


Brief psychosocial interventions improve quality of life of patients receiving palliative care: A systematic review and meta-analysis

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Abstract

Background: Patients with a terminal disease report a high need for psychosocial and spiritual support. Previous literature reviews on psychosocial interventions in palliative care were restricted to certain subtypes of techniques, included a wide range of patients, conditions, and settings, or required a number of sessions unrealistic to be achieved in patients nearing the end of their lives.

Aim: The aim of this study was to review and synthesize the evidence on brief psychosocial interventions (i.e. four sessions or less and less than 21 days) for patients receiving palliative treatment.

Design: We performed a systematic review and meta-analysis in accordance with standard guidelines and a pre-registered protocol (PROSPERO: CRD42018082713).

Data sources: Electronic databases, journals, and references were searched for controlled clinical trials. We used the Cochrane criteria to assess the risk of bias within studies.

Results: In total, 15 studies met the eligibility criteria and reported the effects of 17 interventions and a total of 1248 patients. The most frequently used techniques were life review techniques and music therapy. After exclusion of outliers, psychosocial interventions showed to be superior with regard to the improvement of quality of life (effect size = 0.36, confidence interval = 0.08 to 0.64), and the reduction of emotional (effect size = -0.51, confidence interval = -0.77 to -0.26) and existential distress (effect size = -0.40, confidence interval = -0.71 to -0.07) compared to the control groups.

Conclusion: Brief psychosocial interventions can improve clinically relevant health outcomes and should therefore be made available for patients receiving palliative care.

Keywords

Palliative care, cancer, dignity therapy, life review, music therapy, quality of life, emotional distress, existential distress

What is already known about the topic?

- Emotional and existential suffering associated with a terminal illness has a negative impact on the quality of life of patients receiving palliative care.
- In addition to traditional psychotherapy, novel psychosocial interventions to address end-of-life care issues are emerging.
- Recent literature reviews summarized the evidence on single techniques (e.g. life review interventions, mindfulness, music therapy) and included patients with a longer life expectancy or interventions with a high number of sessions.

What this paper adds?

- This article is the first comprehensive synthesis of clinical trials investigating the effects of different psychosocial interventions in palliative care.

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- By focusing strictly on “brief” interventions, this article addresses healthcare issues specifically relevant for patients nearing the end of their life.
- Meta-analysis revealed beneficial effects of psychosocial interventions on quality of life, emotional distress, and existential distress.

Implications for practice, theory, or policy

- Life review interventions and music therapy were the most frequently used techniques in the included studies.
- Despite their limited number of contact sessions, these interventions showed positive effects on clinically relevant health outcomes and should therefore be made available for terminally ill patients.

Background

The need for psychosocial and spiritual support is very common in patients facing a terminal disease.^{1–4} Psychosocial distress may become manifest in symptoms of depression and anxiety or feelings of hopelessness, demoralization, or loss of control.^{5,6} Moreover, the anticipation of the end of life may induce existential suffering or death anxiety⁴ and family members can experience complications in the process of bereavement and loss.^{2,7} Accordingly, health care regulations in many countries provide a unique team composition in inpatient palliative care units and hospices that—besides specialized physicians and nurses—can include psychologists, social workers, creative arts therapists, physiotherapists, occupational therapists, or chaplains.^{8,9} In order to contribute to improving or maintaining the quality of life of patients and relatives, these disciplines need to develop and scientifically evaluate original approaches that are specifically designed to meet the needs of patients in various stages of a life-threatening disease.¹⁰

The World Health Organization defines palliative care as a multidisciplinary approach “[...] that improves the quality of life of patients and their families facing the problem associated with life-threatening illness.”¹¹ This includes—but is not limited to—end-of-life or hospice care, which in some countries is restricted to the last 6 months of life.^{12,13} Hence, in an end-of-life situation, interventions in palliative care need to adapt to specific requirements that may include physical weakness, mental deficits, rapidly changing physical states, and a very limited amount of time to work with a patient.¹⁴ Therefore, the conception and evaluation of brief interventions may become particularly relevant.

Since an incurable illness not only encompasses the physical burden of the patient, it is surprising that the question of how to “palliate” psychosocial and existential suffering remains poorly addressed.¹⁰ Accordingly, reports and guidelines identified a lack of effective treatments and call for an improvement of evidence-based psychosocial palliative care.^{15–17} Psychosocial interventions can be defined as “any intervention that emphasizes psychological or social factors rather than biological factors.”¹⁸ The

most commonly used psychosocial treatments in palliative care are cognitive-behavioral therapy, life review or meaning-centered interventions, creative arts-based therapies, and mindfulness.^{14,19} Scholarly literature provides evidence for the effectiveness of cognitive-behavioral therapy in cancer care where supportive interventions were shown to reduce emotional distress, to strengthen adaptive coping, or to reduce physical symptom burden in early or advanced stages of the disease.^{5,14} Research in palliative care settings, however, is very rare.¹⁴

Recently, different life review interventions such as dignity therapy,²⁰ the structured life review,²¹ or meaning-centered therapy²² have been developed to address the specific requirements of terminally ill patients.¹⁹ These approaches aim to strengthen the sense of identity and continuity by reflecting important aspects of the patient’s life.²³ Some provide support in creating an intellectual or physical legacy¹⁹ (e.g. a photo book or written transcript), addressing the wish for generativity (i.e. the comfort of knowing that something lasting of oneself will transcend death).²⁴ High patient satisfaction and moderate effects on spiritual well-being, general distress, and quality of life were found in a meta-analysis,²⁵ while primary studies in part failed to show significant effects on other outcomes such as patient dignity.^{26–29}

Creative arts-based therapies have been used in end-of-life care for more than 35 years.^{30–32} The aim is to support the management of symptoms, to enhance emotion regulation and communication skills, and to facilitate spiritual experiences.^{33,34} Expression through arts and music was shown to create feelings of meaning and hope.^{35–39} Systematic reviews on a limited number of clinical trials showed beneficial effects of music therapy on pain relief and quality of life, but also revealed a high risk of methodological bias.^{40–42}

Mindfulness is defined as to pay attention “on purpose, in the present moment, and non-judgmentally.”⁴³ Mindfulness-based interventions—such as the 8-week mindfulness-based stress reduction program—have been shown to improve health outcomes in different settings^{44,45} and to reduce anxiety, depression, sexual difficulties, stress, and sleep disturbances in various oncological conditions.^{46–48} However, with regard to

Table 1. Eligibility criteria.

Domain	Inclusion criteria
Patients	<ul style="list-style-type: none"> • Advanced terminal illness • Receiving inpatient or outpatient palliative/hospice care OR average survival prognosis < 3 months • More than 50% with a primary cancer diagnosis
Interventions	<ul style="list-style-type: none"> • Brief interventions: four sessions or less AND less than 21 days • Provided by a trained therapist or nurse • Psychosocial interventions: psychotherapy (cognitive-behavioral, psychodynamic, supportive), life review interventions (dignity therapy, meaning-centered therapy, life review), mind–body interventions (yoga, mindfulness, meditation), creative arts–based therapies (music therapy, art therapy), other approaches (counseling, forgiveness, compassion-based therapy)
Comparators	<ul style="list-style-type: none"> • No treatment/waitlist • Treatment as usual/standard care • Active attention control
Outcomes	<ul style="list-style-type: none"> • Primary outcome (pre to post): generic quality of life/well-being measures • Secondary (pre to post): <ul style="list-style-type: none"> • Emotional distress, for example, depression, anxiety, mood, emotional well-being • Existential distress, for example, spiritual well-being/distress, hope
Study designs	<ul style="list-style-type: none"> • Randomized controlled trials • Controlled clinical trials

palliative care, only few studies with a high risk of bias evaluated the short-term effects of and single mindfulness techniques (e.g. mindful breathing, body scan meditation).^{49,50}

An increasing number of clinical trials highlight the growing awareness of the importance of emotional and spiritual support in palliative care. Moreover, recently published systematic reviews on life review interventions,^{25–29} creative arts–based therapies,^{41,42,51} and mindfulness⁵⁰ in high-impact journals have promoted an ongoing discourse on the potential of psychosocial interventions. However, the majority of trials included a wide range of patients, stages of a disease, and settings (e.g. advanced cancer patients, nursing homes) or studied interventions which require a number of sessions unrealistic for patients nearing the end of life. Hence, research still lacks a focus on the specific characteristics of palliative end-of-life care. In contrast to, for example, other psychiatric or psychotherapeutic settings, psychosocial interventions in palliative care need to consider high attrition rates due to rapidly changing physical states, abbreviated session time, and a very limited number of sessions per patient.¹⁴ As the distinction between different forms of palliative, hospice, and/or end-of-life care may vary between countries and their health care systems, we tried to address this issue by focusing on *brief* psychosocial interventions in palliative care, which we believe could be applied to different end-of-life care settings. The average length of stay of a patient on a palliative care unit has been reported to be approximately 12 days.⁵² Considering attrition, weekend times, and part-time employment of psychosocial staff,⁵³ we defined a “brief intervention” as one that requires a frequency of four sessions or less and that could therefore take place on every third day. In

addition, this maximum number of four sessions had to be completed within a contact period of less than 21 days in order to exclude studies with a low number of sessions but an unrealistically high overall treatment span. Hence, the aim of this study was to systematically review the literature and to statistically synthesize the evidence on *brief* psychosocial interventions for patients receiving palliative treatment.

Methods

The design of this study was a systematic review and mixed-effects meta-analysis. A systematic literature review “attempts to collate all empirical evidence that fits pre-specified eligibility criteria to answer a specific research question.”⁵⁴ For this purpose, a study protocol was entered in the international prospective register of systematic reviews (PROSPERO-ID: CRD42018082713). The steps were conducted in accordance with the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) guidelines.⁵⁴ We additionally extracted statistics from primary studies to calculate pooled effect sizes for different outcome categories and to perform moderator analyses within meta-analyses.⁵⁵ Ethical approval was not obtained as this was a secondary analysis of published data.

Eligibility criteria

Criteria for study selection are specified in Table 1 according to the PICOS (patients, interventions, comparators, outcomes, study designs) framework.⁵⁴ The population of interest was primarily patients receiving specialized palliative care. By narrowing our search to brief interventions in

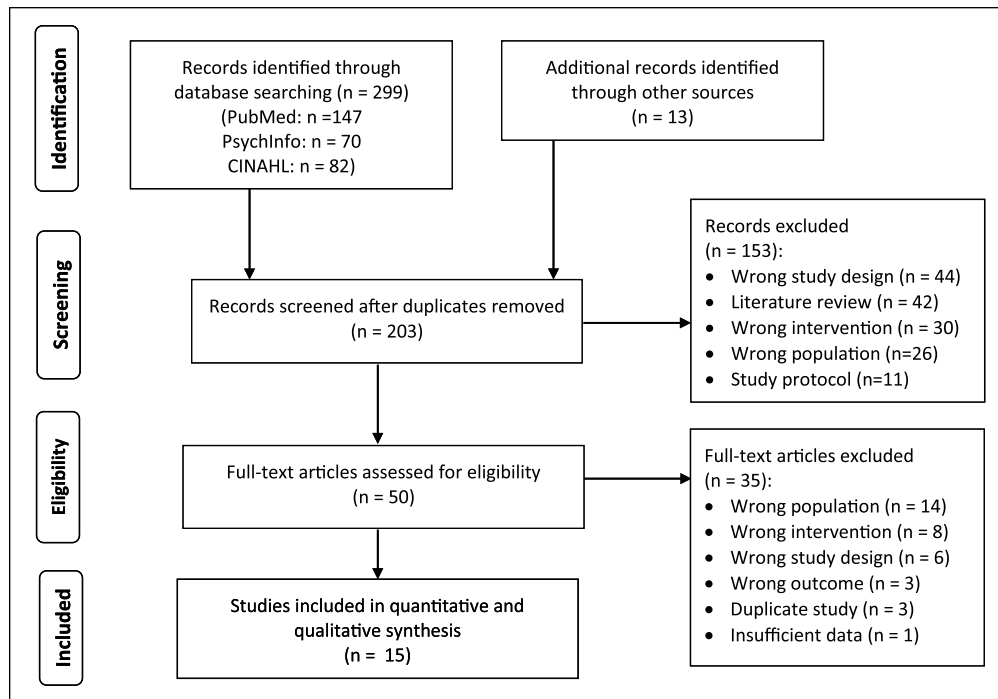


Figure 1. Flow diagram of study selection.

this field, we wanted to ensure that our findings would apply to end-of-life care situations. As stated above we defined a brief intervention by a maximum session number of four and a contact period of less than 21 days. Patients were to receive specialized palliative treatment in either an inpatient or an outpatient setting. Since patients with non-malignant diseases are still rare in palliative care settings in many countries,⁵² we excluded studies with a majority of non-cancer patients (>50%). We thereby wanted to provide high comparability between the included studies, as the requirements may differ substantially in, for example, specialized care institutions for patients with HIV or dementia. To be defined as a “psychosocial” intervention, sessions needed to be provided by a trained therapist or nurse in contrast to, for example, prerecorded exercises. Examples are given in Table 1. Therapies that primarily relied on a physical or sensory mechanism (e.g. physiotherapy, massages, aromatherapy) were excluded. Studies had to be either randomized or non-randomized controlled clinical trials and thus had to compare results to a control group. Studies were excluded if they compared two equal psychosocial interventions with no additional control condition. We searched for studies reporting treatment effects on quality of life, emotional distress, existential distress, and related constructs.

Literature search

We searched the following electronic databases for primary studies: PubMed, PsycINFO, and CINAHL. The following

terms were combined to search titles and abstracts within the respective database:

(palliative OR end-of-life OR terminal cancer OR hospice) AND (psychosocial OR psychotherap OR psychosocial OR mind-body OR yoga OR meaning-centered OR meditation OR cognitive-behavi* OR dignity therapy OR life review OR music therapy OR art therapy OR psychological* OR mindful) AND (random* OR rct OR controlled trial OR cct OR clinical trial) AND (depression OR quality of life OR distress OR well-being OR anxiety)*

In addition, we screened the reference lists of relevant systematic reviews and primary studies and hand-searched the latest issues of scientific journals in the field of palliative care. The literature search was conducted in December 2017. We could only include studies published in English or German language.

Study selection, data extraction, and risk of bias assessment

Basic information on authors, titles, and abstracts of all possibly relevant studies was entered into Rayyan, a web-based tool for systematic reviews.⁵⁶ After removal of duplicates, all abstracts were screened independently by two researchers and discrepancies were resolved by discussion. In case of study exclusion, we documented the most prominent reason (see Figure 1).

We developed a coding sheet that contained information on all data to be extracted from primary studies in accordance with the PICOS domains: general information

(authors, year, country, publication type), patients (age, sex, diagnoses, setting), intervention (name, description, duration, number of sessions), comparator (type of control group), outcomes (data source, constructs, instruments, measurement times, means, standard deviations, sample size), and study design (randomized clinical trial or controlled clinical trial). Outcome data were again coded dually.

The latter domain also contained the Cochrane Risk of Bias Assessment Tool for evaluation of study quality. As some of the quality criteria (e.g. blinding of participants and personnel) are hard to be transferred to psychosocial interventions, we adhered to the recommendations on the usage of this tool in psychotherapy research.⁵⁷ Two authors independently rated the risk of bias for each of the included studies and discussed and resolved discrepancies. We did not use alternative scales for non-randomized trials as we wanted the risk of selection bias in these studies to be represented in the data.

If important information was not reported in the publication, the corresponding authors were personally contacted by e-mail. In case the corresponding author did not respond or could not provide the missing information, the study was excluded.

Statistical analysis

Our aim was to statistically synthesize the available data by means of separate meta-analyses for each outcome category (quality of life, emotional distress, existential distress). To avoid dependencies within these categories, we followed a data-driven approach to decide which instrument to choose, if, for example, multiple quality of life measures were reported in one study, that is, we documented all multiple effect sizes falling within one of the three categories and created a hierarchy based on the total number of studies reporting the instrument. If more than one psychosocial intervention was compared to a control group in one study, we included both if the samples were independent.

We then followed the approach by Morris⁵⁸ to calculate standardized mean change differences as the appropriate effect sizes for pretest–posttest–control group designs. Contrary to common practice, this formula does not neglect baseline differences between groups and is therefore recommended.⁵⁹ As sample sizes are often low and not all studies implement proper methods of randomization in palliative care research, it is likely that such neglect would lead to biased results. Since pretest–posttest correlations are required for the calculation of sampling variance in standardized mean change differences, we estimated a correlation coefficient for quality of life, emotional distress, and existential distress based on two of our own former datasets.^{60,61} This estimate was then applied to all studies within the outcome category. The

calculated effect size can be considered a variation of Cohen's d and therefore classified as small ($d = 0.2$ – 0.5), medium ($d = 0.5$ – 0.8), or large ($d > 0.8$).⁶²

All subsequent meta-analytic calculations were performed using the “metafor” package in R.⁵⁵ For each effect size, we calculated a 95% confidence interval (CI). We chose a mixed-effects model for data synthesis as it seemed unlikely that effects based on different interventions, measurement instruments, and study designs would stem from one true population parameter. Heterogeneity was assessed with the Q -test and I^2 statistic, which we then tried to explain in meta-regression by testing the following moderators: type of setting (inpatient vs outpatient/mixed), type of intervention (life review/meaning-centered vs others), number of sessions (1 to 4), type of control (active control vs standard care), and type of measurement instrument. We visually inspected funnel plots to get an idea of the presence of a potential publication bias and then calculated Egger's regression test for funnel plot asymmetry.⁶³ Graphical outputs of various model diagnostics (e.g. standardized residuals, Cook's distance) were analyzed simultaneously to identify outliers and influential studies. If we observed a pattern of deviation for a certain effect, this case was deleted and sensitivity analysis was performed. Type I error probability was set at $\alpha = 0.05$.

Results

Study selection

Figure 1 shows the results of our literature search. Electronic search resulted in 299 records. Additionally, 13 studies identified by hand were considered relevant. After duplicates were removed, 203 abstracts were screened. Of those, 153 were excluded as they did not meet the eligibility criteria (reasons are listed in Figure 1). Of these, one study was excluded due to insufficient data reporting and non-response of the corresponding author to our request. Authors of three other studies did provide additional data. This led to full-text assessment of 50 studies, of which 15 were finally included in the analysis.

Study characteristics

Table 2 summarizes the characteristics of the included studies according to the PICOS framework.⁵⁴ These 15 studies reported the effects of 17 psychosocial interventions. As noted above, we included a maximum of one effect size per outcome category and intervention, resulting in a total of 36 effect sizes (10 on quality of life, 15 on emotional distress, and 11 on existential distress). The included studies were all published in peer-reviewed journals between 2003 and 2018 and comprised a total sample size of $N = 1248$. They were conducted in Northern

Table 2. Study characteristics.

Study	Patients	Intervention	Control group	Outcomes	Study design
Ando et al. ²¹	Inpatient palliative care, all terminal cancer, N = 68	Short-term life review; two sessions, 30–60 min	Standard care + general support	EMD: HADS, EXD: FACIT-SP	RCT
Chochinov et al. ²⁰	Inpatient and outpatient palliative and hospice care, 12 non-cancer patients, N = 326	1. Dignity therapy, 2. Client-centered care; three sessions, 30 min	Standard care	QOL: single-item scale, EMD: HADS (depression), EXD: FACIT-SP	RCT
Duggleby et al. ⁶⁴	Outpatient palliative care, six non-cancer patients, N = 53	Living with hope program, two sessions, time unclear	Standard care	QOL: MQOL, EXD: HHI	RCT
Hall et al. ⁶⁵	Hospital-based palliative care, all advanced cancer, N = 45	Dignity therapy; two sessions, time unclear	Standard care	QOL: two-item scale, EMD: HADS, EXD: HHI	RCT
Hilliard ⁶⁶	Hospice care, all terminal cancer, N = 80	Music therapy; two sessions, time unclear	Standard care	QOL: HQLI-R (overall), EMD: HQLI-R (psycho-physiological), EXD: HQLI-R (social/spiritual)	RCT
Horne-Thompson and Grocke ⁶⁷	Inpatient palliative care, one non-cancer patient, N = 25	Music therapy; one session, 20–40 min	Standard care + volunteer visit	EMD: ESAS (anxiety item)	RCT
Juliao et al. ⁶⁸	Inpatient palliative care, six non-cancer patients, N = 80	Dignity therapy; three sessions, 30–60 min	Standard care	EMD: HADS (depression)	RCT
Lloyd-Williams et al. ⁶⁹	Hospice day unit, all advanced cancer, N = 57	Focused narrative intervention; one session, 25–60 min	Standard care	EMD: PHQ-9	RCT
Mok et al. ⁷⁰	Inpatient oncology ward (no curative treatment), all advanced cancer, N = 84	Meaning of Life intervention; two sessions, 30–60 min	Standard care	QOL: single-item, EMD: QOLC-E (negative emotions), EXD: QOLC-E (existential distress)	RCT
Ng et al. ⁷¹	Inpatient palliative care, all advanced cancer, N = 60	Mindful breathing; one session, 5 min	Standard care + normal listening	QOL: distress thermometer	RCT
Domingo et al. ⁷²	Inpatient palliative care, advanced cancer, N = 68	Music therapy; four sessions, 30–45 min	Standard care	QOL: single-item, EMD: HADS	CCT
Steinhauser et al. ⁷³	Inpatient and outpatient palliative and hospice care, 34 non-cancer, N = 82	Outlook intervention; three sessions, 45–60 min	Standard care	EMD: CESD, EXD: QOLC-E (existential distress)	RCT
Vulkanovic et al. ⁷⁴	Inpatient and home-based palliative care, two non-cancer patients, N = 56	1. Dignity therapy, 2. Life review; two sessions, 60 min	Standard care	EMD: FACT-G (emotional well-being), EXD: Brief Measure of Generativity	RCT
Warth et al. ⁶⁰	Inpatient palliative care, two non-cancer patients, N = 84	Music therapy; two sessions, 30 min	Standard care + prerecorded mindfulness exercise	QOL: single-item, EMD: QLQ C15-PAL (emotional)	RCT
Xiao et al. ⁷⁵	Home-based hospice care, advanced cancer, N = 80	Life review; three sessions, time unclear	Standard care	QOL: single-item, EMD: QOLC-E (negative emotions), EXD: QOLC-E (existential distress)	RCT

QOL: quality of life; EMD: emotional distress; EXD: existential distress; HADS: Hospital Anxiety and Depression Scale; FACIT-SP: Functional Assessment of Chronic Illness Therapy—Spiritual Well-Being; HHI: Herth Hope Index; HQLI-R: Hospice Quality of Life Index—Revised; ESAS: Edmonton Symptom Assessment Scale; PHQ: Patient Health Questionnaire; QOLC-E: Quality-of-Life Concerns in the End-of-Life; CESD: Center for Epidemiological Studies Depression; FACT-G: Functional Assessment of Cancer Therapy: General; QLQ C15-PAL: Quality of Life Questionnaire for Palliative Care; RCT: randomized controlled trial; CCT: controlled clinical trial.

Table 3. Risk of bias assessment.

Study	RAND	ALLO	BLPP	BLOA	INCDAT	SELREP
Ando et al. ²¹	Unclear	Low	High	High	High	Unclear
Chochinov et al. ²⁰	Low	Low	High	Low	High	Low
Domingo et al. ⁷²	High	High	High	High	Unclear	Unclear
Duggleby et al. ⁶⁴	Unclear	Unclear	High	High	High	Unclear
Hall et al. ⁶⁵	Low	Low	High	High	High	Low
Hilliard ⁶⁶	Unclear	Unclear	High	High	Unclear	Unclear
Horne-Thompson and Grocke ⁶⁷	Low	Low	Unclear	High	High	Unclear
Juliao et al. ⁶⁸	Low	Low	High	High	High	Unclear
Lloyd-Williams et al. ⁶⁹	Unclear	Low	High	High	High	Unclear
Mok et al. ⁷⁰	Low	Low	High	High	High	Unclear
Ng et al. ⁷¹	Unclear	Unclear	Unclear	Unclear	Low	Unclear
Steinhauser et al. ⁷³	Unclear	Unclear	Unclear	Low	High	Unclear
Vuksanovic et al. ⁷⁴	Low	Low	High	High	High	Unclear
Warth et al. ⁶⁰	Low	Low	Unclear	High	Low	Low
Xiao et al. ⁷⁵	Low	Unclear	High	Low	Low	Unclear

RAND: random sequence generation; ALLO: allocation concealment; BLPP: blinding of participants and personnel; BLOA: blinding of outcome assessor; INCDAT: incomplete data; SELREP: selective reporting.

America,^{20,64,66,73} Asia,^{21,70,71,75} Europe,^{60,65,68,69,72} and Australia.²⁰

Mean age of patients ranged from 47 to 75 years and the percentage of female patients varied between 34% and 71%. A total of ten studies^{21,60,65–68,70–72,75} were conducted in an inpatient palliative care setting and five^{20,64,69,73,74} recruited participants from outpatient, home-based, or mixed settings. One study⁷² was a non-randomized controlled trial, and all others^{20,21,60,64–71,73–75} were randomized trials. Eight^{64–66,69,70,72,73} compared their results to standard care, while four studies^{21,60,67,71,75} used an additional active control treatment. Three studies^{20,74,76} implemented a three-arm design, for which we compared each psychosocial intervention to the standard care control group.

The most frequently used interventions were dignity therapy,^{20,65,68,74} music therapy,^{60,66,67,72} and other life review^{21,74,75} and meaning-based techniques,^{69,70,73} with a median frequency of two sessions. Session duration ranged from 20 to 90 min. Eight interventions^{20,21,64,65,68,70,74,75} created some sort of legacy with or for the patient (e.g. transcript, album, audiotape).

Risk of bias assessment

Table 3 presents the results of the risk of bias assessment.⁵⁷ Overall, the methodological quality of the primary studies was low. Only 4 out of 15 studies were rated a low risk of bias in three or more domains. However, risk of selection bias was high in only one non-randomized study.⁷² All other studies either implemented proper methods of randomization and allocation concealment or did not provide sufficient information. Risk of performance bias was generally high in all studies. Although

participants were blinded to the study hypotheses in some studies, none implemented measures of expectancy as recommended in the guidelines.⁵⁷ Outcome assessor blinding to reduce the likelihood of detection bias was performed in only three studies.^{20,73,75} As expected in palliative care research, attrition bias was a severe issue in the majority of studies, but only two^{60,75} performed an intention-to-treat analysis, and one study did not experience any patient drop-out due to a very short intervention duration.⁷¹ None of the trials were rated a high risk of reporting bias. However, for only three studies,^{20,60,65} we could find a published study protocol referring to predefined outcomes.

Meta-analysis on quality of life

Nine studies reported $k = 10$ relevant effects of psychosocial interventions on quality of life. Eight effect sizes were based on single- or two-item scales, while two were summary scores of comprehensive questionnaires. Six comparisons indicated superiority of the psychosocial interventions, while four effect sizes did not differ significantly from zero.

A random-effects model revealed a statistically significant, medium-sized overall effect of $d = 0.73$ (CI = 0.15 to 1.30, $p = 0.02$) in favor of the psychosocial interventions. There was significant heterogeneity among individual effects ($Q = 67.48$, $p < 0.01$, $I^2 = 91\%$). Meta-regression did not find any potential moderator to significantly explain variance across studies (all $p > 0.05$). Egger's test indicated a high likelihood of funnel plot asymmetry ($t = 4.23$, $p < 0.01$). Finally, inspection of model diagnostics led to the identification of two highly influential studies^{72,75} with very large effect sizes ($d = 1.82$ and $d = 2.61$).

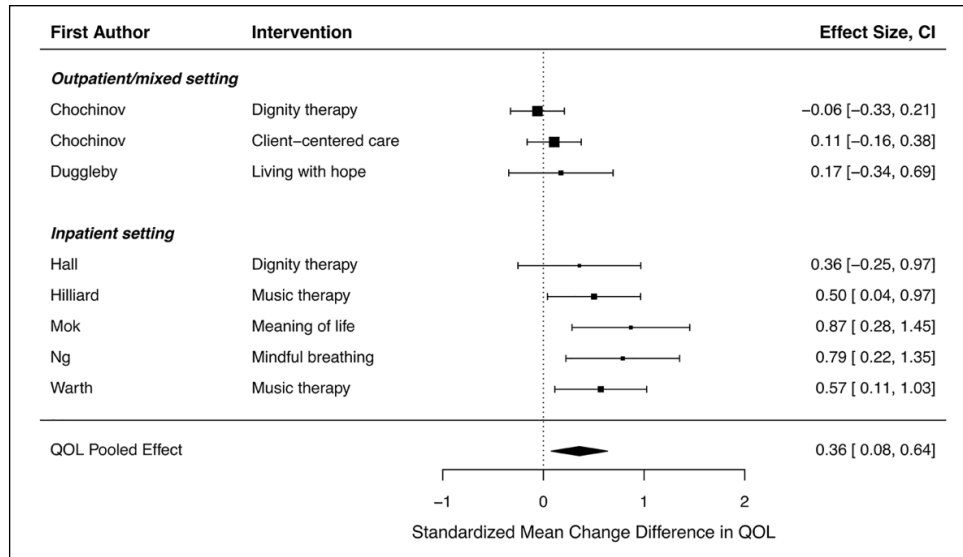


Figure 2. Forest plot for quality of life (after exclusion of two outliers). CI: 95% confidence interval; QOL: quality of life.

Exclusion of the two studies resulted in a small, but significant overall effect again supporting the superiority of psychosocial interventions ($d = 0.36$, CI = 0.08 to 0.64, $p = 0.02$). In total, 60% of the total variability was due to heterogeneity ($I^2 = 60\%$, $Q = 17.23$, $p = 0.02$). Effect sizes were significantly stronger for interventions provided in an inpatient palliative care setting ($d = 0.61$) than for outpatient/mixed settings ($d = 0.04$; $t = -5.26$, $p = 0.002$). The inclusion of this moderator accounted for all residual heterogeneity ($I^2 = 0\%$; $Q = 3.07$, $p = 0.80$). Egger’s regression test did no longer indicate a statistically significant risk for publication bias ($t = 0.92$, $p = 0.40$). Figure 2 depicts the effect sizes and confidence intervals for the final model after exclusion of the two outliers and separate for the categories of the significant moderator “setting.”

Meta-analysis on emotional distress

We could extract $k = 15$ effect sizes from 13 trials on the reduction of emotional distress. The most frequently used questionnaires were the Hospital Anxiety and Depression Scale (HADS; total, depression, or anxiety score, $k = 6$), Functional Assessment of Cancer Therapy: General (FACT-G; emotional well-being scale, $k = 2$), and the Quality-of-Life Concerns in the End-of-Life (QOLC-E; negative emotions scale, $k = 2$). Six single effect sizes were statistically significant favoring the psychosocial intervention, while the CIs of nine effects included zero (Figure 3).

The pooled effect was medium-sized with $d = -0.51$ (CI = -0.77 to -0.26 , $p < 0.01$), indicating a significantly stronger reduction of emotional distress through psychosocial interventions in comparison to the control conditions. The included effects were heterogeneous

($Q = 52.01$, $p < 0.01$, $I^2 = 73\%$), which we could not explain through inclusion of potential moderators (all $p > 0.05$). There was no evidence for funnel plot asymmetry ($t = -1.80$, $p = 0.09$) or influential studies in the data on emotional distress.

Meta-analysis on existential distress

Nine studies examined the effects of $k = 11$ psychosocial interventions on existential distress using the Functional Assessment of Chronic Illness Therapy—Spiritual Well-Being (FACIT-SP; $k = 3$), Herth Hope Index (HHI; $k = 2$), Quality-of-Life Concerns in the End-of-Life (QOLC-E; existential distress scale, $k = 2$), or other scales ($k = 4$). The CIs of $k = 4$ effect sizes differed significantly from zero.

The pooled effect was statistically significant with $d = -0.50$ (CI = -0.84 to -0.14 , $p = 0.01$), indicating stronger reductions in existential distress for psychosocial interventions. Effect sizes were again highly heterogeneous ($Q = 49.58$, $p < 0.01$, $I^2 = 79\%$) and asymmetrically distributed ($t = -2.83$, $p = 0.02$). While none of the proposed moderators could explain this variability, one very large effect size ($d = -1.42$)²¹ was identified as highly influential on the overall results.

Exclusion of this outlier led to a small, but significant overall effect of $d = -0.40$ (CI = -0.71 to -0.07 , $p = 0.02$). The remaining heterogeneity could be explained by the moderator “measurement instrument” ($Q = 8.00$, $p < 0.24$, $I^2 = 0\%$). As shown in Figure 4, reductions in existential distress were significantly stronger for scales other than the FACIT-SP ($F = 6.46$, $p = 0.03$). Regression test no longer indicated funnel plot asymmetry ($t = -0.88$, $p = 0.42$).

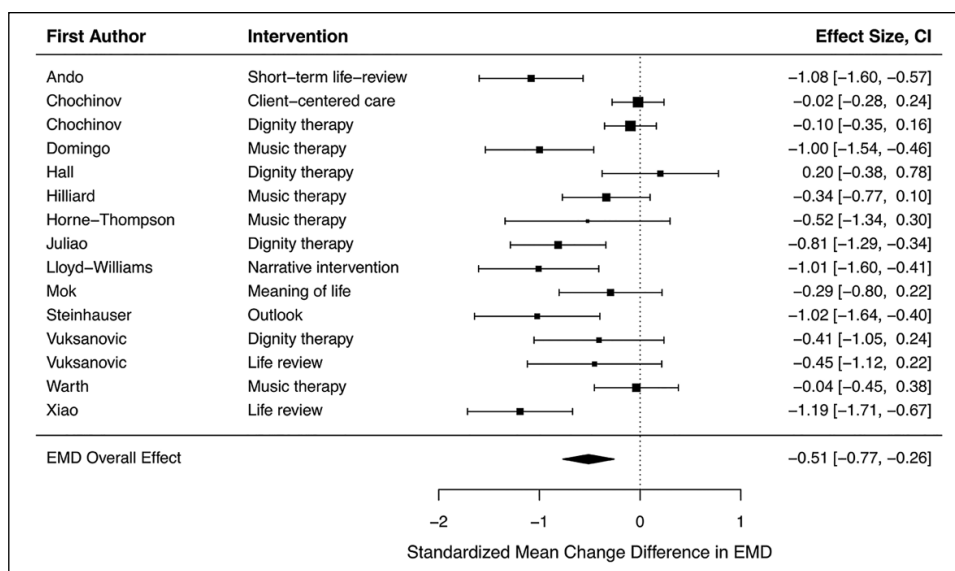


Figure 3. Forest plot for emotional distress.
CI: 95% confidence interval; EMD: emotional distress.

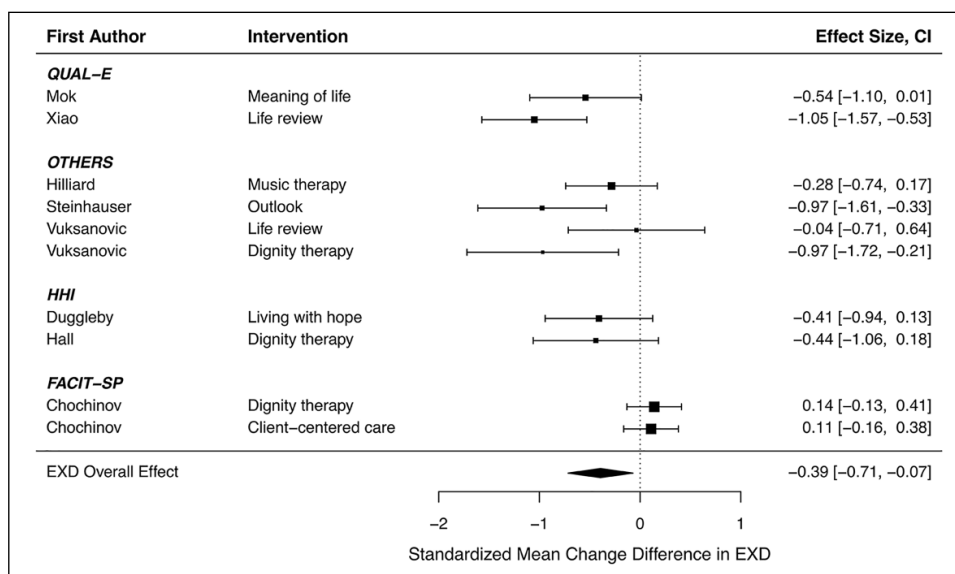


Figure 4. Forest plot for existential distress (after exclusion of one outlier).
CI: 95% confidence interval; EXD: existential distress.

Discussion

In this study, we performed a systematic literature search to identify randomized and non-randomized controlled trials examining the effects of brief psychosocial interventions on quality of life, emotional distress, and existential distress. We thereby defined a “brief” intervention as one that would rely on a maximum number of four sessions and treatment duration of less than 21 days. Abstract and full-text screening led to the inclusion of 15 studies, which reported the effects of $k = 17$ interventions.

Magnitude of pooled effects

Beneficial effects of psychosocial interventions were identified on all three outcome domains (quality of life, emotional distress, existential distress), and all were statistically significant. The initially medium-sized, but highly heterogeneous effect on quality of life was reduced to a small, but better interpretable effect ($d = 0.36$) after the exclusion of two potential outliers. Regarding the reduction of emotional distress, we found a medium-sized, pooled effect ($d = -0.51$) in favor of psychosocial

interventions. The initial calculation of the pooled effect on existential distress was again possibly biased by one outlier. Exclusion of this study led to a small overall effect ($d = 0.40$), indicating that the included interventions were significantly more capable of reducing existential suffering than the corresponding control conditions.

Thus, evidence supports the use of psychosocial interventions in palliative care, as they effectively improve quality of life and reduce emotional and existential distress. Particularly, these results are clinically relevant for end-of-life care, as we included only brief interventions that were assumed to be feasible in such a setting. The observed effects on quality of life and existential distress were small, and the magnitude for emotional distress just passed the threshold of a medium effect size. These findings are in line with previous research.^{25,40,42} Although these effects may seem rather weak at first glance, several arguments need to be taken into consideration: first, our chosen statistical approach compared differences in pre-to-post change between the experimental and control groups. This is a more conservative method than the common post-score-only comparison, which, in general, may lead to larger, but also more biased results.⁷⁷ Second, regarding the restriction in treatment sessions, very large effect sizes would be unrealistic to achieve. Third, small-to medium-sized effects may very well be substantial as they were measured on clinical outcomes highly relevant in palliative end-of-life care (quality of life, emotional distress, existential distress). There is reason to assume that the interventions may be capable of promoting clinically meaningful improvements in at least some groups of individuals, considering results from qualitative⁷⁸ or auxiliary analyses.⁶⁰ Finally, the exclusion of outliers in our final models further decreased the magnitude of pooled effects. This was again a deliberate decision in favor of the most conservative approach, while we cannot rule out that the excluded original studies produced very strong, but valid effects.

Moderating variables

The most frequently used methods in the included studies were dignity therapy, life review, meaning-based interventions, and music therapy. Interestingly, the type of intervention did not explain any variance in the observed effects on any outcome. Hence, we did not find any intervention to be superior or inferior to others. It is possible, however, that the data-driven categorization (life review/meaning-based interventions vs others) and the limited number of studies might be responsible for the lack of moderating effects.

The observed effects on quality of life were stronger if study sites were restricted to inpatient care. The inclusion of outpatient or other recruiting centers may probably have led to heterogeneous samples. The setting of, for

example, an inpatient palliative care ward may facilitate standardization of procedures and reduce the number of confounding variables regarding the definition of “standard care.” The beneficial effects of psychosocial interventions on existential distress were smaller in studies using the FACIT-SP as an outcome. This is surprising, as the scale is a frequently used and validated self-report measure for spiritual well-being.⁷⁹ It could be that the FACIT-SP is not sensitive to change produced by psychosocial interventions.²⁰ However, this seems less likely considering that the excluded study by Ando et al.²¹ also used the FACIT-SP and found a very large effect size.

For both of the above-mentioned moderators, we need to take an alternative explanation into account. That is, both meta-regressions were influenced by data-driven categorization and the exclusion of outliers. Unintentionally, this led to a situation, where effect sizes from one very powerful study with weak results (Chochinov et al.²⁰) were compared to the majority of studies, which had less statistical power but larger effects. Thus, the identification of significant moderators could be biased by a methodological artifact. Contrariwise, these findings may help understand why the well-powered study by Chochinov et al.²⁰ found null effects, whereas life review techniques in other studies were partly more effective.²⁵ While the Chochinov study showed a high methodological quality in general (Table 3), the trial was also conducted in three different countries and recruited patients from different hospital and community settings. As in other recently published international, multicenter studies,⁸⁰ difficulties to create a homogeneous sample and to standardize procedures may have limited internal validity. Moreover, the authors suggest the selection of possibly inappropriate outcomes (e.g. FACIT-SP) as one reason for the lack of significant effects in their study.

The number of sessions alone did not predict any differences between the studies. This is not surprising, as a maximum of four was defined as an eligibility criterion and the majority of interventions were homogeneous in this regard, offering two or three sessions (Table 2). Nor the distinction between active control treatments and standard care did explain any variance. Only four studies^{21,60,67,71} implemented active control conditions and none of them measured effects on all outcome domains, reducing statistical power to detect any moderating influence.

Strengths and limitations

The major strength of this study is its high relevance for clinical practice in end-of-life care. Contrary to previous reviews and meta-analyses, we focused explicitly on interventions that could be offered to patients nearing the end of their lives and receiving specialized palliative care. Thus, our analyses identified psychosocial interventions

that are effective in improving highly relevant health outcomes to this patient population (i.e. quality of life, emotional distress, existential distress). Moreover, we offer a conclusive and sound methodological approach for our data synthesis. Effect size calculations accounted for possible baseline differences, and the robustness of the random-effects models was tested in sensitivity analyses. Personal contact to authors helped to complete data extraction if important information was not reported in the publication. We are therefore confident to have included all available data meeting the eligibility criteria, which the analysis on publication bias confirmed.

This study also faces a number of limitations. First, the validity of our findings may be restricted by the generally low methodological quality of the primary studies, as we observed a high risk of performance, detection, and attrition bias across studies. Second, 15 studies with a total sample size of $N = 1248$ were eligible for inclusion, which we perceived as a reasonable number to perform meta-analysis. As not all outcomes were assessed in each of the primary studies, meta-regression analysis may still have been underpowered. In addition, although some of the studies used the same measuring instruments, the variety of measures concerning our target criteria might account for a minor decrease in comparability between studies. Finally, we chose to not include data on follow-up assessments. While the question on the perpetuation of effects is important, initial screening revealed that only a very limited number of studies were able to provide this information.

Conclusion

This study offers a meta-analytic synthesis of evidence available on the effects of psychosocial palliative care. A vivid discourse in this literature underpins a growing awareness for the importance of psychological, social, and spiritual needs accompanying a terminal illness. Therefore, psychosocial techniques apt for the work in palliative care have been developed recently. These include dignity therapy and related life review or meaning-based interventions, mindfulness, and creative arts-based therapies. Our meta-analysis showed brief psychosocial interventions that rely on a small number of sessions to be effective in improving quality of life and in reducing emotional distress and existential suffering. Hence, such interventions should be offered to patients nearing the end of their lives, if they express psychosocial or spiritual needs.

The present summary of evidence also revealed methodological biases in many of the primary studies. In order to further improve the evidence in this field, future studies may particularly include a documented plan to handle missing data and an a priori definition of primary and secondary outcomes, preferably published in a study protocol.

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