Repeated exposure and conditioning strategies for increasing vegetable liking and intake: systematic review and meta-analyses of the published literature

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ABSTRACT

Background: Vegetable intakes are typically lower than recommended for health. Although repeated exposure has been advocated to increase vegetable liking and consumption, no combination of the evidence yet provides a measure of benefit from repeated exposure or alternative conditioning strategies.

Objective: This work aimed to identify and synthesize the current evidence for the use of repeated exposure and conditioning strategies for increasing vegetable liking and consumption.

Design: Three academic databases were searched over all years of records using prespecified search terms. Published data from all suitable articles were tabulated in relation to 3 research questions and combined via meta-analyses.

Results: Forty-three articles detailing 117 comparisons investigating the use of repeated exposure and conditioning strategies for increasing liking and intakes of vegetables were found. Our analyses demonstrate: *1)* increased liking and intakes of the exposed vegetable after repeated exposure compared with no exposure; *2)* increased liking for the exposed vegetable after conditioning compared with repeated exposure, increased intakes after the use of rewards, and some suggestion of decreased intakes after flavor-nutrient conditioning; and *3)* increased liking and intakes of a novel vegetable after repeated exposure to a variety of other vegetables compared with no exposure or repeated exposure to one other vegetable. Effect sizes, however, are small, and limited evidence suggests long-term benefits. Our analyses, furthermore, are limited by limitations in study design, compliance, and/or reporting.

Conclusions: Based on our findings, we recommend the use of repeated exposure to one and a variety of vegetables, and the use of rewards, for increasing vegetable liking and consumption. Confirmation from further large, well-conducted studies that use realistic scenarios, however, is also required. This study was registered at PROSPERO as CRD42017056919. *Am J Clin Nutr* 2018;108:842–856.

Keywords: vegetable liking, vegetable intakes, repeated exposure, conditioning, systematic review, meta-analyses

INTRODUCTION

A high vegetable consumption has been associated with reduced risk of a number of chronic health conditions, including cardiovascular disease, stroke, type 2 diabetes, some cancers, and dementia and cognitive decline (1–7). Vegetable consumption across the United Kingdom, Europe and the United States, however, remains lower than WHO recommendations (8–10).

One reason frequently given for low vegetable consumption is low liking for a bitter or unappealing taste (11–16). Strategies that specifically focus on changing liking for tastes, therefore, may be beneficial for increasing vegetable intakes. Repeated exposure and conditioning strategies have previously been found to successfully change liking and intakes for a variety of tastes (17–19). Repeated exposure involves repeated experience of a novel taste without any negative association or consequence (17, 18). Conditioning strategies involve the repeated experience of a novel taste in association with: an already liked taste (flavor-flavor conditioning—FFC); a nutrient (flavor-nutrient conditioning—FNC); a rewarding consequence such as a smile or a sticker, or a rewarding consequence for another individual as occurs in social learning or modelling (17, 18). Conditioning strategies thus

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Supplemental Tables 1.1–3.2 and Supplemental Material 1 are available from the "Supplementary data" link in the online posting of the article and from the same link in the online table of contents at https://academic.oup.com/ajcn/.

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Abbreviations used: FFC, flavor-flavor conditioning; FFNC, flavor-flavor-nutrient conditioning; FNC, flavor-nutrient conditioning; ROB, risk of bias; SMD, standardized mean difference.

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involve repeated taste exposure, in association with an additional positive experience.

Repeated taste exposure and conditioning strategies have been used to successfully change liking and intakes of vegetables (20–23). Some work specifically recommends repeated exposure (21, 22), but not all studies demonstrate benefits, and the value of conditioning beyond that of repeated exposure remains controversial (21). Statistical combination and comparison of the current evidence will provide firmer conclusions and may resolve controversies. No work to date has statistically combined studies to allow assessments of degree of benefit or comparisons between different strategies. Assessments of the methodological quality of the studies involved, particularly in relation to potential mechanisms, will also aid conclusions. Comprehensive conclusions are important for ensuring the provision of evidence-based recommendations. Furthermore, although much of the evidence to date aims to increase consumption of a single vegetable, strategies to increase the consumption of vegetables as a whole food group may be more beneficial for health (20).

This work aimed to identify the current evidence for the use of repeated taste exposure and taste-based conditioning strategies for increasing vegetable consumption, assess the details and quality of that evidence, and statistically combine the evidence available to provide a measure of benefit for each of the strategies investigated. Our analyses sought to address the following questions:

1) What is the impact of repeated exposure to the taste of a vegetable for increasing liking and consumption of that vegetable, compared with no taste exposure?
2) What is the impact of conditioning to the taste of a vegetable for increasing liking and consumption of that vegetable, compared with repeated taste exposure?
3) What is the impact of repeated taste exposure or conditioning to a vegetable for increasing liking and consumption of a novel vegetable, compared with no exposure or repeated taste exposure?

METHODS

A systematic search of the published literature was conducted, and data from all studies that could contribute to our research questions were extracted and combined via meta-analysis.

Searches

Three academic databases, PubMed, PsycINFO, and MEDLINE, were searched over all years of records using 2 search strings. The first search string searched for all articles with the terms "vegetable" or "vegetables" in the title field. This search string was previously used for a review of all interventions seeking to increase vegetable intakes (20), and was rerun from the time of the earlier searches to include all articles to date from all years of records. The second search string searched for all articles with the term "vegetable" or "vegetables" in the title or abstract field and the terms "expos*" or "learn*" or "condition*" or "pair*" or "combin*" or "together" or "model*" or "reward*" or "incentiv*" or "experienc*" in the title or abstract field. Both search strings were intended to capture as much of the relevant literature as possible, while avoiding unnecessary redundancy. Reference lists of all relevant articles and all relevant reviews were also hand-searched for additional studies.

Types of study included

Studies were included in the review if they: used a repeated taste exposure or conditioning strategy to increase vegetable liking, acceptance, preferences, or intakes; were conducted in humans; involved a comparator; and included an assessment of liking or intake. We defined repeated exposure as the repeated experience of the taste of a vegetable in the absence of any negative association or consequence. We do not use the term "mere exposure," to signify the importance of the absence of a negative association or consequence. Conditioning strategies were defined as the repeated experience of the taste of a vegetable in association with an additional positive experience, e.g., sweet taste delivery, nutrient delivery, rewards. For an exposure experience to be considered conditioning, a deliberate additional positive experience was required, i.e., the nutrients naturally occurring in vegetables would not constitute an additional benefit, whereas an addition of oil would add contributions to the diet of energy and fat. Studies were included regardless of the repeated taste exposure or conditioning strategy used, but the strategy must have been purposefully implemented, and studies were only included if they used a manipulation solely for vegetables, i.e., we did not include studies that treated fruits and vegetables together, or that targeted vegetables and other foods. Studies were also only included if they used a strategy involving eating/tasting vegetables; strategies involving repeated visual exposure, e.g., via reading books, were not included. We accept that strategies involving eating/tasting vegetables necessarily also include visual exposure and can include exposures to touch and texture, but our focus was on taste. Studies that used multiple vegetables or multiple tastes were permitted. Studies that involved manipulating tastes for other foods and investigating impacts on vegetable consumption were not included, e.g. Mennella et al. (24), Sullivan & Birch (25). Comparisons and studies that involved tasting vegetables and assessed impacts on other vegetables were included, but studies or comparisons that assessed impacts on other foods, e.g., meat, were not included. Studies that used an exposure or conditioning strategy only once were not included, e.g. Correia et al. (26), Staiano et al. (27). Where studies used a repeated exposure or conditioning strategy and an additional different intervention, only the repeated exposure and/or conditioning comparisons were included, e.g. Wardle et al. (28). Studies where repeated exposure and/or conditioning strategies could not be disentangled from other strategies were not included, e.g. Leak et al. (29). Both between-groups (e.g., randomized controlled trials) and within-groups (e.g., crossover) study designs were permitted. Only studies on humans were included, but humans of any age and any population were permitted. There were no exclusion criteria based on context. Studies were included regardless of the comparator used, but a comparator group or condition must have been included. Studies with no control group or condition, therefore, were not included, e.g. Lakkakula et al. (30), Maier et al. (31). We only included studies that included a subjective or behavioral measure of liking, acceptance, preference, or intake. We included studies regardless of the method used for assessing liking, acceptance, preferences, or intakes, and regardless of the

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number of measurements taken, and the time period over which measurements were taken. We also only included full papers that were published in English: we did not include conference proceedings or book chapters, nor did we attempt to uncover unpublished work.

Data extraction

Searches were undertaken by 1 researcher (KMA), and all search results were independently screened by 2 independent researchers (KMA and AH or HH). Search results were first screened for study inclusion via titles and abstracts, and all potentially relevant papers were gained. Search results were only discarded if they were clearly unsuitable for the review. All potentially relevant papers were screened by 2 researchers (KMA and AH), and papers that met our inclusion criteria were included in the review. Data were extracted independently for each relevant study by 2 authors (KMA and AH) from all papers through the use of a data extraction form developed specifically for the work. Data were collated by study as opposed to by paper, to guard against overinclusion of some original studies that have contributed to several reports, e.g., the analyses reported by Anez et al. (32) stem from the original study reported and included in the review as that by Cooke et al. (33). All extracted data were checked and confirmed by an additional author (JR). Data on study methodology and study outcomes were extracted. Subjective ratings and intake data for both exposed vegetables and novel vegetables were treated as outcomes. Only published data were extracted—we did not contact authors.

Risk of bias (quality) assessment

Risk of bias (ROB) was assessed through the use of the following information: comparability of target vegetable and comparator, or intervention procedures and comparator procedures (ROB1); percentage of participants in analyses (ROB2); and percentage of participants correctly undertaking test procedures (regardless of reason for exclusion) (ROB3). These aspects of studies were considered a priori to facilitate a good test of each of our research questions, due to good study design and presentation of an unbiased dataset, and were aspects of each study that were likely to have been reported. Comparability between target and control vegetables or study procedures is important to allow comparisons based on taste exposure/conditioning strategy only. Percentage of participants in analyses and percentage of participants correctly undertaking test procedures are important to ensure conclusions are based on complete samples as opposed to only a select few (34). Comparability between target and control vegetables or target and control procedures was considered to offer low ROB if the majority of vegetable or procedural characteristics were recorded as comparable (>3 out of 6 criteria) and if no evidence of differences between groups/conditions was provided, whereas a judgment of high risk was given if a single characteristic was incomparable. To increase comparability in comparisons that used within-subjects designs, comparisons were only considered suitable for inclusion in the review if the control food was also a vegetable. Percentage of participants in analyses and percentage of participants correctly undertaking test procedures were considered to offer low ROB if all participants were included in analyses (i.e., analyses were conducted on an intention-to-treat basis) and if there was ≥80% compliance at all assessed time points. Judgments of high risk were given if not all participants were included in analyses and compliance was <80%, respectively. The criteria used to make judgments were based on relevant literature in the area. Judgments of unclear ROB were also possible, if information was not available to us. All assessments were made independently by 2 authors (KMA and AH) and checked and confirmed by an additional author (JR).

Data synthesis

All extracted data were tabulated per study at the group level, based on study design type and strategy used. Studies are given based on strategy used, as opposed to strategy proposed where these differed, e.g., where modelling was allowed but not specifically included as part of the study design, this study was considered as one that tested modelling (35, 36). Flavor-flavor-nutrient conditioning (FFNC) was also included as a potential conditioning strategy where this was more appropriate than FFC or FNC, because either flavors were nutritive (37) or nutrients were flavorsome (38), unless efforts were made to control for this (39). Studies are ordered in all results tables and figures by participant age.

Data from studies were subsequently combined via meta-analysis. Only studies of the same design type and investigating the same strategy were combined to ensure combination of like with like. Because randomized controlled studies remain at the top of the hierarchy of evidence (34), analyses on between-subjects comparisons were given greater priority over those that used within-subjects comparisons. Intake and liking data were analyzed separately, and for both outcomes, the most commonly used measure was continuous (grams of intake, and ratings of liking on a 3–9 point scale). Continuous data, corrected to ensure comparable direction in all outcome measures, were analyzed as standardized mean difference (SMD) with 95% CIs, using intention-to-treat data (based on number of participants at study entry) where possible. Data on number or percentage of children reporting to like/taste a vegetable were converted to scale data and included in analyses where possible, to allow inclusion of as much data as possible. Estimates were made using random-effects models primarily, due to likely heterogeneity between studies. Fixed-effect models were also applied as sensitivity analyses (40, 41). Effects in fixed-effect models are more influenced by large studies, thus effects found via fixed-effect models may be driven largely by the findings of 1 or 2 larger studies, and differences between analyses that use random-effects and fixed-effect models may suggest differing effects in small and large studies (40). Where studies used multiple treatment or comparator groups, each treatment/comparator group was treated as an independent study, and numbers involved in single comparison groups were divided. Where studies used multiple outcome assessment time points, data from immediately after the intervention period and from the longest follow-up period were used for analyses. Our focus was on assessments made immediately postintervention, in order to mitigate against additional changes to feeding or eating behavior since the intervention ceased. Analyses on longest follow-up used the data available for longest follow-up, which in some cases was immediately postintervention. Where mean data were missing,

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studies were not included in analyses, with the following 2 exceptions. Where postintervention data were missing, data collected during the intervention were used where possible. Where tests were not conducted, data were transferred from earlier time points. Missing SD data were completed from available baseline data or imputed from SD data from other studies if necessary (42). Analyses of conditioning strategy were conducted through the use of subgroup analyses based on conditioning type, based on our understanding of the strategy used (as above). Heterogeneity between studies was investigated with the use of Higgins' *I*-squared statistic (43, 44). Possible sources of heterogeneity were identified a priori, to include publication bias, and all 3 domains of ROB. Publication bias was investigated via funnel plot asymmetry (45). Funnel plots allow the investigation of treatment effect against study precision, where studies in a standard dataset would be expected to fall in a funnel shape. Funnel plot asymmetry (as detected visually) can suggest publication bias in a dataset (45). The impact of ROB was to be assessed via sensitivity analyses, which included only the studies judged to be of low ROB for each of the 3 domains assessed. For all analyses, however, too few comparisons with no judgments of a high ROB were available for these sensitivity analyses to be conducted. Comparisons with no judgments of high ROB are reported. Analyses were undertaken in RevMan (RevMan version 5.3; Copenhagen: The Nordic Cochrane Centre, The Cochrane Collaboration, 2014) on 1 May 2018. Analyses for within-group comparisons were also conducted in Stata (StataCorp Inc.) to incorporate an adjustment for reduced within-study variance in this type of study design. Findings from our primary analyses are presented as Forest plots. In Forest plots, each contributing study is represented by a filled square with horizontal lines, where the area of the square depicts the contribution of the study to the full analysis, and the horizontal lines demonstrate the 95% CIs for each study. Studies depicted to the right of a 0 line demonstrate a finding in favor of an intervention, whereas those to the left demonstrate a finding in favor of the comparator. The diamond at the base of the plot represents the combined effect (SMD) with 95% CIs (41).

Review registration

The review was registered on PROSPERO as CRD42017056919, on 16 Feb 2017, after the running of initial searches (46). Differences between our protocol and actual methods are given in **Supplemental Material 1**.

RESULTS

Searches were most recently completed on 6 Feb 2018. Our database searches found 37,218 potential articles, and reference list searching found an additional 1 article. A total of 273 full texts were considered once titles and abstracts had been searched, and finally, 43 articles detailing 117 different comparisons were considered relevant to our research questions. Many studies involved multiple comparisons. The full results of our searches are given in **Figure 1**.

All results are presented per research question, by study design type (between-subjects/within-subjects). For each research question and study design type, we present the details of relevant studies and comparisons (28, 33, 35–39, 47–82) in **Supplemental Tables 1.1–3.1**. If relevant to >1 research question, studies are presented in >1 table. Data extracted from these studies on vegetable liking and intake are presented in **Supplemental Tables 1.1a–3.1b**. All analyses on extracted data are presented in **Supplemental Tables 1.3–3.2**. Results from our important analyses are reported or summarized in the text that follows. Results from all primary analyses (analyses investigating vegetable liking or intake postintervention through the use of random-effects models) are also presented as Forest plots.

Research question 1: What is the impact of repeated exposure to the taste of a vegetable for increasing liking and consumption of that vegetable, compared with no taste exposure?

Included studies

Between-subjects comparisons. We found 24 between-subjects comparisons involving 1154 participants investigating liking for the exposed vegetable (28, 33, 36, 47–62), and 27 between-subjects comparisons involving 1536 participants investigating vegetable intake (28, 33, 36, 47–62). Comparisons were typically small in size (~20 participants per group), but 7 comparisons included 43–74 participants per group, and 2 comparisons included 105–125 participants per group (33, 61). Sixteen comparisons involved infants aged <7 mo of age, 9 comparisons included children of 2–10 y, and 2 comparisons involved adults (48). Comparisons in young infants typically used carrot or green beans for the target vegetable, whereas those in children and adults typically used neutral or moderately disliked vegetables as the target. Most comparisons compared repeated exposure with specified no exposure, but some studies used "usual care" as the control, thus some exposure to the target vegetable may have occurred. Target-control comparability was considered good (low ROB1 (ROB1)) in 18 comparisons. Few studies included follow-up periods longer than the end of the intervention, but 1 study included a 12 and 23 mo follow-up (54). Details of all comparisons that used a between-subjects design to compare conditioning with repeated exposure to a target vegetable are given in Supplemental Table 1.1.

Within-subjects comparisons. We found 9 within-subjects comparisons (256 participants) investigating liking for the exposed vegetable (39, 53, 55, 59, 63, 64), and 12 within-subjects comparisons (269 participants) investigating target vegetable intake (39, 53, 55, 60, 63–67). One comparison derived from a within-subjects study, whereas all other comparisons were nested within between-subjects studies. Participant samples ranged from 10–74 participants, aged 24 wk–10 y. All comparisons used an alternative vegetable as the nontarget vegetable, but for many comparisons this was a liked, as opposed to an alternative neutral or disliked vegetable. Only 2 comparisons were judged to have good target-control comparability (59, 65). All studies assessed outcomes immediately after the intervention, and 6 comparisons included a 5 wk or 6 mo follow-up. Details of all comparisons that used a within-subjects design to compare repeated exposure with no exposure to a target vegetable are given in Supplemental Table 1.2.

Study combination

Extracted data from the between-subjects comparisons for liking and intake are provided in Supplemental Tables 1.1a

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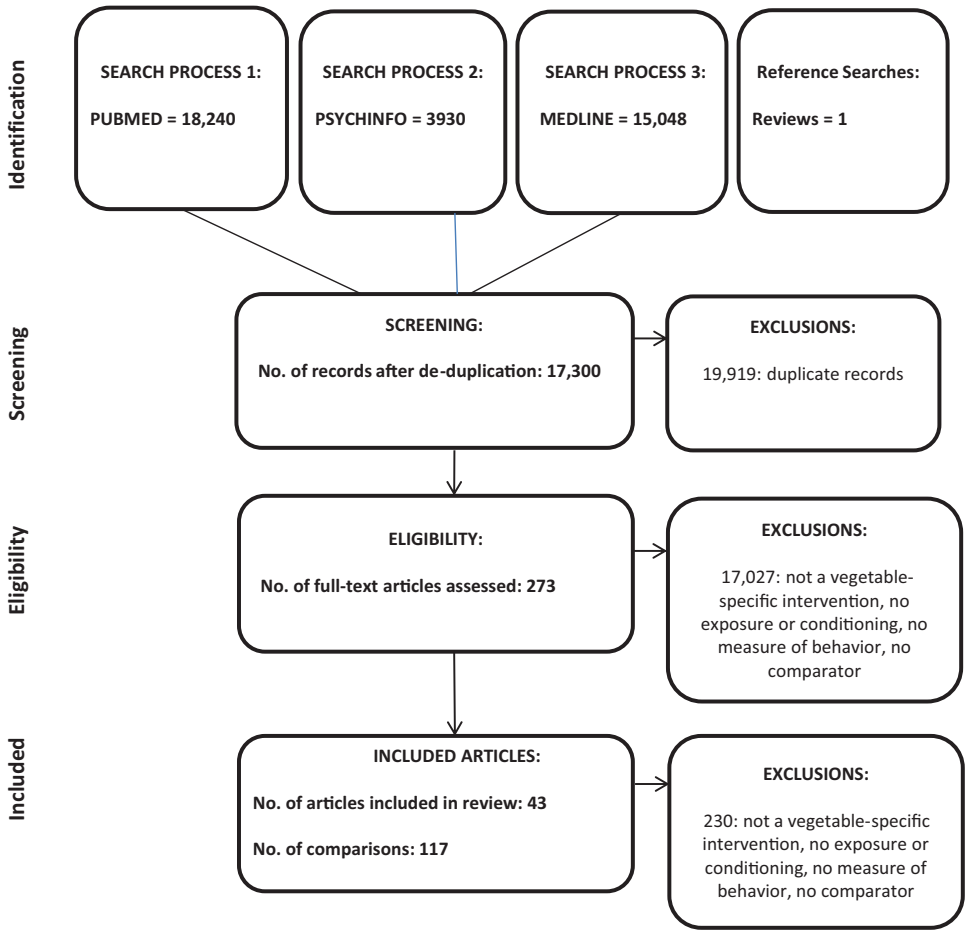

FIGURE 1 PRISMA diagram. PRISMA, Preferred Reporting Items for Systematic Reviews and Meta-Analyses.

and 1.1b, respectively. Extracted data from the within-subjects comparisons for liking and intake are provided in Supplemental Tables 1.2a and 1.2b, respectively. Supplemental Table 1.3 provides the results from all analyses.

Using the data available, in the between-subjects comparisons, repeated exposure resulted in higher liking postintervention compared with no exposure: SMD = 0.35; 95% CI: 0.17, 0.53, $P < 0.01$, 17 comparisons, 820 participants, $I^2 = 25\%$. Effect sizes equate to an increase in liking of ~0.6 points on a 9-point scale. Similar effects were found with the use of data from the longest follow-up (~1.5 mo postintervention) (SMD = 0.30; 95% CI: 0.14, 0.45, $P < 0.01$) and with the use of fixed-effect models (SMD = 0.35; 95% CI: 0.21, 0.49, $P < 0.01$). Only 5 comparisons from 4 studies were without a judgment of high ROB (ROB1–3) (48, 52, 56, 59). The Forest plot for the analysis of all between-subjects comparisons investigating vegetable liking postintervention after repeated exposure compared with no exposure using a random-effects model is given in **Figure 2**.

For vegetable intake, repeated exposure resulted in higher intake postintervention compared with no exposure: SMD = 0.23; 95% CI: 0.07, 0.39, $P < 0.01$, 21 comparisons, 1130 participants, $I^2 = 29\%$. Effect sizes equate to an increase in intake of ~10 g vegetables. Again, similar effects were found with the use of data from the longest follow-up (~1.5 mo postintervention) (SMD = 0.19; 95% CI: 0.04, 0.35, $P = 0.02$) and with the use of fixed-effect models (SMD = 0.18; 95% CI: 0.06, 0.30, $P < 0.01$). Six comparisons from 5 studies were found without a judgment of high ROB (ROB1–3) (48, 52, 56, 59, 62). The Forest plot for the analysis of all between-subjects studies investigating vegetable intake postintervention after repeated exposure compared with no exposure using a random-effects model is given in **Figure 3**.

In the within-subjects comparisons, using the data available, no differences were found in vegetable liking or intake after repeated exposure compared with no exposure, either postintervention or at longest follow-up (~2.5 mo postintervention) (largest SMD (vegetable intake) = 0.13; 95% CI: −0.07, 0.33, $P = 0.19$, 11 comparisons, 265 participants, $I^2 = 19\%$). Limited consistent effects were also found with the use of fixed-effect models. Only 2 comparisons were without a judgment of high ROB (ROB1–3) (59, 60).

Our funnel plots demonstrate limited evidence for publication bias.

Research question 2: What is the impact of conditioning to the taste of a vegetable for increasing liking and consumption of that vegetable, compared with repeated taste exposure?

Included studies

Between-subjects comparisons. We found 35 between-subjects comparisons involving 2132 participants investigating liking for the target vegetable after conditioning compared with repeated

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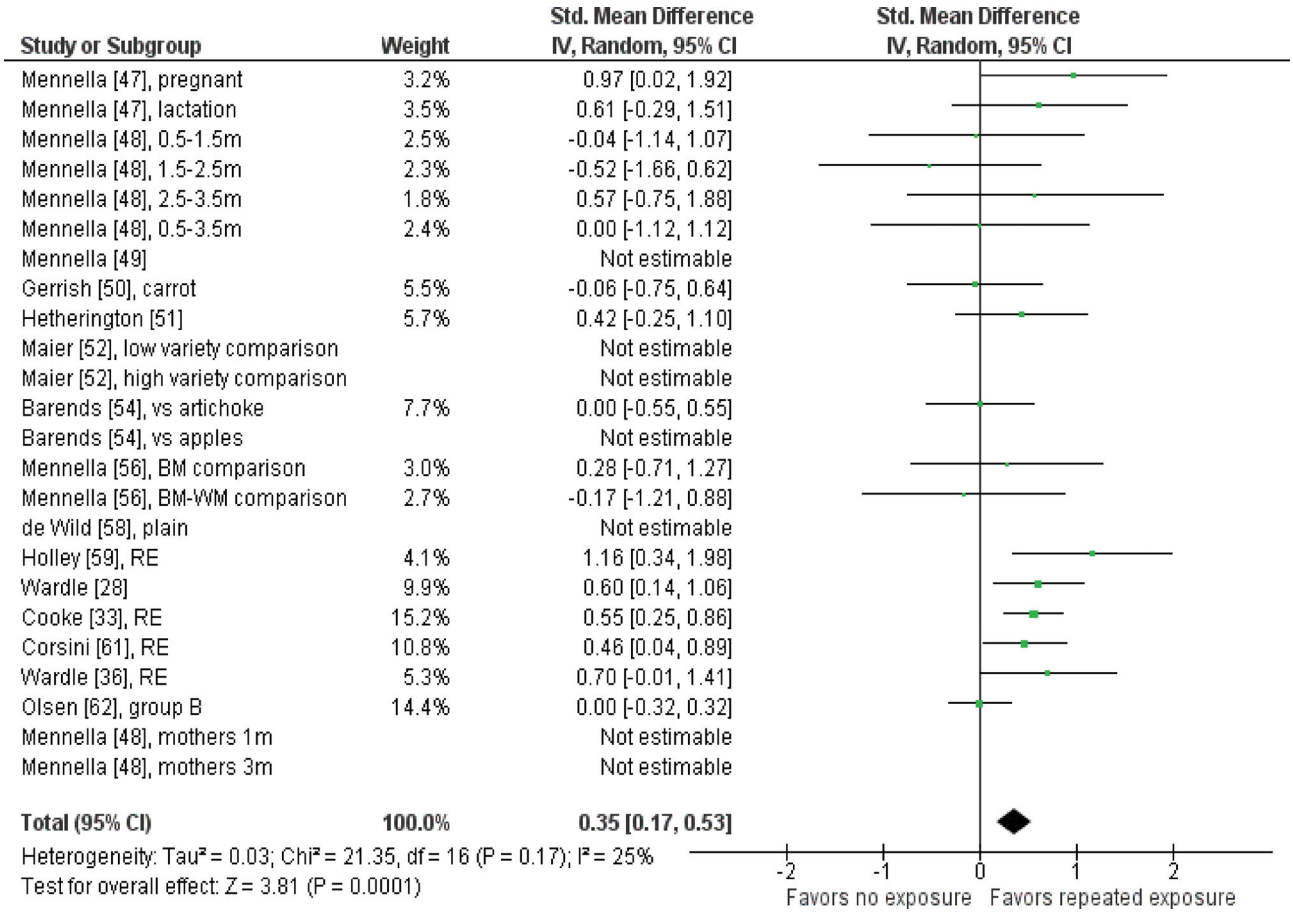

FIGURE 2 Forest plot for the analysis of all between-subjects comparisons investigating vegetable liking postintervention after repeated exposure compared with no exposure using a random-effects model (SMD = 0.35; 95% CI: 0.17, 0.53, $P < 0.01$, 17 comparisons, 820 participants, $I^2 = 25\%$). Studies ordered by age of participants. BM comparison, between meal comparison; BM-WM comparison, between meal, within meal comparison; IV, inverse variance; m, months; RE, repeated exposure; SMD, standardized mean difference; Std., standard.

exposure (33, 35–37, 39, 56–59 63, 64, 68–74), and 40 between-subjects comparisons involving 2497 participants investigating vegetable intake (33, 35–39, 56–58, 62, 63–64, 66–74). Nine comparisons of small/medium-sized groups (539 participants) investigated FFC, all in infants aged <36 mo, with the exception of 2 comparisons in children aged 10 y (59). All comparisons in young infants used comparable procedures in conditioning and repeated exposure groups for an unfamiliar target vegetable (ROB1), and added salt, sucrose, or a spice for the conditioning. The 2 comparisons in older children used different procedures in conditioning and repeated exposure groups for a neutrally liked target vegetable, paired with an alternative liked or disliked vegetable. All studies included follow-up measurements of 1 wk–6 mo postintervention. Three comparisons of small-sized groups (139 participants) investigated FNC, all in infants aged ≤36 mo. All comparisons used comparable procedures in conditioning and repeated exposure groups for an unfamiliar vegetable (ROB1), and 2 comparisons specified the addition of oil for the conditioning. All comparisons included follow-up measurements of 5 wk–6 mo postintervention. Thirteen comparisons of small/medium-sized groups (411 participants) investigated FFNC, all in infants aged ≤5 y, with the exception of 1 comparison in children aged 12 y (38). All studies, excepting 2 where test procedures differed for the differing groups (70, 71), used comparable procedures in conditioning and repeated exposure groups (ROB1) for various target vegetables, and added dips, sauces, sweetened and unsweetened cream cheese, and fruit for the conditioning. Only 1 study included a follow-up measurement, of 1 mo. Seven comparisons of small- to large-sized groups (1003 participants) investigated the use of rewards for consuming a not-liked vegetable, all in children aged 3–6 y. Four comparisons compared the use of rewards with repeated exposure (33, 57, 58), whereas 3 comparisons compared rewards with no exposure (although exposure was not explicitly avoided) (72, 73). Reward and control procedures were judged to be comparable (ROB1) in only 1 comparison (33). Five comparisons included a 3-mo follow-up postintervention (33, 58, 72). Three comparisons of small-sized groups (155 participants) investigated the use of modelling, in infants aged 2–11 y, for consuming neutral/medium-liked (74) and not-liked vegetables (57, 63). Two comparisons compared modelling with repeated exposure (57, 63) and procedures were judged as comparable (ROB1), and 1 comparison compared modelling with reduced exposure (74). One comparison included follow-up measurements, at 9 mo (74). Five comparisons of small/medium-sized groups (250 participants) investigated the use of rewards and modelling, in

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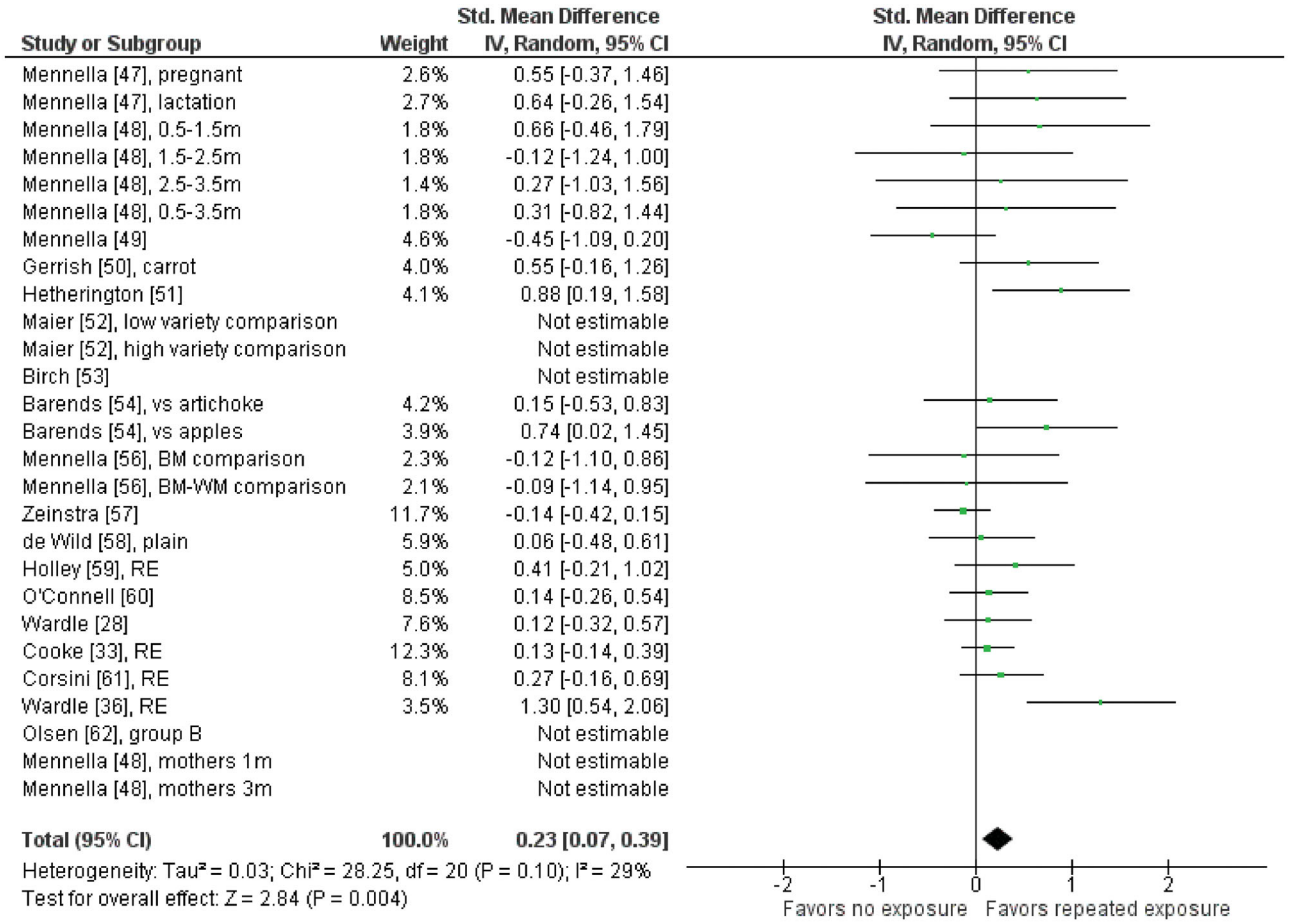

FIGURE 3 Forest plot for the analysis of all between-subjects studies investigating vegetable intake postintervention after repeated exposure compared with no exposure using a random-effects model (SMD = 0.23; 95% CI: 0.07, 0.39, $P < 0.01$, 21 comparisons, 1130 participants, $I^2 = 29\%$). Studies ordered by age of participants. BM comparison, between meal comparison; BM-WM comparison, between meal, within meal comparison; IV, inverse variance; m, months; RE, repeated exposure; SMD, standardized mean difference; Std., standard.

infants aged 3–8 y, for consuming liked (35) and not-liked (36, 57) vegetables. Conditioning and repeated exposure procedures were considered comparable in 4 comparisons (ROB1) (35, 36). One study included follow-up measurements of 7 mo postintervention. Details of all comparisons that used a between-subjects design to compare repeated exposure with no exposure to a target vegetable are given in Supplemental Table 2.1.

Within-subjects comparisons. We found 9 within-subjects comparisons (170 participants) investigating vegetable liking (69, 71, 75–79) and 7 within-subjects comparisons (154 participants) investigating intake of the target vegetable (65, 69, 71, 75, 77, 78). Six comparisons investigated FFC (65, 69, 75, 76), 2 comparisons investigated FNC (77, 78), and 2 comparisons investigated FFNC (71, 79). Participant samples ranged from 7 to 64 participants, aged 15 mo–23 y. All comparisons used a neutral or not-liked vegetable as the target, and an alternative vegetable as the nontarget vegetable, that was considered comparable in 5 (low ROB (ROB1)) comparisons (69, 71, 77, 79). Four comparisons included a 1–6 mo follow-up (65, 71, 75, 78). Details of all comparisons that used a within-subjects design to compare conditioning with repeated with no exposure to a target vegetable are given in Supplemental Table 2.2.

Study combination

Extracted data from the between-subjects comparisons for liking and intake are provided in Supplemental Tables 2.1a and 2.1b, respectively. Extracted data from the within-subjects comparisons for liking and intake are provided in Supplemental Tables 2.2a and 2.2b, respectively. Supplemental Tables 2.3 and 2.4 provide the results from all analyses.

Using the available data, in the between-subjects comparisons, conditioning resulted in higher liking postintervention compared with repeated exposure: SMD = 0.28; 95% CI: 0.03, 0.53, $P = 0.03$, 28 comparisons, 1861 participants, $I^2 = 81\%$. Benefits of conditioning approximate 0.5 points on a 9-point scale. Similar effects were found with the use of data from the longest follow-up (~1.25 mo postintervention) (SMD = 0.34; 95% CI: 0.11, 0.57, $P < 0.01$, 28 comparisons, 1860 participants, $I^2 = 77\%$). Subgroup analyses revealed no differences between subgroups based on conditioning type (largest $\chi^2 = 7.42$, df = 5, $P = 0.19$). Using fixed-effect models, conditioning resulted in higher liking postintervention compared with repeated exposure at both time points (smallest SMD = 0.45; 95% CI: 0.35, 0.55, $P < 0.01$), and differences between subgroups based on conditioning type were found (smallest $\chi^2 = 28.91$, df = 5,

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$P < 0.01$). Beneficial effects were found through the use of rewards: smallest SMD = 0.70; 95% CI: 0.56, 0.84, $P < 0.01$, 7 comparisons, 940 participants, $I^2 = 89\%$; and modelling: SMD = 0.61; 95% CI: 0.22, 0.99, $P < 0.01$, 3 comparisons, 145 participants, $I^2 = 92\%$. Four comparisons from 2 studies without a judgment of high ROB were found (ROB1–3)—2 comparisons that used FFC (68) and 2 comparisons that used FFNC (56). The Forest plot for the analysis of all studies investigating vegetable liking postintervention after conditioning compared with repeated exposure using a random-effects model is given in **Figure 4**.

For vegetable intake, no differences between conditioning and repeated exposure were found postintervention: SMD = 0.12; 95% CI: −0.08, 0.31, $P = 0.23$, 38 comparisons, 2468 participants, $I^2 = 78\%$; but effects were found at longest follow-up (~1.5 mo postintervention): SMD = 0.19; 95% CI: 0.01, 0.37, $P = 0.04$, 38 comparisons, 2402 participants, $I^2 = 74\%$, and subgroup analyses revealed differences between subgroups based on conditioning type (smallest $\chi^2 = 17.13$, df = 5, $P < 0.01$). Investigation of the analyses within subgroups revealed higher vegetable intakes postintervention and at longest follow-up after the use of rewards: smallest SMD = 0.32; 95% CI: 0.15, 0.50, $P < 0.01$, 7 comparisons, 1003 participants, $I^2 = 34\%$; and lower vegetable intakes after the use of FNC: smallest SMD = −0.45; 95% CI: −0.87, −0.03, $P = 0.04$, 3 comparisons, 101 participants, $I^2 = 0\%$. Effect sizes equate to an increase in intake of ~9 g vegetables after rewards and a decrease in intake of ~30 g vegetables after FNC. Analyses that used fixed-effect models found similar effects, and in addition found increased vegetable intakes after the use of modelling: smallest SMD = 0.56; 95% CI: 0.19, 0.94, $P < 0.01$, 3 comparisons, 155 participants, $I^2 = 94\%$; and modelling and rewards: smallest SMD = 0.76; 95% CI: 0.49, 1.03, $P < 0.01$, 5 comparisons, 250 participants, $I^2 = 83\%$. Five comparisons from 3 studies without a judgment of high ROB (ROB1–3) were found—2 comparisons that used FFC (68), 2 comparisons that used FFNC (56), and 1 comparison that used social rewards (33). The Forest plot for the analysis of all studies investigating vegetable intake postintervention after conditioning compared with repeated exposure using a random-effects model is given in **Figure 5**.

In within-subjects comparisons, using the data available, no differences were found between conditioning and repeated exposure in liking or intakes either postintervention or at longest follow-up (~1.4 mo postintervention) (vegetable liking: largest SMD = 0.28; 95% CI: −0.01, 0.57, $P = 0.06$, 9 comparisons, 170 participants, $I^2 = 35\%$; vegetable intakes: largest SMD = −0.12; 95% CI: −0.31, 0.08, $P = 0.24$, 4 comparisons, 72 participants, $I^2 = 0\%$). Subgroup analyses also revealed no differences between subgroups based on conditioning type (largest $\chi^2 = 1.31$, df = 2, $P = 0.52$). A significant benefit of conditioning for liking was found for fixed-effect models (smallest SMD = 0.20; 95% CI: 0.06, 0.34, $P < 0.01$), but no differences were found based on conditioning type. Only 2 comparisons from 1 study in the analyses on liking (76) and 0 studies in the analyses on intakes were found without a judgment of high ROB (ROB1–3).

Funnel plots demonstrated limited evidence for publication bias.

Research question 3: What is the impact of repeated taste exposure or conditioning to a vegetable for increasing liking and consumption of a novel vegetable, compared with no exposure or repeated taste exposure?

Included studies

Between-subjects comparisons. We found 18 between-subjects comparisons involving 607 participants investigating liking for a novel vegetable after repeated exposure or conditioning to other vegetables (48, 50, 52, 59, 80, 82) and 25 between-subjects comparisons involving 633 participants investigating vegetable intake (48, 50, 52, 60, 70, 80–82). One comparison compared the effects of exposure to 1 other vegetable with the effects of no exposure (60), 8 comparisons compared the effects of exposure to several other vegetables with those of no exposure (48, 50, 80), and 12 comparisons compared the effects of repeated exposure to several vegetables with the effects of repeated exposure to 1 other vegetable (52, 59, 81, 82). Six comparisons also compared the impacts of FFNC to other vegetables with repeated exposure to other vegetables (70). Studies were small/medium in size (14–74 participants per group), and with the exception of the studies by Fisher et al. (70) in 4-y-old children and Olsen et al. (59) in 10-y-old children, all studies were conducted on infants aged ≤6 mo. A range of vegetables were used as both the novel vegetable and the previously experienced vegetables. Intervention and control procedures were considered comparable (ROB1) in all but 4 comparisons (50, 80). Two comparisons included a follow-up of 5 wk (59), and 2 comparisons included a follow-up of 6 y (81). Further details of all comparisons that used repeated exposure/conditioning to a vegetable for increasing liking and consumption of a novel vegetable compared with no/repeated exposure are given in Supplemental Table 3.1.

Study combination

Extracted liking and intake data are provided in Supplemental Tables 3.1a and 3.1b, respectively. Supplemental Table 3.2 provides the results from all analyses.

Using the data available, repeated exposure/conditioning resulted in higher liking for the novel vegetable postintervention compared with no/repeated exposure: SMD = 0.38; 95% CI: 0.10, 0.65, $P = 0.01$, 18 comparisons, 607 participants, $I^2 = 53\%$. Effect sizes equate to an increase in liking of ~0.6 points on a 9-point scale. Subgroup analyses revealed no differences between subgroups based on strategy type ($\chi^2 = 3.47$, df = 1, $P = 0.06$). No effects were found at longest follow-up (0–6 y postintervention): SMD = 0.15; 95% CI: −0.21, 0.50, $P = 0.41$, 12 comparisons, 535 participants, $I^2 = 71\%$. Similar effects were found for fixed-effect models. Ten comparisons from 2 studies were found without a judgment of high ROB (ROB1–3) (52, 59). The Forest plot for the analysis of all studies investigating vegetable liking postintervention after repeated exposure/conditioning to other vegetables compared with no/repeated exposure to other vegetables using a random-effects model is given in **Figure 6**.

For vegetable intake, repeated exposure/conditioning resulted in higher intakes of the novel vegetable compared with no/repeated exposure postintervention: SMD = 0.32; 95% CI: 0.10, 0.53, $P < 0.01$, 24 comparisons, 594 participants, $I^2 = 29\%$. Effect sizes equate to an increase in intake

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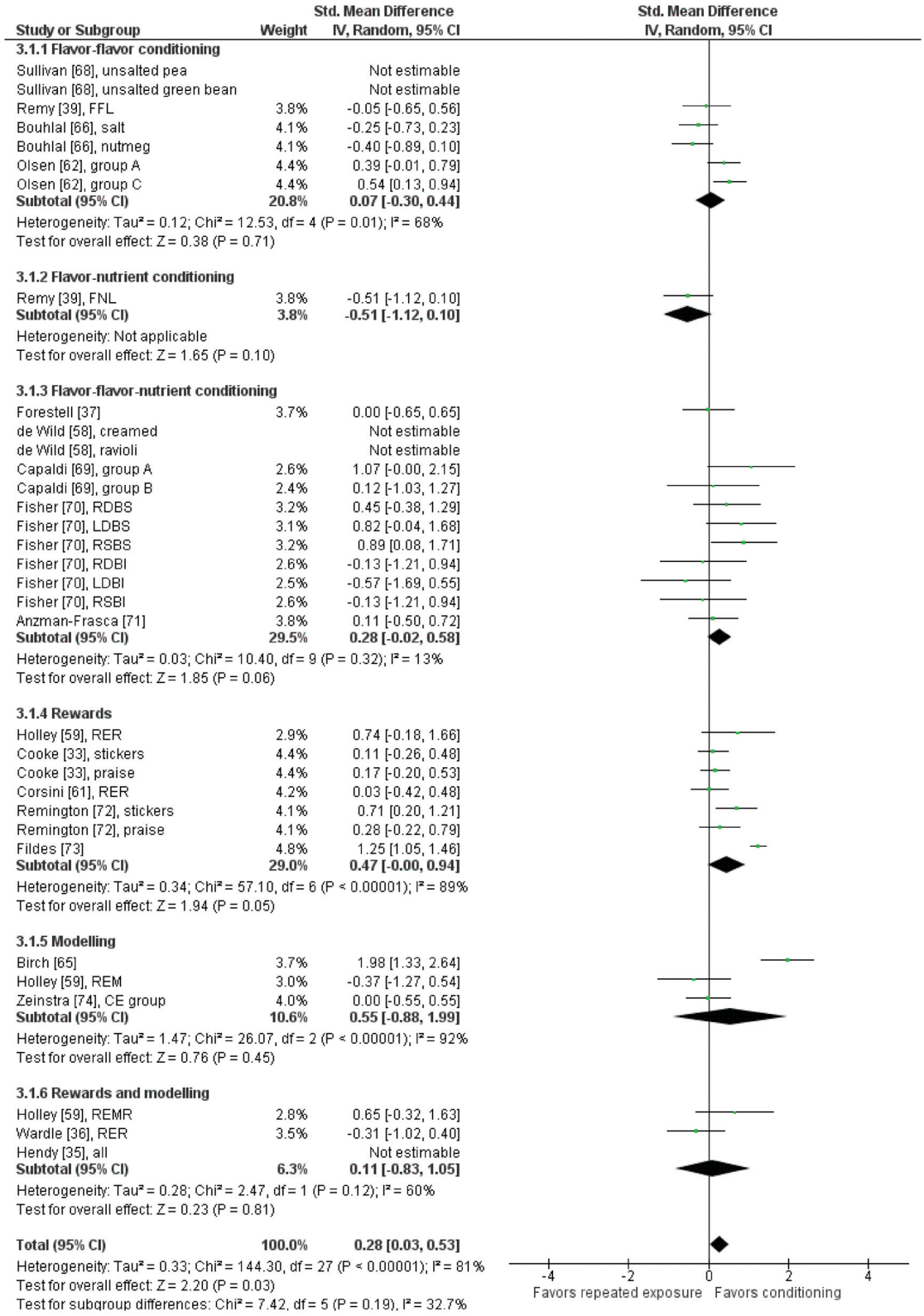

FIGURE 4 Forest plot for the analysis of all studies investigating vegetable liking postintervention after conditioning compared with repeated exposure using a random-effects model (SMD = 0.28; 95% CI: 0.03, 0.53, $P = 0.03$, 28 comparisons, 1861 participants, $I^2 = 81\%$). Studies ordered by age of participants. CE group, convivial eating group; FFL, flavor-flavor conditioning group; FNL, flavor-nutrient conditioning group; IV, inverse variance; LDBI, light dip, bitter insensitive group; LDBS, light dip, bitter sensitive group; RDBI, regular dip, bitter insensitive group; RDBS, regular dip, bitter sensitive group; REM, modelling group; REMR, rewards and modelling group; RER, rewards group; RSBI, regular sauce, bitter insensitive group; RSBS, regular sauce, bitter sensitive group; SMD, standardized mean difference; Std., standard.

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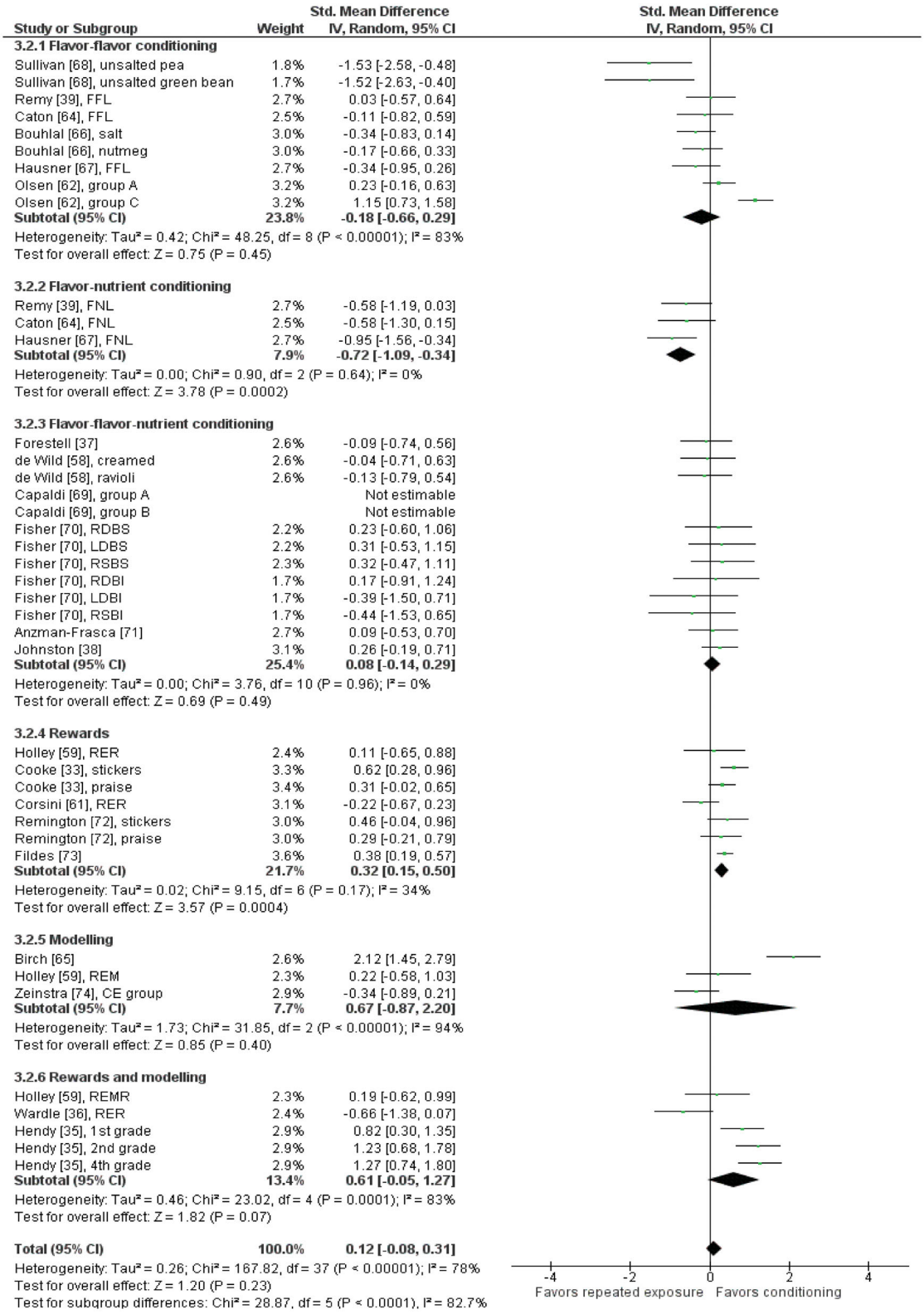

FIGURE 5 Forest plot for the analysis of all studies investigating vegetable intake postintervention after conditioning compared with repeated exposure using a random-effects model (SMD = 0.12; 95% CI: −0.08, 0.31, $P = 0.23$, 38 comparisons, 2468 participants, $I^2 = 78\%$). Studies ordered by age of participants. CE group, convivial eating group; FFL, flavor-flavor conditioning group; FNL, flavor-nutrient conditioning group; IV, inverse variance; LDBI, light dip, bitter insensitive group; LDBS, light dip, bitter sensitive group; RDBI, regular dip, bitter insensitive group; RDBS, regular dip, bitter sensitive group; REM, modelling group; REMR, rewards and modelling group; RER, rewards group; RSBI, regular sauce, bitter insensitive group; RSBS, regular sauce, bitter sensitive group; SMD, standardized mean difference; Std., standard.

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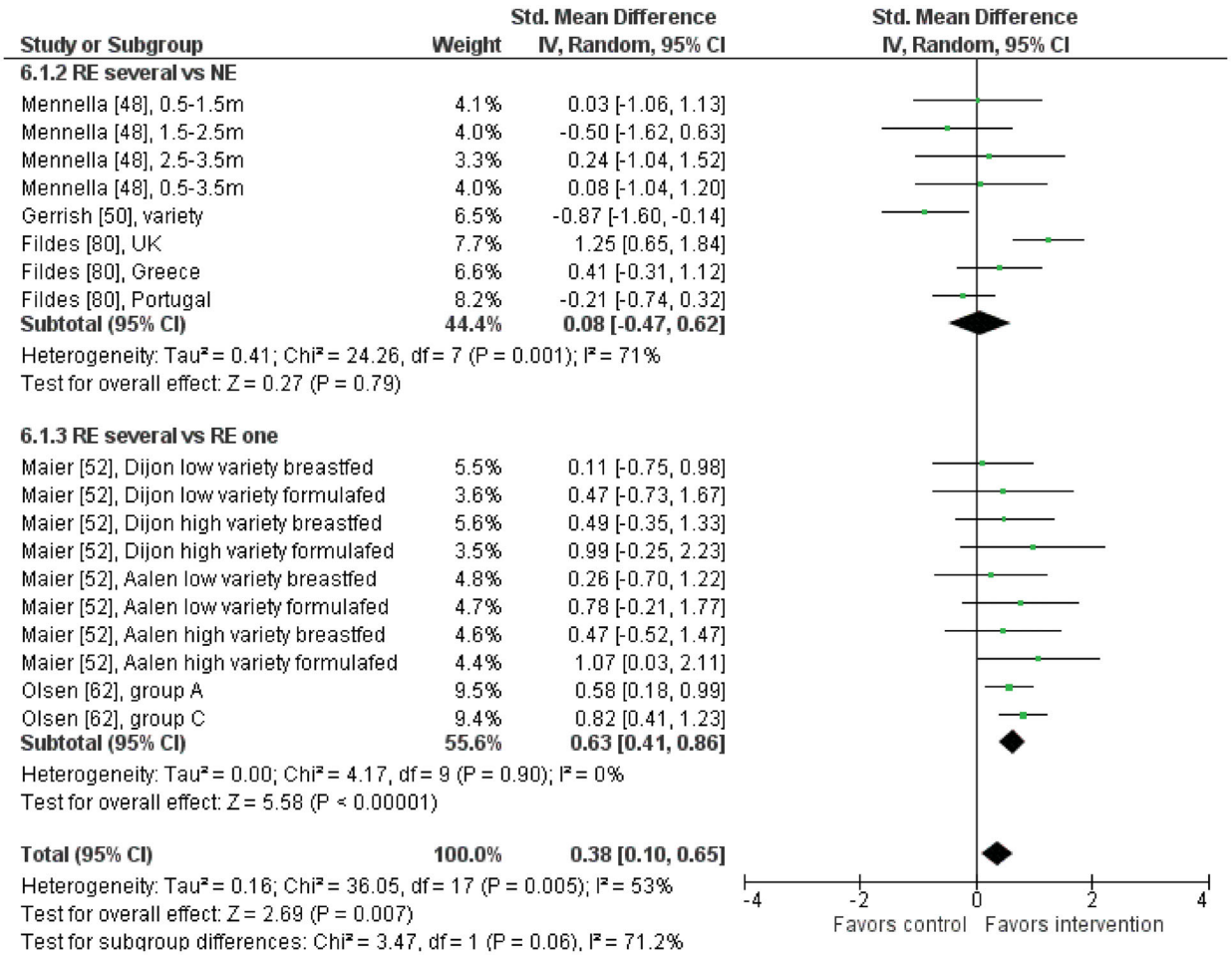

FIGURE 6 Forest plot for the analysis of all studies investigating vegetable liking postintervention after repeated exposure/conditioning to other vegetables compared with no exposure or repeated exposure to other vegetables using a random-effects model (SMD = 0.38; 95% CI: 0.10, 0.65, $P = 0.01$, 18 comparisons, 607 participants, $I^2 = 53\%$). Studies ordered by age of participants. IV, inverse variance; m, months; NE, no exposure; RE one, repeated exposure to 1 other vegetable; RE several, repeated exposure to several other vegetables; SMD, standardized mean difference; Std., standard.

of ~12 g vegetables. No differences were found between subgroups based on strategy type ($\chi^2 = 5.58$, df = 2, $P = 0.06$). No effects were found at longest follow-up (0–6 y postintervention) (SMD = 0.29; 95% CI: −0.04, 0.62, $P = 0.08$, 18 comparisons, 522 participants, $I^2 = 66\%$). Similar effects were found in analyses that used fixed-effect models (smallest SMD = 0.33; 95% CI: 0.15, 0.51, $P < 0.01$, 24 comparisons, 594 participants, $I^2 = 29\%$), and differences between subgroups (smallest $\chi^2 = 7.36$, df = 2, $P = 0.03$) also revealed greater benefits from repeated exposure to several other vegetables compared with no exposure or repeated exposure to 1 other vegetable (smallest SMD = 0.41; 95% CI: 0.14, 0.68, $P < 0.01$, 8 comparisons, 240 participants, $I^2 = 15\%$). Nine comparisons from 2 studies were found without judgments of high ROB (ROB1–3) (52, 60). The Forest plot for the analysis of all studies investigating vegetable intake postintervention after repeated exposure/conditioning to other vegetables compared with no exposure/repeated exposure to other vegetables using a random-effects model is given in **Figure 7**.

Our funnel plots demonstrate some asymmetry, suggesting possible evidence for publication bias.

DISCUSSION

Many comparisons from several studies were available to directly address all 3 research questions, but many studies are small and were considered to include an element of high ROB.

For our first research question, 27 (1536 participants) between-subjects comparisons and 13 (343 participants) within-subjects comparisons investigated liking and/or intake for an exposed vegetable after repeated exposure compared with no exposure. Our main analyses revealed a small, but reliable, benefit of repeated exposure for both liking and intake of the exposed vegetable, in between-subjects comparisons, that was maintained over follow-up periods. Heterogeneity between comparisons was low, CIs were relatively narrow, similar effects were found between random- and fixed-effect models, and limited evidence of publication bias was found, all adding weight to the effects found. This combined effect confirms those previously advocated from other narrative reviews in this area (21–23).

For our second research question, 40 (2497 participants) between-subjects comparisons and 10 (199 participants) within-subjects comparisons investigated vegetable liking and/or intake after conditioning compared with repeated exposure. Our main

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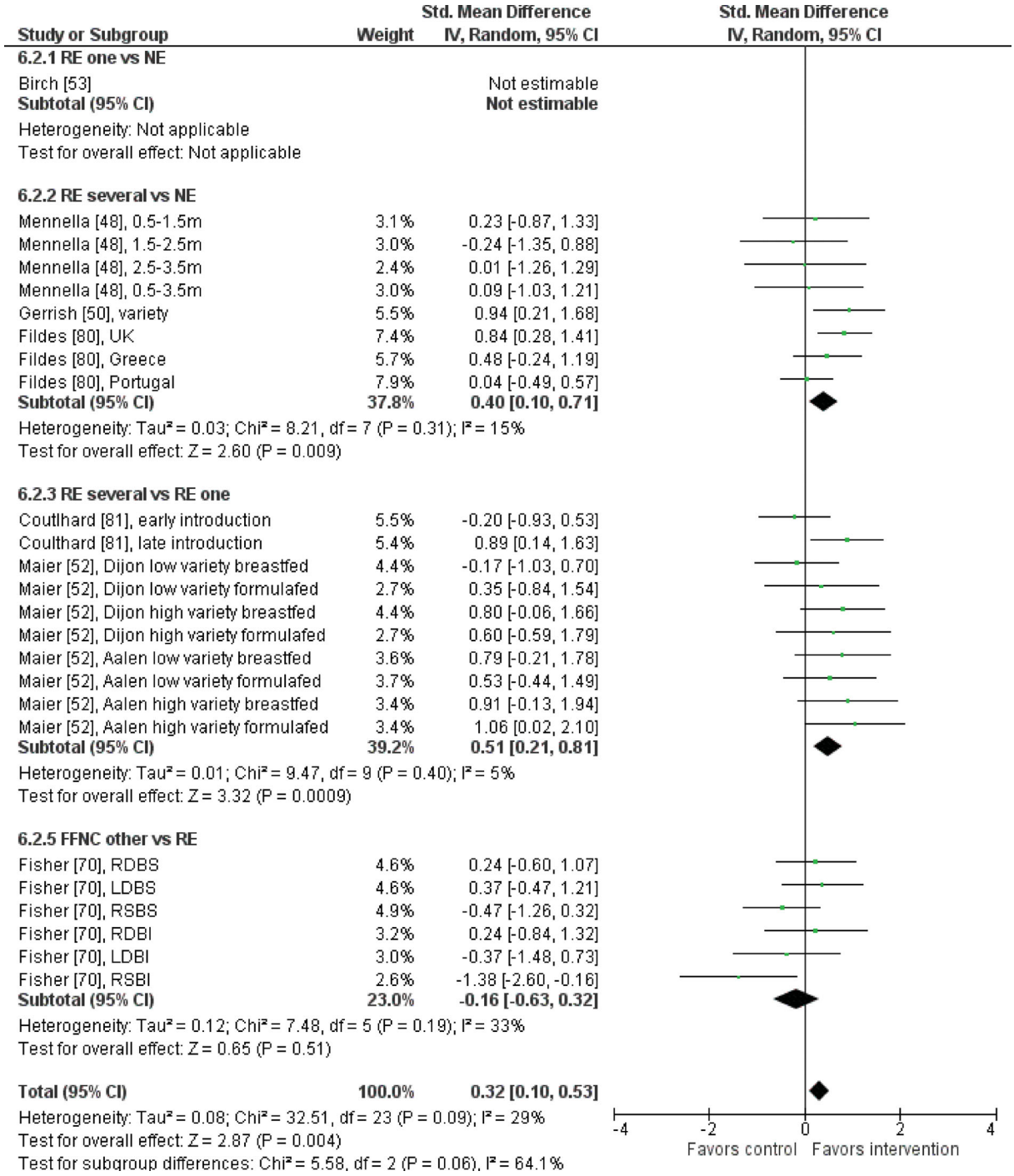

FIGURE 7 Forest plot for the analysis of all studies investigating vegetable intake postintervention after repeated exposure/conditioning to other vegetables compared with no exposure or repeated exposure to other vegetables using a random-effects model (SMD = 0.32; 95% CI: 0.10, 0.53, $P < 0.01$, 24 comparisons, 594 participants, $I^2 = 29\%$). Studies ordered by age of participants. FFNC other, flavor-flavor-nutrient conditioning to other vegetables; IV, inverse variance; LDBI, light dip, bitter insensitive group; LDBS, light dip, bitter sensitive group; m, months; NE, no exposure; RDBI, regular dip, bitter insensitive group; RDBS, regular dip, bitter sensitive group; RE one, repeated exposure to 1 other vegetable; RE several, repeated exposure to several other vegetables; RSBI, regular sauce, bitter insensitive group; RSBS, regular sauce, bitter sensitive group; SMD, standardized mean difference; Std., standard.

analyses revealed a small benefit of conditioning for liking both postintervention and at longest follow-up compared with repeated exposure, although no differences based on conditioning type were found. For vegetable intakes, no effects were found overall, but differences between conditioning types revealed higher vegetable intakes (small effect sizes) both postintervention and at longest follow-up after the use of rewards compared with repeated exposure, and lower vegetable intakes after FNC.

Limited evidence of publication bias was found, but heterogeneity between comparisons was considerable and CIs are relatively wide for most analyses. High heterogeneity and wide CIs suggest considerable differences between individual study

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findings, probably due to differences in study methodology. Differing effects have previously been suggested dependent on participant age (51, 53, 60, 65, 74), breastfeeding history and other individual differences (38, 50, 52, 66, 67, 74, 81), target vegetable (51, 53, 61, 69), and the number, frequency, and quality of exposures (37, 59, 64, 81). Differences based on other elements of exposure, e.g., visual and texture exposure, may also have impacts. Some suggestion of reduced effects as participants age may be made based on study order in our analyses, but more work is required before firm conclusions can be drawn. Sources of heterogeneity were not investigated, owing to low numbers of contributing studies, but further studies would allow greater confidence in effects and investigations of study differences.

Our findings that conditioning may be more beneficial for increasing vegetable liking than repeated exposure, and for vegetable intakes, that rewards may be beneficial and FNC may be detrimental, are novel. These conclusions have not previously been suggested. These findings provide some evidence for recommending conditioning to vegetables, particularly the use of rewards, and for recommending against pairing with nutrients. The use of rewards for increasing intake has been somewhat controversial, owing to concerns over "overjustification" (the use of a reward to justify consumption) and/or a likely reduction in effects once rewards cease (33, 35, 36), but work suggests that intrinsic motivation may only deteriorate for already-liked foods as opposed to neutral or disliked foods (72), and that small and delayed rewards, for eating only small amounts of disliked foods, can overcome these concerns (33, 35, 36, 72, 83). Reductions in intake as a result of FNC demonstrate a negative impact of pairing with energy/nutrients, possibly as a result of "conditioned satiation" (84, 85). Concerns over conditioned satiation have previously been suggested, but are considered to have more of an impact on younger infants than on older individuals (21, 39), and importantly, the studies in our analyses were all conducted in very young infants. Cautions should also be added, because only 3 comparisons contributed to our analyses, and all used the same target vegetable and similar procedures for nutrient delivery (39, 66, 67). All analyses on conditioning in fact are also potentially confounded by failures to ensure that conditioning techniques are distinct and well defined. Some flavor-nutrient pairings may in fact have also involved flavor-flavor associations, etc., and rewards and modelling may be used naturally more than is reported. Narrative reviews can also argue against the use of conditioning strategies based on the effort involved in these procedures (21), but very little formal evidence of this effort is available.

For our third research question, 27 between-subjects comparisons (635 participants) investigated liking for and/or intake of a novel vegetable after repeated exposure/conditioning compared with no/repeated exposure. Our analyses revealed small increases in liking for and intake of a novel vegetable after repeated exposure/conditioning to several different vegetables compared with no/repeated exposure, but these effects reduced over time. High heterogeneity and wide CIs also weaken our findings, and some asymmetry in the funnel plot may suggest possible publication bias.

These findings suggest some transfer or generalization to novel vegetables as a result of exposure to other foods in the same food category (50, 53, 59, 82), and are potentially important considering recommendations to consume a wide variety of vegetables for health benefits (20). These generalization effects may also explain the lack of effects in comparisons that used within-subjects designs. In analyses for both of our first 2 research questions, limited effects were found in within-subjects comparisons. This absence of effects is most plausibly explained by a generalization in learning from a target vegetable to a (similar) control vegetable. The possibility of generalization demonstrates a clear disadvantage of this type of study design, and notably the 1 "failed" conditioning study reported (77) uses a within-subjects design.

For all 3 research questions, high ROB was found in many studies in all 3 elements assessed, limiting the conclusions that can be drawn. High levels of dropout or poor compliance may testify to the difficulties in running these studies (67, 77), but should also add cautions to the findings presented. Our findings, and the findings of many studies, if based on selected or self-selected samples may be highly biased. Differences between analyses that used fixed-effect and random-effects models also suggest differing effects dependent on study size that again may limit generalizability.

Postintervention follow-up was also often limited, and effects typically reduced where measured. Reductions in effect sizes may be expected once an intervention ceases, but other feeding and eating practices may also have an impact on any achieved effects (22). Others also suggest possible benefits in the longer term, whereas reactions immediately after extensive tasting may be negative (59, 64, 74, 77). The high ROB in many studies and short follow-up periods suggest a need for large, well-conducted, between-subjects studies, with long-term follow-up.

Our review is limited by the limited number and nature of the studies and data available. Particularly, poor compliance in many studies, confounds in study methodology, and incomplete reporting have limited the data available to address our research questions. Our analyses furthermore are limited by the estimations and calculations required to convert poorly reported data to the inputs required for meta-analysis. Our search strategies were limited to specific databases and the articles we included in the review were limited to those published, and published in English. The limited evidence for publication bias, however, suggests that these limitations are not likely to have resulted in systematic bias for at least our first 2 research questions.

In conclusion, our review found 43 articles detailing 117 comparisons investigating the use of repeated exposure and conditioning strategies for increasing vegetable liking and intakes. Our analyses confirm benefits from repeated exposure to a target vegetable for increasing liking and intake of that one vegetable. Our analyses also demonstrate benefits from conditioning strategies over those of repeated exposure, a particular value for rewards, and benefits from repeated exposure to a variety of vegetables for increasing liking and intake of a novel vegetable. Based on our findings, recommendations to increase vegetable intakes should focus on the use of repeated exposure to single vegetables and a variety of vegetables, and on the use of rewards. However, effect sizes are small, differences between studies are large, and our analyses are limited through limitations in study design, compliance, and/or reporting. Further large, well-conducted, between-subjects studies that use realistic scenarios and with long-term follow-up assessments are required.

The authors' responsibilities were as follows—KMA: conceived the project, designed the research, undertook all analyses, wrote the manuscript,

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and has primary responsibility for the final content; KMA, AH, and HH: undertook the searches and screening; KMA and AH: undertook all data extraction; JR: checked all extracted data; and all authors: conducted the research, offered critical comments, and read and approved the final manuscript. None of the authors reported a conflict of interest related to the study.

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