompany price-based instruments with measures that correct misperceived descriptive norms or address the social acceptability of environmentally harmful choices. Although the evidence in this dissertation remains limited, the results suggest that correcting misperceived descriptive norms may matter in some settings.

Third, support for environmental regulation appears to depend not only on expected outcomes, social norms, or moral costs, but also on who is constrained by a policy. This highlights the importance of considering decision rights and perceived autonomy when designing and communicating environmental regulation. Even when regulation implements an allocation individuals themselves prefer, they are less willing to impose it once others are affected as well. This indicates that reluctance toward regulation may arise not only from disagreement about outcomes, but also from discomfort with restricting others' freedom of choice.

Finally, the dissertation points to several avenues for future research. A first question is how different degrees and forms of pivotality shape behavior and policy support across settings. While Chapter 1 and Chapter 2 both study reduced responsibility, they do so in differ-

ent ways—through justificatory narratives in the former and through market structure in the latter—and the corresponding mechanism patterns are not identical. Future research could therefore investigate more systematically how pivotality affects moral costs, social norms, and support for regulation. Relatedly, the present studies cannot fully disentangle whether shifts in social norms affect moral costs or vice versa. A better understanding of this relationship would help clarify how perceived responsibility translates into behavior.

A second avenue concerns external validity. The chapters rely on stylized experimental settings to isolate mechanisms cleanly, but future work should examine how these mechanisms operate in more concrete environmental policy settings. This may be particularly relevant in domains such as transportation, clothing, or energy consumption, where market structure, perceived responsibility, and the public visibility of choices may differ substantially. Studying such settings would help assess how far the mechanisms identified in this dissertation carry over to real-world policy environments.

A final avenue for future research concerns the broader applicability of the results on regulation targets and decision rights beyond environmental policy. The question of who is affected by a policy may also matter in other domains in which costs and constraints are distributed across individuals, for example in debates about income taxation, inheritance taxation, or public transfers. Studying such settings would help assess how far the mechanisms identified in Chapter 3 generalize across policy domains.

Chapter 1

Excuse My Behavior: Social Norms and Moral Costs

1.1 Introduction

Individuals care about a positive self-image and want others to hold favorable views about them (Konow, 2000; Bénabou and Tirole, 2011). Yet, individual choices sometimes impose social costs and thereby undermine this self-image. Why does this occur and how do individuals resolve this tension? Prior research highlights motivated reasoning as a tool for preserving a positive self-image while behaving selfishly (e.g., Di Tella et al., 2015; Saccardo and Serra-Garcia, 2023). In particular, individuals may strategically use excuses to justify selfish actions, for instance by diluting personal responsibility or downplaying their perceived impact on aggregate outcomes.

For example, consumers may justify selfish or morally questionable behavior by invoking the actions of others (Danzer, 2024), thinking, "If I don't do it, someone else will." This reasoning reflects the replacement excuse (RE), which can explain why even morally conscientious individuals sometimes act in ways that harm their positive self-image. For instance, some environmentally conscious individuals choose to fly frequently (Barr et al., 2011). Opting for a cheaper plane ticket over a train ride might be rationalized by the belief that someone else will purchase the ticket anyway, leaving total carbon emissions unchanged. Alternatively, the

This chapter is joint work with Alexander M. Danzer. We thank Toker Doganoglu, Hartmut Egger, Sebastian Goerg, Michael Hilweg-Waldeck, Orestis Kopsacheilis, Matthias Sutter, James Tremewan, Bertil Tungodden, and numerous seminar and conference participants for very helpful suggestions and comments. Holzmann thanks FAIR at NHH, Norway, for their hospitality, and the BGPE for supporting his research stay at NHH. This project was pre-registered in the AEA RCT Registry under IDs AEARCTR-0015264 and AEARCTR-0015370. The pre-analysis plan is in Appendix F.1.

same behavior could be excused by a related narrative: "If I don't do it and no one else does, resources may be wasted." We refer to this as the waste excuse (WE), since an empty seat on a scheduled flight might be perceived as a wasted resource. Both excuses justify socially contested behavior by downplaying individual responsibility for the outcome, yet they offer opposing interpretations of the decision context. Understanding how the availability of different excuses shapes economic decision-making is essential, as it may influence the effectiveness of policies designed to regulate externalities (Kaufmann et al., 2024).

Moral values and their behavioral implications are central to the study of excuses. In economic settings, moral considerations typically arise when choices entail a trade-off between private utility (e.g., monetary gains) and negative externalities imposed on others (e.g., carbon emissions, canceling donations). Actions that generate larger externalities are commonly regarded as less moral; for brevity, we refer to them as selfish. Research on the RE and its relation to moral behavior remains nascent and yields mixed findings, and the WE has not yet been examined directly. Bartling and Özdemir (2023) provide experimental evidence that the availability of the RE has only limited effects on the erosion of moral behavior, largely due to countervailing social norms. In contrast, Ziegler et al. (2024) documents a complete erosion of moral values in an experimental market when the RE is present: although individuals endorse a social norm that condemns RE-related behavior, they nonetheless fail to comply with it. Motivated by this discrepancy and the sparse empirical evidence, we examine the relevance of both the RE and the WE for morally relevant behavior in environmentally relevant consumption contexts. A central contribution of our study is that we introduce two distinct forms of excuses without modifying the underlying market structure. Specifically, we provide narratives that invoke the RE and the WE while holding all other features constant. This allows us to isolate how alternative interpretations of identical objective information facilitate excuses for socially suboptimal consumption choices.

Moreover, we investigate how excuses can trigger motivated reasoning and influence behavior by affecting social norms and moral costs. Both excuses frame externalities as inevitable and beyond individual control, but they may affect norms differently. The RE implies that others might consume instead of oneself, potentially increasing beliefs about the prevalence of selfish behavior (descriptive norms). In contrast, the WE assumes that others might not consume at all. This makes resource waste salient and may shift injunctive norms by framing harmful actions as more socially acceptable. Changes in norms could therefore reduce the perceived moral costs of selfish actions by making them appear less morally inappropriate. From a theoretical perspective, a utility-maximizing agent is more likely to choose the selfish option when perceived moral costs fall below the private gains from consumption. While Ziegler et al. (2024) measure moral costs, they assume these costs remain constant with or without the RE.

Our study challenges this assumption by examining whether moral costs change in response to excuses.

Previous research has primarily tested the RE in abstract settings. In contrast, we apply the WE and RE to three different real-world consumer product categories: frequent purchases (apples from New Zealand or Germany), durable goods (buying a normal cotton or organic cotton shirt), and transportation (flights or a train ride). Although these consumption choices are hypothetical, we incentivize them: participants can earn money for selfish actions, while non-selfish choices lead to donations that reduce carbon emissions. In a survey experiment, participants receive statements designed to trigger either the WE or RE in each consumption scenario. The justificatory effect of the RE and WE arises from a self-serving interpretation of the decision environment. Excuses may both justify behavior and convey information, such as the likelihood that others will purchase a product. To isolate the justificatory role of excuses from their informational content, we include an objective information treatment and embed the same information in the excuse treatments. The control group receives neither objective information nor excuse statements. We also elicit injunctive and descriptive social norms as well as moral costs to analyze the impact of excuses.

We show that the RE and WE significantly increase selfish consumption choices, with the WE having the strongest effect and information provision having only a limited effect. Importantly, only the WE differs significantly from the information treatment, suggesting that the effect of the RE does not extend beyond a purely informational mechanism. Both excuses also increase participants' beliefs about the prevalence and social acceptance of selfish behavior, suggesting that such justificatory narratives shift perceived social norms in self-serving ways. Contrary to our expectations, the WE exerts a stronger influence on beliefs than the RE, while their effects on injunctive norms are more similar. Interestingly, the excuse treatments not only increase the social acceptance of selfish actions but also reduce the social acceptability of non-selfish choices. Furthermore, both excuses reduce moral costs, with the WE again showing the strongest effect. This result is particularly noteworthy, as the idea that moral costs can be influenced by excuse narratives has received little attention so far. Overall, our evidence is consistent with motivated reasoning: the WE exerts a stronger influence and is also associated with larger shifts in social norms and moral costs.

In a second survey wave, collected one to two weeks later, participants make the same consumption choices again without receiving the treatment information a second time. The effect of the WE is not only stronger than that of the RE but also persists over time in two consumption scenarios, indicating a medium-run effect of excuses. Because excuses increase selfish consumption choices—and thereby emissions—we test how to offset these effects in the second wave. In the first wave, participants on average overestimated the actual share of indi-

viduals choosing the selfish option by 11 to 23 percentage points. Since these beliefs predict consumption choices, we conduct an additional experiment that corrects these misperceptions and examine its effect on consumption. The intervention is ineffective for consumption choices, as it does not attenuate the WE effect. We also examine effects on political views (e.g., perceived severity of climate change) and pro-environmental intentions (e.g., flying less or reducing home heating). Excuse treatments negatively affect both domains, but the norm correction mitigates these effects only for pro-environmental intentions. This suggests that norm corrections can partially counteract intention-based responses to excuse narratives, even if they do not translate into changes in consumption choices.

This paper contributes to three literatures. First, it contributes to the literature on socially responsible consumer behavior and the RE. Prior work examines how individuals exploit situations in which they are not fully responsible for aggregate outcomes, for instance by engaging in the RE in controlled laboratory experiments or theoretical models (Brütt et al., 2020; Bartling and Özdemir, 2023; Kaufmann et al., 2024; Ziegler et al., 2024). Empirical evidence on the RE is mixed: social norms sometimes prevent increases in externalities, but at other times fail to do so. We extend this literature by introducing the waste excuse (WE) alongside the RE in a representative, non-student sample and across three common consumption domains. Unlike prior work using stylized market settings, we do not alter market structures but use common narratives aimed at reducing perceived personal responsibility.

Second, this study contributes to the large literature on moral wiggle room and motivated reasoning. Prior research shows that individuals self-servingly exploit ambiguity, risk, the potential for mistakes, and beliefs about others' actions to justify selfish choices (Haisley and Weber, 2010; Di Tella et al., 2015; Exley, 2016, 2020; Exley and Kessler, 2024), strategically avoid information and exploit situations with diffuse responsibility to reduce accountability (Dana et al., 2007; Bartling and Fischbacher, 2012; Grossman, 2014; Bartling et al., 2015b; Irlenbusch and Saxler, 2019; Falk et al., 2020), and distort beliefs and social norms to maintain a positive self-image (e.g., Konow, 2000; Bicchieri and Xiao, 2009; Barkan et al., 2015; van der Weele et al., 2014; Schwardmann and van der Weele, 2019; Schwardmann et al., 2022; Bursztyn et al., 2020; Gneezy et al., 2020; Bicchieri et al., 2023). In our setting, excuse narratives shift perceived social norms and reduce moral costs, providing novel empirical evidence on how such narratives increase selfish actions.

Third, this paper contributes to the growing literature on narratives and their role in economic decision-making. Prior work examines how narratives shape macroeconomic expectations (Shiller, 2017; Andre et al., 2026), influence beliefs and thereby affect behavior and policy preferences (Eliaz and Spiegler, 2020; Barron and Fries, 2024), and operate in settings involving moral trade-offs (Foerster and van der Weele, 2021; Barron and Fries, 2024). We extend this

literature by showing how exculpatory narratives—which reinterpret objective information in self-serving ways—are used in environmentally relevant decision contexts to justify selfish behavior.

The remainder of the paper is organized as follows. Section 1.2 presents the main experiment, including the experimental design, conceptual framework, hypotheses, data, and results. Section 1.3 presents the follow-up experiment along the same lines. Section 1.4 concludes with a discussion of the findings and their broader implications.

1.2 Main Experiment

1.2.1 Experimental Design

Each participant makes three hypothetical, yet financially incentivized, choices between two products that differ in price and environmental impact. In every scenario, the lower-priced alternative is associated with higher CO_2 emissions, while the higher-priced alternative entails a smaller environmental footprint. Product prices and emission levels are calibrated to reflect realistic market conditions. To mitigate potential order effects, the sequence of the three consumption decisions is randomized across respondents. The decisions span three major consumption domains: clothing, food, and transportation.

1. Normal cotton shirt (costs of €10 and emissions of 7 kg CO_2) vs. organic cotton shirt (costs of €13 and emissions of 3.8 kg CO_2)

2. 1 kg of apples from New Zealand (costs of €3.75 and emissions of 0.8 kg CO_2) vs. 1 kg of apples from Germany (costs of €4.50 and emissions of 0.3 kg CO_2)

3. Flight vs. train ride: Decision between a flight (costs of €85 and emissions of 180 kg CO_2) and a train ride (costs of €110 and 24 kg CO_2) from Munich to Paris.

To facilitate comprehension of the scale of one kilogram of CO_2 emissions, we ask participants to estimate the distance a typical car would need to be driven to generate this amount. We then provide the correct reference value—approximately six kilometers on average—before participants proceed to the experimental tasks.

For the shirts and apples decisions, participants are explicitly informed that the quality, feel, taste, and other product characteristics are identical across options. In the "flight vs. train ride" scenario, we inform participants that both modes of transport have the same time requirement to travel from Munich to Paris. This accounts for check-in and check-out, and total time spent at the airport. This aims at eliciting consumption choices that are mainly driven

by differences in prices and emissions. In order to measure revealed preferences, we incentivize these consumption choices. Choosing the cheaper, more carbon-intensive option yields a monetary payoff equal to the price differential (e.g., €3 for selecting the normal cotton shirt instead of the organic cotton shirt). Choosing the more expensive, less carbon-intensive option results in a CO_2 reduction proportional to the emission differential (e.g., a 3.2 kg CO_2 reduction for selecting the organic cotton shirt). CO_2 reductions are implemented through a donation to "atmosfair", an NGO that invests in CO_2 reductions, e.g., through emission-free stoves in developing countries. We randomly select 20% of participants and randomly implement one of their three consumption choices. Based on their decision, this either leads to a personal payoff or a donation to the NGO. This way, we generate an immediate trade-off between personal benefit and social cost. The monetary incentives take the form of bonus payments conditional on survey completion.

In total, our experimental design comprises four groups. In the control group (*Control*), participants neither receive additional (objective) information about the decision context nor any excuse statement. In the information treatment (*Info*), participants receive objective information for each decision. In the replacement excuse (*RE*) and waste excuse (*WE*) treatments, participants receive the same objective information *plus* one specific excuse statement for each of the three consumption choices. These excuse statements may influence behavior through two channels: they may convey information about the decision context, and they may offer a favorable interpretation of this information that facilitates justification. To disentangle the informational effect from the justificatory effect of excuse narratives, we include *Info*, which contains the same objective information as the excuse treatments but does not provide an excuse-based interpretation. The objective information in *Info*, *RE* and *WE* states (translated from German):

- **Normal vs. Organic Cotton T-Shirt**: Both the normal and organic cotton T-shirts are available in the store and could also be purchased by other customers. Unsold stock is generally removed from the inventory and disposed of.
- **Apples from New Zealand vs. Germany**: The supermarket offers both apples from Germany and New Zealand, and other customers could also buy them. Unsold food, such as apples, is disposed of in the supermarket.
- **Flight vs. Train**: Both flight and train tickets are still available, and both the flight and the train journey will definitely take place. Other people are also facing this decision.

The *RE* statement adds:

- **Normal vs. Organic Cotton T-Shirt**: The normal T-shirt has already been produced, and if you don't buy it, someone else might.

- **Apples from New Zealand vs. Germany**: The apples from New Zealand are already in the supermarket, and if you don't buy them, someone else might.
- **Flight vs. Train**: The flight will take place anyway, and if you don't take the flight, someone else might take it instead.

The *WE* statement adds:

- **Normal vs. Organic Cotton T-Shirt**: The normal T-shirt has already been produced, and if you don't buy it, it might be thrown away.
- **Apples from New Zealand vs. Germany**: The apples from New Zealand are already in the supermarket, and if you don't buy them, they might be thrown away.
- **Flight vs. Train**: The flight will take place anyway and is not yet fully booked, and if you don't take the flight, a seat might remain empty.

The statements emphasize key features of the decision context that may enable participants to maintain a positive self-image while choosing the selfish option. More specifically, both excuses can be seen as rational moral arguments that create moral wiggle room and diffuse personal responsibility in three ways. First, they emphasize that the occurrence of the externality is beyond the individual's control. Second, they introduce uncertainty: individuals typically cannot be sure whether someone else will make a particular consumption choice. Third, and this is the dimension that differs across the two excuses, they frame the consequence of not choosing the selfish option differently: as potential resource waste in *WE* or as consumption by others in *RE*. We hold the first two dimensions constant across excuse statements. Hence, any observed treatment effects stem solely from differences in the interpretation of the objective information. However, these excuses can easily be challenged, as they neglect the possibility that externalities may decline over time if aggregate demand for harmful products falls.

The shirt choice is the most tightly controlled scenario, with the key trade-off being purely between money and CO_2 emissions, though participants might infer additional ethical dimensions (e.g., labor conditions) or exhibit a general bias toward eco-labeled products. The apples scenario is similarly controlled but involves lower stakes; given the German sample, a potential home bias toward domestic products may exist. The flight-versus-train scenario entails the highest stakes and aligns most naturally with the excuse narratives, though participants may hold preferences beyond the intended trade-off (e.g., comfort or habit). In all cases, however, such confounds should be randomly distributed across treatment groups. Examining excuses across three distinct, yet common consumption contexts strengthens the external validity of

our design. Note also that we do not hold the €/kg CO_2 ratios constant across scenarios in order to ensure realistic trade-offs within each domain.

To measure injunctive norms, we follow the method of Krupka and Weber (2013). Participants rate the social appropriateness of each of the six possible choices (three scenarios, each with two options) on a four-point scale: "Socially very inappropriate", "Socially somewhat inappropriate", "Socially somewhat appropriate", and "Socially very appropriate". The question clarifies: "By socially appropriate, we mean behavior that most people agree is the "correct" or "preferable" thing to do." To avoid order effects, we present the six choices in a randomized order. Beliefs about what others would choose in the three scenarios serve as a measure for descriptive norms: "How large do you believe is the share of people in the survey that make the following choices?". Again, we present the choices in a randomized order.

To measure moral costs, we employ a multiple price list (MPL) approach for each of the three scenarios. The MPL identifies the point at which participants switch between the two options as relative prices change. We hold the price of the more expensive, less CO_2-intensive option constant while gradually reducing the price of the cheaper, more CO_2-intensive option. The switching point provides an estimate of the private disutility associated with choosing the more carbon-intensive option. This reduced-form measure captures internal moral costs and, to the extent present, internalized social norms. For each of the three scenarios, we start with a price difference of €0 and increase the price difference in seven steps. The cotton shirt is €1.30 to €9.10 cheaper in €1.30 increments. The apples from New Zealand are €0.50 up to €3.50 cheaper in €0.50 increments. The flight is €10 up to €70 cheaper in €10 increments. Participants see the three MPLs in randomized order to control for order effects. More details on the coding of the variables for moral costs and social norms can be found below in Section 1.2.4. Since we expect the excuse treatments to shift social norms and moral costs, we do not incentivize their elicitation. Monetary incentives could interfere with these perceptions and would therefore work against our objective of measuring treatment-induced changes in them. However, our main outcomes—the consumption choices—remain incentivized.[1]

1.2.2 Conceptual Framework and Hypotheses

According to standard economic theory, an agent incurs no costs for making a morally contested choice (e.g., choosing an option with higher emissions) and exclusively values his own payoff while disregarding choices of third parties. However, empirical evidence suggests that individual decision-making is influenced by beliefs about others' actions, societal approval

[1]The questionnaire for the experimental questions is shown in Appendix D.1 and the "atmosfair" certificates are shown in Appendix D.3.

through social norms (Bicchieri and Xiao, 2009), and moral costs (Ziegler et al., 2024). To maintain a positive self-image, individuals weigh the material benefits of a decision against the potential costs of social norm violations and the associated moral costs. The following utility function captures these considerations:

$$U_i(x) = F_i(x) - C^m{}_i(x) - C^s{}_i(x) \tag{1.1}$$

where $U_i(x)$ represents the utility that individual i derives from choosing the selfish option x. $F_i(x)$ reflects the utility from obtaining a higher payoff compared to the alternative associated with a lower externality. i also incurs costs from choosing the selfish option: $C^m{}_i(x)$ reflects personal moral costs, which we interpret as the *personal* component of self-image. $C^s{}_i(x)$ reflects perceived social costs from transgressing social norms, which we interpret as the *social* component of self-image. We further assume that moral and norm transgression costs increase in the size of emissions. i will only choose the selfish option if the benefit $F_i(x)$ outweighs the costs $C^m{}_i(x)$ and $C^s{}_i(x)$.

Now consider the effect of excuses on these costs. First, excuses may lower moral costs $C^m{}_i(x)$. They do so by leading individuals to perceive a given action as less immoral, since the consequences for the aggregate outcome are attenuated through reduced personal responsibility. Second, excuses may reduce $C^s{}_i(x)$ by influencing perceptions of social norms. Since the RE emphasizes that others might replace one's own consumption, the perceived prevalence of selfish behavior may increase. In turn, this reduces norm transgression costs, as the social pressure to behave non-selfishly is alleviated. This mechanism aligns with motivated reasoning via strategic belief distortion (e.g., Di Tella et al., 2015).

In contrast, the WE frames selfish actions as preventing resource waste because others may not act selfishly. This framing could lower the perceived prevalence of selfish behavior in the population. At the same time, the focus on resource waste may increase the perceived social acceptance of selfish actions and even reduce the perceived acceptability of non-selfish actions (i.e., injunctive norms). In this case, selfish choices become less harmful to one's positive self-image from an injunctive-norm perspective. For the WE to affect behavior through social norms, its effect on injunctive norms must dominate any opposing effect operating through descriptive norms. In sum, the RE excuses the actor because others might step in, whereas the WE excuses the action by framing it as more socially acceptable. We note that the framework does not rule out interactions between social (norm-violation) costs and moral costs. Accordingly, our design does not allow us to cleanly disentangle the two.

Based on this simple framework and the experimental design, we aim to answer the two core research questions:

1. Does the availability of excuses affect selfish behavior in the three consumption choices?
2. Through which channels do excuses influence consumption choices?
   (a) Does the availability of excuses affect norms (injunctive and descriptive) in the three consumption choices?
   (b) Does the availability of excuses affect moral costs in the three consumption choices?

The first research question focuses on the impact of excuses on consumption choices, while the second research question and its two sub-questions explore the mechanisms driving the effectiveness of these excuses. Our first hypothesis states that:

- H1: The share of individuals choosing the selfish option is higher in treatments *RE* and *WE* compared to *Control* and *Info*.

Generally, we expect the *RE* and *WE* treatments to increase selfish consumption choices compared to *Control* and *Info*. Differences between *Control* and *Info* should be minor. However, if there are any differences, the share of selfish consumption choices should be higher in *Info,* as providing additional informational context may prompt participants to interpret the situation in terms of the *RE* and *WE* narratives. In our framework, the relative effectiveness of the two excuses depends on their influence on descriptive norms, injunctive norms, and moral costs. Additionally, descriptive norms have been shown to be stronger drivers of behavior than injunctive norms (see, e.g., Bicchieri and Xiao, 2009), which may also matter for the relative importance of these channels. Therefore, H2 through H4 examine the mechanisms behind these effects:

- H2: The belief about the proportion of individuals choosing the selfish option in *RE* is higher compared to *Control* and *Info,* while it is the lowest in *WE*.
- H3: The selfish option is perceived as socially more appropriate in *WE* compared to *Control*, *Info,* and *RE*.
- H4: Moral costs are lower in *RE* and *WE* compared to *Control* and *Info*.

Based on our framework, H2, H3, and H4 follow from the narratives of the excuse statements, which emphasize others' behaviors and the framing of moral obligations: replacement of own consumption (targeting beliefs) and waste of resources (targeting beliefs and social acceptance). Consequently, we hypothesize that the perceived prevalence of selfish behavior is lowest in *WE* (H2), while the perceived social acceptance of the selfish option is highest in *WE*

(H3). We further expect moral costs to be lower in *WE* and *RE* than in *Control* and *Info* (H4). Since *WE* more directly legitimizes the selfish action itself, moral costs may be lowest in *WE*. We do not expect significant differences between *Control* and *Info* with respect to H2, H3, and H4, as the additional information is framed objectively.

1.2.3 Data

We fielded a survey with 2,414 individuals drawn to be representative of the adult German population with respect to age, gender, education, and residential federal state. The study is titled "Preferences for Environmental and Sustainable Policies and the Replacement Excuse: An Experimental Investigation" (PRESPARE). To ensure data quality, participants were required to pass an attention check before proceeding.[2] Data collection was conducted between January 27 and February 7, 2025 by the professional survey provider Bilendi, whose panelists received standard compensation for participation. After excluding respondents with missing values on key variables, the effective sample for analysis comprises 2,408 individuals. All participants were re-invited for a second survey wave, fielded from February 12 to February 21, 2025, which achieved an 80% response rate. Median completion time was approximately 24 minutes. The average payment from the 483 implemented consumption decisions was €3.20, while the average CO_2 reduction amounted to 39 kg.

In the first wave, we elicit detailed demographic and socio-economic information, including age, gender, education, employment status, the presence and number of minor children in the household, and federal state of residence. This is followed by the survey experiment. Participants could proceed only after they answered a comprehension question correctly. Finally, respondents complete an extensive post-experimental questionnaire which includes a second attention check. It collects respondents' personal net income, voting intentions, beliefs regarding the existence of climate change, its human causation, the severity of the problem, their perceived personal exposure to climate-related impacts, and attitudes towards climate policies and governmental regulation. The questionnaire also measures individual risk preferences, time preferences, and locus of control using established items from the German Socio-Economic Panel. We exclude variables from the post-experimental questionnaire from the main analysis because they were elicited post-treatment.

Panel A in Table A.2 in the Appendix presents summary statistics for demographic and socio-economic characteristics elicited pre-treatment. Columns (1) to (5) display means and standard deviations for the full sample and the four experimental groups: *Control*, *Info*, *RE*,

[2]Table A.1 in the Appendix compares the sample with official Eurostat statistics and indicates close alignment along these and additional dimensions such as income and employment status. Appendix E contains detailed survey and data documentation and Appendix D the full questionnaire.

and *WE*. Individuals in the sample are, on average, approximately 49 years old, and half of them are female. About 23% of participants have at least a tertiary education degree (college or equivalent), around 27% have minor children, with households containing on average 0.43 minor children. Regarding employment status, 45% of the sample is employed full-time.

In the second survey wave, we conducted a follow-up of the original experiment and a new experiment, described in detail below. Details on the sample can be found in the Appendix in Tables A.13, A.14, and A.15. The first- and second-wave samples are largely comparable, with only minor differences. The 80% response rate is consistent with expectations from the survey company. An attrition analysis (Table A.12) shows that age, tertiary education, and full-time employment ($p < 0.01$) are correlated with participation in the second wave. Treatment status is uncorrelated.

1.2.4 Empirical Strategy

The main outcomes—the three consumption choices—are coded as follows: a dummy equal to 1 if a participant chooses the normal (cotton) shirt, a dummy equal to 1 if a participant chooses the apples from New Zealand, and a dummy equal to 1 if a participant chooses the flight (i.e., always the cheaper options with higher CO_2 emissions). Regarding injunctive norms, we code the social-appropriateness responses as follows: -2 "Socially very inappropriate", -1 "Socially somewhat inappropriate", 1 "Socially somewhat appropriate", and 2 "Socially very appropriate." Descriptive norms—beliefs about what others would choose—range from 0 to 100 percent.

To measure moral costs, we define a switching point, ranging from 0 to 7, as the price difference at which participants switch from the more environmentally friendly option to the less environmentally friendly but cheaper option (e.g., from organic to normal cotton shirt). If a participant exhibits multiple switches, we use the first switch from the environmentally friendly to the conventional option. We exclude individuals who switch in the opposite direction (i.e., from the cheaper to the more expensive option), as such patterns are inconsistent with monotonic preferences. For participants who never switch to the selfish option, we assign the maximum switching value. For participants who always choose the selfish option, we assign the minimum switching value.[3]

First, we compare raw mean differences between the treatment groups for each outcome of interest. Afterwards, we estimate the following equation using OLS:

$$Y_i = \alpha + \beta_1 Info_i + \beta_2 RE_i + \beta_3 WE_i + X_i'\gamma + \epsilon_i \tag{1.2}$$

where i indexes an individual and the *Control* group serves as the reference category. Y_i

[3]Table A.3 in the Appendix shows that inconsistencies in the MPL are uncorrelated with treatment assignment.

represents the respective outcome of interest, i.e., the consumption choices, injunctive and descriptive norms, and moral costs. β_1, β_2, and β_3 measure the average treatment effects of the *Info*, *RE*, and *WE* treatments, respectively, compared to the *Control* group. Standard errors are robust to heteroskedasticity. X_i' is a vector of controls including age, a female dummy, a tertiary education dummy, a full-time employment dummy, a dummy indicating the presence of minor children in the household, and the number of minor children in the household.[4] The error term ϵ_i captures unobserved determinants of the outcome.[5]

Furthermore, to study the relationship between norms, moral costs, and consumption choices, we correlate these measures. Specifically, we include social norms and moral costs to Equation (1.2) as controls with the consumption choices as outcomes. This provides suggestive evidence on the relative importance of norms and moral costs for consumption choices in the experiment.

1.2.5 Results

Balancing. The t-tests in Panel A of Table A.5 in the Appendix indicate that randomization was generally successful, as most variables do not differ significantly across groups. Nonetheless, some imbalances are present: out of 54 tests, four are significant at the 10% level and one at the 1% level, which is within the range expected by chance. In Table A.5, differences are calculated such that, for example, "C vs. I" subtracts the mean in the *Control* group from the mean in the *Info* group; all other comparisons follow the same convention.

Panel B of Table A.2 in the Appendix presents summary statistics for the main outcomes. Column (1) refers to the full sample. Forty-two percent select the normal shirt, 25% opt for apples from New Zealand, and 35% choose the flight option. On average, individuals believe that 53% choose the normal shirt, 48% the apples from New Zealand, and 53% the flight. Social acceptance ratings are positive for choosing a normal shirt (0.34), an organic shirt (0.83), German apples (0.98), and the train (0.88). In contrast, choosing apples from New Zealand (-0.04) and flying (-0.12) are rated negatively. The average switching points (measured in steps and not in €) are 3.83 for switching from organic to normal shirts, 4.20 for switching from German to New Zealand apples, and 4.12 for switching from train to flight. Panel B of Table A.5 shows that there are unconditional differences across experimental groups in consumption choices, beliefs, social acceptance ratings, and moral costs.

[4] The pre-analysis plan defined a larger set of controls, including personal monthly net income, voting intentions, views about climate change, and economic preferences such as risk preferences, time preferences, and locus of control. Because these variables were elicited post-treatment, we exclude them from the analysis. Results are robust to including these variables and are available upon request.

[5] Detailed definitions of all outcome variables and controls are provided in Table A.4 in the Appendix.

Behavioral Outcomes. Panel A of Figure 1.1 displays the mean value of each consumption choice across experimental groups. T-tests indicate that the *Info* treatment leads to a marginally significant increase in choosing the normal shirt (column (1) in Table A.5). Both *RE* and *WE* increase selfish consumption relative to the *Control* group, although the *RE* treatment has no effect on apples from New Zealand (columns (2) and (3)). The effect of *WE* is generally stronger than that of *Info* and exceeds the effect of *RE* for apples from New Zealand and the flight decision (columns (5) and (6)).

Panel B of Figure 1.1 shows the estimated treatment effects on choosing the selfish option across the three product categories. The estimates control for all covariates specified in Section 1.2.4, following the specification in Equation (1.2). The regression results broadly confirm the previous patterns. *Info* has a marginally significant positive effect on normal shirts (+4.6 percentage points, $p < 0.10$), suggesting that merely providing objective information has a limited effect on selfish consumption. In contrast, individuals in the *RE* treatment are more likely to choose the normal shirt (+7.2 percentage points, $p < 0.05$) and the flight (+6.7 percentage points, $p < 0.05$) compared to the *Control* condition, while the effect on choosing apples from New Zealand is insignificant. The *WE* treatment affects all three product categories and the estimated effects are generally larger. Individuals are significantly more likely to choose the normal shirt (+12.2 percentage points, $p < 0.01$), apples from New Zealand (+8.2 percentage points, $p < 0.01$), and the flight (+12.4 percentage points, $p < 0.01$).

The corresponding regression results, with and without controls, are presented in the Appendix (see Table A.6). Wald tests comparing the coefficients for *RE* and *WE* confirm that the effects of *WE* are significantly stronger ($p < 0.10$ for the normal shirt and $p < 0.05$ for the flight and apples from New Zealand). Moreover, the effects of *WE* are significantly larger than those of *Info* across all product categories ($p < 0.01$), whereas this is not the case for *RE* relative to *Info*. Table A.6 further demonstrates that these findings are robust to multiple hypothesis testing, using the Romano–Wolf method. Results remain robust when controlling for comprehension errors, passing the second attention check, and survey completion time. Overall, these results support H1, indicating that both excuse narratives increase selfish consumption, with the *WE* having the stronger effect.

Mechanisms. Panel A of Figure 1.2 displays participants' mean beliefs about the prevalence of selfish consumption choices in the population, capturing descriptive norms. T-tests indicate that the *Info* treatment increases beliefs regarding the normal shirt (column (1) in Table A.5), while the *RE* treatment increases beliefs for the flight decision (column (2)). The *WE* treatment significantly increases beliefs across all product categories (column (3)), with effects that are larger than those of *Info* and *RE* (columns (5) and (6)).

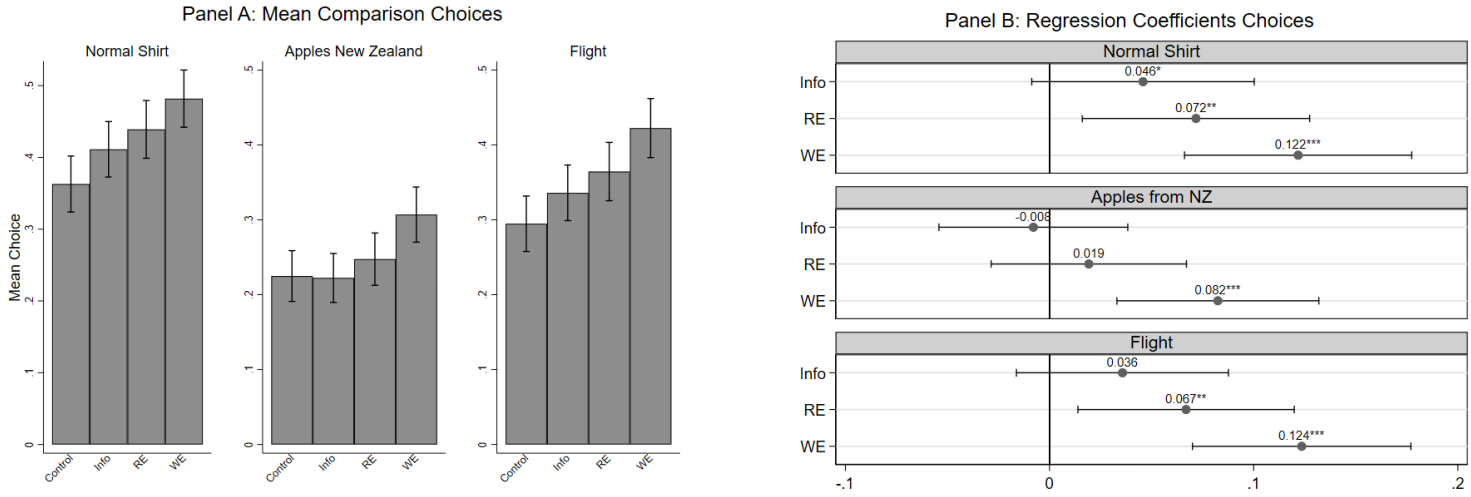

Figure 1.1: Raw Mean Differences and Regression Coefficients for Choices

Notes: Panel A displays raw means of consumption choice dummies for the *Control*, *Info*, *RE*, and *WE* groups, with 95% confidence intervals shown as vertical lines. Panel B presents estimates of the effect of *Info*, *RE*, and *WE* treatments on the consumption choice dummies. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors are robust to heteroskedasticity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Panel B of Figure 1.2 presents the estimated treatment effects on participants' beliefs about others' consumption choices, controlling for all covariates. The *Info* treatment has a small and marginally significant positive effect on beliefs regarding normal shirt choices (+2.2 percentage points, $p < 0.10$), but no significant impact on the other product categories. This suggests that mere information provision does not substantially alter descriptive norms. Participants in the *RE* treatment believe that more individuals choose the flight (+3.6 percentage points, $p < 0.05$), while the effect on the remaining choices is insignificant. Consequently, the *RE* treatment only has a limited effect on the perceived prevalence of selfish behavior. Participants in *WE* report higher perceived shares of individuals choosing the normal shirt (+4.4 percentage points, $p < 0.01$), apples from New Zealand (+4.0 percentage points, $p < 0.01$), and flight (+5.8 percentage points, $p < 0.01$). Contrary to H2, the *WE* treatment does not reduce beliefs about selfish consumption; instead, it increases perceived prevalence and thereby appears to normalize such behavior. In fact, beliefs tend to be higher under *WE* than under *RE*. This pattern is consistent with motivated belief distortion: rather than invoking the logic that waste occurs only if others consume less, the *WE* treatment may lead participants to reinterpret the prevalence of selfish behavior in a self-serving direction. Because selfish consumption is highest in the *WE* condition, participants may face a greater need to justify their choices by adjusting

beliefs.

Panel A of Table A.8 in the Appendix reports the corresponding regression results with and without controls. Moreover, Wald tests indicate that the *WE* treatment effect is marginally larger than that of the *RE* treatment for each of the three consumption choices (Wald tests, $p < 0.10$). The *WE* effect also exceeds that of *Info* across all three consumption scenarios (Wald test, $p < 0.10$ for the shirt and apples from New Zealand; $p < 0.01$ for the flight). In contrast, there are no significant differences between *Info* and *RE*. All results remain robust to multiple hypothesis testing, using the Romano–Wolf procedure. Results for *WE* remain robust when controlling for comprehension errors, passing the second attention check, and survey completion time (see Panel A of Table A.9). Overall, these findings provide only partial support for H2. The *RE* treatment increases beliefs about the prevalence of selfish behavior in line with expectations only regarding the flight. The *WE* treatment does not reduce beliefs as hypothesized; instead, it increases them even more strongly than the *RE*. This pattern suggests that the waste narrative normalizes selfish behavior rather than portraying it as uncommon.

Panel C of Figure 1.2 presents the mean values of social acceptance ratings (injunctive norms) for all three consumption choices across treatments. Corresponding t-tests in Panel B of Table A.5 indicate that, relative to the *Control* group, the *Info* treatment increases the social acceptance of selfish choices across all consumption scenarios (column (1)). The *RE* treatment shows similar increases in social acceptance of selfish options and additionally reduces the acceptance of more sustainable apples from Germany (column (2)). The *WE* treatment also raises the social acceptance of selfish choices while decreasing that of sustainable ones (column (3)). *WE* also exhibits lower social acceptance ratings of choosing the train compared to *Info*.

Panel D of Figure 1.2 confirms that the excuse narratives influence participants' perceptions of social acceptance. Estimates including controls generally exhibit a similar pattern. For the *Info* treatment, we observe significant increases in the social acceptance of normal shirts (+0.15, $p < 0.05$), apples from New Zealand (+0.13, $p < 0.10$), and flights (+0.16, $p < 0.05$). This shows that providing basic information leads participants to view these choices as slightly more socially acceptable. However, for all sustainable choices in *Info*, social acceptance is not significantly different from the *Control* group.

Both *RE* and *WE* increase the perceived social acceptance of normal shirts (+0.24 and +0.23, respectively; $p < 0.01$) and apples from New Zealand (+0.25 and +0.18; $p < 0.01$ and $p < 0.05$). *RE* increases the social acceptance of the flight (+0.15, $p < 0.10$) and the social acceptance of apples from Germany decreases under *RE* (-0.14, $p < 0.10$). In contrast to *RE* and *Info*, we observe negative effects for all sustainable options under *WE*. The social acceptance of organic shirts (-0.16, $p < 0.05$), apples from Germany (-0.15, $p < 0.05$), and train travel (-0.16, $p < 0.05$) decreases. This suggests that the *WE* treatment makes certain selfish behaviors appear more

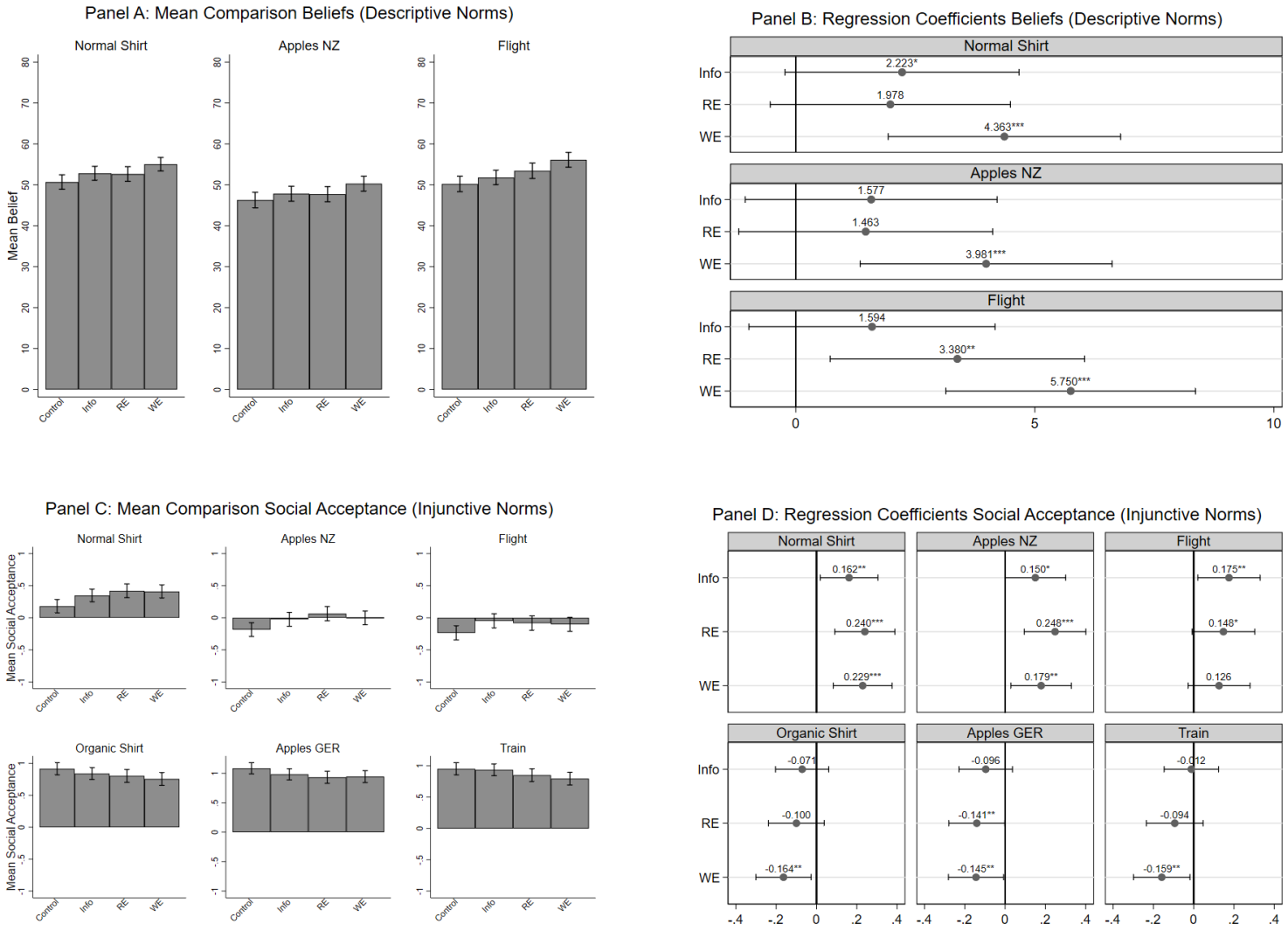

Figure 1.2: Raw Mean Differences and Regression Coefficients for Beliefs (Descriptive Norms) and Social Acceptance (Injunctive Norms)

Notes: Panel A and Panel C display raw means of beliefs about other participants' consumption choices (0–100 percent) and social acceptance ratings (-2 "Socially very inappropriate", -1 "Socially somewhat inappropriate", 1 "Socially somewhat appropriate", and 2 "Socially very appropriate") for the *Control*, *Info*, *RE*, and *WE* groups, respectively. Vertical lines indicate 95% confidence intervals. Panel B and D present estimates of the effect of *Info*, *RE*, and *WE* treatments on beliefs and social acceptance ratings, respectively. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors are robust to heteroskedasticity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

socially acceptable while also reducing the perceived social acceptance of the more sustainable option.

Panels B and C of Table A.8 in the Appendix show regression results with and without controls and replicate the results. Wald tests indicate no significant differences in social acceptance ratings between the *RE* and *WE* treatments. Moreover, differences between the excuse treatments and the *Info* condition are generally small, with one exception: the social acceptance of train travel is significantly smaller under *WE* compared to *Info* (Wald test, $p < 0.05$). The results are robust to multiple hypothesis testing, as confirmed by the Romano-Wolf procedure and when controlling for comprehension errors, passing the second attention check, and survey completion time (see Panels B and C in Table A.9).

Overall, these findings provide partial support for H3: both excuse treatments influence perceptions of social acceptance, with *WE* showing the largest quantitative effects, although these are not significantly stronger than those of *RE*. The direction of effects aligns with motivated reasoning — both excuses make selfish options appear more socially acceptable. Notably, the *WE* treatment goes further by also reducing the perceived social acceptance of the more sustainable options, a pattern not consistently observed under *RE*.

Panel A of Figure 1.3 displays the mean switching points in the MPLs, which measure moral costs. Lower switching points indicate lower moral costs, as participants are willing to switch to less environmentally friendly options at smaller price differences. T-tests reported in Table A.5 indicate that *Info* does not significantly affect moral costs compared to the *Control* group (see column (1)). The *RE* treatment reduces moral costs only for the flight scenario, whereas the *WE* treatment lowers moral costs across all three consumption choices (columns (2) and (3)). Under *WE*, moral costs are lower than under *Info* for apples and the flight scenario.

Panel B of Figure 1.3 presents the results for switching behavior, controlling for all covariates. The effects of *Info* are negative but statistically insignificant. Hence, providing objective information does not meaningfully affect moral costs. The *RE* treatment has limited impact and reduces moral costs only in the flight scenario (-0.33, $p < 0.05$). In contrast, the *WE* treatment reduces moral costs across all three settings. Participants switch from organic to normal shirts (-0.31, $p < 0.05$), from German to New Zealand apples (-0.41, $p < 0.01$), and from train to flight (-0.48, $p < 0.01$) at lower price differences. These findings provide evidence that the *WE* treatment—by framing selfish behavior as preventing resource waste—reduces the moral costs of the more CO_2-intensive options.

Table A.10 in the Appendix shows regression results with and without controls replicating previous findings. Wald tests indicate that the reduction in moral costs under *WE* is significantly larger than under *Info* for apples and the flight scenario. Hence, with respect to moral costs, the *WE* effect goes beyond what information provision alone can explain. Table A.10

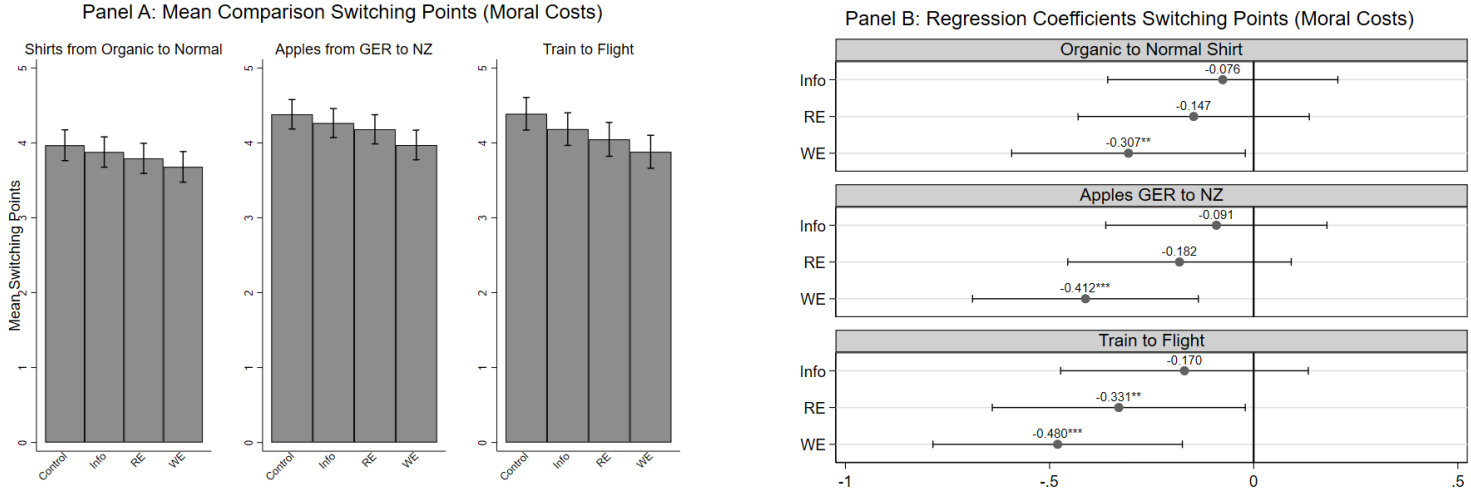

Figure 1.3: Raw Mean Differences and Regression Coefficients for Switching Points (Moral Costs)

Notes: Panel A displays raw means of switching points in the MPLs (range 0–7) from the more sustainable to the less sustainable option as the price difference increases, for the *Control*, *Info*, *RE*, and *WE* groups. Vertical lines indicate 95% confidence intervals. Higher switching points indicate higher moral costs. Panel B presents estimates of the effect of *Info*, *RE*, and *WE* treatments on switching points. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors are robust to heteroskedasticity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

also shows that results are robust to multiple hypothesis testing, using the Romano-Wolf procedure. The varying numbers of observations across consumption choices arise because we include only individuals who switch from the environmentally friendly to the selfish option (e.g., train to flight), but not in the opposite direction. Results remain robust when controlling for comprehension errors, passing the second attention check, and survey completion time (see Table A.11).

Overall, the results partially support H4: the *WE* treatment lowers moral costs, whereas the *RE* treatment only matters in the flight scenario. Taken together, the findings on social norms and moral costs are broadly consistent with the conceptual framework. The *WE* treatment generates larger consumption effects than *RE* and more strongly shifts beliefs and moral costs. By contrast, effects on injunctive norms are less systematic, although *WE* consistently reduces the perceived social acceptance of sustainable options.

Sensitivity. To investigate the relationship between norms, moral costs, and consumption choices, Table 1.1 reports regressions with consumption choices as outcomes. All specifications include the same set of covariates as before. This analysis examines whether our measures of norms and moral costs are meaningful correlates of consumption behavior. Panel A

reports results for the shirt scenario, Panel B for the apple scenario, and Panel C for the flight scenario. Column (1) replicates treatment effects from Figure 1.1. In column (2), we add beliefs about others' selfish choices, which are positively correlated with choosing the selfish option, suggesting that descriptive norms influence behavior. Column (3) incorporates social acceptance measures: higher acceptance of the selfish choice predicts greater likelihood of choosing it, while higher acceptance of the sustainable alternative reduces selfish choices, indicating that injunctive norms also shape decisions. These patterns hold consistently across all product categories.

For comparability, column (4) replicates the results from column (3) using a sample restricted to individuals who exhibit consistent switching behavior. Column (5) adds moral costs, showing that higher moral costs are negatively associated with choosing the selfish option. Including beliefs reduces the *Info* treatment effect for the normal shirt and renders it insignificant. For the normal shirt and flight scenarios, adding norms and moral costs reduces the magnitude of the *RE* and *WE* treatment effects. In these specifications, the effect of *RE* becomes insignificant. However, in the subsample of participants with consistent choices in the MPLs, the *RE* effect becomes significant again for the shirt choice (see columns (4) and (5)). A similar pattern emerges for the apples scenario, where the *WE* effect also becomes insignificant in column (5) once moral costs are included. These patterns suggest a meaningful association between moral costs, perceived norms, and consumption decisions. All correlations are statistically significant at the 1% level.

In summary, the mechanism and sensitivity results suggest that excuses shift social norms in ways that make selfish choices appear less norm-violating, while also reducing the moral costs that might otherwise deter such behavior. Overall, the direction of this distortion tends to make selfish choices more appealing. Moreover, the measures affected by the excuse treatments—particularly beliefs, social acceptance, and moral costs—are meaningful predictors of consumption behavior.

Heterogeneity. Overall, linear interaction models do not reveal systematic patterns of heterogeneous treatment effects. Some suggestive variation emerges with respect to descriptive and injunctive norms and moral costs, but these patterns are not consistent across outcomes. To allow for potential non-linear heterogeneity, we additionally estimate a causal random forest, a machine-learning method that flexibly detects heterogeneous treatment effects (Wager and Athey, 2018). This approach reveals more consistent patterns, with beliefs about others' consumption, age, and switching points in the MPLs (moral costs) emerging as the most important predictors of treatment heterogeneity. These patterns should be interpreted as exploratory rather than causal. Section A.2 in the Appendix provides further details, including a descrip-

Table 1.1: Choices, Norms, and Moral Costs

	Panel A: Choice Normal Shirt				
	(1)	(2)	(3)	(4)	(5)
Info	0.046*	0.039	0.025	0.031	0.035
	(0.028)	(0.028)	(0.027)	(0.028)	(0.025)
RE	0.072**	0.065**	0.045	0.059**	0.058**
	(0.028)	(0.028)	(0.027)	(0.028)	(0.026)
WE	0.122***	0.107***	0.085***	0.103***	0.092***
	(0.028)	(0.028)	(0.028)	(0.029)	(0.026)
Belief: Normal Shirt		0.003***	0.003***	0.003***	0.002***
		(0.000)	(0.000)	(0.000)	(0.000)
Social Acceptance: Normal Shirt			0.063***	0.067***	0.038***
			(0.007)	(0.008)	(0.007)
Social Acceptance: Organic Shirt			-0.063***	-0.067***	-0.038***
			(0.008)	(0.008)	(0.008)
Switch: Shirts from Organic to Normal					-0.084***
					(0.004)
Control Mean	0.363	0.363	0.363	0.363	0.363
Obs.	2,408	2,408	2,408	2,215	2,215
Adj. R-Sq	0.020	0.040	0.084	0.096	0.249
Controls	X	X	X	X	X
	Panel B: Choice Apples NZ				
	(1)	(2)	(3)	(4)	(5)
Info	-0.008	-0.011	-0.023	-0.020	-0.019
	(0.024)	(0.024)	(0.023)	(0.023)	(0.021)
RE	0.019	0.016	-0.003	-0.003	-0.007
	(0.024)	(0.024)	(0.024)	(0.024)	(0.023)
WE	0.082***	0.074***	0.058**	0.055**	0.036
	(0.025)	(0.025)	(0.024)	(0.025)	(0.023)
Belief: Apples NZ		0.002***	0.002***	0.002***	0.001***
		(0.000)	(0.000)	(0.000)	(0.000)
Social Acceptance: Apples NZ			0.047***	0.049***	0.024***
			(0.006)	(0.006)	(0.006)
Social Acceptance: Apples GER			-0.059***	-0.055***	-0.039***
			(0.007)	(0.008)	(0.007)
Switch: Apples from GER to NZ					-0.069***
					(0.004)
Control Mean	0.225	0.225	0.225	0.225	0.225
Obs.	2,408	2,408	2,408	2,269	2,269
Adj. R-Sq	0.032	0.046	0.092	0.090	0.224
Controls	X	X	X	X	X
	Panel C: Choice Flight				
	(1)	(2)	(3)	(4)	(5)
Info	0.036	0.030	0.017	0.015	0.009
	(0.026)	(0.026)	(0.025)	(0.026)	(0.022)
RE	0.067**	0.055**	0.040	0.040	0.023
	(0.027)	(0.027)	(0.026)	(0.027)	(0.022)
WE	0.124***	0.103***	0.088***	0.081***	0.054**
	(0.027)	(0.027)	(0.026)	(0.027)	(0.023)
Belief: Flight		0.004***	0.003***	0.003***	0.002***
		(0.000)	(0.000)	(0.000)	(0.000)
Social Acceptance: Flight			0.080***	0.078***	0.039***
			(0.007)	(0.007)	(0.006)
Social Acceptance: Train			-0.058***	-0.055***	-0.031***
			(0.007)	(0.008)	(0.007)
Switch: Train to Flight					-0.095***
					(0.003)
Control Mean	0.295	0.295	0.295	0.295	0.295
Obs.	2,408	2,408	2,408	2,228	2,228
Adj. R-Sq	0.031	0.060	0.133	0.135	0.384
Controls	X	X	X	X	X

Notes: This table presents estimates of the effect of *Info*, *RE*, and *WE* treatments on consumption choice dummies. Panel A displays results for the shirt choice, Panel B for the apples choice, and Panel C for the flight choice. Column (1) presents treatment effects only. Column (2) adds beliefs about other participants' consumption choices (0–100 percent). Column (3) further includes social acceptance ratings (-2 "Socially very inappropriate", -1 "Socially somewhat inappropriate", 1 "Socially somewhat appropriate", and 2 "Socially very appropriate") for the respective choice. Column (4) replicates the specification from column (3) for the subsample of participants with consistent choices in the MPLs. Column (5) additionally includes the switching points in the MPLs (range 0–7) from the more sustainable to the less sustainable option as the price difference increases. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors are robust to heteroskedasticity.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

tion of the causal random forest approach.

1.3 Follow-up Experiment

1.3.1 Experimental Design

The first-wave results show that participants consistently overestimate the share of individuals choosing the more environmentally harmful option. On average, beliefs exceed actual choices by approximately 11 percentage points for the normal shirt, 23 percentage points for apples from New Zealand, and 18 percentage points for the flight option. The magnitude of this overestimation varies across treatments (see Tables A.2 and A.5). To assess the effect of correcting these misperceptions, we conducted a follow-up experiment in the second survey wave. As in the first wave, participants face the three consumption choices, this time without incentives, and the order of choices is randomized to control for order effects. Beliefs about others' behavior vary systematically by treatment (see Table A.5), so participants are randomly assigned within their original treatment group to one of two conditions. In the norm correction (*NC*) condition, participants receive accurate information on the actual shares of first-wave consumption choices. The values reported to participants depend on treatment status in the first survey wave. In the *Control (NC)* condition, participants receive no additional information. No excuse statements from the first wave are presented; all participants see the control version of the consumption choices. This design allows us to examine both the persistence of initial treatment effects and the impact of correcting misperceptions.

After the norm correction and consumption decisions, participants indicate whether they want to sign a petition advocating stronger climate action. They are informed that the German government will be notified of the share of survey participants supporting the petition. We also elicit self-reported intentions to engage in climate-friendly behaviors, including reducing flights, car rides, home heating, and meat consumption.[6] Given the proximity to the German federal elections, participants report the importance of various policy issues for their voting decisions (environment, economy, public security, migration, pensions, and social security) and indicate their voting intentions. In addition, we measure positive and negative affect using the Positive and Negative Affect Schedule (PANAS), a widely used psychological scale (Watson et al., 1988). These measures allow us to examine whether the norm correction intervention or first-wave treatment status influences emotional responses, alongside consumption behavior and attitudes.

[6] Questions are adapted from Dechezleprêtre et al. (2025).

1.3.2 Hypotheses

The follow-up experiment addresses three research questions: (i) Does the norm correction affect behavior in the three consumption choices?; (ii) Does the norm correction influence willingness to fight climate change, as measured by signing a petition and adopting pro-environmental behaviors?; (iii) Does the norm correction alter the perceived importance of climate policy in voting decisions? This leads us to three hypotheses:

- **H5:** The share of individuals choosing the high-emission option will be lower in the *NC* condition compared to the *Control (NC)* condition.
- **H6:** The share of individuals choosing to sign the petition and adopt pro-environmental behaviors will be higher in the *NC* condition compared to the *Control (NC)* condition.
- **H7:** The share of individuals indicating that climate policy is an important determinant of their voting decision will be higher in the *NC* condition compared to the *Control (NC)* condition.

Providing accurate information through norm corrections may reduce the likelihood of choosing the high-emission option, as participants on average overestimated the actual shares (H5). Moreover, learning that others behave more environmentally friendly than expected may prompt participants to align their own behavior by signing the petition, adopting pro-environmental actions, and assigning greater importance to climate policy in their voting decisions (H6 and H7). However, a potential backfire effect remains possible: if participants perceive that others are already acting responsibly, they may feel less personal obligation to contribute to the common good.

1.3.3 Empirical Strategy

We estimate two specifications based on Equation 1.2. The first adds a dummy indicating whether an individual is assigned to the *NC* treatment, uses the same control variables as in the first experiment, and controls for treatment status in the first wave. This specification tests the overall effect of *NC* on outcomes and the persistence of first-wave treatment effects. The second specification exploits variation in the observed share of selfish choices shown in *NC*, which depends on first-wave treatment status. It fully interacts *NC* with treatment status from the first wave and is our preferred specification, as it accounts for heterogeneity in beliefs and actual choices across treatments.

The outcomes are defined as follows: Dummies indicate whether a participant chooses the flight, normal shirt, or apples from New Zealand in the second survey wave. “Yes to Petition”

is a dummy equal to one if an individual chooses to sign the petition in favor of stronger climate protection. "Climate Change (Voting)" measures the importance assigned to climate policy in voting decisions on a five-point Likert scale ranging from 1 ("Not important at all") to 5 ("Very important"). "Climate Change (Problem)" captures how problematic respondents perceive climate change, measured on a five-point Likert scale from 1 ("No problem at all") to 5 ("Big problem"). The willingness to change behavior is measured by five dummy variables equal to one if an individual indicates to be at least "moderately" willing to adopt the respective pro-environmental behaviors: flying less, driving less, eating less meat, reducing home heating, or owning an electric car. Finally, we define "No AfD/Grüne anymore" as dummies equal to one if an individual reported intentions to vote for the respective party in wave 1 but switched to another party in wave 2. We include these two parties because the AfD openly calls for reducing measures to combat climate change, whereas the Green Party advocates stronger climate policy.

1.3.4 Results

Behavior and Political Views. In the second survey wave, covariate imbalance across treatments in the main experiment (*Control, Info, RE, WE*) is similar to the first wave (see Table A.14). For the follow-up experiment (*Control (NC), NC*), balance tests in the pooled sample indicate some differences between *Control (NC)* and *NC*: two out of six tests are significant at the 10% level (Table A.15). Randomization in the second wave was conducted within strata defined by participants' treatment status in the first wave. Testing balance within these strata shows that only one out of 24 tests is significant at the 10% level, and none are significant at the 5% or 1% level.

The effect of the *WE* treatment remains persistent in the follow-up experiment. As shown in Panel A of Table A.16 in the Appendix, it continues to increase the likelihood of choosing apples from New Zealand and the flight ($p < 0.05$ and $p < 0.01$). The *Info* treatment also raises the likelihood of choosing the flight ($p < 0.1$), although the effect is smaller than for *WE*. Panels B and C report results for political views and behavioral intentions. Regarding political views, being assigned to the *Info* or *RE* treatment reduces the importance respondents assign to climate change when making voting decisions (both $p < 0.1$). Moreover, all three treatments reduce the perceived severity of climate change as a problem (*Info* and *RE*: $p < 0.05$ and *WE*: $p < 0.01$). With respect to behavioral intentions, willingness to fly less declines under *Info* ($p < 0.1$) and *WE* ($p < 0.01$), consistent with the observed flight choice. In addition, *Info* and *RE* reduce the willingness to heat less at home and owning an electric car (all $p < 0.05$). Overall, treatment assignment in wave 1 appears to affect political views and behavioral intentions in

wave 2. In contrast, the *NC* treatment is never statistically significant, suggesting the absence of unconditional effects with respect to H5, H6, and H7.

Taking the treatment structure into account shows that the norm correction has almost no effects on consumption choices and political views. All reported significance levels are corrected for multiple hypothesis testing using the Romano–Wolf method. Panel A of Table 1.2 indicates that, for the flight decision, the interaction of *NC* with the *Info* treatment partially offsets the original *Info* effect ($p < 0.1$), whereas no such pattern appears for *WE* (column (3)). Thus, H5—that norm corrections reduce selfish behavior—receives only weak support and appears to depend on treatment exposure in wave 1. Regarding political views (Panel B), neither the *NC* treatment nor its interactions with the wave 1 treatments are statistically significant. Consistent with the baseline specification, the *Info* treatment reduces both the importance of climate change in voting decisions ($p < 0.05$) and perceptions of climate change as a problem ($p < 0.01$). The *RE* and *WE* treatments also reduce perceptions of climate change as a problem ($p < 0.01$ and $p < 0.1$). Overall, H6 for the petition outcome and H7 for political views receive little support.

In contrast, the norm correction affects intentions to adopt pro-environmental behaviors more systematically (see Table 1.3; all p-values refer to the Romano–Wolf correction). Willingness to fly less declines under *Info* ($p < 0.1$) and *WE* (–15 percentage points, $p < 0.01$), although the latter effect is partially offset by the interaction with *NC* (+13 percentage points, $p < 0.1$).[7] The *RE* treatment reduces the willingness to drive car less (–11.7 percentage points, $p < 0.01$), but this effect is counteracted by the interaction with *NC* (+15.9 percentage points, $p < 0.05$). Similarly, willingness to heat less at home declines under all first wave treatments, but these effects are largely offset by the interactions with *NC*. The interaction with *Info* is significant at the 5% level, while those with *RE* and *WE* are significant at the 10% level. Finally, *Info* and *RE* reduce the likelihood of owning an electric car ($p < 0.1$), consistent with the baseline results, but these effects are not offset by the norm correction.

Overall, these results suggest that the norm correction can mitigate reductions in pro-environmental intentions induced by the excuse treatments. H6 therefore receives partial support, as the effects of *NC* again depend on prior treatment exposure in wave 1. Across all outcome domains—consumption choices, political views, and intentions to change behavior—a common pattern emerges. First, the excuse treatments, including *Info,* exhibit partially persistent effects on consumption and spillover effects on political attitudes and behavioral intentions. Second, these effects are offset by the norm correction primarily for behavioral intentions, while little evi-

[7] We interpret "offsetting" in the interaction model as a reduction in the implied net effect of the wave 1 treatment, given by the sum of the main effect and the interaction term with *NC*.

Table 1.2: Results Norm Correction I (Interaction)

	Panel A: Choices		
	(1) Normal Shirt	(2) Apples NZ	(3) Flight
NC	-0.029	0.025	0.052
	(0.045)	(0.038)	(0.041)
RW p-value	[0.922]	[0.972]	[0.687]
Info	0.010	0.038	0.121***
	(0.045)	(0.039)	(0.042)
RW p-value	[0.994]	[0.840]	[0.002]
RE	-0.013	0.037	0.045
	(0.046)	(0.039)	(0.041)
RW p-value	[0.998]	[0.912]	[0.916]
WE	0.041	0.097**	0.175***
	(0.046)	(0.040)	(0.043)
RW p-value	[0.918]	[0.038]	[0.002]
NC × Info	-0.031	-0.048	-0.129*
	(0.063)	(0.055)	(0.059)
RW p-value	[0.988]	[0.918]	[0.072]
NC × RE	0.032	-0.011	-0.006
	(0.064)	(0.056)	(0.059)
RW p-value	[0.982]	[0.998]	[0.998]
NC × WE	-0.019	-0.026	-0.110
	(0.064)	(0.057)	(0.061)
RW p-value	[0.994]	[0.994]	[0.299]
Control Mean	0.478	0.277	0.364
Obs.	1,934	1,934	1,934
Adj. R-Sq	0.016	0.033	0.044
Controls	X	X	X

	Panel B: Political Views				
	(1) Yes to Petition	(2) Climate Change (Voting)	(3) Climate Change (Problem)	(4) No AfD anymore	(5) No Grüne anymore
NC	-0.028	-0.134	-0.097	-0.021	-0.010
	(0.028)	(0.138)	(0.110)	(0.014)	(0.015)
RW p-value	[0.938]	[0.948]	[0.966]	[0.425]	[0.988]
Info	-0.019	-0.335**	-0.298***	0.006	0.010
	(0.027)	(0.132)	(0.109)	(0.017)	(0.017)
RW p-value	[0.988]	[0.012]	[0.004]	[1.000]	[0.994]
RE	-0.020	-0.277	-0.294***	-0.012	-0.002
	(0.028)	(0.140)	(0.113)	(0.015)	(0.015)
RW p-value	[0.988]	[0.134]	[0.008]	[0.986]	[1.000]
WE	-0.014	-0.200	-0.238*	-0.009	0.004
	(0.027)	(0.137)	(0.113)	(0.015)	(0.016)
RW p-value	[0.998]	[0.491]	[0.082]	[0.994]	[1.000]
NC × Info	0.068	0.308	0.275	0.001	0.009
	(0.038)	(0.191)	(0.156)	(0.020)	(0.023)
RW p-value	[0.188]	[0.337]	[0.198]	[1.000]	[1.000]
NC × RE	0.029	0.192	0.178	0.037	0.001
	(0.041)	(0.199)	(0.160)	(0.021)	(0.020)
RW p-value	[0.988]	[0.948]	[0.882]	[0.188]	[1.000]
NC × WE	0.029	0.083	0.055	0.021	0.008
	(0.040)	(0.198)	(0.161)	(0.020)	(0.022)
RW p-value	[0.988]	[1.000]	[1.000]	[0.914]	[1.000]
Control Mean	0.895	4.201	3.465	0.030	0.032
Obs.	1,934	1,934	1,927	1,934	1,934
Adj. R-Sq	0.002	0.039	0.028	0.001	0.007
Controls	X	X	X	X	X

Notes: The table shows estimates of the effect of the *NC* treatment fully interacted with the three treatment groups of the excuse experiment. Panel A presents results for the three consumption choice dummies, Panel B reports political views, including dummies for signing a petition to fight climate change and changes in voting intention (AfD and Grüne). Climate Change (Voting) measures the importance assigned to climate policy in voting decisions on a five-point Likert scale (1–5). Climate Change (Problem) measures perceived severity of climate change on a five-point Likert scale (1–5). Controls include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. p-values adjusted for multiple hypothesis testing using the Romano–Wolf method are reported in brackets. Significance levels refer to corrected p-values. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

dence of such offsetting effects appears for consumption choices or political views.[8]

Table 1.3: Results Norm Correction II (Interaction)

	Willingness to Change Behavior				
	(1) Fly Less	(2) Drive Less Car	(3) Eat Less Meat	(4) Less Heating at Home	(5) Own an Electric Car
NC	-0.076	-0.067	0.022	-0.117**	-0.024
	(0.044)	(0.044)	(0.044)	(0.044)	(0.043)
RW p-value	[0.200]	[0.325]	[0.960]	[0.012]	[0.960]
Info	-0.090*	-0.048	-0.077	-0.133***	-0.082*
	(0.043)	(0.044)	(0.042)	(0.043)	(0.042)
RW p-value	[0.078]	[0.713]	[0.148]	[0.002]	[0.100]
RE	-0.021	-0.117***	0.019	-0.124***	-0.084*
	(0.044)	(0.043)	(0.044)	(0.044)	(0.043)
RW p-value	[0.960]	[0.010]	[0.960]	[0.008]	[0.100]
WE	-0.150***	-0.037	-0.023	-0.098*	-0.069
	(0.044)	(0.044)	(0.044)	(0.044)	(0.042)
RW p-value	[0.002]	[0.892]	[0.960]	[0.054]	[0.242]
NC × Info	0.076	0.060	0.068	0.138**	0.048
	(0.062)	(0.061)	(0.060)	(0.060)	(0.059)
RW p-value	[0.661]	[0.814]	[0.713]	[0.044]	[0.892]
NC × RE	-0.004	0.159**	-0.098	0.122*	0.037
	(0.062)	(0.062)	(0.062)	(0.061)	(0.060)
RW p-value	[0.960]	[0.014]	[0.253]	[0.100]	[0.956]
NC × WE	0.130*	0.036	-0.030	0.133*	0.071
	(0.063)	(0.061)	(0.062)	(0.062)	(0.060)
RW p-value	[0.078]	[0.960]	[0.960]	[0.066]	[0.679]
Control Mean	0.553	0.344	0.364	0.340	0.302
Obs.	1,931	1,931	1,930	1,932	1,931
Adj. R-Sq	0.053	0.013	0.040	0.013	0.020
Controls	X	X	X	X	X

Notes: The table shows estimates of the effect of the *NC* treatment fully interacted with the three treatment groups of the excuse experiment. It presents results for behavioral change dummies. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors in parentheses are robust to heteroskedasticity. p-values adjusted for multiple hypothesis testing using the Romano–Wolf method are reported in brackets. Significance levels refer to corrected p-values. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Psychological Explanation. As an exploratory analysis, we examine whether exposure to excuse statements has a lasting effect on participants' emotional states. Panel A of Figure 1.4 displays mean PANAS scores across treatment groups. While we find no evidence that excuses influence negative affect, the *WE* (+1.23, $p < 0.05$) treatment significantly increases positive affect. We also test whether these emotional effects are related to the respective consumption

[8] As specified in the pre-analysis plan, we also estimate a third specification in which *NC* is interacted with a dummy indicating overestimation of the actual share of selfish choices. As shown in Table A.17, this interaction is never significant. A potential explanation is limited variation in the overestimation measure, as most participants overestimate the actual share.

choices by controlling for them. The effect of *WE* on positive affect remains robust (results not shown).

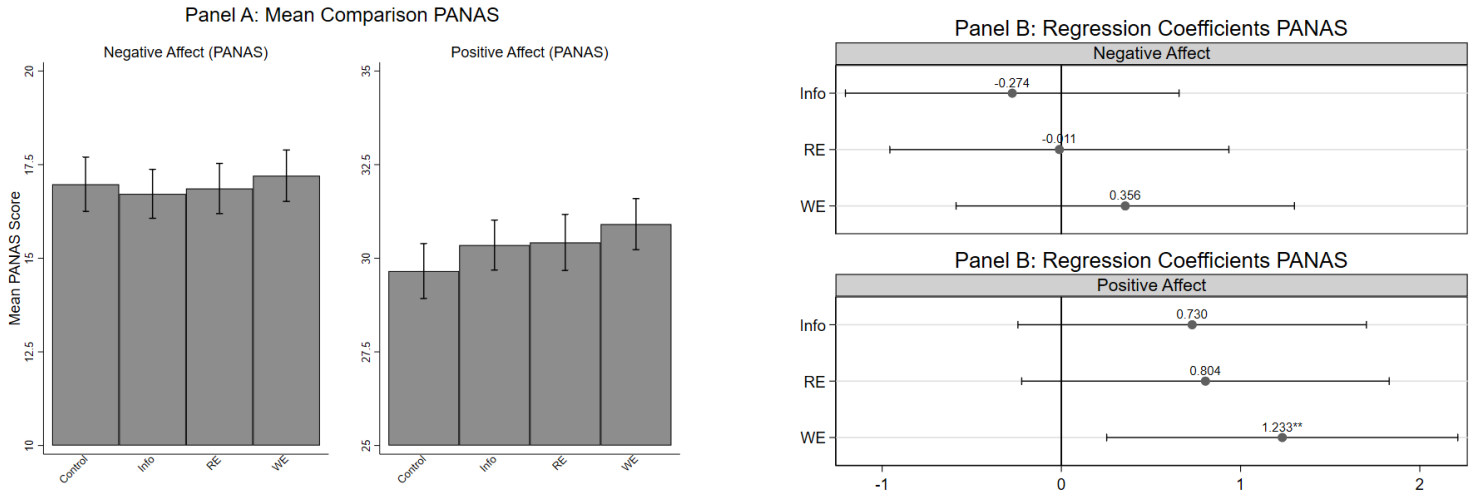

Figure 1.4: Raw Differences and Regression Coefficients for PANAS

Notes: Panel A displays raw means of the PANAS measures of positive and negative affect, each ranging from 10 to 50 for the *Control*, *Info*, *RE*, and *WE* groups in the second survey wave, with 95% confidence intervals shown as vertical lines. Panel B presents the respective treatment effects on PANAS. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors are robust to heteroskedasticity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

1.4 Discussion and Conclusion

Our study examines how justification mechanisms, specifically the RE and WE, influence consumer decision-making in morally and environmentally relevant contexts. Using a survey experiment across three product categories—clothing, food, and transportation—we find that both excuses significantly increase selfish consumption choices, with the WE showing the strongest and most persistent effect. This pattern is consistent with the motivation to maintain a positive self-image: by perceiving norms as less binding and actions as less morally costly, individuals can justify environmentally harmful choices that benefit themselves.

The stronger effect of the *WE* treatment compared to the *RE* can be rationalized through the proposed mechanisms. The *WE* most strongly affects beliefs and moral costs, whereas the effects of *RE* and *WE* on social acceptance ratings are more similar. In line with our framework, distortions of norms and moral costs reduce the disutility associated with choosing the selfish option. Consequently, stronger distortions should lead to more selfish choices, which is consis-

tent with our results. While changes in norms align with mechanisms of motivated reasoning, the reduction in moral costs extends beyond purely belief-based adjustments. This decrease in moral costs could also result from less binding norms—or conversely, weaker norms may arise as a consequence of reduced moral costs—but our design does not allow us to disentangle these channels.

Interestingly, the *WE* treatment increases beliefs about selfish consumption, contrary to our initial hypothesis. This may indicate that participants interpret the excuse not as a statement about others' actual behavior but rather as a reflection of what others might think, which then justifies their own selfish choices. Alternatively, since selfish consumption is highest in the *WE* condition, participants may experience a greater need for justification, consistent with motivated reasoning. Comparisons between the excuse treatments and *Info* further show that the *WE* extends significantly beyond a mere informational effect, while the *RE* does not. Prior research has found mixed evidence for the *RE*. Introducing the *WE* reveals that framing selfish actions as a waste of resources can have considerably stronger effects on behavior than framing them in terms of replacement by others.

Our results establish that the *WE* and *RE* can serve as justifications that increase or even reinforce unsustainable consumption patterns. The arguments provided by these excuses may appear rational at first glance but can be easily challenged or put into perspective. For instance, one could apply the same line of reasoning in favor of the more sustainable option. Whether such counter-excuses could also influence behavior or even offset the effects of the original excuses remains an open question for future research.

A more fundamental concern arises from the observation that these excuses may cause individuals to act inconsistently with their intentions. Although the reasoning behind the excuses may appear justifiable in the short run, the long-term consequences of such behavioral changes could be far more severe. Individuals who rely on these justifications may consume more harmful products than they would otherwise choose. If this pattern occurs on a larger scale, it increases demand for unsustainable goods and may ultimately affect supply, thereby providing even more scope for future excuses. Limiting the use of such justifications could therefore help reduce demand today and, in turn, lower supply in the future. Importantly, we only study how the provision of excuses affects individual behavior. Future research should examine how these narratives may spread and change aggregate consumption.

Identifying ways to limit the effectiveness of excuses and to promote sustainable behavior is an important next step. Since participants systematically overestimate selfish choices and excuses even increase these misperceptions, we test in the follow-up whether a norm-correction treatment can counteract these effects by reducing the scope for motivated reasoning. We find that the effect of the *WE* treatment persists for both the apples and flight choices; however, the

norm correction is not able to offset this negative effect. Because excuse statements and norm corrections are not provided simultaneously in our design, combining both interventions could yield stronger effects.

We further examine whether excuse narratives and norm corrections affect other domains, namely political views and pro-environmental behavioral intentions, in the second survey wave. The excuse treatments reduce the extent to which respondents perceive climate change as a problem and weaken pro-environmental intentions such as flying less or reducing home heating. The norm correction only partially offsets these effects regarding behavioral intentions. One possible interpretation is that, because excuses increase selfish consumption and beliefs about its prevalence, downplaying the relevance of climate change may serve as an additional mechanism to maintain a positive self-image. Behavioral intentions may likewise be justified by the perception that others behave worse. By removing the moral wiggle room created by these misperceptions, norm corrections partially mitigate these negative effects.

We specifically test descriptive norms, while two additional channels for potential corrections remain: injunctive norms and moral costs. Interventions targeting injunctive norms may likewise prove effective. With respect to moral costs, price-based instruments such as taxes could reduce the behavioral impact of excuses. Whereas excuses lower moral costs and weaken intrinsic motivations, financial incentives increase the extrinsic cost of selfish consumption. Such measures would not necessarily eliminate motivated reasoning or norm distortions but could limit their behavioral consequences.

Our study also faces several limitations. While key behavioral questions are incentivized, it remains unclear whether these effects translate to behavior in the field or hold in other populations or societies. Moreover, the follow-up study uses unincentivized consumption scenarios, which might influence how effectively motivated reasoning and excuses can be limited. Another open question concerns the interaction between norms and moral costs. Our findings suggest that targeting these two factors may reduce the effectiveness and negative consequences of excuses. Future research should therefore explore how price-based incentives and norm corrections jointly affect excuse-driven behavior. Moreover, it should also examine whether individuals intervene when others invoke excuses to justify selfish behavior and how such justifications are socially received—accepted, challenged, or sanctioned. Understanding these psychological mechanisms is key to designing policies that effectively limit the externalities associated with excuse-based reasoning.

Our findings have further implications for policy design and public debate. First, they suggest that the availability of excuses can causally increase socially harmful consumption in settings characterized by diffuse externalities, thereby weakening effective public-good outcomes. Second, policy effectiveness depends not only on material incentives, but also on the surround-

ing narrative environment that shapes perceived responsibility and social norms. Interventions operate within this belief structure and may therefore be amplified or undermined by prevailing justificatory frames. Third, our results point to a belief-mediated channel through which such narratives attenuate moral costs and shift both empirical and normative expectations, indicating that communication and framing can materially affect behavior even when economic incentives are held constant. Finally, policies that do not anticipate responsibility-diluting narratives may generate smaller and less persistent effects than intended, highlighting the importance of integrating incentive design with careful attention to the informational and normative context in which decisions are made.

Chapter 2

Pivotality in Consumption and Support for Regulation: Evidence from a Survey Experiment on CO_2 Pricing

2.1 Introduction

CO_2 pricing is widely regarded as an effective tool to reduce CO_2 emissions (Nordhaus, 2019). Yet, it often faces public resistance (Dechezleprêtre et al., 2025). Understanding when and why citizens support such policies is therefore crucial for designing effective and politically feasible climate policy. One potentially overlooked factor is perceived individual pivotality—whether individuals see themselves as responsible for aggregate outcomes and externalities. In most everyday settings—such as flying, eating meat, or buying fast fashion—individuals are often not fully pivotal, which may weaken the perceived impact of individual action. These contexts may invoke the so-called replacement logic (RL), which reduces perceived pivotality: the belief that one's action can be replaced, so that if someone abstains, another person will step in. While prior research shows that reduced pivotality shapes consumption behavior, its implications for policy support remain largely unexplored (see e.g., Ziegler et al., 2024).

Reduced pivotality could matter for the political feasibility of CO_2 pricing because it affects key determinants of policy support, such as personal financial impacts, perceived policy ef-

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fectiveness, and distributional consequences (Kallbekken and Sælen, 2011; Dechezleprêtre et al., 2025). It may operate through two channels: individual consumption behavior and beliefs about others (see e.g., Bartling and Özdemir, 2023; Ziegler et al., 2024). Through consumption, reduced pivotality may alter perceived personal costs; through beliefs about others, it may shape perceived policy effectiveness and distributional implications. Specifically, if individuals believe their own abstention will be offset by others, they may perceive CO_2 pricing as less effective or anticipate higher aggregate consumer costs. These channels create competing hypotheses: reduced pivotality may increase support by strengthening the case for collective regulation, or decrease it if the policy is perceived as ineffective or excessively costly. Since pivotality varies across markets and consumption contexts, understanding its effect on support helps predict where CO_2 pricing is likely to face political resistance.

To causally examine how reduced pivotality in market settings affects support, I conduct an incentivized survey experiment in a representative sample of the German population. The design varies market structure to exogenously manipulate individuals' pivotality in consumption by introducing strategic interdependence. Participants decide whether to purchase up to two neutral products that provide a private payoff but generate an externality by canceling donations intended to reduce actual CO_2 emissions. In the baseline condition (*Control*), participants make purchasing decisions individually and are fully pivotal. In the treatment condition (*RL*), two participants share a market and make sequential purchasing decisions as a first mover (FM) and second mover (SM). This structure reduces individual pivotality and introduces interdependence in consumption, as one participant's purchases may limit the other's ability to consume.

In both conditions, participants make consumption decisions with and without a CO_2 price and vote on whether such a price should be introduced. Within individuals, I randomly vary revenue recycling schemes (state budget, redistribution, and funding climate projects) and elicit injunctive and descriptive social norms regarding product purchases to study potential mechanisms through which perceived pivotality shapes support for CO_2 pricing.

The key result is that the *RL* treatment lowers support for CO_2 pricing by about five percentage points, but only when revenues are allocated to the general state budget. This negative effect disappears when revenues are earmarked for redistribution or environmental projects, indicating that RL matters most when revenues from CO_2 pricing are diffuse and weakly linked to the externality itself. Individuals' own purchasing decisions do not predict support, but beliefs about others' behavior do. When participants expect high aggregate consumption in the absence of a CO_2 price, support declines, as the policy is perceived to impose costs on many consumers. Conversely, when participants expect high consumption even under a CO_2 price, support increases, since fewer consumers are expected to be priced out of the market. Both

patterns are consistent with prior evidence that distributional concerns dominate self-interest in shaping support for CO_2 pricing (Kallbekken and Sælen, 2011).

To examine the underlying mechanism, I test how the *RL* condition relates to perceived policy effectiveness. Accounting for perceived effectiveness — inferred from differences in beliefs about others' behavior with and without a CO_2 price — attenuates the estimated negative effect of *RL* on support. Moreover, the likelihood that the policy is perceived as effective is higher in the *RL* condition. Yet, when revenues are allocated to the state budget, perceived effectiveness is negatively correlated with support. In other words, a more effective policy can imply that more consumers are affected by the price. Since *RL* increases perceived policy effectiveness, this can help explain why *RL* lowers support under state budget recycling. This pattern is consistent with concerns about distributional fairness and market participation: individuals who expect CO_2 pricing to induce strong behavioral responses anticipate higher aggregate costs for consumers and therefore view the policy as less desirable. These concerns appear to be more salient than the environmental benefits from lower emissions in shaping support.

The *RL* significantly increases average market-level purchases by 0.68 products, consistent with prior evidence on replacement dynamics (Ziegler et al., 2024). To shed light on the underlying mechanisms, I next examine individual behavior. *RL* leaves average FM consumption unchanged, but shifts the distribution: more participants buy exactly one product and fewer buy two. This is consistent with fairness considerations, as purchasing both products would exclude the SM from consumption. In contrast, this fairness constraint is never binding for SMs, since they cannot exclude others from consumption. Accordingly, when SMs assume that the FM did not purchase anything, individual consumption exceeds that in the *Control* condition. Results on social norms reinforce this interpretation: buying two products is less socially appropriate under *RL*, presumably because it excludes the SM from consumption.

Although SM behavior is endogenous to FM choices, the increase in externalities is more consistent with realized replacement than with FM engaging in the RL. To better understand the null effect on FM consumption, I examine the role of beliefs about consumption replacement in *RL* and social norms regarding buying all products oneself. Replacement beliefs positively correlate with FM purchases, and accounting for these beliefs reveals a negative association between *RL* and FM consumption. Including social norms, which capture the importance of allowing others to participate in consumption, attenuates these patterns. Overall, the evidence suggests that fairness concerns limit the extent to which the *RL* translates into higher FM consumption.

This paper contributes to the literature on public support for CO_2 pricing. Prior work explores the effects of information provision and nudges (Maestre-Andrés et al., 2021; Dechezleprêtre et al., 2025; Imai et al., 2022), distributional concerns, self-interest, and perceived

policy effectiveness (Kallbekken and Sælen, 2011; Dal Bó et al., 2018; Maestre-Andrés et al., 2019; Umit and Schaffer, 2020; Ewald et al., 2022), as well as revenue recycling schemes (Jagers and Hammar, 2009; Carattini et al., 2017; Kotchen et al., 2017; Klenert et al., 2018; Beiser-McGrath and Bernauer, 2019; Dominioni and Heine, 2019; Hammerle et al., 2021; Woerner et al., 2024). I show that public support for CO_2 pricing is lower when individuals are not fully pivotal in reducing emissions, but that revenue earmarking can mitigate this effect. By highlighting how market structures that shape beliefs about others' consumption influence policy support, this paper introduces perceived pivotality as a new dimension in the study of climate policy acceptance.

Second, this paper contributes to the broader literature on responsibility diffusion and excuse-driven behavior. Prior research shows that individuals self-servingly exploit ambiguity, risk, the possibility of mistakes, and beliefs about others' actions to justify selfish choices (Haisley and Weber, 2010; Di Tella et al., 2015; Exley, 2016; Exley and Kessler, 2024), strategically avoid information to reduce accountability (Dana et al., 2007; Grossman, 2014), and are even willing to pay for a diffused sense of responsibility or the ability to distort beliefs (Falk et al., 2020; Saccardo and Serra-Garcia, 2023). Most closely related are Bartling and Özdemir (2023) and Ziegler et al. (2024), who study the RL: the former show social norms can limit selfishness in donation games, while the latter finds a full erosion of moral behavior in markets despite the presence of norms. In my setting, aggregate market-level consumption increases, consistent with replacement dynamics. However, this increase does not stem from greater individual moral erosion among those who can engage in the RL. These individuals do not increase consumption, in line with norms against excluding others from market participation. This finding is consistent with Falk et al. (2020), who show that reduced pivotality does not increase selfish choices in sequential settings. Still, beliefs that others will replace forgone consumption strongly predict selfish choices, consistent with the RL.

Another strand of the literature examines moral behavior in markets. Early studies by Falk and Szech (2013) and Bartling et al. (2015a) show that market interaction can increase externalities compared to individual decision-making. Subsequent work explores how repeated interactions, anonymity, buyer–seller ratios, market framing, communication, and income affect moral behavior in market settings (Kirchler et al., 2016; Irlenbusch and Saxler, 2019; Sutter et al., 2020; Bartling et al., 2025, 2024). Other contributions address the regulation of markets with externalities, both experimentally (Ockenfels et al., 2020) and theoretically (Herweg and Schmidt, 2022). Most of this literature relies on posted-offer markets. While still stylized, this paper employs a simplified setting that captures another feature of everyday consumption contexts in which CO_2 pricing applies—such as groceries, transport, or clothing—where individuals act as price takers without market power. This perspective complements existing work by shift-

ing the focus from individual moral behavior in markets to democratic support for regulating such markets.[1]

The remainder of the paper is structured as follows: Section 2.2 presents the experimental design, Section 2.3 develops the hypotheses, Section 2.4 describes the sample and procedures, Section 2.5 outlines the empirical strategy, Section 2.6 presents the results, and Section 2.7 concludes.

2.2 Experimental Design

The experimental design combines elements from Woerner et al. (2024) and Ziegler et al. (2024) and includes both within- and between-subject components (see Figure 2.1 for an overview). The key trade-off in the experiment is between monetary gains and CO_2 emissions arising from a stylized product purchase. To elicit revealed preferences, participants receive real payments when they buy products, which reduces a donation to "atmosfair", an NGO that conducts and supports projects aimed at reducing CO_2, for example by funding renewable energy projects in developing countries. This ensures that decisions in the experiment have actual environmental consequences. To help participants understand the magnitude of one kilogram of CO_2 emissions, they are informed about the equivalent distance of a car trip.

In the within-subject component, participants act as consumers in a market and decide whether to buy zero, one, or two identical products. Each product costs €6 and has a value of €8, yielding a monetary payoff of €2 per product. However, purchasing a product generates 25 kg of CO_2 emissions. By default, the experiment includes a donation via "atmosfair" that offsets 50 kg of CO_2 per market (consisting of either one or two consumers). For each product purchased, the donation is reduced by 25 kg; if both products are bought, no donation is made for that market. Because the donation is determined at the market level, total consumption within the market determines aggregate emissions. Participants make two purchasing decisions: first without a CO_2 price and then with a CO_2 price. The CO_2 price increases the cost of each product to €7, reducing the monetary payoff to €1 per product. This corresponds to a CO_2 price of €40/tCO_2, which is below the actual CO_2 price of €55/tCO_2 in Germany at the time of the study.

After participants make the two purchasing decisions, they are randomly assigned to groups of 25 consumers and vote democratically on which purchasing decision — with or without the CO_2 price—will be implemented. A CO_2 price is adopted if at least 13 participants support it.

[1] Generally, in studies on market behavior and externalities, markets are frameworks where producers and consumers negotiate prices, often using posted-offer markets. However, in this study, individuals are price takers and have no bargaining power, and I refer to 'markets" in the context of purchasing a product rather than negotiating prices.

The voting process occurs under three different revenue recycling schemes (within-design): allocating the revenue to the state budget, redistributing it evenly among all 25 consumers, or donating it to environmental projects such as "atmosfair". Importantly, the instructions make it clear to participants that, regardless of the chosen scheme, the revenues will be used for these exact purposes. This ensures that their voting decisions have actual consequences. I present the revenue schemes in a randomized order. If a consumer purchases a product under the CO_2 price condition, an additional €1 in revenue is collected and allocated to a common fund, which would then be distributed according to the designated revenue recycling scheme. At the end of the experiment, one of the three votes is randomly selected for implementation, and participants are paid according to the resulting outcomes.

In contrast to Woerner et al. (2024), I do not use a random dictator scheme but allow for democratic votes, as this more closely reflects actual voting behavior. However, this comes at the cost of potentially introducing strategic voting, which a random dictator scheme would rule out. Because voting takes place in both experimental conditions, any strategic voting incentives are orthogonal to the pivotality manipulation.

The between-subject design is structured as follows. In the *Control* condition, a market consists of a single consumer who independently decides how many products to buy. In the *RL* treatment, the market consists of two participants but still only two products, making purchasing decisions interdependent. Each participant can buy up to two products, but total consumption is constrained by the shared market (i.e., the maximum number of available products is held constant at two). While the consequences of purchasing remain identical to those in the *Control* condition, participants' decisions now directly affect each other. Thus, the key difference between *Control* and *RL* is that consumption takes place either in an individual market or in an interdependent two-consumer market. In the latter, individual pivotality is reduced and one participant's consumption may limit the other's ability to consume. To capture strategic behavior, participants are randomly assigned to one of two roles: first mover (FM) or second mover (SM). Before their role is determined, they submit purchasing decisions for both roles, which ensures elicitation of their full strategy set. The FM's decision determines the SM's options: if the FM buys one product, the SM can purchase at most one; if the FM buys two, the SM cannot buy any; and if the FM buys none, the SM can buy up to two. This structure reduces individual pivotality while simultaneously introducing the possibility that one participant's consumption limits the other's ability to consume. In this setting, foregone consumption may be replaced by another consumer, potentially triggering RL.

All other steps, including the voting procedure, remain identical across both experimental groups. Participants are informed that their role will be randomly assigned and that payments will be made a few days after data collection concludes. The treatment design is a substantial

simplification of the setup used by Ziegler et al. (2024). For example, participants make decisions sequentially rather than simultaneously. The number of products and participants is reduced to two, and consumers act as price-takers with no market power. Nonetheless, I elicit the full strategy set of purchasing decisions, allowing all treated participants to engage in the RL.[2]

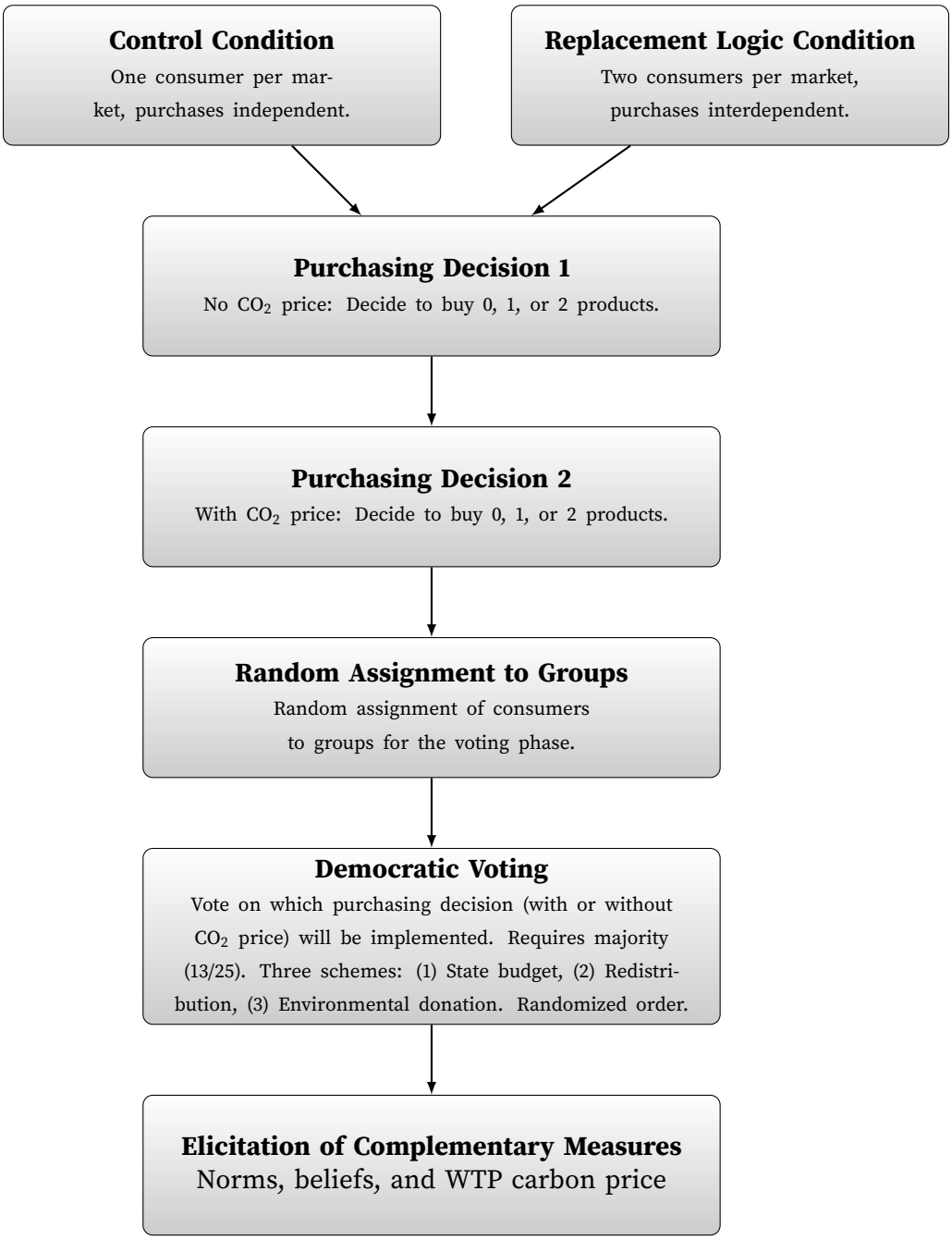
After collecting voting decisions, I ask participants, for each revenue scheme, whether they would have accepted the CO_2 price at different levels, ranging from €0.50 to €2 (excluding €1). Given the literature on the RL (Bartling and Özdemir, 2023; Ziegler et al., 2024), both descriptive and injunctive norms play a crucial role in decision-making. Additionally, the RL treatment may capture not only replacement beliefs but also concerns about negative consumption externalities on the other participant. Social norm measures help distinguish replacement beliefs from fairness considerations. To capture descriptive norms, I elicit participants' beliefs about how many of the 24 other consumers purchased zero, one, or two products, both with and without the CO_2 price. To ensure comparability, this question is conditioned on being the first mover in the *RL* condition. In the *RL* condition, I additionally ask how many products they believe the other consumer in their market would buy if he was the FM and the total number of products bought by the other 24 consumers in the group.

To measure injunctive norms, I follow the approach of Krupka and Weber (2013). Participants rate the social appropriateness of the three possible purchasing decisions (buying zero, one, or two products) both with and without the CO_2 price. Ratings are given on a four-point scale: "Socially very inappropriate", "Socially somewhat inappropriate", "Socially somewhat appropriate", and "Socially very appropriate". I explicitly define social appropriateness as behavior that most people agree is the "correct" or "preferable" course of action. Participants rate the social appropriateness of each action twice: once with and once without the CO_2 price. To avoid order effects, I randomize the presentation of purchase decisions. Finally, I elicit participants' beliefs about the proportion of votes in favor of the CO_2 price under each revenue scheme, presented in a randomized order. To ensure comprehension of the decisions, the survey requires correct answers to four comprehension questions before proceeding. The survey contains an attention check, and I record completion times to monitor engagement.[3]

[2] In the RL treatment, pivotality is belief-based, as outcomes depend on another consumer's purchasing decision. Participants therefore face uncertainty regarding whether foregone consumption will be replaced. The treatment also involves role uncertainty (FM vs. SM). The design addresses role uncertainty using the strategy method, whereby all contingent choices are elicited prior to role assignment.

[3] The questionnaire for the experimental questions is shown in Appendix D.2 and the "atmosfair" certificates are shown in Appendix D.3.

Figure 2.1: Experimental Design



2.3 Hypotheses

I aim to answer three research questions:

1. Does the *RL* condition affect support for a CO_2 price?
2. Does the *RL* condition influence consumption choices?
3. Does the *RL* condition shape social norms (descriptive and injunctive) regarding product purchases?

The first research question examines how the *RL* condition affects voting decisions on a CO_2 price. The second and third research questions explore the mechanisms underlying these decisions by focusing on consumption behavior, beliefs, and social norms.

Between-Subject Design Hypotheses (Main Results).

- **H1:** A higher share of individuals in the *RL* condition votes in favor of the CO_2 price compared to the *Control* condition.
- **H2:** A higher share of individuals in the *RL* condition will accept a higher CO_2 price compared to the *Control* condition.

H1 addresses the main research question. In the *RL* condition, individual consumption choices are less pivotal for aggregate outcomes. Reduced pivotality may increase support for collective regulation if individuals perceive a greater need for policy intervention when individual actions are easily replaced. This reasoning is consistent with Ziegler et al. (2024), who show higher trading activity and stronger beliefs about market participation under reduced pivotality. If individuals expect their own abstention to be replaced, they may view a CO_2 price as a necessary collective instrument. Building on this reasoning, H2 examines whether the *RL* condition not only increases support for CO_2 pricing (as in H1), but also increases individuals' willingness to accept a higher CO_2 price. Due to a stronger perceived collective responsibility, individuals may not only support a CO_2 price but also tolerate higher prices to address the environmental externality.

At the same time, reduced pivotality could decrease support for CO_2. If individuals believe that others will purchase the product regardless of their own choices, they may perceive the policy as less effective or anticipate higher aggregate costs, reducing support (Kallbekken and Sælen, 2011; Dechezleprêtre et al., 2025). The experiment therefore allows both mechanisms to operate. Ex ante, however, evidence on increased trading and stronger beliefs about market participation under reduced pivotality suggests that support for CO_2 pricing will be higher in *RL* than in *Control*. To distinguish between these alternative mechanisms, I examine the roles of descriptive and injunctive social norms as well as perceived policy effectiveness.

The next set of hypotheses explores the impact of RL on consumption behavior, beliefs, and social norms:

- **H3:** More products will be purchased in *RL* markets compared to the *Control* markets.
- **H4:** Participants in *RL* have higher beliefs about the proportion of consumers purchasing one product and the proportion buying two products, compared to *Control*.
- **H5:** Purchasing products will be perceived as more socially appropriate in *RL* compared to the *Control* condition.

These hypotheses build on the idea that reduced pivotality lowers perceived personal responsibility, which may encourage more selfish consumption decisions (H3). However, fairness considerations and norms regarding exclusion from consumption may counteract this tendency, making the overall effect on consumption ambiguous. Additionally, participants in *RL* may assume that others will step in to purchase products, leading to higher expected aggregate consumption (H4). Finally, responsibility diffusion may increase the perceived social acceptability of purchasing behavior (H5).

Within-Subject Hypotheses (Secondary Hypotheses). The within-subject design allows us to analyze how the introduction of a CO_2 price affects purchasing behavior and revenue recycling preferences. These hypotheses focus on the replication of previous results on support for CO_2 and revenue schemes.

- **H6:** More products will be purchased in the no CO_2 price condition compared to the CO_2 price condition.

Increasing the cost of the product in this setting should reduce demand.

- **H7:** Revenue recycling schemes that allocate funds to citizens or climate projects will receive more support than the state budget allocation.

Previous literature suggests that earmarking revenues for specific causes (such as redistribution or environmental projects) increases public acceptance of CO_2 pricing (see e.g., Woerner et al., 2024).

2.4 Data

The experiment was implemented in the second wave of a two-wave panel survey. The first wave comprised 2,414 individuals representative of the German adult population regarding age, gender, education, and residential federal state. The panel is titled "Preferences for Environmental and Sustainable Policies and the Replacement Excuse: An Experimental Investigation" (PRESPARE). Data collection for the first wave took place between January 27 and February 7, 2025. The second survey wave was fielded between February 12 and February 21, 2025. Participants were recruited by the professional survey provider Bilendi and received the standard participation compensation offered by the panel provider. Payments from the experiment constituted an additional bonus payment provided by the experimenter. Participants on average earned approximately €1 in the experiment and total donations led to a reduction of 84 tons of CO_2. A total of 1,934 participants completed the second wave, corresponding to an attrition rate of approximately 20%, which is within the typical range of 20–30% reported by Bilendi. For the main analyses, I restrict the sample to 1,863 observations after excluding respondents with missing information on pre-specified control variables or relevant outcomes.[4]

I use baseline information collected in the first survey wave, including demographic and socio-economic characteristics such as age, gender, education, income, employment status, the presence and number of minor children in the household, and federal state of residence. I also elicit voting intentions and general attitudes toward governmental regulation in markets, consumption decisions, and CO_2 emissions. In addition, the questionnaire captures beliefs about climate change, including whether respondents believe it exists, whether it is human-caused, how serious they perceive the problem to be, and how strongly they expect to be personally affected. Risk preferences, patience, and locus of control are measured using items from the German Socio-Economic Panel (SOEP). A detailed description of all relevant variables is provided in Table B.3 in the Appendix.

Panel A of Table B.4 in the Appendix presents summary statistics for demographic and socio-economic characteristics as well as attitudes. Columns (1)–(3) report means and standard deviations for the full sample and the two experimental groups (*Control* and *RL*). Individuals in the sample are on average 49 years old, and 49% are female. Approximately 24% hold a tertiary education degree, average personal monthly net income is €2,583, 45% are employed full-time, and about 26% have minor children, with an average of 0.42 children per household. The two

[4] An attrition analysis (Table B.1 in the Appendix) shows that age ($p < 0.01$), tertiary education and risk-taking ($p < 0.05$), as well as income and full-time employment ($p < 0.1$), are associated with participation in the second wave. Despite selective attrition, Table B.2 indicates that the sample remains broadly comparable to population benchmarks from Eurostat, although respondents are on average slightly older and more highly educated. Appendix E contains detailed survey and data documentation and Appendix D the full questionnaire.

political parties most relevant to our analysis are the Green Party ("Bündnis '90/Grüne") and AfD ("Alternative für Deutschland"). Twelve percent of the sample supports the Green Party, which advocates strong climate action, while 22% support the AfD, a far-right party known for its opposition to climate policies.

Table B.4 also reports behavioral and attitudinal measures. Sixty-two percent of participants agree that climate change is human-made, and 72% believe that climate change exists. Fifty-one percent agree that governments should regulate CO_2 emissions. The average locus of control score is 2.35 on a scale from 1 to 5, where higher values indicate a stronger sense of personal control. The average willingness to take risks is 4.06, and willingness to postpone rewards is 5.48; both variables are measured on an 11-point scale ranging from 0 to 10, with higher values indicating greater willingness. Participants made on average 0.68 errors in comprehension question 1, 0.79 in question 2, 0.59 in question 3, and 0.74 in question 4. Questions 1 and 2 relate to purchasing decisions, while questions 3 and 4 assess comprehension of the voting procedure.

To assess balance across treatments, the t-tests reported in column (4) of Panel A of Table B.4 indicate that randomization was generally successful, as no pre-determined characteristics differ significantly across groups. Errors in comprehension questions related to purchasing decisions are higher in the *RL* condition ($p < 0.01$).

2.5 Empirical Strategy

Between-Subject Design. First, I discuss the empirical strategy for the between-subject design, focusing on H1 through H5. The primary outcomes include the three votes on the CO_2 price under different revenue recycling schemes: state budget, redistribution, and funding of environmental projects. For each vote, I define a binary variable that takes the value of 1 if the participant supports implementing the CO_2 price. To measure participants' willingness to pay for a CO_2 price, I define a variable indicating the highest CO_2 price that an individual is willing to accept under each revenue scheme.

Aggregate market-level consumption is not directly observed in the *RL* treatment because FM and SM roles are assigned ex post and SM behavior is elicited using the strategy method. I therefore implement a participant-level bootstrap procedure with 1,000 replications to test for statistical differences relative to *Control*. In each replication, participants are resampled with replacement and expected market consumption is recomputed under simulated matching using 100 simulated markets. Within each simulated market, two participants are randomly assigned to the roles of FM and SM, and total purchases are determined by the realized FM decision and the corresponding contingent SM response. The difference relative to *Control* is cal-

culated in each replication, and confidence intervals are obtained from the empirical bootstrap distribution.

I also examine purchasing behavior by creating binary indicators for whether participants purchase at least one product, two products, no products, or exactly one product. Additionally, I define a continuous variable representing the total number of products purchased, both in the absence and presence of the CO_2 price. In the *RL* treatment, I report purchasing decisions separately for FMs and SMs to provide insight into behavioral mechanisms. FM choices are directly affected by treatment assignment and therefore capture how reduced pivotality influences individual purchasing decisions. However, SM choices are contingent on the FM decision and are elicited using the strategy method; accordingly, these role-specific responses are descriptive and do not identify causal effects.

The secondary set of outcomes contains norms and beliefs about others' voting behavior. Regarding injunctive norms, I assign the following values to each of the social appropriateness statements: -2 "Socially very inappropriate", -1 "Socially somewhat inappropriate", 1 "Socially somewhat appropriate", and 2 "Socially very appropriate." For descriptive norms—beliefs about overall purchasing behavior—I construct two continuous variables, ranging from 0 to 24, representing participants' beliefs about the number of individuals purchasing one product and the number purchasing two products. Additionally, beliefs about overall consumption range from 0 to 48 products. A detailed description of all relevant variables is provided in Table B.3 in the Appendix.

First, I compare raw mean differences between the *Control* group and the *RL* treatment across all outcomes of interest, conducting simple t-tests. In the main analysis, I estimate the following equation using OLS:[5]

$$Y_i = \alpha + \beta_1 RL_i + X_i'\gamma + \epsilon_i \tag{2.1}$$

where i indexes individuals, and the *Control* group serves as the reference category. Y_i represents the respective outcome of interest, including voting behavior, product purchases, and injunctive and descriptive norms. The coefficient β_1 captures the average treatment effect of the *RL* treatment relative to the *Control* group. Standard errors are heteroskedasticity robust. The vector X_i' includes control variables such as age, a female dummy, the log of personal monthly net income, a tertiary-education dummy, a full-time-employment dummy, the number of minor children in the household, dummies indicating voting intentions for AfD and the Green Party, beliefs about whether climate change exists and is human-made, whether the

[5] I do not analyze aggregate market-level consumption with the following steps since it is not directly observed and I test for statistical difference via the bootstrapping approach.

state should regulate CO_2 emissions, and measures of risk-taking, patience, and locus of control. The error term ϵ_i captures unobserved determinants of the outcome.

Importantly, I control for the number of errors across the first two comprehension checks in every specification. As the instructions in the *RL* treatment are structurally more complex than the individual *Control* setting, this accounts for the differential cognitive load induced by market interdependence. Controlling for these errors reduces noise; otherwise, treatment effects may be attenuated due to misunderstanding of the experimental incentives.

To study the relationship between voting behavior, norms, product choices, and beliefs about others' voting behavior, I correlate these measures. Specifically, I include both injunctive norms (perceived social appropriateness) and descriptive norms (beliefs about others' behavior), as well as beliefs about others' voting decisions, as explanatory variables in Equation (2.1), using individual voting behavior across the three revenue recycling schemes as the dependent variable. This allows for suggestive evidence on the relative importance of social norms and beliefs about others' actions in shaping support for CO_2 pricing.

While I expect that the *RL* condition increases support for CO_2 pricing, the direction of the effect could plausibly be negative. Therefore, I analyze whether the perceived effectiveness of CO_2 price—measured as the difference in expected product purchases with and without a CO_2 price—predicts support. I construct a set of dummy variables indicating the magnitude of this difference (ranging from 1 to 6 fewer products purchased with the price). Because this variable includes both negative and positive values, it is less intuitive to interpret. To address this, I additionally report specifications where all negative values are recoded to zero, and additionally where all positive values are recoded to zero.

To account for multiple hypothesis testing, I apply the Romano-Wolf correction as a robustness check.

Within-Subject Design. To test H6, I reshape the data set and analyze purchasing behavior in a within-subject design. Each individual contributes two observations: purchasing choices with and without the CO_2 price. I estimate the following equation using OLS:

$$Y_{ij} = \alpha + \delta CO_{2ij} + X'_{ij}\theta + \sigma_{ij} \tag{2.2}$$

where i indexes individuals and j indexes within-individual observations. Y_{ij} represents the respective outcome for observation j of individual i: purchasing one product, purchasing two products, and the total number of products bought. The coefficient δ captures the effect of introducing a CO_2 price on purchasing behavior, with the decision made in the absence of a CO_2 price serving as the baseline condition. Due to the experimental design, individuals first

make purchasing decisions without the CO_2 price and then with it, meaning the effect is not causal since the within-subject variation is not randomized. The vector X'_{ij} includes the same controls as in Equation (2.1), with the addition of an indicator for treatment assignment to the *RL* condition to account for structural differences in market settings. Note that this specification also always includes the number of errors in the first two comprehension questions. σ_{ij} refers to the error term.

To test H7, the data set contains three observations per individual: voting in favor of the CO_2 price, targeted to either the state budget, donations to an environmental project, or redistribution.

$$Y_{ik} = \alpha + \tau_1 redist_{ik} + \tau_2 donate_{ik} + X'_{ik}\Lambda + \rho_{ik} \tag{2.3}$$

where i indexes individuals and k indexes revenue-scheme-specific observations. Y_{ik} is a dummy variable equal to one if individual i voted in favor of the CO_2 price in observation k. The coefficient τ_1 captures the average effect of allocating generated revenues to redistribution instead of the state budget, while τ_2 measures the average effect of donating revenues to an environmental project in Germany instead of the state budget. The vector X'_{ik} is defined as above, and all subsequent analytical steps follow the same procedure as in previous specifications. ρ_{ik} refers to the error term. To assess whether the treatment effect differs across revenue schemes, I additionally estimate interaction models between treatment status and revenue scheme. For Equations (2.2) and (2.3), I cluster standard errors at the individual level.

2.6 Results

2.6.1 Between-Subject (Main Results)

Voting Behavior. Panel A of Figure 2.2 displays the average level of support for each CO_2 pricing scheme across experimental groups. While raw t-tests on the full sample do not reach standard significance levels, they consistently point toward a reduction in support under *RL* (see column (4) in Table B.4). Panel B of Figure 2.2 presents the estimated treatment effects of the *RL* condition on support for CO_2 pricing across the three revenue recycling schemes. These estimates include the full set of covariates specified in Section 2.5 and follow the regression model in Equation (2.1). Unless noted otherwise, all figures displaying regression coefficients use this specification. The results are not consistent with H1; in fact, they point in the opposite direction. Specifically, the *RL* treatment significantly reduces support for CO_2 pricing when revenues are allocated to the state budget (a reduction of approximately 5 percentage points, $p < 0.05$). This suggests that *RL* matters primarily when revenues are not earmarked and are

instead perceived as a fiscal burden. Notably, support exceeds 50% when revenues are redistributed or donated, but remains below majority support when allocated to the general state budget.

Controlling only for comprehension errors, the estimated effect of *RL* on support for CO_2 pricing (state budget) is statistically significant at the 5% level (Table B.5). Adding further controls changes the estimate only marginally, from -0.046 to -0.049, suggesting that differential understanding may introduce measurement noise that attenuates treatment effects. Consistent with this interpretation, restricting the sample to participants without comprehension errors yields larger and more precisely estimated effects (up to -6.8 percentage points; see column (5) in Table B.5).

Voting occurs under a democratic threshold rule and may reflect strategic considerations in addition to underlying policy preferences. However, beliefs about the expected level of support for the CO_2 price for each revenue scheme are balanced across treatment arms (Table B.4), suggesting that election-stage expectations are orthogonal to the *RL* manipulation.

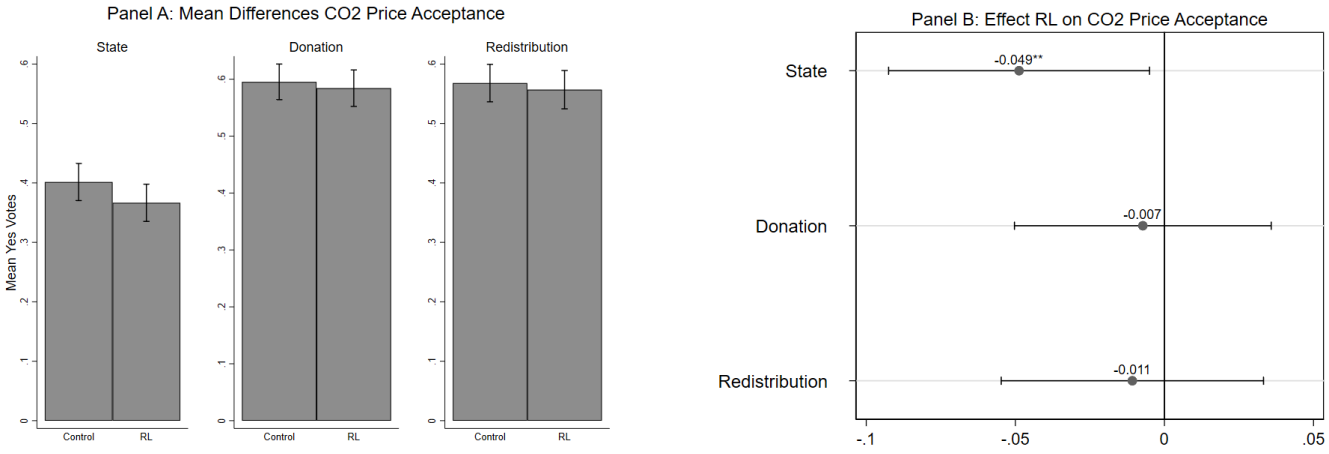


Figure 2.2: Descriptives and Regression Coefficients for Voting Decisions

Notes: Panel A displays raw means of voting decision dummies for the *Control* and *RL* groups, with 95% confidence intervals shown as vertical lines. Panel B presents estimates of the effect of the *RL* treatment on the voting dummies for the revenue schemes: state budget, donation to climate projects, and redistribution. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. Standard errors are heteroskedasticity robust. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Figure 2.3 corroborates this pattern using switching points from the MPLs. Panel A indicates lower switching points in the *RL* condition for the state budget scheme. t-tests confirm

this difference (mean difference: 0.06, $p < 0.05$; see Table B.4, column (4)). In contrast, there are no significant differences under the two earmarked revenue schemes. Panel B presents regression-adjusted estimates: controlling for covariates, the *RL* treatment reduces the willingness to pay by €0.084 ($p < 0.01$) in the state budget condition, with no detectable effects for the other schemes. This pattern contradicts H2, suggesting that reduced pivotality not only reduces support but also lowers the acceptable size of carbon prices, although the effect size of €0.084 is economically modest.

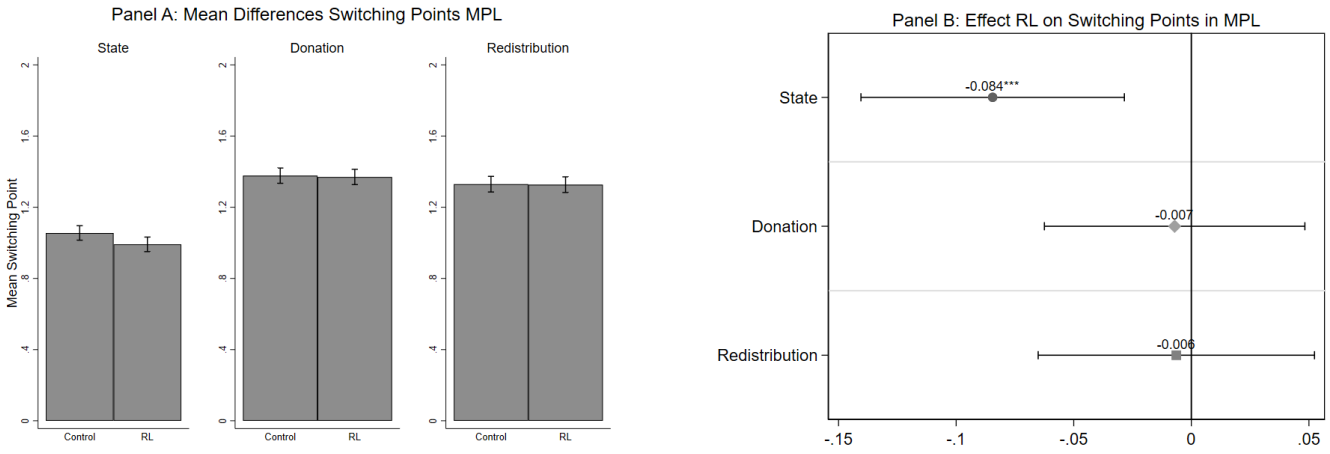


Figure 2.3: Descriptives and Regression Coefficients for Switching Points in MPLs

Notes: Panel A displays raw means of switching points in MPLs measuring CO_2 price acceptance for different price levels (0.50, 1.50, and €2.00) for the *Control* and *RL* groups, with 95% confidence intervals shown as vertical lines. Panel B presents estimates of the effect of the *RL* treatment on these switching points for the revenue schemes: state budget, donation to climate projects, and redistribution. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. Standard errors are heteroskedasticity robust. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Purchasing Behavior. First, I examine the effect of *RL* on aggregate market-level consumption. Based on the bootstrap procedure outlined in Section 2.5, Table 2.1 presents the estimated differences in aggregate market-level consumption between the *RL* and *Control* conditions. The results support H3: aggregate consumption is on average 0.658 products higher without a CO_2 price and 0.514 products higher with a CO_2 price compared to the *Control* condition ($p < 0.01$).

Next, I examine whether *RL* affects individual purchasing behavior at the role level. Panel A of Figure 2.4 shows that FMs in *RL* purchase a similar number of products as participants in *Control*, both with and without a CO_2 price. These differences are not significant (see col-

Table 2.1: Effect of RL on Aggregate Market Consumption

Condition	Control Mean	RL Mean	Difference (RL – Control)	95% CI
No CO2 Price	0.920	1.578	0.658***	[0.526, 0.790]
CO2 Price	0.860	1.374	0.514***	[0.372, 0.656]

Notes: This table reports bootstrap-based estimates of the difference in aggregate market-level consumption (number of products bought) between the *RL* and *Control* conditions, separately for the cases with and without a CO_2 price, along with 95% confidence intervals (CI). Reported *RL* means are constructed by adding the estimated difference to the empirical *Control* mean. Differences are obtained using 1,000 participant-level bootstrap replications. Within each replication, expected *RL* market consumption is computed via simulated random matching and role assignment. Significance levels refer to two-sided bootstrap tests of the null hypothesis that the difference between *RL* and *Control* equals zero. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

umn (4) of Table B.4). This is confirmed by the regression estimates in Panel B, which show no significant change in FM consumption under the *RL* condition. Although SM behavior is not causal due to its dependence on FM choices, it helps to shed light on the aggregate consumption effect. In contrast to FMs, consumption among SMs is significantly higher in *RL* than in *Control*, both with and without a CO_2 price. As shown in Panel A and column (4) of Table B.4, the differences are statistically significant at the 5% and 10% levels, respectively. Panel B confirms that SMs in the *RL* condition purchase 0.075 more products in the no-price condition and 0.074 more in the price condition (both significant at the 5% level). This comparison focuses on SMs who know that the FM did not purchase any products, so that the decision environment is more comparable to the *Control* condition. One possible explanation is that FMs limit consumption to avoid excluding others, whereas SMs—who know the other participant did not consume—do not face this fairness constraint.

Although the *RL* treatment does not affect FMs' total purchases, it alters the distribution of choices. As shown in Figure B.2 and Table B.4 (Appendix), *RL* increases the probability that FMs purchase exactly one product and reduces the likelihood of purchasing two products, both with and without a CO_2 price. For SMs, the pattern differs. When SMs know the FM purchased nothing, *RL* increases the probability of buying two products and of purchasing at least one product (only with a CO_2 price) relative to the *Control*. Conversely, when the FM purchased one product, SMs are less likely to purchase at least one product. These patterns suggest that while *RL* does not increase aggregate FM consumption, it shifts behavior toward allowing others to participate in consumption. This is consistent with fairness considerations: FMs avoid excluding others (by buying two products), while SMs adjust their choices in response to prior consumption.

In sum, the *RL* condition reduces support for CO_2 pricing only when revenues are allocated to the state budget. The *RL* increases aggregate market-level consumption. This increase ap-

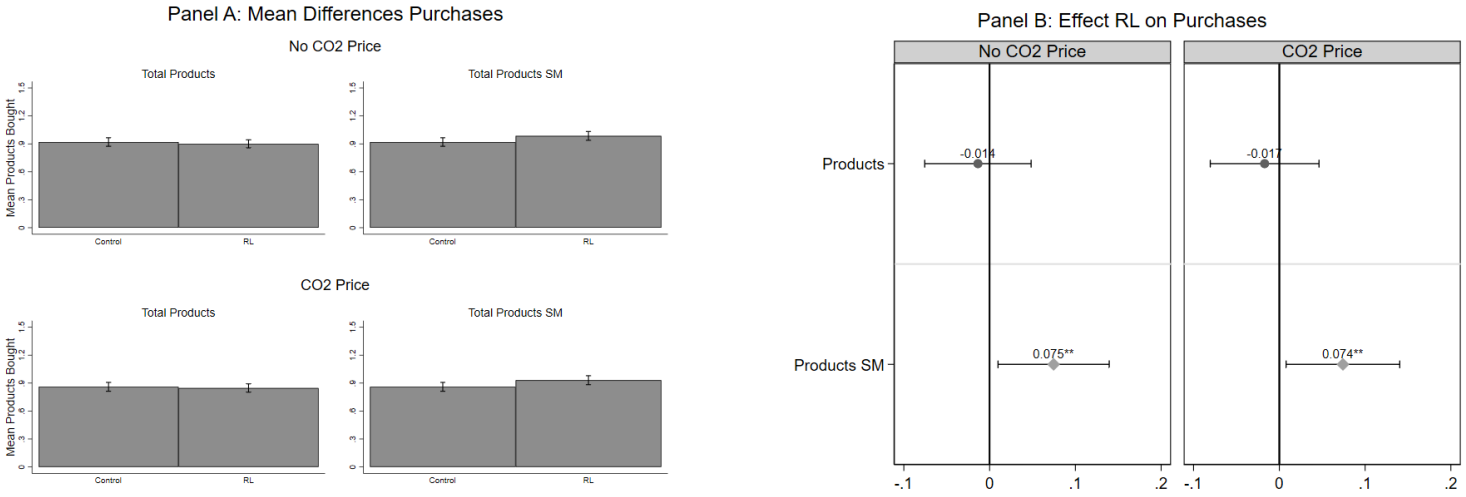


Figure 2.4: Descriptives and Regression Coefficients for Purchasing Decisions

Notes: Panel A displays raw means of purchasing decisions (number of products bought) for FM and SM behavior for the *Control* and *RL* groups, with 95% confidence intervals shown as vertical lines. Panel B presents estimates of the effect of the *RL* treatment on these purchasing decisions. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. Standard errors are heteroskedasticity robust. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

pears to be driven by realized replacement behavior among SMs rather than by anticipated replacement beliefs among FMs. FM consumption remains unchanged overall, although purchasing patterns shift, while SM consumption responds to FM's purchasing decisions. Together, these findings suggest that fairness considerations may shape both consumption and policy support. I explore this interpretation more thoroughly in the following section. [6]

2.6.2 Between-Subject Design (Mechanisms)

Voting Behavior. Potential mechanisms include beliefs about others' behavior and participants' own consumption decisions. Including these variables does not change the estimated *RL* effect on voting behavior (Appendix Figure B.1), but the correlations are informative. According to Figure B.1, beliefs about others' voting behavior positively predict support for CO_2

[6] Appendix Tables B.6, B.7, B.8, and B.9 report regression results mirroring the patterns from the t-tests and figures, and include p-values adjusted for multiple hypothesis testing using the Romano-Wolf method. These adjustments slightly alter significance in a few cases (e.g., from 1% to 5% or from 5% to 10%), and the SM dummy for purchasing two products without a CO_2 price loses its significance (p = 0.104). Otherwise, the main pattern remains robust.

when revenues are allocated to the state budget and to environmental donations, but not under redistribution. Higher expected consumption by others in the absence of a CO_2 price is associated with lower support for the state budget scheme, whereas under a CO_2 price the relationship becomes positive across all revenue schemes. In contrast, respondents' own purchasing decisions are not associated with policy support. This suggests that beliefs about others' behavior are more predictive of support than individual consumption. Although voting in *RL* occurs under uncertainty about whether an individual will be the FM or SM, own purchasing decisions do not predict support for the CO_2 price, suggesting that role uncertainty is unlikely to drive the results. The opposite signs of beliefs about others' consumption with and without a CO_2 price in the state budget condition suggest that perceived policy effectiveness may play an important role. The next step therefore examines whether the *RL* treatment affects beliefs about consumption and the perceived effectiveness of CO_2 pricing.

To study H4, Panel A of Figure 2.5 indicates differences in beliefs about others' purchasing behavior between the experimental groups, which are confirmed by the regression estimates in Panel B. On average, expected total consumption (FM and SM behavior) is 1.086 products lower without a CO_2 price and 1.106 products lower with a CO_2 price (both $p < 0.05$). Note that these beliefs always refer to aggregate consumption in the voting group (0–48 products in total; 0–24 for zero, one, or two products).

Subsequent belief measures refer to FMs only, as SM behavior is context-dependent and can be inferred from aggregate consumption beliefs and FM behavior. The perceived number of individuals purchasing no products decreases by 0.385 without a CO_2 price ($p < 0.10$) and by 0.694 with a CO_2 price ($p < 0.01$). In contrast, the perceived number purchasing exactly one product increases by 0.816 without a price and by 0.829 with a price (both $p < 0.01$). Beliefs about the number purchasing two products decline by 0.431 without a CO_2 price ($p < 0.05$) and also decrease under the price, although the latter estimate is not statistically distinguishable from zero.

The results provide mixed evidence on H4. Consistent with purchasing behavior, participants in the *RL* condition expect greater market participation (in line with H4) but fewer individuals to purchase all available products (contrary to H4). Notably, beliefs do not appear self-serving: respondents expect lower total consumption than in the *Control* group, which may help explain why FM consumption does not increase. Importantly, these results do not contradict a fairness-based interpretation of either the lower support for a CO_2 price under state-budget recycling or the null effect on FM consumption. First, beliefs about total consumption are lower under *RL*. Second, perceived policy effectiveness depends on within-subject differences in expected consumption with and without a CO_2 price, which are not yet considered. Third, beliefs about the number of individuals purchasing zero, one, or two products refer only to FM be-

havior. Beliefs about aggregate consumption (FM and SM behavior) therefore better capture perceived policy effectiveness.

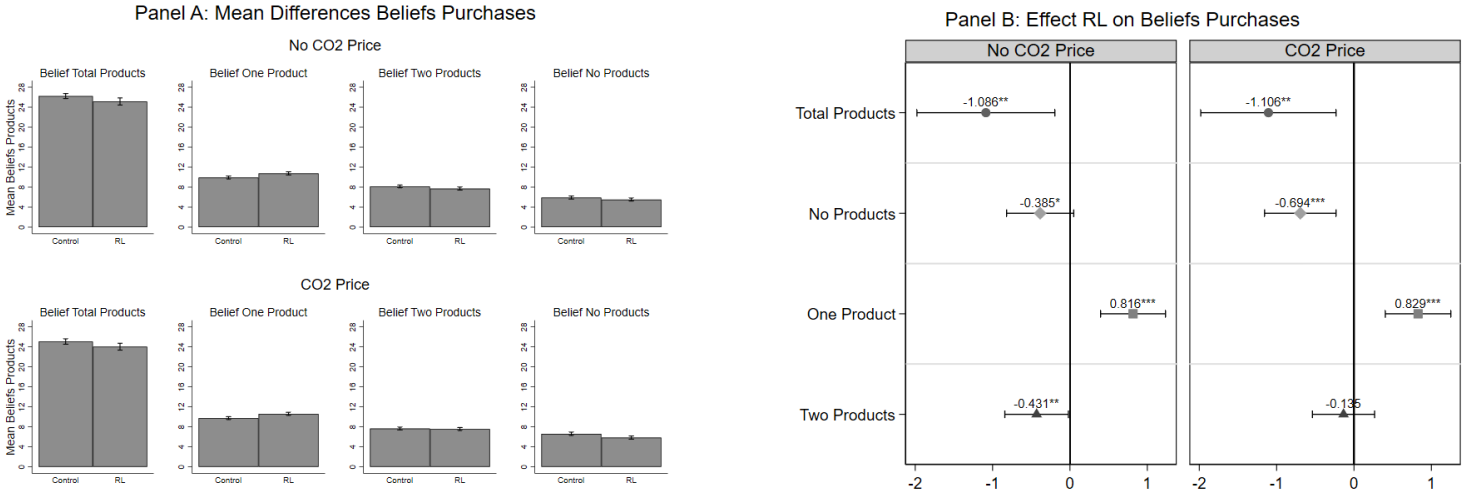


Figure 2.5: Descriptives and Regression Coefficients for Beliefs about Others' Purchasing Decisions

Notes: Panel A displays raw means of beliefs about others' purchasing decisions for the *Control* and *RL* groups, with 95% confidence intervals shown as vertical lines. Panel B presents estimates of the effect of the *RL* treatment on these beliefs. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. Standard errors are heteroskedasticity robust. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Therefore, Table 2.2 examines whether the *RL* treatment affects the perceived effectiveness of CO_2 pricing, measured as within-subject differences in expected consumption. Specifically, the dependent variables in columns (1) through (6) of Panel A are binary indicators for whether respondents expect consumption reductions due to a CO_2 price of at least 1–6 products, respectively. The effects are insignificant for reductions of one or two products. From reductions of at least three products onward, the effect becomes significant and increases in magnitude: *RL* raises the likelihood of expecting reductions of at least 3 products by 4.3 percentage points ($p < 0.10$), 4 products by 5.6 percentage points ($p < 0.05$), and 5 products by 7.1 percentage points ($p < 0.01$). For reductions of 6 or more products, the estimate remains large at 6.7 percentage points ($p < 0.01$). These results indicate that *RL* increases perceived policy effectiveness, particularly for sizable reductions.

Panel B of Table 2.2 examines whether perceived effectiveness accounts for a part of the effect of *RL* on support for CO_2 pricing when revenues are allocated to the state budget. The

Table 2.2: Support for CO_2 (State) and Effectiveness (Dummies)

	Panel A: Effectiveness CO2-Price					
	(1)	(2)	(3)	(4)	(5)	(6)
RL	0.010	0.010	0.043*	0.056**	0.071***	0.067***
	(0.024)	(0.023)	(0.023)	(0.022)	(0.021)	(0.020)
Control Mean	0.453	0.413	0.351	0.302	0.253	0.218
Obs.	1,863	1,863	1,863	1,863	1,863	1,863
Adj. R-Sq	0.011	0.007	0.010	0.012	0.013	0.012
Controls	X	X	X	X	X	X
Errors	X	X	X	X	X	X
	Panel B: Yes CO2-Price (State)					
	(1)	(2)	(3)	(4)	(5)	(6)
RL	-0.048**	-0.048**	-0.046**	-0.045**	-0.042*	-0.042*
	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)
Effectiveness CO2-Price	-0.056**	-0.066***	-0.073***	-0.072***	-0.089***	-0.102***
	(0.022)	(0.022)	(0.023)	(0.023)	(0.024)	(0.025)
Control Mean	0.401	0.401	0.401	0.401	0.401	0.401
Obs.	1,863	1,863	1,863	1,863	1,863	1,863
Adj. R-Sq	0.061	0.062	0.063	0.063	0.065	0.066
Controls	X	X	X	X	X	X
Errors	X	X	X	X	X	X

Notes: Panel A reports the estimated effect of the *RL* treatment on various dummies indicating perceived policy effectiveness. The dependent variables in columns (1) through (6) are binary indicators equal to 1 if the respondent believes that the CO_2 price reduces aggregate consumption by at least 1, 2, 3, 4, 5, or 6 units, respectively. Panel B shows how including these perceived effectiveness measures alters the estimated effect of the *RL* treatment on support for the CO_2 price. The effectiveness measures are the same binary indicators as defined in Panel A. Controls in both panels include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. Standard errors are heteroskedasticity robust. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

specification mirrors Panel A, sequentially including effectiveness thresholds from 1 to 6. Including thresholds for small reductions (columns 1–2) does not affect the *RL* coefficient. From column (3) onward, the coefficient declines in magnitude and its statistical significance weakens (from $p < 0.05$ to $p < 0.10$). This attenuation provides suggestive evidence that perceived effectiveness accounts for part of the treatment effect. All regressions in both panels include the full set of control variables as specified in Equation (2.1).

Appendix Table B.12 reports robustness checks using alternative effectiveness measures. The raw consumption-difference measure, which combines expected increases and decreases, is not affected by *RL* but is negatively associated with support. To clarify interpretation, I separate positive and negative belief updates. *RL* increases perceived consumption reductions and decreases perceived increases. These beliefs predict support and attenuate or strengthen the *RL* coefficient accordingly, supporting the interpretation that perceived effectiveness accounts for a part of the treatment effect.

Interestingly, perceived effectiveness is negatively associated with support for the CO_2 price

in the state budget condition. While one might expect effective policies to receive greater support, in this setting greater effectiveness implies stronger reductions in others' consumption and associated payments. This interpretation aligns with previous evidence: FMs in *RL* refrain from buying all products, potentially to avoid excluding others. Similarly, an effective CO_2 price may reduce support because it limits others' consumption.

Moreover, the negative association between perceived effectiveness and support is less consistent under donation or redistribution schemes, except when consumption is expected to fall by at least six products in the redistribution condition (Table B.13). This is consistent with the fairness interpretation: when the CO_2 price is highly effective, the revenue for redistribution declines, reducing potential benefits for others (and oneself). Earmarking revenues for environmental projects appears to offset these concerns. Effectiveness matters primarily when it implies fewer opportunities for others to benefit.

An alternative explanation is that effectiveness matters in the state budget condition because respondents dislike the state generating larger revenues under *RL*. However, correlations between support and social acceptability ratings of product purchases point toward a market-participation interpretation. Appendix Table B.10 shows that greater social acceptance of buying no products under the CO_2 price is associated with lower support, whereas acceptance of buying one product is associated with higher support. The former suggests resistance when social norms discourage participation, while the latter indicates greater support when participation is socially acceptable. These patterns align with the market-participation interpretation.[7]

Purchasing Behavior. I next explore potential explanations for the null effect of the *RL* treatment on FM purchasing behavior. However, the distribution of choices changes: participants in the *RL* condition are more likely to buy exactly one product and less likely to buy two. This shift mirrors changes in beliefs about others' behavior (see Figure 2.5) and is consistent with a normative channel. Specifically, the reduction in purchasing two products aligns with fairness concerns, similar to those underlying support for the CO_2 price. Participants appear reluctant to exclude others from consumption.

Figure 2.6 investigates H5 and further supports this interpretation. Panel A displays raw means of social appropriateness ratings for purchasing zero, one, or two products. Social acceptance declines with the number of products purchased. Notably, the ratings appear to differ between *RL* and *Control* for buying no products under a CO_2 price and for buying two products,

[7]Section B.3 presents a heterogeneity analysis. It highlights the role of social appropriateness ratings in the linear interaction models and the relevance of beliefs about total product consumption and locus of control in the non-linear analysis.

both with and without the price. Panel B presents regression-adjusted treatment effects of the *RL* condition. Only the effect for buying two products is statistically significant: in the absence of a CO_2 price, purchasing two products is viewed as less socially appropriate in *RL* compared to *Control* (coefficient: –0.166, $p < 0.01$). With a CO_2 price, the effect remains negative (–0.097) but is not statistically significant.[8]

These results do not support H5, which predicted that purchasing products would be viewed as more socially appropriate under the *RL* treatment. Instead, this pattern is consistent with a normative mechanism that explains the opposite result: purchasing both products may be perceived as excluding others from consumption. This interpretation also aligns with previous findings on the effectiveness of the CO_2 price, highlighting fairness concerns about market participation.

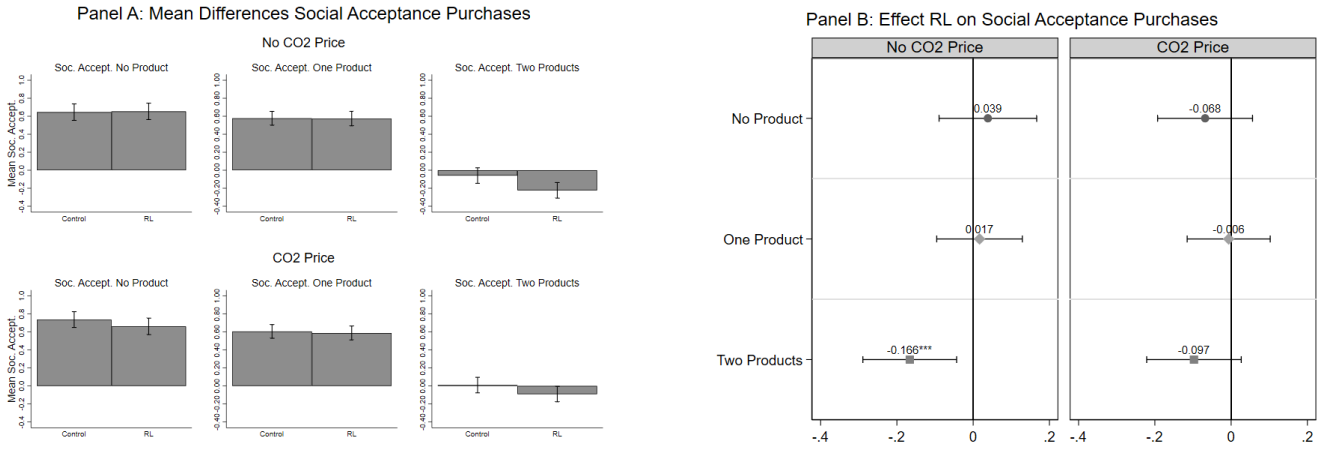


Figure 2.6: Descriptives and Regression Coefficients for Social Acceptance Ratings of Purchasing Decisions

Notes: Panel A displays raw means of social acceptance ratings for buying no, one, or two products for the *Control* and *RL* groups, with 95% confidence intervals shown as vertical lines. Panel B presents estimates of the effect of the *RL* treatment on the social acceptance ratings of product purchases. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. Standard errors are heteroskedasticity robust. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

The next set of results explores two potential mechanisms behind the null effect of the *RL* treatment on FM purchases: (i) the substitution belief central to RL, and (ii) normative concerns reflected in social acceptance ratings. In the *RL* condition, participants are asked how

[8] Appendix Table B.11 reports regression results that mirror the patterns observed in the t-tests and figures and includes p-values adjusted for multiple hypothesis testing using the Romano-Wolff method. These adjustments slightly reduce the significance of the treatment effect to $p = 0.029$.

many products a SM would buy if they, as a FM, chose not to buy anything. By design, this belief is zero in the *Control* group, which allows for a clean comparison. Based on responses, around 60% believe the SM would buy one product, and 30% believe the SM would buy both products, implying that only 10% do not adhere to the RL.

Panel A of Table 2.3 presents results without a CO_2 price; Panel B with the price. Column (1) in both panels shows the already established null effect of the *RL* treatment. However, once I include the belief measure in column (2), a clear pattern emerges: RL now significantly reduces product purchases (Panel A: -0.481; Panel B: -0.474, both $p < 0.01$), while the belief that consumption would be replaced significantly increases purchases (Panel A: 0.391; Panel B: 0.381, both $p < 0.01$). This suggests that the null effect is due to two opposing drivers. The replacement belief—i.e., that someone else will consume in one's place—increases consumption. Controlling for this belief reveals a potential second driver: lower consumption, potentially due to fairness concerns. To further test this normative channel, Column (3) adds the social acceptance rating of buying two products. In both panels, greater social approval of buying two products is associated with higher overall consumption (Panel A: 0.104; Panel B: 0.074; both $p < 0.01$), and its inclusion attenuates both the negative effect of *RL* and the positive effect of the belief. This pattern supports the interpretation that the *RL* effect operates through the two proposed mechanisms: belief-driven RL and fairness concerns (i.e., concern about excluding others from consumption). In short, the observed null effect is consistent with these opposing forces offsetting each other on average.

These results should be interpreted as suggestive evidence. While fairness concerns provide a plausible explanation, the inclusion of social approval norms only modestly attenuates the RL effect and does not fully explain it. Although the regression results are consistent with the proposed mechanisms, they do not rule out the possibility that other, unobserved factors contribute to the null effect. Overall, the patterns observed across both voting and purchasing behavior point to the importance of fairness considerations or distributional concerns. More specifically, individuals appear to care about allowing others to consume, which in this experimental setting translates into monetary payoffs. This stresses the role of normative motivations alongside beliefs in shaping behavior.

2.6.3 Within Variation (CO_2 Price and Voting Decisions)

To investigate H6 and H7, I analyze within-subject variation in product purchases in response to the CO_2 price and in voting behavior across revenue schemes.

Table 2.3: Purchasing Behavior, Beliefs about SM Behavior and Norms

	Panel A: Total Products (No CO2 Price)		
	(1)	(2)	(3)
RL	-0.014	-0.468***	-0.404***
	(0.032)	(0.051)	(0.051)
Belief Products SM		0.383***	0.343***
		(0.037)	(0.037)
Soc. Accept. Two Products (No CO2-Price)			0.102***
			(0.012)
Control Mean	0.920	0.920	0.920
Obs.	1,863	1,862	1,862
Adj. R-Sq	0.065	0.117	0.155
Controls	X	X	X
Errors	X	X	X
	Panel B: Total Products (CO2 Price)		
	(1)	(2)	(3)
RL	-0.017	-0.462***	-0.422***
	(0.032)	(0.051)	(0.052)
Belief Products SM		0.374***	0.347***
		(0.037)	(0.037)
Soc. Accept. Two Products (CO2-Price)			0.074***
			(0.012)
Control Mean	0.860	0.860	0.860
Obs.	1,863	1,862	1,862
Adj. R-Sq	0.089	0.135	0.153
Controls	X	X	X
Errors	X	X	X

Notes: Panel A presents how the estimated effect of the *RL* treatment on the total number of products purchased (in the absence of a CO_2 price) changes when two additional variables are included. Column (1) shows the baseline effect without the two additional variables. Column (2) adds the respondent's belief about how much a SM would consume if a FM buys nothing. Column (3) includes the social acceptability rating of purchasing two products. Panel B follows the same structure but examines purchasing behavior when a CO_2 price is in place. Controls in both panels include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. Standard errors are heteroskedasticity robust. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Effectiveness of CO_2 Price. Panel A of Figure 2.7 shows the mean differences in the number of products purchased with and without a CO_2 price for FMs and SMs, conditional on the FM not purchasing anything. In both cases, the CO_2 price significantly reduces consumption ($p < 0.01$). The effect size is comparable for both FMs and SMs (-0.058 vs. -0.055). In summary, the CO_2 price effectively reduces consumption, although order effects and the fact that SMs act contingent on FM choices may bias these within-subject estimates. While the *RL* effect is

negative but insignificant for FM behavior, it significantly increases consumption of SMs (0.068, $p < 0.05$). In magnitude, the increase associated with the *RL* treatment exceeds the within-subject reduction induced by the CO_2 price. This comparison is descriptive, as the two effects rely on different sources of variation.

Table B.14 in the Appendix replicates these estimates with and without controls and extends the analysis to product-purchase dummies for FMs and SMs. All effects are negative and significant, except that the probability of buying exactly two products does not decline. Overall, the CO_2 price effectively reduces individual consumption but does not significantly lower the likelihood of buying all available products.

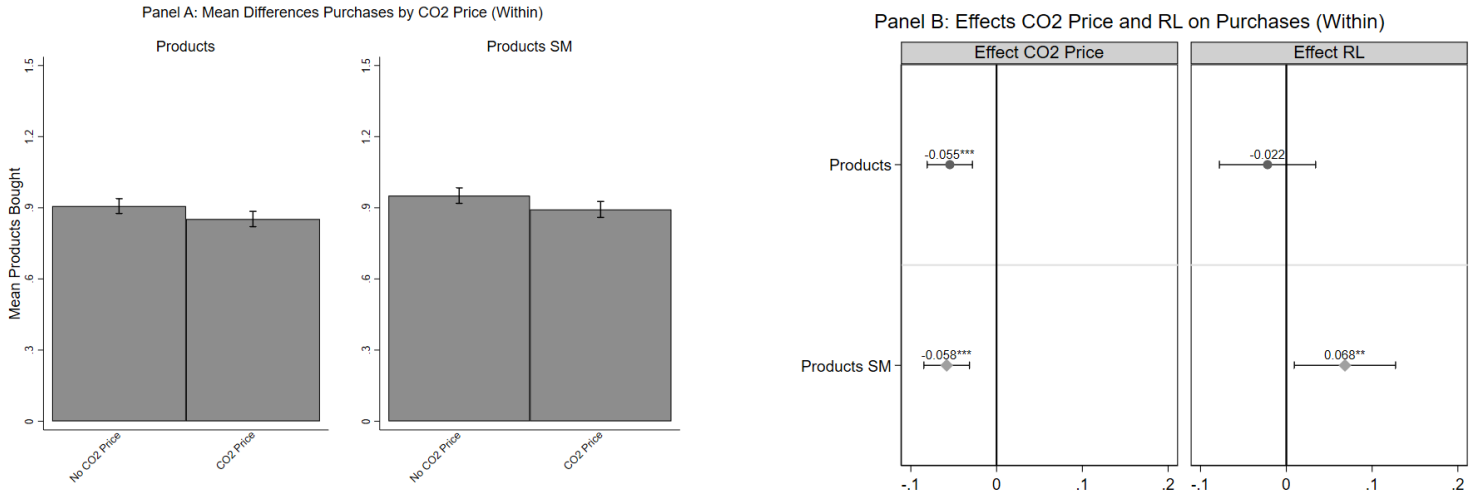


Figure 2.7: Descriptives and Regression Coefficients for Purchasing Behavior (Within)

Notes: Panel A displays raw means for the number of products purchased with and without a CO_2 price for FMs and SMs, conditional on the FM not purchasing anything, with 95% confidence intervals shown as vertical lines. Panel B presents estimates of the effect of a CO_2 price on total products bought by FMs and SMs. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. Standard errors are heteroskedasticity robust. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Voting Behavior. Panel A of Figure 2.8 presents mean differences in support for CO_2 pricing across the three revenue use schemes. As in the between design results, support for allocating revenues to the state budget does not reach majority support. In contrast, both the donation and redistribution schemes are supported by more than 50% of participants, with redistribution receiving slightly higher approval.

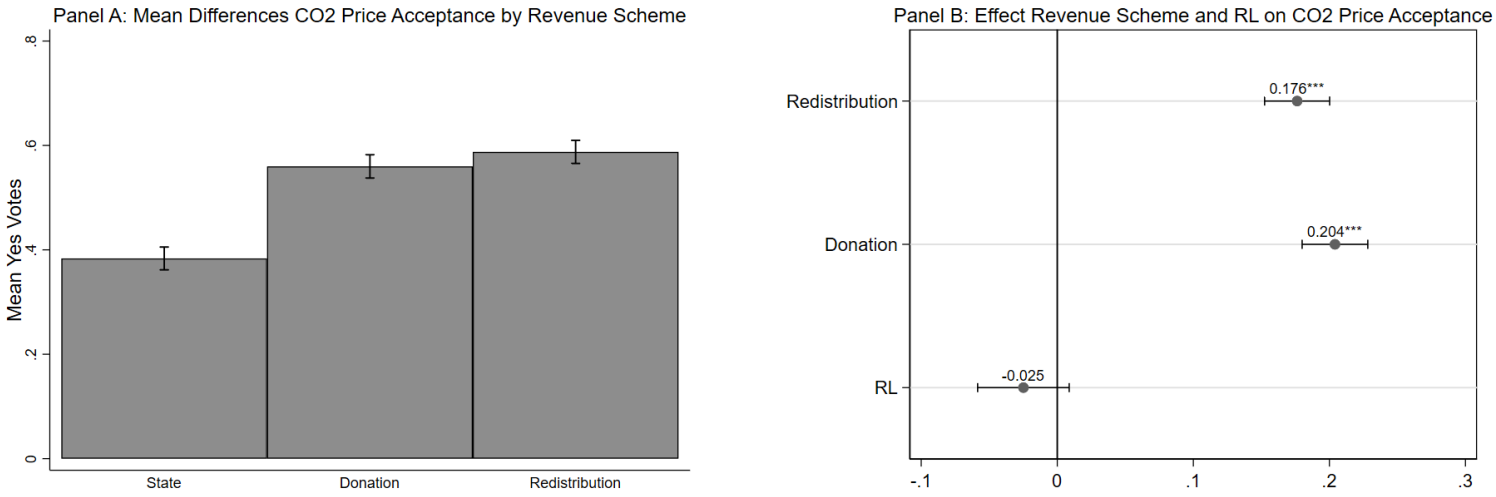


Figure 2.8: Descriptives and Regression Coefficients for Voting Decisions (Within)

Notes: Panel A displays raw means of voting decisions for each revenue scheme (state budget, donation to climate projects, and redistribution) with 95% confidence intervals shown as vertical lines. Panel B presents estimates of the effect of the donation and redistribution schemes on CO_2 acceptance (incl. the coefficient for the *RL* treatment). Controls include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. Standard errors are heteroskedasticity robust. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Panel B displays the corresponding regression coefficients. Compared to the state budget baseline, earmarking revenues for climate-related donations significantly increases support by 17.6 percentage points, while redistribution to participants within the voting group raises support even more; by 20.4 percentage points (both $p < 0.01$). On average, the *RL* treatment does not affect overall support for CO_2 pricing, regardless of how revenues are used.[9]

These results confirm previous findings that earmarking revenues, either through donations or direct redistribution, substantially increases support relative to allocating them to the state budget. Importantly, unlike other studies that relied on random dictator mechanisms, these preferences are elicited through democratic voting procedures. I additionally test whether the treatment effect differs across revenue schemes. Table B.16 indicates that the negative effect of *RL* is concentrated in the state-budget condition and attenuated under earmarking. However, differences in treatment effects across schemes are not statistically significant.

[9]Table B.15 in the Appendix replicates these results in regression format with and without controls.

2.7 Discussion and Conclusion

This paper experimentally examines how reduced individual pivotality—induced by interdependence in consumption decisions—affects support for market regulation in the presence of externalities. When individual choices have less impact on aggregate outcomes, support for CO_2 pricing varies depending on how revenues are used. At the same time, aggregate market-level externalities increase in the treatment. The effects on individual-level consumption depend on roles in the market: while aggregate consumption by FMs remains stable, the distribution of their purchasing decisions shifts. In contrast, SMs adjust their overall level of consumption.

First, I show that reduced pivotality lowers support for CO_2 pricing only when revenues are allocated to the state budget. When revenues are either donated to environmental projects or redistributed among participants, this effect disappears. Previous work such as Woerner et al. (2024) uses a random dictator design, whereas the current study elicits preferences through a democratic voting mechanism. Despite this difference, the ranking of revenue-use options is similar. On average, the state-budget option receives the lowest level of support and does not reach majority approval, while both earmarked alternatives receive higher average support and achieve majority approval. Furthermore, own consumption does not significantly predict support for CO_2 pricing, nor does it account for the observed treatment effect. While beliefs about others' consumption choices correlate with support, they do not explain the treatment effect. In contrast, perceived effectiveness of CO_2 pricing both predicts support and partially explains the impact of reduced pivotality. Crucially, the results suggest that higher perceived effectiveness (i.e., stronger reductions in others' consumption) can reduce support for CO_2 pricing, likely due to fairness concerns related to reductions in others' payoffs.

Second, I examine consumption behavior and its mechanisms. The aggregate market-level effect is consistent with SMs replacing FMs' consumption rather than FMs engaging in the RL and increasing their own consumption. The null effect of the *RL* treatment on FMs is aligned with findings by Bartling and Özdemir (2023). Social norms likely contribute to this outcome; specifically, the concern that purchasing all available products might exclude SMs from consumption. This pattern is consistent with fairness motives driving the observed null effect. SMs do not face this fairness constraint and exhibit higher individual consumption than in the *Control* condition.

As observed in this setting, there is further evidence that suggests that reduced pivotality can significantly increase externalities at the aggregate level. For instance, Ziegler et al. (2024) show that when individuals perceive their influence on aggregate outcomes to be minimal, externalities rise significantly. In my setting, however, higher aggregate externalities are not

driven by moral erosion among those who can engage in the RL. Instead, the aggregate increase arises primarily from replacement behavior. This study adopts a more simplified version of the setup in Ziegler et al. (2024). Several features of the experimental design could explain why consumption among FMs does not increase: consumers have no market power, markets are smaller in scale (both in terms of participants and available products), decisions occur sequentially, and a CO_2 price is present in one condition. Crucially, the *RL* treatment may not reduce perceived pivotality strongly enough. Instead, it may shift the perceived locus of responsibility. FMs may feel less responsible for the total outcome while simultaneously recognizing that purchasing more themselves limits what SMs can consume. This form of social interdependence could be weaker in larger or more anonymous markets, where individual actions are less visible or consequential for others. Consistent with this interpretation, I also find that the stronger participants believe they will be replaced, the more they consume.

From a policy perspective, this paper yields three key insights. First, reducing individuals' pivotality in consumption decisions can lower support for CO_2 pricing, particularly when revenues are allocated to the general state budget. This suggests that public support for CO_2 pricing in real-world settings, such as meat consumption or air travel, may also depend on the degree of pivotality and consumption interdependence. Crucially, I find that this reduction in support disappears when revenues are earmarked, either for redistribution or environmental projects, reinforcing the importance of transparent and targeted revenue use. Second, individuals tend to oppose CO_2 pricing when they believe that it reduces others' consumption—likely because it diminishes the perceived fairness of outcome distribution. This reveals a central tension in CO_2 pricing policy: while effectiveness at reducing emissions is desirable from an environmental standpoint, it may reduce public support due to its perceived social consequences.

An additional implication concerns the relationship between externalities and policy support. In my setting, reduced pivotality increases aggregate market-level externalities while simultaneously lowering support for CO_2 pricing. This creates a tension from a policy perspective: the very conditions that increase the need for regulation may also weaken political support for it. Clear communication about both the environmental benefits and fairness implications of CO_2 pricing may therefore be essential for building public support. In addition, policymakers should consider how perceived responsibility for aggregate outcomes and the interdependence of consumption choices shape individuals' attitudes toward regulation.

This study also faces several limitations. First, SM behavior does not provide a causal estimate, as it is contingent on FM decisions, even though it appears important for understanding the aggregate market-level consumption effect. While fairness concerns seem to limit FMs from engaging in the *RL* and increasing consumption, SMs increase consumption in ways that the current experimental design cannot fully explain. Second, the *RL* treatment is cognitively

more demanding than the *Control* condition, which may generate differences in task understanding. Although comprehension was carefully monitored, future research could aim to further simplify the presentation of the *RL* while maintaining the key features of reduced pivotality. Third, voting behavior in real-world democratic contexts is typically more complex than in an experimental setting. While the design in this paper uses an incentivized voting mechanism, observed choices may reflect not only policy preferences but also strategic considerations.

Moreover, real-world markets typically involve many more consumers, making individual consumption decisions even less pivotal to aggregate outcomes. Future research should therefore explore designs that better reflect such environments in representative samples. A stronger reduction in perceived pivotality may lead individuals to engage more strongly in the *RL*, potentially increasing aggregate consumption further and affecting support for CO_2 pricing. Such changes could also have more pronounced effects on the perceived effectiveness of a CO_2 price. As a result, support might shift not only for the state budget option but also for earmarked revenue schemes.

While the proposed explanation—fairness concerns and distributional considerations—offers a coherent interpretation of the results, it does not fully account for the observed effect of the *RL* treatment on support for CO_2 pricing. In particular, the elicited descriptive norms explain only part of the variation, suggesting that other social or distributional preferences may be at play. Future research should therefore more directly elicit these preferences to better understand their contribution to policy support. Moreover, implementing reduced pivotality in more complex environments (e.g., online experiments with representative samples or field settings) remains methodologically challenging. Thus, further work is needed to develop and validate designs beyond controlled laboratory settings to better understand how reduced pivotality shapes support for policy interventions.

Chapter 3

It Matters Whom You Regulate: The Role of Regulation Targets and Decision Rights for Policy Preferences

3.1 Introduction

A fundamental challenge in policymaking is that regulations impose outcomes on both supporters and opponents alike. Yet, little is known about whether this matters for individual support of policies. Do people take others' preferences into account when deciding whether to regulate, or do they simply want their preferred outcome enforced universally? Behavioral economics has established that, contrary to the standard neoclassical framework, individuals are not purely self-interested but also care about others' outcomes (for a review see e.g., Fehr and Charness, 2025). We therefore study how such other-regarding concerns — particularly autonomy and preference misalignment—shape the willingness to regulate others, especially when the satisfaction of one's own preferences is also at stake. To isolate these mechanisms, we implement an incentivized survey experiment that randomly varies the regulation target: oneself, others, or both as a group.

This chapter is joint work with Alexander M. Danzer. We thank Gonzalo Arrieta, Alexander W. Cappelen, Mathias Ekström, Bertil Tungodden, Holger Herz and several other seminar and conference participants for very helpful suggestions and comments. Holzmann thanks FAIR at NHH, Norway, for their hospitality, and the BGPE for supporting his research stay at NHH. This project was pre-registered under RCT ID AEARCTR-0015267. The pre-analysis plan is in Appendix F.3.

Governments have multiple tools for regulating collective behavior, which can be imposed on different groups and may or may not include oneself, like taxes, for instance. While opposition to such policies can arise from factors such as mistrust in government or concerns about efficiency and revenue use, we focus on a less-studied source of opposition: concerns for those subject to the regulation. Some individuals may be willing to contribute to a public good through taxes yet reject imposing the same obligation on others. Conversely, others may oppose paying personally while supporting imposition on their peers. These asymmetries suggest that policy support is shaped not only by self-interest but also by how individuals weigh their own preferences against others' autonomy and welfare. Because broad public support is crucial for successful policy implementation, understanding whether opposition stems from self-interest or genuine concern for others is essential and has direct implications for how policies are designed and communicated.

Our study focuses on a highly contested policy domain: environmental regulation and taxation. This setting is particularly well-suited to our research question because support for environmental policies is known to be shaped not only by concerns about policy effectiveness, but also by self-interest and social inequality (Dechezleprêtre et al., 2025)—the very forces our experiment is designed to disentangle. More generally, Stantcheva (2021) shows that social preferences strongly correlate with policy views, while Ambuehl et al. (2021) and Bartling et al. (2023) highlight the importance of freedom of choice in the context of paternalistic interventions. Yet, despite this growing body of evidence, causal identification of how individuals trade off self-interest, welfare concerns for others, and decision autonomy when forming support for environmental policies remains limited. We provide such evidence.

We run a two-wave incentivized survey experiment with a representative sample of the German population. In wave 1, participants act as choice architects (CAs) who regulate an allocation task carried out by decision-makers (DMs) in wave 2. DMs allocate €2 between a personal payment and a donation to reduce carbon emissions, capturing a central feature of environmental regulation: the trade-off between private monetary benefits and reduced emissions. CAs are randomly assigned to one of three regulation targets: (i) their own future decision (treatment *Self*), (ii) another participant's decision (treatment *Other*), or (iii) both their own and another participant's decisions (treatment *Group*). In each case, CAs decide whether to impose their preferred allocation or allow the respective DM(s) to choose freely. Critically, these regulation choices are implemented in wave 2, ensuring that all decisions carry real consequences.

We base our predictions on a simple conceptual framework built around three core concepts. First, individuals gain utility from satisfying their current preferences. Second, they anticipate potential costs from preference misalignment: their imposed allocation may differ from their own future preferences (intrapersonal misalignment) or from others' preferences

(interpersonal misalignment). Third, individuals may perceive a cost of restricting freedom of choice, both for their future self and for others. Together, these mechanisms generate predictions across treatments. Regulation should be least prevalent in *Other*, where own preference satisfaction is absent but concerns for others' preferences and autonomy remain. It should be most prevalent in *Self*, where own preference satisfaction is at stake but concerns about others are absent. *Group* falls in between: the presence of own preference satisfaction pushes toward more regulation relative to *Other*, while concerns for others' preferences and autonomy push toward less regulation relative to *Self*. To test these predictions empirically, we elicit preferred personal payments for oneself and others, along with beliefs about future preferred payments to capture potential misalignment, and employ a measure of the non-monetary, instrumental value of decision rights—for both oneself and others—to capture autonomy preferences.

The results confirm our predictions, revealing a clear ranking across treatments: regulation is most prevalent in *Self*, intermediate in *Group*, and least prevalent in *Other*. CAs are significantly more likely to regulate when only their own outcome is affected (*Self*) than when both their own and another person's outcome are affected (*Group*). They also regulate significantly more in *Group* than when only another person's outcome is at stake (*Other*). Together, these patterns show that individuals genuinely trade off their own and others' interests when deciding whether to regulate: the presence of own preference satisfaction increases willingness to regulate (*Self* vs. *Group*), while removing personal stakes and leaving only concerns for others, substantially reduces it (*Other* vs. *Group*).

Testing our conceptual framework, we find that a higher preferred payoff increases the willingness to regulate; that intrapersonal misalignment reduces it while interpersonal does not; and that stronger autonomy preferences, especially regarding others, further reduce regulation. This suggests that when regulating others, individuals are less concerned about imposing outcomes others may dislike and more concerned about restricting their autonomy. Testing the implications of the conceptual framework within each treatment confirms these patterns: intrapersonal misalignment is the relevant friction in *Self*, whereas autonomy considerations dominate in *Other*. Qualitative evidence on the motives behind regulation choices reinforces this picture. The most important reasons not to regulate, ranked by stated importance, are valuing others' freedom of choice, believing that the decision should rest with the DM, and viewing regulation as generally inappropriate, while uncertainty about the DM's preferences ranks last. Taken together, these findings consistently point to the same conclusion: when deciding whether to regulate others, autonomy concerns outweigh misalignment considerations by a substantial margin.

This paper contributes to three strands of literature. First, it adds to the growing literature on paternalism and decision rights. Prior work shows that individuals project their own

preferences onto others when restricting their choice sets (Ambuehl et al., 2021), and that people are willing to regulate others' choices but prefer soft interventions (e.g. nudges) over hard ones (e.g. taxes or bans) (Bartling et al., 2023). Other studies examine paternalistic actions in group decisions (Doerrenberg et al., 2025), prosocial contexts (Ackfeld and Ockenfels, 2021), or analyze the supply and demand for paternalistic policies (Bartling and Srinivasan, 2025). We extend this literature in a novel direction: we study a setting in which the decision to regulate others is intertwined with the satisfaction of one's own preferences; a feature that characterizes many real-world policies yet has received little experimental attention. Our results show that opposition to paternalistic interventions is significantly lower when own preference satisfaction is at stake, highlighting self-interest as an important and previously underappreciated driver of support for paternalistic regulation.

We also contribute to research on the value of decision rights in contexts where individuals make choices for others. Bartling et al. (2014) establish that individuals attach intrinsic value to holding decision rights beyond purely material considerations, while Pikulina and Tergiman (2020) show that individuals are willing to pay to determine others' outcomes. Recent work develops incentivized measures of the intrinsic value of decision rights and documents their prevalence across countries (Freundt et al., 2023; Freundt and Herz, 2025). We apply such a measure to our setting and show that the value individuals attach to decision rights, both for themselves and for others, is a key determinant of regulatory behavior. Crucially, concern for others' decision rights emerges as the dominant force restraining regulation, underscoring the importance of autonomy preferences for understanding opposition to paternalistic policies.

Second, we add to research on the willingness to regulate environmentally relevant behaviors and its determinants. Existing work highlights distributional concerns, self-interest, and perceived policy effectiveness as key drivers of support for environmental policies (Kallbekken and Sælen, 2011; Dal Bó et al., 2018; Maestre-Andrés et al., 2019; Umit and Schaffer, 2020; Ewald et al., 2022; Dechezleprêtre et al., 2025). We complement this literature by providing causal evidence that concerns about others' autonomy—distinct from and beyond purely distributional considerations—are an important additional driver of policy opposition. We also contribute to the smaller experimental literature studying the implementation of environmental policies for oneself and others (Ockenfels et al., 2020; Kölle et al., 2024; Woerner et al., 2024), by providing the first systematic evidence on how varying the regulation target shapes the willingness to regulate.

More generally, this paper contributes to the literature on social preferences (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Charness and Rabin, 2002; Andreoni and Miller, 2002; Cappelen et al., 2007, 2013; Almås et al., 2020, 2025; Fehr and Charness, 2025). We study how individuals trade off self-interest against concern for others in a consequential regulatory

setting. Our results reveal a striking asymmetry: when only others are affected, resistance to regulation is strongest, driven by concern for their autonomy. Yet once own preference satisfaction is also at stake, individuals become substantially more willing to impose regulation on others. This suggests that self-interest and other-regarding concerns interact in ways that have important implications for the political economy of regulation more broadly.

The remainder of the paper is structured as follows. Section 3.2 presents the experimental design. Section 3.3 develops the conceptual framework and hypotheses. Section 3.4 describes the sample and survey procedures. Section 3.5 outlines the empirical strategy. Section 3.6 presents the results. Section 3.7 concludes.

3.2 Experimental Design

The longitudinal experiment involves two roles: a choice architect (CA) and a decision-maker (DM). The task of the DMs is to divide €2 between a personal payment and a donation reducing CO_2 emissions. CAs decide whether to grant the DM freedom to choose or to impose their own preferred allocation (the regulation choice). The €2 constitute an additional bonus payment, conditional on completing both survey waves. Donations are made to "atmosfair", a non-governmental organization that implements projects aimed at reducing CO_2 emissions, for example, by financing renewable energy initiatives in low- and middle-income countries. Participants are informed that each donated €1 corresponds to a reduction of 40 kg CO_2, ensuring that choices have tangible environmental consequences, and that "atmosfair" is a highly rated organization with respect to project standards and implementation.

To facilitate comprehension of the scale of CO_2 emissions, we first ask participants to estimate the distance a typical car would need to travel to generate one kilogram of CO_2. We then provide the correct reference value—approximately six kilometers on average—before participants proceed to the experimental tasks.

The experimental design exploits the two-wave structure of the survey (see Figure C.1 in the Appendix for an overview). In wave 1, participants act as CAs and make the regulation decision. In wave 2, participants act as DMs and perform the allocation task, conditional on the regulation choices implemented in wave 1.

Prior to treatment assignment, we elicit preferred allocations, beliefs, and autonomy valuations, ensuring that these measures are exogenous to the regulation target. CAs report their preferred allocation for three different DM scenarios, presented in randomized order: (i) themselves as DM, (ii) another randomly chosen participant as DM, and (iii) both themselves and another participant as DMs. CAs are informed that their choices may determine future payments and donations for themselves and others. Importantly, each cent allocated to the donation is

doubled by the experimenter when the allocation applies to either oneself or another participant as DM. For the group allocation, the doubling applies automatically since both group members donate the same amount. This ensures that the maximum donation a CA can affect is identical across all three scenarios.

Next, CAs report beliefs about their own and others' allocation decisions in wave 2. To examine whether CAs attach non-instrumental value to autonomy or incur decision costs when making allocation choices for themselves and others, we include a hypothetical delegation task. Participants consider a situation in which a third party implements their preferred allocation on their behalf, and decide whether to delegate this choice or retain autonomy. Delegation reflects decision costs, while non-delegation indicates a positive value of autonomy. To isolate the non-instrumental value of autonomy from decision costs, we explicitly inform participants that outcomes are identical regardless of whether the choice is delegated or made directly. Based on their choice, CAs report their willingness to pay to either delegate the decision or retain autonomy. Participants then consider an analogous situation for another randomly chosen participant. They state whether that participant should actively make the choice or delegate it to the third party, and report their willingness to pay accordingly. These beliefs and autonomy measures are subsequently used to predict regulation behavior.

We then inform CAs that they can regulate future allocation choices regarding the €2 bonus in wave 2. CAs are randomly assigned to one of three regulation targets. In *Self*, participants regulate their own allocation choice in wave 2 (CA = DM). In *Other*, participants are randomly paired and regulate each other's allocation choices (CA $\neq$ DM). To prevent reciprocity and strategic interdependence, participants in *Other* learn only after submitting their regulation decision that another randomly selected participant serves as their CA.

In *Group*, participants are randomly paired, but only one participant in each pair acts as CA for both group members; the other participant makes no regulation decisions. This ensures that decision power is identical across treatments. CAs in *Group* regulate both their own and the other participant's allocation choice in wave 2. If a CA chooses to regulate, the implemented allocation is binding for both herself and the other participant.

In each treatment, CAs make two regulation decisions: whether to implement their previously elicited preferred allocation regarding (i) themselves as DM *and* (ii) another participant as DM, or instead grant the respective DM freedom of choice. One of these two decisions is randomly selected and implemented, ensuring incentive compatibility throughout.

Decision (i) directly addresses our core research question: to what extent are individuals willing to impose their personal preferences on others or on themselves, relative to groups. Decision (ii) serves as a secondary outcome, allowing us to examine whether the patterns observed for allocations preferred for oneself also hold for allocations preferred for others.

Finally, we elicit a pre-specified set of motives capturing self-regarding and other-regarding concerns underlying regulation choices. Motives are organized into four categories plus an open-ended category, elicited separately for regulating and not regulating. Motives for regulating include: ensuring a fair allocation, taking the burden off the DM, ensuring one's own preferences are satisfied, and preventing the DM from making choices the CA considers wrong. Motives for not regulating include: respecting the DM's freedom of choice, being unsure about the DM's preferences, viewing the choice as the DM's responsibility, and considering regulation inappropriate.[1] For each motive, CAs rate its importance on a 7-point Likert scale ranging from 0 (not important at all) to 6 (very important).[2]

3.3 Conceptual Framework and Hypotheses

3.3.1 Conceptual Framework

Consider the general case of a CA i making a regulation decision in the *Group* treatment. The CA weighs three core concepts when forming her regulation choice:

$$U_i = \underbrace{A_i(x_i, d)}_{\substack{\text{utility personal payoff} \\ \text{and donation}}} - \underbrace{\alpha_i \left| d - b_i^{d_{i,t+1}} \right|}_{\text{intrapersonal MG}} - \underbrace{\beta_i \left| d - b_i^{d_j} \right|}_{\text{interpersonal MG}} - \underbrace{C_i}_{\text{DA: Self}} - \underbrace{v_i^{C_j}}_{\text{DA: Other}}$$

Here, j refers to another DM while $i, t+1$ refers to i's future self as DM. $A_i(x_i, d)$ represents the utility derived from fixing the allocation in wave 1, where x_i is the personal payment and d is i's preferred donation which is simultaneously implemented for j.

The weights α_i and β_i capture the importance of potential misalignment: α_i for deviations from i's beliefs about their own future donation ($b_i^{d_{i,t+1}}$), and β_i for deviations from beliefs about another participant's preferred donation ($b_i^{d_j}$). Both weights capture *expected* deviations: $|d - b_i^{d_{i,t+1}}|$ represents the anticipated intrapersonal misalignment gap (MG), and $|d - b_i^{d_j}|$ the anticipated interpersonal MG. Finally, C_i (Decision Autonomy (DA): Self) represents either decision costs (negative) or the value placed on personal autonomy (positive), while $v_i^{C_j}$ (DA: Other) reflects the value CA i places on another person's autonomy, or their decision costs.

We make the following assumptions. $A_i(x_i, d)$ increases with both x_i (the personal payment) and d (the donation amount). Crucially, our experimental design allows us to treat $A_i(x_i, d)$ as exogenous to the regulation target, since preferred allocations are elicited prior to treatment

[1] We do not impose a normative definition of fairness or incorrect choices. These motives capture participants' subjective perceptions and are analyzed exploratorily.

[2] The questionnaire for the experimental questions is shown in Appendix D.1 and the "atmosfair" certificates are shown in Appendix D.3.

assignment. We assume α_i, $\beta_i > 0$, with $|\alpha_i| > |\beta_i|$, reflecting the assumption that individuals seek to satisfy the preferences of the regulation target but place greater weight on alignment with their own future self than with others. It follows that individuals with higher α_i or β_i face larger expected costs from misalignment and are therefore less likely to regulate.

The assumption $\alpha_i > 0$ aligns with regret theory (Loomes and Sugden, 1982). When CAs believe their future preferences will differ from their current ones ($|d - b_i^{d_{i,t+1}}| > 0$), they anticipate potential ex-post regret from regulating, which reduces utility. Similarly, $\beta_i > 0$ is motivated by theories of guilt aversion (e.g., Battigalli and Dufwenberg, 2007). When CAs believe their preferred donation differs from j's ($|d - b_i^{d_j}| > 0$), regulation may induce guilt by failing to meet j's expectations, thereby lowering i's utility. This formulation implicitly assumes that CAs believe DMs act in line with their own preferences.

For simplicity, we assume that misalignment gaps enter utility in absolute terms, such that deviations above and below beliefs are equally costly. We additionally assume $|d - b_i^{d_{i,t+1}}| < |d - b_i^{d_j}|$, reflecting that i's future preferences are on average closer to her current preferred donation than her beliefs about j's preferred donation. This is plausible since i typically has more information about herself, through past behavior and experience, than about j.[3]

Finally, there is evidence that individuals intrinsically value autonomy ($C_i > 0$) beyond the utility derived from the choice outcomes (see, e.g., Fehr et al., 2013; Bartling et al., 2014). While direct evidence on the value individuals attach to others' autonomy ($v_i^{C_j}$) is more limited, it is plausible that those who value personal autonomy also value others' autonomy ($v_i^{C_j} > 0$), albeit to a lesser extent ($C_i > v_i^{C_j}$). We incorporate this term explicitly, as concern for others' autonomy may help to explain regulation choices and has received comparatively little attention in the literature.

In sum, regulating yields positive utility through $A_i(x_i, d)$, while potential intrapersonal and interpersonal misalignment gaps and the suppression of choice autonomy generate disutility. CAs consider only outcomes they can directly affect through their regulation choice. Disutility from preference misalignment and autonomy suppression, as well as the utility from satisfying current preferences via $A_i(x_i, d)$, are realized only if a CA chooses to regulate. We therefore normalize the utility of non-regulation to zero, so that CA i regulates if and only if $U_i > 0$.

We define the utility functions for each treatment as follows:

[3] This assumption can also be justified by theories of projection bias or egocentric bias.

$$\textbf{Self:} \quad U_i = A_i(x_i, d) - \alpha_i |d - b_i^{d_{i,t+1}}| - C_i \tag{3.1}$$

$$\textbf{Other:} \quad U_i = A_i(0, d) - \beta_i |d - b_i^{d_j}| - v_i^{C_j} \tag{3.2}$$

$$\textbf{Group:} \quad U_i = A_i(x_i, d) - \alpha_i |d - b_i^{d_{i,t+1}}| - \beta_i |d - b_i^{d_j}| - C_i - v_i^{C_j} \tag{3.3}$$

3.3.2 Hypotheses

Based on the conceptual framework, we derive hypotheses about differences in the willingness to regulate across regulation targets, driven primarily by misalignment beliefs and autonomy valuations. These predictions concern our primary outcome: how the willingness to enforce one's preferred allocation ("Preferred Payoff (Self)") varies with the regulation target. We further examine whether these patterns extend to the enforcement of allocations preferred for others ("Preferred Payoff (Other)").

The willingness to implement one's preferred allocation for oneself should be higher in *Self* and *Group* than in *Other*, since own preference satisfaction is at stake in the former two treatments but absent in the latter. The willingness to implement the allocation preferred for others should exhibit a qualitatively similar pattern but be lower overall, and may additionally depend on the gap between the allocations preferred for oneself and others. Accordingly, d in the former case reflects preferences when CA = DM, whereas for the latter allocation it reflects preferences when CA $\neq$ DM.

Hypothesis 1 (H1)

Individuals are less likely to regulate in *Group* than in *Self*.

Relative to *Self*, regulation in *Group* additionally affects another participant j. This introduces two sources of disutility absent in *Self*: potential misalignment with j's preferences ($\beta_i > 0$) and the suppression of j's autonomy. Both reduce the net utility from regulating, making regulation less likely in *Group*. This follows directly from comparing Equations (3.1) and (3.3).

Hypothesis 2 (H2)

Individuals are more likely to regulate in *Group* than in *Other*.

Relative to *Other*, regulation in *Group* additionally affects the CA's own payoff in wave 2 ($t+1$). The net effect is positive: the direct utility gain from implementing one's preferred allocation,

$A_i(x_i, d) > A_i(0, d)$, outweighs the associated costs of potential intrapersonal misalignment ($\alpha_i > 0$) and reduced personal autonomy ($C_i > 0$). This dominance is supported by the expectation that intrapersonal misalignment is small in magnitude—that is, $|d - b_i^{d_{i,t+1}}|$ takes low values—and that autonomy costs are unlikely to offset the utility from implementing one's preferred outcome. We therefore predict that the preference satisfaction channel dominates, making regulation more likely in *Group* than in *Other*. This follows from comparing Equations (3.2) and (3.3).

Hypothesis 3 (H3)

Individuals are more likely to regulate in *Self* than in *Other*.

This hypothesis follows directly from H1 and H2. As in H2, the utility gain from satisfying current preferences outweighs the cost of potential misalignment and autonomy suppression—now applied to the *Self* rather than the *Group*. Additionally, intrapersonal misalignment is likely smaller in magnitude than interpersonal misalignment, since current and future self-preferences tend to be closer than one's preferences and those of others $|d - b_i^{d_{i,t+1}}| < |d - b_i^{d_j}|$, further reinforcing this dominance. Together, these forces predict the highest prevalence of regulation in *Self*.

Hypothesis 4 (H4)

The larger the misalignment gaps $|d - b_i^{d_{i,t+1}}|$ and $|d - b_i^{d_j}|$, the less likely individuals are to regulate.

This hypothesis tests whether preference misalignment—both intrapersonal and interpersonal—reduces the utility from regulating and therefore lowers the willingness to do so, as assumed in the conceptual framework.

Hypothesis 5 (H5)

Higher valuations of autonomy C_i and $v_i^{C_j}$ reduce the likelihood of regulation.

This hypothesis tests whether stronger autonomy preferences—over one's own future choices and over others' choices—translate into lower willingness to regulate, consistent with the framework's assumption that autonomy suppression carries a utility cost.

3.4 Data

We conducted a survey with 2,414 individuals drawn to be representative of the adult German population with respect to age, gender, education, and residential federal state. The study is titled "Preferences for Environmental and Sustainable Policies and the Replacement Excuse: An Experimental Investigation" (PRESPARE). To ensure data quality, participants were required to pass an attention check before taking part in the survey; those who failed were excluded.[4] Data collection was carried out between January 27 and February 7, 2025 by the professional survey provider Bilendi, whose panelists received standard compensation for participation. A total of 539 participants remained passive as DMs in the *Group* treatment and were therefore excluded from the analysis, leaving a final analytic sample of 1,875 individuals. The median completion time was approximately 23 minutes. All participants were re-invited for a second survey wave, fielded from February 12 to February 21, 2025, which achieved an 80% response rate. To confirm that the experiment carried real consequences, the average individual payoff (including passive participants) was €1.16 and total donations resulted in a reduction of 111.32 tons of CO_2; participants also received a link to inspect the donation certificate issued by "atmosfair".

The survey instrument proceeds in three parts. Participants first provide detailed demographic and socio-economic information, including age, gender, education, employment status, household composition, and federal state of residence. This is followed by the survey experiment itself, for which participants could proceed only after answering a comprehension question correctly. Finally, respondents complete an extensive post-experimental questionnaire covering voting intentions, beliefs about climate change and its human causation, perceived severity of and personal exposure to climate-related impacts, and attitudes towards climate policies and governmental regulation. It also measures individual risk preferences, time preferences, and locus of control using established items from the German Socio-Economic Panel. Because these variables were elicited after treatment, we exclude them from the main analysis to avoid post-treatment bias.

Panel A of Table 3.1 presents summary statistics for demographic and socio-economic characteristics. Columns (1)–(4) report means and standard deviations for the full sample and the three experimental groups (*Group, Self, Other*), while columns (5)–(7) show mean differences between treatment groups with standard errors from t-tests in parentheses. On average, individuals in the sample are 49 years old; half are female, around 22% hold at least a tertiary education, 44% work full-time, and 27% have minor children in the household. No statistically

[4]Table C.1 in the Appendix compares the sample with official Eurostat statistics and confirms close alignment along these and additional dimensions such as income and employment status. Appendix E contains detailed survey and data documentation, and Appendix D contains the full questionnaire.

significant differences in baseline characteristics are found across treatment groups, confirming successful randomization.

3.5 Empirical Strategy

This section describes our approach to estimating treatment effects, testing the mechanisms underlying our conceptual framework, and ensuring comparability across experimental variables.

We construct binary outcome variables (Regulation: Preferred Payoff (Self) and Regulation: Preferred Payoff (Other)), where $Y_i = 1$ if the CA chooses to regulate in the respective case and $Y_i = 0$ otherwise. All relevant outcomes are measured in the first survey wave.

We begin by comparing raw mean differences across treatment groups and conducting t-tests for each outcome. We then test hypotheses H1, H2, and H3 by estimating the following OLS specification:

$$Y_i = \gamma_0 + \gamma_1 Self_i + \gamma_2 Other_i + X_i'\gamma_3 + \epsilon_i \tag{3.4}$$

where i indexes an individual and Y_i is the respective outcome of interest. γ_1 measures the average treatment effect (ATE) of *Self* and γ_2 the average treatment effect (ATE) of *Other*, both relative to the reference group *Group*. The vector X_i' includes age, a female dummy, a dummy for having at least a tertiary degree, a dummy for full-time employment, a dummy for having minor children, and the number of minor children in the household.[5] Standard errors are robust to heteroskedasticity throughout, and the error term ϵ_i captures unobserved determinants of the outcome.

To test H4 and H5, we augment Equation (3.4) with misalignment gaps (MGs), autonomy valuations, and preferred payoffs. MGs are defined as the absolute difference between a preferred payoff and the corresponding belief about future payoffs. We distinguish between two types.

Intrapersonal MGs capture anticipated misalignment with one's own future preferences:

Interpersonal MGs capture anticipated misalignment with another participant's preferences:

We also include the valuation of personal autonomy and the autonomy of others (DA: Self, Other) and the size of the Preferred Payoffs (Self, Other).[6] For all hypotheses, we additionally

[5] The pre-analysis plan defined a larger set of controls, including personal monthly net income, voting intentions, views about climate change, and economic preferences such as risk preferences, time preferences, and locus of control. Because these variables were elicited post-treatment, we exclude them from the analysis. Results are robust to including these variables and are available upon request.

[6] We do not include donation amounts, as the fixed €2 allocation implies a one-to-one trade-off between private payment and donation, which would result in perfect multicollinearity. Therefore, we also do not report preferred donations in the summary statistics.

Table 3.1: Summary Statistics and Balancing Table

	Panel A: Controls						
	Mean and Standard Deviation				Differences Treatment Groups		
	(1) Full sample	(2) Group	(3) Self	(4) Other	(5) Group - Self	(6) Group - Other	(7) Self - Other
Age	49.05	49.46	49.00	48.71	0.46	0.75	0.29
	(15.34)	(15.16)	(15.56)	(15.29)	(0.87)	(0.86)	(0.87)
Female	0.50	0.48	0.51	0.52	-0.03	-0.04	-0.01
	(0.50)	(0.50)	(0.50)	(0.50)	(0.03)	(0.03)	(0.03)
Tertiary Education	0.22	0.23	0.22	0.22	0.00	0.01	0.00
	(0.42)	(0.42)	(0.42)	(0.41)	(0.02)	(0.02)	(0.02)
Full Time	0.44	0.44	0.45	0.44	-0.00	-0.00	0.00
	(0.50)	(0.50)	(0.50)	(0.50)	(0.03)	(0.03)	(0.03)
Minor Children	0.27	0.25	0.28	0.27	-0.03	-0.02	0.01
	(0.44)	(0.44)	(0.45)	(0.45)	(0.03)	(0.02)	(0.03)
Minor Children in Household	0.44	0.41	0.45	0.46	-0.04	-0.05	-0.01
	(0.83)	(0.80)	(0.84)	(0.84)	(0.05)	(0.05)	(0.05)
Errors	0.73	0.71	0.75	0.74	-0.04	-0.03	0.01
	(0.92)	(0.91)	(0.92)	(0.94)	(0.05)	(0.05)	(0.05)
Attention Passed	0.81	0.82	0.83	0.79	-0.00	0.04*	0.04*
	(0.39)	(0.38)	(0.38)	(0.41)	(0.02)	(0.02)	(0.02)
Duration (Min.)	31.68	31.71	28.79	34.54	2.91*	-2.83	-5.74**
	(45.54)	(32.00)	(27.95)	(66.31)	(1.70)	(2.95)	(2.87)
	Panel B: Experimental Variables						
Preferred Payoff (Self)	1.08	1.08	1.08	1.07	-0.00	0.01	0.01
	(0.63)	(0.64)	(0.63)	(0.63)	(0.04)	(0.04)	(0.04)
Preferred Payoff (Other)	1.02	1.02	1.01	1.04	0.01	-0.02	-0.03
	(0.60)	(0.61)	(0.60)	(0.60)	(0.03)	(0.03)	(0.03)
Preferred Payoff (Group)	1.08	1.07	1.07	1.08	0.01	-0.01	-0.01
	(0.58)	(0.58)	(0.59)	(0.59)	(0.03)	(0.03)	(0.03)
Belief Payoff (Self)	1.10	1.12	1.08	1.11	0.04	0.01	-0.03
	(0.60)	(0.60)	(0.60)	(0.60)	(0.03)	(0.03)	(0.03)
Belief Payoff (Other)	1.12	1.13	1.09	1.14	0.04	-0.02	-0.06**
	(0.48)	(0.46)	(0.48)	(0.50)	(0.03)	(0.03)	(0.03)
MG: Self vs. Self	0.21	0.22	0.20	0.22	0.02	0.00	-0.02
	(0.38)	(0.40)	(0.36)	(0.38)	(0.02)	(0.02)	(0.02)
MG: Self vs. Other	0.41	0.40	0.41	0.43	-0.00	-0.03	-0.02
	(0.44)	(0.45)	(0.43)	(0.45)	(0.02)	(0.03)	(0.02)
MG: Other vs. Self	0.30	0.32	0.30	0.29	0.02	0.02	0.00
	(0.46)	(0.48)	(0.46)	(0.44)	(0.03)	(0.03)	(0.03)
MG: Other vs. Other	0.39	0.40	0.38	0.40	0.01	-0.00	-0.02
	(0.44)	(0.45)	(0.42)	(0.45)	(0.02)	(0.03)	(0.02)
DA: Self (WTP)	0.41	0.40	0.37	0.45	0.02	-0.06	-0.08
	(1.01)	(1.00)	(1.03)	(1.00)	(0.06)	(0.06)	(0.06)
DA: Other (WTP)	0.23	0.21	0.23	0.26	-0.02	-0.06	-0.03
	(0.98)	(0.99)	(0.98)	(0.98)	(0.06)	(0.06)	(0.06)
DA: Self	0.72	0.72	0.70	0.74	0.02	-0.01	-0.03
	(0.45)	(0.45)	(0.46)	(0.44)	(0.03)	(0.03)	(0.03)
DA: Other	0.65	0.65	0.65	0.65	-0.00	-0.00	0.00
	(0.48)	(0.48)	(0.48)	(0.48)	(0.03)	(0.03)	(0.03)
	Panel C: Outcomes						
Regulation: Preferred Payoff (Self)	0.48	0.47	0.57	0.41	-0.10***	0.07**	0.16***
	(0.50)	(0.50)	(0.50)	(0.49)	(0.03)	(0.03)	(0.03)
Regulation: Preferred Payoff (Other)	0.42	0.40	0.49	0.38	-0.08***	0.02	0.10***
	(0.49)	(0.49)	(0.50)	(0.49)	(0.03)	(0.03)	(0.03)
Observations	1875	620	628	627	1248	1247	1255

Notes: This table reports summary statistics and differences in means across the three experimental treatment groups (*Group*, *Self*, and *Other*). Columns (1)–(4) present sample means, with standard deviations in parentheses. Columns (5)–(7) report pairwise differences in means between treatments, with standard errors from t-tests in parentheses. Panel A summarizes demographic characteristics including age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Panel B reports key experimental variables. Preferred Payoff (Self, Other, Group) denotes the amount allocated to the respective DMs pre-treatment. Belief Payoff (Self, Other) captures expectations about future payoff choices of the respective DMs. Misalignment gaps (MG) are defined as the absolute differences between preferred payoffs and corresponding beliefs where the first term refers to the preferred payoff and the second to the belief (intrapersonal: MG: Self vs. Self and Other vs. Self; interpersonal: MG: Other vs. Other and Self vs. Other). Decision Autonomy (DA) measures (DA: Self, Other (WTP)) capture the valuation of decision rights for oneself and for another participant in €. Panel C reports binary outcomes equal to 1 if a CA chooses to regulate by implementing the respective Preferred Payoff (Self, Other) and 0 otherwise. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Variable	Definition
MG: Self vs. Self	| Preferred Payoff (Self) − Belief Payoff (Self) |
MG: Other vs. Self	| Preferred Payoff (Other) − Belief Payoff (Self) |

Variable	Definition
MG: Self vs. Other	| Preferred Payoff (Self) − Belief Payoff (Other) |
MG: Other vs. Other	| Preferred Payoff (Other) − Belief Payoff (Other) |

report results corrected for multiple hypothesis testing using the Romano–Wolf procedure.

To test our conceptual framework more directly, we partition the sample by treatment and estimate OLS models that include only the variables relevant for each treatment according to the framework, alongside the same controls as above. This approach allows us to assess the mechanisms underlying our theoretical predictions for each treatment separately, and avoids the restrictions imposed by pooling with interaction terms.

To ensure comparability across experimental variables, all continuous experimental covariates are standardized to have mean zero and standard deviation one in all specifications.

3.6 Results

3.6.1 Main

The results confirm our theoretical predictions: the regulation target systematically shapes individuals' willingness to regulate, with support for regulation declining as the focus shifts from oneself to others.

Panel B of Table 3.1 summarizes the main experimental variables for the full sample and across the three treatment groups. Of the 39 pairwise tests, only one is statistically significant at the 5% level—*Belief Payoff (Other)* between *Self* and *Other*—indicating no systematic differences across treatment groups. Turning to preferred payoffs, participants select higher payoffs for themselves and the group (1.08 in both cases) than for others (1.02), a difference that is statistically significant ($p < 0.01$).

Regarding misalignment gaps, participants expect to choose a payoff of 1.10 for themselves and believe others will choose 1.12 in wave 2. Table 3.2 summarizes the four MGs:

Intrapersonal misalignment is consistently smaller than interpersonal misalignment, confirming that current and future self-preferences are closer than one's own preferences and those of others. This supports the condition $|d - b_i^{d_{i,t+1}}| < |d - b_i^{d_j}|$ assumed in the conceptual framework.

Table 3.2: Misalignment Gaps

Misalignment Gap	**Type**	**Mean**
MG: Self vs. Self	Intrapersonal	0.21
MG: Other vs. Self	Intrapersonal	0.30
MG: Self vs. Other	Interpersonal	0.41
MG: Other vs. Other	Interpersonal	0.39

The next set of variables captures the non-instrumental value (or cost) of decision autonomy—that is, the value individuals attach to holding decision rights independently of any material consequences—elicited through hypothetical delegation choices. The willingness to pay (WTP) for autonomy, ranging from -2 to 2 where positive (negative) values indicate a preference for retaining (delegating) decision rights, is higher for own autonomy (0.41) than for others' autonomy (0.23). Seventy-two percent of participants prefer to retain decision-making authority for themselves (DA: Self), and 65% prefer that another randomly chosen participant retains their own decision rights (DA: Other). These patterns confirm that autonomy preferences are prevalent and stronger for oneself than for others, consistent with the assumption that $C_i > v_i^{C_j}$ and $C_i > 0$, $v_i^{C_j} > 0$.

Panel C of Table 3.1 reports summary statistics and t-test results for the main outcomes of interest. Across treatments, 48% of participants enforce their Preferred Payoff (Self), and 42% do so for the Preferred Payoff (Other), indicating that individuals are more willing to enforce allocations preferred for themselves than those preferred for others. Turning to treatment differences, we focus first on the primary outcome, Regulation: Preferred Payoff (Self). CAs in *Group* are 10 percentage points less likely to regulate than those in *Self* ($p < 0.01$), supporting H1, and 7 percentage points more likely to regulate than those in *Other* ($p < 0.05$), supporting H2. Consequently, CAs in *Self* are 16 percentage points more likely to regulate than those in *Other* ($p < 0.01$), supporting H3. For the secondary outcomes, Regulation: Preferred Payoff (Other), the pattern partially replicates: the *Group–Self* gap of 8 percentage points supports H1 ($p < 0.01$), and the *Self–Other* gap of 10 percentage points supports H3 ($p < 0.01$). The *Group–Other* difference, however, is not statistically significant and does not support H2 for this outcome.

Table 3.3 reports regression results for treatment effects (H1-H3) and evidence on variables from the conceptual framework (H4 and H5), with all specifications including the controls described in Section 3.5. In both panels, column (1) confirms the treatment effects from the raw comparisons: estimates remain similar in magnitude and statistical significance once controls are included. A Wald test confirms that the coefficients on *Self* and *Other* differ significantly

from each other at the 1% level. Results in Panel A support H1-H3, while Panel B supports H1 and H3.

All results are robust to corrections for multiple hypothesis testing using the Romano–Wolf method (see Table C.3 in the Appendix), as well as to controlling for the number of errors in the comprehension questions, survey completion time, and whether respondents passed a second attention check (see Tables C.4 and C.5 in the Appendix).

The results establish that the regulation target matters decisively for the willingness to regulate. Consistent with prior evidence on paternalistic preferences (see, e.g., Ambuehl et al., 2021), individuals are more willing to regulate their own choices than to impose their preferences on others. Support for regulation is highest in *Self*, declines when another individual is additionally affected (*Group*), and declines further when own payoffs are absent (*Other*), supporting H1, H2, and H3 for the primary outcome.

The results also reveal a novel pattern: the gap between regulating oneself and regulating others narrows once both outcomes are jointly affected, suggesting that individuals genuinely trade off their own and others' preferences when both are under their control. This pattern does not extend to the secondary outcome—implementing preferred payoffs for others—where personal exposure to the consequences of regulation plays no decisive role in shaping CAs' choices in *Group* relative to *Other*.

3.6.2 Mechanisms

We now turn to the mechanisms underlying regulation decisions and examine the predictions of the conceptual framework (H4 and H5). The analysis proceeds in two steps: we first discuss results for the full sample, then assess whether the underlying assumptions hold within each treatment group separately.

Full-Sample Results. Column (2) introduces the size of the respective preferred payoff in Panels A and B. The preferred payoff is positively and statistically significantly associated with the likelihood of regulation ($p < 0.01$), with comparable magnitudes across outcomes (3.6 and 3.7 percentage point increase, respectively). This is consistent with the conceptual framework: higher personal payoffs increase the propensity to regulate, even at the cost of lower donations. Importantly, this does not imply that donations reduce the likelihood of regulation—the total monetary amount is fixed at €2. Rather, CAs on average place more weight on private payoffs when deciding whether to regulate.

Column (3) adds the two relevant MGs in both panels. Intrapersonal misalignment—captured by MG: Self vs. Self and MG: Other vs. Self—is negative and statistically significant at the 1% level

Table 3.3: Regulation Choices

	Panel A. Regulation: Preferred Payoff (Self)			
	(1)	(2)	(3)	(4)
Self	0.099***	0.099***	0.097***	0.097***
	(0.028)	(0.028)	(0.028)	(0.028)
Other	-0.064**	-0.063**	-0.065**	-0.061**
	(0.028)	(0.028)	(0.028)	(0.028)
Preferred Payoff (Self)		0.036***	0.036***	0.036***
		(0.011)	(0.011)	(0.011)
MG: Self vs. Self			-0.039***	-0.041***
			(0.012)	(0.012)
MG: Self vs. Other			0.022*	0.025**
			(0.012)	(0.012)
DA: Self (WTP)				-0.026**
				(0.012)
DA: Other (WTP)				-0.045***
				(0.012)
Control Mean	0.471	0.471	0.471	0.471
Wald Test (F-Stat)	34.23***	34.10***	33.84***	32.86***
Obs.	1,875	1,875	1,875	1,875
Adj. R-Sq	0.019	0.024	0.028	0.041
Controls	X	X	X	X

	Panel B. Regulation: Preferred Payoff (Other)			
	(1)	(2)	(3)	(4)
Self	0.084***	0.085***	0.081***	0.081***
	(0.028)	(0.028)	(0.028)	(0.028)
Other	-0.019	-0.020	-0.024	-0.021
	(0.028)	(0.027)	(0.027)	(0.027)
Preferred Payoff (Other)		0.037***	0.025**	0.024**
		(0.011)	(0.011)	(0.011)
MG: Other vs. Self			-0.077***	-0.076***
			(0.011)	(0.011)
MG: Other vs. Other			0.008	0.007
			(0.012)	(0.012)
DA: Self (WTP)				-0.020*
				(0.012)
DA: Other (WTP)				-0.034***
				(0.012)
Control Mean	0.403	0.403	0.403	0.403
Wald Test (F-Stat)	13.76***	14.31***	14.61***	14.02***
Obs.	1,875	1,875	1,875	1,875
Adj. R-Sq	0.010	0.015	0.036	0.043
Controls	X	X	X	X

Notes: This table reports the estimated treatment effects of *Self* and *Other* compared to *Group* on implementing one's own preferred payoff (Panel A) and the preferred payoff for others (Panel B). Column (2) additionally includes the size of the preferred payoff (self in Panel A; other in Panel B). Column (3) adds the misalignment gaps (MG), defined as the absolute differences between preferred payoffs and the corresponding beliefs, where the first term refers to the preferred payoff and the second to the belief. Column (4) adds decision autonomy (DA) measures (DA: Self and DA: Other (WTP)), capturing the valuation of decision rights for oneself and for another participant in €. All continuous covariates added in columns (2)–(4) are standardized to have mean 0 and standard deviation 1. The F-statistic reports a Wald test of equality between the *Self* and *Other* treatment effects. All specifications control for age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors are robust to heteroskedasticity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

in both panels, indicating that greater anticipated preference conflict with one's future self reduces the willingness to regulate. The effect is more pronounced in Panel B than in Panel A (-7.7 vs. -3.9 percentage points). In contrast, interpersonal misalignment—captured by MG: Self vs. Other and MG: Other vs. Other—is positive and statistically significant only in Panel A (2.2 percentage points, $p < 0.1$). This pattern partially supports H4. While anticipated intrapersonal misalignment reduces regulation as predicted, the positive sign on interpersonal misalignment is contrary to H4 and suggests a utility *gain* from regulation when others' preferences diverge from one's own ($\beta < 0$). This is a noteworthy deviation from the framework and suggests that some individuals may actually be more inclined to impose their preferences when they perceive others as having different tastes.

We additionally examine whether the direction of the MGs matters (see Table C.6 in the Appendix). Replicating column (3) without the preferred payoff variables and interacting the MGs with indicators for positive differences exceeding zero or five cents, we find that direction matters only for intrapersonal misalignment in Panel A. When the preferred payoff exceeds anticipated future preferences by more than zero or five cents, a larger MG significantly reduces the willingness to regulate. This reveals an asymmetric reluctance to regulate: individuals are particularly unwilling to lock in an outcome for their future self that is more self-serving than they expect to prefer—in effect, committing to take more now than they anticipate wanting later, at the expense of CO_2 donations.

Column (4) incorporates autonomy preferences in both panels. A one standard deviation increase in DA: Other (WTP) reduces the likelihood of regulation by 4.5 percentage points ($p < 0.01$) in Panel A and by 3.4 percentage points ($p < 0.01$) in Panel B, while DA: Self (WTP) reduces it by 2.6 ($p < 0.05$) and 2.0 ($p < 0.1$) percentage points, respectively. Both findings support H5. Notably, the larger coefficient on DA: Other (WTP) relative to DA: Self (WTP) across both panels is a key result: concern for others' autonomy is, on the margin, a stronger predictor of regulation choices than concern for one's own autonomy. Table C.7 in the Appendix replicates these results using dummy-based autonomy measures in place of WTP, and all autonomy dummies are negative and statistically significant in both panels, providing additional evidence consistent with the framework.

Taken together, the full-sample results show that several pre-treatment experimental variables meaningfully predict regulation choices. In Panel A, autonomy preferences for others and intrapersonal misalignment are the strongest predictors, followed by the size of the preferred payoff, valuation of own decision autonomy, and interpersonal misalignment. In Panel B, intrapersonal misalignment is the dominant predictor, followed by DA: Other (WTP), the preferred payoff, and DA: Self (WTP), while interpersonal misalignment is insignificant. Importantly, the treatment effects remain robust to the inclusion of all these variables.

Treatment-Specific Results. Partitioning the sample by treatment allows us to assess the mechanisms more directly, by testing whether the variables predicted to matter within each condition by the conceptual framework do so in practice. Table 3.4 reports the results, with Panels A and B corresponding to the primary and secondary outcomes, columns (1)-(2) to *Self*, columns (3)-(4) to *Other*, and columns (5)-(6) to *Group*. Odd-numbered columns exclude controls; even-numbered columns include them.

Self Treatment. The key result here is that anticipated intrapersonal misalignment dominates autonomy concerns. In Panel A, regulation is negatively associated with both MG: Self vs. Self and DA: Self (WTP) (both $p < 0.05$), consistent with the conceptual framework, and the standardized coefficient on the MG exceeds that on DA, confirming that misalignment is the stronger force. The size of the preferred payoff is positive but not statistically significant. Results are robust to controls. In Panel B, only MG: Other vs. Self is statistically significant and negative ($p < 0.01$), with a magnitude more than twice as large as in Panel A—indicating that anticipated intrapersonal preference conflict is particularly salient when implementing payoffs preferred for others.

Other Treatment. The key result here is that autonomy concerns dominate, with misalignment playing no role. DA: Other (WTP) is negative and statistically significant in both panels ($p < 0.01$), consistent with the conceptual framework. In contrast to the full sample, misalignment with beliefs about others' preferences does not predict regulation behavior in either panel. Results remain robust to controls.

Group Treatment. The results here combine features of both preceding treatments, as the framework predicts. In Panel A, the preferred payoff is the strongest predictor ($p < 0.01$), while MG: Self vs. Self and DA: Other (WTP) are negative and of similar magnitude ($p < 0.1$ for both). In Panel B, only MG: Other vs. Self is statistically significant and negative, with a larger and more precisely estimated effect than in Panel A ($p < 0.01$ vs. $p < 0.1$). DA: Self (WTP) is never statistically significant across either panel. As in the full sample, we additionally examine whether MG direction matters and whether a dummy-based autonomy measure yields further insights (Tables C.8 and C.9 in the Appendix). The directional MG interaction yields only a marginally significant effect in Panel A for the *Group* treatment. Using the alternative autonomy measure, the self-autonomy dummy becomes negative and statistically significant in both panels ($p < 0.05$ in Panel A, $p < 0.05$ in Panel B), while the pattern for *Other* is unchanged and *Self* shows greater precision in Panel B ($p < 0.05$).

In summary, the treatment-specific results confirm and sharpen the full-sample findings. Intrapersonal misalignment is the dominant friction in *Self*, whereas autonomy concerns—particularly regarding others—are the primary restraint in *Other*. The *Group* treatment combines both forces, with the preferred payoff emerging as an additional driver. These patterns

Table 3.4: Regulation Choices by Treatment Groups

	Panel A. Regulation: Preferred Payoff (Self)					
	Self Treatment		Other Treatment		Group Treatment	
	(1)	(2)	(3)	(4)	(5)	(6)
Preferred Payoff (Self)	0.007	0.004			0.053***	0.054***
	(0.019)	(0.020)			(0.020)	(0.020)
MG: Self vs. Self	-0.051**	-0.047**			-0.041**	-0.037*
	(0.021)	(0.021)			(0.019)	(0.020)
DA: Self (WTP)	-0.040**	-0.039**			-0.016	-0.017
	(0.019)	(0.019)			(0.021)	(0.021)
MG: Self vs. Other			-0.004	-0.004	0.022	0.023
			(0.019)	(0.019)	(0.021)	(0.021)
DA: Other (WTP)			-0.071***	-0.067***	-0.040*	-0.036*
			(0.020)	(0.020)	(0.021)	(0.021)
	Panel B. Regulation: Preferred Payoff (Other)					
	Self Treatment		Other Treatment		Group Treatment	
	(1)	(2)	(3)	(4)	(5)	(6)
Preferred Payoff (Other)	0.001	0.000			0.031	0.031
	(0.019)	(0.019)			(0.020)	(0.020)
MG: Other vs. Self	-0.107***	-0.105***			-0.069***	-0.066***
	(0.017)	(0.018)			(0.019)	(0.019)
DA: Self (WTP)	-0.031	-0.032			-0.019	-0.023
	(0.019)	(0.019)			(0.021)	(0.021)
MG: Other vs. Other			-0.016	-0.017	-0.002	0.002
			(0.017)	(0.017)	(0.021)	(0.021)
DA: Other (WTP)			-0.059***	-0.058***	-0.012	-0.005
			(0.020)	(0.020)	(0.021)	(0.021)
Obs.	628	628	627	627	620	620
Controls		X		X		X

Notes: This table reports OLS estimates testing predictions of the conceptual framework. Misalignment gaps (MG) are defined as the absolute differences between preferred payoffs and the corresponding beliefs about future allocation choices, where the first term refers to the preferred payoff and the second to the belief. Decision autonomy (DA) measures (DA: Self and DA: Other (WTP)) capture the valuation of decision rights for oneself and for another participant in €. All continuous experimental covariates are standardized to have mean 0 and standard deviation 1. Columns (1) and (2) correspond to the *Self* treatment and include the relevant preferred payoff, MG, and DA measures. Columns (3) and (4) correspond to the *Other* treatment and include the relevant MG and DA measures. Columns (5) and (6) correspond to the *Group* treatment and combine all variables used in the *Self* and *Other* specifications. Panel A presents results for implementing one's own preferred payoff; Panel B presents results for implementing the preferred payoff for others. Columns (1), (3), and (5) exclude controls, while columns (2), (4), and (6) include controls for age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors are robust to heteroskedasticity. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.
Source: PRESPARE

are broadly consistent with the conceptual framework, with one notable exception: interpersonal misalignment does not systematically restrain regulation and, if anything, points in the opposite direction in the full sample. Finally, while the monetary valuation of own autonomy exceeds that of others' autonomy in absolute terms ($C_i > v_i^{C_j}$), the estimates coefficients on DA: Other (WTP) consistently exceed those on DA: Self (WTP) across specifications. This reflects the fact that the coefficients capture marginal behavioral correlations rather than absolute utility levels, and suggests that the salience of others' autonomy—rather than its monetary valuation—is what drives its stronger association with regulation choices.

3.6.3 Motives

This section presents an exploratory analysis of the motives underlying individuals' regulation choices. Beyond the regression evidence, examining stated motives allows us to assess whether the mechanisms identified in the conceptual framework resonate with participants' own reasoning. Based on their regulation decision, participants rated the relevance of a set of pre-defined motives, yielding two naturally partitioned groups: those who regulated and those who did not. For each motive, we estimate regressions on treatment status and the experimental variables, including the preferred payoff, the relevant MGs, and WTP for autonomy (self and other), with the same controls as in equation 3.4.[7]

Motives for Regulation. We first examine the relative importance of motives independent of treatment status (see Table C.10 in the Appendix). The two most important motives for regulation are ensuring a fair allocation and ensuring one's own preferences are satisfied. Taking the burden off the DM ranks third, while preventing the DM from making a wrong choice is the least important motive. All motives except the two most relevant ones differ significantly from each other, and this ranking holds for both outcomes.

Table 3.5 reports regression results for individuals who chose to regulate. The key treatment differences emerge in two motives: taking the burden off the DM (column (2)), and preventing the DM from making a wrong choice (column (4)). Individuals in *Self* attach greater importance to taking the burden off the DM than those in *Group* ($p < 0.1$ in Panel A, $p < 0.01$ in Panel B) and *Other* (Wald test, $p < 0.05$). The reverse holds for preventing wrong choices: individuals in *Self* attach lower importance to this motive than those in *Group*, while individuals in *Other* attach higher importance relative to *Group* ($p < 0.01$ in all cases), with *Self* significantly lower than *Other* on Wald tests.

[7] The tables do not report results for the open-ended "Other Reasons" category, as only few participants provided written responses.

The interpretation of these patterns is intuitive: when individuals regulate their own future choices, regulation primarily serves to avoid revisiting the decision in wave 2—a form of commitment. When regulating on behalf of others, concern about the DM making an incorrect choice becomes the more salient justification.[8]

Turning to the experimental variables, higher preferred payoffs and larger intrapersonal misalignment are both associated with lower importance attached to fairness and greater importance attached to taking the burden off the DM. Larger intrapersonal misalignment is also associated with lower importance attached to ensuring one's own preferences—consistent with the idea that individuals who anticipate greater preference conflict also expect their current preferences to be less reliably satisfied, and may therefore rely less on this as a justification for regulating. Interpersonal misalignment is not systematically related to any stated motive. Autonomy valuations matter primarily when implementing preferred payoffs for others: a higher valuation of own autonomy tends to increase, and a higher valuation of others' autonomy tends to reduce, the importance attached to fairness and to ensuring one's own preferences. These findings are exploratory and should be interpreted as suggestive evidence, given the potential for post-decision rationalisation in stated motives.

Motives for Non-Regulation. We again first examine the relative importance of motives independent of treatment status (see Table C.10 in the Appendix). The most important motive for not regulating is respecting freedom of choice, followed by viewing the decision as the DM's responsibility and considering regulation inappropriate. Being unsure about the DM's preferences is the least important motive. This ranking holds for both outcomes. All motives differ significantly from each other, except for the decision being the DM's responsibility and regulation being inappropriate in the case of the Preferred Payoff (Self).

Table 3.6 reports results for individuals who chose not to regulate. The dominant pattern is that autonomy and responsibility concerns are substantially more salient in *Other* than in the other treatments. Individuals in *Other* attach greater importance to respecting freedom of choice than those in both *Group* ($p < 0.01$ in both panels) and *Self* (Wald tests, $p < 0.01$ in both panels). The same holds for viewing the decision as the DM's responsibility (*Other* vs. *Group*: $p < 0.01$ in both panels, column (2); Wald tests for *Other* vs. *Self*: $p < 0.01$ in both panels, column (2)), with one notable exception: the difference between *Self* and *Group* is statistically significant only in Panel A ($p < 0.01$; column (2)), consistent with the idea that individuals do not view their future self as meaningfully more responsible for the decision than their current self. Once another individual is involved—in *Group* or *Other*—this sense of the DM's responsibility becomes considerably more salient.

[8] Figure C.2 displays average motive ratings across treatment groups for each motive.

Table 3.5: Motives for Regulation

	Panel A. Importance of Motives for Regulation: Preferred Payoff (Self)			
	(1) Ensure Fair Allocation	(2) Take Burden off DM	(3) Ensure Own Prefs.	(4) Prevent DM from Wrong Choice
Self	-0.038	0.243*	0.104	-0.765***
	(0.120)	(0.132)	(0.122)	(0.136)
Other	-0.110	-0.136	0.012	0.788***
	(0.139)	(0.147)	(0.132)	(0.142)
Preferred Payoff (Self)	-0.206***	0.219***	0.083	0.024
	(0.058)	(0.064)	(0.059)	(0.064)
MG: Self vs. Self	-0.102*	0.251***	-0.137**	0.101
	(0.061)	(0.073)	(0.066)	(0.069)
MG: Self vs. Other	-0.006	-0.103	0.100	-0.045
	(0.060)	(0.066)	(0.061)	(0.063)
DA: Self (WTP)	0.082	-0.033	0.072	-0.069
	(0.051)	(0.062)	(0.052)	(0.064)
DA: Other (WTP)	-0.022	0.039	-0.069	0.034
	(0.054)	(0.063)	(0.055)	(0.064)
Control Mean	4.457	3.235	4.325	3.113
Wald Test (F-Stat)	0.31	5.81**	0.56	104.36***
Obs.	900	903	902	901
Adj. R-Sq	0.028	0.032	0.038	0.111
Controls	X	X	X	X

	Panel B. Importance of Motives for Regulation: Preferred Payoff (Other)			
	(1) Ensure Fair Allocation	(2) Take Burden off DM	(3) Ensure Own Prefs.	(4) Prevent DM from Wrong Choice
Self	-0.058	0.535***	0.075	-0.820***
	(0.129)	(0.139)	(0.126)	(0.151)
Other	0.019	-0.009	0.007	0.612***
	(0.142)	(0.149)	(0.139)	(0.148)
Preferred Payoff (Other)	-0.160***	0.138**	0.053	0.080
	(0.062)	(0.070)	(0.057)	(0.072)
MG: Other vs. Self	-0.192**	0.227***	-0.113	0.144*
	(0.078)	(0.085)	(0.075)	(0.086)
MG: Other vs. Other	0.078	-0.031	0.075	-0.034
	(0.063)	(0.074)	(0.068)	(0.080)
DA: Self (WTP)	0.096*	-0.023	0.134**	-0.068
	(0.057)	(0.065)	(0.057)	(0.066)
DA: Other (WTP)	-0.107*	0.013	-0.190***	-0.022
	(0.056)	(0.066)	(0.056)	(0.065)
Control Mean	4.426	3.215	4.375	3.386
Wald Test (F-Stat)	0.34	11.59***	0.27	79.23***
Obs.	791	789	790	792
Adj. R-Sq	0.041	0.025	0.044	0.099
Controls	X	X	X	X

Notes: The table presents estimates from regressions of motives for regulation on the treatment indicators *Self*, *Other*, and experimental variables. These include the respective preferred payoffs, misalignment gaps (MG) defined as the absolute differences between preferred payoffs and the corresponding beliefs about future allocation choices, and decision autonomy (DA) measures (DA: Self and DA: Other (WTP)) capturing the valuation of decision rights for oneself and for another participant in €. All continuous experimental covariates are standardized to have mean 0 and standard deviation 1. Panel A presents results if a CA implements the own preferred payoff; Panel B presents results if a CA implements the preferred payoff for others. The following motives are included: Ensure a fair allocation (column (1)), take burden off DM (column (2)), ensure own preferences are implemented (column (3)), and prevent DM from making the wrong choice (column (4)). All specifications control for age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors are robust to heteroskedasticity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE.

Table 3.6: Motives for Non-Regulation

	Panel A. Importance of Motives not to Regulate: Preferred Payoff (Self)			
	(1) Freedom of Choice	(2) Choice DM's Responsibility	(3) Unsure about DM's Prefs.	(4) Not Appropriate to Regulate
Self	-0.001	-0.379***	-0.038	-0.293**
	(0.112)	(0.126)	(0.123)	(0.122)
Other	0.368***	0.617***	0.161	0.621***
	(0.106)	(0.109)	(0.117)	(0.111)
Preferred Payoff (Self)	0.103**	0.062	0.070	0.102*
	(0.047)	(0.054)	(0.056)	(0.053)
MG: Self vs. Self	-0.071	0.049	0.126**	-0.056
	(0.047)	(0.052)	(0.056)	(0.053)
MG: Self vs. Other	0.107**	0.037	-0.116**	0.116**
	(0.050)	(0.055)	(0.059)	(0.058)
DA: Self (WTP)	0.036	-0.008	-0.032	0.076
	(0.049)	(0.053)	(0.057)	(0.052)
DA: Other (WTP)	-0.004	0.120**	-0.006	0.004
	(0.049)	(0.055)	(0.057)	(0.050)
Control Mean	4.384	3.858	3.520	3.819
Wald Test (F-Stat)	10.15***	54.75***	2.27	49.65***
Obs.	970	970	971	971
Adj. R-Sq	0.038	0.074	0.008	0.067
Controls	X	X	X	X

	Panel B. Importance of Motives not to Regulate: Preferred Payoff (Other)			
	(1) Freedom of Choice	(2) Choice DM's Responsibility	(3) Unsure about DM's Prefs.	(4) Not Appropriate to Regulate
Self	0.126	-0.122	-0.134	-0.219*
	(0.108)	(0.117)	(0.117)	(0.120)
Other	0.503***	0.790***	0.128	0.698***
	(0.105)	(0.106)	(0.115)	(0.111)
Preferred Payoff (Other)	0.073	0.081	0.009	0.016
	(0.048)	(0.050)	(0.052)	(0.051)
MG: Other vs. Self	-0.042	-0.034	-0.004	-0.073
	(0.049)	(0.051)	(0.054)	(0.052)
MG: Other vs. Other	0.024	-0.069	-0.058	0.018
	(0.052)	(0.056)	(0.055)	(0.056)
DA: Self (WTP)	0.034	0.034	-0.013	-0.009
	(0.048)	(0.052)	(0.055)	(0.053)
DA: Other (WTP)	0.044	0.072	0.031	-0.057
	(0.046)	(0.051)	(0.055)	(0.053)
Control Mean	4.230	3.808	3.652	3.747
Wald Test (F-Stat)	11.45***	55.25***	4.08**	52.47***
Obs.	1,071	1,069	1,071	1,070
Adj. R-Sq	0.053	0.101	0.016	0.069
Controls	X	X	X	X

Notes: The table presents estimates from regressions of motives for not regulating on the treatment indicators *Self*, *Other*, and experimental variables. These include the respective preferred payoffs, misalignment gaps (MG) defined as the absolute differences between preferred payoffs and the corresponding beliefs about future allocation choices, and decision autonomy (DA) measures (DA: Self and DA: Other (WTP)) capturing the valuation of decision rights for oneself and for another participant in €. All continuous experimental covariates are standardized to have mean 0 and standard deviation 1. Panel A presents results if a CA does not implement the own preferred payoff; Panel B presents results if a CA does not implement the preferred payoff for others. The following motives are included: freedom of choice for the DM (column (1)), choice is the DM's responsibility (column (2)), CA is unsure about DM's preferences (column (3)), and the belief that regulation is not appropriate (column (4)). All specifications control for age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors are robust to heteroskedasticity. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.
Source: PRESPARE.

Uncertainty about the DM's preferences matters only for the secondary outcome (Panel B), where it is significantly lower in *Self* than in *Other* (Wald test, $p < 0.05$, column (3))—consistent with individuals being more uncertain about others' preferences than about one's own future preferences. Finally, the view that regulation is simply inappropriate is least prominent when regulating one's own future self (*Self*), more so in *Group* ($p < 0.05$ in Panel A, $p < 0.1$ in Panel B; column (4)), and most prominent when regulation exclusively affects another individual (*Other*; $p < 0.01$ in both panels, column (4)).

Regarding the experimental variables, individuals with higher preferred payoffs attach greater importance to freedom of choice as a reason not to regulate. Among the MGs, only intrapersonal misalignment predicts uncertainty about the DM's preferences in Panel A, while interpersonal misalignment is associated with stronger concerns about both freedom of choice and the appropriateness of regulation, also only in Panel A. Autonomy valuations matter only in Panel A: a higher willingness to pay for others' autonomy is associated with greater importance attached to viewing the choice as the DM's responsibility. Taken together, stated motives vary systematically across treatments and align well with the mechanisms identified in the conceptual framework—reinforcing the conclusion that autonomy concerns, rather than preference misalignment, are the primary restraint on regulation when others are affected.

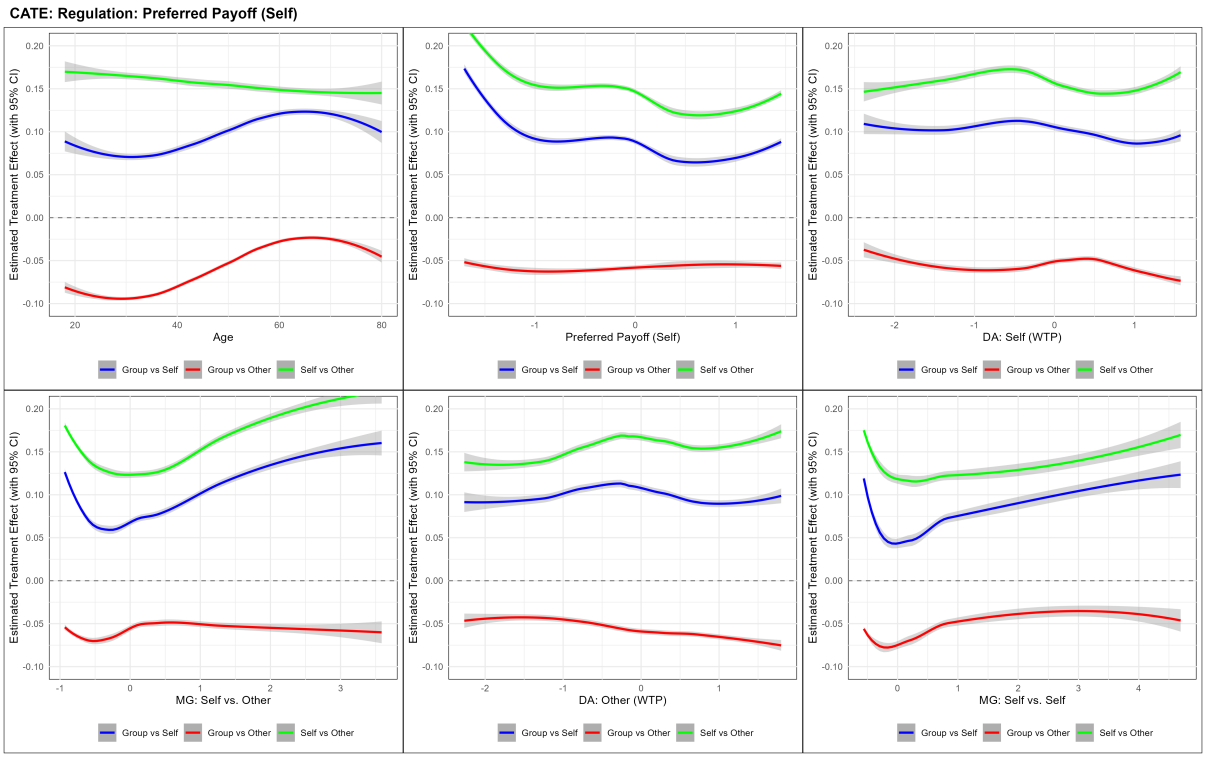
3.6.4 Heterogeneity

The heterogeneity analysis is exploratory and proceeds in two steps, drawing on all pre-treatment control and experimental variables. We first estimate the specification in Equation 3.4 with full interactions between treatment status and each variable. We then employ a causal random forest (Wager and Athey, 2018) on the same set of variables—excluding dummies, which are not well-suited to the splitting algorithm—to capture potential non-linear heterogeneous treatment effects and rank covariates by their importance in explaining treatment effect variation.

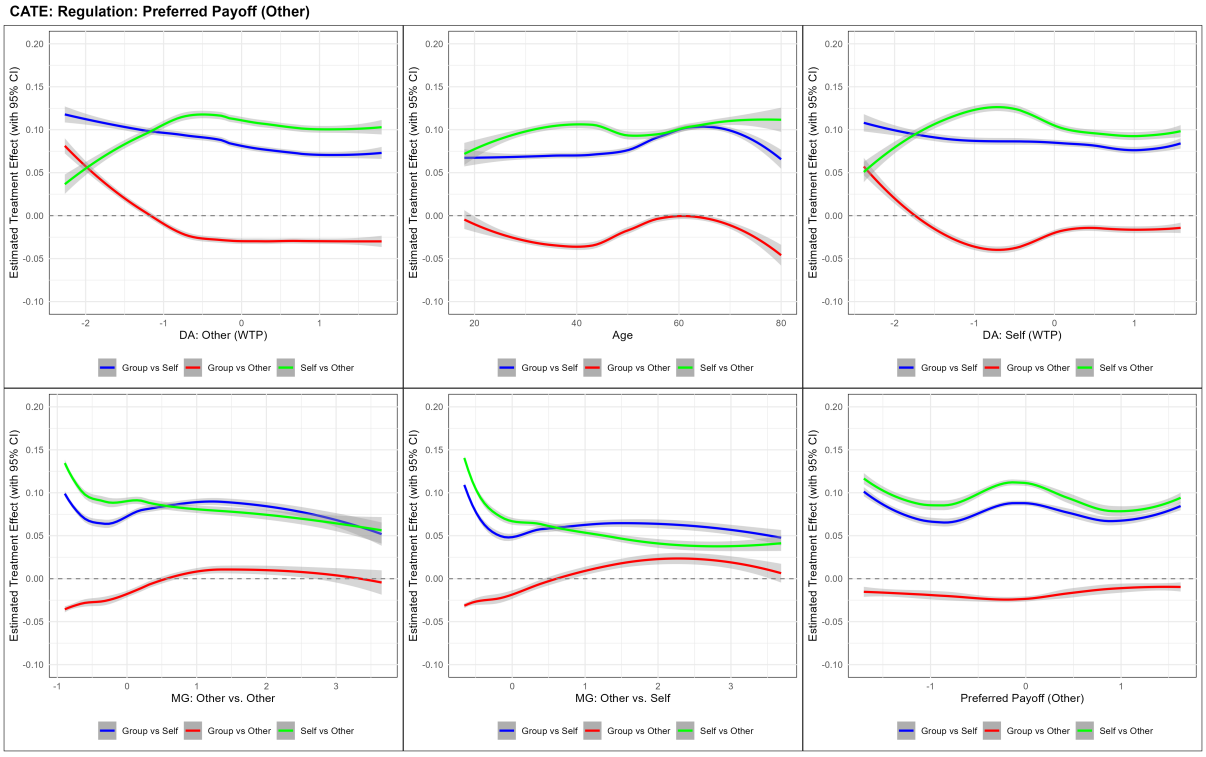
Tables C.11, C.12, and C.13 report results from the interaction specifications. The results reveal no strong or systematic patterns: point estimates are largely insignificant and inconsistent in sign across specifications. Given this absence of clear linear heterogeneity, we focus on the causal forest results, which are better suited to detecting non-linear sources of variation.

Figure 3.1 presents the causal random forest estimates, displaying the six most important covariates for each outcome, ranked by their contribution to explaining treatment effect heterogeneity. Part (a) corresponds to Regulation: Preferred Payoff (Self) and part (b) to Regulation: Preferred Payoff (Other).

The results point to modest and primarily non-linear heterogeneity in treatment effects. The most consistent predictors across both outcomes are age and autonomy valuations—DA:



(a) Results Causal Random Forest: Regulation (Self)



(b) Results Causal Random Forest: Regulation (Other)

Notes: The figure presents results from a causal random forest estimating heterogeneous treatment effects. Each panel shows the six most relevant covariates for explaining treatment effect heterogeneity, ranked by importance. The y-axis depicts the estimated treatment effect (conditional average treatment effect, CATE), and the x-axis shows the corresponding values of the covariate. Colors indicate treatments: "Group vs. Self" (blue), "Group vs. Other" (red), and "Self vs. Other" (green). These estimates represent model-based estimates of how the treatment effect varies across covariate values.
Source: PRESPARE.

Figure 3.1: Conditional Average Treatment Effects

Self (WTP) ranks among the top three covariates for both Regulation: Preferred Payoff (Self) and Regulation: Preferred Payoff (Other), as does age. By contrast, misalignment gap measures consistently rank lower, appearing among the bottom three covariates in both cases. The preferred payoff and DA: Other (WTP) also feature prominently, though their relative ranking differs somewhat across the two outcomes. Importantly, the heterogeneity in treatment effects is modest in magnitude: the causal forest estimates do not reveal subgroups for whom the direction of the treatment effects reverses, reinforcing the robustness and generalisability of the average treatment effects reported in Section 3.6.1.

3.7 Discussion & Conclusion

This paper provides causal evidence that individuals genuinely trade off self-interest and concern for others when deciding whether to regulate. Using a longitudinal incentivized survey experiment with a representative sample from Germany, we show that support for regulation is highest when individuals regulate themselves, intermediate when regulation jointly affects themselves and others, and lowest when regulation targets only another individual. These findings establish that the regulation target systematically shapes willingness to regulate—and that individuals do not simply seek to enforce their preferred outcome universally, but incorporate the consequences for others into their policy considerations.

The *Group* treatment is most closely related to real-world policy settings, where supporting a tax or regulation typically implies enforcing an allocation that affects both oneself and others. A key finding is that self-interest and paternalistic concerns are aggregated in this case: support for regulation in *Group* lies between the purely self-directed case (*Self*) and the purely other-directed case (*Other*), rather than collapsing to either extreme. This pattern demonstrates that individuals do not treat their own and others' outcomes as separable—the presence of personal stakes raises willingness to regulate relative to *Other*, while concern for those also affected constrains it relative to *Self*. Regarding the secondary outcome—implementing allocations individuals consider appropriate for others—we find no significant difference between *Group* and *Other*. Once regulation concerns enforcing what CAs believe is the right outcome for others, it no longer matters whether the CA is personally affected. In this case, the perceived correctness of the outcome appears to crowd out concerns about the other individual's decision autonomy.

To understand these patterns, we develop a conceptual framework centred on three factors: self-interest, anticipated preference misalignment with the regulation target, and valuations of decision autonomy. The evidence broadly supports the framework, but with one important and revealing deviation. Higher personal payoffs robustly increase the likelihood of regulation, and stronger autonomy valuations—both for oneself and for others—reduce it. However, while

anticipated misalignment with one's own future preferences is a robust predictor of regulation behavior, misalignment with beliefs about others' preferences is not. This asymmetry is the central mechanistic finding of the paper: resistance to regulation is not primarily driven by concerns about violating others' preferences, but by concerns about infringing upon others' decision autonomy. When regulation affects both oneself and others (*Group*), willingness to pay for own autonomy loses predictive power, while concern for others' autonomy remains a significant restraint.

The analysis of stated motives reinforces this conclusion. Among those who chose to regulate, the dominant justifications are ensuring a fair allocation and satisfying one's own preferences—suggesting that CAs perceive their chosen allocation as normatively appropriate. Among those who chose not to regulate, the dominant motive is respecting freedom of choice, while uncertainty about the DM's preferences ranks last. This ranking is telling: individuals resist regulation not because they are unsure what others want, but because they value others' right to choose for themselves. Treatment differences in motives further support this: when regulation exclusively affects others, freedom of choice and the DM's responsibility become substantially more salient, while concern about the DM making a wrong choice increases when others are involved in the regulation decision.

From a policy perspective, these findings carry a distinct and potentially challenging implication: support for regulation depends less on whether policies align with what others want, and more on whether they preserve individuals' intrinsic freedom to choose. Addressing autonomy concerns—rather than simply demonstrating preference alignment—may therefore be central to the design and public acceptance of paternalistic policies. The heterogeneity analysis reveals little evidence of systematic linear treatment effect heterogeneity. Non-linear heterogeneity is present, however, and is driven primarily by variables from the conceptual framework—in particular age and autonomy valuations—providing further support for the framework's ability to capture the behavioral mechanisms underlying regulation choices. Socioeconomic characteristics play a comparatively minor role.

While our results provide first evidence that individuals aggregate self-interest and paternalistic concerns when both are at stake, the experimental setting remains intentionally stylized—individuals trade off monetary gains against environmental benefits in a controlled two-person allocation task. An important next step is to assess whether these patterns generalize to more complex and directly policy-relevant contexts. Our treatments map naturally onto a range of real-world settings: the *Self* treatment resembles private commitment devices such as self-imposed screen-time limits or financial withdrawal restrictions; the *Other* treatment corresponds to policies targeting specific groups such as high-income earners; and the *Group* treatment reflects broadly applied regulations such as sin taxes on alcohol, sugar, or tobacco. Future

research could examine whether the observed patterns persist across these settings, scale to regulations affecting large numbers of individuals, and extend to softer interventions such as nudges or information provision. More broadly, our findings suggest that understanding opposition to paternalistic policies requires taking seriously not just what people want for others, but how much they value others' right to want something different.

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Appendix A

Supplementary Materials Chapter 1

A.1 Tables

Table A.1: Comparison Survey Data with Eurostat Data

	(1) Survey Data	(2) Eurostat
Female: 18–35 Years	0.133	0.134
Female: 36–45 Years	0.081	0.081
Female: 46–55 Years	0.091	0.091
Female: 56–65 Years	0.100	0.100
Female: 66–80 Years	0.097	0.095
Male: 18–35 Years	0.139	0.145
Male: 36–45 Years	0.083	0.083
Male: 46–55 Years	0.095	0.091
Male: 56–65 Years	0.099	0.098
Male: 66–80 Years	0.082	0.083
Employed (%)	0.824	0.813
Median Net Income (€)	2250.000	2301.000
Low Education	0.292	0.300
Middle Education	0.314	0.310
Higher Education	0.393	0.390
Baden-Württemberg	0.131	0.134
Bavaria	0.161	0.159
Berlin	0.046	0.045
Brandenburg	0.027	0.030
Bremen	0.007	0.008
Hamburg	0.022	0.023
Hesse	0.074	0.076
Mecklenburg-Western Pomerania	0.019	0.019
Lower Saxony	0.097	0.096
North Rhine-Westphalia	0.217	0.215
Rhineland-Palatinate	0.051	0.050
Saarland	0.011	0.012
Saxony	0.049	0.048
Saxony-Anhalt	0.025	0.026
Schleswig-Holstein	0.036	0.035
Thuringia	0.026	0.025

Notes: This table compares the survey sample to official population statistics from Eurostat. Low education refers to Hauptschule/Mittelschule or equivalent. Middle education refers to Realschule or equivalent. Higher education refers to Abitur or higher. Employment excludes individuals who are already retired. All variables are expressed as shares unless otherwise indicated; income refers to the median.

Table A.2: Summary Statistics

	Panel A: Controls				
	(1) Full Sample	(2) Control	(3) Info	(4) RE	(5) WE
Age	48.68	48.94	48.08	49.18	48.54
	(15.38)	(15.51)	(14.97)	(15.46)	(15.59)
Female	0.50	0.49	0.50	0.47	0.55
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Tertiary Education	0.23	0.24	0.22	0.20	0.23
	(0.42)	(0.43)	(0.42)	(0.40)	(0.42)
Minor Children	0.27	0.26	0.26	0.27	0.28
	(0.44)	(0.44)	(0.44)	(0.44)	(0.45)
Minor Children in Household	0.43	0.44	0.42	0.42	0.44
	(0.81)	(0.87)	(0.80)	(0.78)	(0.80)
Full Time	0.44	0.43	0.44	0.43	0.47
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Errors	0.64	0.64	0.64	0.64	0.64
	(1.01)	(0.90)	(1.07)	(0.89)	(1.15)
Attention Passed	0.82	0.80	0.81	0.81	0.84
	(0.39)	(0.40)	(0.39)	(0.39)	(0.37)
Duration (Min.)	32.31	34.04	30.81	33.52	31.01
	(46.42)	(49.89)	(46.06)	(55.39)	(31.26)
	Panel B: Outcomes				
Choices:					
Normal Shirt	0.42	0.36	0.41	0.44	0.48
	(0.49)	(0.48)	(0.49)	(0.50)	(0.50)
Apples NZ	0.25	0.22	0.22	0.25	0.31
	(0.43)	(0.42)	(0.42)	(0.43)	(0.46)
Flight	0.35	0.29	0.34	0.36	0.42
	(0.48)	(0.46)	(0.47)	(0.48)	(0.49)
Beliefs:					
Normal Shirt	52.81	50.68	52.81	52.64	55.04
	(21.66)	(21.79)	(21.77)	(22.11)	(20.79)
Apples NZ	48.04	46.28	47.82	47.72	50.28
	(23.16)	(23.43)	(23.26)	(23.02)	(22.82)
Flight	52.90	50.22	51.80	53.43	56.11
	(23.02)	(23.24)	(22.54)	(23.22)	(22.76)
Social Acceptance:					
Normal Shirt	0.34	0.18	0.35	0.42	0.41
	(1.29)	(1.28)	(1.25)	(1.33)	(1.28)
Organic Shirt	0.83	0.91	0.84	0.80	0.76
	(1.22)	(1.18)	(1.18)	(1.25)	(1.26)
Apples NZ	-0.04	-0.18	-0.02	0.06	-0.00
	(1.35)	(1.32)	(1.38)	(1.37)	(1.34)
Apples GER	0.99	1.09	0.98	0.93	0.94
	(1.23)	(1.17)	(1.21)	(1.26)	(1.26)
Flight	-0.12	-0.24	-0.05	-0.08	-0.10
	(1.38)	(1.35)	(1.41)	(1.39)	(1.37)
Train	0.88	0.95	0.94	0.85	0.80
	(1.23)	(1.20)	(1.19)	(1.26)	(1.28)
Switching Points:					
Shirts from Organic to Normal	3.83	3.97	3.88	3.79	3.68
	(2.44)	(2.44)	(2.48)	(2.40)	(2.44)
Apples from GER to NZ	4.20	4.38	4.26	4.18	3.97
	(2.38)	(2.35)	(2.39)	(2.33)	(2.41)
Train to Flight	4.12	4.39	4.18	4.05	3.88
	(2.65)	(2.57)	(2.65)	(2.70)	(2.67)
Observations	2408	587	625	590	606

Notes: This table presents summary statistics for the full sample and each experimental group. Standard errors are in parentheses. The sample consists of a *Control* group, and three treatment groups: *Info*, *RE*, and *WE*. Panel A shows control variables used in the analysis, which include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Panel B shows the main outcome variables of the experiment, which include the three consumption choices, beliefs about other participants' consumption choices (0–100 percent), social acceptance ratings (-2 "Socially very inappropriate", -1 "Socially somewhat inappropriate", 1 "Socially somewhat appropriate", and 2 "Socially very appropriate"), and switching points in the MPLs (range 0–7) from the more sustainable to the less sustainable option as the price difference increases.

Table A.3: Consistency Moral Cost Measure and Treatment Assignment

	Consistency Moral Cost Measure		
	(1) Shirts Organic to Normal	(2) Apples GER to NZ	(3) Transportation Train to Flight
Info	-0.003	0.007	0.003
	(0.015)	(0.013)	(0.016)
RE	0.000	-0.008	0.014
	(0.015)	(0.014)	(0.016)
WE	-0.017	0.002	0.017
	(0.016)	(0.013)	(0.015)
Obs.	2,408	2,408	2,408

Notes: The table presents results from a consistency check of the moral cost measure, where a dummy variable equal to one if an individual exhibits consistent switching points is regressed on treatment indicators. Standard errors are robust to heteroskedasticity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Table A.4: Variable Definitions

Panel A. Controls	
Variable	**Definition**
Age	Respondent's age in years.
Female	Dummy equal to 1 if respondent is female, 0 otherwise.
Tertiary Education	Dummy equal to 1 if respondent holds a tertiary (any form of university-level education) degree, 0 otherwise.
Minor Children	Dummy equal to 1 if respondent has at least one child under the age of 18, 0 otherwise.
Minor Children in Household	Number of minor children under 18 living in the respondent's household.
Full Time	Dummy equal to 1 if respondent is employed full time, 0 otherwise.
Errors	Number of errors made in the comprehension question.
Attention Passed	Dummy equal to 1 if respondent passed the second attention check.
Duration (Min.)	Survey completion time in minutes.
Panel B. Outcomes	

Continued on next page

Table A.4 – continued from previous page

Variable	Definition
Choices:	Binary variables of decisions in incentivized consumption scenarios
Flight	Dummy equal to 1 if respondent chooses the flight instead of the train for an equally time-consuming trip from Munich to Paris, 0 otherwise.
Normal Shirt	Dummy equal to 1 if respondent chooses the shirt made from conventional rather than organic cotton (shirts are otherwise identical), 0 otherwise.
Apples NZ	Dummy equal to 1 if the respondent chooses apples from New Zealand instead of Germany, with both options being identical in taste, 0 otherwise.
Beliefs:	Ranging from 0 to 100 percent
Flight	Respondent's belief about the share of participants choosing the flight instead of the train.
Normal Shirt	Respondent's belief about the share of participants choosing the normal instead of the organic shirt.
Apples NZ	Respondent's belief about the share of participants choosing apples from New Zealand instead of Germany.
Social Acceptance:	Scale from –2 ("Socially very inappropriate") to +2 ("Very socially appropriate")
Flight	Social acceptability ranking of choosing the flight instead of the train.
Train	Social acceptability ranking of choosing the train instead of the flight.
Normal Shirt	Social acceptability ranking of choosing the normal instead of the organic shirt.
Organic Shirt	Social acceptability ranking of choosing the organic instead of the normal shirt.
Apples NZ	Social acceptability ranking of choosing apples from New Zealand instead of Germany.

Continued on next page

Table A.4 – continued from previous page

Variable	Definition
Apples GER	Social acceptability ranking of choosing apples from Germany instead of New Zealand.
Switching Points:	Measured on a scale from 0 to 7, indicating the price level at which respondents switch in Multiple Price Lists (MPLs)
Train to Flight	Switching point where the respondent changes from train to flight as the flight becomes relatively cheaper (in steps of 10 € from 0 € to 70 €).
Shirts from Organic to Normal	Switching point where the respondent changes from organic to normal shirt as the normal shirt becomes relatively cheaper (in steps of 1.30 € from 0 € to 9.10 €).
Apples from GER to NZ	Switching point where the respondent changes from German apples to New Zealand apples as the latter become relatively cheaper (in steps of 0.50 € from 0 € to 3.50 €).
PANAS:	Positive and Negative Affect Schedule (PANAS) ranging from 10 to 50. Higher values indicate stronger affect (Watson et al., 1988).
Negative Affect	Negative affect score.
Positive Affect	Positive affect score.
Petition:	
Yes to Petition	Dummy equal to 1 if respondent chooses to sign a petition in favor of stronger climate protection, 0 otherwise.
Climate Change Perceptions:	
Climate Change (Voting)	Importance assigned to climate policy in voting decisions, measured on a 5-point scale from 1 (“Not important at all”) to 5 (“Very important”).
Climate Change (Problem)	Perceived severity of climate change as a problem, measured on a 5-point scale from 1 (“No problem at all”) to 5 (“Big problem”).
Voting Changes:	
No AfD Anymore	Dummy equal to 1 if respondent intended to vote for the AfD in wave 1 but switched to another party in wave 2.

Continued on next page

Table A.4 – continued from previous page

Variable	Definition
No Grüne Anymore	Dummy equal to 1 if respondent intended to vote for the Green Party (Bündnis '90/Grüne) in wave 1 but switched to another party in wave 2.
Willingness to Change Behavior:	Dummies equal to 1 if respondent is at least "moderately" willing to adopt the respective pro-environmental behavior, 0 otherwise.
Fly Less	Willingness to fly less.
Drive Less	Willingness to drive less.
Eat Less Meat	Willingness to eat less meat.
Reduce Home Heating	Willingness to reduce home heating.
Own Electric Car	Willingness to own an electric car.

Table A.5: Balancing Table

	Panel A: Controls					
	(1) C vs. I	(2) C vs. RE	(3) C vs. WE	(4) I vs. RE	(5) I vs. WE	(6) RE vs. WE
Age	-0.85	0.24	-0.39	1.10	0.46	-0.64
	(0.88)	(0.90)	(0.90)	(0.87)	(0.87)	(0.90)
Female	0.01	-0.02	0.06*	-0.03	0.05*	0.08***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Tertiary Education	-0.02	-0.04*	-0.01	-0.02	0.01	0.03
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Minor Children	-0.00	0.00	0.02	0.01	0.02	0.02
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Minor Children in Household	-0.02	-0.02	0.00	0.00	0.02	0.02
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Full Time	0.01	0.00	0.04	-0.01	0.03	0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Errors	0.00	0.01	-0.00	0.00	-0.00	-0.01
	(0.06)	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)
Attention Passed	0.01	0.01	0.04*	0.01	0.03	0.03
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Duration (Min.)	-3.23	-0.52	-3.03	2.71	0.20	-2.51
	(2.76)	(3.07)	(2.40)	(2.92)	(2.25)	(2.59)
	Panel B: Outcomes					
Choices:						
Normal Shirt	0.05*	0.08***	0.12***	0.03	0.07**	0.04
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Apples NZ	-0.00	0.02	0.08***	0.03	0.08***	0.06**
	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)
Flight	0.04	0.07**	0.13***	0.03	0.09***	0.06**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Beliefs:						
Normal Shirt	2.13*	1.96	4.36***	-0.17	2.23*	2.40*
	(1.25)	(1.28)	(1.23)	(1.26)	(1.21)	(1.24)
Apples NZ	1.54	1.43	3.99***	-0.10	2.46*	2.56*
	(1.34)	(1.35)	(1.34)	(1.33)	(1.31)	(1.33)
Flight	1.58	3.22**	5.89***	1.63	4.31***	2.68**
	(1.32)	(1.35)	(1.33)	(1.31)	(1.29)	(1.33)
Social Acceptance:						
Normal Shirt	0.17**	0.24***	0.23***	0.07	0.06	-0.01
	(0.07)	(0.08)	(0.07)	(0.07)	(0.07)	(0.08)
Organic Shirt	-0.07	-0.11	-0.16**	-0.04	-0.08	-0.05
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Apples NZ	0.16**	0.25***	0.18**	0.09	0.02	-0.07
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
Apples GER	-0.10	-0.15**	-0.14**	-0.05	-0.04	0.01
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Flight	0.19**	0.15*	0.13*	-0.04	-0.05	-0.02
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
Train	-0.02	-0.10	-0.16**	-0.09	-0.14**	-0.06
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Switching Points:						
Shirts from Organic to Normal	-0.09	-0.18	-0.29*	-0.09	-0.20	-0.11
	(0.15)	(0.15)	(0.15)	(0.15)	(0.15)	(0.15)
Apples from GER to NZ	-0.12	-0.20	-0.41***	-0.08	-0.29**	-0.21
	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)
Train to Flight	-0.20	-0.34**	-0.51***	-0.14	-0.30*	-0.17
	(0.16)	(0.16)	(0.16)	(0.16)	(0.16)	(0.16)
Observations	1212	1177	1193	1215	1231	1196

Notes: This table presents differences in means across treatment groups, comparing the *Control* group (C), *Info* (I), and the two treatment groups (*RE* and *WE*). Standard errors from t-tests are in parentheses. Panel A displays results for the control variables used in the analysis, which include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Panel B displays results for the main outcome variables of the experiment, which include the three consumption choices, beliefs about other participants' consumption choices (0–100 percent), social acceptance ratings (-2 "Socially very inappropriate", -1 "Socially somewhat inappropriate", 1 "Socially somewhat appropriate", and 2 "Socially very appropriate"), and switching points in the MPLs (range 0–7) from the more sustainable to the less sustainable option as the price difference increases. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Table A.6: Consumption Choices

	Choices					
	Normal Shirt		Apples NZ		Flight	
	(1)	(2)	(3)	(4)	(5)	(6)
Info	0.048*	0.046*	-0.002	-0.008	0.041*	0.036
	(0.028)	(0.028)	(0.024)	(0.024)	(0.027)	(0.026)
RW p-value	[0.056]	[0.082]	[0.900]	[0.665]	[0.088]	[0.162]
RE	0.076***	0.072***	0.023	0.019	0.070***	0.067***
	(0.029)	(0.028)	(0.025)	(0.024)	(0.027)	(0.027)
RW p-value	[0.004]	[0.006]	[0.369]	[0.461]	[0.006]	[0.008]
WE	0.119***	0.122***	0.082***	0.082***	0.128***	0.124***
	(0.028)	(0.028)	(0.025)	(0.025)	(0.028)	(0.027)
RW p-value	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]
Control Mean	0.363	0.363	0.225	0.225	0.295	0.295
RE vs WE (F-Stat)	2.21	3.04*	5.30**	6.09**	4.23**	4.12**
I vs RE (F-Stat)	0.96	0.85	1.06	1.28	1.07	1.32
I vs WE (F-Stat)	6.23**	7.32***	11.36***	13.28***	9.82***	10.30***
Obs.	2,408	2,408	2,408	2,408	2,408	2,408
Adj. R-Sq	0.006	0.020	0.005	0.032	0.008	0.031
Controls		X		X		X

Notes: The table presents estimates of the effect of *Info*, *RE*, and *WE* treatments on consumption choice dummies. Columns (1), (3), and (5) present results without controls, while columns (2), (4), and (6) include the full set of control variables. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Values in parentheses report standard errors robust to heteroskedasticity and values in square brackets show p-values adjusted for multiple hypothesis testing. Significance levels, indicated by stars, refer to the corrected p-values. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Table A.7: Consumption Choices (Robustness)

	Choices		
	(1) Normal Shirt	(2) Apples New Zealand	(3) Flight
Info	0.046*	-0.008	0.036
	(0.028)	(0.024)	(0.026)
RE	0.072**	0.020	0.067**
	(0.028)	(0.024)	(0.027)
WE	0.122***	0.083***	0.124***
	(0.028)	(0.025)	(0.027)
Errors	0.019*	0.019*	0.011
	(0.011)	(0.010)	(0.010)
Attention Passed	-0.000	-0.029	0.001
	(0.028)	(0.025)	(0.027)
Duration (Min.)	-0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)
Control Mean	0.363	0.225	0.295
RE vs WE (F-Stat)	3.01*	6.16**	4.11**
I vs RE (F-Stat)	0.87	1.33	1.32
I vs WE (F-Stat)	7.32***	13.53***	10.29***
Obs.	2,408	2,408	2,408
Adj. R-Sq	0.020	0.034	0.030
Controls	X	X	X

Notes: The table presents estimates of the effect of *Info*, *RE*, and *WE* treatments on the consumption choice dummies. It assesses robustness with respect to the number of errors in the comprehension questions, a dummy for passing the second attention check, and survey completion time (in minutes). Controls include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors in parentheses are robust to heteroskedasticity. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.
Source: PRESPARE

Table A.8: Beliefs about Others' Behavior (Descriptive Norms) and Social Acceptance of Consumption Choices (Injunctive Norms)

	Panel A: Beliefs (Descriptive Norms)					
	Normal Shirt		Apples NZ		Flight	
	(1)	(2)	(3)	(4)	(5)	(6)
Info	2.130*	2.223*	1.536	1.577	1.582	1.594
	(1.252)	(1.248)	(1.342)	(1.344)	(1.316)	(1.313)
RW p-value	[0.092]	[0.052]	[0.240]	[0.240]	[0.240]	[0.240]
RE	1.961*	1.978*	1.434	1.463	3.216***	3.380***
	(1.280)	(1.281)	(1.354)	(1.355)	(1.354)	(1.357)
RW p-value	[0.096]	[0.098]	[0.240]	[0.240]	[0.006]	[0.002]
WE	4.362***	4.363***	3.994***	3.981***	5.894***	5.750***
	(1.234)	(1.239)	(1.340)	(1.343)	(1.332)	(1.332)
RW p-value	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]
Control Mean	50.678	50.678	46.283	46.283	50.218	50.218
RE vs WE (F-Stat)	3.74*	3.66*	3.73*	3.59*	4.06**	3.17*
I vs RE (F-Stat)	0.02	0.04	0.01	0.01	1.55	1.85
I vs WE (F-Stat)	3.38*	3.11*	3.50*	3.32*	11.15***	10.43***
Obs.	2,408	2,408	2,408	2,408	2,408	2,408
Adj. R-Sq	0.004	0.005	0.003	0.001	0.008	0.011
Controls		X		X		X
	Panel B: Social Acceptance Selfish Choices (Injunctive Norms)					
	Normal Shirt		Apples NZ		Flight	
	(1)	(2)	(3)	(4)	(5)	(6)
Info	0.167**	0.162**	0.160**	0.150*	0.187**	0.175**
	(0.073)	(0.073)	(0.078)	(0.077)	(0.079)	(0.079)
RW p-value	[0.022]	[0.034]	[0.048]	[0.064]	[0.018]	[0.034]
RE	0.240***	0.240***	0.248***	0.248***	0.152*	0.148*
	(0.076)	(0.076)	(0.078)	(0.078)	(0.080)	(0.079)
RW p-value	[0.002]	[0.002]	[0.002]	[0.002]	[0.068]	[0.076]
WE	0.229***	0.229***	0.182**	0.179**	0.133	0.126
	(0.074)	(0.074)	(0.077)	(0.077)	(0.079)	(0.078)
RW p-value	[0.002]	[0.002]	[0.018]	[0.018]	[0.106]	[0.146]
Control Mean	0.181	0.181	-0.184	-0.184	-0.235	-0.235
RE vs WE (F-Stat)	0.02	0.02	0.71	0.78	0.06	0.07
I vs RE (F-Stat)	0.97	1.11	1.26	1.57	0.19	0.12
I vs WE (F-Stat)	0.74	0.87	0.08	0.14	0.47	0.39
Obs.	2,408	2,408	2,408	2,408	2,408	2,408
Adj. R-Sq	0.004	0.007	0.003	0.019	0.001	0.014
Controls		X		X		X
	Panel C: Social Acceptance Non-Selfish Choices (Injunctive Norms)					
	Organic Shirt		Apples GER		Train	
	(1)	(2)	(3)	(4)	(5)	(6)
Info	-0.075	-0.071	-0.103	-0.096	-0.016	-0.012
	(0.068)	(0.067)	(0.068)	(0.068)	(0.069)	(0.069)
RW p-value	[0.242]	[0.279]	[0.128]	[0.206]	[0.735]	[0.834]
RE	-0.111	-0.100	-0.153**	-0.141*	-0.101	-0.094
	(0.071)	(0.071)	(0.071)	(0.071)	(0.072)	(0.072)
RW p-value	[0.128]	[0.206]	[0.038]	[0.064]	[0.148]	[0.206]
WE	-0.159**	-0.164**	-0.141*	-0.145*	-0.157**	-0.159**
	(0.070)	(0.070)	(0.070)	(0.070)	(0.072)	(0.072)
RW p-value	[0.026]	[0.018]	[0.054]	[0.052]	[0.028]	[0.034]
Control Mean	0.915	0.915	1.085	1.085	0.952	0.952
RE vs WE (F-Stat)	0.43	0.78	0.03	0.00	0.57	0.77
I vs RE (F-Stat)	0.27	0.17	0.50	0.39	1.46	1.36
I vs WE (F-Stat)	1.46	1.78	0.30	0.48	3.99**	4.35**
Obs.	2,408	2,408	2,408	2,408	2,408	2,408
Adj. R-Sq	0.001	0.009	0.001	0.013	0.001	0.003
Controls		X		X		X

Notes: The table presents results for beliefs about other participants' consumption choices (0–100 percent) and social acceptance ratings (-2 "Socially very inappropriate", -1 "Socially somewhat inappropriate", 1 "Socially somewhat appropriate", and 2 "Socially very appropriate"). Panel A presents estimates of the effect of *Info*, *RE*, and *WE* treatments on beliefs. Panel B and C present these estimates for social acceptance ratings for selfish and non-selfish choices, respectively. Columns (1), (3), and (5) present results without controls, while columns (2), (4), and (6) include the full set of control variables. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Values in parentheses report standard errors robust to heteroskedasticity and values in square brackets show p-values adjusted for multiple hypothesis testing. Significance levels, indicated by stars, refer to the corrected p-values. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Table A.9: Beliefs about Others' Behavior (Descriptive Norms) and Social Acceptance of Consumption Choices (Injunctive Norms): Robustness

	Panel A: Beliefs (Descriptive Norms)		
	(1) Normal Shirt	(2) Apples NZ	(3) Flight
Info	2.145*	1.553	1.508
	(1.241)	(1.345)	(1.306)
RE	1.886	1.435	3.298**
	(1.269)	(1.356)	(1.349)
WE	4.123***	3.909***	5.519***
	(1.230)	(1.343)	(1.331)
Errors	-2.006***	-0.631	-1.475***
	(0.433)	(0.460)	(0.479)
Attention Passed	5.379***	1.586	4.844***
	(1.261)	(1.336)	(1.275)
Duration (Min.)	-0.003	-0.001	-0.008
	(0.009)	(0.009)	(0.008)
Control Mean	50.678	46.283	50.218
RE vs WE (F-Stat)	3.32*	3.48*	2.82*
I vs RE (F-Stat)	0.04	0.01	1.90
I vs WE (F-Stat)	2.72*	3.20*	9.84***
Obs.	2,408	2,408	2,408
Adj. R-Sq	0.026	0.002	0.023
Controls	X	X	X
	Panel B: Social Acceptance Selfish Choices (Injunctive Norms)		
	(1) Normal Shirt	(2) Apples NZ	(3) Flight
Info	0.159**	0.151*	0.173**
	(0.073)	(0.077)	(0.079)
RE	0.237***	0.248***	0.148*
	(0.076)	(0.078)	(0.079)
WE	0.220***	0.179**	0.123
	(0.074)	(0.077)	(0.078)
Errors	-0.023	0.022	0.071**
	(0.027)	(0.028)	(0.032)
Attention Passed	0.211***	0.019	0.022
	(0.073)	(0.076)	(0.078)
Duration (Min.)	-0.000	0.000	-0.001
	(0.000)	(0.000)	(0.001)
Control Mean	0.181	-0.184	-0.235
RE vs WE (F-Stat)	0.05	0.78	0.09
I vs RE (F-Stat)	1.10	1.55	0.10
I vs WE (F-Stat)	0.71	0.13	0.40
Obs.	2,408	2,408	2,408
Adj. R-Sq	0.010	0.018	0.016
Controls	X	X	X
	Panel C: Social Acceptance Non-Selfish Choices (Injunctive Norms)		
	(1) Organic Shirt	(2) Apples GER	(3) Train
Info	-0.078	-0.101	-0.020
	(0.066)	(0.066)	(0.067)
RE	-0.109	-0.149**	-0.104
	(0.068)	(0.068)	(0.069)
WE	-0.187***	-0.167**	-0.187***
	(0.068)	(0.068)	(0.070)
Errors	-0.148***	-0.150***	-0.106***
	(0.030)	(0.038)	(0.032)
Attention Passed	0.546***	0.549***	0.666***
	(0.071)	(0.073)	(0.073)
Duration (Min.)	-0.000	0.001	0.000
	(0.000)	(0.000)	(0.000)
Control Mean	0.915	1.085	0.952
RE vs WE (F-Stat)	1.25	0.06	1.32
I vs RE (F-Stat)	0.20	0.49	1.55
I vs WE (F-Stat)	2.58	0.90	5.88**
Obs.	2,408	2,408	2,408
Adj. R-Sq	0.062	0.068	0.062
Controls	X	X	X

Notes: The table presents results for beliefs about other participants' consumption choices (0–100 percent) and social acceptance ratings (-2 "Socially very inappropriate", -1 "Socially somewhat inappropriate", 1 "Socially somewhat appropriate", and 2 "Socially very appropriate"). Panel A presents estimates of the effect of *Info*, *RE*, and *WE* treatments on beliefs. Panel B and C present these estimates for social acceptance ratings for selfish and non-selfish choices, respectively. It assesses robustness with respect to the number of errors in comprehension questions, a dummy for passing the second attention check, and survey completion time (in minutes). Controls include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors in parentheses are robust to heteroskedasticity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Table A.10: Switching Points (Moral Costs)

	Switching Points (Moral Costs)					
	Shirts Organic to Normal		Apples GER to NZ		Transportation Train to Flight	
	(1)	(2)	(3)	(4)	(5)	(6)
Info	-0.090	-0.076	-0.117	-0.091	-0.204	-0.170
	(0.147)	(0.144)	(0.140)	(0.138)	(0.157)	(0.155)
RW p-value	[0.389]	[0.523]	[0.389]	[0.523]	[0.194]	[0.315]
RE	-0.176	-0.147	-0.200	-0.182	-0.341**	-0.331**
	(0.147)	(0.144)	(0.141)	(0.140)	(0.160)	(0.158)
RW p-value	[0.210]	[0.343]	[0.152]	[0.214]	[0.016]	[0.016]
WE	-0.289**	-0.307**	-0.410***	-0.412***	-0.507***	-0.480***
	(0.148)	(0.146)	(0.142)	(0.141)	(0.158)	(0.156)
RW p-value	[0.032]	[0.016]	[0.002]	[0.002]	[0.002]	[0.002]
Control Mean	3.969	3.969	4.382	4.382	4.388	4.388
RE vs WE (F-Stat)	0.60	1.21	2.19	2.68	1.06	0.89
I vs RE (F-Stat)	0.34	0.25	0.36	0.43	0.74	1.05
I vs WE (F-Stat)	1.83	2.54	4.32**	5.31**	3.69*	4.00**
Obs.	2,215	2,215	2,269	2,269	2,228	2,228
Adj. R-Sq	0.001	0.029	0.003	0.026	0.004	0.034
Controls		X		X		X

Notes: The table presents estimates of the effect of *Info*, *RE*, and *WE* treatments on switching points in the MPLs (range 0–7) from the more sustainable to the less sustainable option as the price difference increases. Columns (1), (3), and (5) present results without controls, while columns (2), (4), and (6) include the full set of control variables. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Values in parentheses report standard errors robust to heteroskedasticity and values in square brackets show p-values adjusted for multiple hypothesis testing. Significance levels, indicated by stars, refer to the corrected p-values. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.
Source: PRESPARE

Table A.11: Switching Points (Moral Costs): Robustness

	Switching Points (Moral Costs)		
	(1) Shirts Organic to Normal	(2) Apples GER to NZ	(3) Transportation Train to Flight
Info	-0.071	-0.093	-0.175
	(0.144)	(0.138)	(0.154)
RE	-0.145	-0.182	-0.334**
	(0.145)	(0.140)	(0.158)
WE	-0.301**	-0.417***	-0.489***
	(0.146)	(0.141)	(0.156)
Errors	-0.016	-0.053	-0.029
	(0.059)	(0.061)	(0.058)
Attention Passed	-0.030	0.039	0.204
	(0.159)	(0.155)	(0.167)
Duration (Min.)	0.001	-0.000	0.000
	(0.001)	(0.001)	(0.001)
Control Mean	3.969	4.382	4.388
RE vs WE (F-Stat)	1.14	2.77*	0.95
I vs RE (F-Stat)	0.27	0.42	1.03
I vs WE (F-Stat)	2.50	5.43**	4.08**
Obs.	2,215	2,269	2,228
Adj. R-Sq	0.028	0.025	0.034
Controls	X	X	X

Notes: The table presents estimates of the effect of *Info*, *RE*, and *WE* treatments on switching points in the MPLs (range 0–7) from the more sustainable to the less sustainable option as the price difference increases. It assesses robustness with respect to number of errors in comprehension questions, a dummy for passing the second attention check, and survey completion time (in minutes). Controls include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors in parentheses are robust to heteroskedasticity. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.
Source: PRESPARE

Table A.12: Attrition Analysis

	(1) Participation both Waves
Age	0.003***
	(0.001)
Female	-0.017
	(0.017)
Tertiary Education	0.052***
	(0.019)
Full Time	0.049***
	(0.019)
Minor Children	-0.001
	(0.035)
Minor Children in Household	0.001
	(0.019)
Info	0.011
	(0.023)
RE	0.002
	(0.023)
WE	0.002
	(0.023)
Obs.	2,408

Notes: The table presents results from an attrition analysis, where a dummy variable equal to one if an individual participated in both survey waves is regressed on the full set of control variables and treatment indicators. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors are robust to heteroskedasticity.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Table A.13: Summary Statistics: Wave 2

	Panel A: Controls				
	(1) Full Sample	(2) Control	(3) Info	(4) RE	(5) WE
Age	49.33	49.08	48.95	50.19	49.13
	(14.89)	(14.85)	(14.40)	(14.96)	(15.37)
Female	0.49	0.48	0.48	0.46	0.54
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Tertiary Education	0.24	0.25	0.23	0.21	0.26
	(0.43)	(0.44)	(0.42)	(0.41)	(0.44)
Minor Children	0.26	0.26	0.26	0.26	0.27
	(0.44)	(0.44)	(0.44)	(0.44)	(0.45)
Minor Children in Household	0.42	0.44	0.41	0.40	0.44
	(0.81)	(0.88)	(0.77)	(0.77)	(0.82)
Full Time	0.45	0.44	0.45	0.43	0.49
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
	Panel B: Outcomes				
Choices:					
Normal Shirt	0.46	0.45	0.45	0.46	0.48
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Apples New Zealand	0.28	0.25	0.26	0.28	0.33
	(0.45)	(0.43)	(0.44)	(0.45)	(0.47)
Flight	0.36	0.31	0.37	0.34	0.43
	(0.48)	(0.46)	(0.48)	(0.48)	(0.50)
PANAS:					
Negative Affect	16.94	16.97	16.72	16.86	17.20
	(7.65)	(8.03)	(7.46)	(7.41)	(7.69)
Positive Affect	30.34	29.66	30.35	30.42	30.91
	(7.92)	(8.12)	(7.62)	(8.27)	(7.65)
Observations	1934	473	504	472	485

Notes: This table presents summary statistics for the full sample and each experimental group in the second survey wave. Standard errors are in parentheses. The sample consists of a *Control* group, and three treatment groups *Info*, *RE*, and *WE*. Panel A shows control variables used in the analysis which include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Panel B shows the main outcome variables of the experiment which include the three consumption choices which are again elicited in the second survey wave and the two PANAS variables.
Source: PRESPARE.

Table A.14: Balancing Table: Wave 2

	Panel A: Controls					
	(1) C vs. I	(2) C vs. RE	(3) C vs. WE	(4) I vs. RE	(5) I vs. WE	(6) RE vs. WE
Age	-0.13	1.11	0.05	1.24	0.18	-1.06
	(0.94)	(0.97)	(0.98)	(0.94)	(0.95)	(0.98)
Female	-0.00	-0.02	0.06*	-0.02	0.06*	0.08***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Tertiary Education	-0.02	-0.05*	0.01	-0.02	0.03	0.05*
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Minor Children	-0.00	-0.01	0.01	-0.00	0.01	0.02
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Minor Children in Household	-0.04	-0.04	-0.01	-0.00	0.03	0.03
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Full Time	0.02	-0.01	0.05	-0.03	0.03	0.06*
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
	Panel B: Outcomes					
Choices:						
Normal Shirt	-0.00	0.01	0.03	0.01	0.03	0.02
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Apples New Zealand	0.02	0.03	0.08***	0.01	0.06**	0.05*
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Flight	0.06*	0.04	0.12***	-0.02	0.06**	0.09***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
PANAS:						
Negative Affect	-0.25	-0.11	0.23	0.14	0.48	0.34
	(0.50)	(0.50)	(0.51)	(0.48)	(0.48)	(0.49)
Positive Affect	0.69	0.76	1.25**	0.07	0.56	0.49
	(0.50)	(0.53)	(0.51)	(0.51)	(0.49)	(0.51)
Observations	977	945	958	976	989	957

Notes: This table presents differences in means across treatment groups, comparing the *Control* group (C), *Info* (I), and the two treatment groups (*RE* and *WE*) in the second survey wave. Standard errors from t-tests are in parentheses. Panel A displays results for the control variables used in the analysis, which include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Panel B displays results for the main outcome variables of the experiment which include the three consumption choice dummies elicited again in the second survey wave, political views (a petition dummy and changes in voting intention for AfD and the Green Party, Climate Change (Voting), measured on a five-point scale (1-5), Climate Change (Problem), measured on a five-point scale (1-5)), five behavioral-change dummies, and the two PANAS measures of positive and negative affect (range 10-50). $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.
Source: PRESPARE

Table A.15: Summary Statistics and Balancing W2: Norm Correction

Panel A: Controls				
	(1) Full sample	(2) Control (NC)	(3) NC	(4) Control (NC) - NC
Age	49.33	48.87	49.78	-0.91
	(14.89)	(14.90)	(14.87)	(0.68)
Female	0.49	0.49	0.49	0.00
	(0.50)	(0.50)	(0.50)	(0.02)
Tertiary Education	0.24	0.26	0.22	0.04*
	(0.43)	(0.44)	(0.42)	(0.02)
Minor Children	0.26	0.27	0.25	0.02
	(0.44)	(0.45)	(0.43)	(0.02)
Minor Children in Household	0.42	0.45	0.39	0.06*
	(0.81)	(0.84)	(0.78)	(0.04)
Full Time	0.45	0.46	0.45	0.01
	(0.50)	(0.50)	(0.50)	(0.02)
Panel B: Outcomes				
	(1) Full sample	(2) Control	(3) Norm Corr.	(4) Control - Norm Corr.
Choices:				
Normal Shirt	0.46	0.44	0.48	-0.04
	(0.50)	(0.50)	(0.50)	(0.02)
Apples NZ	0.28	0.28	0.28	0.01
	(0.45)	(0.45)	(0.45)	(0.02)
Flight	0.36	0.36	0.36	-0.01
	(0.48)	(0.48)	(0.48)	(0.02)
Political Views:				
Yes to Petition	0.90	0.90	0.89	0.00
	(0.30)	(0.30)	(0.31)	(0.01)
Climate Change (Voting)	4.21	4.22	4.20	0.02
	(1.56)	(1.58)	(1.54)	(0.07)
Climate Change (Problem)	3.48	3.50	3.47	0.03
	(1.26)	(1.27)	(1.26)	(0.06)
No AfD anymore	0.03	0.02	0.03	-0.01
	(0.16)	(0.15)	(0.17)	(0.01)
No Grüne anymore	0.03	0.03	0.03	-0.00
	(0.17)	(0.16)	(0.18)	(0.01)
Behavior:				
Fly Less	0.54	0.52	0.55	-0.03
	(0.50)	(0.50)	(0.50)	(0.02)
Drive Less Car	0.34	0.34	0.34	-0.00
	(0.47)	(0.47)	(0.48)	(0.02)
Eat Less Meat	0.37	0.38	0.36	0.01
	(0.48)	(0.49)	(0.48)	(0.02)
Less Heating at Home	0.33	0.32	0.34	-0.02
	(0.47)	(0.47)	(0.47)	(0.02)
Own an Electric Car	0.31	0.33	0.30	0.02
	(0.46)	(0.47)	(0.46)	(0.02)
Observations	1934	965	969	1934

Notes: Columns (1) to (3) of this table present summary statistics for the full sample and each experimental group of the Norm Correction experiment in the second survey wave. Standard errors are in parentheses. The sample consists of a *Control* group and the treatment group *NC*. Column (4) reports the difference in means between the two groups, with standard errors from t-tests in parentheses. Panel A shows control variables used in the analysis which include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Panel B shows the main outcome variables of the experiment which include the three consumption choice dummies elicited again in the second survey wave, political views (a petition dummy and changes in voting intention for AfD and the Green Party, Climate Change (Voting), measured on a five-point scale (1-5), Climate Change (Problem), measured on a five-point scale (1-5)), five behavioral-change dummies, and the two PANAS measures of positive and negative affect (range 10-50). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE.

Table A.16: Results Norm Correction (No Interaction)

	Panel A: Choices		
	(1) Normal Shirt	(2) Apples NZ	(3) Flight
NC	-0.034	0.003	-0.010
	(0.022)	(0.020)	(0.021)
Info	-0.005	0.014	0.056*
	(0.031)	(0.027)	(0.030)
RE	0.003	0.031	0.042
	(0.032)	(0.028)	(0.030)
WE	0.031	0.084***	0.120***
	(0.032)	(0.029)	(0.031)
Control Mean	0.478	0.277	0.364
Obs.	1,934	1,934	1,934
Adj. R-Sq	0.017	0.034	0.042
Controls	X	X	X

	Panel B: Political Views				
	(1) Yes to Petition	(2) Climate Change (Voting)	(3) Climate Change (Problem)	(4) No AfD anymore	(5) No Grüne anymore
NC	0.004	0.014	0.033	-0.006	-0.005
	(0.014)	(0.070)	(0.057)	(0.007)	(0.008)
Info	0.015	-0.181*	-0.161**	0.006	0.015
	(0.019)	(0.096)	(0.078)	(0.010)	(0.011)
RE	-0.006	-0.181*	-0.205**	0.007	-0.002
	(0.020)	(0.100)	(0.080)	(0.010)	(0.010)
WE	0.000	-0.159	-0.211***	0.001	0.007
	(0.020)	(0.100)	(0.081)	(0.010)	(0.011)
Control Mean	0.895	4.201	3.465	0.030	0.032
Obs.	1,934	1,934	1,927	1,934	1,934
Adj. R-Sq	0.002	0.039	0.027	0.000	0.009
Controls	X	X	X	X	X

	Panel C: Willingness to Change Behavior				
	(1) Fly Less	(2) Drive Less Car	(3) Eat Less Meat	(4) Less Heating at Home	(5) Own an Electric Car
NC	-0.025	-0.003	0.009	-0.018	0.015
	(0.022)	(0.022)	(0.022)	(0.021)	(0.021)
Info	-0.053*	-0.018	-0.043	-0.064**	-0.058**
	(0.031)	(0.030)	(0.030)	(0.030)	(0.030)
RE	-0.023	-0.038	-0.030	-0.063**	-0.066**
	(0.031)	(0.031)	(0.031)	(0.031)	(0.030)
WE	-0.085***	-0.019	-0.037	-0.032	-0.033
	(0.032)	(0.031)	(0.031)	(0.031)	(0.030)
Control Mean	0.553	0.344	0.364	0.340	0.302
Obs.	1,931	1,931	1,930	1,932	1,931
Adj. R-Sq	0.052	0.011	0.038	0.011	0.021
Controls	X	X	X	X	X

Notes: The table shows estimates of the effect of the *NC* treatment while controlling for the three treatment groups of the excuse experiment. Panel A presents results for the three consumption choice dummies elicited again in the second survey wave. Panel B for political views (a petition dummy and changes in voting intention for AfD and the Green Party, Climate Change (Voting), measured on a five-point scale (1-5), Climate Change (Problem), measured on a five-point scale (1-5)). Panel C for the five behavioral-change dummies. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors are robust to heteroskedasticity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Table A.17: Results Norm Correction and Misperception

	Panel A: Choices		
	(1) Normal Shirt	(2) Apples NZ	(3) Flight
NC	-0.000	-0.027	0.002
	(0.039)	(0.043)	(0.041)
Overestimation	0.084**	0.036	0.080**
	(0.034)	(0.036)	(0.034)
NC × Overestimation	-0.048	0.039	-0.018
	(0.048)	(0.049)	(0.048)
Info	-0.002	0.014	0.055*
	(0.031)	(0.027)	(0.030)
RE	0.006	0.032	0.040
	(0.032)	(0.028)	(0.030)
WE	0.035	0.087***	0.122***
	(0.032)	(0.029)	(0.030)
Control Mean	0.478	0.277	0.364
Obs.	1,934	1,934	1,934
Adj. R-Sq	0.020	0.036	0.045
Controls	X	X	X

	Panel B: Political Views				
	(1) Yes to Petition	(2) Climate Change (Voting)	(3) Climate Change (Problem)	(4) No AfD anymore	(5) No Grüne anymore
NC	-0.013	-0.056	-0.094	0.009	-0.021
	(0.031)	(0.168)	(0.133)	(0.019)	(0.017)
Overestimation	-0.036	-0.114	-0.203**	0.009	-0.002
	(0.024)	(0.120)	(0.100)	(0.014)	(0.016)
NC × Overestimation	0.021	0.085	0.152	-0.019	0.019
	(0.035)	(0.185)	(0.148)	(0.020)	(0.019)
Info	0.015	-0.182*	-0.162**	0.006	0.015
	(0.019)	(0.096)	(0.078)	(0.010)	(0.011)
RE	-0.007	-0.183*	-0.209***	0.007	-0.001
	(0.020)	(0.100)	(0.080)	(0.010)	(0.010)
WE	-0.001	-0.164	-0.220***	0.001	0.008
	(0.020)	(0.100)	(0.081)	(0.010)	(0.011)
Control Mean	0.895	4.201	3.465	0.030	0.032
Obs.	1,934	1,934	1,927	1,934	1,934
Adj. R-Sq	0.002	0.038	0.028	-0.000	0.008
Controls	X	X	X	X	X

	Panel C: Willingness to Change Behavior				
	(1) Fly Less	(2) Drive Less Car	(3) Eat Less Meat	(4) Less Heating at Home	(5) Own an Electric Car
NC	-0.054	0.007	0.025	-0.075	0.025
	(0.054)	(0.053)	(0.053)	(0.053)	(0.051)
Overestimation	-0.031	-0.023	0.006	-0.055	0.002
	(0.041)	(0.041)	(0.042)	(0.041)	(0.040)
NC × Overestimation	0.034	-0.013	-0.020	0.069	-0.011
	(0.059)	(0.058)	(0.058)	(0.058)	(0.056)
Info	-0.052*	-0.019	-0.044	-0.064**	-0.058**
	(0.031)	(0.030)	(0.030)	(0.030)	(0.030)
RE	-0.024	-0.039	-0.031	-0.063**	-0.066**
	(0.031)	(0.031)	(0.031)	(0.031)	(0.030)
WE	-0.086***	-0.021	-0.038	-0.033	-0.034
	(0.032)	(0.031)	(0.031)	(0.031)	(0.030)
Control Mean	0.553	0.344	0.364	0.340	0.302
Obs.	1,931	1,931	1,930	1,932	1,931
Adj. R-Sq	0.051	0.010	0.037	0.011	0.020
Controls	X	X	X	X	X

Notes: The table shows estimates of the effect of the *NC* treatment fully interacted with a dummy indicating overestimation of actual selfish behaviors. The specification also controls for the three treatment groups of the excuse experiment. Panel A presents results for the three consumption choice dummies elicited again in the second survey wave. Panel B for political views (a petition dummy and changes in voting intention for AfD and the Green Party, Climate Change (Voting), measured on a five-point scale (1-5), Climate Change (Problem), measured on a five-point scale (1-5)). Panel C for the five behavioral-change dummies. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors are robust to heteroskedasticity. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.
Source: PRESPARE.

A.2 Heterogeneity (Main Experiment)

For the heterogeneity analysis, we focus on the treatment effects across the three consumption scenarios. We include all control variables and the three remaining outcome categories—descriptive norms, injunctive norms, and moral costs—in the analysis. The procedure consists of two steps. First, we estimate the specification in Equation (1.2), fully interacting treatment indicators with each covariate of interest. Second, we employ a causal random forest using the same set of variables—excluding dummies for gender, education, employment status, and minor children in the household—to explore potential non-linear patterns in treatment heterogeneity. This approach allows us to identify and rank sources of heterogeneity according to their relevance in explaining treatment effects, and we report results for the most important variables. Importantly, this analysis is exploratory. Because descriptive norms, injunctive norms, and moral costs are themselves affected by the treatments, the estimated heterogeneity patterns do not admit a causal interpretation. Hence, they should be understood as descriptive evidence on predictive variation in treatment effects.

Tables A.18 to A.20 report results from the first step, where we interact each treatment with all covariates. We find no systematic linear heterogeneous treatment effects across consumption scenarios, as significant interactions rarely appear consistently. Some indications of heterogeneity emerge for injunctive and descriptive norms, and moral costs. Overall, the results suggest that linear heterogeneity in treatment effects is limited and not systematic.

Figure A.1 presents the results of the causal random forest estimation. The procedure works as follows. First, we specify the set of covariates used to detect treatment heterogeneity. Second, the algorithm identifies the variables most informative for predicting heterogeneous treatment effects. Third, we plot conditional average treatment effects for the six most important predictors. Within each consumption scenario, predictors are ordered from left to right according to their importance for explaining heterogeneity.

Across all three consumption scenarios, a consistent pattern emerges. While the ranking of covariates varies slightly, the same variables appear among the most important three predictors: beliefs about others' consumption, age, and switching points in the MPLs (moral costs). The remaining predictors are the two social acceptance ratings and the number of minor children in the household. In line with the regression results, estimated treatment effects of *WE* are generally larger than those of *RE*, which in turn exceed those of *Info*.

Beliefs about others' consumption exhibit non-linear relationships with treatment effects, though patterns differ across scenarios. For the normal shirt, the *Info* effect declines with higher beliefs, whereas *RE* and *WE* follow a U-shaped pattern. In the apples scenario, all three treatments display an inverted U-shape. For the flight scenario, the *Info* effect remains rela-

Table A.18: Heterogeneity Analysis I

	Panel A: Age		
	(1) Normal Shirt	(2) Apples New Zealand	(3) Flight
Info	0.129	-0.064	0.003
	(0.094)	(0.082)	(0.089)
RE	0.059	0.001	0.171*
	(0.095)	(0.086)	(0.091)
WE	0.101	0.030	0.185**
	(0.094)	(0.086)	(0.090)
Info × Age	-0.002	0.001	0.001
	(0.002)	(0.002)	(0.002)
RE × Age	0.000	0.000	-0.002
	(0.002)	(0.002)	(0.002)
WE × Age	0.000	0.001	-0.001
	(0.002)	(0.002)	(0.002)
	Panel B: Female		
	(1) Normal Shirt	(2) Apples New Zealand	(3) Flight
Info	0.067*	0.003	0.063*
	(0.040)	(0.035)	(0.038)
RE	0.101**	0.006	0.068*
	(0.040)	(0.035)	(0.037)
WE	0.166***	0.087**	0.116***
	(0.041)	(0.038)	(0.040)
Info × Female	-0.044	-0.021	-0.055
	(0.056)	(0.047)	(0.053)
RE × Female	-0.060	0.029	-0.002
	(0.057)	(0.049)	(0.054)
WE × Female	-0.085	-0.007	0.012
	(0.057)	(0.051)	(0.055)
	Panel C: Tertiary Education		
	(1) Normal Shirt	(2) Apples New Zealand	(3) Flight
Info	0.064**	-0.006	0.030
	(0.032)	(0.028)	(0.031)
RE	0.089***	0.018	0.047
	(0.032)	(0.028)	(0.031)
WE	0.121***	0.063**	0.103***
	(0.033)	(0.029)	(0.032)
Info × Tertiary Education	-0.078	-0.011	0.021
	(0.064)	(0.051)	(0.060)
RE × Tertiary Education	-0.079	0.003	0.087
	(0.067)	(0.055)	(0.064)
WE × Tertiary Education	0.005	0.083	0.087
	(0.066)	(0.058)	(0.063)
Control Mean	0.363	0.225	0.295
Obs.	2,408	2,408	2,408
Adj. R-Sq	0.020	0.032	0.031
Controls	X	X	X

Notes: The table reports estimates from regressions that interact the treatment indicators with individual characteristics. The dependent variables are the consumption choice dummies. Panel A reports results for age, Panel B for the female dummy, and Panel C for the tertiary education dummy. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors are robust to heteroskedasticity. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.
Source: PRESPARE.

Table A.19: Heterogeneity Analysis II

	Panel A: Minor Children		
	(1) Normal Shirt	(2) Apples New Zealand	(3) Flight
Info	0.045	0.005	0.046
	(0.032)	(0.027)	(0.030)
RE	0.078**	0.032	0.079**
	(0.033)	(0.028)	(0.031)
WE	0.134***	0.098***	0.142***
	(0.033)	(0.029)	(0.031)
Info × Minor Children	0.003	-0.049	-0.038
	(0.064)	(0.055)	(0.062)
RE × Minor Children	-0.023	-0.049	-0.048
	(0.065)	(0.056)	(0.063)
WE × Minor Children	-0.046	-0.056	-0.067
	(0.064)	(0.058)	(0.063)
	Panel B: Minor Children in Household		
	(1) Normal Shirt	(2) Apples New Zealand	(3) Flight
Info	0.043	0.001	0.037
	(0.031)	(0.026)	(0.029)
RE	0.079**	0.034	0.089***
	(0.032)	(0.027)	(0.030)
WE	0.136***	0.099***	0.142***
	(0.032)	(0.029)	(0.031)
Info × Minor Children in Household	0.007	-0.021	-0.002
	(0.034)	(0.030)	(0.033)
RE × Minor Children in Household	-0.016	-0.035	-0.052
	(0.035)	(0.030)	(0.033)
WE × Minor Children in Household	-0.033	-0.038	-0.042
	(0.034)	(0.030)	(0.033)
Control Mean	0.363	0.225	0.295
Obs.	2,408	2,408	2,408
Adj. R-Sq	0.019	0.032	0.031
Controls	X	X	X

Notes: The table reports estimates from regressions that interact the treatment indicators with individual characteristics. The dependent variables are the consumption choice dummies. Panel A reports results for a dummy indicating having minor children, and Panel B for the number of minor children in the household. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors are robust to heteroskedasticity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE.

tively stable, while *RE* and *WE* again show a U-shaped pattern. Age also displays heterogeneous patterns. Treatment effects generally decline with age for the normal shirt, increase with age for apples, and rise with age for *Info* and *WE* in the flight scenario, while *RE* follows a U-shaped pattern. Switching points in the MPLs (moral costs) show weaker but systematic variation. Effects for the normal shirt increase slightly with higher switching points, remain largely stable in the apples scenario (with *RE* increasing initially), and follow an inverted U-shape for the flight.

Social acceptance of the selfish option tends to reduce treatment effects for the normal shirt, remains largely stable for apples, and slightly decreases effects of *Info* and *RE* for the flight, while *WE* increases slightly. In contrast, higher social acceptance of the sustainable option

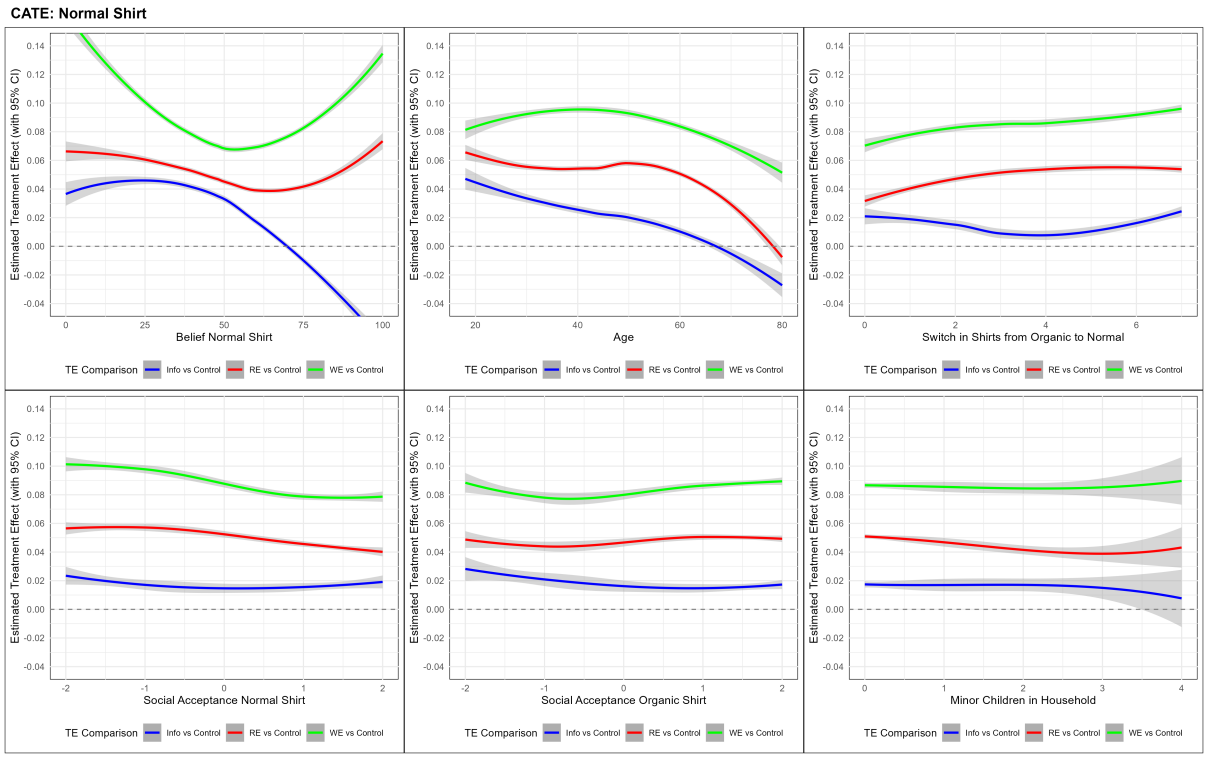
Table A.20: Heterogeneity Analysis III

	Panel A: Belief		
	(1) Normal Shirt	(2) Apples New Zealand	(3) Flight
Info	0.108	-0.018	0.061
	(0.068)	(0.048)	(0.059)
RE	0.004	-0.040	0.078
	(0.068)	(0.050)	(0.062)
WE	0.087	-0.012	-0.060
	(0.073)	(0.055)	(0.062)
Info × Belief	-0.001	0.000	-0.001
	(0.001)	(0.001)	(0.001)
RE × Belief	0.001	0.001	-0.000
	(0.001)	(0.001)	(0.001)
WE × Belief	0.000	0.002*	0.003***
	(0.001)	(0.001)	(0.001)
	Panel B: Norms Non-Selfish		
	(1) Normal Shirt	(2) Apples New Zealand	(3) Flight
Info	0.037	-0.017	0.019
	(0.027)	(0.024)	(0.026)
RE	0.062**	0.006	0.054**
	(0.028)	(0.025)	(0.027)
WE	0.118***	0.072***	0.112***
	(0.028)	(0.025)	(0.027)
Info × Norms Non-Selfish	-0.014	-0.014	-0.010
	(0.021)	(0.017)	(0.018)
RE × Norms Non-Selfish	-0.025	-0.012	0.007
	(0.020)	(0.018)	(0.019)
WE × Norms Non-Selfish	-0.040*	-0.003	-0.003
	(0.021)	(0.019)	(0.019)
	Panel C: Norms Selfish		
	(1) Normal Shirt	(2) Apples New Zealand	(3) Flight
Info	0.062*	-0.033	0.073**
	(0.035)	(0.036)	(0.036)
RE	0.065*	-0.055	0.045
	(0.036)	(0.035)	(0.035)
WE	0.119***	0.040	0.109***
	(0.035)	(0.037)	(0.035)
Info × Norms Selfish	-0.023	0.017	-0.041*
	(0.023)	(0.022)	(0.022)
RE × Norms Selfish	0.003	0.066***	0.019
	(0.023)	(0.021)	(0.022)
WE × Norms Selfish	-0.007	0.032	0.007
	(0.023)	(0.022)	(0.022)
	Panel D: Switching Points		
	(1) Normal Shirt	(2) Apples New Zealand	(3) Flight
Info	0.043	0.020	0.062
	(0.049)	(0.058)	(0.050)
RE	0.047	0.010	0.126***
	(0.051)	(0.059)	(0.048)
WE	0.079	0.101*	0.154***
	(0.049)	(0.056)	(0.047)
Info × Switching Points	0.001	-0.007	-0.010
	(0.010)	(0.010)	(0.009)
RE × Switching Points	0.006	-0.001	-0.021**
	(0.010)	(0.011)	(0.009)
WE × Switching Points	0.008	-0.012	-0.020**
	(0.010)	(0.010)	(0.008)
Control Mean	0.363	0.225	0.295
Obs.	2,215	2,269	2,228
Adj. R-Sq	0.226	0.202	0.360
Controls	X	X	X

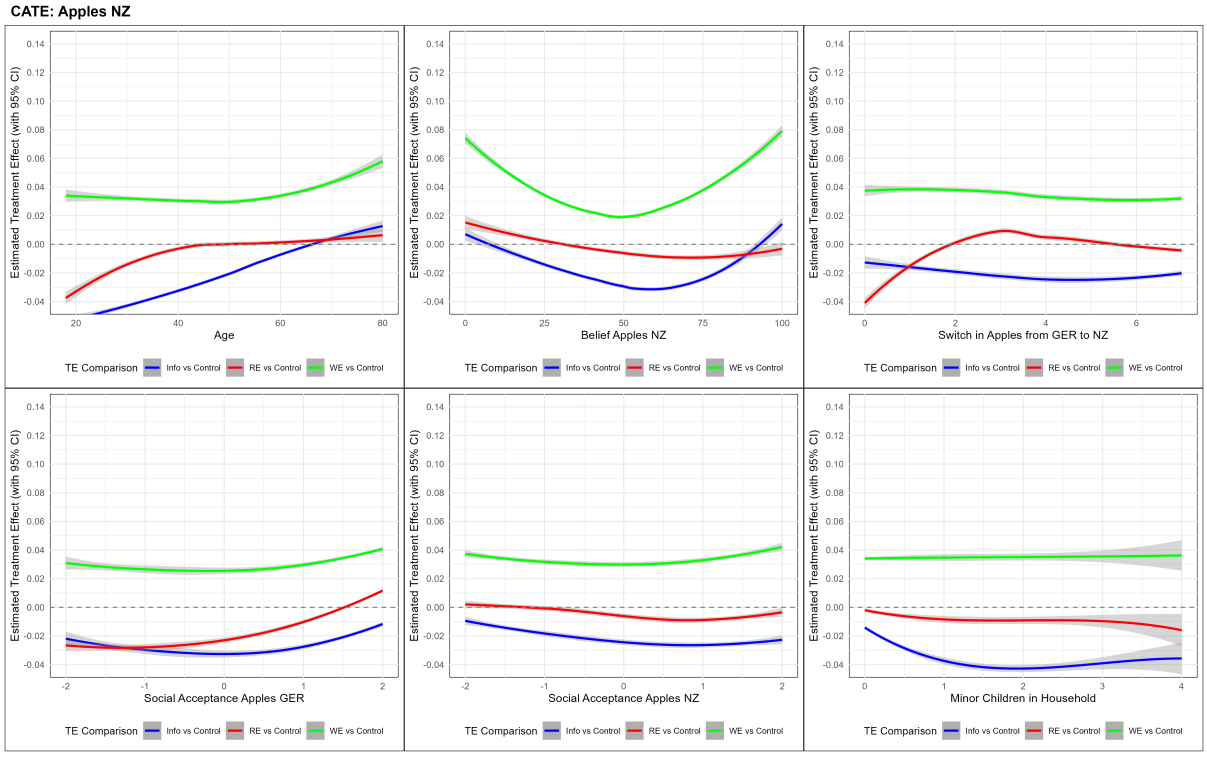
Notes: The table reports estimates from regressions that interact the treatment indicators with norms and moral costs. The dependent variables are the consumption choice dummies. Panel A reports results for beliefs about other participants' consumption choices (0–100 percent). Panel B presents social acceptance ratings for non-selfish behavior, and Panel C social acceptance ratings for selfish behavior (-2 "Socially very inappropriate", -1 "Socially somewhat inappropriate", 1 "Socially somewhat appropriate", and 2 "Socially very appropriate"). Panel D reports results for moral costs measured by switching points in the MPLs (range 0–7) from the more sustainable to the less sustainable option as the price difference increases. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors are robust to heteroskedasticity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE.

strengthens treatment effects for apples but weakens them for the flight, while effects remain largely stable for the normal shirt. The number of minor children shows little evidence of heterogeneous treatment effects.

Overall, the causal random forest reveals heterogeneity along several behavioral dimensions—particularly beliefs, age, and moral costs—while preserving the same qualitative ranking of treatment effects observed in the regression analysis. These patterns should be interpreted as exploratory rather than causal.

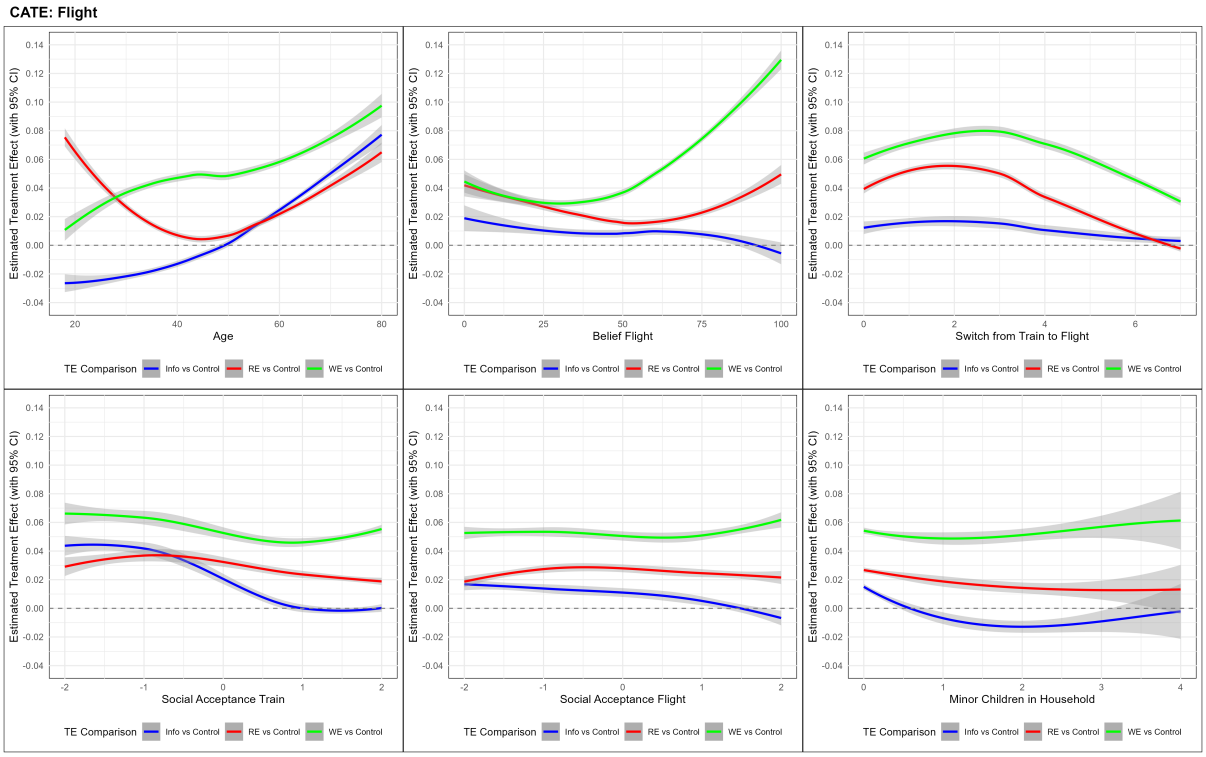


(a) Conditional Average Treatment Effects: Normal Shirt.



(b) Conditional Average Treatment Effects: Apples New Zealand.

Figure A.1: Conditional Average Treatment Effects across Product Domains



(c) Conditional Average Treatment Effects: Flight.

Figure A.1: Conditional Average Treatment Effects across Product Domains (cont.)

Notes: The figure presents results from a causal random forest estimating heterogeneous treatment effects for the three treatments relative to *Control*. Each panel shows the six most relevant covariates for explaining treatment effect heterogeneity, ranked by importance. The y-axis depicts the estimated treatment effect (conditional average treatment effect, CATE), and the x-axis shows the corresponding values of the covariate. Colors indicate treatments: *Info* (blue), *RE* (red), and *WE* (green). These estimates represent model-based estimates of how the treatment effect varies across covariate values.
Source: PRESPARE.

Appendix B

Supplementary Materials Chapter 2

B.1 Figures

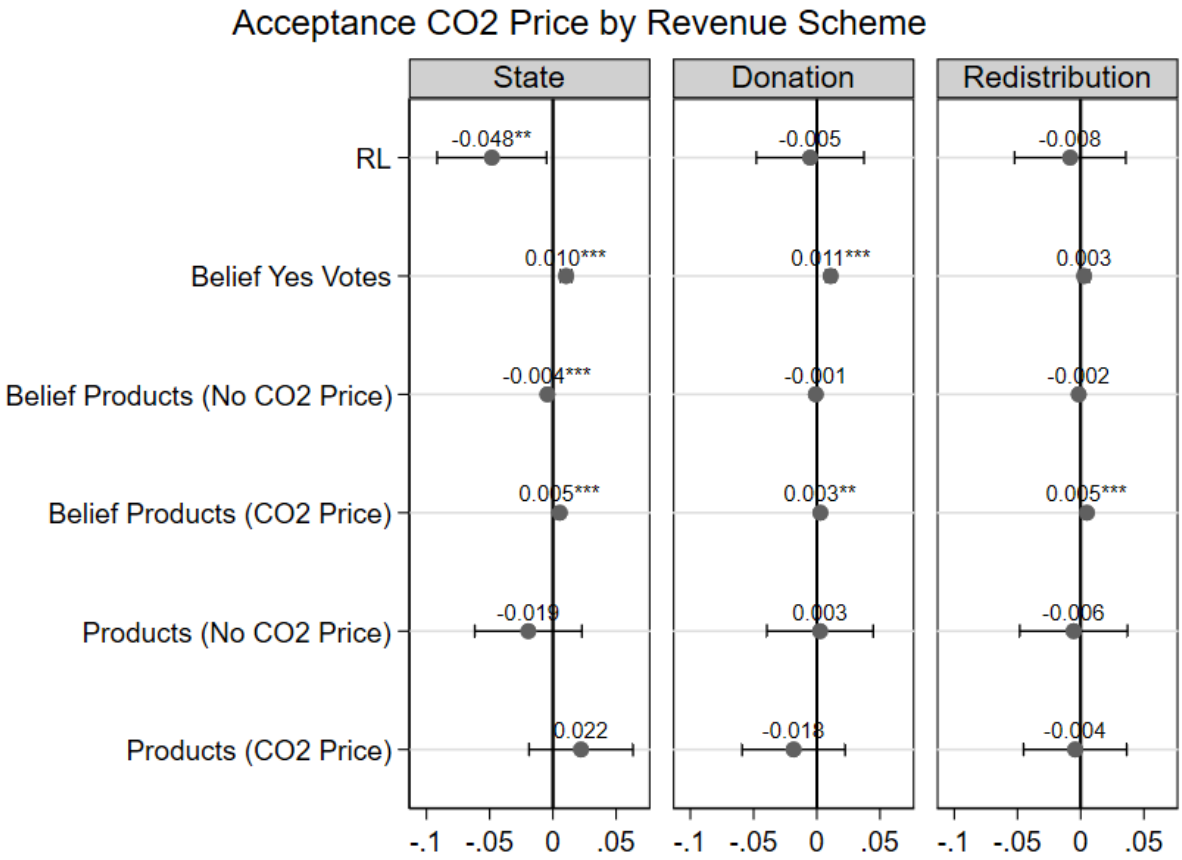


Figure B.1: Determinants of CO_2 Price Acceptance

Notes: This figure presents estimates of how the voting decision for the revenue schemes (state budget, donation to climate projects, and redistribution) correlate with beliefs about others' voting behavior, beliefs about aggregate buying behavior, and own purchasing decisions. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. Standard errors are heteroskedasticity robust. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

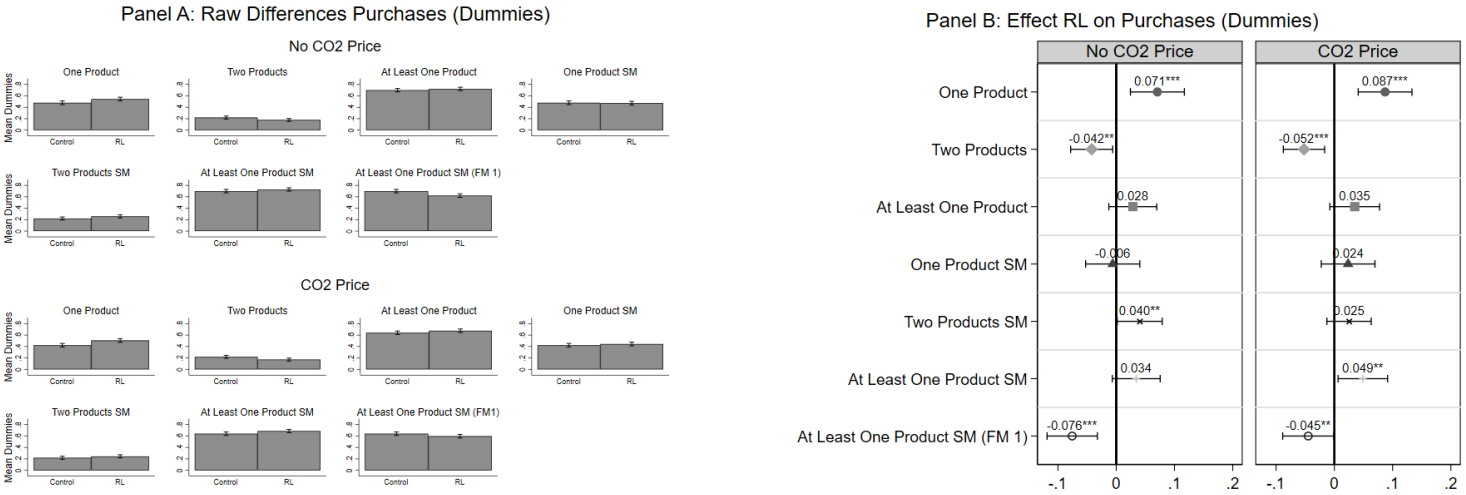


Figure B.2: Descriptives and Regression Coefficients for Purchasing Decisions (Dummies)

Notes: Panel A displays raw means of purchasing decision dummies for FM and SM behavior for the *Control* and *RL* groups, with 95% confidence intervals shown as vertical lines. Panel B presents estimates of the effect of the *RL* treatment on these purchasing decision dummies. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. Standard errors are heteroskedasticity robust. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

B.2 Tables

Table B.1: Attrition Analysis

	(1) Participation both Waves
Age	0.002***
	(0.001)
Female	-0.017
	(0.017)
Tertiary Education	0.037*
	(0.020)
Log(Net Income)	0.023*
	(0.013)
Full Time	0.035*
	(0.020)
Minor Children	-0.024
	(0.036)
Minor Children in Household	0.009
	(0.019)
Green Party	0.015
	(0.025)
AfD	-0.021
	(0.022)
CC Human Made	-0.021
	(0.020)
CC Exists	-0.021
	(0.022)
No Market Intervention by Gov.	0.010
	(0.018)
Yes CO2 Regulation by Gov.	0.003
	(0.019)
Willingness Risks	-0.008**
	(0.003)
Willingness Postpone	0.005
	(0.004)
Locus of Control	0.015
	(0.022)
Obs.	2,405

Notes: The table presents results from an attrition analysis, where a dummy variable equal to one if an individual participated in both survey waves is regressed on the full set of control variables. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, and measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. Standard errors are robust to heteroskedasticity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Table B.2: Comparison Survey Data with Eurostat Data

	(1) Survey Data	(2) Eurostat
Female: 18–35 Years	0.110	0.134
Female: 36–45 Years	0.086	0.081
Female: 46–55 Years	0.092	0.091
Female: 56–65 Years	0.104	0.100
Female: 66–80 Years	0.100	0.095
Male: 18–35 Years	0.138	0.145
Male: 36–45 Years	0.090	0.083
Male: 46–55 Years	0.096	0.091
Male: 56–65 Years	0.106	0.098
Male: 66–80 Years	0.079	0.083
Employed (%)	0.837	0.813
Median Net Income (€)	2250.000	2301.000
Low Education	0.288	0.300
Middle Education	0.303	0.310
Higher Education	0.408	0.390
Baden-Württemberg	0.136	0.134
Bavaria	0.158	0.159
Berlin	0.045	0.045
Brandenburg	0.030	0.030
Bremen	0.005	0.008
Hamburg	0.024	0.023
Hesse	0.071	0.076
Mecklenburg-Western Pomerania	0.018	0.019
Lower Saxony	0.098	0.096
North Rhine-Westphalia	0.220	0.215
Rhineland-Palatinate	0.050	0.050
Saarland	0.011	0.012
Saxony	0.054	0.048
Saxony-Anhalt	0.022	0.026
Schleswig-Holstein	0.034	0.035
Thuringia	0.024	0.025

Notes: This table compares the second wave survey sample to official population statistics from Eurostat. Low education refers to Hauptschule/Mittelschule or equivalent. Middle education refers to Realschule or equivalent. Higher education refers to Abitur or higher. Employment excludes individuals who are already retired. All variables are expressed as shares unless otherwise indicated; income refers to the median.

Table B.3: Variable Definitions

Panel A. Controls	
Variable	**Definition**
Age	Respondent's age in years.
Female	Dummy equal to 1 if respondent is female, 0 otherwise.
Personal Net Income (in €)	Monthly personal net income in €.
Tertiary Education	Dummy equal to 1 if respondent holds a tertiary (any form of university-level education) degree, 0 otherwise.

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Table B.3 – continued from previous page

Variable	Definition
Minor Children	Dummy equal to 1 if respondent has at least one child under the age of 18, 0 otherwise.
Minor Children in Household	Number of minor children under 18 living in the respondent's household.
Full Time	Dummy equal to 1 if respondent is employed full time, 0 otherwise.
Green Party	Dummy equal to 1 if respondent intends to vote for the Green Party (Bündnis '90/Grüne).
AfD	Dummy equal to 1 if respondent intends to vote for the AfD (Alternative für Deutschland).
Locus of Control	Score potentially ranging from 0 to 5, measuring the extent to which individuals perceive control over life outcomes (higher values indicate greater internal control).
Willingness Risks	Self-reported willingness to take risks, measured on an 11-point Likert scale from 0 (not at all willing) to 10 (very willing).
Willingness Postpone	Self-reported willingness to delay gratification, measured on an 11-point Likert scale from 0 (not willing) to 10 (very willing).
CC Human Made	Dummy equal to 1 if respondent agrees or strongly agrees that climate change is caused by humans.
CC Exists	Dummy equal to 1 if respondent agrees or strongly agrees that climate change exists.
Yes CO2 Regulation by Gov.	Dummy equal to 1 if respondent agrees or strongly agrees that governments should regulate CO_2 emissions.
Errors (Q1 - Q4)	Number of errors in comprehension questions 1 to 4.
Panel B. Outcome Variables	
Main Voting Outcomes:	Binary variables of voting decisions for all three revenue schemes.
Yes CO2 Price (State)	Dummy equal to 1 if the respondent voted "Yes" for the CO_2 price with revenues going to the state budget, 0 otherwise.
Yes CO2 Price (Donation)	Dummy equal to 1 if the respondent voted "Yes" for the CO_2 price with revenues being donated to environmental projects, 0 otherwise.
Yes CO2 Price (Redistribution)	Dummy equal to 1 if the respondent voted "Yes" for the CO_2 price with revenues being redistributed among participants, 0 otherwise.

Continued on next page

Table B.3 – continued from previous page

Variable	Definition
Panel C. Experimental Variables	
Beliefs Voting Behavior:	Beliefs about voting behavior in own voting group ranging from 0 to 24.
Yes-Votes (State)	Belief about number of "Yes" votes for state revenue scheme.
Yes-Votes (Donation)	Belief about number of "Yes" votes for donation revenue scheme.
Yes-Votes (Redistribution)	Belief about number of "Yes" votes for redistribution revenue scheme.
Switching Points (CO2 Price):	Measured in Euros (0.50, 1, 1.50, 2) indicating the price level at which respondents switch in Multiple Price Lists (MPLs)
Switching Point CO2-Price (State)	Price/Switching point where the respondent changes from in favor to against under state revenue scheme.
Switching Point CO2-Price (Donation)	Price/Switching point where the respondent changes from in favor to against under Donation revenue scheme.
Switching Point CO2-Price (Redistribution)	Price/Switching point where the respondent changes from in favor to against under Redistribution revenue scheme.
Consumption Behavior:	Variables indicating respondent's consumption choices with and without CO_2 price
Products	Number of products purchased without CO_2 price.
Products (CO2-Price)	Number of products purchased with CO_2 price applied.
Products SM	Number of products purchased without CO_2 price applied.
Products SM (CO2-Price)	Number of products purchased with CO_2 price applied.
One Product	Dummy equal to 1 if respondent purchased exactly one product without CO_2 price, 0 otherwise.
One Product (CO2-Price)	Dummy equal to 1 if respondent purchased exactly one product with CO_2 price, 0 otherwise.
At Least One Product	Dummy equal to 1 if respondent purchased at least one product without CO_2 price, 0 otherwise.
At Least One Product (CO2-Price)	Dummy equal to 1 if respondent purchased at least one product with CO_2 price, 0 otherwise.
Two Products	Dummy equal to 1 if respondent purchased exactly two products without CO_2 price, 0 otherwise.
Two Products (CO2-Price)	Dummy equal to 1 if respondent purchased exactly two products with CO_2 price, 0 otherwise.
One Product SM	Dummy equal to 1 if respondent purchased exactly one sustainable product without CO_2 price, 0 otherwise.

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Table B.3 – continued from previous page

Variable	Definition
One Product SM (CO2-Price)	Dummy equal to 1 if respondent purchased exactly one sustainable product with CO_2 price, 0 otherwise.
At Least One Product SM	Dummy equal to 1 if respondent purchased at least one sustainable product without CO_2 price, 0 otherwise.
At Least One Product SM (CO2-Price)	Dummy equal to 1 if respondent purchased at least one sustainable product with CO_2 price, 0 otherwise.
At Least One Product SM (FM 1)	Dummy equal to 1 if respondent purchased at least one sustainable product in food market 1 without CO_2 price, 0 otherwise.
At Least One Product SM (CO2-Price; FM 1)	Dummy equal to 1 if respondent purchased at least one sustainable product in food market 1 with CO_2 price, 0 otherwise.
Two Products SM	Dummy equal to 1 if respondent purchased exactly two sustainable products without CO_2 price, 0 otherwise.
Two Products SM (CO2-Price)	Dummy equal to 1 if respondent purchased exactly two sustainable products with CO_2 price, 0 otherwise.
Social Acceptance:	Social acceptance rating of product purchase behavior ranging from –2 (“Socially very inappropriate”) to +2 (“Socially very appropriate”)
No Product (No CO2-Price)	Social acceptance rating when no product was purchased without CO_2 price.
One Product (No CO2-Price)	Social acceptance rating when exactly one product was purchased without CO_2 price.
Two Products (No CO2-Price)	Social acceptance rating when exactly two products were purchased without CO_2 price.
No Product (CO2-Price)	Social acceptance rating when no product was purchased with CO_2 price.
One Product (CO2-Price)	Social acceptance rating when exactly one product was purchased with CO_2 price.
Two Products (CO2-Price)	Social acceptance rating when exactly two products were purchased with CO_2 price.
Beliefs Consumption Behavior:	Beliefs about number of individuals buying products with and without CO_2 price in the voting group; values range from 0 to 24 (per product type) or 0 to 48 (aggregated).
One Product (No CO2 Price)	Belief about how many individuals bought exactly one product without CO_2 price.
Two Products (No CO2 Price)	Belief about how many individuals bought exactly two products without CO_2 price.

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Table B.3 – continued from previous page

Variable	Definition
No Products (No CO2 Price)	Belief about how many individuals bought no products without CO_2 price.
One Product (CO2 Price)	Belief about how many individuals bought exactly one product with CO_2 price.
Two Products (CO2 Price)	Belief about how many individuals bought exactly two products with CO_2 price.
No Products (CO2 Price)	Belief about how many individuals bought no products with CO_2 price.
Total Products (No CO2 Price)	Belief about total number of products purchased without CO_2 price across all individuals.
Total Products (CO2 Price)	Belief about total number of products purchased with CO_2 price across all individuals.

Table B.4: Summary Statistics & Balancing

	Panel A: Controls			
	Mean and Standard Deviation			Differences
	(1) Full sample	(2) Control	(3) RL	(4) Control - RL
Age	49.26	49.28	49.24	0.04
	(14.87)	(14.70)	(15.04)	(0.69)
Female	0.49	0.50	0.48	0.02
	(0.50)	(0.50)	(0.50)	(0.02)
Tertiary Education	0.24	0.23	0.25	-0.02
	(0.43)	(0.42)	(0.43)	(0.02)
Personal Net Income (in €)	2583.33	2545.31	2622.81	-77.50
	(1724.85)	(1696.61)	(1753.74)	(79.94)
Full Time	0.45	0.44	0.47	-0.04
	(0.50)	(0.50)	(0.50)	(0.02)
Minor Children	0.26	0.26	0.26	0.00
	(0.44)	(0.44)	(0.44)	(0.02)
Minor Children in Household	0.42	0.42	0.43	-0.01
	(0.81)	(0.80)	(0.82)	(0.04)
Green Party	0.12	0.12	0.13	-0.00
	(0.33)	(0.33)	(0.33)	(0.02)
AfD	0.22	0.22	0.22	0.00
	(0.41)	(0.41)	(0.41)	(0.02)
CC Human Made	0.62	0.61	0.63	-0.02
	(0.48)	(0.49)	(0.48)	(0.02)
CC Exists	0.72	0.72	0.73	-0.01
	(0.45)	(0.45)	(0.45)	(0.02)
Yes CO2 Regulation by Gov.	0.51	0.51	0.51	-0.00
	(0.50)	(0.50)	(0.50)	(0.02)
Locus of Control	2.35	2.36	2.34	0.02
	(0.39)	(0.39)	(0.40)	(0.02)
Willingness Risks	4.06	4.06	4.06	-0.00
	(2.58)	(2.55)	(2.61)	(0.12)
Willingness Postpone	5.48	5.48	5.48	0.01
	(2.45)	(2.52)	(2.39)	(0.11)

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Table B.4: (continued from previous page)

Errors (Q1)	0.68	0.46	0.92	-0.46***
	(1.39)	(1.19)	(1.54)	(0.06)
Errors (Q2)	0.79	0.72	0.87	-0.16**
	(1.48)	(1.33)	(1.62)	(0.07)
Errors (Q3)	0.59	0.60	0.58	0.03
	(1.38)	(1.46)	(1.30)	(0.06)
Errors (Q4)	0.74	0.78	0.70	0.07
	(1.36)	(1.45)	(1.26)	(0.06)
		Panel B: Main Variables Experiment		
Voting Behaviour:				
Yes (State)	0.38	0.40	0.37	0.03
	(0.49)	(0.49)	(0.48)	(0.02)
Yes (Donation)	0.59	0.60	0.58	0.01
	(0.49)	(0.49)	(0.49)	(0.02)
Yes (Redistribution)	0.56	0.57	0.56	0.01
	(0.50)	(0.50)	(0.50)	(0.02)
Switching Point CO2-Price (State)	1.02	1.06	0.99	0.06**
	(0.58)	(0.59)	(0.58)	(0.03)
Switching Point CO2-Price (Donation)	1.37	1.38	1.37	0.01
	(0.62)	(0.63)	(0.61)	(0.03)
Switching Point CO2-Price (Redistribution)	1.33	1.33	1.33	0.00
	(0.62)	(0.63)	(0.61)	(0.03)
Yes-Votes (State)	5.61	5.49	5.72	-0.23
	(5.07)	(4.92)	(5.22)	(0.23)
Yes-Votes (Donation)	10.81	10.81	10.81	0.00
	(5.62)	(5.61)	(5.63)	(0.26)
Yes-Votes (Redistribution)	12.37	12.18	12.56	-0.39
	(5.48)	(5.32)	(5.63)	(0.25)
Consumption Behaviour:				
Products (No CO2-Price)	0.91	0.92	0.90	0.02
	(0.69)	(0.72)	(0.67)	(0.03)
Products (CO2-Price)	0.85	0.86	0.85	0.01
	(0.72)	(0.75)	(0.69)	(0.03)
Products SM (No CO2-Price)	0.95	0.92	0.99	-0.07**
	(0.72)	(0.72)	(0.73)	(0.03)
Products SM (CO2-Price)	0.89	0.86	0.93	-0.07**
	(0.75)	(0.75)	(0.74)	(0.03)
One Product (No CO2-Price)	0.51	0.48	0.54	-0.06***
	(0.50)	(0.50)	(0.50)	(0.02)
One Product (CO2-Price)	0.46	0.42	0.51	-0.08***
	(0.50)	(0.49)	(0.50)	(0.02)
At Least One Product (No CO2-Price)	0.71	0.70	0.72	-0.02
	(0.45)	(0.46)	(0.45)	(0.02)
At Least One Product (CO2-Price)	0.66	0.64	0.68	-0.04
	(0.47)	(0.48)	(0.47)	(0.02)
Two Products (No CO2-Price)	0.20	0.22	0.18	0.04**
	(0.40)	(0.41)	(0.38)	(0.02)
Two Products (CO2-Price)	0.20	0.22	0.17	0.05***
	(0.40)	(0.41)	(0.38)	(0.02)
One Product SM (No CO2-Price)	0.48	0.48	0.47	0.01
	(0.50)	(0.50)	(0.50)	(0.02)
One Product SM (CO2-Price)	0.43	0.42	0.44	-0.02
	(0.50)	(0.49)	(0.50)	(0.02)
At Least One Product SM (No CO2-Price)	0.71	0.70	0.73	-0.03
	(0.45)	(0.46)	(0.44)	(0.02)
At Least One Product SM (CO2-Price)	0.66	0.64	0.69	-0.05**
	(0.47)	(0.48)	(0.46)	(0.02)
At Least One Product SM (No CO2-Price; FM 1)	0.66	0.70	0.62	0.08***
	(0.47)	(0.46)	(0.49)	(0.02)
At Least One Product SM (CO2-Price; FM1)	0.62	0.64	0.60	0.04*

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Table B.4: (continued from previous page)

	(0.49)	(0.48)	(0.49)	(0.02)
Two Products SM (No CO2-Price)	0.24	0.22	0.26	-0.04*
	(0.43)	(0.41)	(0.44)	(0.02)
Two Products SM (CO2-Price)	0.23	0.22	0.24	-0.02
	(0.42)	(0.41)	(0.43)	(0.02)
Social Acceptance:				
No Product (No CO2-Price)	0.65	0.65	0.65	-0.01
	(1.42)	(1.43)	(1.41)	(0.07)
One Product (No CO2-Price)	0.58	0.58	0.57	0.00
	(1.23)	(1.21)	(1.24)	(0.06)
Two Products (No CO2-Price)	-0.14	-0.06	-0.22	0.16***
	(1.34)	(1.35)	(1.33)	(0.06)
No Product (CO2-Price)	0.70	0.74	0.66	0.08
	(1.39)	(1.37)	(1.41)	(0.06)
One Product (CO2-Price)	0.60	0.61	0.59	0.02
	(1.19)	(1.20)	(1.19)	(0.06)
Two Products (CO2-Price)	-0.04	0.01	-0.09	0.10
	(1.34)	(1.35)	(1.32)	(0.06)
No Products (No CO2 Price)	5.73	5.92	5.52	0.40*
	(4.64)	(4.63)	(4.65)	(0.22)
Beliefs:				
One Product (No CO2 Price)	10.33	9.93	10.76	-0.83***
	(4.65)	(4.41)	(4.86)	(0.21)
Two Products (No CO2 Price)	7.94	8.15	7.72	0.43**
	(4.49)	(4.33)	(4.64)	(0.21)
No Products (CO2 Price)	6.23	6.60	5.85	0.75***
	(5.03)	(5.19)	(4.83)	(0.23)
One Product (CO2 Price)	10.16	9.74	10.59	-0.85***
	(4.67)	(4.56)	(4.75)	(0.22)
Two Products (CO2 Price)	7.61	7.66	7.56	0.10
	(4.45)	(4.28)	(4.61)	(0.21)
Total Products (No CO2 Price)	25.70	26.23	25.14	1.09**
	(9.51)	(7.81)	(10.98)	(0.44)
Total Products (CO2 Price)	24.56	25.06	24.04	1.03**
	(9.53)	(8.36)	(10.59)	(0.44)
Observations	1863	949	914	1863

Notes: Columns (1) - (3) of this table present summary statistics for the full sample, and the two experimental groups. The sample consists of a *Control* group , and the treatment group *RL*. Standard errors are in parentheses. Column (4) presents differences in means across experimental groups, comparing the *Control* group and the treatment group *RL*. Standard errors from t-tests are in parentheses. Panel A shows control variables used in the analysis which include demographic characteristics (age, gender, income), employment status, voting intentions regarding political parties, economic preferences and psychological characteristics (risk preferences, time preferences, and locus of control) and views on climate change (CC). Panel B shows the main outcome variables of the experiment which include voting behavior, purchasing decisions, social acceptance ratings of purchasing decisions, and beliefs about the other participants' purchasing decisions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.5: CO_2 Price Acceptance & Comprehension Error Structure

	CO2 Price Acceptance (State)				
	(1) Errors	(2) Errors and Controls	(3) Errors Q1 = 0	(4) Errors Q2 = 0	(5) Errors Q1 = 0 Errors Q2 = 0
	(1)	(2)	(3)	(4)	(5)
RL	-0.046**	-0.049**	-0.051*	-0.059**	-0.068**
	(0.023)	(0.022)	(0.027)	(0.028)	(0.030)
Control Mean	0.401	0.401	0.385	0.386	0.384
Obs.	1,863	1,863	1,267	1,193	1,017
Adj. R-Sq	0.006	0.058	0.002	0.003	0.004
Controls		X			
Errors	X	X			

Notes: This table reports estimates of the effect of the *RL* treatment on voting decisions regarding the state budget, accounting for comprehension errors. Column (1) excludes controls for errors in the first two comprehension questions, column (2) includes these controls, column (3) restricts the sample to respondents without errors in question 1, column (4) to those without errors in question 2, and column (5) to respondents without errors in either question. All columns include the full set of controls. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, and measures for willingness to take risks and willingness to postpone a reward for future benefits. Standard errors are heteroskedasticity robust. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Table B.6: CO_2 Price Acceptance & Switching Points MPL

	Panel A: CO2 Price Acceptance					
	(1) State	(2) State	(3) Donation	(4) Donation	(5) Redistribution	(6) Redistribution
RL	-0.035	-0.049**	-0.011	-0.007	-0.011	-0.011
	(0.023)	(0.022)	(0.023)	(0.022)	(0.023)	(0.022)
Control Mean	0.401	0.401	0.595	0.595	0.568	0.568
Obs.	1,863	1,863	1,863	1,863	1,863	1,863
Adj. R-Sq	0.001	0.058	-0.000	0.116	-0.000	0.089
Controls		X		X		X
Errors		X		X		X
Romano-Wolf p-val		0.074		0.873		0.873
	Panel B: Switching Points MPL					
	(1) State	(2) State	(3) Donation	(4) Donation	(5) Redistribution	(6) Redistribution
RL	-0.064**	-0.084***	-0.007	-0.007	-0.003	-0.006
	(0.029)	(0.029)	(0.031)	(0.028)	(0.032)	(0.030)
Control Mean	1.055	1.055	1.378	1.378	1.330	1.330
Obs.	1,576	1,576	1,603	1,603	1,529	1,529
Adj. R-Sq	0.002	0.113	-0.001	0.211	-0.001	0.147
Controls		X		X		X
Errors		X		X		X
Romano-Wolf p-val		0.014		0.962		0.962

Notes: Panel A reports estimates of the effect of the *RL* treatment on voting decisions for the three revenue schemes: state budget, donations to climate projects, and redistribution. Panel B reports the corresponding effects on CO_2 switching points. Columns (1), (3), and (5) present specifications without additional covariates, while Columns (2), (4), and (6) include the full set of controls. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. The last row in each panel reports Romano–Wolf adjusted p-values (1,000 bootstraps). Standard errors are heteroskedasticity robust. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Table B.7: Purchases

	Panel A: Purchases (No CO2 Price)			
	(1)	(2)	(3)	(4)
	Products	Products	Products SM	Products SM
RL	-0.019	-0.014	0.066**	0.075**
	(0.032)	(0.032)	(0.033)	(0.033)
Control Mean	0.920	0.920	0.920	0.920
Obs.	1,863	1,863	1,863	1,863
Adj. R-Sq	-0.000	0.065	0.002	0.068
Controls		X		X
Errors		X		X
Romano-Wolf p-val		0.669		0.053
	Panel B: Purchases (CO2 Price)			
	(1)	(2)	(3)	(4)
	Products	Products	Products SM	Products SM
RL	-0.013	-0.017	0.071**	0.074**
	(0.033)	(0.032)	(0.035)	(0.034)
Control Mean	0.860	0.860	0.860	0.860
Obs.	1,863	1,863	1,863	1,863
Adj. R-Sq	-0.000	0.089	0.002	0.085
Controls		X		X
Errors		X		X
Romano-Wolf p-val		0.584		0.053

Notes: Panel A reports estimates of the effect of the *RL* treatment on product purchases of FMs and SMs without a CO_2 price, while Panel B does so with a CO_2 price. Columns (1), and (3) present specifications without additional covariates, while columns (2), and (4) include the full set of controls. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. The last row in each panel reports Romano–Wolf adjusted p-values (1,000 bootstraps). Standard errors are heteroskedasticity robust. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Table B.8: Purchasing Decisions (Dummies)

	Panel A: Purchases (Dummies; No CO2 Price)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	One Product	One Product	Two Products	Two Products	One Product SM	One Product SM	Two Products SM	Two Products SM
RL	0.064***	0.071***	-0.042**	-0.042**	-0.008	-0.006	0.037*	0.040**
	(0.023)	(0.024)	(0.018)	(0.018)	(0.023)	(0.024)	(0.020)	(0.020)
Control Mean	0.479	0.479	0.220	0.220	0.479	0.479	0.220	0.220
Obs.	1,863	1,863	1,863	1,863	1,863	1,863	1,863	1,863
Adj. R-Sq	0.004	0.013	0.002	0.049	-0.000	-0.003	0.001	0.051
Controls		X		X		X		X
Errors		X		X		X		X
Romano-Wolf p-val		0.011		0.052		0.807		0.079
	Panel B: Purchases (Dummies; CO2 Price)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	One Product	One Product	Two Products	Two Products	One Product SM	One Product SM	Two Products SM	Two Products SM
RL	0.084***	0.087***	-0.048***	-0.052***	0.022	0.024	0.025	0.025
	(0.023)	(0.024)	(0.018)	(0.018)	(0.023)	(0.023)	(0.020)	(0.019)
Control Mean	0.421	0.421	0.219	0.219	0.421	0.421	0.219	0.219
Obs.	1,863	1,863	1,863	1,863	1,863	1,863	1,863	1,863
Adj. R-Sq	0.007	0.012	0.003	0.075	-0.000	0.002	0.000	0.067
Controls		X		X		X		X
Errors		X		X		X		X
Romano-Wolf p-val		0.002		0.012		0.347		0.347

Notes: Panel A reports estimates of the effect of the *RL* treatment on purchasing dummies regarding FMs and SMs without a CO_2 price, while Panel B does so with a CO_2 price. Columns (1), and (3) present specifications without additional covariates, while columns (2), and (4) include the full set of controls. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. The last row in each panel reports Romano–Wolf adjusted p-values (1,000 bootstraps). Standard errors are heteroskedasticity robust. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Table B.9: Additional Purchasing Decisions (Dummies)

	Panel A: Additional Purchases (Dummies; No CO2 Price)					
	(1) At Least One Product	(2) At Least One Product	(3) At Least One Product SM	(4) At Least One Product SM	(5) At Least One Product SM (FM 1)	(6) At Least One Product SM (FM 1)
RL	0.022	0.028	0.029	0.034	-0.078***	-0.076***
	(0.021)	(0.021)	(0.021)	(0.021)	(0.022)	(0.022)
Control Mean	0.700	0.700	0.700	0.700	0.700	0.700
Obs.	1,863	1,863	1,863	1,863	1,863	1,863
Adj. R-Sq	0.000	0.046	0.000	0.040	0.006	0.044
Controls		X		X		X
Errors		X		X		X
Romano-Wolf p-val		0.183		0.168		0.001
	Panel B: Additional Buying Behaviour with CO2-Price (CO2 Price)					
	(1) At Least One Product	(2) At Least One Product	(3) At Least One Product SM	(4) At Least One Product SM	(5) At Least One Product SM (FM 1)	(6) At Least One Product SM (FM 1)
RL	0.035	0.035	0.046**	0.049**	-0.043*	-0.045**
	(0.022)	(0.022)	(0.022)	(0.022)	(0.023)	(0.022)
Control Mean	0.641	0.641	0.641	0.641	0.641	0.641
Obs.	1,863	1,863	1,863	1,863	1,863	1,863
Adj. R-Sq	0.001	0.056	0.002	0.054	0.001	0.057
Controls		X		X		X
Errors		X		X		X
Romano-Wolf p-val		0.113		0.059		0.085

Notes: Panel A reports estimates of the effect of the *RL* treatment on additional purchases dummies regarding FMs and SMs without a CO_2 price, while Panel B does so with a CO_2 price. Columns (1), and (3) present specifications without additional covariates, while columns (2), and (4) include the full set of controls. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. The last row in each panel reports Romano–Wolf adjusted p-values (1,000 bootstraps). Standard errors are heteroskedasticity robust. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.
Source: PRESPARE

Table B.10: Acceptance CO_2 Price by Revenue Scheme

	(1) State	(2) Donation	(3) Redistribution
RL	-0.049**	-0.008	-0.013
	(0.022)	(0.022)	(0.022)
Belief Yes Votes	0.010***	0.011***	0.002
	(0.002)	(0.002)	(0.002)
Belief Products (No CO2 Price)	-0.004***	-0.001	-0.002
	(0.001)	(0.001)	(0.001)
Belief Products (CO2 Price)	0.005***	0.002**	0.005***
	(0.001)	(0.001)	(0.001)
Products (No CO2 Price)	-0.030	-0.008	-0.016
	(0.022)	(0.022)	(0.022)
Products (CO2 Price)	0.016	-0.024	-0.010
	(0.021)	(0.021)	(0.021)
Soc. Accept. (No CO2 Price)			
No Product	-0.007	0.004	0.004
	(0.010)	(0.009)	(0.010)
One Product	0.007	0.027**	0.027**
	(0.012)	(0.011)	(0.012)
Two Products	-0.001	-0.003	-0.019*
	(0.011)	(0.010)	(0.011)
Soc. Accept. (CO2 Price)			
No Product	-0.023**	-0.018*	-0.025**
	(0.010)	(0.010)	(0.010)
One Product	0.026**	0.019	0.032***
	(0.012)	(0.012)	(0.012)
Two Products	0.011	0.005	0.015
	(0.010)	(0.010)	(0.011)
Control Mean	0.401	0.595	0.568
Obs.	1,863	1,863	1,863
Adj. R-Sq	0.086	0.140	0.108
Controls	X	X	X
Errors	X	X	X

Notes: This table presents estimates of how the voting decision for the revenue schemes — state budget, donation to climate projects, and redistribution (columns 1–3, respectively) — correlates with beliefs about others' voting behavior, beliefs about aggregate purchasing behavior, own purchasing decisions, and social acceptability ratings of purchasing behavior. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. Standard errors are heteroskedasticity robust. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.
Source: PRESPARE

Table B.11: Social Norms Purchases

	Panel A: Beliefs Purchases (No CO2 Price)							
	(1) Total Products	(2) Total Products	(3) No Products	(4) No Products	(5) One Product	(6) One Product	(7) Two Products	(8) Two Products
RL	-1.087**	-1.086**	-0.399*	-0.385*	0.832***	0.816***	-0.433**	-0.431**
	(0.443)	(0.455)	(0.215)	(0.221)	(0.215)	(0.214)	(0.208)	(0.209)
Control Mean	26.230	26.230	5.922	5.922	9.926	9.926	8.152	8.152
Obs.	1,863	1,863	1,863	1,863	1,863	1,863	1,863	1,863
Adj. R-Sq	0.003	0.007	0.001	0.016	0.007	0.036	0.002	0.023
Controls		X		X		X		X
Errors		X		X		X		X
Romano-Wolf p-val		0.043		0.081		0.001		0.078
	Panel B: Beliefs Purchases (CO2 Price)							
	(1) Total Products	(2) Total Products	(3) No Products	(4) No Products	(5) One Product	(6) One Product	(7) Two Products	(8) Two Products
RL	-1.026**	-1.106**	-0.752***	-0.694***	0.854***	0.829***	-0.103	-0.135
	(0.443)	(0.446)	(0.232)	(0.236)	(0.216)	(0.216)	(0.206)	(0.205)
Control Mean	25.061	25.061	6.601	6.601	9.738	9.738	7.662	7.662
Obs.	1,863	1,863	1,863	1,863	1,863	1,863	1,863	1,863
Adj. R-Sq	0.002	0.013	0.005	0.016	0.008	0.033	-0.000	0.019
Controls		X		X		X		X
Errors		X		X		X		X
Romano-Wolf p-val		0.017		0.005		0.001		0.504

	Panel C: Social Acceptance Purchases (No CO2 Price)					
	(1) No Product	(2) No Product	(3) One Product	(4) One Product	(5) Two Products	(6) Two Products
RL	0.008	0.039	-0.003	0.017	-0.164***	-0.166***
	(0.066)	(0.065)	(0.057)	(0.057)	(0.062)	(0.063)
Control Mean	0.646	0.646	0.577	0.577	-0.059	-0.059
Obs.	1,863	1,863	1,863	1,863	1,863	1,863
Adj. R-Sq	-0.001	0.050	-0.001	0.028	0.003	0.026
Controls		X		X		X
Errors		X		X		X
Romano-Wolf p-val		0.793		0.793		0.019
	Panel D: Social Acceptance (CO2 Price)					
	(1) No Product	(2) No Product	(3) One Product	(4) One Product	(5) Two Products	(6) Two Products
RL	-0.076	-0.068	-0.017	-0.006	-0.100	-0.097
	(0.064)	(0.063)	(0.055)	(0.056)	(0.062)	(0.063)
Control Mean	0.738	0.738	0.606	0.606	0.009	0.009
Obs.	1,863	1,863	1,863	1,863	1,863	1,863
Adj. R-Sq	0.000	0.056	-0.000	0.038	0.001	0.008
Controls		X		X		X
Errors		X		X		X
Romano-Wolf p-val		0.482		0.895		0.333

Notes: Panel A presents estimates of the effect of the *RL* treatment on beliefs about others' purchases without a CO_2 price while Panel B does that with a CO_2 price. Columns (1), (3), (5), and (7) present specifications without additional covariates, while columns (2), (4), (6), and (8) include the full set of controls. Panel C presents estimates of the effect of the *RL* treatment on the social acceptance ratings of product purchases without a CO_2 price while Panel D does that with a CO_2 price. Columns (1), (3), and (5) present specifications without additional covariates, while columns (2), (4), and (6) include the full set of controls. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. Standard errors are heteroskedasticity robust. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Table B.12: Alternative Effectiveness Measures

	Panel A: Different Effectiveness Measures		
	(1) Effectiveness (Raw)	(2) Effectiveness (Only Pos.)	(3) (In-)Effectivness (Only Neg.)
RL	0.020	1.083***	-1.064***
	(0.459)	(0.311)	(0.259)
Control Mean	1.169	3.290	-2.121
Obs.	1,863	1,863	1,863
Adj. R-Sq	0.016	0.021	0.019
Controls	X	X	X
Errors	X	X	X
	Panel B: Yes CO2-Price (State)		
	(1)	(2)	(3)
RL	-0.049**	-0.042*	-0.056**
	(0.022)	(0.022)	(0.022)
Effectiveness (Raw)	-0.005***		
	(0.001)		
Effectiveness (Only Pos.)		-0.006***	
		(0.001)	
(In-)Effectivness (Only Neg.)			-0.007***
			(0.002)
Control Mean	0.401	0.401	0.401
Obs.	1,863	1,863	1,863
Adj. R-Sq	0.069	0.065	0.064
Controls	X	X	X
Errors	X	X	X

Notes: Panel A reports the estimated effect of the *RL* treatment on alternative measures of perceived policy effectiveness, constructed from the difference in beliefs about product purchases with and without a CO_2 price. Positive values indicate a stronger expected reduction in purchases due to the CO_2 price. The dependent variable in column (1) is the raw difference measure. Column (2) retains only positive values and recodes negative values to zero, while column (3) retains only negative values and recodes positive values to zero. Panel B shows how including these perceived effectiveness measures changes the estimated effect of the *RL* treatment on support for the CO_2 price. Controls in both panels include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. Standard errors are heteroskedasticity robust. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Table B.13: Support for CO_2 Price (Donation & Redistribution) and Effectiveness

	Panel A: Yes CO2-Price (Donation)					
	(1)	(2)	(3)	(4)	(5)	(6)
RL	-0.007	-0.007	-0.007	-0.007	-0.006	-0.005
	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)
Effectiveness CO2-Price	0.006	0.022	0.004	0.001	-0.011	-0.030
	(0.022)	(0.022)	(0.022)	(0.023)	(0.024)	(0.025)
Control Mean	0.595	0.595	0.595	0.595	0.595	0.595
Obs.	1,863	1,863	1,863	1,863	1,863	1,863
Adj. R-Sq	0.115	0.116	0.115	0.115	0.115	0.116
Controls	X	X	X	X	X	X
Errors	X	X	X	X	X	X
	Panel B: Yes CO2-Price (Redistribution)					
	(1)	(2)	(3)	(4)	(5)	(6)
RL	-0.011	-0.011	-0.010	-0.010	-0.008	-0.007
	(0.022)	(0.022)	(0.022)	(0.023)	(0.023)	(0.022)
Effectiveness CO2-Price	-0.023	-0.007	-0.012	-0.014	-0.035	-0.061**
	(0.022)	(0.022)	(0.023)	(0.024)	(0.025)	(0.026)
Control Mean	0.568	0.568	0.568	0.568	0.568	0.568
Obs.	1,863	1,863	1,863	1,863	1,863	1,863
Adj. R-Sq	0.089	0.089	0.089	0.089	0.090	0.091
Controls	X	X	X	X	X	X
Errors	X	X	X	X	X	X

Notes: The table reports how the estimated effect of the *RL* treatment on support for the CO_2 price varies with alternative dummy measures of perceived policy effectiveness. Panel A presents results for the donation scheme, and Panel B for the redistribution scheme. In both panels, each column includes one effectiveness measure. The effectiveness measure is a binary indicator equal to 1 if the respondent believes that the CO_2 price reduces aggregate consumption by at least 1, 2, 3, 4, 5, or 6 units in columns (1) through (6), respectively. Controls in both panels include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. Standard errors are heteroskedasticity robust. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Table B.14: Purchasing Behavior (within)

	Panel A: Purchasing Behavior: First Mover							
	(1) Products	(2) Products	(3) One Product	(4) One Product	(5) At Least One Product	(6) At Least One Product	(7) Two Products	(8) Two Products
CO2 Price	-0.055***	-0.055***	-0.049***	-0.049***	-0.052***	-0.052***	-0.003	-0.003
	(0.013)	(0.013)	(0.011)	(0.011)	(0.009)	(0.009)	(0.008)	(0.008)
RL	-0.020	-0.022	0.070***	0.076***	0.025	0.027	-0.045***	-0.049***
	(0.030)	(0.029)	(0.020)	(0.020)	(0.019)	(0.019)	(0.016)	(0.016)
Control Mean	0.907	0.907	0.508	0.508	0.708	0.708	0.199	0.199
Obs.	3,812	3,812	3,812	3,812	3,812	3,812	3,812	3,812
Adj. R-Sq	0.001	0.077	0.007	0.019	0.003	0.055	0.003	0.062
Controls		X		X		X		X
Errors		X		X		X		X
	Panel B: Purchasing Behavior: Second Mover							
	(1) Products SM	(2) Products SM	(3) One Product SM	(4) One Product SM	(5) At Least One Product SM	(6) At Least One Product SM	(7) Two Products SM	(8) Two Products SM
CO2 Price	-0.058***	-0.058***	-0.044***	-0.044***	-0.051***	-0.051***	-0.007	-0.007
	(0.014)	(0.014)	(0.011)	(0.011)	(0.009)	(0.009)	(0.008)	(0.008)
RL	0.065**	0.068**	0.004	0.007	0.035*	0.038**	0.030*	0.031*
	(0.031)	(0.030)	(0.020)	(0.020)	(0.019)	(0.019)	(0.018)	(0.017)
Control Mean	0.951	0.951	0.473	0.473	0.712	0.712	0.239	0.239
Obs.	3,812	3,812	3,812	3,812	3,812	3,812	3,812	3,812
Adj. R-Sq	0.003	0.077	0.001	0.006	0.004	0.051	0.001	0.059
Controls		X		X		X		X
Errors		X		X		X		X

Notes: Panel A reports within-subject effects of the CO_2 price on total products purchased and product dummies for FMs. Panel B shows the corresponding results for SMs. Columns (1), (3), (5), and (7) present specifications without additional covariates, while columns (2), (4), (6), and (8) include the full set of controls. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. Standard errors are clustered on the individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Table B.15: Voting Decisions (within)

	Voting Behavior: Acceptance CO2-Price	
	(1)	(2)
Redistribution	0.176***	0.176***
	(0.012)	(0.012)
Donation	0.204***	0.204***
	(0.012)	(0.012)
RL	-0.021	-0.025
	(0.018)	(0.017)
Control Mean	0.384	0.384
Obs.	5,718	5,718
Adj. R-Sq	0.033	0.108
Controls		X
Errors		X

Notes: This table presents estimates of the effect of the donation and redistribution schemes on CO_2 acceptance (incl. the coefficient for the *RL* treatment). Column (1) presents the specification without additional covariates, while column (2) includes the full set of controls. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. Standard errors are clustered on the individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Table B.16: Voting Decisions: Interaction Effects (within)

	Voting Behavior: Acceptance CO2-Price	
	(1)	(2)
Redistribution	0.167***	0.167***
	(0.016)	(0.016)
Donation	0.194***	0.194***
	(0.017)	(0.017)
RL	-0.034	-0.037*
	(0.022)	(0.022)
Redistribution × RL	0.018	0.018
	(0.024)	(0.024)
Donation × RL	0.020	0.020
	(0.025)	(0.025)
Control Mean	0.384	0.384
Obs.	5,718	5,718
Adj. R-Sq	0.032	0.108
Controls		X
Errors		X

Notes: This table presents estimates of the effect of the donation and redistribution schemes on CO_2 acceptance interacted with the *RL* treatment. Column (1) presents the specification without additional covariates, while column (2) includes the full set of controls. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. Standard errors are clustered on the individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

B.3 Heterogeneity

As proposed in the pre-analysis plan, I examine treatment heterogeneity. No systematic heterogeneity emerges with respect to control variables (results available upon request). Table B.17 therefore reports interactions between the *RL* treatment and experimental variables for support of the CO_2 price when revenues are allocated to the state budget.

Panel A presents interactions using variables measured in the absence of a CO_2 price. Two interactions are statistically significant: social acceptance of buying one and two products. Higher acceptance in both cases strengthens the negative effect of *RL* on support (–0.055 for one product, $p < 0.01$; –0.042 for two products, $p < 0.05$). A similar pattern appears in Panel B, where variables are measured under a CO_2 price (–0.058 and –0.042, both $p < 0.01$). In addition, higher beliefs about total products purchased are associated with a more negative *RL* effect (-0.006; $p < 0.01$).

Overall, these results suggest that respondents who view purchasing more products as socially acceptable react more strongly to the treatment. One interpretation is that these individuals perceive restrictions on consumption through pricing as less acceptable. At the same time, higher expected consumption under a CO_2 price further reduces support in the *RL* condition, consistent with the idea that greater market participation increases the number of individuals affected by the policy. This pattern aligns with fairness considerations.

Figure **??** shows results of the causal random forest estimation (Wager and Athey, 2018). In short this approach does the following: I define the set of variables it is supposed to investigate. Next, it estimates which of them are the most predictive of the treatment effects. Then, I plot the predicted heterogeneous conditional average treatment effects (CATEs) for the six most relevant variables. Each panel in the figures shows the most relevant variables by rank from left to right in both rows respectively for the respective consumption choice.

To sum up, beliefs about products matter which again shows that effectivness concerns might play a role regarding support. Addtionally, locus of control is the second most important variable, which is in line with the RL. A higher locus of control could plausibly be interpreted that people care less about the RL because the feel more responsible for their actions.

In contrast to the linear interaction models, norms appear to play only a minor role in the non-linear

Table B.17: Heterogeneity Experimental Variables: Yes CO2 Price (State)

	Panel A: Yes (State) (No CO2 Price)					
	(1)	(2)	(3)	(4)	(5)	(6)
RL	-0.051**	-0.048**	-0.050**	-0.050**	-0.050**	-0.050**
	(0.022)	(0.022)	(0.023)	(0.023)	(0.022)	(0.022)
RL × Total Products (No CO2 Price)	0.053					
	(0.032)					
RL × Total Products SM (No CO2 Price)		0.053				
		(0.032)				
Belief						
RL × Total Products			-0.002			
			(0.002)			
Soc. Accept.						
RL × No Product				-0.001		
				(0.016)		
Soc. Accept.						
RL × One Product					-0.055***	
					(0.018)	
Soc. Accept.						
RL × Two Products						-0.042**
						(0.016)
	Panel B: Yes (State) (CO2 Price)					
	(1)	(2)	(3)	(4)	(5)	(6)
RL	-0.042*	-0.042*	-0.041*	-0.042*	-0.042*	-0.042*
	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)
RL × Total Products (CO2 Price)	0.001					
	(0.031)					
RL × Total Products SM (CO2 Price)		0.001				
		(0.031)				
Belief						
RL × Total Products			-0.006***			
			(0.002)			
Soc. Accept.						
RL × No Product				-0.010		
				(0.016)		
Soc. Accept.						
RL × One Product					-0.058***	
					(0.018)	
Soc. Accept.						
RL × Two Products						-0.042***
						(0.016)
Control Mean	0.401	0.401	0.401	0.401	0.401	0.401
Obs.	1,863	1,863	1,863	1,863	1,863	1,863
Adj. R-Sq	0.072	0.072	0.075	0.072	0.077	0.075
Controls	X	X	X	X	X	X
Errors	X	X	X	X	X	X

Notes: This table reports estimates of the effect of the *RL* treatment interacted with experimental variables on voting decisions for the state budget scheme. The interacting variables include the number of products purchased as a FM (column (1)) and as a SM (column (2)), beliefs about total products purchased (column (3)), and social acceptance ratings of buying zero, one, or two products (columns (4)–(6)). Panel A reports effects when these variables are measured without a CO_2 price, while Panel B reports the corresponding effects with a CO_2 price. All columns include the full set of controls. Controls include age, a female dummy, a dummy indicating having at least a tertiary education, the log of personal net income, a dummy indicating full-time employment, a dummy for having minor children, the number of minor children in the household, a dummy for Green Party support, a dummy for AfD (right-wing party) support, dummies indicating whether an individual believes that climate change is human-made and that it exists, a dummy indicating whether an individual supports government regulation of CO_2 emissions, locus of control, measures for willingness to take risks and willingness to postpone a reward for future benefits, and the number of errors in the comprehension questions. Standard errors are heteroskedasticity robust. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.
Source: PRESPARE

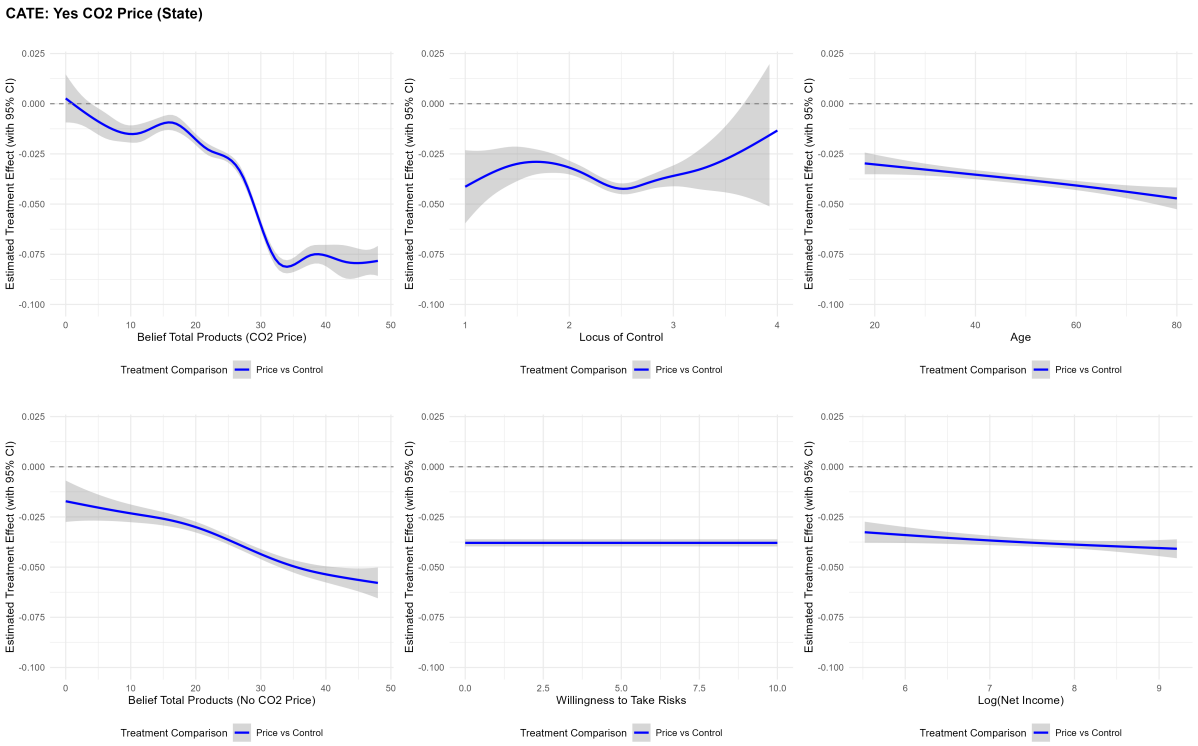


Figure B.3: Conditional Average Treatment Effects: Yes CO_2 Price (State)

Notes: The figure presents results from a causal random forest estimating heterogeneous treatment effects for the *RL* treatment relative to *Control*. Each panel shows the six most relevant covariates for explaining treatment effect heterogeneity, ranked by importance. The y-axis depicts the estimated treatment effect (conditional average treatment effect, CATE). These estimates represent model-based estimates of how the treatment effect varies across covariate values.
Source: PRESPARE.

analysis, since they are not among the six most relevant predictors in Figure B.3. The most pronounced conditional CATE is observed for beliefs about total products purchased under a CO_2 price, showing a clear downward trend to approximately 0.10. Locus of control displays a non-monotonic pattern: the CATE initially declines, then rises again toward zero. Age exhibits a steady downward trend reaching about 0.08. Beliefs about total products without a CO_2 price follow a similar but weaker pattern, reaching roughly 0.05. Willingness to take risks and log net income display virtually no variation.

Overall, beliefs about product consumption appear most relevant, again suggesting that perceived effectiveness concerns play are important in shaping support. Locus of control emerges as the second most important variable, consistent with the RL mechanism: individuals with a stronger internal locus of control may feel more responsible for their own actions and therefore be less influenced by the RL.

Appendix C

Supplementary Materials Chapter 3

C.1 Figures

Figure C.1: Experimental Design

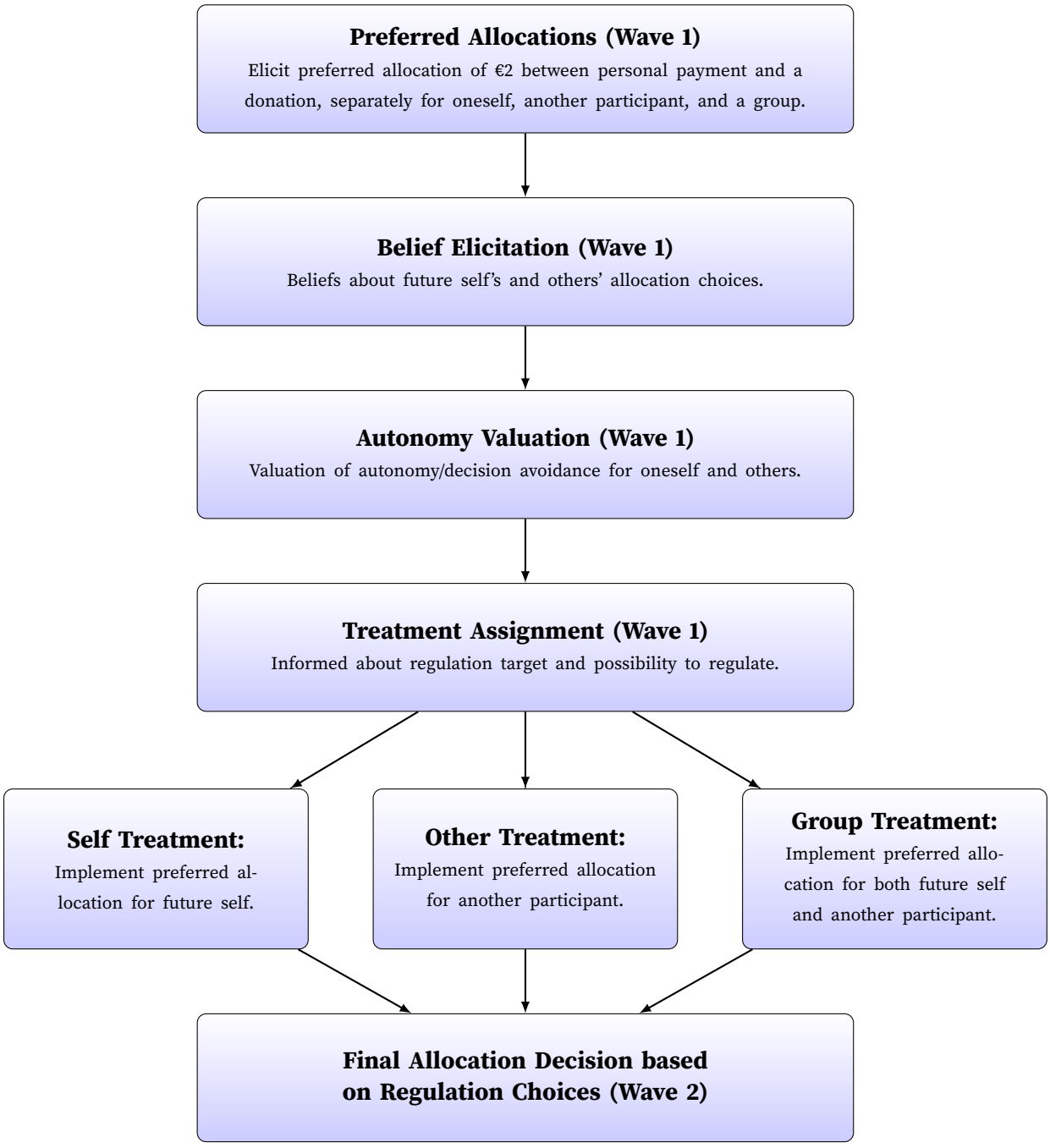
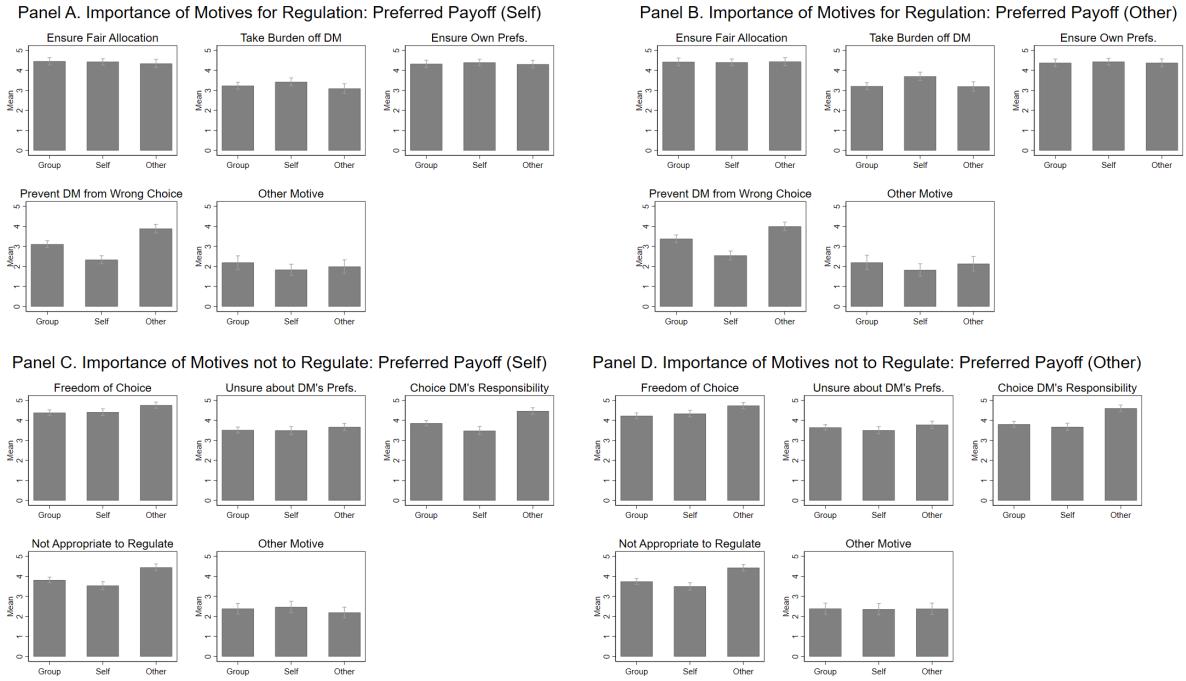


Figure C.2: Motives by Treatment



Notes: This figure shows raw means for the motives underlying regulation (Panels A and B) and non-regulation (Panels C and D) by treatment. Panels A and C refer to decisions concerning the own preferred payoff; Panels B and D to decisions concerning preferred payoffs for others. For regulation (Panels A and B), the motives are: ensure a fair allocation, take burden off the DM, ensure own preferences are implemented, and prevent the DM from making the wrong choice. For non-regulation (Panels C and D), the motives are: freedom of choice for the DM, choice is the DM's responsibility, CA is unsure about the DM's preferences, and the belief that regulation is not appropriate.
Source: PRESPARE.

C.2 Tables

Table C.1: Comparison Survey Data with Eurostat Data

	(1) Survey Data	(2) Eurostat
Female: 18–35 Years	0.133	0.134
Female: 36–45 Years	0.081	0.081
Female: 46–55 Years	0.091	0.091
Female: 56–65 Years	0.100	0.100
Female: 66–80 Years	0.097	0.095
Male: 18–35 Years	0.139	0.145
Male: 36–45 Years	0.083	0.083
Male: 46–55 Years	0.095	0.091
Male: 56–65 Years	0.099	0.098
Male: 66–80 Years	0.082	0.083
Employed (%)	0.824	0.813
Median Net Income (€)	2250.000	2301.000
Low Education	0.292	0.300
Middle Education	0.314	0.310
Higher Education	0.393	0.390
Baden-Württemberg	0.131	0.134
Bavaria	0.161	0.159
Berlin	0.046	0.045
Brandenburg	0.027	0.030
Bremen	0.007	0.008
Hamburg	0.022	0.023
Hesse	0.074	0.076
Mecklenburg-Western Pomerania	0.019	0.019
Lower Saxony	0.097	0.096
North Rhine-Westphalia	0.217	0.215
Rhineland-Palatinate	0.051	0.050
Saarland	0.011	0.012
Saxony	0.049	0.048
Saxony-Anhalt	0.025	0.026
Schleswig-Holstein	0.036	0.035
Thuringia	0.026	0.025

Notes: This table compares the survey sample to official population statistics from Eurostat. Low education refers to Hauptschule/Mittelschule or equivalent. Middle education refers to Realschule or equivalent. Higher education refers to Abitur or higher. Employment excludes individuals who are already retired. All variables are expressed as shares unless otherwise indicated; income refers to the median.

Table C.2: Variable Definitions

Panel A. Controls	
Variable	**Definition**
Age	Respondent's age in years.
Female	Dummy equal to 1 if respondent is female, 0 otherwise.
Personal Net Income (in €)	Monthly personal net income in €.

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Table C.2 – continued from previous page

Variable	Definition
Tertiary Education	Dummy equal to 1 if respondent holds a tertiary (any form of university-level education) degree, 0 otherwise.
Full Time	Dummy equal to 1 if respondent is employed full time, 0 otherwise.
Minor Children	Dummy equal to 1 if respondent has at least one child under the age of 18, 0 otherwise.
Minor Children in Household	Number of minor children under 18 living in the respondent's household.
Panel B. Experimental Variables	
Preferred Payoff (Self)	Payoff that participants prefer for themselves, ranging from 0 to 2 €.
Preferred Payoff (Other)	Payoff that participants prefer for another individual, ranging from 0 to 2 €.
Preferred Payoff (Group)	Payoff that participants prefer when both the respondent and another individual are affected, ranging from 0 to 2 €.
DA: Self (WTP)	Willingness to pay to preserve autonomy rather than delegate decision rights in decisions affecting oneself, ranging from -2 to 2 euros. Negative values indicate a higher willingness to pay for delegation, while positive values indicate a higher willingness to pay for autonomy.
DA: Other (WTP)	Willingness to pay to preserve autonomy rather than delegate decision rights in decisions affecting another individual, ranging from -2 to 2 euros. Negative values indicate a higher willingness to pay for delegation, while positive values indicate a higher willingness to pay for autonomy.
DA: Self	Dummy equal to 1 if the respondent prefers autonomy over delegating decision rights in decisions affecting herself, 0 otherwise.
DA: Other	Dummy equal to 1 if the respondent prefers autonomy over delegating decision rights in decisions affecting another individual, 0 otherwise.
Belief Payoff (Self)	Respondent's belief about her own preferred payoff in the second survey wave, ranging from 0 to 2 €.
Belief Payoff (Other)	Respondent's belief about the preferred payoff of another individual in the second survey wave, ranging from 0 to 2 €.

Continued on next page

Table C.2 – continued from previous page

Variable	Definition
MG: Self vs. Self	Absolute difference between *Preferred Payoff (Self)* and *Belief Payoff (Self)* (ranging from 0 to 2 €). Captures perceived misalignment between the respondent's preferred payoff for herself and her belief about her future preferred payoff.
MG: Self vs. Other	Absolute difference between *Preferred Payoff (Self)* and *Belief Payoff (Other)* (ranging from 0 to 2 €). Captures perceived misalignment between the respondent's preferred payoff for herself and her belief about that individual's preferred payoff.
MG: Other vs. Self	Absolute difference between *Preferred Payoff (Other)* and *Belief Payoff (Self)* (ranging from 0 to 2 €). Captures perceived misalignment between the respondent's preferred payoff for another individual and her belief about her future preferred payoff.
MG: Other vs. Other	Absolute difference between *Preferred Payoff (Other)* and *Belief Payoff (Other)* (ranging from 0 to 2 €). Captures perceived misalignment between the respondent's preferred payoff for another individual and her belief about that individual's preferred payoff.
	Panel C. Outcomes
Regulation: Preferred Payoff (Self)	Dummy equal to 1 if the respondent chooses to impose a binding regulation fixing the allocation at *Preferred Payoff (Self)*, 0 otherwise.
Regulation: Preferred Payoff (Other)	Dummy equal to 1 if the respondent chooses to impose a binding regulation fixing the allocation at *Preferred Payoff (Other)*, 0 otherwise.

Table C.3: Regulation Choices (Robustness: Multiple Hypothesis Testing)

	Regulation Preferred Payoff			
	Self		Other	
	(1)	(2)	(3)	(4)
Self	0.098***	0.099***	0.082***	0.084***
	(0.028)	(0.028)	(0.028)	(0.028)
RW p-value	[0.002]	[0.002]	[0.002]	[0.002]
Other	-0.066***	-0.064***	-0.022	-0.019
	(0.028)	(0.028)	(0.028)	(0.028)
RW p-value	[0.006]	[0.006]	[0.240]	[0.305]
Control Mean	0.471	0.471	0.403	0.403
Wald Test (F-Stat)	34.38***	34.23***	14.08***	13.76***
Obs.	1,875	1,875	1,875	1,875
Adj. R-Sq	0.017	0.019	0.007	0.010
Controls		X		X

Notes: This table reports the estimated treatment effects of *Self* and *Other* compared to *Group* on implementing the own preferred payoff (columns (1) and (2)) and the preferred payoff for others (columns (3) and (4)). Columns (1) and (3) do not include any controls while columns (2) and (4) control for age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. P-values adjusted for multiple hypothesis testing using the Romano-Wolf procedure are in brackets. Standard errors are robust to heteroskedasticity. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.
Source: PRESPARE.

Table C.4: Regulation Choices (Robustness Duration and Errors)

	Regulation Preferred Payoff	
	Self	Other
	(1)	(2)
Self	0.105**	0.086*
	(0.046)	(0.045)
Other	-0.080*	-0.026
	(0.042)	(0.042)
Duration (Min.)	0.000	0.001
	(0.001)	(0.001)
Errors	-0.108***	-0.066***
	(0.021)	(0.020)
Self × Duration (Min.)	-0.000	0.001
	(0.001)	(0.001)
Other × Duration (Min.)	-0.000	-0.000
	(0.001)	(0.001)
Self × Errors	0.005	-0.027
	(0.030)	(0.030)
Other × Errors	0.040	0.032
	(0.030)	(0.029)
Control Mean	0.471	0.403
Wald Test (F-Stat)	17.90***	7.05***
Obs.	1,865	1,865
Adj. R-Sq	0.046	0.027
Controls	X	X

Notes: This table reports the estimated treatment effects of *Self* and *Other* compared to *Group* on implementing one's own preferred payoff (column (1)) and the preferred payoff for others (column (2)). Both columns include survey completion time in minutes (Duration (Min.)) and the number of errors in the comprehension questions (Errors); both variables are fully interacted with treatment indicators. The F-statistic reports a Wald test of equality between the *Self* and *Other* treatment effects. All specifications control for age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors are robust to heteroskedasticity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE.

Table C.5: Regulation Choices (Robustness Attention Check)

	Regulation Preferred Payoff			
	Self		Other	
	(1)	(2)	(3)	(4)
Self	0.099***	0.086	0.084***	0.080
	(0.028)	(0.064)	(0.028)	(0.064)
Other	-0.056**	-0.024	-0.015	0.028
	(0.028)	(0.058)	(0.028)	(0.059)
Attention Passed	0.212***	0.222***	0.122***	0.141***
	(0.028)	(0.049)	(0.028)	(0.049)
Self × Attention Passed		0.015		0.005
		(0.071)		(0.071)
Other × Attention Passed		-0.041		-0.054
		(0.066)		(0.067)
Control Mean	0.471	0.471	0.403	0.403
Wald Test (F-Stat)	31.49***	3.30*	12.58***	0.70
Obs.	1,875	1,875	1,875	1,875
Adj. R-Sq	0.045	0.044	0.019	0.018
Controls	X	X	X	X

Notes: This table reports the estimated treatment effects of *Self* and *Other* compared to *Group* on implementing one's own preferred payoff (columns (1) and (2)) and the preferred payoff for others (columns (3) and (4)). Columns (1) and (3) include a dummy indicating whether a participant passed the second attention check. Columns (2) and (4) fully interact this dummy with treatment indicators. The F-statistic reports a Wald test of equality between the *Self* and *Other* treatment effects. All specifications control for age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors are robust to heteroskedasticity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE.

Table C.6: Sign of Misalignment Gap and Regulation

	Panel A. Regulation: Preferred Payoff (Self)	
	(1)	(2)
Self	0.102***	0.102***
	(0.028)	(0.028)
Other	-0.061**	-0.061**
	(0.028)	(0.028)
MG: Self vs. Self	-0.012	-0.012
	(0.016)	(0.016)
Diff. $> 0 \times$ MG: Self vs. Self	-0.064**	
	(0.027)	
MG: Self vs. Other	0.042**	0.044***
	(0.017)	(0.017)
Diff. $> 0 \times$ MG: Self vs. Other	-0.028	
	(0.030)	
Diff. $> 0.05 \times$ MG: Self vs. Self		-0.066**
		(0.029)
Diff. $> 0.05 \times$ MG: Self vs. Other		-0.028
		(0.031)
Control Mean	0.471	0.471
Wald Test (F-Stat)	35.40***	35.21***
Obs.	1,875	1,875
Adj. R-Sq	0.049	0.049
Controls	X	X

	Panel B. Regulation: Preferred Payoff (Other)	
	(1)	(2)
Self	0.083***	0.083***
	(0.028)	(0.028)
Other	-0.022	-0.021
	(0.027)	(0.027)
MG: Other vs. Self	-0.077***	-0.077***
	(0.014)	(0.014)
Diff. $> 0 \times$ MG: Other vs. Self	0.009	
	(0.028)	
MG: Other vs. Other	0.013	0.016
	(0.016)	(0.016)
Diff. $> 0 \times$ MG: Other vs. Other	-0.008	
	(0.031)	
Diff. $> 0.05 \times$ MG: Other vs. Self		0.016
		(0.029)
Diff. $> 0.05 \times$ MG: Other vs. Other		-0.017
		(0.032)
Control Mean	0.403	0.403
Wald Test (F-Stat)	14.78***	14.46***
Obs.	1,875	1,875
Adj. R-Sq	0.043	0.042
Controls	X	X

Notes: This table reports results from regressions interacting the sign of the misalignment gaps (MG) with the absolute magnitude of the MG. MGs are defined as the absolute differences between preferred payoffs and the corresponding beliefs. All continuous experimental covariates are standardized to have mean 0 and standard deviation 1. Panel A presents results for implementing the own preferred payoff; Panel B presents results for implementing the preferred payoff for others. Column (1) includes interactions using a dummy indicating that the MG is positive, while column (2) uses a dummy indicating that the MG exceeds five cents. The table also reports the estimated treatment effects of *Self* and *Other* relative to *Group*. All specifications control for age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. The F-statistic reports a Wald test of equality between the *Self* and *Other* treatment effects. Standard errors are robust to heteroskedasticity. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.
Source: PRESPARE

Table C.7: Regulation Choices: Alternative Autonomy Measures

	Panel A. Regulation: Preferred Payoff (Self)			
	(1)	(2)	(3)	(4)
Self	0.099***	0.099***	0.097***	0.095***
	(0.028)	(0.028)	(0.028)	(0.028)
Other	-0.064**	-0.063**	-0.065**	-0.064**
	(0.028)	(0.028)	(0.028)	(0.028)
Preferred Payoff (Self)		0.036***	0.036***	0.037***
		(0.011)	(0.011)	(0.011)
MG: Self vs. Self			-0.039***	-0.043***
			(0.012)	(0.012)
MG: Self vs. Other			0.022*	0.025**
			(0.012)	(0.012)
DA: Self				-0.065**
				(0.026)
DA: Other				-0.096***
				(0.025)
Control Mean	0.471	0.471	0.471	0.471
Wald Test (F-Stat)	34.23***	34.10***	33.84***	33.40***
Obs.	1,875	1,875	1,875	1,875
Adj. R-Sq	0.019	0.024	0.028	0.042
Controls	X	X	X	X
	Panel B. Regulation: Preferred Payoff (Other)			
	(1)	(2)	(3)	(4)
Self	0.084***	0.085***	0.081***	0.080***
	(0.028)	(0.028)	(0.028)	(0.027)
Other	-0.019	-0.020	-0.024	-0.023
	(0.028)	(0.027)	(0.027)	(0.027)
Preferred Payoff (Other)		0.037***	0.025**	0.023**
		(0.011)	(0.011)	(0.011)
MG: Other vs. Self			-0.077***	-0.077***
			(0.011)	(0.011)
MG: Other vs. Other			0.008	0.009
			(0.012)	(0.012)
DA: Self				-0.066**
				(0.026)
DA: Other				-0.074***
				(0.025)
Control Mean	0.403	0.403	0.403	0.403
Wald Test (F-Stat)	13.76***	14.31***	14.61***	14.15***
Obs.	1,875	1,875	1,875	1,875
Adj. R-Sq	0.010	0.015	0.036	0.046
Controls	X	X	X	X

Notes: This table reports the estimated treatment effects of *Self* and *Other* compared to *Group* on implementing one's own preferred payoff (Panel A) and the preferred payoff for others (Panel B). Column (2) additionally includes the size of the preferred payoff (self in Panel A; other in Panel B). Column (3) adds the misalignment gaps (MG), defined as the absolute differences between preferred payoffs and the corresponding beliefs. Column (4) adds decision autonomy (DA) dummies (DA: Self and DA: Other), equal to 1 if a participant prefers retaining DA over delegation. All continuous experimental covariates are standardized to have mean 0 and standard deviation 1. The F-statistic reports a Wald test of equality between the *Self* and *Other* treatment effects. All specifications control for age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors are robust to heteroskedasticity. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.
Source: PRESPARE

Table C.8: Regulation Choices by Treatment Groups: Sign of Misalignment Gap

	Panel A. Regulation: Preferred Payoff (Self)					
	Self Treatment		Other Treatment		Group Treatment	
	(1)	(2)	(3)	(4)	(5)	(6)
Preferred Payoff (Self)	0.022	0.017			0.077***	0.079***
	(0.021)	(0.022)			(0.028)	(0.028)
DA: Self (WTP)	-0.042**	-0.042**			-0.017	-0.018
	(0.019)	(0.019)			(0.021)	(0.021)
MG: Self vs. Self	-0.018	-0.017			-0.013	-0.010
	(0.031)	(0.031)			(0.025)	(0.026)
Diff. $> 0.05 \times$ MG: Self vs. Self	-0.041	-0.025			-0.087*	-0.079
	(0.046)	(0.046)			(0.048)	(0.049)
MG: Self vs. Other			-0.011	-0.011	0.032	0.034
			(0.022)	(0.022)	(0.029)	(0.030)
Diff. $> 0.05 \times$ MG: Self vs. Other			0.027	0.029	-0.023	-0.025
			(0.044)	(0.045)	(0.055)	(0.055)
DA: Other (WTP)			-0.071***	-0.068***	-0.039*	-0.034
			(0.020)	(0.020)	(0.021)	(0.021)
	Panel B. Regulation: Preferred Payoff (Other)					
	Self Treatment		Other Treatment		Group Treatment	
	(1)	(2)	(3)	(4)	(5)	(6)
Preferred Payoff (Other)	-0.006	-0.007			0.062**	0.062**
	(0.022)	(0.023)			(0.027)	(0.027)
MG: Other vs. Self	-0.119***	-0.119***			-0.072***	-0.069***
	(0.024)	(0.024)			(0.022)	(0.022)
Diff. $> 0.05 \times$ MG: Other vs. Self	0.063	0.066			0.020	0.015
	(0.044)	(0.046)			(0.053)	(0.054)
DA: Self (WTP)	-0.033*	-0.034*			-0.016	-0.020
	(0.019)	(0.019)			(0.021)	(0.021)
MG: Other vs. Other			-0.031	-0.032	0.023	0.025
			(0.020)	(0.020)	(0.026)	(0.026)
Diff. $> 0.05 \times$ MG: Other vs. Other			0.061	0.060	-0.049	-0.039
			(0.044)	(0.045)	(0.055)	(0.057)
DA: Other (WTP)			-0.060***	-0.058***	-0.012	-0.005
			(0.020)	(0.020)	(0.021)	(0.021)
Obs.	628	628	627	627	620	620
Controls		X		X		X

Notes: This table reports OLS estimates testing predictions of the conceptual framework. It specifically focuses on interacting the sign of the relevant MG with the absolute magnitude of the MG. Misalignment gaps (MG) are defined as the absolute differences between preferred payoffs and the corresponding beliefs about future allocation choices. Decision autonomy (DA) dummies (DA: Self and DA: Other) are equal to 1 if a participant prefers retaining DA over delegation. All continuous experimental covariates are standardized to have mean 0 and standard deviation 1. Columns (1) and (2) correspond to the *Self* treatment and include the relevant preferred payoff, MG, and DA measures. Columns (3) and (4) correspond to the *Other* treatment and include the relevant MG and DA measures. Columns (5) and (6) correspond to the *Group* treatment and combine all variables used in the *Self* and *Other* specifications. Panel A presents results for implementing one's own preferred payoff; Panel B presents results for implementing the preferred payoff for others. Columns (1), (3), and (5) exclude controls, while columns (2), (4), and (6) include controls for age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors are robust to heteroskedasticity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE

Table C.9: Regulation Choices by Treatment Groups: Alternative Autonomy Measure

	Panel A. Regulation: Preferred Payoff (Self)					
	Self Treatment		Other Treatment		Group Treatment	
	(1)	(2)	(3)	(4)	(5)	(6)
Preferred Payoff (Self)	0.010	0.006			0.056***	0.057***
	(0.020)	(0.020)			(0.020)	(0.020)
MG: Self vs. Self	-0.052**	-0.047**			-0.043**	-0.039*
	(0.021)	(0.021)			(0.019)	(0.020)
DA: Self	-0.074*	-0.076*			-0.098**	-0.103**
	(0.043)	(0.043)			(0.046)	(0.046)
MG: Self vs. Other			-0.002	-0.002	0.022	0.023
			(0.019)	(0.019)	(0.021)	(0.021)
DA: Other			-0.152***	-0.151***	-0.081*	-0.070
			(0.041)	(0.041)	(0.043)	(0.043)
	Panel B. Regulation: Preferred Payoff (Other)					
	Self Treatment		Other Treatment		Group Treatment	
	(1)	(2)	(3)	(4)	(5)	(6)
Preferred Payoff (Other)	0.002	0.001			0.029	0.029
	(0.019)	(0.019)			(0.019)	(0.019)
MG: Other vs. Self	-0.107***	-0.105***			-0.070***	-0.067***
	(0.017)	(0.017)			(0.018)	(0.018)
DA: Self	-0.088**	-0.086**			-0.107**	-0.117**
	(0.043)	(0.043)			(0.046)	(0.046)
MG: Other vs. Other			-0.014	-0.015	0.003	0.006
			(0.018)	(0.018)	(0.020)	(0.021)
DA: Other			-0.133***	-0.132***	-0.042	-0.028
			(0.041)	(0.041)	(0.043)	(0.043)
Obs.	628	628	627	627	620	620
Controls		X		X		X

Notes: This table reports OLS estimates testing predictions of the conceptual framework. Misalignment gaps (MG) are defined as the absolute differences between preferred payoffs and the corresponding beliefs about future allocation choices. Decision autonomy (DA) dummies (DA: Self and DA: Other) are equal to 1 if a participant prefers retaining DA over delegation. All continuous experimental covariates are standardized to have mean 0 and standard deviation 1. Columns (1) and (2) correspond to the *Self* treatment and include the relevant preferred payoff, MG, and DA measures. Columns (3) and (4) correspond to the *Other* treatment and include the relevant MG and DA measures. Columns (5) and (6) correspond to the *Group* treatment and combine all variables used in the *Self* and *Other* specifications. Panel A presents results for implementing one's own preferred payoff; Panel B presents results for implementing the preferred payoff for others. Columns (1), (3), and (5) exclude controls, while columns (2), (4), and (6) include controls for age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors are robust to heteroskedasticity. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.
Source: PRESPARE

Table C.10: Mean Comparison Importance of Motives

Panel A. Importance of Motives for Regulation: Preferred Payoff (Self)

Mean and Standard Deviation				Differences (t-tests)					
Ensure Fair Allocation (M1)	Take Burden off DM (M2)	Ensure Own Prefs. (M3)	Prevent DM from Wrong Choice (M4)	M1–M2	M1–M3	M1–M4	M2–M3	M2–M4	M3–M4
4.42	3.28	4.35	3.02	1.14***	0.07	1.40***	-1.07***	0.25***	1.33***
(1.57)	(1.80)	(1.56)	(1.86)	(0.07)	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)

Panel B. Importance of Motives for Regulation: Preferred Payoff (Other)

Mean and Standard Deviation				Differences (t-tests)					
Ensure Fair Allocation (M1)	Take Burden off DM (M2)	Ensure Own Prefs. (M3)	Prevent DM from Wrong Choice (M4)	M1–M2	M1–M3	M1–M4	M2–M3	M2–M4	M3–M4
4.42	3.40	4.40	3.24	1.03***	0.02	1.19***	-1.01***	0.16**	1.17***
(1.55)	(1.74)	(1.51)	(1.88)	(0.07)	(0.06)	(0.08)	(0.07)	(0.08)	(0.08)

Panel C. Importance of Motives not to Regulate: Preferred Payoff (Self)

Mean and Standard Deviation				Differences (t-tests)					
Freedom of Choice (M1)	Unsure about DM's Prefs. (M2)	Choice DM's Respon-sibility (M3)	Not Appropriate to Regulate (M4)	M1–M2	M1–M3	M1–M4	M2–M3	M2–M4	M3–M4
4.53	3.57	3.99	3.98	0.97***	0.55***	0.55***	-0.42***	-0.42***	0.00
(1.43)	(1.58)	(1.60)	(1.57)	(0.06)	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)

Panel D. Importance of Motives not to Regulate: Preferred Payoff (Other)

Mean and Standard Deviation				Differences (t-tests)					
Freedom of Choice (M1)	Unsure about DM's Prefs. (M2)	Choice DM's Respon-sibility (M3)	Not Appropriate to Regulate (M4)	M1–M2	M1–M3	M1–M4	M2–M3	M2–M4	M3–M4
4.45	3.66	4.06	3.93	0.79***	0.39***	0.52***	-0.40***	-0.27***	0.13***
(1.47)	(1.61)	(1.60)	(1.62)	(0.05)	(0.04)	(0.04)	(0.05)	(0.05)	(0.04)

Notes: This table reports summary statistics and differences in means across regulation motives measured on a 7-point Likert scale ranging from 0 to 6. Columns (1)–(4) present means, with standard deviations in parentheses. Columns (5)–(10) report differences in means between motives, with standard errors from t-tests in parentheses. Panel A summarizes the importance of motives for regulating the preferred payoff for oneself, while Panel B refers to regulating the preferred payoff for others. Panel C reports the importance of motives for not regulating the preferred payoff for oneself, and Panel D does so for the preferred payoff for others. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.
Source: PRESPARE.

Table C.11: Heterogeneity Analysis I

	Panel A. Regulation: Preferred Payoff (Self)		
	(1)	(2)	(3)
Self	0.102***	0.129***	0.120***
	(0.039)	(0.032)	(0.033)
Other	-0.081**	-0.062*	-0.043
	(0.040)	(0.032)	(0.033)
Self × Female	-0.005		
	(0.056)		
Other × Female	0.034		
	(0.056)		
Self × Tertiary Education		-0.129*	
		(0.068)	
Other × Tertiary Education		-0.007	
		(0.068)	
Self × Minor Children			-0.079
			(0.064)
Other × Minor Children			-0.079
			(0.064)
Control Mean	0.471	0.471	0.471
Obs.	1,875	1,875	1,875
Adj. R-Sq	0.018	0.020	0.019
Controls	X	X	X
	Panel B. Regulation: Preferred Payoff (Other)		
	(1)	(2)	(3)
Self	0.044	0.105***	0.094***
	(0.040)	(0.032)	(0.033)
Other	-0.044	-0.009	-0.009
	(0.040)	(0.031)	(0.032)
Self × Female	0.082		
	(0.056)		
Other × Female	0.052		
	(0.055)		
Self × Tertiary Education		-0.093	
		(0.067)	
Other × Tertiary Education		-0.044	
		(0.067)	
Self × Minor Children			-0.036
			(0.063)
Other × Minor Children			-0.040
			(0.063)
Control Mean	0.403	0.403	0.403
Obs.	1,875	1,875	1,875
Adj. R-Sq	0.010	0.010	0.009
Controls	X	X	X

Notes: This table reports regressions interacting the *Self* and *Other* treatment indicators with individual characteristics: female (column (1)), tertiary education (column (2)), and having minor children (column (3)). Panel A presents results for implementing the own preferred payoff; Panel B presents results for implementing the preferred payoff for others. All specifications control for age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors are robust to heteroskedasticity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE.

Table C.12: Heterogeneity Analysis II

	Panel A. Regulation: Preferred Payoff (Self)	
	(1)	(2)
Self	0.025	0.109***
	(0.095)	(0.032)
Other	-0.212**	-0.066**
	(0.095)	(0.032)
Self × Age	0.001	
	(0.002)	
Other × Age	0.003	
	(0.002)	
Self × Minor Children in Household		-0.023
		(0.034)
Other × Minor Children in Household		0.004
		(0.035)
Control Mean	0.471	0.471
Obs.	1,875	1,875
Adj. R-Sq	0.019	0.018
Controls	X	X
	Panel B. Regulation: Preferred Payoff (Other)	
	(1)	(2)
Self	0.018	0.080**
	(0.094)	(0.032)
Other	-0.067	-0.018
	(0.093)	(0.031)
Self × Age	0.001	
	(0.002)	
Other × Age	0.001	
	(0.002)	
Self × Minor Children in Household		0.009
		(0.034)
Other × Minor Children in Household		-0.002
		(0.033)
Control Mean	0.403	0.403
Obs.	1,875	1,875
Adj. R-Sq	0.009	0.009
Controls	X	X

Notes: This table reports regressions interacting the *Self* and *Other* treatment indicators with individual characteristics: age (column (1)) and the number of minor children in the household (column (2)). Panel A presents results for implementing the own preferred payoff; Panel B presents results for implementing the preferred payoff for others. All specifications control for age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors are robust to heteroskedasticity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE.

Table C.13: Heterogeneity Analysis III

	Panel A. Regulation: Preferred Payoff (Self)				
	(1)	(2)	(3)	(4)	(5)
Self	0.097***	0.096***	0.097***	0.097***	0.097***
	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)
Other	-0.061**	-0.062**	-0.061**	-0.061**	-0.061**
	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)
Self × Preferred Payoff (Self)	-0.045				
	(0.028)				
Other × Preferred Payoff (Self)	-0.001				
	(0.028)				
Self × MG: Self vs. Self		-0.016			
		(0.029)			
Other × MG: Self vs. Self		0.015			
		(0.027)			
Self × MG: Self vs. Other			0.008		
			(0.028)		
Other × MG: Self vs. Other			0.000		
			(0.027)		
Self × DA: Self (WTP)				-0.011	
				(0.028)	
Other × DA: Self (WTP)				-0.021	
				(0.028)	
Self × DA: Other (WTP)					0.004
					(0.028)
Other × DA: Other (WTP)					-0.024
					(0.028)
Control Mean	0.471	0.471	0.471	0.471	0.471
Obs.	1,875	1,875	1,875	1,875	1,875
Adj. R-Sq	0.042	0.040	0.040	0.040	0.040
Controls	X	X	X	X	X
	Regulation: Preferred Payoff (Other)				
	(1)	(2)	(3)	(4)	(5)
Self	0.116**	0.082***	0.084***	0.100***	0.087**
	(0.052)	(0.029)	(0.028)	(0.033)	(0.037)
Other	-0.041	-0.012	-0.010	-0.029	-0.039
	(0.053)	(0.030)	(0.028)	(0.033)	(0.037)
Self × Preferred Payoff (Other)	-0.037				
	(0.045)				
Other × Preferred Payoff (Other)	0.022				
	(0.045)				
Self × WTP Self Autonomy		-0.009			
		(0.028)			
Other × WTP Self Autonomy		-0.015			
		(0.028)			
Self × WTP Other Autonomy			-0.027		
			(0.028)		
Other × WTP Other Autonomy			-0.036		
			(0.029)		
Self × Wedge Other vs. Self				-0.072	
				(0.053)	
Other × Wedge Other vs. Self				0.036	
				(0.054)	
Self × Wedge: Other vs. Other					-0.022
					(0.060)
Other × Wedge: Other vs. Other					0.052
					(0.058)
Control Mean	0.405	0.405	0.405	0.405	0.405
Obs.	1,845	1,845	1,845	1,845	1,845
Adj. R-Sq	0.052	0.051	0.052	0.053	0.052
Controls	X	X	X	X	X
Prefs	X	X	X	X	X

Notes: This table reports regressions interacting the *Self* and *Other* treatment indicators with experimental variables: the relevant preferred payoff (column (1)), misalignment gaps (MG) defined as the absolute differences between preferred payoffs and the corresponding beliefs about future allocation choices (columns (2) and (3)), and decision autonomy (DA) measures (DA: Self and DA: Other (WTP)) capture the valuation of decision rights for oneself and for another participant in € (columns (4) and (5)). All continuous experimental covariates are standardized to have mean 0 and standard deviation 1. Panel A presents results for implementing the own preferred payoff; Panel B presents results for implementing the preferred payoff for others. All specifications control for age, a female dummy, a dummy indicating having at least a tertiary education, a dummy indicating whether a person works full-time, a dummy for having minor children, and the number of minor children in the household. Standard errors are robust to heteroskedasticity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: PRESPARE.

Appendix D

Questionnaire

D.1 Survey Wave 1

Demographics

Please begin by answering the following questions. All responses will be treated confidentially, and it will not be possible to link your name to your answers.

What is your gender?

- Female
- Male
- Diverse

How old are you? Please use the slider to indicate your age. (Age range: 18–80)

What is your highest educational qualification (or equivalent)?

- No secondary school qualification
- Lower secondary school qualification (Hauptschulabschluss/Mittelschulabschluss)
- Intermediate secondary school qualification (Realschulabschluss)
- Higher education entrance qualification ((Fach-)Abitur or equivalent)
- Bachelor's or Master's degree, Diplom, or equivalent
- Doctorate (PhD)
- Other: _____

In which federal state do you currently live? (List of the 16 German federal states)

This question is a test of your attention. Please select "Somewhat agree".

Strongly agree Agree Somewhat agree Somewhat disagree Disagree Strongly disagree

Which of the following best describes your current employment status?

- Employed or civil servant, full-time
- Employed or civil servant, part-time
- Self-employed, full-time
- Self-employed, part-time
- Not employed
- Employed on short-time work
- Retired
- Irregular or marginal employment
- In education, vocational training, or studying
- On parental leave

How many people currently live in your household? Please include yourself.

1 person 2 people 3 people 4 people 5 people More than 5 people

Do you have any minor children?

- No
- Yes

Do any minor children live in your household?

No Yes, 1 minor child Yes, 2 minor children Yes, 3 minor children Yes, 4 or more minor children

Introduction Decision Tasks

The decision tasks concern CO_2, which is primarily released through the combustion of fossil fuels, for example through driving, heating, or the production of many goods.

Organizations such as "atmosfair" contribute to reducing CO_2 in the atmosphere through climate protection projects, primarily by promoting renewable energy in developing countries. In 2022, "at-

mosfair" was rated "very good" by Stiftung Warentest and was the only German provider to receive this rating.

In the decision tasks, you will have the opportunity to donate to "atmosfair".

Important: There are no right or wrong answers—please decide according to your own judgment. If you participate in a follow-up questionnaire in approximately two weeks, you will receive a receipt confirming that the promised donations were actually made.

Before we begin the decision tasks, one more estimation question:

How many kilometers must one drive by car to emit 1 kg of CO_2? Please assume a gasoline car with an average fuel consumption of 7.7 liters per 100 km. (enter number)

Excuse Experiment

In this game, you will evaluate three purchase decisions that differ in price and CO_2 emissions. As a reference: 6 km of car driving = 1 kg of CO_2.

No actual purchases will be made, but your decisions can either result in a personal bonus payment for you or reduce CO_2 in the atmosphere.

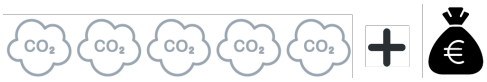
At the end of the survey, we will randomly select 1 out of 5 participants. For each selected participant, we will then draw by lot which of the three decisions will be implemented.

Important: Each of your decisions can therefore actually be implemented. The prices in the purchase decisions are realistic.

Example

Product A causes 10 kg of CO_2 and costs 10 €.

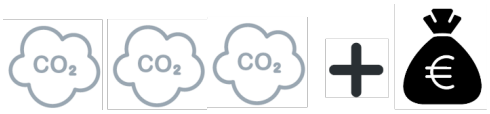
Product B causes 25 kg of CO_2 and costs 5 €.



If your decision is selected, the following happens:

Product A choice: 10 kg instead of 25 kg of CO_2 will be emitted, and thus 15 kg of CO_2 will be saved through a donation to “atmosfair.” You personally will not receive a payment.

Product B choice: 25 kg instead of 10 kg of CO_2 will be emitted, and thus no CO_2 will be saved or donated. You personally will receive a payment corresponding to the price difference of 5 €.



Comprehension Question

Assume that you were selected for payment and the example is implemented. What would be the consequences if you chose the CO_2-saving but more expensive option (Product A)?

- You receive a payment of 5 € and 15 kg of CO_2 are saved.
- You receive a payment of 5 € and no CO_2 is saved.
- You receive no payment, but 15 kg of CO_2 are saved.

Choices

Flight vs. Train

You have to decide whether to travel from Munich to Paris by train or by plane. Assume that both options take the same amount of time (including the time at the airport).

Train

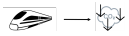
- Cost: 110 €
- CO_2 emissions: 24 kg

Flight

- Cost: 85 €
- CO_2 emissions: 180 kg

The CO_2 emissions are based on calculations by the German Environment Agency.

Which option do you prefer?

- Flight and payment of 25 € 
- Train journey and reduction of 156 kg CO_2 

Treatments

- **Info:** Both flight and train tickets are still available, and both journeys will definitely take place. Other people are also facing this decision.
- **RE:** The flight will take place anyway, and if you don't take the flight, someone else might take it instead.
- **WE:** The flight will take place anyway and is not yet fully booked. If you don't take the flight, a seat might remain empty.

Apples

You are in a supermarket and have to decide whether to buy 1 kg of organic apples from New Zealand or 1 kg of organic apples from Germany. The apples have the same taste and variety; they differ only in origin, price, and CO_2 emissions.

Apples from Germany

- Cost: 4.50 €

- CO_2 emissions: 300 g

Apples from New Zealand

- Cost: 3.75 €
- CO_2 emissions: 800 g

The CO_2 emissions are based on calculations by the Institute for Energy and Environmental Research.

Which option do you prefer?

- Apples from New Zealand and a payment of 0.75 €  ⟶ 
- Apples from Germany and a reduction of 500 g CO_2  ⟶ 

Treatments

- **Info:** The supermarket offers both apples from Germany and New Zealand. Other customers could also buy them. Unsold food is disposed of.
- **RE:** The apples from New Zealand are already in the supermarket, and if you don't buy them, someone else might.
- **WE:** The apples from New Zealand are already in the supermarket, and if you don't buy them, they might be thrown away.

Shirts

You are in a clothing store and have to decide whether to buy a T-shirt made of conventional cotton or a T-shirt made of organic cotton. Both shirts look identical, feel the same, and have the same wearing comfort.

T-shirt made of organic cotton

- Cost: 13 €
- CO_2 emissions: 3.8 kg

T-shirt made of conventional cotton

- Cost: 10 €
- CO_2 emissions: 7 kg

The CO_2 values are based on realistic information from T-shirt manufacturers.

Which option do you prefer?

- T-shirt made of conventional cotton and a payment of 3 €
- T-shirt made of organic cotton and a reduction of 3.2 kg CO_2

Treatments

- **Info:** Both the normal and organic cotton T-shirts are available in the store and could also be purchased by other customers. Unsold stock is generally disposed of.
- **RE:** The normal T-shirt has already been produced, and if you don't buy it, someone else might.
- **WE:** The normal T-shirt has already been produced, and if you don't buy it, it might be thrown away.

Beliefs

The first decision game is almost completed. Please answer the following three blocks of questions.

Important note: These questions concern the behavior of other participants in this survey. Our survey reflects the German population in terms of gender, age, federal state, and educational attainment.

Example: 49% of the German population are male, and therefore approximately 49% of survey participants are also male. If one were to randomly select someone from this survey, there is a 49% probability that they are male.

In your opinion, what percentage of participants in this survey would choose the following?

Remember: Treatment messages for **RE** and **WE** are repeated.

- Choosing apples from New Zealand (3.75 € and 800 g CO_2) instead of from Germany (4.50 € and 300 g CO_2)
- Choosing the flight (85 € and 180 kg CO_2) instead of the train journey (110 € and 24 kg CO_2)
- Choosing the T-shirt made of conventional cotton (10 € and 7 kg CO_2) instead of the T-shirt made of organic cotton (13 € and 3.8 kg CO_2)

Response scale (percentage): 0 to 100 percent

Social Acceptance

Please indicate for each decision how you evaluate the respective behavior. You have four response options ranging from "socially completely inappropriate" to "socially completely appropriate."

By "socially appropriate," we mean behavior that most people would consider "right" or "desirable."

Response options:

- Socially completely inappropriate
- Socially somewhat inappropriate
- Socially somewhat appropriate
- Socially completely appropriate

Evaluated choices:

- Choosing the train journey (110 € and 24 kg CO_2) instead of the flight (85 € and 180 kg CO_2)
- Choosing the T-shirt made of organic cotton (13 € and 3.8 kg CO_2) instead of the T-shirt made of conventional cotton (10 € and 7 kg CO_2)
- Choosing apples from New Zealand (3.75 € and 800 g CO_2) instead of from Germany (4.50 € and 300 g CO_2)
- Choosing the flight (85 € and 180 kg CO_2) instead of the train journey (110 € and 24 kg CO_2)
- Choosing the T-shirt made of conventional cotton (10 € and 7 kg CO_2) instead of the T-shirt made of organic cotton (13 € and 3.8 kg CO_2)
- Choosing apples from Germany (4.50 € and 300 g CO_2) instead of from New Zealand (3.75 € and 800 g CO_2)

Multiple Price Lists

To conclude this decision game, we would like to know how you would have decided if the prices had been different. Please indicate, for the following price differences and for all three purchase decisions, which option you would prefer.

Means of transportation: Please indicate for each row which means of transportation you would prefer to travel from Munich to Paris.

- Train journey with emissions of 24 kg CO_2
- Flight with emissions of 180 kg CO_2

Price differences (flight cheaper):

Same price €10 cheaper €20 cheaper €30 cheaper
€40 cheaper €50 cheaper €60 cheaper €70 cheaper

Apples: Please indicate for each row which apples you would prefer.

- 1 kg of apples from Germany with 300 g CO_2
- 1 kg of apples from New Zealand with 800 g CO_2

Price differences (New Zealand apples cheaper):

Same price €0.50 cheaper €1.00 cheaper €1.50 cheaper
€2.00 cheaper €2.50 cheaper €3.00 cheaper €3.50 cheaper

T-shirts: Please indicate for each row which T-shirt you would prefer.

- T-shirt made of organic cotton with emissions of 3.8 kg CO_2
- T-shirt made of conventional cotton with emissions of 7 kg CO_2

Price differences (conventional cotton T-shirt cheaper):

Same price €1.30 cheaper €2.60 cheaper €3.90 cheaper
€5.20 cheaper €6.50 cheaper €7.80 cheaper €9.10 cheaper

Regulation Experiment

Each person who participates in the second survey in approximately two weeks will receive a bonus of 2 €. In the following decision task, you can decide how the bonus in the second survey (hereafter "Survey 2") is allocated.

Notes:

In the next three questions, you can allocate the 2 € between **personal payout** and a **donation to "atmosfair"** for different groups. Each euro donated reduces 40 kg of CO_2. **Each cent** that you designate as a donation will be **doubled by another person**.

One of the questions may affect **your bonus payment and that of another participant**. More detailed explanations are provided in the respective question.

Please also keep in mind that the survey is representative of the population in Germany.

Click on "Next" to start the decision task.

Pre-Treatment Variables

Preferred Allocations

Note: Questions on allocations, beliefs, and decision rights are randomized within participants.

Preferred Allocation Self

How would you allocate the 2 € between yourself and "atmosfair"?

Please indicate the amount of your personal payout using the slider. The corresponding donation will also be displayed. The doubling of the donation is carried out by us and is already included.

Donation to atmosfair: ()

(Slider from 0 to 2 €)

Personal payout (in €) ()

Preferred Allocation (Other)

How would you allocate the 2 € between a randomly selected other survey participant and “atmosfair”?

Please indicate the amount of the personal payout for the other participant using the slider. The corresponding donation will also be displayed. The doubling of the donation is carried out by us and is already included.

Donation to atmosfair: ()

(Slider from 0 to 2 €)

Personal payout for other participant (in €) ()

Preferred Allocation (Group)

Imagine that you are part of a group consisting of yourself and a randomly selected other survey participant. For each group member, the 2 € are allocated.

How would you allocate the 2 € between a personal payout and “atmosfair” if your allocation applied to each group member?

Example:

- You choose (randomized amount) € as the personal payout.
- You and your group member then each receive (randomized amount) €.
- A total of (based on randomized amount) € is donated (half of donation € per group member).

Please indicate the amount of the personal payout using the slider below. The corresponding donation will also be displayed. Your donation is doubled because your group member donates the same amount.

Group donation to atmosfair: ()

(Slider from 0 to 2 €)

Personal payout per group member (in €) ()

Beliefs about Self Allocation

Imagine that in Survey 2 (in approximately two weeks) you have to decide again how to allocate the 2 € for yourself. How do you believe you would decide then?

Please indicate your estimate of your personal payout using the slider below. The corresponding donation will also be displayed.

Donation to atmosfair: ()

(Slider from 0 to 2 €)

Personal payout (in €) ()

How confident are you that your estimate is correct?

Very unsure Rather unsure Neither nor Rather confident Very confident

Beliefs about Others' Allocation

What personal payout do you believe other survey participants would choose for themselves on average?

Important: If your estimate is within a deviation of 0.10 € from the true average value in our survey, you will receive a bonus of 0.30 €.

Example:

- You estimate that other participants would choose 1.50 € as the personal payout on average.
- If the true value is 1.45 €, you receive a bonus of 0.30 €.
- If the true value lies outside the range of $\pm$0.10 € (here: 1.35 € or 1.55 €), you do not receive a bonus.

Please indicate your estimate using the slider below. The corresponding donation will also be displayed.

Donation to atmosfair: ()

(Slider from 0 to 2 €)

Average personal payout (in €) ()

How confident are you that your estimate is correct?

Very unsure Rather unsure Neither nor Rather confident Very confident

Decision Autonomy

Introduction

The following four questions concern monetary amounts that you do not actually have to pay; they are only meant as reference points. These questions do not affect your payment.

Imagine a person, let's call him Alex, who would allocate the 2 € for each survey participant (including you) exactly according to each participant's preference.

Autonomy over Self Allocation

Alex now offers to make the decision for you. That is, Alex would allocate the 2 € on your behalf.

Important: Alex would allocate the 2 € exactly as you would do yourself.

What would you prefer?

- Definitely make the decision myself
- Rather make the decision myself
- Rather let Alex decide
- Definitely let Alex decide

Willingness to Pay – Self Decision

Now we ask about your willingness to pay.

Reminder: This question does not affect your actual payment.

Since you prefer to make the decision yourself: How much would you be willing to pay to continue making the decision yourself?

(Slider from 0 to 2 €)

Willingness to pay (in €) ()

Willingness to Pay – Let Alex Decide

Now we ask about your willingness to pay.

Reminder: This question does not affect your actual payment.

Since you prefer to let Alex make the decision: How much would you be willing to pay for Alex to make the decision for you?

(Slider from 0 to 2 €)

Willingness to pay (in €) ()

Autonomy over Another Participant's Allocation

Alex now offers a randomly selected other survey participant, let's call him Matthias, to make the allocation for him and take the decision off his hands.

Important: Alex would allocate the 2 € exactly as Matthias would do.

What would you prefer?

- Definitely let Matthias decide for themselves
- Rather let Matthias decide for themselves
- Rather let Alex decide for Matthias
- Definitely let Alex decide for Matthias

Willingness to Pay – Matthias Decides Himself

Now we ask about your willingness to pay.

Reminder: This question does not affect your actual payment.

Since you prefer that Matthias makes the decision himself: How much would you be willing to pay for Matthias to decide for himself?

(Slider from 0 to 2 €)

Willingness to pay (in €) ()

Willingness to Pay – Alex Decides for Matthias

Now we ask about your willingness to pay.

Reminder: This question does not affect your actual payment.

Since you prefer that Alex makes the decision: How much would you be willing to pay for Alex to make the decision for Matthias?

(Slider from 0 to 2 €)

Willingness to pay (in €) ()

Introduction

Note: In all treatments answer options regarding motives are randomized for each question.

Treatment: Self

In the second survey, three weeks from now, the personal bonus of 2 € will actually be allocated. Today, you can decide how free you will be in allocating your personal 2 € bonus in approximately two weeks.

Reminder: Only participants who take part in both surveys will receive the bonus. It will therefore be paid out after Survey 2.

You have two options:

- **Decision freedom:** You can decide yourself how to allocate the 2 € in Survey 2.
- **Fixed allocation:** You set a fixed allocation of the 2 € now, which will be implemented for you in Survey 2. You will not be able to change this allocation later.

Treatment: Other

In the second survey, three weeks from now, the personal bonus of 2 € will actually be allocated. You will be randomly assigned to another survey participant (your partner), and today you can decide how free your partner will be in allocating their personal 2 € bonus in approximately two weeks.

Reminder: Only participants who take part in both surveys will receive the bonus. It will therefore be paid out after Survey 2.

You have two options:

- **Decision freedom:** Your partner can decide themselves in Survey 2 how to allocate the 2 €.
- **Fixed allocation:** You set a fixed allocation of the 2 € now, which will be implemented for your partner in Survey 2. Your partner will not be able to change this allocation later.

Treatment: Group

In the second survey, three weeks from now, the personal bonus of 2 € will actually be allocated. You will be randomly assigned to another survey participant (your partner), and today you can decide how free both you and your partner will be in allocating your respective 2 € bonuses in approximately two weeks.

Reminder: Only participants who take part in both surveys will receive the bonus. It will therefore be paid out after Survey 2.

You have two options:

- **Decision freedom:** Both you and your partner can decide for yourselves how the 2 € will be allocated for each of you in Survey 2.
- **Fixed allocation:** You set a fixed allocation of the 2 € now, which will apply to both you and your partner in Survey 2. Neither of you will be able to change this allocation later.

Comprehension Check

Treatment: Self

To continue, please answer the following comprehension question correctly:

What happens if you choose a fixed allocation?

- I cannot decide freely in Survey 2 – the allocation I set today will be implemented.
- I can decide freely in Survey 2 – the allocation I set today will *not* be implemented.
- I have no influence over which allocation applies to me in Survey 2.

Treatment: Other

To continue, please answer the following comprehension question correctly:

What happens if you choose a fixed allocation?

- My partner cannot decide freely in Survey 2 – the allocation I set today will be implemented.
- My partner can decide freely in Survey 2 – the allocation I set today will *not* be implemented.
- I have no influence over which allocation applies to my partner in Survey 2.

Treatment: Group

To continue, please answer the following comprehension question correctly:

What happens if you choose a fixed allocation?

- Neither I nor my partner can decide freely in Survey 2 – the allocation I set today will be implemented for both of us.
- My partner can decide freely in Survey 2, but I am bound by the allocation I set today.
- I have no influence over how the 2 € is allocated between my partner and me in Survey 2.

Decisions

You will now make two decisions in sequence. One of these decisions will be randomly selected and actually implemented in Survey 2.

Decision 1 – Preferred Allocation (Self)

Treatment: Self

Do you want the allocation you just chose for yourself to be implemented as a fixed allocation in Survey 2, or would you like to retain decision freedom?

Your previously chosen allocation was:

- Personal payout: (Amount Preferred Allocation (Self)) €
- Total donation to "atmosfair": (Corresponding donation) € (doubling already included)

Choice:

- Retain decision freedom
- Fixed allocation (your payout: (Amount Preferred Allocation (Self)) € and donation: (Corresponding donation) €)

Treatment: Other

Do you want the allocation you just chose for yourself to be implemented as a fixed allocation for your partner in Survey 2, or would you like to let your partner retain decision freedom?

Your previously chosen allocation was:

- Personal payout to partner: (Amount Preferred Allocation (Self)) €
- Total donation to “atmosfair”: (Corresponding donation) € (doubling already included)

Choice:

- Retain decision freedom
- Fixed allocation (partner payout: (Amount Preferred Allocation (Self)) € and donation: (Corresponding donation) €)

Treatment: Group

Do you want the allocation you just chose for yourself to be implemented as a fixed allocation for both you and your partner in Survey 2, or would you like to let both of you retain decision freedom?

Your previously chosen allocation was:

- Personal payout per group member: (Amount Preferred Allocation (Self)) €
- Total donation by group: (Corresponding donation) € (corresponding donation per member: (Corresponding donation per member) €)

Choice:

- Retain decision freedom
- Fixed allocation (per member payout: (Amount Preferred Allocation (Self)) €, group donation: (Corresponding donation) €)

Decision 2 – Preferred Allocation (Other)

Treatment: Self

Do you want the allocation you just chose for someone else to be implemented as a fixed allocation in Survey 2, or would you like to retain decision freedom?

The previously chosen allocation was:

- Personal payout: (Amount Preferred Allocation (Other)) €
- Total donation to “atmosfair”: (Corresponding donation) € (doubling already included)

Choice:

- Retain decision freedom
- Fixed allocation (your payout: (Amount Preferred Allocation (Other)) € and donation: (Corresponding donation) €)

Treatment: Other

Do you want the allocation you just chose for someone else to be implemented as a fixed allocation for your partner in Survey 2, or would you like to let your partner retain decision freedom?

The previously chosen allocation was:

- Personal payout to partner: (Amount Preferred Allocation (Other)) €
- Total donation to "atmosfair": (Corresponding donation) € (doubling already included)

Choice:

- Retain decision freedom
- Fixed allocation (partner payout: (Amount Preferred Allocation (Other)) € and donation: (Corresponding donation) €)

Treatment: Group

Do you want the allocation you just chose for someone else to be implemented as a fixed allocation for both you and your partner in Survey 2, or would you like to let both of you retain decision freedom?

Your previously chosen allocation was:

- Personal payout per group member: (Amount Preferred Allocation (Other)) €
- Total donation by group: (Corresponding donation) € (corresponding donation per member: (Corresponding donation per member) €)

Choice:

- Retain decision freedom
- Fixed allocation (per member payout: (Amount Preferred Allocation (Other)) €, group donation: (Corresponding donation) €)

Motives

After answering the following questions, the decision game is complete.

Reasons for Fixed Allocation – Preferred Allocation (Self)

Treatment: Self

Why did you decide to implement a fixed allocation for the payout of (Amount Preferred Allocation (Self)) € and total donation (Corresponding donation) €?

For each of the reasons below, indicate how important it was for you (0 = not at all important, 6 = very important):

- I wanted to ensure that the allocation is fair now
- I wanted to relieve myself of the decision in the future
- I wanted the allocation to reflect my preferences today
- I thought I might make an unfavorable decision in the future
- Other reason (please specify below)

If you have another reason, please indicate it here: _____

Treatment: Other

Why did you decide to implement a fixed allocation for the payout of (Amount Preferred Allocation (Self)) € and total donation (Corresponding donation) € for your partner?

For each reason below, indicate how important it was for you (0 = not at all important, 6 = very important):

- I wanted to ensure that the allocation is fair
- I wanted to relieve the other person of the decision
- I wanted the allocation to reflect my preferences
- I thought the other person might make an unfavorable decision
- Other reason (please specify below)

If you have another reason, please indicate it here: _____

Treatment: Group

Why did you decide to implement a fixed allocation for the payout of (Amount Preferred Allocation (Self)) € and total donation of (Corresponding donation) € for both you and your partner?

For each reason below, indicate how important it was for you (0 = not at all important, 6 = very important):

- I wanted to ensure that the allocation is fair now
- I wanted to relieve myself of the decision in the future
- I wanted the allocation to reflect my preferences today
- I thought I might make an unfavorable decision in the future
- I wanted to give the other person the freedom to decide themselves
- I thought the other person might make an unfavorable decision
- Other reason (please specify below)

If you have another reason, please indicate it here: _____

Reasons for Fixed Allocation – Preferred Allocation (Other)

Treatment: Self

Why did you decide to implement a fixed allocation for the payout of (Amount Preferred Allocation (Other)) € and total donation of (Corresponding donation) €?

For each reason below, indicate how important it was for you (0 = not at all important, 6 = very important):

- I wanted to ensure that the allocation is fair now
- I wanted to relieve myself of the decision in the future
- I wanted the allocation to reflect my preferences today
- I thought I might make an unfavorable decision in the future
- Other reason (please specify below)

If you have another reason, please indicate it here: _____

Treatment: Other

Why did you decide to implement a fixed allocation for the payout of (Amount Preferred Allocation (Other)) € and total donation (Corresponding donation) € for your partner?

For each reason below, indicate how important it was for you (0 = not at all important, 6 = very important):

- I wanted to ensure that the allocation is fair
- I wanted to relieve the other person of the decision
- I wanted the allocation to reflect my preferences
- I thought the other person might make an unfavorable decision
- Other reason (please specify below)

If you have another reason, please indicate it here: _____

Treatment: Group

Why did you decide to implement a fixed allocation for the payout of (Amount Preferred Allocation (Other)) € and total donation of (Corresponding donation) € for both you and your partner?

For each reason below, indicate how important it was for you (0 = not at all important, 6 = very important):

- I wanted to ensure that the allocation is fair now
- I wanted to relieve myself of the decision in the future
- I wanted the allocation to reflect my preferences today
- I thought I might make an unfavorable decision in the future
- I wanted to give the other person the freedom to decide themselves
- I thought the other person might make an unfavorable decision
- Other reason (please specify below)

If you have another reason, please indicate it here: _____

Reasons for Retaining Decision Freedom – Preferred Allocation (Self)

Treatment: Self

Why did you decide to retain decision freedom for the payout of (Amount Preferred Allocation (Self)) € and total donation of (Corresponding donation) €?

For each reason below, indicate how important it was for you (0 = not at all important, 6 = very important):

- I wanted to retain the freedom to decide for myself in the future
- I did not know what I would prefer in the future
- I wanted to take responsibility for the decision only in the future
- I did not think it was appropriate to decide for myself now
- Other reason (please specify below)

If you have another reason, please indicate it here: _____

Treatment: Other

Why did you decide to let your partner retain decision freedom for the payout of (Amount Preferred Allocation (Self)) € and total donation (Corresponding donation) €?

For each reason below, indicate how important it was for you (0 = not at all important, 6 = very important):

- I wanted to let the other person retain the freedom to decide
- I did not know what the other person would prefer
- I wanted the other person to take responsibility for the decision themselves
- I did not think it was appropriate to decide for the other person
- Other reason (please specify below)

If you have another reason, please indicate it here: _____

Treatment: Group

Why did you decide to let both you and your partner retain decision freedom for the payout of (Amount Preferred Allocation (Self)) € and total donation of (Corresponding donation) €?

For each reason below, indicate how important it was for you (0 = not at all important, 6 = very important):

- I wanted to retain the freedom to decide for myself in the future
- I did not know what I would prefer in the future

- I wanted to take responsibility for the decision only in the future
- I did not think it was appropriate to decide for myself now
- I wanted to give the other person the freedom to decide themselves
- I did not know what the other person would prefer
- I wanted the other person to take responsibility for the decision themselves
- I did not think it was appropriate to decide for the other person
- Other reason (please specify below)

If you have another reason, please indicate it here: _____

Reasons for Retaining Decision Freedom – Preferred Allocation (Other)

Treatment: Self

Why did you decide to retain decision freedom for the payout of (Amount Preferred Allocation (Other)) € and total donation of (Corresponding donation) €?

For each reason below, indicate how important it was for you (0 = not at all important, 6 = very important):

- I wanted to retain the freedom to decide for myself in the future
- I did not know what I would prefer in the future
- I wanted to take responsibility for the decision only in the future
- I did not think it was appropriate to decide for myself now
- Other reason (please specify below)

If you have another reason, please indicate it here: _____

Treatment: Other

Why did you decide to let your partner retain decision freedom for the payout of (Amount Preferred Allocation (Other)) € and total donation (Corresponding donation) €?

For each reason below, indicate how important it was for you (0 = not at all important, 6 = very important):

- I wanted to let the other person retain the freedom to decide

- I did not know what the other person would prefer
- I wanted the other person to take responsibility for the decision themselves
- I did not think it was appropriate to decide for the other person
- Other reason (please specify below)

If you have another reason, please indicate it here: _____

Treatment: Group

Why did you decide to let both you and your partner retain decision freedom for the payout of (Amount Preferred Allocation (Other)) € and total donation of (Corresponding donation) €?

For each reason below, indicate how important it was for you (0 = not at all important, 6 = very important):

- I wanted to retain the freedom to decide for myself in the future
- I did not know what I would prefer in the future
- I wanted to take responsibility for the decision only in the future
- I did not think it was appropriate to decide for myself now
- I wanted to give the other person the freedom to decide themselves
- I did not know what the other person would prefer
- I wanted the other person to take responsibility for the decision themselves
- I did not think it was appropriate to decide for the other person
- Other reason (please specify below)

If you have another reason, please indicate it here: _____

Debriefing

Treatment: Other

Another randomly selected participant faced the same decisions as you and either set a fixed allocation for you or let you retain decision freedom. In Survey 2, you will find out how the other person decided for you and can allocate your bonus accordingly.

Concluding Questionnaire

Effectiveness of Policy Measures

We would like to know how you feel about different policy measures aimed at reducing CO_2 emissions from everyday consumption (e.g., in supermarkets or retail stores).

How effective, in your opinion, is a CO_2 price based on CO_2 emissions, in general? A CO_2 price increases the price of a product like a tax and must be paid by all consumers. Please indicate a value between 0 and 10. 0: no effect 10: very large effect

How effective, in your opinion, is a CO_2 label on products to indicate CO_2 emissions, in general? A CO_2 label informs consumers about the amount of CO_2 emissions from production. Consumers are still free to choose their preferred product. Please indicate a value between 0 and 10. 0: no effect 10: very large effect

How effective, in your opinion, is a government ban on certain high CO_2 products, in general? A CO_2 ban prohibits the sale of certain products in Germany. Please indicate a value between 0 and 10. 0: no effect 10: very large effect

How effective, in your opinion, is a government CO_2 limit that specifies the maximum CO_2 emissions allowed in the production of a product, in general? A CO_2 limit tries to take into account as many aspects of production and transport as possible. Please indicate a value between 0 and 10. 0: no effect 10: very large effect

How effective, in your opinion, is preferential placement of CO_2-neutral products in supermarkets, in general? Preferential placement of CO_2-neutral products occurs, for example, on shelves at eye level and with color-highlighted price tags. Please indicate a value between 0 and 10. 0: no effect 10: very large effect

The following questions refer to these policy measures:

- CO_2 price
- CO_2 label
- Placement of CO_2-neutral products at eye level

- CO_2 limit for products
- Ban on high CO_2 products

What percentage of participants in this survey do you believe would generally support the nationwide implementation of these measures in Germany?

Response scale: 0 to 100 percent

What is your personal stance on these measures? Are you for or against the introduction of these measures?

Response options: In favor Rather in favor Neutral Rather against Against

What impact, in your opinion, would these measures have on your personal financial situation?

Response options: Worse Slightly worse No effect Slightly better Better

Attention check: Please enter the value 3 below. Please give a value between 0 and 10. 0 means "no effect" and 10 means "very large effect."

Political Views and Climate Change

If the federal election were held next Sunday, which party would you vote for?

Bündnis Sahra Wagenknecht (BSW) Freie Wähler AfD FDP
Die Linke Bündnis 90/Grüne CDU/CSU SPD Andere: (text field)

Please indicate your agreement with the following statements:

Answer Options

Fully agree Agree Neither agree nor disagree Disagree Fully disagree

Statements

- The state should not interfere in markets.
- The state should not intervene in consumers' purchasing decisions.
- Climate change is human-made.
- Climate change is a societal problem.
- I believe that climate change exists.

- CO_2 emissions should be regulated by the state.
- I am worried about climate change.

How strongly do you believe you are already affected by climate change in your daily life?

Not at all A little Partially Quite strongly Very strongly

Personal Information and Preferences

Almost done! In the last part of the survey, we ask a few questions about yourself. You can also provide feedback about the survey.

What is your monthly disposable income, i.e., how much money (€) do you personally have available after taxes and social security contributions to cover all your expenses?

Please enter the postal code of your current place of residence below.

The following statements describe various attitudes towards life and the future. To what extent do you personally agree with each statement?

Answer Options

Strongly agree Somewhat agree Somewhat disagree Strongly disagree

Statements

- How my life unfolds depends on me.
- Compared to others, I have not achieved what I deserve.
- What one achieves in life is primarily a matter of fate or luck.
- If one engages socially or politically, one can influence social conditions.
- I often experience that others control my life.
- Success must be earned through hard work.
- When I encounter difficulties in life, I often doubt my abilities.
- The opportunities I have in life are determined by social circumstances.
- More important than effort are the skills that one brings to life.
- I have little control over what happens in my life.

How would you describe yourself: are you generally willing to take risks or do you try to avoid them? Please answer on a scale from 0 to 10.

0 = not at all willing to take risks 10 = very willing to take risks

You can use the values in between to indicate the strength of your assessment.

How willing are you to forgo something that benefits you today in order to gain more in the future? Please answer on a scale from 0 to 10.

0 = not willing at all 10 = extremely willing

You can use the values in between to indicate the strength of your assessment.

Credibility of the Study and Feedback

Do you believe that the people responsible for this study actually donate the money to "atmosfair" to reduce CO_2 in the atmosphere?

- Yes
- No

Do you have any comments about this study?

- Enter your comments here

D.2 Survey Wave 2

Demographics

Please first complete the following questionnaire. All answers will be treated confidentially, and it is not possible to link your name to them.

What is your gender?

- Female
- Male
- Diverse

How old are you? Please move the slider to indicate your age. (Age scale: 18–80)

This question is a test of your attention. Please select the value 5.

8 10 5 14 9 12

CO_2 Pricing Experiment

Control Condition (Purchasing)

Part 1

You are in a market with two identical products. You can buy none, one, or both products. The purchase of each product causes CO_2.

You are willing to pay at most €8 for one product and the product costs €6. Therefore, buying one product gives you a benefit and an associated personal payoff of €2.

Product characteristics:

- Your personal value: €8 per product
- Price: €6 per product
- Personal payoff: €2 per product
- CO_2 emissions: 25kg per product

Your options:

- **Buy no product:** 
  - Payoff:  €0
  - CO_2 emissions:  0kg
- **Buy one product:** 
  - Payoff: €8 minus €6 = €2
  - CO_2 emissions:  25kg
- **Buy two products:** 

- Payoff: 2 times (€8 minus €6) =  €4
- CO_2 emissions:  50kg

Impact of your decision on the environment

For each participant, we make a lump-sum donation to "atmosfair" to compensate for 50kg of CO_2 – exactly as much as two products cause.

- **If you buy no product:** 
  - We pay the full donation and 50kg of CO_2 are reduced. 
- **If you buy one product (which causes 25kg CO_2):** 
  - The donation is reduced by 25kg CO_2 and 25kg CO_2 are reduced. 
- **If you buy two products (which cause 50kg CO_2):** 
  - The donation is cancelled completely and 0kg CO_2 are reduced. 

Comprehension question

To be able to continue, you must answer the following comprehension questions correctly.

Question 1: What happens if you buy **one product**?

- 25kg CO_2 are caused and you receive €2.
- 25kg CO_2 are caused and you receive €4.
- 50kg CO_2 are caused and you receive €2.

Comprehension question

Question 2: What happens if you buy **two products**?

- 50kg CO_2 are caused and you receive €2.
- 25kg CO_2 are caused and you receive €4.
- 50kg CO_2 are caused and you receive €4.

Your purchase decision:

How many products would you like to buy?

- **No product:** €0 personal payoff and emissions of 0kg CO_2
- **One product:** €2 personal payoff and emissions of 25kg CO_2
- **Two products:** €4 personal payoff and emissions of 50kg CO_2

Part 2

The rules remain the same as in Part 1.

The difference: A CO_2 price makes the product more expensive.

What does the CO_2 price do?

- A surcharge of €40 is charged for each ton of CO_2
- 25kg CO_2 correspond to an additional price of €1 per product
- The new product price is now €7
- The value of the product for you remains unchanged at €8

Possible situations:

- **Buy no product:** 
  - Payoff:  €0
  - CO_2 emissions:  0kg
- **Buy one product:** 
  - Payoff: €8 minus €7 = €1
  - CO_2 emissions:  25kg
- **Buy two products:** 
  - Payoff: 2 times (€8 minus €7) = €2
  - CO_2 emissions:  50kg

Your purchase decision:

How many products would you like to buy?

- **No product:** €0 personal payoff and emissions of 0kg CO_2
- **One product:** €1 personal payoff and emissions of 25kg CO_2
- **Two products:** €2 personal payoff and emissions of 50kg CO_2

Replacement Logic (Purchasing)

Part 1

You are in a market with a randomly assigned survey participant, your partner. In this market, there are two identical products. Your decisions influence how many products your partner can buy:

- If you buy no product, your partner can buy both products.
- If you buy one product, your partner can buy one product.
- If you buy two products, no product remains for your partner.

Conversely, the same rules apply to your partner's decisions. The purchase of each product – regardless of which person buys it – causes CO_2.

You and your partner are willing to pay at most €8 for one product and the product costs €6. Therefore, buying one product gives you and your partner a benefit and an associated personal payoff of €2.

Product characteristics:

- Your personal value: €8 per product
- Price: €6 per product
- Personal payoff: €2 per product
- CO_2 emissions: 25kg per product

Your options:

- **Neither you nor your partner buy a product:** 
  - Payoff: €0
  - CO_2 emissions: 0kg
- **Either you or your partner buys one product:** 
  - Payoff for buyer: €8 minus €6 = €2
  - CO_2 emissions: 25kg
- **Either you or your partner buys two products:** 

  - Payoff for buyer: 2 times (€8 minus €6) = €4
  - CO_2 emissions: 50kg

- **You and your partner each buy one product:** 
  - Payoff per buyer: €8 minus €6 = €2
  - CO_2 emissions: 50kg

Important: These values are identical for you and your partner.

Impact of your decision on the environment

For each market (consisting of two people), we make a lump-sum donation to "atmosfair" to compensate for 50kg CO_2 – exactly as much as two products cause.

- **No product is bought in your market:** 
  - We pay the full donation.
- **One product is bought in your market (which causes 25kg CO_2):** 
  - The donation is reduced by 25kg CO_2.
- **Both products are bought in your market (which cause 50kg CO_2):** 
  - The donation is cancelled completely.

Important: The donation amount depends on the quantity of products bought in the market and is independent of who buys the products.

Comprehension question

To be able to continue, you must answer the following comprehension questions correctly.

Question 1: What happens if you and your partner buy exactly one product?

- 25kg CO_2 are caused and you and your partner each receive €2.
- 50kg CO_2 are caused and you and your partner each receive €2.
- 50kg CO_2 are caused and you and your partner each receive €4.

Comprehension question

Question 2: What happens if you buy no product, but your partner buys one product?

- 50kg CO_2 are caused and you receive €2 and your partner €0.
- 50kg CO_2 are caused, you receive €4 and your partner €2.
- 25kg CO_2 are caused, you receive €0 and your partner €2.

Order of decisions and your role in the market:

In the market, one person is allowed to decide first. Whether this is you or your partner will be randomly determined after completion of the survey and implemented accordingly.

Important: The decision of the first person influences the options of the second. Therefore, please indicate your purchase decision for both cases.

If you decide as the first person: How many products would you like to buy?

- **No product:** €0 personal payoff and emissions of 0kg CO_2
- **One product:** €2 personal payoff and emissions of 25kg CO_2
- **Two products:** €4 personal payoff and emissions of 50kg CO_2

If you decide as the second person and your partner has bought no product: How many products would you like to buy?

- **No product:** €0 personal payoff and emissions of 0kg CO_2
- **One product:** €2 personal payoff and emissions of 25kg CO_2
- **Two products:** €4 personal payoff and emissions of 50kg CO_2

If you decide as the second person and your partner has bought one product: How many products would you like to buy?

- **No product:** €0 personal payoff and emissions of 0kg CO_2
- **One product:** €2 personal payoff and emissions of 25kg CO_2

Part 2

The rules remain the same as in Part 1.

The difference: A CO_2 price increases the product price.

What does the CO_2 price do?

- A surcharge of €40 is charged for each ton of CO_2

- 25kg CO_2 correspond to an additional price of €1 per product
- The new product price is now €7
- The value of the product for you and your partner remains unchanged at €8

Possible situations:

- **Neither you nor your partner buy a product:** 
  - Payoff: €0
  - CO_2 emissions: 0kg
- **Either you or your partner buys one product:** 
  - Payoff for buyer: €8 minus €7 = €1
  - CO_2 emissions: 25kg
- **Either you or your partner buys two products:** 
  - Payoff for buyer: 2 times (€8 minus €7) = €2
  - CO_2 emissions: 50kg
- **You and your partner each buy one product:** 
  - Payoff per buyer: €8 minus €7 = €1
  - CO_2 emissions: 50kg

If you decide as the first person: How many products would you like to buy?

- **No product:** €0 personal payoff and emissions of 0kg CO_2
- **One product:** €1 personal payoff and emissions of 25kg CO_2
- **Two products:** €2 personal payoff and emissions of 50kg CO_2

If you decide as the second person and your partner has bought no product: How many products would you like to buy?

- **No product:** €0 personal payoff and emissions of 0kg CO_2
- **One product:** €1 personal payoff and emissions of 25kg CO_2
- **Two products:** €2 personal payoff and emissions of 50kg CO_2

If you decide as the second person and your partner has bought one product: How many products would you like to buy?

- **No product:** €0 personal payoff and emissions of 0kg CO_2
- **One product:** €1 personal payoff and emissions of 25kg CO_2

Voting Decision

This part is identical for both experimental groups.

In Part 3, you can vote on whether you prefer the rules from Part 1 "without CO_2 price" or Part 2 "with CO_2 price". Please keep in mind that the CO_2 price is a revenue for the general public. You will learn more details below.

From now on, you are part of a group of 25 consumers. The members of this group are randomly selected from the participants in this survey. Please keep in mind that the survey is a representation of the population in Germany.

You will participate in three votes that follow the majority principle. Afterwards, one of the three votes will be randomly selected. The result of this vote determines your final payment for this decision game.

The vote refers to your previous decisions when purchasing products:

- Without CO_2 price: product price = €6 (Part 1) 

- With CO_2 price: product price = €7 (Part 2) 

You are now voting for or against introducing a CO_2 price:

- If you vote for the CO_2 price, you speak in favour of your decisions from Part 2 determining your payment.
- If you vote against the CO_2 price, you speak in favour of your decisions from Part 1 determining your payment.

Voting result

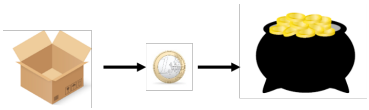
The result is decided according to the majority principle:

- If at least 13 out of 25 people vote for the CO_2 price, your purchase decision from Part 2 will be implemented.
- If at least 13 out of 25 people vote against the CO_2 price, your purchase decision from Part 1 will be implemented.

Revenue from the CO_2 price

If the CO_2 price wins in the vote, revenue is generated:

For each product that your group of 25 buys, the CO_2 price of €1 flows into a shared pool of money that can be used in different ways. A little later, we will show you three possible uses.



If the CO_2 price does not win in the vote, no revenue is generated.

Example: What happens in the vote?

Assume that you bought two products in Part 1 and one product in Part 2.

Case 1:

At least 13 people vote against the CO_2 price. Your decision from Part 1 is implemented. You bought two products there and your payment is 2 times (€8 minus €6) = €4 and nothing is donated to "atmosfair" (the donation is reduced by 50kg CO_2).

Case 2:

At least 13 people vote for the CO_2 price. Your decision from Part 2 is implemented. You bought one product there and your payment is (€8 minus €7) = €1 and only 25kg CO_2 instead of 50kg CO_2 are reduced (donation is reduced by 25kg CO_2 less). In addition, revenue of €1 is generated and goes into the group's shared pool of money.

Comprehension question:

To be able to continue, you must answer the following comprehension questions correctly.

Which of the statements is correct with regard to your decision in the votes?

- My vote is implemented for the whole group if the majority of the group also decides that way.
- There is no chance that my vote will be implemented for all group members.
- My vote will in any case be implemented for all group members.

Comprehension question:

Which of the statements is correct with regard to the votes?

In the following three votes, the issue is the use of the revenue from the CO_2 price. Please note that one of the three votes will be selected at random and that the result will then determine your final payment.

Revenue for the state

- Voting against the CO_2 price means that you are in favour of a higher product price and that revenue for the shared pool should arise when a product is purchased.
- Voting for the CO_2 price means that you are in favour of a higher product price and that revenue for the shared pool should arise when a product is purchased.
- Voting for the CO_2 price means that you are in favour of a lower product price and that no revenue for the shared pool should arise when a product is purchased.

The revenue from the CO_2 price goes to the German federal budget and the exact purpose of its use is unknown. We will actually donate the corresponding amount to the German state.

How do you vote?

- No to CO_2 price (the rules from Part 1 without CO_2 price apply)
- Yes to CO_2 price (the rules from Part 2 with a CO_2 price of €1 apply)

Climate project

The revenue from the CO_2 price goes to an environmental project of "atmosfair" that actively promotes climate protection. We will make sure that the revenue is actually donated.

How do you vote?

- No to CO_2 price (the rules from Part 1 without CO_2 price apply)
- Yes to CO_2 price (the rules from Part 2 with a CO_2 price of €1 apply)

Redistribution

The revenue from the CO_2 price is distributed equally among all 25 group members, a so-called climate bonus. If, for example, the revenue amounts to €10, each group member receives: €0.40 (€10 divided by 25 = €0.40).

How do you vote?

- No to CO_2 price (the rules from Part 1 without CO_2 price apply)
- Yes to CO_2 price (the rules from Part 2 with a CO_2 price of €1 apply)

CO_2 price level

The following three questions no longer have a direct influence on your payment. The point is how you would have voted if the CO_2 price had had a different level.

Would you have voted for or against the following CO_2 prices if the revenue had been transferred to the German federal budget?

- CO_2 price of €0.50: **For / Against**
- CO_2 price of €1.50: **For / Against**
- CO_2 price of €2.00: **For / Against**

Would you have voted for or against the following CO_2 prices if the revenue had gone to an environmental project of "atmosfair" that actively promotes climate protection?

- CO_2 price of €0.50: **For / Against**
- CO_2 price of €1.50: **For / Against**
- CO_2 price of €2.00: **For / Against**

Would you have voted for or against the following CO_2 prices if the revenue had been distributed in equal shares within your group of 25 consumers?

- CO_2 price of €0.50: **For / Against**
- CO_2 price of €1.50: **For / Against**
- CO_2 price of €2.00: **For / Against**

Control Condition (Post-Experiment)

Think again about the decisions regarding product purchases.

How many of your 24 group members do you believe bought zero, one, or two products without a CO_2 price?

The three numbers must sum to 24.

- _____ One product bought
- _____ Two products bought
- _____ No product bought

Think again about the decisions regarding product purchases.

How many of your 24 group members do you believe bought zero, one, or two products with a CO_2 price of €1?

The three numbers must sum to 24.

- _____ One product bought
- _____ Two products bought
- _____ No product bought

You were previously allowed to vote three times on whether you are for or against a CO_2 price in product purchases. The three votes differed with regard to the use of revenues.

Please rank the three types of revenue use in descending order according to your preferred variants. At the top the best variant for you and at the bottom the worst variant for you.

- _____ Revenues to the state treasury

- _____ Equal distribution of revenues to everyone
- _____ Revenues to environmental projects

How many of your 24 group members do you believe voted for a CO_2 price for the following uses?

- Revenues to the state treasury ..... _____
- Equal distribution of revenues to everyone ..... _____
- Revenues to environmental projects ..... _____

Now two more questions and the decision game is completed.

The questions concern your assessment of purchase decisions in the decision game.

For each possible decision, please indicate how socially appropriate you consider the respective behaviour. You have four response options from "socially completely inappropriate" to "socially completely appropriate". Assume that there is no CO_2 price.

By "socially appropriate" we mean behaviour that most people would consider "right" or "desirable".

Response options:

- Socially completely inappropriate
- Socially somewhat inappropriate
- Socially somewhat appropriate
- Socially completely appropriate

Evaluated choices:

- No product purchased (€0 and 0kg CO_2)
- One product purchased (€2 and 25kg CO_2)
- Two products purchased (€4 and 50kg CO_2)

The questions concern your assessment of purchase decisions in the decision game.

For each possible decision, please indicate how socially appropriate you consider the respective behaviour. You have four response options from "socially completely inappropriate" to "socially completely appropriate". Assume that there is a CO_2 price of €1.

By "socially appropriate" we mean behaviour that most people would consider "right" or "desirable".

Response options:

- Socially completely inappropriate
- Socially somewhat inappropriate
- Socially somewhat appropriate
- Socially completely appropriate

Evaluated choices:

- No product purchased (€0 and 0kg CO_2)
- One product purchased (€1 and 25kg CO_2)
- Two products purchased (€2 and 50kg CO_2)

Replacement Logic

Think again about the decisions regarding product purchases.

How many products do you believe your partner bought in the market if he was allowed to buy first in the market?

- ◯ One product bought
- ◯ Two products bought
- ◯ No product bought

Think again about the decisions regarding product purchases.

How many of your 24 group members do you believe bought zero, one, or two products without a CO_2 price if the respective group member was allowed to buy first in the market?

The three numbers must sum to 24.

- _____ One product bought
- _____ Two products bought
- _____ No product bought

Think again about the decisions regarding product purchases.

How many products do you believe were bought in total by your 24 group members without a CO_2 price?

Note: A maximum of 48 products can be bought.

Number of products: _____

Think again about the decisions regarding product purchases.

How many of your 24 group members do you believe bought zero, one, or two products with a CO_2 price of €1 if the respective group member was allowed to buy first in the market?

The three numbers must sum to 24.

- _____ One product bought
- _____ Two products bought
- _____ No product bought

Think again about the decisions regarding product purchases.

How many products do you believe were bought in total by your 24 group members with a CO_2 price?

Note: A maximum of 48 products can be bought.

Number of products: _____

You were previously allowed to vote three times on whether you are for or against a CO_2 price in product purchases. The three votes differed with regard to the use of revenues.

Please rank the three types of revenue use in descending order according to your preferred variants. At the top the best variant for you and at the bottom the worst variant for you.

- _____ Revenues to the state treasury
- _____ Equal distribution of revenues to everyone
- _____ Revenues to environmental projects

How many of your 24 group members do you believe voted for a CO_2 price for the following uses?

- Revenues to the state treasury ..... _____
- Equal distribution of revenues to everyone ..... _____

- Revenues to environmental projects .....

Now two more questions and the decision game is completed.

The questions concern your assessment of purchase decisions in the decision game. Again assume that one is allowed to decide first in the market.

For each possible decision, please indicate how socially appropriate you consider the respective behaviour. You have four response options from "socially completely inappropriate" to "socially completely appropriate". Assume that there is no CO_2 price.

By "socially appropriate" we mean behaviour that most people would consider "right" or "desirable".

Response options:

- Socially completely inappropriate
- Socially somewhat inappropriate
- Socially somewhat appropriate
- Socially completely appropriate

Evaluated choices:

- No product purchased (€0 and 0kg CO_2)
- One product purchased (€2 and 25kg CO_2)
- Two products purchased (€4 and 50kg CO_2)

The questions concern your assessment of purchase decisions in the decision game. Again assume that one is allowed to decide first in the market.

For each possible decision, please indicate how socially appropriate you consider the respective behaviour. You have four response options from "socially completely inappropriate" to "socially completely appropriate". Assume that there is a CO_2 price of €1.

By "socially appropriate" we mean behaviour that most people would consider "right" or "desirable".

Response options:

- Socially completely inappropriate
- Socially somewhat inappropriate

- Socially somewhat appropriate
- Socially completely appropriate

Evaluated choices:

- No product purchased (€0 and 0kg CO_2)
- One product purchased (€1 and 25kg CO_2)
- Two products purchased (€2 and 50kg CO_2)

Regulation Experiment: Final Allocation Decisions

You will receive a €2 bonus for participating in the second survey. In the following part of the survey, we ask how this bonus should be allocated.

Fixed Allocation

Treatment: Self

In the first survey, you allocated €2 between a personal payout for yourself and "atmosfair". You chose a fixed allocation. The allocation was:

- For yourself: () €
- For atmosfair: () € (doubled by us)

Your personal payout is therefore () € and the total donation is () €.

Treatment: Other

In the first survey a few days ago, another participant could allocate €2 between a personal payout for you and "atmosfair". They chose a fixed allocation. The allocation was:

- For you: () €
- For atmosfair: () € (doubled by us)

Your personal payout is therefore () € and the total donation is () €.

Treatment: Group

In the first survey, another participant allocated €2 between a personal payout and a donation to "atmosfair" — both for you and himself. He chose a fixed allocation. The allocation was:

- For him and yourself: () € each
- For atmosfair: () € each

Your personal payout is therefore () € and the total donation by both participants is () €.

In the first survey a few days ago, you allocated €2 between a personal payout for yourself and another randomly selected participant, and a donation to "atmosfair". You chose a fixed allocation. The allocation was:

- For yourself: () €
- For the other participant: () €
- For atmosfair: () € (split evenly between both participants)

Your personal payout is therefore () € and the total donation by you and the other participant is () €.

Decision Freedom

Treatment: Self

In the first survey a few days ago, you allocated €2 between a personal payout for yourself and "atmosfair". You chose to retain decision freedom.

Now you can decide how much of the €2 you would like to receive as a personal payout. Use the slider below to indicate your choice. The remainder will be donated to "atmosfair" (our doubling is already included).

- Donation to atmosfair: () €

Personal payout (in €): ()

Treatment: Other

In the first survey a few days ago, another participant could allocate €2 between a personal payout for you and "atmosfair". They chose to retain decision freedom.

Now you can decide how much of the €2 you would like to receive as a personal payout. Use the slider below to indicate your choice. The remainder will be donated to "atmosfair" (our doubling is already included).

- Donation to atmosfair: () €

Personal payout (in €): ()

Treatment: Group

In the first survey a few days ago, another participant could allocate €2 between a personal payout for you, themselves, and a donation to "atmosfair". They chose to retain decision freedom.

Now you can decide how much of the €2 you would like to receive as a personal payout. Use the slider below to indicate your choice. The remainder will be donated to "atmosfair" (our doubling is already included).

- Donation to atmosfair: () €

Personal payout (in €): ()

In the first survey a few days ago, you allocated 2 € between a personal payout for yourself, another randomly selected participant, and a donation to "atmosfair". You chose to retain decision freedom.

Now you can decide how much of the €2 you would like to receive as a personal payout. Use the slider below to indicate your choice. The remainder will be donated to "atmosfair" (our doubling is already included).

- Donation to atmosfair: () €

Personal payout (in €): ()

PANAS

We would now like to know how you feel. The following words describe different feelings and sensations. Please read each word and indicate on the scale next to each word how intensely you generally feel this way. You can choose between five levels.

Please indicate how intensely you generally feel each of the following emotions.

Response options:

Not at all A little Moderately Quite a bit Extremely

Feelings:

Active	Distressed	Interested	Excited
Upset	Strong	Guilty	Scared
Hostile	Inspired	Proud	Irritable
Enthusiastic	Ashamed	Alert	Nervous
Determined	Attentive	Jittery	Afraid

Follow-Up Excuse Experiment

Please think again about the first decision game from the first survey. It involved three purchase decisions:

- Traveling from Munich to Paris by plane or by train.
- Buying organic apples from New Zealand or Germany.
- Buying a T-shirt made of conventional cotton or organic cotton.

We would now like to know how you would decide today. These decisions will have no impact on your payment this time.

Remember: If you choose the cheaper option, it saves you money and results in a personal payment, but at the same time more CO_2 is emitted (less CO_2 is saved through a donation).

Participants make same consumption choices as in Wave 1 and questions are identical

Treatments:

- In the last survey, (depending on wave 1 treatment) % of participants chose the flight.
- In the last survey, (depending on wave 1 treatment) % of participants chose the conventional T-shirt.
- In the last survey, (depending on wave 1 treatment) % of participants chose the apples from New Zealand.

Additional Outcomes

One policy area among others is climate policy. We will inform the next federal government in an official letter about the share of participants in our representative survey who support stronger climate protection.

Do you want your vote for stronger climate protection to be counted? The vote is completely anonymous.

- Yes, I want my vote to be counted.
- No, I do not want my vote to be counted.

The following are possible behaviors that experts believe would contribute to reducing greenhouse gas emissions such as CO_2. To what extent would you be willing to adopt the following behaviors?

- Restrict flights
- Reduce car travel
- Ownership and use of an electric vehicle
- Reduce meat consumption
- Reduce heating or cooling of your house or apartment

Response scale: Not at all, A little, Moderately, Quite, Definitely

Attention check: Please enter the value 8 below. Provide a value between 0 and 10, where 0 means "no effect" and 10 means "very large effect."

Almost done! Finally, there are two questions about yourself, followed by questions regarding the upcoming federal election on 23.02.2025.

How important are the following issues for your personal voting decision in the federal election on 23.02.2025?

Peace/Security Refugees/Asylum Climate protection
Economy Pensions/Old-age security Social justice

Response scale:

Not at all important Unimportant Rather unimportant
Rather important Important Very important

How serious do you consider the current problems in Germany regarding the following issues?

Climate protection Economy Pensions/Old-age security
Refugees/Asylum Social justice Peace/Security

Response scale:

No problem at all Rather small problem Medium-sized problem
Rather large problem Large problem

Which party would you currently vote for? Your answer is, of course, anonymous and cannot be linked to your name.

FDP Bündnis 90/Grüne Die Linke Bündnis Sahra Wagenknecht (BSW)
AfD CDU/CSU Freie Wähler SPD
Other: (text) Still undecided Prefer not to answer I do not vote

Preferences and Credibility Study

Imagine that you have won a prize of 1000 €, which you can claim immediately. However, you can also wait one month to receive a higher prize.

What is the smallest additional amount of money you would need on top of the 1000 € in one month for you to prefer waiting rather than claiming the prize immediately?

Please enter this additional amount (in €) in the field below.

Example: If you enter 1 €, you would receive 1001 € after one month (1000 € prize plus additional amount of 1 €).

Additional amount (in €):

- Enter value here

Now we ask the same question, but the waiting period is one year instead of one month.

You have won a prize of 1000 €, which you can claim immediately. You can also wait one year to receive a higher prize.

What is the smallest additional amount of money you would need on top of the 1000 € in one year for you to prefer waiting rather than claiming the prize immediately?

Please enter this additional amount (in €) in the field below.

Example: If you enter 1 €, you would receive 1001 € after one year (1000 € prize plus additional amount of 1 €).

Additional amount (in €):

- Enter value here

Trust in the government and public organizations

How strongly do you trust the government or public organizations? 0 means "no trust at all" and 10 means "very high trust."

Response scale: 0 = no trust at all, 1, 2, 3, 4, 5, 6, 7, 8, 9, 10 = very high trust

Proof of Donations

As announced in the previous survey, we provide the following certificate to show that the promised donations to "atmosfair" were actually made. All donations promised in this survey will again be carried out according to your decisions, and you will have the opportunity to verify the proof.

The proof of donations from the first survey can be found here. After the completion of the study, we will also publish the proof for the second survey at the beginning of March on this page. Feel free to save this link to view the proof: `https://sites.google.com/view/ku-mikro/startseite`

Do you believe that the people responsible for this study actually donate the money to "atmosfair" to reduce CO_2 in the atmosphere?

- Yes
- No

Do you have any comments about this study?

- Enter your comments here

D.3 Certificates “Atmosfair”

nachdenken • klimabewusst reisen

atmosfair

Zertifikat

für kompensierte Treibhausgase

KU Eichstätt-Ingolstadt
Forschungsprojekt Entscheidungsverhalten
(Umfrage 27.01.-07.02.2025)
kompensiert am 10.02.2025 mit atmosfair
18.800 kg CO_2Treibhausgase.

Was bewirkt Ihr Klimaschutzbeitrag?
Mit Ihrem Klimaschutzbeitrag in Höhe von 470,00 Euro unterstützen Sie unter anderem folgende Projekte:

Effiziente Kochsysteme für Familien in Nigeria

Mit Ihrem Beitrag können 7 Kochsysteme finanziert werden. Diese sparen 80 % Energie ein.

Stromerzeugung aus Senfernteresten in Indien

Mit Ihrem Beitrag können 24 Haushalte ein Jahr lang mit erneuerbarem Strom versorgt werden.

Bau von Biogasanlagen für Haushalte in Kenia

Mit Ihrem Beitrag kann der Bau von 1 Biogasanlagen finanziert werden.

Seit dem 01.01.2021 betreibt atmosfair die Genehmigung der Projekte nach dem neuen Regelwerk des Klimaschutzabkommens von Paris. Die hier aufgeführten Projekte haben bereits Zusagen der Gastländer erhalten (inkl. sog. Corresponding Adjustments) oder nutzen CO_2-Minderungen von vor dem obigen Stichtag, um Doppelzählungen auszuschließen.

Gold Standard for the Global Goals

United Nations Framework Convention on Climate Change

Mehr auf atmosfair.de

nachdenken • klimabewusst reisen

atmosfair

Zertifikat

für kompensierte Treibhausgase

KU Eichstätt-Ingolstadt
kompensiert am 24.02.2025 mit atmosfair
195.600 kg CO_2 Treibhausgase.

Was bewirkt Ihr Klimaschutzbeitrag?

Mit Ihrem Klimaschutzbeitrag in Höhe von 4.890,00 Euro unterstützen Sie unter anderem folgende Projekte:

Effiziente Kochsysteme für Familien in Nigeria

Mit Ihrem Beitrag können 75 Kochsysteme finanziert werden. Diese sparen 80 % Energie ein.

Stromerzeugung aus Senfernteresten in Indien

Mit Ihrem Beitrag können 245 Haushalte ein Jahr lang mit emeuerbarem Strom versorgt werden.

Bau von Biogasanlagen für Haushalte in Kenia

Mit Ihrem Beitrag kann der Bau von 15 Biogasanlagen finanziert werden.

Seit dem 01.01.2021 betreibt atmosfair die Genehmigung der Projekte nach dem neuen Regelwerk des Klimaschutzabkommens von Paris. Die hier aufgeführten Projekte haben bereits Zusagen der Gastländer erhalten (inkl. sog. Corresponding Adjustments) oder nutzen CO_2-Minderungen von vor dem obigen Stichtag, um Doppelzählungen auszuschließen.

Gold Standard for the Global Goals

United Nations Framework Convention on Climate Change

Mehr auf atmosfair.de

Appendix E

Survey and Data Documentation

Survey Design and Implementation

Survey Company and Fieldwork

The survey was conducted in collaboration with Bilendi, a professional survey provider. Bilendi operates 37 access panels across Europe, comprising more than 400,000 panelists. The company has extensive experience in academic and market research, having worked with over 500 academic institutions. It adheres to high professional standards and holds multiple industry certifications, including ISO certification and membership in organizations such as ESOMAR, DGOF, ASSIRM, BVM, Syntec Conseil, CubeM Insights Analytics, MRS, Swiss Insights, Markkinatutkimusliitto, IAB Europe, Swedish Insighters, Adetem, IREP, and the MOA Expertise Center.

We designed the questionnaire ourselves and used Qualtrics to host the survey. The collaboration with Bilendi was limited to data collection and targeted quota invitations. We conducted a soft-launch for both survey waves. For the first wave, the soft-launch included 290 participants and yielded an average length of interview (LOI) of 24 minutes. Participants received incentives for participation alone averaging €2.40, calculated as €0.10 per minute.

For the second wave, a soft-launch with 59 participants was conducted. The average LOI was 20 minutes, and participants received incentives for participation alone averaging €2.00, again €0.10 per minute. Throughout the first wave, we monitored data quality and the status of quotas in close collaboration with the survey company. Based on daily quota updates, we communicated adjustments to Bilendi so that invitations could be tailored accordingly.

Survey Mode and Protocol

The data were collected through an online survey administered via the Qualtrics platform. Bilendi recruited and invited participants via email. Initially, Bilendi sent broad invitations to eligible panelists. Based on daily updates on the fulfillment of demographic quotas, further invitations were targeted accordingly to ensure a representative sample.

The survey protocol included several quality assurance mechanisms. All respondents had to pass an initial attention check at the start of the survey in order to proceed. A second attention check was included later in the questionnaire but was not used for exclusion. Before entering the experimental sections, participants were required to correctly answer comprehension check questions to demonstrate sufficient understanding of the task. The number of mistakes in these comprehension checks was also recorded as an additional quality indicator.

To monitor and maintain data quality, we observed a slight underrepresentation of males with low educational attainment toward the end of fieldwork. The first wave of data collection ran from January 27 to February 7, 2025, with a target sample size of 2,400 respondents. Bilendi allowed for a 10% oversample (n = 240) at no additional cost to ensure adequate data quality, resulting in a total of 2,639 respondents.

After data collection, we implemented quality filters to ensure both representativeness and data integrity. Specifically, we excluded "speeders", defined as respondents who completed the survey in 9.3 minutes or less, as they tended to perform poorly on comprehension checks. This led to a reduced wave 1 sample of 2,414 participants. To preserve the original quotas, the 9.3-minute threshold was chosen based on its predictive power for overall data quality.

For wave 2, all participants were invited in one batch. A structured reminder schedule was implemented: the first reminder was sent on February 15, the second on February 17, and the survey was closed on February 21, 2025.

All 2,414 valid participants from wave 1 were re-invited for the second survey wave, which achieved a total of 1,936 completed responses and 2,353 link clicks, corresponding to an attrition rate of approximately 20%. After excluding cases with missing control variables, the main analysis sample for wave 2 (specifically for follow-up questions on consumption behavior) includes 1,912 individuals. Importantly, the exact number of observations may vary slightly across outcome variables, but item response rates consistently exceed 99%.

No translation or localization procedures were necessary, as the survey was conducted entirely in German for a German-speaking population. However, some degree of localization was ensured: Qualtrics recorded respondents' geolocation via IP addresses, and participants were explicitly asked to report their residential federal state and zip code.

Sample Design

Target Population and Sampling Frame The target population consists of the adult German population. The sample was drawn from Bilendi's online access panel, which includes individuals who have voluntarily registered to participate in surveys. This panel is frequently used for academic research and includes participants from all federal states in Germany.

Sampling Strategy The sampling followed a quota-based approach to ensure representativeness across key demographics. Specifically, quotas were set for:

- Age × gender (cross-quota),
- Educational attainment, and
- Residential federal state.

Participants were invited accordingly to fill these quotas, and daily monitoring ensured that the sampling frame remained balanced throughout the field period.

Sample Size and Power Considerations Sample size planning was guided by ex ante power calculations for the main behavioral outcomes in each experimental module. The underlying assumptions were informed by the pretest results. All calculations were based on a significance level of $\alpha = 0.05$, statistical power of 0.8, and a minimum detectable effect size of 10 percentage points for the respective binary outcome variables. For the excuse experiment, this implied a required sample size of 285 observations per treatment group. For the regulation experiment, the power analysis indicated a required sample size of 539 observations per treatment. For the CO_2 pricing experiment, the corresponding requirement was 388 observations per treatment group for the binary voting outcomes. The ex ante power calculations focused on the main behavioral outcomes rather than secondary outcomes such as norms or moral costs. In the realized data, final sample sizes were generally at or above these thresholds,

supporting adequate power for the main analyses.

Consideration of Contextual Factors The timing of both survey waves was chosen to avoid seasonal disruptions such as public holidays and to minimize potential external shocks that might influence participant responses. In particular, the survey was completed prior to the 2025 German federal election.

Response and Attrition

Response Rates

Unit response is defined as the proportion of individuals who completed the survey out of all those who accessed the survey link. A total of 6,329 individuals clicked on the invitation link. Of those, 4,077 proceeded to answer the first attention check (which followed initial demographic questions on age, gender, education, and residential federal state), meaning 2,252 individuals dropped off before reaching this point.

Among those who proceeded, 98 failed the first attention check, leaving 3,979 valid participants. 41 individuals were excluded due to overrepresentation in specific quota groups, resulting in 3,938 eligible respondents. Out of those, 1,299 did not complete the full survey. This led to an initial sample of 2,639 completed surveys.

After applying data quality restrictions (excluding speeders with a completion time of $<=$ 9.3 minutes), the final analytic sample for Wave 1 included 2,414 respondents.

Item Response Item response was exceptionally high. Quota-relevant variables such as age, gender, education, and residential federal state were enforced and completed by all respondents. Across control variables used in the main analysis, item response rates consistently exceeded 98%.

Overall and Subgroup Response Rates Subgroup-specific response rates (e.g., by education or gender) are not separately calculated due to the enforced completeness of key variables and high overall item response.

Nonresponse and Attrition Bias

Comparison of Respondents vs. Non-Respondents We are unable to meaningfully compare respondents and non-respondents, as most individuals who did not participate in the survey (i.e., did not answer the attention check) did not provide any background information. Specifically, of the 6,329 individuals who clicked on the survey invitation link, 2,252 dropped out before reaching the first attention check and therefore did not supply any demographic data. Consequently, systematic differences between respondents and non-respondents cannot be assessed.

Adjustments for Nonresponse No statistical adjustments (e.g., weighting, imputation) are applied to account for unit nonresponse or attrition. However, quotas on age, gender, education, and federal state are enforced at the sampling stage to ensure a representative distribution in the sample.

Handling of Partial Completions and Item Nonresponse Participants who did not complete the full survey (i.e., those who were not redirected back to the survey provider upon completion) are excluded from the dataset. Regarding item nonresponse, key demographic and stratification variables are required for quota balancing and thus fully observed. For analytic purposes, individuals with missing values in pre-specified control variables are excluded from the main analyses.

Incentives and Retention Measures

Type and Amount of Respondent Incentives

Respondent incentives consisted of experimental earnings and a time-based participation payment of 0.10 € per minute.

Communication and Retention Strategies

To promote participation and retention, the survey company issued targeted invitations based on quota needs and sent reminders to boost response rates. Specifically, in Wave 1, one reminder was sent on February 6, 2025, and data collection concluded on February 7, 2025. For Wave 2, participants were invited simultaneously, and reminders were sent on February 15 and February 17, with the survey closing on February 21, 2025.

Ethical Approval and Informed Consent

The study received approval from the Ethics Committee of the Catholic University of Eichstätt–Ingolstadt (IRB No. 183–2024; July 29, 2024). All participants provided informed consent prior to taking part in the survey. The consent procedure explicitly authorized the collection and analysis of all survey responses, including the linkage of data across the two survey waves.

Questionnaire Design

Structure and Flow

Number of Sections and Estimated Completion Time The survey was divided into two waves, each with a structured flow and estimated completion times based on soft launches.

- **Wave 1** (estimated duration: 24 minutes) included:
  - Consent form and demographic questions, including an initial attention check for early screen-outs
  - Excuse experiment
  - Regulation experiment
  - Questions regarding measures to regulate CO_2 emissions and economic preferences
  - Final personal remarks on the study
- **Wave 2** (estimated duration: approximately 20 minutes) included:
  - Consent form and selected demographics to verify consistency in age and gender responses
  - CO_2 pricing experiment
  - Final decisions for the regulation experiment
  - Follow-up excuse experiment (norm correction)
  - Questions about the upcoming federal state election and additional economic preferences
  - Final personal remarks on the study

Routing Logic and Filters Routing was straightforward with one conditional filter: participants who indicated they live alone were not asked about the number of minor children in their household.

Pretesting and Cognitive Testing Prior to the main data collection, the survey underwent pretesting with students and a small sample of other individuals to ensure clarity and comprehension of questions. Based on feedback, adjustments were made to improve the design and flow.

Measurement Quality

Use of Validated Items and Scales Wherever possible, we used validated and widely recognized survey items. Specifically, we relied on questions from the SOEP (Socio-Economic Panel) to measure risk and time preferences as well as locus of control. Additionally, we used the PANAS scale (Positive and Negative Affect Schedule), a well-established psychological instrument for assessing affective states.

Randomization of Question Order or Response Options Randomization of question and response option order was implemented to mitigate order effects. For specific details on the randomization procedures and their role within experimental manipulations, please refer to the experimental design sections.

Measures to Reduce Social Desirability Bias or Priming Effects To reduce potential social desirability bias and priming, we implemented the following measures:

- Use of real monetary incentives and actual donation opportunities
- Randomized ordering of relevant questions to avoid systematic priming effects

Open-Ended Comments Participants were given opportunities to provide open-ended responses in two places:

- Final personal remarks at the end of both survey waves
- Motives behind regulation decisions in the regulation experiment (captured via open text fields)

Sensitive Topics and Ethical Safeguards

Measures to Protect Respondent Privacy and Anonymity We ensured privacy and anonymity through the following measures:

- Respondents were identified only via anonymized IDs

- Payments to participants were handled entirely by the survey company
- Donations were executed directly by the research team, without any link to respondent identities

Strategies for Sensitive or Personal Questions Sensitive topics were limited in scope in our study, and only elicited with respect to goals of the study. The survey design ensured anonymity, and no personal identifiers were collected that would compromise individual privacy.

Data Processing and Quality Control

Data Cleaning and Coding

Recoding of Responses and Construction of Derived Variables Recoding, variable construction, and derivation were conducted as outlined in the empirical sections of the respective chapters. The relevant transformations are discussed in detail in the respective chapters.

Treatment of Open-Ended Responses Open-ended responses—particularly final comments and motives related to regulation decisions—are analyzed using Python. Responses are categorized into positive and critical feedback about the survey. (This analysis is currently being finalized.)

Outlier and Error Detection

Identification and Treatment of Implausible or Extreme Values There were no major concerns regarding implausible or extreme values. A limited number of variables included predefined top categories to cap outliers. For instance, monthly income was capped at "10,000 EUR or more" to avoid distortions from extreme values.

Winsorizing, Trimming, or Imputation Practices Due to the very high item response rate, we did not apply winsorization, trimming, or imputation. Individuals are only dropped from analyses if they have missing data on pre-specified control variables, as defined in the pre-analysis plan.

Consistency and Logic Checks

Internal Consistency Across Items We implemented consistency checks across repeated items:

- Age and gender were elicited in each survey wave (via a slider). In 95.9% of cases, respondents gave consistent answers—minor discrepancies likely stemmed from slider imprecision, not intentional misreporting.
- Gender responses were consistent in 99.2% of cases.

Comprehension and Understanding of Survey Questions In experimental modules, participants were required to correctly answer comprehension checks before proceeding. This ensured adequate understanding of task instructions. If comprehension questions were answered incorrectly, participants were not allowed to continue with the experiment in question.

Documentation and Reproducibility

Variable Naming and Labeling All variables are consistently named and clearly labeled in the dataset. Labels follow a systematic structure for ease of interpretation and merging across waves.

Codebook Availability An overview of variable names, definitions, and coding logic is provided in the relevant empirical chapters and appendix materials. Values and coding logic for the used variables are described in the respective chapters. Additionally, please refer to the respective tables on summary statistics throughout the dissertation.

Availability of Scripts and Replication Materials All Stata scripts, as well as supporting Python or R scripts used for data preparation and analysis, are fully documented and will be shared in a public replication archive upon publication.

Weighting and Representativeness

Design Weights and Post-Stratification Weights

Design weights and post-stratification weights were not applied in the analysis. The sampling strategy relied on predefined quotas to ensure representativeness across key demographic dimensions of the German adult population, including age × gender, educational attainment, and residential federal state.

Benchmarking Against Population Statistics

Benchmarking was conducted using official Eurostat data, which formed the basis for the quota sampling targets. These reference statistics were provided by the survey company Bilendi. A detailed comparison between the collected sample and the population benchmarks demonstrates close alignment across all stratification variables. The corresponding comparison table is reported in the appendix (see Table A.1).

Limitations and Robustness Considerations

Known Limitations in the Data

As with any online panel data, limitations may arise due to self-selection, non-coverage of certain population segments (e.g., individuals with low digital literacy), and potential bias in self-reports. While quotas help ensure representativeness along key dimensions (age × gender, education, federal state), residual biases cannot be fully ruled out. No interviewer effects are present due to the self-administered online mode. Recall bias and social desirability bias are minimized through survey design, anonymization, and incentivized, behaviorally oriented tasks.

Robustness Checks Performed

Robustness checks are discussed in the relevant sample and result sections of the respective chapters (see Sections Sections 1.2.3, 1.2.5, 1.3.4, 2.4, 2.6, 3.4, and 3.6).

Appendix F

Pre-Analysis Plans

F.1 Pre-Analysis Plans Chapter 1

F.1.1 Main Experiment

Introduction

Many consumers believe to make independent choices when their decision-making in fact may depend on others. For example, consumers might excuse selfish or morally precarious behavior by invoking actions of others (Danzer, 2024): “If I don’t do it, someone else will”. This reasoning aligns with the so-called replacement excuse (RE) which can explain why even morally conscientious individuals occasionally act in contrast to their own convictions. For instance, environmentally conscious individuals may be frequent fliers (Barr et al., 2011). Choosing a cheaper plane ticket over a train ride might be justified by the belief that someone else will purchase the ticket, meaning total carbon emissions remain unaffected. Alternatively, the same behavior could be excused by a narrative we call the waste excuse (WE): "If I don’t do it and no one else will, resources might be wasted." Consumers may argue that an empty seat in a scheduled airplane represents wasted resources. Although both excuses attempt to justify some socially contested behavior, they offer opposing interpretations of the decision context. Therefore, it is crucial to understand how the availability of different excuses influences economic decision-making.

Moral values and their influence on behavior are central to research on excuses. In economics, morality typically arises when decisions involve a trade-off between personal utility gains (e.g., money) and negative externalities for third parties (e.g., carbon emissions, canceling donations etc.). Actions

associated with externalities are often viewed as immoral. Research on the RE and its connection to morality is still in its early stages and has produced mixed results, while no direct research appears to exist on the WE. Bartling and Özdemir (2023) experimentally show that the availability of the RE has a limited impact on moral behavior, largely due to counteracting social norms. In contrast, Ziegler et al. (2024) demonstrate the full erosion of moral values in an experimental market when the RE is present. Although individuals agree with a social norm that disapproves RE behavior, they do not adhere to the norm. Given this discrepancy and the lack of empirical evidence, our project aims to investigate the relevance of the RE, the WE and social norms for morally and environmentally relevant behavior in greater depth. A key distinguishing feature of our study from prior research is that we introduce and compare two distinct types of excuses but do not alter market design in the experimental setting. We introduce narratives that invoke the RE and WE while keeping all other factors constant: providing different interpretations of the same objective information triggers excuses for socially suboptimal consumption choices.

Previous research has primarily tested the existence of the RE in abstract settings In contrast, we apply the WE and RE to three different real-world consumer product categories: frequent or daily purchases (e.g., food), durable goods (e.g., clothing), and transportation (e.g., flights and train rides). In a survey experiment with one control group and three treatments, we randomly assign "replacement statements" and "waste statements" designed to trigger the WE and RE to a subset of the sample. Additionally, participants are assigned to a control group and another treatment group providing objective information but no excuses. Since these excuses provide interpretations of objective information, our experimental design includes one treatment that presents only the objective information, but no excuse. The additional RE and WE treatments incorporate this same information and add the associated excuse. The control group receives neither objective information nor excuse statements. Objective information, for example, indicates that there are still free seats available on the train and the plane. The RE and WE statements emphasize whether others might choose to fly or not, potentially providing justifications for the decision. Even though the consumption scenarios are hypothetical, we plan to incentivize choices. Additionally, we aim to study mechanisms. Social norms are key predictors and determinants of individual behavior, especially in contexts involving moral trade-offs. Therefore, we elicit (prescriptive and descriptive) social norms to investigate if perceived norms differ with, without and between excuse

statements. This will also provide insights into the diverging results reported in the previous literature.

Both excuses frame externalities as inevitable and beyond individual control, but they differ in one crucial aspect. The RE suggests that others might act selfishly, potentially increasing the perceived prevalence of selfish behavior and shaping descriptive norms accordingly. This framing could shift descriptive norms by increasing the perceived share of selfish individuals in the population. In contrast, the WE assumes that others might act unselfishly, which could lead to resource waste. The WE frames selfish actions as preventing resource waste, which might alter prescriptive norms by making such actions seem socially more acceptable. Understanding the underlying reasons for behavior reminiscent of the excuses is highly policy relevant. Changes in norms could also influence moral costs—–an important factor in individual decision-making—–which we aim to measure. From a theoretical perspective, a utility-maximizing agent will act immorally when the perceived moral costs are lower than the monetary gains from the consumption decision. While Ziegler et al. (2024) measure moral costs, they assume these costs remain constant with and without the RE. Our study directly challenges this assumption by eliciting moral costs using multiple price lists (MPL) under different excuse treatments.

The pre-analysis plan proceeds with details on the sample, the experimental design, a simple conceptual framework, the hypotheses and concludes with the empirical strategy.

Survey and Sample

We plan to collect a sample of 2400 individuals representative for the German population in early 2025. Data collection has started on January 27, 2025 and we have thus far roughly 1000 observations. Survey participants will be recruited by the survey company Bilendi and paid for their participation a usual participation fee. All participants will be invited to a second survey wave two to three weeks after the initial wave. We collect the following information: First, participants will answer demographic and socio-economic questions including age, gender, education, income, employment status, having minor children (and if so how many), and the residential federal state.

Afterwards, the respondents will take part in an incentivized survey experiment that we describe in detail below. Incentives offered in this experiment are additional bonuses. Payment will be conditional on finishing the survey. Since we want to elicit revealed preferences, the experiment will involve donations to "atmosfair". "atmosfair" is a NGO that conducts and supports projects aimed at reducing CO2

in the atmosphere, e.g., by funding renewable energy projects in developing countries. This ensures that decisions in the experiment will have actual environmental consequences. To help participants better understand the magnitude of one kilogram of CO2 emissions, we ask them to estimate how far one would need to drive a car to produce this amount. Afterwards, we provide them with the accurate value of on average 6 kilometers.

Finally, survey respondents will answer a concluding questionnaire. Since the experiment will introduce a trade-off between personal monetary gains and CO2 in the atmosphere it includes questions about support and views on regulations targeting the reduction of CO2 emissions. Among them are CO2 labels, preferential placement of the least carbon intensive products at eye level in supermarkets, bans of carbon intensive products, setting a maximum value of carbon emissions for a product, and carbon pricing in general. For each policy, we elicit personal acceptance, perceived effectiveness, the belief about the acceptance in the population (share of respondents accepting a specific policy) and anticipated personal financial consequences. We also inquire general views towards governmental regulations in markets, consumption decisions and CO2 emissions. Additionally, the questionnaire elicits beliefs concerning whether climate change actually exists, whether it is caused by humans, perceived as a problem and how respondents rate the personal impact caused by climate change. We measure risk preferences, patience, and locus of control according to questions from the German Socio Economic Panel.

Note that we will conduct an unrelated additional survey experiment with the same sample.

Experimental Design

Each participant faces three hypothetical (but incentivized) decisions, each involving a choice between two products with different prices and environmental impact. In each scenario, the cheaper option generates higher CO2 emissions, while the more expensive option has a smaller environmental impact. The product prices and CO2 emissions reflect realistic values based on actual market values. To control for order effects, we randomize the sequence of the three consumption decisions for participants. These decisions cover the consumption categories transportation, food, and clothing:

1. Flight vs. train ride: Decision between a flight (costs of €85 and emissions of 180 kg CO2) and traveling by train (costs of €110 and 24 kg CO2) from Munich to Paris.

2. 1 kg of Apples from New Zealand (costs of €3.75 and emissions of 0.8 kg CO2) vs. 1 kg of Apples from Germany (costs of €4.50 and emissions of 0.3 kg CO2)
3. Cotton shirt (costs of €10 and emissions of 7 kg CO2) vs. Organic cotton (sustainable) shirt (costs of €13 and emissions of 3.8 kg CO2)

Throughout the remaining pre-analyis plan, we refer to the cheaper, more carbon-intensive option as the selfish choice, as it generates a negative externality (no donation to "atmosfair") in exchange for monetary gains. In the "flight vs. train ride" scenario, we inform participants that both modes of transport require the same amount of time from Munich to the destination in Paris. This accounts for check-in and check-out, and total time spent at the airport. For the apples and shirts decisions, participants are explicitly informed that the quality, feel, and other product characteristics are identical across options. This aims at eliciting product choices that are purely driven by differences in prices and emissions. In order to measure revealed preferences, we incentivize these product choices. Choosing the cheaper, more carbon-intensive option yields a monetary payoff based on the price differential (e.g., €25 for selecting the flight). Choosing the more expensive, less carbon-intensive option results in a CO2 reduction through a donation to "atmosfair", proportional to the difference in emissions (e.g., a 156 kg reduction in CO2 for selecting the train). We randomly select 20 % of participants for payment or the donation, respectively and implement randomly one of their three product choices.

In total, our experimental design comprises four groups: the control group ("Control"), where participants neither receive additional objective information about the decision context nor any excuse statement. In the information treatment ("Info"), we provide participants with objective information for each decision, giving them more context. In the replacement excuse ("RE") and waste excuse ("WE") treatments, participants receive both the objective information and excuse statements for each decision. The design triggers the respective excuses by offering specific interpretations of the objective information. Compared to "Control", the excuse statements in the "RE" and "WE" may influence behavior by either altering the interpretation of the information or changing the information itself. To disentangle these effects, we include "Ìnfo", as the objective information could be interpreted in a way that supports either the "RE" or the "WE". We randomize participants into the four groups with a planned sample size of 600 participants in each experimental group. The objective information in "Info", "RE" and "WE"

states (translated from German):

- **Flight vs. Train**: Both flight and train tickets are still available, and both the flight and the train journey will definitely take place. Other people are also facing this decision.
- **Apples from New Zealand vs. Germany**: The supermarket offers both apples from Germany and New Zealand , and other customers could also buy them. Unsold food, such as apples, is disposed of in the supermarket.
- **Standard vs. Organic Cotton T-Shirt**: Both the standard and organic cotton T-shirts are available in the store and could also be purchased by other customers. Unsold stock is generally removed from the inventory and disposed of.

The "RE" statements are:

- **Flight vs. Train**: The flight will take place anyway, and if you don't take the flight, someone else might take it instead.
- **Apples from New Zealand vs. Germany**: The apples from New Zealand are already in the supermarket, and if you don't buy them, someone else might.
- **Standard vs. Organic Cotton T-Shirt**: The standard T-shirt has already been produced, and if you don't buy it, someone else might.

The "WE" statements are:

- **Flight vs. Train**: The flight will take place anyway and is not yet fully booked, and if you don't take the flight, a seat might remain empty.
- **Apples from New Zealand vs. Germany**: The apples from New Zealand are already in the supermarket, and if you don't buy them, they might be thrown away.
- **Standard vs. Organic Cotton T-Shirt**: The standard T-shirt has already been produced, and if you don't buy it, it might be thrown away.

The statements emphasize key features that explain why excuses might influence behavior. They stress that the occurrence of an externality is beyond an individual's control, which diffuses personal responsibility. This ensures comparability between treatments "RE" and "WE". However, these excuses ignore the fact that, in the long term, externalities could be reduced by intentionally decreasing aggregate demand for harmful products. This illustrates that, although the excuse statements may appear

rational and valid, they can be easily debunked. Nevertheless, excuses may still provide moral wiggle room for individuals to justify selfish behavior (Dana et al., 2007). When using excuses, individuals typically cannot be certain whether someone else will purchase the product or not. The presence of such uncertainties creates additional moral wiggle room and further diffuses responsibility for the resulting externalities. Importantly, we hold the level of uncertainty constant across all excuse statements. In sum, our design ensures that any observed differences between "RE" and "WE" are solely driven by the distinct characteristics of the excuses themselves.

Afterwards, we elicit prescriptive norms using a method similar to Krupka and Weber (2013). Participants rate the social appropriateness of each of the six possible choices (three scenarios, each with two product options) on a four-point scale: 'Socially very inappropriate,' 'Socially somewhat inappropriate,' 'Socially somewhat appropriate,' and 'Socially very appropriate.' We clarify to participants: 'By socially appropriate, we mean behavior that most people agree is the "correct" or "preferable" thing to do.' To avoid order effects, we present the six choices in a randomized order. Beliefs about what others would choose in the three scenarios serve as a measure for descriptive norms: "How large do you belief is the share of people in the survey that would make the following choices?". Again we present the choices in a randomized order. Due to budget restrictions, we do not incentivize norm elicitations. However, note that our main outcomes -— the product choices -— are incentivized.

Finally, to measure moral costs, we employ a multiple price list (MPL) approach for each of the three scenarios. The MPL identifies switching points in participants' preferred choices between two products as relative prices change. Specifically, we hold the price of the more expensive, less CO2-intensive option constant while gradually reducing the price of the cheaper more CO2-intensive option. The switching point allows us to estimate the participants' moral costs of avoiding the more carbon-intensive product. For each of the three scenarios we start with a price difference of €0 and increase the price difference in seven steps. The flight is €10 up to €70 cheaper in €10 increments. The apples from New Zealand are €0.50 up to €3.50 cheaper in €0.50 increments. The cotton shirt is €1.30 to €9.10 cheaper in €1.30 increments. Participants see the three MPLs in randomized order to control for order effects. Due to budget constraints, these decisions are not incentivized. More details on the coding of the variables for moral costs and social norms can be found below in section F.1.1.

Conceptual Framework and Hypotheses

According to standard economic theory, an agent incurs no costs for making a morally contested choice (e.g., choosing an option with higher emissions) and exclusively values his own payoff while disregarding choices of third parties. However, empirical evidence suggests that individual decision-making is influenced by beliefs about others' actions, societal approval through social norms (Bicchieri and Xiao, 2009), and moral costs (Ziegler et al., 2024). The following utility function captures these considerations:

$$U_i(x) = F_i(x) - {C^m}_i(x) - {C^s}_i(x) \tag{F.1}$$

where $U_i(x)$ represents the utility that individual i derives from choosing the selfish option and receiving the payoff x. $F_i(x)$ reflects the utility from obtaining a higher payoff compared to the alternative associated with a lower externality. i also incurs costs from choosing the selfish option and receiving x: ${C^m}_i(x)$ reflects personal moral costs, which could be interpreted as the self-image. ${C^s}_i(x)$ reflects perceived social costs by transgressing social norms, which could be interpreted as social image. In our setting a higher x is associated with higher emissions which we assume to increase moral and social costs. i will only choose the selfish option if the benefits $F_i(x)$ outweigh the costs ${C^m}_i(x)$ and ${C^s}_i(x)$.

Now consider the effect of excuses on these costs. First, excuses may lower moral costs ${C^m}_i(x)$ by perceiving a given action as less immoral. Second, excuses may reduce ${C^s}_i(x)$ by influencing perceptions of social norms. The RE might shape descriptive norms by increasing the perceived share of selfish behavior in the population.[1] This is based on the idea that as more individuals choose selfish behavior, the descriptive norm shifts and becomes less binding. In contrast, the WE frames selfish actions as preventing resource waste because others are not acting selfishly. Therefore, the perceived social acceptance of selfish behavior might increase (prescriptive norm) while decreasing the perceived share of selfish behavior in the population. For the WE to be effective, its influence on prescriptive norms must dominate. In sum, the "RE" focuses on excusing the actor because others might step in, while the "WE" focuses on excusing the action by framing it as more socially acceptable. Note that the framework does not rule out the possibility that a change in social costs affects moral costs and vice versa. In our empirical strategy, we address this interdependence by correlating these measures.

[1] An alternative interpretation of this behavior is strategic belief distortion (Di Tella et al., 2015, e. g.)

Based on this simple framework and the experimental design, we aim to answer three core research questions:

1. Does the availability of excuses affect selfish behavior in the three product choices?
2. Does the availability of excuses affect moral costs in the three product choices?
3. Does the availability of excuses affect norms (prescriptive and descriptive) in the three product choices?

The first research question focuses on the impact of excuses on consumptions choices, while the second and third research questions explore the mechanisms driving the effectiveness of these excuses. Our first hypothesis states that:

- H1: The share of individuals choosing the selfish option is higher in treatments "RE" and "WE" compared to "Control" and "Info".

We posit that excuses reduce the moral and social costs associated with choosing the selfish option, thereby increasing the likelihood of selfish behavior in the excuse treatments compared to "Control" and "Info". Ex-ante, we do not anticipate any significant differences between the excuse treatments "RE" and "WE". The relative effectiveness of the excuses hinges on three factors: the dominant norm driving behavior, whether excuses change perceived norms, and how an excuse reduces moral costs. We expect the RE to increase selfish behavior more than the "WE" since it more directly targets beliefs, which has been shown to be a more important driver of behavior than prescriptive norms (look for literature). H2 through H4 investigate these questions. Differences between "Control" and "Info" should be minor. However, if there are any differences, the share of selfish consumption choices should be higher in "Info". Providing additional informational context may prompt participants to interpret the situation in terms of the "RE" and "WE" narratives. The key distinction between "Info" and the excuse treatments is that the latter actively supplies participants with statements serving as excuses.

The next set of hypothesis explores the proposed mechanisms behind the excuse statements

- H2: The belief about the proportion of individuals choosing the selfish option in "RE" is higher compared to "Control" and "Info", while it is the lowest in "WE".
- H3: The selfish option is perceived as socially more appropriate in "WE" compared to "Control", "Info", and "RE".

- H4: Morals costs are lower in “RE” and “WE” compared to “Control” and “ Info”.

Based on our framework H2 and H3 follow from the narratives of the excuse statements, which emphasize the behaviors of others and the framing of moral obligations. The RE suggests that someone else might consume instead of oneself, leading us to hypothesize that the belief about the proportion of individuals buying the selfish product is higher in “RE” compared to the other treatments. The WE emphasizes that others might not choose the selfish option, framing buying the product as a moral obligation. Consequently, we hypothesize that the belief in selfish behavior is lowest in “WE” (H2), while social appropriateness is highest (H3). Since excuses could lead to perceiving a selfish action less immoral, we expect moral costs to reduce in “WE” and “RE” compared to “Control” and “Info” (H4). Moral costs could be lowest in the “WE” since it actively tries to excuse the action. We do not expect significant differences between “Control” and “Info” with respect to H2, H3, and H4 since the additional information is objectively framed. Despite that, it is still important to include “Info” to be able to disentangle the excuse effects from potential information effects.

Empirical Approach

Average Treatment Effects

Our main outcomes are three product choices. These will be coded as follows: a dummy equal to 1 if a participant chooses the flight, a dummy equal to 1 if a participant chooses the apples from New Zealand, and a dummy equal to 1 if a participant chooses the cotton shirt (i.e., always the cheaper options with greater carbon footprint). Regarding prescriptive norms, we will assign the following values to each of the social appropriateness statements: -2 “Socially very inappropriate”, -1 “Socially somewhat inappropriate”, 1 “Socially somewhat appropriate”, and 2 “Socially very appropriate.” Additionally, we will code dummies equal to one if the rating is at least “Socially somewhat appropriate”, zero otherwise; dummies equal to one if the rating is “Socially very appropriate”, zero otherwise; dummies equal to one if the rating is at least “Socially somewhat inappropriate”, zero otherwise; and dummies equal to one if the rating is “Socially very inappropriate”, zero otherwise. Descriptive norms -— beliefs about what others would choose -— will range from 0 to 100 (percent).

To measure moral costs, we will define a switching point as the price difference at which participants switch from the more environmentally friendly option to the less environmentally friendly but cheaper

option (e.g., from train to flight). If a participant has multiple switching points, we will use the first instance where they switch. While it would be inconsistent to switch back (e.g., from flight to train), we will code such instances accordingly and control for them in the regressions.

First, we will compare raw mean differences between all treatment groups for each outcome of interest and perform t-tests as well as Mann-Whitney U tests. Afterwards, we will estimate the following equation using OLS:

$$Y_i = \alpha + \beta_1 RE_i + \beta_2 WE_i + \beta_3 Info_i + X_i'\gamma + \epsilon_i \tag{F.2}$$

where, i indexes an individual, and the "Control" group serves as the reference category. Y_i represents the respective outcome of interest, i.e., the product choices, prescriptive and descriptive norms, and moral costs. β_1, β_2, and β_3 measure the average treatment effects of the "RE", "WE", and "Info" treatments, respectively, compared to the "Control" group. Standard errors (ϵ_i) are clustered at the individual level. X_i' is a vector of controls including age, gender, personal monthly net income, educational background, employment status, having minor children, how many minor children live in the household, voting intentions, and views about climate change. To directly compare the two excuse statements we plan to test for differences in the respective coefficients. We will report results with and without controls, as well as an additional specification including measures for risk-taking, patience, and locus of control. To account for multiple hypothesis testing, we will apply the Romano-Wolf method.

As an exploratory analysis, to study the relationship between norms, moral costs, and product choices, we will correlate these measures. Additionally, we will include social norms and moral costs to equation (F.2) as controls with the product choices as outcomes. This can provide us with suggestive evidence on the relative importance of norms and moral costs for consumption choices in our experiment.

Heterogeneous Treatment Effects

We will test the following moderators: Gender, age, voting intentions, educational background, income, views on whether climate change is menacing, and whether climate change is caused by humans. Additionally, we plan to estimate a random forest to discover other potential heterogeneities as an exploratory analysis.

F.1.2 Follow-Up Experiment

Introduction

This pre-analysis plan extends a previously registered RCT, “Excuse My Behavior: Social Norms and Moral Costs” (RCT ID AEARCTR-0015264). The original experiment, conducted in the first wave of a two-wave panel survey, examined behavioral changes when individuals are confronted with the replacement excuse (RE) or the waste excuse (WE). The RE follows the logic: "If I don’t do it, someone else will." The WE, by contrast, states: "If I don’t do it and no one else does, resources might be wasted." For more details, please refer to the pre-analysis plan of AEA RCT ID AEARCTR-0015264.

The first-wave experiment applied the WE and RE to three different real-world consumer product categories: frequent or daily purchases (e.g., food), durable goods (e.g., clothing), and transportation (e.g., flights and train rides). In a survey experiment with one control group and three treatments, we randomly assign “replacement statements” and “waste statements” designed to trigger the WE and RE to a subset of the sample. Additionally, participants are assigned to a control group or to another treatment group providing objective information but no excuses. Since these excuses provide interpretations of objective information, our experimental design includes one treatment that presents only the objective information, but no excuse. The additional RE and WE treatments incorporate this same information and add the associated excuse. The control group receives neither objective information nor excuse statements. Objective information, for example, indicates that there are still free seats available on the train and the plane. The RE and WE statements emphasize whether others might choose to fly or not, potentially providing justifications for the decision. Even though the consumption scenarios are hypothetical, we incentivized choices. The product choices are between one product which is cheaper but emits more CO_2 (i.e., selfish option), while the other is more expensive but emits less CO_2. Incentives imply that consumers can (with some probability) either cash-in the price differential or reduce CO_2-emissions through a donation to *atmosfair*, an organization investing in CO_2-reduction.

We also aim to study mechanisms. Social norms are key predictors and determinants of individual behavior, especially in contexts involving moral trade-offs. Therefore, we elicit (prescriptive and descriptive) social norms to investigate if perceived norms differ with, without and between excuse statements.

The results from the first wave reveal that, regardless of treatment, individuals significantly overestimate the share of people who would choose to emit CO_2 rather than forgo a monetary payoff which we refer to as the selfish action. However, beliefs about others' behavior varied systematically by treatment. The second-wave experiment exploits these findings by correcting these misperceptions. We randomly assign participants to either receive accurate information about the true share of selfish choices (i.e., correcting beliefs downwards) or to a control condition without correction. Following this, we again elicit their hypothetical consumption decisions in the three product categories. Additionally, we investigate their willingness to engage in more climate-friendly behavior and measure whether they sign a political petition advocating for stronger climate action (targeted at the German government). This design allows us to test whether correcting misperceived descriptive norms increases individuals' willingness to engage in climate-friendly behavior.

The pre-analysis plan proceeds with details on the sample, the experimental design, the hypotheses and concludes with the empirical strategy.

Survey and Sample

This experiment will be conducted in the second survey wave of a panel study. In the first wave, we collected a sample of 2414 individuals representative for the German population. Data collection started on January 27, 2025 and concluded on February 7, 2025. Survey participants are recruited by the survey company Bilendi and paid for their participation a usual participation fee. All participants will be invited to a second survey wave on or after February 12, 2025. Samples from Bilendi typically have an attrition rate between 20 and 30%. Hence, we expect roughly 1900 individuals to participate in both survey waves.

We will also run another unrelated survey experiment in this wave. For more information see the pre-registration and pre-analysis plan for AEA RCT ID AEARCTR-0015264.

Experimental Design

As in the first survey wave, each participant faces three hypothetical decisions, where each involves a choice between two products with different prices and environmental impact. In each scenario, the cheaper option generates higher CO_2 emissions, while the more expensive option has a smaller environmental impact. The product prices and CO_2 emissions reflect realistic values based on actual market

values. To control for order effects, we randomize the sequence of the three consumption decisions for participants. These decisions cover the consumption categories transportation, food, and clothing:

1. Flight vs. train ride: Decision between a flight (costs of €85 and emissions of 180 kg CO_2) and traveling by train (costs of €110 and 24 kg CO_2) from Munich to Paris.
2. 1 kg of Apples from New Zealand (costs of €3.75 and emissions of 0.8 kg CO_2) vs. 1 kg of Apples from Germany (costs of €4.50 and emissions of 0.3 kg CO_2)
3. Cotton shirt (costs of €10 and emissions of 7 kg CO_2) vs. Organic cotton (sustainable) shirt (costs of €13 and emissions of 3.8 kg CO_2)

In the first survey wave, participants were assigned to four conditions: a control group, an information treatment, a replacement excuse (RE) treatment, and a waste excuse (WE) treatment. The results indicated that participants systematically overestimated the share of people choosing the high-emission option.

When participants indicate their preferred options in the three consumption scenarios (this time without incentives) we want to test the effects of a norm correction. Therefore, individuals within each of the original treatment groups will be randomly assigned to one of two new conditions. In the "Correction" condition, participants will receive accurate information about the actual share of product choices observed in the first wave. In the "Control" condition, participants will receive no such information. Participants will not receive additional treatment information from the first wave including excuse statements, and everyone will see the original control version of the consumption choices. This allows us to examine the stability of the original treatment effects over time.

Following the norm correction and consumption decisions, participants will be asked whether they want to sign a petition advocating for stronger climate action. They will be informed that the German government will be notified about the share of survey participants supporting the petition. Additionally, we will elicit self-reported intentions to adopt more climate-friendly behaviors, including reducing flights, car rides, home heating, and meat consumption. Finally, since the survey will be conducted shortly before the German federal elections, we will ask participants to rate the importance of various policy topics in their voting decisions, including environmental issues, the economy, public security/peace, migration, pensions, and social security. Finally, we elicit voting intentions.

Hypotheses

We aim to answer three core research questions:

1. Does the norm correction affect behavior in the three product choices?
2. Does the norm correction influence willingness to fight climate change, as measured by signing a petition and adopting pro-environmental behaviors?
3. Does the norm correction alter the perceived importance of climate policy in voting decisions?

Our first hypothesis is:

- **H1:** The share of individuals choosing the high-emission option will be lower in the *Correction* condition compared to the *Control* condition.

Since individuals typically overestimate the actual share of selfish choices, providing accurate information through norm correction may reduce the likelihood of choosing the high-emission option. However, we account for heterogeneity in prior beliefs, as some individuals may overestimate or underestimate actual choices.

- **H2:** The share of individuals choosing to sign the petition and adopt pro-environmental behaviors will be higher in the *Correction* condition compared to the *Control* condition.
- **H3:** The share of individuals indicating that climate policy is an important determinant of their voting decision will be higher in the *Correction* condition compared to the *Control* condition.

Being informed that others behave more environmentally friendly than expected may shift perceptions of descriptive norms, leading participants to align their behavior accordingly by signing the petition, adopting environmentally-friendly behavior, and assigning greater importance to climate policy in their voting decisions. However, we acknowledge the possibility of a backfire effect: if participants perceive that others are already acting pro-environmentally, they may feel lower personal responsibility to contribute to the common good. Testing these opposing forces empirically is particularly insightful.

As with H1, we allow for heterogeneity in prior beliefs, as individuals may have initially overestimated or underestimated the actual share of pro-environmental behavior.

Empirical Approach

Our main outcomes are the three product choices, changes in pro-environmental behavior, signing a petition, the perceived importance of climate policy for the voting decision, and changes in voting intentions.

First, we will compare raw mean differences between the two treatment groups for each outcome of interest and perform t-tests as well as Mann-Whitney U tests. Afterwards, we will estimate the following equation using OLS:

$$Y_i = \alpha + \beta \text{Correction}_i + Z_i'\delta + X_i'\gamma + \epsilon_i \tag{F.3}$$

where i indexes an individual, and the "Control" group serves as the reference category. Y_i represents the respective outcome of interest, i.e., the three product choices, changes in pro-environmental behavior, signing a petition, and the perceived importance of climate policy for the voting decision. β measures the average treatment effect of "Correction" compared to the "Control" group. Standard errors (ϵ_i) are clustered at the individual level. Z_i' is a vector of controls accounting for treatment status in the first survey wave. This aims at testing the medium-term treatment effect stability of our experiment in survey wave 1. X_i' is a vector of controls including age, gender, personal monthly net income, educational background, employment status, number of minor children in the household, voting intentions, and views about climate change. To account for multiple hypothesis testing, we will apply the Romano-Wolf method.

As a final test, we will run another regression similar to equation (F.3), but fully interacting the correction effect with the treatment status from wave 1 to account for our treatment structure. Additionally, we will create dummy variables indicating whether individuals overestimate or underestimate the actual share of participants choosing the selfish option. These dummies will be fully interacted with the treatment indicator "Correction".

F.2 Pre-Analysis Plan Chapter 2

Introduction

Policymakers and the majority of the public agree that climate change poses a major threat to society and must be addressed: in the Paris Agreement, 194 countries committed to reducing carbon emissions, yet according to a global survey by Ipsos (2023), many citizens believe that governments are not doing enough to combat it. Paradoxically, carbon pricing—an effective tool to reduce emissions and mitigate climate change (Nordhaus, 2019)—often faces public resistance (Dechezleprêtre et al., 2025). As a result, policymakers either refrain from implementing carbon pricing, set prices too low, or exempt specific sectors and products. Thus, it is important to identify the roots of public resistance and explore determinants of support for carbon pricing. Recent studies have shown that revenue use (Woerner et al., 2024) and explaining the mechanics of carbon pricing (Dechezleprêtre et al., 2025) can influence public support.

We aim to investigate another potential determinant by connecting two previously unlinked strands of the literature: the role of pivotality in consumption in the presence of CO_2 externalities and support for carbon pricing. Pivotality in consumption refers to the extent to which an individual's decision affects the overall outcome, and it is closely related to the concept of responsibility diffusion (Bartling and Fischbacher, 2012). This captures the notion that individuals are (or at least feel) less responsible for an outcome when others are involved. This is particularly relevant in the context of CO_2 externalities, as consumers may feel less pivotal when they realize that others might purchase the product if they don't, thus shifting their sense of responsibility for the environmental outcome.[2] This important feature—pivotality through the presence of other consumers—has been neglected when exploring the determinants of support for carbon pricing. Previous studies have either examined how market features such as information provision or communication influence support for carbon pricing (Bartling et al., 2015a, 2024; Pace et al., 2025), or they focus on purchasing behavior under varying degrees of pivotality, such as in multi- versus single-unit markets with multiple consumers Ziegler et al. (2024). However, the potential role of pivotality in consumption in the context of CO_2 externalities and support for carbon pricing remains largely unexplored. This study aims to explore how pivotality in aggregate consumption

[2] In this setting, we assume that individuals are price takers and the market interaction is only one-shot. Therefore, we abstract from the fact that reduced demand can also lead to lower prices in the long run.

affects the support for regulating markets with externalities.[3]

The literature on pivotality in decision-making has shown that individuals tend to behave more selfishly or immorally when their pivotality for outcomes is reduced (Falk et al., 2020, see e.g.). In these contexts, actions are deemed selfish or immoral when individuals impose externalities by canceling donation in exchange for personal monetary gain. Two more closely related studies on pivotality in consumption examine the so-called replacement logic (RL), where individuals are aware that if they do not choose a selfish option, another person might step in to take their place. However, the evidence on the influence of RL is mixed. Bartling and Özdemir (2023) experimentally demonstrate that RL has limited effects on moral behavior, as counteracting social norms often mitigate its impact. In contrast, Ziegler et al. (2024) find that the RL leads to a complete erosion of moral behavior in experimental markets. While these studies primarily focus on individual purchasing behavior under varying degrees of pivotality, they do not explore how the RL might influence support for regulating markets with externalities.

On the other hand, the literature on support for carbon pricing has largely overlooked the relevance of RL. Instead, it primarily focuses on factors such as revenue recycling schemes (Woerner et al., 2024) or the effects of information provision and nudges on public attitudes towards carbon pricing (Maestre-Andrés et al., 2021; Imai et al., 2022; Dechezleprêtre et al., 2025). The most closely related study here is Woerner et al. (2024), which examines how different revenue use schemes (e.g., redistribution, or guaranteed payments) influence support for carbon pricing in an experimental market. Their findings suggest that revenue recycling schemes significantly increase public support for carbon pricing compared to allocating funds to the state budget, with upfront guaranteed payments receiving the highest approval. However, their study is limited to a single consumer who can purchase up to two products that yield monetary payoffs but also impose externalities. They do not investigate how the presence of another individual, who could potentially purchase the products and impose the externality instead, influences support for carbon pricing.

Therefore, we plan to conduct an incentivized survey experiment with two experimental groups, which will be part of the second wave of a two-wave panel representative of the German population. We

[3]Generally, in studies on market behavior and externalities, markets are frameworks where producers and consumers negotiate prices, often using posted-offer markets. However, in our study, individuals are price-takers, and we refer to "markets" in the context of purchasing a product rather than negotiating prices.

base our experimental design on a combination of the two designs by Woerner et al. (2024) and Ziegler et al. (2024). In the "Control" group, participants individually decide whether to purchase up to two products, each yielding a payoff but causing CO_2 emissions. In the "RL" treatment, two participants share a market, and their purchasing choices are interdependent, enabling engagement in the RL. Participants will make decisions both with and without a CO_2 price and vote in groups on whether the price should be implemented. We will further explore the impact of three revenue recycling schemes—state budget, redistribution, and funding climate projects—on support for carbon pricing. We hypothesize that the "RL" will increase support for carbon pricing, as individuals may perceive reduced personal control over emissions and see regulation as a way to mitigate environmental harm. Additionally, we try to verify findings from prior RL studies by examining its impact on purchasing behavior and whether it shifts social norms—both descriptive (beliefs about how others behave) and prescriptive (beliefs about what is socially appropriate). Finally, in a within-subject design, we will also investigate the relevance of revenue recycling schemes such as redistribution or funding climate projects.

The pre-analysis plan proceeds with details on the sample, the experimental design, the hypotheses and concludes with the empirical strategy.

Survey and Sample

This experiment will be run in the second survey wave of a panel study. In the first wave, we collected a sample of 2414 individuals representative for the German population. Data collection started on January 27, 2025 and concluded on February 7, 2025. Survey participants are recruited by the survey company Bilendi and paid for their participation a usual participation fee. All participants will be invited to a second survey wave on February 12, 2025 . Samples from Bilendi typically have an attrition rate between 20 and 30%. Hence, we expect roughly 1900 individuals to participate in both survey waves.

We will use baseline data from the first wave for respondents' attitudes towards climate policies, demographics, and socio-economic characteristics. Specifically, we collect information on demographic and socio-economic questions including age, gender, education, income, employment status, having minor children (and if so how many), and the residential federal state. After two other unrelated incentivized survey experiments, respondents answer a concluding questionnaire including questions about support and views on regulations targeting the reduction of CO_2 emissions besides CO_2 pricing. Among

them are CO_2 labels, preferential placement of the least carbon intensive products at eye level in supermarkets, bans of carbon intensive products, setting a maximum value of carbon emissions for a product, and carbon pricing in general. For each policy, we elicit personal acceptance, perceived effectiveness, the belief about the acceptance in the population (share of respondents accepting a specific policy) and anticipated personal financial consequences. We also inquire general views towards governmental regulations in markets, consumption decisions and CO_2 emissions. Additionally, the questionnaire elicits beliefs concerning whether climate change actually exists, whether it is caused by humans, perceived as a problem and how respondents rate the personal impact caused by climate change. We measure risk preferences, patience, and locus of control according to questions from the German Socio Economic Panel.

In this experiment, incentives are presented as an additional bonus on top of the participation fee offered by Bilendi while the payment will be conditional on finishing the survey. Since we want to elicit revealed preferences, the experiment will involve donations to "atmosfair". "atmosfair" is an NGO that conducts and supports projects aimed at reducing CO_2 in the atmosphere, e.g., by funding renewable energy projects in developing countries. This ensures that decisions in the experiment will have actual environmental consequences. To help participants better understand the magnitude/impact of one kilogram of CO_2 emissions, we already provided them with information on the equivalent distance of a car ride in the first survey wave.

Experimental Design

Our experimental design combines elements from the studies by Woerner et al. (2024) and Ziegler et al. (2024) and includes both within- and between-subject components (also see Figure 2.1 for an overview). In the within-component, participants act as consumers in a market and decide whether to buy zero, one, or two identical virtual products. Each product costs €6 and has a value of €8, providing a monetary payoff of €2 per product. However, purchasing a product results in 25 kg of CO_2 emissions. To introduce environmental consequences, participants are informed that by default, we will donate for each respondent the equivalent monetary amount which offsets 50 kg of CO_2 to "atmosfair". For each product purchase, the donation is reduced by 25 kg, and if both products are bought, we make no donation for that market. Participants make two purchasing decisions: first without a CO_2 price, and then

with a CO_2 price. The CO_2 price increases the cost of each product to €7, reducing the monetary payoff to €1 per product. This corresponds to a CO_2 price of €40/tCO_2.

After participants make their purchasing decisions, they are randomly assigned to groups of 25 consumers and vote democratically on which purchasing decision -— with or without the CO_2 price -— will be implemented. A CO_2 price is adopted if at least 13 participants support it. The voting process occurs under three different revenue recycling schemes (within-design): allocating the revenue to the state budget, redistributing it evenly among consumers, or donating it to environmental projects such as "atmosfair". Importantly, we make it clear to participants that, regardless of the chosen scheme, the revenues will be used for these exact purposes: donated to the German federal government, redistributed among the group of consumers, or donated to environmental initiatives in Germany. This ensures that their voting decisions have actual consequences. We present the revenue schemes in a randomized order. If a consumer purchases a product under the CO_2 price condition, an additional €1 in revenue is collected and allocated to a common fund, which would then be distributed according to the designated revenue recycling scheme. At the end of the experiment, one of the three votes is randomly selected for implementation, and participants are paid according to the resulting outcomes. In contrast to Woerner et al. (2024), we do not use a random dictator scheme but allow for democratic votes, as this more closely reflects actual voting behavior. However, this comes at the cost of potentially introducing strategic voting, which a random dictator scheme would rule out. Democratic voting will take place in both between designs.

The between-subject design is structured as follows: In the "Control" condition, a market consists of a single consumer who independently decides how many products to buy. In the "RL" treatment, the market consists of two participants but still only two products, meaning that purchases are now interdependent. Each participant can buy up to two products, but total consumption is constrained by the shared market. While the consequences of purchasing remain the same as in the control group, participants' decisions now directly affect each other. Importantly, the only difference between "Control" and "RL" is the number of consumers in the market, while market size (i.e., the maximum number of available products) remains constant. To capture strategic behavior, participants are randomly assigned to one of two roles: first mover or second mover. Before learning their role, they submit purchasing decisions for both roles; hence, we ensure to elicit their full strategy set. The first mover's decision

determines the second mover's options: if the first mover buys one product, the second mover can purchase at most one; if the first mover buys two, the second mover cannot buy any; and if the first mover buys none, the second mover can buy up to two. This interdependence allows participants to engage in replacement logic. All other steps, including the voting procedure, remain identical across both experimental groups. Participants are informed that their role will be randomly assigned and that payments will be made a few days after data collection concludes.

After collecting voting decisions we ask participants, for each revenue scheme, whether they would have accepted the CO_2 price at different levels, ranging from €0.50 to €2. Given the literature on the RL (e.g., Ziegler et al., 2024), both descriptive and prescriptive norms play a crucial role in decision-making. To capture descriptive norms, we elicit participants' beliefs about how many of the 25 consumers purchased no product, one product, or two products, both with and without the CO_2 price. To ensure comparability, this question is conditioned on being the first mover in the "RL" condition. In the "RL" condition, we will additionally ask how many products they believe the other consumer in their market would buy if he was the first mover and the total number of products bought in the group of 25 consumers. To measure prescriptive norms, we follow the approach of Krupka and Weber (2013). Participants rate the social appropriateness of the three possible purchasing decisions (buying zero, one, or two products) both with and without the CO_2 price. Ratings are given on a four-point scale: 'Socially very inappropriate,' 'Socially somewhat inappropriate,' 'Socially somewhat appropriate,' and 'Socially very appropriate.' We explicitly define social appropriateness as behavior that most people agree is the "correct" or "preferable" course of action. Participants rank the social appropriateness of each action twice: once with and once without the CO_2 price. To avoid order effects, we randomize the presentation of purchase decisions. Finally, we elicit participants' beliefs about the proportion of votes in favor of the CO_2 price under each revenue scheme, presented in a randomized order. We will also ask participants to rank the three revenue schemes according to their preferences.

Figure excluded because similar to Figure C.1

Research Questions and Hypotheses

We aim to answer three core research questions:

1. Does the "RL" condition affect support for a CO_2 price?

2. Does the "RL" condition influence consumption choices and beliefs about consumption?
3. Does the "RL" condition shape social norms (descriptive and prescriptive) regarding product purchases?

The first research question examines how the RL condition impacts voting decisions on a CO_2 price. The second and third research questions explore the mechanisms driving these decisions by investigating shifts in consumption behavior, belief formation, and social norms.

Between-Subject Design Hypotheses (Main Results)

- **H1:** A higher share of individuals in the "RL" condition votes in favor of the CO_2 price compared to the "Control" condition.
- **H2:** A higher share of individuals in the "RL" condition will accept higher CO_2 prices compared to the control condition.

H1 aims to answer our main research question. In the "RL" markets, individual consumption choices are less pivotal in determining the overall consumption outcome, which may lead participants to regard the CO_2 price as more desirable. This reasoning is based on the findings of Ziegler et al. (2024). There occurs more trading in "RL" markets and individual beliefs about trading frequency are also higher. Consequently, since individual purchases have a weaker direct impact on aggregate consumption in the "RL" condition, participants may feel a greater need for collective regulation through a CO_2 price. Building on this reasoning, H2 investigates whether the "RL" condition not only increases support for carbon pricing (as in H1), but also increases individuals' willingness to accept higher CO_2 prices. This hypothesis reflects the idea that, in the "RL" condition, participants may feel a greater sense of collective responsibility and be more willing to accept higher prices as a means of addressing the environmental externality, compared to individuals in the "Control" condition.

Importantly, we consider the possibility that the "RL" condition could have the opposite effect and reduce support for carbon pricing. If individuals believe that others will purchase the product regardless of their own choices, they may view a carbon price as ineffective, leading to lower support for the policy. We will test this by analyzing participants' beliefs about others' purchasing behaviors and how these beliefs relate to support for the CO_2 price. Specifically, if the belief that more people purchase the product

is positively correlated with support for the CO_2 price, this would align with H1. In contrast, a negative correlation would challenge H1. Additionally, we can infer and analyse perceived effectiveness by comparing beliefs about purchasing behavior with and without the CO_2 price. Furthermore, a higher social appropriateness rating of purchasing the product could also reduce support for the CO_2 price. However, ex-ante, in line with H1 we expect that reduced pivotality and the belief that more trading occurs in RL markets will lead to increased support for the CO_2 price in "RL" compared to the "Control". Hypotheses H3 to H5 will discuss these dynamics more thoroughly.

The next set of hypotheses explores the impact of RL on consumption behavior, beliefs, and social norms:

- **H3:** More products will be purchased in "RL" markets compared to the "Control" markets.
- **H4:** Participants in "RL" have higher beliefs about the proportion of consumers purchasing one product and the proportion buying two products, compared to "Control".
- **H5:** Purchasing products will be perceived as more socially appropriate in RL compared to the Control condition.

These hypotheses build on the idea that reduced pivotality lowers personal responsibility, which may encourage more selfish consumption decisions (H3). Additionally, participants in "RL" may engage in belief distortion to justify their own choices or may assume that others will step in to buy products, leading to higher expectations of overall consumption (H4). Lastly, purchasing behavior may be viewed as more acceptable due to responsibility diffusion in consumption choices (H5).

Within-Subject Hypotheses (Secondary Hypotheses)

The within-subject design allows us to analyze how the introduction of a CO_2 price affects purchasing behavior and revenue recycling preferences. These hypotheses focus on the replication of previous results on support for carbon pricing and revenue schemes.

- **H6:** More products will be purchased in the no-CO_2 price condition compared to the CO_2 price condition.

Increasing the cost of the product in our setting should reduce the demand.

- **H7:** Revenue recycling schemes that allocate funds to citizens or climate projects will receive

more support than the state budget allocation.

Previous literature suggests that earmarking revenues for specific causes (such as redistribution or environmental projects) increases public acceptance of CO_2 pricing (e.g. Woerner et al., 2024).

Empirical Strategy

Average Treatment Effects: Between-Subject Design

We begin by examining hypotheses H1 through H5. Our primary outcomes include the three votes on the CO_2 price under different revenue recycling schemes: state budget, redistribution, and funding of environmental projects. For each vote, we define a binary variable that takes the value of 1 if the participant supports implementing the CO_2 price. To measure participants' willingness to pay for a carbon price, we introduce a variable indicating the highest price that an individual is willing to accept for the CO_2 price for each revenue scheme. We also examine purchasing behavior by creating binary indicators for whether participants purchase at least one product and whether they purchase two products. Additionally, we define a continuous variable representing the total number of products purchased, both in the absence and presence of the CO_2 price. In the "RL" treatment, we specifically focus on the purchasing decisions of first movers within their respective markets.

Our secondary set of outcomes is comprised of norms and beliefs about other's voting behavior. Regarding prescriptive norms, we will assign the following values to each of the social appropriateness statements: -2 "Socially very inappropriate", -1 "Socially somewhat inappropriate", 1 "Socially somewhat appropriate", and 2 "Socially very appropriate." Additionally, we will code dummies equal to one if the rating is at least "Socially somewhat appropriate", zero otherwise; dummies equal to one if the rating is "Socially very appropriate", zero otherwise; dummies equal to one if the rating is at least "Socially somewhat inappropriate", zero otherwise; and dummies equal to one if the rating is "Socially very inappropriate", zero otherwise. For descriptive norms -— beliefs about overall market behavior -— we construct two continuous variables, ranging from 0 to 24, representing participants' beliefs about the number of individuals purchasing one product and the number purchasing two products.

First, we will compare raw mean differences between the "Control" group and the "RL" treatment across all outcomes of interest, conducting both t-tests and Mann-Whitney U tests. Next, we will estimate the following equation using OLS:

$$Y_i = \alpha + \beta_1 RL_i + X_i'\gamma + \epsilon_i \tag{F.4}$$

where i indexes individuals, and the "Control" group serves as the reference category. Y_i represents the respective outcome of interest, including voting behavior, product purchases, and prescriptive and descriptive norms. The coefficient β_1 captures the average treatment effect of the "RL" treatment relative to the "Control" group. Standard errors (ϵ_i) are clustered at the individual level. The vector X_i' includes control variables such as age, gender, personal monthly net income, educational background, employment status, number of minor children in the household, voting intentions, and views on climate change. We will present results both with and without these controls, as well as an additional specification incorporating measures for risk-taking, patience, and locus of control. To account for multiple hypothesis testing, we will apply the Romano-Wolf correction.

As an exploratory analysis, to study the relationship between norms, voting behavior, product choices, and beliefs about others' voting behavior, we will correlate these measures. Additionally, we will include social norms (prescriptive and descriptive) and beliefs about others' voting behavior in equation (F.4) as controls, using individual voting behavior for the three revenue recycling schemes as outcomes. This approach provides suggestive evidence on the relative importance of norms and beliefs about others' voting behavior in shaping personal policy preferences. Furthermore, according to H1, we expect the "RL" condition to increase support for the CO_2 price. However, we acknowledge that the effect could also go in the opposite direction. To explore this possibility, we will investigate whether differences in beliefs about product purchases with and without a CO_2 price influence support for the policy. Additionally, we will examine correlations between support for the CO_2 price and general attitudes toward carbon regulations from the first survey wave.

Average (Treatment) Effects: Within-Subject Design

To test H6, we will reshape the data set and analyze purchasing behavior in a within-subject design. Each individual will contribute two observations: purchasing choices with and without the CO_2 price. We estimate the following equation using OLS:

$$Y_j = \eta_j + \delta CO_{2j} + X_j'\theta + \sigma_j \tag{F.5}$$

where Y_j represents the respective outcome for observation j: purchasing one product, purchasing two products, and the total number of products bought. The coefficient δ captures the effect of introducing a CO_2 price on purchasing behavior, with the decision made in the absence of a CO_2 price serving as the baseline condition. Due to the experimental design, individuals first make purchasing decisions without the CO_2 price and then with it, meaning the effect is not causal since the within-subject variation is not randomized. The vector X'_j includes the same controls as in equation (F.4), with the addition of an indicator for treatment assignment to the "RL" condition to account for structural differences in market settings. As before, we will report results with and without controls, as well as an additional specification including measures for risk-taking, patience, and locus of control.

To test H7, the data set will contain three observations per individual: voting in favor of the CO_2 price, targeted to either the state budget, donations to an environmental project, or redistribution.

$$Y_k = \kappa_k + \tau_1 redist_k + \tau_2 donate_k + X'_k\Lambda + \rho_k \tag{F.6}$$

where Y_k is a dummy variable equal to one if the individual voted in favor of the CO_2 price for observation k. The coefficient τ_1 captures the average effect of allocating generated revenues to redistribution instead of the state budget, while τ_2 measures the average effect of donating revenues to an environmental project in Germany instead of the state budget. The vector X'_k is defined as above, and all subsequent analytical steps follow the same procedure as in previous specifications.

For equations (F.5) and (F.6), we will cluster standard errors at the observation (i.e., intervention) level.

Heterogeneous Treatment Effects

We will test the following moderators regarding our main outcome voting behavior in the three different revenue schemes: Gender, age, voting intentions, educational background, income, views on whether climate change is menacing, whether climate change is caused by humans, and attitudes towards climate policies. Additionally, we plan to estimate a random forest to discover other potential heterogeneities as an exploratory analysis.

F.3 Pre-Analysis Plan Chapter 3

Introduction

From an individual perspective, government policies often regulate both personal and others' behaviors raising two key questions: How willing are individuals to impose own preferences on others and oneself, and if so how do they balance self-regarding and other-regarding concerns? This implicitly touches on the relevance of the regulation target -— whether it affects oneself, others, or both. Behavioral economics has demonstrated that, contrary to the standard neoclassical framework, individuals are not only selfish but also care about others' outcomes (Fehr and Charness, 2025, for a review). However, little is known about how that translates into the willingness to regulate others, and about individual policy support.

For example, steering taxes are a common policy tool for regulating collective behavior, yet they often face public resistance. While such opposition can arise from factors like mistrust in government or concerns about efficiency and revenue use, we focus on concerns for those subject to the regulation, i.e., oneself and others. Some individuals may, for instance, be willing to contribute to a public good through taxes but reject imposing this tax on others. Alternatively, some individuals may resist to pay the tax personally but are in favor of others' paying it. Broad public support is critical for a successful policy implementation. Thus, understanding whether opposition to policies is due to self-interest or concern for others is relevant: To what extent should policy makers promote collective welfare or individual financial impacts?

In our study, we will focus specifically on preferences for environmental regulation. While there is broad consensus that emissions need to be reduced, policies to effectively combat climate change are often difficult to implement. Dechezleprêtre et al. (2025) provide evidence that acceptance of environmental policies — specifically carbon taxes – hinges on effectiveness, self-interest (self-regarding) and social inequality concerns (other-regarding). Additionally, Stantcheva (2021) shows that social preferences strongly correlate with policy views. However, there is limited causal evidence on how self-regarding and other-regarding preferences influence support for environmental regulations. Additionally, a recently emerging literature about paternalism - a form of other-regarding preferences recently gaining more attention - suggests that individuals' preferences may differ depending on whether they

regulate themselves or others (Ambuehl et al., 2021; Bartling et al., 2023). Traditionally, these studies have focused exclusively on regulation of oneself *or* another individual, whereas we specifically focus on regulation when outcomes of oneself *and* other individuals are subject to policies. Moreover, we directly examine differences in the willingness to regulate decisions for oneself, other individuals, and groups that include both oneself and another individual. This combines the idea of commitment devices and paternalistic choices.

We plan to run a two wave survey experiment with a representative sample of the German population to study this question. In wave 1, participants act as choice architects (CAs) and regulate an allocation task faced by decision-makers (DMs) in wave 2. DMs allocate €2 between personal payment and a donation. CAs are randomly assigned to one of three types of DMs: (i) their own future decision (treatment "Self"), (ii) another participant's decision (treatment "Other"), or (iii) both their own and another's decisions (treatment "Group"). CAs decide whether to impose their preferred allocation or allow DMs to choose freely. These regulation decisions are implemented and paid out in wave 2.

To derive predictions, we develop a simple conceptual framework focusing on three core concepts. First, individuals gain utility from satisfying their current preferences (in wave 1). Second, they may experience utility losses due to potential misalignment with their own future preferences or others' preferences (in wave 2). Third, individuals experience a personal cost of restricting freedom of choice for their future self and others (in wave 2). We are particularly interested in differences in "Self" vs. "Group" and "Other" vs. "Group", since this can shed light on how regulation choices change when they affect more than one individual. Our framework predicts that individuals in "Group" will regulate less than in "Self" since CAs additionally account for others preferences and freedom of choice. The prediction for differences in "Other" and "Group" are not as clear-cut. In "Group", individuals may regulate more than in "Other" if the immediate utility of satisfying their current preferences outweighs concerns about future misalignment with own preferences and personal autonomy restrictions.

The pre-analysis plan proceeds with details on the sample, the experimental design, a detailed description of the conceptual framework including predictions, and the empirical strategy.

Survey and Sample

We plan to collect a sample of 2400 individuals representative for the German population in early 2025. Data collection has started on January 27, 2025 and we have thus far roughly 1000 observations. Survey participants will be recruited by the survey company Bilendi and paid for their participation a usual participation fee. All participants will be invited to a second survey wave two to three weeks after the initial wave. Importantly, approximately 600 participants will remain passive and therefore their answers will not be included in the analysis. Therefore, we end up with a sample of 1800 individuals. We collect the following information: First, participants will answer demographic and socio-economic questions including age, gender, education, income, employment status, having minor children (and if so how many), and the residential federal state.

Afterwards, the respondents will take part in an incentivized survey experiment that we describe in detail below. Incentives offered in this experiment are additional bonuses. Payment will be conditional on finishing the survey. Since we want to elicit revealed preferences, the experiment will involve donations to "atmosfair". "atmosfair" is a NGO that conducts and supports projects aimed at reducing CO2 in the atmosphere, e.g., by funding renewable energy projects in developing countries. This ensures that decisions in the experiment will have actual environmental consequences. To help participants better understand the magnitude of one kilogram of CO2 emissions, we ask them to estimate how far one would need to drive a car to produce this amount. Afterwards, we provide them with the accurate value of on average 6 kilometers.

Finally, survey respondents will answer a concluding questionnaire. Since the experiment will introduce a trade-off between personal monetary gains and CO2 in the atmosphere it includes questions about support and views on regulations targeting the reduction of CO2 emissions. Among them are CO2 labels, preferential placement of the least carbon intensive products at eye level in supermarkets, bans of carbon intensive products, setting a maximum value of carbon emissions for a product, and carbon pricing in general. For each policy, we elicit personal acceptance, perceived effectiveness, the belief about the acceptance in the population (share of population) and anticipated personal financial consequences. We also inquire general views towards governmental regulations in markets, consumption decisions and CO2 emissions. Additionally, the questionnaire elicits beliefs concerning whether climate change actually exists, whether it is caused by humans, perceived as a problem and how respondents

rate the personal impact caused by climate change using Likert scales. We measure risk preferences, patience, and locus of control according to questions from the German Socio Economic Panel.

Note that we will conduct an unrelated additional survey experiment with the same sample.

Experimental Design

The experimental design exploits the two wave structure of our survey (see C.1 for an overview of the design). In wave 1, participants act as choice architects (CAs) and regulate an allocation task that will be faced by decision-makers (DMs) in wave 2. DMs divide €2 between personal payment and donation. We inform participants that each donated €1 corresponds to reduction of 40 kg CO2. Our design begins with a within-subject component, eliciting CAs' preferred allocations, their beliefs about their own and others' future allocation choices, and their valuation of personal and others' autonomy. Subsequently, CAs are randomly assigned to different regulation targets in a between-subject design. Before being informed of their treatment assignment and the opportunity to regulate the decisions of DMs, CAs specify their preferred allocations for: (i) self-best (their future self as DM); (ii) other-best (a randomly chosen participant as DM); and (iii) group-best (binding for themselves and another randomly chosen participant as DMs). These three scenarios are presented to participants in random order, and they are informed that these allocation choices may determine future payments for themselves and others. This ensures that preferred allocations are independent of subsequent regulation decisions. Importantly, each cent allocated to the donation is doubled by the experimenter in the self-best and other-best allocations. In the group allocation, the doubling applies automatically since both group members donate the same amount. This ensures that the maximum donation a CA can affect is the same across scenarios.

Next, CAs report beliefs about their own and others' allocation decisions in wave 2. To examine whether CAs attach non-instrumental value (or costs) to the autonomy of allocation choices for themselves and others, we include a hypothetical delegation decision. We ask participants to imagine a third party implementing their preferred choice for themselves and decide whether to delegate this choice or retain autonomy. They also imagine the third party implementing the preferred allocation of another randomly chosen participant for that participant and state whether they prefer the participant to actively make the choice or delegate it. Delegation reflects decision costs, while non-delegation indicates autonomy value. To isolate the non-instrumental value of autonomy, we explicitly inform participants

that the outcome will always be the same, whether they delegate their own choice to a third party or if another participant's choice is delegated to the third party. These beliefs and values attached to autonomy will be used in our conceptual framework to predict regulation behavior.

CAs are informed that they can now regulate (in wave 1) future allocation choices regarding the €2 (in wave 2). We frame the €2 as a bonus payment participants receive for taking part in both survey waves. They are randomly assigned to one of three regulation targets. First, in "Self", participants regulate their own allocation choice in wave 2 (CA equals DM). Second, in "Other", participants are grouped in pairs and can regulate each others' allocation choices in wave 2 (CA does not equal DM). Importantly, participants in "Other" are informed that their partner also acts as their CA only after they submit their regulation choice, which helps mitigate confounding effects such as reciprocity or a more strongly perceived interdependency. Third, in "Group", participants are randomly paired to form a group. Unlike in "Other", only one participant in each pair acts as a CA, with the other participant not making any regulation choices. This ensures that decision power is the same across all treatments. CAs in "Group" can regulate both their own and the other participant's allocation choice in wave 2 (CA equals DM for their future self and but not for the additional DM). If a CA chooses to regulate, the implemented allocation is binding for the group - i. e., herself and the other participant.

In each treatment, participants decide whether to: (1) implement their self-best allocation or grant the DM free choice, and (2) implement their other-best allocation or grant the DM free choice. One of these decisions is randomly selected and implemented. Decision (1) represents our primary outcome of interest as it directly addresses our core research question: To what extent are individuals willing to enforce their personal preferences (self-best) on others and groups compared to themselves? Decision (2) represents our secondary outcome of interest, since it enables us to investigate whether the patterns of the self-best allocation also hold for the other-best allocation. Note we use the term 'regulating' to refer to the act of implementing the respective preferred allocation throughout the paper. Finally, we elicit the motives for regulation choices, such as self-regarding and other-regarding concerns. We offer several pre-specified options based on the regulation choice along with an open-ended category. These options include, for example, the unwillingness to restrict personal freedom or the belief that others will not act in their own best interest.

Figure excluded because similar to Figure C.1.

Conceptual Framework

First, consider the general case of a CA i regulating in the group-treatment. CAs consider three core concepts when making their regulation choice:

$$U_i = \underbrace{A_i(x_i, d)}_{\substack{\text{utility from pre-determined} \\ \text{personal payoff and donation}}} \underbrace{- \alpha_i |d - b_i^{d_{i,t+1}}| - \beta_i |d - b_i^{d_j}|}_{\substack{\text{misalignment with future} \\ \text{and other's prefs.}}} - \underbrace{C_i - v_i^{C_j}}_{\substack{\text{value of personal and other's} \\ \text{autonomy/decision costs}}}$$

j refers to another DM while $i, t+1$ refers to the future self as DM. $A_i(x_i, d)$ represents the utility derived from fixing the allocation now, where x_i is the personal payment and d is i's preferred donation which is also implemented for j. To keep the maximum donation that a CA can regulate constant across treatments, each cent that is donated in Self and Other will be doubled. The terms α_i and β_i are weights reflecting the importance of deviations from i's beliefs about own future donation choices ($b_i^{d_{i,t+1}}$) and others' donation choices ($b_i^{d_j}$). C_i represents the decision costs (negative) or the value placed on personal autonomy (positive), while $v_i^{C_j}$ reflects the value the individual places on another person's autonomy or decision costs.

We make the following assumptions: $A_i(x_i, d)$ increases with both x_i (the allocation to oneself) and d (the donation amount). Importantly, our experimental design allows us to treat $A_i(x_i, d)$ as exogenous to the regulation choice, as these preferences are elicited beforehand in the experiment. We assume α_i and $\beta_i > 0$, with $|\alpha_i| > |\beta_i|$, reflecting that individuals want to satisfy the preferences of the regulation target (avoid mistakes), but prioritize their future self over others. Consequently individuals with higher levels of α_i and β_i are less likely to regulate. $\alpha_i > 0$ aligns with regret theory as introduced by Loomes and Sugden (1982). When individuals believe their future preferences will differ from their current ones ($|d - b_i^{d_{i,t+1}}| > 0$), they anticipate potential ex-post regret from regulating, which negatively impacts their utility. $\beta_i > 0$ follows a similar intuition but is rooted in theories of guilt aversion (Battigalli and Dufwenberg, 2007, e.g). When individuals believe their preferred donation choice differs from j's preferred donation choice ($|d - b_i^{d_j}| > 0$), regulation may induce guilt by failing to meet j's expectations and consequently reducing i's utility. Note that this implicitly assumes that CAs believe that DMs act in line with their preferences.

$|d - b_i^{d_{i,t+1}}| < |d - b_i^{d_j}|$ reflects that, on average, i's future preferences are closer to his current pre-

ferred donation than his beliefs about the preferred donation of others. i typically has more information on himself, i.e., past behavior and preferences, than about j which makes this assumption plausible.[4] There is evidence that individuals intrinsically value autonomy ($C_i > 0$) beyond the utility derived from the outcomes of a choice (Fehr et al., 2013; Bartling et al., 2014, see e.g). While direct evidence for the value of others' autonomy ($v_i^{C_j}$) is limited, it is plausible that individuals who value personal autonomy also value others' autonomy ($v_i^{C_j} > 0$), albeit to a lesser extent ($C_i > v_i^{C_j}$). We incorporate the value attached to autonomy of others since it could explain regulation choices specifically because it has gained little attention yet.

In sum, regulating yields positive utility through $A_i(x_i, d)$, while potential misalignment with the regulation target's preferences and the suppression of choice autonomy yield negative utility. CAs only consider outcomes they can directly affect through regulation—in other words, their decision today. Disutility from preference misalignment and autonomy suppression, as well as the utility derived from satisfying current preferences ($A_i(x_i, d)$), is only realized if a CA chooses to regulate. Accordingly, we normalize the utility for non-regulation to zero, and a CA i will regulate only if $U_i > 0$.

We define the utility functions for each treatment as:

$$\textbf{Self:} \quad U_i = A_i(x_i, d) - \alpha_i |d - b_i^{d_{i,t+1}}| - C_i \tag{F.7}$$

$$\textbf{Other:} \quad U_i = A_i(0, d) - \beta_i |d - b_i^{d_j}| - v_i^{C_j} \tag{F.8}$$

$$\textbf{Group:} \quad U_i = A_i(x_i, d) - \alpha_i |d - b_i^{d_{i,t+1}}| - \beta_i |d - b_i^{d_j}| - C_i - v_i^{C_j} \tag{F.9}$$

Hypotheses

Based on the conceptual framework, we derive our main hypotheses. First, we focus on differences between treatments and, second, on the general willingness to regulate, which depends on beliefs and the valuation of autonomy. These predictions address our primary outcome of interest: how does the willingness to enforce personal preferences (self-best) change with the regulation target? We will specifically discuss whether these predictions might change when focusing on the other-best allocations. Gen-

[4] This assumption can also be justified by theories of projection bias or egocentric bias.

erally, the willingness to implement the self-best allocation should be higher in "Group" and "Self" compared to "Other" while the willingness to implement the other-best allocation should be lower. Moreover, since the preferred allocation choices are exogenous to the regulation choice, d in the self-best allocation represents the preferred donation when the DM is also the CA, whereas in the other-best allocation, d represents the preferred donation when the DM and CA are not the same person.

Hypothesis 1 (H1)

Individuals are less likely to regulate in "Group" treatment compared to "Self".

In "Group" compared to "Self", CAs also incorporate consequences of their regulation choice for j as a DM. There are two main reasons why individuals are less likely to regulate in "Group": potential misalignment with j's preferences ($\beta_i > 0$) and the suppression of j's autonomy when regulating, both of which reduce utility for CAs. To see this, compare equations (F.7) and (F.9).

Hypothesis 2 (H2)

Individuals are more likely to regulate in "Group" compared to "Other".

In "Group" compared to "Other", CAs also incorporate consequences of their regulation choice for themselves (i) in the second survey wave ($t+1$). There are two forces determining predictions (see equations (F.8) and (F.9)): First, regulation directly affects the CA's payoff, which increases their willingness to regulate since $A_i(x_i, d) > A_i(0, d)$. Second, regulation could lead to misalignment with future preferences and ($\alpha_i > 0$) and suppress choice autonomy of the future self ($C_i > 0$), which would predict less regulation in "Group" compared to "Other". We argue that the first force dominates and that individuals are more likely to regulate in "Group" compared to "Other". The utility from satisfying current preferences $A_i(x_i, d)$ is more tangible, while misalignment with future preferences ($\alpha_i > 0$) is a less salient cost reflected in low values of $|d - b_i^{d_{i,t+1}}|$. Additionally, even though individuals attach value to autonomy, it is unlikely to outweigh the utility derived from implementing the preferred outcome. Note that this is less likely to hold for the other-best allocation as preferences about what others should donate may differ more strongly from one's future preferences, thereby increasing the wedge $|d - b_i^{d_{i,t+1}}|$.

Hypothesis 3 (H3)

Individuals are more likely to regulate in "Self" compared to "Other".

From H1 and H2, it follows that there is more regulation in "Self" compared to "Other". However, as for H2, this prediction depends on which of two forces dominates: First, fixing the allocation directly affect the CA's own payoff ($A_i(x_i, d) > A_i(0, d)$) which increases utility from regulating in "Self". Second, a higher weight on the misalignment with future preferences ($|\alpha_i| > |\beta_i|$) and the fact that one's own autonomy is more important than others' autonomy ($C_i > v_i^{C_j}$) yields a lower utility from regulating in "Self". In line with our reasoning in H2, the utility gains from satisfying current preferences through regulation are more tangible and therefore outweigh the less salient costs of regulation, such as misalignment with future preferences and the suppression of personal future autonomy. Moreover, these costs are further reduced by our assumption that $|d - b_i^{d_{i,t+1}}| < |d - b_i^{d_j}|$ since future preferences are more aligned with current preferences than with others' preferences. As in H2, this is less likely to hold for the other-best allocation since the preferred donation is likely to be closer to beliefs about others' preferences than to beliefs about one's own future preferences $|d - b_i^{d_{i,t+1}}| > |d - b_i^{d_j}|$.

Hypothesis 4 (H4)

The larger the wedges $|d - b_i^{d_{i,t+1}}|$ & $|d - b_i^{d_j}|$ the less likely are individuals to regulate.

This hypothesis tests whether our assumption holds that the misalignment of preferences actually reduces utility and, hence, the willingness to regulate.

Hypothesis 5 (H5)

Higher values for C_i & $v_i^{C_j}$ makes individuals less likely to regulate.

This hypothesis tests whether our assumption holds that higher valuations of autonomy reduces utility and, hence, the willingness to regulate.

Empirical Strategy

Average Treatment Effects

We aim to test our hypotheses by estimating average treatment effects (ATEs) for the "Self" and "Other" treatments compared to the "Group" reference treatment. We define two outcomes: whether a CA chooses to regulate in the self-best case and the other-best case, i.e., whether they fix the preferred allocation immediately. Specifically, we code binary outcome variables, where $Y_i = 1$ if the CA chooses to regulate in the respective case, and $Y_i = 0$ otherwise. Note that we measure all relevant outcomes in the first survey wave. First, we will compare raw mean differences between all treatment groups for each outcome of interest and perform t-tests as well as Mann-Whitney U tests. Afterwards, to test H1, H2, and H3, we estimate the following specification using OLS:

$$Y_i = \gamma_0 + \gamma_1 Self_i + \gamma_2 Other_i + X_i' \gamma_3 + \epsilon_i \tag{F.10}$$

where i indexes an individual and Y_i is the respective outcome of interest. γ_1 measures the average treatment effect of "Self" and γ_2 the average treatment effect of "Other" compared to the reference group "Group" respectively. We cluster standard errors on the individual level ϵ_i. X_i' is a vector of controls including socio-economic variables such as age, gender, personal monthly net income, educational background, employment status, having minor children, how many minor children live in the household, voting intentions, and views whether the government should intervene in consumption decisions or regulate CO2 emissions. We will also report results without any controls and the following additional specifications. First, we include measures for risk-taking, patience, and locus of control. To directly compare "Group" and "Self" we test for differences in the respective coefficients. Additionally, in a similar specification we will change the reference group for the treatment effects from "Group" to "Self".

To test H4 and H5, we will estimate equation (F.10) and include the wedges between individuals' beliefs about their future donations and others' donations, the valuation of personal and other's autonomy, and the size of the self-best and other-best donations. We will also report these results excluding controls and including measures for risk-taking, patience, and locus of control. Finally, we plan to partition the data according to the treatments and run a similar specification where we only include the wedges and the valuations of personal freedom that fit the treatment, e.g., in "Group" we would test both wedges

and both valuations of autonomy, whereas in "Self" we only include the wedge regarding the future self and personal value of autonomy.

As an exploratory analysis we will also compare differences in the self-best, other-best, and group-best allocations.

We will correct for multiple hypothesis testing by applying the Romano-Wolf method.

Heterogeneous Treatment Effects

We will test the following moderators using interaction terms: Gender, age, voting intentions, educational background, income, and whether the state should intervene in consumption decisions or regulate CO2 emissions. Additionally, we plan to estimate a random forest to discover potential other heterogeneities as an exploratory analysis.

In another exploratory analysis we will investigate whether the wedges, valuations of autonomy and donation sizes have heterogeneous effects. Since we will elicit motives for regulation decisions, we plan to investigate how these two measures correlate.