

# Can Nudges Be Transparent and Yet Effective?

- Draft October 2016 -

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## Abstract

*'Nudges' receive growing attention as an effective strategy to alter people's decisions without significantly changing economic incentives or limiting options. However, being often very subtle and covert, nudges are also criticized as being unethical. By not being transparent about influencing the individual 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 limiting 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 option increases contributions, and additional information regarding the potential influence combined with the purpose of the default, or just its purpose, does not significantly affect the level of contributions. Nevertheless, the findings are somewhat inconclusive with respect to information solely regarding the potential behavioral influence. Furthermore, we do not find evidence that psychological reactance interrelates with the influence of transparency on the effectiveness of defaults. Therefore, our findings generally support the policy-relevant claim that nudges (in the form of defaults) can be transparent and yet effective.*

## Keywords

Carbon reduction, Experiment, Defaults, Nudging, Transparency, Psychological reactance

## JEL classification

D03, H41, Q58, K23

## **1 Introduction**

Nudges, a term coined by Thaler and Sunstein (2008), describe diverse instruments that utilize behavioral insights in order to affect individual behavior, without limiting options or significantly changing economic incentives. They have become a popular form of soft regulation in various fields such as health, finance, environmental protection, etc. (Sunstein, 2014a; Alemanno and Sibony, 2015; World Bank, 2015; Lourenco et al., 2016). Despite growing popularity, use of behavioral insights in policy-making is subject to criticism (e.g. Hausman and Welch, 2010; Rebonato, 2014). One remarkable aspect of nudges is that they often influence individual behavior without being noticed by the affected subject (Dhingra et al., 2012; Hansen and 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). Thus, this form of regulation lacks the transparency that accompanies other regulatory instruments. For instance, when the government imposes a tax to reduce consumption of a product (e.g. cigarettes), 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 different psychological biases, often without people's awareness (Hansen and Jespersen, 2013). Felsen et al. (2013) demonstrated in a vignette study that a significant proportion of individuals have reservations towards nudges they perceive as covert. Another recent research 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 problem we investigate whether nudges can be made transparent without reducing their effectiveness. In this context, two major points have to be taken into account. On the one hand, the covert nature of nudges is often said to be essential for their effectiveness (Bovens, 2009; House of Lords Report, 2011). On the other hand, telling people that the nudge is used to influence their decision potentially evokes a perceived threat to their freedom leading to 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 and Brehm, 2013, 37). This could not only inhibit the effect of the nudge but even lead to the opposite effect

than the one intended. Therefore, this psychological phenomenon is important when investigating the influence of transparency on the effectiveness of nudges.

To test the interrelation between nudges and transparency we conduct a laboratory experiment where subjects are asked how much they would like to contribute to a climate protection fund. The nudge in the experiment is a default value that aims to increase contributions. The default value is expected to increase the level of 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.<sup>1</sup> The type of transparency that accompanies the default varies across treatments and includes either informing decision makers about its potential behavioral influence and/or informing them about its purpose to increase contributions to climate protection. We assess two different measures of psychological reactance after the experiment. Thus, we can investigate 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 can 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 and Rubinstein (2015) 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<sup>2</sup> show that, although there is widespread support for nudges, transparency concerning the nudge will not diminish its effectiveness.

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<sup>1</sup> There are different mechanisms through which a default influences behavior, e.g. as a reference value and anchor, through provision of social norms or information, or through inertia (by imposing pecuniary or cognitive costs on decisions to deviate from the default). Sunstein and Reisch (2016) provide a review.

<sup>2</sup> In their recent article, Reisch and Sunstein (2016) demonstrate that there is also a general support of nudges in Europe (based on a survey of six selected countries).

To the best of our knowledge, there are 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<sup>3</sup> 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 the pro-self default has any effect. Steffel et al. (2016), in several hypothetical and marginally incentivized consumer-related experiments, find no evidence that stressing the potential behavioral influence of a pro-self, as well as pro-social, default impacts their effectiveness, although it affects perception by the consumer.

While existing evidence unanimously suggests that impact of transparency on effectiveness of nudges is absent, our research augments this in various ways. First, 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 is a reason to expect that informing decision makers about the potential behavioral influence of a nudge has different consequences than informing them about its purpose (and both at the same time). Second, subjects in our experiment face a tradeoff between real monetary payoffs and real contributions to a (global) public good. Previous research either employed highly abstract and stylized environments, or did not demand subjects to make real financial tradeoffs. Third, we enrich our analysis with the concept of psychological reactance, allowing for a deeper understanding of channels through which transparency might influence default effects. Recent research on nudges, although focusing on the role of reactance (Arad and Rubinstein, 2015; Hedlin and Sunstein, 2016), did not investigate its interaction with transparency.

Consequently, we contribute to knowledge on the topic of transparency of nudges in the following ways. First, we enable a more nuanced view on this topic by investigating two types of transparency, thus contributing to a better understand 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 trans-

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<sup>3</sup> Hagman et al. (2015) divide nudges into pro-self and pro-social nudges. While the former nudge people towards making better decisions for themselves, the latter nudge people towards behavior benefiting society as a whole.

parency 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 information regarding the purpose of the default, or the purpose *and* its potential influence. It is not clear, however, how sole disclosure of the potential influence of a default impacts its effectiveness. In addition, contributions in the treatment groups (with or without transparency) do not significantly differ from each other. Finally, we neither find evidence that trait reactance interacts with transparency, nor does data suggest 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 effective even when their nature and purpose is disclosed to the target group.

The remainder of the article is structured as follows. In Section 2 we discuss psychological reactance as a theoretical background to the problem of 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 Theoretical 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 reactance may result in behavioral and cognitive efforts to reestablish freedom as well as uncomfortable, hostile, aggressive, and angry feelings (Dillard and Shen, 2005). This means that 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 devalue the source of threat (the initiator of the nudge), upgrade the restricted freedom, and counter-argue against the imposed option (e.g. Brehm, 1966; Dillard and 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 has an influence on the effectiveness of pro-social nudges (in the form of defaults) we chose the context of environmental protection. With climate change being one of the major challenges faced by society

on a global scale today, information-based instruments and nudges become increasingly important to increase individual contributions to climate and environmental protection (Allcott and Mullainathan, 2010; Araña and León, 2013; World Bank, 2015).

One way to contribute to climate protection is to offset (parts of) one's own yearly CO<sub>2</sub> emissions by donating to specific charitable organizations (in the experiment, referred to as 'climate protection fund'). These organizations use donations to purchase and destroy carbon emission licenses from the European Union Emissions Trading Scheme (EU ETS)<sup>4</sup>. Buying carbon 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 a pure hint on 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 thusly increase the default effect. Finally, accompanying the default with both types of information will be the most transparent form of the nudge. Due to the combined "downside" effect of the reactance and "upside" effect of the saliency of the positive goal 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 decrease compared to when they are not informed.

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<sup>4</sup> The EU ETS is a European market that ultimately prices carbon emissions and allows regulated 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.

H3: If participants are informed of the purpose of the default, contributions will increase 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.

When analyzing psychological reactance, we 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:

H5: If participants are informed that the default may have an influence on their decision, experience of state reactance will increase compared to when they are not informed.

We further hypothesize that trait reactance interacts with the type 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, the default effect for participants with higher trait reactance will be lower than for participants with lower trait reactance.

### **3 Experimental design**

To test hypotheses, we conduct a laboratory experiment<sup>5</sup> consisting of five experimental groups, of which one is the control group. A total of 214 students from the Erasmus University Rotterdam participated in the experiment in June 2016 using the z-tree software (Fischbacher, 2007). The experiment took place in the Econ-lab of the Erasmus School of Economics where participants were randomly assigned to separate computer terminals and instructed not to communicate. The participants were given instruction sheets that were read aloud (see Appendix A). All participants received an endowment of 10 € and were asked to indicate how much (if any) of their endowment

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<sup>5</sup> Prior to the experiment, pilot sessions were conducted in Germany, Sweden, France and the Netherlands. 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 from the pilot-sessions are not included in the data analysis.

they would like to contribute to the ‘climate protection fund’. The remaining amount served as their private payoff. After the experiment, they were paid accordingly to their decisions, and the contributions were used to buy and destroy real carbon licenses from the EU ETS, particularly through ‘TheCompensators\*’<sup>6</sup>.

**Table 1: Experimental design**

Experimental group	Default Value	Transparency Information
Control	No	No information
Default	8€	No information
Default + Info	8€	<i>“Please consider that the preselected default value might have an influence on your decision.”</i>
Default + Purpose	8€	<i>“Please consider that the preselected default value is meant to encourage higher contributions for the climate protection fund.”</i>
Default + Info + Purpose	8€	<i>“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.”</i>

In the control group, participants were presented with a text box where they could enter their contribution in any integer amount between zero to 10€. Neither a preselected default value for the contribution, nor any additional information were presented (see Figures A.1 - A.5 in Appendix A). In the other experimental groups, subjects encountered an 8 € default contribution in form of a button. They could either press this button or choose another button 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 zero to 10 €. Once subjects made their decision, they received information regarding their contribution, their private payoff and the amount of CO<sub>2</sub> that could be retired with the

<sup>6</sup> ‘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 the donations, they buy and retire emission rights and delete allowances (EUAs) from the EU ETS. At the end of the entire experiment, all participants received an email with a confirmation and a certificate of aggregate experimental contributions to ‘TheCompensators\*’.

contributed amount<sup>7</sup>. The treatment groups varied only with respect to the displayed information in the decision screen (see Table 1).

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 treatment-differences of 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 and Shen, 2005). Second, we measure subjects' proneness to psychological reactance, referred to as trait reactance, with Hong's Psychological Reactance Scale (Hong and Faedda, 1996). Both measures were assessed after subjects made their decision of how much to contribute.<sup>8</sup> The different questions to evaluate reactance are in Appendix C.

## 4 Results

We present and discuss findings in the following way. First, we demonstrate our 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 theory to transparency of defaults.

### 4.1 Default effects

Overall, 214 subjects contributed 562 € to retire carbon licenses, resulting in 2.63 € per subject. The average distance of contributions to the default value was 5.54 €<sup>9</sup>. Of all participants, 64.95 % contributed a positive amount, and 11.68 % opted for the default value.

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<sup>7</sup> At that time, "TheCompensators\*" offered to retire licenses at a price of 5.53 €. 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.

<sup>8</sup> 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 data.

<sup>9</sup> Positive and negative distances to the default are treated equally. Thus, e.g., contributions of 6 € and 10 € are both interpreted as a distance of 2 €.

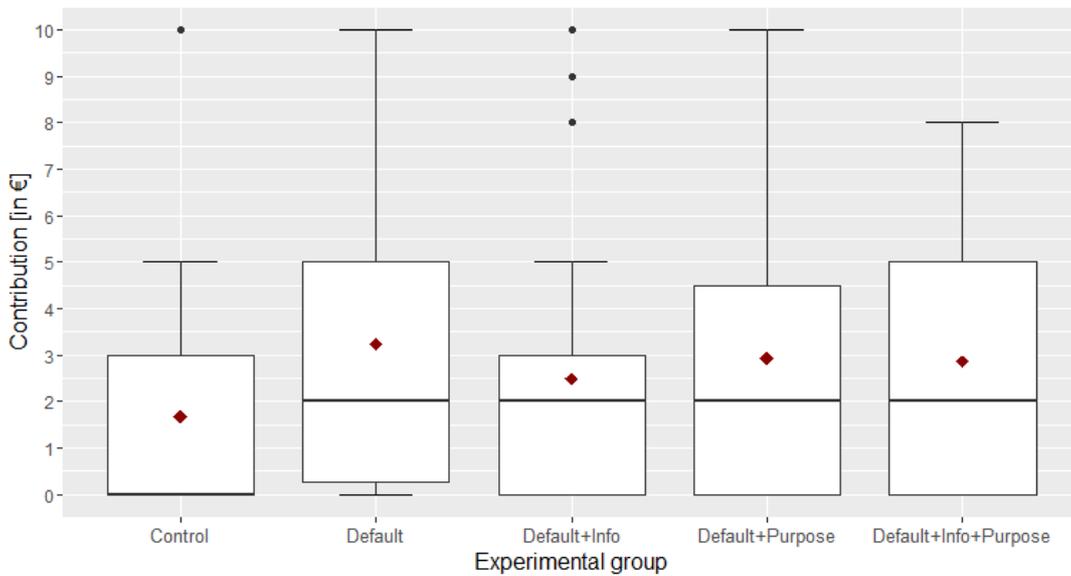
Table 2 presents summary statistics of the variables divided by experimental groups.

Figure 1 presents the respective boxplots.

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

Experimental group	Variable and respective statistic						n
	Contribution		Distance		Con-tributed	Picked default	
	Mean	SD	Mean	SD	%	%	
Control	1.67	2.68	6.6	1.91	46.67	0	45
Default	3.24	3.21	4.89	3.01	73.91	19.57	46
Default+Info	2.49	2.95	5.74	2.45	67.44	6.98	43
Default+Purpose	2.92	3.19	5.28	2.83	71.79	15.38	39
Default+Info+Purpose	2.85	2.95	5.15	2.95	65.85	17.07	41

**Figure 1: Contributions per experimental group**



Notes: The bold black horizontal lines denote the respective median contributions. The lower, resp. upper bounds of the white boxes are the first (Q1), resp. third (Q3) quartiles, i.e. the 25<sup>th</sup> and 75<sup>th</sup> percentiles. The distance between both bounds measures the interquartile range (IQR). The “whiskers” end at the lowest observed value that is within  $Q1 - 1.5 \times IQR$ , resp. the highest observed value within  $Q3 + 1.5 \times IQR$ . Observations that are outside of these whiskers are defined as outliers, shown as black points. Diamonds show the respective mean.

We focus our analysis of treatment effects on two outcome-categories: contributions, and the convergence of decisions toward the default value. Within the first dimension we consider two variables, i.e. frequency of positive contributions and the level of contributions. Within the second dimension we consider each contribution’s distance

to the default value, and the frequency of contributions of this value. Our hypotheses specifically refer to treatment differences of aggregated contributions, but 'default-effects' can be further differentiated. By looking at these distinct outcome variables, we allow for a deeper understanding of potential treatment-effects.

We use non-parametric tests because the boxplots identify outliers, and Shapiro-Wilk normality tests indicate that contributions, as well as distances to the default value, are not normally distributed ( $W = 0.806$ ,  $p = 0$  for contributions;  $W = 0.819$ ,  $p = 0$  for distances). This is caused primarily by a large amount of zero contributions, or distances of 8 € to the default value.

Testing H1 with a Mann-Whitney test rejects the null hypothesis of equal contributions at 5% significance between Control vs. Default ( $W = 707.5$ ,  $p = 0.007$ ), Control vs. Default+Purpose ( $W = 642$ ,  $p = 0.028$ ), and Control vs. Default+Info+Purpose ( $W = 687$ ,  $p = 0.033$ ).

We reject H1 in case of a mere default, as well as in case of a default accompanied by its purpose for all outcome variables. P-values for all outcome variables are in Appendix B, Tables B.1 – B.4. In case of a default with information on its potential influence, the difference of contributions ( $W = 769.5$ ,  $p = 0.084$ ), distances to the default value ( $W = 1177.5$ ,  $p = 0.067$ ), as well as the fraction of subjects contributing ( $\chi^2(1) = 3.068$ ,  $p = 0.080$ ) are only marginally significant at 10%. We cannot reject the hypothesis that subjects in the Default+Info group pick the default value as often as subjects in the control group. In case of a default accompanied by both types of transparency, differences to the control group are significant for all outcome variables except for the fraction of subjects donating.

Overall, we find evidence for H1, i.e. that there is a default- and pull-effect. However, evidence is limited for a default combined with information on its potential influence on behavior.

The pull-effect becomes tangible when looking at Figure 1 and Table 2. More than half of the participants in the control group contributed zero, with no contributions between 6 € and 9 €. In all treatment groups, the fraction of subjects contributing zero is lower, and more subjects choose the default value than in the control group. Together with findings outlined above, this suggests that an overall increase of average contributions in the Default, as well as Default+Purpose groups is partially due to an

increase in the fraction of subjects contributing, as well as a higher fraction of subjects choosing the default, or values near the default. In the Default+Info group, we find a marginally significant increase in the extensive margin, whereas in the Default+Info+Purpose group we find a higher fraction of people picking the default value.

For a robustness check of the default effect we focus on contributions as an outcome variable in regression. 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 then proceed to add other relevant covariates (Table 3). The reason we add importance to protect the climate separately is that a Chi<sup>2</sup>-Test rejects the hypothesis that subjects are equally distributed among the treatment groups with respect to this variable (see Table B.5 in Appendix B). The test is marginally significant for Control vs. Default ( $\chi^2(1) = 3.503, p = 0.061$ ), and for Control vs. Default+Info ( $\chi^2(1) = 3.666, p = 0.056$ ). By controlling for this variable we ensure that estimates of average treatment effects are not conditionally biased. In theory, since covariates come from a questionnaire that is taken by subjects *after* being exposed to treatments, there is the risk of the respective manipulations being the reason for the differences in importance-ratings. However, we argue that, in case of endogeneity, we would expect this difference also be significant for the remaining two treatment groups. This is not the case, i.e. Control vs. Default+Purpose ( $\chi^2(1) = 0.142, p = 0.706$ ) and Control vs. Default+Info+Purpose ( $\chi^2(1) = 0.098, p = 0.754$ ).<sup>10</sup> We therefore judge controlling for perceived importance of climate protection as an adequate mean to estimate unbiased treatment effects.

**Table 3: Stepwise Tobit-Regression models**

	Contribution		
	(1)	(2)	(3)
Default	2.706 <sup>**</sup> (0.962)	2.229 <sup>*</sup> (0.938)	2.363 <sup>**</sup> (0.892)
Default+Info	1.703 <sup>x</sup> (0.983)	1.233 (0.977)	1.065 (0.898)
Default+Purpose	2.307 <sup>*</sup> (1.007)	2.510 <sup>*</sup> (0.984)	2.518 <sup>**</sup> (0.910)
Default+Info+Purpose	2.058 <sup>*</sup> (0.996)	1.863 <sup>x</sup> (0.951)	1.946 <sup>*</sup> (0.913)

<sup>10</sup> Additionally, calculating VIFs for independent variables of the full regression model indicates that multicollinearity is not an issue, with the largest VIF being lower than 10. Furthermore, a Durbin-Wu-Hausman test suggests that endogeneity of *Importance of CP* is not an issue.

Importance of CP		2.485 <sup>***</sup>	2.166 <sup>**</sup>
		(0.673)	(0.660)
Gender (Male=1)			-1.659 <sup>*</sup>
			(0.551)
Age			-0.0518
			(0.0816)
No exp. Experience			0.345
			(0.673)
EUETS not effective			-2.400 <sup>***</sup>
			(0.569)
Constant	-0.129	-1.595 <sup>x</sup>	2.013
	(0.737)	(0.824)	(1.919)
Pseudo-R <sup>2</sup>	0.01	0.03	0.05
Sigma	4.160 <sup>***</sup>	4.020 <sup>***</sup>	3.777 <sup>***</sup>
	(0.250)	(0.254)	(0.229)
<i>N</i>	214	214	214

Standard errors in parentheses

<sup>x</sup>  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Regarding Tobit models in Table 3, un-restricted model (3) includes all covariates, i.e. rating of the importance of climate protection, gender, age, no previous experience with experiments, and judgment of buying emission licenses from the EU ETS as an ineffective tool for climate protection. The Tobit model accounts for left-censored contributions. This means we assume that at least some subjects would choose to take from instead of contribute to the public good. This assumption is common in dictator-games and empirically valid (see Engel, 2011).

Model (1) finds that a mere default, a Default+Purpose, as well as a default plus both types of transparency predict higher average contributions. The effect of a Default+Information is only marginally significant. When controlling for subjects' perception of the importance of climate protection in model (2), all coefficients, except for that of Default+Purpose, decrease. This results in non-significance for Default+Info. As argued above, we judge these coefficients to be conditionally unbiased, and therefore more appropriate in order to estimate average treatment effects. The effect of importance on the outcome is highly significant. A likelihood-ratio test suggests that the un-restricted model fits the data significantly better ( $\chi^2(1) = 14.47$ ,  $p = 0$ ). 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) ( $\chi^2(4) = 26.34$ ,  $p = 0$ ). A default, as

well as a default supplemented by its purpose, increase average conditional contributions to the public good by 2.36 € and 2.52 €, respectively, compared to no default.

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

#### 4.2. Influence of transparency on default effectiveness

We proceed testing H2 – H4. A Kruskal-Wallis test for equal contributions in the treatment groups is insignificant ( $H(3) = 1.117, p = 0.773$ ). So are respective pairwise comparisons with Dunn’s test (not reported). Contributions in the treatment groups do not significantly differ. The same holds for distances to the default value ( $H(3) = 1.459, p = 0.692$ ), the fraction of subjects choosing to contribute ( $\chi^2(3) = 0.860, p = 0.835, \text{Chi}^2\text{-Test}$ ), as well as the fraction of subjects choosing the default value ( $p = 0.346, \text{Fisher's exact test}$ ). Consequently, for neither of the four outcome variables we find significance in the differences suggested by H2, i.e. between Default and Default+Info; H3, i.e. between Default+Purpose and Default; and H4, i.e. between Default+Purpose, Default+Info+Purpose, and Default+Info, even though effects go into the hypothesized direction (see Table 2).

As above, we augment our analysis by focusing on contributions in stepwise tobit-regression (Table 3). 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 allow us to reject the null hypothesis at any conventional significance level ( $F(3, 205) = 1.14, p = 0.333$ ). The same holds for restricted models. We find no evidence for unequal contributions in the treatment groups. Consequently, we find no evidence that transparency significantly reduces contributions.

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).

Regarding the additional covariates, *gender* and *EUETS not effective* are significant. Being male, as well as judging the EU ETS as *not* effective to protect the climate, decrease average contributions. The former finding is consistent with evidence from dictator games (Engel, 2011). Findings on gender differences in public good games are ambiguous, however (Croson and Gneezy, 2009). In the context of real contributions to climate protection, evidence by Diederich and Goeschl (2014), while suggesting that female subjects are less indifferent to climate protection, do not support a higher willingness to pay for emission certificates of women.

### 4.3 Psychological reactance and transparency

In order to find out 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.<sup>11</sup>

We model the log odds of subjects being in a higher level of each of both ordinal indexes on all explanatory variables used above. Regression results are presented in Table 4. Note that this regression excludes observations from the control group since subjects in this group were not presented with the default option.

**Table 4: Ordered logistic model of state reactance**

	Perceived threat to freedom	Anger
	(4)	(5)
Default+Info	0.0933 (0.401)	-0.342 (0.395)
Default+Purpose	-0.00976	-0.0155

<sup>11</sup> We constructed a dummy-variable, which is 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.

	(0.456)	(0.454)
Default+Info+Purpose	-0.0443	-0.588
	(0.357)	(0.478)
Gender (Male=1)	-0.142	-0.199
	(0.303)	(0.328)
Age	-0.0368	-0.0912**
	(0.0373)	(0.0422)
No exp. Experience	-0.00944	-0.0802
	(0.303)	(0.420)
EUETS not effective	-0.0180	0.0365
	(0.321)	(0.331)
Importance of CP	-0.129	-0.349
	(0.308)	(0.352)
Cut1	-2.712***	-2.178**
	(0.917)	(1.016)
Cut2	-1.927**	-1.559
	(0.894)	(1.032)
Cut3	-0.739	-0.792
	(0.870)	(1.021)
Cut4	0.513	-0.203
	(0.857)	(1.047)
Pseudo-R <sup>2</sup>	0.003	0.02
<i>N</i>	169	169

Standard errors in parentheses

<sup>x</sup>  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

None of the coefficients modeling treatment effects are significant.<sup>12</sup>

F5: Combining the default with information about its potential behavioral influence does not increase participants' experience of state reactance.

We find a significant negative influence of age on 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).

To further test if reactions towards the combination of a default value with different types of transparency can be explained by psychological reactance, we measure subjects' proneness to experience psychological reactance. To create an index for trait reactance, we proceeded similarly to the case of state reactance.<sup>13</sup>

<sup>12</sup> This finding is consistent with non-parametric tests for differences of individual items of the scales (not reported).

<sup>13</sup> We constructed dummy variables for each of the 14 items of the scale, which are 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.

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 treatment variable and trait reactance index. The latter is centered on the mean, so that treatment-main-effects are meaningful. Regression results are presented in Table 5. Note that this regression excludes observations from the control group. For reasons of brevity, we focus on main effects of trait reactance, as well as on interaction-effects.

**Table 5: Stepwise Tobit-models with interaction term**

	Contribution		
	(6)	(7)	(8)
Default+Info	-0.914 (0.880)	-0.914 (0.868)	-1.217 (0.832)
Default+Purpose	-0.648 (0.907)	-0.381 (0.849)	-0.462 (0.858)
Default+Info+Purpose	-0.448 (0.901)	0.204 (0.916)	0.115 (0.889)
Reactance	-0.0743 (0.220)	-0.119 (0.211)	-0.0397 (0.210)
Default+Info x Reactance	-0.249 (0.276)	-0.219 (0.269)	-0.193 (0.254)
Default+Purpose x Reactance	0.166 (0.338)	0.227 (0.340)	-0.111 (0.278)
Default+Info+Purpose x Reactance	0.0502 (0.298)	0.0597 (0.279)	0.0687 (0.316)
Importance of CP		2.431** (0.741)	2.120** (0.718)
Gender (Male=1)			-1.597* (0.648)
Age			0.0231 (0.0775)
No exp. Experience			0.485 (0.735)
EUETS not effective			-2.258*** (0.593)
Constant	2.615*** (0.616)	0.721 (0.844)	2.627 (1.763)
Pseudo R <sup>2</sup>	0.005	0.02	0.05
Sigma	4.022*** (0.244)	3.890*** (0.246)	3.686*** (0.232)
<i>N</i>	169	169	169

Standard errors in parentheses

<sup>x</sup>  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

As in previous Tobit models, model (7) fits the data better than model (6) ( $\chi^2(1) = 11.47, p = 0$ ), and model (8) fits the data better than model (7) ( $\chi^2(4) = 19.71, p = 0$ ). 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, the effect of different types of transparency on average contributions is *not* conditional on subjects' trait reactance.

F6: The influence of information on the default effect does not depend on the level of trait reactance of participants.

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**

This study contributes to the discussion of nudges and transparency by providing empirical evidence on this issue. Despite the widespread application of nudges, some people are concerned of the potentially 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 might have an impact on 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 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. Our experiment provides evidence that defaults increase contributions to climate protection even when complemented by a disclosure regarding the potential influence of the default and its purpose, or just its purpose. However, it is not clear whether informing subjects only of the potential influence of the default has an impact on contributions.

Furthermore, we find no evidence that additional information on the potential behav-

ioral influence and/or purpose of the default triggers psychological reactance. Likewise, we find 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. However, the type of transparency might matter. In order to preserve the effect of defaults and increase the legitimacy of behaviorally informed policies, policy makers should be transparent about their motives.

Our findings support and add to previous evidence on the influence of transparency. Loewenstein et al. (2015) and Kroese et al. (2016) found that pro-self defaults were effective in the health-care context 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 contribute to the study by 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 useful also for the private sector and NGOs aiming to include nudges in their repertoire to increase contributions to environmental protection, and possibly other public goods, e.g. charity.

Although several recent studies link nudges to of psychological reactance, they do so either indirectly, or they deal with hypothetical and attitudinal, instead of behavioral outcomes (Haggag and Paci, 2014; Arad and Rubinstein, 2015; Loewenstein et al., 2015; Hedlin and Sunstein, 2016). By measuring both state and trait reactance, we enable a more direct way to assess the role of psychological reactance for the influence of transparency on the effectiveness of a default value. To our best knowledge, Goswami and 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 lesser or no role with respect to behavioral effects of defaults, and, in our case, transparency.

Further research could evaluate the role of trait reactance in how subjects respond to different types of transparency for different types of nudges, i.e. social norms or framing. 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 could also increase the external validity of respective findings. Nevertheless, our experiment is less abstract than a ‘regular’ laboratory experiment due to the fact that contributions have a real effect on climate protection (Harrison and 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.

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

### **Acknowledgments**

We would like to thank Christoph Engel, Olexandr Nikolaychuk, Oliver Kirchkamp, and other participants of the IMPRS Summer School 2015, Jena, for their valuable comments. In addition, we would like to thank the participants of the Behavioural Insights in Research and Policy Making SABE/IAREP Conference 2016, Wageningen, and a guest lecture at the faculty of Governance and Global Affairs, Leiden University, for useful suggestions. We also thank Claudia Schwirplies and Jaroslaw Kantorowicz for their helpful remarks. All possible mistakes remain our own.

Funding: This work was supported by the ESL Innovation Program, Erasmus University Rotterdam, Netherlands. Experimental design, analysis and interpretation of data, writing the report, as well as the decision to submit the article for publication were decided on solely by the authors of this paper.

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## 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<sub>2</sub> 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<sub>2</sub> 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<sub>2</sub>) 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<sub>2</sub> 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<sub>2</sub> relative to a Dutch citizens' average of 9163 kg of CO<sub>2</sub> emitted per year.

Payment to retire CO <sub>2</sub> -allowances	Private payout €	CO <sub>2</sub> 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<sub>2</sub>. 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<sub>2</sub>. 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.1: Experimental screen for Control**

Periode 1 von 1 Verbleibende Zeit (sac): 44

You are given 10 Euros.  
Please decide how much of your 10 Euros you would like to allocate to the climate protection fund.

My contribution to the climate protection fund:

**Figure A.2: Experimental screen for Default**

Periode 1 von 1 Verbleibende Zeit (sac): 53

You are given 10 Euros.  
Please decide how much of your 10 Euros you would like to allocate to the climate protection fund.

My contribution to the climate protection fund:

I would like to choose a different amount:

**Figure A.3: Experimental screen for Default+Info**

Periode 1 von 1 Verbleibende Zeit (sec): 50

**You are given 10 Euros.**

**Please decide how much of your 10 Euros you would like to allocate to the climate protection fund.**

**My contribution to the climate protection fund:**

**I would like to choose a different amount:**

*Please consider that the preselected default value might have an influence on your decision.*

**Figure A.4: Experimental screen for Default+Purpose**

Periode 1 von 1 Verbleibende Zeit (sec): 55

**You are given 10 Euros.**

**Please decide how much of your 10 Euros you would like to allocate to the climate protection fund.**

**My contribution to the climate protection fund:**

**I would like to choose a different amount:**

*Please consider that the preselected default value is meant to encourage higher contributions for the climate protection fund.*

**Figure A.5: Experimental screen for Default+Info+Purpose**

Periode

1 von 1

Verbleibende Zeit (s): 50

**You are given 10 Euros.**

**Please decide how much of your 10 Euros you would like to allocate to the climate protection fund.**

**My contribution to the climate protection fund:**

**I would like to choose a different amount:**

*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.*

## Appendix B: Statistical analyses

Table B.1: P-values for pairwise MW tests of *Contribution*

	Control	Default	Default +Info	Default +Purpose
Default	<b>0.007</b>			
Default+Info	<i>0.084</i>	0.302		
Default+Purpose	<b>0.028</b>	0.625	0.635	
Default+Info+Purpose	<b>0.033</b>	0.544	0.637	0.910

Table B.2: P-values for pairwise MW tests of *Distance*

	Control	Default	Default +Info	Default +Purpose
Default	<b>0.003</b>			
Default+Info	<i>0.067</i>	0.224		
Default+Purpose	<b>0.018</b>	0.575	0.569	
Default+Info+Purpose	<b>0.020</b>	0.627	0.507	0.988

Table B.3: P-values for pairwise Chi<sup>2</sup>-tests of *Contributed*

	Control	Default	Default +Info	Default +Purpose
Default	<b>0.015</b>			
Default+Info	<i>0.080</i>	0.662		
Default+Purpose	<b>0.035</b>	1	0.851	
Default+Info+Purpose	0.116	0.558	1	0.740

*Chi<sup>2</sup>-Test with Yate's continuity correction.*

Table B.4: P-values for pairwise Fisher exact-tests of *Picked default*

	Control	Default	Default +Info	Default +Purpose
Default	<b>0.003</b>			
Default+Info	0.113	0.121		
Default+Purpose	<b>0.008</b>	0.777	0.297	
Default+Info+Purpose	<b>0.004</b>	0.790	0.190	1

*Fisher's exact test for count data.*

Table B.5: Descriptive statistics of covariates

Experimental group	Variable and respective statistic					
	Age		Gender (Male)	Importance of CP	No exp. Experience	EUETS not effective
	Mean	SD	%	%	%	
Control	21.8	3.1	60.0	57.8	31.1	57.8
Default	22.0	2.8	60.9	78.3	30.4	60.9
Default+Info	22.0	3.0	51.2	79.1	20.9	53.5
Default+Purpose	22.3	4.7	53.8	63.4	19.5	58.5
Default+Info+Purpose	22.7	3.7	58.5	51.3	20.5	64.1

## **Appendix C: Questionnaire**

### **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 5-point response scale that ranges from the statement – “strongly disagree” to the statement – “strongly agree”.

1. Regulations trigger a sense of resistance in me.
2. I find contradicting others stimulating.
3. When something is prohibited, I usually think, “that’s exactly what I am going to do”.
4. The thought of being dependent on others aggravates me.
5. I consider advice from others to be an intrusion.
6. I become frustrated when I am unable to make free and independent decisions.
7. It irritates me when someone points out things, which are obvious to me.
8. I become angry when my freedom of choice is restricted.
9. Advice and recommendations usually induce me to do just the opposite.
10. I am content only when I am acting on my own free will.
11. I resist the attempts of others to influence me.
12. It makes me angry when another person is held up as a role model for me to follow.
13. When someone forces me to do something, I feel like doing the opposite.
14. It disappoints me to see others submitting to standards and rules.