Registered report

# The capacity of response training to help resist the consumption of sugary drinks

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# Abstract

Food response training has been shown to reduce the reported value of palatable food items. These approaches may thus help to reduce unhealthy (over)consumption behaviors and its related diseases. Yet, whether and how training-induced devaluation effects translate into reductions in the target items (over)consumption remains unclear. We addressed this issue by testing whether a combined food Go/NoGo and cue-approach training targeting healthy participants' favorite sugary drinks can improve how many days they resist drinking them with a double-blind randomized controlled trial. We found that the 100% mapping of motor inhibition with the target unhealthy sugary drink cues in the experimental group did not increase the number of successful days of diet compared to the 50% mapping in the control group (30.7 vs 29.8 days). One possible interpretation of this result is that the training ~~created~~induced an equivalent effect in both groups, a hypothesis supported by the finding for equivalent target item devaluation in both groups. Another possible interpretation is that the training only induced an effect on the few participants prone to fail the diet early, while we recruited mostly resourceful healthy population, as supported by a difference in dieting adherence found only in participants with early failures (18% failure in the experimental group vs. 28.2% in the control group at first quartile). The other planned contrasts did not confirm a correlation between the devaluation effect induced by the training and the number of successful days of diet (r = -0.05), and identified a weak correlation between the number of days of training and the number of successful days of diet (r = 0.22). We propose ~~conducting~~to conduct another study that includes a control training focused on non-food~~ items. This would provide a clearer answer to our main research question: “Can~~, i.e. without any mapping with food ~~response training modify real-world consumption behavior?”.~~cue.

# Introduction

Unhealthy consumption behaviors contribute to the development of most non-communicable diseases. In particular, overconsumption of energy-dense but nutrient-poor foods leads to diseases ranging from diabetes to cancer [1]. Interestingly, recent evidence suggests these practices of tasks involving the execution or inhibition of motor responses to food cues can modulate their self-reported value and their consumption [2,3].

In the food Go/NoGo (GNG) task, participants have to respond as fast as possible to healthy food cues, while withholding their responses to target unhealthy food cues. The practice of these tasks has been shown to reduce the self-reported valuation of the target NoGo unhealthy items [4–8], as well as their in-lab [9–11] and self-reported consumption [6,12,13] (see [14,15] for discussions on the underlying cognitive mechanisms of action).

In the Cue-Approach Training (CAT), participants have to respond to items when a Go-cue is displayed. Importantly, the Go-cue appears after the item, and the item disappears rapidly after the presentation of the Go-cue [16]. The practice of this task has been shown to increase the self-reported value of the trained Go items through preference tasks [17,18], snack auctions [16], as well as their consumption during bogus taste tests [19] (see [20] for a discussion on the supporting cognitive mechanisms).

Our previous work has demonstrated that the combination of these tasks in a response training intervention robustly reduces the self-reported explicit liking of the targeted unhealthy food cues, alongside a potential increase in the healthy items valuation and a decrease in the unhealthy items self-reported consumption [21,22].

However, whether and how response training intervention impacts consumption behaviors remains largely unresolved. As stated above, current evidence for a reduction in food consumption after food response training relies either on self-reported consumption outcomes such as food frequency questionnaires or food journals [6,12,13], or on laboratory tasks such as food buffets or bogus taste tests [9,10,23–26]. While these studies observed modulations in consumption, they do not directly demonstrate real-world effects. Indeed, the effect of food response training remains mixed on physiological parameters (e.g., BMI, body fat) [6,7,21,27–30], self-report measures are intrinsically biased because of memory and social confounds [31], and laboratory settings only partly mimic ecological situations. To our knowledge, the only study reporting real-world effects focused on eating disorder symptoms and were thus potentially confounded by the clinical condition of the population of interest [8].

We aimed to bridge this gap by testing with a double-blind randomized controlled trial whether a gamified food response training intervention combining a Go/NoGo and CAT can improve adherence to a restrictive diet focusing on the participants' favorite sugary drinks. Adherence to a restrictive diet is valuable to index the real-world effect of food response training because: i) it represents an important use-case for conditions such as diabetes or food intolerance; improving the success rate of restrictive dieting would demonstrate the relevance of such intervention as an adjuvant approach to conventional interventions in (sub-)clinical populations; and ii) letting the participant stop their training whenever they want in a two-weeks window enables to investigate the link of the intervention's length on its real-world effect size.

The intervention was implemented in an online gamified smartphone app, to capitalize on our replicated result showing a robust 20% reduction in the valuation of the target food items [21,22]. The target items in this study were sugary drinks, an ideal target to study real-world consumption behaviors as they display highly recognizable brands with marked and stable interindividual preferences [32], and are rarely shared with peers.

The effect of the intervention was contrasted with a mechanistic control group only differing in the active ‘ingredient’ of the training: the cue-response mapping rules will be 100% in the experimental and 50% in the control group. This contrast allowed us to control for the confounding factors developed by food cue exposure and cognitive training. We expected that: Hypothesis H1) the participants in the experimental training group will maintain more days of successful sugary drinks restrictive dieting than in the control training group; H2) that the amplitude of the reduction in the targeted items’ explicit liking will be positively associated with number successful days of adherence to the diet in the experimental group; H3) that the more a participant in the experimental group will train, the larger the effect of the intervention will be on their dieting behavior.

A detailed design table detailing the hypotheses and their rationales at Stage 1 can be found at the end of the method section (Table 1).

# Method and Materials

All materials, including scripts, data, and stimuli, can be accessed via our Open Science Framework (OSF) project page (view-only link: https://osf.io/s4trh/?view_only=4934c0215f2943cfb42e019792a30b53).

## Sampling plan

Based on the resources at our disposal, we could not allow to recruit more than 140 participants (70 in each group). As such, power sensitivity tests were conducted to determine the minimal effect size detectable with our resource constraints, a power of 90%, and an alpha of 0.05 for each hypothesis (see [33] for discussion).

For H1, power sensitivity analysis using G*Power [34] shows that a Cohen’s d of 0.5 (medium effect) would be the minimal statistically detectable effect for a one-sided independent t-test with the above-mentioned parameters. Based on the large variation in dieting adherence observed in the literature (e.g., [35]), observing a medium difference is enough for us to interpret such effect size as relevant in settings aiming at facilitating restrictive diets. Indeed, an additional 5 days of diet (extracted from a Cohen’s d of 0.5 with an estimated standard-deviation of 10 days) would be associated with physiological and cognitive modifications that might be detectable and considered relevant by the participants and the health care providers (i.e., reduction in appetite, higher energy level stability, induction of consumption habits, and realization by the participant that restriction can be maintained).

For H2 and H3, which only consider the experimental group, the smallest detectable effect size of interest is r = 0.24 (small correlation coefficient [36]) as computed by the pwr R package [37] for a one-sided correlation with the above-mentioned parameters. We consider that the coefficient should be of at least $r \geq 0.4$ to consider the association between the decrease in explicit liking and dieting behavior (H2) or between the length of the intervention and its effect (H3) as non-negligible.

Because correlations capture both causal relationships and indirect connections, the observed correlations in our study will inherently exceed their causal effects. If we were to identify correlations below 0.4 for both H2 and H3 (equivalent to 16% of explained variance), it would signify that less than 16% of the variance is attributable to causation. This criterion is the lowest that we consider ensuring that our findings effectively justify to conduct further research on these relationships' (causal) significance.

While impactful effects of restriction would need longer reduction of sugar intake to take place (reduction in weight, dental health improvement, reduced risk of non-alcoholic fatty liver disease, etc.), we consider that reaching 5 additional days of restriction would represent a proof of principle that response training interventions can facilitate restrictive diets. Likewise, we consider the indication for the correlative association we target between the devaluation, amount of training and days of successful dieting to be minimally sufficient to justify trials testing a causal association between these factors. We acknowledge that smaller effect sizes could also be relevant, but we set these large smallest effect sizes of interests to reinforce the argument to conduct on this basis heavier interventional research efforts.

The study was planned to stop recruiting after reaching 140 participants with complete data. Because of the nature of this study, where participants are continuously recruited, some participants may still be in training after reaching the 140th complete participant, thus resulting in an eventual larger sample size. From previous data [22] in our group, we expected ca. 15 participants to complete the study after the 140th, totaling to a sample size of 155. We expected to exclude 4 or 5 participants to comply with the positive controls, and 8-10 due to the exclusion of distribution outliers (total exclusion: 12 to 15). In the end, we should have reached an estimated 140 participants after exclusion.

## Recruitment and screening

Participants were recruited via public advertisement.

We included 18- to 45-year-old healthy individuals willing to follow a sugary drink restrictive diet. Ineligible participants include self-report of past or current eating disorders, any visual or hearing disability preventing gamified training, and any olfactory or gustative impairment (including smokers consuming ≥10 cigarettes daily). We also excluded participants with previous participation in a food executive control training study, and pregnant participants or participants planning to be pregnant.

## General procedure

Participants signed a consent form and were screened for eligibility criteria through a custom-made health questionnaire. They were then given access to our online training software – The Diner – via an app store and filled out in-app analogue scales of items' drinking frequency and explicit liking.

They then completed a combined gamified GNG and CAT tasks for 20 minutes per day (10min for each task), for a minimum of 7 days and a maximum of 20 days. The trained Go items were water pictures, and the NoGo items were only the participant's 8 most drunk sugary items. Participants had the option to stop the study at any time through an "End training" button appearing in the software after the minimum 7 days of training, which in turn blocks the game and triggers the post-training measures.

After training, participants completed the post-training analogue scales of explicit liking and were asked to avoid their trained sugary drinks (i.e., those selected as their most consumed) for as long as possible. Their adherence to the diet was measured with weekly questionnaires asking if their diet was successful, and if not, the exact earliest day they again consumed one of the target sugary drinks, for a maximum of two months. A debriefing questionnaire assessed whether they consumed other types of sugary drinks as a compensatory strategy for exploratory purposes.

## Stimuli

The stimuli were sugary drinks as they have shown a robust reduction in self-reported consumption after training in our previous study [22], have marked individual preferences, and their consumption is easier to track than for solid snacks.

53 pictures of sugary drinks and 7 pictures of water bottles were used as items. They represent the most popular drinks marketed in Switzerland (they can be downloaded on our OSF page https://osf.io/s4trh/?view_only=4934c0215f2943cfb42e019792a30b53).

## Analogue scales

In-app analogue scales of drinking frequency were used to personalize the training with participants' 8 most drunk items. The question "How much do you drink this?" was asked for all sugary drink items in a randomized order, with a scale ranging from "Never" to "Very often" (0 and 100 points respectively), with a marker in the middle (neutral 50 points). Ties during the personalization process were broken by choosing at random.

The in-app analogue scales of explicit liking were the same as in our previous studies [21,22]. Before and after the training, participants rated in a random sequence their 8 most drunk items as well as the water items, from 0 (‘not at all’) to 100 (‘very much’) according to the question ‘Imagine drinking this, how much do you like it?’.

## Training tasks

The GNG and CAT training tasks were the same as in [21,22] to ensure reproducibility and to capitalize on our robust and replicated findings for an effect of this response training on item valuation.

A demonstration of the app and its training tasks can be found on our OSF page (https://osf.io/s4trh/?view_only=4934c0215f2943cfb42e019792a30b53). In both tasks, the participants had to complete as many trials as they could in one block. Each correct response awards points to the participant. After five correct responses, the reaction time threshold (RTT) was increased of a level (Table 2). After making a certain number of accuracy or speed errors (5 without powerups), as indicated by two distinct life gauges, the run was over. This process is repeated until the participants reached 10 minutes of training for each task. The participant’s highest score for a session was used as ranking in the game’s anonymous scoreboard, as to maximize motivation to the training. At the end of a session, the score was also transformed to in game currency to be exchanged with task-independent power-ups, such as bigger life gauges or a double points temporary boost, to prevent repetition-induced boredom.

***Table 2**. Difficulty parameters at each level for all tasks (in seconds)*

| | **1** | **2** | **3** | **4** | **5** | **6** | **7** | **8** | **9** | **10** | **11** | **12** | **13** | **14** | **15** | **16** | **17** | **18** |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **GNG (RTT)** | 1.1 | 1 | .9 | .8 | .725 | .675 | .625 | .575 | .55 | .525 | .5 | .475 | .452 | .43 | .407 | .387 | .36 | .33 |
| **CAT (1.25-GSD; see Table 3)** | 0.88 | .81 | .74 | .67 | .62 | .57 | .53 | .49 | .455 | .42 | .39 | .36 | .335 | .31 | .29 | .27 | .26 | .25 |

Table 3 summarizes the task parameters. Table 4 depicts the percentages of healthy (water) and unhealthy (sugary drinks) items based on the trial condition and task.

### *Go/NoGo*

For the GNG task (Fig. 1), the participants were presented with drink pictures and instructed to drag the pictures that are circled in green as fast as possible to the bottom of the screen; they had to avoid touching the pictures circled in red. A correct response was defined either by responding to green-cued pictures (hit) below the reaction time threshold (RTT) or not responding to red-cued pictures (correct rejection [CR]). In these situations, a positive green feedback (i.e., the points obtained) was displayed with a rewarding sound. In the case of a hit above the RTT, a negative orange ('too late') feedback was displayed. If they responded to a red-cued picture (false alarm [FA]) or withheld response to a green-cued picture (miss), a negative red cross was displayed as feedback. The Go and NoGo cues were delayed by 50 ms after stimulus onset for the picture to be treated by the participants' visual system before they saw the item's condition. This delay prevented the participants from only treating the cue without giving attention to the item.

To ensure response potency (i.e., a high pre-activation of motoric response), 70% of the trials consisted of Go items, and 30% of NoGo items.

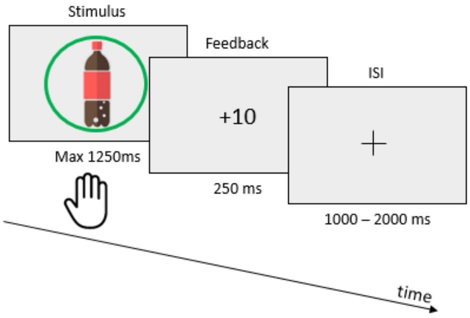

***Figure 1.*** *Schematic GNG task timeline*

*Cue-Approach Training*

In the CAT (Fig. 2), pictures appeared on the screen one after another at random locations on a grid. When a green cue was presented around the picture, accompanied by a bell sound, the participants had to click on the item before its offset occurs. If the participant responded between the cue onset and the item offset, a positive green feedback (the points obtained) was displayed with a rewarding sound. If they responded to a cued picture after the item's offset, a negative orange ('too late') feedback was shown. If they did not respond to a cued picture or responded to a non-cued item, a negative red cross appeared as feedback. In the case of correct response withholding, dark grey-green feedback was displayed with a neutral non-ascending sound, and a third of the hit point was awarded to avoid creating attentional bias during NoGo trials.

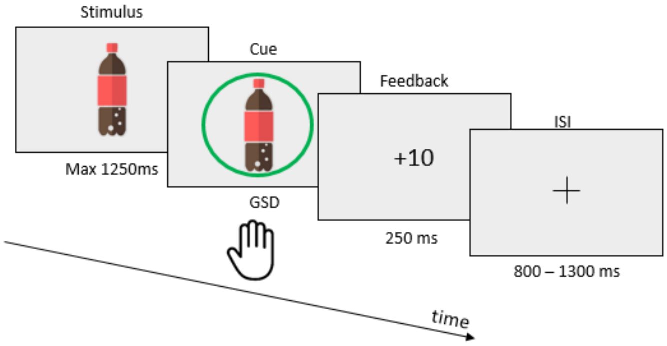

***Figure 2.*** *Schematic CAT timeline*

***Table 3.*** *Task-specific parameters*

| | GNG | CAT |
|---|---|---|
| **Go/NoGo rate** | 70% Go<br>30% NoGo | 25% Go (cued items)<br>75% NoGo (non-cued items) |
| **Stimulus duration** | 1.25 second maximum and disappearing after the response | |
| **Feedback duration** | 250 ms | |
| **Visual cue duration** | Until item offset | |
| **Visual cue delay** | 50 ms | Go Signal Delay (GSD): based on difficulty level (see Table 2) |
| **Auditory cue duration** | 300 ms | NA |
| **Auditory cue delay** | 100 ms | NA |
| **Interstimulus interval (ISI)** | 1000 – 2000 ms | 800 – 1300 ms* |

**Since the participants only respond to 25% of the trials during the CAT, we reduced its ISI to prevent boredom.*

***Table 4****. Proportion of item categories displayed for each trial condition and group*

| **Experimental group** | | |
|---|---|---|
| Item type / Trial condition | Healthy | Unhealthy |
| Go trials | 100% | 0% |
| NoGo trials | 0% | 100% |

| **Control group** | | |
|---|---|---|
| Item type / Trial condition | Healthy | Unhealthy |
| Go trials | 50% | 50% |
| NoGo trials | 50% | 50% |

## Questionnaires

Screening and demographic data were collected with a 10-items custom-made questionnaire about the participant's health and willingness to follow a sugary drink restrictive diet.

At the end of the training phase, participants received a weekly questionnaire asking if they succeeded in not drinking the trained sugary drinks and if not, the exact date of the first consumption. After reporting a drop-off, or at the two-months maximum, they were asked if they drank more of other (non-selected) sugary drinks than before the diet, to assess compensatory strategies. Expectations on the study's hypothesis were also rated using two 5-item Likert scales at the same time, asking the participants: "Do you think the researchers of this study expect that your maintenance of the diet has been improved because of the training?" and "Do you think your maintenance of the diet has been improved because of the training?" with 1 (Not at all) and 5 (Absolutely) as the anchors.

All questionnaires translated from French can be read via our OSF page under the "PROTOCOL" folder: https://osf.io/s4trh/?view_only=4934c0215f2943cfb42e019792a30b53.

## Analysis plan

A R script demonstrating the full analysis pipeline on random data can be found on our OSF page under the "SCRIPT" folder (https://osf.io/s4trh/?view_only=4934c0215f2943cfb42e019792a30b53).

All tests were performed using R base functions if not specified otherwise. The Cohen's ds were computed using the DescTools R package [38].

Only participants who finished completing the weekly dieting questionnaires were considered[1]. Dropouts and participants with missing data were not accounted for in their respective analyses.

Excluded participants (i.e., dropouts, distribution outliers, positive controls exclusion) were not planned to be replaced because of resource constraints (see Sampling plan section). The study was planned to stop recruiting after having 140 participants with complete data (i.e., all questionnaires filled).

All results were interpreted using frequentist statistics, with Bayes Factors against the null hypothesis ($BF_{01}$) reported as supplementary information to support the eventual non-significant results. The BFs were computed using the BayesFactor R package [39] with default priors. Please refer to the package manual for details on the priors (https://cran.r-project.org/web/packages/BayesFactor/BayesFactor.pdf).

H1) Participants in the experimental training will report more successful days of high sugary drinks restrictive dieting than the control training.

For this hypothesis, only the number of successful days of diet for the experimental and control groups were considered.

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[1] A mistake was made in stage 1 to consider all participants finishing the training phase. This was corrected in considering all participants who completed the subsequent dieting phase, as all hypotheses require the number of days of diet.

After the eventual exclusion of participants not respecting the positive controls (see "positive control section"), participants outside a 2.5*MAD (median average deviation; conservative criterion) range around the median of successful days of diet of their respective group was planned to be excluded.

We expected more successful days of diet in the experimental than control training condition, as assessed by a one-sided independent Welch t-test.

H2) The reduction in the explicit liking of trained items in the experimental group will correlate positively with the number of days of successful dieting.

For this hypothesis, only the pre-post reduction in sugary drinks explicit liking and the number of days of training in the experimental group were considered.

When computing the average explicit liking of each participant, we excluded items with a reaction time shorter than 300 ms to ensure a thorough filling of the analogue scales. Then, the pre-post-training differences were computed.

Participants outside a 2.5*MAD range around the median of both variables were planned to be excluded.

We expected a positive linear link between the number of successful diet days and the pre-post reduction in the trained sugary drinks' explicit liking, as assessed by a one-sided correlation test.

H3) The number of days of training in the experimental condition will correlate positively with the number of days of successful dieting

For this hypothesis, only the number of successful days of diet and the number of days of training in the experimental group were considered.

Participants outside a 2.5*MAD range around the median of both variables were planned to be excluded.

Based on previous data showing a uniform distribution of the number of training days across participants [22], we expected a one-sided correlation between the number of successful days of diet and the number of days of training to be applicable as our confirmatory test.

## Positive controls

### *Tolerance for dieting compensatory strategy*

For all hypotheses, the presence of dieting compensatory strategies (see Questionnaire section) in the experimental training condition could be tolerated, as long as the majority of participants did not report one. If the majority of the experimental training participants compensated for their restrictive diet by drinking other types of sugary drinks, then the interpretation of this study's results would have been adapted accordingly.

*Baseline reported consumption*

For H1, the baseline reported consumption frequency of the trained items should be equivalent between the experimental and control training conditions. In case of a Cohen's d above 0.4, participants who impacted this difference the most would have been excluded until this criterion was met.

*Expectation on the study's outcome*

For H1, the expectation on the impact of training on the maintenance of the diet should be balanced between groups to interpret the results without this bias. In case of a Cohen's d above 0.4 on the average score between the two Likert scales (see Questionnaire section) between the experimental and control groups, participants who impacted this difference the most would have been excluded until this criterion was met.

# Results

## Participants

During the recruitment period, a total of 1489 participants consented to participate in the study on our online recruitment page, and 629 were eligible after the screening for exclusion/inclusion criteria. Of them, 194 finished at least the minimum 7 days of training required and completed the post-training questionnaire of explicit liking. In the end, 2 more participants dropped from the study without finishing the weekly maintenance to diet questionnaires, resulting in 192 fully analyzable participants (Fig. 3; see table 5 for the participants' characteristics).

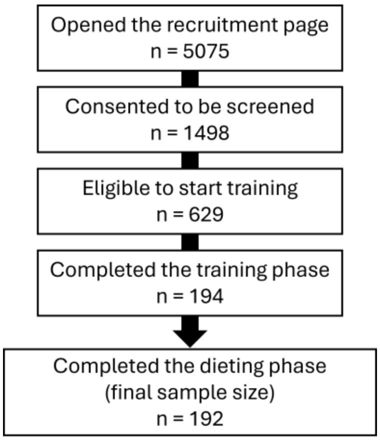


***Figure 3.*** *Sample size progression*

***Table 5.*** *Participant characteristics*

| Mean ±SD | Control (n=92) | Experimental (n=100) |
|---|---|---|
| Age | 27.3 ±6.7 | 27.7 ±6.9 |
| Gender Ratio (M/F) | 0.53 (32M/60F) | 0.79 (44M/56F) |
| Body Mass Index | 25.2 ±5.4 | 24.9 ±4.4 |
| Number of trained days | 8.1 ±2.4 | 8.3 ±3.1 |
| Baseline reported consumption (of trained items) | 57.7 ±18.8 | 59.8 ±20.5 |
| Expectation of study hypothesis (see Questionnaire section) | 3.2 ±1.1 | 3.5 ±1.0 |
| Expectation of training effect (see Questionnaire section) | 2.9 ±1.3 | 3.4 ±1.2 |
| Presence of compensatory strategy (see Questionnaire section) | 18.5% (17 yes) | 17% (17 yes) |

### Positive controls

All positive controls were respected. The presence of compensatory strategy was below the threshold (i.e., < 50%), and the differences between the control and experimental group on the study's expectations and baseline reported consumption were below the threshold (i.e., Cohen's d < 0.4; see table 5 and supp. material).

### H1) Successful days of diet

For this hypothesis, no participant was outside the outlier bounds, resulting in 192 datapoints (92 Control, 100 Experimental). Results of the number of successful days of diet are reported in table 6 and figure 4.

The experimental training group did not report a higher average of successful days of diet than the control condition, as supported by its Bayes Factor, contrary to the hypothesis (delta [95%CI] = 0.97 [-Inf; 3.98], Cohen's d = 0.05, $t_{[186.14]} = -0.32$, p = 0.373, $BF_{01} = 6.07$).

***Table 6**. Number of successful days of diet results*

| Mean ±SD | Control (n=92) | Experimental (n=100) | One-sided t-test |
|---|---|---|---|
| Number of successful days of diet | 29.8 ±21.3 | 30.7 ±20.1 | $t_{[186.14]} = -0.32$<br>p = 0.373<br>$BF_{01} = 6.07$ |

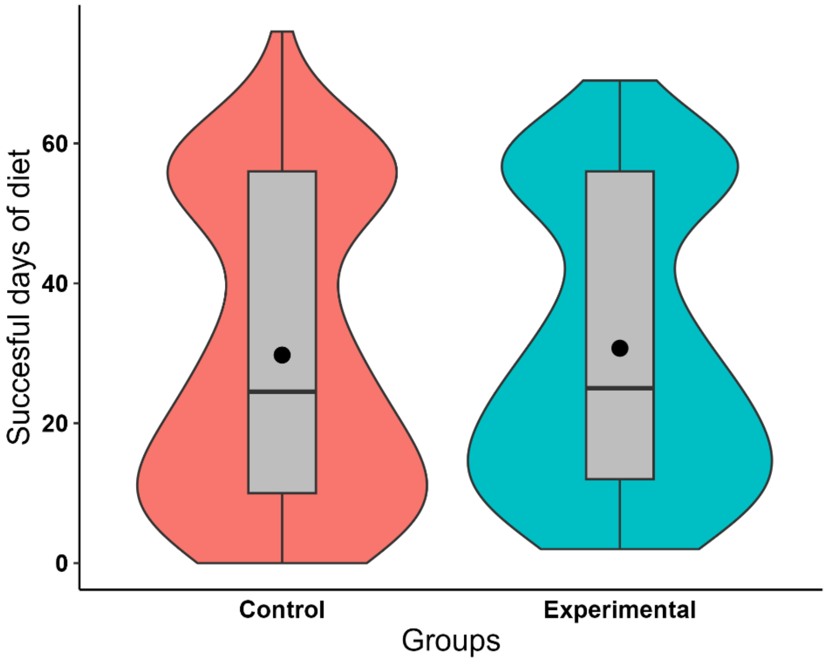
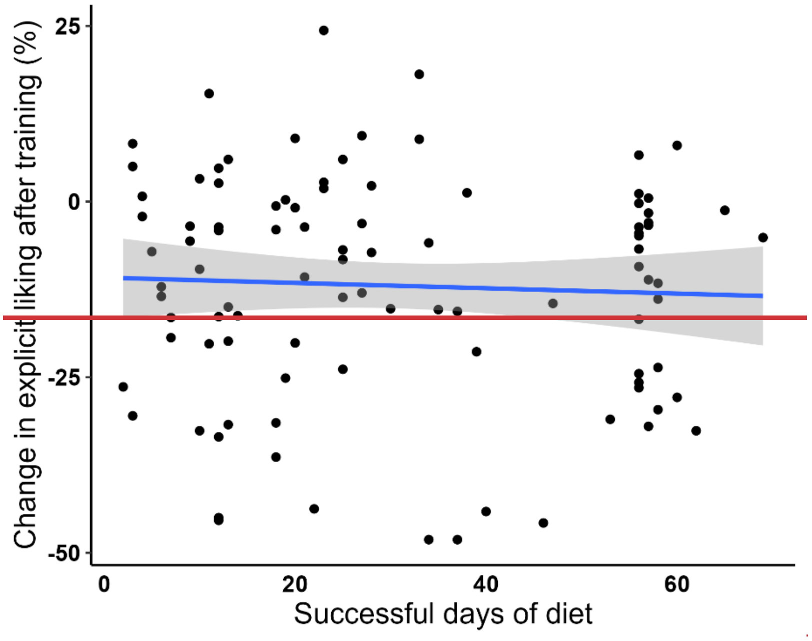


***Figure 4**. Number of successful days of diet for both groups. Means (bold circle), distributions' density (violin), medians, first and third quartiles (horizontal bars), and the 1.5 inter-quartiles range (whiskers) are represented. *: p<.05, **: p<.01, ***: p<.001.*

### H2) Change in explicit liking after training and successful days of diet

No participant was outside the outlier bounds for the number of successful days of diet. For the change in explicit liking, three participants were outside the outliers bounds, resulting in 97 datapoints. Results are plotted in figure 5.

No linear link was found between the post-pre change in explicit liking and the successful days of diet in the experimental condition, as supported by its Bayes Factor, contrary to the hypothesis (r [95%CI] = -0.048 [-1; 0.121], $t_{[95]} = -0.47$, p = 0.322, $BF_{01} = 3.87$).


Change in explicit liking after training (%)
25
0
-25
-50
0
20
40
60
Successful days of diet

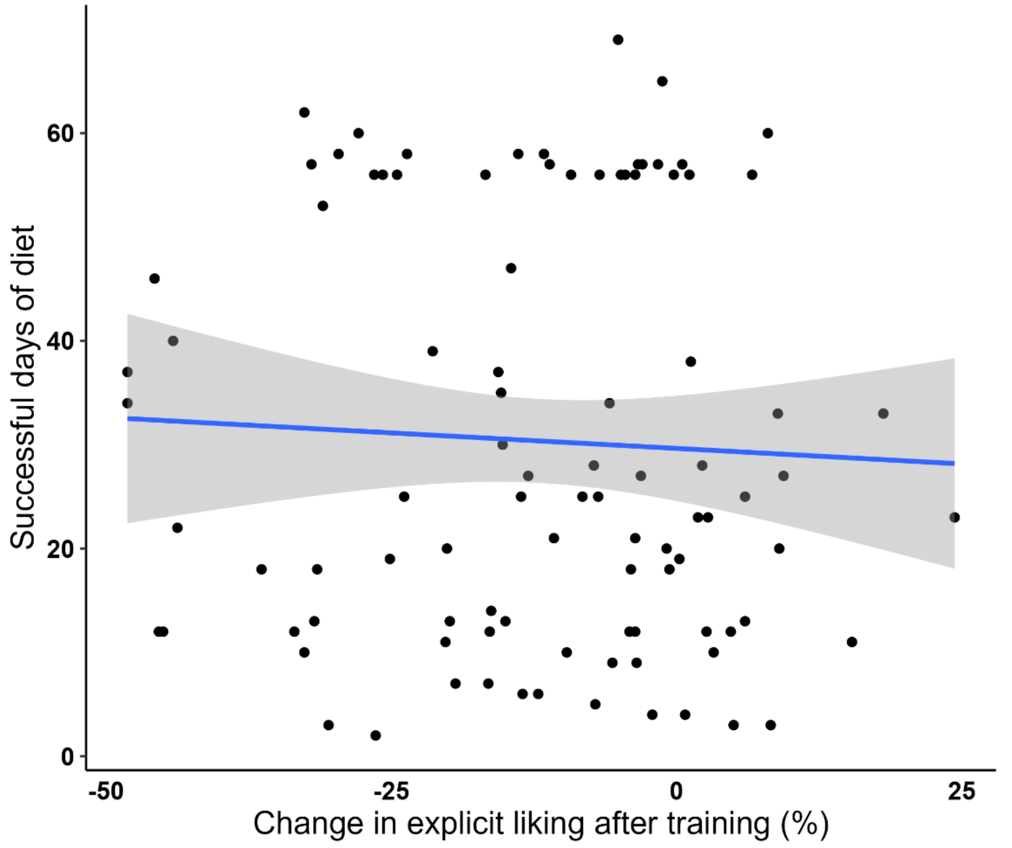

Successful days of diet
60
40
20
0
-50
-25
0
25
Change in explicit liking after training (%)

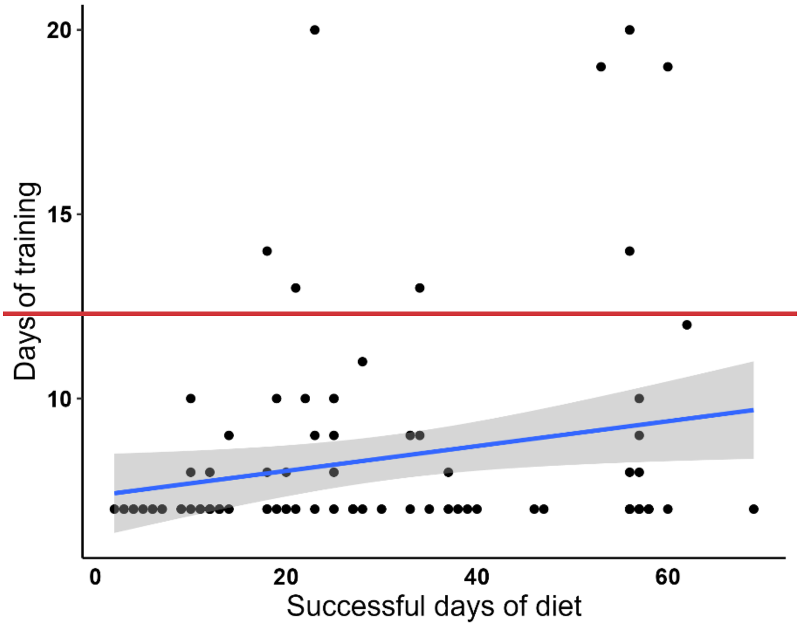
***Figure 5**. Linear link between the number of successful days of diet and the post-pre change in explicit liking for the experimental training condition. Individual points (black dots), linear regression (blue line), and error of the linear regression (grey area) are represented.*

## H3) Days of training and days of successful diet

No participant was outside the outlier bounds for the number of successful days of diet. For the number of days of training, 66 (i.e., 66%) participants stopped the training at the minimum required number of days (i.e., 7). As such, the MAD for this variable was 0, resulting in the outlier lower and upper bounds becoming the median itself. As this is contrary to the study's intent to exclude outliers disrupting the hypothesis interpretability, we decided to not follow the outlier exclusion pipeline planned in Stage 1 for this variable.

Three participants had synchronization issues that prevented our system from knowing their number of training days and were thus excluded from this analysis, resulting in 97 datapoints. Results are plotted in figure 6.

A small and significant positive linear link is found between the number of days of training and the successful days of diet in the experimental group as expected by the hypothesis (r [95%CI] = .22 [0.05; 1], $t_{[95]} = 2.17$, p = 0.016, $BF_{01} = 0.49$).



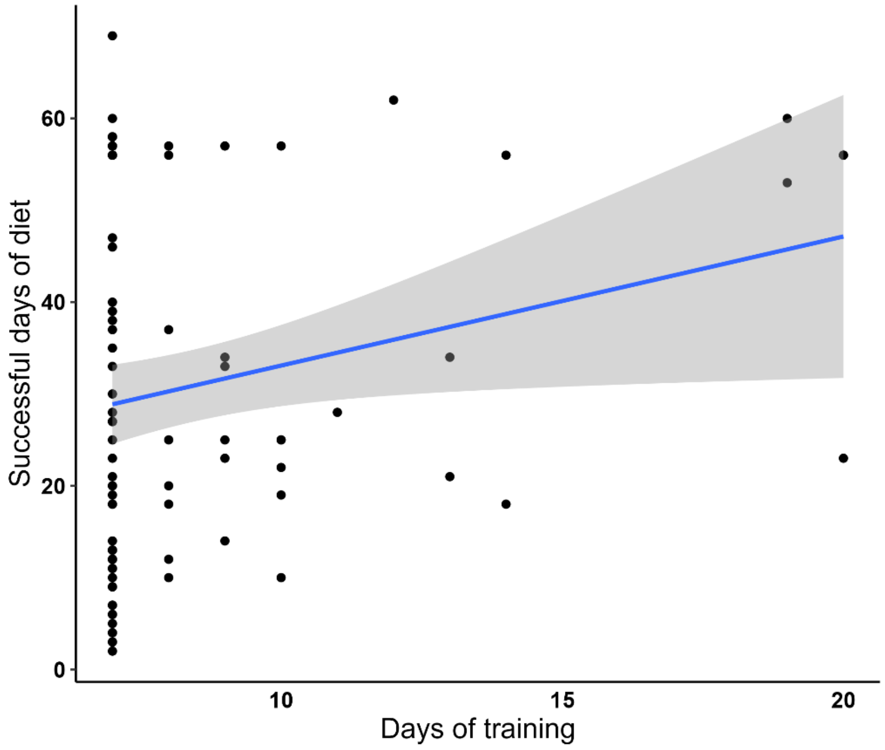


***Figure 6****. Linear link between the number of successful days of diet and the number of days of training for the experimental training condition. Individual points (black dots), linear regression (blue line), and error of the linear regression (grey area) are represented.*

## Exploratory analyses and results

### Pre- post-training effect on explicit liking

There is no Group (Ctrl vs. Exp) x Session (Pre- vs. Post-training) interaction ($F_{[1,190]}$ = 1.06, p = 0.304, $BF_{01}$ = 4.17) of explicit liking of the trained items as assessed by a mixed ANOVA. Detailed results can be found in table and figure 7.

***Table 7****. Explicit liking results*

| **Mean ±SD** | **Control** **(n = 92)** | | **Experimental** **(n = 100)** | | |
|---|---|---|---|---|---|
| | **Pre** | **Post** | **Pre** | **Post** | **Training condition x Session** |
| **NoGo item ratings (%)** | 73.8 ±12.1 | 64.2 ±16.1 | 73.7 ±13.4 | 61.5 ±16.9 | $F_{[1,190]}$ = 1.06<br>p = 0.304<br>$BF_{01}$ = 4.17 |
| **Pre-Post delta [95% CI]** | 9.59 [6.41 ; 12.77]*** | | 12.13 [8.46 ; 15.81]*** | | |

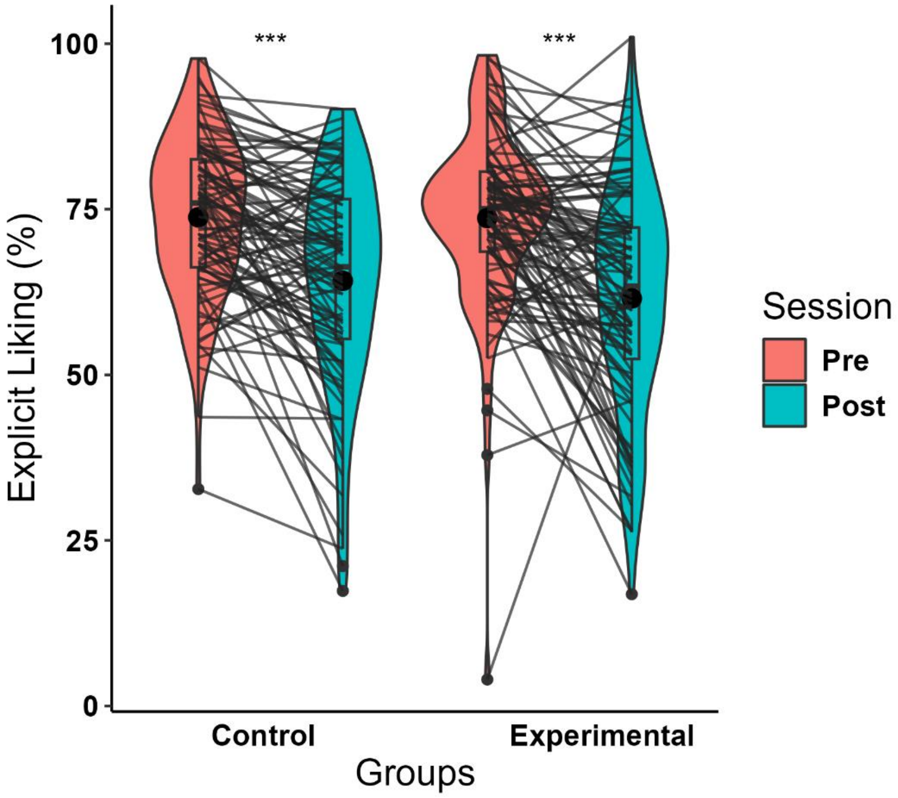


***Figure 7.*** *Explicit liking in % (y-axis) at pre- (red) and post-training (blue) for the control and experimental groups (x-axis). Individual data points (line), means (bold circle), distributions' density (violin), medians, first and third quartiles (horizontal bars), and the 1.5 inter-quartiles range (whiskers) are represented. *: p<.05, **: p<.01, ***: p<.001.*

### Diet success rate at each day

When plotting the dieting success rate across all participants at both conditions for each day (figure 8), we can observe a notable difference during the first quartile of successful days of diet (i.e., 12 days) and no difference anywhere else. The failure rate during this period of interest is higher for the control than experimental conditions (ctrl vs. exp [95% CI]: 0.282[0.185; 0.37] vs. 0.18[0.1; 0.25], $t_{[179]}$ = -1.69, p = .046; CIs computed with bootstrapping; figure 9).

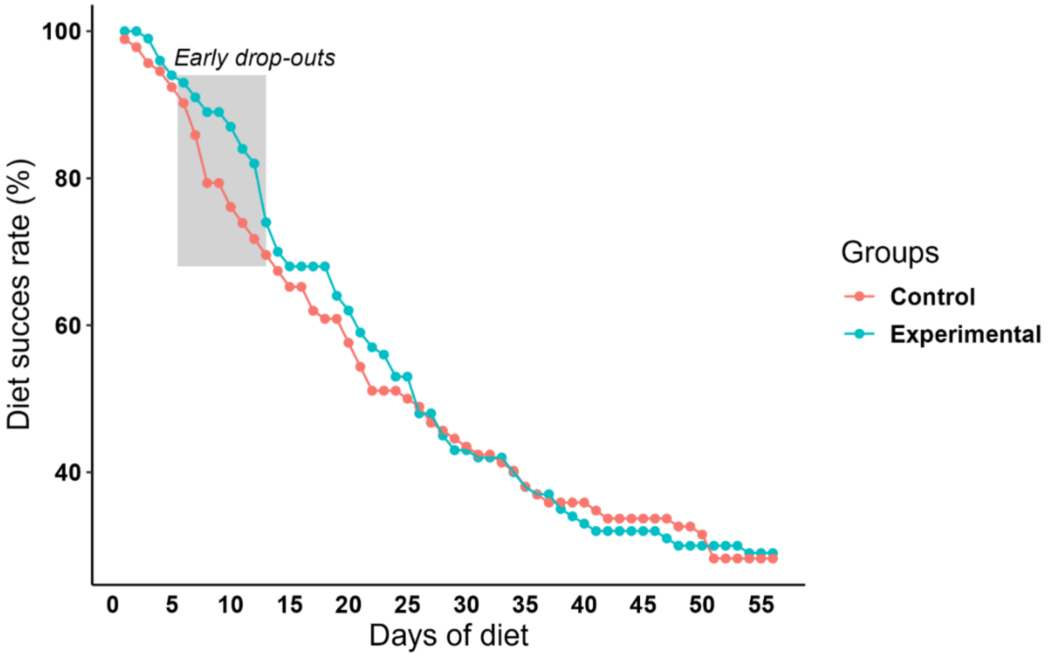


***Figure 8.*** *Diet success rate in % (y-axis) plotted at each day (x-axis) for both the control (red dots and line) and experimental (blue dots and line) groups. The grey area shows the period of interest where a notable difference between both conditions occurs.*

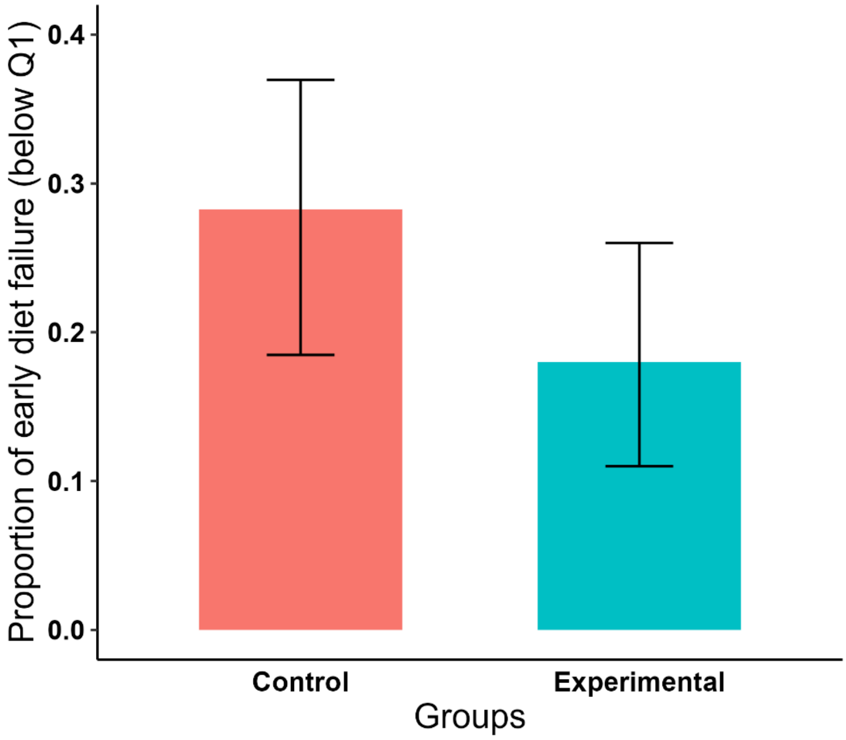


***Figure 9.*** *Proportion of early diet failure (i.e., below Q1; y-axis) for both training groups (x-axis) with 95% confidence intervals (error-bars).*

## Discussion

We assessed the impact of training participants with a seven to twenty days Go/NoGo and cue-approach training targeting the participants' most consumed sugary drinks on their capacity to adhere a restrictive diet on these items. We used a double-blind randomized design with a control group parametrically varying the putative driver of the effect response training, the consistency of associations between sugary drinks with no-response: participants were allocated to either an experimental group, with 100% association, or a control group with a 50% association. Our positive controls further minimized any remaining between-groups unbalance in expectations and in baseline consumption (Cohen's ds < 0.4) as well as compensatory strategies that might have been used during the diet. We found: i) no difference in successful days of diet in the control and experimental group (ctrl: 29.8 days, exp: 30.7 days; $BF_{01} = 6.07$); ii) no relationship between the change in explicit liking of the target items and the number of successful days of diet ($BF_{01} = 3.87$) supporting an independence between explicit liking and real-word consumption behavior; and iii) a weak linear increase in the number of successful days of diet with the number of days of training ($r = 0.22$; $p = .016$) that may be driven by the inner motivation and social desirability of the participants.

### The Choice of ~~the~~ Comparator Group ~~Prevents Observing a Real-World Effect~~

The control group used for this study is by design identical to the experimental group except for the mechanism of action posited to drive its effect: the consistency of the mapping between the unhealthy items and avoidance/inhibitory responses. This control group has been successful in the food response training literature [21,22,40,41] and has the advantage of isolating the effect of food exposure and reducing the difference of expectations to the hypotheses, contrary to non-food or no-inhibition control groups (see [42] for a discussion on control groups). ~~However, it can induce a non-negligible effect of training into the control condition [43]~~Our results demonstrate that there is no difference between the 100% and 50% mapping consistency conditions, indicating that this difference in the 'dose' of the supposed active ingredient of the training does not lead to observable change in dieting behavior in this population.

### The Effects of the Training Might Have Been the Same for Both Groups

While changes in explicit liking did not directly predict dieting success (H2: $BF_{01} = 3.87$), valuation remains a useful measure to understand how the training influenced both groups. Indeed, because we observed that the control group devalued the trained unhealthy items as much as the experimental group ($F_{[1,190]} = 1.06$, $p = 0.304$, $BF_{01} = 4.17$; Figure and table 7), we cannot conclude that response training induced a different effect between the control and experimental group in the first place. One possible interpretation for the null results of the primary hypothesis is thus that response training had the same effect for both groups. Consequently, we cannot conclude whether the food response training had an absolute effect on dieting behavior. As such, it would be important from an application-oriented perspective to investigate whether such training could have absolute effects when compared to a control condition not involving food cue exposure or mapping with motor inhibition [43]. ~~To answer our main research question (i.e., “Can food response training modify real-world consumption behavior?”, see Table 1), our hypothesis’ design relied on the control group having no or a lower effect on devaluation than the experimental group and could thus be used for an unequivocal interpretation of the mechanistic effect of an intervention (see other studies [21,22,41]). However, we observe that the control group devalued as much the trained unhealthy items than the experimental group ($F_{[1,190]}$ = 1.06, p = 0.304, $BF_{01}$ = 4.17; Figure and table 7). Since we could not be sure that the control group experienced a meaningfully lower effect of training than the experimental group, and without pre-training measures of diet capacity, our contrast cannot distinguish if the intervention resulted in an absolute increase in participants' capacity to adhere to a diet. While our primary hypothesis (i.e., “H1: Participants in the experimental training will report more successful days of high sugary drinks restrictive dieting than the control training”, see Table 1) is clearly null, our primary question remains unresolved because of the equivalent non-null effect of the control intervention [43]. This could be solved by a new study including a pre-training assessment of diet adherence compared to a group following an intervention not impacting the target items’ valuation~~We thus propose a new study including a group following an intervention not involving the target items at all.

### Individual Differences in Baseline Dieting Capacities Influence Training Outcomes

By plotting the rate of successful diet adherence at each day for both groups (see figure 8), we found no notable between-group differences except before 12 days of diet, with the experimental group being better at maintaining their diet early on compared to the control group (18% failure in the experimental group vs. 28.2% in the control group before 12 days; see figure 9). Although caution is needed because the results are exploratory and because the p-value barely reached significance (p = 0.046), one possible interpretation is that the intervention is being effective only for participants with a lower capacity to adhere to diets. Indeed, food response training typically works better for at-risk populations (defined as e.g. “overweight or heightened snacking behavior” in Lawrence et al., 2022), as also supported by cognitive bias modification research finding no effect of real-world measures on healthy samples as opposed to clinical studies [47], but our study included only healthy participants with no intent to improve their health. Our result for a different effect between the control and experimental group only on early dieting failure rate suggests that the required diet could have been too easy to follow for healthy individuals not recruited for their consumption of sugary drinks, giving a potential explanation for the primary hypothesis’ null result.

## The Role of the Number of Trained Items on the Effect of Response Training

As this study uses the same intervention as in Najberg et al., 2023, we can capitalize on this previous dataset to interpret our results. Contrary to the 2023 study, the control group devalued as much the trained unhealthy items than the experimental group ($F_{[1,190]} = 1.06$, $p = 0.304$, $BF_{01} = 4.17$; Figure and table 7). The two studies only differed for the recruited population (healthy regular soda drinkers vs. healthy wanting to diet), the average number of days trained (13 in 2023 vs. 8 days in present study) and the number of unhealthy trained items (50 in 2023 vs. 8 in present study). This large number of target items in the 2023 study resulted in less associations between unhealthy items and NoGo cues than in the present study (2023: 80 unhealthy-NoGo associations per item in the experimental group and 40 in the control group, vs. present study: 300 unhealthy-NoGo associations in the experimental group and 150 in the control group). Additionally, both studies had balanced participants' expectation between both groups as positive control (phi < 0.2 in 2023 vs. Cohen's d < 0.4 in present study). Given these differences in parameters, one possible explanation for the smaller Group x Session interaction in the current vs. the 2023 study is that that lower number of trained items, which resulted in a higher number of associations per item (i.e., the only remaining relevant difference between both studies), created  the difference between both results. If we posit that the effect of food response training evolves with the number of S-R occurrences (an assumption not yet resolved in the literature, but explored in [30,44]), then the increase in NoGo associations between the control and experimental should have created a difference in item devaluation, like in the 2023 study. As such, one interpretation for this result would be that a ceiling has been reached for this study with this large number of S-R occurrences, in turn reducing the difference between the control and experimental groups.

Furthermore, for the large number of NoGo associations to create an effect in the control group equivalent to the experimental, this would mean that the Go associations on the target unhealthy items in the control group did not influence the overall effect of the intervention. Otherwise, a smaller devaluation effect should be expected in the control than in the experimental group. This speaks in favor of the recent data that the understanding of the gesture performed in the tasks based on its instructions is crucial for an effect to arise [45,46]. Indeed, the expectations of the intervention's effect of the control group are like those of the experimental group. The unhealthy-Go associations could have disrupted the unhealthy-NoGo associations if the participants were not expecting the intervention to be effective, as they might have interpreted the unhealthy-Go associations as strongly as the unhealthy-NoGos (see [45] for a discussion on the interpretation of the participants' response in food response training).

### ~~Individual Differences in Baseline Dieting Capacities Influence Training Outcomes~~

~~By plotting the rate of successful diet adherence at each day for both groups (see figure 8), we found no notable between-group differences except before 12 days of diet, with the experimental group being better at maintaining their diet early on compared to the control group (18% failure in the experimental group vs. 28.2% in the control group before 12 days; see figure 9). Although caution is needed because of the nature of exploratory results and because the p-value barely reaches significance (p = 0.046), one possible interpretation is that the intervention is being effective only for participants with a lower capacity to adhere to diets. Indeed, food response training typically works better for at-risk populations (defined as e.g. "overweight or heightened snacking behavior" in Lawrence et al., 2022), as also supported by cognitive bias modification research finding no effect of real-world measures on healthy samples as opposed to clinical studies [47], but our study included only healthy participants with no intent to improve their health. Our result for a different effect between the control and experimental group only on early dieting failure rate suggests that the required diet could have been too easy to follow for healthy individuals not recruited for their consumption of sugary drinks, giving a potential explanation for the primary hypothesis' null result.~~

### Further Study Limitations

We posited that self-reported adherence to a restrictive diet on sugary drinks would constitute a reliable index of real-world eating behavior (cf. Introduction section). This measure, however, relies on the capacity and the will of the participants to report their behavior accurately, which limits the ecological value of this data, like with any other self-reporting measure. Furthermore, the dieting instructions required to completely abstain from the target unhealthy items, which prevents to assess if the intervention could have had an impact in modulating eating behavior more subtly, as found in previous literature [2].

### The Advantage of Large-Scale Online Training

In this paper, we were able to collect data from close to 200 participants in Switzerland, in three different languages, and with a large dispersion in socio-economic levels [48] (51.6 ±17.5; Supp. Fig. 2). Even with the requirements for participants to train for one to three weeks and then diet for up to two months, we were able to complete the data collection phase in less than seven months. This shows that online gamified training is well suited to conduct adequately powered studies on food training interventions (see [49] for discussion on technology and food response training).

### Conclusions

The current registered report concludes that seven to twenty days of combined practice of a Go/NoGo and cue-approach 100% mapping training does not improve restrictive dieting maintenance in healthy participants when compared to a control group with 50% mapping. However, exploratory results hint that it could still benefit at-~~risks population~~risk populations, although the evidence is to be treated with caution because of the ~~nature of~~ exploratory nature of the analyses and ~~of the weak~~ p-value~~. The choice of a control group including unhealthy NoGo associations and~~ close to the ~~absence of baseline measures hinder~~preset threshold for significance. Two possible interpretations for the ~~conclusion~~null results of the ~~absence of real-world effect which was our main research question (see Table 1, "Can~~primary hypothesis are: i) food response training ~~modify real-world consumption~~might have induced the same effect in both experimental and control groups, and ii) the recruited population might not have been suited to observe a change in our measure of dieting behavior~~?"). We~~ induced by response training. As it remains important to identify whether food response training can impact dieting behavior from an application-oriented perspective, we suggest conducting a similar study but with ~~an assessment of the capacity to adhere to diets before training and with a control group more adapted to lengthy training interventions~~a control group focused on non-food items.

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## Stage 1 submission

The Stage 1 of this manuscript can be found at https://osf.io/4angm/ and was uploaded by the PCI-RR on behalf of the authors.

**Table 1**: Design

| Question | Hypothesis | Sampling plan | Interpretation of the smallest detectable effect size | Analysis plan | Interpretation given different outcomes | Theory that could be shown wrong by the outcomes | Hypothesis outcome |
|---|---|---|---|---|---|---|---|
| Can food response training modify real-world consumption behavior? | H1: Participants in the experimental training will report more successful days of high sugary drinks restrictive dieting than the control training. | For 90% power, alpha = .05, and n = 140 (70 per group, based on resource constraints) for a one-sided t-test, the smallest detectable effect size would be Cohen's d = 0.50 | Based on the large variation in dieting adherence observed in the literature (e.g., [35]), we consider a medium difference to allow us to consider our effect as non-negligible in a setting aiming at facilitating restrictive diets. An additional 5 days of diet (extracted from a Cohen's d of 0.5 with an estimated standard-deviation of 10 days) would be associated with physiological and cognitive modifications that might be detectable by the participants and be relevant to health care providers (i.e., reduction in appetite, higher energy level stability, induction of consumption habits, and realization by the participant that restriction can be maintained). | One-sided t-test between participants in the experimental vs. control training group. If homoscedasticity assumption violated, GG correction.<br>If p > .05, then $BF_{01}$ will test the null hypothesis. | If the test is significant, then we interpret food response training as improving restrictive dieting capacities. If the test is non-significant and supported by a BF01 ≥ 3, then we interpret the result as null. If the test is non-significant, and not supported by a BF01 ≥ 3, then we interpret our results as non-conclusive. | If the hypothesis is not validated, then it would give support to an independence between the already observed food response training effects on reduction on items' valuation and in-lab consumption, and real-world consumption behavior. | Not confirmed. |
| Does the food response training induced reduction in perceived value influence consumption behavior? | H2: The reduction in the explicit liking of trained items in the experimental group will correlate positively with the | For 90% power, alpha = .05, and n = 140 (based on resource constraints) for a one-sided correlation, the | We consider that the coefficient should be of at least r ≥ 0.4 to consider the association between the decrease in explicit liking and dieting behavior as non-negligible. Because | If H1 is significant, then one-sided correlation between the pre-post reduction in explicit liking and the successful days of diet. | If the test is significant, then the robust devaluation effect of food response training influences restrictive dieting capacities. |  | Not confirmed |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| | number of days of successful dieting. | smallest detectable effect size would be r = .24. | correlations capture both causal relationships and indirect connections, the observed correlations in our study will inherently exceed their causal effects. If we were to identify a correlation below 0.4 (equivalent to 16% of explained variance), it would signify that less than 16% of the variance is attributable to causation. This criterion is the lowest that can still ensure that our findings effectively emphasize the need for further research on these relationships' significance. | If p > .05, then $BF_{01}$ will test the null hypothesis. | If the test is non-significant and supported by a BF01 ≥ 3, then we interpret the result as null. If the test is non-significant, and not supported by a BF01 ≥ 3, then we interpret our results as non-conclusive. | | |
| Is the amount of training linked to the intervention’s effect size? | H3: The amount of days of training in the experimental condition will correlate positively with the number of days of successful dieting. | | We consider that the coefficient should be of at least r ≥ 0.4 to consider the association between the length of the intervention and its effect as non-negligible. If we were to identify a correlation below 0.4 (equivalent to 16% of explained variance), it would signify that less than 16% of the variance is attributable to causation. This criterion is the lowest that can still ensure that our findings effectively emphasize the need for | If H1 is significant, then one-sided correlation between the amount of days of training and the successful days of diet. If p > .05, then $BF_{01}$ will test the null hypothesis. | If the test is significant, then participants should be encouraged to train for longer than one-week to reach a larger effect of food response training on restrictive dieting capacities.<br>If the test is non-significant and supported by a BF01 ≥ 3, then we interpret the result as null. If the test is non-significant, and not supported by a BF01 ≥ 3, then we interpret our results as non-conclusive. | If the hypothesis is not validated, then it would indicate either a ceiling effect appearing before a week of training, or the absence of a link between the amount of training sessions on the effect of restrictive dieting behavior. | Confirmed but with a negligible effect size (r < 0.4) |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| | | | further research on these relationships' significance. | | | | |

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