

# Psychological Interventions for Distress in Cancer Patients: A Review of Reviews

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## ABSTRACT

*We review a decade of review articles concerning psychosocial interventions for cancer patients. We find a distinct progression in the tone of interpretations of the literature, as better quality studies accumulate and the sophistication of reviews improves. The current literature does not make a compelling case for the value of these interventions for the typical cancer patient. The bulk of the literature reviews in this field take a narrative rather than a systematic approach, and serious compromises in standards are necessary to muster an adequate set of studies for review. The more rigorous the review, the less likely it is to conclude there is evidence that psychological interventions are effective.*

(*Ann Behav Med* 2006, 32(2):85–92)

## INTRODUCTION

Dozens of research studies have been conducted, including many randomized controlled trials (RCTs), evaluating the efficacy of psychological interventions for reducing distress in people with cancer. Any reader of this research would be hard pressed to draw general conclusions, because there is a lack of consistency of methods and findings both within and across studies (1,2). Dozens of scholars have now reviewed this literature in an effort to make sense of the diverse and often conflicting findings. Unfortunately, our review of these reviews reveals that they, too, present conflicting and inconsistent conclusions.

For example, Meyer and Mark (3) came to the following conclusion in their oft-cited review:

We believe that the cumulative evidence is sufficiently strong that it would be an inefficient use of research resources to conduct more studies in the U.S. to ask the simple question: Is there an effect of behavioral, educational, social support, and nonbehavioral counseling and therapy interventions on the emotional adjustment, functional adjustment, and treatment-and disease-related symptoms of cancer patients? These interventions have a consistent beneficial effect on all three areas. (p. 106)

Others similarly have touted the efficacy of psychological interventions based on their reviews of the literature:

We conclude that there is compelling evidence indicating that group psychotherapy improves the quality of life of cancer patients. (4, abstract)

Substantial evidence suggests that psychological interventions are effective in reducing the psychological distress associated with cancer. (5, abstract)

Psychoeducational care was found to benefit adults with cancer in relation to anxiety, depression, mood, nausea, vomiting, pain, and knowledge. (6, abstract)

In contrast, Newell, Sanson-Fisher, and Savolainen (7) concluded the following from their review:

Although this is one of the more extensive and rigorous literature reviews conducted in this area of research, we can offer only tentative recommendations for or against most intervention strategies overall or within the different follow-up periods. In addition, it is important to note that most of these recommendations are based on results obtained from only one or two fair-quality trials. ... Thus, these recommendations should be considered with appropriate caution and should not be seen as supporting the current wide-scale adoption of these strategies. (p. 580)

Others similarly have concluded that the current state of the literature does not support strong or even modest claims of efficacy:

The question of whether psychosocial intervention among cancer patients has a beneficial effect remains unresolved. (8, abstract)

There is insufficient evidence to advocate that group psychological therapies (cognitive-behavioral or supportive-expressive) should be made available to all women diagnosed with metastatic breast cancer. If there are benefits of the interventions, these are only evident for some of the psychological outcomes and in the short term. (9, abstract)

How can we make sense of these opposing viewpoints from reviews that, by and large, evaluate the same corpus of evidence? We seek some answers to this question by assessing the quality of the reviews in this area.

## CHARACTERIZING REVIEWS

Currently, most reviewers of psychological interventions in cancer use a traditional, narrative style of review; that is, they list and summarize a group of studies of varying quality with little or no synthesis or guidance on how to weigh divergent findings. Very often, these narrative reviews do not even describe how they identified or selected studies to review. This approach is at odds with the more systematic approach to reviews that characterizes the broader biomedical literature over the past decade.

The primary difference between narrative and systematic reviews is the extent to which they control systematic, random,

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and inferential error in the review process. Systematic reviews are more likely than narrative reviews to reduce bias by using predefined, comprehensive, and reproducible search strategies as well as predefined, uniformly applied, and criterion-based strategies to select studies to review (10,11). Whereas narrative reviews rely on qualitative summaries of studies, systematic reviews can be quantitative or qualitative. For example, a meta-analysis is a quantitative systematic review that uses well-defined methods for selecting multiple studies related to a research or clinical question and appraising and statistically synthesizing the results from those studies to draw a conclusion. Reviews that summarize their systematic findings but do not statistically combine results from different studies are qualitative systematic reviews.

Because of the emphasis on reducing bias, systematic reviews are more likely than narrative reviews to focus on evidence from RCTs. Indeed, within the broader biomedical literature, the gold standard of reviews is the systematic qualitative or quantitative review of multiple RCTs (12). Focusing on RCTs helps to eliminate much of the bias associated with selection and other validity threats that plague nonrandomized studies. Investigators have shown that nonrandomized studies tend to yield larger estimates of treatment effects than studies using random allocation (13–16), so reviews that include nonrandom studies risk introducing some bias. Of course, as discussed next, even RCTs vary greatly in their quality, and reviewers should consider additional methodological and analytical criteria when evaluating and interpreting the quantitative findings from RCTs.

Systematic reviews also are characterized by some degree of quality control over the selection of studies to review. Whether a review includes some form of quality control during study selection and synthesis of results is a critical indicator of the degree of control over bias. It is important to note that inclusion of low-quality trials can distort the results of reviews (17). However, we also acknowledge that systems for evaluating study quality can vary (10), and even systematic reviews applying different quality standards can come to discrepant conclusions about the same literature (18). Despite this and other potential limitations of systematic reviews (e.g., 19), the fact that they apply and describe a methodology for assessing and controlling study quality provides some context for readers to evaluate the credibility of the reviews' conclusions. In contrast, narrative reviews provide little basis for judging the credibility of the reviews' conclusions.

As the evidence-based research and practice movement takes hold, reviewers are becoming increasingly aware of the importance of assessing and controlling the quality of empirical studies that are reviewed (20,21). However, it is often difficult to assess the quality of review articles themselves, because many of them do not describe their methodology for selecting studies to review and relatively few validated guidelines exist for assessing the quality of reviews (10,22). We use a system of ranking the quality of reviews based on two important criteria for reducing bias noted by Cook, Mulrow, and Haynes (11) and West et al. (10): (a) the application of controls to reduce bias related to the quality of studies included in the review, and (b) the compre-

hensiveness and adequacy of the literature search procedures. We acknowledge that there are additional criteria that could be used to characterize reviews—particularly systematic reviews (23)—but the two criteria we identify appear to be fundamental to controlling bias and can be found in most guidelines for assessing reviews. Further, we are interested in characterizing all of the reviews in this field, not just the systematic ones, to illustrate the overall quality of the literature.

As shown in Figure 1, one can sort reviews into different levels of rigor depending on the extent to which they meet these two criteria. The first factor, qualification of studies reviewed, is a primary indicator of degree of control over bias in a review. The middle row of Figure 1 represents the current gold standard of systematic reviews in the broader biomedical literature: a review of multiple published RCTs. Reviews that include published, lower quality studies (e.g., nonrandomized) do not meet the gold standard but meet what we designate as the “silver” standard. Reviews that include only published RCTs of adequate quality are superior to the gold standard and meet what we designate as the “platinum” standard. We originally anticipated that all reviews could be classified according to this table but quickly learned that it was necessary to create yet another field to characterize the majority of reviews because they included no information whatsoever on their search procedures or inclusion/exclusion criteria. This finding alone underscores the fact that most of the reviews in this area are not systematic and, hence, subject to many sources of error and bias.

Empirical studies suggest the importance of controlling study quality in reviews. Moher et al. (24) observed a 30 to 50% exaggeration of treatment efficacy in reviews that pooled the results of lower quality trials. Biases can creep in by including studies that do not adhere to acceptable standards for properly conducting and reporting clinical trials. Most of these standards are related to controls over threats to internal validity. For example, trials with inadequate or unclear treatment allocation concealment, compared with those that used adequate methods, are associated with an increased estimate of benefit (17,24). Currently, the use of summary scores to weigh studies of varying quality is controversial (18,25). The issue is that available scoring systems make arbitrary assumptions about the relative importance to be attached to various threats to the validity of stud-

		Literature Searched	
		Published Only	Published & Gray
Studies Reviewed	RCT & Quasi-experimental	SILVER	SILVER +
	RCT Only	GOLD	GOLD +
	Quality RCT Only	PLATINUM	PLATINUM +

FIGURE 1 A schema for characterizing reviews based on quality of the search and studies included in the review.

ies, with different assumptions yielding very different weights. As an alternative it is recommended that reviewers assess study quality against individual relevant methodological criteria, usually important indicators of internal validity such as those noted in the CONSORT guidelines for reporting of clinical trials (cf. 17,18) and then make decisions about including or excluding particular trials. The Cochrane group and others (26–29) recommend excluding low-quality trials or conducting sensitivity analyses in reviews of interventions. Reviews that set at least a minimum threshold of study quality, such as insisting on RCT data—or, better yet, adequate quality RCT data—can substantially reduce error and bias in interpretation.

The second factor, quality and comprehensiveness of the search, is another important indicator of degree of control over bias in a review. In Figure 1, we have added “+” to designate those reviews that include a search of both published and non-published or difficult to locate intervention studies. Some analysts have argued that reviews that do not include the “gray literature” of unpublished or difficult to find studies may inflate estimates of intervention effects. McAuley et al. (30) argued that reviews excluding gray literature yield significantly larger estimates of interventions, by as much as 15%. Of course, although the search should ideally be comprehensive, it is still necessary to apply some criteria for determining which studies to include in the review, independent of whether they are published. Other potential methods to reduce bias include using and reporting all key search terms, databases, and other search materials or procedures, and a specified time span of the search.

### SEARCH STRATEGY TO IDENTIFY REVIEWS ARTICLES FOR THIS ANALYSIS

To identify the relevant review literature, we chose a 10-year period (January 1995–July 2005). There were several reasons for this period. Two of the most frequently cited review papers on psychological interventions for cancer patients (Meyer and Mark [3]; Fawzy et al. [31]) first appeared in 1995. Prior to 1995, there were relatively few RCTs to review, so many of them were captured by these two reviews or in later reviews. In addition, this 10-year period captures an empirical literature spanning approximately 40 years, with the higher quality studies and reviews appearing in the past decade.

We used OVID to search three computerized databases: OVID MEDLINE(R), PsycINFO, and the Cumulative Index to Nursing and Allied Health Literature (CINAHL). OVID allows multiple searches and deletion of duplicates. Keywords for the search included (cancer or neoplasm\$) and psycho\$ and interv\$ and (review\$ or meta-analys\$). The search was limited to the English language and resulted in 670 papers. From these, we extracted 36 relevant reviews.

Stephen Lepore and a trained graduate student research assistant reviewed the titles and abstracts from the OVID search to extract relevant reviews. When we failed to reach consensus, James Coyne was consulted to review the titles and abstracts of papers. If consensus was still not achieved, full papers were retrieved and reviewed for a final decision. The only papers we extracted had a major focus to review the literature on the effects of

psychological interventions (e.g., cognitive-behavioral therapy, psychosocial/educational support groups, group or individual therapy/counseling) on distress outcomes (e.g., global distress or mood, negative affect, depressive or anxious symptoms) in adult cancer patient populations.

We excluded reviews in which (a) the focus was on non-psychological interventions (e.g., home-based nursing visits, pharmacotherapy, peer support without a professional facilitator, caregiver assistance, only provision of disease information, massage, music therapy, aromatherapy, healing touch, nutrition, prayer); (b) the primary outcomes were survivorship, physical symptoms (e.g., pain, fatigue, incontinence, impotence), or biological outcomes (e.g., immune functioning); or (c) only pediatric populations were included. We also excluded reviews focused on describing psychological interventions to practitioners (e.g., 32,33) and other professional practice guidelines. Practice guidelines are statements developed to assist practitioners and patients in decisions about the best available treatments for particular conditions, such as treatments for reducing depression in cancer (for a recent review of available clinical practice guidelines for the psychosocial care of adults with cancer, see 34). These guidelines vary widely in the extent to which they actually evaluate and synthesize primary empirical data versus rely on professional opinion and established practice, and oftentimes they are commissioned or invited reports. Finally, we excluded reviews that just cited other reviews as a primary source of evidence of efficacy (35, e.g., 36,37) or just described the qualities of psychological intervention studies that might affect efficacy, such as the measures used or the components of the intervention (1, e.g., 38). We should note that the papers that were excluded generally took a narrative approach when they discussed the efficacy of psychological interventions, so adding them into the analysis would not have improved the outcome.

### QUALITY OF PSYCHO-ONCOLOGY REVIEWS

One goal of our review of reviews is to characterize the overall quality of the reviews of psychological interventions to reduce distress in cancer patients. We do this by describing the frequency of reviews that minimize bias by (a) using a systematic and comprehensive search strategy and (b) controlling for the effects of lesser quality studies on results.

#### Search Strategy

Our first analysis examines whether the search strategy reviewers used to identify relevant studies could be considered systematic and comprehensive. A systematic search strategy is one that includes a methodology that allows readers to reproduce the search to a large degree. Guidelines, such as the QUOROM statement checklist (39), suggest a number of key methodological details to include in reviews to limit bias and make the search transparent and reproducible. The Cochrane group also has developed useful guidelines (12). At a minimum, systematic reviews should specify the databases/search engines used, the range of dates, and the key terms used in the search (39). Other details are also helpful, such as any restrictions (e.g.,

language, publication status) and procedures for filtering out studies for review.

We sorted the 36 identified reviews into three categories: (a) search strategy was not reported or inadequate (lacks information on the databases used for the search, the years of the search, or the key search terms); (b) search strategy was described, largely reproducible, and limited to published studies; and (c) search strategy was described, was largely reproducible, and included gray literature. As shown in Table 1, the majority (55.6%) of reviews provided inadequate information on the search strategy. Very few reviews were systematic and comprehensive: 25.0% were limited to published studies, and only 19.4% included gray literature.

### Quality Controls

Our second analysis examines how studies were selected for inclusion in the reviews. We examine whether the reviews controlled for study quality by selecting only those that met certain methodological criteria. Some reviews assessed study quality and described methodological flaws but stopped short of excluding the flawed studies from the synthesis of results. Simply describing study quality, or even critiquing select studies, is not sufficient control over bias in the review process. The majority of the identified reviews did not describe methodological criteria for selecting studies and did not appear to apply methodological criteria for including or excluding studies.

We sorted reviews into three categories based on the kinds of studies they included: (a) included both RCTs and nonrandomized intervention studies, (b) included only RCTs, and (c) included only adequate quality RCTs. None of the reviews that included both RCTs and nonrandomized intervention studies uniformly excluded studies based on specific methodological shortcomings (however, Sheard and Maguire [40] conducted a sensitivity analysis). As shown in Table 1, the majority (72.2%) of reviews did not select studies using even basic methodological criteria, such as randomization. One fourth (25.0%) of the reviews adhered to the gold standard of reviewing only RCTs, and only a single study (2.8%) reviewed just adequate quality RCTs.

To illustrate the importance of removing lesser quality studies from these reviews, we consider the findings from Sheard and Maguire's (40) meta-analysis, which included not only both randomized and nonrandomized interventions but also a sensitivity analysis. Results of the overall meta-analysis revealed a

moderate positive effect for depression when all studies were analyzed in a pooled sample of 1,101 patients. However, the size of the effect was reduced to a clinically negligible value after excluding nonrandomized studies, studies with lower methodological quality, and studies with samples smaller than 40. Similarly, a moderate positive effect was found on anxiety when all studies were analyzed in a pooled sample of 1,023 patients, but the size of the effect was reduced when nonrandomized, lesser quality, and smaller scale studies were excluded.

A handful of the 36 reviews provided assessments of quality but did not present independent analyses of the higher quality studies (6, 41–46). The downside of this approach is that the reviews tend to send mixed messages to the reader. For example, Uitterhoeve et al. (46) identified many methodological problems in RCTs of psychosocial interventions for patients with advanced cancer but did not use this information to exclude studies. In the abstract, the authors concluded,

13 trials were included. ... A total of 12 trials evaluating behavior therapy found positive effects on one or more indicators of QoL, for example depression. The results of the review support recommendations of behavior therapy in the care of patients with advanced cancer. (p. 1050)

It is not until the Conclusion section of the article that the reader encounters this more tempered conclusion:

In summary, there is an indication that psychosocial intervention using cognitive-behavioral techniques are beneficial for the QoL of patients with advanced cancer, especially in the domain of emotional functioning. However, evidence is limited as there have been few large methodological strong trials. (p. 1060)

### ARE REVIEWS GETTING BETTER WITH TIME?

As the previously discussed analyses show, the identified reviews from the past decade would be difficult to reproduce, and most of them took few steps to reduce bias associated intervention study quality. To assess trends in the quality of the literature, we examined the correlation between the time since publication (in years) and the two search quality codes: search strategy (1 = *inadequate or unreported*; 2 = *published only*; 3 = *published and gray*) and quality of studies included (1 = *RCTs and nonrandomized*; 2 = *only RCTs*; 3 = *adequate quality RCT*). There was a statistically significant inverse correlation between

TABLE 1  
Reviews Sorted by Search Strategy and Methodological Quality (Design Criteria) of Studies Included in the Reviews

Methodological Quality (Design Criteria)	Search Strategy		
	1. Unknown or Inadequate	2. Systematic, Published	3. Systematic, Published, and Gray
1. RCTs and nonrandomized	<i>n</i> = 17 (47.2%) (references: 2,4,31,42, 51–63)	<i>n</i> = 6 (16.7%) (references: 41,43,64–67)	<i>n</i> = 3 (8.3%) (references: 5,6,40)
2. RCTs only	<i>n</i> = 2 (5.5%) (references: 68,69)	<i>n</i> = 4 (11.1%) (references: 3,8,44,45)	<i>n</i> = 3 (8.3%) (references: 9,46,70)
3. Adequate quality RCTs	<i>n</i> = 0	<i>n</i> = 0	<i>n</i> = 1 (2.7%) (reference: 7)

Note. *N* = 36 reviews. RCT = randomized controlled trials.

study quality and years since publication ( $r = -.48, p = .003$ ). There was a nonsignificant trend in the same direction between study search strategy and years since publication ( $r = -.29, p = .08$ ). These measures of association suggest that reviews that are more recent tend to be more systematic than papers from earlier in the decade, particularly with respect to excluding lower quality studies. Not surprising, the two quality indicators are significantly and positively correlated ( $r = .48, p < .004$ ), indicating that the more comprehensive reviews also tend to be more selective in study quality.

### FINDINGS FROM THE PLATINUM+ REVIEW

As shown in Table 1, the study by Newell et al. (7) stands alone in the Platinum + category. As this is the most rigorous of the reviews we identify, it is instructive to consider its findings in some detail. The review examined psychosocial outcomes (e.g., distress, functional status, relationships), side effects (e.g., nausea, vomiting, pain), and survival or immune outcomes. We focus here only on the aspects of the review pertaining to psychological distress outcomes.

Through a comprehensive search of the literature, Newell et al. identified 129 potentially relevant trials, which were assessed for methodological quality using 10 internal validity criteria recommended in the Cochrane Collaboration guidelines. These criteria included the following: conceal allocation to condition; adequate patient selection (random, consecutive, all); patients blinded to treatment group, care providers blinded to treatment group; except trial intervention, other treatments equivalent (or usual care); care providers adherence monitored; detailed loss to follow-up information; report percentage of patients not in analyses; conduct intention-to-treat analyses; outcomes measured blind. Each criterion for each trial was scored using a 4-point scale (3 = *entirely fulfilled*; 2 = *mostly fulfilled*; 1 = *mostly not fulfilled*; 0 = *not at all fulfilled/insufficient information to determine*). The total points were summed (range = 0–30). Studies scoring below 11 were considered poor or inadequate to review, whereas those scoring above 11 were included in the review. Of 129 reviews, a full 87 (64%) scored in the poor range and were excluded from further review. An additional 8 trials were excluded because they appeared in two papers that did not present separate results for each trial. The remaining 34 trials were included in the fuller review of efficacy.

The reviewed trials often had multiple dependent outcome measures (e.g., depressive and anxious symptoms). Each statistically significant test of the effect of an intervention on one of these outcomes was counted. There were few statistically significant tests across the different distress outcomes. Statistically significant tests for specific outcomes were 45/175 (26%) for anxiety, 26/120 (22%) for depression, 43/154 (28%) for general affect, and 19/74 (26%) for global stress/distress.

Newell et al. (7) also searched for evidence of beneficial effects of specific kinds of psychological interventions on distress, but this analysis was equally disappointing. For example, the number of statistically significant tests of cognitive-behavioral therapy was consistently low across distress outcomes:

3/11 (27%) for anxiety, 2/10 (30%) for depression, 3/7 (43%) for global stress/distress, and 5/10 (50%) for general affect. Other forms of therapist-delivered one-on-one therapy were no more impressive. Statistically significant effects were quite infrequent: 9/33 (27%) for anxiety, 4/24 (17%) for depression, 3/15 (20%) for global stress/distress, and 8/26 (31%) for general affect. Trials of group therapy, which is frequently touted in narrative reviews of the field, also evidenced few statistically significant effects: 1/4 (25%) for anxiety, 2/6 (33%) for depression, 1/4 (25%) for global stress/distress, and 3/6 (50%) for general affect. Music therapy was the only type of intervention that was effective for anxiety 100% of the time (1 trial), and this is hardly a mainstream psychological intervention. Moreover, for anxiety and depression, none of the psychological intervention approaches was effective more than 33% of the time. Based on this relatively rigorous and comprehensive review, the evidence on the efficacy of psychological intervention at reducing distress is inconclusive, at best.

Overall, the review by Newell et al. (7) suggests that adequately conducted RCTs in this field are more likely to yield the conclusion that interventions are ineffective rather than effective at reducing patients' distress. Similar conclusions were drawn by Edwards et al. (9) in their recent and more narrow review of psychological RCTs with women with metastatic breast cancer (also see 8). Some critics contend that the Newell review was overly conservative (47), but in many ways it really used a minimal set of validity requirements in selecting the studies for review. It would not be difficult for an intervention trial to score an 11 on the scale used by Newell and colleagues. Indeed many of the criteria are easily achieved by simple reporting. For example, reporting how patients were selected, what percentage of patients were not in analyses, and whether the data collectors were blind to condition would give a trial a score of 9. By mostly fulfilling any of the remaining seven criteria, a trial would score an 11. Some of the more questionable validity criteria for psychological intervention studies, such as care providers or patients being blind to treatment, could be unfulfilled, and a study could still score an 11 or better. Further, we note that Newell et al. ignored a host of other important features of trials that could influence validity, such as power; number of sites used in the RCT; reliability, validity, and sensitivity of outcome measures; and portability of the intervention to clinical practice or external validity/effectiveness in practice. As suggested by Coyne, Lepore, and Palmer (48), when one looks closely at even the "better quality" RCTs, they tend to lose their sheen, which is all the reason to exclude those that do not even meet minimal standards of control over bias.

### CONCLUSIONS

Analysis of a decade's worth of reviews exposes a field rife with nonsystematic, uncritical analyses about the efficacy of psychological interventions for cancer patients. The overabundance of narrative as opposed to systematic reviews undermines the credibility of the field. The failure to adopt rapidly standards of evidence and systematic review procedures that are accepted in the broader biomedical field could contribute to the public

perception that psychological interventions are unproven treatments. Results from the analysis of trends over time hint that the fields may be on the right track, as recent reviews are more likely than older ones to attend to the quality of trials being reviewed. However, only one review set a threshold for the quality of RCTs to be included, most reviews included nonrandomized trials, and most reviews did not exclude lower quality trials even when they assessed quality. Furthermore, nearly half of the identified reviews did not include even the most basic details of how studies were identified for review, despite the availability of guidelines, such as QUOROM. Even narrative reviews would be improved substantially by including a description of the search method (e.g., key terms, years, databases).

Our review of reviews, especially the more systematic reviews, provides no convincing evidence of broadly effective psychological interventions for reducing a wide range of distress outcomes in cancer patients. This finding is consistent with Petticrew's (49) "stainless steel law" of reviews, which maintains that the more rigorous the review, the less likely it is to conclude there is evidence that interventions are effective. Some would argue that the more rigorous reviews apply unreasonably restrictive criteria for evidence; to the contrary, we observe that even the more rigorous of reviews included studies that on scrutiny have many significant flaws. For example, the reviews of RCTs, including two meta-analyses (9,44) and the review by Newell et al. (7) of adequate quality RCTs, did not exclude small sample trials. These reviews included trials with as few as five patients in a cell. According to Piantadosi (50) small clinical trials are more susceptible than larger, sufficiently powered trials to chance imbalances between experimental and control groups, even if they appear to be carefully designed and conducted. Small trials have been demonstrated to be subject to a reporting bias. If there is a large trial conducted with requisite greater resources, there is a tendency to report results regardless of whether they are positive. On the other hand, given all that could have gone wrong with a smaller trial and the investment of fewer resources, null findings are more likely to go unreported. This is labeled as the "small trial effect" in the biomedical literature.

Obviously, to some extent reviewers are bound by the limits of the empirical evidence base. If standards were too high, then reviewers would have nothing to say about the state of the evidence, because no evidence would be satisfactory. We do not maintain that reviewers should reject all present forms of evidence related to psychological interventions. Rather, we urge reviewers to address the limitations of the evidence by taking a more systematic approach to reviewing the literature than currently exists. Preliminary steps toward this goal include establishing predefined, comprehensive, and reproducible search strategies as well as predefined, uniformly applied, and criterion-based strategies to evaluate and select studies to review. These and other steps (11,23) will help to reduce bias in reviewers' conclusions and present more objective and compelling evidence to consumers of psychological interventions in cancer care.

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