



The role of autonomy and reactance for nudging — Experimentally comparing defaults to recommendations and mandates

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ARTICLE INFO

Keywords:

Autonomy
Climate protection
Experiment
Default
Public good
Reactance

ABSTRACT

Scholars, policymakers and decisionmakers sometimes criticize behavioral public policies, such as nudges, for undermining behavioral autonomy. We provide evidence from an experiment where participants encountered a recommendation, default value, or mandatory minimum contribution accompanied by varying information on the source, before contributing to climate protection and answering an autonomy-related questionnaire. We find that decisionmakers perceive defaults as more freedom threatening than recommendations and less threatening and angering than mandatory minimum contributions. Intrinsic motivation to protect the climate moderates these differences. An expert, but not the political source reduces threat to freedom and anger. Findings improve our understanding of decisionmakers' perceptions of nudges relative to other interventions.

1. Introduction

Nudges influence behavior presumably without limiting behavioral freedom. They do not incentivize or limit choice options and go beyond the provision of information (Thaler & Sunstein, 2008). In practice, nudges exploit the biases and heuristics characterizing human decision making. They change the context in which decisions take place, to affect them systematically. Despite attempts to make nudges transparent (see Bruns & Paunov, 2021, for an overview), they are frequently subtle and covert, such that their presence and purpose remain oblique to decisionmakers. This sparked criticism labeling nudges as unethical and limiting to behavioral autonomy (Hausman & Welch, 2010; Rebonato, 2014; Sunstein, 2018). Decisionmakers – those targeted by nudges – voice such criticism, as well (Felsen et al., 2013; Hagman et al., 2015; Jung & Mellers, 2016; Yan & Yates, 2019). Such critical perceptions can reduce the effectiveness of a nudge, create unintended effects, or reflect badly on the policymaker. The theory of psychological reactance serves to explain such unintended effects. Proposed by Brehm (1966), this theory states that freedom of behavior is a central requirement in people's decision making. When threatened, they are motivated to restore that freedom. Restoring freedom can mean behavior opposing a nudge, or expressing threat and hostility towards the source of the threat. Perceived threat to freedom and anger are two main components of psychological reactance (Dillard & Shen, 2005). Knowing this is

relevant for policymakers, since it can affect their choice of what intervention to choose.

For example, policymakers might have to decide which intervention to choose in order to increase the uptake of green energy contracts which may be slightly more expensive than non-green alternatives (Ebeling & Lotz, 2015). Policymakers can recommend green energy contracts, make them the default option, or outright ban non-green alternatives. These interventions can be perceived as autonomy-limiting to different degrees. Notably, this can cause reactance within decisionmakers, characterized by perceptions of threatened freedom and anger. This reaction can be amplified if the recommended, defaulted, or mandated option costs exceed decisionmakers' valuations of green energy. Furthermore, the reaction can depend on who decisionmakers perceive as the source of the chosen intervention. A recommendation by an expert might be seen as informative, while one coming from a politician might be perceived as threatening. In a democratic society, where voters can in principle deprive policymakers of their legitimacy to set boundaries to behavioral freedom, the most practical policy will depend, to some extent, on how threatening and angering decisionmakers rate interventions.

We report evidence from an online framed field experiment where decisionmakers can make real contributions to climate protection. For a sample representative of the German internet-using population, we measure a proxy for their intrinsic climate protection motivation. We

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then investigate how participants perceive recommended, defaulted, and mandatory minimum contributions to climate protection. Specifically, we investigate the extent to which they perceive the intervention as a threat to their freedom of choice or as a source of anger. In addition, we randomize the source responsible for implementing the intervention. Complementary findings from the same experiment are reported in Bruns and Perino (2021). They provide evidence for a default effect amenable to a reactance explanation. They show that a default, but not a recommendation or mandatory minimum contribution set below decisionmakers' intrinsic motivation to contribute to climate protection, reduces their contributions. Here, we investigate intervention and source effects on people's perceptions of these policies.

Our findings show that decisionmakers perceive defaults as more freedom threatening than recommendations, but not as more angering. Decisionmakers perceive mandatory minimum contributions as most threatening and exhibit the highest levels of anger in response. The intrinsic motivation of subjects to contribute to the public good is an important moderator of these differences: Decisionmakers forced to increase their contribution due to their low intrinsic motivation feel more threatened and angry than those facing a default or recommendation do. However, this is not the case for subjects with high intrinsic motivation, for whom these interventions promote a decrease of donations with respect to what they would have donated without any interventions. Furthermore, framing the source as an expert on the decision context causes decisionmakers to feel less threatened and angry. Yet, a political frame of the source has no effect on these perceptions. Our experiment reports immediate responses only. Further research is needed to test for the longevity and robustness of effects. These and further limitations regarding behavioral effects and generalizability have implications for the interpretation of the findings and suggest avenues for further research.

The findings shed light on how conventional and behavioral interventions' autonomy-related perceptions of decisionmakers differ. First, such evidence has been lacking in the literature on perceptions of nudges so far (e.g. Almqvist & Andersson, 2021; Bang et al., 2018; Reisch & Sunstein, 2016), as these almost exclusively compare different types of nudges. A recent exception from the COVID-19 context is provided by Dudás and Szántó (2021). Second, while reactance is an often used ex-post explanation for limited nudge effectiveness (Arad & Rubinstein, 2018; Costa & Kahn, 2013; Haggag & Paci, 2014; Hedlin & Sunstein, 2016; Yan & Yates, 2019), it is rarely measured directly (for exceptions, see Bruns et al., 2018; Goswami & Urmitsky, 2016).

For policymakers, a better understanding of how nudges are perceived in relation to policy alternatives facilitates selection of measures to solve policy problems. This is especially important given evidence that the availability of nudges can reduce preferences for traditional interventions, which may be more effective (Hagmann et al., 2019).

The remainder of this article is structured as follows: First, we outline the central literature and derive the behavioral predictions in Section 2. Then, we outline the experimental design, procedure, and a detailed description of our data in Section 3. We analyze findings in Section 4. Section 5 discusses important aspects of experimental design and statistical analyses, while Section 6 makes concluding remarks and suggests pathways for future research.

2. Important literature and behavioral predictions

Psychological reactance theory encompasses four core components: presence of freedom, elimination of or threat of freedom, arousal of reactance, and restoration of freedom (Dillard & Shen, 2005; Rosenberg & Siegel, 2018). When people expect the ability to act in a free and autonomous way, an elimination or threat to that freedom can arouse reactance. How strong this arousal will be depends on characteristics of the freedom and the threat itself. Notably, restricting the freedom of a behavior where no freedom is expected will generally not lead to reactance. Furthermore, an intervention that is perceived as restrictive,

although factually it is not, can arouse reactance. The arousal usually consists in a cognitive component, a perception of threat of freedom, and an emotional component, usually anger (Dillard & Shen, 2005). This is referred to as *state* reactance. Reactance usually manifests itself in two ways: Engagement in the restricted behavior to restore freedom, and threat and hostility towards the source of the threat (Rosenberg & Siegel, 2018).

By definition, nudges do not threaten freedom of choice. They neither restrict choices nor force people to behave in a specific way. Still, there is evidence suggesting that people feel defaults threaten their behavioral freedom (Hagman et al., 2015; Jung & Mellers, 2016; Reisch & Sunstein, 2016; Sunstein, 2018). Thus, in principle, they can arouse reactance. In the most extreme case, decisionmakers can perceive a (governmental) nudge as an attempt to subvert human agency, as a covert psychological trick to manipulate human behavior, exploiting the behavioral biases of "irrational" people (Hansen & Jespersen, 2013). Although these are extreme reactions, there are some explanations.

First, feeling threatened and angry because of policy interventions may result from a predisposition to value freedom and dislike paternalism. This can also be referred to as a proneness to experience reactance (also referred to as *trait* reactance), defined as a "consistent tendency to perceive and react to situations as if one's freedoms were being threatened" (Kelly & Nauta, 1997, p. 1124). Consequently, highly reactant people would rate interventions as more threatening to their behavioral freedom and also react with angry feelings.

H1: Effect of reactance proneness on perceived threat and anger. Decisionmakers with higher proneness to experience psychological reactance score higher on perceived threat to freedom and anger when facing either intervention.

Second, different types of interventions may evoke different levels of state reactance. We hypothesize that a recommendation, a default, and a mandatory minimum contribution, from first to last, lead to higher threat perception and more anger. While a recommendation is a relatively benign, overt instrument to influence behavior, a default is more subtle and covert. For instance, investigating the effect of defaults on buying carbon offsets, Yan and Yates (2019) find that opt-out policies are rated as more manipulative and deceptive, less autonomy preserving, more restrictive of choice freedom, and more coercive, than opt-in policies. As stated above, whether people experience reactance depends on their *perception* of an intervention as freedom threatening, *not* the intervention's actual restriction of freedom which, arguably, is absent for both recommendations and defaults. A mandatory minimum contribution, on the other hand, threatens freedom and autonomy, as it prohibits some options. Therefore, this bears the highest potential to elicit perceptions of threat to freedom and angry feelings (Falk & Kosfeld, 2006).

H2: Intervention effects on perceived threat and anger. Decisionmakers' experience of threat to freedom and anger depends on the type of intervention. A recommended, defaulted, and mandated minimum contribution each lead to increased perceived threat to freedom and each elicit more anger than the previous intervention, respectively.

Third, feeling threatened and angry because of a policy may result from the source perceived as responsible for its implementation. Whether decisionmakers perceive a government, a scientific expert, or an undisclosed source as the originator of an intervention can affect how they assess various aspects of an intervention. For example, findings by Costa and Kahn (2013) and Tannenbaum et al. (2017) show that nudges imposed either by an energy utility or government can have unintended effects for people with certain political beliefs. The reason could be that people with certain political beliefs feel more easily threatened by and thus respond negatively to nudges. Specifically, someone may view an intervention as a means to decrease negative

externalities when coming from a source framed as environmentally concerned, while they may view the same intervention as a covert means to distort decision making for personal profit when initiated by an opposed political party.

We expect that an intervention imposed by an expert source, an anonymous source, and a political source are, from first to last, increasingly perceived as intrusive. A source with a qualification in the field where he or she establishes any intervention is most likely considered legitimized. A politician, however, might elicit negative associations with the government (Perino et al., 2014), but could also be considered legitimized to implement an intervention. Because predictions are less clear, we formulate our hypothesis more cautiously.

H3: Regulator effects on perceived threat and anger. Decisionmakers' experience of threat to freedom and anger depends on the source responsible for implementing the intervention.

3. Experiment

3.1. Experimental procedure and treatments

The experiment, which was conducted online between November 2016 and January 2017, had two stages: In the first stage, subjects answered a questionnaire. The questionnaire measured trait reactance (Hong & Faedda, 1996), pro-environmental attitudes via the short version of the New Environmental Paradigm (NEP) questionnaire (Dunlap et al., 2000), and party preferences during the then-upcoming German federal election in September 2017.

In the second stage 1–4 weeks later, participants from Stage 1 made two contribution decisions and filled out a post-experimental questionnaire. The average attrition rate between both stages was 31.70% (see Appendix B.1 in the supplementary data for a detailed description). The stage 2 experiment is a modified dictator game, where subjects can decide to contribute any amount of their endowment to climate protection (see Diederich & Goeschl, 2017). After reading the instructions, participants were endowed with 100 Credits (5 EUR or ~\$6), answered a comprehension test question, and decided how much to contribute to climate change. This was their baseline decision without treatment exposure, serving as a proxy for intrinsic motivation.

Subjects were then randomly allocated to one of ten experimental groups. Based on their assignment, they encountered either no intervention, a recommendation, default, or mandated minimum amount of a 35 Credits (1.75 EUR) contribution to climate protection. The respective intervention was combined with either no information on the source responsible for its implementation, with information framing the source as an expert on climate policy, or with information characterizing her as a politician. In both cases, the person was identical. Only her description was varied, highlighting either her being an expert on climate change without mentioning her being a politician, or *vice versa*.² The detailed framings, instruction texts for participants, and screenshots of decision screens are in Appendix A, and questionnaires in Appendix C in the supplementary data. Subjects in the control group made the baseline decision twice and were not exposed to an intervention or source information. Fig. 1 shows the sequence of the experiment.

Subjects in the recommendation group were recommended by the source (if any) to contribute 35 Credits. When facing the default, subjects saw two radio buttons, of which the option to contribute 35 Credits was pre-selected. Subjects were free to choose the second radio button to specify another amount on the next screen. Again, the

default was linked to the respective source. The decision context for the mandatory minimum contribution was identical to the control- and recommendation setup, but subjects had to contribute at least 35 Credits. Here again, the intervention was linked to the respective source, if any was provided. In each of the cases without source information, the interventions were introduced neutrally.

After participants decided how much they wanted to contribute to climate protection for the second time, we informed them which of their two decisions was realized. This was chosen randomly. Private payoffs were paid to the participants and contributions were donated to the NGO "TheCompensators*". This NGO buys and retires emission rights from the European Union Emissions Trading System (EU ETS). The participants then answered several questions, one of which related to their perceptions of threat and anger regarding the intervention they faced. Some questions were only presented to subjects that encountered source information. The experiment was conducted online using Limesurvey. Participation was possible via personal computers and smartphones from any place with internet access. The experimental design was registered and made available online at the American Economic Association's registry for randomized controlled trials (AEARCTR-0001661).

3.2. Outcome measures

There are three main variables of interest. First, perceived threat to freedom due to the respective intervention. This variable was created by adding responses to four five-level Likert items, with response options ranging from "not agree at all" to "strongly agree". The alpha reliability of this scale was 0.94. Principal axis factor analysis followed by varimax rotation indicated that the first principal component explained 84.53% of the total variance.

Second, perceived anger due to the respective intervention. This variable was created by adding responses to four five-level Likert items, with response options ranging from "not agree at all" to "strongly agree". The alpha reliability of this scale was 0.82. Principal axis factor analysis followed by varimax rotation indicated that the first principal component explained 64.76% of the total variance. The two variables threat to freedom and anger are referred to in unison as state reactance.³

Third, individual proneness to experience psychological reactance, referred to as trait reactance. This variable was created by adding responses to 11 five-level Likert items, with response options ranging from "not agree at all" to "strongly agree". The alpha reliability of this scale was 0.79. Principal axis factor analysis followed by varimax rotation indicated that the first principal component explained 33.26% of the total variance.

Perceived threat to freedom and anger are measured using the questionnaire by Dillard and Shen (2005). Trait reactance is measured using the questionnaire by Hong and Faedda (1996). The questions corresponding to all items can be found in Appendix C in the supplementary data.

3.3. Participants

We analyze data of 806 participants, recruited via Lightspeed, a German panel provider. The mean age is 49.90 (SD = 15.66, Median = 51) years. About half (51.86%) of the respondents are women. 33.75% have lower, 33.37% middle, and 14.76% higher education,

² During the experimental design, we cooperated with a German politician who has a PhD in climate policy. To avoid deceiving participants, we asked her about preferred values for the contribution amount, which she could choose from a range of pre-specified contribution levels. This process led to 35 Credits as the focal value.

³ Alternatively, threat to freedom and anger variables can be specified by first dichotomizing responses to the single items, such that the variable has the value 1 if respondents "agreed" or "strongly agreed", and 0 otherwise. Then, these dichotomized variables can be added up to create the scale. We conducted the analyses presented in the following sections with these variables as well. Results appear robust to this alternative specification.

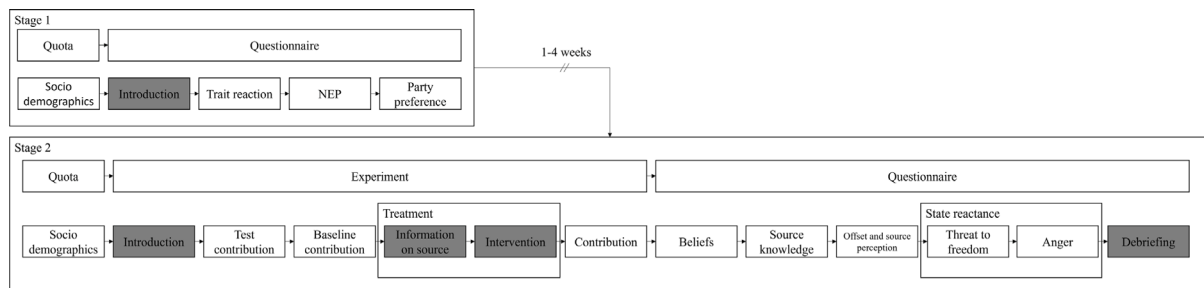


Fig. 1. Experimental sequence.

Notes: Shows the sequence of the two experimental stages. White boxes represent stages where participants provide input. Boxes with gray background represent stages during which participants receive information. Participants stages in both stages, with a time of 1 to 4 weeks in between both stages.

while 17.74% hold a university degree. One subject responded to have no education or to be in school. The median household earns 2,000–2,499 EUR. Appendix B in the supplementary data presents aggregated and disaggregated sample distributions of covariates, and of answers given to central questions from the questionnaires.

3.4. Power analysis

A power analysis for a regression model testing an interaction effect shows that 160 observations (two experimental groups) allow detecting a standardized minimum effect size of $f^2 = 0.05$ (medium effect) for an interaction, i.e. one tested, and 8 overall predictors 80% of the time (Faul et al., 2009). While power considerations have been taken into account *a priori* (Bruns & Perino, 2021), we did not conduct such calculations for the dependent variables used in this article. Therefore, we regard our analyses as exploratory and suggest to interpret the findings accordingly.

4. Findings

4.1. Descriptive statistics

Table 1 shows descriptive statistics of perceived threat to freedom and feeling of anger, for every treatment (Tables B.6 and B.7 in Appendix B in the supplementary data show these statistics for the individual items of both the threat to freedom and anger scale).⁴ Qualitatively, average (and to a lesser degree, median) threat perceptions and angry feelings follow the pattern of recommendation, default, mandatory minimum in increasing order, across all sources. Differences in the median are less pronounced when the source is an expert and more pronounced when the source was framed as a politician (esp. for anger). All of the interventions have higher ratings on threat perception and angry feelings when initiated by a politician, compared to no source or an expert. In addition, reported feelings of anger are lower on average than reported threat perception.

4.2. Intervention effects on reactance

We investigate whether proneness to reactance, the type of intervention and the source affect how much threat and anger decisionmakers report. Table 2 shows the estimates from OLS regressions modeling both threat perception (models 1–3) and angry feelings (models 4–6) under three different specifications, respectively. In contrast to models 1 and 4, models 2 and 5 include an interaction term between intrinsic motivation (dichotomized, where the variable takes the value 1 if the intrinsic contribution was greater than 35 Credits) and the intervention

Table 1

Descriptive statistics of threat to freedom and anger by experimental group.

	Threat to Freedom			Anger			n
	M	SD	Med	M	SD	Med	
No Source							
Recommendation	11.53	3.69	12	8.63	4.38	8	90
Default	12.43	3.54	13	9.70	3.98	9	81
Mandatory Minimum Contribution	13.36	4.30	14	10.20	4.76	9.5	84
Expert							
Recommendation	10.27	3.09	11	7.66	3.53	8	77
Default	11.52	3.54	12	8.34	3.54	8	73
Mandatory Minimum Contribution	12.10	4.07	12	9.01	4.84	8	77
Politician							
Recommendation	11.17	3.23	12	9.00	3.98	8	83
Default	12.16	4.08	12	9.29	4.39	9	80
Mandatory Minimum Contribution	13.63	4.49	14	10.92	4.62	12	79

Notes: Shows mean (M), standard deviation (SD), and median (Med) for the respective variables. n indicates the number of observations.

type (with the default as the base category), whereas models 3 and 6 include an interaction term between intervention and source type (with no source as the base category), instead.

In line with H1, we find that a propensity to experience psychological reactance positively predicts threat perception and anger as a response to either intervention (see estimates for *Reactance* in Table 2). These findings are in line with Dillard and Shen (2005).

F1: Decisionmakers with higher proneness to experience psychological reactance score higher on perceived threat to freedom and feeling of anger when facing either intervention.

We now turn to the findings relevant to H2. Model (1) shows that participants perceived the recommendation as less threatening to their behavioral freedom than the default ($b = -1.01, CI_{95} = [-1.64, -.39], p = 0.001$). However, model (4) reveals that the recommendation did not arouse less anger than the default ($b = -.67, CI_{95} = [-1.38, .04], p = 0.062$). Decisionmakers perceived the mandatory minimum contribution as more threatening than the default and as more angering (Threat: $b = .98, CI_{95} = [.29, 1.67], p = 0.005$; Anger: $b = .9, CI_{95} = [.16, 1.65], p = 0.018$).

F2: Decisionmakers perceive the recommendation as less threatening than the default but not less angering. They perceive the mandatory minimum contribution as more threatening to freedom and angering than the default.

The finding that defaults do not lead to more anger than recommendations, but are perceived as more threatening might be explained by anger being a consequence of reactance that is usually preceded by the cognitive perception of threat of freedom (Dillard & Shen, 2005). Therefore, anger occurring conditional on threat to freedom attenuates the intensity of the former. Relatedly, we measure both reactions using

⁴ 7 observations are missing for these analyses, since at least one of the 8 questions used to construct both scales was missing due to technical problems while conducting the experiment.

Table 2
Regression results of different model specifications for perceived threat to freedom and anger.

	(1) Threat To Freedom	(2) Threat To Freedom	(3) Threat To Freedom	(4) Anger	(5) Anger	(6) Anger
Rec (Base: Def)	-1.012** [-1.64,-0.39]	-0.740 [-1.60,0.12]	-0.961 [-2.05,0.12]	-0.670 [-1.38,0.035]	-0.356 [-1.38,0.66]	-1.174 [-2.40,0.054]
MMC (Base: Def)	0.978** [0.29,1.67]	2.182*** [1.26,3.10]	0.805 [-0.33,1.94]	0.904* [0.16,1.65]	2.123*** [1.06,3.19]	0.378 [-0.90,1.65]
Exp (Base: No)	-0.992** [-1.63,-0.36]	-0.924** [-1.55,-0.30]	-0.960 [-2.08,0.16]	-1.020** [-1.74,-0.30]	-0.959** [-1.67,-0.25]	-1.442* [-2.61,-0.27]
Pol (Base: No)	-0.104 [-0.77,0.56]	-0.0948 [-0.76,0.57]	-0.247 [-1.42,0.92]	0.217 [-0.52,0.96]	0.225 [-0.51,0.96]	-0.429 [-1.73,0.87]
Reactance	0.0554* [0.0055,0.11]	0.0514* [0.0018,0.10]	0.0557* [0.0056,0.11]	0.0910** [0.036,0.15]	0.0870** [0.033,0.14]	0.0913** [0.037,0.15]
IM > 35 (Base: IM ≤ 35)	-2.053*** [-2.59,-1.52]	-0.913 [-1.85,0.026]	-2.048*** [-2.59,-1.51]	-2.152*** [-2.74,-1.56]	-0.961 [-1.95,0.031]	-2.157*** [-2.75,-1.56]
IM > 35 × Rec		-0.639 [-1.89,0.61]			-0.731 [-2.11,0.65]	
IM > 35 × MMC		-2.808*** [-4.17,-1.44]			-2.827*** [-4.28,-1.37]	
Rec × Exp			-0.0479 [-1.56,1.46]			0.770 [-0.89,2.43]
Rec × Pol			-0.112 [-1.67,1.44]			0.778 [-1.01,2.56]
MMC × Exp			-0.0372 [-1.66,1.58]			0.466 [-1.33,2.26]
MMC × Pol			0.556 [-1.17,2.28]			1.136 [-0.73,3.00]
Constant	11.34*** [9.55,13.1]	10.96*** [9.17,12.8]	11.37*** [9.48,13.3]	7.125*** [5.15,9.10]	6.724*** [4.71,8.74]	7.466*** [5.39,9.55]
Observations	727	727	727	727	727	727
R ²	0.136	0.159	0.137	0.118	0.137	0.121
LL	-1970.0	-1960.3	-1969.5	-2051.9	-2044.2	-2050.9
AIC	3954.0	3938.6	3961.0	4117.8	4106.3	4123.7

95% confidence intervals based on robust standard errors in brackets.

Notes: Shows unstandardized OLS point estimates. *Base* denotes the base category of the respective variable. *Rec* is equal to 1 if subjects encounter the recommendation, 0 otherwise. *MMC* is equal to 1 if facing the mandatory minimum contribution, 0 otherwise. *Pol* is equal to 1 if source is framed as a politician, 0 otherwise. *Exp* is equal to 1 if source is framed as an expert, 0 otherwise. *IM>35* is a dummy equal to 1 for subjects with intrinsic motivation above 35 Credits, 0 otherwise.

* $p < 0.05$.

** $p < 0.01$.

*** $p < 0.001$.

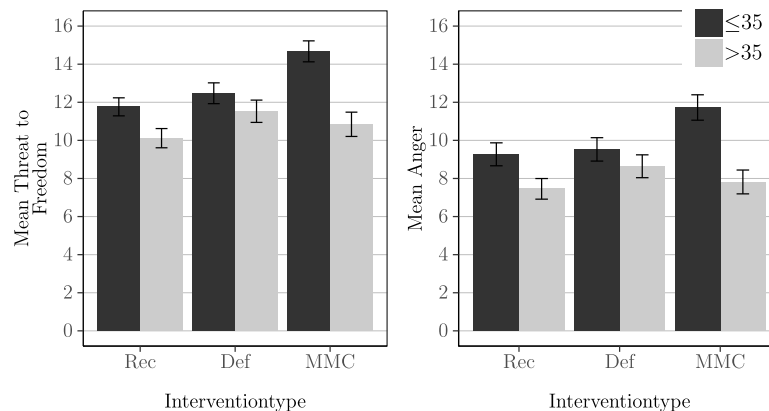


Fig. 2. Mean threat perception and anger scores by experimental group and intrinsic motivation.

Notes: Shows mean threat perception and anger scores with 95% confidence intervals. Disaggregated by intervention type and intrinsic motivation. Intrinsic motivation is dichotomized. Black bars denote mean outcomes for subjects with intrinsic motivation smaller than or equal to 35 Credits, and gray bars denote mean outcomes for subjects with intrinsic motivation larger than 35 Credits. *Rec* labels outcomes from participants facing the recommendation, *Def* the default, *MMC* the mandatory minimum contribution. Aggregated over source type.

self-reported measures. Measuring affect or emotions via these means can be noisy, especially since people are less accustomed to evaluating and reporting their affective as opposed to cognitive states.

Furthermore, it is insightful to look at these perceptions conditional on the intrinsic motivation of decisionmakers. While psychological reactance might occur when an intervention attempts to *change* contributions in either direction, it is more straightforward to expect that

participants experience threat and anger when an intervention aims to *increase* their contribution. This is what we find in Fig. 2 and models (1) and (4). Having contributed more than the focal value (meaning the recommended, default, or mandatory minimum value) prior to the second decision entails lower levels of perceived threat to freedom ($b = -2.05$, $CI_{95} = [-2.59, -1.52]$, $p < 0.001$), and felt anger ($b = -2.15$, $CI_{95} = [-2.74, -1.56]$, $p < 0.001$).

Decisionmakers that contributed less than or equal to 35 Credits in the first round of the experiment perceived the restriction of the choice set as more threatening ($b = 2.18, CI_{95} = [1.26, 3.10], p < .001$) and angering than the default ($b = 2.12, CI_{95} = [1.06, 3.19], p < .001$). Strikingly, however, the significant interaction effects change these relations for subjects with intrinsic motivation above the focal value both for threat ($b = -2.81, CI_{95} = [-4.17, -1.44], p < .001$) and anger ($b = -2.83, CI_{95} = [-4.28, -1.37], p < .001$). For highly intrinsically motivated subjects, a mandatory minimum leads to less threat perception and anger than a default (see Fig. 2). This pattern is interesting because, as shown in Bruns and Perino (2021), the default has the largest effect on subjects with high intrinsic motivation, causing them to decrease their contribution considerably. In combination with our findings, this could be explained by the finding that decisionmakers feel threatened and angry when doing so.

4.3. Source effects on reactance

Turning to the effects of source characteristics, indicated by the estimates for *Exp* and *Pol* in Table 2, we find that, relative to no source information, expert framing decreases subjects' perceived threat to behavioral freedom ($b = -.99, CI_{95} = [-1.63, -.36], p = .002$) and felt anger ($b = -1.02, CI_{95} = [-1.74, -.31], p = .005$). Providing information about the political source, relative to no information, has no effect on threat ($b = -.1, CI_{95} = [-.77, .56], p = .759$) and anger ($b = .22, CI_{95} = [-.52, .96], p = .566$). Moreover, political source information leads to higher threat perception ($b = .89, CI_{95} = [.23, 1.55], p = .008$), and felt anger ($b = 1.24, CI_{95} = [.51, 1.97], p = .001$) than expert source information.

F3: An expert source decreases the perception of behavioral threat and anger relative to no framing and political framing. Relative to no framing, political source framing has no effect.

Models (3) and (6) show that the differences in experienced threat and anger induced by the different interventions do not depend on the responsible source (all interaction estimates are insignificant). Answers given to some of the questions from the post-experimental questionnaire provide deeper insights into possible reasons. Based on logit models (Table B.8 in Appendix B in the supplementary data), we find that a political, compared to expert source is less likely perceived as an expert ($OR = .24, CI_{95} = [.16, .35], p < .001$), and less likely as having the goal to reduce CO₂ emissions ($OR = .44, CI_{95} = [.30, .65], p < .001$). However, a political source is *not* more likely to be regarded as having the goal of affecting decisionmakers free decision ($OR = 1.43, CI_{95} = [.97, 2.11], p = .07$). These findings indicate that source information affects how decisionmakers' perceive the source's ulterior motives.

Furthermore, as shown in Table B.9 in Appendix B in the supplementary data, perceptions that the source is an expert, and has the goal to influence decision making, predict perceived threat to freedom negatively and positively, respectively (Regulator is expert: $b = -.88, CI_{95} = [-1.61, -.16], p = .016$; Regulator has goal to influence: $b = 3.49, CI_{95} = [2.81, 4.18], p < .001$). Perceiving the goal as reducing CO₂ emissions does not significantly predict threat perception ($b = -.48, CI_{95} = [-1.19, .24], p = .192$).

All three source-related perceptions predict felt anger. Specifically, if the source is perceived as an expert, this predicts less anger ($b = -1.61, CI_{95} = [-2.39, -.84], p < .001$). If the goal is perceived to be influencing decision making, this predicts higher anger ($b = 2.92, CI_{95} = [2.11, 3.72], p < .001$), while perceiving the goal as reduction in CO₂ emissions predicts less anger ($b = -1.57, CI_{95} = [-2.39, -.74], p < .001$). The finding that politicians are regarded as having less expertise and as less likely to be aiming at emissions reductions instead of other motives fits the observation that experts cause less threat perception and anger, than a neutral, as well as political source.

Reactance theory posits that if the decisionmaker (a) regards the source's goals as valid, (b) has relatively weak preferences concerning

the respective choice, and (c) is accustomed to following the requests of that source, then compliance is likely to outweigh reactance (Clee & Wicklund, 1980). Consequently, when a source is neither familiar nor perceived as an expert, reactant responses are likely. This fits our observation that perceiving a source as having expertise predicts less anger and threat perception. Furthermore, decisionmakers may conjecture vested interests (ulterior motives) of the source based on the information and intervention type used (Bénabou & Tirole, 2003; Kelley & Michela, 1980). There is suggestive evidence for that in our data, as the political source is less likely perceived as having the goal to reduce CO₂ emissions, compared to the expert source. Subjects might conjecture that the political source has underlying, covert motives to disrupt their choice.

5. Discussion

Our previous analyses show that (1) the more decisionmakers are prone to show reactance, the more threatening to freedom and angering they perceive either intervention, (2) defaults are perceived as less threatening to freedom and angering than mandatory minimum contributions, and as more threatening, yet not more angering, than recommendations (3) framing the source as an expert, but not as a politician, reduces these perceptions.

The first finding is as expected and in line with Dillard and Shen (2005). The second finding suggests that defaults are perceived as more intrusive than mere recommendations. Although defaults are nudges, which are, by definition, not restrictive to choice, decisionmakers perceive them as threats to their freedom, although it does not appear to lead to anger. Defaults have various mechanisms explaining their effectiveness. They can communicate social norms (Everett et al., 2015), serve as anchors or reference-points (Dhingra et al., 2012; Dinner et al., 2011; Samuelson & Zeckhauser, 1988), simplify the decision for lazy decisionmakers (Johnson & Goldstein, 2003; Madrian & Shea, 2001), or facilitate coordination (Barron & Nurminen, 2020; Cappelletti et al., 2014). Another mechanism is being an implicit recommendation (McKenzie et al., 2006). Our findings suggest that one of the default mechanisms other than being a recommendation is responsible in making decisionmakers perceive it as more threatening than a mere recommendation.

Our findings also show that decisionmakers' intrinsic motivation is a relevant moderator. Decisionmakers with low intrinsic motivation to contribute to climate change see either intervention as more threatening and angering than decisionmakers with high intrinsic motivation. While this is expected, there is some evidence that this leads defaults to be perceived as more threatening and angering than mandatory minimum contributions for decisionmakers with high intrinsic motivation. This is striking, as it suggests that there might be something inherent in a default that decisionmakers disapprove of, even if (or because) it would get them to reduce their donation with respect to their intrinsic motivation.

The third finding suggests that there are source effects, which partially contradicts null effects reported by Gold et al. (2020). They do not find any evidence that whether the government, researcher, or advertising agent implemented nudges matters for their acceptability. We show that it might, however, matter for perceptions of threat and anger, at least for an expert source (which would correspond to the "researcher" in the experiment by Gold et al., 2020). We also explore potential mechanisms of these effects by investigating how these sources are perceived differently with respect to their expertise and goals.

These findings offer some insights for policy. First, although defaults are praised as effective and efficient nudges that, per definition, leave decisionmakers free to choose as they wish, they may be perceived as threatening and arouse anger. These perceptions might influence policymakers decisions of which intervention to choose from a set of alternatives. In certain situations, mere recommendations might be

more advisable, if perceptions of decisionmakers are relevant to make sure the policy intervention is accepted by those it influences.

Second, policymakers should take into account that perceptions may differ based on decisionmakers' intrinsic characteristics, suggesting heterogeneous treatment effects. This also provides a chance for defaults to be more effective, if they are personalized based on specific characteristics of decisionmakers (Bruns & Perino, 2021; Sunstein, 2013; Sunstein & Reisch, 2021; Yeung, 2018).

Third, political actors might want to make sure that an expert is regarded as the source of an intervention.

Fourth, there is a vast literature showing almost unambiguously that nudges work equally well when opaque or transparent (Bruns & Paunov, 2021). Our results suggest that what makes people angry and what makes them feel their autonomy is threatened is actually the strength of the intervention, rather than its opacity, even though opacity is sometimes used as a downside of nudging (Bovens, 2009).

There are some important caveats. The findings, and insights for policymakers should be interpreted in light of the downsides of the used methodology. Participants knew they were participating in an experiment, and the decision situation was largely artificial, although we took steps to make decisions on climate protection consequential. This limits the generalizability of the findings and calls for additional research in more realistic environments, in which participants are unaware of being in an experiment. Since part of our findings relate to heterogeneous treatment effects, we also note the limited sample size. The pre-analysis plan did not specify the exact hypotheses that were going to be tested, and we conducted the *a priori* power analysis solely in order to power one of the interaction tests conducted in Bruns and Perino (2021).

Specifically with respect to the primary outcome variables, threat to freedom and anger, there are reasons to expect that subjects were unlikely to experience high levels of these. While this is not a problem with respect to treatment effects, per se, small treatment effects decrease the power of statistical tests. Reactance theory posits that expecting the ability to behave freely is a precondition of reactance (Clee & Wicklund, 1980). In an experiment, participants likely expect to act in narrow bounds set by the experimenter. Additionally, beliefs about the ability to behave autonomously are influenced by perceiving the freedoms of reference groups (Clee & Wicklund, 1980). In our case, people were not aware that others faced different interventions than themselves, i.e., had more or less freedom to choose. This might have reduced the likelihood to experience reactance among people in the default- and mandatory minimum conditions, relative to a setting where subjects knew about others' higher behavioral freedom. Additionally, there were no future consequences for participants outside of the experiment due to either intervention. This can result in low experienced reactance since no future freedom is threatened. In addition, psychological reactance is a transitory state, meaning that it recedes over time (Clee & Wicklund, 1980). This is problematic for self-reported measures of state reactance, operationalized here by perceived threat to freedom and anger.

6. Conclusion

We present evidence from an online framed field experiment with a sample representative of the German internet using population on how different types of interventions (recommendation, default, mandatory minimum contribution), as well as information provided on the source of such interventions (no information, expert source, political source), affect decisionmakers' autonomy related perceptions of these interventions.

Decisionmakers perceive the recommendation as less threatening but not less angering than a default. They perceive the mandatory minimum contribution as more threatening and angering than both

default and recommendation. Decisionmakers with high intrinsic motivation see a default as more threatening and angering than a mandatory minimum contribution. Framing the source as an expert decreases perceived threat to freedom and anger, while we find no effect of political framing.

Future research should investigate the reliability and generalizability of our findings. While the controlled setting and representative sample benefit generalizability, future studies, especially natural field experiments, could measure the reactance specific variables in order to verify or reject that their behavioral effect, despite their influence on perceptions, is absent or negligible. Larger samples, as well as investigations of other nudges, like social norms, which have been shown to create backfire effects (e.g., Costa & Kahn, 2013; Schultz et al., 2007), would help to find out whether these effects occur irrespective of reactance-specific perceptions.

Our findings advance the literature on perceptions of behavioral interventions, especially defaults in order to alleviate negative externalities (Carlsson et al., 2021; Carlsson & Johansson-Stenman, 2019), shifting the focus on the psychology and perceptions of decisionmakers who are influenced by behavioral interventions (Bruns & Perino, 2021; de Jonge et al., 2018). Investigating how different decisionmakers perceive nudges, and exploring whether these perceptions are associated with behavioral changes will advance the understanding of these policies. This will aid policymakers to assess whether using behavioral interventions instead of conventional alternatives, such as recommendations, taxes, or even mandates, is fruitful for their target population.

Acknowledgments

We are grateful for the funding received by the Graduate School of the Faculty of Business, Economics and Social Sciences at University of Hamburg, Germany. Hendrik Bruns received funding from the Graduate School of the Faculty of Business, Economics and Social Sciences at the University of Hamburg, Germany, as well as the International Max-Planck Research School on Earth System Modelling, Germany. Grischa Perino acknowledges financial support of the University of Hamburg, Germany.

We thank participants of the following workshops, conferences, and summer schools: The 9th Summer School of the International Max Planck Research School on Adapting Behavior in a Fundamentally Uncertain World in Jena, the PhD students seminar at the Department of Economics at the University of Hamburg, the AURÖ at the Helmholtz-Centre for Environmental Research (UFZ) in Leipzig, the workshop on Experiments for the Environment in Osnabrueck and Bremen, the Manchester Environmental Economics Workshop on Social Preferences, and the International Conference on Social Dilemmas (ICSD) in Taormina. Furthermore, we thank Claudia Schwirplies, Fredrik Carlsson, Peter Martinsson, and Christina Gravert for valuable comments and suggestions.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at <https://doi.org/10.1016/j.socrec.2023.102047>.

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