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Ford, C. G., Vowles, K. E., Smith, B. W., & Kinney, A. Y. (2019). Mindfulness and Meditative Movement Interventions for Men Living With Cancer: A Meta-analysis. *Annals of Behavioral Medicine* .  
<https://doi.org/10.1093/abm/kaz053>

**Published in:**  
Annals of Behavioral Medicine

**Document Version:**  
Peer reviewed version

**Queen's University Belfast - Research Portal:**  
[Link to publication record in Queen's University Belfast Research Portal](#)

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Mindfulness and Meditative Movement Interventions for Men Living with Cancer:  
A Meta-analysis

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16 October 2019

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**Conflict of Interest:** The authors declare that they have no conflict of interest.

**Ethical Approval:** All procedures performed in studies involving human participants were in accordance with the ethical standards of the institutional and/or national research committee and with the 1964 Helsinki declaration and its later amendments or comparable ethical standards. For this type of study formal consent is not required. This article does not contain any studies with human participants or animals performed by any of the authors.

**Acknowledgments:** NIH National Cancer Institute (R01CA203930; PI: Kinney), “Biobehavioral Effects of Tai Chi Qigong for Prostate Cancer Survivors with Fatigue”. This research used the facilities or services of the University of New Mexico Comprehensive Cancer Center (UNMCCC), and the Behavioral Measurement and Population Sciences (BMPS) and Biostatistics Shared Resources, facilities supported by the State of New Mexico and the UNMCCC P30CA118100. This research was also supported by the University of New Mexico Health Sciences Clinical and Translational Science Center (CTSC UL1TR001449).

## Abstract

**Background** Mindfulness-Based Interventions, Tai Chi/Qigong, and Yoga (defined here as meditative cancer interventions (MCIs)) have demonstrated small to medium effects on psychosocial outcomes in female breast cancer patients. However, no summary exists of how effective these interventions are for men with cancer.

**Purpose** A meta-analysis was performed to determine the effectiveness of MCIs on psychosocial outcomes (e.g. quality of life, depression, post-traumatic growth) for men with cancer.

**Methods** A literature search yielded 17 RCTs (N= 666) meeting study inclusion criteria. Authors were contacted to request data for male participants in the study when not reported.

**Results** With the removal of one outlier, there was a small effect found in favor of MCIs across all psychosocial outcomes immediately post-intervention ( $g=.23$ , 95% CI .02 to .44). Studies using a usual care control arm demonstrated a small effect in favor of MCIs ( $g=.26$ , 95% CI .10 to .42). However, there was insufficient evidence of a superior effect for MCIs when compared to an active control group, including attention control. Few studies examined both short-term and long-term outcomes.

**Conclusions** There is evidence for MCIs improving psychosocial outcomes in male cancer survivors. However, this effect is not demonstrated when limited to studies that used active controls. The effect size found in this meta-analysis is smaller than those reported in MCI studies of mixed gender and female cancer patient populations. More rigorously designed randomized trials are needed that include active control groups, which control for attention, and long-term follow-up. There may be unique challenges for addressing psychosocial needs of male cancer patients that future interventions should consider.

Keywords: *men, cancer, mindfulness-based interventions, survivorship*

## Introduction

Cancer, the second leading cause of death in the United States, is a significant and extensive public health problem (1,2). The probability of a lifetime diagnosis of cancer is close to 40% for those living in the United States (2). There are approximately 1.6 million new cancer cases and nearly 600,000 cancer-related deaths annually, in the United States alone (2). Despite relatively high global and US cancer mortality rates, and the persistence of racial and ethnic disparities in cancer screening and treatment outcomes, people are living longer than ever before with cancer (3). Given that the risk of receiving a cancer diagnosis increases with age, survivorship is often marked by the challenges of managing the late and long-term effects of treatment along with age-related morbidities. It is estimated that there are currently 9 million survivors over the age of 65 in the United States (4), a number that is projected to increase to more than 19 million by the year 2040 (5,6).

Cancer survivorship, especially for an aging population, entails unique psychosocial stressors (6,7). Even treatment completion does not mark the end of the cancer experience as there are often unaddressed psychosocial stressors that persist, including shame and social isolation (8,9). These stressors negatively impact wellbeing through an increased occurrence of psychological conditions, most notably higher rates of depression and anxiety (10,11), adjustment difficulties (12), fear of recurrence (13), and relationship challenges (6).

Over the past few decades, there has been increasing interest in mindfulness training to aid individuals with cancer-related psychosocial difficulties (14-16). There are two broad categories that these mindfulness interventions fall into. The first, Mindfulness Based Interventions (MBI) (17) includes group-based formal training in mindfulness, especially Mindfulness Based Stress Reduction (MBSR) and Mindfulness Based Cognitive Therapy

(MBCT). These MBIs are the most common to cancer care populations and have a common origin with Kabat-Zinn's (1982) protocol developed originally for hospital patients with complex physical and psychological needs (18). The second fall into the "meditative movement" category and include Yoga and Tai Chi/Qigong, both ancient practices (19). While there is heterogeneity in MBI and meditative movement treatment protocols (20), they share a focus on breathing, promotion of a cleared or calm state of mind, facilitation of present-focused awareness (17,21), and a goal of deep states of relaxation (19).

In cancer care, both MBIs and meditative movement interventions have been increasingly used as adjunctive treatments (14-16,22). Recent reviews of MBSR interventions found medium effect sizes for anxiety symptoms and small effects for depressive symptoms in RCTs for women with breast cancer (23). Similarly, research on both Tai Chi/Qigong and Yoga, predominantly in breast cancer survivors, demonstrate improved psychosocial outcomes for depression, anxiety, and distress (24-26). Given similar proposed mechanisms of action in those with cancer, both MBIs and meditative movement interventions can be considered part of a larger category, which we will refer to as Meditative Cancer Interventions (MCI).

One significant shortcoming of the MCI literature in cancer survivors is that evidence regarding efficacy in men is largely absent. For example, despite cancer mortality being higher among men than women, most of the MCI clinical trials are comprised primarily or exclusively of female populations, especially among those with breast cancer (23,27). For those trials with a variety of cancer types, men typically comprise less than a quarter of the participants (25). There have been several sex-specific MCI randomized trials with prostate cancer populations although these have only been conducted in past few years and little summary data exists of their outcomes.

That men are under-represented in MCI trials is not surprising. For one, men are less likely than women to engage in mindfulness practices overall in the United States (28). Given that men, along with racial and ethnic minorities, are less likely to engage in these practices, they are considered to be underserved, vulnerable, and “priority populations” for future study of such interventions (29). Similarly, men are less likely than women to access supportive cancer care in general (30), and may also be under-reporting the emotional distress associated with physical side effects of cancer treatments (9,31). Thus, there is a need to understand more about the evidence base for MCIs for men with cancer (32) in order to successfully adapt these treatments to the rapidly growing population of male cancer populations (33). Therefore, the goal of the present meta-analysis was to quantitatively review and summarize the evidence for male cancer patient psychosocial outcomes at short or longer-term follow-up in MCIs.

## **Methods**

### **Eligibility Criteria**

Studies that matched the following eligibility criteria were included: (1) published in the English language, (2) cancer population (any stage, including pre, current and post treatment), (3) randomized controlled trial, (4) at least 10% of the study population was male, (5) published in a peer reviewed journal as of December 2017, (6) study included an intervention that fits the definition of MCI (e.g. MBSR, MBCT, Tai Chi/Qigong, Yoga), and (6) psychosocial outcomes, defined in this study as patient reported outcomes that capture domains related to psychological and social functioning (e.g. anxiety, depression, health related quality-of-life, post-traumatic growth). Studies that only had patient reported outcomes related to physical health (e.g. fatigue, pain, sleep) were excluded. The search was restricted to randomized controlled trials in order to include only the highest quality evidence for quantifying the effect of MCIs.

The literature search was completed in December 2017 and included psychological (PsychINFO) and medical (MEDLINE) databases using the terms (Qigong OR Qi Gong OR Tai Chi OR TCC OR MQG ) AND (Cancer) AND (RCT or Randomized or Randomised) (MBSR OR MBCT OR Mindfulness OR Meditation) AND (Cancer) AND (RCT or Randomized or Randomised) AND (Yoga) AND (Cancer) AND (RCT or Randomized or Randomised). If primary articles did not report outcomes separately for male participants, corresponding authors were contacted to request these data. The Preferred Reporting Items for Systematic Review and Meta-Analyses (PRISMA) reporting guidelines were used for reporting for study inclusion (34).

### **Data Extraction**

Data was extracted by three coders (CGF and 2 students trained in psychology research methods and experienced with coding) independently using a data extraction sheet (Electronic Supplementary Appendix 1). The extracted data included bibliographic information, study information, participant demographics at baseline, details on treatment and control groups, psychological outcome measures, and quality assessment coding using an NIH Assessment tool customized for the studies included in the current meta-analysis (35). Risk of bias was assessed as low, unclear, or high across twelve domains. Studies that were at high or unclear risk in at least half of these domains were considered to be at a “high-risk of bias”. An inter-rater reliability analysis using the Cohen’s Kappa statistic was performed to determine consistency among raters. Conflicts were discussed among the three raters to achieve consensus.

### **Statistical Methods**

A meta-analysis of eligible studies was performed to summarize the outcomes of interest for intervention groups compared to controls. Effect sizes were calculated using Hedges’ standardized mean difference (Hedges’  $g$ ). Hedges’  $g$  was chosen as the effect size calculation

instead of Cohen's  $d$  as it is a more conservative estimate of effect, especially with smaller samples (36).

The formula for the effect size was conducted using the mean and standard deviation from the post-treatment data only. The Cochrane Handbook, a standard for best-practices in the reporting of systematic reviews and meta-analyses, advises against the use of meta-analysis of change scores in the absence of using a meta-regression approach (37). Given that only randomized studies were used in the analysis, there were only minimal baseline differences between the intervention and control groups.

Given the heterogeneity of study type, a random effects model, rather than a fixed-effects model was used for the meta-analysis, on the assumption that effects would differ among studies (37). Most studies included multiple psychosocial measures from the same study raising the issue of dependence arising from the use of multiple outcomes (38). Studies that included multiple measures of a domain, depending on the outcomes, may be inappropriately under-weighted or over-weighted, as multiple outcomes are not independent information. Borenstein's conservative approach was used to calculate a synthetic summary variable which assumes a .5 correlation between the outcome variables and corrects for the number of outcomes from each study (36). The overall psychosocial summary variable created for multiple outcomes in a study included studies that used multiple psychosocial domains (e.g. Health Related Quality of Life (HRQOL), Negative Mental Health, and Positive Mental Health). Although these domains are related, they are conceptually distinct. Positive and negative mental health are sufficiently distinct to be routinely considered as independent unipolar dimensions in much of the literature (39). While measures of mental health (both positive and negative) constitute a portion of HRQOL, itself a multidimensional construct, they are sufficiently distinct to be understood as separate domains



(40). Although these three domains are necessarily correlated, given their conceptual and statistical independence, we believed it was possible to summarize an overall effect for these outcomes.

Statistical heterogeneity, an indication of inconsistent results across studies, was assessed using two related statistics,  $Q$  and  $I^2$ . Studies were characterized as possibly having heterogeneity using the interpretation guide provided in the Cochrane Handbook for  $I^2$  which is expressed in percentages: 0-30% might not be important heterogeneity, 30-50% is considered to be moderate heterogeneity, 50% and above as substantial heterogeneity. To assess for outliers within the meta-analysis, visual inspection of the forest plots as well as a formal statistical test identified by Viechtbauer for use with meta-analyses (DFBETAs) was used (41). All study analyses were run in the Metafor package for R statistical software (42).

## Results

### Literature Search

Figure 1 summarizes the flow of the search process corresponding to PRISMA guidelines for meta-analytic searches. A total of 17 studies were included in the final quantitative meta-analysis (43-59). Complete psychosocial data for the male participants was available in the published articles or obtained from study authors for 13 of the 17 articles included in the present analysis. Four authors (Foley, Oh, Lehto, Zhang) did not respond to requests for data on male participants (48,50,52,59). For these studies, male data was assumed to reflect the published findings for both men and women. This assumption was made after obtaining a “male data only” ratio, which compared the cumulative effect sizes found for studies that provided data on men alone in the study and compared these effect sizes to the effect found by the same studies including the full sample of both men and women. We found that there was no appreciable difference between these two calculations ( $g=.58$ , 95% CI  $-.34$  to  $1.5$ ,  $p=.2$ , for the published

data which pooled men and women together, and  $g=.61$ , 95% CI  $-.32$  to  $1.5$ ,  $p=.2$ , for the author supplied data of male only data from the same studies) suggesting that the assumption to include these four studies' published data in the final analysis is reasonable for the purposes of the present analysis. Sensitivity analyses were conducted without including these four studies which did not significantly change the reported results.

### **Participant characteristics**

A total of 666 male cancer patients from 17 studies were included in the meta-analysis. Demographic information collected included age, race, marital status, employment, education level, cancer stage, and cancer type. Results are described in Table 1. The average age of participants was 61.9 years (SD 11.1). Of those ( $n = 12$  studies) reporting race/ethnicity, 73% of the sample was Non-Hispanic white. For studies outside of China and Taiwan ( $n = 10$ ), 87% of the participants were Non-Hispanic white. Overall, 80.5% of the men were married and 45% were employed. Of those reporting education level ( $n = 9$  studies), the majority of the sample was college educated (62%). There were a variety of cancer types represented, with more than three cancer types represented in five studies, followed by exclusively prostate and lung cancer populations in four studies.

### **Study Designs**

Electronic Supplementary Table 1 displays the relevant characteristics of the selected studies. Descriptively, the MCIs varied widely both within and between the three major intervention categories (Mindfulness-based (MB), Tai Chi/Qigong (TCQ), and Yoga). Most interventions were held in-person and in a group-based setting ( $n=11$ ), although a few took place at patient's home ( $n=2$ ) by phone or online ( $n=2$ ) or in a hospital setting ( $n=1$ ). Intervention length ranged from as few as three sessions to as many as two sessions per week for 12 weeks,

with session length ranging from 20-150 minutes. Control groups were categorized as either usual care (which included waiting lists) or active controls (e.g. exercise, education groups, and attention controls). Inclusion criteria varied widely with most studies having minimal criteria beyond a cancer diagnosis.

### **Risk of Bias**

Studies were assessed for their risk of bias using the NIH Quality Assessment of Controlled Intervention Studies (Electronic Supplementary Appendix 1). The inter-rater reliability for the raters was found to have substantial agreement with Kappa = 0.84 ( $p < 0.001$ ), 95% CI (.72,.94), for the full set of quality coding. All disagreements were resolved via meetings of the three coders to achieve consensus. Only one study, Ben-Josef, was flagged as having a 50% of the bias categories as high or unclear (Table 2). The most common category that flagged for a high risk of bias was high attrition, defined as greater than 20% of the intervention or control group. The majority of the studies (12 of 17) demonstrated this risk. The other categories in which more than half of the studies demonstrated potential for bias were in measuring high adherence to the intervention, having adequate power to detect a small effect, and conducting an intention-to-treat analysis to account for study drop-outs.

Publication bias was assessed via visual inspection of the funnel plot of the studies and carried out by an Eggers' regression test of plot asymmetry. Aside from one outlier, the funnel test appeared to be appropriately asymmetrical and the regression test had an insignificant p-value ( $p = .60$ ) and thus no publication bias seemed to exist in the current sample (60).

### **Outcome measures**

The psychosocial outcomes measured covered a diverse range of psychological and social outcomes and therefore were broadly grouped into three categories: Health-Related

Quality of Life (HRQOL), Negative Mental Health (NMH), and Positive Mental Health (PMH) (c.f. Electronic Supplemental Tables 1 and 2 for a complete list and summary of included measures). Because of the lack of uniformity in outcome measurements, even within individual constructs, there was greater statistical power to detect differences between groups when these were pooled together rather than analyzed at the construct or individual measure level. The meta-analysis results below considered these three groups as moderators of the overall effects found.

### **Effect of MCIs on Combined Psychosocial outcomes**

Table 3 provides a statistical summary found across all outcomes as well as within subgroups. The 17 studies included 43 separate psychosocial outcomes measures at short or longer-term follow-up. When multiple measures were used to assess one of the three outcome measure categories, a variable was created for average effect size, using the weighted variance formula suggested of Borenstein et al (36). For the calculation of the overall effect, measures for which a lower score indicates better health (e.g. negative mental health measures) were multiplied by -1 to compare all outcomes in the same direction, such that higher scores were indicative of greater symptom burden.

For post-intervention follow-up scores using the synthetic variables, the Q value ( $p < .0001$ ) suggested substantial heterogeneity with  $I^2$  of 89.98%. The overall effect size, based on a random effects model, indicated a beneficial effect of MCIs on psychosocial outcomes in male cancer patients (Hedges'  $g = .46$ , 95% CI:  $-.03 - .95$ ,  $p < .06$ ). Follow-up analyses using both a visual inspection of the forest plot and statistical test for outliers indicated that one study (Chuang) had an outsized impact on the heterogeneity value and the overall effect size (46). As studies with a DFBETA value greater than 1 are considered unusually influential (the value was 1.98 for the Chuang study), there was reasonable rationale for removal of this study from the

analysis. Removing this one study from the dataset produced a modestly heterogeneous finding with a  $Q$  value of  $p=.03$ , with an  $I^2$  of 40%. There was also a significant but more modest beneficial effect found for MCIs when excluding this one study (Hedges'  $g = .23$ , 95% CI .02 to .44,  $p=.03$ ). This study was removed from subsequent results in this results section

For short-term follow-up scores (e.g. those conducted within three months of intervention completion), 15 measures from 5 studies were available for analysis (45,47,49,53,59) .

Heterogeneity was very low ( $p$ -value of  $Q = .25$ ,  $I^2$  of 0%) and the effect size was much more modest, and non-significant than the post-intervention effect found for all studies (Hedges'  $g = .09$ , 95% CI  $-.10$  to  $.29$ ,  $p=.35$ ). For comparison, these same five studies, that measured a short-term follow up, had similar post-intervention results (Hedges'  $g=.12$ , 95% CI  $-.07$  to  $.32$ ). Only two of the seventeen studies reported long-term follow up scores (i.e. longer than 3 months' post-intervention) (45,57). For these two studies, the effect was nearly identical to the five studies evaluated for short-term follow-ups. There was low heterogeneity but a non-significant small effect size (Hedges'  $g = .08$ , 95% CI  $-.15$  to  $.31$ ,  $p=.50$ ).

### **Active versus usual care control**

An analysis of studies with usual care groups ( $n=10$ ), was non-significantly heterogeneous ( $Q=7.7$ ,  $p=.56$ ,  $I^2=1\%$ ) and had strong evidence in favor of a small effect (Hedges'  $g = .26$ , 95% CI .10 to .42,  $p=.001$ ) (43,45,47,48,50,52,53,55,57,58) . This was in stark contrast to an analysis of studies with active control groups ( $n=6$ ) which had substantial heterogeneity ( $Q=18.3$ ,  $p=.002$ ,  $I^2=76\%$ ) and did not have strong evidence for an effect in favor of the control group (Hedges'  $g = -.07$ , 95% CI  $-.79$  to  $.64$ ,  $p=.84$ ) (44,49,51,54,56,59).

Removing the one study that found an intervention effect, and which included significantly more

intervention time and intensity (Campo), produced a highly non-heterogeneous result ( $Q=2.32$ ,  $p=.6$ ,  $I^2=5\%$ ) of a null effect (Hedges'  $g = -.31$ , 95% CI  $-.68$  to  $.07$ ,  $p=.11$ ).

### **Meditative Movement versus Mindfulness Based Interventions**

Another subgroup analysis that was carried out was based on whether the intervention was a MBI intervention ( $n=9$ ) or whether the study was a meditative movement intervention ( $n=7$ ) (i.e. pooled studies of TCQ or Yoga). The MBIs were non-significant for heterogeneity of  $Q$  ( $p=.23$  and  $I^2$  of 0%) (45,48-51,53,55,57,58). The effect size, which was statistically significant, approached the threshold for a small effect (Hedges'  $g = .17$ , 95% CI  $.00$  to  $.35$ ,  $p=.05$ ). For the meditative movement interventions, there was a high degree of heterogeneity  $Q$  with a  $p=.007$ , and  $I^2$  of 74% (43,44,47,52,54,56,59). The non-significant effect size was slightly larger for meditative movement than for the MBIs (Hedges'  $g = .28$ , 95% CI  $-.24$  to  $.80$ ,  $p=.29$ ). The large heterogeneity for this pooled analysis prompted a follow-up analysis of the TCQ and Yoga separately. Neither category suggested strong evidence of an effect at the level of a summary psychosocial variable (Yoga was not heterogeneous,  $Q$  ( $p =.54$ ) and  $I^2$  of 0%, Hedges'  $g = .29$ , 95% CI  $-.17$  to  $.76$ ,  $p=.21$ , TCQ was highly heterogeneous,  $Q$  ( $p=.001$ ) and  $I^2$  of 88%, Hedges'  $g = .29$ , 95% CI  $-.5$  to  $1.1$ ,  $p=.5$ ).

### **Negative Mental Health (e.g. Depression, Anxiety, Distress)**

Fourteen studies had at least one measure of negative mental health (e.g., depression, anxiety, psychological distress) (44,45,47-53,55-59). There was moderate heterogeneity across these measures ( $Q=23.8$ ,  $p=.03$ ;  $I^2= 44\%$ ) and non-significant trend toward a small beneficial effect on negative mental health among the MCIs (Hedges'  $g = .20$ , 95% CI  $-.03$  to  $.43$ ,  $p=.09$ ). As with the results from considering all psychosocial measures together, when sub-analyzed by control condition ( $n=9$ ), usual care again showed a small effect (Hedges'  $g = .20$ , 95% CI  $.02$  to

.3,  $p=.02$ ;  $Q=7.17$   $p=.5$ ;  $I^2=0\%$ ) (45,47,48,50,52,53,55,57,58). The studies with active controls ( $n=5$ ) did not demonstrate strong evidence for any beneficial effect on negative mental health (Hedges'  $g = .02$ , 95% CI  $-.72$  to  $.76$ ,  $p=.02$ ;  $Q=15.6$   $p=.003$ ;  $I^2=77\%$ ).

### **Health Related Quality of Life**

Eight studies measured Health Related Quality of Life (HRQOL) (43,45,48,51-54,56). There was strong evidence for a small beneficial effect of MCIs on HRQOL at post-intervention (Hedges'  $g = .22$ , 95% CI  $.01$  to  $.44$ ,  $p=.04$ ;  $Q=8.1$ ,  $p=.32$ ;  $I^2=2\%$ ). A difference was found when comparing the studies utilizing active controls versus those with usual care. The usual care group had strong evidence for a small beneficial effect of MCIs on HRQOL immediately post-intervention (Hedges'  $g = .30$ , 95% CI  $.05$  to  $.53$ ,  $p=.01$ ;  $Q=3.6$ ,  $p=.44$ ;  $I^2=10\%$ ;). Meanwhile the active control groups had a trend toward a medium negative effect on HRQOL for MCIs at post-intervention (Hedges'  $g = -.59$ , 95% CI  $-1.40$  to  $.21$ ,  $p=.15$ ;  $Q=3.6$ ,  $p=.44$ ;  $I^2=10\%$ ).

### **Positive Mental Health**

Five studies measured positive mental health constructs (e.g., post-traumatic growth, self-compassion, spirituality) at the post-intervention time-point (45,51,53,57,58). An analysis of these five studies yielded a small beneficial effect in favor of MCIs, that was non-heterogeneous, but not statistically significant (Hedges'  $g = .21$ , 95% CI  $-.22$  to  $.65$ ,  $p=.33$ ;  $Q=8.9$ ,  $p=.06$ ;  $I^2=51\%$ ). All of these studies were mindfulness-based interventions so no comparison was possible with meditative movement interventions. Only one of the studies used an active control group (Nakamura), while four of the studies were usual care comparison groups. Analyzing the four studies that were usual care conditions together, also demonstrated a small beneficial effect in favor of the MBIs, that was non-heterogeneous (Hedges'  $g = .29$ , 95% CI  $.05$  to  $.53$ ,  $p=.01$ ;  $Q=3.7$ ,  $p=.44$ ;  $I^2=10\%$ ). Two studies measured positive mental health at short-term and long

term follow-up. There was a statistically significant small beneficial effect at short-term follow-up (Hedges'  $g = .30$ , 95% CI .00 to .60,  $p=.05$ ;  $Q=.56$ ,  $p=.45$ ;  $I^2=0\%$ ) and a non-significant small effect at long-term follow up (Hedges'  $g = .18$ , 95% CI -.10 to .47,  $p=.21$ ;  $Q=.18$ ,  $p=.67$ ,  $I^2=0\%$ ).

## Discussion

This meta-analysis is among the first to report on the effect of mindfulness-based and meditative movement interventions (MCIs) in male cancer patients. Several analyses demonstrated evidence of an effect in favor of MCIs immediately post-intervention on psychosocial outcomes for men with cancer. With the removal of one outlier from the seventeen eligible studies for this meta-analysis, the overall impact of MCIs on male cancer psychosocial outcomes immediately post intervention was small ( $g=.23$ ). Because of the modest heterogeneity considering the total sample, sub-groups were analyzed to test for intervention effects. Whether studies had a usual care group versus an active control condition differed in psychosocial outcomes as studies that had a usual care control group demonstrated a small effect in favor of MCIs ( $g=.26$ ) while studies with active control groups (except one study) failed to demonstrate a significant effect for or against the interventions. MBIs, as a category, demonstrated a small effect when considering all psychosocial outcomes together ( $g=.17$ ). Considering outcomes by psychosocial domain yielded nearly identical effect sizes across each of the three categories with only health related quality of life outcomes reaching significance ( $g=.22$ ). At short term follow-up assessments, there was a small effect for positive mental health outcomes ( $g=.30$ ).

There are several important findings from this meta-analysis of male cancer patients who participated in MCI randomized controlled trials. Perhaps most notable are the small effect sizes found for psychosocial outcomes immediately post-intervention. These effect sizes are



appreciably more modest than the outcomes reported in much of the mindfulness-based and meditative movement literature for both the general population as well as for (predominantly female sex) cancer populations (61). Owing perhaps to the relatively modest statistical power with smaller sub-samples of men in the present analysis, there were non-significant effects for a number of the major outcomes of interest. The effects found are quite close to those found in a recent meta-analysis of Tai Chi and Qigong for cancer related symptoms that included a similar number of RCTs although 75% of the study population was female and it had twice as many participants as the current meta-analysis (25).

Another important finding of the current study was the moderating effect of the type of control group used. Although usual care and wait-list conditions are more standard for establishing an effect of an intervention relative to no treatment, MBSR, Tai Chi/Qigong, and Yoga interventions are at the stage of needing to be tested against active controls in randomized controlled trials in cancer populations (62).

In addition to concerns about the quality of the control comparisons used, there was a lack of long-term, and even short-term, follow-up assessments beyond the immediate intervention period in otherwise very high-quality studies. One intriguing signal was the presence of statistically significant small effect for positive mental health found at the short-term follow-up. Although there was limited power and results to demonstrate effects across other domains, this finding highlights the possibility that some effects may grow larger over time for certain interventions, and thus immediate post-intervention results may downplay their ultimate impact. For instance, breast cancer survivors randomized to a MBSR condition improved on post-traumatic growth scores slowly such that their long-term follow-up scores were greater than their post-intervention results (63). Long-term follow-up studies are also important for

establishing to what degree the gains seen on psychosocial outcomes post-intervention are durable, as well as what factors (e.g. continued home practice) might lead to longer lasting benefits.

There was a high degree of heterogeneity in the current sample as it included large definitive and longitudinal trials as well as pilot studies. However, given the opportunity costs of administering high quality clinical trials, there is a need to demonstrate durable effects for interventions. There is fairly robust evidence for MBI and meditative movement interventions to yield small to medium effect sizes on psychosocial outcomes at post-intervention, but it is imperative that these gains be maintained over time to demonstrate efficacy of effects. Given the time and cost of recruiting and running large scale trials, longitudinal follow-ups (e.g. 3, 6 and/or 12 months) should be required for consideration of future trials in supportive oncology. In addition to measuring the long-term outcomes of participants in MCIs, studies should also take care to measure adherence, particularly post-intervention. Optimizing dosage and design of interventions is necessary but not sufficient if adherence to continuing practice is necessary to maintain improvements to psychosocial health.

Contrary to expectations in the creation of the male data ratio, there was no significant effect size change between the data reported by authors for men and the data reported for all participants, men and women, in the published studies. While it is possible that men may do worse in the few studies that did not provide the data on male participants, it seems more likely that on average, men experience small psychosocial gains following the completion of MCIs. However, there is not strong evidence for improvement on these outcomes when an active control group is used as the comparison. This finding differs from the published meta-analytic reviews for breast cancer. However, even the effect sizes found for MCIs in breast cancer

survivors have become more modest over time, perhaps reflecting higher quality studies included in the analyses (23). It could be that men, and studies that include a significant number of men, are different in important ways that impact study outcomes. These could be due to differences in expectancy effects. It could also be true as well that as higher quality studies are conducted, the initial moderate to large effect sizes for psychosocial outcomes will become more modest across all kinds of MCIs.

Although there is unlikely sufficient power to detect a difference in this analysis between MBIs and meditative movement studies, there is a signal in the direction of men benefitting more on psychosocial outcomes from meditative movement interventions (e.g. Yoga and Tai Chi) as compared to interventions that do not include exercise (e.g. MBIs). Notably the one study (Campo) that included an active control group, and still found a significant effect in favor of the MCI, had an intervention that included significantly higher exercise intensity and frequent classes (twice a week for 12 weeks) compared to other interventions tested. Whether meditative movement interventions have a superior effect for men with cancer is an empirical question that has yet to be demonstrated. However, meditative movement interventions may be viewed as more active and “athletic” compared to mindfulness based interventions, which could increase the acceptability of these interventions among men. Although our meta-analysis has referred to biological sex as the description for men, the impact of gender expectations and roles is certainly an important factor to consider. There could be cohort effects as the male cancer survivors are, on the whole, over the age of sixty. Although the present study suggests that men may still improve on these outcomes, perhaps less than compared to studies of women, there is still a need to assess the impact of such interventions on racially and ethnically diverse populations, and the mechanisms underlying intervention effects. There may well be additional challenges in

recruiting such populations, and the interventions may have to be adapted to be culturally relevant.

There are also several important limitations to the current study. One limitation is the relatively high drop-out rate in 12 of the 17 studies under consideration. Few studies used intent-to-treat analyses to account for study drop-outs, and higher rates of drop out are a risk for survivor bias. Drop-out, as well as lack of interest in study participation, is particularly problematic with male populations in health care settings in general as well as in mindfulness interventions. It may be that the men who could benefit the most, are the least likely to enroll in such programs (9). This is a particular concern with a male population which may be less likely to join mindfulness-based and meditative moment kinds of interventions (27). Several studies included in the present analysis reported potential participant's reasons for declining participation (Campo, Nakamura, Sohl, Victorson, Zernike) with only one (Victorson) formally reporting on percent enrolled. Common reasons for study refusal of those reporting included lack of interest, study distance, and lack of time. As reporting of study uptake is usually limited to pilot and feasibility studies, this too should be considered a limitation for assessing potential study bias.

Another limitation is the use of a "male data only ratio" to assume the total sample findings applies to the men in the studies when gender-specific effects were not reported and could not be obtained from the authors. While we felt this approach was a reasonable one given the comparison to other studies reporting total sample and male sex information separately, this remains an assumption on our part. A related limitation is our use of a composite summary variable that summarized all psychosocial outcomes from a single study. Given the relative lack of studies focused on psychosocial outcomes for men in MCI interventions, we wanted to cast a

wide net to summarize existing evidence. We used the  $I^2$  test of heterogeneity as one test of the appropriateness of combining across three domains and found an acceptably low value (40%) for all psychosocial outcomes using a single value from each of the 16 included studies. In addition to reporting the combined psychosocial scores, we have also examined each psychosocial domain independently. However, this approach has not been widely used and should be considered as a limitation. As more high quality RCTs using MCIs in diverse populations are conducted, it will be possible to summarize the efficacy of these interventions with greater precision with studies using the same outcomes within different psychosocial domains.

Another limitation is the significantly heterogeneous population of men included in terms of cancer type, stage, and treatment. Prostate cancer, which has high survival rates, is the most common type of cancer represented although men with cancers that have poorer survival prognosis, including lung cancer, are also included. The lack of reported information on these cancer types limits the power to detect potential moderators to understand what works, when, and for whom along the cancer care continuum. It could be that there are particularly important times for certain men to join in MCIs. And while the men included in the present analysis is quite diverse in terms of cancer treatment, they are over-represented by college-educated, white non-Hispanic men. Given this lack of diverse representation, it may also be important to consider how to adapt existing MCIs, including increasing awareness of and access to these interventions, as well as to develop new interventions that better target the downstream effects of cancer in men. These interventions should be developed with particular attention to feasibility and acceptability with respect to enrolling and retaining men from diverse backgrounds.

Psychosocial outcomes were chosen as the outcomes of consideration for this study as they are the most commonly measured across the MCIs we studied. They are also plausibly the most sensitive to change based on the traditional “symptom” targets of mindfulness-based interventions (e.g., MBSR). More than half of the studies had psychosocial outcomes as their reported primary outcome. As noted above, even when they are the primary outcome of interest of the study, participants are not always recruited or stratified on symptom levels. This could be an example of range restriction and having little room for improvement on outcomes such as anxiety and depression if the majority of participants are neither anxious nor depressed at baseline. A related limitation is that few studies included measures of personality traits (e.g. openness to experience, conscientiousness) which contribute to enrollment bias and may have accounted for some of the benefits seen post-intervention.

In sum, this meta-analysis suggests that men participating in MCIs appear to gain some benefit, albeit small, on psychosocial outcomes compared to control conditions. However, there is only limited evidence that these benefits are found when there is an active control group, or that these psychosocial improvements are durable when assessed at future time points. Future research is needed to help confirm these findings and also to clarify the boundary conditions of these effects. Greater number of participants, and larger numbers of studies, will help to more precisely identify men who might benefit most from these treatments of interest. In addition to finding out what works, including mechanisms underlying intervention effects, there may be particularly sensitive times or interventions which may not demonstrate a benefit for men. Continuing to summarize and expand this evidence base will help oncologists and healthcare providers make appropriate referrals for men diagnosed with cancer, undergoing cancer treatment, and those who have completed cancer treatment.

Figure 1

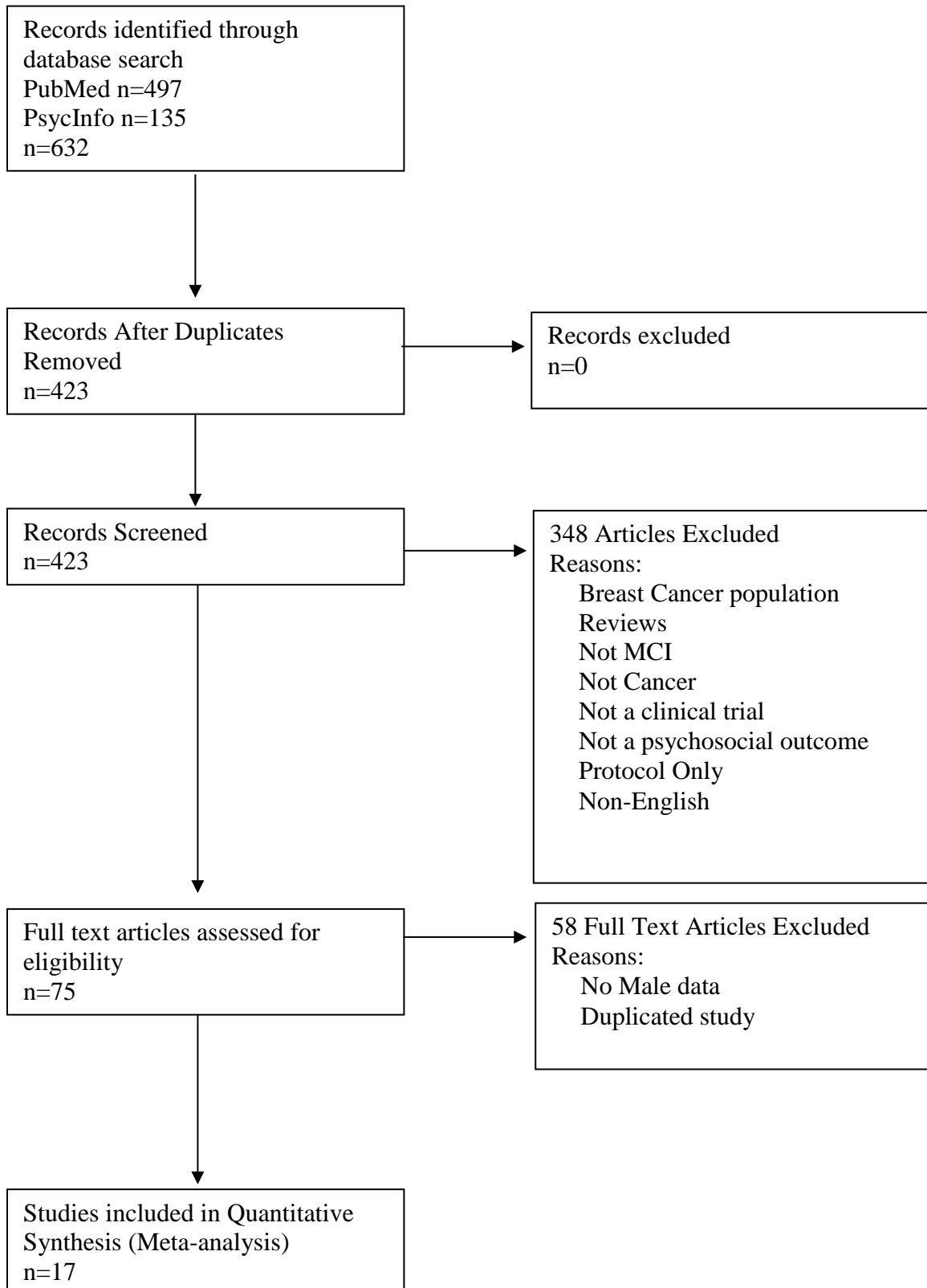
*PRISMA Search Flowchart*

Table 1

*Demographic Information: Limited to information supplied by study authors on demographics of male participants*

	Category	Frequency	Percent
Age	Mean (SD)	61.19 (11.14)	
	Total	666	
Race/Ethnicity	White	433	72
	Non-White	168	28
	Total	601	
Relationship Status	Married	481	80
	Non-Married	120	20
	Total	601	
Employment	Employed	196	46
	Retired or Not-Working	231	54
	Total	427	
Education	HS or Less	164	38
	College of More	268	62
	Total	432	
Cancer Stage	1 or 2	68	38
	3 or 4	111	59
	Total	179	



Table 2

*Risk of Study Bias*

	Random	Tx Allocation	Blind Assessor	Baseline Similar	Low Drop Out	Diff Drop Out	High Adherence	Background Tx	Valid Measures	Power	Pre-specified	ITT Analysis
Ben-Josef	Yes	Yes	CD	No	No	No	No	Yes	Yes	Yes	Yes	No
Campo	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes	No
Chambers	Yes	Yes	Yes	Yes	No	Yes	No	Yes	Yes	Yes	Yes	Yes
Chuang	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	No
Cohen	Yes	Yes	NR	Yes	No	NR	No	Yes	Yes	Yes	Yes	No
Foley	Yes	Yes	Yes	Yes	Yes	Yes	Yes	NR	Yes	Yes	Yes	Yes
Garland	Yes	Yes	Yes	Yes	No	No	CD	Yes	Yes	No	Yes	Yes
Lehto	Yes	Yes	CD	Yes	Yes	Yes	NR	CD	Yes	No	Yes	No
Nakamura	Yes	Yes	NR	CD	Yes	Yes	Yes	Yes	Yes	NR	Yes	Yes
Oh	Yes	Yes	CD	Yes	No	Yes	No	Yes	Yes	Yes	Yes	Yes
Schellekens	Yes	Yes	CD	Yes	No	Yes	NR	Yes	Yes	No	Yes	Yes
Sohl	Yes	Yes	NR	CD	No	Yes	Yes	CD	Yes	No	Yes	No
Specia	Yes	Yes	CD	Yes	No	Yes	No	Yes	Yes	No	Yes	Yes
Vanderbyl	Yes	Yes	Yes	Yes	No	No	No	Yes	Yes	No	Yes	NR
Victorson	Yes	Yes	Yes	Yes	No	Yes	CD	Yes	Yes	No	Yes	No
Zernicke	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes
Zhang	Yes	Yes	No	Yes	No	Yes	CD	Yes	Yes	No	Yes	No

No	High Risk of Study Bias
CD	Unclear Risk of Study Bias
Yes	Low Risk of Study Bias

Table 3

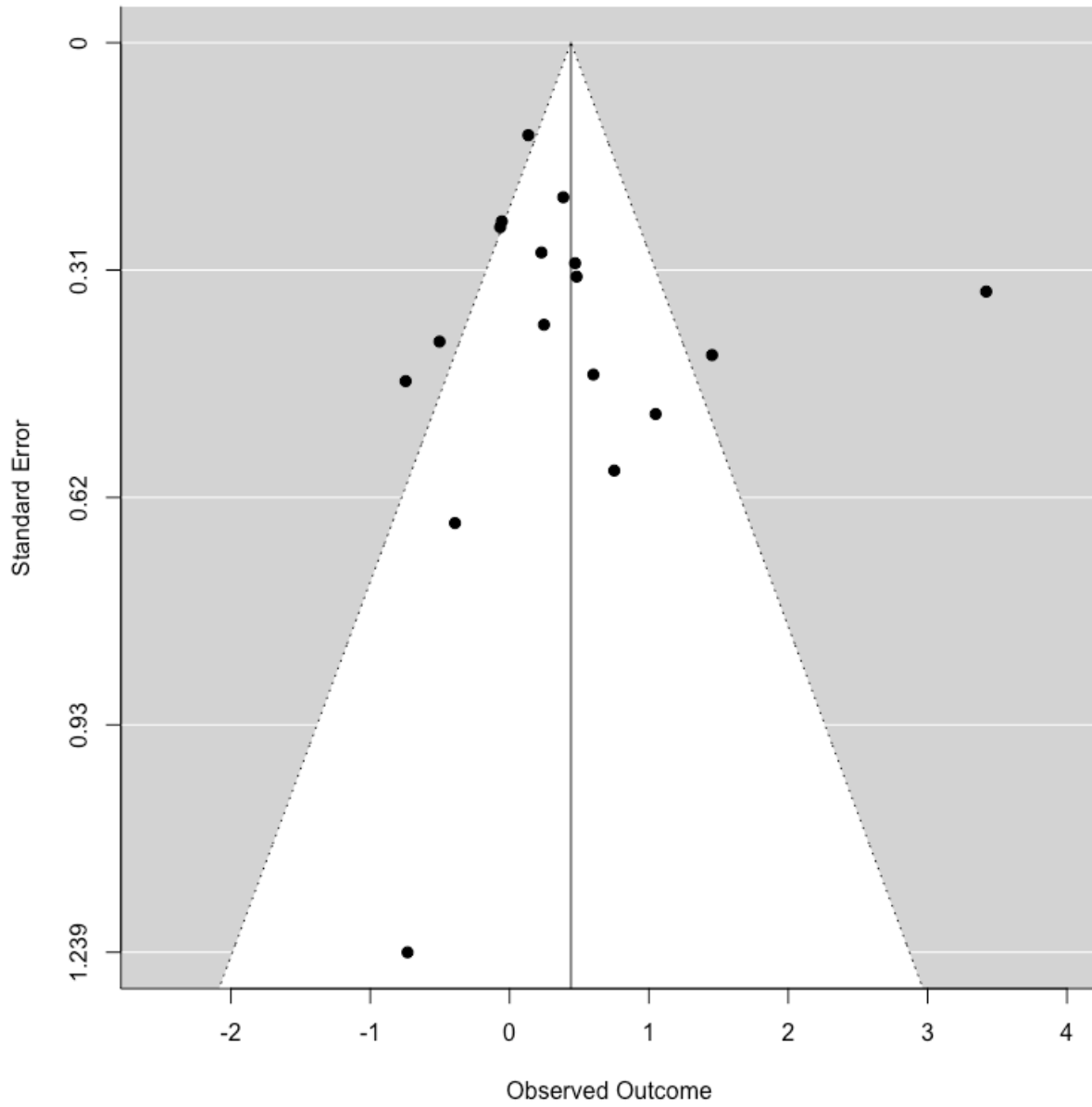
*Summary of Effect Sizes*

Analysis	Hedges'		CI LL	CI UL	k	Q	Q p value	I <sup>2</sup>
	<i>g</i>	<i>p</i>						
All Outcomes								
Post	0.44	0.06	-0.02	0.90	17	112.48	0.00	89
<b>Post (-outlier)</b>	<b>0.23</b>	<b>0.02</b>	0.02	0.44	16	26.50	0.03	40
Short-Term follow-up	0.09	0.35	-0.10	0.29	5	7.07	0.13	0
Long-Term follow-up	0.18	0.21	-0.10	0.47	2	0.18	0.67	0
Active Control Only	-0.07	0.84	-0.79	0.65	6	16.53	0.01	73
<b>Usual Care Only</b>	<b>0.26</b>	<b>0.00</b>	0.10	0.42	10	7.71	0.56	1
<b>MBI Only</b>	<b>0.17</b>	<b>0.05</b>	0.00	0.35	9	10.41	0.23	0
Med. Movement Only	0.28	0.29	-0.24	0.80	7	17.47	0.00	74
Yoga Only	0.30	0.21	-0.16	0.76	3	1.19	0.54	0
TCQ Only	0.29	0.52	-0.59	1.18	4	16.20	0.00	88
HRQOL								
<b>Post</b>	<b>0.22</b>	<b>0.04</b>	0.01	0.44	8	8.04	0.32	2
Active Control Only	-0.59	0.15	-1.40	0.21	3	0.15	0.92	0
<b>Usual Care Only</b>	<b>0.30</b>	<b>0.01</b>	0.05	0.53	5	3.69	0.44	10
MBI Only	0.23	0.17	-0.10	0.56	4	3.37	0.34	14
Med. Movement Only	0.15	0.56	-0.35	0.66	4	4.63	0.20	39
NMH								
Post	0.20	0.09	-0.03	0.43	14	23.82	0.03	44
Active Control Only	0.02	0.96	-0.72	0.76	5	15.67	0.00	77
<b>Usual Care Only</b>	<b>0.20</b>	<b>0.02</b>	0.02	0.38	9	7.18	0.51	0
MBI Only	0.16	0.16	-0.05	0.31	9	8.99	0.34	0
Med. Movement Only	0.30	0.37	-0.35	0.95	5	14.45	0.00	78
PMH								
Post	0.21	0.33	-0.22	0.65	5	8.94	0.06	51
<b>Usual Care Only</b>	<b>0.29</b>	<b>0.01</b>	0.05	0.53	4	3.70	0.44	10
<b>Short- Term Follow-up</b>	<b>0.30</b>	<b>0.05</b>	0.00	0.60	2	0.56	0.45	0
Long-Term Follow-up	0.18	0.21	-0.10	0.47	2	0.18	0.67	0

Notes: Significant values in bold text. MBI: Mindfulness-Based Interventions, HRQOL: Health-Related Quality of Life, NMH: Negative Mental Health, PMH: Positive Mental Health. MBI and Med. Movement subgroups not reported for PMH outcomes as all five studies were MBI interventions, Active control only not reported as four of the five studies were usual care only.

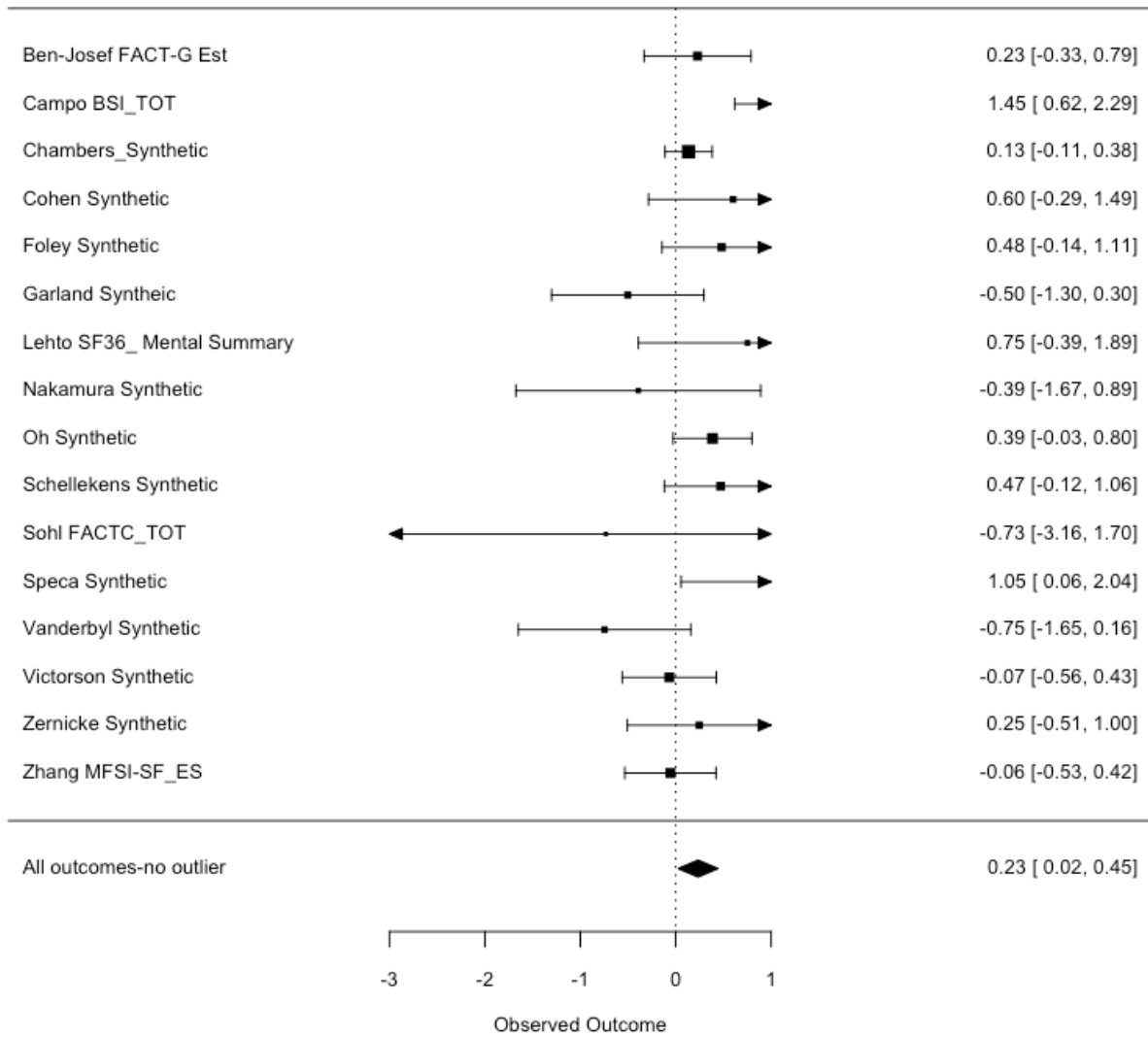
Analysis 1

*Funnel Plot for Visual Inspection of Publication Bias (n=17)*



## Analysis 2

*Forest plot of all outcomes with outlier removed (effect size above zero favors intervention)*



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## Supplemental Table 1

*Study Characteristics*

<b>First Author</b>	<b>Year</b>	<b>Intervention</b>	<b>Intervention Dose</b>	<b>Inclusion Criteria</b>	<b>Total N</b>	<b>Total Men</b>	<b>%Men</b>	<b>Cancer Type</b>
Ben-Josef	2017	Yoga	Twice Weekly, 75 min each, 6-9 weeks	Non-Mets PCa	50	50	100.00%	Prostate
Campo	2014	TCQ	Twice weekly, 60 min each, 12 weeks	Self reported fatigue (clinically meaningful), non-mets PCa	40	40	100.00%	Prostate
Chambers	2017	MBCT	Once weekly, 75 min, 8 weeks	Mets PCa	189	189	100.00%	Prostate
Chuang	2017	TCQ	21 consecutive days, at home, 20 min	TCQ Naïve, Receiving Chemotherapy	96	55	57.29%	NHL
Cohen	2004	Yoga	Once, weekly, time not specified, 7 weeks	Lymphoma	38	26	68.42%	Lymphoma
Foley	2010	MBCT	Once weekly, 120 min, 8 weeks	Adult cancer patients	115	26	22.61%	Mixed (Lymph, PCa, Bowel)
Garland	2014	MBSR	MBSR, once weekly, 90 min, 8 weeks; CBT-1 once weekly, 90 minutes, eight weeks	Insomnia, completed chemo or radiation one month prior	72	20	27.78%	Mixed (PCa,Lymph, Head/Neck)
Lehto	2015	MBSR	once weekly, 45 min, 6 weeks, in home	Active treatment, stage 3 or 4 NSCLC	40	13	32.50%	Lung
Nakamura	2013	MBSR	once weekly, 2 hours, 3 weeks	Any cancer, self-reported sleep disturbance	57	14	24.56%	Mixed (PCa, NHL, Testicular)
Oh	2010	TCQ	twice weekly, 90 min each, 10 weeks	TCQ Naïve, any stage of cancer	162	69	42.59%	Mixed (Lung, PCa, Colorectal)

<b>First Author</b>	<b>Year</b>	<b>Intervention</b>	<b>Intervention Dose</b>	<b>Inclusion Criteria</b>	<b>Total N</b>	<b>Total Men</b>	<b>%Men</b>	<b>Cancer Type</b>
Schellekens	2017	MBSR	once weekly, 150 min, 8 weeks	MBSR Naïve, any stage lung cancer	63	30	47.62%	Lung
Sohl	2017	Yoga	three sessions, time not specified	within 2 weeks starting chemo	15	9	60.00%	Colorectal
Specia	2000	MBSR	once weekly, 90 minutes, seven weeks	Adult cancer patients	90	17	18.89%	Mixed (PCa, NHL, Colorectal)
Vanderbyl	2017	TCQ	once weekly, 45 min, 6 sessions	Stage 3 or 4 Lung or GI cancer	24	14	58.33%	GI/Lung
Victorson	2016	MBSR	once weekly, 150 min, 8 weeks	PCa Active Surveillance	43	43	100.00%	Prostate
Zernicke	2014	MBCT	once weekly, online, 120 min, 8 weeks	Completed Ca treatment, moderate distress, MBSR naïve	62	17	27.42%	Mixed (Lymph, Colorectal, PCa)
Zhang	2016	TCQ	every other day, one hour, four courses over 85 days of treatment	lung cancer, receiving chemo, TCQ naïve	91	68	74.73%	Lung

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Notes: TCQ: Tai Chi Qigong, MBCT: Mindfulness Based Cognitive Therapy, MBSR: Mindfulness-Based Stress Reduction, MBI: Mindfulness-Based Interventions, HRQOL: Health-Related Quality of Life, NMH: Negative Mental Health, PMH: Positive Mental Health, NSCLC: Non-small-cell lung carcinoma, PCa: Prostate Cancer, Non-Mets: Cancer not metastasized, GI: Gastrointestinal, NHL: Non-Hodgkin's Lymphoma

Supplemental Table 1 Cont.

*Study Characteristics Cont.*

<b>First Author</b>	<b>Psychosocial Measures Used</b>	<b>Psychosocial Outcomes as Study Aims (Primary vs. Secondary)</b>	<b>Intervention Type</b>	<b>Control Group</b>	<b>Active Control?</b>	<b>Country</b>	<b>Follow-up Timepoints</b>	<b>Included Mindfulness Measure</b>
Ben-Josef	FACT-G	Secondary	In-person, Group	Usual Care	No	USA	Pre,Post	NR
Campo	BSI-18	Secondary	In person-Group	Stretching	Yes	USA	Pre,Post	NR
Chambers	BSI-18, IED, MAX-PC, PTG, FACT-P	Primary	Telephone	Usual Care	No	Australia	Pre,3,6,9 months	FFMQ
Chuang	QLQ 30 (Global)	Secondary	Instruction +At home	Usual Care	No	Taiwan	Pre, Post (3 wk)	NR
Cohen	STATE, CES-D, IES	Primary	In-person - Group	Waitlist	No	USA	Pre, Post (3 mos)	NR
Foley	DASS, FACT-G, HAM-A, HAM-D	Primary	In person - Group	Waitlist	No	Australia	Pre,Post, 3 month	FMI
Garland	CSOSI, POMS	Secondary	In Person - Group	CBT-I	Yes	Canada	Pre, Post, 5 month	NR
Lehto	SF-36 (Mental Health Summary)	Primary	In Home	Usual Care	No	USA	Pre, Post, 1 month FU	NR
Nakamura	CES-D, FACT-G, IES, PSS, SCS	Secondary	MBSR and MBB	Active (Sleep Hygiene)	Yes	USA	Pre, Post, 2 month	FFMQ
Oh	FACT-G, POMS	Primary	In person-Group	Usual care	No	Australia	Pre, Post	NR

<b>First Author</b>	<b>Psychosocial Measures Used</b>	<b>Psychosocial Outcomes</b>	<b>Intervention Type</b>	<b>Control Group</b>	<b>Active Control?</b>	<b>Country</b>	<b>Follow-up Timepoints</b>	<b>Mindfulness Measure</b>
Schellekens	HADS, IED, QLQ-30 (Global), SCS	Primary	In Person - Group	Usual Care	No	Netherlands	Pre, Post, 3 month	FFMQ
Sohl	FACT-C	Secondary	In-person, Hospital	Attention control	Yes	USA	Pre, Post	NR
Specia	POMS, SOSI	Primary	In Person-Group	Waitlist	No	Canada	Pre, Post	NR
Vanderbyl	HADS	Primary	In Person-Group	Exercise	Yes	Canada	Pre, Post (Crossover design)	NR
Victorson	MAX-PC, PGH-10 MH, PTGI	Primary	In Person-Group	Usual Care + Book	No	USA	Pre, Post, 6 mos, 12 mos	MAAS
Zernicke	FACIT-SP, POMS, PTGI, SOSI	Primary	Online	Waitlist	No	Canada	Pre, Post	FFMQ
Zhang	MFSI-SF	Secondary	At home	Light Exercise	Yes	China	Pre, 6 week, 12 week	NR

Notes: BSI-18: Brief Symptom Inventory 18, C-SOSI: Calgary Symptoms of Stress Inventory, CES-D: Center for Epidemiological Studies-Depression, DASS: Depression Anxiety Stress Scales, QLQ-30: European Organisation for Research and Treatment of cancer Quality of Life Questionnaire, FACT-G: Functional Assessment of Cancer Therapy-General, FACT-P: Functional Assessment of Cancer Therapy-Prostate Cancer, FACIT-SP: Functional Assessment of Chronic Illness-Spirituality, FFMQ: Five Factor Mindfulness Questionnaire, FMI: Freiburg Mindfulness Inventory, HADS: Hospital Anxiety and Depression Scale, HAM-A: Hamilton Anxiety Scale, HAM-D: Hamilton Depression Scale, IES: Impact Event Scale, MAAS: Mindful Attention and Awareness Scale MAX-PC: Memorial Anxiety Scale for Prostate Cancer, MFSI-SF: Multidimensional Fatigue Symptom Inventory, Emotional Fatigue Subscale, NR: Not Reported, PGH-10 MH: PROMIS Global Health-10 Mental Health Subscale, PSS: Perceived Stress Scale, PTGI: Post Traumatic Growth Inventory, POMS: Profile of Mood States, SCS: Self-Compassion Scale, STAI: Spielberger State-Trait Anxiety Inventory

Supplemental Table 2

*Type and Frequency of Psychosocial instruments used in the 17 studies*

<b>Instrument Name</b>	<b>Construct</b>	<b>Domain</b>	<b>Number of studies (out of 17 possible) using these instruments</b>
Five Facet Mindfulness Questionnaire (FFMQ)	Mindfulness	Mindfulness	4
Freiburg Mindfulness Inventory (FMI)	Mindfulness	Mindfulness	1
Spielberger State-Trait Anxiety Inventory (STAI)	Anxiety	NMH	1
Memorial Anxiety Scale for Prostate Cancer (MAX-PC)	Anxiety	NMH	2
Hamilton Anxiety Scale (HAM-A)	Anxiety	NMH	1
Hamilton Depression Scale (Ham-D)	Depression	NMH	1
Center for Epidemiological Studies-Depression (CES-D)	Depression	NMH	2
Brief Symptom Inventory (BSI)	Distress	NMH	2
Profile of Mood States (POMS)	Distress	NMH	4
Hospital Anxiety and Depression Scale (HADS)	Distress	NMH	2
Depression Anxiety Stress Scales (DASS)	Distress	NMH	1
Emotional Subscale from Multidimensional Fatigue Symptom Inventory	Distress	NMH	1
Calgary Symptoms of Stress Inventory (C-SOSI)	Stress	NMH	3
Perceived Stress Scale (PSS)	Stress	NMH	1
Impact Event Scale (IES)	Trauma	NMH	4
Post Traumatic Growth Inventory	Trauma	PMH	4
Self-Compassion Scale (SCS)	Self Compassion	PMH	2
Functional Assessment of Chronic Illness-Spirituality (FACIT_SP)	Spirituality	PMH	1
Functional Assessment of Cancer Therapy-General (FACT-G)Quality of Life	QOL	QOL	5
Functional Assessment of Cancer Therapy-Prostate Cancer (FACT-P)Quality of Life	QOL	QOL	1
36-item Short Form Survey (SF-36)	QOL	QOL	
EORTC Quality of Life Questionnaire (QLQ)	QOL	QOL	2

Notes: NMH: Negative Mental Health, PMH: Positive Mental Health, QOL: Quality of Life

Supplemental Appendix 1

*Coding sheet used for data extraction*

**Bibliographic Info:**

First Author's Last Name: \_\_\_\_\_

Journal: \_\_\_\_\_

Publication Year: \_\_\_\_\_ Issue: \_\_\_\_\_ Pages: \_\_\_\_\_

**Study Information**

Country where Data was collected: \_\_\_\_\_

Total N (Men only)

Treatment Outset: \_\_\_\_\_

Treatment conclusion: \_\_\_\_\_

At follow-up 1 (<12 mos): \_\_\_\_\_

At follow-up 2 (<12 mos): \_\_\_\_\_

**Demographics at Treatment Onset**

<b>Baseline Characteristics for Men</b>					
	<b>Mean</b>	<b>SD</b>	<b>Median</b>	<b>Range</b>	
<b>% White European:</b>					<input type="checkbox"/> <b>NR/NA</b>
<b>Age</b>					<input type="checkbox"/> <b>NR/NA</b>
<b>% Married</b>					<input type="checkbox"/> <b>NR/NA</b>
<b>% Employed</b>					<input type="checkbox"/> <b>NR/NA</b>
<b>Years of Education:</b>					<input type="checkbox"/> <b>NR/NA</b>

Cancer Type(s) Specify all: \_\_\_\_\_

Cancer Stage(s) (check all that apply)  Not Described  I  II  III  IV

Includes Active Treatment for Cancer (Y/N) \_\_\_\_\_

Includes Cancer Treatment Completed (Y/N) \_\_\_\_\_

**Treatment**

Intervention Type:  MBSR  MBCT  ACT  QiGong  Tai Chi  Yoga  Other: \_\_\_\_\_

Intervention Delivery Mode:  In Person,  Remote  Blended

Home Practice (number of hours/week) M \_\_\_\_ SD \_\_\_\_; Median \_\_\_\_ Range \_\_\_\_

Intervention Time (total hours) \_\_\_\_\_

Control Group Type:  TAU  Waitlist  Attention Control  Exercise  Other \_\_\_\_\_

Control Group Active (Y/N) \_\_\_\_\_

Adherence Rates (%) Intervention \_\_\_\_ Control \_\_\_\_ Overall \_\_\_\_  Not Reported

Follow-up 1 Length (months) \_\_\_\_\_

Follow-up 2 length (months) \_\_\_\_\_

Excluded those practicing mindfulness (Y/N/NR) \_\_\_\_\_

Included Partners in the Intervention (Y/N/NR) \_\_\_\_\_

**Psychological Outcome Measures**

Depression Measure: _____								
	Intervention				Control			
	Pre	Post	FU 1	FU 2	Pre	Post	FU 1	FU 2
Mean								
SD								
_____	<input type="checkbox"/> N/A	<input type="checkbox"/> N/A	<input type="checkbox"/> N/A	<input type="checkbox"/> N/A	<input type="checkbox"/> N/A	<input type="checkbox"/> N/A	<input type="checkbox"/> N/A	<input type="checkbox"/> N/A

Anxiety Measure: _____								
	Intervention				Control			
	Pre	Post	FU 1	FU 2	Pre	Post	FU 1	FU 2
Mean								
SD								
_____	<input type="checkbox"/> N/A	<input type="checkbox"/> N/A	<input type="checkbox"/> N/A	<input type="checkbox"/> N/A	<input type="checkbox"/> N/A	<input type="checkbox"/> N/A	<input type="checkbox"/> N/A	<input type="checkbox"/> N/A

Quality of Life Measure: _____								
	Intervention				Control			
	Pre	Post	FU 1	FU 2	Pre	Post	FU 1	FU 2
Mean								
SD								





**Quality Assessment: NIH Quality Assessment of Controlled Intervention Studies**

<https://www.nhlbi.nih.gov/health-pro/guidelines/in-develop/cardiovascular-risk-reduction/tools/rct>

(adapted for this study design) CD= cannot Determine; N/A = not applicable; NR = Not reported

1. Was the method of randomization adequate (i.e., use of randomly generated assignment)?	<input type="checkbox"/> Yes	<input type="checkbox"/> No	<input type="checkbox"/> CD/NR/NA
2. Was the treatment allocation concealed (so that assignments could not be predicted)?	<input type="checkbox"/> Yes	<input type="checkbox"/> No	<input type="checkbox"/> CD/NR/NA
3. Were the people assessing the outcomes blinded to the participants' group assignments?	<input type="checkbox"/> Yes	<input type="checkbox"/> No	<input type="checkbox"/> CD/NR/NA
4. Were the groups similar at baseline on important characteristics that could affect outcomes (e.g., demographics, risk factors, co-morbid conditions)?	<input type="checkbox"/> Yes	<input type="checkbox"/> No	<input type="checkbox"/> CD/NR/NA
5. Was the overall drop-out rate from the study at endpoint 20% or lower of the number allocated to treatment?	<input type="checkbox"/> Yes	<input type="checkbox"/> No	<input type="checkbox"/> CD/NR/NA
6. Was the differential drop-out rate (between treatment groups) at endpoint 15 percentage points or lower?	<input type="checkbox"/> Yes	<input type="checkbox"/> No	<input type="checkbox"/> CD/NR/NA
7. Was there high adherence to the intervention protocols for each treatment group?	<input type="checkbox"/> Yes	<input type="checkbox"/> No	<input type="checkbox"/> CD/NR/NA
8. Were other interventions avoided or similar in the groups (e.g., similar background treatments)?	<input type="checkbox"/> Yes	<input type="checkbox"/> No	<input type="checkbox"/> CD/NR/NA
9. Were outcomes assessed using valid and reliable measures, implemented consistently across all study participants?	<input type="checkbox"/> Yes	<input type="checkbox"/> No	<input type="checkbox"/> CD/NR/NA
10. Did the authors report that the sample size was sufficiently large to be able to detect a difference in the main outcome between groups with at least 80% power?	<input type="checkbox"/> Yes	<input type="checkbox"/> No	<input type="checkbox"/> CD/NR/NA
11. Were outcomes reported or subgroups analyzed prespecified (i.e., identified before analyses were conducted)?	<input type="checkbox"/> Yes	<input type="checkbox"/> No	<input type="checkbox"/> CD/NR/NA
12. Were all randomized participants analyzed in the group to which they were originally assigned, i.e., did they use an intention-to-treat analysis?	<input type="checkbox"/> Yes	<input type="checkbox"/> No	<input type="checkbox"/> CD/NR/NA