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Within-person associations of psychological factors with healthy or unhealthy diet in EMA studies in general adult populations: A systematic review and meta-analysis

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Abstract

Background. (Un)healthy eating behaviours may vary by moment and psychological context. Ecological momentary assessment (EMA) is well-suited to study within-person, momentary, and context-dependent behaviours and correlates, as it uses repeated sampling of real-time experiences. Insights in such within-person relations could inform ecological momentary interventions. A systematic review and meta-analysis of these insights in a healthy population is, however, currently lacking. The aim of this systematic review and meta-analysis was to synthesise within-person associations between psychological factors and eating behaviour in healthy adults, and summarise methodological aspects of these studies.

Methods. Observational and experimental studies on psychological predictors of (un)healthy eating behaviours in adult healthy populations were included, based on a systematic search of Ovid MEDLINE, Embase, PsycINFO and Web of Science. Meta-analysis of ≥5 studies per predictor/outcome was performed in *metafor* R-package.

Results. During title and abstract screening, n=2046 articles were reviewed (n=119 from overarching review, n=1927 from updated search), n=208 were screened in full-text screening. Forty-nine (review) and fifteen studies (meta-analysis) were included. Most studies were observational, and used fixed time-based sampling, and most assessed food eaten since the last prompt/timeframe. Various measures reported on study adherence (between 79-90%). Only around half reported on reliability and/or validity of the predictors or outcomes, most often referring to an adaptation of non-EMA scales, with little information of their exact adaptation. Due to high diversity in conceptualization and reporting of effect sizes, not all could be meta-analyzed. Meta-analyses showed negative psychological triggers (NPT, e.g., negative affect, daily stress, and ruminations) predicted lower levels of healthy eating behaviour (β=-0.007, p=.028, k=8, n total=1,489), higher levels of unhealthy eating behaviour (β=0.054, p=.022, k=6, n total=1,578), but had no significant association with unhealthy behaviour event occurrence (logOR=.469, p=.095, k=5, n total=934). All meta-analyses were characterized by small effect sizes and important heterogeneity.

Conclusions. Associations at the within level of NPT with (un)healthy eating highlight their potential utility for inclusion in health promoting ecological momentary interventions. Nevertheless, the methodological and conceptual heterogeneity of the studies highlight a need for harmonized practices, including broader adoption of reporting guidelines, to facilitate cumulative knowledge in the field.

Keywords

Healthy eating; healthy diet; Ecological Momentary Assessment; Experience Sampling; Within-person; Affect; Systematic Review; Meta-analysis

Introduction

Adopting a healthy diet has a substantial health impact: it reduces the risk of mortality due to cardiovascular diseases (1, 2), certain types of cancer (1, 2), diabetes type II (1) and stroke (2). It moreover increases cognitive functioning (3), and improves mental health by reducing the risk of depression (4) and enhancing general psychological well-being (5). As such, a healthy diet is estimated to contribute to a decrease in risk of mortality by around one-fifth in adults (1) and older adults (6).

A healthy diet for adults relates to an energy balance between the amount of food that is consumed and the person's energy expenditure, and to the nature of the food that is consumed. General recommendations for a healthy diet for adults consist of a regular intake of grains, seeds, and nuts; a sufficient consumption of fruit and vegetables (FV, minimum 5 portions per day); whilst limiting the intake of sugars (maximum 10% of daily food intake), fats (maximum 30% of intake), and salt (maximum 5g per day) (7). Conversely, an unhealthy diet consists of a diet high in sugars (e.g., added sugar that can be found in processed foods such as snack bars, commercial-baked goods, fast food), saturated and trans fats (e.g., red meat, whole-fat dairy, fast food), low fibre foods (e.g., refined wheat bread, white rice), and sugar-sweetened beverages (e.g., soft drinks) (8). Improving the adoption of a healthy diet is a public health priority, with a need for effective interventions to support people to make healthy dietary choices (1).

Although some people tend to eat healthier than others, reflecting between-person differences, there are also moments or contexts in a day when people are more or less attracted to unhealthy food, which reflects within-person variation (9). Ecological momentary assessment (EMA) is well-suited to study these within-person, momentary and context-dependent behaviours and their correlates, as it uses repeated sampling of individuals' behaviours and experiences in real-time, in the natural environment (10). Prior EMA research suggests that eating behaviour is influenced by momentary or context-specific fluctuations in psychological correlates, including food cravings (11), dietary restraint, disinhibition, and susceptibility to hunger (12), and affect such as momentary stress (11). Psychological correlates are defined as behavioural cognitions (e.g., beliefs, attitudes, goals), emotions (e.g., negative affect, cravings) and processes operating on these (e.g., self-regulation, learning) (13).

Our review focuses on diet from a health promotion and disease prevention perspective. Several systematic reviews have summarised the use of EMA to study adult eating behaviours in healthy populations, focusing on a narrative synthesis of correlates of adult eating behaviour (14), and on EMA measurement methods in relation to dietary intake (11,15). However, we currently lack a comprehensive review and meta-analysis of EMA studies that examine associations of psychological variables and eating behaviours at the within-person level, that describe how these eating behaviours fluctuate within a person depending on their momentary psychological state. Such a review would provide a useful starting point for researchers and intervention designers interested in the development of just-in-time adaptive interventions (JITAI) (16) which aim to provide the right type of support to individuals at the right moment in time. As a recent scoping review indicated that few JITAIs or ecological momentary interventions for eating behaviour currently exist, with the large majority focusing on disordered eating or patient populations, that due to comorbidities and specific etiologies may not be comparable to a healthy population (17), this review is timely and relevant (18).

In addition to providing the opportunity to study within-person fluctuations, EMA is considered to have a higher ecological validity, i.e., the extent to which measurements reflect daily life, than retrospective assessments because of recording phenomena in their natural context and during their actual occurrence with lower recall bias (19). However, for this method to be valid, some design aspects need to be considered. For the measurement to accurately reflect the fluctuations in the phenomenon, it needs to be sampled at the frequency in which it naturally occurs, whilst also keeping (systematic) dropout and non-response low (20). The item(s) used for measurement need to have good validity, which is, however, inconsistently reported (21,22). Measurement reactivity may occur, meaning that the measurement itself changes the behaviour and its natural context (23). Moreover, analytical methods need to avoid grouping together measurements or individuals that have distinct patterns of daily life (23). EMA research is hence still facing several methodological challenges (19) and any review of EMA findings should consider their methodology alongside the study results.

The aim of this systematic review and meta-analysis was to synthesise within-person associations between psychological factors and eating behaviour at the event level in healthy adults, and summarise several methodological aspects of these studies that may impact interpretations of results, such as participant adherence and validity and reliability of EMA items. The specific study aims were:

1. To synthesise within-person associations between psychological factors and healthy or unhealthy eating behaviour in healthy (i.e., non-clinical), adults;
2. To describe the measures that were used to capture momentary eating behaviour in EMA studies;

3. To summarise the characteristics of the items and how measurement validity and reliability were assessed; and
4. To summarise the adherence and measurement reactivity of participants in EMA eating behaviour studies.

Methods

Study design

This systematic review and meta-analysis formed part of a larger, systematic review of EMA studies of five key public health behaviours (i.e., physical activity and sedentary behaviour, eating behaviour, alcohol consumption, tobacco smoking, and sexual health) (24,25). The Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) checklist and the American Psychological Association's Meta-Analysis Reporting Standards (APA-MARS) (26) were used in the design and reporting of this systematic review (27). The review protocol for the overarching review and for this review on eating behaviour studies were both pre-registered on the Open Science Framework (respectively, www.osf.io/cmnvw and https://osf.io/3peu5).

Inclusion criteria

This systematic review focuses on studies that targeted the recruitment of participants as healthy adults aged 18+ years. EMA studies reporting psychological correlates as a predictor (hence as of now referred to as 'predictor') of any healthy or unhealthy eating behaviour in non-clinical populations were included. No restrictions on geographical location or publication date were set.

Eating behaviour

Healthy (e.g., FV, whole-wheat grains, nuts) and unhealthy dietary intake (e.g., fast food, sugar-sweetened beverages, sweets, sugary or fatty snacks) were considered in line with WHO recommendations of including FV, legumes, grains, and nuts in a diet, preferring unsaturated above saturated fats, and limiting salt, sugar and saturated fat intake (7). We also included studies with dietary intake that was self-reported as healthy or unhealthy. Studies on dietary lapses (failure to keep up with the planned diet) were only included if they met the abovementioned criteria.

General adult population

Studies that targeted adults (18+) were included. In case adults were not mentioned explicitly as the target population (e.g. 'students'), age ranges and average ages were verified to ensure only studies targeting adult populations were included.

EMA measurement

Studies needed to include multiple (i.e., two or more) within-day, daily or weekly assessments of at least one psychological predictor and dietary intake, and to have reported within-person predictor-behaviour associations. EMA studies can differ in how prompts are scheduled. Fixed time-based (signal-contingent) sampling delivers prompts at pre-specified, predictable times (e.g., at 9 AM, 1 PM, 5 PM, and 9 PM each day). Random time-based sampling delivers prompts at unpredictable times within pre-specified windows (e.g., a prompt randomly triggered within each three-hour window across the day). Event-based sampling requires participants to self-initiate an entry when a specific event occurs (e.g., initiating a survey each time they have a craving, or each time they eat). To be included in the meta-analysis, within-person predictor-behaviour associations needed to be reported with dietary intake as outcome.

Psychological predictors

Psychological variables were defined as cognitions (e.g., beliefs, attitudes, goals), emotions (e.g., negative affect, cravings) and processes operating on these (e.g., self-regulation, learning), which are linked to behaviour (13). This means that environmental variables (e.g., social, physical context) are only included if they are part of the person's perception of this influence (e.g., social norm, perceived visibility of fast-food in the environment, but not the mere presence of others or objective indicators of the built environment). Some predictor-behaviour associations may involve same-time (rather than lagged) EMAs of psychological variables and diet. Although the term correlate may be better suited for these studies than predictor, for ease of reporting, we refer to both as ‘predictors’.

Types of studies and measurements

Both observational and experimental studies were included, as long as they met the abovementioned criteria (e.g., having measured predictors and outcomes in EMA, instead of assuming that the intervention reflects the psychological predictor). Predictors and outcomes could be self-reported, device-measured (e.g., pictures taken of food), or physiological measures (e.g., cortisol or heart rate variability).

Exclusion criteria

Studies where participants were recruited based on having a physical or mental health condition such as cancer, cardiovascular disease, depression, eating disorder or substance use disorder were excluded as per the larger systematic review (24). Studies on disordered eating such as eating disorders, binge eating, were excluded. Studies that only investigated caloric intake as outcome were excluded: despite the importance of the number of calories consumed for health, to assess whether this is part of a healthy diet we would also need to consider the energy expenditure that goes with the caloric intake. As on the one hand these are not always reported in dietary EMA studies, and on the other hand, energy expenditure is part of another focused review on physical activity and sedentary behaviour, these studies were excluded here.

Search methods for the identification of studies

Electronic searches

We searched Ovid MEDLINE, Embase, PsycINFO, and Web of Science (see the Supplementary Materials for the full search strategy). Terms were searched in titles and abstracts as free text terms or as index terms (e.g., Medical Subject Headings), as appropriate. We combined two groups of terms, the first group included terms relevant to EMAs and within-person study designs; the second group included terms relevant to diet (28). Electronic and hand searches were conducted in January 2020 and updated in

February 2021 for the overarching review. Following the protocol for the present review, two updates were performed only for the eating behaviour EMA studies in April 2023, and in May 2025. We restricted the search to human studies available in English that were published in peer-reviewed journals.

Searching for other sources

Reference lists of available systematic reviews of EMA studies were hand searched and expertise within the review team was used to identify additional articles of interest.

Data collection and analysis

Selection of studies

Identified records were merged using Covidence (29) and duplicate records were removed. For the initial search, two reviewers *(DKw, OP)* independently screened titles and abstracts (yes, maybe, no) against the inclusion criteria. Full texts were independently screened by two reviewers (*all*) from the larger review team (yes, no); discrepancies were resolved by three reviewers *(BA-A, AD, PG, DKw, DP*), and inclusion was further discussed with other team members, if needed. For the updated search, the same procedure was used, with two reviewers (*AD, PG*) independently screening titles and abstracts, two reviewers (*AD, PG*) independently screening full texts and discrepancies being resolved with a third reviewer *(DP, DKw, or BA-A)*. We did not calculate inter-rater reliability. In line with the PRISMA checklist, a primary reason for exclusion for each study was recorded at the full text screening stage. Exclusion criteria were hierarchically ordered and included: a full text could not be obtained; study protocol; study not published in English; conference abstract; duplicate; wrong study design (i.e., not an EMA study); participants recruited based on a physical or mental health condition; participants younger than 18 years; study did not focus on healthy or unhealthy diet as outcome; and study did not report a psychological predictor-diet association.

Data extraction and management

A data extraction form was developed in Microsoft Excel by five reviewers *(BA-A, AD, PG, DKw, DP)* in collaboration with the larger review team to extract information on study description (lead author, year, country, study type); participant characteristics (sample size, mean age and standard deviation, % female, educational attainment, ethnicity, motivation for dietary intake in healthy population (e.g., weight loss / weight management)); dietary-intake related characteristics (healthy diet, unhealthy diet, FV consumption, snacks, fast food or processed food); dietary intake measurement methods (dietary records, dietary recalls, video or photo registrations; barcode scan; food hashtags); EMA characteristics (device, sampling method, sampling frequency, study duration, reference time); adherence and measurement reactivity (participation retention, prompts answered (but possibly not completed to the end), prompts completed, disabling of prompt, latency time, cut-off for validity, backfilling, data imputation for missing data, level and measure of measurement reactivity), reliability and validity (level and method for reliability and validity of measure of eating behaviour and psychological predictor), psychological predictors (see earlier, adapted Theoretical Domains Framework, TDF) (24,30), pre-registration of study and analysis.

Data on study and participant characteristics were extracted by one reviewer from the full team (studies were distributed across several members), with 20% of studies double checked by a second reviewer from the full team for accuracy and completeness. Adherence, reactivity, reliability and validity info were coded by one reviewer *(AD)* and 20% of studies were double checked by a second reviewer *(PG).* Discrepancies were resolved through discussion.

Quality appraisal

As no fit-for-purpose quality appraisal tool was identified prior to conducting the larger EMA review, we amended an available checklist for the reporting of EMA studies (31) to include the following five criteria: rationale for using the EMA design ('Quality 1'); whether an a priori power analysis had been conducted ('Quality 2'); adherence to the EMAs ('Quality 3'); treatment of missingness ('Quality 4'); and whether data had been aggregated ('Quality 5'). We applied a standardised classification system based on the Effective Public Health Practice Project quality assessment tool (32) by rating the quality of each EMA study according to the abovementioned criteria as 'Weak', 'Moderate' or 'Strong' (28). For the studies included in the present review, the quality indicators were coded by one reviewer *(PG, BA-A)*, with 20% double checked for accuracy and completeness by a second reviewer *(DK, JK, FN, DP, VS, GtH).* Discrepancies were resolved through discussion and consensus. Information on the quality coding tool is available on OSF: https://osf.io/3peu5/files/pj7rc.

Data synthesis

Data pre-processing

All coding of all variables was performed by researchers at post-doctoral level in health psychology or a related field, and trained in the coding protocol. Scoring of the **psychological predictors** was done in two steps: a first step consisted of listing the variables according to their domain, using the Theoretical Domains Framework, TDF (30). From the TDF, the following domains were retained as psychological variables: 'Knowledge' (e.g., procedural knowledge); 'Skills' (e.g., cooking skills), 'Social/Professional Role and Identity' (e.g., main person in charge of meal preparation), 'Beliefs about Capabilities' (e.g., self-efficacy), 'Optimism', 'Beliefs about Consequences' (e.g., outcome expectancies), 'Reinforcement' (e.g., positive goal achievement), 'Intentions', 'Goals' (e.g., goal focus), 'Memory, Attention and Decision Processes' (e.g., attentional bias), 'Social Influences' (e.g. subjective norm), 'Emotions' (e.g. positive and negative affect), and 'Behavioural Regulation' (e.g., coping). 'Environmental context and resources' as a domain was excluded in line with the definition of psychological variables. The psychological variables were coded by one reviewer and double checked by two reviewers. Discrepancies were resolved through discussion among the three reviewers.

A second step consisted of further detailing the affective variables. Regarding the categorisation of affect, there is still an ongoing debate regarding whether negative and positive affect should be considered on a single continuum or separate continua, which is particularly relevant in within-person research (see (33)). In the present review, positive affect and negative affect were considered as separate predictors, based on evidence showing that positive affect and negative affect have sometimes comparable and sometimes opposite influences on diet (34). Daily hassles and stressors were merged into a single category of perceived stressors to account for the subjective difficulties experienced by participants. Because of the small number of studies investigating this variable, rumination (e.g., worry, brooding) was also added as a perceived (internal) stressor, as it has been shown to lead to negative affect (35) and eating behaviours (36) in cross-sectional studies.

In the included studies, it was observed that authors often directly or indirectly relied on the "(di)stress-induced eating" paradigm (37). They hypothesized that (unhealthy) eating is caused by either negative emotions (e.g., anger), or external (e.g., daily hassles) and internal stressors (e.g., worries). Distress caused by these emotions and stressors would lead individuals to consume unhealthy food in response to or aiming to alleviate that distress. Although these constructs are theoretically distinct, they appear to share a common mechanism relevant to eating behavior: dysregulated affective state, which may trigger compensatory or avoidant eating as emotion regulation attempt. This transdiagnostic framing is supported by affect regulation models of eating (e.g., in binge eating, (38)) and the observation that, at the item level in EMA designs, these constructs often show substantial conceptual overlap. Consequently, negative emotions, as well as external and internal stressors (including craving) were considered together as *negative psychological triggers (NPT)* in the meta-analysis to increase statistical power. This aggregation was not pre-registered and was driven to increase statistical power. As such, sub-construct sensitivity analyses were conducted to examine whether the pooled NPT effect was driven by one specific sub-construct (see Meta-analysis method subsection).

Methodological characteristics of the studies included several aspects that can impact the ecological validity of the study. First, adherence and drop-out were synthesized. These included % of participants retained in the study; % of prompts answered and completed; and whether a cut-off was used for level of completion to retain a participant or prompt in the analyses or how missing data were handled. As measures are aimed to be momentary, latency time (answering the prompt outside of the timeframe targeted by the study) or backfilling (answering all prompts in one go, irrespective of the time they were received) were also recorded as issues, where reported, in data quality. Electronic EMA can reduce the risk of high latency time or backfilling by disabling the prompt after the desired time window, so info on disabling of prompts, where reported, was recorded. Reporting of measurement reactivity was also coded. Next, validity and reliability of measurement tools were synthesized. We extracted whether any information was provided, for which variable (predictor and/or outcome), which type of validity or reliability was mentioned, and what the source of the measurement items were (no information; face or content validity in pilot studies with experts or users; using validated non-EMA scales; adapting non-EMA scales to EMA format without formal validation; self-constructed without formal validation; construct or criterion validation as EMA items).

Narrative synthesis

A narrative synthesis was conducted to summarise the characteristics of the included studies and the within-person predictor-diet associations which could not be included in the meta-analyses (e.g., lack of reporting of statistical information). Results were grouped by the type of predictor.

Meta-analysis

We pre-specified in the review protocol that if enough studies (i.e., ≥5) with similar within-person psychological predictor variables were identified, meta-analyses could be performed. Quantitative data from studies included in the systematic review were extracted by two authors and double-checked by a third author. Meta-analyses were performed on R (version 4.5.3) using the *metafor* R-package version 4.8-0 (39), in two steps. Within-person coefficients and their Standard Errors (SE) were included in the meta-analysis if they were from uni- or multi-variable hierarchical/multilevel models or comparable analytical approaches. To have one effect size per study, fixed-effect meta-analyses were performed on the different effect sizes within studies, if applicable. To compute the summary effect size, a random effect meta-analysis was then performed, due to the heterogeneity in designs and settings between studies (40). Summary effect sizes were computed separately for each outcome (healthy and unhealthy behaviours) and for each predictor (positive affect and the group of negative affect, stress, and negative cognitions). Only logOdds Ratio (OR) and standardized linear coefficients were included, as unstandardized linear coefficients do not allow comparison of the different outcome scales. Analyses were conducted separately for logOR and standardized linear coefficients, as they represent distinct statistical constructs. To handle studies having multiple effect sizes (media = 2, range 1-9 effect sizes per study-outcome-estimate combination), fixed effect meta-analyses were first performed within each study to produce a single composite effect size, which was then entered into a random effect meta-analysis across studies (40). This two-stage approach was selected over a three-level random effect model given the limited number of effect sizes in the combinations, which would have provided insufficient information to reliably estimate the within-study variance component.

To explore heterogeneity, the I^2 and τ^2 were calculated. To further explore the variability of true effect across studies, 95% prediction intervals were computed in addition to the confidence intervals (40). Quality of the studies, incentive schedule, and study design were pre-specified as categorical moderators. However, the small number of studies per meta-analysis (k = 5–8) meant that no categorical moderator met the pre-specified subgroup threshold across all three meta-analyses, and these analyses were not conducted. Continuous moderators (mean age, gender proportion of the sample, average percentage of adherence, and EMA duration in days) were retained (as they do not require subgroup partitioning) and computed in meta-regressions.

Risk of bias due to missing results, potentially reflecting reporting biases, was explored with funnel plots and Egger’s test by entering the sampling variance as a moderator variable in the multilevel, random-effects meta-analyses. As Egger's test is dependent on statistical power (i.e., number of ES included) (40), results of these tests should be interpreted with caution in the current work.

Although the pooled NPT analyses included negative affect, stress, rumination, and craving together, sensitivity analyses were performed to examine whether the pooled effect held when sub-constructs were considered separately. Effect sizes were grouped into three sub-constructs (negative affect, stress, cognition-craving). To formally test whether sub-constructs differed in their pooled effects, Q-between (Q_M) tests were conducted by entering sub-construct as a categorical moderator. These analyses were not-registered and came as a clarification of the NPT approach.

Results

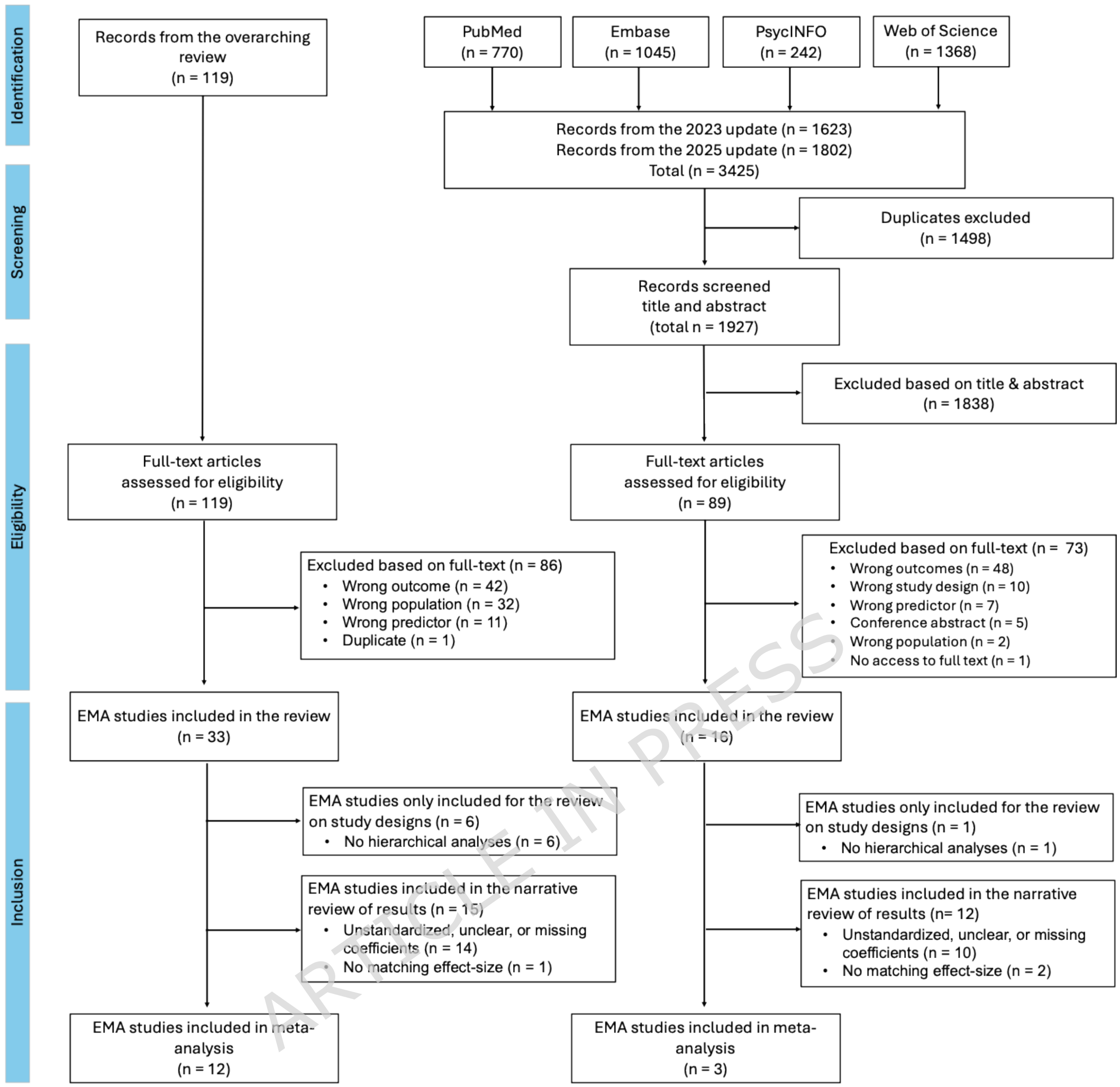

Figure 1. Flowchart of the selection process.

After removing duplicates, 15,733 records were identified as part of the larger review, with 1,078 studies screened at the full text stage. Of the 633 studies included in the larger review, 119 (119/633; 19%) focused on diet. Of these, 33 (33/119; 28%) studies reported on a healthy or unhealthy diet and were included in the present review. Twelve studies (12/33; 36%) had estimated effect sizes that could be included in the meta-analyses (Figure 1).

The search was updated in April 2023 and in May 2025, specifically with the search terms relevant for diet. This yielded 1,927 hits (after removing duplicates), of which 89 went to full text screening (89/867, 5%). Sixteen of these were included in the narrative review (16/89, 18%) and three were eligible for the meta-analysis. This resulted in a total number of studies included in the review of n=49 of which n=15 were included in the meta-analysis.

Study characteristics

Most studies were conducted in the United States (23/49, 46.9%), followed by Europe (19/49, 38.8%), Australia (4/49, 8.2%), and China (3/49, 6.1%). Studies reported an average sample size of Mean=163.9 ± SD=162.2 participants. Participants were aged on average 33.2 ± 11.1 years, 76.2% ± 17.3% were women. Ethnicity was reported in 24/49 studies (48.9%), with 66.0% ± 27.9% of participants identifying as of White ethnicity. Educational level (% with a university degree) was reported in 19/49 (38.8%) of the studies: on average, 52.1% ± 27.4% of participants had a university degree. Most studies recruited participants from the general population (24/49, 49.0%) or from a student population (16/49, 32.7%) (Table 1).

Most studies used observational (43/49; 87.8%) rather than interventional EMA designs. Average study duration was 17.6 days ± 35.6 (Table 1). No study used a burst design. In twenty-one studies (21/49, 42.9%), data were collected fully via mobile phones. In seven of those studies (7/22, 33.3%), all participants were asked to use their own mobile device for data collection. Most studies used fixed time-based sampling (33/49, 67.3%), followed by random time-based sampling (6/49, 12.2%) and event based-sampling (2/49, 4.1%). Seven relied on multiple forms of sampling (7/49, 14.3%). One did not report their sampling method (1/49, 2.0%). The most used EMA sampling frequencies were multiple times per day (24/49; 48.9%) and once daily (20/49, 49.8%) (Table 2).

A minority of studies did not report the use of incentives for participation or data completion (15/49; 30.6%). The remaining studies reported the use of some form of incentives, including, but not limited to, multiple forms of incentive (8/34, 23.5%), flat payment based on study completion (11/34, 32.4%), course credit (4/34, 11.8%), flat payment irrespective of the study completion (3/34, 5.9%), prize draw (3/34, 8.8%), and incentive per EMA prompt completed (5/34, 14.7%) (Table 2).

<include Table 1 here>

<include Table 2 here>

Study quality

Studies generally received a 'Strong' rating for Quality 1 (i.e., rationale provided for the EMA design; 41/49, 83.7%), a 'Weak' rating for Quality 2 (i.e., whether an a priori power analysis had been conducted, 41/49, 83.7%), half of the studies received a 'Strong' (28/49, 57.1%) rating and a quarter of them a 'Weak' (12/49, 24.5%) ratings for Quality 3 (i.e., adherence to the EMAs), a 'Weak' rating for Quality 4 (i.e., treatment of EMA missingness, 34/49, 69.4%) and a 'Strong' rating for Quality 5 (i.e., aggregation of data, 38/49, 77.6%) (Table 1).

Definitions and measurement of healthy and unhealthy diet

Most studies explored diet retrospectively (41/49, 83.7%), while the rest were event-based (6/49, 12.2%), focused on eating behaviours in the moment of the prompt notification (1/49, 2.0%) or used a mix of retrospective and event-based sampling of eating behaviours (1/49, 2.0%). The reporting relied on text (46/49, 93.9%, either requiring participants to type entries or select predefined labels) or on icons displaying categories of food (2/49, 4.1%). This was supplemented with taking pictures of food for two event-based studies (2/49, 4.1%).

While all studies explored healthy and/or unhealthy eating behaviours, they used heterogeneous methods to qualify it. The majority relied on reporting the food eaten (42/49, 85.7%), at various levels of precision. They mainly focused on grouping eating behaviours as healthy and unhealthy, i.e., FV only (9/42, 21.4%), unhealthy food and FV (5/42, 11.9%), healthy or unhealthy food (3/42, 7.1%). Two studies (2/42, 4.8%) relied on a list of specific food items (e.g., fried food, sweets) without merging them and one focused on sugar-sweetened beverage intake (1/42, 2.4%). Nineteen studies (19/42, 45.2%) focused on snacking. Half of them (9/42, 21.4%) included both healthy and unhealthy snacks, while the other half only focused on unhealthy snacks (9/42, 21.4%), and one study on healthy snacking only (1/42, 2.4%).

Among these 42 studies, different methods of reporting diet were used. Six studies (6/42, 14.3%) relied on open text entries, that were later coded by the research team based on recommendations or food composition tables. The majority relied on reporting the presence or quantity of specific food item categories (32/42, 76.2%) (e.g., fruit & vegetables, fast-food, soda), which was then coded by the research teams. In that group, most studies relied on reporting the absence/presence of one food item in their diet since the last prompt notification (17/32, 53.1%), while others tried to quantify it through the number of occurrence of eating these items (4/32, 12.5%), the number of portions eaten (7/32, 21.1%), or the weight/volume (2/32, 6.3%), the subjective quantity (1/32, 3.1%), or an unclear method (1/32, 3.1%). Aside from reporting the food eaten either through open text or a list of items, the last two studies (2/42, 4.8%) relied on asking the extent to which the person met several requirements since the last beep (e.g., including dietary fibre including five food groups in each meal). Two studies had unclear methodologies (i.e., unclear adaptation of cross-sectional retrospective items).

The remaining seven studies relied on a subjective evaluation of the healthiness of the food consumed (7/49, 14.3%). When evaluating subjectively the healthiness of the diet, they explored if it was more or less healthy than usual or if what they ate was perceived as healthily/unhealthily, using yes/no questions or Likert scales.

Retention, adherence and measurement reactivity

Most studies included at least one indicator of retention/adherence (40/49, 81.6%). Twenty-four studies (24/49, 49.0%) reported adherence as the number of prompts that were completed. Fifteen studies (15/49, 30.6%) used a cut-off point to consider a prompt as completed, or to include the participant based on their completion rate of the prompts. The cut-off points varied substantially: one study excluded participants or prompts for less than 10% completion; one for less than 33%; five studies if completion was less than 50%; two if it was less than 70%; and one if it was less than 86%. Prompt completion rate was on average 79.5% ± 18.3.

Twenty-three mentioned retention or drop-out in terms of the percentage of participants that completed the study (23/49, 46.9%). Study completion rate was on average 90.4% ± 9.5. Nine reported the number of prompts answered (9/49, 18.4%). Prompt answering response rate was on average 87.3% ± 8.6.

Only one study reported the average time it took for participants to answer when a prompt was sent (latency time) (41), which was M=29.8 minutes. Seven studies (7/49, 14.3%) disabled the link after a certain time has elapsed, ranging from 9-10 minutes (n=2), 30 minutes (n=2), to one hour (n=3). Five studies (5/49, 10.2%) checked for backfilling of prompts (responding to several interspersed prompts at the same time). Two studies did not report rates of backfilling, the other three studies reported between 0% and 7% of backfilled prompts. Three studies (3/49, 6.1%) considered potential measurement reactivity, one of these studies did not report levels nor how it was measured. One remaining study measured this on a self-reported measurement scale from 0-100, reporting an average of 20.7 on this scale. The other study conducted a regression analysis of study day number on comfort food eaten, and indicated a 4% decrease of comfort food consumption across the study period as measurement reactivity (Additional File 2).

Validity and reliability

In 21 of 49 studies (42.9%), some form of reliability of EMA items was reported. Of these, 14 studies only reported reliability for at least one of the predictors (66.7%), three studies only for at least one of the outcomes (14.3%), and four studies for both predictor(s) and outcome(s) (19.0%). Internal consistency of multiple items per scale was the most often reported as reliability indicator for the predictors (17/18, 94.4%), with the remaining study reporting interrater reliability of coding of an open-entry format question. Interrater reliability (i.e., agreement between coders on how to classify a certain food) was most often reported for the reliability of the outcomes (6/7, 85.7%) with the remaining study reporting internal consistency (14.3%). For nine studies reporting internal consistency, it was unclear at which level of aggregation these were reported (9/17, 52.9%), six studies reported both between-person and within-person internal consistencies for predictors (35.3%), one reported internal consistency on scores averaged across days (5.9%) and one reported internal consistency of the measure at each time point (5.9%).

For the validity of predictor variables, most studies reported having adapted items from a non-EMA scale (25/49; 51.0%). Most of these provided no specific information on what the adaptation consisted of (12/25, 48.0%). Six had adapted the time frame to fit with the EMA study (6/25, 24.0%) and eight had selected a few items from a larger scale (8/25, 32.0%). Nine studies used a self-constructed scale (9/49, 18.4%), eight used scales that had been used before in EMA studies without formal validation (8/49, 16.3%). Three studies mentioned having constructed items in pilot tests with end-users (3/49, 6.1%). Three studies provided no info on the source of the items (3/49; 6.1%). None used measures that had been previously validated for EMA studies. Of the studies mentioned here, three combined types of items for different predictors (two using a non-validated EMA scale and adapted scale for different predictors; one using an adapted scale and self-constructed scale for different predictors).

For the validity of the outcome variables, most studies reported having adapted items from a non-EMA scale (n=22/49, 44.9%). Most adaptations consisted of transforming scale items or open-ended questions used in cross-sectional studies to a daily or within-daily timeframe (n=18/22, 81.8%), often followed by a coding of the nutritional value of these meals using food tables (n=11/22, 50.0%). Only one study mentioned having checked the adaptation with members of the target group. Some studies explicitly mentioned having used an adapted version, but did not provide further details on what this adaptation consisted of (n=3/22, 13.6%). Twelve studies designed a new scale (12/49, 24.5%), whereas seven out of 49 used a scale that had been used before in an EMA study without formal validation (14.3%). Three out of 49 studies used a validated EMA scale (6.1%), whereas one tested the scale in a pilot study with end-users (2.0%), and six out of 49 studies did not report any source of the items (12.2%). Only five studies explicitly mentioned a specific form of validity for the measure used in EMA. Two studies referred to previous research for the validity of the measure, which was used but not validated in that study (2/49, 4.1%) or used in a different format (1/49, 2.0%). One refers to another paper that demonstrated predictive validity (1/49, 2.0%). One study mentions criterion validity against 24-hour recalls or food frequency questionnaires (1/49, 2.0%) (Additional File 3).

Meta-analyses and narrative review

Eighteen studies were eligible for the meta-analysis (42–58). These led to the extraction of eighty-three effect sizes (from 1 to 24 per study). Among these eighty-three effect sizes, three studies were excluded because these were the only ones exploring associations between eating behaviours and role of inhibitory control (one effect size, (54)) or choices and preference (four effect sizes, (51)) or relying on risk ratios (one effect size, (42)). Among the fifteen remaining studies, seventeen effect sizes were excluded because they did not meet inclusion criteria (twelve had eating outcomes that could not be categorized as healthy or unhealthy, four were about apathetic affect, one was the only effect size on the association between self-efficacy and eating behaviour). This led to sixty-one potential effect sizes to meta-analyse from fifteen studies.

Based on our criteria, we needed at least five studies per predictor/outcome set (i.e., based on TDF categories; (30)) to compute a meta-analysis. As displayed in Table 3, only three meta-analyses could be conducted. The first explored the associations between negative psychological triggers (NPT, i.e., negative affect, daily stress, and ruminations) and healthy eating behaviours, while relying on standardized regression coefficients. The meta-analysis showed that NPT are negatively associated with healthy eating behaviours (β = -0.007, p = .006, k = 8, n total = 1,489), meaning that when more-than-usual NPT was reported by individuals, less healthy eating behaviour was reported (Figure 2). Prediction intervals crossed 0 [-0.020, 0.005], indicating that true effect in a new comparable study could be absent. Heterogeneity analyses showed that the results were heterogeneous (τ^2 = 0, I^2 = 60%, Q(6) = 19.77, p = .006). The visual analysis of the funnel plot (Additional File 4) did not show asymmetry (z = -1.56, p = .118). The second and third explored the associations between NPT and unhealthy eating behaviours, respectively relying on standardized regression coefficients and logOddsRatios (logOR). The second showed that NPT are positively associated with unhealthy eating behaviours ($\beta = 0.054$, $p = .022$, $k = 6$, n total = 1,578), meaning that when individuals reported more-than-usual NPT, they reported more unhealthy eating behaviours (Figure 3). The meta-analysis however showed the prediction interval crossed 0 [-0.059, 0.166], and evidence of heterogeneity ($\tau^2 = 0.003$, $I^2 = 97.9\%$, $Q(5) = 200.86$, $p < .001$).

A visual analysis of the funnel plot supports important heterogeneity (Additional File 4) but did not show significant asymmetry ($z = -1.19$, $p = 0.233$). The third meta-analysis did not show a significant association between NPT and unhealthy behaviours when pooling the logOR (logOR = .469, $p = .095$, $k = 5$, n total = 944) (Figure 4). The results were importantly heterogeneous ((95% prediction interval = [-0.846, 1.785], $\tau^2 = 0.372$, $I^2 = 99.2\%$, $Q(5) = 395.73$, $p < .001$). The Funnel plots also highlight that important heterogeneity exists (Additional File 4), though the Egger test did not display significant asymmetry ($z = 1.10$, $p = .270$). Moderator analysis (Table 3) showed that a) the older the sample was, the stronger (more negative) was the association between NPT and healthy eating behaviours (based on standardized coefficients) and b) the longer the EMA study duration was, the larger (more positive) was the association between NPT and unhealthy eating behaviours (based on standardized coefficients). All studies included in the different meta-analyses have a moderate study quality, with the exception of (56), that had a strong study quality (effect size = -0,07).

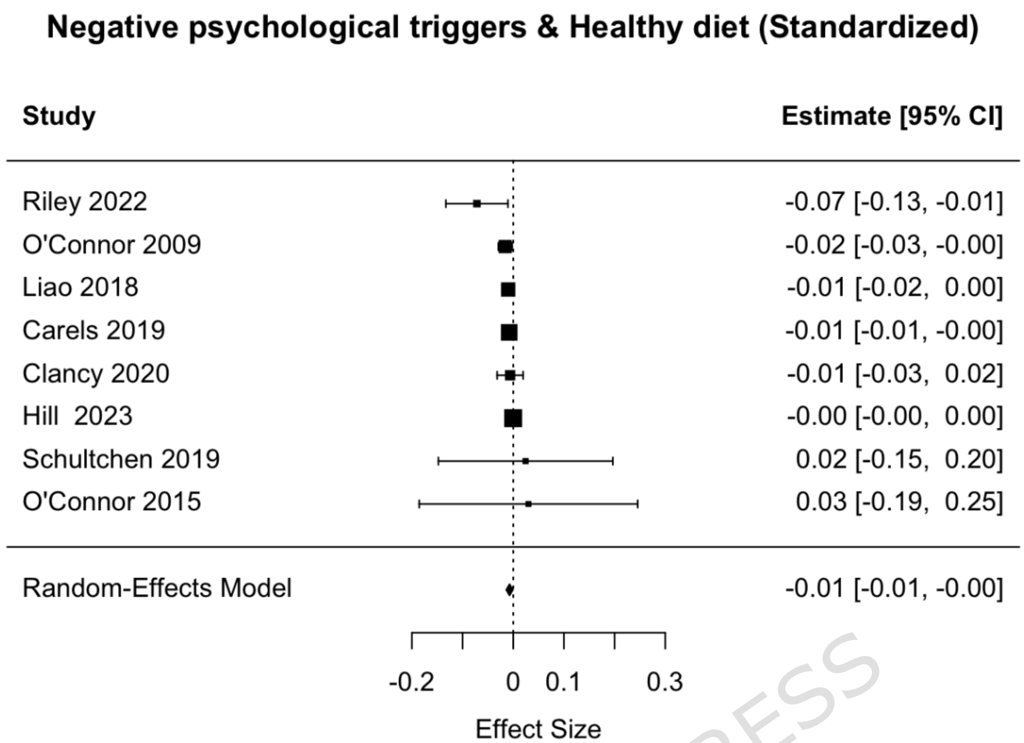

Figure 2. Meta-analysis on the association between negative psychological triggers and healthy diet based on standardized coefficients (k = 8, n total = 1,489).

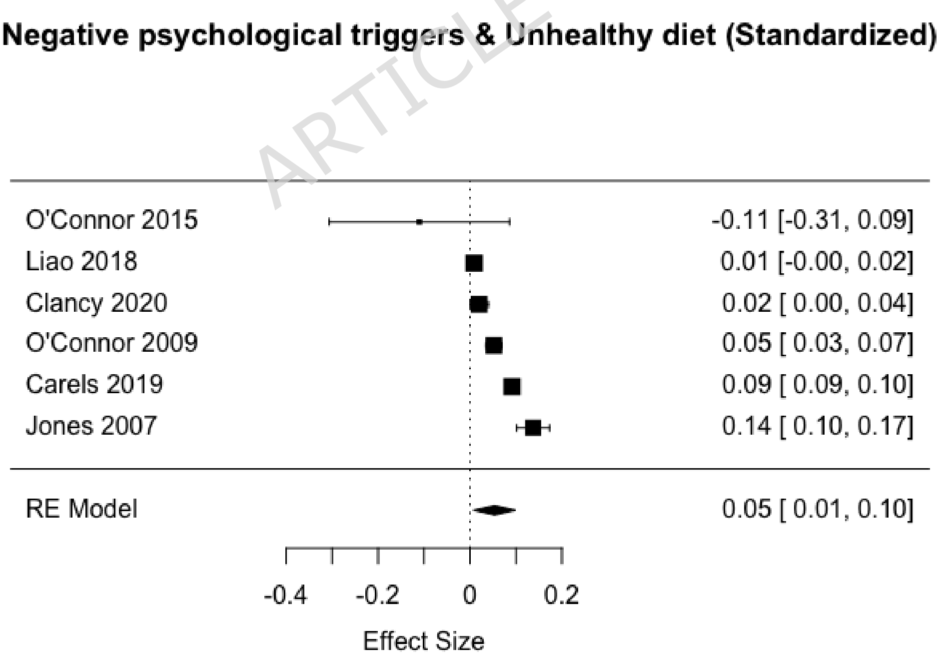

Figure 3. Meta-analysis on the association between negative psychological triggers and unhealthy diet based on standardized coefficients (k = 6, n total = 1,578).

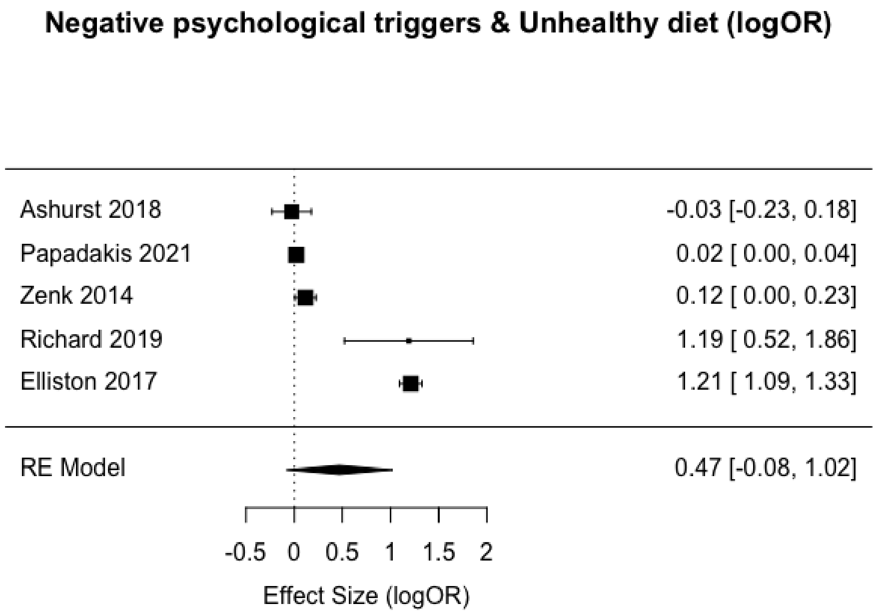

Figure 4. Meta-analysis on the association between negative psychological triggers and unhealthy diet based on log odd ratios (k = 5, n total = 944).

<include Table 3 here>

<include Table 4 here>

To examine whether the pooled NPT effect was driven by one specific sub-construct, separate analyses were conducted for negative affect, stress, and the combined cognition + craving category (Table 5). For healthy eating (standardized coefficients), the cognition + craving sub-construct showed a significant association (β = -0.009, p = .004, k = 3), whereas negative affect alone (β = -0.010, p = .071, k = 2) and stress alone (β = -0.006, p = .420, k = 4) did not. For unhealthy eating (standardized), none of the three sub-constructs reached significance individually (k = 2 each, all p > .11). For unhealthy eating (logOR), only negative affect could be meta-analysed (logOR = 0.306, p = .319, k = 4); stress and cognition + craving were available in single studies only. Q-between tests did not detect significant differences between sub-constructs for the healthy eating analysis (Q_M = 0.31, df = 2, p = .857) or the unhealthy eating standardized analysis (Q_M = 0.58, df = 2, p = .748). For the unhealthy eating logOR analysis, the test could not be performed because only one sub-construct had k ≥ 2 studies. Although Q-between tests are underpowered at small k, they do not show a significant difference between sub-constructs. Notably, residual heterogeneity within sub-constructs remained substantial (e.g., I^2 = 97.8% for the unhealthy eating standardized analysis), indicating that heterogeneity in this literature is not primarily attributable to construct-level differences within the NPT category.

<place Table 5 here>

In addition, a narrative review of effect sizes was conducted to describe the results of studies that could not be included in the meta-analysis (k = 34). Among these, seven studies (7/34, 20.6%) did not report within-person associations between psychological predictors and eating behaviours (59–65) and were therefore not included in the narrative review either. Consequently, the narrative review of effect sizes is based on the remaining 27 studies.

Narrative review of effect sizes from single studies on unhealthy eating behaviours

Stress was inconsistently associated with unhealthy eating behaviours. In some studies, stress was not significantly associated with unhealthy eating behaviours (66,67) (both moderate study quality) whereas in others it was associated with an increase in unhealthy eating behaviours (68,69) (both moderate study quality). Moderation analyses showed that this association could also be negative, notably by a moderation of mindfulness, where stress was negatively associated with comfort food when individuals score high on mindfulness (67) (moderate study quality). Regarding the role of affect, results were heterogeneous for both positive and negative emotions. For negative emotions, some studies showed that negative affect is associated with an increase in unhealthy eating behaviours (12,70,71) (all moderate study quality), with two showing no significant association (72,73) (both moderate study quality).

For positive emotions, most of the studies showed no association (12,71–73) (all both moderate study quality), with one study showing that positive emotions were more prevalent in high caloric situations (74) (moderate study quality). Apathetic emotions did not show significant association with unhealthy eating behaviours (43) (moderate study quality). An additional study explored the temporal trend of emotions before snacking and showed that there is a decline in positive affect preceding unhealthy snacking (59) (moderate study quality). Irrespective of level of stress and emotions, one study showed that intention to avoid snacking was associated with reduced snack consumption (75) and one that lower inhibitory control is associated with increased snacking (54) (both moderate study quality).

Narrative review of effect sizes from single studies on healthy eating behaviours

For the role of stress, internal and external stress were inconsistently associated with healthy eating behaviours. One study showed that stress was more present when choosing healthier dietary options (74) (moderate study quality) suggesting a positive association. This was also shown for same day associations between financial stress and increased consumption of grains (66) (moderate study quality). Stress was also shown to be negatively associated with healthy eating behaviours, where a reduction of stress is associated with increased healthy eating behaviours (driven by an increase in self-compassion, (76), moderate study quality). Other studies did not show significant associations between work and interpersonal conflicts (66), stress (69,77), or rumination (56) with healthy eating behaviours (all moderate study quality). Regarding the role of affect, most studies did not find associations between negative emotions and healthy eating behaviours (12,72,78,79) while two showed that negative emotions and distress were associated with reduced healthy eating behaviours (80,81) (all moderate study quality). The role of positive emotions, however, seems more consistent, with several studies showing that an increase in positive affect and well-being is associated with increased healthy eating behaviours (41,72,79,82,83) (all moderate study quality). However, one study showed that, among non-significant emotional states, cheerfulness was associated with a decrease in healthiness of a snack at the within-person level (84) (moderate study quality). Two studies did not find any association (12,78) and one study exploring the temporal dynamic of emotions showed that positive affect was stable when preceding taking a healthy snack (59) (all moderate study quality). Apathetic emotions were not significantly associated with healthy eating behaviours (43) (moderate study quality). Irrespective of stress and emotions, studies did not show associations between general planning for the day (80) and perceived support (85) with an increase in fruit and vegetable consumption, but showed that when students interacted more than usual with their parents, they consumed more fruit and vegetables (86) (all moderate study quality). Finally, intention to eat a healthy snack was also positively associated with the healthiness of the snack eaten (84) (moderate study quality).

Discussion

This review aimed to explore the association between psychological factors and healthy or unhealthy dietary behaviours in daily life. It relied on a systematic review and several meta-analyses to aggregate different effect sizes of these associations. The first and main statistical result was a significant association between negative psychological triggers (i.e., stress, negative emotions, and ruminations) with (un)healthy eating behaviours in two out of three meta-analyses. It confirmed the hypothesis that negative psychological triggers are associated with more unhealthy and less healthy eating behaviours. However, beyond that result, a central finding was that the available evidence remains limited and heterogeneous, which impairs the possibility and reach of meta-analysis on the topic. This issue arose at different levels, adding to the difficulty of computing aggregated effect sizes.

The substantial heterogeneity is first observed at the meta-analytical level. Two of the three meta-analyses displayed I^2 values close to 99% and prediction intervals crossing 0 in all three analyses. As such, the pooled estimates are best understood as summaries of a distribution of true effects rather than as estimates of a single common effect (38). This heterogeneity likely reflects the multilayered differences across included studies discussed below: differences in eating behavior definitions; temporal alignment of predictors and outcomes; EMA sampling protocols; and measurement approaches. Moderator analyses were conducted on continuous moderators, but the limited number of effect sizes per meta-analysis constrained the power to detect systematic sources of heterogeneity. Pre-registered categorical moderators could not be analyzed given the small number of studies per subgroups, which calls for a cautious interpretation of the results.

The results of this meta-analysis, when significant, remain small. Taking that into account, these results across EMA studies do suggest that momentary negative emotions and stress are associated with an unhealthier dietary intake, showing a small effect size (for standardized coefficients, but not for the log odds ratios) with high heterogeneity. Much existing literature has focused on between-person differences in increased food intake in response to emotions, attributing these differences to a trait of emotional types of eating, such as restrained (i.e., restriction of food intake), external (i.e., eating in response to food cues), disinhibited (i.e., combination of emotional and external eating), or concerned (i.e., worrying about their eating behaviour) eating, most pointing to mixed evidence and concerns over how these traits are measured in self-report scales (87,88). These may just be measuring a tendency among concerned eaters to attribute overeating to emotions for lack of a better explanation, rather than be predictive of food intake. EMA can bypass this bias by not asking for self-reflection on how emotions explain food intake. Our meta-analysis did not focus on the amount of food intake but on its healthiness, plus it did not consider moderators such as contextual cues, preferences and personality profiles (89,90).

While literature on emotional eating in eating behaviour in a general population shows mixed evidence, it appears to be more conclusive in patient populations or on disordered eating (88). Indeed, a meta-analysis of EMA studies has shown that negative affect has a moderate effect size on binge eating (38), while a narrative synthesis of EMA studies showed negative affect was associated with more lapses from weight loss dieting (91). Although based on few included studies, and with small effect sizes, our meta-analysis also partially supports the role of negative psychological triggers, including negative emotions, in an unhealthier dietary intake in a general, healthy adult population.

Given this important role of negative emotions in a (un)healthy diet, Ecological Momentary Interventions (EMI) or JITAIs would do well to measure negative affect in the moment and offer emotion regulation techniques to counter their negative effect on dietary behaviour. Only a handful of EMI/JITAI studies on affect and dietary intake have been published (18,92). For example, Goldstein et al. (2021) conducted an

EMI on weight loss and dietary lapses and offered intervention options such as breathing exercises to reduce stress and information on the link between emotions and eating (93). Forman et al. (2019) measured affect as a predictor of dietary lapse but did not report affect-based components in the intervention (94). Momentary emotion regulation strategies are also rarely included in EMIs for eating disorders (95). Inspiration for a more extensive emotion regulation component in healthy dietary EMI's could be drawn from a positive psychology EMI, that includes noticing and increasing positive events, gratitude, mindfulness and positive reappraisal exercises, focus on personal strengths, behavioural activation and acts of kindness (96).

Research has shown that positive emotions and negative emotions have distinct associations with dietary intake (97). Unfortunately, we could not perform a meta-analysis on positive emotions as a predictor of dietary behaviour due to lack of studies reporting statistical info to calculate effect sizes.

Moreover, there was evidence of important heterogeneity in the outcomes and their reporting. Eating outcomes widely varied in their nature. They focused on reporting food eaten – whether fruit and vegetables, (un)healthy food, (un)healthy snacks, or specific food items – or a subjective evaluation of healthiness. As such, the composite scores included in the present review already present an important level of heterogeneity in what was measured. In addition, several studies were excluded because they only reported unstandardized effect sizes. As the outcomes were often different, unstandardized effect sizes could not be computed as they would not have measured comparable constructs. Based on that, we therefore recommend that authors consider the need for cumulative evidence when designing studies, selecting measures, and reporting results. This can take the form of systematic reporting of standardized coefficients, in a comparable fashion to correlation matrixes for between-person studies and available open-access to allow reuse of material and data. More globally, it reflects the lack of standardized measures in EMA studies, which is an issue not exclusive to eating behaviour studies (e.g., (21,22)).

Beyond the sole reporting and outcome measurement issues, the psychological constructs measured were also limited in their span. Based on available evidence, "negative" psychological triggers focused on stress, negative emotions, and ruminations, whereas the "positive" psychological triggers only encompassed positive emotions. While we could have further merged other available constructs, it would have impaired the conceptual homogeneity of considering them together. Several other constructs were therefore not integrated into the meta-analysis as they were underrepresented and could not fit into these two conceptual categories, even though they present promising research avenues (e.g., inhibitory control, (54)).

As observed in previous work (98), the heterogeneity also lies in the temporal focus of the eating behaviours measured, which may impair the cumulative conclusion's validity (23). While most studies relied on a retrospective focus (e.g., what they ate in the last hour), others relied on event-based or in-the-moment reporting. Studies therefore did not all measure the same phenomenon. This must be coupled with the fact that predictors were not always coherent in the temporal focus of the eating behaviours (e.g., emotion in the moment and snacking in the last hour). This mix of temporalities echoes key issues in the field, with the global concern that EMA tools should be adequately used to measure momentary experiences (19).

Another key issue in EMA methodology is study and measurement validity. Several factors could influence the ecological validity of study findings. First, non-adherence with the sampling protocol may impact validity. Non-adherence is especially a problem if the rate of missing data is so high that it threatens the sampling's representativeness, and if the data are not missing at random, meaning that data are missing at certain moments or are related to the values of variable at interest (e.g., high fatigue, during eating) (19). Ideally, adherence should be assessed for its missingness at random, and be reported by several detailed indicators, including whether these are related to time of day, rules for being considered as a valid data entry and assess differences by user characteristics (99). Previous reviews indicated that few studies considered adherence or validity (11). Adherence was only reported in time-based sampling studies, with an average of 79% completion rates (72). In these reviews, the most often reported adherence measure was participant drop-out (48-54% of studies) and number of prompts answered (41% of studies). About a third set criteria for valid EMA data, either by completion rate or by latency time, with a median of 15 minutes (14). Our review showed that most studies reported some form of adherence, with around half of the studies reporting completion rate of prompts (49.0%), and also around half reporting participant drop-out (46.9%). Around a third set criteria for valid EMA data based on adherence rates, with cut-off points largely varying between 10-86% adherence. Adherence rates (between 79.6% - 90.4%) were much in line with those reported in other reviews (15,20). Much less often reported were disabling of prompts (14.3%), latency time (2.0%) or backfilling (10.2%), which raises questions about how momentary exactly the assessments were. Measurement reactivity was also rarely reported (6.1% of studies). Another review on dietary EMA (in youth) indicated that studies that did report measurement reactivity, reported it to be low (100).

Second, reliability and validity of the measures impact the study validity. Only a small fraction of the studies used tools that were validated as EMA tools to measure eating behaviours while there were many *ad hoc* adaptations made to existing cross-sectional questionnaires or items that were created for the study. References made to validity often related to the validity of the coding frames used to categorize dietary intake reported in the open-ended answers, but very infrequently examined the validity of the items themselves. A similar finding occurred for the measured psychological predictors. The reporting of the reliability of the EMA items was also importantly heterogeneous, as it was often unclear what the authors were reporting as reliability measurement, if any was present. This is particularly striking considering that existing reporting standards suggest a more systematic and structured approach to listing these elements, notably to be able to compare them (92). To ensure validity of the questions, it does not suffice to transform survey items from a scale with a longer time-period as reference frame to a momentary assessment, as these are often differently interpreted which may hinder content validity. Assessing the target group's interpretation of items prior to EMA data collection is therefore recommended, for example via focus groups or cognitive interviewing using think-aloud techniques (21). The low attention to validation of EMA measures is worrisome given that a systematic review of validity of EMA measures in several health domains showed there was a substantial variation in criterion validity of EMA measures across studies, when examining correspondence with behaviour observations (22). Only two included studies in their review measured food intake, both in obese populations, one showing no validity of the EMA, and one showing that validity increased as prompting was more frequent. Stinson et al. suggest providing participant training to increase validity of measures, consider the sensitivity to change by aligning prompt frequency with the frequency and severity of the natural occurrence of the behaviour, and tailoring prompts to times relevant to the individual participant (22).

Implications for EMA research

Most studies in this review were rated as of moderate quality, with only one study receiving a weak score and four studies receiving a strong score. This highlights that although EMA studies are generally of relative methodological quality, many improvements are yet to be made. These seem to be situated around two axes: one being not considering methodological checks and justifications that are apparent in non-EMA research; and a second being not sufficiently addressing the specificity of repeated measurements that are assessing momentary, situational fluctuations. On the first axis, for example, it is self-evident for most non-EMA research to report a power analysis and to (re)validate the measurement scales (after adaptations). In our research, only eight out of 49 studies had reported a power analysis or justified why it was not needed (quality criterion 2). This may be due to power calculations being more complicated for multi-level analyses. Some resources exist that make these more accessible, such as Kleiman's lab power calculator (https://kleimanlab.org/resources/power-curves/) or the tutorial for power calculations in mixed-models by Brysbaert & Stevens (101). Making these resources more visible to all EMA researchers could enhance methodological quality. Next, many studies did not use a validated scale for EMA, instead often relying on non-EMA validated scales that were slightly adapted to a momentary format, without any further testing. This lack of attention to content validity of scales was also noted in EMA research on physical activity (21). Possibly, there is a lack of awareness that changing the timeframe may impact comprehension and applicability of items, or that selecting a few items from a larger scale may change content validity. As EMA requires short, easy to understand items, more efforts are needed in validation of the EMA items themselves, e.g., in comparison of single items to objective measures (22). On this second axis, we moreover notice that missingness of data and how this may be related to the variables of interest is rarely addressed (quality criterion 4). In EMA research, however, missingness that is not at random is a serious threat to its ecological validity (see e.g. (102)). The same goes for reactivity to repeated measurements, this is an inherent risk to EMA but infrequently addressed in EMA studies. Several of these specific methodological considerations for EMA are included in the CREMAS checklist (31), but not all (e.g., validity, reactivity). As EMA research is continuously growing, turning this checklist into a living document with updates as new insights arise, as well as increasing the use of these checklists may help to further increase methodological quality of the studies.

Strengths and limitations

This review and meta-analysis added to existing literature on the role of emotions in dietary intake by focusing on a general adult population and examining effect sizes, whereas existing reviews have mainly addressed clinical populations or disordered eating and methodological aspects of EMA studies. As emotional eating in healthy adults shows inconsistent findings in the literature and could contribute to the societal trend of increasing overweight and obesity, these insights are relevant for intervention design. To the best of our knowledge, this is the first meta-analysis on the role of momentary affect in healthy or unhealthy dietary intake in a general adult population. Our review also pinpointed some recommendations for future research that would allow us to expand the predictors and outcomes included in the meta-analysis, and increase confidence in the validity of findings from EMA studies.

The limited number of exploitable studies led to overall statistically underpowered analyses. This was particularly critical for the handling of within-study dependency between effect sizes, given that some studies contributed multiple effect sizes to the same meta-analysis. The two-stage approach used here (fixed-effect aggregation within studies, random-effects across studies) does not fully model this dependency. While this method is recommended by some (38), it must be acknowledged that three-level random-effects models (e.g.,(103)) are increasingly recommended for this purpose, as they explicitly partition variance into between-study and within-study components. However, given that most studies in our analyses contributed few effect sizes, and that the number of studies per meta-analysis was small ($k = 5–8$), three-level models would have provided insufficient information to reliably estimate the within-study variance component. As such, we did not pursue them here. Future EMA syntheses with larger numbers of studies and effect sizes per study should adopt three-level approaches as a more principled way of handling effect-size dependency. Relatedly, Egger's tests for small-study effects were underpowered at the available sample sizes ($k < 10$), and their results should therefore be interpreted as exploratory rather than as conclusive evidence regarding the absence of publication bias.

A limitation of the present review and meta-analysis – that arose from the included studies – lies in the difficult classification of eating behaviours into healthy or unhealthy categories. While we focused on unambiguously categorizing eating behaviours (e.g., not automatically considering unlabelled snacking as unhealthy) to ensure a meaningful distinction between healthy and unhealthy behaviours, this distinction only allowed for the inclusion of a limited set of studies. This reflects that several eating behaviours (or their measurement) could be considered healthy or unhealthy depending on the context (e.g., total calorie intake beyond a certain quantity or number of snacks). In other words, effect sizes had to be excluded from the review and meta-analysis because their potential association with health was unclear. In addition, when authors labelled themselves the (un)healthiness of the measured eating behaviours, they did not all use the same criteria. Some relied on dieticians rating the reported food, others relied on a priori categorizations or simply categories of food items (e.g., fruit & vegetables as healthy). Finally, studies relying on the subjective evaluation by the participants of the healthiness of their eating behaviours added another layer of complexity in trying to build a cumulative picture of this field.

All these elements point to a multilayered heterogeneity in the field, whether in the concept, the categorization, the measurement, the temporality, the validity, the predictors, or even the reporting. Even though we were able to conduct three meta-analyses and a systematic review of the associations between psychological factors and eating behaviours, we can only conclude that the field remains difficult to interpret and challenging to build cumulative evidence now. This includes the important heterogeneity scores obtained when a meta-analysis could be conducted, which crystallises this challenge. Based on that observation, we make a strong case for harmonizing all these practices, while keeping scientific relevance or even improving it by doing so. First, it appears critical to rely on existing resources, such as existing reporting standards for EMA studies (31) . This would include reporting adequate coefficients and reliability measures. Second, this would also be an opportunity to take into consideration developments in scale building for EMA (104). Using scales that can be re-used in other studies improves the cumulative capacity of the field while providing the opportunity to clarify the construct measured. Third, while studies have their own goals and relevance, they must also be designed with a cumulative mindset to contribute to building more solid evidence (105).

Conclusion

This review provided a comprehensive overview of the literature on within-person psychological correlates of (un)healthy eating behaviours. It showed that, at the within-person level, there are small associations between the two, even if non-significant with logOR, suggesting that psychological predictors can have an influence on eating behaviours. Nevertheless, this review also showed the important heterogeneity of the included studies. The diversity in effect sizes, outcome definitions, temporal alignment, measurement tools, and reporting practices severely impairs the efforts of cumulative knowledge. This calls for a greater harmonization in EMA research on eating behaviours on different levels to support the generalizability of the conclusions that can be drawn.

List of abbreviations

EMA: Ecological momentary assessment

JITAI: Just-in-time adaptive interventions

PRISMA: Preferred Reporting Items for Systematic Reviews and Meta-Analyses

APA-MARS: American Psychological Association's Meta-Analysis Reporting Standards

FV: Fruits and vegetables

WHO: World Health Organization

TDF: Theoretical Domains Framework

NPT: Negative psychological triggers

EMI: Ecological Momentary Interventions

OR: Odds ratio

NR: not reported

Declarations

Ethics approval and consent to participate

Not applicable

Consent for publication

Not applicable

Availability of data and materials

Generated data for the meta-analysis are available on the OSF page of the project: https://osf.io/3peu5/files/nwx49.

Competing interests

The authors declare that they have no competing interests.

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Authors' contributions

All authors contributed to the study conceptualization. AD, PG, DKw, DP and BA-A contributed to the methodology. All authors contributed to at least one part of the analysis (abstract and title, full text review, data extraction, data synthesis for meta-analysis). AD and PG performed the data visualisation and wrote the initial draft. All authors contributed to review and editing of the final version of the manuscript.

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Table 1. Study characteristics

Author	Country	Sample size	M age ± SD	% female	% White ethnicity	% university degree	Target popu-lation	Study duration in days	**Study type**	Study quality criterion 1	Study quality criterion 2	Stud qual crite 3
Amrein et al., 2021 (42)	Germany	45	21.90 ± NR	66.7	NR	NR	Students	7	Observational	Strong	Weak	Wea
Anderson & Fowers, 2021 (80)	United States	76	40.29 ± 13.69	57.9	85.5	NR	General population	14	Observational	Strong	Weak	Stro
Anderson, 2020 (82)	United States	76	40.29 ± 13.69	57.9	85.5	NR	General population	14	Observational	Strong	Weak	Stro
Ashurst et al., 2018 (43)	United States	663	18.40 ± 0.50	70.6	48.9	NR	Students	7	Observational	Strong	Weak	Stro
Berge et al., 2018 (66)	United States	61	34.90 ± 7.10	95.0	NR	NR	General population	8	Observational	Strong	Weak	Stro
Boh et al., 2016 (74)	Netherlands	100	31.70 ± 10.30	88.0	NR	23.0	General population	14	Observational	Strong	Weak	Stro
Carels et al., 2019 (44)	United States	66	NR	85.0	59.0	89.0	Overweight/obese	30	Observational	Strong	Weak	Stro

Author	Country	Sample size	M age ± SD	% female	% White ethnicity	% university degree	Target popu-lation	Study duration in days	**Study type**	Study quality criterion 1	Study quality criterion 2	Stud qual crite 3
Clancy et al., 2020 (45)	United Kingdom	273	20.20 ± 4.10	93.0	NR	0.0	Students	7	Observational	Strong	Weak	Stro
Cnudde et al., 2024 (84)	Belgium	48	66.20 ± 6.20	52.9	NR	67.7	Older adults	7	Observational	strong	strong	mod e
Comulada et al., 2018 (65)	United States	14	30.80 ± 6.40	100	18.0	68.0	Other	252	Observational	Strong	Weak	Wea
Disabato et al., 2022 (81)	United States	289	50.70 ± 16.10	55.0	78.5	37.4	General population	10	Observational	Strong	Weak	Mod e
Dunton et al.,2017 (69)	United States	167	41.10 ± 6.10	100	NR	60.0	General population	8	Observational	Strong	Weak	Stro
Elliston et al., 2017a (70)	Australia	51	31.81 ± 14.87	66.7	78.4	49.0	General population	14	Observational	Strong	Weak	Stro
Elliston et al., 2017b (46)	Australia	61	32.33 ± 12.90	68.9	97.0	49.2	General population	14	Observational	Strong	Weak	Stro
Finkelstein-Fox et al., 2020 (67)	United States	261	18.90 ± NR	74.3	62.8	NR	Students	11	Observational	Strong	Weak	Stro
Franja et al., 2021 (59)	Australia	149	32.20 ± 11.85	67.2	75.9	NR	General population	14	Observational	Strong	Weak	Wea

Author	Country	Sample size	M age ± SD	% female	% White ethnicity	% university degree	Target population	Study duration in days	**Study type**	Study quality criterion 1	Study quality criterion 2	Stud qual crite 3
Fredrickson et al., 2021 (78)	United States	217	48.60 ± 9.00	59.9	76.5	NR	General population	63	Interventional	Strong	Strong	Wea
Headrick et al., 2022 (71)	United States	469	41.64 ± 10.74	93.0	95.0	NR	Other	35	Observational	Moderate	Weak	Mod e
Hill et al., 2023 (47)	United Kingdom	123	17.93 ± NR	83.0	69.0	NR	Students	14	Interventional	strong	weak	stro
Hochgraf et al., 2024 (77)	United States	497	35.86 ± 6.69	91.0	NR	NR	Parents	8	Observational	strong	weak	stro
Inauen et al., 2016 (106)	Germany	45	22.00 ± 2.3	66.3	NR	NR	Students	7	Observational	Strong	Weak	Wea
Jeffers et al., 2020 (12)	United States	30	21.00 ± 3.99	50.0	43.3	NR	Students	6	Observational	Strong	Weak	Stro
Jones et al., 2007 (48)	United Kingdom	420	40.50 ± NR	48.8	NR	NR	General population	28	Observational	Strong	Weak	Mod e
Jones et al., 2022 (41)	United States	121	42.12 ± 12.01	60.0	89.0	78.0	General population	7	Observational	Strong	Weak	Stro
Jones et al., 2024 (60)	United Kingdom	36	51.91 ± 12.56	69.4	NR	NR	Family members of people with substance abuse	14	Observational	strong	strong	mod e
Kilb et al., 2023 (85)	Germany	128	22.74 ± 4.66	89.1	NR	NR	Students	7	Interventional	Moderate	Strong	Wea

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Author	Country	Sample size	M age ± SD	% female	% White ethnicity	% university degree	Target popu-lation	Study duration in days	**Study type**	Study quality criterion 1	Study quality criterion 2	Stud qual crite 3
Kos et al., 1997 (61)	Switzerland	55	NR	100	NR	NR	General population	8	Interventional	Weak	Weak	Wea
Li et al., 2020 (76)	China	82	35.18 ± 5.80	63.4	NR	85.3	General population	7	Observational	Strong	Weak	Wea
Liao et al., 2018 (49)	United States	197	40.90 ± 6.20	100	NR	60.0	Other	8	Observational	Strong	Weak	Stro
Lin et al., 2021 (50)	United States	70	42.20 ± 9.70	100	0.0	58.5	General population	7	Observational	Strong	Weak	Mod e
Mason et al., 2019 (72)	United States	191	40.98 ± 6.13	100	45.5	34.1	General population	8	Observational	Strong	Weak	Stro
Masterton et al., 2023 (51)	United Kingdom	49	28.82 ± 9.58	51.0	NR	NR	General population	7	Observational	Moderate	Weak	Stro
Meule et al., 2019 (62)	Austria	105	23.40 ± 5.10	85.7	NR	NR	Other	10	Interventional	Strong	Strong	Stro
Moore et al., 2022 (79)	United States	142	35.00 ± 0.57 (SE)	45.1	NR	NR	General population	7	Observational	Weak	Weak	Wea
Moss et al., 2021 (68)	United Kingdom	49	19.13 ± 0.87	100	80.0	NR	Students	4	Observational	Strong	Weak	Wea
O'Connor et al., 2015 (107)	United Kingdom	202	22.96 ± 9.89	78.2	94.0	NR	General population	7	Interventional	Strong	Weak	Mod e

Author	Country	Sample size	M age ± SD	% female	% White ethnicity	% university degree	Target popu-lation	Study duration in days	**Study type**	Study quality criterion 1	Study quality criterion 2	Stud qual crite 3
O'Connor et al., 2009 (52)	United Kingdom	420	40.26 ± NR	54.3	NR	NR	General population	28	Observational	Strong	Weak	Wea
Papadakis et al., 2021 (53)	Australia	60	32.37 ± 12.96	69.0	91.7	55.0	General population	14	Observational	Strong	Weak	Stro
Powell et al., 2017 (54)	United Kingdom	64	38.58 ± 15.54	76.6	NR	NR	General population	7	Observational	Strong	Strong	Stro
Richard et al., 2019 (55)	Austria	61	20.30 ± 2.34	100	NR	NR	Students	7	Observational	Strong	Weak	Stro
Riley et al., 2019 (108)	United States	285	19.30 ± NR	76.8	79.4	NR	Students	11	Observational	Strong	Strong	Stro
Riley et al., 2022 (56)	United States	157	19.70 ± 1.22	78.2	74.9	NR	Students	11	Observational	Strong	Strong	Stro
Roefs et al., 2019 (63)	Netherlands	100	NR	88.0	NR	NR	General population	14	Observational	Moderate	Weak	Stro
Schultchen et al., 2019 (57)	Germany	51	23.50 ± 2.60	78.4	NR	0.0	Students	7	Observational	Strong	Weak	Stro
Skoyen et al., 2013 (64)	United States	124	28.30 ± 12.50	50.0	57.0	30.0	Heterosexual couples	7	Observational	Moderate	Weak	Mod e
Small et al., 2013 (86)	United States	746	NR	NR	NR	NR	Students	14	Observational	Weak	Weak	Stro
Wen et al., 2022 (83)	China	113	19.84 ± 1.50	83.2	NR	NR	Students	14	Observational	Strong	Weak	Wea

Author	Country	Sample size	M age ± SD	% female	% White ethnicity	% university degree	Target popu-lation	Study duration in days	**Study type**	Study quality criterion 1	Study quality criterion 2	Stud qual crite 3
Zenk et al., 2014 (58)	United States	100	44.30 ± 10.50	100	0.0	45.0	General population	7	Observational	Strong	Weak	Mod e
Zhong et al., 2024 (73)	China	120	20.75 ± 1.25	70.0	NR	100	Students	28	Observational	strong	weak	stron

*Legend: * included in meta-analysis. NR= not reported*

Table 2. EMA study characteristics

Author	**Method**	**Frequency**	**Delivery mode**	**Own device**	**Burst design**	**Incentive sch**
Amrein et al., 2021 (42)	Signal contingent - fixed timing	Multiple times per day	Mobile phone - multiple/other	All	No	Prize draw or
Anderson & Fowers, 2021 (80)	Signal contingent - fixed timing	Daily	Website/online	All	No	Payment per
Anderson, 2020 (82)	Signal contingent - fixed timing	Daily	Website/online	All	No	Payment per
Ashurst et al., 2018 (43)	Signal contingent - random timing	Multiple times per day	Multiple	Some	No	Other
Berge et al., 2018 (66)	Event contingent	NA (event contingent)	Mobile phone - app	None	No	NR
Boh et al., 2016 (74)	Event contingent	NA (event contingent)	Mobile phone - app	All	No	Flat paymen completion
Carels et al., 2019 (44)	Signal contingent - fixed timing	Daily	Website/online	All	No	Flat paymen completion
Clancy et al., 2020 (45)	Signal contingent - fixed timing	Daily	Website/online	All	No	Multiple
Cnudde et al., 2024 (84)	Multiple	Multiple times per day	Mobile phone - app	Some	No	Multiple
Comulada et al., 2018 (65)	Signal contingent - fixed timing	Daily	Mobile phone - app	None	No	NR
Disabato et al., 2022 (81)	Signal contingent - fixed timing	Daily	Website/online	NR	No	NR

Author	Method	Frequency	Delivery mode	Own device	Burst design	Incentive sch
Dunton et al., 2017 (69)	Signal contingent - random timing	Multiple times per day	Mobile phone - app	NR	No	NR
Elliston et al., 2017a (70)	Multiple	Multiple times per day	Mobile phone - app	No	No	Flat paymen completion
Elliston et al., 2017b (46)	Multiple	Multiple times per day	Mobile phone - app	No	No	Flat paymen completion
Finkelstein-Fox et al., 2020 (67)	Signal contingent - fixed timing	Daily	Website/online	All	No	Course credit
Franja et al., 2021 (59)	Multiple	Multiple times per day	Mobile phone - app	No	No	Flat paymen completion
Fredrickson et al., 2021 (78)	Signal contingent - fixed timing	Daily	NR	NR	No	NR
Headrick et al., 2022 (71)	Signal contingent - fixed timing	Weekly	Website/online	All	No	Prize draw
Hill et al., 2023 (47)	Signal contingent - fixed timing	Daily	Website/online	All	No	Flat paymen study comp credits
Hochgraf et al., 2024 (77)	Signal contingent - fixed timing	Multiple times per day	Website/online	All	No	NR
Inauen et al., 2016 (106)	Signal contingent - fixed timing	Multiple times per day	Website/online	All	No	Prize draw &
Jeffers et al., 2020 (12)	Signal contingent - fixed timing	Multiple times per day	Website/online	All	No	Payment per
Jones et al., 2007 (48)	Signal contingent - fixed timing	Daily	Pencil-and-paper	None	No	Flat paymen study comple
Jones et al., 2022 (41)	Multiple	Multiple times per day	Website/online	All	No	NR
Jones et al., 2024 (60)	Signal contingent - fixed timing	Multiple times per day	Website/online or Pencil-and-paper	All	No	No
Kilb et al., 2023 (85)	Signal contingent - fixed timing	Multiple times per day	Website/online & Mobile phone - app	All	No	Payment per
Kos et al., 1997 (61)	Signal contingent - fixed timing	Multiple times per day	Mobile phone - app	None	No	Flat paymen completion

Author	Method	Frequency	Delivery mode	Own device	Burst design	Incentive sch
Li et al., 2020 (76)	Signal contingent - fixed timing	Daily	Website/online	All	No	NR
Liao et al., 2018 (49)	Signal contingent - random timing	Multiple times per day	Mobile phone - app	Some	No	NR
Lin et al., 2021 (50)	Signal contingent - random timing	Multiple times per day	Website/online	None	No	Flat paymen completion
Mason et al., 2019 (72)	Signal contingent - random timing	Multiple times per day	Mobile phone - app	Some	No	NR
Masterton et al., 2023 (51)	Signal contingent - fixed timing	Multiple times per day	Mobile phone - app	All	No	Payment pe credits
Meule et al., 2019 (62)	Signal contingent - fixed timing	Daily	Mobile phone - app	NR	No	Flat paymer study comp credits
Moore et al., 2022 (79)	NR	NR	Website/online	All	No	Payment per
Moss et al., 2021 (68)	Signal contingent - fixed timing	Daily	Website/online	All	No	NR
O'Connor et al., 2015 (107)	Signal contingent - fixed timing	Daily	Website/online	All	No	Course credit
O'Connor et al., 2009 (52)	Signal contingent - fixed timing	Daily	Pencil-and-paper	None	No	NR
Papadakis et al., 2021 (53)	Multiple	Multiple times per day	Mobile phone	None	No	Flat paymen completion
Powell et al., 2017 (54)	Signal contingent - fixed timing	Hourly	Mobile phone - app	None	No	Flat paymer study comple
Richard et al., 2019 (55)	Signal contingent - fixed timing	Multiple times per day	Mobile phone - app	All	No	Flat paymen completion &
Riley et al., 2019 (108)	Signal contingent - fixed timing	Daily	Website/online	All	No	Course credit
Riley et al., 2022 (56)	Signal contingent - fixed timing	Daily	Website/online	All	No	Course credit
Roefs et al., 2019 (63)	Multiple	Multiple times per day	Mobile phone - app	All	No	Flat paymen completion

Author	Method	Frequency	Delivery mode	Own device	Burst design	Incentive sch
Schultchen et al., 2019 (57)	Signal contingent - fixed timing	Multiple times per day	Mobile phone - app	All	No	Flat paymen completion &
Skoyen et al., 2013 (64)	Signal contingent - fixed timing	Daily	Website/online	All	No	Prize draw
Small et al., 2013 (86)	Signal contingent - fixed timing	Daily	NR	NR	No	NR
Wen et al., 2022 (83)	Signal contingent - fixed timing	Daily	Website/online	All	No	Flat paymen completion
Zenk et al., 2014 (58)	Signal contingent - random timing	Multiple times per day	Mobile phone - app	All	No	Flat paymen completion
Zhong et al., 2024 (73)	Signal contingent - fixed timing	Multiple times per day	Mobile phone - app	NR	No	NR

Legend: NR not reported.

Table 3. Summary of the meta-analyses

	Predictor	Coeff	k	β	SE	z-val	p	LL	UL	Tau2
Healthy	NA/Stress/cog - Standardized	Stand	8	-0.007	0.003	-2.20	.006	-0.014	-0.008	0
		logOR	2	NA						
	Positive Affect	Stand	2	NA						
		logOR	1	NA						
Unhealthy	NA/Stress/cog - Standardized	Stand	6	0.054	0.023	2.29	.022	0.008	0.099	0.0027
		logOR	5	0.469	0.281	1.67	.095	-0.081	1.020	0.372
	Positive Affect	Stand	2	NA						
		logOR	2	NA						

Legend: ** p < .01. *** p < .001. NA: not available

Table 4. Meta-regressions on the influence of potential moderators.

Meta-reg	Moderator	Coefficient	SE	Z	p
Healthy - Neg - Stand	Duration of EMA	-0.0002	0.0004	-0.46	.648
	Age	-0.0005	0.0001	-3.70	<.001

	Women (%)	0.0002	0.0003	0.65	.513
	Adherence	0.0002	0.0005	0.44	.664
Unhealthy - Neg - Stand	Duration of EMA	0.0038	0.0014	2.80	.005
	Age	0.0034	0.0031	1.11	.269
	Women (%)	-0.0016	0.0010	-1.67	.096
	Adherence	-0.0014	0.0036	-0.38	.705
Unhealthy - neg - OR	Duration of EMA	0.0338	0.0917	0.37	.713
	Age	-0.0104	0.0343	-0.30	.761
	Women (%)	0.0057	0.0218	0.26	.793
	Adherence	0.0439	0.0303	1.45	.147

Table 5. Sub-construct sensitivity analyses and Q-between tests.

Outcome	Estimate	Sub-construct	k	n total	beta / logOR	SE	p	CI lower	CI upper	PI lower	PI upper	I² (sub)
Healthy	Standardized	Negative affect	2	248	-0.010	0.006	0.071	-0.021	0.001	-0.021	0.001	0%
Healthy	Standardized	Stress	4	796	-0.006	0.007	0.42	-0.021	0.009	-0.03	0.018	56%
Healthy	Standardized	Cognition-Craving	3	496	-0.009	0.003	0.004	-0.014	-0.003	-0.015	-0.002	1%
Unhealthy	Standardized	Negative affect	2	617	0.072	0.064	0.262	-0.054	0.198	-0.144	0.288	98%
Unhealthy	Standardized	Stress	2	622	0.002	0.075	0.983	-0.145	0.149	-0.228	0.231	62%
Unhealthy	Standardized	Cognition-Craving	2	339	0.057	0.036	0.115	-0.014	0.127	-0.064	0.177	98%
Unhealthy	logOR	Negative affect	4	874	0.306	0.307	0.319	-0.296	0.908	-1.025	1.637	99%
Unhealthy	logOR	Stress	1	100	0.130	0.06						NA
Unhealthy	logOR	Cognition-Craving	1	60	1.190	0.34						NA

Note: Q_M not performed for logOR as only 1 sub-construct with k>=2 is present.