Effects of group therapy in breast cancer: survival and psychosocial benefits

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The effects of psychosocial group therapy on breast cancer patients are a subject of increasing interest, due to recent studies demonstrating potential benefits to survival and psychosocial wellbeing. However, these positive findings are balanced by many papers finding no such benefits, and a lack of consensus persists on the existence, form and degree of the advantages conferred by supportive group psychotherapies. This review assesses evidence for the survival and psychosocial effects via key supporting and critical papers. Additionally, this review examines the third major debate in this area of research by assessing whether such psychotherapies are of more value for participants with high distress at baseline assessments. It will be concluded that claims of survival benefits from psychosocial group therapies are unjustified on the existing evidence, but that there is good evidence for some psychosocial advantages being conferred by such therapies. These benefits may be stronger in those with high baseline distress, this is unclear. Finally, recommendations are made for future research and amendments to common study methodology.

Introduction

Breast cancer is one of the most common female cancers, and the second-greatest cause of cancer-related deaths. There are 1.2 million diagnoses and 500,000 deaths annually (World Health Organisation, cited in Grassi, Sabato, Rossi, Marmai and Biancosino (2010)).

Interest in the role of psychological and behavioural variables in cancer has grown with increasing recent awareness of their impact on aetiology/prognosis. Although there have been recent improvements in treatment and survival, psychological interventions for breast cancer are required (Peto, Boreham, Clarke, Davies and Beral, 2000; Riggs and Hartmann, 2003; Smith and Dowsett, 2003). Early delivery of efficacious psychological interventions may potentially improve mental health, treatment-related health behaviours and biological outcomes in breast cancer (Hewitt, Herdman and Holland, cited in Andersen et al., 2008).

Improvements to survival and psychosocial variables have been demonstrated using various structured/unstructured group interventions (detailed in relevant sections below), which are more resource-efficient and as effective as individual interventions (Blake-Mortimer, Gore-Felton, Kimerling, Turner-Cobb and Spiegel, 1999; Classen et al., 2008; Fawzy and Fawzy, 1998; Hosaka, 1996; Trijsburg, van Knippenberg and Rijpma, 1992).

However, the evidence for the efficacy of group psychotherapies is mixed and many papers have failed to detect positive outcomes. Cunningham and Edmonds (1996) noted the lack of consensus on the matter due to limited evidence and, despite the greater volume of research accessible now, any effects of psychosocial group therapy with breast cancer patients remain in question.

There are three main debates in this area of research; whether psychosocial group therapy confers improvements to survival and biomedical outcomes such as cancer recurrence, whether it confers improvements to psychosocial wellbeing, and whether greater improvements are conferred to participants with high baseline distress. For each area, a supporting and contradictory paper will be presented, enabling an overall conclusion regarding the value of psychosocial group therapies for breast cancer.

Survival

A key study of the effect of psychosocial interventions on cancer survival was carried out by Spiegel et al. (1989), and the findings have prompted a great deal of research. The authors conducted a randomised controlled trial (RCT) and found that supportive-expressive group therapy (SEGT) participants with breast cancer survived 18 months longer on average than controls who received routine oncological care.

Subsequent studies have found survival benefits for psychosocial interventions in gastrointestinal/geriatric/hematologic/skin cancers (Fawzy, Canada and Fawzy, 2003; Fawzy et al., 1993; Kuchler et al., 1999; McCorkle et al., 2000; Richardson, Shelton, Krailo and Levine, 1990). Suggested mechanisms for such improvements are improved self-care and treatment adherence, changed disease progression through reduced stressor immune activation or decreased inflammatory response mediated by depressive symptom reduction (Antoni et al., 2006; Kissane et al., 2007; Thornton, Andersen, Schuler and Carson, 2009).

However, other papers have demonstrated no such survival advantages in cancer (Cunningham et al., 1998; Edelman, Lemon, Bell and Kidman, 1999; Ilnyckyj, Farber, Cheang and Weinerman, 1994; Kissane et al., 2004). Stefanek et al. (2009) have claimed that studies showing positive changes have been methodologically flawed.

Spiegel (2002) argued that intervention success may depend on the quality of the therapy, suggesting that psychological and physical benefits may be causally linked. This argument is weak, as several studies have demonstrated decreases in distress/pain/anxiety but no difference to survival, despite good therapy quality (Goodwin et al., 2001; Kissane et al., 2007; Linn, Linn and Harris, 1982).

Meta-analyses reflect this lack of consensus. Some systematic reviews claim ambiguous results (Evans et al., 2005; Spiegel, 2002; Spiegel and Giese-Davis, 2003), while a systematic review and two meta-analyses identified no effect of group psychotherapy on cancer patient survival (Chow, Tsao and Harth, 2004; Edwards, Hailey and Maxwell, 2004; Newell, Sanson-Fisher and Savolainen, 2002).

There is therefore little consensus at present on the effects of psychosocial interventions for breast cancer.

No survival advantage

Spiegel et al. (2007) replicated Spiegel et al. (2002), hypothesising that participants who underwent psychosocial supportive therapy and an educational programme would survive longer than controls who received only the educational programme.

The 125 participants had metastatic or recurrent breast cancer. Exclusion criteria were comorbid cancers or life-threatening illness. Average breast cancer diagnosis was 72 months prior to study commencement, and average metastasis was 25 months prior.

Participants were recruited through oncologists/social workers/self-referral, and were assigned by location to one of three sites. All received standard cancer treatment throughout the study.

Biased coin-design randomisation to conditions was used, promoting between-condition comparability in prognostic demographic/medical variables.

The 64 intervention participants received three weekly 90-minute therapy sessions in groups of 3-15, each led by a different pair of therapists from psychologists/social workers/one psychiatrist. Spiegel (the psychiatrist) led one group and supervised the others. Minimum therapy duration was 1 year, though most continued for longer (up to 12.5 years).

The intervention was semi-structured SEGT, with participants encouraged to confront mortality-related fears/express emotion/strengthen support and healthcare relationships/find meaning in life. Additionally, participants learned self-hypnosis for controlling anxiety/pain (Spiegel and Classen, 2000; Spiegel and Spiegel, 2004). SEGT appears to improve mood disturbance/stress/pain/emotional regulation in cancer patients, and such supportive interventions including coping skills and stress management training are most likely to produce positive outcomes (Classen et al., 2001; Fawzy and Fawzy, 1998; Giese-Davis et al., 2002).

In the self-directed educational programme, participants received materials on medical/emotional aspects of breast cancer and a 1-year health library membership. 53% of controls and 55% of intervention participants utilised these resources. The 61 control participants received only this component, while intervention participants also received SEGT.

Intention-to-treat Kaplan-Meier survival analysis was used for all 125 original participants. Median survival time was 32.8 months, substantially longer than the 17 months reported in past studies with similar participant

disease characteristics (Goodwin et al., 2001; Yalom and Greaves, 1977). Spiegel et al. (2007) attribute this to improving breast cancer treatment (Peto et al., 2000).

There was no significant effect of treatment on survival time, but there was a significant site-by-condition interaction; at one site, control survival time was significantly better, in another, control survival time was worse, and in the third between-conditions survival time was approximately equal.

There were significant differences between sites in age, education and hours worked per week, but this did not account for site differences in treatment effects on survival. However, moderator analysis detected a significant treatment effect by OR status interaction, suggesting that OR-negative participants survived longer in the intervention condition (Kraemer, Frank and Kupfer, 2006). When OR status was included in analysis, the significant site-by-treatment interaction disappeared.

OR status seems to affect breast cancer prognosis, which is poorer in OR-negative women (Sorlie et al., 2001). OR-negative women gain survival benefits from joint chemotherapy and hormonal therapies and OR-positive women do not. This may be due to higher resistance to hormonal treatment in OR-negative women (Berry et al., 2006; Kristensen et al., 2005). Dietary intervention also reduces breast cancer recurrence more in OR-negative women (Chlebowski et al., 2005). This may relate to OR-status breast cancer subtypes deriving from different cell lineages and conferring different treatment responses/prognoses (Perou et al., 2000; Sorlie et al., 2003). Applied to Spiegel et al.'s (2007) findings, SEGT in OR-positive women may have been superseded by greater efficacy of routine hormonal treatments, while showing an effect in OR-negative women because poorer hormonal treatment effectiveness leaves capacity for improvement via SEGT. This is a valid target for future research.

There were several positive aspects to the study methodology, such as the use of intention-to-treat Kaplan-Meier survival analysis. This was appropriate/necessary due to the 86% mortality rate; analysis based on completion would be misleading (Stefanek et al., 2009).

The statistics were well-planned, with 125 participants being determined a priori to be sufficient to determine effects of condition on survival. Additionally, regression was centred to avoid erroneous interpretations of multi-site data (Kraemer and Blasey, 2004).

The control group provided a reasonable comparison for the intervention, since a similar proportion used the educational resources in both groups.

The primary dependent variable of survival was a well-defined, ecologically valid assessment of success (avoiding surrogate outcome measures based on presumed relationships).

There were weaknesses in the design. Firstly, participant attitudes were misjudged. Since Spiegel et al. (1989) encountered problems with SEGT adherence, the authors anticipated similar issues. However, due to increasing acceptance of cancer-related therapies, control participants expressed disappointment after randomisation. This may explain why significantly more control participants attended external cancer groups, potentially explaining the lack of significant difference in survival time between conditions (if controls were gaining benefits independently).

Spiegel et al. (2007) also noted the significant differences between conditions at baseline (in age/education/employment) which may have produced the false site-by-condition interaction for period disease-free and metastasis/recurrence location. This may be due to stratification in a small sample. Replications should include a larger sample or develop a better method of equalising between-group differences, possibly randomising by site.

Additionally, the authors note that the study director ran the randomisation program. This may have enabled researcher bias if input is required. Use of an independent observer (or, if no input is required, full description of the process) would be advisable for replications.

Further, there was great unexplained variability in intervention group size (3-15 participants). Presumably, participants in a group of 3 will speak and be directly involved more than in a group of 15. This may cause differential effects, reducing internal validity and potentially confounding results.

There may have been inter-therapist differences in intervention delivery. Therapists were drawn from psychologists/social workers/a psychiatrist, and although the psychiatrist supervised other groups, this is unlikely to have controlled for differences in experience/approach. This is important as quality of facilitation and therapist experience may affect outcomes (Crits-Christoph et al., 1991; Kissane et al., 2003; Sheard and Macguire, 1999; Sherman et al., 2004; Stein and Lambert, 1995).

Further, there was differential SEGT attendance; participants attended for 1-12.5 years. Presumably, longer attendance confers stronger effects. The lack of detailed information on attendance periods in the intervention/control conditions is unhelpful, and may mask a confound.

Other key information was omitted, including participant use of mood-altering medication and psychological interventions independently of the study. Since these produce changes in the psychosocial variables targeted by SEGT, this should have been included.

The sample size (and hence statistical power) may have been too low to produce externally valid findings (Coyne, Stefanek and Palmer, 2007). For instance, Andersen et al. (2008) had twice as many participants for a similar study and this was insufficient (Stefanek et al., 2009).

Despite these flaws, this is a significant finding because this study failed to replicate Spiegel et al.'s (1989) original findings, which was one of very few papers to find a survival advantage to psychosocial group therapy.

Survival advantage

Based on Andersen, Kiecolt-Glaser and Glaser's (1994) biobehavioural model of cancer stress and disease course, Andersen et al. (2008) conducted an RCT to test whether a psychological intervention could protect against adverse effects of stress, thereby altering cancer outcomes.

The 227 female participants had received regional breast cancer surgery and were awaiting adjuvant therapy (Andersen et al., 2004). Exclusion criteria were previous cancers, treatment refusal, being aged under 20 or over 85 years, severe/untreated psychopathology, neurological disorders, dementia or immune disorders.

Using White and Freedman's (1978) minimisation method (biased towards the smallest group), participants were randomised to condition by medical/demographic variables. There were no significant between-condition differences in sociodemographics/disease/prognosis/surgery/adjuvant treatments (Andersen et al., 2004).

The first endpoint anticipated was breast cancer recurrence, but the key factor studied was survival.

Assessment was via interviews/questionnaires/medical records/blood sample, carried out at baseline, at 4 and 12 months, at 6-monthly intervals in years 2-5 and annually thereafter. Questionnaires included the Profile of Mood

States, the Impact of Events Scale, four social adjustment assessments and eight health behaviour measures. Participants received standard oncological follow-up throughout the study.

Controls (113 participants) received only assessments. Intervention participants (114 participants) received assessments plus a psychological intervention. This was conducted in groups of 8-12 by two psychologists. An initial intensive phase (4 months of weekly 90-minute sessions) preceded a maintenance phase (8 monthly sessions), with 26 sessions over 12 months. Patient attendance and treatment adherence were high (Andersen et al., 2007).

The intervention attempted to reduce distress, improve quality of life, improve health-related behaviours, facilitate treatment compliance, teach stress-reduction, assertiveness and coping strategies, and build support networks.

By 1-year follow-up, intervention participants had significantly improved on psychological/behavioural/health/immunity secondary outcomes compared to controls (Andersen et al., 2004, 2007). There were no significant differences between groups on mood-altering medication or psychological interventions independently sought/received.

Intention-to-treat analyses were used. Many prognostic covariates were included in multivariate comparison of conditions, removing known predictors of survival/recurrence.

After a median of 11 years, breast cancer recurrence, death with breast cancer as primary cause and all-cause mortality was significantly lower in the intervention group than the control group. Median time to recurrence or death (all-cause or due to breast cancer) was longer for the intervention group. These findings were reconfirmed with post-hoc analysis excluding intervention participants who attended less than 80% of the intervention. The intervention and control groups diverged at approximately 20 months.

These unusual results contradict the majority of studies. For instance, Spiegel et al. (2007) delivered a more intensive, longer-term intervention and found no survival benefit. Andersen et al. (2008) note three potential explanations for their results.

Firstly, degree of engagement with the intervention may relate to health improvements. Distress reduction at 4-month assessment predicted health improvements at 12 months; those with most distress reduction had practiced relaxation most (Andersen et al., 2004; 2007).

Secondly, intervention-mediated decreases in stress level may have reduced negative biological effects (Chida, Hamer, Wardle and Steptoe, 2008). Stress was inversely related to immunity at initial assessment, and stress decreases were linked to reduced symptoms (Andersen et al., 1998; 2004; 2008). This fits existing research demonstrating that stress hormones mediate immune responses, tumour growth and survival (Antoni et al., 2006; Mormont et al., 2000; Raison and Miller, 2003; Rich et al., 2005; Sood et al., 2006; Thaker, Lutgendorf and Sood, 2007).

Thirdly, stress-related inflammatory processes increase during tumour growth (Elenkov and Chrousos, 2002; Kim et al., 2008; Merritt et al., 2008; Thornton, Andersen and Carson, 2008). At present this is only correlation; if there is a causal relationship, however, a stress-reducing intervention like Andersen et al.'s (2008) may limit disease progression (Bunt, Sinha, Clements, Leips and Ostrand-Rosenberg, 2007).

Intervention participants who experienced reduced stress may therefore have indirectly gained improvements to breast cancer recurrence/death and all-cause mortality.

There are positive methodological elements. Firstly, intervention group sizes are reasonably consistent, removing the aforementioned potential confound.

The primary dependent variables were well-defined with direct measures (recurrence defined by biopsy, death time/cause by certificate).

Use of intention-to-treat analysis was wise (see above) and excellent follow-up produced 100% mortality data and 93% recurrence data (Kaufmann, 2009).

The authors ensured that there were no significant between-condition differences on mood-altering medication/psychological interventions received independently, which was wise as either could affect secondary outcomes and, theoretically, disease outcomes.

Intervention groups were run by professional psychologists, reducing the aforementioned potential confound of differential experience/training.

Follow-up was reasonably long-term, which was important as survival differences did not appear until 20 months.

There were no significant between-condition differences in race, which was positive because differential race-based mortality and treatment response could present a confound if imbalanced (Chu, Tarone and Brawley, 1999; Li, Malone and Daling, 2003; Mandelblatt et al., 2004). Mixed race samples increase ecological validity, although findings cannot be applied to individual races as differences may exist.

Many design weaknesses are presented by Stefanek et al. (2009). Firstly, survival was not identified as a primary endpoint a priori, nor did they set a fixed observation period or justify the period eventually chosen (Antoni et al., 2006; Miller, Ancoli-Israel, Bower, Capuron and Irwin, 2008).

Use of post hoc analyses, analyses capitalising on chance, and failing to report unadjusted outcomes by e.g. Kaplan-Meier survival analyses were questionable. Additionally, although some prognostic covariates were rightly retained in analysis to compensate for baseline differences, many were pointlessly retained despite baseline equivalence, leading to a false positive (Stefanek et al., 2009). The authors cited Scott, McPherson, Ramsay and Campbell (2002) in support of this method, although this paper warns against such misapplication as it may produce invalid results.

Andersen et al. (2008) also used an unreasonably high number of covariates in proportion to outcome variables; such over-fitting of a regression model can produce high predicted variance using random numbers as predictors (Babyak, 2004; Peduzzi, Concato, Feinstein and Holford, 1995). Although Kaufmann (2009) argues that the many covariates demonstrate good exploratory research into biobehavioural mechanisms, such research should not make treatment-related claims (Stefanek et al. (2009).

Once these errors were amended, Stefanek et al.'s (2009) reanalysis found no significant difference between conditions on recurrence/breast cancer death/all-cause mortality.

Additionally, the power analysis estimate was far lower than that of a similarly-structured study (Romond et al., 2005; Stefanek et al., 2009). This assumes a highly potent intervention testable with a small sample, which often lack statistical power to produce externally valid, replicable results (Coyne et al., 2007; Kaufmann, 2009).

Stefanek et al. (2009) also note the authors' failure to cite studies which reported no effect of group psychosocial interventions (Kissane et al., 2004) and exaggeration of favourable findings (Andersen et al., 2007).

Further, 25% of participants had anxiety/depression. This provides an ecologically valid sample, but such conditions may produce differential responses to psychosocial therapies, increase recurrence and worsen prognosis, potentially biasing results (Watson, Homewood, Haviland and Bliss, 2005; see relevant section below also). However, there were no significant differences between conditions on number of depressed/anxious participants, so condition findings can be compared. Further research to differentiate responses from depressed/non-depressed participants would be useful in determining intervention value, though.

The authors note that participants had non-metastatic breast cancer, which has a better prognosis, different concerns (coping versus mortality) and psychosocial profiles to metastatic cancer (Vos, Visser, Garssen, Duivenvoorden and Haes, 2007). Findings cannot generalise to advanced disease (Classen et al., 2001; Goodwin et al., 2001; Spiegel et al., 1981).

Additionally, control participants may have been disappointed (as aforementioned) and sought independent support, reducing control effectiveness as a comparison for the intervention. Data on external support should have been gathered, as Spiegel et al. (2007) did, to eliminate this potential confound. The ethics of failing to provide control participants with any kind of support are questionable too, since 20-25% of participants typically develop affective disorders by a year post-diagnosis (though the study was in addition to, not instead of, existing treatment) (Grassi et al., 2010; Parle, Jones and Macguire, 1996).

The assessment-only control condition failed to control for the effect of regularly attending a sympathetic group; benefits identified may be due to this, not the specific intervention. Also, any intervention is likely to outperform assessment-only; comparison with an alternative such as a manualised relaxation/support programme (e.g. Kissane et al., 2007) would be more informative as to relative efficacy.

The reanalysis using participants who attended over 80% of the intervention provides a better estimate of therapeutic efficacy, but those removed from analysis are likely to have been more ill or less engaged, i.e. those with poorest outcomes (Andersen et al., 2007). The reanalysis therefore represents an overly-optimistic interpretation of the data.

This was a flawed study, invalidated by statistical and methodological errors. While perhaps having value as exploratory research, the findings are unconvincing, particularly given Stefanek et al.'s (2009) reinterpretation of results as non-significant.

Survival: conclusion

Due to the flaws in Andersen et al.'s (2008) study, accepting their highly unusual findings is unjustified. The authors do not explain how they have produced a finding contrary to the vast majority of research, nor do they address Stefanek et al.'s (2009) critique in their follow-up report (Andersen et al., 2010).

Given Stefanek et al.'s (2009) reanalysis, Spiegel et al.'s (2007) failed replication of their own findings and the lack of corroborating studies showing survival benefits in the literature, there seems little support for such an effect.

There may be survival benefits for a subgroup of OR-negative women, but much further research is necessary to investigate this possibility.

On current evidence, it is unfounded/unethical to claim survival effects of psychosocial interventions.

Psychosocial

The second debate on the value of group psychotherapy for breast cancer lies in psychosocial improvements; many studies suggest that psychosocial group therapies reduce distress and improve coping, and may produce clinically relevant improvements (Andersen, 1992; Antoni et al., 2001; Classen et al., 2001; Cunningham and Edmonds, 1996; Edmonds, Lockwood and Cunningham, 1999; Fobair, 1997; Leszcz and Goodwin, 1998; Meyer and Mark, 1995; Reuter, Scholl, Sillem, Hasenburg and Harter, 2010). Particularly, studies using SEGT or similar existentially-oriented therapies typically produce psychosocial improvements (Goodwin et al., 2001). Spiegel et al. (1981) found reductions to fatigue/confusion/tension in addition to psychosocial factors, suggesting holistic effects.

However, findings are inconsistent and other studies have demonstrated no psychological/psychosocial benefits, or only transient effects (Cunningham et al., 1998; Edelman et al., 1999; Ilnyckyj et al., 1994; Van der Pompe, 1997; Vos et al., 2007). As with survival, there is presently little consensus.

SEGT improved psychosocial wellbeing and social functioning

Kissane et al. (2007) conducted an RCT of breast cancer survival times and psychosocial outcomes after SEGT. The psychosocial outcomes are the focus here.

227 Caucasian female participants with stage IV cancer and survival prognosis of 1+ years were recruited via oncologist referral. Exclusion criteria were previous cancers, being aged 70+ years, intellectual disability or dementia. Participants received oncological treatment externally throughout the study.

Baseline assessments were conducted pre-randomisation, then at four 6-month intervals. Assessment involved the Monash Interview for Liaison Psychiatry, the EORTC Quality of Life C-30 Questionnaire, Impact of Event Scale and Mini Mental Adjustment to Cancer Scale (Aaronson et al., 1993; Horowitz, Wilner and Alvarez, 1979; Watson et al., 1994). Demographic/medical/psychosocial activity data were recorded.

Power analysis determined that 220 participants with a 2:1 intervention to control ratio was sufficient. Stratified randomisation (within 10 months of cancer recurrence on average) using an adaptive biased coin design equalised medical variables between conditions.

80 control participants attended three 1-hour relaxation classes over 3 consecutive weeks annually, learning progressive relaxation and guided imagery manualised techniques from an occupational therapist. SEGT-related themes were avoided.

147 intervention participants received the control component (from the same therapist) plus weekly 90-minute SEGT sessions (Spiegel and Spira's manual (cited in Kissane et al., 2007)). This model focuses on improving coping/relationships/communication and confronting mortality-related issues. The hypnosis component of SEGT was omitted.

To improve integration, participants were assimilated into intervention groups via a preparatory procedure. Therapy participation was for one year or longer. Each group had up to 12 participants.

SEGT groups were facilitated by pairs drawn from 9 experienced therapists (psychiatrists/psychologists/social workers), who received standardised training from Spiegel (Spiegel and Spira's manual, cited in Kissane et al. (2007)), were supervised by a senior analyst and supported by quarterly meetings of all therapists/supervisors to promote manual adherence.

Intention-to-treat Kaplan-Meier survival analysis found no significant difference in survival time between conditions, and no main effect of condition on survival when baseline characteristics were controlled for in a multivariate Cox model (supporting this review's conclusion, above).

There were no between-condition demographic/clinical/psychosocial activity differences at baseline. However, the range of intervention session attendance was 1-226, and 17% attended 5 or fewer sessions. 42% of the intervention participants attended no relaxation classes.

Slopes analysis demonstrated significant psychosocial improvement in the intervention group on the EORTC questionnaire, the helplessness-hopelessness scale of the Mini-MAC, and on intrusive thoughts on the Event Scale over controls.

Approximately a third of participants in both conditions had some form of depression and/or were taking antidepressants at baseline. By 6 months later, there was a significantly greater reduction in depression in intervention compared to control participants, and more intervention participants had remained depression-free after being depression-free at baseline (and the same applied at final assessment). There may therefore be a prophylactic effect of SEGT in breast cancer treatment. Kissane et al. (2007) argue that SEGT should therefore be used as a support model for patients with advanced cancer.

Cancer treatment adherence was higher in the intervention condition, though the main predictor of treatment received was time from randomisation to final assessment (and a lesser predictor was depression).

There are many strong points to this study. Firstly, intention-to-treat analysis and Kaplan-Meier survival analysis was used (see above).

The provision of an active, participant-engaging relaxation condition was intended to prevent demoralisation/disappointment in control participants (as recorded by Spiegel et al. (2007)). While mean experience rating of the control group was slightly higher than for the intervention group, adherence was poor and dissatisfaction was reported. Kissane et al. (2007) suggest that better pre-randomisation briefing may resolve this; perhaps some feedback-based modifications to the preparatory procedure used (itself a good idea) would be valuable.

Group size was reasonably consistent, though inevitably affected by absences. This partially avoided the group size confound noted in criticism of Spiegel et al. (2007).

The relaxation training component was effectively controlled with identical classes run by the same therapist for the same amount of time/sessions, eliminating between-therapist differences in background/experience. This was also a more realistic control than education or assessment-only (as above). Although it detracts from control effectiveness that 42% of the intervention participants did not attend relaxation classes, mandating relaxation/SEGT attendance could discourage participants so this is unavoidable.

Good measures were taken to improve intervention manual adherence and negate inter-therapist differences (due to the mixture of therapist backgrounds/experience). All therapists received manualised training and regular supervision/meetings. This confound cannot be entirely eliminated (even if only e.g. psychiatrists were used, there would still be variation) but this was a good attempt to reduce it.

The choice of dependent measure for psychiatric diagnosis (the MILP) was defended on the ground that it compares well to the more typical Structured Clinical Interview for DSM-IV on reliability/validity (Clarke, Smith, Herrman and McKenzie, 1998).

The findings may actually represent a weaker response to SEGT than is accurate, due to the high variability in intervention session attendance (1-226 sessions, 17% attended 5 or less). Including only those with higher attendance might produce stronger findings; however, problems with this approach are discussed above.

There were negative aspects to this study, however. The authors note that 53% of eligible participants dropped out post-assessment; some were dissatisfied with the travelling distance, others expressed fear/concern about SEGT based on other participants' reports, suggesting that the preparatory procedure was inadequate. Yet others may be unwilling to take on unproven treatments in addition to existing adjuvant treatment.

Additionally, RCTs (though valuable) present problems; many participants relapse/drop out during the extensive recruitment/follow-up periods. It is likely that drop-outs will be those experiencing the worst symptoms or psychiatric illnesses, meaning results based on completers may be overly optimistic (Kissane et al., 2007). Intention-to-treat analysis attempts to rectify this; there was missing data, but imputing it did not significantly change the study outcomes.

The authors also note that these findings cannot be generalised to non-Caucasian women, due to aforementioned racial differences. However, the findings are likely more externally valid for the Caucasian group than a study with mixed racial groups.

Again, a large proportion of participants (a third) had some form of depression. Concerns with this potential confound were described above.

Centring of regression should have also been used for this multi-site trial (Kraemer and Blasey (2004) (see above).

Information on independently-obtained psychological interventions was not included, which may be problematic (see above). Information on mood-altering medication (antidepressants) was included, which is helpful.

Finally, the number of participants deemed necessary by power analysis was again lower than might be expected, and possibly inaccurate (Stefanek et al., 2009).

Overall, this was a well-planned, convincing study, particularly as the participant-engaging, manualised control group was still outperformed by SEGT.

No improvement in psychosocial adjustment with group psychotherapy

Vos et al. (2007) compared experiential-existential group psychotherapy (EEGT) with a social support group (Yalom, 1980).

The 87 female participants were aged 18-70 years, were recruited via oncological follow-up meetings, and had received surgery for primary breast cancer four or more months before first assessment. Participants had no distant metastases or psychiatric illness.

Pre-randomisation, post-intervention and at 12-month follow-up, participants completed a demographic/anamnestic questionnaire, the Profile of Mood States, the Sexual Functioning and Body Image

subscales of the breast cancer EORTC QLQ-30 and the Social Interactions/Recreation subscales of the Sickness Impact Profile (Bergner, Bobbit, Carter and Gilson, 1981; de Groot (cited in Vos et al., 2007); Sprangers et al., 1996). There were no baseline differences in psychosocial/medical/demographic variables.

Participants were randomised to EEGT (33 participants) or social support (34 participants); both comprised 12 2.5-hour weekly sessions in groups of 6-10, with a follow-up session at one and two months post-intervention. Psychotherapy group leaders were trained therapists with several years' experience, and social support group leaders were social workers/oncology nurses. For both conditions, at least one leader was female.

The psychotherapy intervention (33 participants) followed experiential themes (awareness/freedom/acceptance/meaning in life), relaxation exercises and discussion of personal experience (Spiegel and Bloom, 1983; Yalom and Greaves, 1977). Common topics were support systems/self-image/coping. The follow-up sessions focused on maintaining changes made in therapy (Vos and Remie, cited in Vos et al. (2007)).

The social support group programme (34 participants) enabled sharing of experiences, problem-solving and reception of support/information. Session topics were participant-selected at the previous session, with similar common topics to EEGT. Follow-up sessions were based on participant-selected subjects.

Due to the high number of dependent variables, "Vitality" and "Distress" dimensions were produced from the POMS subscales using the high intercorrelation coefficient via Principal Component Analysis (Vos, Garssen, Visser, Duivenvoorden and de Haes, 2004).

There were no significant changes from baseline to 12-month follow-up in Distress, Vitality/Sexual Functioning/Social Interactions. However, Recreation (the effect of the cancer on spare-time activities) and Body Image improved significantly over (Kiebert et al., 1991; Moyer, 1997). This only weakly supports the hypothesis that psychosocial improvements would occur in both conditions by 12-month follow-up.

No effect of the intervention on psychosocial adjustment indicators was found according to Random Regression Modelling, with testing of the influence of time, condition, age, surgery type and disease stage on all outcome variables. There was therefore no significant effect of condition.

These results are contrary to the majority of past research. Vos et al. (2007) suggest several causes of this discrepancy.

Firstly, this study may have targeted participants too early (Samarel et al. (1997) encountered a similar problem, finding improvements but no significant differences between conditions). Psychosocial maladjustment occurs in 31% of women by 1-year post-diagnosis, so there may be a decline in psychosocial variables over this period, meaning that no improvement was recorded (Maguire, 1995). However, there may have been reduced rate of decline; without an assessment-only control group this is unknown (though this would be unethical) (Bottomley et al., 1996; Telch and Telch, 1986).

Secondly, participants had similar levels of psychosocial adjustment to the general population throughout the study (Vos et al., 2004). However, a third of breast cancer patients typically have mild to moderate psychosocial problems (Fallowfield, 1991; Greer, 1994; Irvine, Brown, Crooks, Roberts and Browne, 1991; Stiefel & Razavi, 1994; Zabora, Brintzenhofeszoc, Curbow, Hooker and Piantadosi, 2001). Typically, studies that gain positive effects from therapy used participants with emotional problems at baseline (Bottomley et al., 1996; Edelman et al., 1999; Spiegel et al., 1999; Youssef, 1984), while studies that find no effect often use participants with normal mental state (Antoni et al., 2001; Berglund, Bolund, Gustafsson and Sjoden, 1994; Classen et al., 2001; Spiegel et al., 1981).

Since this study did not incorporate participants with existing psychosocial problems, this may explain the lack of positive results.

Thirdly, participants had primary breast cancer with good prognosis, while many studies examine participants with metastatic breast cancer (Spiegel et al., 1981; Van der Pompe, 1997). As noted, the concerns/psychosocial profile of the two participant types are very different, and EEGT is particularly suited for addressing mortality-related concerns typical of metastatic cancer. Hence, EEGT may have been unsuitable.

There are also methodological concerns. This was a small study; with only 67 completers, statistical power is limited. Vos et al. (2007) note that only 27.5% of approached women participated, a common problem (Berglund et al., 1994; Edelman et al., 1999; Edgar, Rosberger, & Collet, 2001).

Further, the main intervention lasted only 12 weeks; claiming a lack of effects after such a short period seems unwarranted, since many studies run for years and, for example, Andersen et al. (2008) only found differential effects after 20 months. Additionally, Sheard and Maguire (1999) found that shorter interventions are less effective, and existentially-oriented interventions may require longer than other approaches to generate effects (Classen et al., 2008). However, interventions lasting only 6 weeks have been shown to assist cancer patients (even at 10-year follow-up), so perhaps the intervention genuinely had no effect (Fawzy, Canada and Fawzy, 2003; Fawzy et al., 1993; McCorkle et al., 2000)

Additionally, the 20 non-completers were significantly older than completers, so there may have been age-based differences in e.g. prognosis (as non-completers are typically more unwell (Kissane et al., 2007)). There is no information regarding proportion of non-completers in conditions, so this may have unintentionally biased findings (as a condition with more non-completers might have more positive findings).

Further, controls received occasional presentations (the frequency is not specified), reducing the time spent on emotional support and potentially reducing the efficacy of the control condition. Additionally, the intervention participants received two sessions on maintaining changes post-therapy which the controls did not. Had these discrepancies been equalised, the control condition might have conferred benefits.

Additionally, the support groups were extended from the typical 90 minutes to 2.5 hours to provide a more internally valid comparison for EEGT. Since the support groups discussed one subject throughout each session, while the intervention group engaged in multiple activities, engagement may have been lower in the support condition. This could confound results. Similarly, control participants may have been less engaged in the subject matter because it was chosen the previous week (rather than on the day, as intervention subjects were); controls might research the topic themselves over the preceding week, or may have lost interest.

The participants covered in both conditions was similar (e.g. support, coping and self-image). These were theoretically approached differently, but it is possible that the material covered (rather than the method of delivery) may confer psychosocial benefits – if so, the similar material may confound results and explain the lack of difference between conditions.

Participant age range was broad (18-70), possibly a confound as older cancer patients benefit more from psychosocial therapies; it may be that older participants improved but this was masked by lack of improvement in younger participants (Cunningham, Lockwood and Edmonds, 1993). Future studies might incorporate age into analysis to examine this possibility (or use participants more similar in age, if enough can be found).

The measures used may have assessed the wrong areas of psychosocial improvement. Vos et al. (2007) note their qualitative impression at 12-month follow-up that participants had gained social/emotional support and improved communication. These variables were not measured. Future studies should select appropriate measures to target this possible improvement.

A further concern is that details of randomisation are (unusually) not included. This may indicate poor procedures. Similarly, there is no race information or information on mood-altering medications/psychological interventions, which could present confounds (as discussed above).

Positive aspects of this study include good attempts to ensure manual adherence in the intervention and appropriate leadership in the control groups, with a day of training provided for each and backup support available. However, inter-therapist differences could confound results, since psychotherapy leaders were trained therapists with several years' EEGT experience, while control leaders were social workers/oncology nurses not accustomed to EEGT.

There were good attempts to ensure reasonably consistent intervention group size (discussed above).

The control group provided a good comparison, providing a supportive group and equal contact time to the intervention group (particularly since there was approximately equal attendance between conditions). This provides a more realistic assessment of therapeutic efficacy than comparison to assessment-only controls.

Additionally, a priori choice of endpoints was positive (again, previously discussed).

Further, one or two of every leading pair was female. This was an interesting choice which may have been improved participant engagement, but participant opinions on this were not requested. Given the difficulty of recruiting/retaining participants, it is important to gather feedback on the success of such modifications to the paradigm for future research.

Participants were also offered an active intervention, avoiding the disappointment and potential seeking of external support noted above (e.g. Spiegel et al., 2007).

The measures chosen, despite not covering some areas of psychosocial adjustment, were reliable/valid for the Dutch population they were used for (de Groot, cited in Vos et al. (2007); Sprangers et al., 1996; Bergner et al., 1981).

Although there was a high number of dependent variables risking type I errors, some attempt was made to counter this by collapsing the POMS subscales into two factors. The use of RRM instead of the more typical ANCOVA/classical regression analysis compensated for missing/unequal data, and permitted inclusion of fixed/time-varying covariates (Gibbons et al., 1993).

In conclusion, no effect of the intervention on psychosocial adjustment was found. This was a well-designed study, but the poor recruitment choices, small sample, short intervention and dubious control condition reduce external validity.

Psychosocial: conclusion

Kissane et al. (2007) produced an excellent study with an extremely well-planned comparison group and good therapist training/manual adherence procedures. Since SEGT outperformed an active, engaging relaxation condition

offering a supportive group and training in relevant skills, this is a strong result. This is even more convincing because it is supported by the majority of existing studies. Vos et al.'s (2007) study, while reasonably well-planned, is flawed in many ways (as recognised by the authors) and therefore is insufficient to dismiss Kissane et al. (2007) and previous research. It therefore seems reasonable to claim that psychosocial group therapy can produce psychosocial improvements in late-stage breast cancer patients. Interestingly, Vos et al.'s (2007) suggestion that this may only apply to late-stage cancer is supported, as they did not gain positive findings with their primary cancer participants.

The recommendation by some researchers that only highly distressed patients should be offered SEGT therefore seems unfounded (Grassi et al., 2010; Newell et al., 2002).

Distress

There is some evidence that more highly distressed women may respond better to group therapy than less distressed women. As noted above, studies using women with high distress at baseline typically produce positive findings, while those with lower distress do not. For instance, Goodwin et al.'s (2001) study of SEGT in metastatic breast cancer found that the intervention only conferred benefits on women with high distress at baseline. Similarly, Vachon, Lyall, Rogers, Cochrane and Freeman (1982) found that in women receiving radiotherapy, those with highest distress gained most benefit from a supportive intervention, and Andersen et al. (2008) also found that those with highest cancer-related stress made greatest gains. SEGT improves coping mechanisms and adjustment at baseline; it may be that those with highest distress have most capacity for improvement (Classen et al., 2001; Maunder and Esplen, 2001; Spiegel et al., 1999; Youssef, 1984).

Since 22-43% of cancer patients have a psychiatric disorder by six months post-diagnosis (and 20-25% have affective disorders by one year), it is key to differentiate between the effects of supportive group psychotherapy in those with and without disorders/high distress (Ford, Lewis and Fallowfield, 1995; Gallagher, Parle and Cairns, 2002; Parle et al., 1996).

Although some researchers have argued that only patients with affective disorders should be referred for psychosocial group therapies, this seems unlikely given this review's conclusion on survival effects in the general population (Grassi et al., 2010; Newell et al., 2002). However, there may be differential responses in low- and high-distress participants.

Benefits with higher distress

Grassi et al. (2010) conducted the first study to examine the effects of SEGT in breast cancer patients with affective disorders.

205 participants with non-metastatic cancer diagnosed within the past year and scoring 80+ on the Karnofsky Performance Status scale (Karnofsky and Burchenal, 1949). Assessments at baseline and 6 months later were the Composite International Diagnostic Interview, the Brief Symptom Inventory, the Mini-MAC, the Multidimensional Scale of Perceived Social Support, the Openness Scale and a Cancer Worries Inventory (Derogatis and Spencer,

1982; D'Errico, Galassi, Schanberg and Ware, 1999; Mesters et al., 1977; Watson et al., 1994; World Health Organisation, 1993; Zimet, Dahlem, Zimet and Farley, 1988).

73 participants received an affective disorder diagnosis (adjustment disorder, major/minor depression or anxiety-depressive state). Those with non-affective psychiatric diagnoses were excluded.

The semi-structured SEGT intervention consisted of 16-24 90-minute sessions once-weekly for up to 6 months, in groups of 6-8. Groups were led by a psychiatrist and a psychologist with experience conducting group therapy. One psychiatrist experienced in SEGT trained the other conductors according to the Italian manual (Classen et al., 1997; Spiegel and Classen, 2000). The therapist helps participants to build relationships, express emotion, confront mortality-related fears and improve coping (Kissane et al., 2004). Session content was developed from unexplored themes in past sessions (Grassi et al., unpublished).

127 participants did not receive a psychiatric diagnosis at baseline, so received assessments only.

Results showed a significant difference between the intervention and control conditions on age (the former group were younger). As anticipated, intervention participants scored higher on the BSI-GSI, Mini-MAC anxious preoccupation/hopelessness factors and CWI, and lower on Mini-MAC fighting-spirit factor, MSPSS and Openness Scale.

6 intervention participants and 22 controls dropped out before 6-month follow-up. Non-completers did not differ significantly from completers.

The intervention group improved significantly by 6-month follow-up on interpersonal sensitivity/depression/anxiety/hostility/paranoia/psychoticism/stress/hopelessness/openness/ preoccupation/intensity of concerns. SEGT participants demonstrated greater improvement than controls on several psychosocial variables including anxiety/hostility/depression/anxious preoccupation/hopelessness. SEGT is therefore argued to be effective for breast cancer patients with affective disorders.

Almost 10% of controls developed psychopathological symptoms by 6-month follow-up. This is unsurprising, since 20-25% of breast cancer patients develop affective disorders by a year post-diagnosis (Parle, Jones and Macguire, 1996).

There are positive elements to the methodology. Assessments were conducted by an independent, condition-blind researcher, eliminating potential bias in e.g. interpreting responses.

The authors note that therapist experience was not accounted for, although greater experience typically produces better results (see above). However, Grassi et al. (2010) all psychiatrists/psychologists were trained by the same SEGT-trained psychiatrist to encourage manual adherence and reduce inter-therapist differences.

There was also a priori identification of endpoints, which is positive (as discussed).

Additionally, all assessment measures were presented in their appropriate validated forms for Italian populations (Grassi et al., 2005; Grassi, Rasconi, Pedriali, Corridoni and Bevilacqua, 2000; Grassi, Travado, Moncayo, Sabato and Rossi, 2004).

Further, intervention group sizes were consistent (this is important, as discussed above).

There are negative aspects, however. The authors note the lack of randomisation, meaning the intervention cannot be assumed responsible for improvements. For instance, 13 intervention participants were receiving treatment for depression/disturbances, which may have contributed. This major objection may explain this study being published in a relatively obscure journal.

Secondly, the authors note the small sample size (relative to other multi-site trials) and low statistical power (see above), attributed to difficulties in recruiting participants with affective disorders.

Thirdly, the authors note that the majority of intervention participants had adjustment disorders, though the group represented a wide range of affective disorders. Any differential effects of SEGT on different disorders were not distinguished (e.g. only the adjustment disorders may have responded to SEGT); future research may investigate this.

As discussed, use of participants with non-metastatic cancer reduces comparability with other research.

Statistical tests used were appropriate, but insufficient. There was no analysis of the effect of attendance (which varied from 16-24 and no condition means/medians are given), so internal validity suffers. The authors argue that Classen et al. (2001) found no correlation between attendance and psychosocial improvements, but one study does not excuse missing this key information/analysis.

Grassi et al.'s (2010) final criticism was that the ICD-10 system may be inadequate for recognising psychosocial disorders (Galeazzi, Ferrari, Mackinnon and Rigatelli, 2004). However, the DSM-IV system has similar problems so how this could be amended is questionable (Grassi, Sabato, Rossi, Biancosino and Marmai, 2005).

Additional concerns include the risk of type I errors due to the very high number of assessment measure subscales (Stefanek et al., 2009). Centring of regression was not used to avoid erroneous interpretations of this multi-site data (Kraemer and Blasey, 2004).

Again, controls did not receive an active intervention, risking disappointment or seeking of external support (as discussed). This may explain why substantially more control participants dropped out than intervention participants. The control group may therefore have demonstrated more positive findings than might be gained if non-completers were included.

Further, participants were recruited within a year of diagnosis, meaning participants were at a different stage of the disease process to a great deal of research (as discussed above).

In addition, several pieces of information are (unusually) not included in the study. For example, the study is apparently a multicentre trial; however, this is implied from the discussion rather than stated clearly, and participant numbers from each location are not recorded. However, Spiegel et al. (2007) demonstrated that individual sites in multicentre trials can produce very different outcomes; information by site would therefore have been useful. Similarly, racial sample proportions are not reported; as noted above, those from different races may respond differently to treatment, so this may have been important. Session attendance information was also omitted, as was information on mood-altering medication and psychological interventions (see above).

The authors' method of investigating whether SEGT is more effective in those with affective disorders is questionable, since those with affective disorders receiving SEGT are compared with those without affective disorders receiving no SEGT; this introduces two major variables. A better design might have been to deliver SEGT to both conditions and to compare the improvement seen in both, hypothesising that participants with affective disorders would demonstrate greater psychosocial improvement. This would also eliminate the concern that it is quite unusual for any therapy to be equal to no therapy at all, hence why new therapies are typically compared to the best available rather than to assessment-only control (as the latter proves little).

As noted in discussion of Andersen et al. (2008), the failure to provide any support to control participants is ethically dubious – particularly given that almost 10% of controls developed an affective disorder during the study. Again, following the suggested design above would remedy this concern as well as providing a more valid structure.

Grassi et al. (2010) claim their study proves that SEGT is a useful intervention in breast cancer patients with affective disorders. However, the many flaws preclude the results from being valid/generalisable. This is a very weak paper and may only have been published due to positive publication bias.

No benefits with higher distress

Classen et al. (2008) conducted a multicentre RCT of the benefits of SEGT for mood disturbance in primary breast cancer, hypothesising that women with more distress at baseline would gain greater benefits.

326 female participants were recruited in two waves from 8 academic sites and community oncology programmes. All had stage I-IIIA breast cancer diagnosed in the past year and had received surgical treatment. Exclusion criteria included distant metastases, cancer recurrence pre-randomisation, other cancer diagnosis during the past 10 years, life expectancy of under 10 years due to major medical problems, history of major psychiatric illness and attendance at a cancer support group for 2+ months.

There was poor uptake from eligible participants (25% at rural sites, 35-45% at urban sites), due to travel difficulties/disinterest.

Assessments were pre-randomisation, at 3 months (immediately post-intervention), and at 6, 12, 18 and 24 months follow-up. The primary outcome measure was the POMS, with mood disturbance the main outcome (McNair, Lorr and Droppleman, cited in Classen et al., 2008). Secondary measures were the Mini-MAC, Impact of Event Scale, Hospital Anxiety and Depression Scale, Courtauld Emotional Control Scale, Stanford Self-Efficacy Scale for Serious Illness, Cares Medical interaction Subscale, Family Relations Index, a sleep measure, a pain measure and the Yale Social Support Index (Giese-Davis et al., cited in Classen et al. (2008); Holahan and Moos, 1983; Horowitz et al., 1979; Schag and Heinrich, 1990; Spiegel and Bloom, 1983; "Stanford Sleep Disorders", cited in Classen et al., 2008; Watson and Greer, 1983; Watson et al., 1994; Zigmond and Snaith, 1983).

Participants were randomly assigned to the support or education control condition using biased coin and adaptive randomisation, weighting probability of randomisation to equalise medical/demographic variables at baseline (Hannigan and Brown, cited in Classen et al., 2008).

For the education condition (167 participants), educational materials (videotapes and pamphlets, avoiding SEGT themes) were posted to participants.

The 159 intervention participants received the education component and brief SEGT model which retained the main themes of SEGT (described above) (Classen et al., cited in Classen et al. (2008); Spiegel et al., 1981; Spiegel and Classen, 2000; Spiegel and Yalom, 1978). Groups of up to 10 met for 90 minutes weekly for 12 weeks, under two co-therapists (social workers/nurses/psychologists). Average attendance was 8 sessions.

Therapist training comprised a 2-day training workshop, familiarising therapists with the manual and offering practice with a pilot group.

Modified intention-to-treat analysis was used, including only participants who attended at least one follow-up assessment (in order to produce regression slopes). True intention-to-treat analysis produced the same outcomes.

Distress level was divided into high or low by POMS score (the cut-off between categories was 37, determined a priori as the mean cancer patient score 2 months post-diagnosis) and researchers were blind to score changes over time (Cella et al., 1989). This produced empty high-distress cells in the 2x2x19 ANOVA in 3 cohorts, so two models were test (one imputing missing data, the other eliminating these cohorts).

There were no differences between treatment and control groups on demographic/medical variables except for disease stage (significantly more participants had stage 3 disease in the control condition).

No effect on distress for brief SEGT was found with either model after removal of a single extreme control-condition outlier which produced an apparent significant difference (a participant who left the study due to high distress). High distress participants did not benefit more than low distress and no effect was found on secondary outcomes using either model.

The authors suggest several explanations for the lack of effect of SEGT. Firstly, that this existential intervention may be inappropriate for primary cancer patients, whose concerns are more practical (Vos et al., 2007).

Secondly, the intervention may have been too short to effect changes (discussed above).

Third, only a third of participants met the high-distress criteria, so there may have been insufficient capacity for SEGT-conferred improvement. However, no baseline distress-by-condition interaction was found; even highly distressed participants did not gain benefits from SEGT.

Fourthly, the intervention may have been inadequately facilitated; therapist capability was not assessed, and 42% of therapists attended no training (Classen et al., 1997). Although therapist training was unrelated to effect size, many studies (discussed above) identify an effect so inadequate training could reduce SEGT efficacy. Group supervision by SEGT experts to support manual adherence may have mitigated this problem. The authors recommend filming/analysis of therapist performance in replications to eliminate this possible confound.

Fifthly, the sample size may have been inadequate to detect any benefit of SEGT (although the relatively large sample improved on much similar research).

Finally, participants were randomised by prognostic medical variables (typical of such research) rather than stratifying by baseline distress level. This would have been more appropriate, since the best predictor of distress changes over time is initial distress, and would have avoided statistically problematic empty cells, produced more high-distress participants and increased statistical power.

The relatively passive education component may have disappointed controls and increased independent support-seeking (as discussed). Additionally, as noted, the lack of a supportive group and unequal condition duration compared to the SEGT group renders this a less effective comparison condition. However, this strengthens the conclusion that brief SEGT was ineffective, since it had no effect in comparison to a control group offering few benefits.

Additionally, centring of regression to avoid erroneous interpretations of multi-site data should have been used (Kraemer and Blasey, 2004).

Key information was omitted, such as session attendance (only the mean is given, without measures of spread), group size consistency and prevalence of mood-altering medication and psychological interventions. These factors may have influenced outcomes so this information would be valuable (for reasons discussed above).

There were positive points to the methodology; firstly, ensuring that researchers were blind to POMS scores throughout the study, to eliminate researcher bias.

Secondly, assignation to high/low distress was well-planned. The POMS score cut-off point was determined a priori using the mean POMS (37) for pre-surgical cancer patients 2 months post-diagnosis (Cella et al., 1989; Stein, 1984). Mean baseline POMS was lower (25), perhaps because participants were 8.7 months post-diagnosis and surgery had removed the disease, reducing distress. The authors argue that the failure to find significant differences between high-distress and low-distress treatment response may be due to mean sample distress being too low, and recommend replicating the study with more distressed participants (as Grassi et al. (2010) did) or those diagnosed more recently.

Use of the POMS primary outcome measure is well-justified; it is change-sensitive and widely used, increasing comparability with other papers (Cella et al., 1989; Classen et al., 2001; Spiegel et al., 1981). Support is also offered for the secondary outcome measures, though the many dependent variables risked type I errors. However, a power analysis and 1% test was used, and substantially more than the 251 necessary participants were acquired to remove this risk.

The use of a modified intention-to-treat analysis risked obtaining inaccurately optimistic findings (as noted above), but verification of findings using a true intention-to-treat analysis removed this concern.

A priori identification of endpoints and assessment times was also positive, improving validity in comparison to Andersen et al. (2008) and avoiding accusations of ending the study when findings offered most support for their hypothesis (Stefanek et al., 2009).

Removing the extreme outlier mentioned above was positive, as it misleadingly carried treatment effects for the entire sample.

Additionally, a valuable exclusion criterion rejected participants who had attended a cancer support group for 2+ months. Such groups might have ongoing effects that could mitigate the effects of SEGT (e.g. degenerating psychosocial wellbeing after leaving an established/familiar group), so this was a good decision that should be implemented in other studies.

This study also avoided the potential race-related confound noted above, as there were no significant between-group differences.

This was a reasonably strong study with a large sample, though the weak therapist training/assessment, overly short intervention and poor choice of stratification reduce validity.

Distress: conclusion

Classen et al.'s (2008) study is more convincing than Grassi et al. (2010); the latter is extremely flawed and may be best considered exploratory research inadequate for healthcare recommendations. However, past research supports Grassi et al. (2010) and the flaws in Classen et al. (2008) reduce the study's external validity and capacity to supersede past research. It remains plausible that a longer intervention conducted by appropriately-trained therapists may confer psychosocial improvements to those with high distress, perhaps (as noted) because they have poorest coping mechanisms at baseline and gain most from SEGT-taught coping strategies.

Additional support for the high number of cancer patients with affective disorders is needed, particularly given the poorer prognosis with depression (Watson et al., 1999). However, both studies' recommendations to offer SEGT to highly distressed patients are premature.

Both studies recommended research with participants with higher initial distress levels or more severe psychiatric disorders. Additionally, determining whether there are differential effects by type of affective disorder (or other reason for high distress) would be wise before blanket-recommending additional therapy to patients undergoing difficult treatments.

Conclusion

Overall, as described, it is possible to draw tentative conclusions. While benefits to survival seem unsupported, psychosocial group therapy appears to produce psychosocial benefits. Whether these are more effective in those with high baseline distress is uncertain, given the lack of research available.

However, these conclusions are of limited validity due to flaws in the literature.

The lack of methodological consistency concerning participant criteria is notable. Despite recognised differences in psychosocial profile, disease course and treatment response, studies select participant disease stages without justification, routinely fail to differentiate between low- and high-distress participants, and disregard the effects of age, race and time since diagnosis (Vos et al., 2007). Many factors seem to be determined by participants available rather than by choice, e.g. Andersen et al.'s (2008) incorporation of participants ranging from 20-85 years old. Such heterogeneity may be partially responsible for the poor agreement in findings. Future research must investigate subpopulation effects rather than seeking an overall effect in this disorganised manner.

Similar concerns regard interventions utilised. Duration varies from weeks to years, therapist training varies widely and therapies are applied inappropriately (e.g. SEGT in primary cancer). Content also varies; studies enact individual changes to manualised treatments without justification, affecting comparability (e.g. Kissane et al.'s (2007) omission of SEGT hypnosis training), while different methods used in separate studies may be very similar (e.g. Vos et al.'s (2007) experiential-existential group therapy is very similar to SEGT). Support groups and psychosocial therapeutic groups are often conflated (Vos et al., 2007). Comparing such disparate methods baselessly assumes a class effect of psychosocial interventions (Kaufmann, 2009).

Control conditions also vary from assessment-only (Grassi et al., 2010) to manualised programmes (Kissane et al., 2007). Drawing conclusions using so many approaches is difficult.

The field is riddled with methodological errors (detailed above). Given the importance of findings in this research area to a vulnerable population and their healthcare professionals/policy-makers, this is unhelpful/harmful.

Key information is often omitted; for example, many studies do not obtain information on mood-altering medication and psychological interventions undergone, even though these will clearly influence psychosocial outcomes.

Further, high non-completion and low uptake from eligible cohorts must be addressed, as low sample sizes are prevalent. These are often linked to poor information or planning (e.g. travel difficulties) (Kissane et al., 2007; Vos et al., 2007). An attempt to work with local oncological practices encountered administrative problems (Classen et al.,

2008). Improved preparatory/informative procedures may encourage participation, so this may be a fruitful amendment for future research (Kissane et al., 2007).

Dependent measures improvements are required; Spiegel et al. (2007) and Vos et al. (2007) note qualitative impressions of emotional benefits from SEGT, but these were undetected by measures. Including a measure of subjective wellbeing might determine whether an effect of psychosocial group therapy is going undetected.

Additionally, the theoretical support for most papers is weak. While explorations of survival differences assign medical hypotheses to their findings (e.g. OR status/stress models) (Andersen et al., 2008; Spiegel et al., 2007), theoretical models in psychosocially-oriented papers are routinely absent and justification for research is empirically based. A better understanding of the impact of psychosocial therapies on cancer patients may enable intervention modifications for greater efficacy.

Overall, there is clearly still much to be done to determine the value of psychosocial group therapies for breast cancer patients. However, the promising psychosocial improvements and potential to aid those who are most distressed are good reason to continue this research.

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