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Would you accept some coffee with your sugar? A natural field experiment on the efficiency and acceptability of setting zero sugar by default in coffee vending machines

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Would you accept some coffee with your sugar? A natural field experiment on the efficiency and acceptability of setting zero sugar by default in coffee vending machines

Abstract: This paper aims to replicate the effect of a nudge on behavior (efficiency) and acceptability in a natural field experiment. The nudge in our study consists in setting zero sugars as the default level of sugar in hot drinks–vending machines in a French university. We compared Campus A (default option set to 0 sugars) to Campus B (default option set to 3 sugars). We measured the efficiency of this default option by observing the level of sugar actually chosen by the participants, and we measured acceptability through a questionnaire. We hypothesized a high level of efficiency for the nudge and a higher acceptability in Campus A (default option set to 0 sugars) compared to Campus B (default option set to 3 sugars). Our results show that participants with the default option set to zero sugars (Campus A) consumed less sugar than those with the default option set to 3 sugars (Campus B). We also found a high level of acceptability on both campuses, though with no difference between Campus A (where the nudge was implemented) and Campus B (where a future nudge would be implemented). The discussion addresses the applied perspectives and ethical implications of these results.

Keywords: behavioral policy; nudge; efficiency; default option; acceptability

1. Introduction

The view that policymakers could use knowledge from psychology as an input to public policy has become increasingly popular in recent years, particularly with the development of various “nudge units” since the establishment of the UK Behavioural Insights Team in 2010. Traditional forms of policy interventions can be classified into more or less coercive and intrusive interventions – from bans and financial incentives (e.g. taxation) to the mere release of public information. Within this spectrum, *nudges* (Benartzi et al., 2017) seem to constitute a relatively “soft” intervention. Indeed, they exploit decision biases when designing the choice environment to steer people’s behavior in a predictable direction, without significantly impacting their material

incentives or limiting the set of available opportunities. While the original justification of nudges was to help people make better decisions in terms of their own well-being, “as judged by themselves” (Thaler & Sunstein, 2009, p. 5), they have progressively become just another type of policy intervention in the “toolbox” of policymakers.

Various empirical studies seem to suggest that nudges are “efficient” in the sense of effectively managing to shift people’s behavior in the desired direction (see Szaszi et al., 2018 for an extensive review) while representing a significantly lower cost for public authorities – for a given impact on behavior (Benartzi et al., 2017). One of the most efficient nudges consists in changing the default option in a given decision environment (see Jachimowicz et al., 2019 for a meta-analysis). For instance, registering people as organ donors by default significantly increases the rate of organ donations (Johnson & Goldstein, 2003). In another context, automatically enrolling new employees in a retirement savings plan, such as a 401(k) in the USA, significantly increases employees’ retirement savings (Choi et al., 2004).

There are multiple explanations as to why the default option is efficient (Smith et al., 2013). Dinner et al. (2011) identified three major sets of such explanations. First, choosing another option may require too much effort, whether it be a physical effort (e.g. filling in a new administrative form or searching for a postage stamp) or a cognitive effort (e.g. figuring out what one really prefers or computing the costs and benefits of the different options). Second, people may stick to default options because they endorse the choice made by the choice architect. That is, people trust that the person(s) who implemented the change in default options had their best interests in mind and so go along with the default. Third, people may experience a sense of endowment about the default option. That is, they feel like they own the default option and that changing to another option means losing something.

Default options are often interpreted as exploiting people’s cognitive biases and can thus constitute a form of manipulation (e.g. Hansen & Jespersen, 2013). Most of the studies conducted so far to assess the acceptability of nudges nonetheless have suggested a relatively high acceptability by people, with some fluctuations depending on the details of the intervention.¹ The typical design of these studies consists in presenting nudge scenarios to experimental participants (e.g. through web surveys) and asking them if they find the intervention acceptable or not. Participants thus make a hypothetical acceptability judgment in at least two senses. First, they judge the acceptability of the descriptions of nudges without experiencing the nudge. Second, even when they do experience the nudge insofar as subjects must make

decisions (Bang et al., 2018), these decisions remain hypothetical in the sense that subjects do not experience the *consequences* of their choice. Furthermore, Weber (2015) suggests that the acceptability of a nudge may increase after its implementation, meaning that measures of acceptability should take this effect into account.

This paper aims to test the efficiency of a nudge implemented in a French university in a natural field experiment, consisting in setting zero sugars as the default level in hot drinks–vending machines. Even though the European Vending Machine Manufacturers Association unanimously decided in 2018 to set this kind of zero sugar level by default on all their hot drinks–vending machines, it seems (to the best of our knowledge) that no analysis of the efficiency of this measure has been conducted, at least in the academic literature. Besides measuring the efficiency of this nudge, we consider a complementary set of measures to identify which of the theoretical alternatives outlined earlier best explain our observations. A complementary objective of the study is to measure whether the users of those machines find the intervention “acceptable” or not. Indeed, our experimental setup gives us the opportunity to contribute to the literature on the acceptability of nudging interventions by measuring the *in-situ* acceptability of a nudge (i.e. by measuring acceptability in non-hypothetical real-life situations). In other words, the main goal of our study is to investigate the external validity of the efficiency and acceptability of a default option nudge.

2. Current research

This study is a replication in a sense similar to what Hüffmeier et al. (2016) refer to as “conceptual replication in the field.” In the present research, we seek to replicate, in a natural field experiment, previous experiments aiming to measure the efficiency and acceptability of nudges consisting in changing a default option. Unlike previous studies showing the efficiency of the default option, our experimental setup is a case of a “natural” experiment in the sense that the researchers were not responsible for creating the two conditions (the universities’ decisions were independent of our research).

Two papers suggest that the nudge we are studying should be very efficient in reducing sugar consumption. First, Steffel et al. (2016, Exp. 1c) offered free hot chocolate in a communal area of a university, with or without whipped cream as the default. They observed that an overwhelming majority of people (between 90% and

100% depending on the sub-conditions) chose the default option. Second, Cesareo et al. (2019) observed in a coffee shop that reducing the default amount of sugar in sugar packets (from 7.5 g to 4g) led people to add a lower quantity of sugar to their coffee (i.e. people did not use more packets). These two cases suggest that participants should stick to a preselected number of sugars. In other words, if a hot beverage-vending machine is set to 0 sugars as default option, sugar consumption should be lower than if the vending machine is set to 3 sugars as default option. We therefore formulated the following hypothesis:

H1: *Participants in the “0 sugars” condition should consume less sugar than the participants in the “3 sugars” condition.*

Vetter and Kutzner (2016) showed that attitude strength might moderate the efficiency of nudges in promoting pro-environmental behavior. Consequently, we measured strength of attitude to reducing sugar consumption in the current research. In the context of sugar consumption, there was no element that allowed us to develop a precise hypothesis about attitude strength to Observed Consumption of sugar. Nevertheless, we controlled its effects by allowing interaction between exposure to the nudge (Default option) and attitude strength.

Our contribution on default option acceptability is twofold. On the one hand, we measure *in situ* acceptability judgments in a natural field experiment. We found only two other studies that measured *in situ* acceptability, though those were field experiments rather than natural field experiments. That is, unlike in the present study – where we (the researchers) merely observed the implementation of a university policy – the other interventions were designed and implemented by the researchers themselves. Venema et al. (2018) observed (in a governmental organization) that changing the default position of sit-stand desks from sitting to standing reduced sedentary behavior at work and that 56.5% of the employees found this intervention to be acceptable, 25.4% had no opinion about the acceptability of the intervention, and 11.0% found it unacceptable. Kroese et al. (2015) observed that changing the snacks at the cash register from unhealthy to healthy food items increased the purchase of healthy food items. Out of 91 customers, 76 found the nudge to be acceptable, 9 were indifferent, and 4 found it unacceptable.

The second contribution of our work on default option acceptability is to propose a multiple-item Likert scale to measure acceptability. Most of the acceptability

scales on which participants rate nudges are indeed quite limited. They usually consist of a single-item Likert scale, which is presented among other scales intended to measure other variables (e.g. Bang et al., 2018; Davidai & Shafir, 2018; Felsen et al., 2013; Hagman et al., 2015; Jung & Mellers, 2016; Steffel et al., 2016). Sometimes, acceptability is even “measured” through simple binary “yes/no,” “approve/disapprove,” or “support/oppose” questions (e.g. Kroese et al., 2015; Osman et al., 2018; Reisch & Sunstein, 2016; Sunstein, Reisch, & Kaiser, 2018; Venema et al., 2018). The only study that has used a multiple-item Likert scale to measure the acceptability of nudges is the one by Tannenbaum et al. (2017), which is an online study, therefore measuring hypothetical acceptability. We chose to use their measure of acceptability which seems to be the least biased.

Our second hypothesis about default-option acceptability relies on Weber’s (2015) work suggesting that the acceptability of a nudge increases after its implementation. This would mean that the level of acceptability is lower when people think that the nudge *will be* implemented, compared to the level of acceptability when they think that this nudge *has been* implemented. We therefore formulated the following hypothesis:

H2: *the acceptability of a default option set to 0 sugars should be stronger in the condition where the nudge has been already implemented (Campus A) than in the condition where the nudge is planned (Campus B).*

3. Method

3.1. Procedure

We collected data between 1 March 2021 and 12 January 2022,² with 145 students from two campuses of two French universities: The Arts and Humanities campus of the University of Montpellier 3 (Campus A) and the Arts and Humanities campus of the University of Aix-en

-Provence (Campus B). Both campuses were equipped with similar hot drinks-vending machines selling the same products and offered the option of adjusting the desired sugar level from a minimum of 0 to a maximum of 5. While the machines on Campus A (Montpellier 3) were programmed to offer a level of 0 sugars by default, the machines on Campus B (Aix-en-Provence) were programmed

to offer a level of 3 sugars by default. After ordering a hot drink, participants were contacted near the vending machine.

Experimenters (one male and one female on each campus) noted the level of sugar chosen by the participants. This is our measure of observed consumption (OC). Immediately after measuring consumption, an experimenter contacted the participants and asked them whether they agreed to participate in a study about the default level of sugar in the vending machines. Experimenters certified the anonymity and confidentiality of the responses.

3.2. Materials

After securing the participation agreement (i.e. agreement for the data to be examined in the study), the experimenters asked participants to fill in a questionnaire. The first question was about the “usual” consumption (UC) of sugar in this type of context:

- “Usually, when you buy a coffee in a standard coffee machine, how many sugars do you choose?” (0, 1, 2, 3, 4, or 5).

Before answering the acceptability questions, a short text explained what a default nudge is (Figure 1). The experimenter ensured that the participant understood if they had been exposed to a nudge (i.e. 0 sugars default) or not (i.e. 3 sugars default).

Instruction about a default nudge:

“The university plans to change/has changed the program for coffee machines. These machines are/were programmed to offer 3 levels of sugar at the outset. New machines will be/are now programmed to offer 0 sugars.”

This procedure is called “nudge.” It aims at steering behavior in a way that is better for your health.

Knowing that they were either exposed to the nudge (0 sugars default) or not (3 sugars default), participants responded to six questions to assess the acceptability of the nudge. In our study, we used the Tannenbaum et al. (2017) scale to measure *in-situ* acceptability. Their original questions were the following:

- (1) How much do you support this approach to policy?

- (2) How much do you oppose this approach to policy?
- (3) How ethical is this approach to policy?
- (4) How manipulative is this approach to policy?
- (5) How unethical is this approach to policy?
- (6) How coercive is this approach to policy?

These six questions were translated and adapted in French in accordance with the process described by Vallerand (1989).³ Participants responded to the six questions using a 5-point scale ranging from “1 = not at all” to “5 = very much so.” Cronbach’s alpha was calculated. The internal consistency was satisfactory ($\alpha = .75$). Consequently, the scores for the six items were combined and averaged to compute an index of nudge acceptability. Then, we measured attitude strength toward reducing sugar consumption ($\alpha = .71$) with self-report measures inspired by Brannon et al. (2007). For instance, we asked participants to indicate on a 7-point scale how important it was for them to reduce sugar consumption (see Appendix 2). Indeed, Vetter and Kutzner (2016) have shown that such an attitude strength might moderate the efficiency of nudges that consist in changing the default option.

At the end of the questionnaire, we collected sociodemographic data to ensure that the two samples (Campus A and Campus B) were comparable. Participants were thanked and debriefed after collecting details of age, gender, education level, the department in which they were enrolled, and the social class to which they thought they belonged.

3.3. Participants and power analysis

Jachimowicz et al. (2019) carried out a meta-analysis on the efficiency of nudges that consist in changing the default option. They found a medium-to-strong effect size $d = 0.68$ (95% IC = [0.53, 0.83]). We ran an *a priori* power analysis with G*Power 3.1.9.2® (using $d = 0.68$, power = 95%, $\alpha = .05$, $N1/N = 1$) to determine the sample size. The required total sample size was estimated at 96 participants, with 48 participants in each condition (see Table 1). We ran a sensitivity analysis (Figure 2) to explore the variations of the effect size as a function of the sample size. We observed that with 150 participants, we could expect a medium effect size ($d = 0.54$). In the meta-analysis of Jachimowicz et al. (2019), the lower limit of the confidence interval was $d = 0.53$. Consequently, it was safer to collect data from up to 165 people. We approached

163 participants (84 in Campus A and 79 in Campus B), and after having applied exclusion criteria, 145 participants remained (78 in Campus A and 67 in Campus B).

FIGURE 1: power analysis

FIGURE 2: sensitivity analysis

We excluded 18 participants in total, including 1 who had diabetes (Campus B, 3 sugars default), 3 who declined to have their data used in a study (Campus B, 3 sugars default), 1 participant who did not understand that they would be exposed to a nudge (i.e. Campus B, default = 3 sugars), 7 who did not belong to the university where the study took place⁴ (3 on Campus A and 4 on Campus B), and 6 who were considered as outliers. Leys et al. (2013) recommend using the median absolute deviation to identify outliers. As we announced in the preregistration, we ran this method on Acceptability, Observed Consumption (OC), and Usual Consumption (UC). Participants with a cutoff score higher than 3 or lower than -3 were considered as outliers. Regarding the results of the MAD method, we identified six outliers on Acceptability (3 on Campus A and 3 on Campus B).⁵

The final sample was composed of 145 participants, with 78 on Campus A (i.e. 0 sugars) and 67 on Campus B (i.e. 3 sugars). The mean age of the participants was $M_{age} = 21.5$ ($SD_{age} = 3.16$), and gender was unbalanced, with more participants who identified as female ($n = 95$) than those who identified as male ($n = 48$) or as non-binary ($n = 2$). Participants identified themselves with different social classes: 16 participants (11%) considered they were high class, 80 (55.2%) considered they were middle class, and 49 (33.8%) considered they were lower class. The usual sugar consumption was relatively low ($M = 1.34$, $SD = 1.44$). A detailed presentation of this sample is presented in [Table 1](#).

It is important to specify that participants had the opportunity to terminate the study at any time. The study was terminated immediately after participants expressed their desire to terminate the experiment. The participants' answers were individual. The equipment and scales used were designed for this study. The study was single-blind. The experimenters knew the objectives of the project. They were trained to explain what a nudge is and to report back at the end of the study. After the last item of the questionnaire, the experimenter debriefed the

participants, explaining the purpose of this study and allowing them to withdraw if they wished.

TABLE 1: Descriptive statistics comparing Campus A (0 sugar, Montpellier 3) to campus B (3 sugars, Aix-en-Provence)

4. Results

4.1. Preregistered analyses⁶

We performed a *t*-test to check that participants in one condition did not have a greater attitude strength toward reducing sugar consumption than those in the other condition. We found no statistically significant difference between the groups, $t(143) = -0.8$, $p = .43$, $d = -0.13$, 95% IC = $[-0.45, 0.19]$. Attitude strength was equivalent between Campus B, with the default option set to 3 sugars ($M = 4.43$; $SD = 1.12$), and Campus A, with the default option set to 0 sugars ($M = 4.29$; $SD = 0.98$).

4.1.1. Efficiency of the default option

As announced in the preregistration, we tested the normality of the Observed Consumption distribution ($W_{\text{Shapiro-Wilk}} = 0.82$, $p < .001$). Due to the strong violation of normality assumption, we applied several transformations, but none of them corrected this issue. Therefore, we ran non-parametric tests (Kruskal–Wallis ANOVA) on Observed Consumption to limit this issue. The effect of the default option was significant, $\chi^2_{\text{Kruskal-Wallis}}(1, N = 145) = 39.48$, $p < .001$, $\varepsilon^2 = .28$,⁷ which is consistent with hypothesis 1. This result supports the idea that the nudge on Campus A (setting 0 sugars as default) led to a lower consumption of sugar ($M = 0.82$; $SD = 1.32$) than on Campus B ($M = 2.29$; $SD = 1.3$) where the default was set to 3 sugars. Nevertheless, as discussed before, Usual Consumption (UC) affected Observed Consumption. We computed a measure of Corrected Consumption (CC = OC-UC) to take this effect into account. The results of the Kruskal–Wallis ANOVA were consistent (but weaker) with the previous results about nudge efficiency, $\chi^2_{\text{Kruskal-Wallis}}(1, N = 145) = 7.06$, $p = .008$, $\varepsilon^2 = .05$. The mean difference was greater for Campus B, with the default option set to 3 sugars ($M = 0.64$; $SD = 1.28$), than for Campus A, with default option set to 0 sugars ($M = 0.18$; $SD = 1.11$).

We also coded Observed Consumption (OC) of sugar as a binary variable equal to 0 if $OC = 0$ and 1 if $OC \geq 1$. We called this variable Binary Observed Consumption (BOC). Then, we ran a binary logistic regression with BOC as a DV and “Default (0 sugars vs. 3 sugars),” “Usual Consumption,” “Attitude Strength toward reducing sugar consumption,” and the interaction between “Default” and “Attitude Strength” as predictors. Results indicated that the interaction between “Default” and “Attitude Strength” ($\chi^2(1) = 1.16, p = .28$), did not have a significant effect on Binary Observed Consumption (BOC) of sugar. However, we observed significant effects of Attitude Strength toward reducing sugar consumption on BOC ($\chi^2(1) = 4.76, p = .028, \exp(\beta) = 0.56, 95\% \text{ IC} = [0.31, 0.94]$). Usual Consumption had a significant effect on BOC too ($\chi^2(1) = 40.45, p < .001, \exp(\beta) = 3.35, 95\% \text{ IC} = [2.18, 5.56]$). As predicted by hypothesis 1, more people on Campus B (85.1%) put sugar in their beverages than on Campus A (33.3%, $\chi^2(1) = 40.81, p < .001, \exp(\beta) = 26.07, 95\% \text{ IC} = [8.36, 101.88]$). These results are summarized in [Table 2](#).

TABLE 2: Results of binary logistic regression using Binary Observed Consumption (BOC) as the criterion

Taken together, all these analyses supported H1, which predicts that the default option set to 0 sugars induces a lower sugar consumption compared to the default option set to 3 sugars. The nudge used in the hot drinks–vending machine seemed to be efficient in reducing sugar consumption.

4.1.2. Default option acceptability

We hypothesized (H2) that the nudge (setting default option to 0 sugars) would be more accepted on Campus A (after being nudged) than on Campus B (before being nudged). We measured acceptability with the 6-item scale from Tannenbaum et al. (2017) described earlier.

Acceptability distribution strongly violated the normality assumption ($W_{\text{Shapiro-wilk}} = 0.92, p < .001$). We applied several transformations, but none of them corrected this issue. We ran a Kruskal–Wallis ANOVA to test whether acceptability varied between the campuses. We found no statistically significant differences, $\chi^2_{\text{Kruskal-Wallis}}(1, N = 145) = 0.09, p = .76, \epsilon^2 = .001$. The acceptability of the nudge was not judged

differently by those who were going to be nudged (Campus B, $M = 4.35$; $SD = 0.56$) and those who had been nudged (Campus A, $M = 4.38$; $SD = 0.52$). We can note that the level of acceptability was very high for both groups, indicating a strong acceptance of nudging regardless of whether it had been or was going to be implemented.

4.1.3. Exploratory analyses

As an exploratory analysis, we computed the ranked correlation between acceptability and Observed Consumption (i.e. efficiency) as Cadario and Chandon (2019) did. We added Usual Consumption of sugar, Age, and Attitude Strength to the correlation matrix (cf., Table 3). Acceptability correlated weakly but significantly with Attitude Strength ($\rho = .17$, $p = .041$), which was the only significant correlation with acceptability. The more favorable participants were to the reduction of sugar consumption, the more they accepted the nudging policy.

TABLE 3: Means, standard deviations, ranked correlations, and reliabilities

4.2. Deviations from the IPA

4.2.1. Comparisons of the two campuses

Data collection was disrupted by the COVID-19 crisis and led to deviations from the IPA. There were significantly fewer participants who identified themselves as females on Campus A (45 out of 78 = 57.7%) than on Campus B (50 out of 67 = 74.5%) [$\chi^2(1) = 4.58$, $p = .03$]. Consequently, we controlled gender effects in complementary analyses in order to test our hypotheses in a more appropriate way. Otherwise, comparing the two campuses on variable age with a *t-test*, we could not reject equality of means, $t(143) = -0.98$, $p = .33$, $d = -0.16$, 95% IC = [8.36, 101.88]. The mean age on Campus A ($M = 21.69$; $SD = 3.05$) was very close to the mean age on Campus B ($M = 21.18$; $SD = 3.27$). Regarding social class belonging, there was no statistically significant difference between the campuses [$\chi^2(2) = 1.28$, $p = .53$]. We compared the two campuses on usual sugar consumption (UC). Due to the distribution of the data, we used Poisson regression with overdispersion. Results

showed that participants from Campus A (i.e. 0 sugars) reported a statistically significant lower sugar consumption ($M_{UC} = 1$, $SD_{UC} = 1.34$) than those ($M_{UC} = 1.66$, $SD_{UC} = 1.49$) from Campus B (i.e. 3 sugars), $\chi^2(1) = 7.50$, $p = .006$, $exp(\beta) = 0.6$, 95% IC = [0.42, 0.87]. Consequently, we controlled sugar consumption in the complementary analyses.

4.2.2. Complementary analyses on default option efficiency

We ran complementary analyses to determine whether the nudge in Campus A was efficient when controlling for variables differing between the two campuses. Due to the distribution of Observed Consumption, we used Poisson's regression with overdispersion. Our sample presented specificities (gender imbalance, usual sugar consumption variations for each campus. . .). Consequently, we included covariates in our analyses such as the Usual Consumption of sugar (UC: number of sugars usually added to coffee), Gender (Female = 0.5; Others = -0.5),⁸ Attitude Strength, and its interaction with Default (0 sugars vs. 3. Sugars). The results of this Poisson regression are summarized in Table 4.

TABLE 4: Results of Poisson regression (overdispersion) using Binary Observed Consumption (BOC) as the criterion

Our results support hypothesis 1 about the efficiency of the nudge. Observed sugar Consumption (OC) was lower on Campus A with the default option set to 0 sugars ($M_{OC} = 0.82$, $SD = 1.33$) than on Campus B with the default option set to 3 sugars ($M_{OC} = 2.3$, $SD = 1.3$, $\chi^2(1) = 25.83$, $p < .001$, $exp(\beta) = 2.24$, 95% IC = [1.63, 3.11]). Usual Consumption was a significant predictor of Observed Consumption ($\chi^2(1) = 42.31$, $p < .001$, $exp(\beta) = 1.39$, 95% IC = [1.26, 1.53]). We did not find a significant effect on Observed Consumption for Gender ($\chi^2(1) = 0.01$, $p = .97$) nor Attitude Strength ($\chi^2(1) = 0.88$, $p = .35$) nor its interaction with Default ($\chi^2(1) = 0.42$, $p = .52$).

We then used Binary Observed Consumption (BOC) as DV. We ran a binary logistic regression with the same variables as those we included in the Poisson regression. The results of this binary logistic regression are presented in Table 5. They are quite similar to those obtained with the Poisson regression, except for the effect of attitude strength toward reducing sugar consumption which is significant ($\chi^2(1) = 4.92$, $p = .03$, $exp(\beta) = 0.55$, 95% IC = [0.3, 0.93]). The greater the

Attitude Strength, the lower the Observed Consumption. Gender ($\chi^2(1) = 0.17, p = .68$) and the interaction between Default and Attitude Strength ($\chi^2(1) = 1.17, p = .28$) did not have a significant effect on Binary Observed Consumption (BOC) of sugar. Conversely, results indicated that Usual Consumption was a significant predictor of Binary Observed Consumption ($\chi^2(1) = 40.49, p < .001, \exp(\beta) = 3.35, 95\% \text{ IC} = [2.18, 5.55]$). As predicted by hypothesis 1, there were more people on Campus B (85.1%) who put sugar in their beverage than on Campus A (33.3%, $\chi^2(1) = 40.18, p < .001, \exp(\beta) = 25.76, 95\% \text{ IC} = [8.25, 100.45]$).

TABLE 5: Results of binary logistic regression using Binary Observed Consumption (BOC) as the criterion

All the complementary analyses supported hypothesis 1 according to which the default option set to 0 sugars would lead to lower sugar consumption compared to a default option set to 3 sugars.

5. Discussion

This study highlights two main results. First, we found a strong effect of the default option on Observed sugar Consumption and Binary Observed Consumption. When controlling for Usual sugar Consumption, students on Campus A (0 sugars default) consumed about 1.5% less sugar than those on Campus B (3 sugar default). Moreover, students on Campus A (33.3%) were half as likely to put sugar in their coffee during the experiment as those on Campus B (85.1%). These results aligned with previous studies (for meta-analysis, see Jachimowicz et al., 2019). However, the strength of the preselected option on Binary Observed Consumption is surprisingly high ($\exp(\beta) = 26.09, 95\% \text{ IC} = [8.6, 101.88]$). If we convert this effect size into Cohen's d , it is about $d = 1.62$, which is twice the upper bound found by Jachimowicz et al. (2019) ($d = 0.68, 95\% \text{ IC} = [0.53-0.83]$). Mertens et al. (2021) conducted a meta-analysis testing the effectiveness of different types of nudges in various domains. Their results highlight that the strongest effect sizes are observed for default option ($d = 0.62, 95\% \text{ CI} = [0.52-0.73]$) – which they categorize as a “structure nudge” – in

particular in the food domain ($d = 0.78$, 95% CI = [0.53–0.83]). The strength of the effect size found in our study could thus be explained by our focus on a default option in the food domain. However, Maier et al. (2022) challenged Mertens et al.'s (2021) results, by pointing out that Bayesian methods controlling for publication bias question the effectiveness of most nudges. They, however, acknowledged that the effect remains uncertain for structure nudges. Maier et al. (2022) also report that nudges appear to be ineffective in most domains, with the exception of food (only when they used precise estimates). In other words, it seems that the type of nudge in our study (default option in the food domain) could constitute an exception and that our results (high efficiency of the nudge) could be very specific and not generalizable to other types of nudges.

Moreover, Jachimowicz et al. (2019) specified that the preselected option is more efficient when it operates through endorsement (i.e. when the consumer believes that the choice architect has the consumer's best interests in mind). This was exactly the case in this study. In other words, students may have thought that the default option was set for their own good (Dinner et al., 2011). Consequently, even though they like sugar in their hot beverages, participants did not change the level of sugar. Dinner et al. (2011) stated that choosing another option may require too much effort and lead to the status quo. These two phenomena (endorsement and least effort option) could explain the strong efficiency of this nudge.

The second main finding is that there is no difference between Campus A (0 sugars default) and Campus B (3 sugars default) in terms of acceptability. Regardless of whether participants thought the nudge will be implemented or had been implemented, they accepted it. The acceptability is very high (4.37 on a 5-point scale), indicating that being nudged by a default option was far from being a problem for the participants. Several dimensions of the nudge we studied may explain this high level of acceptability. First, the nudge favored the participants' health. Nudges that aim to improve health are usually among the most acceptable ones (Hagman et al., 2015). Second, reducing sugar consumption may be seen as challenging for some people. In this case, nudges are more accepted than for other domains over which people think they can easily control their behavior (Hansen & Jespersen, 2013). The attitude strength towards reducing sugar consumption could therefore partially explain why it is so well accepted. Indeed, the results of the exploratory analyses showed that only the attitude strength towards sugar consumption reduction was correlated with acceptability. The stronger the attitude, the more the nudging policy was accepted. Further research could test if this relation is

moderated by perceived control over sugar reduction. This could support the idea that sugar consumption may be seen as challenging.

Although this study allows us to draw up perspectives for future research, it has several limitations. The first concerns the difference between the two experimental groups regarding gender balance. It would therefore be useful to replicate this study with more equivalent samples, by forming pairs of participants with common characteristics identified as important in our study, such as gender, attitude towards sugar consumption reduction, past sugar consumption, and social class. It would then be possible to test the effects of the intervention in a pairwise comparison. Thus, if a difference was to appear, the probability that it is due to a difference in sample composition would be reduced.

The second limitation regards the acceptability measure taken by Tannenbaum et al. (2017). This measure has not been validated following the APA procedure. An acceptability scale could be developed, and different forms of validity of this scale could be tested (concurrent, construct, content, criterion, etc.). A more valid tool would reduce the measurement error, making the results more reliable.

The third limitation regards the positive presentation of the nudging policy. It could be the origin of a ceiling effect on acceptability, which is likely to limit our ability to measure a possible increase in acceptability after the implementation of the nudge. To test this hypothesis, we can reduce the acceptability of the default option by presenting the nudging policy more negatively. We could explain that “we used a procedure called nudge. It aims at increasing profits for the company that owns the coffee machines since the price of the drinks does not depend on the number of sugars.” Putting forward the motives of the private sector and their use of nudges⁹ for their commercial interest (see e.g. Peticrew et al., 2020 about the use of nudges by the alcohol industry) may probably decrease the general acceptability of nudging techniques. Comparing a “positive” presentation of the nudging policy (i.e. as aiming to steer behavior in a way that is better for health) to a “negative” one (i.e. as aiming to maximize profit) could generate differences in acceptability. This would give us more scope to test if a nudge is better accepted after its implementation.

6. Conclusion

To conclude, we propose to briefly comment on the broader debate about the ethics of nudging in light of our study, notably to identify variables that are worth testing in

further studies. Thaler and Sunstein (2009) consider that nudging through a change in the default option is ethical because people can still opt-out. However, Bovens (2009) argued that nudges (and especially switching the default option) are unethical because they may infantilize decision-makers, reduce their autonomy, and take away a part of their responsibility for their decisions. The strong efficiency of the default option in this study seems to plead in this sense. Indeed, the probability of putting sugar in a hot beverage was twice as strong when the default option was set at 3 sugars than when it was set at 0 sugars. It could be argued that such a drastic behavior change requires to disclose that a nudge has been implemented (i.e. that the default has been switched) to ensure some form of consent from the consumers.¹⁰ This would also limit the risk of reactance since, as Arad and Rubinstein (2018) suggested, some people prefer to be informed rather than benudged.

Another dimension worth discussing is the procedure for eliciting acceptability judgement and whether the framing of the question may influence the participants' judgment. As an illustration, we describe the strategy of changing the default option as a "mild influence" technique. The acceptability judgement could have been lower if we presented this nudge as a "manipulation" technique. Future research may investigate whether acceptability judgements are sensitive to change in the framing of the information – either in the definition of the nudge (as a mild influence or manipulation technique) or in its objective (emphasizing the interest for the target of the nudge or for its designer).

Notes

1. Significant sources of fluctuation include the source of the nudge (e.g. government or employer, Arad & Rubinstein, 2018), the intention behind the nudge (e.g. reducing cost, health protection, limiting overconsumption, Bang et al., 2018), the target of the nudge (e.g. an individual or a firm, Branson et al., 2012), and the beneficiary of the nudge (e.g. the targeted individual or the society, Hagman et al., 2015), if the nudge relies mostly on conscious or unconscious decision processes (e.g. giving advice or changing the default option; Felsen et al., 2013). Moreover, Sunstein, Reisch, and Kaiser (2018) and Sunstein, Reisch, and Rauber (2018) have shown that individual characteristics (e.g. age, sex, political orientation) are also predictors of an acceptability judgment. It appears, however, that these measures of

acceptability are not immune to manipulation. For example, Davidai and Shafir (2018) have shown that nudges are judged less acceptable when they are jointly evaluated with traditional public policy compared to separate evaluation.

2. Data collection was initially planned in 2020, from 1 March to 30 April. We had to significantly postpone data collection because of the COVID-19 crisis, as almost all courses in French universities were taught online during this period.
3. First, three translators translated items from English to French and three other translators back-translated them into English. Second, French items were subjected to context adaptation. Third, 30 French native speakers rated content clarity for the six questions.
4. Participants who did not belong to the university where the study took place could have different sugar consumption habits, a different attitude toward sugar consumption reduction, or a different cultural or academic background. For these reasons, they were considered as outliers as we announced in the preregistration.
5. Analyses including these outliers are available in supplemental materials.
6. Data and outliers' analyses can be found here: https://osf.io/wt8bm/?view_only=43de441510c046f4ba28c02c35e09135.
7. As recommended by Green and Salkind (2008), we divided the chi-square value by $N-1$ to obtain an index equivalent to η^2 .
8. There were no non-binary participants in the final Campus A sample. Consequently, we were not able to use this variable with three modalities (Female vs. Male vs. Non-binary). We decided to compare Female to others (Male + Non-binary). We ran the analyses without Non-binary participants and the results were equivalent.
9. As a note of terminology, the expression "dark nudge" is occasionally used to designate nudges aiming to steer people's behavior in a direction that is not in their best interest. The notion of "sludge" (Thaler, 2018) is also sometimes used to refer to interventions aiming to refrain certain behaviors, in general, against the person's best interests.
10. Steffel et al. (2016) argue that making a nudge by default explicit does not reduce its efficiency.

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FIGURES AND TABLES

Figure 1. Power analysis

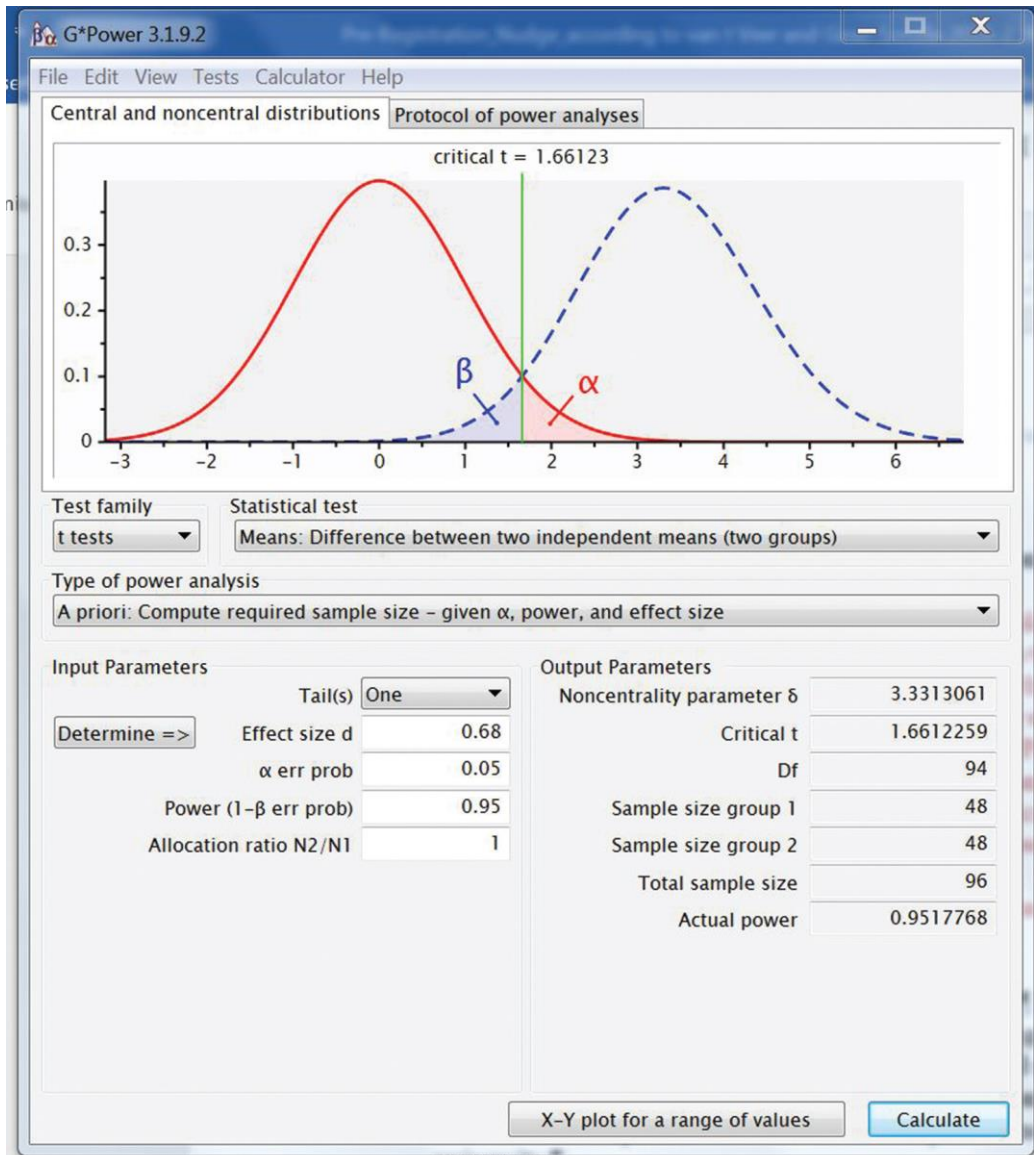


Figure 2. Sensitivity analysis

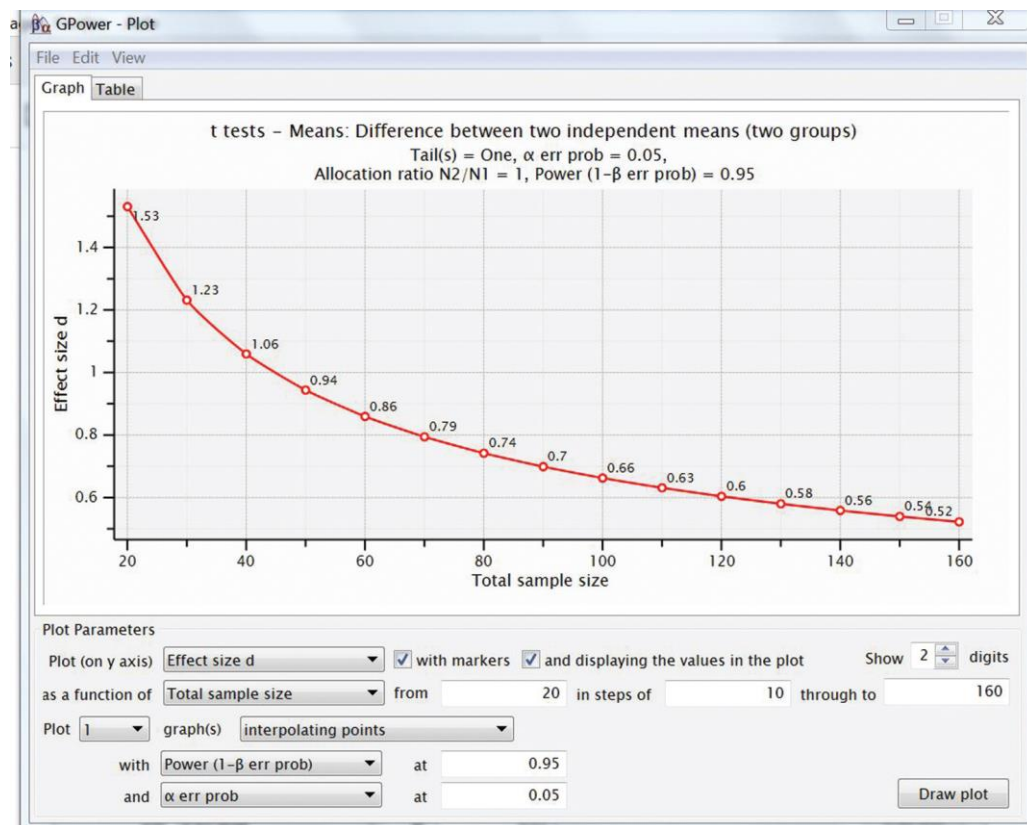


Table 1. Descriptive statistics comparing Campus A (0 sugar, Montpellier 3) to campus B (3 sugars, Aix-en-Provence)

		Campus A	Campus B	Total
Gender	Male	45 (57.7%) ^a	50 (74.6%) ^b	95 (65.5%)
	Female	33 (42.3%) ^a	15 (22.4%) ^b	48 (33.1%)
	Non-binary	0 (0%) ^a	2 (3%) ^a	2 (1.4%)
Social class	Low	29 (37.2%) ^a	20 (29.9%) ^a	80 (33.8%)
	Middle	42 (53.8%) ^a	38 (56.7%) ^a	49 (55.2%)
	High	7 (9%) ^a	9 (13.4%) ^a	16 (11%)
Age		21.7 (3.07) ^a	21.2 (3.25) ^a	21.5 (3.16)
Usual sugar consumption		1 (1.34) ^a	1.66 (1.49) ^b	1.33 (1.44)
Attitude strength toward reduction of sugar consumption		4.29 (0.98) ^a	4.43 (1.12) ^a	4.35 (1.04)

Note. The absence of shared letters indicates a significant difference at $p < .05$. For Gender and Social class, counts are indicated with the percentage in parentheses. For Age, Usual sugar consumption, and Attitude Strength toward reducing sugar consumption, means are indicated with the standard deviation in parentheses. Campus A is the default option set to 0 sugars. Campus B is the default option set to 3 sugars.

Table 2. Results of binary logistic regression using Binary Observed Consumption (BOC) as the criterion

Predictor	<i>b</i>	SE	<i>exp</i> (β)	<i>exp</i> (β) 95% CI [LL, UL]	<i>z</i>
(Intercept)	1.05***	0.32	2.87***	[1.61, 5.65]	3.33***
Default (Campus A – B)	3.26***	0.63	26.09***	[8.6, 101.88]	5.17***
UC	1.21***	0.24	3.35***	[2.18, 5.56]	5.1***
Attitude Strength	-0.58	0.28	0.56.	[0.31, 0.94]	-2.03*
Default x Attitude Strength	-0.8	0.62	0.45	[0.13, 1.47]	-1.30

Note. *b* represents unstandardized regression weights. *exp*(β) represents the incidence rate ratio (effect size). *LL* and *UL* indicate the lower and upper limits of a confidence interval, respectively. Campus A represents a 0 sugars default, Campus B represents a 3 sugars default. UC stands for Usual Consumption.

* indicates *p* < .05. ** indicates *p* < .01. *** indicates *p* < .001.

Table 3. Means, standard deviations, ranked correlations, and reliabilities

Variable	<i>M</i>	<i>SD</i>	1	2	3	4	5	6
1. Gender	0.65	0.48						
2. Age	21.45	3.16	-.15					
3. UC	1.3	1.45	.1	-.16				
4. OC	1.5	1.5	.12	-.22**	.64**			
5. Attitude Strength	4.35	1.04	.11	.2*	-.14	-.16	(.76)	
6. Acceptability	4.37	0.51	.08	.05	-.12	-.14	.17*	(.74)

Note. *M* and *SD* represent the mean and standard deviation, respectively. Values in parentheses indicate indexes of reliability. * indicates *p* < .05. ** indicates *p* < .01. The dichotomous variable was coded as follows: Gender (0.5 = Female; -0.5 = Others). UC stands for Usual Consumption and OC stands for Observed Consumption.

Table 4. Results of Poisson regression (overdispersion) using Binary Observed Consumption (BOC) as the criterion

Predictor	<i>b</i>	SE	<i>exp</i> (β)	<i>exp</i> (β) 95% CI [LL, UL]	<i>z</i>
(Intercept)	0.18**	0.09	1.2**	[1, .1.43]	2*
Default (Campus A – B)	0.81***	0.17	2.24***	[1.63, 3.11]	4.88***
UC	0.33***	0.05	1.39***	[1.26, 1.53]	6.57***
Gender F – O	0.01	0.16	1.01	[0.74, 1.39]	0.04
Attitude Strength	-0.08	0.08	0.93.	[0.79, 1.09]	-0.94
Default x Attitude Strength	0.1	0.16	1.11	[0.8, 1.53]	0.63

Note. *b* represents unstandardized regression weights. *exp*(β) represents the incidence rate ratio (effect size). *LL* and *UL* indicate the lower and upper limits of a confidence interval, respectively. Campus A represents a 0 sugars default, Campus B represents a 3 sugars default. Gender F refers to female, Gender O refers to other (male and non-binary). UC stands for Usual Consumption.

* indicates *p* < .05. ** indicates *p* < .01. *** indicates *p* < .001.

Table 5. Results of binary logistic regression using Binary Observed Consumption (BOC) as the criterion

Predictor	<i>b</i>	<i>SE</i>	<i>exp(β)</i>	<i>exp(β)</i> 95% CI [LL, UL]	<i>z</i>
(Intercept)	1.02***	0.33	3.77***	[153, 5.56]	3.13***
Default (Campus A – B)	3.25***	0.63	25.76***	[8.25, 100.45]	5.15***
UC	1.21***	0.24	3.34***	[2.18, 5.55]	5.11***
Gender F – O	0.22	0.54	1.25	[0.43, 3.66]	0.42
Attitude Strength	-0.6*	0.29	0.55*	[0.3, 0.93]	-2.11*
Default x Attitude Strength	-0.59	0.56	0.55	[0.17, 1.16]	-1.06

Note. *b* represents unstandardized regression weights. *exp(β)* represents the incidence rate ratio (effect size). *LL* and *UL* indicate the lower and upper limits of a confidence interval, respectively. Campus A represents a 0 sugars default, Campus B represents a 3 sugars default. Gender F refers to female, Gender O refers to other (male and non-binary). UC stands for Usual Consumption.

* Indicates $p < .05$. ** Indicates $p < .01$. *** Indicates $p < .001$.