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Action and Inaction in Multi-Behavior Recommendations: A Meta-Analysis of Lifestyle Interventions

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Abstract

This meta-analysis examined theoretical predictions about the effects of different combinations of action (e.g., start an exercise regime) and of inaction (e.g., reduce screen time, rest in between weight lifting series) recommendations in smoking, diet, and physical activity multiple-domain interventions. The synthesis included 150 research reports of interventions promoting multiple behavior domain change and measuring change at the most immediate follow-up. The main outcome measure was an indicator of overall change that combined behavioral and clinical effects. There were two main findings. First, as predicted, interventions produced the highest level of change when they included a predominance of recommendations along one behavioral dimension (i.e., predominantly inaction or predominantly action). Unexpectedly, within interventions with predominant action or inaction recommendations, those including predominantly inaction recommendations had greater efficacy than those including predominantly action recommendations. This effect, however, was limited to interventions in the diet and exercise domains, but reversed (greater efficacy for interventions with predominant action vs. inaction recommendations) in the smoking domain. These findings provide important insights on how to best combine recommendations when interventions target clusters of health behaviors.

Keywords

multiple behavior change; physical activity; diet; smoking cessation; lifestyle intervention; action goals; multiple behavior domain change; multi-domain intervention; multi-behavior intervention; action; inaction

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Designing interventions to modify co-occurring health risks (Fine, Philogene, Gramling, Coups, & Sinha, 2004; Klesges, Eck, Isbell, Fulliton, & Hanson, 1990; Pronk et al., 2004) appears to be a practical way of promoting wellbeing and reducing the numerous diseases associated with smoking, poor diet, and physical inactivity. Multiple-behavior-domain interventions promote change in two or more health behavior domains, such as smoking

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cessation and exercise, with the recommended changes being delivered within a limited time frame (Goldstein, Whitlock, & DePue, 2004; Nigg, Allegrante, & Ory, 2002; Nigg & Long, 2012; Prochaska, Nigg, Spring, Velicer, & Prochaska, 2010; Prochaska & Prochaska, 2008). Interventions target multiple behaviors for two reasons. The first reason is practicality. Delivering an intervention usually entails expenses for the provider (e.g., marketing and promotion, incentives, space, staffing), and requires time, money, and energy for the client. The second reason is that disease risks exist in clusters. Since unhealthy dietary habits, smoking, and physical inactivity all contribute to diabetes and cardiovascular disease (Kulzer et al., 2009), it is only natural to attempt to address all risks simultaneously.

As multiple-behavior interventions are primarily driven by practical considerations, there is limited understanding of how best to **combine** the behavior recommendations. Behavior recommendations are the goals, or behavioral categories, targeted in an intervention and determine the specific prescribed behaviors (e.g., walk for at least 30 minutes per day) included in an intervention. Although behavior recommendations may differ on a number of dimensions, one important distinction is whether a recommendation addresses an action (e.g., starting an exercise regime) or an inaction (e.g., achieving a low fat intake; resting to reduce stress). In the case of lifestyle interventions, smoking generally involves the prescription to decrease a behavior, although vaping may be introduced as an active substitute. Physical activity recommendations generally involve actions, although resting in between sessions (an inaction) is often recommended as well. Dietary behaviors vary widely from reducing caloric consumption (an inaction), to reducing fat intake (an inaction), to increasing vegetable and fruit intake (two actions). Thus, the design of a multi-behavior intervention offers an appropriate context to study the efficacy of different combinations of recommendations.

The current article presents a meta-analysis to determine how to most effectively combine recommendations of action with recommendations of inaction. We synthesized the results of interventions targeting change in the behavioral domains (broad risk factors being targeted) of diet, exercise, or smoking to determine whether more homogeneous combinations of recommendations for actions or inactions (i.e., the prescribed dietary, exercise, or smoking behaviors, such as beginning nicotine replacement therapy) are more efficacious than less homogeneous ones. Answers to these questions may further health psychology and clinical practice because multiple domain change interventions target a broad range of critical health problems, from cardiovascular disease (Ebrahim et al., 2011; Ketola, Sipila, & Makela, 2000), to depression (Kieffer et al., 2012; Naar-King, Parsons, Murphy, Kolmodin, & Harris, 2010; Prochaska et al., 2008), to posttraumatic stress disorder (Feldner, Smith, Monson, & Zvolensky, 2013; McFall et al., 2010, Rosenbaum et al., 2011), to diabetes care (Norris et al., 2002; Conn et al., 2008), to addictions (Prochaska, Delucchi, & Hall, 2004; Spring et al., 2009; Ussher, Taylor, & Faulkner, 2012), to sexual risk reduction (Crepaz et al., 2006; Jackson, Geddes, Haw, & Frank, 2011; Kalichman, Cain, Eaton, Jooste, & Simbayi, 2011).

Psychological Dimensions of Action and Inaction

General goals for action and inaction are motivational end states that regulate the pursuit of behavior towards more or less cognitive or motor output (Albarracín et al., 2008, 2011;

Feldman & Albarracín, 2017; Ireland et al., 2015), and provide a useful framework for understanding the effects that different combinations of behavioral recommendations have on intervention efficacy. Once in place, goals for action and inaction can guide subsequent behaviors, such that individuals prefer to be active when goals for action have been activated and to be inactive when goals for inaction have been activated (Albarracín et al., 2008; Albarracín, Wang, & Leeper, 2009; Hepler & Albarracín, 2014). For example, Albarracín et al. (2008; Experiment 1) instilled goals for action or inaction and subsequently gave participants the option of drawing on a piece of paper (i.e., an active task) or resting with their eyes closed (i.e., an inactive task). Setting goals for action can also influence food consumption and impulsive responses. Specifically, priming goals for action has been found to increase food intake relative to an inaction condition (Albarracín et al., 2008; Experiment 2), and showing pictures containing action phrases (e.g., *Go for a walk*) or subliminally presenting action words (e.g., *active*, *go*) have been shown to increase food consumption relative to a control condition (Albarracín et al., 2009). Likewise, instilling action has been shown to lead to more false alarms in a go-no go task than instilling inaction (Hepler, Wang, & Albarracín, 2012), and inaction goals have been linked to inhibitory responses using event-related potentials (Hepler & Albarracín, 2013). All in all, these findings suggest that the same cognitive process can trigger a variety of both productive and impulsive active or inactive behaviors, leading to the possibility that attempts at changing one of these behaviors will facilitate or hinder change in other behaviors in the set. If different actions occur in response to an action goal, recommending health-relevant actions or inactions may produce benefits that go beyond the particular recommended behavior and affect the efficacy of the overall set of health recommendations.

The dimensions of action and inaction are linked to the processes of action initiation and inhibitory control of behavior. Action preparation, for example, is at least in part an executive process that enables an organism to orchestrate a unitary response (Jennings & van der Molen, 2005). This preparation is effortful and involves executive mechanisms common to a variety of tasks. In the same way, inhibitory processes appear to generalize across tasks and behaviors. An internal meta-analysis of 18 experiments conducted by Tuk, Zhang, Sweldens (2015) showed that exerting self-control in one domain (i.e., controlling thoughts, attention, food consumption, or emotions) improves self-control in other domains, such as unhealthy food consumption. These conclusions are consistent with Berkman, Burklund, and Lieberman’s (2009) findings that inhibitory signals originate in the same areas of the brain across various tasks, suggesting a general inhibitory control response. In Berkman et al.’s work, intentionally inhibiting a motor response (in a go/no-go task) produced activation of the inhibitory network in the right prefrontal cortex, which in turn resulted in the unintentional inhibition of emotional responses in the amygdala.

The common processes underlying action preparation in different contexts suggest that a particular health recommendation of action is likely to facilitate compliance with other action recommendations. Likewise, the common processes of inhibitory control in different behaviors suggest that a particular health recommendation of inaction is likely to facilitate compliance with other inaction recommendations. Based on this notion, we predict that a more homogeneous set of health recommendations of action will maximize action initiation across the set of recommended health behaviors. Thus, recommending increases in daily activity may tap action initiation processes that favor eating more fruits and vegetables as well. Likewise, a set of health recommendations of inaction may maximize inhibitory processes across a set of recommended health behaviors. If so, recommending a decrease in fat intake may tap inhibitory processes that favor reducing sugar intake as well.

Current Meta-Analysis

In our meta-analysis, reports were required to include a pretest assessment to allow for the examination of change over time, and the final database included 216 multiple behavior change intervention groups, which provided approximately 74,000 participants. We hypothesized that interventions would be more efficacious when recommendations were more homogeneous in the action/inaction dimension. To isolate the effect of the combination of recommendations, analyses were conducted to examine whether the balance of action and inaction recommendations correlated with the use of specific intervention strategies. The meta-analysis sought to control for factors that may be confounded with recommendation combinations. For example, interventions based on social cognitive theory (Bandura, 1986, 1989, 1994; Carver & Scheier 1981, 1982) usually contain components to increase self-efficacy, prompt goal setting and provide feedback on performance. Therefore, our meta-analysis controlled for techniques, as well as for other factors including the lifestyle domains addressed in the interventions.

Method

Review and Inclusion Criteria

Computerized searches of the MEDLINE and PsycInfo databases were conducted to locate reports published in English using a number of keywords for intervention, including *Intervention, Health education, Persuasion, Recommendation, Treatment, Educational program, Rehabilitation, Counseling outcomes, Treatment effectiveness evaluation, Treatment compliance, Health promotion, Behavior change,* and *Randomized trial*. Multiple behavior change interventions were identified by entering these keywords in combination with keywords for interventions promoting change in (a) diet, (b) exercise, and (c) smoking. Diet interventions were searched using the keywords *Binge eating, Body image, Bulimia, Caloric intake, Craving, Diet, Dietary restraint, Eating behavior, Eating disorders, Fat intake, Food intake, Fruit intake, Metabolism disorders, Healthy nutrition, Obesity, Sugar intake, Vegetable intake, Weight control, Weight loss,* and *Healthy eating*. To identify exercise interventions, the search terms *Aerobic exercise, Body image, Physical activity, Sport training, Strength training, Weight control, Weight loss, Lack of exercise, Walking, Gymnastics, Going to gym, Running, Biking, Work out,* and *Physical inactivity* were used. To locate smoking interventions, the keywords *Tobacco* and *Smoking* were used. Additional strategies were used to search for published and unpublished work. These strategies included using the same keywords to search *Proceedings* and *Papersfirst* for conference titles, emailing the most published authors in our database to request their published and unpublished work, and examining the reference lists of prior reviews and reports included in our database to identify other possible reports for inclusion.

Once our search for relevant reports was complete, reports that met the following criteria were selected for inclusion:

1. ***Presence of at least two groups.*** To be considered for inclusion, reports had to include a control group that did not expose participants to any kind of intervention at the time of the study (e.g., wait list group, no-intervention group), or an intervention group promoting change in a single behavior domain, or a usual care group. In addition, reports were required to include an intervention group targeting multiple behavior domains.

2. ***Presence of an intervention promoting change in more than one behavior domain.*** Reports were required to include an intervention promoting change in diet, exercise, or smoking. Because the focus of this meta-analysis was to examine the impact of various combinations of recommendations on the efficacy of interventions promoting change in multiple lifestyle domains, only reports that included an intervention targeting at least two of these three domains were included. (Some of the studies included single-domain controls, however, which were retained.)

3. ***Presence of information about the recommendations.*** Reports were included that provided a description of the intervention that permitted determining the number of behavioral recommendations and whether the recommendations were for action or inaction. Control groups were often excluded from our analyses because the description of the usual care group did not provide enough detail to code for the number of behavioral recommendations ($k = 96$).

4. ***Presence of appropriate statistics.*** Only reports that provided information to calculate effect sizes representing change over time were included. Thus, reports without a pretest ($k = 140$) were excluded. In some case, authors of the synthesized reports provided supplementary information to calculate effect sizes.

Coding of Study Characteristics

Two independent raters coded relevant characteristics of the reports, as well as the methods used in the studies, as described below. Intercoder-reliability coefficients (kappas for categorical variables and simple correlations for continuous variables) are summarized in Table 2. When disagreements occurred between coders, they were resolved by discussion and further examination of the reports.

Description of the report.—For each report, general characteristics of the report were coded, including the (a) publication year, (b) the first authors' institution (e.g., college, research center), (c) the first authors' institutional area (e.g., psychology, community/public health, medicine), (d) publication type (e.g., journal article), (e) location of the intervention, and (f) language of the intervention.

Behavior change domains.—Reports were coded for whether the intervention promoted change in the primary domains of (a) diet, (b) exercise, and (c) smoking, as well as other secondary domains of (d) alcohol use, (e) medication adherence, and (f) cancer screening.

Interventions were coded as multiple behavior domain interventions when they targeted change in more than one domain (e.g., exercise and diet), whereas those targeting change in a single domain (e.g., exercise) were classified as single behavior domain interventions.

Recommendations for behavioral action and inaction.—Recommendations are the goals, or broad behavioral categories, targeted in an intervention and are reflected in the specific prescribed dietary, exercise, or smoking behaviors (e.g., reducing sedentary time) made by an intervention. The total number of recommendations made in the intervention was coded by counting the total number of primary goals (e.g., reduce calories, increase fruit and vegetable intake, increase physical activity) that interventions were described as targeting. For each recommendation, we also coded (a) the domain of the recommendation (e.g., diet, exercise) and (b) whether the recommendation was for action (e.g., increase physical activity; increase fruit intake) or inaction (e.g., reduce fat intake; quit smoking; rest and relax).

For example, Ussher, West, McEwen, Taylor, and Steptoe (2003) targeted change in the domains of exercise and smoking and was coded as presenting two recommendations. The exercise recommendation was coded as an action recommendation because participants were instructed to engage in physical activity for 30-minutes or more on at least 5 days per week, whereas the recommendation for smoking cessation was coded as an inaction recommendation because participants were instructed to stop smoking. Another intervention was coded as making three behavioral recommendations, two for inaction (in the domains of smoking and diet) and one for action (in the domain of exercise), because the intervention recommended that participants quit smoking, reduce cholesterol and saturated fat intake, and walk for at least 20-minutes per day (DeBusk et al., 1994). Finally, Baranowski et al., (2003) targeted change in the domains of diet and exercise and the intervention was coded as including four recommendations for action because the intervention recommended participants to increase physical activity to 60-minutes per day, as well as increase intake of fruits and vegetables, increase the intake of fiber, and increase the intake of water.

An indicator of the balance of action and inaction recommendations was created by classifying interventions as recommending predominately action behaviors, an equal number of action and inaction behaviors, predominately action behaviors. As can be seen in Table 1, there was variability in this index as a function of domain. Samples that received dietary, tobacco use, and exercise recommendations had received predominantly action recommendations, predominantly inaction recommendations, or an equal number.

Intervention strategies.—Intervention strategies are the techniques (e.g., normative arguments, skills training) utilized to promote change in the behavioral recommendations made in an intervention. To rule out potential confounds between these techniques and combinations of recommendations, interventions were coded for the strategies used in each intervention to promote behavior change (see Albarracín et al., 2005). Specifically, interventions were coded for inclusion of (a) attitudinal arguments highlighting the benefits of the recommended behavior (e.g., daily exercise as being good for weight loss), (b) normative arguments about support for the recommended behavior by friends, family members, or partners (e.g., opinion and behavior of one’s social group related to tobacco use), (c) informational arguments, such as educational statements about disease outcomes, (d) threat inducing arguments describing disease risk, (e) arguments designed to enhance perceptions of control, and (f) arguments designed to model behavioral skills necessary to overcome barriers to reaching health targets. Also, interventions were coded for the inclusion of (a) training on behavioral skills necessary to perform the recommended behavior, (b) training on interpersonal skills (e.g., role playing exercises), (c) inclusion of cues to remind recipients to engage in recommended behaviors, (d) identifying and overcoming barriers, (e) analysis of factors contributing to relapse after initial positive change, (f) setting and reviewing past goals, (g) stressing relapse prevention, (h) use of relaxation techniques to reduce anxiety and stress, (i) use of self-monitoring prompts for the recommended behavior (e.g., keeping a record of behaviors), (j) teaching time management, and (k) teaching stress management.

Other characteristics.—Demographics of the sample were coded, including characteristics such as the (a) sample size, (b) percentage of male participants, (c) lowest, highest and mean age, (d) percentage of participants of European, African, Latin, Asian, and Native American descent[1], (e) percentage of participants who completed high school and mean years of education, and (f) percentage of participants with a health condition (e.g., diabetes, obesity).

Characteristics related to the **intervention setup** were coded as well. Each intervention group was coded according to (a) the setting of exposure (i.e., clinic, school, community, mass media), (b) the form of media used to deliver the intervention (i.e., face-to-face interactions, video- or audio-taped materials), (d) the type of facilitator used to deliver the intervention (i.e., professional expert or lay community member), (e) whether exposure to the intervention was to groups or individuals, (f) whether the intervention was described by the authors as culturally appropriate, (g) the location of recruitment (e.g., drug treatment facility, classroom, and hospital), (h) the duration of the intervention in terms of total number of counseling visits, the length of each visit in minutes, and (i) the number of days from the baseline until the end of the intervention.

Finally, to fully describe our sample, reports were coded for characteristics related to the **research design and implementation**. Reports were coded for (a) whether the design was within-subjects or whether the samples were different at pre- and post-test, (b) whether participants were randomly assigned to conditions, (c) the amount of money (in U.S. dollars) received as compensation, (d) the number of days between the intervention and the posttest, (e) the population targeted in the intervention (e.g., mental health patients, smokers, obese adults), and whether the intervention was targeted to a specific (f) ethnic or (g) gender group. We also coded for whether the sample was (i) self-selected (i.e., participants took part in the study on a voluntary basis versus more captive groups, such as participants in classrooms or inpatient hospital settings).

[1]. When ethnicity data were not reported and countries were highly ethnically homogeneous (e.g., the Netherlands, Italy), we obtained the information from population reports from those countries.

Retrieval of Effect Sizes

Two coders independently calculated effect sizes representing change over time. For reports containing more than one measure of a construct, we first calculated effect sizes for each measure and then obtained the average, which we used as the effect size for that particular variable (See B.T. Johnson, 1993). We used Becker's (1988) *g* to indicate a change in pre- to post-test measures, which was calculated by subtracting the mean at posttest from the mean at pretest and then dividing the difference by the standard deviation of the pretest measure. Depending upon the domains targeted in an intervention, effect sizes were calculated to represent changes in outcomes related to diet, exercise, and smoking, as well as other secondary outcomes related to alcohol use, medication adherence, cancer screening, and more general health outcomes. Effect sizes were calculated to represent improvements from a health perspective (e.g., decrease in BMI, increase in fruit intake). Outcomes were assessed using behavioral, clinical, and psychological measures, and we describe typical measures below.

Behavioral measures.—The reports in our meta-analysis included a large variety of behavioral measures. Common measures in the area of *diet* were energy intake (e.g., kcal/week); carbohydrate, protein, fiber, fat, fruit, and vegetable intake (in grams or servings); number of meals per day; whether participants achieved dietary recommendations; whether participants achieved recommendations for fruit and vegetable intake; whether participants checked blood pressure in the past 12 months, and presence of unhealthy eating; presence of overeating. Frequent behavioral measures in the *exercise* domain were whether participants engaged in daily exercise; weekly hours of physical activity; whether participants participated in occupational physical activity, whether participants had regular physical activity; whether participants achieved exercise recommendations; whether participants were sedentary; whether participants reported high impact physical activity; self-monitoring of pulse and blood pressure; self-monitoring with pedometer (daily pace); time spent in physical activity; energy expenditure in physical activity (k/cal); and number of TV hours per day. In the *smoking* domain, the most frequent behavioral measures were current smoking status; number of cigarettes smoked per day (often via diaries); number of years of smoking; whether participants were ex-smokers, and longest quit duration.

Supplementary behavioral measures were often included related to medication and screening in areas of disease associated with diet, exercise, and smoking. Measures related to medication included forgetting to take medication; lack of adherence to the treatment plan; refilling medication; missing medication doses; using medication delivery methods; incorporating the medication regime into daily life; and acquiring social support for adherence (involving friends, attending support groups). Measures assessing screening behaviors included whether participants had a PAP test in the past 2 years; whether participants had a mammogram conducted in the past 2 years; whether participants had other health seeking measures (e.g., specific lab tests) taken within the past year; whether participants had a lipid panel test; and whether participants had a chest X-ray conducted within the past year; whether participants received a dental cleaning within the last 6 months.

Clinical measures.—The most frequent clinical measures assessed body weight in kilograms; body mass index; hip size; waist size; hip/waist ratio; body fat; whether participants were overweight; whether participants were obese; systolic/diastolic blood pressure; triglycerides level; HDL/LDL cholesterol; fasting blood glucose; presence of diabetes; presence of metabolic syndrome; pulse; results from spirometer tests; results from VO2 Max tests; results from chest X-ray; presence of nicotine in blood; lab tests to confirm right dose of medication in blood; lab tests for diabetes; results from PAP tests; results from mammogram reports; results from dental records; and results from colonoscopy reports.

Psychological measures.—Psychological measures assessed beliefs about recommended behaviors for improving clinical outcomes (e.g., *Physical activity is beneficial for lowering blood pressure*; Burke, Giangiulio, Beilin, Houghton, & Milligan, 1999, p. 275); self-efficacy for performing the recommended behavior (e.g., *How confident are you that you will be able to quit smoking for the next 3 months*?; Kinnunen et al., 2008, p. 693); worry about weight (e.g., *How concerned are you about gaining weight as a result of quitting*?; Borrelli & Mermelstein, 1998; p. 622); and knowledge (e.g., ability to identify LDL targets; Lichtman et al., 2003).

Analytic Strategy

Weighted mean effect sizes were calculated to examine change over time for interventions with varying combinations of recommendations for action and inaction, and performed corrections for sample-size bias to estimate the effect size of *d*. Hedges and Olkin's (1985) procedures were used to correct for sample size bias[2], calculate weighted mean effect sizes (*d*), confidence intervals, and estimate homogeneity statistics (*Q*), which test the hypothesis that the observed variance in effect sizes is no greater than that expected by sampling error alone. When designs were between-subjects (pre and post measures were obtained from different participant groups), the variance of effect sizes were calculated following Hedges and Olkin's procedures. When designs were within-subjects, we followed Morris' (2000) procedures to calculate the variance of effect sizes, and the correlation between the pre- and post-test measures was estimated at $r = .5$. After calculating effect sizes for each outcome measure, an average effect size combining behavioral and clinical effects was computed for each case indicating overall change across all study measures.

Analyses were performed using fixed- and random-effects procedures. Effect size calculations and moderator analyses were obtained with SPSS. For fixed-effects models, weighted effect sizes using the inverse of the effect size's variance were used, and this allowed effect sizes from studies with larger sample sizes to carry more weight than effect sizes obtained from studies with smaller sample sizes.[3] When conducting random-effects analyses, a random variance component was added to the variance of each effect size, and then the inverse variance was recalculated prior to weighting the effects sizes. Analyses controlled for the effects of intervention duration by including this variable as a covariate in

2. When the *N* at the pretest differed from the *N* at the posttest, the smaller *N* was used.
3. The distribution of weights was skewed due to sixteen cases with large weights. To correct for this, we curved weights over 999 to fit between the range of 1,000–2,00. Because results were similar regardless of whether we used the original or curved weights, we present only results from analyses using the curved weights.

the model, along with whether the sample was self-selected.[4] In addition, because recommendation content likely differs based on the domains targeted (e.g., smoking interventions may be more likely to recommend behavioral inaction), all analyses controlled for the domains that the intervention targeted by including this potential confound.

Analysis of variance (ANOVA) procedures were used to examine the effects of different combinations of recommendations on overall change. As the primary focus of this meta-analysis was to determine the effect of various combinations of recommendations in multiple behavior domain change interventions, these analyses excluded no-intervention control groups ($n = 39$) and single behavior domain intervention groups ($n = 15$). When conducting analyses, the inverse of the variance of the effect size being predicted was entered as a weight. The significance of the between-category-homogeneity index *QB* was examined, which is a sum of squares analogous to an *F* ratio but distributed as a chi-square, to determine whether effects were significant. *QBs* were obtained for the main effect of the balance of action and inaction recommendations included in the intervention. Follow-up tests were conducted to determine whether the *d*s differed significantly across specific cells as indicated by the subscripts in Table 3. Results focus on the findings from fixed-effects analyses, which are more powerful and produce narrower confidence intervals (Rosenthal, 1995; Wang & Bushman, 1999).

Results

Description of Database

The data (https://osf.io/jfawz/) includes 150 reports, which provided 216 intervention groups recommending multiple behaviors. Of the 150 reports, 50 provided a single data set, 80 provided two data sets, 16 provided three data sets, and 3 provided four data sets. Table 2 presents information about the included reports, as well as the intervention participants, recommendations, strategies, and methods, and presents information about only the multiple behavior intervention groups included in the database. As can be seen, most of the studies were published around 2002, and the median sample size was about 86 participants. Thirty-one countries were represented in our database. The majority of studies were conducted in the United States, and of the studies conducted in the U.S., 24 states were represented but California provided more groups than any other state.

All interventions included recommendations targeting change in exercise, dietary, or smoking behaviors. Interventions less frequently included recommendations targeting change in other behaviors as well, such as alcohol use, medication adherence, and cancer screening. On average, multiple behavior change interventions included 3.41 ($SD = .86$, Range = 2–5) recommendations. The reports examined were diverse, in terms of the participants, intervention set-up, and research design and implementation. The sample consists of approximately 74,000 participants, samples comprised both females and males, and participants were on average middle age. On average, 63% of participants were of European descent, 47% of participants completed high school, and 92% were described as

[4]We controlled for whether samples were self-selected, as self-selected samples were associated with stronger improvements in overall change than samples that were not self-selected, fixed-effects $QB = 91.99$, $p < .001$, $k = 216$.

having a risk factor, a precursor to a health condition, or a health condition at pre-intervention. The samples included individuals at-risk or with a history of obesity, type-1 or type-2 diabetes, coronary heart disease, chronic kidney disease, congestive heart failure, hypertension, and high cholesterol. Interventions were most frequently delivered in clinics, although they were also conducted in schools and workplaces, as well as through mass media. Most interventions were presented face-to-face (86%), involved more resource demanding strategies (71%), exclusively used an individual delivery format in 44% of the cases, and exclusively used professional experts as facilitators in 69% of cases. On average, interventions lasted approximately 18 hours.

Finally, there was variability in research design and implementation across studies. All studies included pre- and post-test measures. The majority of the studies used within-subject designs, although some studies did use different participants at pre- and posttest. The assignment of participants to study condition was done at random in 87% of cases and participants were compensated on average US $55. The mean length of time between the intervention and the posttest was slightly over three months. The majority of interventions were targeted to a specific population, such as a population with a particular health condition or risk factor (e.g., women with coronary heart disease), and samples were frequently self-selected.

Average Intervention Effect Size

A weighted-mean average of overall change was obtained and tested for variability among effect sizes. For interventions recommending multiple behaviors, the average effect size was $d. = .17$ (95% $CI = [.16, .18]$, $Q(216)$ for homogeneity = 2,829.83, $p < .001$ according to the fixed-effects model, and $d. = .23$ 95% $CI = [.19, .26]$, $Q(216)$ for homogeneity = 346.47, $p < .001$ according to the random-effects model. Given our hypotheses and the observed heterogeneity, analyses were conducted to determine whether the balance of action and inaction recommendations included in the interventions accounted for a significant amount of this variability. Exploratory analyses were conducted to ascertain if various combinations of action and inaction recommendations correlated with the inclusion of specific intervention strategies.

Although the primary focus of this meta-analysis was determining the effects of different combinations of behaviors, the weighted-mean effect sizes for the single behavior intervention and no-intervention control groups were calculated for comparison purposes as well. In interventions recommending a single behavior, the average effect size according to the fixed-effects analysis was $d. = .07$ (95% $CI = .02, .13$; $Q(14)$ for homogeneity = 48.66, $p < .001$), and the average effect size according to the random-effects analysis was $d. = .11$ (95% $CI = -.04, .24$; $Q(14)$ for homogeneity = 11.06, $p > .05$). For no-intervention control groups, the average effect size from the fixed-effects analysis was $d. = .06$ (95% $CI = -.01, .12$; $Q(38)$ for homogeneity = 267.48, $p < .001$), and $d. = .04$ according to the random-effects analysis (95% $CI = -.06, .13$; $Q(38)$ for homogeneity = 31.40, $p > .05$).[5]

[5]The weighted-mean effect sizes for the multiple behavior intervention, single-behavior intervention and no-intervention control groups have been reported elsewhere (Wilson et al., 2015). However, the main analyses reported in this article have not been included in any other article.

Effect of the Balance of Recommendations for Action and Inaction

To test the hypothesis that multiple behavior interventions would be more effective when behavioral recommendations were more homogeneous in the action/inaction dimension, the effect of the balance of action and inaction recommendations on overall change was examined. We used the indicator of the balance of action and inaction recommendations created by classifying interventions as recommending predominately action behaviors, an equal number of action and inaction behaviors, predominately action behaviors.[6]

Table 3 presents the weighted average effects for the index of action and inaction recommendations on overall change. As shown by the QB statistics in Table 3, the index of action and inaction recommendations significantly predicted overall change. As hypothesized, interventions that predominantly included recommendations for either action or action were more effective than recommendations that were equal in the inclusion action/ inaction recommendations. In fact, interventions with equal numbers of action and inaction did not differ significantly from single-behavior programs or control conditions (see subscripts e and d in Table 3). Furthermore, a predominance of inaction recommendations produced more change than a predominance of action recommendations. Importantly, results were the same when the covariates of intervention domain, duration, and sample self-selection (volunteers vs. captive samples) were included (see the second horizontal panel of Table 3).

The analyses in Table 3 were consistent with expectations but could be spurious results of confounds between the action/inaction dimension and the domains of the interventions. Therefore, we replicated these results by separating included domains rather than by covarying out domain. These analyses appear in the last horizontal panel of Table 3 (Model 3) and replicated the result that more homogeneous combinations lead to greater efficacy in each domain. The previously observed advantage of the predominance of inaction versus of action was present in the diet and exercise domains. However, in the case of smoking, predominant action recommendations were more efficacious than predominant inaction recommendations.

Exploratory Analyses of Potential Confounds for Inaction Combinations

Analyses were conducted to determine whether various proportions of action and inaction recommendations correlated with the inclusion of specific intervention strategies. As can be seen in Table 4, overall the use of specific intervention strategies was not correlated with the index of action and inaction recommendations included in an intervention. Overall then, these additional analyses help to strengthen our conclusion that the effects in Table 3 are due to the combination of recommendations for action and inaction, rather than spurious associations with strategies of behavior change.

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6. Attempts were made to compute a more precise measure of the balance of action and inaction recommendations. Because little was gained due to empty cells in our analyses, we present only results from this measure of the balance of action and inaction recommendations.

Assessment of Publication and Eligibility Biases

We checked for the presence of a major type of bias that leads to the greater likelihood of samples yielding statistically significant findings for publication (see Rothstein, Sutton, & Borenstein, 2005). We used both visual and numerical inspections, i.e., funnel plots using the trim-and-fill method and contour-enhanced funnel plots (see Figure 1), as well as regression-based analyses and selection methods (see Table 5). Publication bias refers to a pattern in which, in the present context, positive and statistically significant (vs. non-significant) findings are more likely to be published. Eligibility bias suggests that statistically significant (vs. non-significant) findings are more likely to be included in a meta-analysis. In the absence of bias, studies with greater precision (smaller standard errors) should be closer to the mean than less precise ones, which should create an even funnel shape around the mean-effect size. If there is bias, the funnel plot should appear asymmetrical and may have gaps in the bottom right-hand or bottom left-hand side of the plot (Sterne, Becker, & Egger, 2005). Trim-and-fill procedures, which go beyond a visual inspection of plots, use a nonparametric method to correct for any funnel-plot asymmetry by removing the smaller samples that cause the asymmetry, re-estimating the mean of the effect sizes, and filling the omitted samples (open circles) to ensure that the funnel plot is more symmetrical [3,4]. Figure 1 presents funnel plots of fixed- and random-effects models using estimators L_0 and R_0.

In the first panel of Figure 1, the *x*-axis corresponds to effect sizes and the *y*-axis to the precision of each study. These funnel plots are based on fixed-effects model and estimators L_0 and R_0, as advised by Duval and Tweedie (2000). Depending on estimation methods, between two and forty-three effect sizes to the left of the mean effect size are filled as missing, a pattern that can be attributable to publication or inclusion bias. As the test of heterogeneity (Cochran's Q) suggests a heterogeneous population, $Q = 593.75$, $p < .0001$, the random-effects model may be more accurate (Hedges & Vevea, 1998). Hence, we drew the funnel plots again using random-effects models (see the third panel of Figure 1) and identified two missing effect sizes to the left of the mean effect size. The missing effects on the left-hand side of the plots may be indicative of publication or eligibility bias. However, they can also reflect other sources of bias and require further assessment.

We then used the contour-enhanced funnel plots with the trim and fill method to investigate the nature of the bias (Borenstein et al., 2009; Peters et al., 2008). The contour-enhanced funnel plots with the trim and fill procedures are designed to distinguish different forms of bias by estimating the statistical significance of the missing studies. The contours delineate three regions: (a) the white-colored region corresponds to *p*-values greater than .10; (b) the gray-colored region corresponds to *p*-values between .10 and .05; (c) the dark gray-colored region corresponds to *p*-values between .05 and .01; and (d) the region outside of the funnel corresponds to *p*-values below .01 (Peters et al., 2008). As shown in the fourth panel of Figure 1, only two missing effect sizes appear in areas of higher statistical significance (i.e., regions farther outside the funnel plot), indicating the likely presence of bias. Nonetheless, the asymmetry of the funnel plot, either visually inspected or statistically tested, is not in an of itself confirmation of publication bias because many methodological aspects of studies can alter the shape of the funnel plot (Lau, Ioannidis, Terrin, Schmid, & Olkin, 2006).

To further assess the presence of bias, we performed several statistical analyses of the funnel plot asymmetry, including Begg and Mazumdar's rank correlation, Egger's *z* test of the intercept, and the precision-effect estimate with standard error (PEESE). Begg and Mazumdar's nonparametric method, which provides rank correlations between the effect size and the standard error, yielded nonsignificant results. Egger's test also assesses the association between standardized effect sizes and precision. If bias is present (i.e., the funnel plot distribution is asymmetric), the intercept of the regression line should be non-zero and significant (Borenstein et al. 2009; Sterne and Egger 2001). This parametric test revealed significant results for the effect size, suggesting bias ($p < .0001$). The Precision Effect Estimation with Standard Error (PEESE) uses a quadratic approximation, rather than a linear term used in the Egger's test, to assess the link between effect size and sample size (Stanley & Doucouliagos, 2014). The PEESE adjusted mean-effect sizes using the random-effects models are considerably deviant from all other results and likely reflect a large amount of sample heterogeneity (Stanley & Doucouliagos, 2014).

Additionally, we used selection methods to assess and adjust for publication biases relating to the size, direction, and statistical significance of effect sizes. Selection methods assume that the probability of publication depends on the *p*-value of an effect size and that different *p*-values tend to have different chances of being included in a meta-analysis (Vevea & Woods, 2005). We used a *p*-method, i.e., the *p*-uniform test (van Assen et al., 2015), and a general selection method, i.e., the sensitivity analysis (Vevea & Woods, 2005). The *p*-uniform tests of publication bias were significant, $p < .0001$, which suggested bias. Vevea and Woods (2005) defined the probability levels of the intervals of the effect sizes' *p*-values that can be assumed to be moderately or severely biased. In our analysis, the adjusted estimate of the severe one-tailed random-effects model shows 0.09 reduction (see Table 5), but all other adjusted mean effect sizes are largely identical to the unadjusted mean effect. Overall, the results suggest bias, but the bias may have little effect on the estimated effect sizes and may not be due to selective publication.

Given the possible presence of publication/eligibility bias, we re-estimated the effect of the action/inaction predominance after adjusting for publication bias. The results appear in Table 6. These analyses revealed bias in the studies with balance action/inaction recommendations as well as the effects produced by a predominance of action recommendations. However, the adjusted *d*s are almost identical to the ones in Table 3, except for the random-effects PEESE estimate and the Vevea and Wood (2005) one-tailed estimation assuming *severe* biases with random-effects models.

Discussion

The purpose of this meta-analysis was to examine whether interventions recommending multiple behavior changes that are more homogeneous on the action/inaction dimension were associated with greater efficacy than interventions that included both recommendation types. The findings of this meta-analysis suggest that the efficacy of interventions recommending multiple behavior changes is greater when interventions include predominant recommendations for either action or action.

Public Health Implications of our Findings

This meta-analysis speaks to the design of effective multiple-domain interventions. Specifically, interventions including a predominance of action or inaction recommendations appear to be associated with the strongest effects. Readers may be interested in estimating the magnitude of change that might result from different combinations of recommendations for behavioral action and inaction. To this end, we applied the average effect sizes for overall change presented in Table 3 to national averages of health statistics obtained from the National Health and Nutrition Examination Survey (National Center for Health Statistics, 2011). Imagine a population of men with a mean of 172 pounds (SD = 46.01). A d. of .35 for interventions including predominately recommendations for inaction implies a loss of about 16 pounds, whereas a d. of .19 for interventions including predominately recommendations for action implies a loss of about 9 pounds.

Although not hypothesized, across all domains, recommendations for behavioral inaction were associated with stronger effects than recommendations for behavioral action. One possibility is that interventions with predominantly inaction recommendations are more effective due to the presence of a common inhibitory mechanism that allows for a change in one behavior involving inaction to lead to inhibitory spillover effects on other recommended action (e.g., Berkman et al., 2009; for other mechanisms, see McDonald, McDonald, Hughes, & Albarracín in press). However, this effect was stronger for interventions in the diet and exercise domains and reversed when interventions also concern tobacco use, for which a predominance of actions was more beneficial than a predominance of inactions. It could be that reducing tobacco use specifically benefits from engaging in activity, whereas diet and exercise benefit from reductions of stress. We believe it is premature to make strong recommendations about whether the recommendation set should be predominantly for actions or inactions, but these findings are intriguing and should be confirmed experimentally.

Limitations of This Meta-Analysis and Future Directions

The current meta-analysis has several limitations that are important to discuss. These limitations are related to the coding of recommendations, the inability to explore why recommendations for action are more effective, the correlational nature of the results, the accuracy of self-reported behaviors, and the generalizability of our findings.

Coding of recommendations.—Recommendations were operationalized as the goals, or broad behavioral categories, targeted in an intervention. Other options for coding behavioral recommendations were considered. For example, although the specific strategies discussed as means to reaching those goals were coded, these were not included in the count of number of recommendations. Operationalizing number of recommendations is complicated by the fact that reports vary in the extent to which interventions are described in detail, with the degree to which specific details on the strategies recommended to assist intervention recipients in modifying the recommended behaviors varying greatly across reports. As such, coding recommendations by counting only the primary goals targeted by intervention likely resulted in a more consistent coding of recommendations across studies. Nonetheless, it is important to acknowledge that interventions often recommend a number of strategies as a means to reach the primary goals set by the intervention. Future research should examine whether the current effects replicate using more precise methods of counting behavioral recommendations.

Correlational nature of our results.—Another limitation of this meta-analysis is related to the correlational nature of the analyses we reported. Although assignment to intervention and control groups was often conducted at random in the reports included in our meta-analysis, the characteristics of participants and an intervention are subject to the preferences of particular researchers and may covary with other study characteristics. Various controls were included in analyses to help rule out spurious findings, however, other co-associations cannot be ruled out completely. Despite this limitation, the conclusions from the current meta-analysis provide insights that fill important gaps in our knowledge of multiple behavior change interventions.

Inaccuracy of self-report.—Another potential limitation of this meta-analysis relates to inaccuracies in self-reported behavior. The accuracy of self-report data can be influenced by a variety of factors, including (a) the length of time respondents are asked to recall behaviors (Schroeder, Carey, & Vanable, 2003; Newell, Girgis, Sanson-Fisher, & Savolainen,1999), (b) lack of knowledge required to answer questions accurately (Newell et al., 1999), and (c) demand characteristics (Beach & Mayer, 1990; Furnham, 1986). Although prior research questions the accuracy of self-report data, it is important to note that many of the studies included in our meta-analysis reported data on objectively measured clinical biomarkers.

Potential sleeper effects.—In the present study, we only considered change at the immediate follow-up point without investigating the possibility of sleeper effects (see Kumkale & Albarracín, 2004). It remains unknown whether the effects of different combinations of recommendations for action and inaction change over long periods of time. For example, action may be easier for intervention recipients to implement initially, but over time behavior change regimes involving only behavioral action may be difficult to maintain. Similarly, intervention recipients may initially have a difficult time initiating or increasing a behavior, but these newly acquired behaviors may become easier to implement over time. This possibility should be examined in future research to further our understanding of the implications of differing combinations of recommendations on intervention efficacy.

Generalizability to the study sample and to the population of all possible studies.—This is the largest meta-analysis to examine the effects of different combinations of recommendations in interventions promoting change in multiple lifestyle behaviors. As such, our findings are likely to be the most generalizable to date and suggest that interventions are most effective when they recommend changes that involve only behavioral action. Although the main effect for the balance of action and inaction recommendations remained significant in our random-effects model, the number of significant follow-up tests declined. In the future, a meta-analysis with a sufficiently large number of effect sizes may allow for the estimation of population variance and establish our findings in the broader universe of all possible studies.

Conclusion

In this research, our objective was to examine the efficacy of interventions recommending multiple behavior changes as a function of how homogeneous the recommendations are on the action/inaction dimension. Therefore, it seems advisable to more directly test these predictions in a randomized controlled trial. Based on these findings, interventions that recommend increasing physical activity may be best if accompanied with recommendations to eat fruits and vegetables than with recommendations to decrease fat intake. Correspondingly, interventions that recommend quitting smoking may be best if accompanied with recommendations to reduce fat and sugar intake than with recommendations to eat more fruits and vegetables.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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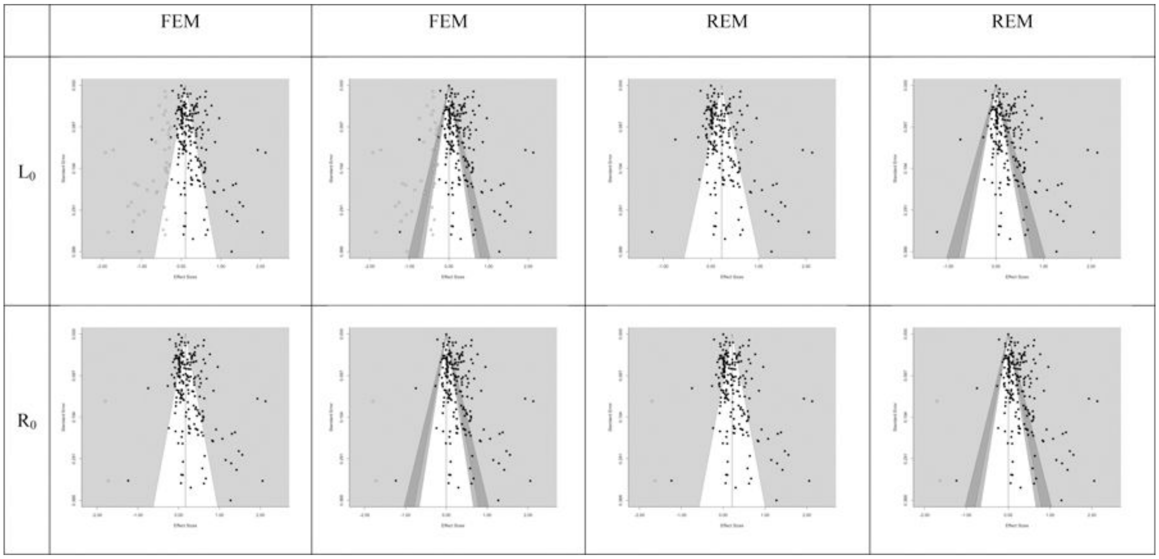

Figure 1.
Overall funnel plots using fixed- and random-effects models with L_0 and R_0 estimation.

Table 1

Presence of Action and Inaction Recommendations by Behavior Change Domain

Behavior domain	Predominantly Inaction	*k* (%) Equal	Predominantly Action
Diet			
Yes	48 (16)	149 (50)	102 (34)
No	2 (9)	19 (86)	1 (5)
Exercise			
Yes	46 (15)	165 (53)	102 (33)
No	4 (50)	3 (38)	1 (13)
Tobacco use			
Yes	47 (25)	118 (54)	20 (11)
No	3 (2)	50 (37)	83 (61)

Table 2

Descriptive Statistics

Variable	Multiple behavior groups (*k* = 216)
General characteristics of the reports	
Publication year (*r* = 1)	
M	2002.24
Mdn	2003
SD	6.70
k	216
% Source type (κ = 1)	
Journal article	97.2(208)
Conference proceeding	.0(0)
Doctoral dissertation	2.8(6)
Master's thesis	.0(0)
% Academic affiliation (κ = .91)	
University	39.8(86)
College	4.2(9)
Research center	2.4(44)
Hospital or health center	16.2(35)
Medical school	15.3(33)
Other	4.1(11)
% Institutional area (κ = 1)	
Psychology	9.3(20)
Epidemiology	4.6(10)
Community/Public health	6.9(15)
Medicine	58.8(127)
Education	2.8(6)
Other	4.2(9)
Not identified	13.4(29)
% Country (κ = 1)	
United States	48.2(104)
Finland	6.0(13)
United Kingdom	6.0(13)
Other	39.8(112)
% Language (U.S. only; κ = 1)	
English	10.0(216)
Types of intervention strategies	
% Attitudinal arguments (κ = 1)	
Yes	32.4(70)
No	67.6(146)
% Normative arguments (κ = 1)	
Yes	6.5(14)

Variable	Multiple behavior groups (*k* = 216)
No	93.5(202)
% Control arguments (κ = .85)	
Yes	18.5(40)
No	81.5(40)
% Threat arguments (κ = 1)	
Yes	5.1(11)
No	94.9(205)
% Informational arguments (κ = 1)	
Yes	91.7(198)
No	8.3(18)
% Behavioral skills arguments (κ = 1)	
Yes	1.4(3)
No	98.6(213)
% Behavioral skills training (κ = 1)	
Yes	48.6(105)
No	51.4(111)
% Communication skills training (κ = 1)	
Yes	3.7(8)
No	96.3(208)
% Setting of goals or review of past goals (κ = 1)	
Yes	44.4(96)
No	55.6(120)
% Role playing exercises (κ = 1)	
Yes	4.2(9)
No	95.8(207)
% Teaches cues to engage in behavior (κ = 1)	
Yes	5.1(11)
No	94.9(205)
% Training on coping with barriers (κ = 1)	
Yes	18.1(39)
No	81.9(177)
% Relapse prevention training (κ = 1)	
Yes	7.9(17)
No	92.1(199)
% Relaxation training (κ = 1)	
Yes	1.2(22)
No	89.8(194)
% Time management training (κ = 1)	
Yes	3.7(8)
No	96.3(208)
% Teaches self-monitoring prompts (κ = 1)	
Yes	26.4(57)

Variable	Multiple behavior groups (*k* = 216)
No	73.6(159)
% Stress management skills training (κ = 1)	
Yes	13.4(29)
No	86.6(187)
Participant characteristics	
Sample size (*N*) (*r* = 1)	
Sum total	73,858
M	341.94
Mdn	85.50
SD	1,125.39
k	216
Age in years (*r* = 1)	
M	46.22
Mdn	5.00
SD	15.64
k	206
% men (*r* = 1)	
M	46.58
Mdn	47.30
SD	31.30
k	213
% women (*r* = 1)	
M	54.25
Mdn	52.70
SD	31.33
k	213
% high school graduates (*r* = 1)	
M	46.48
Mdn	56.00
SD	35.72
k	87
% with risk factor or health condition at pretest (*r* = 1)	
M	91.53
Mdn	10.00
SD	25.13
k	116
Ethnic decent	
% European (*r* = 1)	
M	62.99
Mdn	75.00
SD	36.55

Variable	Multiple behavior groups (*k* = 216)
k	203
% African ($r = 1$)	
M	29.92
Mdn	4.35
SD	31.55
k	150
% Latin American ($r = 1$)	
M	11.08
Mdn	.00
SD	23.87
k	132
% Asian ($r = 1$)	
M	18.21
Mdn	2.90
SD	35.45
k	134
% North American Indian ($r = 1$)	
M	1.09
Mdn	.00
SD	9.14
k	120
Intervention set-up	
Domains targeted	
% Diet ($\kappa = 1$)	
Yes	96.8(209)
No	3.2(7)
% Exercise ($\kappa = 1$)	
Yes	99.1(214)
No	.9(2)
% Tobacco use ($\kappa = 1$)	
Yes	53.2(115)
No	46.7(101)
% Alcohol use ($\kappa = 1$)	
Yes	9.7(21)
No	9.3(195)
% Medication adherence ($\kappa = 1$)	
Yes	7.4(16)
No	92.6(200)
% Cancer screening ($\kappa = 1$)	
Yes	.5(1)
No	99.5(217)
Number of recommendations ($r = 1$)	

Variable	Multiple behavior groups (*k* = 216)
M	3.41
Mdn	3.00
SD	.86
k	216
% Setting of exposure (κ = 1)	
School	
Yes	7.9(17)
No	92.1(199)
Clinic	
Yes	56.0(121)
No	44.0(95)
Community (street, community center, bar)	
Yes	4.6(10)
No	95.4(206)
Business	
Yes	8.3(18)
No	91.7(198)
Mass media	
Yes	8.8(19)
No	91.2(197)
% Medium of delivery (κ = .97)	
Face to face	
Yes	86.1(186)
No	13.9(30)
% Delivery format (κ = 1)	
Groups	2.4(44)
Individuals	44.0(95)
Both	35.6(77)
% Facilitator (κ = .93)	
Professional expert	69.0(149)
Lay community member	25.9(56)
Both	5.1(11)
% Culturally appropriate intervention (κ = .89)	
Yes	11.1(24)
No	88.9(192)
% Duration of intervention in hours (*r* = 1)	
M	18.46
Mdn	1.00
SD	22.83
k	158

Research design and implementation

% Random assignment to conditions (κ = .97)

Variable	Multiple behavior groups (*k* = 216)
Yes	86.5(187)
No	13.5(29)
Payment received (U.S. dollars; *r* = .93)	
M	55.00
Mdn	2.00
SD	146.44
k	23
Days between intervention and posttest (*r* = .88)	
M	103.90
Mdn	28
SD	143.71
k	195
% Specific population targeted (κ = 1)	
Yes	98.6(213)
No	1.4(3)
% Intervention targeted by ethnicity (κ = 1)	
Yes	11.6(25)
No	88.4(191)
% Intervention targeted by gender (κ = 1)	
Yes	26.9(58)
No	73.1(158)
% Self-selected sample (κ = 1)	
Yes	89.4(193)
No	1.6(23)

Note. *k* = number of cases. *r* = intercoder reliability for continuous variables. κ = intercoder reliability for categorical variables. Parenthetical entries represent k.

Table 3

Overall Change as a Function of the Balance of Action and Inaction Recommendations

	Predominately Inaction	Equal	Predominately Action	QB_2	*k*
Model 1					
k	30	106	80		
Fixed-effects *d.*	$.35_a$	$.11_{bde}$	$.19_c$	395.84^{***}	216
Random-effects *d.*	$.48_a$	$.16_b$	$.23_c$	35.73^{***}	216
Model 2 (controlling for domain, duration, and self-selection)					
k	30	106	80		
Fixed-effects *d.*	$.36_a$	$.10_{bde}$	$.19_c$	378.91^{***}	216
Random-effects *d.*	$.50_a$	$.15_b$	$.23_c$	36.01^{***}	216
Model 3 (by domain, controlling for duration and self-selection)					
I Includes diet domain					
k	99	30	80		
Fixed-effects *d.*	$.37_a$	$.10_{bde}$	$.20_c$	448.57^{***}	209
Random-effects *d.*	$.51_a$	$.15_b$	$.21_c$	40.34^{***}	209
Includes exercise domain					
k	29	105	80		
Fixed-effects *d.*	$.34_a$	$.09_{bde}$	$.18_c$	314.95^{***}	214
Random-effects *d.*	$.51_a$	$.16_b$	$.20_c$	36.01^{***}	214
Includes tobacco domain					
k	28	76	10		
Fixed-effects *d.*	$.26_a$	$.10_{bde}$	$.36_c$	367.36^{***}	114
Random-effects *d.*	$.30_a$	$.16_b$	$.53_c$	29.51^{***}	114

Note. d. = weighted means. No-intervention control groups (*k* = 39, *d.* = .06, confidence interval = −.01, .12) and single-behavior intervention groups (*k* = 15, *d.* = .07, confidence interval = .02, .13) were excluded. Following the means, we present the *QB* for *the action/inaction* index. *QB* = homogeneity coefficient for the difference across levels of a factor, distributed as a chi-square with degrees of freedom equal to the number of factor levels – 1. *d*s with similar subscripts a-c are not significantly different from one another. Subscript d indicates that change is not different from change in the no-intervention control group. Subscript e indicates that change is not different from change in the single-behavior control group. Model 1 is the baseline model without controlling for intervention type, Model 2 includes controls for intervention domain, duration, and self-selection, Model 3 includes controls for duration and self-selection but separates data for subsets when a given domain was included.

* $p < .05$

** $p < .01$

*** $p < .001$.

Table 4

Correlation between Balance of Action/Inaction Recommendations and Intervention Strategies.

Intervention strategy	*r*
Attitudinal arguments	−.12
Normative arguments	−.02
Control arguments	−.10
Threat arguments	.02
Informational arguments	−.13
Behavioral skills arguments	−.04
Behavioral skills training	.05
Communication skills training	.09
Setting of goals or review of past goals	.05
Role playing exercises	.04
Teaches cues to engage in behavior	.00
Training on coping with barriers	−.02
Relapse prevention training	−.08
Relaxation training	−.06
Time management training	−.04
Teaches self-monitoring prompts	−.08
Stress management skills training	−.06
Any skills training	.01

Two of these correlatios were significant at $p < .05$ using conventional significance but were no longer significant after applying the Bonferroni correction for multiple tests.

Table 5

Publication Bias Analyses: Overall Sample of Multi-Behavior Interventions

Mean Effect Sizes	Begg and Mazumdar's test	Egger's test	Trim and fill				PEESE	Puniform		Sensitivity analysis			
d. **[95% CI]**	**Kendall's τ**	***z***	ik^R	**Adj.** d^R **[95% CI]**	ik^L	**Adj.** d^L **[95% CI]**	b_0	**Est. *d* [95% CI]**	**L.pb**	Mod_{1t} **adj. *d***	Sev_{1t} **adj. *d***	Mod_{2t} **adj. *d***	Sev_{2t} **adj. *d***
Fixed-effects models													
.17 [.16, .18]	.02	1.41^{***}	2^a	.17 [.16, .17]	43^a	.10 [.10, .11]	0.14^{***} [0.13, 0.15]	---	-8.85^{***b}	0.16	0.16	0.16	0.16
Random-effects models													
.22 [.18, .27]	.02	6.04^{***}	2^a	.21 [.16, .26]	0^a	.22 [.18, .27]	-0.19^{***} [−0.30, −0.07]	---	-5.16^{***b}	0.17	0.13^c	0.21	0.19

Note. *d.* = weighted mean

95% CI = 95% confidence interval in square brackets

Kendall's τ = rank correlation between standard error and effect size (and one-tailed p value)

Egger's *z* = test of funnel plot asymmetry with standard error as predictor

[a] = Left side of the funnel plot where samples were imputed

[R] = the estimator "R0" was used for estimating the number of missing studies

[L] = the estimator "L0" was used for estimating the number of missing studies

ik = number of trim and fill imputed samples

Adj. *d* [95% CI] = trim and fill adjusted observed mean and trim and fill adjusted 95% confidence interval in square brackets

b_0 = an unbiased effect when taking publication bias into account

L.pb = test of publication bias using the LNP method

[b] = Left side of puniform test were computed

Mod1t adj. *d* = moderate one-tailed selection model's adjusted mean

Sev1t adj. *d* = severe one-tailed selection model's adjusted mean

Mod2t adj. *d* = moderate two-tailed selection model's adjusted mean

Sev2t adj. *d* = severe two-tailed selection model's adjusted mean

[c] = customized weights (i.e., averages of moderate one-tailed selection weights and severe one-tailed selection weights were used

--- = puniform effect size cannot be estimated due to small ksig.

* < .05

** < .01

*** < .001

Table 6

Publication Bias Analyses: Different Combinations of Recommendations

Mean Effect Sizes	Begg and Mazumdar's test	Egger's test	Trim and fill				PEESE	Puniform		Sensitivity analysis			
d. [95% CI]	Kendall's τ	*z*	ik^R	Adj. d^R [95% CI]	ik^L	Adj. d^L [95% CI]	b_0	Est. *d* [95% CI]	L.pb	Mod1t adj. *d*	Sev1t adj. *d*	Mod2t adj. *d*	Sev2t adj. *d*
Fixed-effects models													
Predominantly inaction ($k = 30$)													
.35 [.32, .36]	.15	7.72***	7[a]	.32 [.30, .34]	5[a]	.33 [.31, .35]	0.32*** [0.29, 0.34]	---	---	0.35	0.35	0.35	0.35
Equal ($k = 106$)													
.11 [.10, .12]	.01	9.02***	15[a]	.08 .07, .09]	12[a]	.10 [.09, .11]	0.08 [0.07, 0.10]	---	−0.01^{***b}	0.10	0.09	0.10	0.10
Predominantly action ($k = 80$)													
.19 [.16, .22]	.02	4.28***	0	.19 [.16, .22]	13[a]	.14 [.11, .16]	0.15 [0.11, 0.18]	---	−8.25^{***b}	0.18	0.16	0.18	0.17
Random-effects models													
Predominantly inaction ($k = 30$)													
.48 [.30, .67]	.15	3.03**	7[a]	.31 [.07, .55]	0[a]	.48 [.30, .67]	−0.17 [−0.46, 0.12]	---	---	0.44	0.38[c]	0.49	0.45
Equal ($k = 106$)													
.16 [.11, .20]	.01	4.63***	15[a]	.09 [.03,.15]	0[a]	.16 [.11, .20]	−0.29**[−0.48, −0.10]	---	−0.34^{***b}	0.11	0.08[c]	0.14	0.12
Predominantly action ($k = 80$)													
.23 [.16, .30]	.02	3.00**	0[a]	.23 [.16, .30]	0[a]	.23 [.16, .30]	−0.06 [−0.27, 0.14]	---	−4.91^{***b}	0.17	0.14[c]	0.21	0.19

Note. d. = weighted mean

95% CI = 95% confidence interval in square brackets

Kendall's τ = rank correlation between standard error and effect size (and one-tailed *p* value)

Egger's *z* = test of funnel plot asymmetry with standard error as predictor

[a] = Left side of the funnel plot where samples were imputed

[R] = the estimator "R_0" was used for estimating the number of missing studies

[L] = the estimator "L_0" was used for estimating the number of missing studies *ik* = number of trim and fill imputed samples

Adj. *d* [95% CI] = trim and fill adjusted observed mean and trim and fill adjusted 95% confidence interval in square brackets

b_0 = an unbiased effect when taking publication bias into account

L.pb = test of publication bias using the LNP method

[b] = Left side of puniform test were computed

Mod1t adj. *d* = moderate one-tailed selection model's adjusted mean

Sev1t adj. *d* = severe one-tailed selection model's adjusted mean

Mod2t adj. *d* = moderate two-tailed selection model's adjusted mean

Sev2t adj. *d* = severe two-tailed selection model's adjusted mean

[c] = customized weights (i.e., averages of moderate one-tailed selection weights and severe one-tailed selection weights were used

--- = puniform effect size cannot be estimated due to small ksig.

* < .05

** < .01

*** < .001