Psychological treatment of cardiac patients: a meta-analysis

Wolfgang Linden1*†, Melanie Jayne Phillips1, and Jocelyne Leclerc2

1Department of Psychology, University of British Columbia, 2136 West Mall, Vancouver, BC, Canada V6T 1Z4; and
2Department of Psychology, University of Regina, Saskatchewan, Canada

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Keywords
Cardiac rehabilitation; Stress management; Psychological therapy; Outcome; Mortality; Event recurrence; Depression; Distress

Psychological treatment (PT) often is a component of cardiac rehabilitation (CR). The term CR describes a broad class of interventions targeting risky behaviours, namely smoking, lack of exercise, poor eating habits, and often also targets psychological distress.1,2 CR programs that focus on psychological factors are largely similar in that they use cognitive–behavioral interventions to reduce distress and teach psycho-physiological self-regulation skills.3–5 While previous meta-analytic reviews6–11 document the effectiveness of multi-component CR programs for reducing mortality and secondary event rates, the benefit of added PTs is still in question. To resolve this question, we begin with a review of previous meta-analyses and then provide an updated meta-analysis of the outcome of PT. We also report gender-specific effects and evaluate the impact of varying program characteristics on outcome. The core features and results from meta-analyses published since the mid-1990s are described in Table 1. Table 1 reveals differences in review methodologies that range from obvious (i.e., publication year) to less obvious discrepancies (i.e., study selection and categorization procedures). Predictably, there is overlap in the respective reference lists; therefore these reviews cannot be considered independent of each other. With respect to classification decisions, Rees et al.9 and Clark et al.11 included interventions where the CR had been amalgamated with psychological and ‘other’ (i.e., health education) rehabilitation components thus making it impossible to isolate benefits solely attributable to psychological components. Observed rates of mortality and morbidity reductions for PT relative to usual care (UC) control varied considerably with mortality reductions for the treatment groups at follow-up being as high as 71%10 and as low as 3%11 at 1-year follow-up. In none of the reviews were the effects of different active PTs statistically different from each other. Importantly, Duseldorp et al.8 showed that PT reduced cardiac-specific mortality (31% decrease) when distress had been reduced, but that studies in which distress was unchanged also failed to show mortality or morbidity benefits (14% increase).

The observed mortality benefits reported in the previous reviews were not systemically different in long vs. short follow-ups. Two reviews8,11 reported greater mortality reductions with longer follow-up periods, whereas Linden...
et al.\textsuperscript{7} reported the opposite trend. The reasons for such inconsistent mortality effects over time are unclear. It is surprising that no meta-analysis has reported tests of gender differences in mortality outcomes although important differences in individual studies were reported as early as 1997.\textsuperscript{12} Given these previous conclusions, we sought to test the following hypotheses: (i) Does additional PT reduce mortality and morbidity, over short vs. longer follow-up, for both genders? (ii) Do program characteristics like timing of treatment and effectiveness of the distress reduction differentially affect outcomes?

### Methods

#### Study selection

Study selection steps are shown in Figure 1 and the studies and supplementary articles ultimately included\textsuperscript{12–66} are described in Table 2. The search utilized the protocol outlined by the Cochrane Database of Systematic Reviews, using PsycInfo, Web of Science, Ovid Medline, PubMed, and EMBASE databases. In addition to the computer searches for the years 2002–2006, all secondary references from the earlier meta-analyses were followed, and the first author personally contacted key researchers in the field to inquire about relevant unpublished research. We reviewed studies in English, German, Portuguese, Chinese, Russian, Dutch, Swedish, Spanish, and Danish. Where necessary, interpreters with knowledge of psychological research assisted data extraction. Only one of the four foreign language articles provided mortality data.\textsuperscript{44} In general, non-English publications provided fewer design and protocol details and the quality of the designs were therefore more difficult to assess. The search terms we used are listed in the appendix.

#### Inclusion criteria

For inclusion of a trial, it was necessary that one treatment condition involved a predominantly psychological or behavioural intervention. This psychological intervention had to exist against a backdrop of at least one UC control condition so that the additional benefit of psychological intervention could be evaluated. Uncritical

<table>
<thead>
<tr>
<th>Table 1</th>
<th>Summary of review features and reported effects of previous meta-analyses</th>
</tr>
</thead>
<tbody>
<tr>
<td>Studies included</td>
<td>Linden et al.\textsuperscript{7}</td>
</tr>
<tr>
<td>Treatment conditions</td>
<td>23 A clearly identifiable, distinct psychological treatment</td>
</tr>
<tr>
<td>Control conditions</td>
<td>UC (not further defined)</td>
</tr>
<tr>
<td>Follow-up (FU)</td>
<td>&lt;2 years;&gt;2 years</td>
</tr>
<tr>
<td>Mortality OR (treatment vs. control)</td>
<td>0.57 (FU &lt;2 years)</td>
</tr>
<tr>
<td></td>
<td>0.72 (FU &gt;2years)</td>
</tr>
<tr>
<td></td>
<td>0.66 (&gt;2 years) 1.14 (when distress was not reduced); 0.69 (when distress was reduced)</td>
</tr>
<tr>
<td>Gender-specific outcomes</td>
<td>No</td>
</tr>
<tr>
<td>Consideration of treatment length</td>
<td>No</td>
</tr>
<tr>
<td>Distress reduction as a moderator</td>
<td>No</td>
</tr>
<tr>
<td>Non-English articles</td>
<td>No</td>
</tr>
</tbody>
</table>
acceptance of the term 'usual care' was deemed inappropriate because it harboured considerable heterogeneity in treatment exposure. The identified trials were subjected to a four-category system that promised highest inter-rater agreement. M.J.P. and J.L. then conducted independent categorization of all studies and an inter-rater agreement of 89% was reached (83 agreements out of 93 total decisions). Disagreements were evenly distributed across different pairings of categorization decisions suggesting that no one decision type was particularly problematic; remaining disagreements were resolved in a conference of the authors. This process resulted in initial grouping of studies into two types of control conditions and two active treatment conditions, namely:

(1) Basic UC control. Defined as 'under medical care but not parti-
cipating in structured exercise and/or lifestyle counselling'.

(2) Multi-component UC control. Intervention including: nutrition
counselling, exercise, and/or instruction/education about the
disease and medications, typically offered by cardiac nurses, exer-
cise specialists, or case managers. The instruction component was
usually short (range 2-6 sessions) and not individually-tailored.

(3) Multi-component PT (active treatment). Given that the term stress
management usually refers to a multi-component psychological
intervention,67 all treatments labelled 'stress management'
were categorized as multi-component PT, as were all treatments
labelled psychological therapy, cognitive–behavioural, or beha-
viour therapy. To qualify for the multi-component PT category,
the intervention had to have at least two-thirds of the following
characteristics: (i) majority of treatment exposure was for psycho-
logical targets (distress, depression, etc.); (ii) therapists were
trained in mental health care at a graduate level; and (iii) treat-
ment targets and choices were individually-tailored.

(4) Biological/self-regulation (BSR) treatments (active treatment).
These included meditation, autogenic training, biofeedback,
breathing, yoga, and/or muscular relaxation. When self-
regulation training was part of multi-component stress manage-
ment regime, such a study was categorized as PT. We created
the BSR category as distinct from the multi-component category
because BSR has a more specific psycho-physiological rationale
for cardiac function than does 'stress management'.68 It is
easier to standardize for service delivery,10 and can be offered
at lower expense than multi-component PT.

We included studies without follow-up (i.e. studies that were
excluded in the Cochrane Review8) because, in addition to mortality
and disease recurrence, we wished to learn about gender and
process predictors of immediate outcomes. Given that a single pub-
lication may contain more than one active treatment, we only
included the one considered most intensive. Excluded were (i) inter-
ventions (or conditions within a study) that were not fully random-
ized; (ii) interventions that had combined psychological and
non-psychological components of CT and did not isolate the PT com-
ponent; (iii) randomized trials without psychosocial or mortality/
morbidity outcomes (e.g. those that only measured lipid changes);
and (iv) studies where the intervention arm had less than 20 parti-
cipants (to avoid unreliable findings).

Data extraction and analysis
Information regarding the following outcomes was extracted: all-cause
mortality, cardiac mortality, CHD progression and recurrence, CHD risk
factors (e.g. blood pressure, lipids), and/or psychological well-being
(e.g. depression, hostility/anger, anxiety, social support, quality of
life). Because all studies that provided mortality data provided all-
cause mortality data but only some differentiated 'all-cause' from
'cardiac' mortality, we chose 'all-cause mortality' for consistency.
Although technically possible, comparisons involving less than 200
patients were not conducted. The number 200 was chosen to mini-
mize random variation in findings and is based on the fact that 200
participants are needed to detect a significant effect of r>0.3 with
80% power. Data on mortality and morbidity (i.e. event recurrence)
were computed as odds-ratios (ORs), and continuous data were
initially computed as Cohen's effect sizes $d$, then converted to r.
The frequently used Comprehensive Meta Analysis Version 2 soft-
ware package69 (www.Meta-Analysis.com) was used for statistical
comparisons. The more conservative random effects model was
chosen for between-group comparisons because of (i) the known
heterogeneity of effects as reported by other reviewers (Table 1),
and (ii) the fact that the random effects model makes fewer
assumptions about shared population and treatment characteristics
and are therefore more conservative.70 The CMA program automati-
cally weighs ORs and effect sizes for sample size, tests for homogen-
eity of variance, and provides confidence intervals. In a random
effects model, very large sample studies have slightly less impact
on obtained ORs than is true in a fixed-effects model70 (p. 215).

For morbidity scores, the following endpoints were summed:
rehospitalization for cardiac emergency and new cardiac proce-
dures, new MI, newly diagnosed arrhythmias, or persistent
angina. The computed data reflect numbers of patients with a
recurring event and not absolute number of events because the
latter was often not reported. Results are displayed in tabular
format and forest plots are shown for the most critical analyses.
Mortality reductions of at least 20% were considered as clinically
meaningful even if they did not reach traditional cutoffs of $P<
0.05$; this decision was based on the results of Lau et al.71 who
have shown that beta-blockers and exercise rehabilitation each
brought about reductions of mortality of about 20%, and this rate
was of sufficient importance to drive current clinical practice. The
OR analyses addressed the following core questions:

(1) Does PT (aggregated across the BSR and multi-component PT
types) confer additional benefits relative to UC (aggregated
across the two types of UC)?

(2) Do benefits vary by gender? Female patients were an average of
6 years older than male patients, thus age was included as a cov-
ariate in these analyses.

(3) Are PEs that are implemented right after a cardiac event
 defmed as less than 2 months post event) more effective
 than treatment that began later? The 2 months cutoff was
 chosen because it reflects the median length of wait times for
 CR access in Canada.72

(4) Can we replicate the results of Dusseldorp et al.8 that patients
whose distress (or depression) was not reduced also did not
<table>
<thead>
<tr>
<th>Study, Year (Location)</th>
<th>Cardiac population</th>
<th>Group</th>
<th>Type of intervention</th>
<th>Age (years)</th>
<th>Women (%)</th>
<th>Tx Length/Follow-up (months)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Appels et al.,12 2005 (Netherlands)</td>
<td>PCI</td>
<td>C</td>
<td>Basic UC (n = 344)</td>
<td>53.6</td>
<td>20</td>
<td>6/18</td>
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<tr>
<td>Baumbauer et al.,13 2005 (USA)</td>
<td>CA, MI, CHD</td>
<td>C</td>
<td>Basic UC (n = 34)</td>
<td>59.9</td>
<td>31</td>
<td>2/6</td>
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<tr>
<td>Bishop et al.,14 2005 (Singapore)</td>
<td>CABG</td>
<td>E</td>
<td>Multi-component PT (n = 366)</td>
<td>54.7</td>
<td>0</td>
<td>1.5/3</td>
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<tr>
<td>Black et al.,15 1998 (USA)</td>
<td>CHD</td>
<td>C</td>
<td>Multi-component UC (n = 30)</td>
<td>60.7</td>
<td>12</td>
<td>2/21</td>
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<td>Blom et al.,16 in press (Sweden)</td>
<td>MI, PCI, CABG</td>
<td>E</td>
<td>Multi-component UC (n = 122)</td>
<td>62</td>
<td>100</td>
<td>12/17</td>
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<td>Blumethal et al.,17,18 2002 and 1997a (USA)</td>
<td>CHD+MI</td>
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<td>Multi-component UC (n = 26)</td>
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<td>4/60</td>
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<td>Blumethal et al.,19 2005 (USA)</td>
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<td>34</td>
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<td>Burgess et al.,22 1987 (USA)</td>
<td>MI</td>
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<td>Basic UC (n = 91)</td>
<td>62</td>
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<td>Claesson et al.,23 2005 (Sweden)</td>
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<td>C</td>
<td>Multi-component PT (n = 89)</td>
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<td>69.6</td>
<td>61</td>
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<td>27</td>
<td>1.5/24</td>
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<td>CHD</td>
<td>C1</td>
<td>Multi-component PT (n = 33)</td>
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<td>MI</td>
<td>C</td>
<td>Multi-component PT (n = 1245)</td>
<td>61</td>
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<td>Fielding et al.,31 1979 (UK)</td>
<td>MI</td>
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<td>34</td>
<td>12/60</td>
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<td>Frasure-Smith et al.,32,75 1997 and 2002a (Canada)</td>
<td>MI</td>
<td>C</td>
<td>Basic UC (n = 684)</td>
<td>58</td>
<td>0</td>
<td>12/60</td>
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<td>Frasure Smith et al.,33,34 1985 and 1989a (Canada)</td>
<td>MI</td>
<td>C</td>
<td>Basic UC (n = 229)</td>
<td>60</td>
<td>27</td>
<td>1.5/24</td>
</tr>
<tr>
<td>Gallagher et al.,35 1997 (UK)</td>
<td>CA</td>
<td>C</td>
<td>Basic UC (n = 225)</td>
<td>67</td>
<td>100</td>
<td>1.5/3</td>
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<tr>
<td>Gallagher et al.,36 2003 (Australia)</td>
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<td>C</td>
<td>Multi-component PT (n = 93)</td>
<td>67</td>
<td>100</td>
<td>1.5/3</td>
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<th>Experimental group characteristics</th>
<th>Gillis et al., 1993; Gortner et al., 1988 (USA)</th>
<th>Cardiac surgery</th>
<th>C</th>
<th>Basic UC (n = 81)</th>
<th>59.2</th>
<th>9</th>
<th>2/4</th>
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<td></td>
<td>Gruen et al., 1975 (USA)</td>
<td>MI</td>
<td>C</td>
<td>Basic UC (n = 37)</td>
<td>50</td>
<td>...</td>
<td>0.75/4</td>
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<td>Guzzetta et al., 1989 (USA)</td>
<td>MI</td>
<td>C</td>
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<td>12.5</td>
<td>0.07/0</td>
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<tr>
<td></td>
<td>Ibrahim et al., 1975 (USA)</td>
<td>MI</td>
<td>C</td>
<td>Basic UC (n = 60)</td>
<td>54.5</td>
<td>19.5</td>
<td>11.5/6</td>
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<td></td>
<td>Johansen et al., 2003 (Denmark)</td>
<td>MI</td>
<td>C</td>
<td>Basic UC (n = 143)</td>
<td>73.0</td>
<td>100</td>
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<td></td>
<td>Jones and West, 1996 (UK)</td>
<td>MI</td>
<td>C</td>
<td>Basic UC (n = 1160)</td>
<td>...</td>
<td>23</td>
<td>.../...</td>
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<tr>
<td></td>
<td>Janz et al., 1975; Clark et al., 1992 (USA)</td>
<td>CHD</td>
<td>C</td>
<td>Basic UC (n = 1168)</td>
<td>...</td>
<td>...</td>
<td>1.75/12</td>
</tr>
<tr>
<td></td>
<td>Kanji et al., 2004 (UK)</td>
<td>PCI</td>
<td>C</td>
<td>Basic UC (n = 29)</td>
<td>64.5</td>
<td>37</td>
<td>2/3</td>
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<tr>
<td></td>
<td>Krucoff et al., 2005 (UK)</td>
<td>PCI</td>
<td>C</td>
<td>Basic UC (n = 192)</td>
<td>66</td>
<td>29</td>
<td>.../6</td>
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<td>Liao et al., 2004 (China)</td>
<td>Heart valve surgery</td>
<td>C</td>
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<td>C</td>
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<td>Mayou et al., 2002 and 2005 (UK)</td>
<td>MI</td>
<td>C</td>
<td>Basic UC (n = 58)</td>
<td>...</td>
<td>20</td>
<td>.../12</td>
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<td>Michalsen et al., 2005 (Germany)</td>
<td>CHD</td>
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<td>Basic UC (n = 53)</td>
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<td>MI</td>
<td>C</td>
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<td>0.27/6</td>
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<td>Rahe et al., 1975; Rahe et al., 1979 (USA)</td>
<td>MI</td>
<td>C</td>
<td>Basic UC (n = 21)</td>
<td>48.3</td>
<td>8</td>
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<td>Schulte et al., 1986 (Netherlands)</td>
<td>MI</td>
<td>C</td>
<td>Basic UC (n = 16)</td>
<td>55</td>
<td>...</td>
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<td>Sebregts et al., 2005 (Netherlands)</td>
<td>MI, CABG</td>
<td>C</td>
<td>Basic UC (n = 98)</td>
<td>55.6</td>
<td>14</td>
<td>2/9</td>
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<td></td>
<td>Stern et al., 1983 (USA)</td>
<td>MI</td>
<td>C1</td>
<td>Basic UC (n = 29)</td>
<td>59</td>
<td>11</td>
<td>3/9</td>
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<td></td>
<td>Sundin et al., 2003 (Sweden)</td>
<td>PCI, CABG, MI</td>
<td>C</td>
<td>Multi-component PT (n = 35)</td>
<td>58.8</td>
<td>0</td>
<td>12/0</td>
</tr>
</tbody>
</table>
benefit in terms of mortality? We used actual observed median effect sizes of distress and depression reduction obtained from the current data to create relevant success/failure groupings.

Non-categorical data

Non-categorical outcome data were either psychological in nature (e.g., anxiety) or reflected indices of biological risk (e.g., blood pressure). The effect sizes were calculated for each outcome within each individual study, weighted for sample size and reported for all studies, aggregated across treatment conditions and genders, and (where possible) reported separately for men and women.

Results

Mortality and morbidity

As the data in Table 3 indicate, 23 studies provided data on mortality. Patients who received PT benefited in terms of short-term mortality reductions (OR 0.72; 95% CI 0.56–0.94) (Figure 2), but this effect became non-significant as follow-up lengthened. Event recurrence was reduced at long-term follow-up (OR 0.57; 95% CI 0.37–0.86) but not at short-term follow-up (OR 0.84; 95% CI 0.70–1.02).

When the analyses were repeated separately for the two genders, only 10 studies met inclusion criteria; these studies described results from 1190 women and 2034 men (Figures 3 and 4). Men benefited from PT with a 27% short-term mortality reduction but women did not (OR 0.73 vs. 1.01; 95% CI 0.51–1.05 vs. 0.46–2.23). Note that the observed OR of 0.73 for men was accompanied by a marginal significance level of \( P = 0.057 \) (Tables 4 and 5). There was a similar but weaker pattern of gender differences for morbidity.

Mortality reductions were strong for studies that initiated treatment late (72% reduction in short-term mortality; 95% CI 0.12–0.70) (Figure 5), whereas early initiation was not associated with a significant change in mortality (–13%; 95% CI 0.51–1.05 vs. 0.46–2.23). Note that the observed OR of 0.73 for men was accompanied by a marginal significance level of \( P = 0.057 \) (Tables 4 and 5). There was a similar but weaker pattern of gender differences for morbidity.

Biological risk factor and psychological variables

Univariate F-tests (aggregated for both genders and weighted for sample size) revealed that PT was statistically superior to control for reduction of heart rate (the difference in \( r \)-scores \( r \) was \(-0.21\); a minus score indicates superiority of PT over control), and it improved both social support (\( r = -0.16 \)) and quality-of-life (\( r = -0.34 \)). Treated women showed significant reductions of distress and improved social support, whereas treated men showed greater reduction of depression and improved social support. Irrespective of significance, all signs in front of the effect sizes point in the same direction, namely superiority of treatment over control.

Additional computations of zero-order correlations revealed that older participants showed less reduction in depression (\( r = -0.48; P = 0.014 \)) and anxiety (\( r = -0.43; P = 0.057 \)). Obtained correlation coefficients for reductions in various types of negative affect were highly consistent,
positively inter-correlating with r-coefficients ranging from 0.50 to 0.95, thus indicating that changes in various indices of psychological well-being moved in a synchronous manner.

Discussion

Key findings

Together our findings reveal that PT offered in addition to UC reduces mortality for at least the first 2 years. Our results also provide the first meta-analytic evidence of gender differences in outcomes, and illustrate for the first time that treatment program characteristics differentially affect outcome. Overall, PT of cardiac patients reduced mortality by 27% within 2 months of the cardiac event produced no significant short- and long-term follow-up, respectively. PT initiated post-event within 2 months of the cardiac event (as ENRICHD did) show little benefit finding that studies which initiate psychological PT soon after the cardiac event (as ENRICHD did) show little benefit than the one originally reported by Dusseldorp et al. Failure or success in the reduction of depression did not affect mortality, although this finding is in contrast to a re-analysis of the ENRICHD findings where it had been shown that successful reduction of depression also accounted for observed mortality benefits.

Overall, the conclusions are largely consistent with the ones drawn in the earlier reviews (Table 1). The previously observed reductions in mortality attributable to PT ranged from −3% to −71% and places the values obtained in this review—for follow-up of 2 years or less—approximately in the centre of this spectrum. We posit that the beneficial findings reported here are attributable to clean definitions of treatments to be included, to wide sampling of relevant studies, and to exclusion of unreliable small-n studies thus reducing opportunities for publication bias. Similar to previous reviews, the observed mortality and morbidity benefits were paired with only small-to-moderate-sized reductions in negative affect and biological risk factors. In light of the high costs of cardiac surgery and extended hospital stays, an investment of about $2000 for PT (average treatment exposure was 16.3 h, range 0.67–52.5 h) is not a prohibitive expense.

The largest trial to date (ENRICHD) was characterized by a significant reduction in depression that, however, failed to achieve superiority over controls because the UC control group had also improved. Original to our analysis is the finding that studies which initiate psychological PT soon after the cardiac event (as ENRICHD did) show little benefit...
compared with UC controls, whereas those that start PT at least 2 months post-event showed an impressive mortality reduction of 72% in the first 2 years. It is possible that patients recruited late differ in a number of ways from those recruited early. We speculate that early recruitment may capture a subgroup of patients (in treatment and control groups alike) who possess excellent resources and resilience, and who will often recover even without professional help. This latter observation is consistent with the results of Schrader et al. who showed that the prevalence of depression in post-myocardial infarction patients changes over time, revealing spontaneous improvement in some, but also worsening of depression in other patients. Therefore, screening for residual distress and depression and subsequent treatment provision might be more cost-effective than screening and offerings of PT very early after a cardiac event. This interpretation needs to be gauged against the fact that the question of effectiveness of PT as a function of timing of treatment has never been directly compared within a single study, and we believe that such a comparison is urgently needed to assure that our suggested explanation is accurate and that resulting decisions for clinical practice can be based on solid empirical data.

Limitations

Some of the conclusions drawn are hampered by the inevitable shortcomings of meta-analyses. Although we did control statistically for the existing age difference of men and women, this cannot be seen as a strong test due to the fact that in a meta-analysis only the mean age for total samples is provided, hence creating possible range restriction problems. Note, however, that for our analyses the range of mean ages was 38.2 to 73 years and thus not overly narrow.
There is widespread consensus that publication bias may exist in any meta-analysis and thus paint an overly positive picture of outcomes due to the fact that unsuccessful studies may never get published. In this meta-analysis, publication bias does not appear to be a major threat in that the three largest trials\textsuperscript{29,45,75} had been described in...
major peer-reviewed journals and had provided 71% of the patients available for analyses on mortality outcomes. All three trials reported essentially null findings and this, in turn, runs counter to a claim of positively biased outcomes.

In a related fashion, potential heterogeneity of outcomes places constraints on the ease of interpretation of findings; our analysis is no exception. The reported results were mostly based on homogeneous outcomes (Table 3) but that is not equally true for all reported analyses. Unfortunately, the meta-analytic researcher cannot change this, only report the relevant statistics and ask for caution in interpretation.

We have conducted a number of additional analyses on various aspects of treatment program design but these were unfortunately statistically underpowered and did not allow meaningful conclusions. Therefore, remaining, unresolved questions are:

(1) Is type of PT critical? Previous reviews (Table 1) suggest that it may not be of importance. In our review, only two studies were found in the ‘pure’ relaxation/biofeedback category and their samples were small to moderate in size (n = 40 and n = 377); the mortality benefits obtained for <2 years (OR = 0.38, CI 0.13–1.10, n.s.) looked promising although inconclusive. We posit that resolving this question is of critical importance for cost-effective service delivery.

(2) Is distress screening needed to avoid floor effects? Evidence for other outcome research suggests that it may be important.76

(3) How much treatment is needed for lasting benefits? Strong support for lengthy interventions was provided in an earlier, influential, large sample rehabilitation study.77

(4) Does PT add more or less benefit when the quality of UC is high vs. low? In this meta-analysis, data from these two types of control groups were ultimately aggregated into one control group because analyses separated for two types of control groups had been underpowered and thus inconclusive.

Recommendations

We suggest assessing distress and depression repeatedly throughout the CR process and offering PT primarily to those who continue to struggle with adjustment for months after the critical event. When PT is initiated, it
should continue until distress is clearly reduced. An a priori fixed length of treatment (as is typical in clinical trials) may be unsuitable for clinical practice. Furthermore, we think it is urgent to develop PTs for female cardiac patients that meet their unique needs (i.e. emotional processing, being listened to, and attention to family role issues). While men respond well to direct advice about required lifestyle changes, evidence has suggested that women do not. This gender-specific treatment need may in part be a consequence of the greater age and social isolation of female cardiac patients. A promising direction for effective PT of women is suggested via two Swedish interventions where the program had been specifically tailored for women participants. Given that the sample sizes of these two studies were substantially smaller than those of two large studies where women had failed to reap benefits from PT, the ultimate value of gender-tailored programs needs to be determined via replication in different health care environments. Nevertheless, these Swedish programs provide a beacon for further PT research with female cardiac patients.

In sum, our findings largely concur with those of earlier narrative and meta-analytic reviews. We were able to show for the first time that women with cardiac illness do not benefit from traditional PT and that early treatment initiation may not be effective.

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Appendix

Appendix 1 Search terms employed in online database search

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<thead>
<tr>
<th>Patient characteristics</th>
<th>Psychological interventions</th>
<th>Outcomes</th>
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<td>hostil*</td>
<td>autogen* train*</td>
<td>anxiety</td>
</tr>
<tr>
<td>myocard* infar*</td>
<td>biofeedback</td>
<td>distress</td>
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<td>myocard* adj5 infarct</td>
<td>cardiac rehab*</td>
<td>mortality</td>
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<tr>
<td>MI</td>
<td>relax* train*</td>
<td>morbidity</td>
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<tr>
<td>heart adj5 surgery</td>
<td>psychosocial nursing therap*</td>
<td>cholesterol</td>
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<td>coronary adj5 by pass</td>
<td>hypno*</td>
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<tr>
<td>percutaneous transluminal coronary angioplasty</td>
<td>relax* therap*</td>
<td>weight</td>
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<td>progressive muscle relax*</td>
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<td>meditat*</td>
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<td>psychologic* adj5 intervention</td>
<td>depression</td>
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<td>stress manag*</td>
<td>anger</td>
</tr>
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<td>cardiovascular adj5 disorder</td>
<td>counsel*</td>
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<td>psychoeduca*t adj5 intervention</td>
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<td>relax*</td>
<td>blood pressure</td>
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<td>behavior adj5 modif*</td>
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<td>cognit* adj5 therap*</td>
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References


