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# Meta-Analysis of Patient Education Interventions to Increase Physical Activity among Chronically Ill Adults

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## Abstract

**Objective—**This meta-analysis integrates primary research testing the effect of patient education to increase physical activity (PA) on behavior outcomes among adults with diverse chronic illnesses.

**Methods—**Extensive literature searching strategies located published and unpublished intervention studies that measured PA behavior outcomes. Primary study results were coded. Fixed- and random-effects meta-analytic procedures included moderator analyses.

**Results—**Data were synthesized across 22,527 subjects from 213 samples in 163 reports. The overall mean weighted effect size for two-group comparisons was 0.45 (higher mean for treatment than control). This effect size is consistent with a difference of 48 minutes of PA per week or 945 steps per day. Preliminary moderator analyses suggest interventions were most effective when they targeted only PA behavior, used behavioral strategies (vs. cognitive strategies), and encouraged PA self-monitoring. Differences among chronic illnesses were documented. Individual strategies unrelated to PA outcomes included supervised exercise sessions, exercise prescription, fitness testing, goal setting, contracting, problem solving, barriers management, and stimulus/cues. PA outcomes were unrelated to gender, age, ethnicity, or socioeconomic distribution among samples.

**Conclusion—**These findings suggest that some patient education interventions to increase PA are effective, despite considerable heterogeneity in the magnitude of intervention effect.

**Practice Implications—**Moderator analyses are preliminary and provide suggestive evidence for further testing of interventions to inform practice.

### Keywords

patient education; exercise; meta-analysis; health behavior change

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## 1. Introduction

Health care providers routinely recommend physical activity (PA) to many adults with chronic illnesses. Despite widespread recognition of potential health and well-being benefits of PA, most adults with chronic illnesses remain sedentary. Numerous primary studies have tested diverse patient education interventions to increase PA, and many authors have reviewed these investigations. Most reviews, including meta-analyses, have focused on benefits of PA. The many narrative reviews focused on PA behavior outcomes have examined limited domains of this broad literature, such as computer-tailored interventions [1]; primary care-based interventions [2-5]; environmental interventions [6,7]; mass media-delivered interventions [8]; interventions designed to increase 'lifestyle' PA (versus episodic exercise) [9,10]; interventions targeting older adults [11,12]; studies addressing subpopulations such as African-Americans [13-19]; and intervention studies based on particular theoretical models [20]. Narrative reviews discussing PA behavior outcomes often address very few of the available studies [21-24]. Narrative reviews often rely heavily on previous reviews [19], perhaps because it is difficult to conduct a narrative summary across many studies. Narrative reviews generally do not offer conclusions about the efficacy of interventions because evidence that appears contradictory is difficult to synthesize without quantitative integration [25,26]. This quantitative synthesis meets the current pressing need to integrate this large body of research to inform future research and facilitate behavior change theory development.

Few meta-analyses have examined PA outcomes following patient education interventions. Hillsdon, Foster, and Thorogood [27] synthesized across only 11 primary studies with healthy adults that reported PA outcomes. Conn et al. [11] synthesized intervention studies conducted with healthy and chronically ill aging adults. Ashworth, Chad, Harrison, Reeder, and Marshall [28] further limited their meta-analysis of studies conducted with older adults to comparisons between home- versus center-based interventions. Over a decade ago, Dishman and Buckworth [29] conducted the broadest meta-analysis across 127 studies of healthy and chronically ill adults and children. Fewer than 20% of the studies included in that study focused on persons with chronic illness. The authors reported that intervention effects sizes (ESs) were smaller among studies with chronically ill subjects than in studies with healthy subjects. In contrast, Conn et al. [11] reported that among studies of older adults, interventions targeting samples with specific chronic illnesses reported larger ESs than studies with diverse older adults. No comprehensive meta-analyses have been reported that address PA behavior following patient education interventions among adults with diverse chronic illnesses. This meta-analysis was designed to integrate primary study findings from interventions designed to increase PA behavior among adults with chronic illnesses.

This synthesis addressed the following questions (1) What are the overall effects of interventions to increase PA on PA behavior after interventions? (2) Do interventions' effects on PA behavior vary depending on characteristics of interventions, sample, or methodology? (3) For controlled trials, do control groups' post-test outcome measures different significantly from pre-test values?

## 2. Methods

We used standard quantitative review methods to identify and retrieve potential primary studies, determine study eligibility, reliably code data, synthesize results across primary studies, and interpret findings [30-35].

### 2.1 Inclusion Criteria

English-language reports of PA interventions among chronically ill subjects more than 18 years old were eligible for inclusion. We included studies with both an explicit intervention to

increase PA and a measure of PA behavior following the intervention reported with adequate detail to calculate ESs. We included interventions broadly defined as patient education, that is planned activities which could include teaching, counseling, behavior modification, and exercise practice sessions which could influence subsequent PA behavior. We operationalized PA as any bodily movement that results in elevated energy expenditure beyond basal levels. Exercise, a subset of PA, was defined as structured, planned, and repetitive movement intended to increase fitness. A few studies addressed the broader construct of PA, though most targeted exercise.

Both published and unpublished studies were included. Meta-analyses that only include published studies are likely to overestimate the overall ES because the most consistent difference between published and unpublished studies is the statistical significance of the findings [36]. Small-sample studies were included to synthesize across the broad scope of investigations. Although these studies may lack statistical power, they can contribute important information from difficult-to-recruit subjects or innovative interventions [37]. We weighted studies so that smaller studies had proportionally less impact on aggregate findings. Pre-experimental studies with pre-intervention and post-intervention data to calculate ESs were included because some investigators find it unethical to withhold potentially beneficial treatments, and because novel interventions may be tested initially in these designs [38,39]. These pre-experimental studies were analyzed separately from two group comparisons. We were able to include a broader variety of studies by including studies with multiple designs.

## 2.2 Search Strategies

We used multiple search strategies to ensure a comprehensive search and widen the scope of studies beyond those identified in previously published reviews [36]. The broad searching was an important technique to avoid the bias resulting from narrow searches [37]. An experienced reference librarian conducted searches in MEDLINE, PsycINFO, EMBASE, CINAHL, Cochrane Controlled Trials Register, Database of Abstracts of Reviews of Effectiveness, Healthstar, Combined Health Information Database, Sport Discus, Dissertation Abstracts International, and Educational Resources Information Center. We searched the National Institutes of Health Computer Retrieval of Information on Scientific Projects (CRISP) data of funded studies back to 1973. To ensure comprehensive searches of computerized databases, we employed broad search terms for intervention (adherence, behavior therapy, clinical trial, compliance, counseling, evaluation, evaluation study, evidence-based medicine, health care evaluation, health behavior, health education, health promotion, intervention, outcome & process assessment, patient education, program, program development, program evaluation, self care, treatment outcome, validation study) and PA (exercise, physical activity, physical fitness, exertion, exercise therapy, physical education & training, walking). Computerized author database searches were conducted on principal investigators of funded studies and all authors of eligible studies. Senior authors of eligible studies were contacted to solicit additional papers. Ancestry searches were conducted on all eligible studies and previous reviews. We conducted hand searches for articles from 1970 (or initial year of publication) through 2004 in 215 journals where eligible studies were published, review papers were retrieved, or research team members suggested primary studies might be published. Internet searches were conducted but did not yield any primary studies not located through other mechanisms.

## 2.3 Data Coding

We developed, pilot tested, and refined a coding frame to record results of primary studies and characteristics of sources, study participants, study methods, and interventions. Source characteristics include presence of funding, dissemination vehicle, and year of distribution. Gender, minority, age, socio-economic status, and medical diagnoses were coded to describe subjects. Interventions could include motivational/educational sessions and/or supervised

exercise sessions. Because supervised exercise sessions are a common health behavior change intervention with chronically ill adults, details of supervised exercise sessions were coded as available in study reports (intensity, duration, frequency per week, weeks of supervised exercise). Coded intervention attributes include intervention social context (group vs. individual), weeks over which the intervention was delivered, behavioral target (PA only vs. multiple health behaviors including PA), recommended duration of exercise sessions, recommended exercise frequency per week, recommended form of exercise, and several intervention content attributes (Table 1). We coded data at a micro-level with very specific details to enhance coder reliability. Coding specific details was appropriate because universal definitions of broad categories of interventions (e.g. self-management) are not available. We recorded PA behavior only if the PA measure clearly was separated from any intervention supervised exercise sessions (e.g. cardiac rehabilitation participation). The most distal data collection point was used to calculate ESs since enduring PA behavior changes will most likely lead to health benefits. To establish reliability, two extensively trained coders independently extracted all data. The senior author or other member of the research team resolved any coding discrepancies.

### 2.4 Data Management and Analyses

We used standard meta-analytic approaches for data calculations. We calculated a standardized mean difference (*d*) as each comparison's ES. More favorable outcome scores for treatment groups or at post-test are reflected in a positive *d*. We analyzed single treatment group pre- and post-intervention ESs separately from two-group ESs, but expressed both types of ESs in the raw-score metric to facilitate comparisons. Although both random- and fixed-effects analyses were conducted, we present only random-effects analyses. See Table 2 for details of statistical analyses.

Between-study ES homogeneity was assessed using a conventional heterogeneity statistic (*Q*). Heterogeneity was expected because it is very common among studies of educational and behavior interventions and in studies with diverse methodologies [40]. Recent Cochrane reviews of behavioral interventions have accepted heterogeneity and reported management strategies consistent with our approach [40-43]. We emphasize the random-effects analyses because it is a heterogeneous model that takes into account heterogeneity not fully explained by moderators. The random-effects analyses present a location parameter (mean ES) and quantify residual heterogeneity in a variance component [44]. We explored potential study-level moderators to understand potential sources of heterogeneity [41,44-48]. Finally, we interpreted our findings in the context of heterogeneity discovered. Interpreting the extent to which heterogeneity affects conclusions is more valuable than merely testing for the presence of heterogeneity [45]. Interested readers may consult Conn, Hafdahl, Mehr, LeMaster, Brown, and Nielsen [30] for a more detailed rationale for synthesizing across heterogeneous studies.

We conducted exploratory moderator analyses for two-group comparisons. Moderators were selected based on availability in primary reports and our ability to reliably code the attribute. We could not analyze many of the intriguing potential moderators because primary studies reported them infrequently. For example, we could not effectively analyze the impact of interventions based on social cognitive theory because reports provide too little information about conceptual frameworks and papers that claim to be based on particular theories often provide little evidence that the intervention is consistent with the theory. We grouped interventions into behavior and cognitive categories but did not make other groupings, such as self-management training, because universal definitions are not available and studies often provide insufficient information to accurately categorize studies. Because little previous research has provided a foundation for confirmatory hypothesis testing, the moderator analyses are intended as a hypothesis-generating contribution.

## 3. Results

A total of 213 samples including 22,557 subjects from 163 reports were eligible for the meta-analysis (numbers of subjects should be considered close approximations, given vagaries of primary-study reporting) [55-217]. We calculated independent groups ESs for 17,147 subjects in 129 samples from 105 reports. Pre-post treatment group comparisons were calculated for 146 samples, which included 9,275 subjects from 107 reports. A total of 3,218 subjects from 59 samples reported in 56 papers were included in the pre-post analysis of control subjects. Most reports were published articles, though some dissertations or theses (*s*=18) were included as well as a few presentation reports (*s*=4) and one book chapter (*s*=number reports, *k*=number comparisons). Most reported on studies with financial support (*s*=111). Only 21 reports appeared before 1990, and 73 were disseminated in 2000 or more recently.

Studies recruited samples with diverse chronic illnesses: mixed medically and surgically managed cardiac diseases (s=22), hypertension (*s*=10), peripheral vascular disease (*s*=11), myocardial infarction (*s*=10), coronary artery disease (*s*=12), coronary artery bypass graft (*s*=6), heart failure (*s*=6), mixed medically managed cardiac diseases (*s*=2), coronary angioplasty (*s*=1), type 2 diabetes (*s*=22), type 1 diabetes (*s*=6), mixed diabetes subjects (*s*=4), rheumatoid arthritis (*s*=9), osteoarthritis (*s*=8), heterogeneous arthritis (*s*=9), osteoporosis (*s*=5), breast cancer (*s*=4), gastrointestinal cancer (*s*=1), renal disease (*s*=2), chronic obstructive pulmonary disease (*s*=2), Parkinson's disease (*s*=1), multiple sclerosis (*s*=1), fibromyalgia (*s*=1), chronic fatigue syndrome (*s*=1), chronic back problems (*s*=1), mixed unspecific musculoskeletal diseases (*s*=1), hypopituitarism (*s*=1), and mixed chronic illnesses (*s*=15). Although not well reported, many studies did not exclude subjects with the co-morbidities commonly associated with the chronic illness target population. Further details about subjects with diabetes, cancer, arthritis, and cardiac disease are available in previous publications reporting on health outcomes among these studies [30,31,35,218-220].

Plots depicting ESs of PA versus sampling variance suggested the absence of studies with low or negative ESs for treatment group versus control group comparisons and for treatment group pre-post comparisons. Although this is consistent with a publication bias against non-significant or negative results, it may also arise from phenomena such as certain patterns of heterogeneity [221]. There was no evidence of publication bias for comparison group pre-post ES plots.

### 3.1 Primary study characteristics

Descriptive statistics for primary study characteristics are displayed in Table 3. Sample size varied extensively from 4 to 1173 subjects, with a median of 62 subjects. The median value for mean age was 59 years. Typically, almost half of the subjects were women, among studies that reported gender distribution. Minority inclusion was often not reported: 33 reports indicated including African Americans, 10 included Hispanics, and 2 included Native Americans. Only four papers reported on predominantly (50% or more) African-American subjects, only one on predominantly Hispanic subjects, and none on predominantly Native-American subjects. A few studies experienced significant attrition, though losses were typically modest and were similar between treatment and control groups.

Tables 1 and 3 include intervention attributes. Supervised exercise was the most common PA behavior change strategy among the studies (*s*=88). Typical attributes among studies with supervised exercise included 2 weekly 60-minute sessions over 12 weeks. Fifty-four studies included some form of mediated motivational or educational intervention delivery (e.g. telephone, mail, Internet), and 13 used only mediated intervention delivery. Several studies asked subjects to monitor their PA behavior (*s*=66) as a strategy to increase PA. Fitness testing (*s*=57) and individualized exercise prescription (*s*=65) were common intervention components.

Modeling, either by research staff members (*s*=33) or by people similar to subjects (*s*=40), was very common. Many studies asked subjects to generate specific PA behavioral goals (*s*=55) in an effort to change behavior. Fifty studies either taught problem-solving skills to subjects or included problem solving during contacts with the research staff. Monitoring of PA behavior by research staff was also common (*s*=39). Forty-three studies provided explicit feedback to subjects about their PA behavior. Twenty-eight studies either provided rewards for subjects for increasing PA or taught subjects to reward themselves for PA. Thirty-five studies attempted to increase social support for PA. Thirty-nine studies specifically dealt with barriers to PA as a strategy to increase activity. Strategies less frequently reported include stimulus/cues to PA (*s*=14), relapse prevention education (*s*=14), health risk appraisal (*s*=14), symptom monitoring and management (*s*=16), and contracting (*s*=16). Intervention content reported by 10 or fewer studies included decision making (*s*=9), commitment activities (*s*=8), thought restructuring (*s*=7), values clarification (*s*=7), exercise variation (*s*=5), cognitive modification (*s*=4), decisional balance (*s*=3), personal trainer (*s*=2), shaping (*s*=2), community development (*s*=1), emotional arousal (*s*=1), imagery (*s*=1), and motivational interviewing (*s*=1). Most studies delivered the same interventions to all subjects, only six interventions were individually tailored (intervention content to motivate PA behavior change individually modified in systematic manner), and only two were targeted (different interventions for subsets of samples).

### 3.2 Effect of Patient Education Interventions on PA Outcomes

Table 4 presents the overall effects of interventions. The overall mean effect in two-group studies was 0.45. The treatment group pre- vs. post-test mean ESs were 0.41 ($\rho_{12}$=0.80) and 0.51 ($\rho_{12}$=0.00). The treatment group versus control group comparison and both treatment group pre- vs. post-test comparisons demonstrated significant ES heterogeneity according to the homogeneity test (*Q* in table 3). These findings document that although intervention effects were highly variable, interventions resulted in improved PA behavior scores on average. In contrast, control subjects generally experienced no improvement, with ES estimates of 0 that yielded 95% confidence intervals between -0.07 and 0.07.

To enhance interpretability, mean ESs were transformed to minutes of PA per week and steps per day, using results from appropriate reference groups pooled across available studies. The mean effect in terms of the pooled treatment and control SD (106.7) is a raw mean difference of 48.0 (0.45 × 106.7) PA minutes per week. Relative to the pooled post-test mean of 116.4 minutes for control subjects, this translates to a mean of 164.4 minutes of PA per week among treatment subjects. For single-group pre-post comparisons, the minutes per week raw mean difference is 38.4 minutes ($\rho_{12}$=0.80 assumption); relative to the pooled mean baseline of 92.5 minutes per week this translates into mean outcome 130.9 minutes per week. For steps per day, the mean effect in terms of the pooled treatment and control SD (2101) is a steps-per-day raw mean difference of 945 (2101 × .45). Relative to the pooled post-test mean of 6108 steps for control subjects, this translates to a mean of 7053 steps per day among treatment subjects for the two-group comparison. For single-group pre-post comparisons, the steps per day raw mean difference is 870 steps ($\rho_{12}$=0.80 assumption); relative to the pooled mean baseline of 4254 steps this translates into mean outcome 5124 steps per day. The CLES for the two-group comparisons was 0.62, indicating that 62% of the time, a random treatment subject would have higher PA scores than a random control subject. For single-group comparisons the random-effects estimates would be translated to CLES of .64 and .74 for assumptions of a low and high pre-post association, respectively. The CLES for control subjects was .50 indicating these subjects were no more likely to have higher PA scores after interventions than before interventions.

### 3.3 Moderator Analyses

The results from the dichotomous and continuous moderator analyses for two-group comparisons are presented in Tables 5 and 6. Studies reported more recently yielded slightly smaller ESs than studies distributed earlier $(\widehat{\beta}_1 = -.009)$. Studies published in journals did not report significantly different ESs as compared to unpublished studies. Studies with funding reported significantly lower ESs (.41) than studies without funding (.60). Gender distribution, ethnic minority proportion, sample age, and socioeconomic characteristics of samples were unrelated to ESs. Neither random assignment of subjects, attrition, nor the length of the follow-up period between intervention completion and PA outcome measurement were related to ESs.

Interventions that targeted only PA behavior (.57) reported larger ESs than those attempting to change multiple behaviors such as PA and diet (.38). Interventions with supervised exercise sessions were no more effective in changing PA than those that relied exclusively on educational or motivational sessions. Individually tailored interventions were not associated with larger ES than more traditional interventions where all subjects receive the same content. The presence of mediated delivery (e.g. telephone, mail) was unrelated to ESs. Interventions were similarly effective regardless of whether they were delivered to individuals or groups.

Interventions based on social cognitive theory, the most commonly reported conceptual foundation for interventions, were unrelated to ESs. Studies of interventions based on the transtheoretical model reported significantly smaller ESs (.22) than studies that did not use this framework (.48). Studies that used any behavioral strategy (e.g. consequences, contracting, feedback, goal setting, self-monitoring, stimulus/cues, personal trainer) to increase PA reported larger ESs (.51) than studies that did not use any behavioral strategies. Studies that used exclusively behavioral strategies (.53) reported larger ESs than studies that did not consist entirely of behavioral intervention (.40). In contrast, the presence or absence of cognitive strategies (barriers management, decisional balance, motivational counseling, problem solving, social cognitive theory) was not associated with ESs. The difference between studies using only cognitive strategies (.10) versus those using cognitive strategies plus other strategies (.47) did not achieve statistical significance. Neither the number of cognitive strategies nor the number of behavioral strategies in interventions was related to ESs. Studies that directed subjects to self-monitor their PA behavior reported significantly larger ESs (.56) than studies that did not promote self-monitoring (.40). Other intervention content potential moderators were unrelated to ESs: barriers management, consequences, contracting, exercise prescription, feedback, fitness testing, goal setting, problem solving, and stimulus/cues. The number of strategies in the intervention did not predict ESs. Neither the weeks over which the intervention was delivered nor the amount of contact time between intervention staff and subject were related to the ESs of PA. The nature of the PA recommended to subjects (specific forms, intensities, minutes/week) was unrelated to PA behavior outcomes. Contact the senior author for information about associations among moderators and moderator analyses of treatment group pre-post comparisons.

The moderators reported above and in Tables 5 and 6 all involve single-df moderator effects. Multiple-df moderator analyses were conducted on a few moderator variables for independent group ESs. This entailed testing either one moderator with more than two levels or two dichotomous moderators, the latter of which permits examining moderator interactions. The ESs of PA differed significantly by type of chronic illness ($Q_{between}$=9.1). The largest ES was for arthritis (.61), followed by diabetes (.49) and cardiac (.40). The smallest ES was among studies of cancer patients (-.03). The single moderator analyses we report for behavioral and cognitive strategies was supported in a multiple moderator analyses ($Q_{between}$=10.5). The analyses found the largest ES for studies with only behavioral strategies (.53) followed by

studies with both behavioral and cognitive intervention (.50). ESs were smallest among studies with cognitive strategies and no behavioral intervention (.10).

## 4. Discussion and Conclusion

### 4.1 Discussion

This is the first meta-analysis to examine PA behavior following patient education interventions designed to increase PA among diverse chronically ill populations. Our extensive search strategies successfully located a sizable and diverse literature. A moderate overall ES was calculated. The overall ES is between the values previously reported for older adults and for predominantly healthy adults [11,29]. In subgroup analyses, Dishman and Buckworth reported considerably smaller ESs for the few studies they included of people with cardiac disease. These differences probably reflect population differences among the primary studies. Few primary studies included in this meta-analysis were included in the previously reported syntheses.

The calculated differences in PA minutes per week (164.4 vs. 116.4) and in steps per day (7053 vs. 6108) are clinically meaningful for adults with many chronic illnesses. Although the treatment subjects generally did not achieve the 10,000 steps/day commonly recommended for healthy adults [222,223], the PA dose necessary to achieve health outcomes has not been definitively documented for adults with chronic illness. It is unclear whether people gain the greatest health benefits from initial increases in PA above sedentary rates or from moving from moderate levels of PA to more active levels. The amount of increase is clear evidence that some interventions are effective in changing PA behavior. The extent of heterogeneity documents that not all interventions are equally effective.

Our exploratory moderator analyses documented some intriguing findings. Since the moderator analyses were designed to be hypothesis generating, these findings should be explored in further primary research. The largest ESs came from patient education interventions that were designed exclusively to change PA behavior, as compared to those focused on multiple health behaviors. These findings about effectiveness of PA-only interventions are consistent with a previous meta-analysis of interventions to increase PA among healthy and chronically ill older adults [11]. It may be easier to change one behavior at a time. Chronically ill adults may feel overwhelmed when asked to make multiple changes simultaneously. This finding is for PA behavior outcomes and may not extend to health outcomes. For example, a study of adults with type 1 diabetes found better metabolic control outcomes when interventions focused on multiple diabetes-related health behaviors [218]. In contrast, a previous meta-analysis of people with type 2 diabetes found better metabolic control outcomes when interventions targeted only PA behavior [30]. Attempts to change multiple behaviors may be appropriate when the health benefits of some combination of behavior changes are more important than changes achieved by changing only one behavior. We need further primary research which examines both behavior change and health outcomes of interventions which target single behaviors as compared to those which encourage changing multiple behaviors [224].

The moderator analyses provide evidence that patient education interventions were similarly effective regardless of gender, age, minority, and socioeconomic status. Differences in the ESs of PA among the major categories of chronic illnesses were interesting. Subjects recruited because they had arthritis were the most likely to increase their PA, despite possible immediate arthritis-related physical discomfort associated with doing so. It is possible that arthritis symptoms improve after establishing a PA program, which reinforced PA behavior. People recruited because they had diabetes and cardiac disease improved their PA, though less dramatically. With increased PA, people with diabetes may experience improved blood glucose levels and people with cardiac disease may increase fitness while delaying progression of

cardiac disease. These objective health outcomes are clearly important, but they may be less perceptible and persuasive thus providing less motivation to continue PA. Further primary research which tests behavior changes, symptom outcomes, and health consequences of interventions is needed. These findings should be interpreted with the understanding that most studies in this meta-analysis did not exclude subjects with co-morbidities. Subjects recruited for a specific illness often had other diseases as well (e.g. diabetes and cardiac disease is a common combination). Co-morbidities were too infrequently reported to be included in the analyses.

Supervised exercise was the most common intervention strategy, but our moderator analyses revealed that it worked no better than educational/motivational sessions. These findings are consistent with previous meta-analyses of older adults and predominantly healthy adults [11, 29]. Supervised exercise is an expensive intervention because it requires specialized facilities and equipment as well as highly trained personnel. For some chronically ill populations or subpopulations (e.g. severe heart failure), supervised exercise may be useful for safety or other reasons.

In our moderator analyses, self-monitoring of PA behavior was associated with larger ESs. A previous meta-analysis of interventions with older adults also found self-monitoring an effective strategy [11]. Self-monitoring provides subjects with real-time information and may increase their awareness of existing behavior as well as changes in behavior. Self-monitoring is an inexpensive strategy. Most other single intervention strategies were not associated with differences in ESs.

The findings regarding behavioral versus cognitive categories of interventions were interesting in light of previous research. These findings are consistent with two previous meta-analyses of healthy and older adults that reported behavior strategies were effective in changing PA [11,29]. Mass media attention to the importance of PA may have raised subjects' consciousness of its value. People may need more behavior change strategies to successfully change PA behavior. Interventions which were found to be effective included varying combinations of goal setting, contracting, feedback, consequences, self-monitoring, and/or prompts. Intervention content that this meta-analysis found to be unrelated to PA behavior change includes decisional balance activities, problem solving, barriers management, motivational counseling, or an emphasis on self-efficacy and outcome expectancy. Future intervention tests that directly compare combinations of behavioral and cognitive strategies are necessary to provide direction for practice. Further primary research which examines mediators of behavior change would help clarify the links between intervention components and outcomes. When considerably more primary research is available, future meta-analyses may be able to address the specific constellation of intervention components that when grouped together make the largest changes in PA behavior.

We were unable to directly address specific theoretical frameworks for several reasons. First, studies often use selected components or language commonly associated with particular theories but fail to name the theory or cite theorists. Thus decisions about including these studies would rely on considerable coder attribution that may not be justified. Conceptual frameworks that address health behavior change have many overlapping constructs. For example, self-efficacy constructs are found in social cognitive theory, the theory of planned behavior, and the transtheoretical model. Some primary reports describe interventions as based on multiple theories. Interventions often only partially implement theoretical frameworks. For example, some studies claim social cognitive theory origins but do not address the sources of self-efficacy. Setting criteria for when interventions are based on the theoretical model would depend entirely on research team opinions and would be difficult to operationalize (e.g. does an intervention need to address three of the four sources of self-efficacy to be based on social

cognitive theory?). Given current reporting practices, it is unclear if future quantitative syntheses will be able to fully address conceptual frameworks.

We conducted the first reported analyses of joint moderators and found that this may be effective for discovering patterns among studies (e.g. exercise prescription and fitness testing; age and weeks of intervention, interactions between random allocation and other moderators). Relationships among moderators may be complex. Unfortunately, too little is understood about PA behavior change to suggest meaningful directions for these analyses at this time. This will be an important strategy, after many more primary studies are available, to detect the grouping of intervention strategies that combine to make the most effective intervention.

The methodological findings provide a context for interpreting future studies. ESs were similar between single- and two-group comparisons, though there are many reasons to prefer findings from two-group studies. Previous meta-analyses have not reported ESs for control groups. We found that, on average, the PA behavior of control subjects did not change. Thus, researcher used methods for collecting PA data and delivering information to controls generally did not change PA behavior. We were somewhat surprised to find that attrition was unrelated to ESs, given the folk wisdom that subjects who are not exercising are the most likely to drop out of PA studies. Since no previous meta-analysis has examined attrition, future quantitative syntheses should address this whenever examining health behavior change outcomes. The finding that unfunded studies reported larger ESs is interesting. Unfunded studies may differ in ways other than funding status. For example, unfunded studies may include particularly powerful or novel interventions that researchers pilot tested prior to requesting funding.

These findings should be interpreted within the limitations of this meta-analysis. Funnel plots suggested potential publication bias. It remains unclear whether investigators do not attempt to publish or are unable to publish findings from studies without statistically significant intervention effects [37]. We expected the findings of considerable heterogeneity. These should stimulate continued research to determine which primary study features (e.g. sample, intervention, or design attributes) account for differences among studies. Findings of moderator analyses must be interpreted cautiously in the presence of significant heterogeneity. We do not know the extent to which primary study samples represent populations of chronically ill adults. It is possible that, with safety in mind, investigators recruited participants who were less ill and perhaps more physically active [225]. It is also possible that less active subjects were purposefully recruited [225]. Many important features of studies, such as treatment fidelity, were unreported and thus we could not analyze them. Important potential subject characteristic moderators could not be analyzed because they were infrequently reported. For example, co-morbid medical conditions and mental health issues (e.g. depression) are infrequently reported in primary research but could be important moderators of intervention effects. Systematic testing of findings from moderator analyses from future studies will be important.

### 4.2 Conclusion

Overall, our quantitative synthesis documented moderate PA behavior effects following diverse patient educational interventions. PA is especially important for adults with chronic illnesses because it may delay progression of some chronic illnesses and manage symptoms of others. Our exploratory moderator analyses have suggested directions for future research. In order to increase knowledge about effective patient education interventions, it is essential to continue conducting new primary research studies as well as quantitative syntheses.

### 4.3 Practice Implications

Our findings document that patient education intervention can increase PA but that not all interventions are effective. Careful outcome assessments of programs are essential. Patient

education programs may be effective across gender, age, and income varied subjects, though adults with diabetes or cardiac disease may be less likely to change their behavior than those with arthritis. The suggestive moderator analyses found associations between PA behavior change and interventions that target only PA behavior, include self-monitoring, and emphasize behavioral strategies such as goal setting, contracting, feedback, consequences, and/or cues. The analyses found that interventions that do not include supervised exercise were as effective as interventions with this more costly component. The importance of increasing PA among many chronically ill adults justifies continuing to develop diverse patient education efforts.

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## Table 1

### Intervention Content Reported by More than 10 of the Studies*

| Characteristic | Description | Frequency |
|---|---|---|
| Supervised exercise | Provision of supervised exercise sessions at a specified location. | 88 |
| Self-monitoring | Subjects record physical activity (PA) behavior. Includes activity diaries. | 66 |
| Exercise prescription | Individualized specific form, intensity, frequency and/or duration of exercise suggested to each subject. May include prescription for progression of exercise. | 65 |
| Fitness testing | Testing subjects' physical fitness when the testing was intended to change behavior. Not coded when fitness testing measured outcomes but information was not provided to subjects as a strategy to change behavior. | 57 |
| Goal setting | Subjects generate written or oral behavioral change goals. | 55 |
| Problem-solving | Teaching subject problem-solving strategies or conducting problem-solving activities (name problems, generate potential solutions specific to exercise behavior, decide on a course of action, and evaluate effects of solution implemented) during sessions with the investigator. | 50 |
| Feedback | Explicit information provided to subjects about their performance. Could include a comparison with a group norm or standard or with subjects' own past behavior. | 43 |
| Social modeling | Opportunities to watch similar others exercise. | 40 |
| Barriers management | Teaching subjects to overcome or manage barriers (obstacles, deterrents). Could include having subjects identify barriers and brainstorm strategies to overcome them or could be teaching methods to overcome barriers that researchers identify. | 39 |
| Monitoring | Research staff monitors subjects' physical activity performance. | 39 |
| Social support | Deliberate building of social relationships to foster increased physical activity (e.g. self-help groups, 'social support'). Includes team building but not team competition for rewards. | 35 |
| Modeling | Opportunities to watch others exercise who may not be similar to subjects. | 33 |
| Consequences | Specific planned tangible or intangible positive consequences of behavior change (e.g. lottery for prizes, praise, token economy, competition that has a reward, contracting for a reward, social reinforcement) when the research team or subject administers the reward. | 28 |
| Contracting | Written agreements including specific behavior change in stated time frame. | 16 |
| Symptom monitoring/management | Subjects taught to monitor PA-related symptoms and modify PA to manage symptoms. Not coded when subjects were taught which symptoms would prompt cessation of PA for safety. | 16 |
| Health risk appraisal | Specific researcher-provided algorithm of quantifiable risks of major illness that can be altered by PA behavior. | 14 |
| Relapse prevention education | Specific program to prepare subjects for lapse, relapse, slip, backsliding, failures, abstinence violation effect. Only coded when report used the phrase relapse prevention education. | 14 |
| Stimulus control | Object of event that stimulates PA behavior (e.g. cues, prompts, telephone calls, reminders located strategically). | 14 |

* Studies could report multiple intervention strategies.

## Table 2

### Statistical Management of Data

| Analysis component | Approach or rationale |
|---|---|
| Standardized mean difference [33,34,49] | Post-intervention difference between treatment and control group divided by the pooled SD, for two-group comparisons.<br>Difference between pre- and post-test scores divided by pre-intervention SD for single-group comparisons, under assumptions of no ($\rho_{12} = 0.0$) and high association ($\rho_{12} = 0.80$) between pre- and post-scores; calculated for all pre-post treatment and control groups with adequate data.<br>Adjusted for small sample bias.<br>Weighted by inverse of variance to address sample size differences.<br>95% confidence intervals for mean ES constructed from normal-theory stand errors.<br>Studies with multiple treatment groups without a control group were treated as single-group studies. |
| Dependencies in data for few studies with one control group and two or three treatment groups | Each study's dependent ESs combined in a single independent ES by generalized least-squares approach and then treated as standard univariate random-effects analyses. [50] |
| Outlier determination | ESs examined graphically.<br>Externally standardized residuals evaluated as removed each ES one at a time.<br>Homogeneity analyses after omitting each case in turn. |
| Publication bias determination [51] | ES plotted against sampling variance.<br>Examined for funnel-shaped plot indicating symmetrical distribution around estimated population mean ES. |
| Homogeneity | Q calculated from weighted sum of squares (chi-squared distribution). |
| Random-effects model | Assumes individual ESs vary due to both subject-level sampling error and other sources of study-level error such as variations among interventions.<br>Consistent with heterogeneous study implementation.<br>Weighted method of moments used to estimate between studies variance component. |
| Common Language Effect Size (CLES) [52] | Probability that a random treatment subject would score higher than a random control subject (an ES of $d = 0.0$ corresponds to a CLES of 0.50). |
| Conversion to original metric [47] | ES converted to original metric for variables with multiple studies reporting identical measures.<br>Reported for minutes of PA per week and for steps per day. |
| Moderator analysis [53,54] | Mixed- and fixed-effects calculated, fixed-effects available from senior author.<br>Continuous moderators: effects tested by unstandardized regression slope ($\beta$) in meta-analytic analogue of regression.<br>Dichotomous moderators: effects tested by between-group heterogeneity statistic ($Q_{between}$) using meta-analytic analogue of ANOVA.<br>Interpret findings cautiously when significant heterogeneity exists, large variance components decrease statistical power. |
| Multiple-df moderator analysis | Categorical moderator with more than two levels or two dichotomous moderators in factorial design.<br>Multicategory moderator: Omnibus test of heterogeneity among levels ($Q_{between}$) followed by focus contrasts among particular levels.<br>Two dichotomous moderators: Omnibus test comparing mean ES among four cells ($Q_{between}$), tests of each main effect and interaction, and contrasts comparing particular cells (e.g., simple main effects). |

**Table 3**

**Characteristics of Primary Studies Included in Combined-Illness Physical Activity Meta-Analyses**

| Characteristic | *k* | Min | $Q_1$ | *Mdn* | $Q_3$ | Max |
|---|---|---|---|---|---|---|
| Mean age (years) | 156 | 22 | 54 | 59 | 65 | 82 |
| Sample size per study | 163 | 4 | 33 | 62 | 153 | 1173 |
| Proportion of sample assigned to treatment[a] | 109 | .25 | .50 | .51 | .58 | .79 |
| Proportion attrition from treatment group | 133 | .00 | .04 | .11 | .21 | .52 |
| Proportion attrition from comparison group[a] | 87 | .00 | .03 | .11 | .22 | .59 |
| Proportion female | 140 | .00 | .25 | .49 | .79 | 1.00 |
| Minutes of supervised exercise per session | 51 | 10 | 40 | 60 | 60 | 300 |
| Total number of supervised exercise sessions | 60 | 4 | 17 | 36 | 38 | 202 |
| Number of weeks intervention was delivered | 143 | 1 | 6 | 12 | 24 | 208 |

*Note*. Includes all studies that contributed at least one independent-groups or pre-post effect size to primary analyses. Independent samples within studies aggregated by summing sample sizes before computing proportions and using weighted mean of other characteristics (weighted by sample size). *k* = number of studies providing data on characteristic; $Q_1$ = first quartile, $Q_3$ = third quartile.

[a]Computed for independent-groups studies only.

**Table 4**

**Random-Effects Physical Activity Behavior Outcome Estimates and Tests**

| ES type | *k* | *Q* | $\widehat{\mu}_{\delta}$ | SE $(\widehat{\mu}_{\delta})$ | $\mu_{\delta}$ 95%CI | $\widehat{\sigma}_{\delta}$ |
|---|---|---|---|---|---|---|
| Independent-groups[a] | 129 | 444.08*** | .46*** | .035 | (.39,.52) | .301 |
| Independent-groups[b] | 129 | 440.73*** | .45*** | .037 | (.38,.52) | .300 |
| Treatment pre-post, $\rho_{12}$ = .80 | 146 | 2058.55*** | .41*** | .029 | (.35,.47) | .325 |
| Treatment pre-post, $\rho_{12}$ = .00 | 160 | 820.84*** | .51*** | .038 | (.43,.58) | .389 |
| Comparison pre-post, $\rho_{12}$ = .80 | 59 | 463.49*** | -.00 | .035 | (-.07,.07) | .235 |
| Comparison pre-post, $\rho_{12}$ = .00 | 56 | 54.67 | .00 | .026 | (-.05,.06) | 0 |

*Note*. Under homogeneity *Q* is distributed approximately as chi-square with $df = k - 1$, where *k* is the number of (possibly dependent) observed effect sizes; this tests both homogeneity ($H_0$: $\delta = \delta_i$) and the between-studies variance component $\sigma^2_{\delta}$ $(H_0 : \sigma^2_{\delta} = 0)$. Weighted method of moments used to estimate $\sigma^2_{\delta}$. Potential outliers excluded based on random-effects standardized residuals.

[a] Dependence due to 11 multiple-treatment pairs and 4 multiple-treatment triplets not accommodated.

[b] Multiple-treatment dependence accommodated.

† $p < .10$.

* $p < .05$.

** $p < .01$.

*** $p < .001$ (for $\bar{d}$, $\widehat{\mu}_{\delta}$, and *Q*).

**Table 5**

**Independent-Groups Post-test Comparisons: Dichotomous Moderator Analyses**

| Moderator | Coded values | $k_0$ | $k_1$ | $\bar{\mu}_{\delta 0}$ | $\bar{\mu}_{\delta 1}$ | $Q_B$ | $Q_W$ | $\bar{\sigma}_{\delta}$ |
|---|---|---|---|---|---|---|---|---|
| Publication status | 0 = unpublished, 1 = published | 15 | 114 | .44*** | .46*** | 0.0 | 443.8*** | .302 |
| Funding | 0 = no funding, 1 = funding | 35 | 94 | .60*** | .41*** | 5.1* | 399.0*** | .283 |
| SES | 0 = low, 1 = moderate or high | 4 | 6 | .37† | .27 | 0.1 | 25.7** | .314 |
| Random allocation | 0 = absent, 1 = present | 23 | 106 | .58*** | .43*** | 2.6 | 440.1*** | .301 |
| Behavioral target | 0 = physical activity only, 1 = multiple health behaviors | 55 | 74 | .57*** | .38*** | 7.4** | 426.8*** | .295 |
| Supervised exercise | 0 = absent, 1 = present | 79 | 50 | .41*** | .53*** | 2.5 | 441.0*** | .302 |
| Tailoring | 0 = absent, 1 = present | 125 | 4 | .45*** | .66*** | 1.2 | 418.8*** | .295 |
| Mediated delivery | 0 = face-to-face only, 1 = mail/telephone/mass media | 97 | 32 | .47*** | .41*** | 0.6 | 443.1*** | .304 |
| Intervention social context | 0 = individual, 1 = group | 43 | 85 | .48*** | .45*** | 0.1 | 439.6*** | .303 |
| Social cognitive theory | 0 = absent, 1 = present | 97 | 32 | .45*** | .46*** | 0.0 | 439.9*** | .303 |
| Transtheoretical model | 0 = absent, 1 = present | 117 | 12 | .48*** | .22* | 4.7* | 427.8*** | .295 |
| Behavioral–any | 0 = no behavioral strategy, 1 = at least 1 behavioral strategy | 30 | 99 | .28*** | .51*** | 8.8** | 412.7*** | .289 |
| Behavioral–pure | 0 = cognitive or no behavioral, 1 = behavioral and no cognitive[a] | 72 | 57 | .40*** | .53*** | 3.1† | 438.0*** | .301 |
| Cognitive–any | 0 = no cognitive strategy, 1 = at least 1 cognitive strategy | 81 | 48 | .46*** | .44*** | 0.1 | 442.0*** | .303 |
| Cognitive–pure | 0 = behavioral or no cognitive, 1 = cognitive and no behavioral[b] | 123 | 6 | .47*** | .10 | 5.6 | 437.8*** | .302 |
| Barriers management | 0 = absent, 1 = present | 101 | 28 | .45*** | .46*** | 0.0 | 441.7*** | .303 |
| Consequences | 0 = absent, 1 = present | 111 | 18 | .45*** | .49*** | 0.2 | 430.4*** | .299 |
| Contracting | 0 = absent, 1 = present | 118 | 11 | .44*** | .60*** | 1.6 | 440.2*** | .303 |
| Exercise prescription | 0 = absent, 1 = present | 90 | 39 | .42*** | .53*** | 2.1 | 438.8*** | .301 |
| Feedback | 0 = absent, 1 = present | 99 | 30 | .47*** | .42*** | 0.4 | 442.4*** | .304 |
| Fitness testing | 0 = absent, 1 = present | 97 | 32 | .43*** | .55*** | 1.8 | 444.0*** | .303 |
| Goal setting | 0 = absent, 1 = present | 92 | 37 | .47*** | .43*** | 0.2 | 443.8*** | .305 |
| Problem solving | 0 = absent, 1 = present | 98 | 31 | .44*** | .50*** | 0.5 | 436.3*** | .301 |
| Self-monitoring | 0 = absent, 1 = present | 87 | 42 | .40*** | .56*** | 5.1* | 397.1*** | .281 |
| Stimulus/cues | 0 = absent, 1 = present | 121 | 8 | .45*** | .56*** | 0.5 | 442.2*** | .301 |
| Recommended specific exer. form | 0 = other than walking, 1 = walking | 115 | 14 | .46*** | .45*** | 0.0 | 442.8*** | .302 |

| Moderator | Coded values | $k_0$ | $k_1$ | $\widehat{\mu}_{\delta_0}$ | $\widehat{\mu}_{\delta_1}$ | $Q_B$ | $Q_W$ | $\widehat{\sigma}_{\delta}$ |
|---|---|---|---|---|---|---|---|---|
| Recommended intensity | 0 = low,<br>1 = moderate or high | 5 | 40 | .39$^{\dagger}$ | .60$^{***}$ | 0.9 | 142.5$^{***}$ | .396 |
| Days after intervention | 0 = none,<br>1 = at least one | 43 | 80 | .45$^{***}$ | .46$^{***}$ | 0.0 | 435.6$^{***}$ | .314 |

*Note.* $k_j$ = number of (possibly dependent) ES estimates in group coded *j*. Heterogeneity statistics: $Q_B$ = between groups (distributed as chi-square on *df* = 1 under $H_0$: $\mu_{\delta 0} = \mu_{\delta 1}$), $Q_W$ = combined within groups (distributed as chi-square on $df = k_0 + k_1 - 2$ under $H_0$: $\sigma^2_{\delta_0} = \sigma^2_{\delta_1} = 0$). Weighted method of moments used to estimate between-studies variance component $\sigma^2_{\delta}$. Analysis reported if $k_0 \geq 3$ and $k_1 \geq 3$.

$^{\dagger}p < .10$.

$^{*}p < .05$.

$^{**}p < .01$.

$^{***}p < .001$ (for $\widehat{\mu}_{\delta_0}$, $\widehat{\mu}_{\delta_1}$, $Q_B$ and $Q_W$).

[a] 0=no behavioral strategy or at least 1 cognitive strategy, 1=at least 1 behavioral strategy and no cognitive strategy.

[b] 0=no cognitive strategy or at least 1 behavioral strategy, 1=at least 1 cognitive strategy and no behavioral strategy.



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**Table 6**

**Linear Continuous Mixed-Effects Moderator Analyses for Physical Activity**

| Moderator | *k* | $Q_{residual}$ | $\widehat{\beta}_0$ | $\widehat{\beta}_1$ | $SE(\widehat{\beta}_1)$ | $\widehat{\sigma}_\delta$ |
|---|---|---|---|---|---|---|
| Publication year | 129 | $430.2^{***}$ | 0.685 | $-0.009^{\dagger}$ | 0.0056 | .297 |
| Mean age | 125 | $425.9^{***}$ | 0.198 | 0.004 | 0.0040 | .305 |
| Proportion female | 115 | $367.3^{***}$ | 0.384 | 0.162 | 0.1225 | .304 |
| Proportion ethnic minority | 24 | $47.7^{**}$ | 0.427 | -0.317 | 0.2951 | .312 |
| Attrition proportion | 92 | $328.0^{***}$ | 0.467 | -0.157 | 0.3865 | .310 |
| Days after intervention | 80 | $291.8^{***}$ | 0.757 | -0.133 | 0.0969 | .308 |
| Number behavioral strategies | 129 | $425.6^{***}$ | 0.407 | 0.031 | 0.0232 | .296 |
| Number cognitive strategies | 129 | $438.5^{***}$ | 0.451 | 0.005 | 0.0310 | .302 |
| Number strategies | 129 | $436.7^{***}$ | 0.437 | 0.004 | 0.0094 | .301 |
| Sup. cont.–$\log_{10}$(min) | 34 | $78.2^{***}$ | -0.342 | 0.286 | 0.1835 | .348 |
| Mot. cont.–$\log_{10}$(min) | 43 | $132.0^{***}$ | 0.204 | 0.087 | 0.0995 | .253 |
| Recom. min/wk | 34 | $114.7^{***}$ | 0.142 | 0.003 | 0.0021 | .409 |
| Weeks of intervention | 115 | $421.4^{***}$ | 0.420 | 0.057 | 0.0646 | .310 |

*Note. k* = number of (possibly dependent) ES estimates. Moderators: publication year (year – 1972); mean age (years); Pr. ethnic minority (proportion Black, Hispanic, or Native American); days after intervention (common log of days); number of strategies (number of coded strategies used in intervention, including behavioral, cognitive, and others); supervised contact–$\log_{10}$(min) (common log of positive minutes of supervised contact); motivational contact–$\log_{10}$(min) (common log of positive minutes of motivational/educational contact); weeks of intervention (common log of weeks of intervention). $\widehat{\beta}_0$ = unstandardized intercept estimate for uncentered moderator; $\widehat{\beta}_1$ = unstandardized slope estimate. $Q_{residual}$ = residual heterogeneity statistic, beyond that due to moderator (distributed as chi-square on $df = k - 2$ under $H_0$: $\sigma^2_{\delta|x} = 0$, where $x$ is moderator value). Weighted method of moments used to estimate between-studies variance component $\sigma^2_\delta$. Analysis reported if $k \geq 6$.

$^{\dagger}p < .10$.

$^{*}p < .05$.

$^{**}p < .01$.

$^{***}p < .001$ (for $\widehat{\beta}_1$ and $Q_{residual}$).