Department of
Geography and
Environment

Papers in Environmental Economics and Policy

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Paper No. 32
Geography and Environment Discussion Paper Series

April 2022

What works best in promoting climate citizenship? A randomised, systematic evaluation of nudge, think, boost and nudge+

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April 27, 2022

Abstract

Nudges have been increasingly deployed to deliver climate policies in the last decade. Recent evidence shows nudges are hard to scale–up. So can we use nudges more effectively, or should we rely on other tools of behaviour change? We argue that reflective strategies can enhance nudges by encouraging agency and ownership in citizens. We test this by systematically comparing nudges to reflective interventions like thinks, boosts, and nudge+ over orders of low-carbon meals using an online experiment with 3,074 participants in the United Kingdom. We find all behavioural interventions increase intentions for climate-friendly diets, but encouraging reflection prior to nudging ("nudge+") strengthens these treatment effects. There is no evidence of negative behavioural spillovers as measured by participants' donations to pro-social charities. There is potential for reflective policies in promoting climate citizenship.

Keywords: Nudge, Think, Boost, Nudge+, Climate–friendly diets, Climate citizenship

JEL: C90, D91, I12, Q18, Q58

Acknowledgements: We are grateful to colleagues at LSE (Department of Geography and Environment, The Grantham Research Institute on Climate Change and the Environment, Department of Psychological and Behavioural Sciences), Ralph Hertwig, Till Grüne-Yanoff for their feedback on earlier versions of the manuscript. We also thank the organisers and attendees of the following conferences, where the findings of this paper was presented: Royal Economic Society, University College Dublin, Annual Conference on Economics and Public Policy (Jindal School of Government and Public Policy), UK-China Workshop on Energy and Climate Change Economics (UCL and BIT), LSE Environmental Economics and Policy Seminar Series, OSWEET Seminar Series 2021 on Environment, Behaviour and Experiments, Brunel University (Health Economics and Public Policy seminar series), Max Plank Institute Berlin (Adaptive Rationality Centre), University of Birmingham (Department of Economics:

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TEDE and BEADS research group), Yale University on Climate and Food (GAUC 2021), University of Exeter Business School LEEP Seminar 2021, LSE Economic Geography Seminar 2021, UK Network of Environmental Economists Webinar Series 2021, SABE-IAREP 2021, Grantham Research Institute Behavioural Group, World Bank (The Mind and Development Unit)

Funding Declaration: MMG acknowledges financial support from the Department of Psychological and Behavioural Science, LSE. PJ acknowledges financial support from the Department of Political Economy, King's College London. SB and SM acknowledge financial support from the Department of Geography and Environment, LSE.

Ethics Approval: This research study was approved by the LSE Research Ethics Committee vide reference number 28723.

Competing interests: The author(s) have no conflict of interests to declare.

1 Introduction

Nudges offer simple modifications to the design and framing of choice sets, without limiting any options (Thaler and Sunstein, 2008). They have been generally successful in steering welfare-improving behaviours (Thaler, 2016). Nudging is simple and cost-effective (Benartzi et al., 2017), and most people tend to like it (Hagman et al., 2015; Sunstein, 2016; Reisch and Sunstein, 2016; Jung and Mellers, 2016; Sunstein, 2017b; Loibl et al., 2018; Sunstein, 2019; Sunstein et al., 2019; Pe'er et al., 2019; Reisch et al., 2021). In the last decade, nudging has been increasingly deployed in climate policies (e.g. Gosnell and Bazilian, 2021; Stern, 2011; Dietz et al., 2009), with recent consensus[1] from psychologists (APA, 2022) to do more. The current challenge, therefore, is to enhance nudges to tackle these global challenges more effectively, such as overcoming limitations in their scalability (Mertens et al., 2022; DellaVigna and Linos, 2022; Beshears and Kosowsky, 2020) and delivering them transparently (Bovens, 2009; Sugden, 2009; Wilkinson, 2013; Sunstein, 2015; Nys and Engelen, 2017; Sugden, 2017; Schmidt and Engelen, 2020). So we ask, can we upgrade nudges to address the big problems of our age? Should we rely on other tools of behaviour change, such as thinks and boosts?

To assess what works best in promoting climate citizenship[2], we randomly compare different behavioural frameworks to nudges (Thaler and Sunstein, 2008), namely boosts, thinks, and nudge+. This is the first systematic evaluation of these four behavioural tools, and also first formal evaluation of nudge+. As such, we contribute to a growing comparative literature on nudging versus alternate forms of behaviour change (van Roekel et al., 2022; John et al., 2022; Krawiec et al., 2021; Hertwig, 2017; Bradt, 2019; Franklin et al., 2019). First, we test "nudge+" (Banerjee and John, 2021). Nudge+ are interventions that inform recipients about nudging and enable them to think about it. Nudge+ adds consciousness to the nudge and helps motivated decision–makers make better choices. They address theoretical shortcomings of a nudge in two fundamental ways: (1) they make the nudge completely transparent to its receivers, and (2) they empower receivers to consciously think about their self-need for a nudge, before or after providing them with one. Psychologically, nudge+ works like a hybrid tool that combines fast and slow processes (Kahneman, 2011) of the brain–they enable individuals to think slowly about the nudge. Next, we test purely reflective interventions, such as "thinks" (John et al., 2009) or "system–2" nudges (Sunstein, 2016). These interventions include deliberative policies that educate people to think about their available and affordable choice alternatives before they engage in optimal choice processes. Finally, we test "boosts" (Grüne-Yanoff and Hertwig, 2016; Hertwig, 2017; Hertwig and Grüne-Yanoff, 2017). Boosting relies on building people's competencies to enable them to make better decisions.

We do this by administering a preregistered[3], online experiment to 3,074 participants[4] (Banerjee et al., 2022). The survey experiment, available online here, was designed on Qualtrics and distributed to a pool of 127,488 eligible participants registered on Prolific in two waves[5], using preset filters to exclude (1) experienced participants who had participated in two prior pilot studies and/or (2) non–residents of the United Kingdom. All participants were

[1] for details, see Report of the APA Task Force on Climate Change

[2] Climate citizenship is a form of environmental citizenship, for details see here and Vihersalo (2017)

[3] The experiment was preregistered on Open Science Foundation (OSF) platform and the pre-analysis plan is available here

[4] for power analysis, see here

[5] on 12^{th} and 19^{th} November, 2020

rewarded[6] for their participation time[7], on an hourly basis, based on Prolific reward rates. The survey[8] experiment worked as follows. Participants were entered into a consequential[9], experimental task, where they had to place an order for an online meal delivery. The task was set-up in four stages:

Stage 1: All participants were informed of the rules of the task. They were told they would be presented with a restaurant menu and will have to place an order for an online meal delivery. They were informed that they had a chance to win a food voucher to replicate their choice.

Stage 2: Participants were randomly assigned to one of the nine treatment conditions, mimicking a behavioural policy, or the control[10] condition. The treatment design plan is available here.

Stage 3: Participants were redirected to a check-out screen to place their intended order for an online meal delivery.

Stage 4: Participants were given the option to donate to a charity. They were reminded that, if successful, their final voucher payment will be adjusted for any donations made at this stage.

Each treatment condition in stage 2 corresponded to a specific type of the four behavioural policies, namely nudge, think, boost or nudge+, that manifested as a variation of the restaurant menu in the control condition. For example, our first nudge was a green *default*[11], where participants were automatically opted in to a shorter menu consisting of sustainable food items with an option to opt-out. Our second nudge was a carbon *labelled*[12] menu which used a traffic lighting scheme to colour code all food items by the carbon intensiveness of the main ingredient[13] in the dish. These nudges were upgraded to a nudge+ in two ways. In "nudge+ with information", we combined the green default and the labelling nudge with an information disclosure that informed recipients of the purpose and the construct of nudge they had received. Set-up this way, nudge+ with information advances the literature on transparent nudging (Loewenstein et al., 2015; Bruns et al., 2018). Then, in "nudge+ with reflection", we provided participants with an opportunity to think about a pledge[14] to a

[6]the average completion time for the survey was 24 minutes (σ of 10.4 minutes) and the average reward was £3.35 GBP/hour

[7]rewarding participants based on actual completion time minimises concerns of strategic behaviour by participants to complete surveys hastily

[8]The survey had four parts, of which the experiment took place in the third part. In its first part, after seeking explicit consent and informing participants about study details, we measured their mood, attitudes and beliefs using standard survey questions. Then, in the second part, participants revealed their risk and time preferences using an incentive-compatible monetary task. Participants were randomly selected to receive an Amazon voucher (up to £80) equivalent to their earnings in the risk and time preference task. In this part, we also measured baseline altruism levels as participants were asked to donate a part of these monetary earnings to a charity of their choice. In the last part, we measured participants palatability towards the food menus in the experiment, and re-assessed their mood. Standard socio-demographic characteristics were also measured.

[9]Participants were randomly selected to be awarded a restaurant (or, equivalent Amazon) voucher worth £20 to replicate their order

[10]In the control, participants were shown a restaurant menu which had 36 main course items. These items were chosen from Deliveroo and Just Eat top 100 items ordered in the UK in 2019 and were adjusted following pilot surveys. Each menu item was priced at less than or equal to £20. Of these 36 items, 18 items each were vegetarian and non-vegetarian

[11]these green default represent standard set-menus in restaurant business

[12]carbon labelling was recently adopted at COP26 in Glasgow, for details see UK Cabinet office, 2021

[13]This categorisation was done using the McCance and Widdowson's CoFID user guide(England, 2021).

[14]the pledge explained the meaning of a sustainable diet. Then it asked people to think about whether they would accept such a pledge. They could choose to be indifferent to it. For those who accepted the pledge, or were indifferent to it, goal motivations were assessed to comply with their reflective outcomes.

sustainable diet, either before or after providing them with the default nudge. In this way, nudge+ with reflection prompts people to think about their own (dietary) preferences and where they want to be, which is made easier with the nudge. It helps ease goal compliance after enabling people to realise their true goals and preferences.

To evaluate the full effect of deliberation, our think intervention was designed to test what happens when people actively think through all their choices. Unlike its nudge+ counterpart, the think treatment did not provide people with the default nudge to ease compliance. After reflecting on the pledge and making a decision on whether they wanted to take it, participants were enrolled into a second reflective task where they had to customise a self-nudge[15] from a set of pre–available nudges, the default, the labelling or full menu. Finally, we also designed two boosting interventions. Our first boost was a quick rules intervention, which asked participants to memorise and use three new food rules while choosing what to eat[16]. After this, they were provided with the full menu to test their new heuristics and place an intended order. Our second boost was an implementation intentions (Gollwitzer, 1999), where participants were asked to make *if-then* goal plans to follow a sustainable diet. Each participant made six goal plans. These boosts differ in their operational design. While the quick rules boost is heuristically–driven, as it updates existing decision–rules; the implementation–intentions boost is reflective, as it engages people to think through actionable strategies to achieve their future goals. The latter is comparable to a think, where people could devise self–nudges to follow through with their pledges.

A common critique to nudging and its applications to behavioural science has been its conceptual malleability. Not every behavioural change intervention is a nudge (Hansen and Jespersen, 2013; Baldwin, 2014; Oliver, 2017). Our experimental design contributes to clarify this scholarly debate by highlighting the practical similarities and differences in the application of these different behavioural frameworks. For example, nudge+ retains the simplicity of nudging, in automating decision processes which makes nudges very attractive to people and policymakers. Yet it upgrades the nudge by making it transparent or adding a reflective prompt to it. Contrarily, boosts and thinks rely on a purely deliberative cognitive channel of behaviour change (Banerjee, 2021) as they school people to build a better repertoire of skills. Nonetheless, boosts, thinks and nudge+ share a common feature, that they all work towards empowering citizens so that they can make better decisions for themselves. Consequently, these toolkits put a greater emphasis on restoring human agency and autonomy which offer advantages to nudging. First, reflection can overcome the failing effectiveness of nudges. There is the possibility that nudges are likely to be ignored, or attenuate in effects over time when taken away (Allcott and Rogers, 2014). So, when people are made to think through the nudges, such as in nudge+, or build their own capacities as in boosts or thinks, they can own the process of behaviour change, resulting in more effective newer behaviours. Second, they strengthen the ethical construct of behaviour change. For example, covert nudges that compromise autonomy (Bovens, 2009) are likely to be disproved (Sunstein, 2016), more so by people with strong antecedent preferences (Sunstein, 2017a). Nudges that crowd out motivation, can generate reactance towards better policy alternatives (Hagmann et al., 2019; Maki et al., 2019). Reflective strategies offer a fix to this possible disengagement of citizens with nudges by enabling a participatory approach to nudging. We test these claims through our randomised, systematic evaluation.

We present three main experimental findings. First, we find all behavioural policies,

[15] see self-nudging (Reijula and Hertwig, 2022)

[16] these rules were to eat (a) a balanced diet, (b) meat occasionally, and (c) mostly vegetarian items

namely nudge, boost, think and nudge+, are significantly and substantially effective in minimising the intended consumption of carbon-intensive foods. This reaffirms the credibility of behaviour change strategies to successfully mitigate dietary carbon emissions (Gravert and Kurz, 2021; Garnett, 2021; Cadario and Chandon, 2020; Kurz, 2018) in this transition towards net–zero climate targets (Stark et al., 2019; Carmichael, 2019). Second, we posit new ways of scaling–up the effectiveness of nudges by engaging citizens in nudging (nudge+). We find when citizens are given an opportunity to reflect on their own preferences before being presented with the nudge, it strengthens the treatment effects of the nudge. Compared to its standalone nudge, nudge+ with reflection reduces carbon emissions associated with intended dietary choices by an additional 30%. While building transparency in the nudge is only as good as the nudge in itself, it is self-reflection that remains key to scale–up its effects. Third, we contribute to a growing literature on behavioural spillovers by providing a causal estimate (Alacevich et al., 2021; Maki et al., 2019) of any treatment induced indirect behavioural effects. We do not find any evidence of negative behavioural spillovers, as measured by participants' charitable donations.

These experimental findings generate policy insights relevant for a growing food–delivery sector, mainly as diets continue to contribute substantially to greenhouse gas emissions, globally as well as in the United Kingdom (Gerber et al., 2013; Poore and Nemecek, 2018). Recently, there has been an unprecedented expansion of this market (Nunn, 2021), currently valued at more than 150 billion USD globally (Ahuja et al., 2021). While it has already expanded four-to-seven times during the pandemic, it is expected to grow even more. Our online experiment was simulated to mimic an online meal delivery experience to make it real for participants and to minimise potential concerns of hypothetical bias. Our findings suggest that food delivery companies can contribute to net–zero goals by introducing small design changes to their user–engagement platforms. For example, our best case of a nudge+, where reflection precedes the nudge is readily implementable through push–in notifications that engage with citizen's pro–environmental preferences before they check out to order their meal. These will be low-cost interventions for citizens who can easily opt-out if they do not want to comply with the reflective prompts. Long–term customer rewards will further incentivise such in–app interactions.

We discuss our experimental results and the broader policy implications of embedding reflection in (behavioural) public policy in the next section.

2 Methods

2.1 Testable hypothesis

One can consider nudge+ as an attempt to upgrade nudges and scale them up by making citizens a part of it. The ability of oneself to think slowly about the nudge, in fact, can improve the uptake of the nudge, particularly when the nudge is effective (i.e. leads to positive treatment effects). In this way, a nudge+ is conjectured to be more effective than its standalone nudge counterpart. A nudge+ is also fully transparent to the receiver. Hence, it should improve people's self-perceived autonomy, lest be unchanged. Thinking through the nudge and owning it reduces moral warm-glow effects as people are no longer tricked into good behaviours. For those who respond to the nudge+, they truly want to improve their intentions, actions and behaviours. Such hybrid interventions save people substantial cognitive effort when compared to purely reflective policies. Hence, a nudge+ is also conjectured to produce more effective outcomes compared to standalone reflective policies. We expect a nudge+ to produce optimal behaviour change along the reflective spectrum in economic policies.

Whilst nudge+ effects are conjectured to hold true for the population on average, we believe that increased deliberation comes at substantial cognitive costs to people (and economic costs to the society). Hence, the effectiveness of these economic policies will increase cognitive fatigue in people after treatment. By extension, people who are cognitively fatigued should be less responsive to nudge+. In validating this theory of nudge+, we propose these pre-registered hypotheses that we test using an online survey experiment.

Research Question 1: Do behavioural policies promote climate-friendly behaviours compared to doing nothing?

Hypothesis 1: A behavioural policy will significantly improve pro-environmental behaviours compared to the control condition.

Research Question 2: Does adding reflection in the nudge improve climate-friendly behavioural outcomes?

Hypothesis 2: A nudge+ will be more effective than its standalone nudge.

Hypothesis 3: A nudge+ with reflection will be more effective than standalone reflection.

Hypothesis 4: A nudge+ with reflection will be more effective than a nudge+ with information.

Research Question 3: Do behavioural policies promoting climate-friendly behaviours lead to any adverse behavioural spillovers?

Hypothesis 5: A behavioural policy will not produce adverse behavioural spillovers compared to the control condition.

Research Question 4: Are some people more responsive to behavioural policies than others?

Hypothesis 6: Treatment effects of behavioural policies will vary by participant's prior level of (a) anxiety (b) tiredness and (c) calmness.

Research Question 5: Do behavioural policies lead to loss of autonomy?

Hypothesis 7: A behavioural policy will lead to no change in self-perceived autonomy of people compared to the control condition.

2.2 Variables

We use *Greenhouse gas emissions (GHGe)* as a proxy for participants' dietary choices in the experimental task. In particular, the outcome measure corresponds to the life cycle emissions of the main ingredient[17] in their chosen food item. The GHGe variable ranges from 0.8 to 68.8 kilos of CO2e, with an average emissions score of 17.1 kilos of CO2e. For robustness, we also measure such choices discretely with an ordinal variable called *Carbon Intensity(CI)* [18]. We measure indirect behaviours as participants' level of *Charitable Donations* in stage

[17]This variable was constructed as follows: we identify the primary food type and ingredient of each dish on our menu using the McCance and Widdowson's CoFID user guide(England, 2021). Each food item is assigned a carbon score (in kgCO2e) using the UK Greenhouse gas emissions scale developed by Scarborough and colleagues(Scarborough et al., 2014); for details, see here

[18]The GHGe variable has discrete jumps due to measurement of carbon intensiveness of each food item. To account for these value breaks, we further discretise the GHGe outcome into an ordinal variable. CI is an ordered categorical transformation of the GHGe outcome variable. It has nine categories, starting with the

4 of the experimental task. This is a continuous variable and reflects pro-social charitable contributions by participants.

Our main explanatory variables are dummy variables, called $Treatment_i$, indicating experimental conditions to which participants were randomly assigned to, such that

$$Treatment_i = 1, \{\text{if participant is in } i^{th} \text{ experimental condition, 0 otherwise}\}$$
$$\forall i = Treatment_{default}, ..., Treatment_{nudge+reflection}$$

Further, we construct variables for participants' mood measures, namely *anxiety*, *tiredness*, and *calmness*, measured on a 5-point likert scale. To measure differences in levels of autonomy, we construct $diff_{autonomy} = autonomy_{posttreat} - autonomy_{pretreat}$, where we measure $autonomy_t$ on a 5-point likert scale $\forall t = \{pretreat, posttreat\}$. We also construct other pre-registered covariates to use as controls in regressions and to check for balance of means in assessing randomisation (for details, see here).

2.3 Empirical Strategy

We test hypothesis H1 by measuring the average treatment (intent–to–treat) effect of being assigned to an experimental condition, relative to the control group. We do so using a regression-based least-square approach, which in its simple form corresponds to a means-comparison of *greenhouse gas emissions* between the treatment and control group, as outlined by specification [1].

$$\text{GHGe} = \alpha + \sum \beta_i \text{Treatment}_i + \epsilon \quad [1]$$
$$\forall i = Treatment_2, ..., Treatment_{10}$$

For robustness[19], we then control for n covariates, selected using a lasso-based regression technique (Bloniarz et al., 2016), outlined by specification [2]

$$\text{GHGe} = \alpha + \sum \beta_i \text{Treatment}_i + \sum \delta_k \text{Control}_\text{k} + \epsilon \quad [2]$$
$$\forall i = Treatment_2, ..., Treatment_{10} \quad \& \quad k = Control_1, ..., Control_n$$

Finally, in order to test hypotheses H2-H4, which compares a nudge+ to its corresponding nudge condition, we re–use model specification [2] by setting the nudge+ condition as our reference category, instead of the control group.

Next, we test for behavioural spillovers to validate hypothesis H5. In its first definition, behavioural spillovers are considered as the direct causal effects of a policy intervention on people's indirect behaviours. In following this definition, we re–use model specification [2] with *Charitable Donations* as our outcome variable of interest. This is specified in specification [3].

$$\text{CharitableDonations} = \alpha + \sum \beta_i \text{Treatment}_i + \sum \delta_k \text{Control}_\text{k} + \epsilon \quad [3]$$
$$\forall i = Treatment_2, ..., Treatment_{10} \quad \& \quad k = Control_1, ..., Control_n$$

In its second definition, we re-estimate behavioural spillovers as the effect of changes in *GHGe* on *Charitable Donations*. To account for endogeneity in the measurement of the *GHGe* variable, we use a two-stage least-square regression-based approach[20]. Here, we

food type: beans, and lentils at the lowest level (0) of carbon emissions, to the food type: ruminant meat at the highest level (8)

[19] for additional robustness, we use a generalised ordered logistic regression approach, using *Carbon Intensity*. Our findings are robust, and these results are available on request.

[20] for robustness, we use a Baron and Kenny (1986) mediation analysis to determine if experimental conditions mediate behavioural spillover effects

use our initial random assignment to experimental conditions to instrument for changes in emissions, which are then used to predict any charitable donations. Set up this way, we can use model specification [2] as our first-stage reduced-form equation. The TSLS estimator can be estimated from a second-stage model specification as outlined in [4].

$$\text{CharitableDonations} = \alpha + \sum \beta_i^{TSLS} \widehat{\text{GHGe}}_i + \sum \delta_k \text{Control}_\text{k} + \epsilon \quad [4]$$
$$\forall k = Control_1, ..., Control_n$$

While the first definition proposes a direct causal estimate of behavioural spillovers resulting from policy intervention, we believe that the second definition identifies the pathway of this indirect behaviour change. This is because spillovers effects are best thought of as cascading or ripple effects mediated by a change in direct behaviours (Shreedhar and Galizzi, 2021; Margetts and Kashima, 2017; d'Adda et al., 2017; Dolan and Galizzi, 2015; Lanzini and Thøgersen, 2014; Truelove et al., 2014).

We also test for any heterogeneity in our average treatment effects. In order to test hypotheses H6a-c, we re-use model specification [2] by adding a linear interaction with our pre-specified mood measures, namely, *anxiety*, *tiredness*, and *calmness*. This is outlined in specification [5].

$$\text{GHGe} =$$
$$\alpha + \sum \beta_i \text{Treatment}_i + \sum \gamma_{ij} (\text{Treatment}_\text{i} * \text{Mood}_\text{j}) + \sum \delta_k \text{Control}_\text{k} + \sum \rho_j \text{Mood}_\text{j} + \epsilon \quad [5]$$
$$\forall i = Treatment_2, ..., Treatment_{10} \quad \& \quad k = Control_1, ..., Control_n \quad \& \quad j =$$
$$Mood_{anxiety}, Mood_{tired}, Mood_{calm}$$

Finally, we assess if any of these experimental conditions lead to a change in participants' levels of self-perceived autonomy, as set out in hypothesis H7. In this, we re–use model specification [2] once again, by using $diff_{autonomy}$ as our outcome variable. We outline this in specification [6].

$$\text{diff}_\text{autonomy} = \alpha + \sum \beta_i \text{Treatment}_i + \sum \delta_k \text{Control}_\text{k} + \epsilon \quad [6]$$
$$\forall i = Treatment_2, ..., Treatment_{10} \quad \& \quad k = Control_1, ..., Control_n$$

We follow Young (2019) to account for joint and multiple hypotheses testing. All analysis has been performed using Stata 17.

3 Results

All participants were randomised[21] effectively in the ten different experimental conditions. We also satisfy our ex-ante sampling requirements[22]. As such, our study is powered to test our pre-registered confirmatory hypotheses (see methods 2.1). Next, we follow Hadi (1994) in identifying and removing 65 outliers by the age of participants and their time taken to complete the survey[23]. The remaining sample consists of 3,009 participants, of which 2,494 participants are residents of the United Kingdom. Our sample consists of young adults with a mean age of 29 years (σ=10.73). It is relatively balanced by gender with 52% male and 46% female representation. More than a half of the participants are in full- or part-time employment, and 44% of them are students. We recruit only English-speaking participants,

[21]for balancing checks, see here
[22]for sensitivity analysis, please see here
[23]Our sample has young adults, representative of age of online food delivery customers. We remove older adults who can be outliers.

with 29% of them self-reporting English as their first language. Furthermore, all participants are well-educated with at least 50% having a first degree from the university or more. The sample is pre-dominantly white in ethnic origin, and 85% of them have religious affiliations. These sample characteristics by the broad treatment categories are provided in Table 1.

Table 1: Descriptive statistics by treatment categories[24]

	Control	**Heuristic**	**Reflective**	**Hybrid**	**All**
			Outcomes		
GHG emissions	$\mu = 23.48$	$\mu = 12.12$	$\mu = 9.14$	$\mu = 9.83$	$\mu = 12.34$
	$\sigma = 28.35$	$\sigma = 22.49$	$\sigma = 17.27$	$\sigma = 18.21$	$\sigma = 21.34$
Donations	47.6%	47.15%	45.93%	45.88%	46.45%
			Demographics		
Age	$\mu = 29.12$	$\mu = 29.55$	$\mu = 28.60$	$\mu = 29.14$	$\mu = 29.17$
	$\sigma = 10.51$	$\sigma = 11.001$	$\sigma = 10.15$	$\sigma = 10.84$	$\sigma = 10.73$
Male	51.33%	51.60%	51.40%	53.04%	52.11%
First Degree or more	55.67%	52.27%	54.71%	51.38%	52.74%
Employed	49%	51.93%	50.91%	52.38%	51.61%
Student	41.67%	42.76%	45.45%	44.45%	43.87%
Christian	41%	39.78%	46.12%	45.62%	43.50%
White-UK	35.67%	32.38%	32.73%	32.86%	32.97%
Married	27.33%	31.05%	27.93%	27.86%	27.86%
			Survey characteristics		
Score	99.55	99.44	99.44	99.47	99.46
Completion time	23.76	24.16	25.27	24.22	24.37
Observations	298	902	600	1,191	3,009

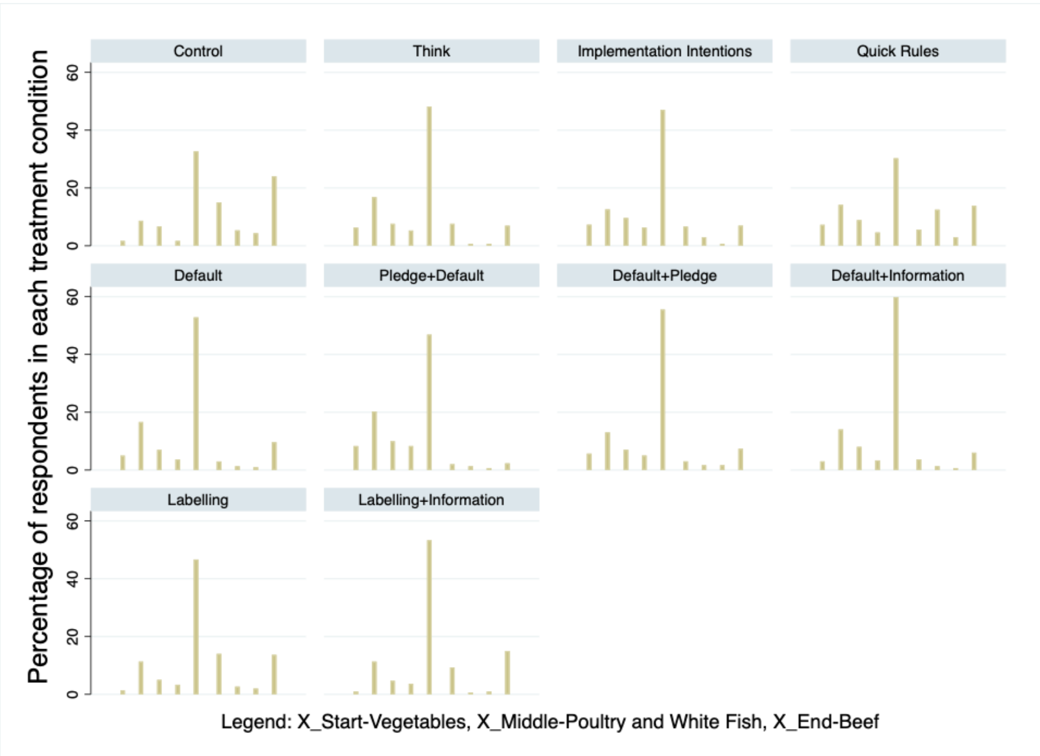

Figure 1: Frequency plot of meal orders across experimental conditions

The mean emissions from all intended meal orders is 12.34 kilos of carbon–equivalent (CO_2e) and the modal food type consumed is white fish and poultry, consistent across all treatments including the control condition. We find that the convergence to this modal food category is further exacerbated by our treatments[25] (see Figure 1). This has implications for our average treatment effects, as we discuss later, since a simple shift from a ruminant–based food item to a poultry– or fish–based food item can reduce emissions by ten times or more (Scarborough et al., 2014; Poore and Nemecek, 2018). In terms of charitable donations, we find donations are distributed with three clear peaks: participants are likely to donate nothing, half their endowments or mostly everything. Altruistic people, those who donate their monetary earnings from the risk and time preference tasks, are more likely (ρ=0.495; p<0.0001) to donate in the post-treatment task. 29% of our sample chose to donate to an environmental charity[26], whereas the remaining donate to pro-social charities[27].

Result 1: All behavioural interventions significantly promote intentions for low–carbon diets.

Our first finding tells us that nudges, thinks, boosts, and nudge+ are all significantly effective in reducing the intended emissions over orders of meals in the experiment. Figure 1 plots the mean emissions in these different treatments, including the control condition. Table 2 further summarises these average treatment (intent–to–treat) effects of being randomly assigned to an experimental condition on the intended *greenhouse gas emissions*. Columns 1 corresponds to linear regression, while column 2 repeats this analysis for robustness by controlling for covariates chosen using an adaptive lasso-based selection technique. In absolute terms, the green default cuts emissions by 53% (μ=-12.475, σ=1.669), on average, relative to the baseline. Other purely heuristic policies, like the labelling nudge and quick rules, reduce emissions by 35% and 30%, respectively. Thus, simply re-directing people's attention towards pro-environmental options using nudging or quick rules boosting increases intentions to consume climate-friendly items significantly.

So, what about the nudge+? First, consider nudge+ with information. We find that adding disclosures to the default nudge reduces absolute emissions by 63% (μ=-14.768, σ= 1.673), on average, compared to the baseline. In absolute terms, this reduction is greater than that offered by the standalone default. Contrarily, adding disclosures to the labelling nudge reduces these emissions by 36% (μ=-8.497, σ=1.671), which is similar to the absolute reductions offered by the standalone labelling nudge. Now, consider nudge+ with reflection. Facilitating reflection on one's own preferences before re-directing attention towards pro-environmental choices with the default reduces absolute emissions by 76% (μ=-17.905, σ=1.669), on average, relative to the baseline. However, when the sequence of this nudge+ is reversed, such that participants are first steered towards pro-environmental choices with the nudge, and then facilitated to reflect and revisit choices, the intent-to-treat effect is attenuated. The average absolute reduction offered by this nudge+ variant is 57% (μ=-13.396, σ=1.673), similar to its standalone nudge.

Finally, do purely reflective strategies work? We find when participants are made to reflect fully on their decisions first (1) on the offer of the pledge, (2) and subsequently on how to follow through with it, either by choosing a self-nudge (such as in the *think* condition) or by making goal plans (such as in the *implementation intentions* condition), the absolute

[25] a two-way tabulation test of the type of food consumed and treatments returns a chi^2=468.9978 at p¡0.00001

[26] WWF, Keep Britain Tidy, Greenpeace, PETA, and Friends of Earth

[27] British Heart Foundation, Samaritans, Children in Need, UNICEF, LGBT Foundation and Abortion Rights

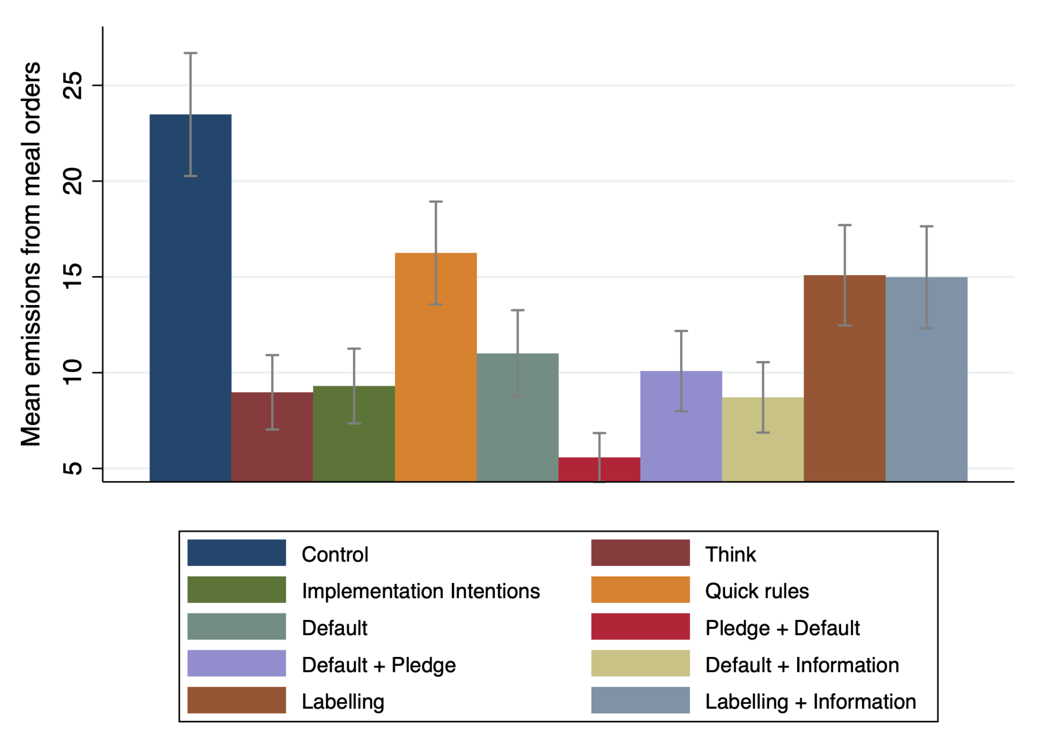

Figure 2: Mean emissions from meal orders in experimental conditions

emissions are reduced by just more than 60%, on average, relative to the baseline.

Result 2: Adding reflection before the nudge improves climate-friendly behavioural outcomes.

Next, if we rank these behavioural policies in terms of their effectiveness relative to the control group, we find the nudge+ is at least as good as its corresponding standalone nudge or reflection, if not better. However, we have not yet compared these policies directly so far. Consequently, to assess if these pairwise differences are statistically significant, we now set our comparison directly to nudge+ categories. These findings are listed in Table 2 in columns 3-6, which correspond to linear regressions, controlling for covariates as selected by an adaptive lasso–based technique. As we move across these columns, we find average treatment effects of an experimental condition with respect to the *default+information* (column 3), *labelling+information* (column 4), *pledge+default* (column 5) and *default+pledge* (column 6), respectively.

We find that nudge+ with information is no better than the nudge, boost or think. Adding these information disclosures to the default or labelling nudge do not offer any significant reductions in emissions. As an exception, the default nudge with information disclosures is better than the quick rules boosts. However, this is not true for the labelling nudge with information disclosures. We then find nudge+ with reflection offers significant emissions reduction. Adding reflection to the default nudge significantly improves intentions for climate-friendly diets compared to all other treatments. Figure 2 shows these treatment effects, relative to this nudge+ category, with 95% confidence intervals.

Nonetheless, enabling participants to think about their own preferences is effective only when it precedes the nudge (Col 5). When people are steered with a nudge first, any reflection that follows the nudge fails to modify initial choices. These findings in column 5 validate our confirmatory hypotheses that a nudge+ can be more effective than its standalone nudge

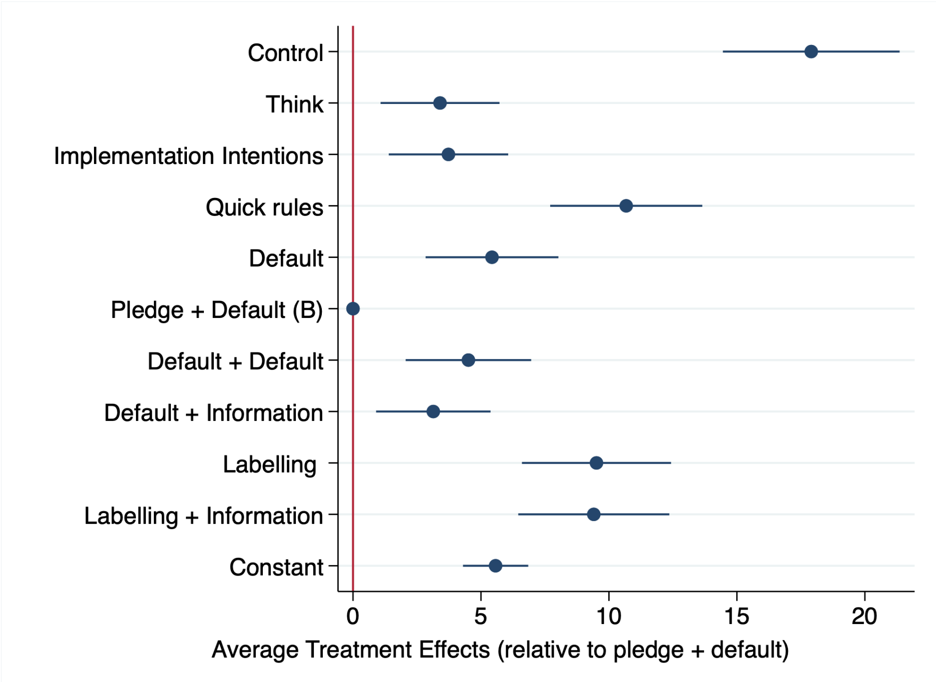

Figure 3: Average treatment effects relative to pledge before default (nudge+)

but with the caveat that it is not true for nudges combined with information disclosures. Moreover, we test for pairwise differences between purely reflective strategies, particularly, the standalone think (but also implementation intention boost) and the nudge to assess if the effectiveness of the nudge+ with reflection is purely driven by the reflective component in it. We fail to find any significant pairwise differences between them. Thus, reflection by itself cannot scale–up these emission reductions. However, when combined with the nudge sequentially, reflection stands to offer substantial benefits.

Further, our treatment effects are not driven by participant's time spent on the survey. Being randomly assigned to a treatment condition does not significantly correlate with the time taken to complete the survey. In our experiment, therefore, it is unlikely to have triggered demand effects (Mummolo and Peterson, 2019). We do not find any evidence of heterogeneity[28] in treatment effects of these behavioural policies, by pre-experimental mood levels of participants, as conjectured in our pre-analysis plan (see methods 2.1). Being exposed to these treatments does not change participants' self–reported levels of perceived autonomy.

Result 3: Behavioural interventions do not lead to negative behavioural spillovers.

There is increasing interest in measuring behavioural spillovers in economics and psychology (Alacevich et al., 2021; Galizzi and Whitmarsh, 2019; Maki et al., 2019). However, there is limited agreement on identification of causal pathways effecting such indirect behaviour change. We conduct a narrative review of studies estimating spillover effects in behavioural economics; see here.

We then contribute to this literature by estimating spillover effects from our designed behavioural interventions using two commonly accepted definitions of spillover effects (see methods 2.3). In its first definition, we consider behavioural spillovers as direct causal ef-

[28]for heterogeneity analysis see here

fects of policy interventions on indirect behaviours. We test this using linear regression[29] of *Charitable Donations* on *Treatment Indicator*. We do not find any statistically significant evidence to suggest that random assignment to a behavioural economic policy leads to any significant difference in pro-social contributions relative to the control condition, on average. While this first definition proposes a direct causal estimate of behavioural spillovers resulting from policy intervention, we believe it fails to identify the pathway of this indirect behaviour change. This is because spillovers effects are best thought of as cascading or ripple effects mediated by a change in direct behaviours (Shreedhar and Galizzi, 2021; Margetts and Kashima, 2017; d'Adda et al., 2017; Dolan and Galizzi, 2015; Lanzini and Thøgersen, 2014; Truelove et al., 2014). As such, we use re–test for spillovers using a two–stage least–squares regression–based approach, where we use our initial random assignment to an experimental condition to instrument for changes in emissions, which are then used to causally infer effects on donations to charities. Yet again, we do not find any evidence of (adverse) spillover effects. These results are available online here.

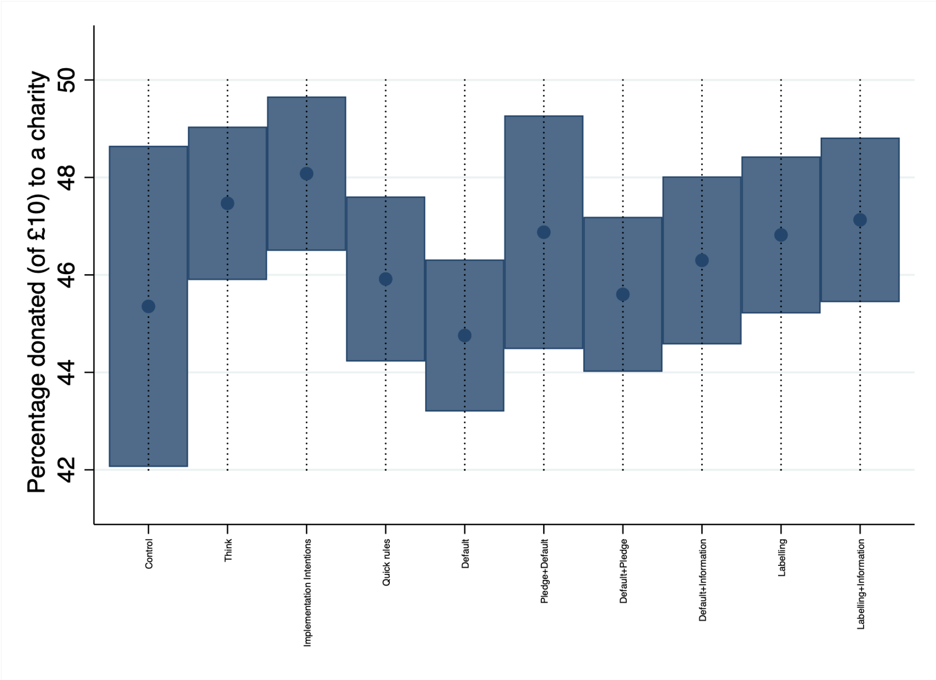

Figure 4: Average donations to a charity across treatments

[29] we check for robustness by controlling for covariates

GHG emissions	(1)	(2)	(3)	(4)	(5)	(6)
Control	Baseline	Baseline	13.583	7.506	16.996	12.849
	-	-	(1.628)	(1.655)	(1.644)	(1.632)
	-	-	[0.0052]	[0.00067]	[0.00067]	[0.00098]
Quick Rules	-7.23	-6.983	6.599	0.523	10.013	5.866
	(1.666)	(1.623)	(1.618)	(1.630)	(1.612)	(1.616)
	[0.00084]	[0.00077]	[0.00091]	[0.78169]	[0.00048]	[0.00245]
Default	-12.475	-12.230	1.353	-4.724	7.766	0.618
	(1.669)	(1.622)	(1.619)	(1.636)	(1.623)	(1.621)
	[0.00045]	[0.00044]	[0.39618]	[0.00226]	[0.00259]	[0.69587]
Labelling	-8.497	-7.493	6.089	0.013	9.503	5.356
	(1.671)	(1.629)	(1.619)	(1.626)	(1.621)	(1.621)
	[0.00087]	[0.00016]	[0.00092]	[0.99229]	[0.00037]	[0.00148]
Default+Information	-14.768	-13.583	Baseline	-6.076	3.413	-0.734
	(1.673)	(1.628)	-	(1.632)	(1.624)	(1.621)
	[0.00041]	[0.00081]	-	[0.00038]	[0.03632]	[0.63466]
Labelling+Information	-8.497	-7.507	6.077	Baseline	9.489	5.343
	(1.671)	(1.655)	(1.632)	-	(1.624)	(1.628)
	[0.00077]	[0.00022]	[0.000001]	-	[0.00062]	[0.00097]
Pledge+Default	-17.905	-16.996	-3.412	-9.489	Baseline	-4.147
	(1.669)	(1.643)	(1.624)	(1.624)	-	(1.623)
	[0.00071]	[0.004]	[0.03657]	[0.00069]	-	[0.0125]
Default+Pledge	-13.396	-12.849	0.724	-5.342	4.167	Baseline
	(1.673)	(1.632)	(1.621)	(1.627)	(1.623)	-
	[0.00028]	[0.00044]	[0.6347]	[0.00093]	[0.01223]	-
Pledge [Think]	-14.505	-12.807	0.776	-5.3	4.189	0.042
	(1.667)	(1.642)	(1.619)	(1.612)	(1.612)	(1.618)
	[0.00097]	[0.00086]	[0.63899]	[0.00247]	[0.01272]	[0.97657]
Impl. Intentions	-14.176	12.031	1.551	-4.524	4.964	0.818
	(1.668)	(1.669)	(1.639)	(1.631)	(1.623)	(1.633)
	[0.00014]	[0.00083]	[0.35944]	[0.00774]	[0.00384]	[0.64819]
Constant	23.477	35.113	21.53	27.607	18.117	22.264
	(1.182)	(7.896)	(7.957)	(7.956)	(7.917)	(7.953)
Observations	3009	2991	2991	2991	2991	2991
R-squared	0.0544	0.1298	0.1298	0.1298	0.1298	0.1298
Controls	No	Yes	Yes	Yes	Yes	Yes

Table 2: Intent to Treat effects[30]

[30]Notes: OLS estimates of specification [1] in column 1, [2] in columns 2-6 (with baseline set to control, default+information, labelling+information, pledge+default, and default+pledge). Robust standard errors reported in parentheses. Young (2019) randomised p-values in box brackets. Columns (2-6) includes control variables. The list of controls included correspond to a Lasso-based selection technique, and include indicators of palatability towards menu, dietary styles, pro-conservation beliefs, gender, climate change scepticism, age, scores on healthy eating index, ONS measures of anxiety and life satisfaction, beliefs for command and control regulation, religious beliefs, effect of COVID-19 on income, and whether one's favour the environment over

4 Discussion

In this paper, we provide experimental insights from the first randomised and systematic evaluation of nudges, thinks, boosts, and nudge+. We find all four behavioural tools are significantly effective in promoting climate citizenship by improving intentions of climate-friendly diets. However, adding reflection before the nudge improves the effectiveness of green nudge policies. This nudge+ with reflection is better than nudges combined with informational disclosures or purely reflective strategies. While we do not find evidence[31] to suggest that adding an information disclosure to a nudge always improves its effectiveness our evidence reaffirms findings in the literature that transparency about the nudge, in the form of disclosures, does not generate reactance, on average. We also do not find any evidence to support the claim that behavioural policies reduce autonomy or lead to negative spillovers.

Through our work, we also introduce the first formal tests of nudge+ to the literature in behavioural science and economics. We show these hybrid tools of cognition can offer us a way to enhance existing nudges by involving citizens in the process of behaviour change. While nudging simply changes the design and framing of choice architecture presented to people, the nudge+ framework can strengthen nudges by empowering receivers of the nudge to make good decisions for themselves, albeit guided with the nudge in place. At face value, a nudge+ is merely an informational tool that helps people become consciously aware of nudging, and think about the reasons that they might need a nudge. However, they can be effective in scaling–up nudges to deliver climate policies. Further applications reasonably extend beyond promoting climate citizenship, to boost vaccine uptake by citizens, improve job search outcomes and financial decisions, limit reactance to policies, and prevent sludging, tests of which are underway. Contrary to the perception that nudges work best when they are in the dark (Bovens, 2009; Hansen and Jespersen, 2013), we also show that adding transparency to a nudge, in the form of disclosures, does not reduce its effectiveness. This reaffirms findings in the literature that transparent nudging is as good as nudging (Loewenstein et al., 2015; Bruns et al., 2018) and does not necessarily imply reactance from citizens. Nonetheless, denying citizens the right to engage with the nudge can actually dampen its effects and limit its true purpose in facilitating welfare improving behaviours. With a nudge+, the traction of the nudge significantly increases.

Our results generate novel insights for policy makers who design the choice architecture to include citizens in the process of behaviour change. With nudge+, we propose easy modifications to nudges where autonomy of citizens are respected in how nudges are designed for them. While our experimental findings are promising for the future of nudge+ policies, there still remains a lot to be done. While we believe that scaling–up nudges by reflectively empowering citizens may deliver persistent changes, we still need to rigorously evaluate the long-run effects of the nudge+ interventions, and to compare them with those of nudges. A nudge+ will be effective only when people are motivated to change behaviours. Unfortunately, this has inherent limitations. A nudge+ can generate reactance, arguably stronger than a nudge, from those that resist them. If reflection makes antecedent preferences stronger, then a nudge+ will do worse. These welfare effects of a nudge+ should be theorised, with more empirical tests planned. Unlike the current experimental set-up which relies on intended behaviours, we will need tests to validate our findings with actual behaviours in the field. Nonetheless we are confident that if we design nudges transparently and reflectively, we can prove them even more effective to sustain behavioural change.

economic growth. We follow Young (2019) to account for joint and multiple hypotheses testing.

[31] study was not designed by power to detect pairwise differences

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