

Effects of Psychosocial Interventions With Adult Cancer Patients: A Meta-Analysis of Randomized Experiments

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Meta-analytic methods were used to synthesize the results of published randomized, controlled-outcome studies of psychosocial interventions with adult cancer patients. Forty-five studies reporting 62 treatment-control comparisons were identified. Samples were predominantly White, female, and from the United States. Beneficial effect size *ds* were .24 for emotional adjustment measures, .19 for functional adjustment measures, .26 for measures of treatment- and disease-related symptoms, and .28 for compound and global measures. The effect size of .17 found for medical measures was not statistically significant for the few reporting studies. Effect sizes for treatment-control comparisons did not significantly differ among several categories of treatment: behavioral interventions, nonbehavioral counseling and therapy, informational and educational methods, organized social support provided by other patients, and other nonhospice interventions.

Key words: neoplasms, meta-analysis, psychotherapy, counseling, patient education

Though the field of psychosocial oncology is relatively young, intervention studies and indeed even narrative reviews of those studies are no longer rare. Meta-analytic investigations, however, are conspicuously absent from the literature. In the present article, the results of treatment-control studies of psychosocial interventions with adult cancer patients are assessed meta-analytically. The focus is on the effects of nonpharmacological interventions intended to improve the quality of life of adults who have already been diagnosed with

one of the neoplastic diseases. Outcomes of interest are measures of emotional adjustment, functional adjustment, treatment- or disease-related symptoms, medical status, or some combination of these categories.

Some previous narrative reviewers have addressed psychosocial interventions broadly, whereas others have focused on specific types of interventions; in both cases, past reviews have tended to conclude cautiously that controlled studies show the interventions to be at least promising and probably beneficial to cancer patients (e.g., Andersen, 1992; Trijsburg, van Knippenberg, & Rijma, 1992; Watson & Marvell, 1992). A mixture of significant and nonsignificant results in controlled studies has led traditional reviewers to be cautious in their conclusions. As many meta-analysts (e.g., Hedges & Olkin, 1985; Hunter & Schmidt, 1990) have noted, such a pattern of significant and nonsignificant outcomes will arise if the underlying effect size is positive but moderate and study samples are small. By combining results across studies, a meta-analysis can more powerfully address the question: Is there an overall benefit to psychosocial interventions with cancer patients, and if so, how large is it?

In addition to the question of overall effectiveness of psychosocial interventions, we examine a set of more specific questions. First, because many researchers have suggested that certain types of interventions are preferable to others, we assess whether different classes of interventions are equally effective. Second, we investigate whether treatment effectiveness varies as a function of the severity of disease and treatment, factors that Andersen (1992) focused on in a recent review (also see Ahles, Cohen, & Blanchard, 1984; Watson, 1986). Third, we consider whether effect size differs for studies that screen potential participants and include only those with clear difficulties relative to studies without such screening. Several researchers have suggested that scarce services need

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only be offered to (e.g., Greer, 1987; Worden & Weisman, 1980), or are more effective for (e.g., Watson, 1983, 1986; Watson & Marvell, 1992), those at risk or in need (though Greer also noted that studies have shown that medical staff underestimate need). Finally, we attempt to determine if effect size depends on whether the intervention is focused specifically on coping with chemotherapy, pain, or radiation treatment.

Method

Search Strategy

Following common practice in many behavioral medicine meta-analyses (e.g., Fredrikson & Matthews, 1990), only published randomized experiments were included in the analyses reported here. Appropriate studies were defined as published randomized trials in which (a) a group of adult cancer patients receiving a psychosocial, behavioral, or psychoeducational intervention was compared with another group of cancer patients receiving either no psychosocial intervention or an extremely minimal sham procedure, and (b) the outcome variables included the patients' behavioral, emotional, physiological, or medical state. Hospice and terminal home care studies were excluded because there were few randomized studies and because preliminary work had indicated that they were quite distinct from other psychosocial interventions (Meyer, 1991). Studies were located from *Psychological Abstracts*, *Medline*, the reference sections of located studies and review articles, by writing to researchers in the field and through informal inquiries (see Meyer, 1991, for details).

Results of the Search

Forty-five studies, reporting 62 treatment-control comparisons¹ relevant to the meta-analysis, were retrieved.² Five studies were from Great Britain, 2 from Canada, 1 each from Colombia and Egypt, and the remaining 36 from the United States. The primary source articles are listed in the Reference section. Supplementary information was sometimes obtained directly from the authors, from a dissertation report on which the study was based, or from an additional published report.

Table 1 presents information about patient age, diagnosis, and sex. For those studies in which the mean age was reported, samples clustered around the 50s. Fourteen of the studies were limited to a single type of cancer, including four with only breast cancer patients. There was a tendency for studies to have a preponderance of women such that in 55% of the studies reporting gender more than 60% of the patients were female, whereas in only 12.5% of the studies 60% of the patients were male.

Only eight of the studies from the United States reported quantitative information about race or ethnicity. Three of those reported samples in which at least 10% of participants were African American; no North American studies reported as many Latino/Hispanic, Asian American, or Native American Indian patients.

Variables

Dependent measures. Five higher order categories of dependent measures were developed. Emotional adjustment involved measures of such constructs as mood state, fear and anxiety, depression, denial or repression, self-esteem, locus of control, satisfaction with medical care, other attitudes, personality traits, and any other type of emotional adjustment or distress. Functional adjustment consisted of indicators of behavioral functioning in normal life settings (e.g., socializing and going back to work). Illustrative measures included the Karnofsky Performance Status, self-report measures of social behaviors, and other life indicators of adjustment and functioning. The

Table 1
Age, Type of Cancer, and Sex

Group	No. studies	% of studies reported
Mean age of sample		
49 or below	10	28.6
50-59	18	51.4
60 and over	7	20.0
Not reported	10	—
Single type or location of cancer		
Yes	14	31.1
Breast	4	8.9
Hodgkins	2	4.4
Lung	2	4.4
Skin	2	4.4
Bladder	1	2.2
Female reproductive	1	2.2
Male reproductive	1	2.2
Hematological	1	2.2
No	31	69.9
Proportion (in percentages) of study sample that is female		
0	2	5.0
1-20	0	0.0
21-40	3	7.5
41-59	13	32.5
60-79	14	35.0
80-99	2	5.0
100	6	15.0
Not reported	5	—

category treatment- or disease-related symptoms related to chemotherapy-related nausea and vomiting, pain, coughing, nutritional measures, including body weight, and similar symptoms related to cancer and its treatment. The medical measures category included such indicators as leukocyte activity, tumor response to chemotherapy, and physician rating of disease progression. The category compound or global measures included measures combining clear, core aspects of more than one of the preceding categories. An example would be the Cancer Inventory of Problem Situations (Schag, Heinrich, & Ganz, 1983), which explicitly addresses emotional adjustment, functional adjustment, and treatment- and disease-related symptoms. This category of measures also included global measures for which overall ratings implicitly cut across the preceding categories (despite potential interpretive problems, this compound category was included for completeness).

Treatment style categories. The five categories of interventions were defined as follows. Cognitive-behavioral interventions include cognitive, cognitive-behavioral, and behavioral methods focused on changing specific thoughts or behaviors or on learning specific coping skills.

¹ The results for the two biofeedback treatment groups in Shartner, Burish, and Carey (1985) have been combined and analyzed as a single treatment-control comparison because of the small numbers involved (8 intervention patients and 4 controls). Because this review does not focus on different types of control groups, results for multiple control groups were combined for the two studies with two control groups.

² Two psychotherapy studies reported by Grossarth-Maticek (Grossarth-Maticek & Eysenck, 1989; Grossarth-Maticek, Schmidt, Vetter, & Arndt, 1984) are excluded from all results in this article. The integrity of some of Grossarth-Maticek's other data has been challenged by researchers (van der Ploeg, 1991) including one of his coauthors (Vetter, 1991).

Procedures coded here included progressive muscle relaxation training, meditation, hypnotherapy, systematic desensitization, biofeedback, and behavior modification or reinforcement. If a treatment had a substantial behavioral component, it was placed in this cognitive-behavioral category even if it also had considerable emphasis on information and education (cf. Mazzuca, 1982). This category is similar to the behavioral supercategory developed by Smith and Glass (1977) in their pioneering meta-analysis of the psychotherapy outcome studies except that cognitive interventions have been transferred into it, to some extent dealing with Presby's (1978) objections to Smith and Glass's category.

Informational and educational treatments included interventions primarily providing sensory, procedural, or medical information; coping information, if provided, did not include active rehearsal of new behaviors. An example is the booklet Goodwin (1979) provided to patients before lung surgery that described normal breathing, symptoms to expect after the surgery, and self-care measures to promote optimum recovery and cope with symptoms.

Nonbehavioral counseling or psychotherapy interventions referred to noncognitive and nonbehavioral verbal psychotherapy and counseling, including psychodynamic, existential, supportive or general counseling, and crisis intervention. This category is similar to Smith and Glass's (1977) verbal psychotherapy supercategory except that it does not include cognitive interventions. This category included social support by professionals.

Social support (by nonprofessionals) referred to cases in which fellow patients or family members provide the intervention by being supportive (e.g., in a support group run by fellow patients rather than professionals). Structured social support interventions by nonprofessionals (other patients or family members) are important in the cancer literature (Lieberman, 1988) and made a natural category.

Another category included unusual treatments (e.g., music therapy) and cases in which clearly dissimilar or incongruous approaches were combined, especially when the different aspects of the intervention were given by different practitioners. For instance, this label was given to Spiegel and Bloom's (1983) well-known study because it combined hypnosis, classified here as a behavioral method, with a psychodynamic process group. The category 'other' was not used for the many interventions with some aspects of education and behavioral coping skills. In such cases, we determined whether the content of the intervention was nearly exclusively educational or whether there was a substantial behavioral component, and the intervention was assigned accordingly.

Moderator variables. Three potential moderator variables were examined: Andersen's (1992) categories of risk for psychological distress, whether the intervention was focused on a particular symptom or treatment side effect, and whether patients were screened. Following Andersen, we categorized studies as having patients low, medium, or high in risk for psychological distress by weighing three dimensions. Low risk was defined as corresponding with local disease, low intensity of treatment, and favorable prognosis. High risk was defined as corresponding with disseminated disease, high intensity of treatment, and bleak prognosis. For the intervention-focus variable, studies were classified as being focused on treating chemotherapy-related symptoms, radiation-related symptoms, and pain or as not being so focused. For the patient-screening variable, studies with screening of patients were defined as those in which only patients with clear signs of distress (emotional distress, particular chemotherapy side effects, or pain) were treated, as contrasted with studies that included cancer patients regardless of distress level.

Method of Coding and Analysis

Coding strategy. We attempted to calculate the effect size for every measure in an article. Following Matt (1989), if a measure was discussed in the Method section but no detailed results were reported,

the effect size was assumed to be zero. However, if most effect sizes reported only as nonsignificant were in fact nonzero in the same direction as the average effect size, then treating these unreported effect sizes as zero provides a conservative estimate. To find an upper bound estimate so as to bracket effect sizes, calculations were also conducted with such results (i.e., those reported only as nonsignificant or not explicitly reported) completely omitted from the analysis.

To avoid overrepresenting studies with multiple measures, when a treatment-control comparison provided more than one effect size for a dependent measure category (e.g., several different emotional adjustment measures) the results were averaged (weighted by sample sizes if the different measures or sampling occasions had different N s).

Multiple treatment-control comparisons. So that overall significance levels would not be based on subjects counted twice or even three times, the number of subjects in the control group was divided by the number of treatment groups compared with it.

Reliability coding. All of the studies on which the present meta-analysis is based were coded by the first author. Reliability coding conducted in preliminary analyses (Meyer, 1991) indicated that it was reasonable to accept the primary coder's judgments for this work. For example in a broader set of studies, for frequently encountered classifications, kappas ranged from .81 to .88 (Meyer & Mark, 1994).

Data analysis. Hedges and Olkin's (1985) methods were used to conduct separate meta-analyses of effect sizes for each of the five types of dependent measures. A unit-free effect size g was obtained from outcome measure scores by obtaining the difference between the control group mean and the treatment group mean and dividing the result by the pooled standard deviation. For certain complex designs, Shadish and Montgomery's (1986) methods were used to determine g . The effect size g was then multiplied by a small sample size correction factor to obtain an unbiased value of d , the effect size used in the rest of the analysis (Hedges & Olkin, 1985).

Homogeneity tests were also conducted to test whether a set of effect sizes could be considered as a sample from a single underlying effect size. The different treatment categories were compared in terms of the magnitude of effect size associated with each. Similar analyses were conducted for the moderator variables. Johnson's (1989) software for meta-analyses was used.

Results

Homogeneity

For four of the five categories of dependent measure categories, the set of effect sizes was homogenous. The exception was for measures of emotional adjustment; when one outlier was removed, however, the remaining studies had homogenous effect sizes. The discrepant study (Ali & Khalil, 1989) was the only one from a country (Egypt) in which patients were not notified of their cancer diagnosis. Following the method of Hedges and Olkin (1985), that study has been deleted from all the results.

Average Effect Sizes

Overall average effect sizes are presented in Table 2. Significant beneficial effects ranging from .19 to .28 were found for the four dependent measure categories for which the most patients were studied. For the fifth category, medical measures, the slightly smaller effect size of .17 was not statistically significant.

For the analyses summarized in Table 2, effect sizes reported as nonsignificant are assumed to be zero. To establish

Table 2
Weighted Effect Sizes for Dependent Measure Categories

Measure	Studies	Comparisons	Total N	d	d 95% CI
Emotional adjustment	41	56	2,840	.24	.17/.32
Functional adjustment	16	21	940	.19	.06/.32
Treatment- and disease- related symptoms	28	39	1,606	.26	.16/.37
Medical	5	7	232	.17	-.10/.44
Compound and global	5	7	373	.28	.08/.49

Note. CI = confidence interval.

an upper bound, weighted average effect sizes were also calculated excluding these effect sizes. The resulting average effect sizes were .31 for emotional adjustment, .32 for functional adjustment, .41 for treatment- and disease-related symptoms, .39 for medical measures, and .28 (unchanged) for compound and global measures.

Treatment Style and Potential Moderator Variables

For all five outcome variables, there were no significant differences on post hoc comparisons among any of the treatment categories. Table 3 presents the effect sizes, by dependent measure category, for each intervention type.

There was no significant effect of Andersen's (1992) risk categories; for emotional adjustment, $QB(2) = 3.13, p = .21$; for functional adjustment, $QB(1) = 2.50, p = .11$, with high risk tending to be associated with larger effects than moderate risk; and for symptoms, $QB(2) = 0.14, p = .93$. There was no significant effect of study focus (e.g., pain-focused and radiation-focused; all $ps > .15$) or screening for distress on effect sizes (all $ps > .20$) for any of the dependent measure categories. As with the nonsignificant differences among treatment style categories, these results must be interpreted with caution. Type II errors are possible because the effect sizes and the number of treatment-control comparisons are relatively small.³

Discussion

We consider first the overall average effect size for the interventions. The results clearly indicate that psychosocial interventions have positive effects on emotional adjustment, functional adjustment, and treatment- and disease-related symptoms in adult cancer patients. Previous reviewers (e.g., Greer, 1987; Trijsburg et al., 1992) have been concerned that studies have not consistently shown statistically significant results. However, such a pattern of results could be expected because of the small sample size and consequent low statistical power of the average study in this area (Hedges & Olkin, 1985; Hunter, Schmidt, & Jackson, 1982). In the few studies (5 of 45) that included medical outcome measures, there was no statistically significant effect of psychosocial interventions on those variables. However, these studies represented far fewer patients than the other outcome categories (232, as contrasted with 2,840 for emotional adjustment, the most-reported category).

Interpretation of Effect Sizes

Are the statistically significant effect sizes clinically significant? Depending on one's perspective, it is possible to inter-

pret the effect sizes found as very important or as relatively small. First, consider the effect sizes in relation to other psychological interventions. The magnitude of the present effects is somewhat smaller than that found by Matt (1989) for psychotherapy outcome studies. Matt found a d of .35 when he replicated Smith and Glass's (1977) meta-analysis using methods similar to those used here (though Matt did not exclude unpublished and nonrandom studies). Thus, the present effects represent a range somewhat smaller in clinical significance than those of psychotherapy in general. Note, however, that Hunter and Schmidt (1990) considered effect sizes in the range of .20 to .40 as fairly typical of psychological interventions that work, and the effects observed here generally fall within that range.

It is nevertheless possible to view the observed effects simply as small. Cohen (1977) has called effect sizes of .2, .5, and .8 "small," "medium," and "large," respectively. From this perspective, the observed effect sizes tend to be fairly small.

On the other hand, Rosenthal (1984) has pointed out that in the medical field even tiny effects may be of crucial importance. An effect size d of .08, which raises success rates from 48% to 52%, can be of considerable importance to the individuals affected. Using the methods of Rosenthal and Rubin (1982), differential success rates of intervention versus control subjects were 56% versus 44% for measures of emotional adjustment, 55% versus 45% for functional adjustment, 57% versus 43% for treatment- and disease-related symptoms, 54% versus 46% for medical measures, and 57% versus 43% for compound and global measures.

Even if one views the effect sizes as relatively small, it is still possible to find reasons to be impressed that any positive effect has occurred. It is also possible that cancer patients will benefit in ways that are not reflected in the dependent measures used in outcome studies. For example, Cella et al. (1989) have documented that cancer patients score significantly lower on Total Mood Disturbance on the Profile of Mood States than the published normative groups. Thus, cancer patients may not be able to improve much from better than average. Phrased differently, it may be that cancer patients who are fairly well-adjusted before diagnosis show no improvement because of ceiling effects. Those who are not well-adjusted may require extensive intervention for obvious benefits (Watson, 1983); their illness is an ongoing objective stressor complicating the

³ The present article is adapted from a longer manuscript by Meyer and Mark (1994), available from Melvin M. Mark, which presents a meta-analysis including both nonrandomized and unpublished studies. No significant differences were observed between published and unpublished studies or between random and nonrandom studies. Nevertheless, in the larger set of studies, effect sizes were generally larger, with $d = .29$ for emotional adjustment, .26 for functional adjustment, .27 for treatment- and disease-related symptoms, .21 for medical measures, and .13 for compound and global measures. In addition, screening was a significant moderator, with studies that included only patients in distress having higher effect sizes on measures of emotional adjustment and treatment- and disease-related symptoms. The Meyer and Mark (1994) manuscript also includes a discussion of the file drawer problem and analyses of fail-safe N .

Table 3
Weighted Effect Size (ES), N, and 95% Confidence Interval (CI) for Treatments by Dependent Measure

Treatment	Emotional adjustment	Functional adjustment	Symptoms	Medical	Compound and global
Behavioral					
ES	.19, <i>n</i> = 1,323	.10, <i>n</i> = 194	.32, <i>n</i> = 727	.13, <i>n</i> = 184	.20, <i>n</i> = 203
CI	+0.08/+0.30	-0.20/+0.40	+0.16/+0.48	-0.17/+0.43	-0.07/+0.49
Informational and educational					
ES	.25, <i>n</i> = 988	.27, <i>n</i> = 465	.21, <i>n</i> = 416	.80, <i>n</i> = 26	.35, <i>n</i> = 118
CI	+0.12/+0.37	+0.08/+0.46	+0.01/+0.40	+0.00/+1.60	-0.02/+0.72
Nonbehavior counseling/therapy					
ES	.39, <i>n</i> = 422	.12, <i>n</i> = 263	.17, <i>n</i> = 339	-.20, <i>n</i> = 22	.45, <i>n</i> = 52
CI	+0.19/+0.58	-0.12/+0.37	-0.05/+0.39	-1.04/+0.64	-0.12/+1.02
Social support					
ES	-.23, <i>n</i> = 19	-.08, <i>n</i> = 18	—	—	—
CI	-1.14/+0.67	-1.02/+0.85			
Other					
ES	.33, <i>n</i> = 88	—	.45, <i>n</i> = 124	—	—
CI	-0.10/+0.76		+0.09/+0.82		

resolution of premorbid issues. Vachon (1988) has suggested another possibility: “[by intervening] we may risk having . . . patients, family members and survivors appear to be doing worse because they have learned to identify and express their emotions and emotional needs, rather than repressing them” (p. 48). In sum, we believe the positive effects are noteworthy.

Substantive Implications of Meta-Analytic Methodology

There were a number of methodological choices that may result in conservative effect size estimates. First, we assumed nonsignificant results were zero in the primary analyses. Second, we took a comprehensive approach to including dependent measures (following Matt, 1989). As we reported, excluding nonsignificant results, rather than assuming them to be zero, caused effect size estimates to increase from a range of .17–.28 to .28–.41. Similarly, Matt (1989) obtained effect size estimates for psychotherapy interventions half the size of those reported by Smith, Glass, and Miller (1980). A third possible source of conservative bias is that all interventions were included from multiple treatment studies even though some were studies in which components of complex treatments were examined both separately and in combination and in which not all interventions were expected to be equally effective. Fourth, multiple measures were weighted equally even though some researchers included measures on which they expected little or no change. Fifth, measures were not corrected for unreliability even though many studies used instruments with as few as one item (see Hunter & Schmidt, 1990, for a discussion). In addition, larger effect sizes are obtained if nonrandom and unpublished studies are included (see Footnote 3).

Impact of Treatment Styles and Moderator Variables

The failure to reject the null hypothesis of no differences between categories in all subset analyses raises two possibilities: There really may not be differences between the effect sizes of different categories, or there may have been insufficient statistical power to find true differences. Given that the

main effects were not large, the power to find interaction effects in this sample of studies was small. To move beyond our general finding that psychosocial interventions have a beneficial effect on most outcome measures, reporting of interaction effects needs to be improved and cumulated across studies.

Note that questions about the relative efficacy of treatment styles or the impact of the moderator variables can only be partially illuminated by study-level analyses such as those undertaken here. A relationship based on the average values for variables across studies can conceal a different relationship between the variables within studies (Light & Pillemer, 1984). For instance, individual studies that included a range of risk among the patients served might have found a consistent relationship between risk and treatment effect if this had been investigated. In addition, within-study comparisons avoid problems of confounding between studies, as Shapiro and Shapiro (1982) have noted. Future attention to potential moderators in primary research seems desirable.

Questions of External Validity

The studies included in the meta-analysis predominantly included White women from the United States.⁴ The search strategy uncovered only two studies from nonindustrialized countries. Some caution is indicated in assuming that the results apply equally to men, to ethnic minorities in the United States, and to other nations. Future studies focusing on these populations would be desirable to assess the extent to which the present results can be extended (cf. Burish, 1991, regarding the need for research on ethnic minorities).

Many researchers have noted that substantial numbers of patients refuse the opportunity to receive psychosocial interven-

⁴ Although there was no direct evidence to this effect, the underrepresentation of men might indicate some subtle biases about the appropriateness of psychosocial interventions for men versus women.

tions. The results reported here represent only those who agreed to participate in studies and who completed outcome measures. The beneficial results found should therefore be considered to apply only to those patients interested in participating in psychosocial interventions.

Future Research

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

On the other hand, several other directions for future research seem important and should replace simple tests of the efficacy of psychosocial interventions on emotional and functional adjustment and on treatment- and disease-related symptoms. First, more direct comparisons of different treatments should be made. We believe it would be premature to conclude that there is no difference between treatment categories on the basis of the present meta-analysis, given possible confounds.

Another productive direction involves focusing on medical outcomes and survival, especially in longitudinal studies that simultaneously measure emotional adjustment, treatment compliance, treatment- and disease-related symptoms, and social support. Few long-term controlled studies of survival time have been reported, ironically in some cases with results opposite the predictions of the authors: Morgenstern, Gellert, Walter, Ostfeld, and Siegel (1984, with additional follow-up by Gellert, Maxwell, & Siegel, 1993) found no evidence that their intervention increased survival time, whereas Spiegel, Bloom, Kraemer, and Gottheil (1989) unexpectedly did. One desirable strategy would be to integrate such research on psychosocial interventions into new or existing studies of medical interventions. The increased monitoring of patients' treatment in cancer medical treatment could promote psychosocial research (W. H. Redd, personal communication, August 31, 1994). In addition, as Greer suggested in 1987, increased attention to studying the mechanisms of beneficial outcomes would be useful. This approach has proved helpful in the behavioral treatment of chemotherapy side effects (Watson & Marvell, 1992) and merits more study. In other words, mediational processes should be studied.

Finally, there is a need to investigate ways of increasing the impact of interventions and of decreasing their cost (Rimer, Keintz, & Glassman, 1985). Some research on replacing expensive professionals with audiotapes in relaxation training has had disappointing results (Carey & Burish, 1988), but it seems worthwhile to consider other methods to increase cost-effectiveness, including alternative approaches to treatment delivery and implementation. In a related vein, improving the acceptability of psychosocial interventions for both medical personnel and patients, as well as ensuring easy accessibility, would be worthwhile. Additional research, de-

signed with attention to statistical power, might also fruitfully address whether psychosocial interventions are less effective for low-distress and for low-risk patients.

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