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End-of-life care pathways for improving outcomes in caring for the dying

Review information

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Abstract

Background

In many clinical areas, integrated care pathways are utilised as structured multidisciplinary care plans which detail essential steps in caring for patients with specific clinical problems. Particularly, care pathways for the dying have been developed as a model to improve the end-of-life care of all patients. They aim to ensure that the most appropriate management occurs at the most appropriate time and that it is provided by the most appropriate health professional. Clinical pathways for end-of-life care management are used widely around the world and have been regarded as the gold standard. Therefore, there is a significant need for clinicians to be informed about the utilisation of end-of-life care pathways with a systematic review.

Objectives

To assess the effects of end-of-life care pathways, compared with usual care (no pathway) or with care guided by another end-of-life care pathway across all healthcare settings (e.g. hospitals, residential aged care facilities, community).

Search strategy

The Cochrane Register of controlled Trials (CENTRAL), the Pain, Palliative and Supportive Care Review group specialised register, MEDLINE, EMBASE, review articles and reference lists of relevant articles were searched. The search was carried out in September 2009.

Selection criteria

All randomised controlled trials (RCTs), quasi-randomised trial or high quality controlled before and after studies

comparing use versus non-use of an end-of-life care pathway in caring for the dying.

Data collection and analysis

Results of searches were reviewed against the pre-determined criteria for inclusion by two review authors.

Main results

The search identified 920 potentially relevant titles, but no studies met criteria for inclusion in the review.

Authors' conclusions

Without further available evidence, recommendations for the use of end-of-life pathways in caring for the dying cannot be made. RCTs or other well designed controlled studies are needed for evaluating the use of end-of-life care pathways in caring for dying people.

Plain language summary

End-of-life care pathways for the dying

End-of-life pathways are used for people who are in the last days of their life to guide care; aid decision making; and provide efficient care. This review examined whether using end-of-life care pathways in caring for the dying was effective. No studies meeting the inclusion criteria were found that used an end-of-life care pathway in caring for the dying. Therefore, there is insufficient data at present to make recommendations regarding the use of such end-of-life care pathways for the dying.

Background

Description of the condition

It is well recognised that populations in developed countries are ageing ([United Nations 2002](#)). As populations age, the pattern of diseases that people die from also changes ([WHO 2004](#)). With advanced ageing, there is an increased risk of death from chronic diseases such as cancer and heart failure ([WHO 2001](#)). For example, cancer was estimated to account for about 7 million deaths (12% of all deaths) worldwide in 2000 ([WHO 2001](#)). Therefore, palliative care has been identified as one of the worldwide public health priorities due to the ageing population ([WHO 2004](#)). While palliative care is concerned with "the quality of life of patients and families who face life-threatening illness, by providing pain and symptom relief, spiritual and psychosocial support from diagnosis to the end of life and bereavement" ([WHO 2009](#)), end-of-life care focuses on the last days and hours of life ([Lunney 2003](#)). The need to provide high quality end-of-life care is essential. The needs of dying people may include, but are not limited to, knowing when death is coming, understanding what can be expected, being able to maintain a sense of control and having their wishes given preference, having access to information and excellent care and having access to spiritual and emotional support as required ([Steinhauser 2000](#); [Steinhauser 2001](#)). Quality end-of-life care may vary from person to person and may be difficult to define and accurately measure. However, such care should at least include the following domains: quality of life, physical symptoms, emotional and cognitive symptoms, advanced care planning, functional status, spirituality, grief and bereavement; satisfaction and quality of care, as well as caregiver well being ([Mularski 2007](#)).

Obstacles to quality end-of-life care have also been identified and may include failure to recognise treatment futility, lack of communication among decision makers, no agreement on a course of end-of-life care, and failure to implement a timely end-of-life plan of care ([Travis 2002](#)). In recent years, there has been a variety of initiatives developed worldwide to target such issues by developing systemic approaches towards end-of-life care. These initiatives include programmes such as the National End of Life Care Programme ([Department of Health 2008](#)), Gold Standards Framework in Care Homes ([Badger 2007](#)) and the Liverpool Care Pathway (LCP) ([Ellershaw 1997](#); [Ellershaw 2003](#)).

Description of the intervention

Integrated care pathways are documents which outline the essential steps of multidisciplinary care in addressing a specific clinical problem. They can be used to introduce clinical guidelines and systematic audits of clinical practice ([Hockley 2005](#)). The LCP is an example of an integrated care pathway specifically for the dying phase of palliation.

Historically, dying patients receiving general hospital care tended to lack adequate attention from senior medical staff and nursing staff ([Mills 1994](#)). The quality of symptom control and basic nursing care were considered to be inadequate ([Mills 1994](#)). It was thought that much could be learned from the way patients were cared for in the hospice movement ([Mills 1994](#)). The LCP was an example of strategies developed by the Royal Liverpool University Trust and the Marie Curie Centre Liverpool ([Ellershaw 1997](#); [Ellershaw 2003](#)) based on the care received by those in the hospice setting. Other objectives of the pathway were to promote cost-effective health care by appropriate prescribing, and avoiding crisis interventions and inappropriate hospital admissions. The document is patient-centred and focuses on the holistic needs of people who are dying. It incorporates the physical, psychological, social, spiritual and religious aspects of care ([Ellershaw 2007](#)). The LCP defines 19 goals considered essential in the management of dying patients and for the care of their relatives/carers after death ([Ellershaw 1997](#); [Ellershaw 2003](#)). These goals were established with the issues identified from surveys, focus groups, expert opinion and consensus best practice.

Later, several other groups developed care pathways for the dying based on the concept of Ellershaw and colleagues ([Bookbinder 2005](#); [Fowell 2002](#); [Pooler 2003](#)). Whilst the professional conjecture is that end-of-life care pathways promote best possible patient outcomes ([Ellershaw 2007](#)), recent speculations have suggested possible adverse effects. These controversies included premature use of the pathway leading to death due to the premature diagnosis of imminent death, the care pathway masking the signs in improvement in patients and causing carers' dissatisfaction ([Delvin 2009](#); [Smith 2009](#)). Therefore, a systematic review is warranted to substantiate claims as to whether the end-of-life care pathways are beneficial or harmful for dying patients and their carers.

How the intervention might work

In many clinical areas, integrated care pathways are utilised as structured multidisciplinary care plans which detail essential steps in caring for patients with specific clinical problems ([Campbell 1998](#)). Care pathways for the dying have been developed as a model to improve the end-of-life care of all patients. They ensure that the most appropriate management occurs at the most appropriate time and that it is provided by the most appropriate health professional.

Why it is important to do this review

Two systematic reviews report that clinical pathways enhance efficiency of care without adverse effects on outcomes amongst patients who undergo gastrointestinal surgery ([Lemmens 2008](#)) and show a significant length of stay reduction in patients who undergo invasive procedures ([Rotter 2008](#)). Both of these systematic reviews included evidence involving designs such as RCTs and other types of controlled studies.

In contrast, the findings from a Cochrane systematic review reported that there was no significant benefit in functional outcome and patient satisfaction and that quality of life might actually be made worse for patients following stroke care pathways ([Kawn 2004](#)). Therefore, clinical pathways seem to be beneficial for managing certain clinical problems, but not all.

Clinical pathways for end-of-life care management are used widely around the world and have been set as the main part of the End-of-Life Care Strategy by the Department of Health in the UK ([Department of Health 2008](#); [Veerbeek 2006](#)) as well as being the Gold Standard Framework (GSF) by the National Health Service ([National Health Service 2005](#)). There is a significant need for clinicians to be informed about the utilisation of end-of-life care pathways with a systematic review.

Objectives

We aimed to assess the effects of end-of-life care pathways, compared with usual care or with care guided by another end-of-life care pathway across all healthcare settings (hospitals, residential aged care facilities, community). In particular, we aimed to assess the effects on symptom severity and quality of life of people who are dying and/or those related to the care such as families, caregivers and health professionals.

Methods

Criteria for considering studies for this review

Types of studies

We aimed to include clinical trials in which the effect of the end-of-life care pathway could be compared with a control group which receives usual care or with trials comparing one end-of-life care pathway with another end-of-life care pathway. We planned to include randomised controlled trials (RCTs), cluster RCTs and quasi-RCTs.

If limited RCTs and quasi-RCTs were available, we planned to consider including controlled before-and-after studies. The review authors have adopted the criteria for inclusion of controlled before-and-after studies from the Cochrane Effective Practice and Organisation of Care Review Group Guidelines ([EPOC 2002](#)). These criteria include (1) contemporaneous data collection, (2) appropriate choice of control site and (3) a minimum of two intervention sites and two control sites. We did not plan to include any non-controlled studies ([EPOC 2002](#)). The analysis for randomised and non-randomised studies were to have been undertaken separately because non-randomised comparisons may overestimate treatment effects ([Chalmers 1983](#); [Sacks 1982](#)), and the size and direction of the bias can be unpredictable ([Deeks 2003](#)).

Types of participants

Participants in the included studies were to be patients and families who received care guided by an end-of-life care pathway. There was no age limitation on participants. Participants included may have had different diseases such as cancer or organ failure. However, participants who received interventions must have been receiving care guided by an end-of-life care pathway for their last days and hours of life. There was to have been no restriction on age of the patient, diagnosis or setting (hospital, home, nursing home). There was to have been no age limit for participants included in this review.

Types of interventions

The planned comparisons were:

- intervention (receiving care guided by an end-of-life care pathway) versus usual care.
- intervention A (pathway A) versus intervention B (pathway B).

An end-of-life care pathway may have been part of a larger intervention, these studies were only to be included if the effect of the pathway could be isolated.

Types of outcome measures

Primary outcomes

- Physical symptom severity (measured by any instrument used by the author such as Edmonton Symptom Assessment Scale ([Bruera 1991](#)), Memorial Symptom Assessment Scale ([Portenoy 1994](#)).
- Psychological symptom severity (measured by any instrument used by the author. For example, Hospital Anxiety and Depression Scale ([Zigmond 1983](#)).
- Quality of life (measured by any instrument used by the author such as McGill Quality of Life Questionnaire ([Cohen 1995](#)).
- Harms (any adverse effects as determined by the researchers, health professionals or carers/families).

Secondary outcomes

- Advanced care planning (as measured by whether advanced care planning has happened or not).
- Communication between healthcare teams and families (as measured by the occurrence of any family meetings).
- Caregivers well being.
- Grief and bereavement.
- Patient/staff/caregivers' satisfaction.
- Staff confidence.
- Cost of intervention.
- Cost of care.
- Medication/treatment use.
- Spiritual needs.

We planned to include any tools used by the authors of the included studies. The validity and reliability of the tools used were to have been discussed in appraisal of studies.

Search methods for identification of studies

Electronic searches

The Pain, Palliative and Supportive Care Review group searched their Specialised Register.

We searched:

- the Cochrane Central Register of Controlled Trials (CENTRAL) on *The Cochrane Library*,
- MEDLINE (1950 to 7/9/2009),
- EMBASE (1980 to 7/9/2009),
- PsycINFO (1980 to 7/9/2009),
- CINAHL (1982 to 7/9/2009),
- Web of Science (7/9/2009),
- ProQuest Dissertations and Theses (7/9/2009),
- Google World Wide Web (7/9/2009).

The search strategy was developed to comprise searches both for keywords and medical subject headings under existing database organizational schemes. The strategy for MEDLINE (Ovid SP) is presented in [Appendix 1](#).

There was no restriction by language. Foreign language abstracts were initially translated for the application of the inclusion and exclusion criteria, and where necessary the methods, results and discussion sections would have been translated for inclusion in the review.

Searching other resources

We searched the reference lists of any relevant reviews or other studies, scanning paper issues of journals relevant to interventions of end-of-life care pathway and scanning abstracts from relevant conference proceedings. We also contacted experts in the field and authors of included studies for advice as to other relevant studies.

We used Google to search the World Wide Web, Caresearch (www.caresearch.com.au), the ProQuest Dissertations and Theses database for grey literature and conference abstracts. We searched databases in TrialsCentral (www.trialscentral.org), the WHO Clinical Trial Search Portal (www.who.int/trealsearch) and Current Controlled Trials (www.controlled-trials.com) to identify ongoing or recently completed studies. We planned, if applicable, to present relevant ongoing studies in a table in the review.

Data collection and analysis

Selection of studies

Two review authors pre-screened all search results (titles and abstracts) for possible inclusion, and those selected by either or both authors were subject to full-text assessment. Two review authors independently assessed the selected articles for inclusion. We had planned to resolve any discrepancies by consensus, overseen

by a third review author acting as arbiter, with approval by one review author and the arbiter being sufficient. We had also planned to list those studies excluded after full-text assessment in the table '[Characteristics of excluded studies](#)', giving reasons for exclusion.

Data extraction and management

We developed a data extraction form based on the Cochrane Pain, Palliative and Supportive Care Review Group's template. We planned to extract the following main sets of data from each included study:

- lead author;
- date;
- study participant inclusion criteria;
- participants (participant diagnoses/condition(s) and demographics: race/ethnicity, gender, religion/culture, socioeconomic status, age);
- study design and timetable; randomisation; allocation concealment;
- interventions (end-of-life care pathway type);
- intervention setting (hospital, home, residential aged care facilities);
- numbers of participants in each trial arm, withdrawals and dropouts;
- outcome measures; time(s) at which outcomes were assessed

At least two review authors were to have independently extracted data in to the data extraction form. Any discrepancies were to have been referred to a third review author and any errors or inconsistencies resolved. The first review author was to have entered the data into RevMan, with another review author checking accuracy of the data entered.

Assessment of risk of bias in included studies

We intended to assess and report on the risk of bias of included studies in accordance with the guidelines in the Cochrane Handbook for Systematic Reviews of Interventions ([Higgins 2008](#)), which recommends the explicit reporting of the following individual domains:

The criteria for RCTs were:

- sequence generation;
- allocation concealment;
- blinding of participants, personnel and outcome assessors (assessed for each main outcome or class of outcome);
- incomplete outcome data (assessed for each main outcome or class of outcome);
- selective outcome reporting;
- other sources of bias.

The criteria for CBA studies were:

- baseline measurement of outcomes;
- baseline characteristics of studies using second site as control;
- protection against exclusion or selection bias;
- protection against contamination;
- reliable primary outcomes measures;
- appropriate analysis of data.

We were also to have examined and reported the following:

- validation and reliability of outcome measures;
- whether the study obtained ethics committee approval and ensured informed consent for participation;
- use of standardised protocols for information delivery. We were to have checked for consistency of the delivery of interventions where possible.

Two review authors were to have independently assessed the risk of bias in included studies, with any disagreements resolved by discussion and consensus, and with a third review author acting as arbiter. We planned to present our assessment in risk of bias tables for each included study. We were to contact study authors for additional information about the study methods as necessary. We were to have incorporated the results of the risk of bias assessment into the review through narrative description and commentary about each of the items mentioned.

Measures of treatment effect

For individual studies, effect measures for categorical outcomes were to include relative risk (RR) with their 95% confidence intervals (CI). For statistically significant effects, number needed to treat to benefit (NNT) were to have been calculated. If possible for continuous outcomes, the effect measure was to have been mean difference (MD) or, if the scale of measurement differed across trials, standardized mean difference (SMD), each with its 95% CI. For meta-analyses (see below), for categorical outcomes, typical estimates of RR with their 95% CI were to have been calculated; and for continuous outcomes, the weighted mean difference (WMD) or a summary estimate for SMD, each with its 95% CI, was to have been calculated.

Data would have been analysed using the Cochrane Collaboration's Review Manager 5 software.

Unit of analysis issues

Unit of analysis issues were to have been checked if cluster randomised trials were included. Cross over trials were not expected for this type of intervention due to the end-of-life pathway nature. If cluster randomised trials had been identified, we were to have reported the intra-cluster correlation coefficient and adjust for clustering if possible. Or we were to have presented the cluster randomised studies as point estimates of the intervention effect without any statistical analysis or confidence intervals.

Dealing with missing data

If some outcome data remained missing despite our attempts to obtain complete outcome data from authors, we would have performed an available-case analysis, based on the numbers of participants for whom outcome data were known. If standard deviations were missing, we would have imputed them from other studies, or where possible, computed them from standard errors using the formula $SD = SE \times \sqrt{N}$, where these were available ([Higgins 2008](#)). We also planned to report on levels of drop outs in the intervention and comparison groups as an indicator of 'acceptability' of the intervention, and the likelihood of bias.

Assessment of heterogeneity

Heterogeneity would have been tested using the Chi² statistics and any heterogeneity was to have been further quantified with the I² statistic (which describes the percentage of the variability in effect estimates that is due to heterogeneity rather than sampling error). A value greater than 50% was to have been considered as representing substantial heterogeneity ([Higgins 2008](#)).

Assessment of reporting biases

Reporting bias was to have been assessed using guidelines in Cochrane Handbook for Systematic Reviews of Interventions ([Higgins 2008](#)). We did not expect to find a large number of studies, so we thought it was unlikely that publication or inclusion bias would be assessed. However, if enough studies were available to do a meaningful assessment of publication bias, a funnel plot was planned.

Data synthesis

If studies had been sufficiently similar in terms of population, inclusion criteria, interventions and/or outcomes (including the time(s) at which these are assessed), we would have considered pooling the data statistically using meta-analysis. We would have reported the results of the individual trials separately where the outcome data was unsuitable for meta analysis. We planned to use fixed-effect models when populations measures were similar and random-effects models where population parameters varied from study to study.

Subgroup analysis and investigation of heterogeneity

Subgroup analyses were to have been conducted if sufficient data could support the analyses. Subgroups may have included disease types and settings where care was received.

Sensitivity analysis

If there were other sources of heterogeneity, we planned to explore further by using sensitivity analysis to determine the effects of the end-of-life care pathways, overall methodological quality and use of ITT analysis. Studies with high attrition rates (over 50%) would have been removed from the meta-analysis to determine whether the results would be significantly different without them.

Results

Description of studies

See: '[Characteristics of included studies](#)'; '[Characteristics of excluded studies](#)'; '[Characteristics of ongoing studies](#)'.

Results of the search

In total, 920 titles and abstracts were retrieved in electronic format and assessed.

Included studies

No studies fulfilled the study eligibility criteria.

Excluded studies

Twenty eight studies were excluded because the study designs did not meet the criteria for included studies. Three controlled before and after studies were excluded because they did not meet the minimum criteria to be included in this review. These criteria include (1) contemporaneous data collection, (2) appropriate choice of control site and (3) a minimum of two intervention sites and two control sites ([EPOC 2002](#)).

Risk of bias in included studies

There were no included studies, so bias could not be evaluated.

Effects of interventions

There were no included studies, so effects could not be evaluated.

Discussion

No RCTs, quasi-experimental studies or controlled before and after studies meeting our eligibility criteria were identified for this review.

The results of a number of case series and non-eligible controlled before-and-after studies indicate that end-of-life care pathways may have the potential to improve symptom management ([Bailey 2005](#); [Veerbeek 2008](#)), clinical documentation and assessment ([Bookbinder 2005](#); [Luhrs 2005](#); [Veerbeek 2008](#)), knowledge of end-of-life care amongst internal medicine students ([Okon 2004](#)), prescription of medications for end-of-life ([Bailey 2005](#); [Mirando 2005](#)), and bereavement levels of relatives ([Veerbeek 2008a](#)). However, the effects of pathways are difficult to ascertain from these designs. It is also worth noting that, no studies have reported the adverse effects of any end-of-life care pathway.

Although end-of-life care pathways have been recognised to be the gold standard for practice and are widely used; there is a lack of sound evidence supporting such practice. In the UK, the registered users of the LCP reached over 1800 health care institutions across all settings including hospitals, hospices, care homes and community services ([MCPCIL 2009](#)). This may be because of the ethical issues around randomising patients to a study arm that does not include an intervention which many clinicians, irrespective of the lack of RCTs, believe to be effective.

It is well accepted that designing and conducting trials involving the dying is difficult and challenging due to methodological and ethical issues ([Fowell 2004](#); [Karlawish 2003](#)). These issues may include difficult patient recruitment due to the patient being too ill to participate or unable to give informed consent, or the heterogeneous nature of palliative populations ([Addington-Hall 2007](#)). However, researchers should attempt to investigate end-of-life interventions with the most rigorous research methodology possible. For example, cluster randomised trials across multiple centres may be considered. A recent feasibility study revealed that cluster randomised trials are possible and may be more effective in recruiting patients in this population than randomised consent ([Fowell 2006](#)). Randomised consent requires informed consent after randomisation, but only if the patient is to receive the experimental treatment ([Zelen 1990](#)). Moreover, a range of other strategies can also be considered to make clinical trials possible. These include designing a shorter term study, limiting number of outcomes, undertaking frequent follow-ups, advanced consent and proxy consent where appropriate for studies involving this population ([Reyna 2008](#)).

Authors' conclusions

Implications for practice

Due to the lack of available evidence, recommendations for the use of end-of-life pathways in caring for the dying cannot be made at the present time. With regards to evidence on the use of other clinical care pathways, it appears that clinical pathways are effective in managing certain clinical problems and not all. Therefore, we do not recommend decision making based on indirect evidence. Until there is evidence indicating harms caused by the end-of-life care pathways, the use of the end-of-life care pathways may be continued.

Implications for research

RCTs or other well designed controlled studies are needed for the evaluation of the use of end-of-life care pathways in caring for dying people. In future studies, outcome measures should include the outcomes of interest in this review in relation to patients, families, caregivers and health professionals. These may include patients' symptom control, harms, communication between health care team and families, caregivers well being, grief and bereavement, staff and caregivers' satisfaction, staff confidence, cost of intervention, cost of care and medication use. Further, investigations of the effects of such pathways for specific populations are warranted. These specific populations may include, but are not limited to, children and patients with end-stage organ failure or dementia.

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Contributions of authors

Writing the protocol: RC, JW
Developing the search strategy: RC
Searching for trials: RC, JW
Selecting trials: RC, JW
Data entry: RC, JW
Analysis: RC, JW
Interpreting analysis: RC, JW
Drafting final review: RC, JW

Updating the review: RC

Declarations of interest

None known

Differences between protocol and review

Published notes

Characteristics of studies

Characteristics of included studies

Footnotes

Characteristics of excluded studies

Bailey 2005

Reason for exclusion	Before and after study (without control)
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Bookbinder 2005

Reason for exclusion	Controlled before and after study: Non-contemporaneous data collection, non-comparable sampling
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Chaplin 2009

Reason for exclusion	Non-experimental study: case report
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Ellershaw 1997

Reason for exclusion	Non-experimental study: case report
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Ellershaw 2001

Reason for exclusion	Non-experimental study: audit
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Ellershaw 2003

Reason for exclusion	Review
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Ellershaw 2007

Reason for exclusion	Letter
----------------------	--------

Fowell 2002

Reason for exclusion	Non-experimental study: audit
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Fowell 2003

Reason for exclusion	Non-experimental study: audit
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Hardy 2007

Reason for exclusion	Non-experimental study: audit
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Hockley 2005

Reason for exclusion	Non-experimental study: qualitative study
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Jack 2003

Reason for exclusion	Non-experimental study: qualitative study/focus groups
----------------------	--

Johnson 2004

Reason for exclusion	Non-experimental study: cross sectional survey study
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Luhrs 2005

Reason for exclusion	Controlled before and after study: Non-contemporaneous data collection, non-comparable sampling and does not have at least 2 intervention and 2 control sites
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Main 2006

Reason for exclusion	Non-experimental study: case report
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Matthews 2006

Reason for exclusion	Non-experimental study: audit
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Mellor 2004

Reason for exclusion	Qualitative study
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Mirando 2005

Reason for exclusion	Non-experimental study: audit
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Okon 2004

Reason for exclusion	Controlled before and after study: does not have at least 2 intervention and 2 control sites
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Osterlind 2008

Reason for exclusion	Non-experimental study: qualitative study
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Peterson 2000

Reason for exclusion	Non-experimental study: case report
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Pooler 2003

Reason for exclusion	Non-experimental study: audit
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Rose 2006

Reason for exclusion	Non-experimental study: audit
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Taylor 2007

Reason for exclusion	Before and after study (without control)
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Thompson-Hill 2009

Reason for exclusion	Before and after study (without control)
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Veerbeek 2006

Reason for exclusion	Non-experimental study: audit
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Veerbeek 2008

Reason for exclusion	Before and after study (without control)
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Veerbeek 2008a

Reason for exclusion	Before and after study (without control)
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Footnotes

Characteristics of studies awaiting classification

Footnotes

Characteristics of ongoing studies

Footnotes

Summary of findings tables

Additional tables

References to studies

Included studies

Excluded studies

Bailey 2005

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Other published versions of this review

Classification pending references

Data and analyses

Figures

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External sources

- No sources of support provided

Feedback

Appendices

1 MEDLINE search strategy

Database: Ovid MEDLINE(R)

- 1 Palliative Care/ or palliat\$.mp.
- 2 end-of-life.mp.
- 3 terminally ill.mp. or Terminally Ill/
- 4 dying.mp.
- 5 hospice.mp. or Hospices/
- 6 end-stage.mp.
- 7 or/1-6
- 8 Critical Pathways/
- 9 ((clinical or critical or care) adj path\$).mp.
- 10 (care adj (map\$ or plan\$)).mp.
- 11 exp Guideline/
- 12 Health Planning Guidelines/
- 13 Guideline Adherence/
- 14 (compliance adj (protocol? or policy or guideline?)).mp.
- 15 (guideline? adj2 (introduc\$ or issu\$ or impact or effect? or disseminat\$ or distribut\$ or implement\$)).mp.
- 16 nursing protocol?.mp.
- 17 professional standard\$.mp.
- 18 (practice guidelin\$ or practice protocol\$ or clinical practice guidelin\$).mp.
- 19 or/9-18
- 20 Guideline.pt.
- 21 randomized controlled trial.pt.
- 22 controlled clinical trial.pt.
- 23 Intervention Studies/
- 24 experiment\$.mp.
- 25 (time adj series).mp.
- 26 (pre test or pretest or post test or posttest).mp.
- 27 Random Allocation/
- 28 impact.mp.
- 29 intervention?.mp.
- 30 Evaluation Studies/
- 31 Comparative Study.pt.
- 32 Humans/
- 33 or/ 20-31
- 34 7 and 19 and 32 and 33

Graphs