Can Nudges Be Transparent and Yet Effective?

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Abstract

Nudges receive growing attention as an effective concept to alter people's
decisions without significantly changing economic incentives or limiting op-
tions. However, being often very subtle and covert, nudges are also criticized
as unethical. By not being transparent about the intention to influence in-
dividual choice they might be perceived as limiting freedom of autonomous
actions and decisions. So far, empirical research on this issue is scarce. In this
study, we investigate whether nudges can be made transparent without limit-
ing their effectiveness. For this purpose we conduct a laboratory experiment
where we nudge contributions to carbon emission reduction by introducing
a default value. We test how different types of transparency (i.e. knowledge
of the potential influence of the default, its purpose, or both) influence the
effect of the default. Our findings demonstrate that the default increases con-
tributions, and information on the potential influence, its purpose, or both
combined do not significantly influence the default effect. Furthermore, we
do not find evidence that psychological reactance interacts with the influence
of transparency. Findings support the policy-relevant claim that nudges (in

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the form of defaults) can be transparent and yet effective.

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1. Introduction

Nudges, a concept coined by Thaler & Sunstein (2008), describe a diverse set of instruments that utilize behavioral insights in order to affect individual behavior, without limiting options or significantly changing economic incentives. They have become an alternative to economic interventions. While nudges affect behavior by changing the context, thus primarily focusing on automatic decision processes, incentives can be seen to change cognition instead, thus focusing on conscious decision making (Dolan et al., 2012). The recent success of this approach is as a direct consequence of conceiving individual behavior as bounded, instead of perfectly rational and selfish (Bolton & Ockenfels, 2012). Nudges are evolving into a popular form of soft regulation in various fields such as health, finance, and environmental protection (Sunstein, 2014a; Alemanno & Sibony, 2015; World Bank, 2015; Lourenco et al., 2016). Despite its growing popularity, the use of behavioral insights in policy-making is subject to criticism (e.g. Hausman & Welch, 2010; Rebonato, 2014). One remarkable and often criticized aspect of nudges is that they often influence individual behavior without being noticed by the affected subject (Dhingra et al., 2012; Hansen & Jespersen, 2013; Sunstein, 2016). This raises the concern that nudges covertly violate individual autonomy and are therefore unethical (Bovens, 2009; House of Lords Report, 2011). Such regulation thus lacks the transparency that characterizes other regulatory instruments. For instance, when the government imposes a tax to reduce consumption of a product (e.g. cigarettes, or carbon dioxide), people are aware of this tax and can compel the government to justify it (Sunstein, 2014b). On the other hand, when the government sets an opt-out system instead of an opt-in system to promote certain behavior (e.g. organ donation) it exploits several psychological biases, often without people's awareness (Hansen & Jespersen, 2013). Felsen et al. (2013) demonstrate in a vignette study that a significant proportion of individuals have reservations towards nudges they perceive as covert. Additionally, another recent research

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stream provides evidence of the intrinsic value of decision rights and autonomy (Fehr et al., 2013; Bartling et al., 2014; Owens et al., 2014). To address this criticism we investigate whether nudges can be made transparent without reducing their effectiveness. In this context, we take into account that the covert nature of nudges is often said to be essential for their effectiveness (Bovens, 2009; House of Lords Report, 2011). Also, we acknowledge that telling people that the nudge is used to influence their decision potentially evokes a perceived threat to their freedom, leading them to experience psychological reactance. The latter can be defined as "the motivational state that is hypothesized to occur when a freedom is eliminated or threatened with elimination" (Brehm & Brehm, 2013, p. 37). This could not only inhibit the effect of the nudge but could even lead to the opposite effect than the one intended. We presume that experiencing reactance is mitigated when information on its purpose substitutes or complements the nudge. According to salience theory (Bordalo et al., 2012), providing the purpose increases the degree to which the ultimate goal of the nudge, relative to its means of behavioral influence, is taken into account during the decision process. This hypothetically reduces the propensity to elicit a state of psychological reactance. Therefore, this phenomenon is important when investigating the influence of different types of transparency on the effectiveness of nudges. We report evidence from a laboratory experiment where subjects can contribute to real climate protection. The nudge is a default value that intends to increase contributions. Such a default in a public goods context, unlike nudges aiming to improve individual outcomes, attempts to increase positive external effects that only benefit the individual in the aggregate, but affords them to forfeit immediate personal economic gains.[1] Thus, this context is more likely to produce a state of psychological reactance, and is thus suitable for testing it.

In general, there are different mechanisms through which a default potentially influences behavior, e.g. as a reference value and anchor (for construction of preferences), through provision of social norms or information, or through inertia (by imposing pecuniary or cognitive costs on deviating from the default). Sunstein & Reisch (2016) provide a review on default-

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[1]Hagman et al. (2015) divide nudges into pro-self and pro-social. While the former nudge people towards making better decisions for themselves, the latter nudge people towards behavior that benefits society.

mechanisms. Note that Cappelletti et al. (2014) provide evidence from a public good game that defaults do not work as recommendations, i.e. as information provision in such a context. We expect the default value to increase contributions through two possible ways. First, it can increase the fraction of people picking the default value. Second, it can induce people to increase their contribution towards this value. We discuss our possible mechanisms in the second section and relate them to our findings in the last section.

The type of transparency that accompanies the default varies across treatments and consists of either informing decision makers about its potential behavioral influence and/or informing them about its purpose to increase contributions to climate protection. After the experiment, we assess two different measures of psychological reactance. Thus, we test whether the influence of transparency is limited to a sub-group of participants distinct in their proneness to show psychological reactance (trait reactance). Additionally, we test whether transparency influences the perception of a nudge as a threat for freedom of choice, and whether it functions as a source of anger (state reactance).

Recent findings from Arad & Rubinstein (2017) illustrate why our investigation of transparency and psychological reactance in the context of nudges is important. Their findings suggest that some subjects may consciously act contrary to the encouraged action, presumably in order to protest against the intervention of the government. The authors argue that full transparency of nudges, thus, may even lead to the opposite outcome than the one intended (as opposed to simply eliminating the effectiveness of a nudge). Some people behave in a completely different way simply out of protest against being manipulated. Contrary to this argument, findings by Sunstein (2016) from a nationally representative survey in the USA show that there is widespread support for nudges, and that transparency concerning the nudge will not diminish its effectiveness. Reisch & Sunstein (2016) show that there is also a general support of nudges in six European countries.

To the best of our knowledge, there are only three empirical studies directly relevant to our research question. Loewenstein et al. (2015), in a laboratory experiment, find no evidence that informing subjects that they were presented with a pro-self default option influences their effectiveness. Similarly, Kroese et al. (2016), in a field experiment, find no evidence that making subjects aware of the purpose behind a pro-self default has any effect. Steffel et al. (2016), in several hypothetical and marginally incentivized

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consumer-related experiments, find no evidence that stressing the potential behavioral influence of a pro-self, as well as a pro-social default impacts their effectiveness, although it affects perception by the consumer.

While existing evidence unanimously suggests the impact of transparency on effectiveness of nudges is absent, our research augments the extant literature in various ways. First, subjects in our experiment face a trade-off between real monetary payoffs and real contributions to a (global) public good. By contrast, two of the previous studies employed relatively abstract and stylized environments, and did not demand subjects to make (substantial) financial tradeoffs. Although Kroese et al. (2016) investigate behavior in the field, they do neither study pro-social nudges, nor do they incorporate both types of transparency. Second, we investigate the distinct, as well as combined effect of two types of transparency on the default effect. Previous research focused exclusively on either of these two categories. However, there are reasons to expect that informing decision makers about the potential behavioral influence of a nudge has different consequences than informing them about its purpose. Third, we enrich our analysis with the concept of psychological reactance, allowing for a deeper understanding of potential channels through which transparency influences default effects. Recent research on nudges, although focusing conceptually on the role of reactance (Arad & Rubinstein, 2017; Hedlin & Sunstein, 2016), did not investigate its interaction with transparency.

Consequently, we contribute to the topic of transparency of nudges in various ways. First, we enable a more nuanced view by investigating two types of transparency, thus contributing to a better understanding on how transparency works and whether policy-makers can make nudges more transparent without diminishing effectiveness. Second, our experimental setup, albeit controlled, sets up a realistic context, enabling us to make more valid inferences about the impact of transparency on nudges in "the real world". Third, we widen the discussion on transparency by investigating its connection to the concept of psychological reactance.

To preview our results, defaulted contributions are significantly higher than in the control group, even when accompanied by either type of transparency, including both types. In addition, contributions in the treatment groups (with or without transparency) do not significantly differ from each other. Thus, we replicate the lack of an effect of transparency, indicated by evidence from the studies outlined above. Finally, we neither find evidence that trait reactance interacts with transparency, nor that transparency

changes the perception of nudges as freedom threatening or sources of anger. Therefore, our findings advocate that nudges (in the form of defaults) can be transparent and effective.

The remainder of the article is structured as follows. In Section 2 we discuss psychological reactance as a conceptual background to covert nudges, followed by derivation of behavioral predictions. We lay out the experimental design in Section 3. In Section 4 we present and analyze the results. Section 5 concludes.

2. Conceptual framework and behavioral predictions

Since Brehm (1966) introduced the theory of psychological reactance, many studies have explored this phenomenon. Social influence attempts (such as nudges) that are detected by an individual may be perceived as a threat to freedom of choice (Brehm, 1966). The elicited state of psychological reactance may result in behavioral and cognitive efforts to reestablish freedom as well as uncomfortable, hostile, aggressive, and angry feelings (Dillard & Shen, 2005). Consequently, people may try to restore their freedom by exhibiting exactly the restricted behavior, thus, in our case, strongly deviating from the default value. In addition, they may devaluate the source of threat (the initiator of the nudge), increase their liking for the restricted freedom, or counter-argue against the imposed option (Brehm, 1966; Dillard & Shen, 2005). People react in such a manner not only to obvious and direct, but also to subtle and subliminal threats (Chartrand et al., 2007).

In order to investigate whether transparency influences the effectiveness of pro-social nudges, specifically defaults, we chose the context of climate protection. With climate change being one of the major challenges faced by society on a global scale today, information-based instruments and nudges are becoming increasingly important to increase individual contributions to climate and environmental protection (Allcott & Mullainathan, 2010; Araña & León, 2013; World Bank, 2015).

One way to contribute to climate protection is to offset (parts of) one's own yearly CO_2 emissions by donating to specific charitable organizations (in the experiment, referred to as 'climate protection fund'). These organizations use donations to purchase and delete carbon emission licenses from the European Union Emissions Trading Scheme (EU ETS).[2] Buying carbon

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[2]The EU ETS is a European market that prices carbon emissions and allows regulated

licenses is an effective way for individuals to contribute to climate protection, when compared to, e.g. electricity-saving (Perino, 2015). Therefore, individual payment for carbon license retirement is a relevant context in which the influence of transparency on the effectiveness of a pro-social nudge can be investigated.

Based on psychological reactance theory we expect that mentioning the potential influence of a default will evoke the most reactance and thus reduce its effectiveness. In contrast, the sole provision of the purpose, i.e. climate protection, should evoke little reactance since this induces perspective taking. In addition, it renders the positive goal of the contribution more salient. According to salience theory formulated by Bordalo et al. (2012), more salient attributes will be over-weighted in the decision process. Based on this argument, providing the purpose will work as an additional nudge and thus increase the default effect. Finally, accompanying the default with both types of information will be the most transparent form of the nudge. Due to combining the hypothesized "downside" effect of reactance and "upside" effect of the salience of the purpose of the nudge we expect the contribution level to be in between the other treatments. In sum, hypotheses concerning people's contribution decisions in the presence of the default are as follows:

H1: If participants are confronted with a default, contributions will be higher compared to when there is no default.

H2: If participants are informed that the default may have an influence on their decision, contributions will be lower compared to when they are not informed.

H3: If participants are informed of the purpose of the default, contributions will be higher compared to when they are not informed.

H4: If participants are informed of the potential influence of a default and of its purpose, contributions will be higher than with information solely on influence and lower than with information solely on purpose.

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industries to trade their emission rights. Buying licenses off the market increases the scarcity of emission rights, resulting in higher prices and thus increasing the incentives for regulated firms to invest in emission-reducing technology.

Although it is not the purpose of this paper to identify the mechanism underlying the potential default effect, hypothesizing about a transparency-effect relies on certain assumptions regarding this mechanism. Transparency can only exert an effect if subjects are aware of the transparency and consequently of the default. This necessity rules out default effects that rely on unawareness (Madrian & Shea, 2001). If defaults work via costs of opting out (Johnson & Goldstein, 2003), providing a reference point (Samuelson & Zeckhauser, 1988; Dinner et al., 2011) or an anchor (Dhingra et al., 2012), transparency could have an impact.[3] More precisely, information regarding the potential influence of the default then increases the awareness of decision makers to the manipulated structure of the decision. This in turn then may cause reactance. Mentioning the purpose of the default and thus justifying its use has the potential to mitigate reactance. However, note that Wilson et al. (1996) observe anchoring effects despite forewarning, suggesting an unintentional and subconscious working mechanism that could also apply to defaults working as anchors. If defaults work as an implicit recommendation (McKenzie et al., 2006), a persuasion attempt (Brown & Krishna, 2004), or a coordination device (Cappelletti et al., 2014) it is less clear whether transparency has an effect. Informing decision makers on the potential influence given their interpretation of the default as a recommendation, persuasion attempt, or coordination device would provide no additional information, because decision makers would already be aware of this potential influence. Mentioning the purpose would increase the salience of the climate protection goal, causing a similar effect as when any of the previous mechanisms is at play.

When analyzing findings with respect to psychological reactance, we hypothesize that trait reactance interacts with the type of transparency accompanying the default value. Specifically, we expect that:

H5: If participants are informed that the default may have an influence on their decision, the default effect for participants with higher trait reactance will be lower than for participants with lower trait reactance.

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[3]Note that the potential impact can vary considerably between these mechanisms, and that it can also be close to zero. The point is that here, as opposed to the case of unawareness, transparency could logically influence the default effect.

We further hypothesize that the evaluation of a default as freedom-threatening, autonomy-decreasing, manipulative, and pressuring (perceived threat to freedom), as well as its potential to elicit negative emotions (anger) differs with respect to the types of transparency accompanying the default value. Specifically, we expect that:

H6: If participants are informed that the default may have an influence on their decision, experience of state reactance will be higher compared to when they are not informed.

We deduce hypotheses H5 and H6 exclusively with respect to a default accompanied by information on its potential influence, because we expect this type of transparency to increase the salience of the potentially manipulative and autonomy-threatening default-characteristic. For the purpose of the default, the conceptual link to reactance is less clear. We therefore abstain from formulating specific hypotheses.

3. Experimental design

The laboratory experiment consisted of five experimental groups, of which one was the control group.[4] We conducted 11 sessions in the Econ-lab of the Erasmus School of Economics at the Erasmus University Rotterdam, the Netherlands, recruited with ORSEE in June 2016, and additional 15 sessions in July 2017 in the WiSo-lab of the University of Hamburg, Germany, recruited with hroot (Bock et al., 2014). A total of 498 students participated in the experiment using the z-tree software (Fischbacher, 2007). Of these, 53.21% were female, the average age was 23.74 years (median: 23 years), and about half (53.01%) studied economics. More information on the differences between samples from both locations, as well as a disaggregated analysis of effect-differences are provided in Appendix B.1.

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[4]Prior to the experiment, pilot sessions were conducted in Germany ($n = 16$), Sweden ($n = 25$), France ($n = 29$) and The Netherlands ($n = 32$). The pilot session in Germany focused on developing the design, which was further improved on and tested among Master students in the Netherlands, Sweden, and Bachelor students in France. The experimental design was not identical in all these pilots. Therefore, findings these sessions are not included in the data analysis.

All participants were randomly assigned to separate computer terminals and were instructed not to communicate. They were given instruction sheets that were read aloud (see Appendix A). All participants received an endowment of 10 Euro and were asked to indicate how much (if any) of their endowment they would like to contribute to the 'climate protection fund'. The remaining amount was their private payoff. After the experiment, they were paid according to their decisions, and contributions were used to retire real carbon licenses from the EU ETS, through donations to 'TheCompensators*'.[5]

In the control group, participants were presented with a text box where they could enter their contribution in any integer amount between 0 and 10 Euro. Neither a preselected default value for the contribution, nor any additional information were presented. In the other experimental groups, subjects encountered an 8 Euro default contribution in form of a button (see Figures A.2 - A.3 in Appendix A). They could either press this button or choose another one that stated 'Different amount'. In the latter case they were referred to another screen that contained exactly the same information but with the addition of a text box where they could insert any amount between 0 and 10 Euro. In three of four default treatments, the default was complemented by a sentence that induced transparency, respectively on the default's potential influence, its purpose, or both. Table 1 shows the exact wording used to provide each type of transparency in the respective treatment group.

[Table 1 about here]

The Default+Info transparency message informs subjects about the fact that they may be (subconsciously) affected by the default value. It resembles the wording by Steffel et al. (2016) which they use in order to deploy a default *ethically*. We expect that this wording stimulates the participants defensive systems against the threat to their behavioral autonomy, potentially motivating reactant behavior. The Default+Purpose transparency message informs subjects about the purpose of the default, i.e. increasing contributions to

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[5]'TheCompensators*' is a non-profit association founded in 2006 by researchers from the Potsdam Institute for Climate Impact Research. They offer a way for individuals and firms to compensate for their emissions. With donations, they buy and retire emission rights from the EU ETS. At the end of the experiment, all participants received an email with a confirmation and a certificate of aggregate experimental donations to 'TheCompensators*'.

the climate protection fund. The wording implies the existence of a default effect, increases the salience of the purpose and, contrary to Default+Info, causes subjects to focus on the goal instead of the fact that it potentially threatens their behavioral autonomy. The Default+Info+Purpose combines both messages. Once subjects made their decision, they received information regarding their contribution, their private payoff and the amount of CO_2 that would be retired with the contributed amount.[6]

After making their decision, participants answered a questionnaire measuring, among others, their attributed importance to climate protection, and their belief in the effectiveness of retiring emission rights as a measure to protect the climate. In order to find out whether reactions to the different types of transparency can be explained by psychological reactance, we have two approaches. First, we assess participants' perception of the default value as freedom threatening, autonomy-decreasing, manipulative, and pressuring, as well as its tendency to evoke negative emotional reactions, such as irritation, anger, annoyance, and aggravation. We refer to this as state reactance (Dillard & Shen, 2005). Second, we measure subjects' proneness to psychological reactance, referred to as trait reactance, with Hong's Psychological Reactance Scale (Hong & Faedda, 1996). Both measures were assessed after subjects made their decision of how much to contribute.[7] Relevant questions are in Appendix C.

After conducting the sessions in Rotterdam, we calculated observed power for the most important tests. For H1, simulated post-hoc observed power analyses produced power coefficients of 0.72, 0.26, 0.51, and 0.46, respectively for Control vs. Default, Control vs. Default+Information, Control vs. Default+Purpose, and Control vs. Default+Info+Purpose. Concerning Findings 2-4, post-hoc observed power analyses for the estimates in

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[6]At that time, "TheCompensators*" offered to retire licenses at a price of 5.53 Euro. Note that this price can be different from the actual spot-price at the time we conducted the experiment, since "TheCompensators*" buy batches of licenses at a specific price and then retire them based on the donations they receive, irrespective of price-changes that appear in the meantime.

[7]We assume that measuring reactance items before treatments would have introduced an "additional nudge" with a potential influence on contributions. Kruskal-Wallis tests and Steel-Dwass-Critchlow-Fligner multiple comparison tests do not show any significant difference between treatments for all state and trait reactance items. This suggests there is no significant effect of treatments. However, we cannot completely exclude a potential common impact of all treatments on reactance.

model (1) produced power coefficients of 0.22, 0.87, 0.95, respectively for Default vs. Default+Information, Default vs. Default+Purpose, and Default+Info+Purpose vs. Default+Information vs. Default+Purpose. In order to further substantiate Finding 2, we conducted additional sessions for the Control group, Default, and Default+Information groups. The number of additional observations based on an a priori power analysis. The simulation suggested that pooling data from all sessions allowed to detect a true difference of roughly 1.15 EUR (Cohen's $d = 0.37$) in mean contributions between the Default and Default+Information group 78.81 % of the time.

4. Results

We present and discuss findings in the following way: First, we demonstrate main results regarding the effectiveness of defaults and their interrelation with transparency. Second, we analyze the measures used to investigate the relevance of psychological reactance to transparency of defaults.

4.1. Default effects

Overall, 498 subjects contributed 1,385.5 Euro to retire carbon licenses, resulting in 2.78 Euro per subject. Of all participants, 68.27% contributed a positive amount, and 9.44% opted for the default value. Table 2 presents summary statistics of the variables divided by experimental groups. Figure 1 presents the respective mean contributions.

[Table 2 about here]

[Figure 1 about here]

A Mann-Whitney test of H1 rejects the null hypothesis of equal contributions between Control vs. Default ($W = 5486, p = 0.001$), Control vs. Default+Info ($W = 4974, p < 0.001$), Control vs. Default+Purpose ($W = 1275, p = 0.032$), and Control vs. Default+Info+Purpose ($W = 1376.5, p = 0.046$). Overall, we find evidence for a default- and pull-effect.

To check robustness of the default effect we focus on contributions as an outcome variable in Tobit regression. The Tobit model accounts for left-censored contributions and allows testing effects on the latent, unobserved contribution variable. This means we assume that at least some subjects would choose to take from instead of contribute to the public good. Thus, we interpret the dependent variable as desired contributions, and indeed even

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damages, to climate protection. This assumption is common in dictator-games and empirically valid (Engel, 2011).

We begin with a restricted model limited to the treatment variable, then add a dummy variable indicating that subjects perceive climate protection to be (very) important, and proceed to add other relevant covariates shown in Table 3. The reason we add importance to protect the climate separately is that a Chi^2-Test rejects the hypothesis that subjects are equally distributed among the treatment groups with respect to this variable ($\chi^2(4) = 34.37, p < 0.001$).

[Table 3 about here]

By controlling for this variable we ensure that estimates of treatment effects are not conditionally biased. Because the questionnaire is taken by subjects after being exposed to treatments, there is a risk of the respective manipulations being the reason for the differences in importance-ratings. Regarding Tobit models in Table 4, un-restricted model (3) includes all covariates, i.e. rating of the importance of climate protection, gender, age, no previous experience with experiments, judgment of buying emission licenses from the EU ETS as an ineffective tool for climate protection, and a location dummy.

Model (1) predicts that a mere default, a default plus info, and a default plus its purpose lead to higher average contributions compared to no default. The effect of Default+Info+Purpose is marginally significant. When controlling for subjects' perception of the importance of climate protection in model (2), coefficients change. This results in significance for Default+Info+Purpose. *Importance of CP* positively predicts the latent contribution variable. A likelihood-ratio test suggests that model (2) fits the data significantly better than model (1) ($\chi^2(1) = 33.09, p < 0.001$). Controlling for additional covariates increases precision of the estimated average treatment effects. A likelihood-ratio test suggests that un-restricted model (3) fits the data significantly better than restricted model (2) ($\chi^2(5) = 66.40, pp < 0.001$).

F1: There is a default effect on contributions for a default, a default plus information, a default with added purpose, as well as for a default with both types of transparency.

[Table 4 about here]

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4.2. Influence of transparency on default effectiveness

A Kruskal-Wallis test for equal contribution distributions in the treatment groups is not significant ($H(3) = 0.484, p = 0.922$). So are respective pairwise comparisons with Dunn's test (not reported). Consequently, there is no evidence for either of H2, H3, and H4.

As above, we augment our analysis by focusing on contributions in stepwise Tobit-regression (Table 4). In un-restricted model (3), an omnibus Wald-test for equality of parameter estimates for Default, Default+Info, Default+Purpose, and Default+Info+Purpose does not lead us to reject the null hypothesis ($F(3, 488) = 0.49, p = 0.692$). The same holds for the restricted models. There is no evidence of unequal contributions in the treatment groups. Consequently, there is no evidence that transparency significantly reduces contributions.[8]

F2: Informing participants that the default may have an influence on their decision does not significantly decrease contributions compared to when they are not informed.

F3: Informing participants about the default's purpose does not significantly increase contributions compared to when they are not informed.

F4: Informing participants that the default may have an influence on their decision, as well as of the default's purpose does not decrease or increase contributions, compared to the other types of transparency (including no transparency at all).

Of the additional covariates, *Gender* and *EU ETS not effective* are significant. Being male, as well as judging the EU ETS as not effective to protect the climate, negatively predict the latent outcome variable. The former finding is consistent with evidence from dictator games (Engel, 2011). Findings on gender differences in public good games are ambiguous, however (Croson & Gneezy, 2009). In the context of real contributions to climate protection, evidence by Diederich & Goeschl (2014), while suggesting that female subjects are less indifferent to climate protection, do not support a

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[8]Estimated treatment-effects of un-restricted regression models are plotted in Appendix B (Figures B.5, B.6, and B.7).

higher willingness to pay for emission certificates of women. Findings with respect to age somewhat align with those of Borghans & Golsteyn (2015) who find, in a less restricted sample, that the default effect does vary with age. However, at around 22 years (the mean of our sample) they find a relatively large default effect. This may explain why we find a default effect, but no effect of age.

4.3. Psychological reactance and transparency

To test if reactions towards the combination of a default value with different types of transparency can be explained by psychological reactance, we measured subjects' proneness to experience psychological reactance.[9]

Specifically, we test whether subjects' reactions towards different types of transparency accompanying the default differ depending on subjects' trait reactance. Therefore, we run regressions with an interaction term of the treatment variable and the trait reactance index. The latter is centered on the mean, so that treatment-main-effects are meaningful (Table 4). Note that this regression excludes observations from the control group. For reasons of brevity, we focus on the main effects of trait reactance, as well as on interaction-effects.

As in previous Tobit models, model (5) fits the data better than model (4) ($\chi^2(1) = 28.42, p < 0.001$), and model (6) fits the data better than model (5) ($\chi^2(4) = 50.11, p < 0.001$). We find no significant main effect of trait reactance, nor do we find that the different types of transparency and the trait reactance index interact significantly for any of the three model-specifications. In other words, there is no evidence that the effect of different types of transparency on average contributions is conditional on subjects' trait reactance.

F5: The influence of information on the default effect does not depend on the level of trait reactance of participants.

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[9]To create an index for trait reactance, we constructed dummy variables for each of the 14 items of the scale, which are equal to 1 when the subject responded with "Agree" or "Strongly agree" to the respective question, 0 otherwise. We then added the dummies for each subject to create the index, which ranges from zero to 14. Findings are consistent for trait reactance included as a (un-weighted) factor-based score.

In order to test whether reactions to different types of transparency can be explained by psychological reactance, we create an index for each of the two state reactance-categories, i.e. for the perceived threat to freedom and the anger-category.[10]

We model the log odds of subjects being in a higher level of each of both ordinal indexes on all explanatory variables used above (Table 5). Note that this regression excludes observations from the control group since subjects in this group were not presented with the default option which they could rate. None of the coefficients modeling treatment effects are significant.[11]

F6: Combining the default with information about its potential behavioral influence does not increase participants' experience of state reactance.

Age negatively predicts experienced anger triggered by the default value. The finding that experiencing negative emotions decreases with age is known in the literature (e.g. Charles et al., 2001).

[Table 5 about here]

Both approaches that are linking different types of transparency of a default to psychological reactance suggest that subjects neither perceive a default value differently based on the type of transparency accompanying it, nor does their inherent propensity to show psychological reactance change the way they react to these different types of transparency.

5. Discussion and conclusion

The experiment advances the discussion of nudges and transparency by providing empirical evidence on the effect of transparency on the performance of a pro-environmental default value. Despite the widespread application of nudges, many researchers and consumers are concerned of the potentially

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[10]We constructed a dummy-variable, which is equal to 1 when the subject "agreed" or "strongly agreed", resp. replied with "to some extent" or "very" to the respective statements, for each item (see Appendix C). Then, we added the respective dummies in each category, to form two indexes, each ranging from zero to four. Findings are consistent for when both dependent variables are included as (un-weighted) factor-based scores in linear OLS-regression.

[11]This finding is consistent with non-parametric tests for differences of individual items of the scales (not reported).

manipulative nature of behavioral interventions. In democratic societies, public authorities are expected to be transparent with regard to their actions and intentions. Therefore, covertly 'exploiting' people's psychological biases potentially inhibits perceived legitimacy, and ultimately effectiveness of such policies. The most straightforward solution to this problem is to instruct policy-makers to disclose information regarding the potential influence of the nudge, and its purpose. However, this suggestion raises the concern that nudges will no longer be effective. As expressed by Bovens (2009), nudges "work best in the dark". The results of this study suggest that this concern might be overstated.

The experiment provides evidence that defaults increase contributions to climate protection even when complemented by disclosure regarding the potential influence of the default, its purpose, or both. Furthermore, there is no evidence that information on the potential behavioral influence and/or purpose of the default triggers psychological reactance. Likewise, there is no evidence that subjects differing in their proneness to experience reactance also differ in how they react towards the default with additional information.

These findings suggest that despite the initial concern over the inhibiting influence of transparency, nudges in the form of defaults can be transparent and at the same time effective. In order to preserve the effect of defaults and increase the legitimacy of behaviorally informed policies, policy makers should be transparent about their motives, as well as the potential behavioral influence of the instrument. The motive and how it is perceived by the decision maker has been found to matter for advice (Kuang et al., 2007).

Our findings replicate and add to previous evidence on the influence of transparency. Loewenstein et al. (2015) and Kroese et al. (2016) reported that pro-self defaults were effective in health contexts even after disclosing information about them. Our study extends this conclusion to pro-social nudges, a type that is widely used in the context of public policy-making. Moreover, we extend findings of Steffel et al. (2016) by examining the influence of transparency in a more realistic setting where participants' decisions have an actual consequence for them, and for the environment. Findings are also useful for the private sector and NGOs aiming to include nudges in their inventory to increase contributions to environmental protection, and possibly other public goods, e.g. charity.

Although several recent studies link nudges to psychological reactance, they do so either indirectly, or they deal with hypothetical and attitudinal, instead of behavioral outcomes (Haggag & Paci, 2014; Arad & Rubinstein,

2017; Loewenstein et al., 2015; Hedlin & Sunstein, 2016). By measuring both state and trait reactance, we enable a more direct way of assessing the interaction of psychological reactance with the influence of transparency on the effectiveness of a default value. To our best knowledge, Goswami & Urminsky (2016) is the only study that assesses the interaction of trait reactance with the size of a default value on behavioral outcomes, i.e. charitable giving. They find no significant interaction effect. On a more general level, our findings, in line with theirs, suggest that psychological reactance plays a minor or no role with respect to behavioral effects of defaults, and, in our case, transparency. In fact, a possible explanation of this might be the relatively high default value, which is 80 % of the experimental endowment. Instead of eliciting psychological reactance, such a high default might lead subjects to ignore it altogether.

Findings suggest that the default value is an effective way of increasing individual voluntary contributions to climate protection. Increased aggregate contributions are consistent with inertia, as well as anchoring. A higher fraction of participants picking the default value instead of specifying another amount in the default, compared to the control group, supports the inertia/ effort reduction explanation. However, deviation costs in the experiment are marginal (the subject had to make two mouse-clicks, as well as to type in the contribution amount, instead of just making one mouse click on the default button), and contributing the default value is also consistent with an anchoring explanation: Subjects may choose the default value not only because of inertia, but also because they consider this value first and only then employ reasons against it, conditional on what they wanted to contribute initially. This anchoring-explanation is consistent both with picking the default and moving towards the default, whereas inertia is only consistent with the former behavior (Dhingra et al., 2012).

We observe that subjects who contribute a positive amount do contribute more on average, when there is a default value with either type of transparency, but the differences to the control group are not significant. Additionally, we observe an increase of subjects giving a positive amount due to the default, which is consistent with the anchoring explanation. Together, our findings suggest that increased aggregate contributions in the default groups are due to an increase of the fraction of subjects contributing, as well as of an increase of the fraction of subjects choosing the default value, but not because of increased average contributions of subjects that contribute. Inertia, as well as anchoring may therefore both be reasons for why we ob-

serve default effects. Intuitively, we would expect anchoring to play a more pronounced role in real world applications of pro-environmental nudges, especially if defaults result in repeated and/or significant financial costs. For someone who highly values environmental- and climate protection, deviating from a default, which may be perceived as conveying information about social norms, can incur non-financial costs, especially if he or she aims to uphold a positive self-image. Maintaining a positive self image, as well as being consistent with social norms, can be achieved by decreasing (not necessarily closing) the gap between default value and initially intended contribution. Note that our design does not allow to unambiguously identify the underlying mechanisms causing the default effect in the experiment. Anchoring is consistent with the interpretation of the default value as an implicit recommendation, a persuasion attempt, coordination device, or a reference point. If a decision maker regards the default as an implicit recommendation, she may consequently increase/decrease her donation relative to her preferences, after seeing the default. However, we cannot identify whether she interpreted the default as a recommendation.

Furthermore, while being able to differentiate between the effects of different types of transparency is insightful for policy-makers, the difference between the information and purpose treatments is not analytically clear.[12] Communicating the purpose of the default implicitly reveals that the default is expected to have an effect on individual decision making, without spelling it out. Still, we think that the findings concerning this type of transparency are important for practical purposes.

Further research could evaluate the role of trait reactance on how subjects respond to different types of transparency for different types of nudges, i.e. social norms or framing. Additionally, building on the shortcoming of our experimental design, further studies should further investigate the link between transparency and the different underlying working mechanisms of defaults and other types of nudges. Since our experiment has a rather limited amount of subjects, field experiments can establish statistically more powerful findings for interaction effects. Due to a more realistic context, a field experimental approach would also increase external validity. Nevertheless, our experiment is less abstract than a 'regular' laboratory experiment due to the fact that contributions have a real effect on climate protection

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[12]We thank an anonymous reviewer for this remark.

Harrison & List (2004). The current study focuses on one type of nudge, and a specific context. Further research is needed in order to determine the overall influence of transparency on the effectiveness of nudges. Moreover, results might be context-specific, thus requiring further investigation into pro-social nudges. Delving into the welfare implications of transparency can also become a promising research endeavor (Sunstein, 2015).

Overall, our findings advance the understanding of how nudges in general, and defaults specifically, affect individual behavior with social consequences, and how policy-makers can increase their transparency without limiting their effectiveness.

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Table 1: Experimental design

| Experimental group	Default value	Transparency information
Control	No	No information
Default	8 Euro	No information
Default+Info	8 Euro	"Please consider that the preselected default value might have an influence on your decision."
Default+Purpose	8 Euro	"Please consider that the preselected default value is meant to encourage higher contributions for the climate protection fund."
Default+Info+Purpose	8 Euro	"Please consider that the preselected default value might have an influence on your decision. This is meant to encourage higher contributions for the climate protection fund."

Notes: The table reports the experimental group, the respective default value presented to participants, as well as the respective transparency information as it was shown to the subjects.

Table 2: Descriptive statistics of all outcome variables to assess the default effect

Group	Contri-bution Mean	SD	Con-tributed Mean	Picked default Mean	n
Control	1.82	2.66	51.76	0	85
Default	2.95	2.98	70.76	12.28	171
Default+Info	3.04	2.98	74.07	8.02	162
Default+Purpose	2.92	3.19	71.79	15.38	39
Default+Info+Purpose	2.85	2.95	65.85	17.07	41

Notes: The table reports summary statistics (means and standard deviations) of different outcome variables, as well as the number of subjects per experimental group. Outcome variables are: contributions to the climate protection fund, the percentage of subjects contributing a positive amount, as well as the percentage of subjects contributing the default value.

Figure 1: Mean contributions per experimental group
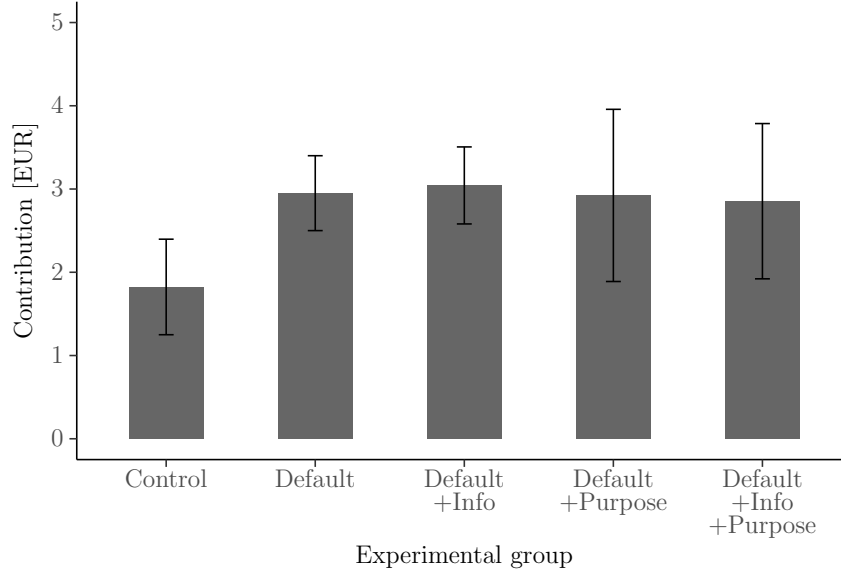
Notes: The figure shows mean contribution levels in the experimental groups. Error bars represent 95% confidence intervals.

Table 3: Descriptive statistics of covariates

Experimental group	Age		Gender (Male)	Impor- tance of CP	No exp. Exp- erience	EU ETS not effective
	Mean	SD	Mean	Mean	Mean	Mean
Control	23.75	4.94	48.24	76.47	23.53	60
Default	24.16	4.29	43.27	82.46	29.82	60.23
Default+Info	23.92	4.53	45.06	88.27	25.93	56.79
Default+Purpose	22.28	4.65	53.85	51.28	20.51	64.1
Default+Info+Purpose	22.68	3.72	58.54	63.41	19.51	58.54

Notes: The table reports summary statistics (means and standard deviations) of different covariates per experimental group. Covariates are: age of participants, percentage of males, percentage of subjects perceiving climate protection as (very) important, percentage of subjects without prior experience with experiments, as well as the percentage of subjects judging license retirement as an ineffective mean for climate protection.

Table 4: Stepwise Tobit-models with and without interaction term

	(1) Contribution	(2) Contribution	(3) Contribution	(4) Contribution	(5) Contribution	(6) Contribution
Default	1.868**	1.718**	1.659**			
	(0.587)	(0.571)	(0.539)			
Default+Info	2.056***	1.758**	1.670**	0.165	0.0152	0.00216
	(0.586)	(0.577)	(0.538)	(0.438)	(0.429)	(0.410)
Default+Purpose	1.866*	2.612**	2.528**	-0.0343	0.858	0.841
	(0.839)	(0.845)	(0.784)	(0.730)	(0.750)	(0.775)
Default+Info+Purpose	1.628^x	1.921*	1.896*	-0.260	0.169	0.174
	(0.829)	(0.779)	(0.779)	(0.726)	(0.670)	(0.756)
Importance of CP		2.806***	2.350***		2.810***	2.353***
		(0.517)	(0.502)		(0.558)	(0.534)
Gender (Male)			-1.045**			-1.065**
			(0.353)			(0.391)
Age			-0.0406			-0.0200
			(0.0403)			(0.0431)
No exp. Experience			-0.577			-0.522
			(0.425)			(0.451)
EU ETS not effective			-2.512***			-2.329***
			(0.347)			(0.368)
Hamburg			-0.0494			-0.102
			(0.453)			(0.504)
React				-0.0897	-0.0977	-0.0783
				(0.106)	(0.102)	(0.0971)
Default+Info × React				-0.108	-0.109	-0.0764
				(0.145)	(0.141)	(0.133)
Default+Purpose × React				0.183	0.208	0.114
				(0.276)	(0.285)	(0.250)
Default+Info+Purpose × React				0.0646	0.0316	-0.0483
				(0.224)	(0.206)	(0.190)
Constant	0.357	-1.824**	1.986^x	2.259***	-0.0734	3.072**
	(0.497)	(0.644)	(1.094)	(0.314)	(0.563)	(1.100)
Sigma	3.969***	3.848***	3.591***	3.888***	3.766***	3.550***
	(0.152)	(0.153)	(0.143)	(0.153)	(0.152)	(0.147)
Observations	498	498	498	413	413	413
Log Pseudolikelihood	-1088.416	-1071.872	-1038.671	-929.4	-915.187	-890.107
F	(4, 494)=3.33	(5, 493)=8.64	(10, 488)=13.19	(7, 406)=0.76	(8, 405)=3.98	(13, 400)=7.36
Prob > F	0.010	<0.001	<0.001	0.624	<0.001	<0.001
Pseudo R^2	0.007	0.022	0.052	0.002	0.018	0.044

Notes: The table reports estimates of Tobit models with contributions censored at 0 as the dependent variable, with and without interaction terms. Robust standard errors are in brackets. *Default+Info, Default+Purpose*, and *Default+Info+Purpose* denote the respective treatment group, with *Default* as the base category. *React* measures subjects' proneness to experience reactance in a metric scale, and is mean centered. *Def+Inf × React, Def+Pur × React*, and *Def+Inf+Pur × React* are interaction terms of the transparency type with proneness to experience reactance. *Importance of CP* is a dummy that takes the value 1 if the subject perceives climate protection as (very) important. *Gender* takes the value 1 if the subject is male. *Age* denotes the age of the subject. *No exp. Experience* is a dummy which takes the value 1 if a subject did not participate in another experiment before. *EU ETS not effective* is a dummy that takes the value 1 when a subject judges license retirement as an ineffective mean for climate protection. *Hamburg* takes the value 1 if the subject is from the Hamburg, as opposed to the Rotterdam sample. Significance levels: ^{x} ($p < 0.10$), * ($p < 0.05$), ** ($p < 0.01$), *** ($p < 0.001$).

Table 5: Ordered logistic model of state reactance

	(1) Threat To Freedom	(2) Anger
Default+Info	-0.00294	-0.167
	(0.199)	(0.223)
Default+Purpose	-0.0297	0.0868
	(0.418)	(0.453)
Default+Info+Purpose	-0.0686	-0.560
	(0.330)	(0.470)
Importance of CP	-0.0275	-0.334
	(0.232)	(0.276)
Male	-0.0798	-0.300
	(0.190)	(0.217)
Age	-0.0594**	-0.0832**
	(0.0183)	(0.0268)
Participated	-0.0221	-0.0560
	(0.192)	(0.242)
EU ETS not effective	0.183	0.173
	(0.191)	(0.216)
Hamburg	-0.0120	-0.325
	(0.250)	(0.260)
Cut 1	-3.125***	-2.029**
	(0.528)	(0.683)
Cut 2	-2.270***	-1.126^x
	(0.524)	(0.679)
Cut 3	-1.088*	-0.251
	(0.517)	(0.685)
Cut 4	0.346	0.508
	(0.525)	(0.718)
Observations	413	413
Log Pseudolikelihood	-640.583	-443.190
Wald Chi^2(9)	12.96	19.80
Prob > Chi^2	0.165	0.019
Pseudo R^2	0.008	0.024

Notes: The table reports estimates of ordered logit models with ratings of defaults as threatening to freedom, and anger arousing as the respective dependent variable. Robust standard errors are in brackets. *Default+Info*, *Default+Purpose*, and *Default+Info+Purpose* denote the respective treatment group, with *Default* as the base category. *Importance of CP* is a dummy that takes the value 1 if the subject perceives climate protection as (very) important. *Gender* takes the value 1 if the subject is male. *Age* denotes the age of the subject. *No exp. Experience* is a dummy which takes the value 1 if a subject did not participate in another experiment before. *EU ETS not effective* is a dummy that takes the value 1 when a subject judges license retirement as an ineffective mean for climate protection. *Hamburg* takes the value 1 if the subject is from the Hamburg, as opposed to the Rotterdam sample. Significance levels: ^{x} ($p < 0.10$), * ($p < 0.05$), ** ($p < 0.01$), *** ($p < 0.001$).

Appendix A. Experimental design

Instructions

Welcome and thank you very much for participating in this experiment. This experiment is about decision-making. Please read the following instructions carefully. Everything that you need to know in order to participate in this experiment is explained below. If you have any difficulties in understanding these instructions please raise your hand and I will come to you. Please note that communication between participants is strictly prohibited during the experiment. Communication between participants will lead to the exclusion from the experiment. The experimental procedure will be as follows. You will receive 10 Euro. Please decide how much of the 10 Euro you would like to spend on climate protection. You can choose freely how much, if any, you contribute to climate protection (whole numbers between 0-10). Should you decide to contribute, we will realize your contribution to climate protection by buying and retiring carbon emission licenses from the European Union Emissions Trading System (EU ETS) at the end of the experiment (please read the respective paragraph below for a description). By this, you have the possibility to make a real contribution to climate protection. The rest of the money is your private pay-out that you will receive in cash at the end of the experiment.

After making the decision you will be kindly asked to complete a short questionnaire. Please note that your decisions in this experiment are anonymous and will not be revealed at any stage to the other participants. (If relevant) a confirmation of the aggregated real payment to the climate protection fund will be sent to all participants at the end of the whole experiment.

The Climate Protection Fund

If a person wants to protect the climate, emitting climate gases such as CO_2 should be avoided. But it is possible to do even more: Individuals can buy and delete emission certificates from the EU Emission Trading System (ETS) through certified organizations and NGOs. By doing so, a private person reduces the amount of CO_2 which can be emitted by European industries, protects the environment and ensures that the development of climate-friendly technologies is accelerated. In this experiment, the participants' contributions to the climate protection fund will be used to buy real carbon dioxide (CO_2) emission licenses on the market of the European Union Emissions Trading Scheme (EU ETS) via the website "TheCompensators.org". It is

one example of an NGO that allows ordinary people to directly participate in the EU ETS scheme, and where they can make decisions on CO_2 reductions.

The following table shows how much kilograms of carbon you reduce with your payment, and how much money you receive for yourself. The far right row indicates the respective amount of reduced CO_2 relative to a Dutch citizens' average of 9163 kg of CO_2 emitted per year.

Payment to retire CO_2-allowances	Private payout €	CO_2 abated [kg]	Share of average emissions per year per person [%]
0	10 €	0	0%
1	9 €	181	2%
2	8 €	362	4%
3	7 €	542	6%
4	6 €	723	8%
5	5 €	904	10%
6	4 €	1,085	12%
7	3 €	1,266	14%
8	2 €	1,447	16%
9	1 €	1,627	18%
10	0 €	1,808	20%

For example, with a payment of 3 Euro to retire carbon licenses, you retire 542 kg CO_2. This corresponds to approximately 6% of the average emissions per capita per year of a Dutch person. As a private pay-out you get 7 Euro. With a payment of 8 Euro to retire carbon licenses, you retire 1,447 kg CO_2. This corresponds to approximately 16% of the average emissions per capita per year of a Dutch person. As a private pay-out you get 2 Euro.

Figure A.2: Experimental screen for Control

Notes: The figure shows the decision screen shown to participants in the Control group. They could choose any integer between 0 and their endowment of 10 EUR. By clicking on the red OK button, subjects went to the next screen, providing them with information about the consequences of their decision, i.e. their payoff, their contribution, as well as kg of CO_2 offset.

Figure A.3: Experimental screen for Default + transparency

Notes: The figure shows the decision screen shown to participants in the Default groups. They could choose to contribute the default value of 8 EUR by clocking on the respective red button, or they could click on the button below to choose any other amount. The transparency message was written where indicated in the figure. The following screen provided subjects with information about the consequences of their decision, i.e. their payoff, their contribution, as well as kg of CO_2 offset.

Appendix B. Statistical analyses

Figure B.4: Distribution of contributions
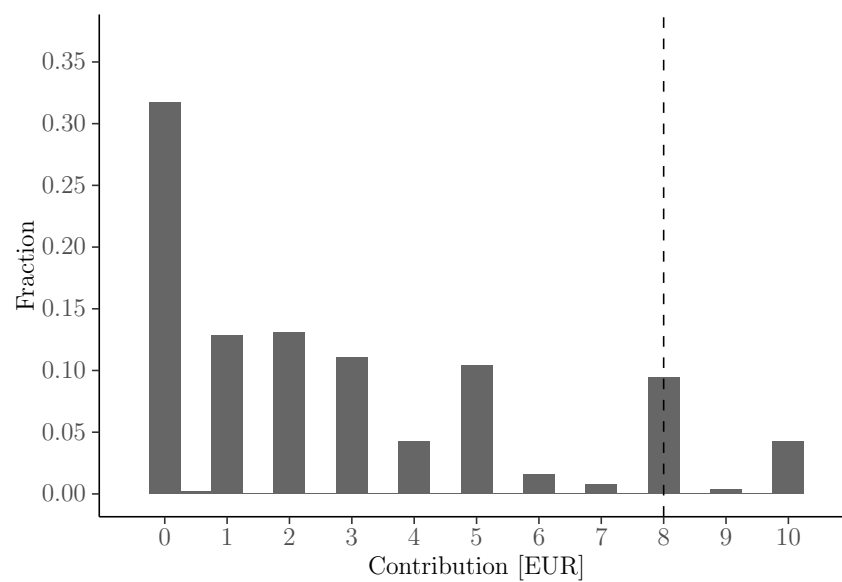
Notes: Shows the distribution of contribution amounts, more precisely the fraction of subjects contributing the respective amount. The dashed line indicates the default value.

Table B.6: P-values for pairwise MW tests of Contribution

|	Control	Default	Default +Info	Default +Purpose
Default	**0.001**			
Default+Info	<**0.001**	0.665		
Default+Purpose	**0.032**	0.843	0.591	
Default+Info+Purpose	**0.046**	0.785	0.606	0.91

Notes: P-values of pairwise Mann-Whitney tests for equality of distributions of contributions to the climate protection fund. Comparisons are indicated by the treatment names provided in the first column and first row, respectively. Significance levels: $p < 0.05$ in bold, $p < 0.1$ in cursive.

Figure B.5: Default and transparency effects on contributions for different base-categories
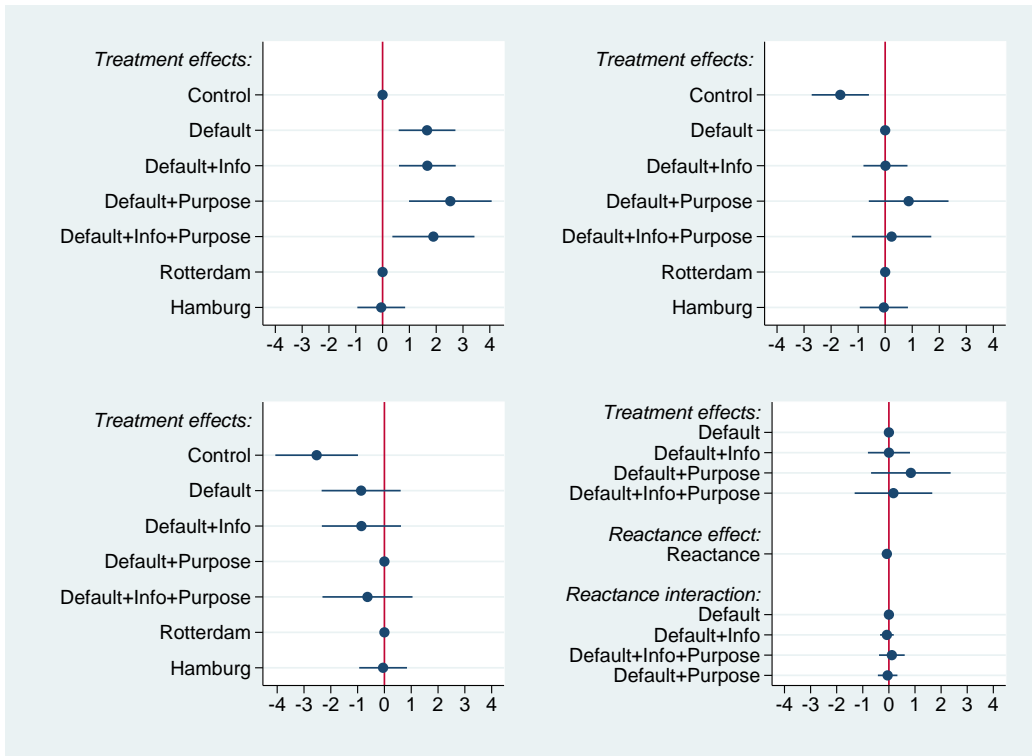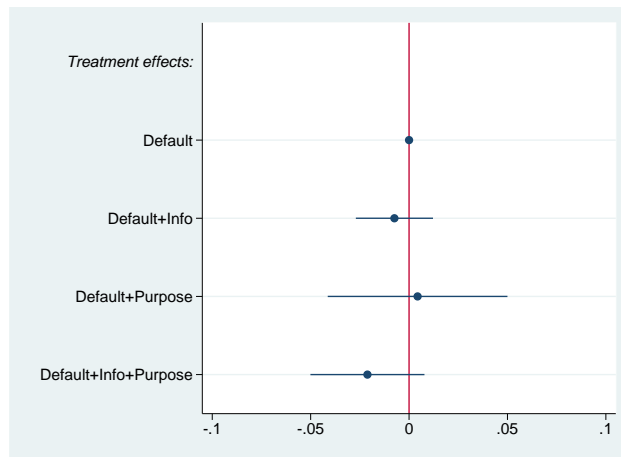

Notes: The figure graphically depicts results from some of the findings from the Tobit models. Dots with horizontal lines indicate point estimates with 95% confidence intervals. Dots on the zero line without confidence intervals denote the reference category. Models (3) and (8) in Table 4 display the underlying regression results. The top left panel refers to finding F1, the top right panel to F2 and F3, the bottom left panel to F4, and the panel on the bottom right to F6. Covariates are not shown.

Figure B.6: Default and transparency effects on perceived Threat to freedom
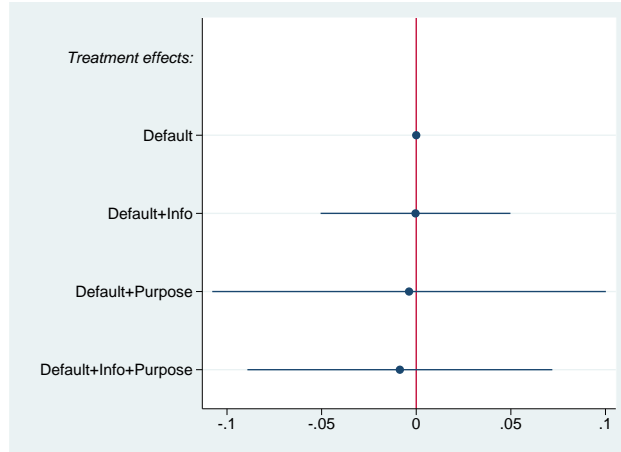
Notes: Dots with horizontal lines indicate point estimates with 95% confidence intervals from marginal effects of ordered logistic models. Dots on the zero line without confidence intervals denote the reference category. Model (4) in Table 5 displays the underlying regression results (albeit not showing marginal effects). It refers to finding F5. Covariates are not shown.

Figure B.7: Default and transparency effects on Anger

Notes: Dots with horizontal lines indicate point estimates with 95% confidence intervals from marginal effects of ordered logistic models. Dots on the zero line without confidence intervals denote the reference category. Model (5) in Table 5 displays the underlying regression results (albeit not showing marginal effects). It refers to finding F5. Covariates are not shown.

Appendix B.1. Comparing subjects from Rotterdam and Hamburg

We conducted experimental sessions in two different cities. Findings from the first eleven experimental sessions relied on data solely from Rotterdam, while additional observations where gathered in Hamburg primarily in order to increase the reliability of the null result presented in F2 (and to a minor degree F3-F4 by increasing the n in the control group). The number of additional observations gathered in Hamburg relied on an a priori power analysis. Based on this analysis we conducted additional sessions to gather 284 additional observations for the Control, Default, and Default+Info groups. The experimental protocol in all sessions was identical.

Table B.7 shows summary statistics of the main outcome variables disaggregated by treatment and location of the experiment. Contribution distributions in the Control ($W = 795.5, p = 0.329$), Default ($W = 3053.5, p = 0.528$), and Default+Info ($W = 2119.5, p = 0.092$) groups do not differ by location. The same is true for the remaining outcome variables. Figure B.8 shows the mean contributions disaggregated by location and treatments, including bars indicating 95 % confidence intervals. Mann-Whitney tests indicate that, while the default effect is significant in the Rotterdam sample ($W = 707.5, p = 0.007$), but not the Hamburg sample ($W = 2040.5, p = 0.074$), this is reversed with respect to the Default+Info effect, which is significant in Hamburg ($W = 1732.5, p = 0.009$), but not in Rotterdam ($W = 769.5, p = 0.084$). Differences between Default and Default+Info are insignificant in both samples (R: $W = 1113, p = 0.302$; H: $W = 6799, p = 0.24$)

Table B.8 shows summary statistics of the covariates included in the regression models disaggregated by treatment and location of the experiment. Aggregated over treatments, participants in Hamburg are on average older than participants in Rotterdam ($M = 24.94(SD = 4.81)$ vs. $M = 22.16(SD = 3.45), t(494.84) = -7.517, p < 0.001$), less likely to be male ($M = 39.08$ vs. $M = 57.01, \chi^2(1) = 15.038, p < 0.001$), and also have a different distribution of study areas ($\chi^2(6) = 156.65, p < 0.001$). Additionally, participants in Hamburg are more likely than their Rotterdam colleagues to rate climate protection as (very) important ($\chi^2(1) = 37.06, p < 0.001$). They do not differ with respect to prior experience in experiments ($chi^2(1) = 0.16, p = 0.69$) or their views regarding the effectiveness of the EU ETS ($\chi^2(1) = 0.002, p = 0.961$).

Aggregated over location, subjects are not balanced among treatments according to some variables. Subjects' ratings of the importance of climate protection correlate with the treatment ($\chi^2(4) = 34.37, p < 0.001$). So does

age ($H(4) = 16.294, p = 0.003$), and the distribution of study areas ($\chi^2(6) = 156.65, p < 0.001$).

Figure B.9 shows standardized effect sizes and 95 % confidence intervals of the relevant pairwise comparisons for which we gathered additional data. While the effect size of the default effect (Con vs. Def) included zero in the Hamburg sample, it does not include zero in the Rotterdam- and the aggregate sample. The default+info effect size (Con vs. Def+Inf) is different from zero in the Hamburg and aggregated sample, but not in the Rotterdam sample. Although the standardized effect sizes for the Def vs. Def+Inf comparison is opposite between Hamburg and Rotterdam, neither those nor the aggregated sample exclude an effect size of zero. Figure B.10 shows the regression coefficients and 95 % confidence intervals from Tobit model (3). These are qualitatively similar to the respective effect sizes, with the exception that the standardized effect size for the Con vs. Def comparison in Hamburg includes zero, whereas this is not the case for the respective regression coefficient.

Table B.7: Descriptive statistics of all outcome variables by experimental group and location

|		Contri-bution		Con-tributed	Picked default	n
Group	Location	Mean	SD	Mean	Mean	
Control	R	1.67	2.68	46.67	0	45
Control	H	2	2.66	57.5	0	40
Default	R	3.24	3.21	73.91	19.57	46
Default	H	2.84	2.9	69.6	9.6	125
Default+Info	R	2.49	2.95	67.44	6.98	43
Default+Info	H	3.24	2.98	76.47	8.4	119
Default+Purpose	R	2.92	3.19	71.79	15.38	39
Default+Info+Purpose	R	2.85	2.95	65.85	17.07	41

Notes: The table reports summary statistics (means and standard deviations) of different outcome variables, as well as the number of subjects per experimental group. Outcome variables are: contributions to the climate protection fund, the percentage of subjects contributing a positive amount, as well as the percentage of subjects contributing the default value. Statistics are disaggregated by experimental group and location of the experiment.

Table B.8: Descriptive statistics of covariates by experimental group and location

| Group	Location	Age Mean	Age SD	Gender (Male) Mean	Impor-tance of CP Mean	No exp. Exp-erience Mean	EU ETS not effective Mean
Control	R	21.8	3.08	60	57.78	31.11	57.78
Control	H	25.95	5.7	35	97.5	15	62.5
Default	R	22.02	2.79	60.87	78.26	30.43	60.87
Default	H	24.95	4.48	36.8	84	29.6	60
Default+Info	R	22.07	2.96	51.16	79.07	20.93	53.49
Default+Info	H	24.59	4.81	42.86	91.6	27.73	57.98
Default+Purpose	R	22.28	4.65	53.85	51.28	20.51	64.1
Default+Info+Purpose	R	22.68	3.72	58.54	63.41	19.51	58.54

Notes: The table reports summary statistics (means and standard deviations) of different covariates per experimental group. Covariates are: age of participants, percentage of males, percentage of subjects perceiving climate protection as (very) important, percentage of subjects without prior experience with experiments, as well as the percentage of subjects judging license retirement as an ineffective mean for climate protection. Statistics are disaggregated by experimental group and location of the experiment.

Figure B.8: Mean contributions by experimental group and location
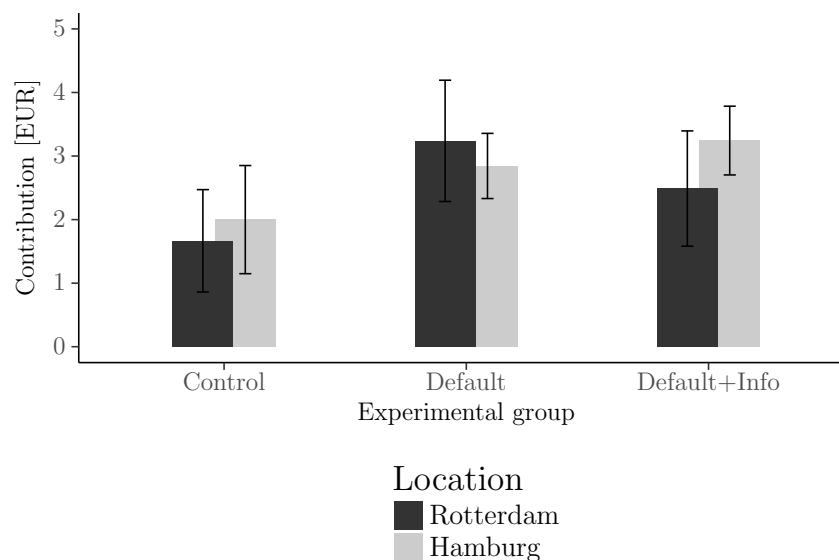
Notes: Shows mean contributions by experimental group and location, including 95 % confidence intervals.

Figure B.9: Effect sizes by location and for aggregated data
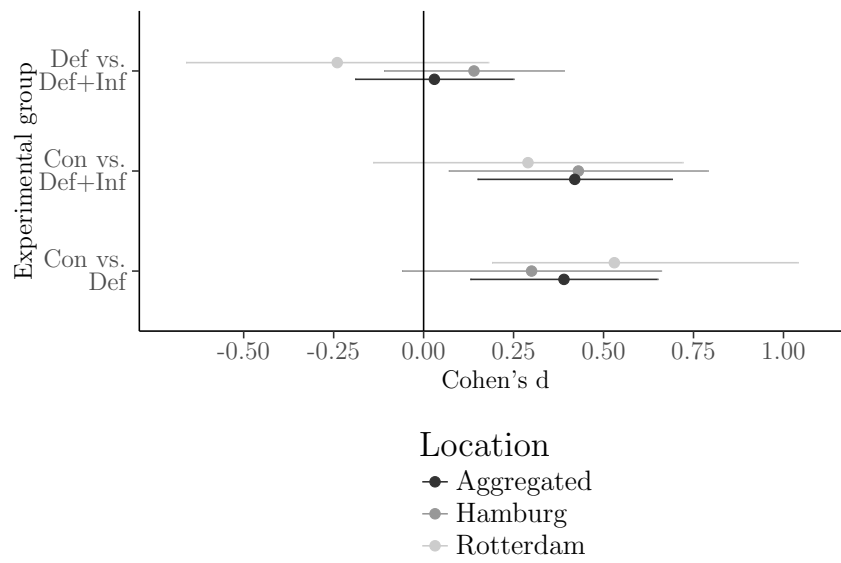
Notes: Shows Cohen's d for each pairwise comparison for which additional data in Hamburg was gathered, including the 95 % confidence intervals.

Figure B.10: Coefficients from tobit model by location and for aggregated data
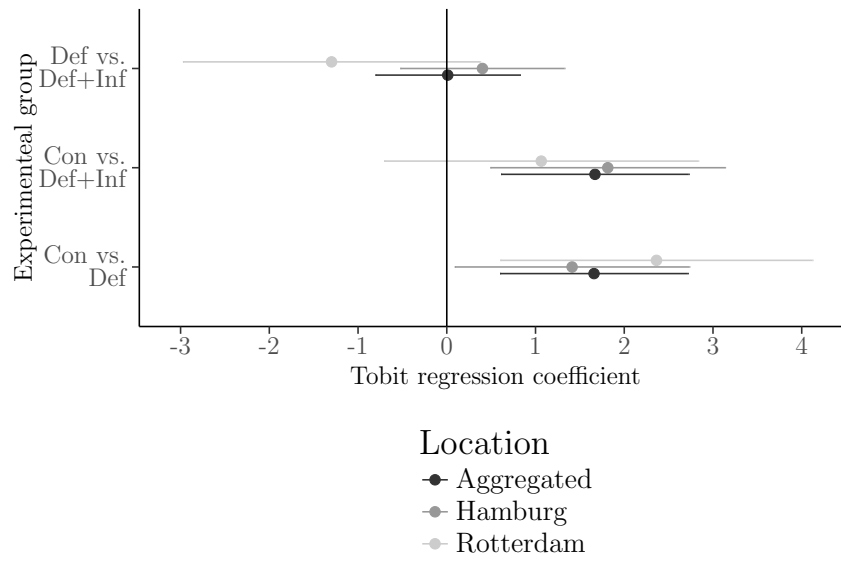
Notes: Shows estimated coefficients from Tobit model (3) for effect for which additional data in Hamburg was gathered, including the 95 % confidence intervals.

Appendix C. Questionnaire

Questionnaire on covariates

What is you gender? O Male O Female

What is your age?

Have you participated in other experiments before today? O Yes O No

How important is climate protection for you? Please circle the most suitable answer.
O Not important at all O Not important O Indifferent O Important O Very important

Do you think that buying real carbon dioxide (CO_2) emissions licenses on the market of the European Union Emissions Trading Scheme (EU ETS) is an effective method to contribute to climate protection? O Yes O No

Questionnaire on state reactance

Please indicate to what extent do you agree with the following statements on a 5-point response scale that ranges from the statement "strongly disagree" to the statement "strongly agree". (Perceived threat to freedom)

- The default value threatened my freedom to choose.

- The default value tried to make a decision for me.

- The default value tried to manipulate me.

- The default value tried to pressure me.

Please indicate to what extent do you agree with the following statements on a 5-point response scale that ranges from the statement "Not at all" to the statement "Very". (anger)

- Please indicate how irritated you were with regard to the given default value.

- Please indicate how angry you were with regard to the given default value.

- Please indicate how annoyed you were with regard to the given default value.

- Please indicate how aggravated you were with regard to the given default value.

Questionnaire on trait reactance

Please indicate to what extent do you agree with the following statements on a p-point response scale that ranges from the statement "strongly disagree" to the statement "strongly agree".

- Regulations trigger a sense of resistance in me.

- I find contradicting others stimulating.

- When something is prohibited, I usually think, "that's exactly what I am going to do".

- The thought of being dependent on others aggravates me.

- I consider advice from others to be an intrusion.

- I become frustrated when I am unable to make free and independent decisions.

- It irritates me when someone points out things, which are obvious to me.

- I become angry when my freedom of choice is restricted.

- Advice and recommendations usually induce me to do just the opposite.

- I am content only when I am acting on my own free will.

- I resist the attempts of others to influence me.

- It makes me angry when another person is held up as a role model for me to follow.

- When someone forces me to do something, I feel like doing the opposite.

- It disappoints me to see others submitting to standards and rules.