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Do nudges make use of automatic processing? Unraveling the effects of a default nudge under type 1 and type 2 processing

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ABSTRACT

Nudges have become increasingly popular among policymakers as a tool to stimulate desirable behavior for individuals or society. One of the most prevailing assumptions of nudges is that they make use of automatic processing. Yet, this assumption has received little attention in experimental research. In two preregistered and high powered studies, we investigated this hypothesized working mechanism by using a nudge that has most typically been described as a Type 1 nudge: defaults. In both studies, we used a scenario in which participants could choose from a list of green amenities, which were either preselected (opt-out condition) or not (opt-in condition). In Study 1, we investigated the effectiveness of this default nudge under Type 1 processing by manipulating cognitive load. In Study 2, we investigated its effectiveness under Type 2 processing by explicitly instructing half of the participants to deliberate upon their choice. Both studies revealed strong and robust evidence for the default effect. Study 1 further revealed that this default effect was statistically equivalent under cognitive load. Study 2 revealed that the default effect was not attenuated when participants deliberated upon their decision, but instead showed a main effect of deliberation. Together, this implies that default nudges are not dependent on elaborate processing in order to be effective, but that deliberation can in parallel lead to different choice outcomes.

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Nudging; default; choice architecture; dual processing; dual systems

Over the past decade, public policymakers have embraced choice architecture interventions as a means of stimulating desirable behavior for individuals or society at large. These interventions are oftentimes called nudges, which are defined as simple changes in the choice architecture that alter behavior in a predictable way without forbidding or interfering financially with any of the options (Thaler & Sunstein, 2008). This term, nudging, comprises a wide variety of interventions that share the idea of making the desirable behavior the easy option. Early nudging studies showed promising results on behavioral outcomes and revealed additional benefits of nudges as a policy tool such as ease of implementation and cost-effectiveness (Benartzi et al., 2017). The widespread implementation of these nudges is a novel development, but the behavioral principles on which they are based or not necessarily new. In fact, the idea of nudging is based on decades of

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research in psychology and human judgment and decision-making (e.g. Evans, 2008; Kahneman, 2003). As such, nudges challenge rational choice theory and are rather based on the idea of bounded rationality (Kahneman, 2003; Simon, 1955). That is, nudges are thought to shape the environment in such a way that it supports individuals in making desirable decisions without taxing cognitive resources. For that reason, nudges have received a lot of positive reactions as a novel public policy tool, but also negative criticism regarding the ethics of nudging (e.g. Hausman & Welch, 2010).

Thus far, a large majority of the studies on nudging have investigated the effects of nudges on several behavioral outcomes, but few studies have focused on the working mechanisms or boundary conditions of nudges (Szaszi et al., 2017), highlighting the need to investigate when and why nudges work. One of the pressing issues is that it is not clear to what extent nudges operate through automatic processes. It is often claimed that nudges take advantage of automatic processes by employing the very heuristics and biases that are often blamed as the reason for suboptimal decisions. By making strategic use of these "flaws" in decision-making, it is argued that nudges can stimulate desirable behavior. While the assumed automatic nature of the processes involved in nudging initially has been proposed as the foundation of all nudging interventions, experimental research on this premise remains rather scarce. Consequently, it remains unclear to what extent nudges take advantage of automatic processes, and what this means in practice.

In the present studies, we aim to experimentally study this basic premise of nudging by investigating the underlying cognitive mechanism of one of the nudges which has most consistently been described as relying on automatic processes: defaults. Previous research has investigated some of the hypothesized working mechanisms of defaults (e.g. Dinner et al., 2011; McKenzie et al., 2006), but research on the more fundamental question of whether defaults take advantage of automatic processes is still rather scarce. In two studies, we aim to investigate the effectiveness of a default nudge under circumstances in which deliberation is either inhibited (by a cognitive load manipulation; Study 1) or stimulated (with instructions; Study 2). We first review the literature on dual-process theories and nudging in general, before focusing on defaults as the focal nudge in these studies.

Dual Process Models

The theoretical framework on which the concept of nudging was built is the dual system framework (Kahneman, 2011; Thaler & Sunstein, 2008). According to this, and some related frameworks (for an overview, see Evans 2008), human judgment and decision-making originates from two distinct cognitive systems: System 1 and System 2. System 1 is commonly described as automatic, heuristic-based, fast and frugal, and has typically been given responsibility for biased or erroneous decisions. System 2 is commonly described as deliberate, analytical, slow and effortful (Evans & Stanovich, 2013; Kahneman, 2011). System 2 processes typically demand working memory capacity, while System 1 processes demand fewer cognitive investments (Evans & Stanovich, 2013; De Neys, 2006). Given that people are boundedly rational and thus not always willing or able to invest cognitive effort, they often resort to System 1 processes. The idea behind nudging is to befriend these processes, rather than to fight them, in order to help people make more desirable decisions through making this desirable decision the easy decision.

Yet, current theorizing on dual processing has become more nuanced. As such, current theorizing speaks of two *types* of reasoning, with defining features and typical correlates, rather than two systems of reasoning, in which each system is governed by a list of features. Also, Type 1 processes are no longer seen as responsible for errors and biases (Bago & De Neys, 2017; Evans & Stanovich, 2013), but represent adaptive reasoning in their own right (Gigerenzer & Gaissmaier, 2011). There continues to be debate about the dual system frameworks (e.g. Melnikoff & Bargh, 2018), but most theorists would nevertheless agree that a distinction can be drawn between processes that require working memory resources (Type 2 processes) and processes that do not require working memory resources (Type 1 processes). Generally, it is thought that people act intuitively (Type 1) by default, and that reflective reasoning (Type 2) can intervene if people are willing and able to invest cognitive effort. This view has been referred to as the default-interventionist framework (Evans & Stanovich, 2013).

For nudging research, however, this revamped conceptualization of dual-process theories bears important implications. As nudges are thought to take advantage of automatic processes, the question becomes what this exactly implies. One possibility is that under Type 1 processing, nudges become more effective. That is, as nudges take advantage of the heuristics and biases that are correlated with Type 1 processing, they should be more effective in exactly those circumstances where people are most likely to use these processes. Therefore, inhibiting Type 2 reasoning, for example, by time pressure (Evans & Curtis-Holmes, 2005) or concurrent working memory load (Bago & De Neys, 2017), could in theory increase the effectiveness of nudges. Another possibility, however, is that the effectiveness of nudges is unaffected by inhibition of Type 2 processes. This would be in line with the idea that people are cognitive misers (Fiske & Taylor, 2013) and that Type 2 only intervenes when people are able and motivated to invest cognitive effort. That is, people generally rely on Type 1 processing and inhibiting Type 2 processing would not change that. Consequently, nudges could remain equally effective in those circumstances.

Another intriguing question is what happens to the effectiveness of nudges when Type 2 processing is stimulated. After all, the mere availability of cognitive resources does not necessarily imply engagement of Type 2 processes (Thompson et al., 2011). These Type 2 processes are oftentimes stimulated with instructions to deliberate on a decision, for example, by instructing participants to reason deductively (Evans et al., 2010) or to provide reasons for choosing a particular option (e.g. Dijkstra et al., 2013; Horstmann et al., 2009). Research on biases has revealed that people are less biased in their reasoning skills when instructed to think deductively, provided that they have sufficient cognitive capacity to do so (Evans et al., 2010). Exposing someone to a nudge while instructing them to think carefully may thus render the nudge less effective. In order to investigate the effectiveness of nudging under Type 1 and Type 2 processing, we will make use of a nudge that has most consistently been put forward as relying on Type 1 processes: defaults.

Defaults

A default is an option that is preselected, such that, in the absence of an active decision, the decision-maker will stick with the preselected option. Which option is set as the default can have considerable effects for the option that is chosen most frequently. Typically, in an opt-out system (with a default) frequencies are considerably higher than in an opt-in system (without a default). Defaults are commonly embedded in binary decisions (e.g. being an organ donor or not, or having a green or grey energy plan), but can also be used for more continuous measures (e.g. the amount of money donated, or the number of green amenities selected). While some nudges (e.g. reminders) can impact behavior via Type 2 processes, defaults are generally seen as the most prototypical example of nudging (e.g. Thaler & Sunstein, 2008) and have most consistently been classified as Type 1 (Hansen & Jespersen, 2013) or non-educative (e.g. Sunstein, 2016) nudges.

Examples abound of default effects across many behavioral domains such as sustainable behavior (Pichert & Katsikopoulos, 2008; Vetter & Kutzner, 2016) and financial behavior (Madrian & Shea, 2001). The most illustrative difference between having an opt-in and an opt-out system was revealed by Johnson and Goldstein (2003) who showed a dramatic difference in the proportion of citizens registered as organ donors. While those countries that adhered to an opt-in system had consent rates ranging from 4.25% to 27.5%, countries that adhered to an opt-out system had consent rates ranging from 85.9% to 99.98%. Whether this difference in consent rates as a matter of fact translates in higher donation rates has been debated, but the difference compellingly illustrates the power that defaults can have on behavior.

A recent meta-analysis of default effects (Jachimowicz et al., 2019) further revealed that defaults can have a considerable influence on behavior, while also noting substantial variation in effect-sizes. This meta-analysis showed that defaults have a considerable effect on behavior with an average medium to large effect size of d =.68, meaning that the likelihood of choosing a particular option is .68 *SD*s higher when this is set as the default compared to an opt-in situation. Yet, the authors also revealed significant heterogeneity in effect sizes, suggesting possible moderation. Regarding study characteristics, it was revealed that default effects are larger in consumer domains (than in non-consumer domains) and weaker for decisions pertaining to sustainable behavior (as opposed to decisions not pertaining to sustainable behavior).

Recently, more and more research has been devoted to studying the underlying mechanisms of defaults. To date, three major, but not mutually exclusive, underlying mechanisms of this default effect have been put forward (Jachimowicz et al., 2019; Johnson & Goldstein, 2003). First, it has been argued that defaults are effective because it requires effort, either physical or cognitive, to override the status quo. Research on the status quo bias (Samuelson & Zeckhauser, 1988) has demonstrated that people tend to disproportionally stick with the status quo. Consequently, choice architects can strategically create a status quo by setting a default option from which people can opt-out. Changing away from that default often involves physical or cognitive effort, and people may not be motivated enough to invest the required amount of effort into the decision in order to opt-out (Fiske & Taylor, 2013). Second, it has been shown that defaults imply endorsement by the choice architect (McKenzie et al., 2006). According to this explanation, the decision-maker infers a recommendation from the choice architect and may decide to follow up on this recommendation. A series of studies by McKenzie et al. (2006) revealed that policymakers tend to leak their own preferences in setting the default option, and that decision-makers tend to infer an implicit recommendation from the choice architect to choose the option that has been set as the default. Third, it has been suggested that changing away from the default is evaluated in terms of losing something already endowed (Dinner et al., 2011; Park et al., 2000; Tversky & Kahneman, 1981). This explanation suggests a valuation shift such that the default option is valued relatively more merely because the decision-maker envisions to own this option (Dinner et al., 2011).

While all three explanations of the default effect seem plausible and have received experimental support, thus far no coherent and robust explanation for the default effect has been found. It has been reasoned that all three explanations could explain default effects, depending on the situation and the type of default (Dinner et al., 2011). The previously alluded to meta-analysis by Jachimowicz et al. (2019) revealed moderation of the default effect by both endorsement and endowment, but not by effort, but caution should be taken in interpreting these results as these hypothesized mechanisms were coded afterward rather than integral aspects of the studies included in the analysis.

Besides, there is little empirical research regarding the more fundamental question of whether defaults make use of automatic processing. As far as we are aware of, there is currently only one published paper that explored Type 1 processing in relation to default effects (Gärtner, 2018). This study showed that there is no additional effect of time pressure on prosocial decisions when participants were presented with a default in a dictator game. Similarly, as far as we know, there is only one published paper that investigated Type 2 processing in relation to default effects (Steffel et al., 2016). In a set of studies, these authors showed that the effect of a transparent default is attenuated when participants are instructed to articulate their preferences before choosing. In a follow-up study, it was shown that forcing participants to take extra time to choose did not attenuate the default effect, but simply having more time does not necessarily guarantee deliberation (Horstmann et al., 2010; Payne et al., 2008). Interestingly, this paper revealed that the default effect was attenuated because the manipulation to articulate preferences resulted in more balanced reasoning. In line with these results, we hypothesize that deliberation instructions will attenuate the default effect.

The Current Studies

To investigate the effectiveness of a default nudge under Type 1 and Type 2 processing, we conducted two studies. In the first study, we aim to load working memory capacity by a commonly used cognitive load manipulation, thereby inhibiting Type 2 processing among half of the participants. In the second study, we aim to stimulate Type 2 processing, by instructing half of the participants to think deliberately about their decision. We aim to manipulate these processing types concurrently with a task in which participants are asked to choose from a list of green amenities. For half of the participants, all options will be preselected as the default such that participants can opt-out if they want, while for the other half of the participants none of the options will be preselected such that participants can opt-in if they want. We chose this default, as previous research has demonstrated large overall effect sizes with sufficient variation between participants for it to be sensitive to other manipulations (Steffel et al., 2016).

Across both studies, we hypothesize that more green amenities will be selected if these are set as the default. In line with the default-interventionist perspective and in line with the study by Gärtner (2018), we further expect that this effect is not affected by cognitive load, such that the default is effective in both the condition with low and high load (Study 1). Further, we expect that deliberation instructions will activate Type 2 processes and will lead to a reasoned decision. Consequently, in line with previous research (Steffel et al., 2016), we expect that people are willing to deviate from the status quo. Thus, we expect that giving instructions to deliberate on the decision attenuates the default effect

(Study 2). In both studies, we will explore for downstream effects on satisfaction, as this may be an important proxy for future behavior (Wirtz et al., 2003).

Study 1

Study 1 aimed to investigate the effectiveness of a default nudge on sustainable behavior under Type 1 processing. Therefore, we subjected participants to a scenario in which they had just rented a newly constructed apartment. Participants were shown a list of green amenities in either an opt-in (no default) or opt-out (default) format and were asked to choose the amenities that they wanted to have. We aimed to inhibit Type 2 processing by manipulating cognitive load. Therefore, half of the participants had to remember a simple pattern of four dots, while the other half had to remember a highly difficult pattern of five dots. We expected to find a significant main effect of the default, and no interaction effect with cognitive load.

Method

Participants and Design

The smallest effect size observed in Steffel et al. (2016) for the main effect of the default on the number of green amenities chosen was $\eta^2_p = .37$. Using this most conservative effect size for the main effect in G*Power (Faul et al., 2009), and assuming power of 80% and a significance level of 0.05, we would require a total sample size of 16 participants to replicate this main effect. As we aimed to demonstrate that cognitive load did not affect the effectiveness of the default, we resorted to equivalence testing. We defined $d = -0.3$ as the lower bound and $d = 0.3$ as the upper bound, as these bounds approximately correspond with a meaningful effect of selecting 1 amenity more or less in the studies by Steffel et al. (2016). Using the TOSTER package in R (Lakens, 2017) with these bounds, and using 80% power and a significance level of 0.05, we would require a sample size of 191 participants per condition. In order to guarantee enough power for our proposed analyses with subsamples, we oversampled by 10%, resulting in a final sample size of 840 participants (507 female, 329 male, 4 Other/Rather not specify; $M_{age} = 34.17$, $SD_{age} = 13.64$).

Participants were recruited from Prolific Academic and we included adult participants with a UK nationality. In order to ensure quality of the data, we set approval rates to 95%. Participants were rewarded with £0.60 for their participation. We used a 2 (choice format: opt-in vs. opt-out) x 2 (cognitive load: low load vs. high load) between-subjects design with the number of green amenities chosen as dependent variable.

Procedure

The study was run on Gorilla (Anwyl-Irvine et al., 2020), a platform for online experiments. After having signed the informed consent, we administered a so-called lifestyle questionnaire that included items related to the motivation to behave sustainably. Next, participants were shown a pattern of dots and were asked to remember it and to reproduce it after having completed the main part of the study. Next, participants read a scenario adapted from Steffel et al. (2016), in which participants had to choose from a list of green amenities with either an opt-in or an opt-out format. After having chosen the amenities, participants were asked to reproduce the pattern of dots by clicking on the location of the dots in the matrix. After that, we asked four questions about the difficulty of remembering the pattern of dots. Next, we measured satisfaction with their choice as well as demographics (age and gender). Finally, we queried for suspicion of the goal of the study, before debriefing the participants.

Materials

Cognitive load task. We used the dot memorization task, a task in which participants have to memorize a pattern of dots that is presented in a matrix. This is a secondary load task that burdens cognitive resources and thereby reduces the possibility of Type 2 engagement (Bago & De Neys, 2017; Miyake et al., 2001). Consequently, several studies have shown that this manipulation affects performance on reasoning tasks, independent of individual differences in working memory capacity (e.g. De Neys, 2006). The task has successfully been used in online studies before (Bago & De Neys, 2017). To further enhance the strength of the manipulation, we used the "extra high load" manipulation from Johnson et al. (2016) as our high load condition.

For this task, all participants were first shown an empty 4 × 4 matrix. Next, for 1600 ms a pattern of dots appeared, and participants were asked to remember the pattern of dots and to reproduce it later. In the low load condition participants were shown a simple 1-piece pattern of four dots (i.e. the dots were placed in a straight vertical line), while in the high load condition participants were shown a complicated 4-piece pattern of five dots, meaning that only two of those five dots were adjacent to each other (Bethell-Fox & Shepard, 1988; Johnson et al., 2016; See Figure A1 in Appendix A for the patterns). After having completed the main part of the study, participants were shown an empty matrix again and were asked to reproduce the pattern of dots by clicking on the correct cells within the matrix. We measured the number and percentage of correctly localized dots in the matrix. After having filled in the matrix, we informed participants that they could forget about the pattern of dots. In order to see whether participants wrote down the pattern of dots, we asked them to reproduce the pattern once more at the very end of the study and asked them to honestly indicate if they had written down the pattern of dots.

Scenario. We used an adapted version of the scenario used by Steffel et al. (2016), in which participants were instructed to imagine that they had just rented a newly constructed apartment. Before signing the contract with the landlord, participants were offered a list of 14 optional green amenities. We updated the list of amenities so that it was suited for our UK sample and in line with current standards (See Appendix B for the list of amenities). In the opt-in condition, participants were shown the list of amenities, with none of them preselected. They were told that none of the amenities were currently included in the rent, but that they could choose to select the amenities of their liking for an additional monthly price, ranging from £2 to £8 for each amenity chosen. In the opt-out condition, participants were shown the list of amenities, and all of them were preselected. They were told that the amenities were currently included in the rent, but that they could choose to not install the amenities which they did not want, such that the landlord would deduct a small amount of money from the monthly rent (again, ranging from £2 to £8). Following Steffel et al. (2016), all options were either or not preselected in order to maximize the treatment effect. We measured the number of green amenities selected by the participants. This variable ranged from 0 to 14.

Measures

Motivation to behave sustainably. We measured the motivation to behave sustainably with four items based on research on personal strivings (Emmons, 1986). We used items that measured commitment ("How committed are you to behaving sustainably?"), importance ("How important is behaving sustainably to you in your life?"), and value ("How much joy or happiness do you or will you feel when you are successful in behaving sustainably?" and "How much sorrow or unhappiness do you or will you feel if you fail to succeed in behaving sustainably?"). These four items were embedded in a lifestyle questionnaire that also measured the same strivings for two other behaviors: healthy eating and saving money. All questions were asked on a 7-point Likert scale, ranging from 1 (not at all) to 7 (very much). The filler items were not analyzed and were solely included to conceal the goal of the study. The measure had high internal reliability (*Cronbach's* α = .812), and thus, according to the proposed analysis plan, all four items were combined into one score by taking the mean over the four items.

Subjective difficulty. We measured the subjective difficulty of having had to remember the pattern of dots, by asking participants four questions on a 7-point Likert scale, ranging from 1 (not at all) to 7 (very much). The four items are: "How difficult was it to remember the pattern of dots?", "How much effort did it cost to remember the pattern of dots?", "How much were you preoccupied with remembering the pattern of dots?", and "How easy was it for you to remember the pattern of dots?" (Reverse coded). The measure had high internal reliability (*Cronbach's* α = .887), and thus, according to the proposed analysis plan, all four items were combined into one score by taking the mean over the four items.

Satisfaction with choice. Satisfaction with choice was measured with a single item ("How satisfied are you with the amenities that you chose?") on a 7-point Likert scale, ranging from 1 (not at all) to 7 (very much). We included this item to explore whether there were differences between the conditions in satisfaction, but we did not have a priori hypotheses for this.

Demographics. We asked participants for their age in years and gender (female, male, other/rather not specify).

Goal of study. We inquired for suspicion of the goal of the study with one open-ended question ("What do you think was the goal of the study?").

Results

Data are available on the Open Science Framework (https://osf.io/uqgwv/).

Preprocessing steps

All data were screened for outliers, which we defined as 3 *SD*s above or below the mean for each variable. If we detected outliers, these values were set missing. We also checked whether participants detected the goal of the study, by having two independent coders code (1) whether participants mention any association between the cognitive load task and the amenity selection task, and (2) whether participants expected that the cognitive load task affected their choice. The two independent coders reached agreement in 88.93% of the cases for criterion 1 and 94.29% of the cases for criterion 2. A third independent coder evaluated the answer for the remaining cases. 45.12% of the sample (n = 379) detected any association between the cognitive load task and the amenity selection task (criterion 1), while 13.45% of the sample (n = 113) expected that the cognitive load task affected their choice (criterion 2).

The main dependent variable was the number of green amenities chosen. For completeness, we checked for normality by performing a Shapiro–Wilk test, which turned out significant ($W = .92$, $p < .001$). However, given our large sample size we suspected that the proposed ANOVA would be robust regardless of the normality of the data.

Preregistered analyses

Randomization check. In order to check whether randomization of participants across the four conditions was successful, we ran separate ANOVAs with the four conditions as independent variable and age or motivation to behave sustainably as dependent variable. For gender, we ran a Chi-squared analysis. As expected, randomization was successful (all ps > .129).

Manipulation check. In order to check whether our manipulation was successful, we conducted an independent samples t-test with cognitive load (low load vs. high load) as independent variable and subjective difficulty as dependent variable. As expected, our manipulation was successful such that participants in the high load condition (M = 4.09, SD = 1.33) found it more difficult to remember the pattern of dots than participants in the low load condition (M = 1.58, SD = .76), t (672.69) = −33.69, p < .001, d = 2.31. For robustness, we also ran the manipulation check with the subsample of participants in the high load condition who did not perfectly recall the pattern of dots during the second measurement (n = 646). We also did the same with the subsample of participants in the high load condition who reported that they had not written down the pattern of dots (n = 830). Results remained similar across these two proposed subsamples. Table 1 presents the descriptives for the proportion of correctly remembered dots.

Main analyses. In order to evaluate our hypothesis, we ran a factorial ANOVA with choice format and cognitive load as independent variables and the number of green amenities as dependent variable. As expected, this revealed a significant main effect of the default nudge, F (1, 836) = 462.22, p < .001, η_p^2 = .36, such that participants in the opt-out condition (M = 11.75, SD = 2.56) chose more green amenities than participants in the opt-in condition (M = 5.60, SD = 3.22). There was no main effect of cognitive load, F (1, 836) = .19, p = .663. Crucially, as expected we also did not find a significant interaction effect, F (1, 836) = .002,

Table 1. Mean, SD, minimum and maximum proportion of correctly remembered dots in measurement 1 and measurement 2.

	Measurement 1	Measurement 2
Low load	1.00 (.04) [.25–1]	1.00 (.05) [.25–1]
High load	.81 (.23) [.40–1]	.80 (.23) [.40–1]

p = .964. For robustness, we had proposed to run this main analysis with a variety of subsamples: 1) the subsample of participants who correctly remembered the complete pattern of dots at the initial measurement ($n = 615$; cf., Johnson et al., 2016), 2) the subsample of participants in the high load condition who did not perfectly recall the pattern of dots during the second measurement ($n = 646$), 3) the subsample of participants in the high load condition who did not write down the pattern of dots ($n = 830$), 4) the subsample of participants who had not detected the goal of the study according to criterion 1 ($n = 461$), and 5) the subsample of participants who had not detected the goal of the study according to criterion 2 ($n = 727$). Results remained similar across these five proposed subsamples, indicating a robust pattern (See Appendix C). We also ran an additional Poisson model, which again revealed a strong default effect ($p < .001$), and no main effect of load or an interaction effect.

In order to further test the hypothesis that cognitive load does not affect default effectiveness, we ran a TOST independent samples t-test for the two opt-out groups (i.e. high and low cognitive load). We set the lower bound to $d = -0.3$ and the higher bound to $d = 0.3$, and used alpha level 0.05. As expected, the equivalence test was significant, $t(418) = -2.65$, $p = .004$, while the null hypothesis test was not significant, $t(418) = 0.42$, $p = .673$. Taken together, this implies that the default effect is statistically equivalent, and we reject the existence of a meaningful effect.

Exploratory analyses (Preregistered)

We explored whether there are differences between the conditions in satisfaction with the choice. Therefore, we conducted a factorial ANOVA with choice format and cognitive load as independent variables and satisfaction as dependent variable. This analysis revealed a significant main effect of the default nudge, $F(1, 805) = 11.70$, $p < .001$, $\eta_p^2 = .01$, such that participants in the opt-out condition ($M = 5.81$, $SD = 1.11$) were more satisfied with their decision than participants in the opt-in condition ($M = 5.45$, $SD = 1.10$).

Exploratory analyses (Unregistered)

In order to shed more light on the possible working mechanisms of this specific nudge, we conducted an exploratory factorial ANOVA with choice format and cognitive load as independent variables and the number of deviations from the status quo as dependent variable. This analysis revealed a significant main effect of the default nudge, $F(1, 836) = 147.07$, $p < .001$, $\eta_p^2 = .15$, such that participants in the opt-out condition ($M = 2.25$, $SD = 2.56$) changed away from the status quo to a lesser extent than participants in the opt-in condition ($M = 5.60$, $SD = 3.22$). We did not find a main effect of cognitive load ($p = .663$) nor an interaction effect ($p = .568$).

Study 2

In Study 2, we aimed to experimentally stimulate Type 2 processes. In Study 1, half of the participants received high cognitive load, thereby taxing working memory capacity, while the other half received low cognitive load. However, the mere availability of cognitive resources does not imply engagement with Type 2 processes per se. Therefore, in Study 2 we aimed to stimulate Type 2 processes by instructing participants to think thoroughly about their decision. We used the same scenario and default manipulation. Again, we expected to find a significant main effect of the default, and in Study 2 we expected to find a significant interaction effect, such that the default effect was attenuated when participants were instructed to deliberate.

Method

Participants and Design

The smallest effect size for the main effect of the default on the number of green amenities chosen was η^2_p = .37 (Steffel et al., 2016). Using this most conservative effect size for the main effect in G*Power (Faul et al., 2009), and assuming power of 80% and a significance level of .05, we would require a total sample size of 16 participants to replicate this main effect. As we aimed to demonstrate that deliberation instructions attenuated the default effect, we were interested in the possibility of finding an interaction effect. We defined our smallest effect size of interest as 20% attenuation, which would correspond with a decrease in the number of amenities chosen of about 1. Using the script for simulations in R by datacolada (Simohnson, 2014), we ran 2000 simulations and found that we needed at least 336 participants to find a significant attenuation effect of 20% with 80% power and a significance level of 0.05. In order to guarantee enough power for our proposed analyses with subsamples, we oversampled by 10%, resulting in a final sample size of 372 (220 female, 152 male; M_{age} = 35.75, SD_{age} = 13.29).

Participants were recruited from Prolific Academic and we included adult participants with a UK nationality. In order to ensure quality of the data, we set approval rates to 95%. An additional exclusion criterion in Study 2 was that participants should not have participated in Study 1. Again, participants were rewarded with £0.60 for their participation. We used a 2 (choice format: opt-in vs. opt-out) x 2 (instructions: no instructions vs. deliberation instructions) between-subjects design with the number of green amenities chosen as dependent variable.

Procedure

Study 2 was also run on Gorilla (Anwyl-Irvine et al., 2020), and we used the same lifestyle questionnaire as in Study 1, followed by the same scenario as in Study 1 (either in an opt-in or an opt-out format). Half of the participants received no additional instructions, while the other half of the participants were instructed to think carefully about their decision. This latter group of participants was also told that they had to provide reasons for their decisions. After they had chosen their amenities, we asked participants to report their reasons for choosing the amenities that they chose. The other half of the participants received no additional instructions and did not have to report the reasons behind their decisions. Next, satisfaction with their choice as well as demographics (age and gender) were measured. Finally, we queried for suspicion of the goal of the study, before debriefing the participants.

Materials

Deliberation instructions. Participants in the deliberation instructions conditions were instructed to think carefully about their decision (Horstmann et al., 2010), and received the following instructions: “Please think carefully and thoroughly about your decision of which amenities to choose. After you have made your decision, we will ask you to provide at least three reasons for choosing the amenities that you chose. You can take as much time as you need to consider all options and reach a balanced decision." Participants in the no instructions conditions received no additional instructions and were not asked to report the reasons behind their decisions.

Scenario. We used the same scenario as in Study 1 and measured the number of green amenities selected by participants in the same way as in Study 1. This variable ranged from 0 to 14.

Measures

Motivation to behave sustainably. We measured the motivation to behave sustainably with the same lifestyle questionnaire as in Study 1. The measure had high internal reliability (*Cronbach's* α = .817), and thus, according to the proposed analysis plan, all four items were combined into one score by taking the mean over the four items.

Satisfaction with choice. Satisfaction with choice was measured in the same way as in Study 1. We explored whether there are differences between the conditions in satisfaction, but, again, did not have a priori hypotheses for this.

Demographics. We asked participants for their age in years and gender in the same way as in Study 1.

Goal of study. We inquired for suspicion of the goal of the study with the same question as in Study 1.

Results

Data are available on the Open Science Framework (https://osf.io/gyujs/).

Preprocessing steps

All data were screened for outliers, which we defined as 3 *SD*s above or below the mean for each variable. If we detected outliers, these values were set missing. We also checked whether participants detected the goal of the study, by having two independent coders code (1) whether participants mentioned any association between the instructions and the amenity selection task, and (2) whether participants expected that the instructions affected their choice. The two independent coders reached agreement in 99.73% of the cases for criterion 1 and 98.92% of the cases for criterion 2. A third independent coder evaluated the answer for the remaining cases. None of the participants detected the goal of the study according to criterion 1 or 2.

The main dependent variable was the number of green amenities chosen. For completeness, we checked for normality by performing a Shapiro–Wilk test, which turned out significant ($W = .93$, $p < .001$). However, given our large sample size, we suspected that the proposed ANOVA would be robust regardless of the normality of the data.

Preregistered analyses

Randomization check. In order to check whether randomization of participants across the four conditions was successful, we ran separate ANOVAs with the four conditions as independent variable and age or motivation to behave sustainably as dependent variable. For gender, we ran a Chi-squared analysis. As expected, randomization was successful (all *p*s > .497).

Main analyses. In order to evaluate our hypothesis, we ran a factorial ANOVA with choice format and deliberation instructions as independent variables and the number of green amenities as dependent variable. As expected, this revealed a significant main effect of the default nudge, F (1, 368) = 205.17, $p < .001$, $\eta_p^2 = .36$, such that participants in the opt-out condition ($M = 11.06$, $SD = 2.69$) chose more green amenities than participants in the opt-in condition ($M = 5.45$, $SD = 2.68$). Unexpectedly, we did not find a significant interaction effect, F (1, 368) = .45, $p = .501$, but instead we found a main effect of deliberation, F (1, 368) = 8.59, $p = .004$, $\eta_p^2 = .02$, such that participants who received deliberation instructions chose fewer green amenities ($M = 7.80$, $SD = 3.96$) than participants who did not receive such instructions ($M = 8.76$, $SD = 3.77$). Initially, we proposed to run the same analyses with the subsamples of participants who were not able to identify the goal of the study, but this had become redundant since none of the participants had successfully done so. We also ran an additional Poisson model, which again revealed a strong default effect ($p < .001$) and a significant effect of deliberation instructions ($p = .001$). This analysis further demonstrated a marginally significant interaction effect ($p = .067$).

Exploratory analyses (Preregistered)

Just as in Study 1, we explored whether there are differences between the conditions in satisfaction with the choice. Therefore, we conducted a factorial ANOVA with choice format and deliberation instructions as independent variables and satisfaction as dependent variable. This analysis again revealed a significant main effect of the default nudge, F (1, 364) = 5.97, $p = .015$, $\eta_p^2 = .02$, such that participants in the opt-out condition ($M = 6.05$, $SD = .81$) were more satisfied with their decision than participants in the opt-in condition ($M = 5.85$, $SD = .91$).

Exploratory analyses (Unregistered)

We again conducted an exploratory factorial ANOVA with choice format and deliberation as independent variables and the number of deviations from the status quo as dependent variable. This analysis again revealed a significant main effect of the default nudge, F (1, 368) = 81.94, $p < .001$, $\eta_p^2 = .18$, such that participants in the opt-out condition ($M = 2.94$, $SD = 2.69$) changed away from the status quo to a lesser extent than participants in the opt-in condition ($M = 5.45$, $SD = 2.68$). We also found a small main effect of deliberation, F (1, 368) = 8.59, $p = .004$, $\eta_p^2 = .02$, such that participants who were instructed to deliberate upon their decision were less likely to change away from the status quo ($M = 4.08$, $SD = 2.78$) than participants who did not receive such instructions ($M = 4.26$, $SD = 3.12$). Finally, we found a small interaction effect of the default and deliberation instructions on the number of times participants changed away from the status quo, F (1, 368) = 12.29, $p = .001$, $\eta_p^2 = .03$. Post-hoc multiple comparison tests using Tukey HSD revealed that deliberation reduced the number of times participants changed away from the status quo in the opt-in conditions, $p = .019$, while this was not the case in the opt-out conditions, $p = .183$.

Discussion

Ever since the introduction of nudges, they have received an increasing amount of interest from both scholars and policymakers, as early nudging studies revealed promising results on behavior and cost-effectiveness (Benartzi et al., 2017). At the same time, there have been numerous discussions on the legitimacy of nudging interventions, mostly based on the core assumption of nudges as taking advantage of automatic processes (Bovens, 2009; Hansen & Jespersen, 2013). In two studies, we investigated this fundamental premise by using a default nudge which is often seen as the most prototypical of Type 1 nudges (Hansen & Jespersen, 2013; Jung & Mellers, 2016; Sunstein, 2016). We investigated the effectiveness of this default nudge under circumstances in which deliberation was either inhibited (Study 1) or stimulated (Study 2). Across two preregistered and high-powered studies, we found a strong and robust effect of the default nudge on the number of green amenities that were chosen. Thereby, we replicated the default effects as observed in Steffel et al. (2016) with effect sizes that were larger than typically observed in experiments with defaults (Jachimowicz et al., 2019). The default effect was similar in size across the two studies and also was robust to inclusion or exclusion of certain participants based on predetermined subsamples.

In Study 1, this main effect of the default nudge was, as expected, not moderated by cognitive load. In fact, the default effect was statistically equivalent in the low and high load conditions, even though participants found it considerably more difficult to remember the pattern of dots in the high load condition than in the low load condition. Together, this indicates that the default effect is not strengthened or weakened when people are bound to resort to Type 1 processes. Rather, the default nudge is similarly effective when Type 2 processing is successfully inhibited. This is in line with the default-interventionist perspective on dual processing (Evans & Stanovich, 2013), which posits that Type 1 processing is already the default mode of operation, unless Type 2 processing deliberately intervenes. In other words, the mere availability of cognitive resources does not directly imply engagement of these processes. Thus, inhibiting Type 2 processing via a demanding cognitive load manipulation does not alter nudge effectiveness, as it does not alter the processes that are being used to reach this decision.

This does not imply that this default nudge cannot be overruled by extensive deliberation, as deliberation was not actively involved in Study 1. This is what we addressed in Study 2, by instructing participants to carefully deliberate upon their decision. Study 2 again revealed a significant main effect of the default, and also a main effect of deliberation on the number of green amenities that were chosen. Contrary to our expectations, we did not find a significant interaction effect that would reveal attenuation of the default effect under deliberation. Instead, we found a main effect of deliberation, indicating that fewer green amenities were chosen if people deliberated upon their choice. This diminishing effect of deliberation was present in both the opt-in and opt-out conditions. Together, this implies that default effects are strong and robust, but not necessarily impregnable as deliberation may operate in parallel and impact choice simultaneously.

Implications

Our results first and foremost shed light on the hypothesized working mechanisms of nudges as making use of automatic processes. Our results most stringently imply that this default nudge is effective as it is not *dependent* on elaborate processing. It is equally effective whether people have the capacity to engage in careful thought or not. Yet, if people are able and willing to deliberate upon their decision, this may in parallel lead to different choice outcomes. In other words, nudges are indeed effective as they do not depend on executive processing, which gives people the opportunity to stick with the default without investing cognitive resources, or to change away from the status quo by deliberating upon the decision.

Our results do also indirectly add to the growing amount of literature on the specific working mechanisms of defaults. We explored the extent to which participants would change away from the default, and in both studies we found that the strength of the status quo was larger for participants in the opt-out condition than for participants in the opt-in condition. This points towards a qualitative difference between the opt-in and opt-out condition, which aligns well with the notion of endowment as a possible working mechanism of defaults (Dinner et al., 2011; Park et al., 2000; Tversky & Kahneman, 1981). In our studies, participants were less likely to give up to something already endowed as compared to actively choosing for specific amenities. Based on our results, it is less likely that effort is a driving force behind this default nudge, as participants took more effort to change away from the status quo in the opt-in condition than in the opt-out condition, while objectively the amount of effort required to change away from this status quo was equal. Similarly, implied endorsement is less likely to drive our effects, unless the implied endorsement is asymmetrical in the sense that the endorsement is more genuinely felt in the opt-out condition than in the opt-in condition. This latter possibility could indirectly be reflected in our exploratory findings that participants in the opt-out conditions were more satisfied with their decisions than participants in the opt-in conditions.

Furthermore, our results fit in with recent developments in literature on dual processing. While nudges were originally based on the idea of dual systems, current evidence points towards two types of reasoning where the distinction lies in the involvement of working memory resources (Melnikoff & Bargh, 2018). In our studies, we inhibited working memory capacity in Study 1 and stimulated working memory involvement in Study 2. We thus focused on the core mechanisms of these processing types, and not on the typical correlates such as speed of the decision or level of consciousness. Extrapolating from our results and findings in the control conditions without high load or deliberation instructions, we indeed see a pattern of results that suggests that Type 1 processing is the default mode of operation. Inhibiting Type 2 processing did not alter our behavioral outcomes, while stimulating Type 2 engagement led to a parallel effect, thus suggesting that Type 1 reasoning is the standard mode of operation.

Finally, our results reveal whether people effectively use the cognitive resources that are available to them, which also bears implications for research on transparency information in nudging desirable behavior. Current evidence suggests that nudges remain effective if accompanied by transparency information (e.g. Bang et al., 2020; Bruns et al., 2018; Kroese et al., 2015; Loewenstein et al., 2015; Steffel et al., 2016). In fact, there is also some evidence that default nudges become more effective with transparency information (Paunov et al., 2019a, 2019b). Yet, the mere provision of transparency information does not guarantee that decision-makers take this information (sufficiently) into account (Kroese et al., 2015), and that they deliberate upon their decision (Loewenstein, Sunstein, & Golman, 2014). Our direct manipulation of deliberation underscores this explanation of why transparency information does not necessarily affect behavioral outcomes, as this would only be the case if this information is sufficiently incorporated in the decision by the decision-maker. If not, the decision-maker may resort to Type 1 processing without deliberating upon the information that is given.

Further research

Further research should attempt to increase knowledge on this fundamental premise of nudges as making use of automatic processes. In doing so, we like to point out that all too often it is assumed that control conditions in which participants do not receive cognitive load reflect Type 2 processing. However, the mere availability of cognitive resources does not guarantee engagement of those processes, and it is important to further study nudge effectiveness by considering both sides of the same coin.

Further research should also investigate different (kinds of) nudges. In this study, we used defaults, which are arguably the most prototypical Type 1 nudges. After the initial introduction of nudges as simple interventions in the choice architecture, conceptual nuances have been made distinguishing between Type 1 vs. Type 2 nudges (Hansen & Jespersen, 2013; Lin, Osman, & Ashcroft, 2017), or educative vs. non-educative nudges (Sunstein, 2016). It remains to be determined if our findings apply to other kinds of nudges, such as social proof nudges and reminders, which could appeal more to deliberate processes than default nudges.

In conclusion, across two high-powered studies, we showed that defaults can strongly stimulate sustainable choices. Most importantly, we shed light on the hypothesized working mechanisms of nudges as making use of automatic processing and show that defaults are effective as they are not dependent on elaborate Type 2 processing. For behavior change interventions this is a promising result, since other interventions like educational campaigns typically rely more on Type 2 processing. For the debate on the ethics and legitimacy of nudging, these results provide empirical data that show that people with less cognitive capacity will not be more vulnerable to fall victim to the implementation of defaults.

Disclosure statement

No potential conflict of interest was reported by the authors.

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Appendices

Appendix A

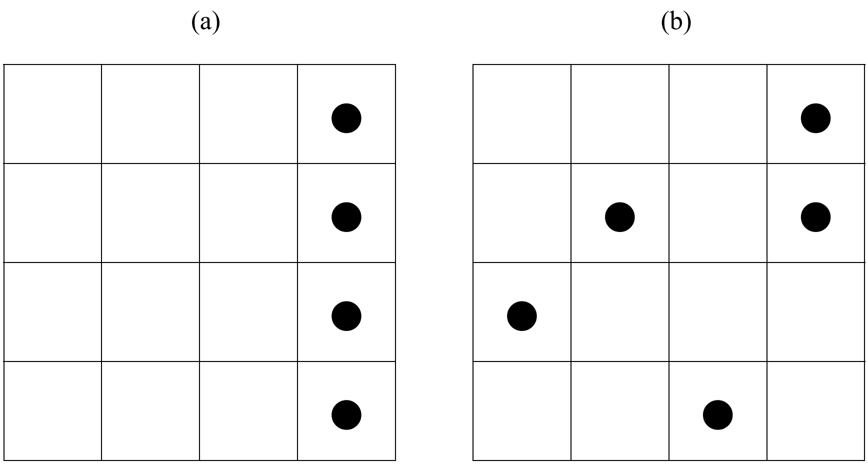

Figure A1. Dot patterns as presented in the dot memorization task in the (a) Low load condition and (b) High load condition.

Appendix B

List of amenities to be used in the study (adapted from Steffel et al., 2016):

- Energy-efficient oven and stove
- Tankless water heater
- Smart thermostat
- Double glazed windows
- Energy-efficient range hood and bathroom fan
- Radiator foil
- Energy-efficient dishwasher and refrigerator
- LED light bulbs
- Energy-efficient washer and dryer
- Dimmer switches for indoor lighting
- Low-flow toilets
- Solar-powered outdoor lighting
- Low-flow faucets and shower heads
- Motion sensors for outdoor lighting

Appendix C

Main results of Study 1 with the proposed subsamples:

(1) the subsample of participants who correctly remembered the complete pattern of dots at the initial measurement (n = 615)

	F (1, 611)	p	η_p^2
Default	459.54	<.001	.43
Cognitive load	.19	.666	<.01
Default x Cognitive load	<.01	.953	<.01

(2) the subsample of participants in the high load condition who did not perfectly recall the pattern of dots during the second measurement (n = 646)

	F (1, 642)	p	η_p^2
Default	467.24	<.001	.42
Cognitive load	2.12	.146	<.01
Default x Cognitive load	.23	.634	<.01

(3) the subsample of participants in the high load condition who did not write down the pattern of dots (n = 830)

	F (1, 826)	p	η_p^2
Default	465.22	<.001	.36
Cognitive load	.14	.705	<.01
Default x Cognitive load	<.01	.921	<.01

(4) the subsample of participants who had not detected the goal of the study according to criterion 1 (n = 461)

	F (1, 457)	p	η_p^2
Default	250.50	<.001	.35
Cognitive load	2.15	.143	<.01
Default x Cognitive load	3.14	.077	<.01

(5) the subsample of participants who had not detected the goal of the study according to criterion 2 (n = 727)

	F (1, 723)	p	η_p^2
Default	382.21	<.001	.35
Cognitive load	1.03	.310	<.01
Default x Cognitive load	.95	.330	<.01