

Bruns, Hendrik; Perino, Grischa

Article — Published Version

Point at, nudge, or push private provision of a public good?

Economic Inquiry

Provided in Cooperation with:

John Wiley & Sons

Suggested Citation: Bruns, Hendrik; Perino, Grischa (2021) : Point at, nudge, or push private provision of a public good?, *Economic Inquiry*, ISSN 1465-7295, Wiley Periodicals, Inc., Boston, USA, Vol. 59, Iss. 3, pp. 996-1007, <https://doi.org/10.1111/ecin.12981>

This Version is available at:

<https://hdl.handle.net/10419/241262>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.



<http://creativecommons.org/licenses/by/4.0/>

ORIGINAL ARTICLE

Point at, nudge, or push private provision of a public good?

Hendrik Bruns, Policy Analyst^{1,2}  | Grischa Perino, Professor^{2,3} 

¹European Commission Joint Research Centre (JRC), Brussels, Belgium

²Department of Socioeconomics, University of Hamburg, Hamburg, Germany

³Center of Earth System Research and Sustainability, University of Hamburg, Hamburg, Germany

Correspondence

Hendrik Bruns, European Commission Joint Research Centre (JRC), Rue du Champ de Mars 21, 1049 Brussels, Belgium.
Email: hendrik.bruns@ec.europa.eu

Funding information

International Max-Planck Research School on Earth System Modelling; Universität Hamburg

Abstract

How decision makers respond to behavioral and traditional interventions might depend on their and the regulator's attributes. This online experiment investigates the effect of defaults, recommendations, and mandatory minimum contributions accompanied by regulator information on the private provision of climate protection, accounting for intrinsic motivation. Findings show that all interventions increase the propensity of individuals to choose the focal value. There is no evidence that recommendations and defaults change average contributions. We report a negative interaction of the default with intrinsic motivation. Expert or political regulator information decreases intervention effectiveness. The study improves our understanding of behavioral public policy instruments.

KEYWORDS

climate protection, default, experiment, motivation crowding, nudge, public good

JEL CLASSIFICATION

H41; D04; Q54; Q58

1 | INTRODUCTION

Nudges alter behavior without using financial incentives or limiting choice options (Thaler & Sunstein, 2008). Supposedly, this preserves choice autonomy. For example, defaults can foster public good contributions, by setting green energy contracts as the default (Ebeling & Lotz, 2015; Pichert & Katsikopoulos, 2008; Sunstein & Reisch, 2016), or by lowering default thermostat settings (Brown et al., 2013). Default effectiveness also depends on its costs for the decision maker (Brown et al., 2013; Choi et al., 2003; Goswami & Urminsky, 2016; Haggag & Paci, 2014; Jachimowicz et al., 2018). These costs can be monetary and psychological. Monetary costs are straightforward. The default option may be a green energy contract that is costlier than the gray alternative. Psychological costs can be more complex. Defaults could decrease perceived behavioral autonomy; people may feel steered or even manipulated. For example, a study by Hagman et al. (2015) finds that 77.2% of respondents rate carbon emission offset defaults as intrusive to individual freedom. These perceptions likely depend on an individual's intrinsic motivation to protect the climate. Someone highly motivated might rate and respond to a default differently than someone with low intrinsic motivation, *ceteris paribus*. Additionally, their response might depend on how they perceive those who initiated the behavioral intervention. A default set by a politician may threaten autonomy more than a default from a climate scientist.

Abbreviations: EU ETS, European Union Emissions Trading System; MMC, mandatory minimum contribution.

This is an open access article under the terms of the Creative Commons Attribution License, which permits use, distribution and reproduction in any medium, provided the original work is properly cited.

© 2021 The Authors. *Economic Inquiry* published by Wiley Periodicals LLC on behalf of Western Economic Association International.

To explore when and why defaults work, we contrast them to softer and harder interventions, taking into account their origin, and decision makers' intrinsic motivation. We present data from an online framed field experiment with a sample representative of the German internet using population. Participants decide how much money to contribute to climate protection. We measure a proxy for intrinsic motivation prior to this decision. Participants face either a recommendation, default, or mandatory minimum contribution. These interventions all target the same contribution level. In addition, participants receive information on the intervention's origin. It was implemented neutrally, by an expert, or by a politician.

First, we find that all interventions increase the likelihood that the focal value is chosen. Only the mandatory minimum contribution, however, increases average contributions. Second, providing any source information decreases the likelihood that the focal value is chosen, but does not change average contributions. Third, we find that the default negatively interacts with intrinsic motivation.

The absence of a statistically significant recommendation, and default effect on aggregate contributions, taking into account intrinsic motivation, is important in light of the recent praise of nudges. The findings highlight the role of population characteristics, that is, intrinsic motivation, in predicting nudge effectiveness. Additionally, while the positive effect of the mandatory minimum on average contributions is by design, given average contributions in the baseline are below the mandated value, the non-negative effect on higher contribution levels aligns with evidence on motivation crowding by Falk and Kosfeld (2006) who find no crowding out when the mandate originates from a neutrally framed experimenter.

This study contributes to the extant literature on the relation between regulator information and intervention effect (Perino et al., 2014; Silverman et al., 2014), as well as to the literature investigating the potential of behavioral interventions to induce no or unintended effects (Osman, 2020; Sunstein, 2017). The findings have implications for the emerging literature on personalized default rules outlined in the conclusion section. To the best of our knowledge, we are the first to provide direct and controlled experimental evidence on the relative performance of these three interventions, the role of regulator-attributes, as well as their interaction with intrinsic motivation to make real contributions to a large public good. Findings improve our understanding of the distinction between nudges and other instruments. They help regulators and policymakers predict who should prefer to use which intervention, that is, a pointer, a nudge, or a push to increase private contributions to a public good.

The remainder of this article is structured as follows: Section 2 outlines the experiment, behavioral predictions, and the sample. Section 3 presents the data analysis. Section 4 discusses important caveats of the design, while Section 5 concludes.

2 | EXPERIMENT

The experiment was conducted online between November 2016 and January 2017. Participants received an endowment and were free to divide the amount between themselves and climate protection (see Diederich & Goeschl, 2017). Their final payoff was their endowment minus the contribution amount. Data collection was divided into two stages and three elements: Stage 1 consisted of a questionnaire conducted about 3 weeks prior. Stage 2 had two elements: the incentivized experiment and an exit questionnaire.

Here, the description of the experiment is limited to Stage 2.¹ As part of the experiment, subjects made two contribution decisions, of which the second was modified according to the treatment. The first measured a proxy for an individual's intrinsic climate protection motivation. After the experiment, we donated one randomly chosen contribution per participant to the nongovernmental organization "Compensators*" (<http://www.compensators.org/>), which buys and retires emission rights from the European Union Emissions Trading System (EU ETS). The experiment was implemented online with Limesurvey. Participation was possible via personal computers and smartphones from any place with internet access. The experimental design was registered and made available online at the American Economic Association's registry for randomized controlled trials (AECTR-0001661).²

2.1 | Experimental procedure and treatments

Subjects stated their age, gender, and education, before reading the instructions. After a comprehension test question, they were endowed with 100 Credits (5 EUR or ~\$6). First, they were asked to decide how much to donate to retire carbon emissions, without any intervention. This was the baseline contribution, a proxy for intrinsic motivation. Subjects were then randomly allocated to treatments. They encountered either no intervention (control), a recommendation, a

default, or a mandated minimum amount. Each intervention recommended, defaulted, or mandated the same value, that is, 35 Credits (1.75 EUR). This amount was chosen by a person that was identified as the source of the intervention in some treatments. This person was a real German politician with a prior career in environmental sciences and climate policy. She chose from a range of contribution levels prespecified by us.

Every intervention was combined with true information on the source responsible for its implementation. The information was either absent, labeled the source as an expert on environmental science and climate policy, or characterized her as a politician.

Both intervention factors were crossed, resulting in a 3 (recommendation, default, mandatory minimum) $\times 3$ (none, expert, politician) design, plus a control group without regulator and intervention. Subjects in the control made the same decision twice. All subjects were informed that the payout-relevant contribution would be randomly selected at the end. Thus, both decisions were framed as independent. Subjects chose between 0 and 100 Credits for any decision except the second decision in the mandatory minimum contribution (MMC) treatment, where subjects had to contribute at least 35 Credits. The amount that was not contributed was paid out to participants at the end of the experiment.

Regulator information was presented to subjects after their baseline choice. First, they saw a screen with a picture of the person and a short paragraph about her background (expert or politician, see Appendix A.4 for details). Second, they could make a new contribution decision with the intervention linked to the source. Participants were truthfully told that the person was real, that she was involved in the design of the experiment, and that her name was going to be revealed at the end of the experiment.

Subjects in nonregulator-information treatments skipped these screens and directly proceeded to the second contribution decision. Subjects in the control group saw an interface identical to the first decision. Subjects in the recommendation group were told that the respective regulator (if any) recommended them to contribute 35 Credits. They entered the amount into a text box. In the default group, they saw two radio buttons. The top button contributed 35 Credits and was preselected. Clicking the second button allowed them to specify another amount on a separate screen. The set-up was accompanied by a sentence that the respective default was implemented by the person previously presented to them (if any). When facing the mandatory minimum, participants could choose an amount between 35 and 100 Credits. Again, the intervention was linked to the respective regulator information, if any was provided. In cases without source information, the interventions were introduced neutrally by stating “35 Credits were set as a default/mandatory minimum contribution,” or “It was recommended to contribute 35 Credits.” After the contribution decision, subjects were informed which of their two decisions had been realized. Subjects then answered a postexperimental questionnaire.³

2.2 | Important literature and behavioral predictions

The three interventions tested in this experiment have been shown to be effective in increasing the likelihood that the focal value is contributed to a public good. Croson and Marks (2001) provide evidence for recommendation effects in a public goods game. A meta-analysis of studies testing the effects of defaults finds general support for their effectiveness, although they are relatively weak in environmental domains (Jachimowicz et al., 2018). Mandatory minimum contributions do this by design, since subjects are not allowed to donate less than the focal value (Engelmann et al., 2017; Keser et al., 2017).

H1. All interventions increase the likelihood that the focal value is chosen.

A factor expected to affect intervention effectiveness is the information on who is responsible for it. When a source is identified, the default can be interpreted as an endorsement. The effect of this endorsement will likely depend on the underlying motives decision makers attribute to the regulator (Bénabou & Tirole, 2003; Kelley & Michela, 1980). Evidence indicates that decision makers treat defaults as informative about default-setters' intentions: Perceptions of the default setter as being profit-maximizing (Brown & Krishna, 2004) or ill-informed (Altmann et al., 2015) can decrease its stickiness. Kuang et al. (2007) highlight the negative effect of self-interested advisers for advice effectiveness. The effect of a mandate has been shown to vary based on whether it was established by the experimenter or a principal in a principal-agent setting (Falk & Kosfeld, 2006; Goeschl & Perino, 2012). We vary regulator information along the dimensions related to (a) competence, and (b) intention to influence. We expect that informing decision makers that the regulator is an expert or politician changes their motivation attribution along these dimensions, respectively. We expect

decision makers primarily attribute the intention of competence to an expert, and the intention to influence to a politician. We expect this to result in a negative effect on contributions for a political regulator, and a positive effect for an expert regulator.

H2. Expert source information positively, political source information negatively affects behavioral responses to the interventions.

2.3 | Participants

The sample was drawn from the panel of a professional survey and market research company in Germany. The final data consist of 806 individuals with a mean age of 49.90 years ($SD = 15.66$, Median = 51), 51.86% women, with 33.75% having lower, 33.37% middle, and 14.76% higher education, while 17.74% hold a university degree.⁴ The median household has a monthly income of 2000–2499 EUR. Aggregated and disaggregated sample distributions of covariates, answers given to central questions from the questionnaires, a regression predicting baseline contributions as a function of most of these variables, and a description of the data cleaning procedure are in Appendix B.

3 | FINDINGS

3.1 | Descriptive statistics

Table 1 shows statistics of intrinsic motivation, second-round contributions, their difference, and the differences above 35 Credits. We refer to the latter as “Falk–Kosfeld adjusted” (cp. Falk & Kosfeld, 2006). Because subjects in the mandatory minimum treatments had a different choice set in the second decision, that is, a mandatory minimum contribution of 35 instead of 0 Credits, distributions differ by design.⁵ After adjustment, we can compare individual changes of contributions between both rounds that take place above 35 Credits between all treatment groups.

Due to randomization, the distributions of baseline contributions do not significantly differ between groups (Kruskal–Wallis test: $H(9) = 6.626$, $p = .676$). However, aggregating baseline contributions of all treatment groups and

TABLE 1 Descriptive statistics of contributions by experimental group

	Base contribution			Contribution			Contribution change			Contribution change >35			n
	M	SD	Med	M	SD	Med	M	SD	Med	M	SD	Med	
Control	40.27	34.91	40	35.29	34.86	20	−4.97	23.26	0	−2.65	14.33	0	75
No source													
Recommendation	31.59	33.68	20	31.73	29.14	35	0.14	20.37	0	−3.66	15.78	0	90
Default	30.73	30.51	20	26.66	20.89	35	−4.07	26.63	0	−8.31	20.94	0	83
Mandatory minimum	31.56	33.96	20	47.86	21.35	35	16.30	26.11	20	−0.22	18.03	0	86
Expert													
Recommendation	36.77	36.09	40	29.88	28.48	35	−6.88	19.19	0	−7.66	18.04	0	77
Default	31.77	33.42	20	27.05	22.37	35	−4.71	22.43	0	−8.22	18.72	0	73
Mandatory minimum	36.03	32.81	30	48.57	20.56	40	12.54	24.18	10	−1.11	15.28	0	79
Politician													
Recommendation	32.48	32.43	20	29.00	28.32	25	−3.48	11.10	0	−3.92	9.94	0	83
Default	33.31	30.41	30	25.81	20.66	35	−7.49	22.28	0	−8.60	17.08	0	81
Mandatory minimum	30.11	32.67	20	46.29	18.20	40	16.18	22.42	20	−0.61	11.61	0	79

Notes: Contribution changes are constructed by subtracting baseline contributions from second-round contributions. Contribution changes >35 are constructed by setting all baseline and second-round contributions below 35 to 35 Credits, and then subtracting the former from the latter.

Abbreviations: M, mean; Med, median; SD, standard deviation.

comparing them to the control results in a marginal significant difference ($H(1) = 3.626, p = .057$). The baseline contributions in the control group are 10–25% higher than in the other treatments. We cannot explain this other than by chance. Information on sample characteristics across experimental groups (Tables B.2 and B.3 in Appendix B) and their prediction of baseline contributions (Table B.4 in Appendix B) cannot explain the relatively high baseline contributions in the control group.

A Wilcoxon signed rank test marginally rejects equal within-subjects distributions of first- and second-round contributions in the control group ($V = 235.5, p = .050$). This finding suggests that subjects change their contribution in the absence of experimental manipulation from round one to round two. Both findings warrant a difference-in-differences analysis. Using contribution levels as the dependent variable while controlling for baseline contributions would underadjust for these differences (Allison, 1990).

Figure 1 shows the fraction of subjects contributing specific amounts in the second round. The default gets 103 subjects (43.46%) to contribute the focal value, across source types. The mandate gets 117 subjects (47.95%), while the recommendation only gets 42 subjects (16.80%) to comply. It is notable that the neutral default is more effective in attracting contributions than either of the three mandatory minimum contribution treatments. At the same time, only 35 subjects (14.77%) contribute more than 35 Credits across all default treatments. Respectively, these are 35 (46.67%) in the control, 88 (35.20%) in the recommendation, and 127 (52.05%) in the mandatory minimum treatments. We investigate this further in the following subsections.⁶

3.2 | Finding 1: Effect of intervention type

The fraction of participants choosing the focal value is significantly higher in all intervention groups, aggregated over source type, compared to the control (Chi-squared tests: Rec: $\chi^2(1) = 13.02, p < .001$; Def: $\chi^2(1) = 46.71, p < .001$; MMC: $\chi^2(1) = 54.75, p < .001$). In the control, no one chose it (see Figure 1). We compare interventions in a logistic regression model controlling for source information. The dependent variable is 1 when the focal value was contributed, 0 otherwise (see Table B.5 in Appendix B). The odds of contributing the recommended value are a quarter of the odds of contributing the default ($OR = 0.26, CI_{95}[0.17, 0.40], p < .001$). The odds of picking the mandated value are virtually identical to the odds of choosing the default ($OR = 1.20, CI_{95}[0.84, 1.72], p = .324$). The odds of picking the mandated value are almost 5 times the odds of choosing the recommended value ($OR = 4.56, CI_{95}[3.01, 6.93], p < .001$).

F1. All interventions increase the likelihood that the focal value is chosen. The default and mandate exert the strongest effects.

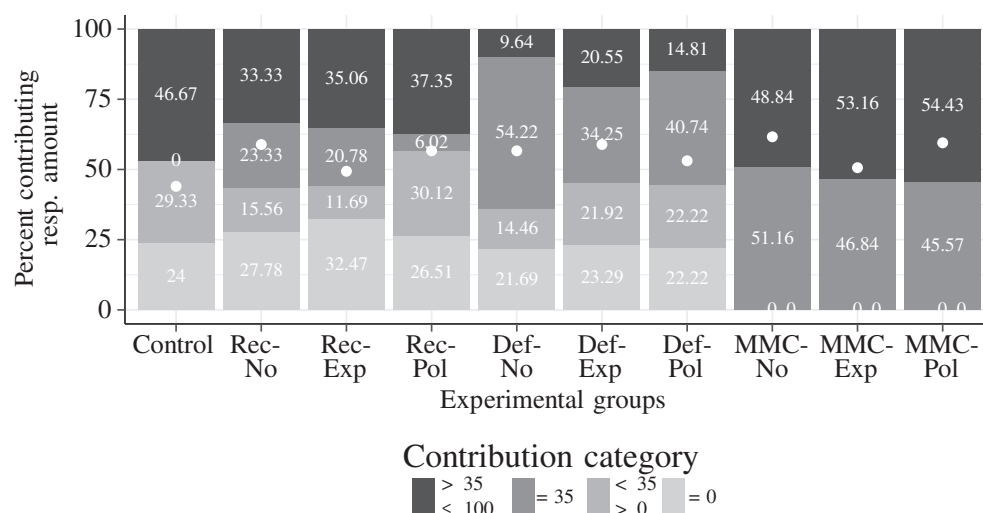


FIGURE 1 Categorized contributions by experimental group. Shows the distribution of second-round contributions after categorizing the latter. The white dots show the respective fractions of subjects contributing less than 35 Credits in round one. Dots thus are the first-round equivalent to the sum of the two light shaded areas. Def, default; Exp, expert; MMC, mandatory minimum contribution; No, no source; Pol, politician; Rec, recommendation

Effects from ordinary least squares regressions modeling average contributions are more nuanced (see Table B.6 in Appendix B). In these regressions, we model (adjusted) changes between intrinsic motivation and the second-round contribution with the type of intervention, controlling for regulator information. Relative to the reduction of contributions between round 1 and 2 in the control group, the recommendation ($B = 3.75$, $CI_{95}[-2.46, 9.96]$, $p = .236$) and default ($B = 1.56$, $CI_{95}[-5.04, 8.16]$, $p = .642$) do not have a significant impact. As expected, the mandatory minimum significantly increases mean contribution changes relative to the control ($B = 22.06$, $CI_{95}[15.5, 28.63]$, $p < .001$), recommendation ($B = 18.31$, $CI_{95}[14.56, 22.06]$, $p < .001$), and default ($B = 20.5$, $CI_{95}[16.18, 24.82]$, $p < .001$).

To get a better idea of the nuanced relative impacts and to compare the effects of the mandatory minimum contribution to the other interventions without bias due to the different choice sets, we use the “Falk–Kosfeld adjustment” as described above. For the adjusted dependent variable, the default induces a much stronger drop in contribution levels above 35 than observed in the control group ($B = -5.12$, $CI_{95}[-9.6, -0.64]$, $p = .025$). This is not the case for the recommendation ($B = -1.71$, $CI_{95}[-5.84, 2.41]$, $p = .415$) and the mandatory minimum contribution ($B = 2.65$, $CI_{95}[-1.59, 6.9]$, $p = .22$). The default induces mean reductions of high contributions (>35) relative to the recommendation ($B = -3.4$, $CI_{95}[-6.45, -0.36]$, $p = .029$) and mandatory minimum ($B = -7.77$, $CI_{95}[-10.86, -4.68]$, $p < .001$). The recommendation induces significantly more negative changes compared to the mandatory minimum ($B = -4.36$, $CI_{95}[-7.04, -1.69]$, $p = .001$).

Insights into causal channels by which the default works can be drawn from the result that subjects are more likely to contribute 35 Credits when it is set as a default, than when it is recommended. Both interventions provide a reference value or anchor. Deviating from it might be experienced as a loss, especially by subjects who are insecure about their intrinsic motivation (Jachimowicz et al., 2018). There is evidence that the default works due to inertia (Johnson & Goldstein, 2003), that is, peoples' tendency to stick to it in order to reduce effort (see the response time analysis in Appendix B).

The experimental design allows us to explore whether the effect of an intervention varies with the level of intrinsic motivation. The upper panel of Figure 2 plots the estimated intervention effects on contribution changes relative to no intervention, conditional on intrinsic motivation (Table B.9 in Appendix B shows the underlying regression results).⁷ There is evidence for a scale-back effect of defaults (see Goswami & Urmitsky, 2016). This effect characterizes situations where low defaults decrease average contributions. The default increases contributions for subjects with intrinsic motivation below the default value and decreases them for subjects with higher intrinsic motivation. This is shown by the negative slope visualizing the negative interaction between baseline contributions and the default effect. Both effects cancel out on the aggregate. This slope is not significantly different from zero for recommendations, providing

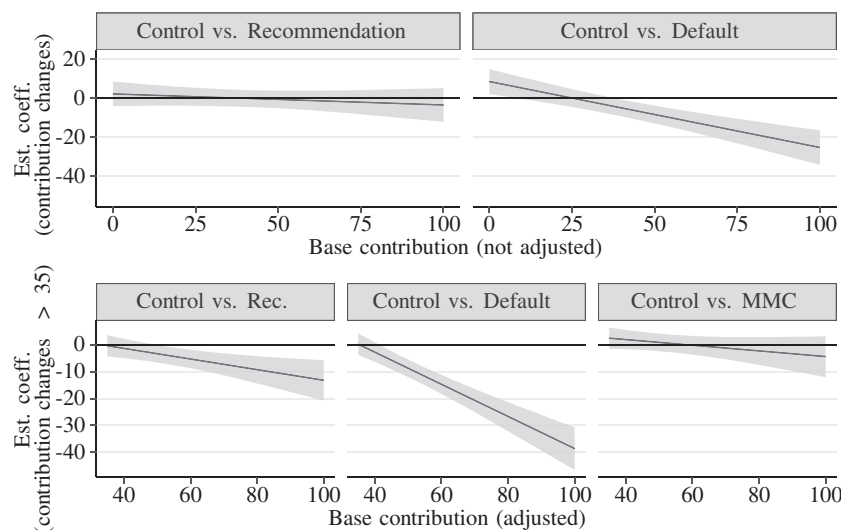


FIGURE 2 Intervention effects on contribution changes (>35) relative to control conditional on baseline contributions. Shows estimated effects of using an intervention relative to no intervention conditional on baseline contributions on contribution changes (top), as well as on contribution changes >35 conditional on baseline contributions >35 (bottom). Aggregated over source types. Gray areas represent simulated 95% confidence intervals (10,000 draws). MMC, mandatory minimum contribution; Rec, recommendation

no evidence for a scale-back effect. The slopes differ significantly between the default and recommendation in the upper panel of Figure 2 ($B = -0.28$, $SE = 0.061$, $p < .001$).

The lower panel of Figure 2 shows estimated coefficients of adjusted contribution changes caused by the default, recommendation, and mandatory minimum, relative to no intervention, conditional on adjusted baseline contributions. The same pattern as for unadjusted contributions emerges. The effect is relatively small for the recommendation and nonexistent for the mandatory minimum. Again, the slope of the default effect is significantly steeper than the slope for the recommendation effect, shown in the lower panel of Figure 2 ($B = -0.4$, $SE = 0.1$, $p < .001$), as well as the slope of the effect of the mandatory minimum contribution ($B = -0.49$, $SE = 0.09$, $p < .001$). There is no difference between the slopes for mandatory minimum contribution and for the recommendation ($B = 0.09$, $SE = 0.09$, $p = .329$). While there is some evidence that a recommendation decreases *high* contributions, this effect is much more pronounced for the default.

Interestingly, in our setting, enforcement of a minimum contribution did not reduce individual contributions made by those who previously contributed more than mandated. We find this for all source treatments. Thus, our findings partly align with Falk and Kosfeld (2006) and Goeschl and Perino (2012) who both find no evidence of crowding out for mandatory minimums originating from a *neutral experimenter*. Along the line of reasoning of Reeson and Tisdell (2008), this may have been caused by subjects with high intrinsic motivation perceiving the mandatory minimum contribution as a means to coerce free-riders to conform to a perceived social expectation of contributing to climate protection.

3.3 | Finding 2: Effect of source type

Controlling for intervention type, the provision of either source information decreases the likelihood that the focal value is contributed (see Table B.5 in Appendix B). The odds of picking the focal value when expert source information is provided are 0.66 times the odds when no information is provided ($OR = 0.66$, $CI_{95}[0.45, 0.98]$, $p < .041$). The odds of picking the focal value when political source information is provided are 0.56 times the odds when no information is provided ($OR = 0.56$, $CI_{95}[0.38, 0.82]$, $p < .003$). Odds ratios between both sources are indistinguishable from 1 ($OR = 0.85$, $CI_{95}[0.57, 1.26]$, $p = .41$).

F2. Providing expert and political source information decreases the likelihood that the focal value is chosen.

The finding on the negative effect of political information corresponds to what we expected based on the intention-to-influence explanation. However, the negative effect of expert information is unexpected. We explore this further by (a) disaggregating the effects by intervention type, and (b) looking at questionnaire responses on how participants perceive the source of the intervention. In a logit model regressing the odds of choosing the focal value on both the intervention and source type, including their interaction (Table B.7 in Appendix B), we find the following: Providing political source information decreases the odds that the recommendation is chosen relative to no ($OR = 0.21$, $CI_{95}[0.07, 0.59]$, $p = .003$) or expert ($OR = 0.24$, $CI_{95}[0.08, 0.71]$, $p = .009$) source information. For the default, only providing expert instead of no source information decreases the odds that it is chosen ($OR = 0.44$, $CI_{95}[0.23, 0.84]$, $p = .013$). In the remaining cases, the provision of source information has no discernible effect. The regression also shows that providing political source information is much less detrimental when accompanying a mandatory minimum compared to a recommendation, as indicated by the positive interaction effect ($OR = 3.79$, $CI_{95}[1.14, 12.67]$, $p = .03$).

We have information on attitudes towards the source of the respective intervention from a postexperimental questionnaire. Respondents that encountered source information indicated on a 5-point Likert scale whether the regulator (1) “is knowledgeable and competent with respect to climate protection,” (2) “tries to influence me in my free decision,” and (3) “tries to reduce carbon emissions.”

In three logit models, we regress a dummy that is equal to 1 if a subject (strongly) agreed, 0 otherwise, to the respective statement, on source- and intervention type (Table B.8 in Appendix B). The coefficients reveal that an expert is regarded 4.27 times more knowledgeable than a politician ($OR = 4.27$, $CI_{95}[2.86, 6.29]$, $p < .001$), and 2.27 times more likely to have reduction of carbon emissions as the goal ($OR = 2.27$, $CI_{95}[1.54, 3.36]$, $p < .001$). An expert is perceived as less likely to aim at influencing decision making, although this effect is just marginally significant ($OR = 0.7$, $CI_{95}[0.47, 1.03]$, $p = .07$).

This can only partly explain the finding that either source information decreases the likelihood that the focal value is chosen. The fact that the politician is perceived as less knowledgeable and less focused on carbon reduction than the expert is consistent with the observation that less people follow the former's, compared to the latter's recommendation.

However, this is not the case for the default and the mandatory minimum contribution. The observation that a neutral recommendation (default) is followed more often than a political recommendation, and a neutral default more often than an expert default, are both consistent with an experimenter demand effect. An intervention framed neutrally might have been interpreted by participants as coming from the experimenter. Those might have increasingly been followed because they were interpreted as part of the experimental design or in order to appease the experimenter.

There is no evidence that source information affects average contribution changes, or average contribution changes above 35 Credits in either direction (Table B.6 in Appendix B). While the negative effect of providing expert instead of no source information on average contributions is marginally significant ($B = -3.89$, $CI_{95}[-8.02, 0.24]$, $p = .07$), all others are negative but insignificant. Neutrally framed interventions appear to be followed by people with lower and higher intrinsic motivation, resulting in no changes for average contributions.

4 | DISCUSSION

Clearly, our experimental design has limitations, most importantly with respect to generalizability. First, our findings should only be generalized to the German internet using population. Other populations can differ with respect to intrinsic motivation and other characteristics. While we investigate preferences towards the target behavior (intrinsic motivation towards climate protection) as a moderator of intervention effectiveness, it is reasonable to assume that attitudes towards policy interventions moderate the effects as well. In general, attitudes towards climate protection (Metag et al., 2017), as well as acceptance of nudges as public policy instruments are relatively high in Germany (Reisch & Sunstein, 2016). However, nudge acceptance also depends on the type of nudge and the behavioral domain that is targeted. A global survey conducted by Branson et al. (2011) found broad support for behavior change policies across 24 nations, but at the same time on average half of the respondents indicated that the state should not get involved in individual environment-related decisions. Germany has the fourth-lowest average support (49% of respondents) of outright bans as a public policy instrument. Given the lack of evidence for motivation crowding of a mandatory minimum in this experiment, it is unlikely that respondents from a country with *higher* support for bans would show motivation crowding, *ceteris paribus*. In contrast, the United States have even less support for bans than Germany, that is, 33%. Motivation crowding, due to a mandatory minimum or default, might thus be more likely in a US-American sample. Green defaults might even be less effective in samples heterogeneous with respect to environmental and political attitudes, especially given the moderating role of political orientation on climate beliefs (McCright, 2011), nudge attitudes (Tannenbaum et al., 2017), and nudge effectiveness (Costa & Kahn, 2013).

Second, how generalizable are the findings with respect to different values for the recommendation, default, or mandatory minimum contribution? The focal value 35, that is, 35% of the endowment, is (a) low compared to other experiments that aim to increase contributions to public goods (see Altmann & Falk, 2009; Bruns et al., 2018; Carlsson et al., 2015), and (b) close to the preintervention average of contributions (33.37 Credits) in our experiment. Similar studies report lower mean and median contributions without any intervention (see Bruns et al., 2018; Diederich & Goeschl, 2014, 2017). Higher focal values might lead to less compliance with either intervention, *ceteris paribus*. This has been shown for defaults (see Goswami & Urminsky, 2016; Haggag & Paci, 2014) and mandates (e.g., Falk & Kosfeld, 2006). Still, due to inertia, higher default values would be more likely to have a positive effect on average contribution levels than comparable recommendations and mandates. Further research should investigate the relation between defaults and other interventions as their values increase and whether the reasons for the decreasing effects of defaults are identical with those of mandates, that is, control aversion (see Bruns & Perino, 2019).

Third, it remains for future research to explore how generalizable our findings are to alternative behavioral interventions, for example, social norm provision, or commitment devices. The effects of those interventions can be caused by different behavioral processes and can thus be more or less affected by regulator characteristics and intrinsic motivation. Often, these instruments also rely on setting specific values that decision makers evaluate with respect to their intrinsic motivation. Thus, similar effects can occur. For example, if an external goal for future behavior is set below current levels, this mismatch can be used to justify effort reduction.

Fourth, we discuss the economic significance of the negative crowding effect of the default. For every increase of intrinsic motivation, a default decreases the following contribution by -0.34 Credits more than the respective reduction in the control group (i.e., -0.22 Credits). The standardized interaction effect corresponds to Cohen's $d = 0.63$ ($\eta^2 = 0.09$), which is an intermediate effect size.

5 | CONCLUSION

This paper presents data from an online framed field experiment on how three prominent interventions from different regulators influence individual contributions to climate protection. Based on a representative sample, findings show that, while all interventions increase the likelihood that participants choose the focal value, *neither* the recommendation, *nor* the default change *average* contributions to climate protection. The mandatory minimum contribution increases average contributions. While the default leads some participants to increase their previously low contributions to the focal value, this is offset by its negative effect on subjects with high intrinsic motivation (motivation crowding). Data do not indicate that either recommendations or mandated minimum contributions crowd out contributions. The provision of information on the source responsible for implementing either intervention decreases the likelihood that the focal value is chosen. This is surprising given that respondents rate experts as more competent and more likely to have carbon reductions as the goal.

The findings suggest that default values which are low relative to the intrinsic motivation can crowd out contributions from highly motivated decision makers. This appears not to be the case for the levels of recommendations and mandated minimum contributions. Thus, we augment findings on the scale-back effect provided by Goswami and Urminsky (2016), showing that not just the size of the default value matters, but rather how it relates to decision makers' intrinsic motivation. This finding is important given the argument by Jonge et al. (2018), who stress the need to shift the focus to the psychology of the decision makers being influenced by behavioral public policy.

Our findings may serve as an explanation for the sometimes observed inefficiency of some defaults to change aggregate outcome measures found in earlier contributions (Jachimowicz et al., 2018). If the default value and the average intrinsic motivation of decision makers are close, the default will not have an effect on mean contribution levels. The absence of a recommendation effect, however, is likely to be due to the generally low impact of the intervention.

Our findings appear inconsistent with the account that defaults cause reactant behavior. Psychological reactance, often defined as a cognitive or behavioral response occurring when an individual *feels* her freedom threatened, followed by behavior to re-establish freedom (Brehm, 1966), is sometimes used to rationalize nudge effects occurring in the opposite direction as intended. For example, Hedlin and Sunstein (2016) ascribe the occurrence of low approval ratings for green energy defaults entailing additional costs to psychological reactance (see also Arad & Rubinstein, 2018; Haggag & Paci, 2014; Sunstein, 2017; Yan & Frank Yates, 2019). Whether nudges, specifically defaults, create reactant feelings or thoughts that lead to discernible effects on behavior is still unclear, however. Evidence by Goswami and Urminsky (2016) and Bruns and Perino (2019) do not suggest that psychological reactance affects the behavioral response to defaults, although cognitive and affective reactions are detectable (see also Arad & Rubinstein, 2018; Yan & Frank Yates, 2019). If a participant feels her freedom of behavior threatened by the default and consequently strives to re-establish her freedom, she would not deviate from her initial contribution in direction of the default value. However, this is what we observe in this experiment. Given that we do not find a similar effect for an objectively more controlling intervention leads us to believe that the default effect cannot be explained by psychological reactance.

It becomes increasingly possible to dynamically adapt default values in online environments based on, for example, the individual characteristics of decision makers. Altmann et al. (2019) suggest that, because of their crowding out effect, defaults should be set above intrinsic motivation. Their structural model suggests that setting the default to twice the level of intrinsic motivation could result in up to 6.2% higher donation revenues relative to no default. However, successfully personalizing defaults requires much richer data in order to set them adequately. Especially since intrinsic motivation can change over time, possibly in response to defaults. In combination with our findings on an interaction with donors' intrinsic motivation, this suggests research on machine learning algorithms setting personalized default values may be promising (Mills, 2020; Peer et al., 2020; Sunstein, 2013; Yeung, 2018), but also challenging (Reiley & Samek, 2019). Based on available donor information, defaults could be set so as to maximize the probability of realizing the highest possible contribution to the charitable organization, or environmental cause.

To summarize, findings presented here suggest that a nudge, opposed to a pointer and a push, can crowd out contributions to a public good. Better understanding regulator and decision maker characteristics will likely contribute to a more effective application of these interventions.

ACKNOWLEDGMENTS

We thank participants of the following workshops, conferences, and summer schools: The 9th Summer School of the International Max Planck Research School on Adapting Behavior in a Fundamentally Uncertain World in Jena, the PhD students seminar at the Department of Economics at the University of Hamburg, the AURÖ at the Helmholtz-Centre for Environmental Research (UFZ) in Leipzig, the workshop on Experiments for the Environment in Osnabrueck and Bremen, the Manchester Environmental Economics Workshop on Social Preferences, the 23rd Annual Conference of the European Association of Environmental and Resource Economists (EAERE) in Athens, and the International Conference on Social Dilemmas (ICSD) in Taormina. Furthermore, we thank Claudia Schwirplies, Fredrik Carlsson, Peter Martinsson, and Christina Gravert for valuable comments and suggestions. We gratefully acknowledge comments from the editor and three anonymous referees. We acknowledge funding by the University of Hamburg and the International Max-Planck Research School on Earth System Modelling. Contribution to the Cluster of Excellence 'CLICCS - Climate, Climatic Change, and Society'.

CONFLICT OF INTEREST

No potential conflict of interest was reported by the authors.

DATA AVAILABILITY STATEMENT

The data that support the findings of this study are openly available in OSF at <https://osf.io/6bdnk>

ORCID

Hendrik Bruns  <https://orcid.org/0000-0003-4301-0009>

Grischa Perino  <https://orcid.org/0000-0001-6519-1029>

ENDNOTES

¹ Descriptions of all questionnaires are in Appendix C.

² In April 2016, we conducted a pilot experiment in the experimental research lab at the University of Hamburg. Findings from these sessions were used to develop and improve the experiment.

³ Details about the regulator information, as well as screenshots and instructions are in Appendix A.

⁴ One subject indicated to have no education or to still be in school.

⁵ The adjustment sets all contribution levels below 35 to 35, that is, it simulates a mandatory minimum in all choice situations based on the assumption that all participants that chose a level below 35 would have chosen 35 if the mandate were present.

⁶ Distributions aggregated for the first round and disaggregated by intervention and source for the second round are in Figure B.9 in Appendix B.

⁷ Relevant estimates are robust to the inclusion of covariates. These results can be requested from the authors.

REFERENCES

- Allison, P.D. (1990) Change scores as dependent variables in regression analysis. *Sociological Methodology*, 20, 93–114. <https://doi.org/10.2307/271083>.
- Altmann, S. & Falk, A. (2009) *The impact of cooperation defaults on voluntary contributions to public goods*. URL: http://www.socialpolitik.ovgu.de/sozialpolitik_media/papers/altmann_steffen_uid406_pid354.pdf [Accessed June 1, 2020].
- Altmann, S., Falk, A. & Grunewald, A. (2015) *Incentives and information as driving forces of default effects*. URL: <https://ssrn.com/abstract=2329060> [Accessed May 15, 2018].
- Altmann, S., Falk, A., Heidhues, P., Jayaraman, R. & Teirlinck, M. (2019) Defaults and donations: evidence from a field experiment. *Review of Economics and Statistics*, 101(5), 808–826. https://doi.org/10.1162/rest_a_00774.
- Arad, A. & Rubinstein, A. (2018) The people's perspective on libertarian—paternalistic policies. *The Journal of Law and Economics*, 61(2), 311–333. <https://doi.org/10.1086/698608>.
- Bénabou, R. & Tirole, J. (2003) Intrinsic and extrinsic motivation. *The Review of Economic Studies*, 70(3), 489–520. <https://doi.org/10.1111/1467-937X.00253>.
- Branson, C., Duffy, B., Perry, C. & Wellings, D. (2011) *Acceptable behaviour? Public opinion on behavior change policy*. URL: <https://www.ipsos.com/sites/default/files/publication/1970-01/sri-ipsos-mori-acceptable-behaviour-january-2012.pdf> [Accessed May 8, 2019].
- Brehm, J.W. (1966) *A theory of psychological reactance*. Oxford, England: Academic Press.
- Brown, C.L. & Krishna, A. (2004) The skeptical shopper: a metacognitive account for the effects of default options on choice. *Journal of Consumer Research*, 31(3), 529–539. <https://doi.org/10.1086/425087>.

- Brown, Z., Johnstone, N., Haščič, I., Vong, L. & Barascud, F. (2013) Testing the effect of defaults on the thermostat settings of OECD employees. *Energy Economics*, 39, 128–134. <https://doi.org/10.1016/j.eneco.2013.04.011>.
- Bruns, H. & Perino, G. (2019) *The role of autonomy and reactance for nudging: experimentally comparing defaults to recommendations and mandates*. <https://doi.org/10.2139/ssrn.3442465>.
- Bruns, H., Kantorowicz-Reznichenko, E., Klement, K., Jonsson, M.L. & Rahali, B. (2018) Can nudges be transparent and yet effective? *Journal of Economic Psychology*, 65, 41–59. <https://doi.org/10.1016/j.joep.2018.02.002>.
- Carlsson, F., Johansson-Stenman, O. & Khanh, N.P. (2015) Funding a new bridge in rural Vietnam: a field experiment on conditional cooperation and default contributions. *Oxford Economic Papers*, 67(4), 987–1014. <https://doi.org/10.1093/oxep/gpv039>.
- Choi, J.J., Laibson, D., Madrian, B.C. & Metrick, A. (2003) Optimal defaults. *American Economic Review*, 93(2), 180–185. <https://doi.org/10.1257/000282803321947010>.
- Costa, D.L. & Kahn, M.E. (2013) Energy conservation “nudges” and environmentalist ideology: evidence from a randomized residential electricity field experiment. *Journal of the European Economic Association*, 11(3), 680–702. <https://doi.org/10.1111/jeea.12011>.
- Croson, R. & Marks, M. (2001) The effect of recommended contributions in the voluntary provision of public goods. *Economic Inquiry*, 39(2), 238–249. <https://doi.org/10.1111/j.1465-7295.2001.tb00063.x>.
- Diederich, J. & Goeschl, T. (2014) Willingness to pay for voluntary climate action and its determinants: field-experimental evidence. *Environmental and Resource Economics*, 57(3), 405–429. <https://doi.org/10.1007/s10640-013-9686-3>.
- Diederich, J. & Goeschl, T. (2017) To mitigate or not to mitigate: the price elasticity of pro-environmental behavior. *Journal of Environmental Economics and Management*, 84, 209–222. <https://doi.org/10.1016/j.jeem.2017.03.004>.
- Ebeling, F. & Lotz, S. (2015) Domestic uptake of green energy promoted by opt-out tariffs. *Nature Climate Change*, 5(9), 868–871. <https://doi.org/10.1038/nclimate2681>.
- Engelmann, D., Munro, A. & Valente, M. (2017) On the behavioural relevance of optional and mandatory impure public goods. *Journal of Economic Psychology*, 61, 134–144. <https://doi.org/10.1016/j.joep.2017.04.002>.
- Falk, A. & Kosfeld, M. (2006) The hidden costs of control. *American Economic Review*, 96(5), 1611–1630. <https://doi.org/10.2307/30034987>.
- Goeschl, T. & Perino, G. (2012) Instrument choice and motivation: evidence from a climate change experiment. *Environmental and Resource Economics*, 52(2), 195–212. <https://doi.org/10.1007/s10640-011-9524-4>.
- Goswami, I. & Urminsky, O. (2016) When should the ask be a nudge? The effect of default amounts on charitable donations. *Journal of Marketing Research*, 53(5), 829–846. <https://doi.org/10.1509/jmr.15.0001>.
- Haggag, K. & Paci, G. (2014) Default Tips. *American Economic Journal: Applied Economics*, 6(3), 1–19. <https://doi.org/10.1257/app.6.3.1>.
- Hagman, W., Andersson, D., Västfjäll, D. & Tinghög, G. (2015) Public views on policies involving nudges. *Review of Philosophy and Psychology*, 6(3), 439–453. <https://doi.org/10.1007/s13164-015-0263-2>.
- Hedlin, S. & Sunstein, C.R. (2016) Does active choosing promote green energy use? Experimental evidence. *Ecology Law Quarterly*, 43(1), 107–142. <https://doi.org/10.2139/ssrn.2624359>.
- Jachimowicz, J.M., Duncan, S., Weber, E.U. & Johnson, E.J. (2018) When and why defaults influence decisions: a meta-analysis of default effects. *Behavioural Public Policy*, 3, 1–28. <https://doi.org/10.1017/bpp.2018.43>.
- Johnson, E.J. & Goldstein, D.G. (2003) Do defaults save lives? *Science*, 302(5649), 1338–1339. <https://doi.org/10.1126/science.1091721>.
- Jonge, P.d., Zeelenberg, M. & Verlegh, P. (2018) Putting the public back in behavioral public policy. *Behavioural Public Policy*, 13, 1–9. <https://doi.org/10.1017/bpp.2018.23>.
- Kelley, H.H. & Michela, J.L. (1980) Attribution theory and research. *Annual Review of Psychology*, 31(1), 457–501. <https://doi.org/10.1146/annurev.ps.31.020180.002325>.
- Keser, C., Markstädter, A. & Schmidt, M. (2017) Mandatory minimum contributions, heterogeneous endowments and voluntary public-good provision. *Games and Economic Behavior*, 101, 291–310. <https://doi.org/10.1016/j.geb.2016.06.001>.
- Kuang, X., Weber, R.A. & Dana, J. (2007) How effective is advice from interested parties? An experimental test using a pure coordination game. *Journal of Economic Behavior & Organization*, 62(4), 591–604. <https://doi.org/10.1016/j.jebo.2005.03.010>.
- McCright, A.M. (2011) Political orientation moderates Americans’ beliefs and concern about climate change. *Climatic Change*, 104(2), 243–253. <https://doi.org/10.1007/s10584-010-9946-y>.
- Metag, J., Fücks, T. & Schäfer, M.S. (2017) Global warming’s five Germanys: a typology of Germans’ views on climate change and patterns of media use and information. *Public Understanding of Science*, 26(4), 434–451. <https://doi.org/10.1177/0963662515592558>.
- Mills, S. (2020) Personalized nudging. *Behavioural Public Policy*, 1–10. <https://doi.org/10.1017/bpp.2020.7>.
- Osman, M. (2020) *Backfiring, reactance, boomerang, spillovers, and rebound effects: can we learn anything from examples where nudges do the opposite of what they intended?* <https://doi.org/10.31234/osf.io/ae756>.
- Peer, E., Egelman, S., Harbach, M., Malkin, N., Mathur, A. & Frik, A. (2020) Nudge me right: personalizing online nudges to People’s decision-making styles. *Computers in Human Behavior*, 109, 106347. <https://doi.org/10.1016/j.chb.2020.106347>.
- Perino, G., Panzone, L.A. & Swanson, T. (2014) Motivation crowding in real consumption decisions: who is messing with my groceries? *Economic Inquiry*, 52(2), 592–607. <https://doi.org/10.1111/ecin.12024>.
- Pichert, D. & Katsikopoulos, K.V. (2008) Green defaults: information presentation and pro-environmental behaviour. *Journal of Environmental Psychology*, 28(1), 63–73. <https://doi.org/10.1016/j.jenvp.2007.09.004>.
- Reeson, A.F. & Tisdell, J.G. (2008) Institutions, motivations and public goods: an experimental test of motivational crowding. *Journal of Economic Behavior & Organization*, 68(1), 273–281. <https://doi.org/10.1016/j.jebo.2008.04.002>.

- Reiley, D. & Samek, A. (2019) Round giving: a field experiment on suggested donation amounts in public-television fundraising. *Economic Inquiry*, 57(2), 876–889. <https://doi.org/10.1111/ecin.12742>.
- Reisch, L.A. & Sunstein, C.R. (2016) Do Europeans like nudges? *Judgment and Decision Making*, 11(4), 310–325. <https://doi.org/10.2139/ssrn.2739118>.
- Silverman, D., Slemrod, J. & Uler, N. (2014) Distinguishing the role of authority “in” and authority “to”. *Journal of Public Economics*, 113, 32–42. <https://doi.org/10.1016/j.jpubeco.2014.02.003>.
- Sunstein, C.R. (2013) *Impersonal default rules vs. active choices vs. personalized default rules: a triptych*. <https://doi.org/10.2139/ssrn.2171343>.
- Sunstein, C.R. (2017) Nudges that fail. *Behavioural Public Policy*, 1(1), 4–25. <https://doi.org/10.2139/ssrn.2809658>.
- Sunstein, C.R. & Reisch, L.A. (2016) *Climate-friendly default rules*. <https://doi.org/10.2139/ssrn.2796786>.
- Tannenbaum, D., Fox, C.R. & Rogers, T. (2017) On the misplaced politics of behavioral policy interventions. *Nature Human Behaviour*, 1, 1–7. <https://doi.org/10.1038/s41562-017-0130>.
- Thaler, R. & Sunstein, C.R. (2008) *Nudge: improving decisions about health, wealth, and happiness*. New Haven, CN: Yale University Press.
- Yan, H. & Frank Yates, J. (2019) Improving acceptability of nudges: learning from attitudes towards opt-in and opt-out policies. *Judgment and Decision Making*, 14(1), 26–39. <https://www.sas.upenn.edu/~baron/journal/18/181018/jdm181018.pdf>.
- Yeung, K. (2018) Algorithmic regulation: a critical interrogation. *Regulation & Governance*, 12(4), 505–523. <https://doi.org/10.1111/rego.12158>.

SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of this article.

How to cite this article: Bruns, H. & Perino, G. (2021) Point at, nudge, or push private provision of a public good? *Economic Inquiry*, 59:996–1007. <https://doi.org/10.1111/ecin.12981>